In conclusion, it may be desirable to describe in a few words the method by which a sextant may be verified by means of this apparatus.

Let us suppose the collimators to be accurately and quite immoveably fixed, and the angular distances between them to be accurately determined. Also let the distance between the two lenses *a* and *b* of any collimator be such that the collimator F may be seen through *a* at the horizon-glass, and the collimator G through *b* at the index-glass of an ordinary sextant placed on the table B.

In order to test the index-error of a sextant, the vertical line of a collimator is made to bisect the cross lines belonging to the same collimator in the field of view of the telescope of this sextant. If the sextant is accurate, it should read *zero*, since these two lines are infinitely distant and in the same direction.

The sextant is next placed with its horizon-glass receiving the rays from the vertical line collimator F_4 (G_4 being covered), and its index-glass receiving the rays from the cross line collimator G_5, and the telescope arm is moved until F_4 bisects G_5 in the field of view; if the instrument is correct, the reading ought to be (by Table III.) 15° 0′ 10″·4. By pursuing this method it is evident from Table III. that the error of graduation of the sextant may be determined at every 15° of its arc.

In conclusion, it ought to be mentioned that perhaps no artifical light easily obtainable is sufficiently powerful to allow of the darkest glasses of a sextant being examined, and that for this purpose we may ultimately have to resort to other means.

II. "On the Observations made with a Rigid Spectroscope, by Captain Mayne and Mr. Connor, 2nd Master of H.M.S. 'Nassau,' on a voyage to the Straits of Magellan." By J. P. Gassiot, F.R.S. Received May 25 and June 3, 1867.

In a communication I made to the Royal Society on the 18th of May 1865 (Proceedings, vol. xiv. page 320), I described the rigid spectroscope which, at the suggestion of Mr. Balfour Stewart (in connection with a plan jointly conceived by Prof. Tait and himself), I had had constructed, the object sought being to determine by observation whether the index of refraction does not vary with the coefficient of terrestrial gravity, for which purpose it was thought desirable that the observation should be entrusted to some officer on board one of H.M. ships visiting various latitudes on both sides of the equator.

Through the kindness of Captain Richards, Hydrographer to the Admiralty, I obtained an introduction to Captain Mayne, of H.M. Ship 'Nassau,' at that time (August 1866) fitting out at Woolwich, preparatory to making a survey of the Straits of Magellan, and by appointment with Captain Mayne I visited the 'Nassau' in company with Captain Richards.

After carefully examining one or two positions in which the instrument could be placed, Captain Mayne selected a place in his own cabin, and explained to us that it was his intention to place the spectroscope in charge of an intelligent young officer, Mr. Connor, the 2nd Master, by whom the observations would be made; but as the instrument necessarily remained in Captain Mayne's cabin, the observations would be made generally in his presence, and under his immediate superintendence.

Captain Mayne and Mr. Connor shortly afterwards examined the spectroscope at the rooms of the Royal Society, in the presence of Mr. Stewart and myself, when they practised the mode of observing; but, in order to ensure the observations being subsequently made without any bias as to obtaining particular results, no further explanation was given to Captain Mayne or Mr. Connor, the latter being merely requested day by day to note the result of his observations, and to enter the same in printed forms with which he would be supplied, Captain Mayne promising to forward the particulars to Captain Richards at his convenience.

The form supplied was as follows:—

Date.	Latitude.	Barometer.	Temp. of air.	Temp. of prisms.	Reading of micro-meter for D line.	Remarks.

Mr. Browning, who constructed the instrument, took charge of it on the 21st of August 1866, and proceeding to Woolwich placed it on board the 'Nassau,' in the position which had been arranged by Captain Mayne.

On the 4th inst. I received a letter from Captain Mayne, of which the following is an extract:—

"H.M. Surveying Ship 'Nassau,' Straits of Magellan,
Feb. 16, 1867.

"MY DEAR MR. GASSIOT,—As we are on our way to the Falkland Isles, and my time will probably be fully occupied when we get there, I write you a few lines to say that I am sending to the Hydrographer a diagram of the observations of the spectroscope taken since we left England; with it I am sending a few remarks. I can only say that our observations have been carefully taken, and I hope a discussion upon them may throw some light upon the subject in which you and others are so interested. Mr. Connor plotted the diagram with great care. Usually the observations have been made by him, but I have taken

them occasionally as a check, and also during the time he was laid up with a wound which these wretched Fuegians gave him. You will see the method pursued in the diagram is to give the whole voyage complete, and also the fluctuations during our stay at the various places named. Let me add, what I also said to the Hydrographer, that we shall be happy to carry out any changes in position of the instrument, or mode of observation, you may wish, so far as our other duties will permit. I shall be very glad also to hear that our observations have been in any way useful. The weather we have hitherto experienced has been rather better than we expected, but gale, gale, gale, the wind seems never tired; if it does for a few hours forget to maintain the credit it has obtained, be sure you will find a current of five or six knots directly opposed to the course you wish to pursue.

"Please remember me kindly to Mr. Stewart when you see him, and

"Believe me yours sincerely,

(Signed) "R. C. Mayne."

"*J. P. Gassiot, Esq., F.R.S.*"

The following is a copy of the letter referred to in the preceding extract:—

(Copy.)

"H.M. Surveying Ship 'Nassau,' Straits of Magellan,
Feb. 15, 1867.

"Sir,—I beg to forward the following remarks on the rigid spectroscope which was placed on board this ship at the request of Mr. Gassiot, V.P.R.S., with the view of determining whether the position of the D line of the spectrum changes with the coefficient of terrestrial gravity. Accompanying the remarks is a diagram, intended to show at a glance the fluctuations which have actually occurred in the line of the spectrum during our voyage from Plymouth to the Straits of Magellan, as well as those of the barometer and "air" and "prism" thermometers during the same period. In addition to this it has been thought advisable to plot in the same way the fluctuations which occurred during the ship's stay at the various ports of call on the voyage out independently, as they cannot of course be in any way due to change of gravitation. They will be seen on the lower part of the diagram, the same number of observations having been plotted after our arrival in the straits as were taken at Plymouth, the two places being so nearly in the same latitude.

"A description of this instrument has been given by Mr. Gassiot before the Royal Society. Its position on board being selected by Mr. Gassiot himself, in concert with Mr. Browning, it was placed, at their desire, on the port side of my cabin, and its position has in no way been altered since. All the observations have been made either by Mr. Connor or myself: Mr. Connor, in whose special charge the instru-

ment was placed, having observed it by far the most frequently, and having given considerable attention to it. The observations were made at noon daily, and, owing to the deficiency of light, have (at Mr. Browning's instance) been made by bringing the moveable micrometer wire into the centre of the right bright space instead of its right edge, as I understand was the use at Kew.

"The diagram seemed to show that the barometer affects the instrument, the micrometer reading increasing as the barometer falls, and *vice versâ*. In this regard it is curious that, from a series of twenty observations, taken since we have been in the Straits of Magellan, Mr. Connor deduced that the micrometer should read 3·84 when the barometer was 30 inches and attached thermometer 54° F., and that a few days since, when the barometer and thermometer were as above, the micrometer reading was 3·86. The fluctuations shown when the ship was stationary seemed to point out that at least the barometer has very considerable effect upon the reading of the instrument: what amount of the general changes shown may be due to change in the coefficient of terrestrial gravity I leave to those who have made the spectrum their study to determine.

"The barometer readings shown on the diagram have purposely not been reduced to the mean temperature of 32° Fahr., as it is thought they show better what is desired than if they were so; should it be thought advisable to reduce them it can easily be done, as the temperature of the air-thermometer plotted hardly differs perceptibly from that attached to the barometer. The observations are still being made daily, and the record of them kept; if any change of position, other method of observing, or any other alteration is thought advisable by those interested in the observations, it shall receive all the attention the nature of the service on which we are employed will permit us to bestow.

"I have the honour to be, Sir, your obedient Servant,

(Signed) "R. C. MAYNE, Captain."

"*Captain Richards, R.N., Hydrographer, Admiralty.*"

As the diagrams referred to could not be conveniently engraved for insertion in the Proceedings, they remain at the Royal Society.

The observations of Captain Mayne and Mr. Connor have evidently been made with great care; the diagrams executed by the latter gentleman exhibit at a glance the actual results.

In these diagrams two series of observations are recorded; the first exhibits the spectroscope reading, along with the reading of the barometer, of the thermometer imbedded in the glass prism, and of that showing the temperature of the air around the instrument as the vessel proceeded on its course.

The second exhibits similar records, made when the vessel stopped

at various places during the voyage. In order to make use of these records, it became necessary to ascertain the corrections of the instrument.

Immediately on receipt of Captain Mayne's letters, I forwarded them to Mr. Stewart, to whom I am indebted for the following observations, with the necessary corrections for temperature, &c. These corrections are,—

I. *The Temperature Correction.*

In order to determine the correction, very complete sets of experiments were made by Mr. Balfour Stewart at Kew Observatory, and by Mr. Browning at the Minories.

In January 1866 the spectroscope was conveyed to Kew Observatory, and was there exposed to a change of temperature equal to 30° Fahr. The change was applied very gradually, the experiments for one set lasting nearly one month; in respect to duration these changes were consequently analogous to those to which the instrument would be exposed at sea, but, on the other hand, the instrument, when at Kew, was not subjected to vibrations.

In February 1865 the spectroscope had been subjected to similar changes of temperature at Mr. Browning's house of business in the Minories, which abuts on the Blackwall railway. The results there obtained are recorded in the Proceedings of the Royal Society (vol. xiv. June 1865). These observations extend over a range of 30° Fahr.; and as in the greater portion of the time during the observations the instrument was subject to constant vibration from the ordinary work carried on in the work-rooms, as well as from the abutting railroad, these constant vibrations were so far analogous to the action to which the instrument would be subjected to on board a vessel.

On the other hand, the heating and cooling each took place on the same day, and as far therefore as duration is concerned, the temperature changes at the Minories were dissimilar to those to which the instrument was exposed at sea.

The result of these experiments was that, for an increase of temperature of 30° Fahr., there was observed an increase in the reading of 1·32 revolution of the micrometer screw.

The following Table exhibits the result of the observations made at Kew Observatory:—

Date.	Temperature of prism.	Reading.	Date.	Temperature of prism.	Reading.
1865.	°		1865.	°	
Jan. 13.	48·2	0·67	Feb. 12.	52·8	1·26
14.	51·5	0·75	18.	52·5	1·22
16.	75·6	2·00	23.	49·9	1·04
17.	80·8	2·24	24.	50·8	1·02
18.	76·0	2·22	March 2.	73·7	2·26
19.	74·8	2·05	3.	76·8	2·43
20.	75·1	2·04	4.	76·6	2·43
21.	70·5	1·71	5.	75·0	2·36
22.	75·0	2·04	6.	78·3	2·64
23.	74·5	1·87	7.	79·0	2·76
24.	75·4	1·86	8.	78·9	2·68
25.	74·6	1·83	9.	76·8	2·46
26.	75·4	1·92	10.	75·5	2·28
27.	77·4	2·15	12.	56·3	1·37
28.	75·9	2·09	21.	51·0	1·26
29.	76·7	2·14			

These experiments consequently consist—

(1) of a set of readings at low temperature.
(2) „ „ high „
(3) „ „ low „
(4) „ „ high „
(5) „ „ low „

If we compare (2) with the mean of (1) and (3), and (4) with the mean of (3) and (5), we obtain as the Kew correction for temperature an increase of 1·44 revolution for an increase of 30° Fahr.

On comparing the temperatures obtained under very different treatment at the Minories at Kew, and we find—

At the Minories 30° Fahr. gives 1·32 revolution.
At Kew . . . 30° „ 1·44 „

Mean . . . . 1·38 „

These two results are therefore extremely consistent with each other, although the treatment to which the instrument was exposed differed materially in the two cases. We should therefore say that the temperature correction should not vary with change of treatment, and have therefore considerable confidence in applying the above value of it to observations made on board the ‘Nassau.’

II. *Correction for Change of Atmospheric Pressure.*

It is justly remarked by Captain Mayne that the readings seemed to vary with the barometer.

This correction is, however, small; and as the change of mean barometric pressure between the different latitudes is small also, the correction will not affect the range, but it will affect the comparability of individual observations, and ought therefore to be determined. This is best done by means of the observations themselves after the temperature corrections have been applied.

Thus we find that, when the ship was stationary at Plymouth, there were considerable fluctuations of the barometer, and from the observations made there, it appears that a rise of half an inch creates a fall in the reading =0·18. Similar fluctuations took place while the ship was stationary at Magellan, and from these we obtain a fall of 0·24 for every rise of half an inch.

The mean of the two, or a fall of 0·21 in the reading for a rise of half an inch, may safely be adopted.

No correction has been applied for the hygrometric state of the air. M. Jamin has found that aqueous vapour, at the temperature of 0° Cent., and under the pressure of 0·76 mètre, would have for its index of refraction (if it could exist under such a pressure) the value of 1·000261, which is less than that of air, which is 1·000294; the superior quantity of aqueous vapour would thus tend to diminish the index of refraction of air at the equator, as compared with that of the same pressure at higher latitudes.

This action of vapour, which is very small, will be such that without it the residual range (which will be afterwards exhibited) would appear to be somewhat larger than it at present appears; it cannot therefore account for the residual range, and may be in the meantime neglected.

If we now tabulate from the diagram sent by Captain Mayne, and if, by means of the above-named corrections, we reduce all observations to 60° Fahr. and to 30 inches barometric pressure, we obtain the following readings for the various latitudes between 45° N. and 45° S., the ship being in motion.

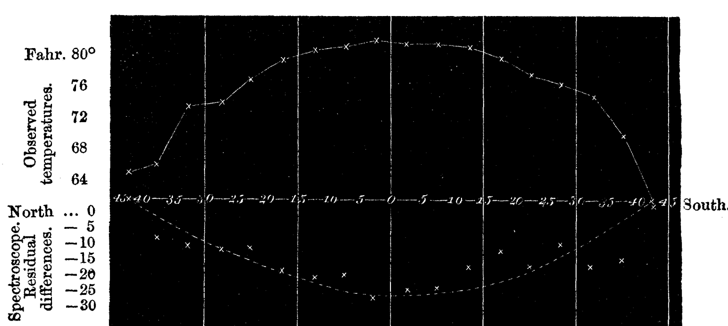
Note.—Since the barometer correction is small, and the range of temperature from the equator to the high latitudes of the voyage not much more than 20° Fahr., it has been thought unnecessary to reduce the barometer readings to 32° Fahr.

Latitude.	Readings reduced.	Reading reduced and corrected for fall of zero.	Difference (negative).
45° to 40° N.	4·71	4·71	·0
40 — 35	4·56	4·58	·13
35 — 30	4·50	4·55	·16
30 — 25	4·47	4·54	·17
25 — 20	4·45	4·54	·17
20 — 15	4·35	4·47	·24
15 — 10	4·31	4·45	·26
10 — 5	4·29	4·46	·25
5 — 0	4·20	4·39	·32
0 — 5 S.	4·20	4·42	·29
5 — 10	4·19	4·43	·28
10 — 15	4·22	4·49	·22
15 — 20	4·25	4·54	·17
20 — 25	4·18	4·49	·22
25 — 30	4·16	4·56	·15
30 — 35	4·13	4·49	·22
35 — 40	4·13	4·52	·19
40 — 45	4·29	4·70	·0

On comparing the last number of the second column with the first it will appear that there is a change in the zero of the instrument; presuming that this change took place during the voyage at the same rate, we obtain column 3 corrected for change of zero, and column 4 representing differences.

This residual difference, which remains after all known corrections have been applied, is exhibited for the different latitudes in the following diagram (fig. 1), while the observed temperatures of these latitudes are also exhibited.

Fig. 1.



In the next place, let us take the diagrams sent by Captain Mayne, which exhibit the readings of the spectroscope at the various places of

call, and correcting these as before for temperature and atmospheric pressure, we obtain the following results:—

Station.		Date.	Mean of reading reduced to 60° Fahr. & 30 inches pressure.	Readings corrected for change of zero.
Name.	Latitude.			
	° ′	1864.		
Plymouth . . .	50 22 N.	Sept. 15	4·72	4·72
Funchal . . .	32 38	Sept. 28 to Oct. 2 .	4·42	4·53
St. Vincent . . .	16 54	Oct. 9 to 13 . .	4·22	4·44
Rio Janeiro . .	22 55 S	Nov. 3 to 13 . .	4·29	4·62
Monte Video Bay .	34 54	Nov. 23 to Dec. 3 .	4·09	4·53
Magellan Straits .	52 30	Dec. 1866 to Jan. 1867	4·16	4·72

We have here a result precisely similar to that obtained by considering the observations when the ship was in motion, and we may exhibit the following Table, showing the residual unexplained difference appearing to be connected with latitude, as determined by these observations when the ship was at rest.

Station.		Residual difference from Plymouth.
Name.	Latitude.	
Plymouth	50° 22′ N.	0
Funchal	32 38	−19
St. Vincent	16 54	−28
Rio Janeiro	22 55 S.	−10
Monte Video	34 54	−19
Magellan	52 30	0

Thus, whether we take the obervations when the ship was in motion, or those when she was at rest, we find a very perceptible residual difference, the cause of which is unknown, and the tendency of which is to make the readings at the equator about 0·33 revolution lower than those at high latitudes; a decrease in reading, it may be well to mention, denotes a decrease of refraction, as may be seen by considering the construction of the instrument, and as has been determined by direct experiment.

J. P. G.

Clapham Common, May 25, 1867.

In forwarding the preceding communication to Professor Stokes,

I ventured to request his opinion thereon, and with his permission beg to annex his reply with Mr. Stewart's remarks.

"Cambridge, 29th May, 1867.

"MY DEAR SIR,—I have read and carefully considered your Spectroscope paper, and send you my remarks.

"On examining Captain Mayne's diagram, we are struck with the paramount influence of change of temperature. I say temperature, without specifying whether of air or prism, for the two are nearly equal, so that we have not data to decide. It appears therefore that the small variation for temperature, previously known to exist, forms the leading variation observed; and the variation due to any other cause must be sought for in the residue left on eliminating this. Hence accuracy in the temperature-correction applied is of much importance.

"To what degree of accuracy then can we trust the temperature correction? To form a notion of this, I took* the mean temperatures and mean readings for the five groups of Kew observations, and the mean of the means. Taking the mean of the mean readings to correspond with the mean of the mean temperatures, and applying Mr. Stewart's temperature-correction (Kew observations), namely, +1·44 reading for +30° temperature, I calculated the reading for each of the five groups, subtracted the results from the observed readings, and regarded the differences as errors. The mean error is ·19; and the difference between two quantities subject both to errors having a mean value of ·19 would be given to within a mean error of ·27, which being for 25° would correspond to ·32 for 30°. No doubt the mean of several comparisons would come closer than this, but ·20 for 30° or ·13 for 20° may be taken to be a very probable uncertainty.

"Mr. Stewart's corrected numbers are plotted in fig. 2. The curve pretty plainly exhibits two features, (1) a general descent, (2) a concavity turned upwards.

"The general descent Mr. Stewart attributes, most probably correctly, to a progressive change in the instrument. *On the assumption* that the progressive change is uniform, the readings would be represented, so

*

Mean temperature.	Mean reading.	Calculated reading.	$Obs^d - calc^d$.
49·8	0·71	0·98	−·27
75·5	2·01	2·21	−·20
51·5	1·13	1·05	+·08
76·7	2·48	2·25	+·23
53·6	1·31	1·16	+·15
61·4 (Mean of means.)	1·53 (Mean of means.)		·19 Mean error.

far as this cause of variation is concerned, by the ordinates of the straight line (dotted), fig. 2; and accordingly Mr. Stewart, *assuming the uniformity of instrumental change,* takes the difference of ordinates of the dotted and curve lines (fig. 2) as a quantity unexplained by known causes. This quantity follows very well the march of the latitude, *or of the temperature which marches with the latitude.* It would be removed by supposing the temperature-correction applied to be too great by about

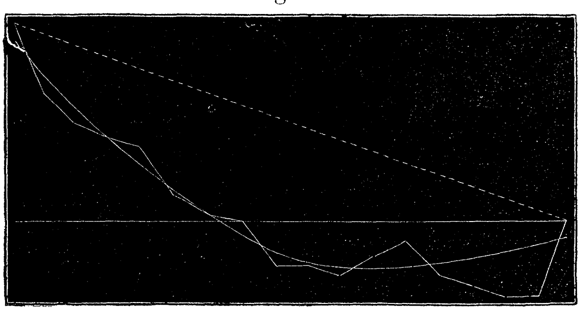
0·30 or 0·33 for 20°.

"Such an error in the temperature-correction can hardly be said to be too great to be admitted. Still it is greater than we should expect *if the London and Kew determinations apply to the state of things on board ship.*

"I do not think Mr. Browning's conjecture that the progressive change was due to an alteration in the glass of the prisms probable. I should think it more probably due to a slow release from a state of constraint in which the instrument was left by the boltings, &c. If so, I should expect that the instrument would shake itself down to a permanent state—that the instrumental change would be more rapid in the early than in the later part of the voyage.

"In fig. 2 I have drawn a smooth curve by the eye following generally the irregular curve, with a view to clearing in some degree the observa-

Fig. 2.



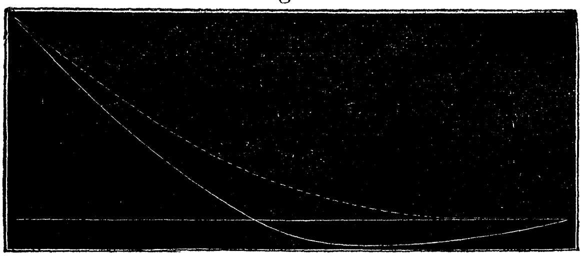
tions from casual errors. The curve ought perhaps to lie a little lower on the right, but I was anxious to lean rather towards Mr. Stewart's view than the reverse. The smooth curve of fig. 2, lifted so as to cut the axis on the extreme right, is transferred to fig. 3. If we suppose the instrumental change more rapid at first than afterwards, the correction thence arising will be represented by the ordinate, not of a straight line as in fig. 2, but of some such *concave* (upwards) curve as the dotted curve of fig. 3, and the unexplained residue will be reduced to the ordinates intercepted between the curves of fig. 3. If the curves are somewhat as I have drawn them, this residue will follow very well the march

of the latitude, *or of the temperature* which marches with it, and may be removed by supposing the temperature-correction applied by Mr. Stewart to have been too great by about

0·10 for 20°,

a quantity well within the limits of uncertainty.

Fig. 3.



"I think therefore that the statement made in the paper—that a residue so and so exists after all known causes have had their effects eliminated—is too boldly advanced.

"The inference I should be disposed to draw from the observations is:—

"That the influence of the variation of gravity (or rather, I believe it is supposed to be, of the potential of the Earth's attraction) does not exceed, in passing from lat. 45° to the equator, a change of refraction for the yellow of the spectrum equal to about three-fourths of the interval of the D-lines; and that even this small apparent change may be referred with great probability to known causes.

"As to future observations I should say—

"I. Let the observations be repeated on the homeward voyage (I presume the survey is not yet finished, and the 'Nassau' is still in the Straits of Magellan). If the instrument has shaken down into permanence the result will be different (I mean the result *un*corrected for change of zero), and one source of uncertainty will be removed.

"II. Should the observations taken in the homeward voyage lead to the same result, repeat the observations for temperature-correction by means of the change naturally occurring with change of season, relying chiefly on observations taken during pretty *uniform* weather of whatever kind.

"Yours very truly,

"*J. P. Gassiot, Esq.*" "G. G. Stokes."

"Kew Observatory, 31st May, 1867.

"My dear Sir,—I have read Prof. Stokes's remarks on your proposed communication regarding the rigid spectroscope, and before adverting to any point on which we may have a difference of opinion, it may be well to remark that we both agree that the experiment in its present state is not decisive—in any case more observations must be

made. Acknowledging with him that the variation due to any other cause than temperature must be sought for in the residue left on eliminating the temperature-correction, I yet venture to differ with him regarding the degree of accuracy to which we can trust the temperature-correction.

"Referring to Prof. Stokes's numerical results, I should object to found any theory upon the first-noted reading ·71.

"The rigid spectroscope was unfortunately brought to Kew only one day before the temperature experiments (instituted more particularly for the magnetographs) commenced, and only two readings were taken before the temperature was raised. I believed it only right to include these two in the account given of temperature observations, but I take this opportunity of saying that I do not attach much value to the mean of these two.

"If these two be excluded, Prof. Stokes's Table will be modified in the following manner:—

Mean temperature of set.	Mean reading observed.	Deviation from mean temp. of the four sets.	Calculated reading.	Observed − calculated.
75·5	2·01	+11·2	2·26	−·25
51·5	1·13	−12·8	1·13	·00
76·7	2·48	+12·4	2·31	+·17
53·6	1·31	−10·7	1·23	+·08

giving an error much less than that shown by Prof. Stokes.

"If now we compare together the temperature-corrections for 30° of range, as determined at the Minories and at Kew, we find—

		Difference from mean.
(A) Temperature-correction for 30° at the Minories	=1·32 ...	−·06
(B) " " Kew	1·44 ...	+·06
Mean	1·38	

"I should imagine the set of observations taken on board to be comparable in number and accuracy with either (A) or (B), and I should expect, *on the supposition that the temperature-correction may be determined equally well by land and by sea observation*, to obtain a result differing from the mean of (A) and (B) by a quantity something like ·06. If temperature-correction be the same at land and at sea, I think therefore that the residual difference observed at sea cannot be attributable to an imperfect estimation of temperature-correction.

"Prof. Stokes next suggests that the progressive change of the instrument may have been greatest at the first part of the voyage. Allowing as the greatest possible extreme though very improbable case, that it all took place before the equator, that would still leave a difference of ·09 revolution to be accounted for.

"But this supposition cannot evidently be entertained; indeed, as the

instrument was nearly two years old when it went to sea, there is a difficulty in supposing that the correction was very much greater at the first half than at the second half of the voyage.

"On the whole I should be disposed to state the result in the following manner:—

"That the influence of the variation of gravity does not exceed, in passing from lat. 45° to the equator, a change of refraction for the yellow of the spectrum equal to about three-fourths of the interval of the D-lines; but more observations must be made before it can be asserted that this apparent change is not due to known causes.

"Yours very truly,

"*J. P. Gassiot, Esq.*" "B. STEWART."

So favourable an opportunity of making correct observations with a delicate apparatus like the rigid spectroscope may not again offer, and consequently, in acknowledging the receipt of Captain Mayne's letter of 17th Feb., an extract from which is inserted in the preceding communication, I explained how desirable it will be while the 'Nassau' remains in the Straits of Magellan if one observation on each day is taken, or two when any very considerable range of temperature occurs, for the purpose of being made use of both for change of zero and as checks upon temperature observations to be taken on shore on the return to this country, as otherwise the observations thereon would not be so useful.

Should it be found practicable on the return of the 'Nassau,' it is purposed to take a few days' readings before the spectroscope is removed from the ship to Kew Observatory, as this would much promote the correctness of the final result which may then be anticipated.

J. P. G.

Clapham Common, June 3, 1867.

III. "On some Elementary Principles in Animal Mechanics." By the Rev. SAMUEL HAUGHTON, M.D., Fellow of Trinity College, Dublin. Received May 15, 1867.

There are some elementary principles in animal mechanics which are so natural that they may be assumed as probable, and as such, have not received from observers the attention they really deserve.

Among these principles I select for illustration the two following:—

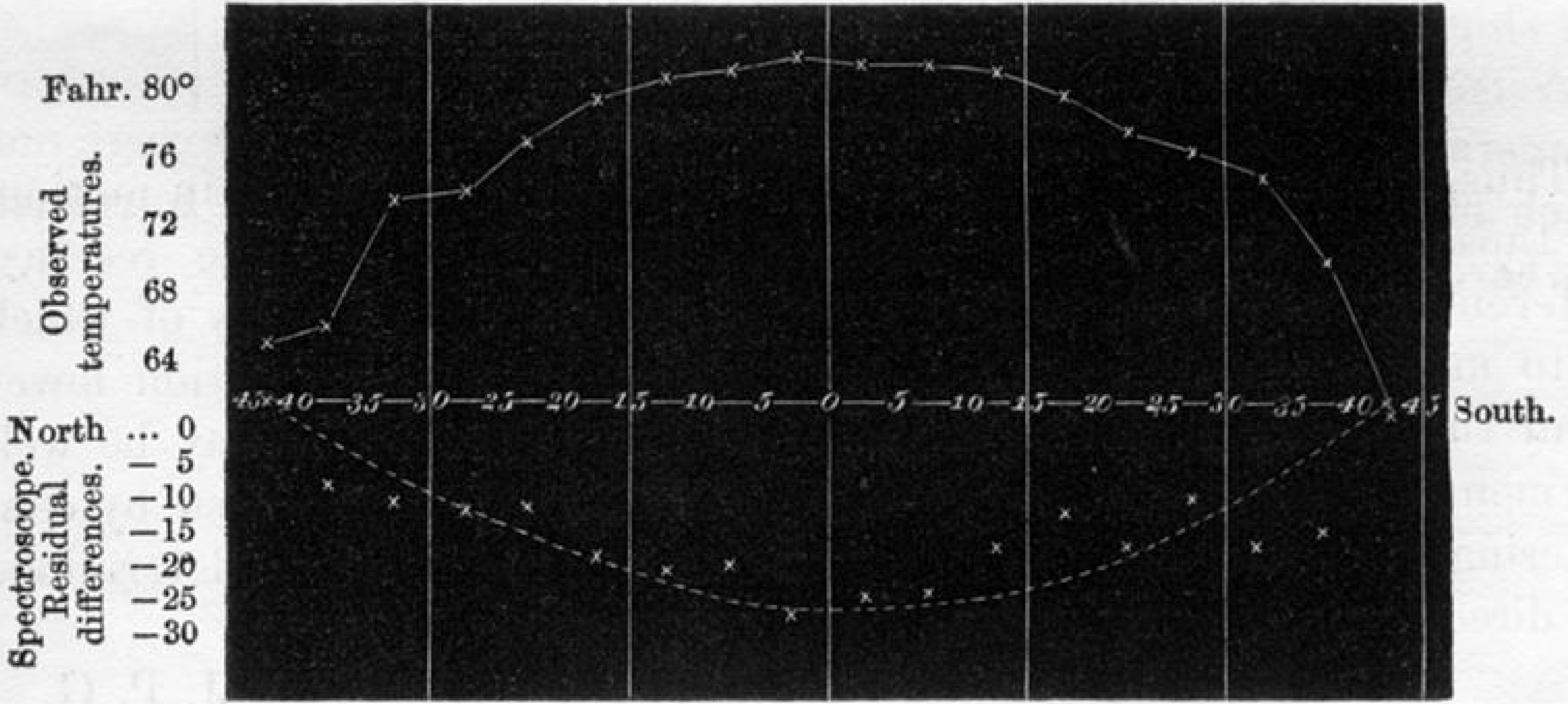
i. *The force of a muscle is proportional to the area of its cross section.*

ii. *The force of a muscle is proportional to the cross section of the tendon that conveys its influence to a distant point.*

i. In order to test the first of these statements, I made a careful examination of the cross sections of the muscles that bend the fore arm and leg, in a very finely developed male subject, with the following results:—

Neglecting the slight effect of the *Supinator radii longus* in flexing

Fig. 1.



Fahr. 80°
76
72
68
64
Observed temperatures.
45 40 35 30 25 20 15 10 5 0 5 10 15 20 25 30 35 40 45
North ... 0
South.
− 5
−10
−15
−20
−25
−30
Spectroscope. Residual differences.

Fig. 2.

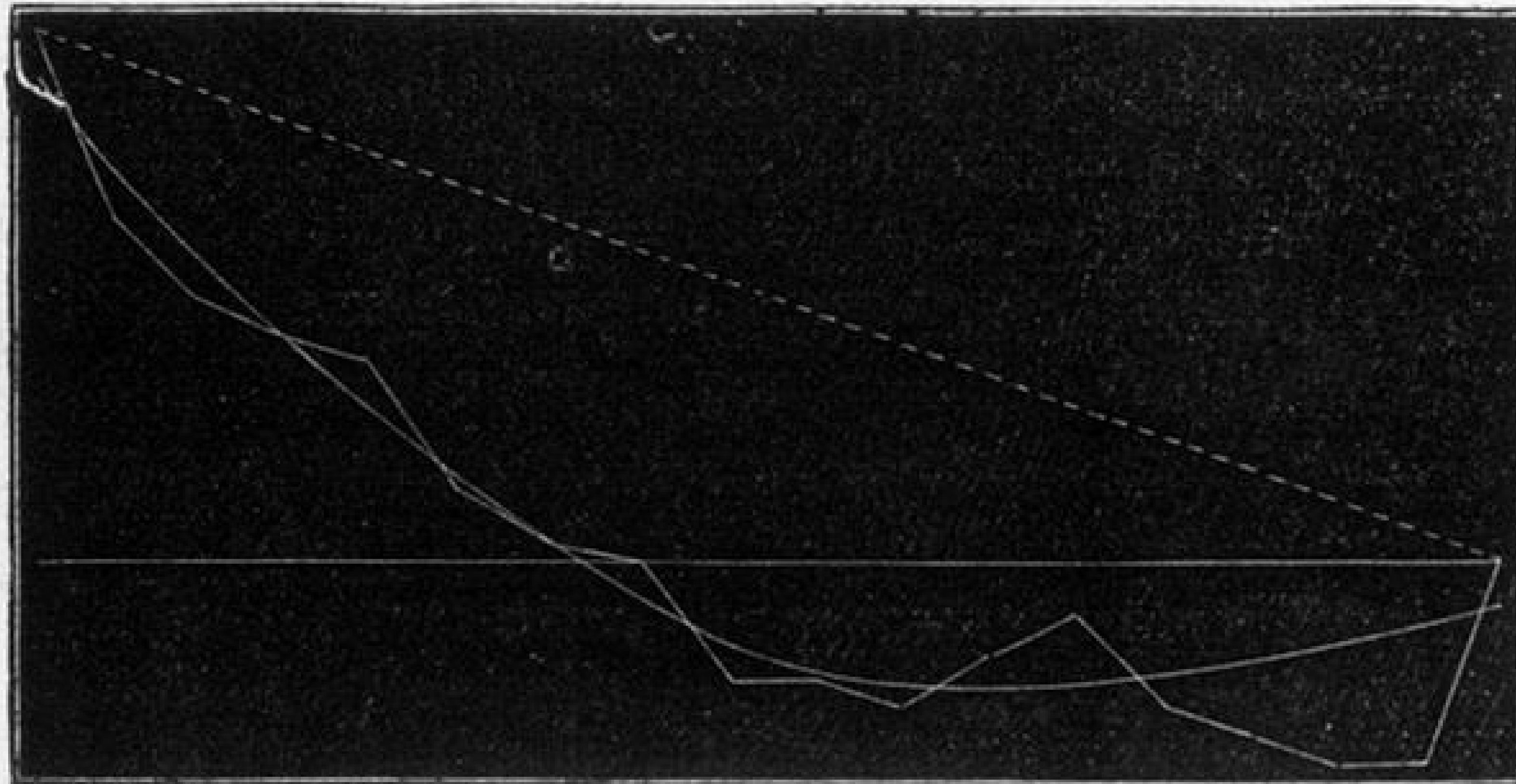


Fig. 3.