IX. *On the means adopted in the British Colonial Magnetic Observatories for determining the Absolute Values, Secular Change, and Annual Variation of the Magnetic Force. By Lieut.-Colonel* EDWARD SABINE, *R.A., For. Sec. R.S.*

Received February 6,—Read April 25, 1850.

SO many of the magnetic observatories which professed to adopt and pursue the system of observation recommended by the Royal Society have confined themselves, apparently even in what they have attempted, to investigations into the *diurnal* fluctuations of the magnetic elements, and into what have been called magnetic *disturbances*, that it may not be inappropriate to recall to recollection the far more extensive system of observation which it was the purpose of the Royal Society to institute.

The diurnal variations and the magnetic disturbances form, it is true, a portion, and an important portion, of the objects contemplated; but they can only be regarded as the effects of minor forces, superimposed upon the far more powerful and important agency of the terrestrial magnetism itself, and from which they are probably distinct both in their nature and in their origin.

In the provision of instruments and in the elaborate instructions contained in the Report of the Royal Society for the determination of the *absolute values* and *secular changes*, as well as of the periodical variations of the three magnetic elements, it was obviously the purpose to comprehend as the objects of investigation the *whole* of the phenomena by which the magnetic state of our planet is either permanently or temporarily affected; and particularly the permanent and systematic part of the phenomena which results from that more powerful agency to which the name of "Terrestrial Magnetism" more strictly belongs.

The determination of the mean numerical values of the elements of terrestrial magnetism in direction and force at different points of the earth's surface, (the force being expressed in absolute measure, intelligible consequently to future generations however distant, and conveying to them a knowledge of the present magnetic state of the globe,) and the determination of the nature and amount of the secular changes which the elements are at present undergoing, are the first steps in that great inductive inquiry, by which it may be hoped that the inhabitants of the globe may at some date, perhaps not very distant, obtain a complete knowledge of the laws of the phenomena of terrestrial magnetism, and possibly gain an insight into the physical causes of one of the most remarkable forces by which our planet is affected.

It is true that the instruments proposed by the Royal Society, as well as the instructions for their use, were found on the experience of the first few years to be not fully adequate to the satisfactory attainment of all the purposes for which they had been designed. This was especially the case in regard to the Bifilar Magnetometer, the magnet of which, 15 inches long, was directed to be used, in conjunction with the magnet of the Declinometer (which was of similar length), for the determination of the horizontal component of the magnetic force. It was soon found that at a suitable distance from each other, necessarily regulated by their length, the magnets were too far apart in the experiments of deflection, to produce angles of deflection bearing a sufficient proportion to the unavoidable errors of observation; whence the results so obtained were charged with a probable error far too great for the purposes contemplated. It was also found, that, whenever the magnets of the Bifilar and Declinometer were thus employed, a break was unavoidably made in the series with the Bifilar, on the continuity of which its value as a differential instrument, and therefore as an instrument for determining secular changes, must necessarily in great measure depend. To obviate these inconveniences, and particularly the latter, "Revised Instructions" were drawn up and circulated by the Royal Society, recommending the employment of an auxiliary apparatus, by which the experiments of vibration and deflection required for the determination of the horizontal component of the Force might be made in a separate apartment, and therefore without disturbing the magnetometers of the observatory, which from thenceforward would be employed solely as differential instruments. The magnets of the auxiliary apparatus were reduced from 15 inches to 12 and 9 inches in length. It was found however on trial, that when these magnets were placed at the minimum admissible distance from each other corresponding to their length, their distance apart was still too great to produce angles of deflection of sufficient magnitude, to lessen the probable error of the results to an amount, which would enable the secular change from month to month, or even from year to year, to be ascertained by their means within any reasonable period of observation. A few months' additional experience also served to show that when the Bifilar was thus set free to be appropriated wholly as a differential instrument, and when the magnet was left perfectly undisturbed, the conclusions which might be derived from it were still subject to two instrumental irregularities, which did not conveniently admit of elimination by any known process of experiment, and which prevented the observations made with the Bifilar from being strictly comparable with each other for more than very short intervals of time. One of these sources of irregularity had been anticipated, and it was hoped might have been surmounted; but the other had been apparently wholly unforeseen. The first consisted in the liability of the magnet bar to lose a portion of its magnetism. It was hoped if the magnet were entirely undisturbed by removals, that after a few months it might gradually attain a state in which its magnetism might undergo no further change (at least while it remained undisturbed), and that even whilst the period of change continued, the loss might be of a sufficiently uniform character to admit of its being allowed for. This has however been by no means found to be the case with the magnets employed in the different Bifilars of the Colonial Observatories, whose loss of magnetism appears to be subject to no general or systematic law, and even occasionally to intermit and to recommence without any apparent or discernible cause. To eliminate the effect of the loss, the magnetic moment of the bar would require therefore to be examined from time to time, and at short intervals; but the removal of the magnet for this purpose would break the connexion and thus interrupt the continuity of the series.

The source of the other irregularity in the indications of the Bifilar, and which does not appear to have been anticipated, is still somewhat obscure: the effect is of the opposite character to that of the loss of magnetism in the magnet bar; and the position of equilibrium of the bar has been in more than one instance so much affected by it, that the bar has in a few months progressively passed out of the field of view of the telescope. The position of equilibrium is determined on the one side by the two variables, the magnetism of the earth and that of the bar; and on the other side by the supposed constants, the weight of the bar, the length and distance between the parallel suspension wires, and the angle of torsion. The weight of the bar cannot alter; the angle of torsion and the distance between the wires, observed before and after the change in the instances referred to, were ascertained to have undergone no alteration. A lengthening of the suspension wire has been therefore supposed to be the cause of the irregularity in question, the effect being precisely that which would be produced by an elongation of the wire; but no direct proof of this has yet been obtained, because when the Bifilars were first adjusted no such effect was anticipated, and the exact length of the suspension wire at the period of the first adjustment appears to have been in no case measured*.

The Bifilar being thus found to be affected by two sources of instrumental irregularity opposite in character, neither of which admitted of being satisfactorily eliminated, the comparability of the differential results obtained with that instrument, constructed according to the specifications in the Instructions of the Royal Society, could only be relied on for short intervals, and they would consequently admit of no certain inference being drawn from them in respect to secular changes.

The observations made during the first years of the Colonial Observatories having thus failed in accomplishing a very important, if not the most important, part of the objects contemplated, I requested Captain RIDDELL, who was at that time my assistant in the superintendence of the British Colonial Magnetic Observatories, to make

* The Toronto Bifilar was adjusted on the 25th February 1843, with the intention that it should remain undisturbed. Between this date and the 11th of October of the same year the reading of the scale had altered 470 scale divisions, equivalent (approximately) to ·044 parts of the force. At Hobarton the Bifilar was adjusted, with the intention of its being left undisturbed, on the 16th of July, 1843. On the 1st of March, 1846, the scale reading had altered 78 scale divisions, equivalent (approximately) to ·017 parts of the force. The alterations in both cases are in the opposite direction to what would be occasioned by a loss of force in the magnet bar, and must be regarded as the excess of one instrumental irregularity over a second, the real value of either being unknown.

such modifications in the Portable Magnetometer originally devised by Professor WILHELM WEBER*, the magnets of which were only from 3 to 4 inches in length, as would be likely to remedy several practical defects which experience had pointed out in that instrument; and to draw up such instructions for its use as might make it, when thus modified, an efficient instrument for effecting those purposes, originally prescribed by the Royal Society, which had hitherto failed of accomplishment; viz. the determination of the absolute values, secular changes, and annual variations of the horizontal component of the magnetic force. The modifications thus introduced and the methods of observation recommended, are contained in a work entitled "MAGNETICAL INSTRUCTIONS *for the use of Portable Instruments adapted for Magnetical Surveys and Portable Observatories, and for the use of a set of small Instruments adapted for a fixed Magnetic Observatory; with Forms for the Registry of Magnetical and Meteorological Observations*;" in which work, various other forms of instruments, suited for particular magnetic purposes, are also described, together with the best methods of using them, and of determining the several constants required in calculating the final results. The Portable Unifilar Magnetometer, with the modifications thus introduced, was shown to the magneticians assembled at the meeting of the British Association at Cambridge in 1845; and Captain RIDDELL's Manual, printed by the authority of Government, has been extensively circulated amongst those persons who are engaged in magnetic researches, and has been found extremely useful, I believe, by those who have consulted it. A statement was made at the meeting of the British Association in 1845 of what had then been accomplished by the Colonial Observatories, and also of what still remained to be accomplished, to fulfil the objects for which the institution of those establishments had been recommended jointly by the Royal Society and British Association; and the probability was shown of speedily fulfilling *all* those objects by the means which had been adopted. Upon this statement a further but limited continuance of the observatories was recommended to Government and sanctioned. I am desirous of taking the earliest opportunity in my power of laying before the Royal Society a statement of the success which has attended the employment of the means thus referred to; because I am in hopes it may be an inducement to other observatories, which have either been instituted for the purpose of cooperating in the plan proposed by the Royal Society, or who have expressed the intention or desire of cooperating, to persevere in the fulfilment of *all* the objects originally contemplated; either by the adoption of the means which will be now described, or by the invention and employment of others which may serve the purpose as well or still more effectually. I shall avail myself of the observations which have been made at the Colonial Observatory of Toronto in Canada, under the direction of Captain

* A description of this instrument, translated by Mrs. SABINE, partly from an original account printed by M. WEBER in the 'Resultate des Magnetischen Vereins,' and partly from manuscript communications kindly furnished by M. WEBER himself, was presented to the Editor of TAYLOR's Scientific Memoirs, and forms art. XVIII. in the second volume of that work.

Lefroy, R.A., F.R.S., for the purpose of illustrating the objects which I have in view. I might also have availed myself of similar observations made at the observatory at Hobarton in Van Diemen Island, under the direction of Commander Kay, R.N., F.R.S., but for the fear of encumbering this communication with too much detail.

The magnets employed for the monthly series of observations on the absolute horizontal force at Toronto are solid cylinders of 0·3 inch diameter; the suspension magnet being 3 inches and the deflecting magnet 3·67 inches in length. The same magnets have been used throughout the series. The observations are made about the same period in every month, and are extended over three days, usually the 16th, 17th and 18th of the month. Three distances are employed, the least being 1 foot, and the greatest 1 foot and four-tenths from the centre of the suspended magnet. The deflections are read on a circle of 6 inches diameter, having two verniers reading to 20″. The deflections vary, according to the distance, from 6° to 10°. The reading telescope is attached to and moves with the azimuth circle; the deflecting magnet is therefore always perpendicular to the suspended magnet when the deflections caused by the latter are read off. The deflecting magnet is suspended for vibration in a stirrup with a mirror, in a detached wooden box, by a silk thread, of which the line of detorsion is brought approximately into the magnetic meridian. The time of vibration is determined by the mean of 300 vibrations in very small arcs, the commencing arc being always the same, *i. e.* 50′, and a correction for the arc is applied. Actual changes in the horizontal force of the earth occurring between the two parts of the experiment, *i. e.* between the experiments of deflection and vibration, are eliminated by a correction derived from the Observatory Bifilar, which is read off concurrently with the deflections and vibrations, and both are reduced by this means to what they would have been, had the horizontal force, at the time of each observation, coincided with the mean horizontal force shown by the Bifilar on the same day. The reduction might obviously have been made with equal convenience to the mean reading of the Bifilar in the *month*; but this reduction would have involved the question of the dependence to be placed on the Bifilar itself for longer periods than for a few hours; and as the absolute determinations extend in every month over three days, I have preferred to keep their results independent of this reduction, except in two instances, viz. October and November 1848, when disturbances of unusual magnitude and continuance lessened the force on the days of observation to a more than ordinary degree. In those two instances therefore the results of the absolute determinations have been reduced to the mean reading of the Bifilar in the respective *months*, instead of its reading on the respective *days* of observation.

Differences of temperature occurring in the experiments of deflection and vibration have been eliminated by a temperature correction applied to the deflecting magnet, in which the coefficient has been very carefully determined by the usual process directed in the Revised Instructions of the Royal Society.

The constant, depending upon the value of the moment of inertia of the deflecting magnet when suspended for vibration, has been carefully determined by repeated experiments made at Toronto with inertia rings of different weights and dimensions; and for greater security these experiments will be repeated with other rings at Woolwich when the series is closed. The intercomparison of the partial results with all the rings will give to the determination of the value of this constant a probable error, which, converted into terms of the intensity of the force, will enter as one of the constituents into the probable error of the value of the force at Toronto corresponding to the mean period of observation derived from the complete series.

There is also another constant which enters into the absolute value, which has yet to be determined for the Toronto deflecting magnet, namely, that which enables us to eliminate the variation induced by the magnetism of the earth in the magnetic moment of a bar, in the different positions in which it is employed in the experiments of deflection and vibration. An apparatus for the determination of this constant has been constructed at Woolwich, where the necessary experiments will be made at the close of the series; here also a comparison of different trials will give the probable error of the determination of this constant, which will thus enter into and be made a part of the probable error which shall ultimately be assigned to the final mean determination of the absolute horizontal force at Toronto.

For the purposes to be considered in this paper, however, it is not necessary that the values of the constants, representing the moment of inertia and the variation of the induction moment, should be precisely known: the mutual relation of the results obtained in different months would manifestly be the same, although the whole series might be affected by some slight inaccuracy in one or more of the constants employed in the calculation. It is not necessary therefore to wait until the constants above described have been determined with ultimate precision, in order to discuss the probable error of a single monthly determination by the absolute method, and the value of a series of monthly determinations of this nature in investigating the secular change and the annual variation of the force. These will be the same, although the absolute value of the force when finally determined might prove to be one thousandth part greater for example, or one thousandth part less, than we may at present assume it to be.

I subjoin therefore the following series of the results of the monthly observations at Toronto, from January 1845 to April 1849, as *relatively* correct; and as exhibiting the values of the horizontal force on the days of the respective months on which the observations were made, with an accuracy which, as respects observation error strictly so called, must be greater than that which would be inferred from the probable error of a single monthly determination obtained in the usual manner; because the probable error so obtained will include, besides observation error properly so called, the effects of regular or irregular variations which may have affected the force itself on the particular days of observation.

TABLE I.

	1845.	1846.	1847.	1848.	1849.
January	3·5377	3·5378	3·5337	3·5222	3·5213
February............	3·5376	3·5313	3·5320	3·5255	3·5210
March...............	3·5375	3·5341	3·5284	3·5270	3·5240
April	3·5351	3·5323	3·5249	3·5261	3·5260
May	3·5388	3·5317	3·5283	3·5286	
June	3·5421	3·5367	3·5302	3·5274	
July..................	3·5413	3·5365	3·5287	3·5305	
August	3·5383	3·5313	3·5335	3·5279	
September	3·5373	3·5303	3·5257	3·5239	
October	3·5363	3·5290	3·5251	3·5223	
November	3·5360	3·5278	3·5266	3·5184	
December	3·5379	3·5334	3·5237	3·5214	

Regarding each monthly determination as entitled to equal weight, and taking the arithmetical mean of all the observed values as the most probable mean value, we find the mean value to be 3·53043 with a probable error of ±·00055; and the probable error of a single monthly determination ±·0040.

This is on the most simple hypothesis, in which neither secular change nor annual variation is supposed to exist. If we call X the arithmetical mean as above derived, and X'_1, X'_2, $X'_3 \ldots X'_{52}$ the several observed monthly results, we shall have the several errors remaining over, X'_1-X, X'_2-X, ... $X'_{52}-X$, as follows:—

TABLE II.

	1845.	1846.	1847.	1848.	1849.
January	+·0073	+·0074	+·0033	−·0082	−·0091
February............	+·0072	+·0009	+·0016	−·0049	−·0094
March...............	+·0071	+·0037	−·0020	−·0034	−·0064
April	+·0047	+·0019	−·0055	−·0043	−·0044
May	+·0084	+·0013	−·0021	−·0018	
June	+·0117	+·0063	−·0002	−·0030	
July..................	·0109	+·0061	−·0017	+·0001	
August	+·0079	+·0009	+·0031	−·0025	
September	+·0069	−·0001	−·0047	−·0065	
October	+·0059	−·0014	−·0053	−·0081	
November	+·0056	−·0026	−·0038	−·0120	
December	+·0075	+·0030	+·0067	−·0090	

The prevalence of + signs in the earlier portion of the period, and of − signs in the later portion, points obviously to the existence of secular change, viz. to a decrease of the horizontal force in successive years during the period of observation. For the purpose of obtaining the mean annual value of this decrease, we may derive an equation from each of the monthly determinations of the form $X'=X+ay$, in which X is the most probable value of the horizontal force in the middle period of the series, *i. e.* on the 1st of March, 1847, X′ the observed horizontal force in any other month,

a the interval in months between the date of X′ and March the 1st, 1847, and y the monthly variation occasioned by secular change. We have fifty-two such equations furnished by the series, which, treated by the method of least squares, give $X=3{\cdot}53043$, and $y=-{\cdot}000347$ the monthly secular change, the latter number being equivalent to an annual decrease of ·0042 in the horizontal force during the period comprehended by the observations. For the purpose of obtaining the errors remaining over on this hypothesis of secular change, we must apply to each of the results in Table I. a correction, equivalent to the effect of secular change in the interval elapsed between the dates of the particular observation and the mean epoch of the 1st of March, 1847; and having done so, we now find, on the hypothesis of the existence of a uniform secular decrease of the horizontal force annually of ·0042, 3·53043 as the value of the horizontal force on the 1st of March, 1847, with a probable error of $\pm{\cdot}00025$; whilst the probable error of a single monthly determination is reduced to $\pm{\cdot}0018$ instead of $\pm{\cdot}0040$ as before; and as the weights of different hypotheses are measured by the inverse squares of the probable errors, the hypothesis which supposes a secular decrease of force amounting to ·0042 annually is more probable than the hypothesis which supposes no secular change, in the proportion of 4·7 (nearly) to 1.

Having thus obtained the value of the horizontal component of the magnetic force corresponding to the 1st of March, 1847, and the mean value of the secular change of this element during the period of the observations, we require, for the purpose of deriving the values of the *total* magnetic force and its secular change, at the same epoch, to know the magnetic inclination corresponding to the epoch, and the secular change of that element also. For the first we have to seek the mean result of the observations of inclination, which were also made monthly during the same fifty-two months. In the three first years, 1845 to 1847 inclusive, the observations of inclination were made on every Tuesday and Friday, three hours before noon on the Tuesdays, and three hours after noon on the Fridays; thus furnishing eight or nine partial determinations in each month according to the number of Tuesdays and Fridays contained in it; each determination being complete in respect to the several positions of the circle and needle required for that purpose. In 1848 and 1849 the same number, or occasionally a greater number, of partial determinations was made monthly; but instead of the Tuesdays and Fridays, the days of observation were the same as those on which the horizontal force was observed. The circle employed, from January 1845 to March 1846 inclusive, was one of Gambey's well-known 9-inch circles, and from April 1846 to April 1849 one of Robinson's, of the same dimension and of the same pattern. In Gambey's circle two needles were used, one from January 1845 to December of the same year, and the second from January 1846 to March of the same year; in Robinson's circle also two needles were used, one from April 1846 to August 1847, when an accident befell it, and a second from September 1847 to April 1849. The needles used in Gambey's and Robinson's circles were made by those artists respectively, and both circles and needles were probably as perfect of their kind as any that have ever been made. The intercomparability of the results might possibly have been more perfect if one and the same instrument had been used throughout (an important consideration in respect to the secular change); but, on the other hand, the probable accuracy of the general result as regards the mean inclination must be viewed as in some degree strengthened by the employment of different instruments. The following Table exhibits the fifty-two monthly results:—

TABLE III.

	1845.	1846.	1847.	1848.	1849.
January	75° 18′·4	75° 13′·8	75° 15′·0	75° 20′·3	75° 19′·5
February	19·5	14·2	15·2	18·7	18·1
March	14·5	13·8	16·2	17·2	16·7
April	11·5	14·3	15·9	18·0	18·4
May	15·4	14·4	16·1	17·2	
June	15·0	14·8	13·1	16·8	
July	14·5	14·0	11·3	16·4	
August	14·6	14·4	12·6	19·0	
September	16·6	15·7	12·5	17·3	
October	14·3	15·4	17·6	19·0	
November	16·8	15·0	17·7	19·4	
December	15·2	15·0	17·0	20·6	

On the hypothesis of the terrestrial magnetic inclination having been constant during the period of observation (*i. e.* constant in respect to secular change and annual variation, and subject only to irregular and diurnal fluctuations), we obtain from the results in the Table a mean inclination of 75° 16′·09, with a probable error of ±0′·20, and ±1′·46 as the probable error of a single monthly determination. These probable errors include of course the effects of irregular fluctuation as well as those of observation error properly so called, besides the possible influence of secular change and annual variation; the two latter being excluded by the hypothesis.

The errors remaining over on this hypothesis are shown in the following Table:—

TABLE IV.

	1845.	1846.	1847.	1848.	1849.
January	+2′·3	−2′·3	−1′·1	+4′·2	+3′·4
February	+3·4	−1·9	−0·9	+2·6	+2·0
March	−1·6	−2·3	+0·1	+1·1	+0·6
April	−4·6	−1·8	−0·2	+1·9	+2·3
May	−0·7	−1·7	0·0	+1·1	
June	1·1	−1·3	−3·0	+0·7	
July	- 1·6	−2·1	−4·8	+0·3	
August	1·5	−1·7	−3·5	+2·9	
September	+0·5	−0·4	−0·6	+1·2	
October	1·8	−0·7	+1·5	+2·9	
November	+0·7	−1·1	+1·6	+3·3	
December	−0·9	−1·1	+0·9	+4·5	

The prevalence of — signs in the earlier months, and of + signs in the later months, indicates distinctly the existence of secular change. Pursuing therefore the same method of obtaining its mean value during the period of observation as was adopted in the case of the horizontal force, we obtain from the fifty-two conditional equations $y=+0'\cdot0741$ as the monthly value of the secular change, equivalent to a mean annual increase of the inclination during the period of observation of $0'\cdot89$; and $75^\circ\ 16'\cdot09$ as the mean inclination on the 1st of March, 1847, with a probable error reduced to $\pm0'\cdot17$, the probable error of a single monthly determination being $\pm1'\cdot23$; whence we may infer that the hypothesis of a secular increase in the inclination of $0'\cdot89$ annually during the period of observation is more probable than the hypothesis of no secular change, in the proportion of 1·4 to 1.

Total Force; its mean value and secular change.—Having thus derived from the series of fifty-two months of observation the mean value of the horizontal force $=3\cdot53043$, and of the inclination $75^\circ\ 16'\cdot09$, each for the epoch of the 1st of March, 1847, we have for the value of the total force in absolute measure at the same epoch $3\cdot53043\times$ sec. $75^\circ\ 16'\cdot09=13\cdot8832$. With reference to the inclination element of the result, we might safely regard this value as a final determination; but we cannot quite do as much in respect to the element of the horizontal force, as it may yet have to receive the corrections already noticed (though they are likely to be extremely small), when the values of the constants of inertia and induction shall be finally ascertained on the return of the instruments to Woolwich. When these constants and their probable errors are known, the probable error of the finally corrected value of the total force will also be assignable.

The elements from which we have to infer the *secular change* of the total force are the secular changes of the horizontal force and of the inclination derived from the observations; these are an annual decrease of ·0042 in absolute measure of the horizontal force, and an annual increase of $0'\cdot89$ of inclination. A secular change of the horizontal force may be produced, either by a secular change of the inclination affecting the horizontal component of the total force according to the known principles of the resolution of forces, or by a secular change in the total force itself; or, finally, it may be the joint production of both. An increase of the inclination causes a decrease of horizontal force, and *vice versâ*; so far therefore we may regard the annual decrease of the horizontal force at Toronto as attributable in part at least to the annual increase of the inclination. But an annual increase of $0'\cdot89$ in the latter element is equivalent to an annual decrease in the horizontal component of the force of not more than ·0035. There remains, therefore, an excess of ·0007 in the secular decrease of the horizontal force, which is unaccounted for by the secular change of the inclination, and is indicative of the existence of a small annual decrease in the total force during the period of observation. The uncompensated portion of the horizontal force on which this inference is founded is indeed small in absolute amount, but its magnitude must be judged of in relation to the probable errors of the determinations of secular change of the inclination and horizontal force. Viewed in this light, the probabilities are in favour of the existence of a small annual decrease in the total force, as the legitimate conclusion from the portion of the series of absolute determinations in progress at Toronto which has been received in this country, and is here discussed: whilst it is obvious that the groundwork is laid of a positive conclusion, admitting of no uncertainty, attainable by steady perseverance in the prolongation of the series; avoiding as far as possible, upon all occasions, all changes in the instruments employed or in the methods of observation. It will be shown in the sequel that certainty in respect to the question, whether the total force at Toronto is at the present epoch increasing or decreasing, may have a very considerable theoretical importance.

Annual Variation.—We may now proceed to a consideration of the inferences which the observations will afford in regard to annual variation; but in entering on this investigation, we must remember, in the first place, that fifty-two months constitute but a short period from which to derive an *annual* variation; and in the second place, that we are as yet unable to eliminate the effects of the *irregular* disturbances from the residual errors, which consequently remain charged with them to the last; and that if these effects are not themselves the sole cause of an annual variation, by reason of their greater frequency or magnitude at certain seasons of the year than at others, we must be prepared to expect that they will embarrass the research, by rendering the effects of other causes less apparently systematic than they would otherwise have been found. In the horizontal force particularly we may have reason to apprehend the influence of disturbances, because that element is greatly affected by them at Toronto, and their average effect appears to be to depress the force at the periods of their occurrence below its mean value.

We have shown in the preceding pages the fifty-two monthly results of the observations of the inclination and horizontal force, and their arithmetical means constituting the mean values of those elements on the 1st of March, 1847; we have also derived from the observations the most probable values of the secular change of the elements during the period of observation. When each of the fifty-two results has received a correction for secular change proportioned to the interval of time elapsed between the date to which it refers and the 1st of March, 1847, and the differences are taken between the fifty-two results thus corrected and the arithmetical means, we obtain a suite of residual quantities, by which the influence of annual variation, if it exists, might be expected to be indicated. The following Table exhibits the mean difference in each month of the observed results, (when corrected as above noticed for secular change,) from the arithmetical means.

TABLE V.

	Residual quantities.	
	Inclination.	Horizontal force.
January, mean of 5 years	+1′·40	—·0004
February, mean of 5 years	+1·06	—·0012
March, mean of 5 years	—0·44	·0000
April, mean of 5 years	—0·58	—·0010
May, mean of 4 years	—0·38	+·0002
June, mean of 4 years	—1·03	+·0028
July, mean of 4 years	—1·93	+·0033
August, mean of 4 years	—0·90	+·0022
September, mean of 4 years	+0·13	—·0009
October, mean of 4 years	+0·35	—·0017
November, mean of 4 years	+0·97	—·0023
December, mean of 4 years	+0·60	—·0002

When due allowance has been made for the shortness of the period of observation, and for the influence of disturbing action, we find in this tabular view a much more conclusive indication of the existence of annual variation than we might perhaps have been prepared to expect. The inclination is obviously highest in the winter months and lowest in the summer months, passing through its mean value about the period of the equinoxes. The horizontal force has a corresponding variation, but with opposite signs. The occasional irregularities are more marked in the horizontal force than in the inclination, and in both they prevail chiefly in the months of spring and autumn. It must remain for a separate discussion, to deduce from the great mass of facts which have now been collected at Toronto, the numerical conclusions which they will afford in regard to the frequency and magnitude of the disturbances in the different months of the year; but antecedently to the certain conclusions to be drawn from such numerical values, it is not an improbable supposition, that the months of spring and autumn (and notably those of autumn) may prove to be generally the most disturbed months, and consequently those of the greatest depression of the horizontal force resulting from the disturbances. The irregularities may be expected to diminish as the series is extended; but if they are, in part at least, occasioned by actual irregularities in the force itself produced by the disturbances, they may have a character of permanency in certain months which no continuation of the series would remove, if it should prove that the disturbances prevail more in some months than in others, and if their action has on the average a special tendency.

In commenting on the fortnightly means of the Bifilar observations at Toronto in 1842*, (in which year the Bifilar observations were suitable for the investigation, inasmuch as the scale-reading returned nearly to the same division at the close, as that at which it had stood at the commencement, of the year,) I called attention to the remarkable feature, indicated by the Bifilar readings, of an excess in the value of the horizontal force in the summer months over the other months of the year; and I

* Toronto Observations, vol. i. p. xxxvii and xxxviii.

remarked on that occasion, that "the excess appeared to be too large to be caused by any conceivable error in the determination of the temperature correction of the magnet, or generally of the apparatus by which it was suspended." The average difference between the summer and winter months, derived from the observations of the Bifilar in the single year referred to, was ·00161 parts of the whole horizontal force, or ·0056 in absolute measure.

The question of an annual variation of the horizontal force appeared to me so important either to verify or disprove, that, at my request, Captain Lefroy employed, during the years 1847 and 1848, a third method of experimenting, which, although it may not be quite so satisfactory in respect to the individual monthly results as the method of absolute determinations, in consequence of the magnetic moment of the bar not being subject to monthly examination, has yet the advantage of affording a third conclusion perfectly independent of the others, and but little inferior to the absolute method in proportion to the time of their respective continuance. One of the cylindrical magnets of 3·67 inches in length, which had been employed in the North American survey, and appeared to have attained a state of steady magnetism (which however did not prove so thoroughly steady as was expected), was suspended in the usual manner in a light stirrup, with an attached mirror and a detached telescope. The horizontal force of the earth was measured at stated hours, twice in every day, at 10 A.M. and 5 P.M., by the times of vibration of the bar derived from four hundred vibrations observed in the usual manner, and reduced to a standard temperature and to infinitely small arcs. The magnetic moment of the bar was carefully examined before the commencement and after the conclusion of the series, viz. on the 31st of December, 1846, and on the 3rd of January, 1849, and also intermediately on January 5th, 1848. The magnetic moment at these periods was as follows:—

		Loss of magnetism in nearly equal intervals.
1846. December 31st	=0·6104	·0104
1848. January 5th	=0·6000	·0087
1849. January 3rd	=0·5913	

The value of the magnetic moment has been assumed on the hypothesis of uniform loss of magnetism in the whole period, and has been computed for every day of observation. Now if we take the arithmetical mean of the absolute values of the force in the twenty-four months derived by this bar as the mean result corresponding to the 1st January 1848, and if for the purpose of eliminating secular change we combine the values in January 1847 and December 1848, February 1847 and November 1848, &c., we obtain the excess or defect in the horizontal force in absolute measure for the months of winter and summer as follows:—

TABLE VI.

Winter	January 1847	and December 1848	−·0043	} −·0038
	December 1847	and January 1848	−·0034	
	February 1847	and November 1848	−·0039	
	November 1847	and February 1848	−·0037	
Summer	May 1847	and August 1848	−·0006	} +·0047
	August 1847	and May 1848	+·0042	
	June 1847	and July 1848	+·0084	
	July 1847	and June 1848	+·0070	

We have here a still further confirmation of the greater amount of the horizontal force in the summer than in the winter months; the difference between the two seasons is in this experiment greater than that shown by the Bifilar observations for 1842, or than that derived from the more extended absolute series from January 1845 to April 1849. It is quite conceivable however that, independently of errors of measurement, the actual numerical difference between the summer and winter months may be liable to vary in different years.

By three independent methods of experiment, therefore, the general fact of an annual variation of the horizontal force at Toronto has been shown, the force being greater in the summer than in the winter months; but the question of whether this variation, as well as that of the inclination, is progressive from one extreme in midwinter to the opposite extreme in midsummer, and *vice versâ*, the regularity of the progression being only interrupted by the complication of irregular disturbances,—or whether, as in the case of the diurnal variation, the change from one half-yearly phase to the other takes place (subject to the same complication) about the time of the equinoxes,—will require a longer period for its determination than that which we have at present before us. Upon the latter supposition, we find, from the absolute series at Toronto, that the inclination is on the average 0′·88 above its mean value, and the horizontal force ·0015 below its mean value during the five months when the sun is in the southern signs,—and the inclination 0′·90 below, and the horizontal force ·0011 above, their respective mean values, when the sun is in the northern signs.

The sum of the differences of the inclination at the opposite seasons (1′·78) is equivalent, in the resolution of the total force into its horizontal and vertical components, to ·0070 of horizontal force. The annual variation of the horizontal force derived from the observations, corresponds in *direction* in each of the seasons to that which is indicated for it by the change of the inclination, but the *amount* falls considerably short of that which would be the equivalent of the alteration in the latter element. Hence we must infer the probable existence of an annual variation of the total force, the force being greatest in the winter months, or when the sun is in the southern signs; and least in the summer months, or when the sun is in the northern signs.

Although I have been unwilling to encumber this communication with details from the Hobarton Observatory similar to those from Toronto, I may be permitted to state very briefly the *results* obtained at Hobarton in respect to the annual variation of the Inclination and Force, as they have a very considerable interest when viewed in connection with those obtained at Toronto.

A series of monthly determinations of the inclination, in which no change was made in the instruments employed, or in the methods or place of observation, was commenced at Hobarton in June 1843, and was still continuing at the date of the last returns received from thence in December 1848. From this series we have sixty-eight consecutive monthly determinations, strictly intercomparable, bearing on the question of annual variation. It will be remembered that the summer of the southern hemisphere is when the sun is in the *southern* signs, and *vice versâ*; and that at Hobarton it is the *south* end of the needle which dips below the horizon. The investigation, conducted in the same manner as at Toronto, shows at Hobarton a decrease of *south* inclination of 0'·89 on the average of the months from April to August inclusive, *i. e.* in the southern winter; and an increase of 0'·85 from October to February inclusive, *i. e.* in the southern summer. Thus in the months from April to August the North Inclination at Toronto and the South Inclination at Hobarton are both diminished; and from October to February inclusive they are both increased. The North Inclination at Toronto is lowest and the South Inclination at Hobarton highest, in the respective summers of the two stations, and *vice versâ*, and in both cases the variation is nearly to the same amount.

In the case of the horizontal force, a regular and consecutive series of monthly determinations, similar to that at Toronto, was commenced at Hobarton in January 1846, and the results have been received in England to December 1848 inclusive. The series treated in a similar manner to that at Toronto shows an annual variation of the same character as respects the seasons, and almost identical in amount with that at Toronto. In the months from October to February inclusive (or in the summer months at Hobarton), the horizontal force is ·0017 *greater* on the average than its mean amount, and from April to August inclusive (or in the winter months at Hobarton), it is on the average ·0013 *less* than its mean amount.

The inferences to be drawn from these variations of the inclination and horizontal force taken conjointly, as respects the total force at Hobarton, are as follows: the inclination being greater from October to February than from April to August, if the total force remained unaltered, the horizontal force should be *below* its mean amount in the months from October to February, whereas we find a *higher* amount in those months; therefore, so far as the observations have yet gone, the total force at Hobarton appears to be subject to an annual variation, being *higher* than its mean amount from October to February, and *lower* than its mean amount from April to August.

It may assist the recollection of the facts regarding the annual variation of the two magnetic elements at Toronto and Hobarton to state;—that in the months from October to February the magnetic needle more nearly approaches the *vertical* direction at both stations, and from April to August the *horizontal* direction;—and that the total force is greatest at both stations from October to February, and least from April to August.

It is much to be desired that so remarkable a result should receive a full confirmation, by the continuance of the observations at Toronto and at Hobarton for such an additional period as may appear necessary for that purpose; and that the general conclusion indicated by the observations at those stations should be verified by similar investigations in other parts of the globe, especially at the observatories which now exist. The facts, as far as they go, indicate the existence of a general affection of the whole globe having an annual period, and would appear to conduct us naturally to the position of the earth in its orbit as the first step towards an explanation of the periodic change*. It might possibly be regarded as premature, in the present stage of the inquiry, to enter on the discussion of such physical hypotheses as may present themselves on the supposition of a causal connection of this nature; but it cannot be open to the same objection, to press on the consideration of those who are engaged in experimental researches in terrestrial magnetism, (or of others who may have it in their power, from station or influence, to give countenance or support to those who are so engaged), the importance of following up without delay, and in the most effective manner, a branch of the research which gives so fair a promise of establishing a conclusion of so much theoretical moment upon the basis of competent experiment†.

I may be permitted, in conclusion, to advert, though very briefly, to considerations which may give a particular importance to accurate numerical values of the magnetic elements and their secular changes determined at Toronto.

That station was selected on account of its proximity to one of those remarkable points on the globe which have a peculiar importance in theoretical respects: viz. to one of the two points in the northern hemisphere which are the centres of the loops

* The portion of the year when the magnetic force is greatest and the direction of the needle most vertical in both hemispheres, viz. from October to February, coincides with that in which the earth is nearest to the sun, and also moves with greatest velocity in her orbit. There is another curious annual coincidence of a wholly different nature, unconnected with the position of the earth in her orbit; during the months from October to February, when the magnetic force is greatest and the direction of the needle in both hemispheres most vertical, Mr. Dove's recent investigations have shown that, owing to meteorological causes traceable to the unequal distribution of land and sea in the two hemispheres, the aggregate temperature of the whole earth is lower than in the opposite period of the year.

† I am glad to be able to add, that by a letter received from Lieut. Gilliss of the United States Navy, Director of the Astronomical Observatory established at Santiago in Chili, magnetical observations, similar to those referred to in this communication as being in progress at Toronto and Hobarton, were commenced at Santiago in January of the present year, with instruments which had been prepared at Woolwich at the expense and by the request of the Government of the United States.

of the isodynamic lemniscates (as they have been usually called), and are the points of greatest intensity of the force (on the surface of the globe) of apparently two magnetic systems, distinguished from each other by the very remarkable difference in the rate of secular change to which the phenomena in each system appear to be subject. In the present state of the terrestrial magnetic phenomena, the principal of these two points, or the centre of the larger loop of the lemniscates, is situated within the British territories in North America; and by the magnetic survey of those territories, undertaken by the British Government on the recommendation of the Royal Society, and executed in 1842 and 1843 by Captain Lefroy, its geographical position was approximately ascertained, and the difference between the magnetic force at this central point and at the Toronto Observatory was very carefully measured, and is recorded in the Philosophical Transactions (Part III. for 1846). In this point of view therefore the accurate determination of the Force at the observatory at Toronto has a peculiar value, both for the present and for after times. It will, I think, be clear to those who have followed the details of this communication, that by the skill, assiduity and perseverance of the Director of the Toronto Observatory and his assistants, (non-commissioned officers of the Royal Artillery,) this object has been accomplished within very small limits of uncertainty as dependent on observation or accidental error; and that when the small corrections which have been noticed, as requiring to be investigated on the return of the instruments to England at the close of the series, have been ascertained and applied, the value of the total force in absolute measure at Toronto, and by its means the value at the central point, will be assigned with a degree of accuracy which we may believe will be regarded as satisfactory, not only at the present day, but at those distant periods, when the determination may be referred to as presenting the earliest record of the value of the terrestrial magnetic force at its point of maximum in the northern hemisphere.

The determination of this value at this particular time may derive an additional importance from the present relative situation of the two magnetic centres* which are not yet far removed from their greatest distance apart, viz. 180° in geographical longitude; a state of the phenomena constituting possibly an epoch in the cycle of secular change, characterized by that portion of the force at each centre which is derived from mutual influence being a minimum. The analogy of the southern hemisphere, where the two centres are nearer to each other in respect to geographical longitude than in the northern hemisphere, and where the force at each is higher than at the corresponding northern centre, may justify this supposition. The geographical longitude of the principal northern maximum was ascertained by Captain Lefroy, in the years 1842 and 1843, to be about 270° East; that of the minor maxi-

* It will of course be understood that by the employment of the word "centres" it is not intended to convey that the points of maximum are themselves centres of the magnetic force of the systems to which they may respectively belong. The expression is merely used to designate central points of certain phenomena observed on the earth's surface, where alone it is in our power to observe them.

mum, in the same hemisphere, was determined a few years earlier (1828 and 1829), by the expedition of MM. Hansteen, Erman and Due; and we may gather the result from the following passage in M. Erman's 'Reise um die Erde,' which I quote from Mr. Cooley's translation, vol. ii. p. 365:—"The magnetical results of the last journey were now examined more narrowly, and it was clear that we had in fact crossed the meridian of the Siberian magnetic pole between Irkutsk and Yakutsk. The magnetic attraction of the earth was decidedly greater between Kirensk and Beresovoi Ostrov than at any other point which we had visited in the same parallel of latitude to the east or west. The pole sought for had there exhibited its greatest force, and extended its influence furthest to the south; and consequently we must have been there on the same meridian with it. This probably took place at Parshinsk in longitude 111° 27′ E.*." Omitting the consideration of the small amount of secular change which may have taken place between the expedition of MM. Hansteen, Erman and Due and that of Captain Lefroy, we have here an interval of (270° −111°=)159° as the approximate difference of longitude (on the side of Behring's Straits and the adjacent continents) between the two northern centres. This difference is diminishing by the effect of secular change, and the epoch, when the centres were 180° apart, must therefore have taken place a few years antecedently to either of the determinations above referred to; probably about the close of the last century, when, as we learn by Professor Loomis's discussion of the observations of magnetic inclination in the northern parts of the United States†, the inclination which had previously diminished in that quarter began to increase.

The change in the geographical position of both the points of maximum in the northern hemisphere has been from west to east since the earliest period at which inferences of this nature could be drawn from the phenomena; the diminution of the meridional distance between them on the one side, and its increase on the other, being occasioned by the more rapid movement of translation of the minor maximum. It has been conjectured that the motion of the principal maximum might cease to be progressive in the easterly direction when the two centres or maxima should be 180°

* It is obvious that M. Erman uses the term "magnetic pole" to designate the central point of a loop of the isodynamic lemniscates, or the point of greatest intensity of the force. This also is the sense in which it is employed by M. Kupffer, when he says "il y a un pole magnétique dans la Sibérie." The term "pole" cannot however be understood to have the same signification in those writings which assert a supposed connection between "two magnetic poles" and "two poles of cold" in the northern hemisphere; for, in North America at least, the point of maximum intensity of the force is certainly very far distant from that of the lowest annual temperature: the "magnetic poles" in this case may possibly be intended to refer to the centres of the loops of the *isoclinal* instead of those of the *isodynamic* (so-called) lemniscates. But either of these significations differs materially from M. Gauss's definition of a magnetic pole, *i. e.* "where the horizontal terrestrial force is zero." I have subjoined this note in illustration of some remarks which I ventured to make in a former communication on the inconvenience of the employment of a term which appears to be used in different meanings by different writers.

† In Silliman's Journal.

of geographical longitude apart, and that it might thenceforward be retrograde. The observatory at Toronto seems well situated for deciding this question. If the progression of the secular change of both systems continues to be the same after that epoch is past as it was before, the force at Toronto might be expected to sustain on the whole an annual *decrease*, as it would be more diminished by the recession of the neighbouring greater maximum, than increased by the approach of the far more distant minor maximum. If, on the other hand, the movement of the principal maximum should be retrograde, the force at Toronto might be expected to undergo a considerable annual *increase*. By the observations which have been discussed in this paper, it has been shown that the probabilities are considerably in favour of the existence of a small annual *decrease* of force at the present time: and as time alone is wanting to convert this probability into certainty, it would seem particularly desirable that the series of monthly observations of the horizontal force and of the inclination at Toronto should be continued, until the direction and approximate value of the secular change of the total force shall be thoroughly determined.