REPORT

OF THE

THIRTY-FOURTH MEETING

OF THE

BRITISH ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE;

HELD AT

BATH IN SEPTEMBER 1864.

LONDON:
JOHN MURRAY, ALBEMARLE STREET.
1865.

Report of the Council of the British Association, presented to the General Committee, Wednesday, September 14, 1864.

1. The Council have received the Report of the Kew Committee for the past year, which will be laid before the General Committee on Monday.

2. The Report of the Parliamentary Committee has been received, and the Council recommend the adoption of this Report by the General Committee.

3. The Council have added to the list of Corresponding Members the names of the following Foreign Men of Science, who have been present at Meetings of the Association:—

Dr. Torell, Dr. Buys Ballot, M. Des Cloizeaux, and Prof. Adolph Steen.

4. The Council have received invitations to hold the next Meeting of the Association at Birmingham, and another to hold it at Nottingham. An invitation has also been received from Dundee for the year 1867.

Report of the Kew Committee of the British Association for the Advancement of Science for 1863–1864.

The Committee of the Kew Observatory submit to the Association the following statement of their proceedings during the past year:—

A set of Self-recording Magnetographs, of the same pattern as those at Kew, have been ordered by the Italian Government for Professor Donati of Florence; these have been completed by Adie of London, and despatched to their destination.

General Sabine has received letters from Mr. Meldrum, Director of the Mauritius Observatory, and from Mr. Ellery, Director of that at Melbourne, from which there seems to be a good prospect that at no distant date Self-recording Magnetographs may be in operation in these localities. This would be a result of very great scientific importance, since there are as yet none of these instruments established in the southern hemisphere.

The Committee have lost with regret the valuable services of Mr. Chambers, who left the Observatory about the middle of November last for an appointment in India. His place as Magnetical Assistant has been supplied by Mr. George Whipple, who has given much satisfaction in his new office.

The sum of £50 has been received from the Government Grant Fund of the Royal Society for the purpose of obtaining printed copies of magnetic curves. This has been spent in procuring photolithographic copies of a number of the most interesting traces simultaneously produced by the Magnetographs at Kew and Lisbon. These have been published by the Kew and Lisbon Observatories, and distributed to scientific men likely to take an interest in the subject.

A Unifilar and Dip Circle have been verified at Kew and forwarded to the Lisbon Observatory, and a Self-recording Electrometer, on Professor W. Thomson's principle, has also been despatched to that institution.

Two Unifilars and two Dip Circles have likewise been ordered by Colonel Walker, Director of the Trigonometrical Survey of India, and they are at present in the hands of the opticians.

The usual monthly absolute determinations of the magnetic elements continue to be made at Kew, and the Self-recording Magnetographs are in constant operation as heretofore, under the superintendence of Mr. Whipple, Magnetical Assistant.

Advantage has again been taken of these automatic records of the earth's

magnetism by the Committee engaged in the preparation of electrical standards, who have found it desirable, for some of their experiments, to ascertain the contemporaneous readings of the Declination Magnetograph.

The following papers having reference to Kew Observatory have been communicated to the Royal Society by Major-General Sabine, President of that body:—

1. Results of hourly Observations of the Magnetic Declination made by Sir Francis Leopold M'Clintock, R.N., and the Officers of the Yacht 'Fox,' at Port Kennedy in the Arctic Sea, in the Winter of 1858–59; and a Comparison of these Results with those obtained by Captain Maguire, R.N. and the Officers of H.M.S. 'Plover,' in 1852, 1853, and 1854, at Point Barrow.

2. A Comparison of the most notable Disturbances of the Magnetic Declination in 1858 and 1859 at Kew and Nertschinsk; preceded by a brief Retrospective View of the Progress of the Investigation into the Laws and Causes of the Magnetic Disturbances.

A Table of the Mean Declination of the Magnet in each Decade from January 1858 to December 1863, derived from the Observations made at the Magnetic Observatory at Lisbon, has been drawn up by Senhor da Silveira, Director of that Observatory.

This Table exhibits the semiannual inequality to which that element is subject at Lisbon, and which is of the same nature as that derived from the Kew photographs by General Sabine.

Mr. Stewart, Superintendent of the Kew Observatory, in conjunction with Senhor Capello of the Lisbon Observatory, has communicated to the Royal Society a paper, entitled "Results of a Comparison of certain Traces produced simultaneously by the Self-recording Magnetographs at Kew and at Lisbon, especially of those which record the Magnetic Disturbance of July 15, 1863."

Mr. Stewart has likewise communicated to the same Society two short papers, one "On the Sudden Squalls of 30th October and 21st November 1863," and another, entitled "Remarks on Sun-Spots." He has also communicated to the Royal Society of Edinburgh a paper on "Sun-Spots, and their Connexion with Planetary Configurations."

Mr. A. H. Burgess, M.A., being desirous to obtain magnetical instruction, is at present visiting the Observatory for the purpose of acquainting himself with our method of observation.

The Meteorological work of the Observatory is now performed by Mr. Thomas Baker, who likewise takes charge of the photographic department connected with the self-recording instruments, and executes both offices very satisfactorily.

During the past year 97 Barometers
" " " 389 Thermometers

have been verified, and five Standard Thermometers have been supplied to men of science and opticians. A set of weights, a standard scale, and a measure of capacity have likewise been verified. The Self-recording Barograph continues in constant operation. Through an ingenious suggestion of Mr. Beckley traces in duplicate have been obtained, and one of these has been regularly forwarded to Admiral FitzRoy.

The Self-recording Electrometer of Professor W. Thomson has continued in constant operation until the beginning of August, when it was sent to the optician for repairs.

The arrangements at the Observatory for testing Sextants remain as before.

During the past year eight Sextants, two Quadrants, and one Transit-instrument have been verified.

The sun-spots continue to be observed, after the method of Hofrath Schwabe, of Dessau.

The Kew Heliograph in charge of Mr. De la Rue has been continuously worked by a qualified assistant, under the immediate supervision of Mr. Beckley, who has proved of much service to the Committee in this as well as in other matters. During the past year 175 negatives have been taken, and four sets of positives have been printed from each, one of which has been presented to the Royal Society. The negatives are being reduced under the superintendence of Mr. De la Rue, and by means of an instrument of his construction. Mr. B. Loewy, formerly assistant in the Flagstaff Observatory, Melbourne, has been engaged in this reduction, which he is executing very satisfactorily at Kew.

Mr. De la Rue is also having an arrangement made, by means of which the proportion of the sun's disk obscured by spots may be conveniently measured.

At Mr. De la Rue's request Mr. Loewy is now examining all pictures preserved at Kew, with reference to distribution of faculæ and general appearance, and it seems that, out of more than 500 groups hitherto examined, about 250 show a nearly equal distribution of faculous matter round the penumbra, while of the rest more than 200 have the faculæ decidedly, either entirely or mostly, on the left side. After concluding the examination, which will extend over more than 1000 spots, Mr. Loewy will submit the result to Mr. De la Rue.

The Spectroscope belonging to the Chairman has been supplemented with a set of eleven sulphuret-of-carbon prisms, made by Mr. Browning, and giving the very great angular separation of more than 3′ between the two lines D. The Chairman has communicated a short description of these prisms, and of the appearance of the two lines obtained by this arrangement, to the Royal Society.

That portion of the spectrum between D and E is now being mapped, and all the measurements have already been made. The results obtained show that the position of any line can be determined with very great accuracy. Mr. Loewy has been the principal observer, and he seems well qualified for the work.

Preliminary arrangements have been made, under the superintendence of Professor Stokes, for experiments on the retardation of the pendulum in different gases.

At the request of the Secretary of State for India, received through the Royal Society, arrangements have been made for the preparation of apparatus to be used for the vibration of pendulums *in vacuo* at the different stations of the Trigonometrical Survey in India; and the request has also been made that the officer who may conduct this experimental investigation should receive instructions at this Observatory.

The instrument constructed by Mr. Broun for the purpose of estimating the magnetic dip by means of soft iron remains at present at the Observatory.

The balance of the £40 granted by the British Association in 1861, for an additional photographic assistant, has been expended under the superintendence of Mr. De la Rue, along with further sums which have been defrayed by Royal Society grants received by that gentleman.

The Superintendent has likewise received grants from the Royal Society for special experiments to be made at Kew, and when these are completed

an account will be rendered to that Society. It will thus be seen that other experiments and observations of a nature to further science are made at Kew besides those which form the constant work of the Observatory, and of these the Spectroscope measurements at present in progress may be mentioned as an example; it will also be noticed that the British Association do not bear the expense of these experiments, but this is defrayed by those who bring them before the Committee.

From the financial statement which accompanies this Report, it will be seen that the adverse balance of last year has been considerably reduced, but there is still a balance against this Observatory amounting to £45 17*s.* 9*d.* The Committee recommend that a sum of £600 should be granted for the expenditure of the current year.

A correspondence, which is appended to this Report, has taken place between the Astronomer Royal and the Chairman, relative to a paragraph contained in the Report of the former to the Visitors of the Royal Observatory.

The Astronomer Royal has further suggested that certain experiments should be made in this Observatory:—

1st. For the purpose of investigating the discordances which he has found in his observations of the dipping-needle.

2nd. For the purpose of investigating the displacements which occur in the trace of his vertical-force photograph.

3rd. On the temperature corrections of the force of a magnet made by heating it in hot air instead of by hot water.

The Committee, for the reasons contained in the letter of the Superintendent (No. VII. Correspondence), considerd that it was not advisable to undertake the experiments suggested by the Astronomer Royal, as one of these would necessarily involve the displacement of the Kew vertical-force magnetograph, while the others refer to points which, in the opinion of the Committee, have been already decided by previous observations and experiments.

J. P. Gassiot, *Chairman.*

Kew Observatory,
26th August, 1864.

Correspondence*.

I.

Kew Observatory, Richmond, 27th June, 1864.

My dear Sir,—The attention of the Kew Committee has been drawn to the following paragraph in your Report to the Visitors of the Royal Observatory:—

"I consider it certain that the small probable errors which have been attributed to ordinary needles are a pure delusion. I know no instrumental determination in which, without any breach of faith, the wish for uniformity of results will be so certainly followed by uniformity of results as in the determination of dip."

It having been suggested that the preceding paragraph may possibly be considered to refer to other observations than those made at Greenwich, I am requested by the Committee to inquire whether it is intended in any measure to refer to dip-observations made at this Observatory, and published in the publications of the Royal Society; the object of the Committee being

* A copy of this correspondence was forwarded to the Astronomer Royal on 26th August.

that, in the interest of Magnetical Science, the precise value of dip-observations made in this Observatory should be definitely ascertained.

Believe me, my dear Sir,

Yours very truly,

To G. B. Airy, Esq., F.R.S., (Signed) J. P. Gassiot,
Astronomer Royal, Observatory, Greenwich. *Chairman.*

II.

Royal Observatory, Greenwich, S.E., 28th June, 1864.

My dear Sir,—I have to acknowledge the receipt of your letter of 27th inst., in which you state that the attention of the Kew Committee has been drawn to a paragraph in my Report to the Visitors of the Royal Observatory, wherein I express my opinion on the inaccuracy of the small probable errors which have been attributed to ordinary dipping-needles; and in which you further remark that the cited paragraph may be considered to refer to other observations than those made at Greenwich, and therefore, on the part of the Kew Observatory Committee, you inquire whether the paragraph in question is intended in any measure to refer to dip-observations made at the Kew Observatory, and published in the publications of the Royal Society; the object of the Committee being that, in the interest of Magnetical Science, the precise value of dip-observations made in the Kew Observatory should be definitely ascertained.

It gives me great pleasure to enter fully upon any matter to which you may invite my attention, and particularly so when the object is such as is characterized in the last paragraph of your letter.

The inquiries in your letter are in fact two:—

First. Whether the paragraph of my Report refers to other observations than those made at Greenwich?

To this I reply that it necessarily refers to other observations. I have never succeeded in producing the agreement of results which is implied by the smallness of the probable errors, except by unfair selection among the discordant primary elements of observation on which the result is founded. I have stated this repeatedly in my Reports to the Board of Visitors (the whole series of which, I believe, are lodged in the Kew Observatory), and I have in one at least particularly remarked that the discordance still exists with the very fine instrument now in use at the Royal Observatory.

Second. Whether the paragraph of my Report is intended in any measure to apply to dip-observations made at the Kew Observatory, and published in the publications of the Royal Society?

To this I reply that it is intended so to apply, inasmuch as the degree of accuracy, to which I do not give my assent as real or well founded, is claimed for the dip-observations made at the Kew Observatory. In support of my statement of that claim, I will refer to a pamphlet by General Sabine, which I am unwilling further to describe, but which, as I am aware, has been forced on your attention and on that of the other members of the Committee of Recommendations of the British Association. In it will be found the following sentences:—"The probable error of a *single observation of the dip* with reliable instruments of easy procurement is known to be $\pm$ 1'·5. It has been shown to be so by a series of 282 observations made at Kew, employing 12 circles and 24 needles, all of the pattern which has been in use at Kew for several years past. The observations were made by seven different observers: the results are published in the 'Proceedings of the Royal Society,' March 1861, from entries in the Kew Observatory books, not a single ob-

servation having been omitted. The probable error ± 1″·5 may be regarded as including *constant* errors, considering the number of different circles and needles which were employed, as well as the peculiarities of different observers, of whom there were seven." (The italics are General Sabine's.) These are the probable errors which I cannot accept as accurate.

It may not be superfluous to add that I have conversed with several foreign observers (one of whom has very lately quitted me), and that all have found discordances comparable to those which I have myself observed. I have therefore no novelty to claim, except the suggestion (made by me some years ago) of instability in the position of the magnetic axis, and the construction (within little more than a year) of an instrument whose results appear to support that suggestion.

I should be much gratified if the powers of the Kew Observatory could be devoted to the examination of this and analogous instrumental difficulties. These experimental inquiries are not well suited to the system of the establishment over which I preside. And, speaking as a member of the British Association, I think that the Kew Observatory would be better employed in that way than in the course which now absorbs so much of its strength. It was originally intended, and in my opinion wisely intended, for the verification and improvement of instruments, and not for continuous observations. If the examination which I propose should be taken up, I shall be happy to cooperate, by repetition of observations (as my opportunities might serve), and by communication of my results.

I am, my dear Sir,

Yours very truly,

J. P. Gassiot, Esq., (Signed) G. B. Airy.

Chairman of the Kew Observatory Committee.

III.

Clapham Common, June 30, 1864.

My dear Sir,—I have to acknowledge receipt of yours of 28th inst., wherein you state that the paragraph in your recent report "was intended to apply to the dip-observations made at Kew, and published in the publications of the Royal Society, inasmuch as the degree of accuracy, to which you do not give your assent as real or well founded, is claimed for these observations."

I have forwarded your letter to Mr. Stewart, the director of the Observatory, under whose immediate directions the observations were made, and I hope you will find that the explanation he will offer will satisfy you as to the entire truthfulness of the results he obtained, and to the reliability that should be placed thereon.

I have always understood, that to the continued magnetical observations which have been made at Kew Observatory has been mainly due the establishment of so many magnetical observatories abroad; it would, however, ill become me to offer to you any opinion as to their value, although I cannot but regret that they do not appear to have met your approval.

I am sure it would afford Mr. Stewart, as well as the Members of the Committee, much pleasure to follow out any experimental inquiries which you may at any time suggest.

Believe me, my dear Sir,

Yours most truly,

To G. B. Airy, Esq., (Signed) J. P. Gassiot.

Astronomer Royal, Greenwich.

IV.

Kew Observatory, Richmond, July 4th, 1864*.

My dear Sir,—I have perused Mr. Airy's letter to you, in which he states that the passage in his Report to the Board of Visitors, about which you wrote to him as Chairman of the Kew Committee, was intended to refer to the dip-observations made at the Kew Observatory, and published in the publications of the Royal Society. I have likewise perused your reply, and now, in accordance with your request, I shall describe the mode of dip-observation at Kew, in order that you may see that Mr. Airy's remark is inapplicable to our determinations.

But before doing so it may be well to state that the list of dip-observations recorded in the publication to which Mr. Airy refers *is* a faithful and complete catalogue of those which have been made at this Observatory. My connexion with the publication referred to is therefore this: I look upon it simply as an authorized and compendious catalogue of the dip-observations which have been made at Kew; and regarding the method in which these have been discussed in the publications of the Royal Society as not falling within the scope of my reply, I shall confine myself to the question of mental bias, and endeavour to show you that our dip-observations are quite free from any such source of error.

In the first place, the circles used at Kew are all of the same pattern; this being one which combines the united experience of several eminent magneticians, and which they were several years in bringing to perfection. The circles and needles are all likewise made by the same optician (Mr. Henry Barrow), who has devoted very great pains to the construction of these instruments. I mention this latter circumstance, because in this observation it is absolutely essential to have a needle constructed with the greatest care. Before commencing the observation, the fine hard axle of the needle is gently inserted into a piece of soft cork, in order that it may be thoroughly cleansed, and the agate knife-edges upon which it is to rest are likewise rubbed with cork The needle itself has been previously magnetized by being rubbed ten times on each side from centre to pole by a pair of bar magnets. After the plane of the magnetic meridian has been determined in the usual way, the circle is placed in this plane, and the needle is observed in the four following positions:—

I.	Face of needle to face of instrument		....	Face of instrument		East.
II.	,,	,,	....	,,	,,	West.
III.	Face of needle reversed		....	,,	,,	West.
IV.	,,	,,	....	,,	,,	East.

The poles of the needle are then reversed by ten strokes of the bar magnets on each side, and the same set of observations is repeated, the mean of the whole eight positions giving the dip.

Both extremities of the needle are in each case successively viewed by microscopes attached to an arm, which also carries the verniers by means of which the position is read. Before making an observation, the needle is gently raised from its support and lowered again by means of a lifter twice or thrice, after which its position is noted. I ought likewise to remark that in magnetizing the needle it is always placed in a wooden frame in such a manner that the magnets are obliged to pass symmetrically over it.

In this process it appears to me that the only possible effect a mental

* This letter, although written on July 4th, was not sent to Mr. Airy until it had been approved of by the Committee at their meeting on August 26th.

bias can be imagined to have is to induce the observer to continue lifting the needle before reading, until it has come into what he considers the proper position; but even this is totally precluded by the method of observation, for the vernier is not read, and the observer does not know the position of his needle until it is at rest and the lifting process at an end. Besides, if the observer did know the position of his needle it would avail him little; for while the *mean* of the *eight* positions is nearly the same for different instruments, yet the reading of any one position of the needle may be, and usually is, very different from the true or finally deduced dip.

From all this it will be seen how little scope there is in the dip-observations for the operation of mental bias; but the observers who are supposed to have worked our instruments with an unconscious predetermination to produce certain results must have had still more formidable difficulties than even these to contend with. For, in order that mental bias should have operated in the case under discussion, the preconceived idea of uniformity with which the observer approached the instrument must have varied in such a measure from season to season and from year to year as to produce in the results obtained an annual variation, as well as a secular change, and these of such a nature as to conform with the results of other observatories. Mr. Airy must acknowledge that the uniformity to which he alludes, and the wish for which he supposes has created a mental bias, is that which remains after the annual and secular variations have been allowed for.

Next, with regard to observers; we have frequently at Kew gentlemen connected with foreign observatories, who come to receive a magnetical equipment. Their desire is to obtain the best possible instruments, but at the same time they view those presented to them with a very critical eye. One of these was Dr. Bergsma, who spent nearly a month in thoroughly examining the dip-circle and in suggesting refinements, but who went away convinced of its accuracy. Senhor da Souza of Coimbra, and Senhor Capello of Lisbon, have likewise made dip-observations at Kew, and with the same object, namely, to satisfy themselves by their own practical experience as to the best dip-circle with which to furnish their respective observatories.

I shall only allude to one observer more, who, though he only made a single observation, has frequently expressed his wish to make a series, but has hitherto been prevented by his numerous engagements. I speak of Mr. Glaisher, of Greenwich Observatory, who, on 21st October last, obtained with Circle No. 40 a dip of 68° 12′·2, while with Circle No. 33 Mr. Chambers on 19th and 20th October obtained 68° 12′·3.

I have thus endeavoured to show that in the Kew dip-observations there is absolutely no opportunity for mental bias to act, and that even if there were, many of our observers are not likely to have been the subjects of such an influence.

In thus fulfilling your request, it is within my province to notice the second part of Mr. Airy's letter only in as far as this is connected with the subject of discussion. You will, therefore, perhaps permit me to refer you to the following paragraph of his letter, which I shall now quote:—"I have therefore no novelty to claim, except the suggestion (made by me some years ago) of instability in the position of the magnetic axis, and the construction (within little more than a year) of an instrument whose results appear to support that suggestion. I should be much gratified if the powers of the Kew Observatory could be devoted to the examination of this and analogous instrumental difficulties. These experimental inquiries are not well suited to the system of the establishment over which I preside. And, speak-

ing as a member of the British Association, I think that the Kew Observatory would be better employed in that way than in the course which now absorbs so much of its strength. It was originally intended, and in my opinion wisely intended, for the verification and improvement of instruments, and not for continuous observations. If the examination which I propose should be taken up, I should be happy to cooperate, by repetition of observations (as my opportunities might serve), and by communication of my results."

These words, while they imply a request which has been courteously acknowledged by you in your reply, appear also to convey the idea that the Kew Observatory has left the burden of an experimental inquiry regarding dip-circles to the Greenwich establishment, which is not well suited to undertake such a task.

I think that, whatever opinion be entertained regarding the functions of the Kew Observatory, it may be shown that it has fulfilled its duties as respects the dip-circle. I give you the following short sketch of our connexion as an observatory with this problem.

The Kew Committee, being desirous to promote the construction and employment of improved magnetical instruments, procured a dip-circle which was too little known, but which they had reason to think was a good practical instrument. In making monthly determinations of the dip with this instrument at Kew, and in bringing these before the notice of men of science, the Committee have given the most convincing experimental proof which it was in their power to afford of the excellence of this instrument, and they have the satisfaction to think that their work has not been in vain, for the directors of many foreign observatories have supplied themselves with these circles, and as many as could do it have personally inspected them at Kew. Mr. Airy appears to have adopted a different course; as far as I am aware, he has not yet honoured us with a visit to Kew, in order to inspect our dip-circle and become personally acquainted with our method of observation. On the other hand, he has instituted experiments of his own, but has not succeeded in producing a good instrument, and the results which he has thus obtained have induced him to believe that the Kew determinations (although made with a different instrument, which is also handled in a somewhat different manner) are not correct.

The Kew Committee have combated this conclusion, and are not shaken in their belief that they have obtained a nearly perfect dip-circle. They may be right or wrong in this opinion; but while they retain it they cannot surely be justly reproached with having left to the Greenwich Observatory the burden of an experimental inquiry which they can only regard as superfluous and self-imposed.

I remain, my dear Sir,
Yours very truly,
(Signed) B. Stewart.

To J. P. Gassiot, Esq., F.R.S.,
Chairman of the Kew Committee.

V.

Royal Observatory, Greenwich, July 11, 1864*.

My dear Sir,—You were so good as to hold out to me the expectation that probably the Kew Observatory Committee might be able to assist this observatory in some important examinations of discordances in the results of magnetic observations, which have given me great anxiety and trouble. To bring this matter more distinctly to a point I will indicate three subjects,

* At the date of this letter Mr. Airy had not received a copy of Mr. Stewart's letter of July 4th.

of which two have been before me for several years, and the third has lately come before me with great force.

1st. You are in some measure aware of the discordances which I have found in observations of the dipping-needle, made with the smallest conceivable change in the circumstances of bearing, or even (as in some experiments which I have lately transmitted to Prof. Stokes) without lifting the needle at all. I am sure the Kew Observatory would do well in thoroughly investigating this matter by experiment.

2nd. I have been troubled for many years with small displacements in the trace of the vertical-force photograph. I should be glad to have these investigated at the Kew Observatory; but it will be necessary for this purpose to modify the adjustments of the vertical-force instrument at Kew, which at present is incompetent to exhibit such displacements, and masks all that may ever have occurred.

3rd. I should be very glad indeed to have a set of experiments on the temperature corrections of the force of a magnet, made by heating it in hot air instead of by hot water. My own experiments leave us in most distressing doubts.

It will give me great pleasure to cooperate as far as possible with the Kew Committee in these matters; any record of our experiments and any apparatus that we can possibly spare will be at their command.

I am, my dear Sir,
Yours very truly,
(Signed) G. B. AIRY.

To J. P. Gassiot, Esq.,
Chairman of the Kew Observatory Committee.

VI.

Clapham Common, July 13, 1864.

MY DEAR SIR,—I have your letter of the 11th, suggesting certain experiments in relation to magnetic instruments, which I will lay before the Kew Committee at its next meeting.

I have in the mean time forwarded your letter to Mr. Stewart, the Director of Kew Observatory, who will, I am confident, give it his best attention.

I remain, yours truly,
(Signed) J. P. GASSIOT.

To G. B. Airy, Esq., Astronomer Royal.

VII.

Kew Observatory, July 30, 1864.

MY DEAR SIR,—I have perused Mr. Airy's letter, addressed to yourself as Chairman of the Kew Committee, in which he suggests that certain experiments should be made at the Kew Observatory, and I now reply to your request that I should report concerning this letter for the information of the Committee.

From the correspondence which has passed between Mr. Airy and yourself, I have little difficulty in finding the proper basis for this report; the question resolves itself into the following:—Is it expedient in the interest of magnetical science that the Committee should undertake these experiments?

If the suggestions of Mr. Airy refer to points which have not been settled, the Committee are surely indebted to him for bringing these before them; but if, on the other hand, it be the opinion of the Committee that these points have already been discussed and finally disposed of, Mr. Airy cannot blame them if they decline making the experiments which he suggests.

I will take these requests in succession.

1. His first relates to dip experiments and observations. About twenty-five years ago, a few magneticians, including General Sabine and the late Sir J. C. Ross, who were zealous for the advance of magnetical science, set themselves to work to improve the dip-circle. In this problem they had the advantage of the cooperation of the late Mr. Robinson, an excellent mechanician, who had also the subject very much at heart, and whose attention was especially directed to the *axle* of the needle with remarkable success. On his premature death, his process was continued by Mr. H. Barrow. Other improvements were afterwards made, and the Kew Observatory having in the mean time been established, that institution was not slow to recognize the practical excellence of this circle, and the Committee felt themselves able to recommend its general adoption. In order to justify their preference, they instituted a series of monthly observations, the result of which, in their opinion, as well as in that of very many scientific men, has been to demonstrate the practical goodness of this instrument. Not fewer than forty-two of these instruments have been made by Mr. Barrow, and these are, for the most part, in use in different parts of the globe. Many directors of foreign observatories who were previously acquainted with other dip-circles, suspecting these to be inferior to that at Kew, have repaired to our observatory for the purpose of convincing themselves by their own experience that the performance of the Kew circle was not exaggerated. I believe that, without exception, they have been satisfied with our results; but I need not dwell on this topic, as I have already in a previous letter endeavoured to show that our observations are quite trustworthy.

It was the wish of General Sabine, who had taken such an active part in dip-observations, as well as in the construction of the new circle, to exhibit in a scientific manner the probable error of a complete observation of the dip with any Kew instrument; and for this purpose he requested me to furnish him with a complete list of the results obtained at Kew since 1857, omitting none.

These observations were printed in the publications of the Royal Society, and I may be admitted to express my belief that, in the method of reduction employed, the observations were combined in the manner most approved by physicists. I may likewise mention that the probable error therein obtained, small as it is, must not be regarded as wholly due to instrumental inaccuracy, but in part at least to the occurrence of disturbances during some of the observations, a source of error which cannot be avoided. If Mr. Airy will refer to the results of the Kew observations in the Philosophical Transactions for 1863, art. 12, he will see an example of the advantage of employing an inclinometer with the small probable error of that of Kew, in problems of much theoretical importance.

It would thus appear that the Kew Committee have already obtained an almost perfect dip-circle, so that it is not easy to conceive what advantage is to be derived from the experiments proposed by Mr. Airy, especially since, in order to obtain the result which he desires, he has only to become personally acquainted with the working of our instrument, as has been done by those scientific men who have already visited Kew for this purpose.

2. Mr. Airy states,—"I have been troubled for many years with small displacements in the trace of the vertical-force photograph. I should be glad to have these investigated at the Kew Observatory; but it will be necessary for this purpose to modify the adjustments of the vertical-force instrument at Kew, which at present is incompetent to exhibit such displacements, and masks all that may ever have occurred."

I shall take this request in connexion with the following paragraph from Mr. Airy's last Report to the Board of Visitors of Greenwich Observatory:—

"The vertical-force magnetometer still exhibits sometimes the dislocations in the photographic trace. There is no evidence, I believe, that these dislocations do not exist in the curves of every vertical-force instrument, for they are always accompanied with vibration; and no vertical-force instrument, I believe, except that of Greenwich, gives a trace strong enough to exhibit vibrations, and the dislocations, therefore, with any other instrument would appear merely as interruptions of the trace, and would not attract much attention"*.

Before discussing Mr. Airy's request, I shall endeavour to show that our vertical-force instrument is free from objection. In the first place I am able to state, from having examined our vertical-force curves in conjunction with my assistant, that when cause of disturbance takes place the vibrations of our needle *are* impressed upon the photographic paper. Whenever a change takes place in the direction of the forces acting upon a freely suspended magnet, the impulse is followed, and the magnet, after an interval, which may be longer or shorter according to its time of vibration, assumes the new direction. If the changes of force succeed each other more rapidly than will admit of the magnet becoming stationary between their occurrence, it does not cease to vibrate until the intervals between the changes become long enough to permit it to do so†. This state of vibration *is* quite perceptible in the photographic records at Kew; but when the time of vibration is so small as in the Kew instrument, where it is seven seconds only, the *mean* place corresponding to a desired instant is almost always obtainable from the trace. It may suffice that in the six months from July 1 to December 31, 1863 (the records of which are now under reduction), and in which there should be 4416 equidistant hourly positions, there are only five wanting by reason of failures from *all causes whatever*. In one of these the disturbance was so excessive that the trace ran off the recording paper; in the other four the vibrations corresponding to the fluctuations in the directions of the disturbing force were too rapid to permit the trace to be sufficiently distinct for measurement. Should it be hereafter desirable to investigate more particularly the phenomena of the changes thus rapidly succeeding each other, a shorter, not a longer, magnet than the one in use at Kew would be required, having a shorter time of vibration than seven seconds; but in the mean time, and for the present wants of science, there is, I think, every reason to believe that Mr. Welsh exercised a sound judgment in determining the dimensions, shape, and weight of the Kew vertical-force magnet. The self-recording instruments at Kew are now in the seventh year of their performance, and the curves of each magnetograph, including those of the vertical force, have been carefully examined preparatory to reducing them, with the view of eliminating everything of the nature of displacements, whether due to instrumental defects or to the approach of magnetic matter. The curves of the vertical force under this very severe scrutiny have proved themselves as perfect as those of the other magnetometers, that is to say, they are practically faultless as far as one can judge by this means.

General Sabine has kindly undertaken the reduction of the traces afforded by our magnetographs, and finds that the vertical-force magnet is capable

* As far as I am aware, Mr. Airy has not seen any original negative from our vertical-force magnetograph.

† It has already been recognized by Gauss as a law, that no magnet can correctly record those changes of which the period is not considerably more than that of its own vibration.

of being applied in conjunction with the horizontal force to several important problems in which the theoretical bearings of the variations of the dip and total force are concerned, which will be shown as soon as the reductions, already far advanced, are completed; meanwhile instruments of the same pattern have been ordered by the directors of several foreign observatories, who have themselves personally examined the Kew instruments and the records of their performance, and have expressed their intention of working in concert with Kew.

The displacements and dislocations which have occasioned Mr. Airy so much trouble for several years past in the Greenwich vertical-force instruments are obviously due to a cause or causes very different from that which has been noticed above. From his own description of them, we learn that the results in one sheet cannot be compared with those in another, and that in 1859 the vertical-force magnet exhibited for the daily magnetic curve a form approaching much more nearly to a straight line than it had usually given. The imperfection of such an instrument is sufficiently manifest, and it would not be difficult, perhaps, to assign its probable cause or causes; but as it is no longer designed to be used by Mr. Airy himself, I submit that it would be inexpedient to employ the time of the observatory in investigating how much the defect of an instrument which is given up by its employer may be due to one cause and how much to another. The Kew instrument has no such defect; in other words, it is, to use Mr. Airy's expression, "incompetent to exhibit the displacements" (or dislocations) which take place in the Greenwich instrument.

Again, in order to investigate these dislocations experimentally, it would be necessary that the Committee should dismount our present instrument and mount one similar to that which Mr. Airy has discarded, if not that very magnet itself, and Mr. Airy in his request intimates that some such change would be necessary. To dismount an instrument so usefully employed as that at Kew, and with the performance of which for the purposes for which it was devised we have reason to be fully satisfied, for the chance of constructing one of a different form, which might probably not give us equal satisfaction, would seem to be a species of treason to the branch of science which we are endeavouring to advance, as well as to ourselves, and to those who have provided themselves with similar instruments to work in concert with us.

3. Mr. Airy's third request is that we should make experiments in order to determine if there be any difference in the temperature correction as derived when the magnet employed is placed in hot and cold air instead of in water, as is usually the case.

Let me first of all direct your attention to the principle on which the Kew Committee have proceeded for several years past in reference to the subject of temperature corrections. This principle has been *to avoid, as far as possible, the occasion for such corrections,* and the Committee will be glad to learn that Mr. Airy has latterly expressed his intention of adopting the same principle. At the Kew Observatory the variation of temperature to which the magnetographs are exposed is only half a degree Fahr. in twenty-four hours. In like manner, in the instrument for absolute determinations, by making the deflections and vibrations sufficiently near to one another in point of time, the correction for temperature is reduced to a minimum.

But in *former* days a number of experiments *were* made on the temperature correction, some with the purpose of proving that magnetic changes are not caused by the varying temperature of the air, and others which exactly correspond to the point referred to by Mr. Airy, and these lead to the belief that

temperature corrections determined by hot and cold water experiments are almost identical with those determined by hot and cold air.

I find that at Toronto the temperature change of the vertical-force magnet found by comparing together days of different natural temperature was ·00011 for 1° Fahr., while the same determined by hot and cold water experiments was ·00009. At Makerstoun, also, the temperature correction of the balance magnet, as determined by hot and cold days, was ·000079, while that determined by hot and cold water experiments was ·000073.

These agreements are very near, and the first had induced General Sabine to remark that the hot and cold water method was sufficiently correct; while the same conclusion was also arrived at by Mr. Broun of Makerstoun, and, as far as I am aware, has been generally received.

It is impossible for me, after such evidence that both methods give very nearly the same corrections, to doubt that Mr. Airy's *very great difference* must have been occasioned by error of experiment.

As a principle, Mr. Airy will, I think, allow that in such an experiment it is better to have the hot and cold air filling a whole room than filling only a copper box; while at the same time it may be extremely difficult to indicate the precise source of error in his arrangement. I do not think that the Kew Committee are called upon to undertake this task, especially since (as has been shown) the comparison of corrections derived from heated air and heated water *has* already received due attention, the result of which has been to set that matter at rest in the minds of other magneticians; and also since the temperature corrections which will be hereafter required at Greenwich will not be of such magnitude as heretofore, and therefore are not likely to occasion Mr. Airy the same distressing doubts as those spoken of by him.

I remain, my dear Sir,

Yours very truly,

(Signed) B. Stewart.

J. P. Gassiot, Esq.,
Chairman of the Kew Committee.

The two following letters, although of later date than the Kew Report, have been attached to this correspondence by order of the Council.

VIII.

Royal Observatory, Greenwich, S.E., 1864, October 19th.

My dear Sir,—I have to thank you for your kindness in transmitting for my inspection the Kew Vertical-Force Photograms for the months of June, July, and August 1863. They shall be returned by hand at an early opportunity. I have examined them with much interest, and take leave to communicate to you the following remarks on them.

1. The curves are traced more strongly than those which I had previously seen. I think this change a great improvement.

2. The sheets are very neat, uniform, and distinct—more uniform than the Greenwich sheets have been to the end of 1863, but, I think, not more uniform than the Greenwich sheets are now. This change in the Greenwich sheets, I believe, is to be attributed entirely to our gain of nearly uniform temperature, every part of our chemical process being the same as formerly.

3. The small perturbations are recorded with great delicacy—more clearly than in the former Greenwich sheets (though I believe that nearly all can be traced in our curves), but not more clearly than in our new sheets. I make the latter statement from examination of the general character of both, as I have not been able to compare corresponding sheets.

4. The vibrations of the magnet are not well shown. The largest are those of June $9^d\ 2^h\ 10^m$ (some doubt about this), June $20^d\ 14^h\ 17^m$, and July $30^d\ 0^h\ 8^m$; all these are really small, yet they are exhibited very feebly. It seems probable that a larger vibration would leave no visible trace.

5. I conclude from this that very violent and rapid changes of magnetism could not be shown.

6. In this respect the process used at Greenwich (fully detailed in the "Magnetical and Meteorological Results," 1862), which appears to be more sensitive to rapid movements, seems preferable to that used at Kew. I do not propose to make any change for our magnetic instruments; although for our exposed thermometers, in which the changes are not sudden, is yet under consideration whether a process like that of Kew should be introduced.

7. I find upon close inspection that the Kew curves are not free from dislocations; these are, however, smaller than those of the Greenwich curves. A few of them have caught the attention of the Kew observer, and are indicated by dots of red ink. Among these, I think, are June $17^d\ 23^h\ 50^m$, and June $19^d\ 22^h\ 40^m$. But there are many others (all small), as June $23^d\ 23^h\ 40^m$, June $29^d\ 23^h\ 0^m$, July $1^d\ 21^h\ 30^m$, July $3^d\ 22^h\ 50^m$, $23^h\ 30^m$, $23^h\ 35^m$, &c. &c.; July $13^d\ 5^h\ 0^m$ (which I note as occurring at a different hour of the day), &c. These are unimportant as affecting the readings of the curves, but not unimportant as affecting the possible explanation.

8. The comparison of the readings at Greenwich and at Kew, on days when the dislocations at Greenwich are sensible, entirely supports the view which I have entertained for many years, that the dislocations are transient phenomena, in no wise affecting the zero-measurement, and whose effects can by judicious attention be entirely remedied.

I am, my dear Sir,

Yours very truly,

John P. Gassiot, Esq.,
Chairman of the Kew Committee.

(Signed) G. B. Airy.

IX.

Royal Observatory, Greenwich, S.E., 1864, November 15th.

My dear Sir,—You are aware, perhaps, that Mr. Glaisher has visited the Kew Observatory, and that Mr. B. Stewart and Mr. Whipple have visited this Observatory, and that two of the Kew Dip instruments have been transported backwards and forwards; and that observations have been made with them by all the gentlemen whom I have mentioned, at Greenwich and at Kew; and that during these operations I have myself carefully examined the principal parts of the instruments, though I have not made any complete observations with them. The result of these operations is as follows:—

1. As far as depends on the mechanical construction, of the instruments including the needles, the workmanship of the instruments (I am not now speaking of the extent of applicability but of the workmanship of the important parts) is very good, of the same class as ours; I think ours better in some respects, but they may be considered as the same class.

2. As regards the results of observations, those made with the Kew instruments are consistent to a degree which I never saw before. And in the experiment which, as made with our needles, has perplexed me most, namely, that of rotating the instrument in azimuth without touching or lifting the needles, and remarking the change in their indications, the Kew needles appear to be nearly or entirely free from such change.

RECEIPTS.

	£	s.	d.	£	s.	d.
Received from the General Treasurer				600	0	0
„ (Donation)				100	0	0
„ for the verification of Instruments from the Board of Trade	11	14	0			
„ from the Admiralty	12	0	0			
„ from Opticians	19	18	0			
„ for Barograph Curves sent to the Meteorologic Office, London	26	2	10			
„ from the Russian Government for the verification of Magnetographs sent to St. Petersburg	30	0	0			
„ for the construction of standard Thermometers	3	6	0			
				103	0	10
Balance				45	17	9
				£848	18	7

PAYMENTS.

	£	s.	d.	£	s.	d.
Balance from last account				189	11	0
Salaries, &c. :—						
To B. Stewart, four quarters, ending 1st October, 1864	200	0	0			
Ditto, allowed for petty travelling expenses	10	0	0			
C. Chambers, until departure	12	10	0			
G. Whipple, four quarters, ending 18th September, 1864	75	0	0			
T. Baker, four quarters, ending 29th September, 1864	50	0	0			
R. Beckley, 55 weeks, ending 12th September, 1864, at 40*s.* per week	110	0	0			
F. Page, 26 weeks, ending 4th April, 1864, at 8*s.* per week	10	8	0			
Ditto, 23 weeks, ending 12th September, 1864, at 10*s.* per week	11	10	0			
				479	8	0
Apparatus, Materials, Tools, &c.				23	17	4
Ironmonger, Carpenter, and Mason				23	5	9
Printing, Stationery, Books, and Postage				29	10	11
Coals and Gas				49	13	9
House Expenses, Chandlery, &c.				25	6	8
Porterage and petty expenses				17	5	2
Rent of Land to 10th October, 1864				11	0	0
				£848	18	7

I have examined the above account and compared it with the vouchers presented to me.

The excess of Expenditure over Income for preceding years, as stated in the Report of 12th August, 1863, amounted to			£189	11 0
The Expenditure for the present year			659	7 7
			848	18 7
The Receipts amount to	£703	0 10		
A Donation from the Chairman	100	0 0	803	0 10
Leaving a balance of			£ 45	17 9

R. HUTTON.

26th August, 1864.

The results for Dip obtainable with the Kew Dip instruments are undoubtedly more consistent and more certain than I had supposed them to be.

In considering the possible cause of this difference in the phenomena of the two sets of needles, I am led to the strong belief that it is not in any way mechanical. The mechanical structure and treatment is the same. I am inclined to suppose that it depends on the original quality and the subsequent tempering of the steel. I am not aware that the Kew Committee have published anything on this point.

I am, my dear Sir,
Yours very truly,
(Signed) G. B. AIRY.

John P. Gassiot, Esq.,
Chairman of the Kew Committee.

Report of the Parliamentary Committee to the Meeting of the British Association at Bath, September 1864.

The Parliamentary Committee have the honour to report as follows:—

The Dukes of Devonshire and Argyll, the Earls of Harrowby and Enniskillen, and Sir John Pakington, have vacated their seats, but your Committee recommend their re-election.

Your Committee recommend that the Vacancy in the House of Commons' List be supplied by the election of Mr. Goschen.

Your Committee suggest that they should be permitted to propose for Election Members of either House of Parliament, in addition to the thirteen Members now constituting their Committee, whenever such addition may appear desirable.

These additional Members might be considered as Supernumerary, and any Vacancy in the Supernumerary List supplied, or not, as may be thought expedient, when the Vacancy occurs.

Your Committee also recommend that a Resolution, passed at Liverpool in 1854, be rescinded, and the following substituted:—

"That any Member of the Parliamentary Committee, who shall not attend any one of four consecutive Meetings of that Committee, shall be considered as having resigned, but shall be eligible for re-election."

No subject was referred to your Committee at Newcastle, but several of its Members have supported, or signified their intention to support, the valuable suggestion of the Royal Commissioners, that the study of Natural Science should be introduced into certain Public Schools, and likewise the Bill for legalizing the use of the Metric System of Weights and Measures.

In thus acting, the Committee conceive that they are properly fulfilling the important duty imposed upon them of "Watching over the interests of Science."

WROTTESLEY, *Chairman.*

17th August, 1864.