PHILOSOPHICAL

TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXXXVI.

PART I.

LONDON:

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MDCCCXXXVI.

ADVERTISEMENT.

The Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions,* take this opportunity to acquaint the Public, that it fully appears, as well from the Council-books and Journals of the Society, as from repeated declarations which have been made in several former *Transactions,* that the printing of them was always, from time to time, the single act of the respective Secretaries till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgement of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

A List of Public Institutions and Individuals, entitled to receive a copy of the Philosophical Transactions of each year, on making application for the same directly or through their respective agents, within five years of the date of publication.

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The Observatory at Armagh.
The Observatory at the Cape of Good Hope.
The Observatory at Paramatta.
The Observatory at Madras.
The Observatory at St. Helena.
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The Royal Institution of Great Britain.
The Royal Society, Edinburgh.
The Astronomical Institution, Edinburgh.
The President of the Royal Society.
The Lowndes's Professor of Astronomy, Cambridge.
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The Bowden College, United States.
The Waterville College, United States.

ROYAL MEDALS.

HIS MAJESTY KING WILLIAM THE FOURTH, in restoring the Foundation of the Royal Medals, graciously Commanded a Letter, of which the following is an extract, to be addressed to the Royal Society, through His Royal Highness the Duke of Sussex, K.G., President:

"Windsor Castle, March 25, 1833.

"It is His Majesty's wish,—

"First, That the Two Gold Medals, value of Fifty Guineas each, shall "henceforth be awarded on the day of the Anniversary Meeting of the "Royal Society, on each ensuing year, for the most important discoveries "in any one principal subject or branch of knowledge.

"Secondly, That the subject matter of inquiry shall be previously settled "and propounded by the Council of the Royal Society, three years pre- "ceding the day of such award.

"Thirdly, That Literary Men of all nations shall be invited to afford the "aid of their talents and research: and,

"Fourthly, That for the ensuing three successive years, the said Two "Medals shall be awarded to such important discoveries, or series of in- "vestigations, as shall be sufficiently established, or completed to the "satisfaction of the Council, within the last five years of the days of award, "for the years 1834 and 1835, including the present year, and for which "the Author shall not have previously received an honorary reward.

(Signed) "H. Taylor."

The Royal Medals for the year 1833 were awarded to

Sir JOHN FREDERICK WILLIAM HERSCHEL, K.H. F.R.S.,

for his Paper on the Investigation of the Orbits of Revolving Double Stars; and to

Professor AUGUSTE PYRAME DE CANDOLLE, of Geneva, Foreign Member of the Royal Society,

for his Discoveries and Investigations in Vegetable Physiology.

Those for 1834 were awarded to

JOHN WILLIAM LUBBOCK, Esq., V.P. & Treas. R.S.,

for his Papers on the Tides published in the Philosophical Transactions; and to

CHARLES LYELL, Esq., F.R.S.,

for his Work entitled "Principles of Geology."

Those for 1835 were awarded to

MICHAEL FARADAY, D.C.L., F.R.S.,

for his Investigations and Discoveries contained in the Series of Experimental Researches in Electricity, published in the Philosophical Transactions, and more particularly for the Seventh Series, relating to the definite nature of electro-chemical action; and to

Sir WILLIAM ROWAN HAMILTON, Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland,

for the Papers published by him in the 16th and 17th volumes of the Transactions of the Royal Irish Academy, entitled "Supplement to an Essay on the Theory of "Systems of Rays," and more particularly for those Investigations at the conclusion of the third and last Supplement, which relate to the discovery of Conical Refraction.

The Council propose to give one of the Royal Medals in the year 1836, to the most important unpublished paper in Astronomy, communicated to the Royal Society for insertion in their Transactions, after the present date (May 13th, 1833,) and prior to the month of June in the year 1836.

The Council also propose to give one of the Royal Medals in the year 1836 to the most important unpublished paper in Animal Physiology, communicated to the Royal Society for insertion in their Transactions, after the present date (May 13th, 1833,) and prior to the month of June in the year 1836.

The Council propose to give one of the Royal Medals in the year 1837 to the most important unpublished paper in Physics, communicated to the Royal Society for insertion in their Transactions, after the present date (November 27th, 1834,) and prior to the month of June in that year.

The Council also propose to give one of the Royal Medals in the year 1837 to the author of the best paper, to be entitled "Contributions towards a System of Geological Chronology founded on an examination of fossil remains, and their attendant "phenomena," such paper to be communicated to the Royal Society after the present date (December 1st, 1834,) and prior to the month of June 1837:—but in case no paper is presented to the Society fulfilling the conditions implied by the above Resolution, or possessing sufficient merit, the Council propose to give one of the Royal Medals in the year 1837 to the author of the best paper in Geology and Mineralogy, communicated to the Royal Society for insertion in their Transactions after the present date and prior to the month of June in that year.

The Council propose to give one of the Royal Medals in the year 1838 to the most important unpublished paper on Chemistry, communicated to the Royal Society for insertion in their Transactions, after the present date (November 12th, 1835,) and prior to the month of June 1838.

The Council also propose to give one of the Royal Medals in the year 1838 to the most important unpublished paper in Physics, communicated to the Royal Society for insertion in the Philosophical Transactions, after the present date (November 19, 1835,) and prior to the month of June 1838.

Those for 1836 were awarded to

Sir JOHN FREDERICK WILLIAM HERSCHEL, K.H. F.R.S.,

for his Papers on Nebulæ and Clusters of Stars, published in the Philosophical Transactions for 1833; and to

GEORGE NEWPORT, Esq.,

for his Series of Investigations on the Anatomy and Physiology of Insects, contained in his two Papers published in the Philosophical Transactions within the last three years.

The Council propose to give one of the Royal Medals in the year 1839 to the most important unpublished Paper in Astronomy, communicated for insertion in their Transactions after the present date, (November 30th, 1836,) and prior to the termination of the Session in June 1839.

The Council also propose to give one of the Royal Medals in the year 1839 to the most important unpublished Paper in Physiology, communicated for insertion in their Transactions after the present date, (November 30th, 1836,) and prior to the termination of the Session in June 1839.

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APPENDIX.

Meteorological Journal kept at the Apartments of the Royal Society, by order of the President and Council.

CORRIGENDA.

Page 350, line 9, *for* of, *read* in which, and *after* called, *insert* is employed.

Page 352, line 25, *for* it is excited, *read* the stimulus which excites it is applied.

Page 353, line 9 from bottom, *for* effects, *read* effect.

Page 355, in the beginning of the first note, *insert* Philosophical Transactions for 1822.

Page 358, line 1, *after* shocks, *insert* "a fact analogous to the division of the spinal marrow leaving unimpaired its influence on the secreting and assimilating organs."—*And line* 7, *for* 1829, *read* 1833.

Page 360, *line* 11 *from bottom, after* as well as, *insert* the maintenance of animal temperature and.—*And in the 5th line below this line, after* 1815, *insert* and 1831.

Page 361, *line* 10 from bottom of text, *after* muscular, *insert* and sensorial, and *for* power, *read* powers.

Page 362, line 5, *for* agents of that, *read* external.

Page 368, lines 11 and 16, *before* nerves, *insert* vital, *and after* or, *insert* the.—*And line* 14, *after* part, *insert* every part being supplied with nerves belonging to both systems.—*And in line* 27, *for* this class of nerves, *read* nerves of sensation.

Page 369, line 7, *after* system, *insert* and through the latter system over all parts of our frame.—*And line* 8, *after* latter, *insert* system.

Page 371, line 6, *after* system, *insert* independently of the means of obtaining nourishment.—*And line* 6 *from the bottom, for* system, *read* systems.

Page 376, line 7, *after* organs, *insert* under which circumstances medicine always operates as a permanent tonic.—*And in line* 24, *after* dominion, *insert* see Dr. ROGET's Treatise on Electricity.

PHILOSOPHICAL TRANSACTIONS.

I. *Researches on the Tides.—Fourth Series.* On the Empirical Laws of the Tides in the Port of Liverpool. By the Rev.* W. Whewell, *M.A. F.R.S.*

Received November 10,—Read November 19, 1835.

1. In the Philosophical Transactions for 1831 Mr. Lubbock published the results of a discussion of nineteen years of the London tide observations; and from the materials there given, I endeavoured to obtain the mathematical laws of the inequalities of the phenomena, in a memoir which was published in the Transactions for 1834. Mr. Lubbock having now, in Part II. of the Transactions for the present year, published the results of a similar discussion of nineteen years of the Liverpool tide observations, I intend in the present paper to use these results in testing and improving the formulæ to which I was led by the London observations.

Perhaps the precise object of such investigations as this may be best understood by comparing them with corresponding steps in the history of other parts of astronomy; as, for instance, in the progress of our knowledge respecting the Moon's motions. After Hipparchus had singled out and reduced to rule the great inequality of the Moon's motion, the *Equation of the Centre*, it was the employment of succeeding astronomers, as, for instance, Ptolemy and Tycho, to discover, by examination of long-continued observations, other smaller inequalities, and the laws which they follow; as the *Variation, Evection*, and others. In the same manner, the great inequality of the tides, the *Semimenstrual Inequality* of the time, is now well understood; and the agreement which Mr. Lubbock showed to exist between the London observations and the formulæ leaves nothing to desire. But formulæ for the observed effects of *lunar* and *solar parallax* and *declination* (although some such formulæ may have been

* For convenience of reference I shall take the liberty of thus numbering the papers in the Philosophical Transactions in which I have attempted to make out the Laws of the Tides. The preceding papers are,

First Series. Essay towards a First Approximation to a Map of Cotidal Lines.—1833, Part I.

Second Series. On the Empirical Laws of the Tides in the Port of London.—1834, Part I.

Third Series. On the Results of Tide Observations made in June 1834 at the Coast Guard Stations in Great Britain and Ireland.—1835, Part I.

employed by the calculators of Tide Tables) have never been published, as far as I am aware, except in the memoir already mentioned. It was therefore a matter of great interest to examine whether the formulæ obtained from the London tides are confirmed by those of Liverpool, and whether any further light is thrown upon the subject by this addition to our materials.

2. The results of this examination have been very satisfactory. The Liverpool observations have both confirmed, in general, my formulæ, and have given me the means of very much improving them. The corrections for lunar parallax and declination, which, as far as they depended on the former investigation, might be considered as in some measure doubtful, and probably only locally applicable, have been so fully verified as to their general form, that I do not conceive any doubt now remains on that subject; and the nature of the local differences in the constants of the formula has also in part come into view. This investigation shows, that notwithstanding the great irregularities to which the tides are subject, the results of the *means* of large masses of good observations agree with the formulæ with a precision not far below that of other astronomical phenomena; as, for example, a fraction of a minute in the times, and a fraction of an inch in the heights.

This precision is the more worthy notice, because the formulæ which we obtain point directly to a very simple general law of the tides; namely, that the tide at any place occurs in the same way as if the ocean imitated the form of equilibrium corresponding to a certain antecedent time. This Equilibrium-Theory (the constant quantities which it introduces being suitably modified,) expresses, with very remarkable exactness, most of the circumstances in my results: I will therefore, before stating them, explain it a little further.

3. The theoretical formula for the position of the pole of the equilibrium-spheroid is

$$\tan 2\,\theta' = -\frac{h \sin 2\,\varphi}{h' + h \cos 2\,\varphi},$$

where h and h' are the elevation of the spheroid due to the sun and the moon respectively, φ the angular distance of the moon from the sun, θ' the angular distance of the pole of the spheroid from the moon's place.

In the case of the tides, we may suppose the actual ocean-spheroid to follow the equilibrium-spheroid at an angular distance λ', the spheroid being that which corresponds to a distance of the sun and moon $\varphi - \alpha$, instead of φ. Thus we have

$$\tan 2\,(\theta' - \lambda') = -\frac{h \sin 2\,(\varphi - \alpha)}{h' + h \cos 2\,(\varphi - \alpha)}.$$

In the same manner the theoretical height of the pole of the equilibrium-spheroid above the mean surface is $\sqrt{\{h'^2 + h^2 + 2\,h\,h' \cos 2\,\varphi\}}$; and on the equilibrium-theory the height of the tide above the mean surface is $\sqrt{\{h'^2 + h^2 + 2\,h\,h' \cos 2\,(\varphi - \alpha)\}}$.

By assuming properly the values of h and h', α and λ', these expressions may be made to agree very closely with the mean results of observation. This was shown with respect to the expression for the time by Mr. Lubbock from the London observations. The Liverpool observations give a still closer agreement, assuming $\lambda' = 11^{h}\,6^{m}$, $\alpha = 1^{h}\,15^{m}$, $\frac{h}{h'} = \sin 89^{m} = \sin 22^{\circ}\,15'$.

The expression for the heights also agrees very nearly with observation, as I shall show, but for this purpose we must suppose $\alpha = 1^{h}$, $\frac{h}{h'} = \sin 23^{\circ}\,30'$.

The agreement in these cases is the more remarkable, on account of the want of symmetry in the functions which thus occur. The curve, the ordinate of which represents the time of high water (reckoned from the moon's transit), is not symmetrical with regard to its maximum ordinates. The curve, the ordinate of which represents the height of high water, is not symmetrical with regard to its mean line of abscissas. Yet in both these cases the theoretical and observed curve agree within a minute and an inch during their whole course. It is impossible to doubt, under these circumstances, that the theoretical formula truly represents the observed facts.

4. But this agreement belongs to the mean of all the observations; and we have further to seek for the alteration in the formula, which is requisite in order to represent the effect of changes in the parallax and declination of the sun and moon. In these respects also we find a near agreement of the theory and observation. By the equilibrium-theory, the height h' of the lunar tide ought to be proportional to the cube of the moon's parallax; it is exactly or nearly so: by the same theory the height h' ought to diminish when the moon's declination increases, and by a quantity proportional to the square of the sine of the declination. It is found to do so with great precision.

5. But the equilibrium-theory, since it does not point out the existence of the quantities λ' and α, does not indicate what changes these quantities may be expected to undergo, when the moon's force is altered by the effects of parallax and declination. We find that in that case, these quantities also are altered, and the resulting change in the phenomena may be conceived in the following manner.

If we suppose the moon to revolve about the earth by the diurnal motion, perpetually drawing the waters towards the position of equilibrium, we may conceive that the ocean would form a spheroid, the pole of which would revolve round the earth, following the moon at a certain distance of terrestrial longitude. For the sake of distinctness, let this distance be called the *Retroposition* of the theoretical tide *in longitude*. Its mean value is what I have termed in other communications the "*corrected establishment*" of a place in the open ocean.

If, from an original equilibrium-tide, a derivative tide were sent off, along any channel in which it is no further influenced by the forces of the moon and sun, it would take a certain time in reaching any place in that channel; and the circumstances of the tide at that place would not depend upon the positions and distances of the moon and sun at the time when the tide happens, but upon the positions and distances of those luminaries at a certain time, anterior to the time of the tide by the interval occupied in the transmission of the tide along the channel. Let this interval of time be called the *Retroposition* of the theoretical tide *in time.* It is what, on former occasions, I have called the "*age of the tide.*"

6. This phraseology being adopted, the phenomena of the Liverpool tides may be expressed as follows.

(1.) *The effects which the changes of the Moon's force produce upon the Tides, are the same as the effects which those changes would produce upon a Retroposited Equilibrium-tide.*

(2.) *The Retroposition of the Tide in longitude is affected by small changes, which changes are proportional to the variations in the moon's tidal force.*

(3.) *The Retroposition of the Tide in time is also affected by small changes, which changes depend on the variations of the moon's force.*

7. The former of these propositions is proved by the verification of the formulæ already mentioned, since these agree with the formulæ for equilibrium-tides, except in the circumstance of having $\varphi - \alpha$ for φ. Now this difference is equivalent to a retroposition of the tide in time, of such magnitude that, during this time, the distance of the sun and moon is changed from $\varphi - \alpha$ to φ. If α be $1^h\ 15^m$, as collected from the law of the times, the retroposition in time is the time requisite for the moon to increase its right ascension from the sun by $1^h\ 15^m$; that is, it is $\frac{75}{48}$ days nearly, or 1 day $13\frac{1}{2}$ hours. The tide at Liverpool agrees nearly with an equilibrium-tide produced in the southern ocean, $37\frac{1}{2}$ hours previously to the moon's transit at that port, and transmitted thither unchanged.

8. The second of the above propositions is proved by tracing the effect of changes of lunar parallax and declination upon the results compared with the above formula for the times and heights, namely,

$$\tan 2\,(\theta' - \lambda') = - \frac{h \sin 2\,(\varphi - \alpha)}{h' + h \cos 2\,(\varphi - \alpha)} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (a.)$$

$$y = \surd\ \{h'^2 + h^2 + 2\,h\,h' \cos 2\,(\varphi - \alpha)\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (b.)$$

By the equilibrium-theory, the change which would be produced by any alteration of the moon's force would correspond to the effect of an alteration in the value of h', the amount of the lunar tide. It appears from the examination of the observations, that this change takes place in fact, but that we must also suppose a change in λ' in order that the formula (*a.*) may represent the observed intervals of time. This change in λ', the retroposition of the tide in longitude, is

$2^m\!\cdot\!5\ (p - 57)$ for parallax p minutes. (See Art. 15.)
$84^m \sin^2 \delta$ for declination δ. (Art. 21.)

Now, by the theory, the effect of a change in the moon's parallax on the equilibrium-tide is as the change of parallax; and the effect of the moon's declination is a change proportional to the square of the declination. Therefore the second of the above three propositions is established.

According as the moon's parallax is less, and according as her declination is greater, the moon's tidal force is less, and h' in the above formulæ is less. Yet it is remarkable that these two circumstances affect the magnitude of the retroposition of the tide in opposite ways. In one case λ' is augmented, in the other case it is diminished. When the moon's force decreases, by her receding further from the earth, the tide follows the moon at a greater interval; the mean interval increasing from $10^h\,55^m$ to $11^h\,12^m$, while the parallax diminishes from $61'$ to $54'$. But when the moon moves away from the equator, which also diminishes her tidal force, the tide follows her more closely, the interval decreasing from $11^h\,12^m$ to $10^h\,55^m$, while the declination increases from $0°$ to $27°$.

The Liverpool tide happens about 11 hours after the next preceding transit; and as the retroposited tide happens about $37\frac{1}{2}$ hours before this transit, we must suppose the Liverpool tide to be produced at an interval of $48\frac{1}{2}$ hours preceding the time at which it is observed, in order to make it agree nearly with the equilibrium-theory; and we may suppose this time to be employed in the transmission of the tide along its channel. If we suppose the original tide to lag behind the position of equilibrium, we may suppose the amount by which it lags to vary with the changes of the moon's force, to the amount above stated as the variation of λ'. On this supposition we may suppose the time of transmission of the tide along its channel to be constant. Or we may suppose that the changes of the moon's force not only affect the lagging of the original tide behind the equilibrium position, but also affect the velocity of transmission to Liverpool. In either of these ways the circumstances of the tide may be hypothetically represented; but it will, of course, be understood that we use such hypotheses at present only for the sake of connecting and representing the facts.

9. The effect which changes in the moon's force produce upon the retroposition of the tide in time, that is, on the value of α in the formulæ (*a.*) and (*b.*), is more difficult to determine with any precision. It is, however, manifest from the general course of the quantities in the Tables, that α is greater as the moon's parallax is greater, and as her declination is greater. This is proved by each Table independently. Thus I have collected as the amount of this change,

$2^{m}\cdot 5\,(p - 57')$ from the effect of parallax on the times (Art. 18.),
$4^{m}\,(p - 57')$ from the effect of parallax on the heights (Art. 20.),
$75^{m}\sin^2\delta$ from the effect of declination on the times (Art. 23.);

the effect of declination on the heights offers no clear evidence of a change in α.

Since, in the change of parallax from $54'$ to $61'$, the value of α, as given by the times, changes from about $1^h\,8^m$ to $1^h\,24^m$, the retroposition of the tide in time varies from about 34 to 42 hours.

10. The circumstances of the Liverpool tides may be represented hypothetically

in the following way. Let it be supposed that the ocean-spheroid assumes a form agreeing with the equilibrium-spheroid at the moment, and that the pole of this spheroid follows the position of the pole of the equilibrium-spheroid at a certain mean interval, say 90°. Let it be supposed that at a certain time a tide is sent off from this ocean-spheroid along a channel in which it is no longer affected by the moon or sun, and thus reaches Liverpool, producing the tide there. The following assumptions will then represent the facts.

When the horizontal parallax is 54′, the tide is sent off along the channel in longitude $48\frac{1}{2}°$ east of Liverpool, at $45^h\ 6^m$ before the time of Liverpool high water, and the pole of the ocean-spheroid follows the position of equilibrium at a distance 90° 24′.

When the horizontal parallax is 57′, the tide is sent off along the Liverpool channel in longitude $94\frac{1}{2}°$ east, at $48^h\ 36^m$ before the Liverpool high water, and the actual spheroid is 90° behind the position of equilibrium.

When the horizontal parallax is 61′, the tide is sent off along the channel in longitude $159\frac{1}{2}°$ east, at $53^h\ 0^m$ before the occurrence of high water, and the actual spheroid is 89° 16′ behind the position of equilibrium.

This hypothesis thus modified represents the circumstances of the Liverpool tide as affected by lunar parallax. The effect of lunar declination might be represented in a similar manner.

It is not to be imagined that this hypothetical representation is near to the true state of the case. The changes in the lagging, in the length of the channel of transmission, and in the velocity of transmission, are not such as the forces can be supposed likely to produce. Nor is it likely that the original tide is exactly what it would be if the condition of equilibrium were fully attained. The tide-spheroid not only lags behind the position of equilibrium, but deviates from the form of equilibrium; and other differences, besides the retroposition in longitude and in time, are introduced by the waters being in motion instead of at rest. This is seen in our results; for the tidal force of the moon, which, in the equilibrium-spheroid, varies as the cube of the parallax, appears in the observations to vary more nearly as the square of the parallax: and though this difference may be referred to the inaccuracy of the observations, it may, I think with more probability, be considered as resulting from the condition of the waters being a condition of motion, not of equilibrium. The temporary variations of the force do not affect the form of the waters in the same proportion as the mean force, which is constantly dragging the waters after it, round the earth.

11. In what has been hitherto stated with regard to the hypothetical representation of the tides, we have had a reference solely to Liverpool. It cannot, however, be doubted that the general laws of the tides at other places would resemble those of that port, and therefore might be represented in a similar manner. It has already been shown in my former memoir, though less satisfactorily and precisely than in this, that the tides of London follow the same rules as those now described.

The numbers, however, which enter into these formulæ will not necessarily be the same at two places; and since the empirical formulæ have not been determined for any places except those of London and Liverpool, we have not the means of discovering the relation of the constants at various places. The following comparison of the data of observation at London and at Liverpool is instructive as far as it goes.

The greatest difference arising from the mean semimenstrual inequality is the same at the two ports, being 88^m at both. This coincidence is striking; yet I am disposed to believe it accidental, although, according to theory, this quantity ought to be the same at all places, since it depends only upon the mean ratio of the solar and lunar tidal forces; for the semimenstrual inequalities at different places differ so much by other observations, (varying from 79^m at Brest to 96^m at Plymouth,) that I do not conceive the difference can arise from the incompleteness of the observations.

The effects of the parallax and declination at London were given by approximate formulæ, less exact than those which we have now obtained for Liverpool; but, comparing the London formulæ with corresponding approximations at Liverpool, we have the following results.

If Λ' represent the value of λ' for the mean parallax $57'$ and the declination $0°$, we have for the parallax p, and declination δ,

at London $\lambda' = \Lambda' - 3^m (p - 57) - 132^m \sin^2 \delta$,

at Liverpool $\lambda' = \Lambda' - 2\frac{1}{2}^m (p - 57) - 84^m \sin^2 \delta$, by Art. 15 and 21.

Also the maximum semimenstrual inequality,

at London $= 40^m + 3^m (p - 57) + 84^m \sin^2 \delta$,

at Liverpool $= 41^m + 2^m (p - 57) + 30^m \sin^2 \delta$, by Art. 17 and 23.

Also if H' be the value of h' for the mean parallax and the declination $0°$, we have,

at London $h' = H' + 0^{ft}\cdot 17 (p - 57) - 3^{ft} \sin^2 \delta$,

at Liverpool $h' = H' + 1^{ft}\cdot 47 (p - 57) - 6^{ft} \sin^2 \delta$, by Art. 19 and 24.

And at London $h = 1^{ft}\cdot 7$.

at Liverpool $h = 2^{ft}\cdot 8$.

12. The resemblances of the formulæ at the two places are remarkable, but the differences are still more so. The differences in the heights of the tide at different places are indeed what we know to prevail universally, and to depend upon local circumstances in an intelligible manner: but the differences in time are more difficult to explain, since both the tides come from the same origin. The difference in the effect of parallax may indeed be due to the inaccuracy of the data, but it is scarcely possible that this can be true of the difference in the effect of declination, which appears to be in the ratio of 132 to 84 for the non-periodical, and 84 to 30 for the periodical, part. Similar discussions of observations at other places will best throw light on this difficulty.

I now proceed to state the method by which the above empirical formulæ have been obtained.

The Semimenstrual Inequality.

13. In order to obtain the semimenstrual inequality of the *times* of high water, I take Mr. LUBBOCK's Table VII., and from each column of intervals (of tide and moon's transit) I subtract the mean of that column; and I thus obtain Table VII. (a), which exhibits the semimenstrual inequalities for each minute of parallax. I then take the means of the horizontal lines in this, interpolating in H. P. 60′ and omitting H. P. 61′. The resulting intervals are those of the mean tide.

TABLE VII. (a.)

Mean of each column subtracted from the column "*Interval*" of times.

H. P.......	54′.	55′.	56′.	57′.	58′.	59′.	60′.	61′.	Mean.
Mean Interval	h m 11 12·7	h m 11 11·5	h m 11 8·3	h m 11 6·5	h m 11 3·7	h m 11 0·3	h m 10 58·5	h m 10 54·5	h m 11 6
☽'s Transit.	Remainder.	Remainder.	Remainder.	Remainder.	Remainder.	Remainder.	Remainder.	Remainder.	Remainder.
h m									
0 30	+13·7	+11·6	+ 8·0	+11·6	+12·2	+12·9	+13·3	+11·8	+12·2
1 30	− 5·2	− 6·8	− 6·5	− 3·9	− 5·3	− 2·8	− 2·5	− 2·8	− 4·6
2 30	−23·2	−22·0	−22·0	−20·8	−18·6	−17·5	−16·6	−17·1	−20·0
3 30	−41·4	−36·7	−34·7	−33·0	−32·1	−29·3	−30·4	..	−33·9
4 30	−49·0	−47·8	−44·0	−41·8	−40·4	−38·4	−38·7	..	−42·8
5 30	−47·7	−45·7	−43·6	−43·2	−39·4	−38·5	−37·5	..	−43·2
6 30	−27·2	−26·4	−24·5	−25·7	−25·6	−24·8	−21·5	..	−25·0
7 30	+14·3	+13·4	+11·9	+ 9·2	+ 6·5	+ 1·6	+11	..	+ 9·6
8 30	+44·8	+41·8	+40·1	+37·4	+34·1	+31·1	+20·2	..	+36·6
9 30	+50·9	+49·3	+47·9	+45·0	+44·1	+41·6	+39·8	+39	+45·6
10 30	+42·5	+41·8	+41·3	+40·1	+38·6	+38·0	+36·1	+35·8	+39·8
11 30	+28·5	+28·1	+26·6	+25·7	+23·6	+25·7	+24·4	+25·4	+26·1
Max. Diff.	99·9	95·1	91·9	88·2	84·5	80·1	78·5		88·8

On comparing the mean numbers in the last column with the theoretical formula

$$\tan 2(\theta' - \lambda') = -\frac{c \sin 2(\varphi - \alpha)}{1 + c \cos 2(\varphi - \alpha)},$$

it appears that they may be very accurately represented by making $\lambda' = 11^h\ 6^m$, $\alpha = 1^h\ 15^m$, $c = \sin 1^h\ 29^m$. The agreement of this formula with observation is as follows:

Moon's Transit.	Formula.	Obs.	Diff.
h m	m s	m s	m s
0 30	+12 16	+12 12	− 0 4
1 30	− 4 7	− 4 36	− 0 29
2 30	−20 6	−20 0	+ 0 6
3 30	−34 0	−33 54	+ 0 6
4 30	−43 6	−42 48	+ 0 18
5 30	−42 40	−43 12	− 0 32
6 30	−25 8	−25 0	+ 0 8
7 30	+ 9 2	+ 9 6	+ 0 4
8 30	+36 28	+36 36	+ 0 8
9 30	+44 30	45 36	+ 1 6
10 30	+39 40	39 48	+ 0 8
11 30	+27 36	26 6	− 1 30

This accordance is complete, the difference amounting in only two cases to 1^m.

14. The semimenstrual inequality of the *heights* for each minute of horizontal parallax, and the mean semimenstrual inequality of the heights, are in like manner obtained by subtracting from each column of heights in Mr. Lubbock's Table VII. the mean of that column, and taking the means of the horizontal lines as is done in Table VII. (a.)

Table VII. (b.)

Mean of each column subtracted from column "*Height* of Tide."

H. P.......	54′.	55′.	56′.	57′.	58′.	59′.	60′.	61′.	Mean.
Mean height	14·20	14·41	14·84	15·22	15·63	16·02	16·43	16·66	
☽ 's Transit.	Remainder.	Remainder.	Remainder.	Remainder.	Remainder.	Remainder.	Remainder.	Remainder.	Remainder.
h m									
30	+2·38	+2·47	+2·45	+2·31	+2·33	+2·35	+2·19	+2·51	+2·35
1 30	+2·26	+2·23	+2·25	+2·41	+2·49	+2·43	+2·66	+2·73	+2·39
2 30	+1·62	+1·61	+1·83	+1·82	+1·91	+2·02	+2·32	+2·21	+1·88
3 30	+0·49	+0·70	+0·74	+0·95	+0·95	+1·17	+1·27		+ ·90
4 30	−0·61	−0·55	−0·64	−0·33	−0·27	−0·15	−0·10		− ·38
5 30	−1·92	−1·92	−1·93	−1·83	−1·62	−1·53	−1·57		−1·76
6 30	−3·13	−3·05	−3·14	−2·69	−2·79	−2·73	−2·85		−2·91
7 30	−2·81	−2·96	−2·74	−2·89	−2·97	−3·06	−3·14		−2·94
8 30	−1·52	−1·62	−1·69	−1·86	−1·86	−2·09	−2·33		−1·85
9 30	−0·06	−0·08	−0·18	−0·43	−0·54	−0·60	−0·75	−0·60	− ·38
10 30	+1·18	+1·16	+1·13	+0·82	+0·80	+0·60	+1·61	−0·65	+1·04
11 30	+2·10	+1·89	+1·95	+1·78	+1·63	+1·64	+1·65	+1·78	+1·81
Max. diff.	5·51	5·52	5·59	5·30	5·46	5·49	5·80	......	5·33
Mean ..	5·44								
Half ...	2·74								

The theoretical height of the high water above the mean surface of the ocean is $\surd\,\{h'^2 + h^2 + 2\,h\,h' \cos 2\,(\varphi - \alpha)\}$; and therefore if k be the mean of all the high-water heights, we shall have for the semimenstrual inequality of height the expression

$$\surd\,\{h'^2 + h^2 + 2\,h\,h' \cos 2\,(\varphi - \alpha)\} - k.$$

This will agree very nearly with the result of observation if we make

$$h = 2{\cdot}74,\ h' = 6{\cdot}872,\ k = 7{\cdot}19,\ \alpha = 1^{\text{h}}.$$

The accordance is as follows:

Moon's Transit.	Formula.	Obs.
h m	f.	
0 30	2·35	2·35
1 30	2·35	2·39
2 30	1·83	1·88
3 30	0·84	0·90
4 30	−0·48	−0·38
5 30	−1·89	−1·76
6 30	−2·90	−2·91
7 30	−2·90	−2·94
8 30	−1·89	−1·85
9 30	−0·48	−0·38
10 30	0·84	1·04
11 30	1·83	1·81

The greatest deviation is about an inch, the mean a small fraction of an inch.

Effect of the Moon's Parallax.

15. In Mr. LUBBOCK's Table VII., which contains the effect of lunar parallax, and has a column for each minute of parallax, we have, in Art. 13, taken the mean of each column, and subtracted it from every number in the column. In this way, it is evident that the *mean* contains the *non-periodical* part of the effect, and the *remainder* contains the part which goes through its *period* in a semi-lunation.

The *non-periodical* part of the *interval* stands in the uppermost line of Table VII. (a) Art. 13.; and its variations are manifestly nearly or exactly proportional to the variations of the parallax. If we take $57'$ as the mean parallax, we may express these means very nearly by the formula

$$11^h\ 6^m{\cdot}5 - 2{\cdot}5\ (p - 57'),$$

p being the H. P. The agreement of the formula with observation is as follows, and is a near approximation.

H. P.	54′.	55′.	56′.	57′.	58′.	59′.	60′.	61′.
	h m	h m	h m	h m	h m	h m	h m	h m
Obs.	11 12·7	11 11·5	11 8·3	11 6·5	11 3·7	11 0·3	10 5·85	10 54·5
Formula....	11 13	11 11·5	11 9	11 6·5	11 4	11 1·5	10 5·9	10 56·5

The column for H. P. 60′ is completed by interpolation, and the column for H. P. 61′ is omitted. The latter is defective in half the hours of moon's transit, which arises from the effect of the Moon's Variation on the parallax. The parallax has a term depending on the sine of twice the distance of the moon from the sun, and cannot be so great as 61′ except near syzygy. The "observed" mean for 61′ is that which makes the numbers in that column follow nearly the same law as the rest.

The *periodical* part of the effect of lunar parallax is shown in the lower part of Table VII. (a). It appears there that the *intervals* for all the values of H. P. follow nearly the same law as the mean interval already considered, but with a difference in the maximum value of the inequality. If we add together the greatest positive and negative numbers in each column, we obtain the double of the maximum inequality nearly, but not exactly, since the maximum does not correspond exactly to times of moon's transit contained in the Table. Making a slight addition on this account, we have,

H. P............	54′.	55′.	56′.	57′.	58′.	59′.	60′.	61′.
	m	m	m	m	m	m	m	
Sum	99·9	95·1	91·9	88·2	84·5	80·1	78·5	
Double Max.	101	96·1	92·8	89	85·2	80·7	79	
Formula....	101	97	93	89	85	81	77	

Now in the expression

$$\tan 2\,(\theta' - \lambda') = -\frac{c \sin 2\,(\varphi - \alpha)}{1 + c \cos 2\,(\varphi - \alpha)}$$

the maximum value occurs when $\cos 2\,(\varphi - \alpha) = -c$, and is equal to $\frac{c}{\sqrt{1 - c^2}}$. If we make $c = \sin\gamma$, we have for the maximum $\tan 2(\theta' - \lambda') = \tan\gamma$; and therefore maximum $2(\theta' - \lambda') = \gamma$. Hence we have γ for each H. P. by reducing the above double maxima to arcs, and thence we have c, by finding the sines of these arcs.

16. By the equilibrium-theory, c should be inversely as the cube of the H. P.; therefore, if C be the value of c for H. P. 57′, we have $\frac{c}{C} = \frac{57^3}{p^3}$; $\log c + 3\log p = \log C + 3\log 57$; and therefore this quantity, $\log c + 3\log p$, should be constant. It is found that we get a quantity much more nearly constant by taking $\log c + 2{\cdot}2 \log p$. The following is the result:

H. P.	γ.	$\log c$ ($c = \sin\gamma$).	$\log p$.	$\frac{22}{10}\log p$.	$\log c p^{2\cdot 2}$.
54′.	25° 15′	9·62999	1·73239	346478 3·46478	3·44125
55.	24 15	9·61354	1·74036	348072 3·48072	3·44233
56.	23 12	9·59543	1·74819	349638 3·49638	3·44145
57.	22 15	9·57824	1·75587	351174 3·51174	3·44115
58.	21 18	9·56021	1·76343	352686 3·52686	3·43976
59.	20 10	9·53751	1·77085	354170 3·54170	3·43338
60.	19 45	9·52881	1·77815	355630 3·55630	3·44074
61.					

Hence, $c = C\left(\frac{57}{p}\right)^{2\cdot 2}$ nearly.

17. We may, however, express the result more conveniently for some purposes by expanding this expression; for the variation of the maximum will be very nearly as the variation of the parallax; and the double maximum may be nearly expressed by the following formula:

$$89^{\mathrm{m}} - 4^{\mathrm{m}}(p - 57).$$

The accordance is shown in the lowest line of the second Table in Art. 15.

18. By comparing, in Table VII. (a), the inequalities for moon's transit $0^{\mathrm{h}}\,30^{\mathrm{m}}$, $1^{\mathrm{h}}\,30^{\mathrm{m}}$, and for $6^{\mathrm{h}}\,30^{\mathrm{m}}$ and $7^{\mathrm{h}}\,30^{\mathrm{m}}$, it is clear that they are equal to 0 at a later hour for the larger than for the smaller parallaxes, which also appears by the maxima. Hence α is larger for large parallaxes than for small ones. The exactness of the observations hardly allows us to determine its variation exactly. It appears, however, that it may be sufficiently well represented by $\alpha = 1^{\mathrm{h}}\,15^{\mathrm{m}} + 2^{\mathrm{m}}{\cdot}5\,(p - 57)$.

19. The effect of the lunar parallax on the *heights* will be found from Table VII. (b.) in the same way as the effect on the times, by taking the *mean* of each column as the *non-periodical*, and the *remainder* as the *periodical*, part of the inequality. The origin of the measurements is arbitrary, the low water not being given. The *non-periodical* part is represented with great accuracy (except for the extreme parallaxes) by the formula $15{\cdot}22 + {\cdot}4\,(p - 57)$. The accordance is as follows:

H. P. ...	54′.	55′.	56′.	57′.	58′.	59′.	60′.	61′.
Obs. ..	14·20	14·41	14·84	15·22	15·63	16·02	16·43	16·66
Formula	14·02	14·42	14·82	15·22	15·62	16·02	16·42	16·82

We cannot eompare this effect of parallax on the heights with the whole height of the tide, or with theory, from not having any observations of low water for this series of tides.

The periodieal part of the heights, as appears by the remainder of Table VII. (b.), whatever be the parallax, follows nearly the law of the mean, whieh has already been explained; and the magnitude of the maximum differenees does not appear to be steadily different for different H. P. In faet, theory would lead us to expeet it to be the same in all these eases, beeause the amount of this inequality is $2h$, double the mean solar tide.

20. But it appears from the Table VII. (b.) that the time of moon's transit, when the periodical inequality vanishes, is later for the larger parallaxes, and the maxima indieate the same ehange: the amount of the ehange is about $4^{m}(p-57)$, at the mean between the greatest and least values of the height.

When $\phi = \frac{\pi}{4} + \alpha$, the formula for the inequality beeomes $h' - h$, which is the mean between the greatest and least values. In this ease $\alpha = 1^{h}$. Henee the value of α is $60^{m} + 4^{m}(p-57)$.

Effect of the Moon's Declination.

21. The effeet of the ehanges of lunar deelination upon the tide will be found in nearly the same way as the effeet of ehanges in the parallax. Mr. Lubbock's Table XII. gives the *intervals* for eaeh 3° of deelination. By finding the mean of eaeh column, and subtraeting it from the eolumn, we obtain the *non-periodical* and the *periodical* part respeetively of the inequality as is done in Table XII. (a.)

Table XII. (a.)

[*Intervals* of times.] Mean of eaeh eolumn subtraeted from the eolumn.

Decl.	0°.	3°.	6°.	9°.	12°.	15°.	18°.	21°.	24°.	27°.
Mean Interval	h m 11 12·1	h m 11 11·0	h m 11 11·3	h m 11 10·4	h m 11 8·4	h m 11 6·5	h m 11 3·6	h m 11 1·2	h m 10 59·0	h m 10 55·6
☽'s Transit.	Remainder.	Remainder.	Remainder.	Remainder.	Remainder.	Remainder.	Remainder.	Remainder.	Remainder.	Remainder.
h m										
0 30	+10·0	+ 8·4	+ 8·8	+ 9·1	+ 9·5	+10·8	+12·1	+12·3	+13·2	+13·6
1 30	− 7·1	− 6·3	− 5·6	− 6·1	− 5·5	− 5·3	− 4·6	− 3·2	− 3·8	− 3·3
2 30	−20·7	−20·4	−20·5	−21·1	−20·4	−20·5	−20·0	−20·8	−20·8	−20·6
3 30	−33·0	−31·9	−32·7	−31·5	−33·0	−33·7	−30·4	−33·0	−34·4	−33·7
4 30	−40·5	−41·2	−40·9	−40·3	−41·1	−42·2	−42·0	−43·3	−44·6	−45·7
5 30	−37·6	−36·0	−37·2	−38·3	−40·2	−40·4	−43·4	−43·5	−45·7	−46·6
6 30	−17·4	−17·2	−19·6	−20·7	−22·2	−23·1	−25·3	−26·9	−28·4	−31·7
7 30	+11·3	+14·0	+11·5	+12·0	+10·0	+11·4	+ 8·6	+ 3·4	+ 9·3	+ 4·3
8 30	+35·0	+33·0	+37·1	+34·3	+37·2	+36·4	+36·5	+39·7	+37·3	+40·9
9 30	+41·7	+41·2	+44·7	+43·1	+42·2	+43·6	+44·1	+46·0	+46·6	+49·9
10 30	+36·0	+31·8	+34·7	+36·0	+38·5	+37·9	+38·5	+40·9	+42·1	+43·6
11 30	+22·3	+24·6	+21·9	+23·9	+25·6	+25·6	+26·6	+28·2	+28·9	+30·0
Greatest Diff.	82·2	82·4	83·6	83·4	83·3	85·8	87·5	89·5	92·3	96·5
Excess above 82	0·2	0·4	1·6	1·4	1·3	3·8	5·5	7·5	10·3	14·5

The first line of that Table contains the *non-periodical* part. In order to find its law, subtract each mean from $11^h\ 12^m$ corresponding to decl. 0. We obtain a series of numbers which increase faster than the declination; and it is found that they may be nearly represented by the expression $84 \sin^2 \delta$, δ being the declination. The agreement is as follows:

Declination......	0°.	3°.	6°.	9°.	12°.	15°.	18°.	21°.	24°.	27°.
Obs. Diff.....	0	1·0	0·7	1·7	3·6	5·5	8·0	10·8	13·0	16·4
Formula	0	0·2	0·8	2·0	3·6	5·6	8·0	10·8	13·9	17·3

Hence the *non-periodical* part is $11^h\ 12^m - 84^m \sin^2 \delta$.

The *remainder* of the Table XII. (a.) exhibits the periodical part of the inequality; and it will be seen that each column follows nearly the law of the mean semimenstrual inequality as already obtained. In order to obtain the law of the coefficients, I take, as before, the sum of the two maximum values. This sum converted into arc gives γ, and $c = \sin \gamma$.

Decl.	γ.	$\operatorname{cosec} \gamma = \frac{1}{c}$.	Excess of Decl. 0°.	Log. Excess.	Log. $\sin^2 \delta$.	Difference.
0°	20° 33′	2·8488028				
3	20 36	2·8421877				
6	20 54	2·8031777				
9	20 51	2·8091995	·0396033	$\bar{2}$·59769	18·38866	·20903
12	20 50	2·8117471	·0370557	$\bar{2}$·56878	18·63576	$\bar{1}$·93302
15	21 27	2·7345630	·1142398	$\bar{1}$·05778	18·82600	·23178
18	21 52	2·6849391	·1638637	$\bar{1}$·21447	18·97996	·23451
21	22 23	2·6260406	·2227622	$\bar{1}$·34783	19·00866	·33917
24	23 5	2·5505680	·2982348	$\bar{1}$·47455	19·21862	·25593
27	24 8	2·4458163	·4029865	$\bar{1}$·60528	19·31410	·29118

For the smaller declinations, the differences are too small to be depended on. The numbers corresponding to the resulting logarithms from 15° to 27° are from 1·7 to 2·1. If we take the mean 1·85 as the number, we have for $\frac{1}{c}$ the value $2{\cdot}85 - 1{\cdot}85 \sin^2 \delta$, which is sufficiently near.

22. By the theory of equilibrium $\frac{1}{c} = \frac{h'}{h}$. And by the same theory, if H′ be the height of the lunar tide at the equator when the declination is 0, we shall have in latitude l, when the declination is δ, two tides, of which the heights are $H' \cos^2 (l + \delta)$ and $H' \cos^2 (l - \delta)$. Now as we have not distinguished these two tides, our result will be the mean of them. Therefore,

$$\begin{aligned} h' &= \tfrac{1}{2} H' \{\cos^2 (l + \delta) + \cos^2 (l - \delta)\} \\ &= H' \{\cos^2 l \cos^2 \delta + \sin^2 l \sin^2 \delta\} \\ &= H' \{\cos^2 l - (\cos^2 l - \sin^2 l) \sin^2 \delta\} \\ &= H' \cos^2 l \{1 - (1 - \tan^2 l) \sin^2 \delta\}. \end{aligned}$$

But we have found that h' varies as

$$2{\cdot}85 - 1{\cdot}85 \sin^2 \delta.$$

$$\text{Hence } 1 - \tan^2 l = \frac{1{\cdot}85}{2{\cdot}85} = \frac{37}{57}; \ \tan^2 l = \frac{20}{57}$$

$$l = 30^\circ\ 38'.$$

Hence it appears that the amount of the effect of declination on the time H. W. at Liverpool corresponds to the effect on an equilibrium-spheroid at a place in latitude 30°.

23. It will be convenient to express the double maximum of the semimenstrual inequality by a formula. The formula $60^{m} \sin^2 \delta$ gives the following accordance with the excess above 82^{m}:

Decl.	0°.	3°.	6°.	9°.	12°.	15°.	18°.	21°.	24°.	27°.
Obs. ...	0·2	0·4	1·6	1·4	1·3	3·8	5·5	7·5	10·3	14·5
Formula	0	0·2	0·3	1·4	2·6	4·0	6·0	7·7	9·9	12·4

Hence $82^{m} + 60^{m} \sin^2 \delta$ nearly expresses the double maximum.

It appears also that the times when the inequality is 0 and max. are later as the declination is greater, the difference being about 15^{m} from decl. 0° to decl. 27°, and apparently increasing as the square of the decl. or of sin decl. Now $\sin^2 27 = \frac{1}{5}$ nearly: hence the correction of α is nearly $75^{m} \sin^2 \delta$, and we have $\alpha = 65^{m} + 75^{m} \sin^2 \delta$.

24. The effect of lunar declination on the *heights* will be found in the same manner from Mr. LUBBOCK's Table XVI. The mean of each column is obtained in the first line of the following Table.

TABLE XVI. (b.)

[Heights.] Mean of each column subtracted from the column.

Decl.	0°.	3°.	6°.	9°.	12°.	15°.	18°.	21°.	24°.	27°.
Mean Height ...	15·74	15·78	15·77	15·72	15·54	15·41	15·23	14·92	14·74	14·39
☽'s Transit. h m	Remainder.	Remainder.	Remainder.	Remainder.	Remainder.	Remainder.	Remainder.	Remainder.	Remainder.	Remainder.
0 30	+2·59	+2·64	+2·30	+2·52	+2·65	+2·58	+2·27	+2·53	+2·18	+2·38
1 30	+2·53	+2·52	+2·73	+2·57	+2·61	+2·51	+2·46	+2·32	+2·34	+2·40
2 30	+1·75	+1·97	+1·95	+1·82	+1·99	+1·81	+2·03	+1·86	+1·99	+2·10
3 30	+1·01	+0·69	−0·77	+0·88	+0·80	+0·84	+1·10	+1·03	+1·21	+1·21
4 30	−0·70	−0·48	−0·53	−0·74	−0·54	−0·52	−0·70	−0·27	−0·22	−0·22
5 30	−1·62	−2·10	−1·85	−1·96	−1·83	−1·81	−2·01	−1·86	−1·75	−1·61
6 30	−2·66	−2·74	−3·00	−2·86	−3·08	−2·96	−2·88	−3·12	−3·05	−3·24
7 30	−2·83	−2·66	−2·70	−3·10	−2·95	−3·02	−2·91	−3·01	−3·10	−3·20
8 30	−1·61	−2·28	−2·11	−1·72	−1·89	−1·99	−1·90	−1·95	−1·94	−1·88
9 30	−0·60	−0·37	−0·42	−0·32	−0·53	−0·41	−0·30	−0·40	−0·21	−0·39
10 30	+1·12	+0·92	+1·03	+0·84	+0·95	+1·12	+0·99	+1·01	+0·80	+0·85
11 30	+1·09	+1·93	+1·84	+2·14	+1·87	+1·81	+1·90	+1·87	+1·78	+1·60
Greatest Diff.	5·42	5·38	5·73	5·67	5·73	5·60	5·37	5·65	5·44	5·64
Mean	5·63									

If we subtract these mean heights from 15·8, the remainders are very nearly as $\sin^2 \delta$. The formula $6 \sin^2 \delta$ gives the following accordance:

Decl.	0°.	3°.	6°.	9°.	12°.	15°.	18°.	21°.	24°.	27°.
Obs.	·06	·02	·03	·08	·26	·39	·57	·88	1·06	1·41
Formula..	·00	·02	·08	·14	·26	·40	·60	·77	·99	1·24

Hence $15·8 - 6 \sin^2 \delta$ nearly is the non-periodical part of the Table.

The *periodical* part, as appears by the remainder of Table XVI. (b.), follows nearly the law of the mean height already explained. The sum of the maximum inequalities is not definitely different for the different declinations, which agrees with the theory, according to which it is constant and equal to $2h$.

Also by comparing the columns for decl. 0° and 27°, it appears that the interval between the times when the inequality is 0, is less for the greater decl., which also agrees with the theory, for in that case the fraction $\frac{h}{h'}$ is greater, and the defect of symmetry in the curve increases with this fraction.

There is no clear evidence of a variation of α in this Table.

Trinity College, Cambridge,
November 12, 1835.

II. *Researches towards establishing a Theory of the Dispersion of Light. No. II. By the Rev.* Baden Powell, *M.A. F.R.S. Savilian Professor of Geometry in the University of Oxford.*

Received Nov. 5,—Read December 17, 1835.

In my paper inserted in the last part of the Philosophical Transactions, I have commenced a comparison between the results of M. Cauchy's system of undulations, expressing the theoretical refractive index for each of the standard rays of the spectrum, and the corresponding index found from observation in different media. This comparison is there carried on for all the results obtained by M. Fraunhofer. But these include only a limited range of transparent bodies; and close as is the accordance in these instances, the theory cannot be considered as fully verified, until we shall have extended a similar examination to a greater number of media, and especially to those of higher dispersive power. In this research I am now engaged: but as it will necessarily occupy a considerable period to carry it on, from time to time, as data are furnished, I venture for the present to submit to the Royal Society the following portion of my calculations in continuation of the preceding.

In my former communication I had referred to M. Fraunhofer's results as affording the *only* precise data which observation had as yet furnished. But through the kindness of Prof. Miller, of Cambridge, I have since become acquainted with the series of results obtained by M. Rudberg. They are given in Poggendorff's Annalen, band xiv. and xvii., and comprise the indices observed by him for the standard rays, or the ratios of the velocities in air to the velocities within the crystal, in a direction perpendicular to the axis of the rhombohedron, in a prism of calcareous spar, having its edge parallel to that axis; and in a prism of quartz similarly cut; in either case, both for the ordinary and extraordinary ray: also the ratios of the velocities in the direction of the three axes of elasticity respectively, in aragonite and topaz.

This valuable series of data I have now examined: and the comparison of them with theory constitutes the present communication. The calculations are made by precisely the same method as those described in my former paper; and the results are here stated in exactly the same tabular form, which will consequently need no explanation. The coincidences of observation and theory will be found at least as close as those already obtained from M. Fraunhofer's results, and I think will be allowed to afford a satisfactory extension of the theory to the cases here discussed.

Thus the hypothesis of undulations assigns the law and cause of dispersion in ten new cases, in addition to the ten considered in my former paper.

Oxford, November 1, 1835.

Postscript.

It may be right here to mention, that since my former paper was printed, I have learned from M. Cauchy that he has also investigated the relation between the length of a wave and the refractive index. And in a memoir on his new method of interpolation he has applied that method to this case, and has also given an example of the comparison of numerical values. This, however, is only made for one single case, viz. the Flint Glass, No. 23. of Fraunhofer.

Also, while this paper has been passing through the press, some other important observations closely connected with the subject have been made, for which the reader must refer to the London and Edinburgh Philosophical Magazine and Journal of Science, Nos. 44 and 45.

Comparison of Refractive Indices from Cauchy's *Theory and from observation.*

Calcareous Spar. Rudberg.

The edge of the prism parallel to the axis of the rhombohedron.

Ordinary Ray.

Ray.	Observed value of μ.	$\left(\frac{\theta}{\lambda}\right)$	Ratio $\left(\frac{\text{arc}}{\text{sine}}\right)$.	Calculated value of μ $=\text{const}\times\left(\frac{\text{arc}}{\text{sine}}\right)$.
		° ′ ″		
B	1·6531	13 16 0	1·009	1·6531
C	1·6545	13 55 2	1·010	1·6547
D	1·6585	15 29 59	1·0123	1·6584
E	1·6636	17 19 45	1·0156	1·6638
F	1·6680	18 47 30	1·0181	1·6680
G	1·6762	21 14 30	1·0233	1·6765
H	1·6833	23 1 30	1·0277	1·6834
			const=1·6384	

Extraordinary Ray.

Ray.	Observed value of μ.	$\left(\frac{\theta}{\lambda}\right)$	Ratio $\left(\frac{\text{arc}}{\text{sine}}\right)$.	Calculated value of μ $=\text{const}\times\left(\frac{\text{arc}}{\text{sine}}\right)$.
		° ′ ″		
B	1·4839	9 30 0	1·0045	1·4838
C	1·4845	9 57 59	1·0051	1·4847
D	1·4863	11 5 58	1·0063	1·4864
E	1·4887	12 24 38	1·0080	1·4889
F	1·4907	13 17 20	1·0092	1·4908
G	1·4945	15 12 30	1·0119	1·4948
H	1·4978	16 29 15	1·0140	1·4978
			const=1·4772	

Quartz. RUDBERG.

The edge of the prism parallel to the axis of the Rhombohedron. Extraordinary Ray.

Ray.	Observed value of μ.	$\left(\frac{\theta}{\lambda}\right)$.	Ratio $\left(\frac{\text{arc}}{\text{sine}}\right)$.	Calculated value of μ $=\text{const.}\times\left(\frac{\text{arc}}{\text{sine}}\right)$.
		° ′ ″		
B	1·5499	10 33 0	1·0056	1·5497
C	1·5508	11 4 0	1·00635	1·5508
D	1·5533	12 19 30	1·008	1·5533
E	1·5563	13 46 50	1·0097	1·5560
F	1·5589	14 56 50	1·0114	1·5585
G	1·5636	16 53 15	1·0147	1·5636
H	1·5677	18 18 30	1·0173	1·5677
			const. = 1·541	
Ordinary Ray.				
B	1·5409	10 20 0	1·0054	1·5409
C	1·5418	10 50 30	1·006	1·5418
D	1·5442	12 4 20	1·0075	1·5442
E	1·5471	13 30 0	1·0093	1·5469
F	1·5496	14 38 15	1·0109	1·5493
G	1·5542	16 32 45	1·0141	1·5541
H	1·5582	17 56 0	1·0166	1·5582
			const. = 1·5326	

Aragonite. RUDBERG.

Ray in the direction of the axes of elasticity. First Axis.

B	1·5275	9 40 0	1·0047	1.5275
C	1·5282	10 8 27	1·0051	1·5282
D	1·5301	11 17 34	1·0065	1·5303
E	1·5326	12 37 40	1·0081	1·5328
F	1·5348	13 41 25	1·0095	1·5348
G	1·5388	15 28 33	1·0123	1·5390
H	1·5423	16 46 36	1·0144	1·5424
			const. = 1·5204	
Second Axis.				
B	1·6763	12 50 0	1·0084	1·6763
C	1·6778	13 27 53	1·0092	1·6776
D	1·6816	14 59 35	1·0115	1·6815
E	1·6863	16 45 56	1·0144	1·6863
F	1·6905	18 10 42	1·0168	1·6903
G	1·6984	20 32 55	1·0217	1·6984
H	1·7051	22 16 27	1·0257	1·7050
			const. = 1·6623	
Third Axis.				
		° ′ ″		
B	1·6806	13 0 0	1·0086	1·6805
C	1·6820	13 38 20	1·0095	1·6820
D	1·6859	15 11 17	1·0118	1·6858
E	1·6908	16 59 0	1·0148	1·6908
F	1·6951	18 24 45	1·0175	1·6952
G	1·7032	20 48 52	1·0223	1·7033
H	1·7101	22 33 50	1·0263	1·7101
			const. = 1·6662	

Topaz. Rudberg.

First Axis of elasticity.

Ray.	Observed value of μ.	$\left(\frac{\theta}{\lambda}\right)$.	Ratio $\left(\frac{\text{arc}}{\text{sine}}\right)$.	Calculated value of μ = const. × $\left(\frac{\text{arc}}{\text{sine}}\right)$
B	1·6084	10° 5′ 0″	1·0051	1·6085
C	1·6093	10 34 42	1·0056	1·6092
D	1·6116	11 46 48	1·0070	1·6114
E	1·6145	13 10 22	1·0089	1·6145
F	1·6170	14 16 53	1·0104	1·6172
G	1·6215	16 8 40	1·0133	1·6216
H	1·6254	17 30 2	1·0157	1·6254
			const. = 1·6003	

Second Axis.

Ray.	Observed value of μ.	$\left(\frac{\theta}{\lambda}\right)$.	Ratio $\left(\frac{\text{arc}}{\text{sine}}\right)$.	Calculated value of μ
B	1·6105	10° 7′ 0″	1·00515	1·6105
C	1·6114	10 36 47	1·0058	1·6115
D	1·6137	11 49 10	1·0071	1·6136
E	1·6167	13 12 58	1·0090	1·6165
F	1·6191	14 19 45	1·0104	1·6189
G	1·6236	16 11 53	1·0133	1·6236
H	1·6274	17 33 29	1·0158	1·6275
			const. = 1·6022	

Third Axis.

Ray.	Observed value of μ.	$\left(\frac{\theta}{\lambda}\right)$.	Ratio $\left(\frac{\text{arc}}{\text{sine}}\right)$.	Calculated value of μ
B	1·6180	10° 7′ 0″	1·00515	1·6180
C	1·6188	10 36 47	1·0058	1·6189
D	1·6211	11 49 10	1·0071	1·6209
E	1·6241	13 12 58	1·0090	1·6240
F	1·6265	14 19 45	1·0104	1·6264
G	1·6312	16 11 53	1·0133	1·6310
H	1·6351	17 33 29	1·0158	1·6351
			const.= 1·60955	


Map extending from
LATITUDE 30° S. to LATITUDE 44° S.
(Copied from the large Map of De la Rochette.)
Vents in activity previous or at the moment of the Earthquake of Febʸ 20ᵗʰ 1835.
Islas de Juan Fernandez
Isla de Afuera
Isla de Tierra
Bahia (Cumberland Bay)
QUILLOTA
Quillota
Valparaiso
Volcano Aconcagua
Aconcagua 23000 feet.
S.Felipe el Real
MELIPILA
SANTIAGO
Mendoza
Volcan de Maypu
Volcan de Peteroa
Chiquillanes
COLCHAGUA
S.Fernando
MAULE
Talca
CHILLAN
CONCEPCION
ARAUCANOS
Volcan de Antoco
Pampas de Buenos Ayres
Ropocura
Maquegua
Huilliches
R. Tolten
Valdivia
Charaucabis
Osorno
Vol. de Quechu cabi
Calbuco
Corcovado 7500 feet.
CASTRO
GOLFO DE GUAITECA
P.ᵗᵉ y Volcan del Guafo
la Mocha
Ensenada y Rio Bueno
35
40
70
75
80
85
J. Basire sc.

III. *An Account of the great Earthquake experienced in Chile on the* 20*th of February,* 1835; *with a Map. By* Alexander Caldcleugh, *Esq. F.R.S. F.G.S., &c.*

Received October 15,—Read November 26, 1835.

The phenomena attending this great disturbance of the surface of the earth have been so varied, and the extent of its effects so considerable, that I should almost deviate from my duty if I did not endeavour to draw up and transmit to the Royal Society some account of a convulsion which has laid in ruins three provinces, and caused incalculable damage to the southern part of this country. I am the more inclined to take this step from a happy concurrence of circumstances having drawn several scientific observers to Concepcion shortly after the catastrophe, who have obligingly confided their notes to me. I trust therefore the Royal Society will not consider that I am about to trespass upon its time.

An idea, in some degree fanciful, prevailed for some time after the conquest of these countries by the Spaniards, that these convulsions of the earth's crust occurred at intervals of a century; afterwards it was supposed that about fifty years was the term which usually elapsed between great shocks; but, since the commencement of this century, the repeated catastrophes which have occurred, especially in the years 1812 in Caraccas, 1818 in Copiapo, 1822 in the province of Santiago, 1827 in Bogota, 1828 in Lima, 1829 in Santiago, and 1832 in Huasco, have prepared the minds of the inhabitants to expect at all times these frightful oscillations of the earth, which, although they cause little sensation at first, after some time affect the nerves in a manner not easy to account for by ordinary causes. That they happen at all times and in all states of the atmosphere seems clearly decided. The finest weather, and the most variable, equally prevail at the moment; but many are the fancied signs by which the coming earthquakes are predicted, and in the faith of which the inhabitants confide, as they think their experience bears them out. While some place great confidence in rats running violently over the ceilings of the room, others prepare for a shock when they observe the stars twinkling more than usual, and all fears are removed when much lightning coruscates in the Cordillera. As far as my own observations go, little reliance can be placed on the two former prognostics; something more certain seems to be due to the latter. A few hours previous to the earthquake which I am about to describe, immense flocks of sea birds proceeded from the coast towards the Cordillera, a circumstance which occurred prior to the great shock of 1822; and it is affirmed by too many respectable persons not to be entitled to some degree of credit, that on the morning of the convulsion all the dogs disappeared from Talcahuano.

The summer in Chile had been rather colder than in preceding years. The mean of the thermometer in Santiago (two thousand feet above the level of the sea) for the months of January and February was 72° of Fahr. The mean of the barometer for the same period was 28·25, which is about one tenth of an inch below its usual height.

From the 1st of February the barometer was unusually low in Santiago; and on the 14th, six days prior to the earthquake, the barometer at half past 6 A.M. stood at 28·1, the thermometer at the same time being 73°. A slight oscillation, which lasted twenty seconds, was felt on this day; on the 20th the barometer marked 28·17, and the thermometer rose to 76°: the weather fine. In Concepcion, in the night of the 17th to 18th the barometer fell four tenths of an inch, but gradually recovered itself, and indicated nothing extraordinary on the morning of the 20th. In Valdivia, according to the observations most obligingly communicated to me by Capt. FitzRoy, of the Beagle Surveying Ship, the barometer stood on the 16th of February at 29·92, and continued to rise gradually until the end of the month, with an increased temperature. From my own observations, deduced from many oscillations, I have remarked that the barometer usually falls shortly before any considerable shock, and then returns to its ordinary mean. On the 26th of September 1829, a very severe earthquake was experienced in this city, which did much damage to most buildings; the front of the house I then inhabited fell down; and it is worthy of remark, that the instant after every shock a burst of rain fell, which soon moderated, until a fresh tremor caused it to recommence.

The igneous vents of the whole range of the Cordillera may be said to have been in remarkable activity both preceding and at the moment of the late convulsion. From the flat-topped volcano of Yanteles, in front of Chiloe, to the lofty range of the Andes in Central America, all the information which has been obtained gives details of violent eruptions. On the 20th of January the volcano of Osorno, north-east of Chiloe, burst forth with inconceivable fury; and the lava was seen at night rushing out of the crater and rolling down the side of the mountain, elevated 3900 feet above the level of the sea. The reflection of the flame reached double that height, and is described to me by Mr. P. G. King, of the Beagle, as presenting the most magnificent object he had ever beheld. From the plains of Talca, eighty leagues to the south of the capital, two volcanos were observed in activity a few days after the 20th of February. They are both situate near the lake of Mondaca, twenty-five leagues eastward in the Cordillera: and another new rent was observed on the estate called Cerro colorado, on the right bank of the river Maule, and near its source. The volcano of Petoroa, and another near it whence a stream of asphaltum flows, and those of Maypu and Aconchagua have also been for some months in a state of activity.

In the month of January the volcano of Coseguina in Central America became exceedingly active, and ejected a body of lava which covered a circumference of eight leagues three yards and a half deep, burying all the farm-houses, sugar-works, and cattle: the ashes continued falling for five days, and reached upwards of three hundred leagues from the centre of desolation and ruin.

It was at half past eleven o'clock on the morning of the 20th that the earthquake commenced, with an atmosphere as serene and beautiful as the elements beneath were convulsed and threatening. The first oscillation, gentle, and attended with little noise, was but the precursor of the two succeeding undulations, which were extremely violent; the duration from the first to the last vibration was about two minutes and a half, and the direction appeared to be from south-west to north-east. The sensation occasioned by the undulatory movements, seemed to me to be similar to that which would be produced by standing on a plank the ends of which rose and fell two feet from the ground. The small streams of water which run down the streets were checked and thrown over the edges of their channels. In Talca, eighty leagues to the south, the effects were still more violent: the oscillation commenced without being accompanied by that rumbling noise which usually is the forerunner of these awful phenomena. In Concepcion, where the great violence of the earthquake was felt, it was the second undulation which caused the havoc in the buildings; and previous to that and the many succeeding shocks, a violent report was heard, proceeding from the southward, as from a volcano in that direction. All the houses in the port of Talcahuano, which were situate on the low lands beneath the hills, were laid prostrate; and about half an hour after the vibration, when the inhabitants were returning to their houses from the heights and open spaces, it was remarked that the sea had retired so much beyond its usual limits that all the rocks and shoals in the bay were visible. It flowed again, and again retired, leaving the ships dry which were at anchor in the harbour. Then an enormous wave was seen slowly approaching the devoted town, from the direction of the Boca Chica. For ten minutes it rolled majestically on, giving time to the inhabitants to run to the heights, whence they saw the whole place swallowed up by this immense breaker.

In this moment of terror, men saw the roller with little accordance as to size; some compared it to the height of the loftiest ship, others to the height of the island of Quiriquina. It carried all before it, and rose by accurate measurement twenty-eight feet above high-water mark. A small schooner of eighty tons, nearly ready for launching, was lifted over the remains of the walls, and found lying among the ruins three hundred yards from her stocks. The reflux of this roller carried everything to the ocean. Another and a larger wave succeeded; but taking a more easterly direction, the ruins of Talcahuano escaped, but the Isla del Rey was ravaged by it. A fourth and last roller, of small dimensions, advanced, but nothing was left for further devastation. While these great waves were rushing on, two eruptions of dense smoke were observed to issue from the sea. One, in shape like a lofty tower, occurred in the offing; the other took place in the small bay of San Vicente, and after it had disappeared, a whirlpool succeeded, hollow, in shape like an inverted cone, as if the sea were pouring into a cavity of the earth. In every direction in this bay, as well as in Talcahuano, vast bubbles broke, as if an immense evolution of gas were taking place, turning the colour of the water black, and exhaling a fetid sulphureous odour.

At San Tomé, on the other side of the bay, the roller did immense damage; and on the island of Quiriquina the cattle dashed off the cliffs from panic. In this island the waves injured houses forty feet above the present level of high water, and during the three following days the sea ebbed and flowed irregularly.

In the bay of Concepcion, the strata of clay slate have been visibly elevated, from about three to four feet. This alteration of the relative position of sea and land is clearly distinguishable, by a rock off the landing-place, which previous to the shock was nearly level with high water, being subsequently found to be raised three feet higher; and the buoy on the Belen Bank has now four feet less water than formerly. A vessel lying at anchor had one fathom less water alongside her than before the shock; but it is very likely that she changed her position. At the port of San Vicente, a little to the south of Talcahuano, the land has also risen about a foot and a half; and along the shore of the latter bay, even at high water, beds of dead muscles were left as proofs of the upheaval of the strata.

To the southward of the entrance of the bay of Concepcion there is a small island called Santa Maria, about seven miles long and two wide. Capt. FitzRoy examined with great care the line of beach in the southern cove, as well as the northern part of the island; and from the visible evidence of beds of dead shell-fish, from soundings, and from unbiassed oral testimony, it appears placed beyond the shadow of doubt, that on the latter side the elevation of the land has not been less than ten feet, in the centre of the island about nine, and in the southern cove about eight feet. This upheaving has almost destroyed the southern port of the island, for it now affords but little shelter to vessels, and the landing is bad. Everywhere around the island the soundings have been diminished a fathom and a half, and the cliffs, of the height of 150 or 200 feet, are split and rent in all directions, and huge masses precipitated below. Both Capt. FitzRoy and Capt. Simpson, of the Chilian Navy, are of opinion that the uprising of the strata, both in this island and in Concepcion, at the time of the earthquake, was considerably greater, and that the many subsequent minor oscillations may have caused a subsidence to the level before recorded. At Subul, a little to the south-east of Santa Maria, the elevation of the strata appears to have been about six feet.

At Nuevo Bilbao, the port of the river Maule, seventy leagues north of Concepcion, about an hour and a half after the shock, the sea flowed above the usual water mark, and continued for half an hour in that state before a reflux took place. Fifty minutes afterwards the sea, greatly agitated, rolled on the coast and up the river with extraordinary violence, and reached a height of twelve feet above the water mark.

By this last inroad, two schooners, anchored in the port, carried away their cables, and were found among the bushes one hundred and fifty yards from the beach.

A third rush of the sea occurred half an hour afterwards, which ascended to a height of nine feet; and for the space of forty-eight hours repeated rollers came forward, but with diminished violence. No elevation of the coast has been discovered at this port, but on the bar at the mouth of the river, which has always rendered the entrance to the port both difficult and dangerous, two feet more water has been remarked; and in consequence of the fall of an immense point of a mountain into the sea, it is hoped that, owing to the new direction given to the current, no further accumulation of sand will take place.

In Valparaiso the sea was observed to advance and recede rapidly, but gently and without violence.

It would be of little avail to distress the Society with the details of the ruin caused in all the southern provinces of Chile by this convulsion. To the southward of Talca scarcely a wall has been left standing, and even to the north of this line the damage caused to every description of building has been most serious. Throughout the provinces of Canqueues and Concepcion, the entire crust of the earth has been rent and shattered in every direction. In some places fissures of several feet in depth and width have been discovered intersecting the country for considerable distances. On one estate near Chillan, thirty leagues from the coast, extensive fissures have been the vents of muddy eruptions of salt water, which have made large deposits of a kind of grey pulverulent tufa; and on the same estate a great many circular pools were discovered of salt water, and many new thermal springs have burst forth. In many places the ground swelled like a large bubble, and then bursting, poured forth black and extraordinarily fetid water.

The limits to which the oscillations extended were, to the north as far as Coquimbo, and to Mendoza on the eastern ridge of the great chain of the Andes. Vessels navigating the Pacific within a hundred miles of the coast experienced the shock with considerable force. The bark Glenmalia, bound to Valparaiso, when ninety-five miles from the coast and in front of the Maule, had her course through the water suddenly checked, and her rate of sailing altered from seven knots to one, and the master conceived the vessel was dragging over a sand-bank. The sea was strongly agitated, and appeared to lift the vessel twenty feet. Such was the alarm that the boats were nearly lowered: no soundings were met with.

The Island of Juan Fernandez, a mass of basalt three hundred and sixty miles from the coast, experienced the earthquake, but with less violence; the sea rose to the height of the Mole in a similar manner to that of Concepcion, and then receded, leaving the bottom of Cumberland Bay dry to some distance from the shore, and in the second rush rose fifteen feet above the usual level, carrying all before it. At the same time the Governor, Major Sutcliffe, observed a dense column of smoke issuing from the sea about a mile off the Point Bacalao, which lasted until 2 o'clock in the morning, when an immense explosion took place, which threw the water in every direction; during the rest of the night great bursts of flame rising from the same spot illumined the whole island. Captain SIMPSON, about a month after, sounded near the spot in every direction and found no bottom in less than sixty-nine fathoms. It is worthy of remark, that when on the 24th of May 1751 the city of Concepcion was destroyed by an earthquake, and by the inroad of the sea, the rising colony of Juan Fernandez was swallowed up in a similar manner by immense rollers. The Governor, his family, and thirty-five persons perished by the catastrophe.

After the earthquake the usual atmospheric changes occurred. In many places the most awful hurricanes completed the dismay of the inhabitants and added to the catastrophe. To these succeeded deluges of rain, a circumstance most unusual at that period of the year. At the Hot Springs of Canqueues, where the water issues at the temperature of 118° of FAHR., the heat was lowered after the earthquake to 92°, a circumstance which occurred after the shock of 1822. The diminished temperature lasted but a short time.

At the risk of being tedious, I have given a detail to the Society of the changes effected in the earth's surface by this violent convulsion. After examining the extensive area of its vibration, after observing the uprising of an island and the adjacent coast, together with the eruption of a submarine volcano, it is difficult to deny that the same causes are still in operation which ages since raised tertiary formations to their present lofty site in the great range of the Cordillera. Surrounded with these continued changes on the surface of the earth, it is impossible not to respect the opinions of those philosophers who conceive that the Continent of America has risen into existence at a more modern period than that which therefore may, with more propriety, be termed the Old World.

Owing to the early hour on the 20th that the oscillation commenced, comparatively few lives were lost, but the frequent occurrence of these catastrophes, by causing organic defects, may very probably explain the causes of the short duration of human existence in these countries.

Santiago de Chile,
12th June, 1835.

IV. *Some Account of the Volcanic Eruption of Cosegüina in the Bay of Fonseca, commonly called the Bay of Conchagua, on the Western Coast of Central America.*

By Alexander Caldcleugh, *Esq., F.R.S. F.G.S., &c.*

Received December 29, 1835,—Read January 7, 1836.

There is perhaps no country on the face of the globe which shows more signs of vast geological disturbances than that part of the western hemisphere which, situate between its great northern and southern divisions, has obtained in more modern times the name of Central America. Its shores, extending to both oceans, are in spots precipitous, while other and extended lines of coast are low, and abound in mangrove creeks, intersected by mountains and volcanic vents, and excavated by a series of lakes, which in the province of Nicaragua interrupt and appear to replace the great chain of the Andes. The finely comminuted scoria affords a soil which produces the richest vegetation, and a vast and new field is offered to the man of science who will boldly face the miasma of the forest, or penetrate the rich mines with which one part of the country abounds.

At the termination of a narrow promontory, which runs in a northerly direction from the southern and eastern side of the Bay of Fonseca, stands the volcanic mountain of Cosegüina, washed on three sides by the ocean, of insignificant height, and flat-topped; two eruptions are on record, viz. those of the years 1709, and 1809. Since this last date it has remained in a state of quiescence, until the period of that stupendous eruption on the 20th of January last, the details of which I now beg permission to lay before the Royal Society. These details I have drawn up partly from official documents transmitted from the various towns to the government of Centro-America, and partly from the information of intelligent friends, eye-witnesses of all that occurred in those days of terror. The reports to the Government, which are voluminous, fully agree on the main points; in others, probably owing to the changes of locality and consequent variation in the direction of the wind, some slight differences are observable. It is, however, impossible to read these official reports, written too by persons little versed in classical learning, without being struck with the similarity of their description, even to the very terms he used, with that of the younger Pliny in relating to Tacitus the commencement of that eruption of Vesuvius which, nineteen centuries since, buried two cities under its ashes.

On the 19th of January, after twenty-six years of repose, a slight noise attended with smoke proceeded from the mountain Cosegüina. On the following morning about half-past six o'clock, a cloud of very unusual size and shape was observed by the inhabitants of the neighbouring places to rise in the direction of this volcano. When viewed from San Antonio, about sixteen leagues to the southward, it took the appearance of an immense plume of the whitest feathers, rising with considerable velocity, and expanding in every direction. Its colour, at first of the most brilliant white, became tinged with grey, then passed into yellow, and finally became of a crimson hue. Columns of fire shot up directly through what was still imagined to be but a nebulous exhalation of extraordinary appearance; a severe shock of an earthquake was then felt. During the whole of the 20th the cloud preserved its appearance, although unattended by that magnificence which at first predominated.

At three P.M. on the 21st, two severe shocks were felt at San Antonio and Realejo, and at midnight five others were experienced; the two first undulations were not severe, but the third and the last were terrific.

On the morning of the 22nd the sun shone brightly at San Antonio, but a line of intense darkness was observed in the direction of the cloud which had excited so much attention two days before. At the same time a fine white ash was observed to fall around, the black line rose rapidly, the light began to fail, and darkness commenced with such quickness, that in half an hour it was more utterly dark than during the most clouded night. So intense was this darkness that men could touch without seeing each other, the cattle came in from the country showing all the signs of alarm and uneasiness, and the fowls went to roost as on the approach of night. This state of complete darkness prevailed until the following day, when at twelve o'clock it became a little brighter, and objects became visible at ten or twelve yards distance. This atmospheric darkness prevailed two days longer, during all of which time a fine white impalpable dust continued to fall. This deposit covered the ground at San Antonio about two inches and a half in three layers of different colours, the lower stratum being of a darker hue, the next of a greyish, and the upper of a whitish appearance. For ten or twelve days a murky light continued to prevail.

At Nacaome, a city in a northerly direction, eight leagues distant from the volcano, the same cloud was observed to rise at half-past six o'clock in a pyramidal shape. At half-past eleven on that day the darkness commenced, and at twelve nothing whatever could be distinguished; shortly before this a kind of ash had begun to fall, and at five o'clock had covered the earth to the depth of three inches. At six o'clock it fell in diminished quantity, and respiration became relieved. During the following night various undulations of different degrees of intensity were experienced, preceded either by heavy rumbling noises, or loud explosions. On the 21st at Nacaome the morning broke clear, but at eight o'clock the atmosphere became again thick and hazy, and during the twenty-four hours following, the volcanic matter continued to fall, attended with repeated noises and undulations of the earth. The darkness continued to prevail during the 22nd, and the depth of the ashes was from four to five inches; a fetid sulphurous smell proceeded from this deposit, which the slightest breath of air drove into every interstice. At midnight violent explosions were heard, and, a quarter of an hour after, a severe shock was experienced, the forerunner of new eruptions. At 5 o'clock on the morning of the 23rd it was sufficiently light to observe that a fresh eruption had taken place from Cosegüina, and at 8 o'clock the darkness had returned as on the 20th. At 9 o'clock the obscurity was complete, and new and new awful echoes of vast discharges of volcanic matter, attended with flashes of lightning in all directions, convinced the panic-stricken inhabitants that the day of judgement had arrived.

No pen, says the Governor, is capable of describing the scene of dismay which prevailed. On the 24th the atmosphere became clearer. The houses were covered to the depth of eight inches with the ashes which had fallen, and many small birds and animals were found suffocated in them. Deer and other wild animals sought the town for protection, and the banks of the neighbouring streams were strewed with dead fish.

It would be but useless to tire the Society by giving extracts of all the reports made from different places within the sphere of the eruption. I shall confine myself to stating that at Macuelizo, in Segovia, the colour of the sand which fell was black; and on the Hacienda of Cosegüina, belonging to Don Bernardo Benevio, eight leagues to the southward of the crater, the ashes covered the ground to the depth of three yards and a half, destroying the fine woods and dwellings.

It also avails little to mention the great mortality which prevailed among the cattle. Thousands perished, and those which after the eruption reached the abodes of men, presented sad spectacles; their bodies in many instances being one mass of scorched flesh.

Within the bay of Fonseca, and two miles from the volcano, it is stated that two islands have been thrown up, of from 200 to 300 yards in length, their surface, but a few yards above the sea, presenting, it is said, a mass of scoria and ashes: their elevation has probably been caused by the heavy fall of scoriaceous matter on shoals previously existing in those places. However probable, the evidence is not conclusive, although the fact of the beach on the eastern or inner side of the promontory being extended by the ashes about 800 feet further out, gives additional reason to credit the statement.

On the 3rd of March, nearly two months after this great eruption, the volcano remained in a state of activity, but not ejecting ashes. By some geologists it has been considered that heavy eruptions of fine scoriaceous matter tend, by their falling again into the crater, to restore the volcano to a quiescent state, and that therefore this phenomenon more usually attends the conclusion of an explosion. In this particular instance it appears that the first effect of the explosion was to blow out of the crater, and finely triturate, the scoria and ashes left there twenty-six years before.

In the districts of Segovia, Comagagua, Choluteca, Nacaome, and Tegusigalpa, immense deluges of rain followed these clouds of ashes, and again gave rise to a fetid, disagreeable odour. At this season such an occurrence was extraordinary, and almost unprecedented in Central America.

I shall conclude by stating that the ashes reached as far as Chiapa to the north, upwards of 400 leagues to windward of the volcano: thus proving the existence of a counter current of wind in the higher regions of the atmosphere. At St. Anne's, Jamaica, on the 24th and 25th of January, the sun was obscured, and not only there but over the whole island, showers of fine ashes were observed to fall. The distance in a direct line north-easterly is about 700 miles; consequently the ashes must have travelled at the rate of about 170 miles per diem.

Captain EDEN, of His Majesty's ship Conway, informs me, that in lat. 7° 26′ north, and long. 104° 45′ west, when 900 miles from the nearest land and 1100 from the volcano, he ran forty miles through floating pumice, some of which was in pieces of considerable size.

The latitude of Cosegüina is 13° north, and longitude 87° 3′ west. Its height above the sea is computed at 500 feet.

No volcanic eruption in modern times has been recorded that reached the frightful extent of the one of which I have now had the honour of laying an account before the Royal Society. The explosion of Tomboro in Sumbaya in 1815, described in the Memoirs of the late lamented Sir THOMAS STAMFORD RAFFLES, more nearly approaches it than any other with which I am acquainted.

Santiago de Chile,
18*th August,* 1835.

V. *Memoranda made during the appearance of the Aurora Borealis on the 18th of November, 1835. By* Charles C. Christie, *Esq. M.A. Communicated by* S. Hunter Christie, *Esq. M.A. F.R.S. &c.*

Received and Read December 10, 1835.

As the following Memoranda, noted down during the remarkable Aurora of the 18th of November, have been considered of sufficient interest to be read before the Royal Society, I think it right to state, that I did not anticipate that this would be the case, and that my only object in making them was, to record carefully whatever remarkable appearance might present itself, for the information of one who, I knew, felt a deep interest in every phenomenon of the kind, and whose simultaneous observations I hoped to have the pleasure of comparing with my own. Being, however, unwilling to make any alteration in a statement of facts, I have preferred leaving them exactly as they were communicated, merely offering a few additional explanations and remarks, suggested by striking exhibitions of the phenomenon.

Memoranda.

Wednesday evening, 9 o'clock.—Remarked, on looking accidentally from the drawing-room window of Deal Castle, a bright light over Ramsgate, exactly as if the moon were about to rise in that quarter. Saw, on proceeding to the roof, a perfect and very bright arch to the north; the lower edge being sharply defined on the dark cloud beneath, the upper shaded off into the sky. The sky, except beneath the luminous arch, perfectly cloudless; the stars shining brightly down to its upper edge. The wind rather high and gusty, not particularly cold, and north-north-west (by the ventometer). Altitude of the arch about that of γ Ursæ Majoris. Western extremity terminating exactly below α Aquilæ.

$9^h\ 5^m$.—The arch itself motionless, but large bodies of faint vapoury light continually ascending from it, and whirled in every direction, across the zenith, &c., as if by the wind, and with such rapidity as scarcely to be followed by the eye. These frequently rose perpendicularly, and were then sharply whisked off towards the south-east. (See Sketch 1. Plate II.)

$9^h\ 15^m$.—A fine outbreak of pencils of light from the centre and eastern extremity of the arch; none of them stationary, or in straight lines, but waving more or less and flickering, as if with the wind; masses of vapoury light whirled up occasionally; the whole presenting the appearance of an immense and not distant, conflagration, while the paleness of the light and the absence of noise gave it a spectral and unearthly character, which was very striking. The gusts of wind increased the illusion. (See Sketch 2. Plate II.)

9^h 20^m.—The arch becoming very irregular; a large indentation on the eastern side, thus:

Fig. A.

The pseudo-flames have almost entirely subsided; they still exhibit the same appearance of burning, but in a steadier manner.

The centre of the arch is about equidistant from γ Ursæ Majoris and α Lyræ.

9^h 25^m.—The western extremity suddenly blazed up; one very broad pencil of rose-coloured light forming the western boundary to the rest; through it α Aquilæ shone with great brilliancy. All these westerly pencils perfectly straight, of greater altitude, and of a more defined and steady light than the easterly. Extremely narrow brilliant jets of light issuing from the central part, and having their base in the midst of the dark cloud. (See Sketch 3. Plate III.)

9^h 27^m.—Arch dilapidated. (See Sketch 4. Plate III.)

9^h 30^m.—Arch entirely broken up.

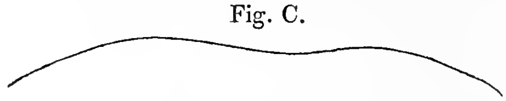
9^h 35^m.—The arch restored, but of an irregular undulating form, thus:

Fig. B.

The light fainter, as also the pencils which continue to rise from every part, but more distinctly from the two extremities.

9^h 55^m.—The arch much depressed, and in form thus:

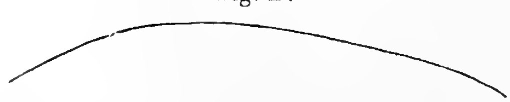
Fig. C.



The cloud below much darker, the pencillings very faint.

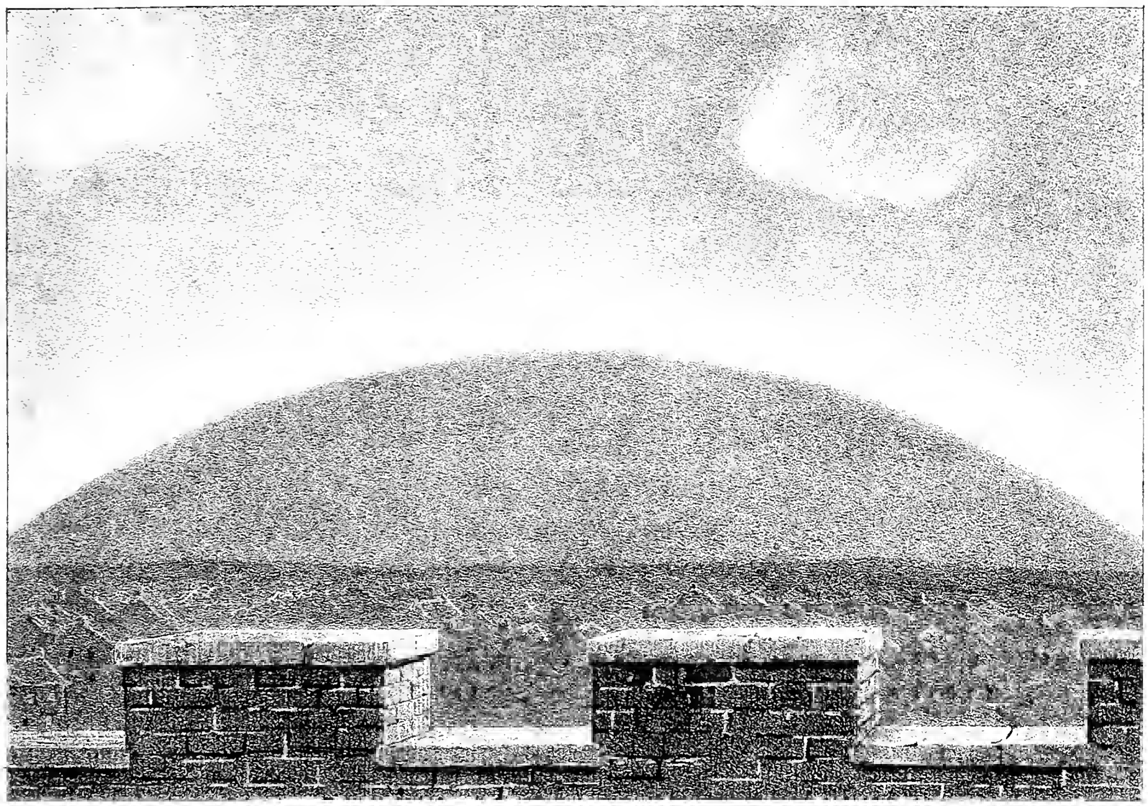
10^h 20^m. The arch strongly defined and steady; but occasional gleams, as of the vapoury light rising in a body simultaneously from the whole extent of it. The cloud very dark, and of this form:

Fig. D.

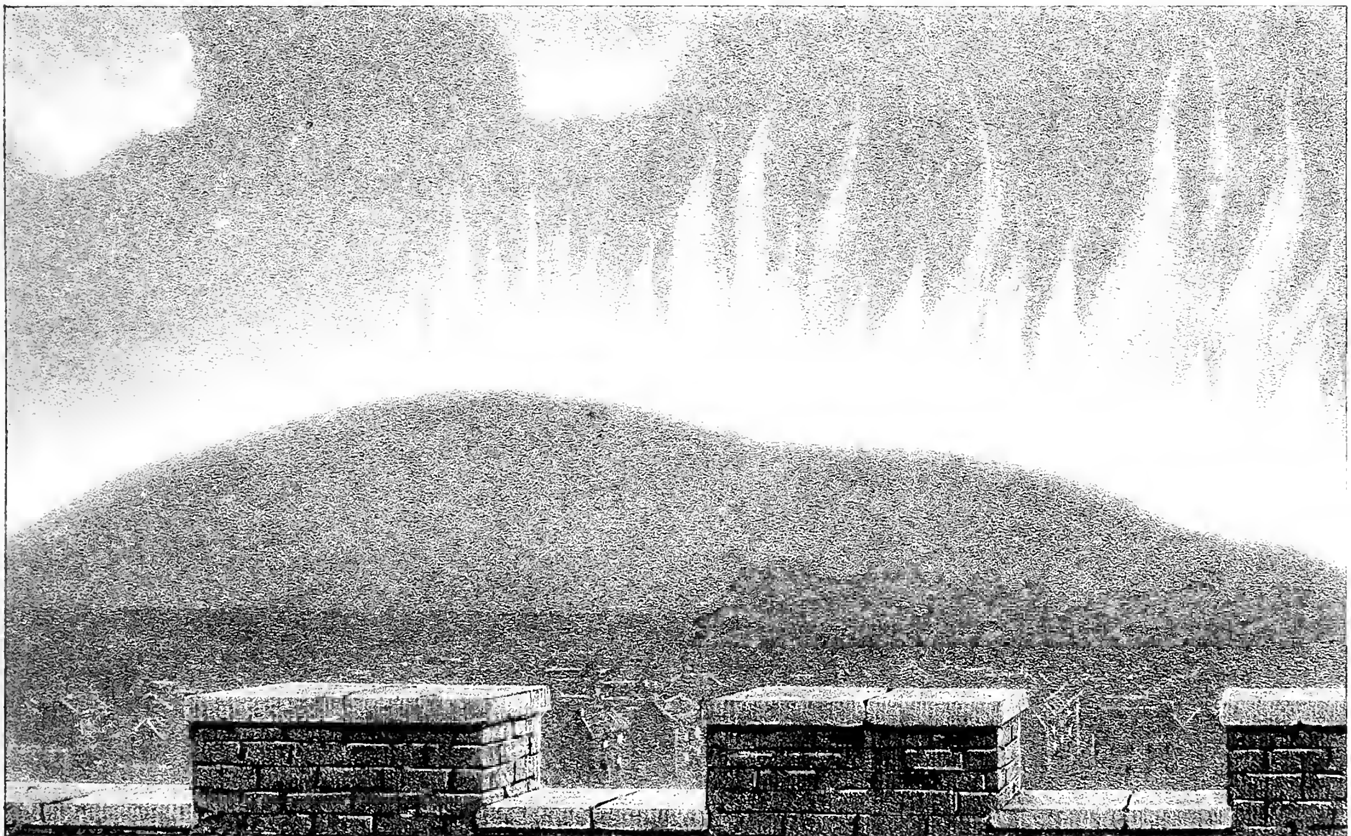


I regret that I was unable to wait for the total disappearance of the phenomenon, though for nearly an hour there had been no promise of any fresh display.

Note.—By the term *arch* is indicated the under surface of the body of light.



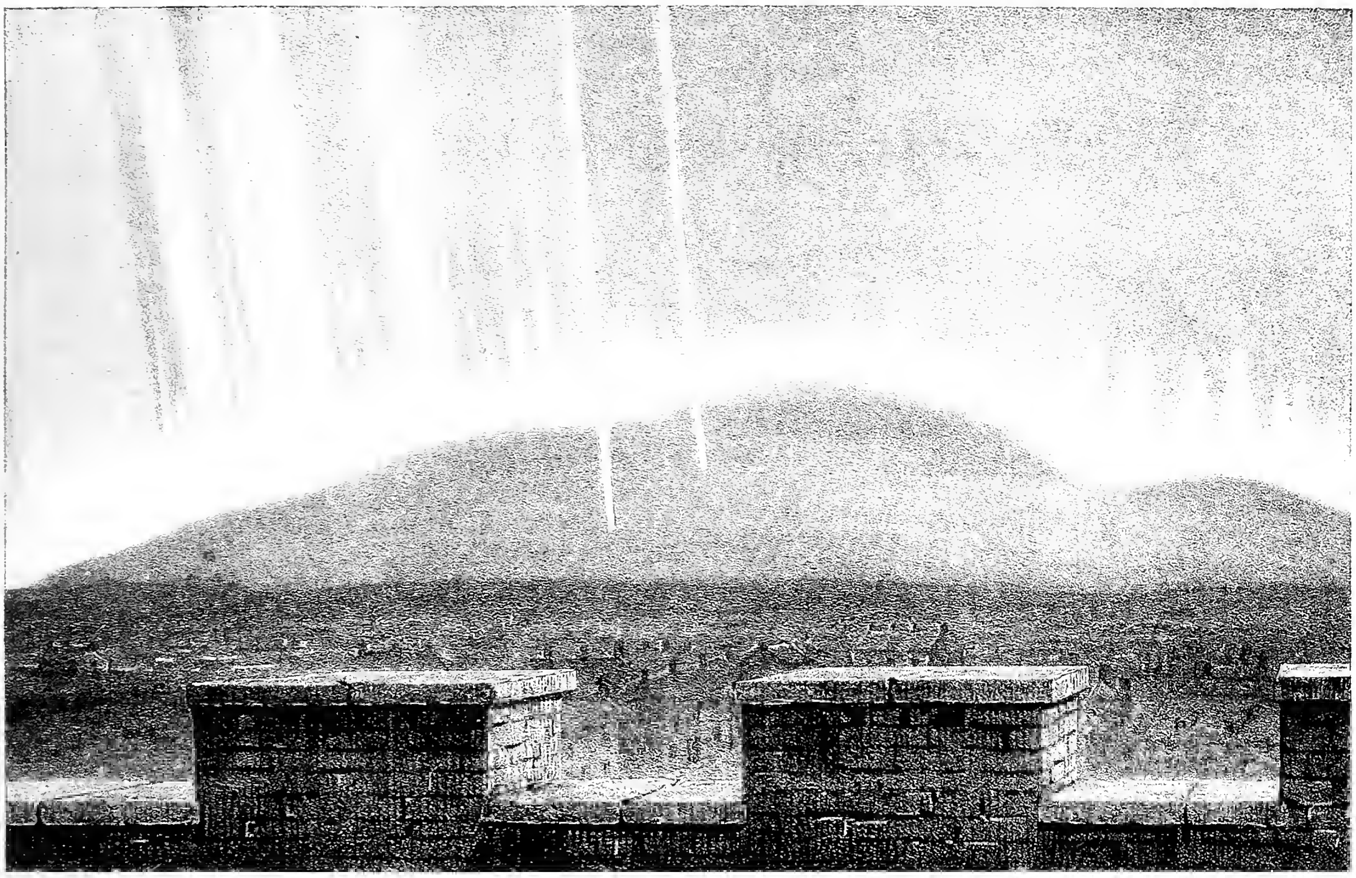
Nº 1. *The Aurora Borealis at 9h 5m.*



Nº 2. *The Aurora Borealis at 9h 15m.*

C. C. Christie delt.

Basire sculp.



N° 3. *The Aurora Borealis, at 9^h 25^m*

N° 4. *The Aurora Borealis at 9^h 27^m*

C. C. Christie del^t

J. Basire lithog

Remarks.

Having at the time no instruments for determining the bearing or altitude of the arch, I was obliged to depend upon the positions of some conspicuous stars, which were conveniently situated for that purpose. According to these rough data, the altitude was 18°; the angle subtended by the span of the arch about 130°; the bearing of the centre of the arch north-north-west, true, or very nearly magnetic north; and the arch was consequently at right angles to the magnetic meridian.

The body of light was nearly colourless; its brightness was similar to that seen on the edge of a cloud when the moon is about to rise behind it, with, however, this striking difference, that the stars were distinctly seen through the diffused light of its upper surface, and those in the tail of the Bear shone clearly in the very body of the light on the right hand.

With regard to the sketches with which I have attempted to illustrate the preceding notes, it is necessary to observe, first, that the extent of horizon renders it impossible to give in one view any idea of the magnificent scale on which the original was depicted, or even to preserve very correctly the relative proportions of height and breadth; and next, that in sketches I. and II. the rapid motion of the bodies of vapoury light, and of the flame-like pencils, must be held in mind: the former bore an exact resemblance to the faint reflected light darting across the sides of a room from a mirror turned sharply in the hand, and the latter to the lambent flames which diluted spirit of wine, poured on a flat surface and ignited, will exhibit when half extinguished.

The pencils which appeared in front of the dark cloud, of which there were not more than three, were very distinct in their character from the others; they were of a yellower tinge, and extremely narrow throughout their whole height. I have stated, that they *issued from* the dark cloud; perhaps it would be more correct to say, that they *pierced through* it; for although I did not observe the instant of their appearance, being at the moment engrossed by the display on the left, yet, in each, the brightness of the base, which was, as it were, the nucleus of its light, seemed to warrant this idea. The mere circumstance, however, of their appearance in front of the cloud, tends to elucidate a point on which there exists much difference of opinion, the height of the aurora in the atmosphere. The dark cloud itself can scarcely be supposed to have occupied a very elevated region; and it is manifest, that if these brilliant pencils had their origin *in*, or *in advance of*, the cloud, their bases must have been of inferior altitude to its upper portion, and equally so, if they were identical with any continuation of the luminous matter of the arch, concealed by the cloud.

The first appearance of the aurora at nine o'clock, was that of a dark convex cloud, cutting off the luminous arch, and concealing a body of light behind, the eye naturally referring the light to a more distant region, while the sharp line of division threw the cloud forward. Subsequent appearances, however, did not seem to confirm this notion, but, on the contrary, induced me to consider, whether the dark cloud might not be a *substratum* of matter differing in nature and density from the superincumbent arch of light. The following are the facts which appear to favour this supposition. First; every great outbreak of coruscations from the luminous arch produced a corresponding disturbance in the part of the cloud immediately below. Thus, during the display at 9^h 15^m, the arch was gradually losing its regular form on the right, and at 9^h 20^m I noted it "very irregular, with a large indentation on the eastern side," while on the west, where the body of light was undisturbed, the arch remained perfect. Thus also, immediately after the western half had been in vivid coruscation, the whole of the arch was "dilapidated," and finally, "entirely broken up." Secondly; neither the straight nor waving pencils appeared to proceed from behind the cloud, but always from the upper surface of the light. Thirdly; when the arch was "dilapidated" (see Sketch IV.), it was not merely its upper surface which was of irregular form, but masses of it were lying in confusion, separated from each other by a boundary of light, not appearing in the least as if light behind were shining through, but rather as if the substances of the arch and cloud had unwillingly interpenetrated each other, and refused to mix together more intimately, while at the same time the light above became more diffused and of diminished brightness. Lastly; when at 9^h 35^m the continuity of the arch is restored, it remains "of irregular undulating form" (fig. B.), while fainter pencils continue to rise from every part; at 9^h 55^m the cloud is "much darker," "the pencillings very faint," while its form is evidently becoming more regular (from fig. B. to fig. C.); and at 10^h 20^m it is nearly perfect in form (fig. D.), "strongly defined and steady," the pencillings having entirely ceased. All these circumstances struck me as so closely resembling the disturbance of two fluids, the one superposed on the other, mutually repulsive, but compelled to mingle by forces, of whose action the vividness of the pencillings seemed to indicate the intensity, and requiring intervals of repose to re-collect their scattered energies, that I cannot but conclude the luminous matter of an aurora to be a superincumbent stratum, and, consequently, that its altitude is dependent on that of the dark mass immediately beneath.

Bath, January 4th, 1836.

No. II.

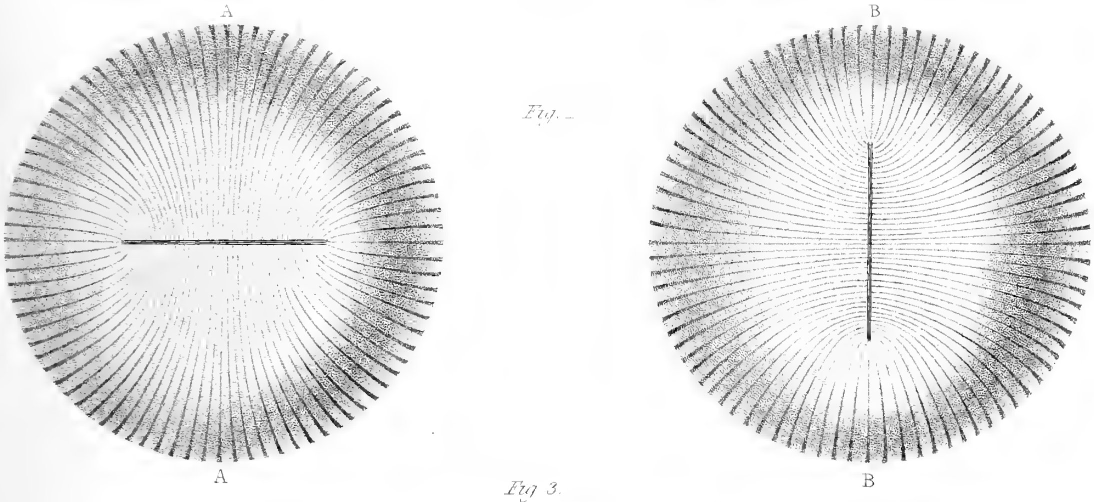
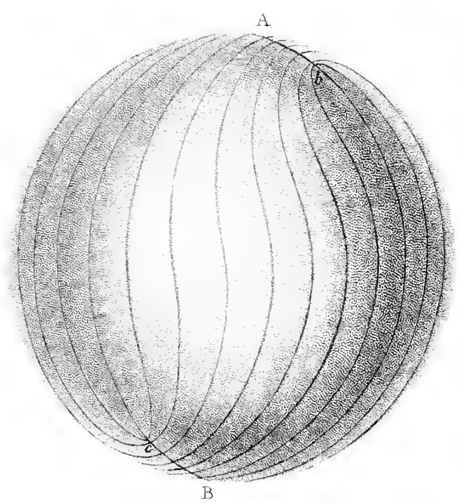


Fig. 1.

Fig. 3.



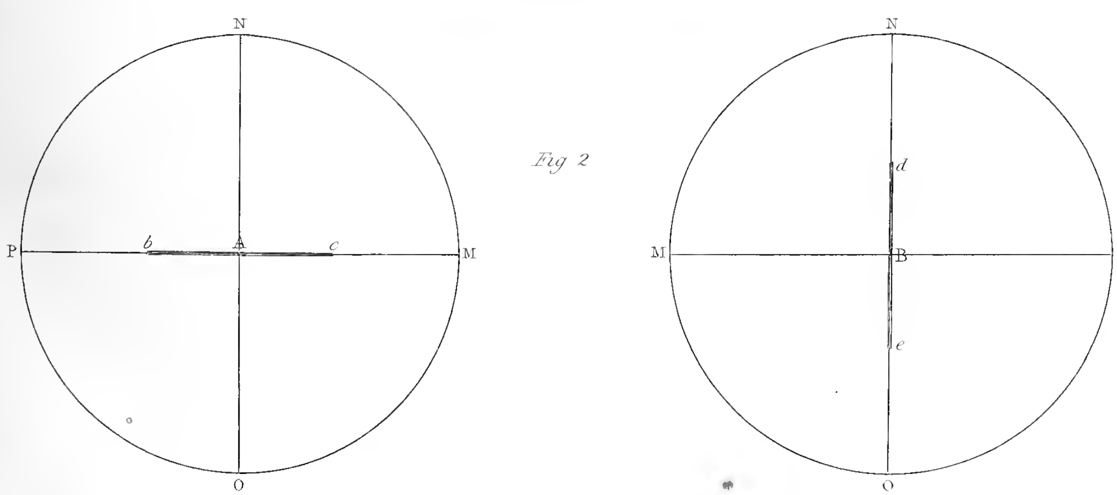


Fig. 2.

VI. *On the Anatomical and Optical Structure of the Crystalline Lenses of Animals. Continued from a former Paper (Phil. Trans. 1833, p. 332.). By Sir* David Brewster, *K.H. LL.D. F.R.S. &c. &c.*

Received November 26, 1835,—Read January 21, 1836.

§ 2. *On the Anatomical and Optical Structure of the Crystalline Lenses of Animals, particularly those of the Hare and the Salmon.*

In describing the various structures which exist in the crystalline lenses of animals, I shall proceed from the most simple to the most complex combination of fibres. In the paper which I have already submitted to the Society, I took the lens of the cod as the type of the *first* or simplest structure, in which the fibres, like the meridians of a globe, converge to two opposite points of a spheroidal or lenticular solid; and I shall now proceed to describe the *second* or next simplest structure, as exemplified in the lenses of the salmon and the hare.

This structure is shown in Plate IV. fig. 1, where A A is the anterior and B B the posterior hemisphere of the spheroidal lens of the salmon. A lens thus constituted is said to have *two septa* at each pole, A and B (fig. 2.), namely, the septa A *b*, A *c*, and B *d*, B *e*, in different points of which all the fibres have their origin and termination. One of the fibres, for example, which has its origin in A, passes over N, and terminates in *d*; while the other passes from A to O, and terminates in *e*. In like manner, the fibre which begins at *b* passes over P, and ends in B; while the fibre which begins at *c* passes over M, and terminates in B. The different parts of these four fibres lie in one plane, like the meridians of a globe. All the other fibres, which have their origin between A and B, have their termination between *d* and B; and all those which have their origin between A and *c* have their termination between *e* and B. All these fibres, or every fibre in each lamina except *four*, have their different parts lying in different planes, or form curves of contrary flexure. In order to understand this structure, and appreciate its beauty, it is necessary to draw the fibres upon a globular surface. A perspective view of such a globe is given in fig. 3, where A *b* is one of the anterior septa, and B *c* one of the posterior ones. The two curves which go from the poles A and B to the ends of the septa *c*, *b*, are ellipses, while all the rest are curves of contrary flexure.

The length of the septa A *b*, A *c*, &c. varies in different fishes; and when they are very short, as they must necessarily be in small lenses, we are apt to mistake the structure which they indicate for that of a diffused polarity round the two extremities of the axis of the lens. When such an ambiguity, however, presents itself, we must observe carefully if the diffusion is more elongated in one direction than another. If it is not, and especially if there is seen a small circular dimple or depression at the poles when we follow the light reflected from the surface of the lens, there can be no doubt that the fibres converge to two opposite poles, like the meridians of a globe. But if the diffused polarity be of an *oval* form, and if the greater axis of the oval on one side of the lens be perpendicular to the same axis on the other side, we may then safely infer that the fibres are related, as in the lens of the salmon, to *two* septa at each extremity of the axis of vision. When these methods fail, the ambiguity may sometimes be removed by boiling the lenses, and observing the manner in which they crack at the poles.

From a too hasty generalization of a small number of facts, LEEUWENHOEK maintained that all fishes and birds have two short septa; and M. SATTIG * has advanced the same opinion in reference to fishes, and committed the additional mistake of making every part of the fibres lie in the same plane. Dr. THOMAS YOUNG maintains that there are no septa in the lenses either of birds or fishes †; and in pursuing the chimæra of a muscular lens, he not only renounced discoveries of his own, after LEEUWENHOEK, but adopted grave errors which have no foundation whatever either in observation or analogy.

It is a very remarkable circumstance, that the hare and the rabbit should be the only quadrupeds whose lenses have two septa, like that of the salmon. This fact was observed by LEEUWENHOEK‡ and also by SATTIG, who remarks that the septa are larger in the lenses of these animals than in those of fishes.

The fibres of the lenses of the hare and the rabbit are curves of contrary flexure, like those of the salmon, and they are distinctly toothed, though the teeth are much smaller than those of the salmon and the cod, of which I have given a representation in a former paper.

Although I have stated that the hare and the rabbit are the only quadrupeds whose lenses are known to have the same structure as that of the salmon, yet there is a rare marsupial quadruped, the *Perameles nasuta* of GEOFFROY, whose lens will probably be found to have two septa on each of its surfaces. Professor GRANT was so good as to send me a single lens of this curious animal; but as one of its faces was much injured, I was able only to discover the two septa on the side of the lens which was uninjured. It is highly probable that the fibres will have a similar arrangement on the other side of the lens; but until this is actually determined, it is possible that

* Lentis Crystallinæ Structura Fibrosa. Preside Reil Defendit SAMUEL GODOFREDIUS SATTIG SILESIUS. Halæ, 1794. It is not surprising that the author of this thesis should have followed LEEUWENHOEK in this mistake, as he seems to have examined the lenses *only* of the carp and the perch, in which the two septa are most distinctly developed.

† Elements of Natural Philosophy, vol. ii. p. 599. col. 1. and Plate XII. fig. 100.

‡ Opera, vol. ii. p. 66. Lugd. Batav. 1722.

the *Perameles nasuta* may have its lens formed according to another class of structures which will afterwards be described. The fibres of the lens of this animal are extremely small, and the teeth upon them, though very minute, are distinctly seen with a high magnifying power.

The structure indicated by two septa is perceived very distinctly in the lens of the *Cobra Capella* and the *Lacerta Gecko*, and indistinctly in that of the *Stellio Gecko* and the *Frog*. The lenses of the *Cobra Capella* and the *Lacerta Gecko* are nearly spherical; and the laminæ of the lens of the *Stellio Gecko* are composed of a fibrous tissue, and not of fibres united by teeth.

In the following Table I have given the names of the different animals whose lenses have the structure shown in fig. 1.

	QUADRUPEDS.	
Hare.	Rabbit.	*Perameles nasuta* (one side).
	REPTILES.	
Cobra Capella.	*Lacerta Gecko.*	*Stellio Gecko* (probably).
Frog (probably).	Alligator.	
	FISHES.	
Salmon.	Tench.	Hickory Chad.
Dolphin.	Carp.	Cavala (Georgia).
Shark.	Perch.	Stingarie.
Porpoise.	Sturgeon.	Skip-jack.
Skate.	Gudgeon.	Chad.
Thornback.	Cat-fish.	Black-fish (Georgia).
Boneto.	Par.	Fish from Singapore.
Dog-fish.	Red Trout.	Sheep-head.
Sword-fish.	River Trout, common.	

1. *Hare.*—I have examined the lenses both of the common hare and the blue mountain hare, which have the same structure. In observing their action upon polarized light, I find that they depolarize *two series* of luminous sectors, the inner sectors having the *negative* structure like *calcareous spar*, and the outer sectors the *positive* structure like *zircon*. In order to perceive the inner sectors, the lens must be taken out of the eye with great care, and subjected to no pressure, and the polarized light must be transmitted through its axis.

2. *Rabbit.*—The lens of the rabbit resembles that of the hare in its general properties. The cornea of a rabbit depolarized faint sectors of light, and its polarizing structure was *negative*.

3. *Perameles nasuta.*—When the lens of this animal was taken out of the spirits which preserved it, and the outer coat removed, its fibres were crossed perpendicularly by irregular and slightly serrated lines, much more distinct than the fibres. As

these lines are not now visible in the indurated lens, I cannot even form a conjecture respecting their origin.

4. *Stellio Gecko* and *Frog*.—The lenses of these animals require to be re-examined.

5. *Salmon*.—The length of the septa in the lens of the salmon is less than those in the hare and rabbit, and the teeth upon the fibres are extremely distinct. The lens depolarizes *three* series of luminous sectors, the *inner* and outer series being *negative* and the intermediate series *positive**. The polarizing structure of the cornea was *negative*, and it depolarized very high tints at its junction with the sclerotic coat. The structure of the *sclerotic coat* is very remarkable. In the eye which I examined, the thickness of the sclerotic was about the fifteenth of an inch, and with a sharp knife it could be cut like a piece of cheese. It had a milky transparency like some opals. When a slice with parallel faces, nearly perpendicular to the surface, was exposed to polarized light, it exhibited the system of biaxal rectilinear fringes†, exactly like those in a plate of glass heated by boiling water or oil, and in the act of rapid cooling. The same structure exists in the sclerotic coat of the *Cod*.

6. *Dolphin*.—The lens of the dolphin is decidedly an oblate spheroid, the axis of which is that of vision. In an indurated lens the axis of the spheroid is 0·254 of an inch, and the equatorial diameter 0·307. The teeth of the fibres are small and irregular, like those of quadrupeds.

7. *Shark*.—The lenses of the common and blue-eyed shark have the same structure. The sclerotic coat has the remarkable property (when cut in the manner already described in the case of the salmon) of depolarizing light like *a plate of bent glass*‡; but, what is very curious, the *concave* side of the sclerotic has the same action upon light as the *convex* side of the bent glass.

8. *Alligator*.—The lens of the alligator is nearly spherical: the teeth of the fibres, as in the dolphin, are shorter than those in fishes.

9. *Skate*.—The lens of this fish depolarizes *three* series of luminous sectors, but the inner series is not so distinct and near the axis as those of the cod. The inner and outer series are *negative*, and the intermediate series *positive*. The horny sclerotic of the skate has the very same polarizing structure as that of the salmon. The teeth of the fibres of the lens are exceedingly small.

10. *Thornback*.—The fibres are very delicate, and their teeth small but distinct. The lens depolarizes luminous sectors; but from the imperfection of the lens I could not observe their character.

11. *Boneto*.—The lens is an oblate spheroid, whose axis is that of vision. The teeth of the fibres are very distinct§.

12. *Sword-fish*.—The diameter of the lens of this fish which I examined was 1·10th of an inch. The teeth of the fibres were so distinct that *three* circles of the secondary colours produced by them were distinctly seen‖.

* Philosophical Transactions, 1816. † Ibid., 1816. ‡ Ibid., 1816.

§ Ibid., 1833, p. 332. ‖ Ibid., 1833, p. 327.

13. *Tench.*—This fish has a very small eye, and a small lens. Owing to the faintness of the polarized tint I have observed only one series of luminous sectors, whose character is *positive.*

14. *Perch.*—The lens of this fish, like that of the tench, exhibits only one set of luminous sectors by polarized light, and their character is also *positive.*

15. *Gudgeon.*—The teeth of the fibres are very distinct. I have observed only one series of luminous sectors, which are *positive.*

16. *Cat-fish.*—This is a poisonous fish from Georgia. The teeth of the fibres are very fine.

17. *Red Trout.*—Its lens was nearly spherical, and 0·244 of an inch in diameter. It distinctly depolarized *three* series of luminous sectors, the inner and outer ones being *negative,* and the intermediate one *positive.* The teeth of the fibres are very distinct.

18. *Hickory Chad.*—This fish is from Georgia. The fibres are distinctly seen.

19. *Cavala.*—This fish is from Georgia. On one side of the lens there are two short septa. The secondary colours produced by the teeth of the fibres are very distinct.

20. *Stingarie.*—This is a poisonous fish from Georgia. The lens is of an oblong form when viewed in the direction of the axis of vision. The two septa on one side are very long.

21. *Skip-jack.*—The teeth of the fibres are short, like those of quadrupeds.

22. *Black-fish from Georgia.*—The fibres of its lens are large, and the colours which they produce very finely seen near the septa.

23. *Sheep-head from Georgia.*—The fibres of its lens are unusually large*.

24. *Fish from Singapore.*—Mr. George Swinton sent me a number of the eyes of this fish, but the name has been lost. Its lens has the form of an oblate spheroid, the axis of which is the axis of vision. This axis is 0·60 of an inch, and the equatorial diameter 0·70. The secondary colours produced by the teeth are finely displayed.

§ 3. *On the Anatomical and Optical Structure of the Crystalline Lenses of Animals, particularly those of the Lion, Tiger, Horse, Ox, and other Quadrupeds.*

In the two preceding sections I have described the two simplest combinations of fibres which characterize the structure of the crystalline lens in animals. The lenses of birds possess the *first* or simplest structure; the lenses of fishes in general exhibit either the *first* or the *second* structure; and we shall now show that the lenses of the Mammalia in general, with the exception of the *hare* and the *rabbit,* and other quadrupeds of peculiar habits, are characterized by a *third* and a more complex structure, in which *three* septa diverge from each pole of the lens at angles of 120°, the

* See Philosophical Transactions, 1833, p. 329.

septa of the posterior surface bisecting the angles formed by the septa of the anterior surface.

This beautiful structure is shown in Plate V. figg. 1. and 2; fig. 1. representing the anterior, and fig. 2. the posterior, surface of the lens, which in quadrupeds is a real lens or lenticular solid, the curvature of the posterior surface having a shorter radius than that of the anterior surface.

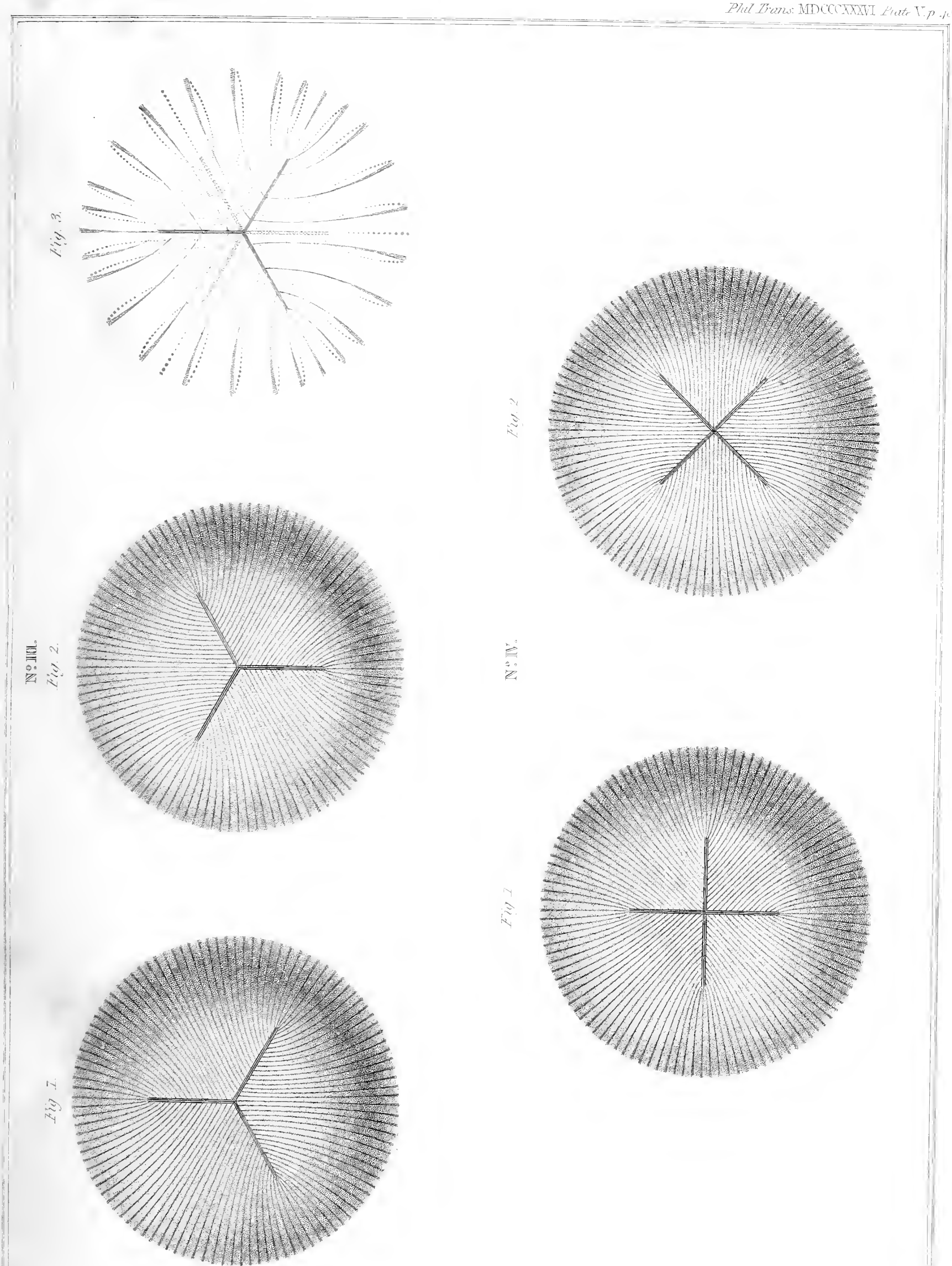
The progress of the fibres round the edges of the lens in their passage from the one surface to the other is shown in fig. 3, where the lens is supposed to be transparent, the dark continuous lines representing the *three septa* and the fibres of the anterior surface, and the dotted lines the three septa and the fibres of the posterior surface. From this representation it will be seen that there are *three* fibres having their origin in the anterior pole, and terminating at the extremities of the posterior septa; and other three having their origin in the posterior pole, and terminating in the extremities of the anterior septa, which have their parts all lying in one plane, while every other fibre of the lens forms a curve of contrary flexure, in order to carry it to its proper termination in the opposite septum. Hence it follows, that with the exception of the six fibres originating in the poles, the parts of all the other fibres which constitute the margin or rim of the lens are not parallel to its axis.

The arrangement of fibres shown in figg. 1. and 2. may frequently be seen, particularly in the lenses of old animals when they are large, by examining them in their entire and transparent state within their capsule. In some cases I have seen them distinctly by looking down upon the surface of the lens; but when they are visible in this way they may be seen to most advantage by looking through the surfaces with a small magnifier, when the lens is placed in a fluid of nearly the same refractive power.

In tracing the fibres to their termination in three *septa*, I employed the optical method already described in a preceding paper; but in general the superficial coloured images are not so distinct as those in the lenses of fishes, though, like them, they may be transferred to wax, and are like the colours of mother-of-pearl.

The fibres of the lenses of quadrupeds gradually diminish in size from the equator or margin of the lens, where they are a maximum, to their termination in the anterior and posterior septa. They are united together by small teeth, like those of fishes; but generally speaking the teeth are smaller and less distinctly pronounced; and in some lenses I have found it extremely difficult to exhibit them with the finest microscopes. As the teeth can only be seen in the indurated state of the fibres, it is probable that their form may be in a great measure altered or obliterated by the process of induration, especially when we consider that the lenses of quadrupeds are very much softer than those of fishes, and that the evaporation of the aqueous portion must produce a greater change upon the indurated albumen when it is most abundant.

The existence of three septa was observed by LEEUWENHOEK, and afterwards by SATTIG, in the lenses of the ox, the horse, the sheep, the goat, the fox, the dog, and



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No. IV.

Fig. 3.

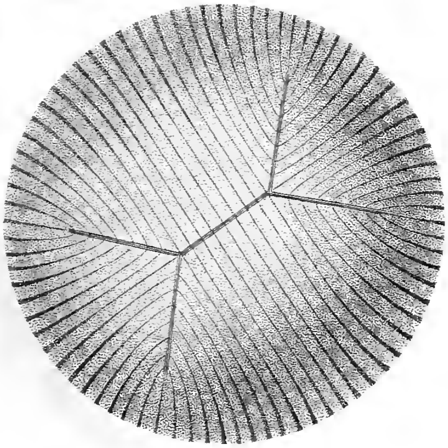


Fig. 4.

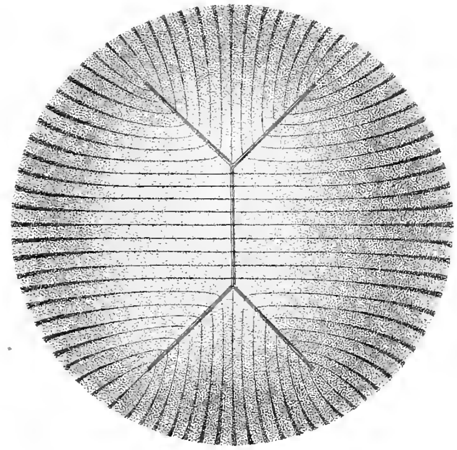


Fig. 5.

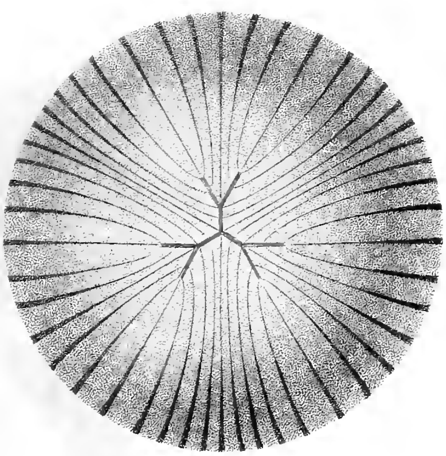
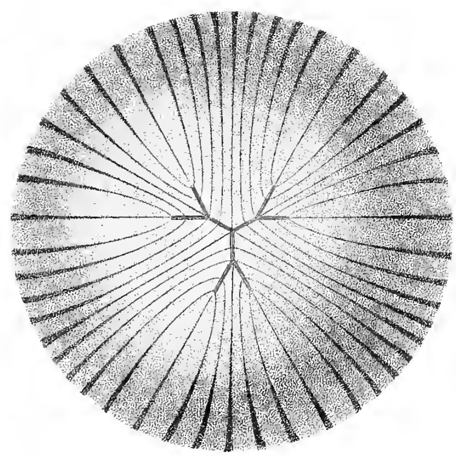


Fig. 6



No. V.

Fig. 1.

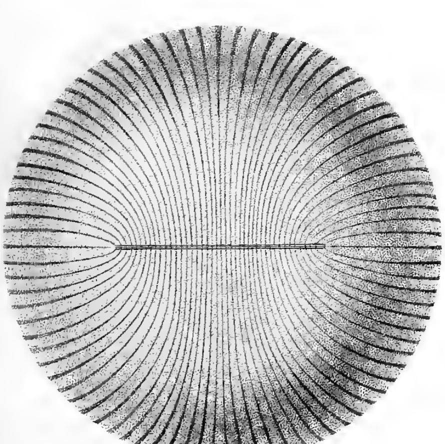
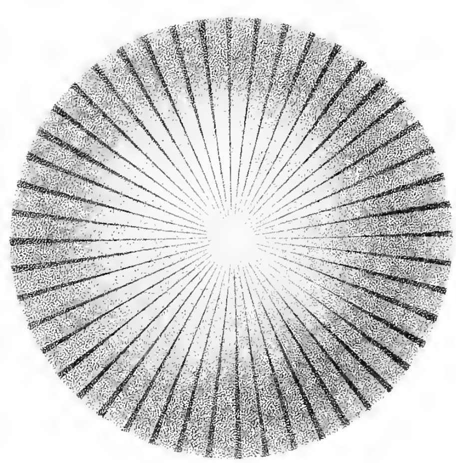


Fig. 2



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the cat; but LEEUWENHOEK committed the strange mistake of supposing that each coat or lamina of the lens consists of circumvolutions of a single fibre, whereas each fibre has a distinct termination in the septum at both its extremities. Dr. THOMAS YOUNG seems to have been occupied with the examination of the crystalline lens of the ox in 1792 or 1793, previous to the publication of SATTIG's thesis; and in his observations on Vision, read before the Royal Society on the 30th of May, 1793, he has given a drawing and description of the result of his observations*. This description is exactly the same as that previously given by LEEUWENHOEK, excepting that Dr. YOUNG erroneously maintains that each of the coats of the lens "consists of fibres intermixed with a gelatinous substance," which we presume he considered to be necessary for cementing the fibres into a compact body.

In Dr. YOUNG's subsequent and more elaborate paper on the Mechanism of the Eye†, he renounces as erroneous the description which he had given of the arrangement of the fibres in the crystalline of the ox; and he substitutes, in place of his former correct drawing, another, which is altogether visionary. "In man," says he, "and in the most common quadrupeds, the structure of the lens is *nearly similar*. The number of the radiations is of little consequence; but I find that, sometimes at least, in the human crystalline there are *ten* on each side, not *three*, as I once, perhaps from a too hasty generalization, concluded. Those who find any difficulty in discovering the fibres must have a sight very ill adapted to microscopical researches." Notwithstanding the assertion in this passage that the structure of the lens in man and the most common quadrupeds is *nearly similar*, yet fig. 93, in which he represents "the order of the fibres of the human crystalline," is essentially different from fig. 95, in which he shows the "ramifications (of the fibres) from the margin of the crystalline lens" in quadrupeds. In the first of these figures there are no ramifications of the fibres from the margin of the lens, whereas in the second there are no fewer than six, placed without any symmetrical relation either to the septa or to one another.

The following Table contains the names of the different animals in whose lenses I have found the structure shown in Plate V. figg. 1, 2, and 3. These animals are all quadrupeds, with the exception of the anonymous fish caught near the Azores, to which I shall have occasion again to refer‡.

Lion.	Ox.	Goat.
Tiger.	Cow.	Sow.
Horse.	Sheep.	Deer, Fallow.

* Elements of Natural Philosophy, vol. ii. p. 525. "In examining," says he, "the crystalline from an ox, I discovered a structure which appears to remove all the difficulties with which this branch of optics has long been obscured."

† Elements of Natural Philosophy, vol. ii. p. 597.

‡ For the lenses of several of the rarer animals contained in the following Table, I have been indebted to the liberality of the Zoological Society of London.

Deer, Roe.	Dog.	Baboon.
—— Indian.	Cat.	Monkey, Dourouecouli.
—— Moose.	Otter.	——— Blaek Spider.
Buffalo, Wild.	Rat.	——— Entellus.
Nilgao, India.	Mouse.	——— Green.
Llama.	Opossum.	——— Lesser White-nosed.
Puma.	Squirrel.	——— Jaeketed.
Cheetah.	Lemur, Blaek.	Vieugua.
Antelope, Melampus.	——— Red-fronted.	Capybara.
——— Pygmy.	——— Noeturnal.	Chinehilla.
Fox, Common.	Coati, Brown.	Iehneumon.
—— Blaek, from Ameriea.	Suricate.	Fish eaught near the Azores.

1. *Lion.*—The posterior surfaee of the lens is the most eonvex. The fibres whieh compose the lens are exeeedingly distinet, and the teeth smaller and sharper than those of fishes.

2. *Tiger.*—The fibres and the teeth whieh unite them are very distinet, and like those of the lion. In a lens preserved in spirits, and deprived of its external lamina, the lamina shone with all the ineommunieable eolours of mother-of-pearl. When the lens was dried these eolours disappeared.

3. *Horse.*—When the lens of the horse is thrown for a second or two into boiling water, and is allowed to dry slowly, it splits in sueh a manner as to show the septa and the general strueture of the lens very satisfaetorily. The fibres are well seen, and the teeth distinet and small like those of the lion and the tiger.

4. *Llama, Puma, Capybara,* &c.—The fibres and teeth are exeeedingly distinet in the lenses of these animals, and equally or less so in all the other animals in the Table.

5. *Cat.*—In order to observe if the fibrous strueture was the same in the young animal, I took the eyes of three kittens, six in number, about eighteen hours after birth; and what was very remarkable, I found that the lenses in all the six were white and opake, with a perfeetly transparent rim. The three septa were distinetly seen in all the lenses. It will be interesting to aseertain, if in all animals whieh are born blind, the crystalline is opake at their birth, and gradually beeomes transparent from the margin to the eentre during the period in which the eye is elosed against the admission of light.

6. *Chinchilla.*—The lens of the ehinehilla seems to fill the whole ball, and the eornea is exceedingly large, forming almost a hemisphere.

7. *Suricate.*—The eornea of the surieate is nearly of the same size as that of the chinchilla.

8. *Unknown Fish from the Azores.*—The lens of this fish, whieh was sent to me without a name by Mrs. Green of Cumberland Island, is in every way remarkable. It is the only lens of a fish whieh has the structure belonging to quadrupeds; and it is equally peculiar in having the form of a prolate spheroid, the axis of whieh eoin-

cides with that of the eye. The length of the axis is 0·327, and its equatorial diameter 0·273 of an inch.

In some quadrupeds I have observed an irregularity in the septa, which may have arisen either from an original malconformation of the lens, or from some accidental injury. In the lens of a horse I observed a spurious septum, one of the three being double. The same fact will be more particularly noticed in describing the lens of the elephant, in which it is more common.

§ 4. *On the Anatomical and Optical Structure of the Crystalline Lenses of Animals, particularly those of the Whale, the Seal, the Bear, and the Elephant.*

From the lenses of quadrupeds in which the fibres are related to three septa, I shall now proceed to describe the fourth and last class of symmetrical structures, in which the fibres are related to four septa, placed at right angles to one another. This combination of fibres is of rare occurrence, and I have found it only in the lenses of the *whale,* the *seal,* and the *bear.*

The character of this structure will be understood from Plate V. figg. 1 and 2, where fig. 1 represents the anterior surface, and fig. 2 the posterior surface, of the lens. The septa on the posterior surface are inclined 45° to those on the anterior surface, so that if the lens were transparent, the septa when seen at the same time would appear like the eight radii of an octagon, inclined 45° to one another. In this structure there are eight fibres, all the parts of which lie in one plane, passing through the axis of the lens; namely, *four* extending from the extremities of the four anterior septa and terminating in the posterior pole, and other *four* extending from the extremities of the *four* posterior septa and terminating in the anterior pole. All the other fibres of the lens, except these *eight,* are curves of contrary flexure, which necessarily change their direction in passing from one septum in the one face to another septum in the other inclined 45° to it.

The structure which I have now described is exhibited in the crystalline lenses of the *whale* and the *seal.* I found it distinctly developed in the lens of a whale, forty-six feet long, caught by Captain Ross in his first voyage in the Arctic regions, and also in a specimen of the great seal, or *Phoca barbata,* which Lieutenant Robertson of the Isabella brought home from Baffin's Bay in the same year.

In the lenses of other whales and seals, however, I have found a different structure, which is represented in Plate VI. figg. 3 and 4. This combination of fibres differs from that in figg. 1 and 2, in having two centres of divergence in place of one in each surface of the lens; but if we conceive these two points to coincide, the two structures become identical. In both, the principal septa are at right angles to each other on the same face, and inclined 45° to those on the opposite face; so that the general character of the two structures is the same.

In the lenses of one of the bears killed by Captain Ross during his first voyage, I found the structure shown in figg. 2 and 3. The distance between the centres of divergence was greater on one side of the lens than on the other, in both lenses; and in one it was so small as to show very nearly the structure in figg. 1 and 2.

In the lenses of other whales, one of which was thirty-five feet long, I found the structure shown in figg. 1, 2, 3, and 4, in which there are *four* radiations of fibres, or vortices, as I believe Leeuwenhoek calls them, on each surface of the lens; but I have found in one lens a spurious structure, in which there are *five** radiations on one face and *three* on the other face of the lens.

When the lens of the whale has been preserved in spirits, the coats have often a brilliant pearly lustre, a phenomenon which I have seen with equal beauty in the lenses of some quadrupeds, especially in that of the tiger. The fibres of the lens of the whale are extremely distinct, and the teeth upon them, which are visible with high powers, resemble those in the fibres of quadrupeds.

The crystalline lens of the *elephant* possesses many remarkable peculiarities. The following are the dimensions of the eyeball of an elephant, which Mr. George Swinton was so kind as to send me from Bengal.

Eyeball.

	Inches.
Longest diameter	1·82
Shortest diameter	1·10

Cornea.

Longest diameter	1·35
Shortest diameter	0·92

Crystalline Lens.

Longest diameter	0·700
Shortest diameter	0·627
Thickness	0·400

Ratio of the two diameters, 1 to 1·1125.

In another eye the dimensions of the lens were as follow:

Crystalline Lens.

	Inch.
Longest diameter	0·784
Shortest diameter	0·700
Thickness	0·450

Ratio of the two diameters, 1 to 1·116.

* Leeuwenhoek says that there are *five* septa in the lens of the whale; but I have not his paper beside me in order to ascertain whether he means by this five radii inclined 72° to each other, or *five* radiations such as I have found in the spurious structure here mentioned.

Hence it is obvious that the crystalline lens of the elephant differs from that of most other animals, in being of an *elliptical* form, the horizontal diameter of the ellipse being the longest.

When the lens has been preserved in spirits, and the outer coats are removed, it resembles a piece of the finest amber. In this condition, when dry, it does not crack, like other lenses, but exfoliates in thin scales, which give it the appearance of a flat pearl. When these scales are rubbed off it resumes its appearance of amber, and when well dried, after two or three exfoliations, it becomes as permanent in its appearance and colour as that substance, and almost as hard and durable.

This peculiar property of the elephant's lens arises from the peculiar structure of its coats or laminæ. These coats are not, properly speaking, composed of fibres, but are of a fibrous tissue, the elementary fibres of which are not united mechanically by teeth, but by some other process, probably that of agglutination, which I cannot discover by the finest microscopes which I possess *.

Owing to this structure of the laminæ, the superficial colours are not displayed, as in other lenses, and it is very difficult to trace the elementary fibres into the septa, to which they are related. I have succeeded, however, in determining that there are *six* radiations of fibres and *three* centres of divergence on each surface of the lens.

This structure is shown in Plate VI. figg. 5 and 6, in which there are *three* septa diverging from the poles of the lens, as in quadrupeds, and from the extremity of each *two* additional septa, which are the real septa, to which the fibrous radiations are principally related. The three central septa are inclined 120° to each other, and the two additional septa seem to be inclined at an angle of 60° to each of the central ones; but these measures are of course only rude estimates of the inclination of lines, which in animal and vegetable organizations, and even in those of the mineral world, approximate only to the mathematical type of their characteristic structure.

In the combination of fibres shown in figg. 5 and 6, there are twelve fibres whose parts all lie in the same plane, all the rest forming curves of contrary flexure.

In some lenses of the elephant, I have found the three septa which meet in the poles of the lens exceedingly small, and approaching to evanescence; and I have no doubt that, as happens in the case of four septa, these three central septa will in some lenses be wanting; so that the other six septa will diverge from the poles at angles of 60°, like the radii of a hexagon. Such a lens will bear the same relation to the structure shown in figg. 5 and 6 as the structure in figg. 1 and 2 bears to that in figg. 3 and 4.

* In extremely thin and highly-dried fibres, the fibres are better seen with the microscope; and I have observed something like a mechanical union of them.

§ 5. *On the Structure of the Crystalline Lens of the Turtle and other animals in which the Fibres are differently combined in the anterior and posterior Surfaces.*

In the various structures of the crystalline lens which have been described in preceding communications, the fibres are similarly arranged on both the surfaces of the lens, whether these two surfaces are similar, or have different degrees of convexity. In this respect they resemble the artificial lenses of the optician, in which there is no other deviation from symmetry but in the curvature of their surfaces. I have discovered, however, in the lens of the turtle, and in that of several fishes, a new combination of fibres, in which they are differently arranged in the anterior and posterior faces of the lens.

Ever rich in her forms and fertile in her resources, Nature thus presents to us in the crystalline lens four singular properties, which the most skilful optician, even if he knew their design, is not likely ever to attempt to imitate. But the study of these properties is not on this account the less interesting; for though we may never be able to produce the same effect, either by similar or analogous means, yet we may be led to discover some other principle within the sphere of art by which the desired result may be obtained. The four properties to which I refer are the increase of density from the surface to the centre of the lens; the alternations of negative and positive structures, as exhibited by the action of the lens on polarized light; the arrangement of the fibres in reference to different numbers of septa; and the defect of symmetry in this arrangement in the turtle and a few fishes. The first of these properties, namely, a variation of density, is no doubt intended to correct spherical aberration, an effect which may be produced by the union of several spherical surfaces, or by hyperbolical or elliptical surfaces, or by surfaces of contrary flexure; but the design of the other three properties has not even excited the ingenuity of conjecture, and will probably remain among the numerous problems which will exercise the sagacity of another age.

When I first observed a defect of symmetry in the arrangement of the fibres in the two halves of the lens of the turtle, I was extremely doubtful of the accuracy of my observations, and was therefore at peculiar pains to confirm the result by examining several lenses of the turtle. In every lens, however, I found the deviation from symmetry was clearly indicated, though it did not possess the same character in every lens which came under my notice.

In the eye of the *turtle* which I first examined, the ball was one inch in diameter, and the diameter of the lens only 0·200 of an inch. The lens was nearly spherical; and it had on its anterior face *two* septa, like the hare and the salmon, as shown in Plate VI. fig. 1; but on its posterior face the fibres converged to a single pole, as shown in fig. 2. In this structure there are only four fibres in each lamina, which have their different parts lying in the same plane. All the other fibres are concave towards a plane passing through the two anterior septa, and of course convex towards a plane at right angles to it, so that in this structure the fibres exhibit no contrariety of flexure, as in all the other lenses with septa at each pole. The very same structure appeared in the other lens of the same turtle; and was found likewise in both the lenses of another turtle, in which the diameter of the eyeball was 1·2 of an inch, and that of the lens 9·23.

The fibres and their teeth are nearly the same in the turtle as in the lenses of quadrupeds, the teeth being very short, though perfectly distinct.

The structure represented by figg. 1 and 2 (Plate VI.) is possessed by the lenses of the following fishes :

The Drune.	Crocer.	Grey Gurnard.
Bass.	Angel Fish.	Red Gurnard.
Whiting from Georgia.	Mullet? from Georgia.	

1. The *Drune*, or *Drum*, from Georgia.—The lenses of this fish, along with those of the other fishes in the above table, except those of the gurnard, were sent to me with their common names by Mrs. GREEN of Cumberland Island. In both the lenses of the drune the different structures were very distinctly seen. The fibres varied greatly in diameter as they approached the pole; and though the secondary colours were not visible, yet the teeth of the fibres were beautiful and distinct.

2. The *Bass* from Georgia.—In this lens the double arrangement of the fibres is distinctly seen.

3. The *Whiting* from Georgia.—The same structure is clearly seen in both the lenses of this fish.

4. The *Crocer* from Georgia.—In both the lenses two short septa are seen in the anterior face, and a single pole in the posterior one. The teeth of the fibres are very close, but distinct.

5. The *Angel Fish* from Georgia.—In the lenses of this fish the two structures are very distinct, and also the teeth of the fibres.

6. *Mullet* from Georgia.—I am in some doubt respecting the existence of the double structure in the lens of this fish. The observation which I made many years ago is thus recorded: "A sort of diffused polarity round two poles, as if there were two septa on each side, like those of the hare, or on one, like that of the turtle." As a diffused polarity was the only fact actually observed, I find that I have placed the mullet in the list of fishes without septa.

7, 8. *Red* and *Grey Gurnard*.—In the lenses of both these fishes there were two short septa on one side and none on the other.

In examining the lens of a very large turtle, in which the eyeball was 1·4 of an inch in diameter, and that of the lens 0·25 of an inch, I found to my great surprise that there were three septa in front, as in quadrupeds, and *four* septa, or rather *six*, as in the seal. The following is the account of the observation which I recorded at the time: "I saw distinctly the above septa on both sides when the lens was fresh and transparent, and plunged in oil of almonds, and I confirmed this result by boiling the lens and removing the laminæ. The four septa require nice observation to be seen. One of the *three* was inclined about 45° to one of the *four* septa. The other lens of this turtle has most distinctly four septa, but *only two on the other side*." A similar variation of structure has already been referred to as existing in the lenses of the horse and the whale.

Belleville by Kingusie,
November 14*th*, 1835.

VII. *On an Artificial Substance resembling Shell: by* LEONARD HORNER, *Esq. F.R.SS. Lond. & Edinb. With an Account of an Examination of the same: by Sir* DAVID BREWSTER, *LL.D. F.R.S. &c.*

Received October 5, 1835,—Read February 25, 1836.

WHILE I was, some time ago, officially inspecting the cotton-factory of Messrs. J. FINLAY and Co., at Catrine, in the county of Ayr, on going over the bleaching-establishment attached to it, I was struck with an unusual appearance of a part of the machinery, which, at a distance, looked as if it were made of brass. On a closer examination, I found that it was a large circular wooden box coated with an incrustation of a brown compact substance, having a highly polished surface, a metallic lustre, in some places beautifully iridescent, and when broken exhibiting a foliated texture *. This resemblance in structure and pearly lustre to some species of shells, such as the *Meleagrina, Malleus, Avicula, Ostrea, Pinna,* and others, induced me to examine the substance more closely, conceiving that it might possibly throw some light on the formation of shell.

The part of the machinery on which I observed the incrustation is called a Dash-wheel, and consists of a circular box, about seven feet in diameter and three feet in width, revolving upon a horizontal axis, and having its interior divided into four compartments, into each of which there is a circular opening on one side. The purpose of this wheel is to wash or rinse the cloth in pure water, after it has been boiled or steeped in the bleaching-liquors. It makes twenty-two revolutions in a minute, which is found to be the proper degree of speed, in order that the cloth may be tossed about and *dashed* against the sides as the wheel turns; a greater velocity causing it to keep at the circumference without shifting its position.

I was told that the incrustation was a deposit of carbonate of lime, and the source of the lime was mentioned. But whence the brown colour, and the metallic nacreous lustre? If the substance were analogous to shell, it ought to contain animal matter; and whence could that be derived? It was necessary to trace the operations from the beginning.

The cotton cloth is brought to the bleach-field in the state in which it is taken from the weaver's loom. The first process is to steep it in water for several hours, after which it is immersed in cream of lime. This is made in the following manner: fresh-burned lime is slaked and passed through a fine sieve, and added to water in the proportion

* Specimens are deposited at the British Museum.

of 38 lbs. of dry lime to 1000 lbs. of cloth. The cloth is boiled in this liquor from four to six hours, the lime acting as an alkali; and it is used only from being considerably cheaper than potash or soda. After this boiling, the cloth is taken to the dash-wheel to be thoroughly cleared of the lime, which is effected by its being tossed about for ten minutes in clear water in the interior of one of the compartments into which the wheel is divided. Here, then, is the source of the calcareous matter of the incrustation; and we have the lime dissolved or suspended in the water in a state of extremely minute division, and from which it is deposited, most probably, by a partial evaporation. It is difficult to say whether the deposit takes place while the wheel is revolving, by the water being broken into a kind of spray, and so presenting a greater surface for evaporation, or during the night, when the wheel is still: some of the properties, to be afterwards described, render the latter supposition the most probable. But in whatever way it takes place, the operation is an exceedingly gradual one; for the wheel had been in constant use for ten years, and the coating in the interior did not exceed one tenth of an inch in thickness. It had been in operation about two years before any perceptible deposit showed itself in the inside; but it had not been going half a year before an incrustation began to be formed on the outside of the wheel. I remarked that the deposit was in greatest quantity around the orifice where the cloth is put in and taken out. The deposit in the interior, and which coated the whole surface of the compartment, was of a darker brown colour, and was as smooth and splendent as a lining of highly polished bronze would have been. The high polish is no doubt partly produced by friction; and I observed that it was highest on that part of the outside nearest the opening.

So far we have *calcareous*, but no *animal*, matter; but in going a little further back in the history of the process to which the cotton had been subjected, before it came to the bleach-field, I discovered that animal matter might be contained in the incrustation. I learned that the cloth had been woven in power-looms; and on making inquiry as to the composition of the dressing or paste used to smooth and stiffen the warp before it is put into the loom, I was told that in the factory from whence the cloth had come, it is the practice to mix glue with the wheaten flour, generally in equal proportions by weight.

We have thus lime and gelatine, the same materials which are employed by the molluscous animal in the formation of its covering, and apparently in the same degree of minute division as that in which they are exuded from its mantle.

Chemical examination of the Substance.

1. *The external deposit.*—Exposed to the flame of a wax candle, it blackens, and gives out the usual smell of burning animal matter, the thin laminæ of which it is composed separating and curling up like films of horn; appearances similar to those exhibited by membranous shells when heated. When the flame is urged by the blowpipe, the laminæ separate still more, and are changed into an extremely light and

brittle enamel, pure white, and having a pearly lustre. A fragment moistened on the back of the hand gives a sensation of heat, as quicklime does when so treated. The substance, when thrown into dilute muriatic acid, is entirely dissolved; the fluid is tinged yellow, and the effervescence produces a froth, like beer. When the acid is very much diluted, and a portion of the substance is suspended in it, the solution takes place gradually, minute flocculi of animal matter being separated, and floating in the fluid.

2. *The internal deposit.*—This is separable into extremely thin laminæ, and these, when in small fragments, are hardly distinguishable from scales of brown mica, showing also the most beautiful play of colours. The action of heat produces the same effect as on the external deposit, except that the separated laminæ are thinner. The action of muriatic acid is the same, but the yellow tinge is deeper, and the froth is more permanent, indicating a larger proportion of animal matter than in the other. The nacreous lustre is also much more conspicuous in this.

Mr. Gray, in his paper on the Structure of the Shells of Molluscous Animals, observes that the pearly or iridescent lustre appears to be confined to shells of the concretionary structure, which when broken exhibit a nearly uniform texture, but separate when heated into numerous thicker or thinner laminæ; and he adverts to the observation of Mr. Hatchett, that when they are digested in weak muriatic acid, the lime is dissolved, leaving a great number of thin plates of animal matter, which retain the original shape of the shell. He adds, "This variety of structure is found to constitute the whole shell of the *Anomiæ* and *Placunæ*, and to form the inner coat of those shells which have pearly insides, as the *Turbines*, *Haliotides*, *Uniones*, &c., as well as the laminar portion of the *Pinnæ* and Mother-of-pearl shells *."

Besides the laminated structure, there is, in the case of the Pinna and some other shells, a prismatic crystalline arrangement of the particles perpendicular to, and passing uninterruptedly through, the laminæ; but I have not discovered such an arrangement in any portion of the incrustation, even when examined by the microscope.

I felt very desirous that this singular deposit should be examined by Sir David Brewster; the more especially as he had long since directed his attention to the peculiar structure of mother-of-pearl †. On showing him the specimens in my possession he observed, that it was one of the most remarkable artificial productions he had seen; and he readily undertook to examine it carefully. He shortly afterwards sent me the particulars of that examination, which had afforded some curious and interesting results. Having subsequently visited Catrine, I procured more perfect specimens; and I sent these to Sir David Brewster, in order to ascertain whether they might not afford something new, in addition to the results he had obtained from the fragments he had formerly examined. They did so, and I now subjoin the very interesting account which Sir David has given me of the properties he has discovered in this new substance.

* Philosophical Transactions, 1833, p. 794.

† Id., 1814.

My dear Sir, Belleville, January 1st, 1836.

In the communication which I had the pleasure of addressing to you on the 20th of January 1835, I gave a brief account of the observations I had made on the highly interesting substance which you had put into my hands; but as the specimens which you sent me a few days ago are so much superior to those with which I made my former experiments, and have led me to some new and I think rather extraordinary results, I shall include in the present letter all my former observations.

The substance in question does not resemble in its general aspect any natural or artificial production which I have seen. It is, generally speaking, brown where the surface is not iridescent, and in very thin plates: it is almost perfectly transparent, with a slight yellowish brown tinge like plates of glue or lac of the same thickness. The laminæ of which it is composed are sometimes separated by vacant spaces, at other times slightly coherent, but generally adhering to each other with a force greater than that of the laminæ of sulphate of lime or mica, and less than those of calcareous spar. When the adhering plates are separated, the separated surfaces are sometimes colourless, especially when these surfaces are corrugated or uneven; but they are almost always covered with an iridescent film of the most brilliant, and, generally, uniform tint, which exhibits all the variety of colours displayed by thin plates or polarizing laminæ.

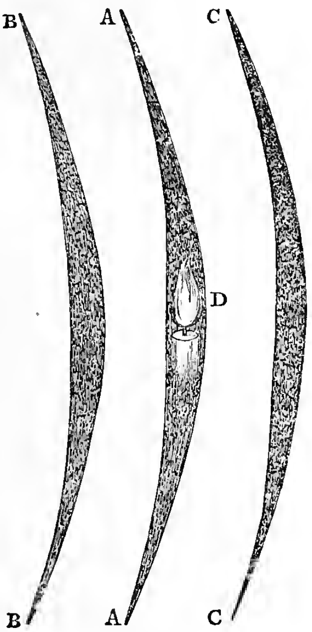
The substance is of intermediate hardness between calcareous spar and sulphate of lime. It scratches the latter easily, and is not scratched by mother-of-pearl. Its specific gravity is shown in the following Table, which indicates its relation to analogous substances.

Calcareous spar	2·72
Oriental pearls	2·68
New substance	2·44
Mother-of-pearl	2·19
Oyster-shell	2·02

The new substance has the property of refracting light doubly, like most crystallized bodies; and, as in agate, mother-of-pearl, &c., one of the two images is perfectly distinct, while the other contains a considerable portion of nebulous light, varying with the thickness of the plate and the inclination of the refracted ray. It has one axis of double refraction, like calcareous spar, which is negative, as in that mineral, and, like it also, it gives a beautiful system of coloured rings by polarized light. The double refraction of the substance is very considerable, though greatly less than that of calcareous spar. A plate, one seventy-fifth of an inch thick, makes the first red ring of the system *eight* inches in diameter at a distance of twenty-six inches from the eye. The substance belongs to the rhombohedral system, and, as in the *Chaux carbonatée basée* of Haüy, the axis of the rhombohedron, or that of double refraction, is perpendicular to the surface of the thin plates. As mother-of-pearl has two axes of double refraction like aragonite, this new substance may be considered as having the same optical relation to calcareous spar that mother-of-pearl has to aragonite.

When we look through a plate of this substance perpendicularly to its surface, or along the axis of double refraction, the flame of a candle is seen encircled with a nebulous haze like a halo. By the slightest inclination of the plate in any azimuth whatever, three elongated and curved nebulous images are seen, as in fig. 1., the central one, A A, having a distinct image, D, of the candle in the middle of it, and the other two, B B and C C, being equidistant from A A. These elongated images are parallel and concave towards the end of the plate nearest the eye. In the direction of the axis of double refraction, when all the nebulous light is in one mass, the distinct image, D, is redder than in any other direction; and by slightly inclining the plate the red light disappears, and the distinct image becomes brighter and whiter. All the three images, A A, B B, and C C, are united into a mass round D, at a perpendicular incidence, but they separate upon inclining the plate, and their distance increases with the inclination.

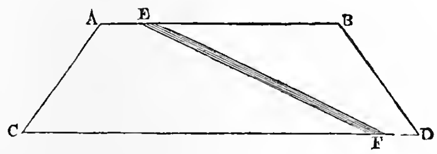
Fig. 1.



If we examine the nature of the light of which these images are composed, we shall find that the nebulous images, A A, B B, are wholly polarized in a plane passing through the direction of their length, while C C and the greater part of D are polarized in an opposite plane. As the thickness of the plate increases, more and more of the distinct image, D, is polarized in the same plane, as in mother-of-pearl*, till at a certain thickness the whole of it is thus polarized. In this case all the doubly refracted light which forms the nebulous image, A A, and the bright one, D, consists of two oppositely polarized pencils, the one forming the nebulous and the other the distinct image.

In investigating the cause of these phenomena, we must take as our guide the analogous facts presented by certain composite crystals of calcareous spar. Having long ago described this class of phenomena very fully†, I shall only state at present the general fact. Let A B C D, fig. 2., be a section of a rhomb of calcareous spar having its axis perpendicular to the faces A B, C D, and let E F be another crystal, or vein of the same substance crossing it according to the law of crystallographic composition.

Fig. 2.



If we now look at a candle through this compound crystal, it will appear single in the direction of the axis of A B C D; but if we incline the plate in a plane passing through A B, we shall see two images together as at A and D, fig. 3., and other two, namely one at B

Fig. 3.

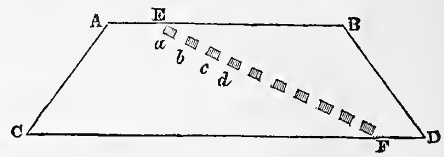
* See Philosophical Transactions, 1814.

† Id., 1815. Edinburgh Encyclopedia, art. Optics.

and the other at C. These images B, C, separate by the inclination of the plate exactly like those in fig. 1., and all the four, A, B, C, and D, have the same absolute and relative polarization as the four analogous images seen through the new substance, with this difference only, that none of them are nebulous.

If we conceive the vein E F to consist, as in fig. 4., of a great number of small crystals, *a*, *b*, *c*, *d*, &c. in place of one, the very same effects will be produced.

Fig. 4.



When we look through the new substance, the multiplication of images takes place *in whatever azimuth* we incline the plate, the elongated images being always perpendicular to the azimuth of inclination. Hence it follows, that these images are produced by *numbers of minute crystals* lying in or near the azimuth in which the plate is inclined; and that these crystals have their axes all inclined to that of the plate which contains them, at the same angle as the vein E F, figg. 2. & 4., is inclined to the axis of the rhombohedron of Iceland spar. But the remarkable result of these observations is, that in place of one set of crystals, or sometimes three sets, which occur in calcareous spar in three different azimuths, we have here *an infinite number of them* lying in every possible azimuth, and these so small in their dimensions that they cannot be recognised by the most powerful microscopes, except as dark specks disseminated through the general mass; and yet they indicate by their action on light, not only their existence, but the position of their axes, and their doubly refracting and polarizing structure, as unequivocally as if we could handle them, and cleave them, and place them upon the goniometer.

It may now be asked why the images are nebulous, and not distinct as in calcareous spar. The reason is, that the substance is imperfectly crystallized like the agate, mother-of-pearl and other bodies in which the doubly refracting force separates the incident light into two oppositely polarized pencils, which are not perfectly equal and similar, but which differ from each other, sometimes in the intensity of their light, sometimes in the distinctness of the image, sometimes in the nature or brightness of the colour, and sometimes in more than one of these characters. But though the new substance resembles the crystals above mentioned in giving dissimilar pencils of doubly refracted light, it stands unique among all bodies with which I am acquainted in possessing the extraordinary system of composite crystallization, in which an infinite number of crystals are disseminated equally in every possible azimuth through a larger crystalline plate, having their axes all inclined at the same angle to that of the larger plate, and producing similar phenomena in every direction, and through every portion of the plate; or we may describe this remarkable structure by saying that the minute elementary crystals form the surfaces of an infinite number of cones whose axes pass perpendicularly through every point of the larger plate*.

* A rude idea of this structure is given by the beautiful cones, or rather pyramids of microscopic crystals of titanium which I have somewhere described as existing within the pyramids of many crystals of amethyst from the Brazils.

The iridescent phenomena exhibited by the new substance are extremely interesting, and I have been at much pains to examine them in a great number of specimens. The plates into which the substance is divisible have been formed in succession, and certain intervals of time have elapsed between their formation. In general every two contiguous laminæ are separated by a thin iridescent film, varying from the three to the fifty millionth part of an inch in thickness, and producing all the various colours of thin plates which correspond to intermediate thicknesses. Between some of the laminæ no such film exists, probably in consequence of the interval of time between their formation being too short; and between others the film has been formed of unequal thickness, as happens in the oxidations upon steel when they are formed upon or around hard parts of the metal called *pins* by the workmen.

There can be no doubt that these iridescent films are formed when the dash-wheel is at rest during the night, and that when no film exists between two laminæ, an interval too short for its formation (arising perhaps from the stopping of the work during the day,) has elapsed during the drying or induration of the one lamina and the deposition of the other.

That these iridescent films are not thin films of the substance itself, may be inferred from the fact that light is reflected from their surfaces when they firmly adhere to the laminæ which inclose them. If, for example, we remove or raise up from a piece of mica a thin film which gives a bright green tint, and press it again into optical contact with the surface from which it was separated, it will then cease to exhibit any colour, because no light is reflected from its posterior surface; but if we press it into optical contact with another surface which has a different refractive power, its green colour will still be exhibited. It is owing to this cause that the colours of the oxidations on steel are so distinctly visible, and that the analogous oxidations are seen upon glass even before the film has begun to separate into coloured scales.

The iridescent films in the new substance possess another source of interest, in so far as they promise to throw a new light on the origin of the *incommunicable* colours of mother-of-pearl, which arise from the interior structure of the shell, and which cannot therefore be communicated to wax. These colours have frequently occupied my attention since the year 1814, when I described the phenomena of the colours communicable to wax*; but though I have devoted much time to the inquiry, I never could obtain a single result worthy of being communicated to the public. I took plates of mother-of-pearl that exhibited different bright colours through different parts of their surface, and by getting the mother-of-pearl ground away in different places by the seal-engraver's wheel, I endeavoured to discover the thicknesses at which the colours were produced, and the cause of the capricious variation of tints which arose from every inclination of the plate: but all my experiments were fruitless, and I abandoned the subject as beyond my reach. The phenomena, however, presented by the new substance seem to me to disclose the secret of which I was in

* Philosophical Transactions, 1814.

quest. The layers of mother-of-pearl are deposited in succession like those which are formed upon the dash-wheel; and there can be no doubt that the animal whose mucous secretions form the shell that incloses it, rests occasionally from its toils, and affords a sufficient interval for the formation of an iridescent film upon the surface of the plate of shell which it daily deposits. Owing to the firm adhesion of the successive layers of the shell, we cannot, as in the more imperfectly formed new substance, separate each stratum in order to see the iridescent film upon their surfaces; but we can easily determine what phenomena would be produced if the layers of the new substance were as transparent as those of mother-of-pearl. If this were the case, we should see, both by reflected and transmitted light, the combined colours of all the iridescent films in the plate. When these films are numerous and flat, and of various thicknesses, the union of all their colours would form a pearly whiteness by reflected light, and when films of a particular colour predominate, both the reflected and the transmitted light would exhibit that prevailing colour; but if their surfaces are undulated as in mother-of-pearl, from the form of the shell and other causes,—if the iridescent films vary in thickness, and consequently in colour,—if they are wanting in some parts of the shell, and abound in others,—and if films of equal thicknesses occur in several laminæ in succession, and films of other thicknesses in other laminæ, which must necessarily take place from the varying and remitting action of the animal agent, then we shall have the very structure which is necessary for the production of the incommunicable colours of mother-of-pearl.

I have no doubt that this is the true cause of the phenomena which had so long perplexed me; and the results which I formerly obtained, though I could then attach no meaning to them, are in perfect unison with the preceding views. In order, however, to obtain something like an experimental confirmation of this opinion, I have examined the *fracture* of a mother-of-pearl shell where the laminæ have been all deposited with considerable regularity, and where their overlying edges are exhibited, and I find distinct and positive proofs of the existence of iridescent films, sometimes green, and sometimes red in several successive strata.

I am, my dear Sir,

Ever most truly yours,

D. Brewster.

To Leonard Horner, Esq.

VIII. *Discussion of Tide Observations made at Liverpool.* *By* J. W. LUBBOCK, *Esq. F.R.S.*

Received and Read January 28th, 1836.

I AM enabled, through the indefatigable perseverance of M. DESSIOU, to present to the Society other Tables, in continuation of those published in the Philosophical Transactions for 1835, Part II., founded upon the observations instituted by Mr. HUTCHINSON at Liverpool.

The chief intention of the Tables now offered is to exhibit the diurnal inequality in the height of high water, which is, I believe, insensible in the river Thames, but which at Liverpool amounts to more than a foot. So that, for example, in January, when the moon is in quadrature, (neap tides,) the evening tide may be a foot higher than the morning tide.

Table XXVII. gives the results as immediately deduced from observation.

Table XXVIII. was formed by reducing the argument, (moon's transit,) by interpolation, to the even half-hours, and then taking the differences between the numbers so found and those in Table III.*

The results exhibited in Table XXVIII. are extremely irregular: these irregularities were arbitrarily removed in forming Table XXIX., which is intended to be used in predicting the phenomena. As the question of the diurnal inequality of the tides is important, and as the numbers in Table XXVIII. are so irregular, I have thought it desirable to exhibit them in a diagram, together with the inequalities definitively adopted in Table XXIX., in order that the nature and extent of the alterations we have introduced may be perceived. The diurnal inequality in the *interval* appears to be insensible.

BERNOULLI's Theory of the Tides leads to the expressions

$$h = D + E\,\{A \cos(2\,\mu - 2\,\alpha) + \cos(2\,\mu - 2\,\alpha')\}$$

$$\tan 2\,\psi = \frac{A \sin 2\,\phi}{1 + A \cos 2\,\phi} \qquad A = \frac{m \cos^2 \delta\, P^3}{m' \cos^2 \delta'\, P'^3} \qquad E = C\, m' \cos^2 \delta\; P'^3$$

$$h = D + E\,\{\cos 2\,\psi + A \cos(2\,\phi - 2\,\psi)\},$$

in which expressions α denotes right ascension, μ sidereal time, δ declination, m the mass of the luminary, P the sine of the horizontal parallax, C a constant depending upon geographical latitude, and D a constant depending only on the zero line, from which the heights are reckoned. The unaccented quantities refer to the sun, and those which are accented to the moon. h is the height of the water above any given line, and ψ is a small variable angle to be added with a certain constant to the time of the moon's transit, in order to obtain the time of high water.

* Philosophical Transactions, 1835, p. 283.

The constant A has the same value for London and Liverpool, and I find for both places the mean value of log $A = 9{\cdot}5784858$. The following Tables, calculated by Mr. Jones, will assist in comparing results deduced from observation with Bernoulli's expressions.

	Semimenstrual Inequality.				Semimenstrual Inequality.		
φ.	ψ in Time.	Height of Tide.	φ.	φ.	ψ in Time.	Height of Tide.	φ.
°	+ m	feet.	°	°	+ m	feet.	°
0	0	22·81	180	45	42	21·44	135
5	6	22·80	175	50	43	21·15	130
10	11	22·75	170	55	44	20·86	125
15	16	22·65	165	60	44	20·57	120
20	21	22·52	160	65	41	20·28	115
25	26	22·36	155	70	37	20·02	110
30	31	22·17	150	75	32	19·79	105
35	36	21·95	145	80	24	19·61	100
40	40	21·71	140	85	13	19·49	95
45	42	21·44	135	90	0	19·44	90
	−				−		

The preceding Table has been calculated with log $A = 9{\cdot}5784858$ and log $E = 0{\cdot}6481648$.

Sun's Declination.

d ψ in Time.

	Decl. 0°.	Decl. 3°.	Decl. 6°.	Decl. 9°.	Decl. 12°.	Decl. 15°.	Decl. 18°.	Decl. 21°.	Decl. 24°.	
φ.	+						−			φ.
°	m	m	m	m	m	m	m	m	m	°
0	0	0	0	0	0	0	0	0	0	180
15	1	1	1	1	1	0	0	1	1	165
30	2	1	1	1	0	0	1	2	3	150
45	2	2	1	1	0	0	2	3	5	135
60	4	3	2	2	1	0	2	3	5	120
75	3	2	2	1	1	0	2	4	5	105
90	0	0	0	0	0	0	0	0	0	90
	−						+			

Moon's Parallax.

d ψ in Time.

	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.	
φ.	+				−				φ.
°	m	m	m	m	m	m	m	m	°
0	0	0	0	0	0	0	0	0	180
15	2	2	1	0	0	1	2	2	165
30	4	3	1	0	1	3	4	5	150
45	6	4	2	0	2	4	6	8	135
60	9	6	3	0	2	5	7	9	120
75	8	5	2	0	3	5	6	8	105
90	0	0	0	0	0	0	0	0	90
	−				+				

Moon's Declination.

d ψ in Time.

	Decl. 0°.	Decl. 3°.	Decl. 6°.	Decl. 9°.	Decl. 12°.	Decl. 15°.	Decl. 18°.	Decl. 21°.	Decl. 24°.	Decl. 27°.	Decl. 30°.	
φ.			−						+			φ.
°	m	m	m	m	m	m	m	m	m	m	m	°
0	0	0	0	0	0	0	0	0	0	0	0	180
15	1	1	0	0	0	0	1	1	2	2	3	165
30	2	2	2	1	1	0	1	2	3	4	6	150
45	3	3	3	2	1	0	1	2	4	6	8	135
60	3	3	3	2	1	0	2	4	6	9	12	120
75	3	3	3	2	2	0	1	3	5	8	11	105
90	0	0	0	0	0	0	0	0	0	0	0	90
			+						−			

Sun's Declination.

d *h*.

φ.	Decl. 0°.	Decl. 3°.	Decl. 6°.	Decl. 9°.	Decl. 12°.	Decl. 15°.	Decl. 18°.	Decl. 21°.	Decl. 24°.	φ.
°	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	°
0	+·12	+·11	+·10	+·08	+·04	·00	−·05	−·11	−·18	180
15	·11	·10	·09	·07	·04	·00	·05	·11	·17	165
30	·09	·08	·07	·05	·03	·00	·04	·08	·13	150
45	+·04	+·04	+·03	+·02	+·01	·00	−·02	−·04	−·06	135
60	−·02	−·02	−·02	−·01	−·01	·00	+·01	+·02	+·03	120
75	·09	·09	·09	·06	·03	·00	·04	·08	·12	105
90	−·12	−·11	−·10	−·07	−·04	·00	+·06	+·12	+·18	90

Moon's Parallax.

d *h*.

φ.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.	φ.
°	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	°
0	−·66	−·45	−·23	·00	+·24	+·49	+·75	+1·01	180
15	·66	·45	·23	·00	·24	·48	·73	0·99	165
30	·64	·44	·23	·00	·23	·47	·72	0·97	150
45	·62	·42	·21	·00	·22	·45	·70	0·95	135
60	·61	·42	·21	·00	·22	·45	·69	0·94	120
75	·64	·44	·22	·00	·23	·46	·71	0·97	105
90	−·66	−·45	−·23	·00	+·24	+·49	+·75	+1·01	90

Moon's Declination.

d *h*.

φ.	Decl. 0°.	Decl. 3°.	Decl. 6°.	Decl. 9°.	Decl. 12°.	Decl. 15°.	Decl. 18°.	Decl. 21°.	Decl. 24°.	Decl. 27°.	Decl. 30°.	φ.
°	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	°
0	+·32	+·31	+·27	+·21	+·12	·00	−·13	−·29	−·47	−·66	−·87	180
15	·31	·30	·26	·20	·11	·00	·14	·29	·47	·66	·87	165
30	·31	·30	·26	·20	·11	·00	·13	·28	·45	·63	·84	150
45	·30	·29	·25	·19	·10	·00	·13	·27	·44	·62	·81	135
60	·30	·29	·25	·19	·11	·00	·13	·27	·43	·60	·79	120
75	·30	·29	·25	·19	·11	·00	·13	·28	·45	·61	·79	105
90	+·31	+·31	+·27	+·21	+·12	·00	−·13	−·29	−·47	−·66	−·87	90

I have ascertained that Bernoulli's expressions present a very remarkable accordance with observation. But it must be recollected, that the phenomena of the tides at London and Liverpool cannot be considered as depending (mechanically) upon the coordinates of the sun and moon at the transit immediately preceding the times of high water; and if I had taken in my discussions a transit more remote, the law of the intervals would not have been the same. This difference in the law of the intervals depends upon the difference in the intervals between the successive transits of the moon. Hence the law of the intervals, when the discussion is instituted with reference to the transit immediately preceding the time of high water, whether at London, Liverpool, or Brest, depends partly upon the phenomena as deducible from Bernoulli's expressions, and partly upon the law of the intervals between the moon's successive transits, which latter interval may be considered roughly as depending upon her parallax and declination. For practical tables intended to serve in predicting the phenomena, this is of no consequence; but in order safely to compare the results of theory with those of observation, it is absolutely necessary first to obtain the law of the changes in the moon's motion, which may easily be done. This consideration does not apply to the heights.

I have little doubt from comparisons which I have made, and which I mean to extend, that the results of observation present a very remarkable agreement with Bernoulli's theory, and with the formulæ in p. 57, by which this theory is approximately represented. In this comparison, however, it cannot be expected that the phenomena at any given place are the same (as Laplace seems to imply*) as would belong to that geographical latitude, if the figure of the ocean were that of a perfect spheroid, and were not intersected by continents. I apprehend that the inequalities of the tides at Brest are produced *mechanically* some time previous, and are not due to the position of the luminaries (in a certain sense) at the time of high water.

Mr. Deacon has furnished me with a continuation of Tables A, B, C, D, for the last six months of the year 1835, which serve to show the degree of congruity in the observations at the London and St. Katherine Docks with each other and with the predicted tides in the British Almanac.

* "Si le port a une latitude, ces pleines mers pourraient être fort différentes; et quand la déclinaison des astres est égale à l'obliquité de l'ecliptique, la marée du soir à Brest serait environ huit fois plus grande que celle du matin."—*Mécanique Celeste*, tom. v. p. 148.

Table A.

Showing a comparison of the observations of the Times of High Water made at the London Docks, increased by five minutes, and those at the St. Katherine Docks. The observations marked with an * appear doubtful.

Date. 1835.	July.			August.			September.			October.			November.			December.		
	London Docks. +5 min.	St. Kath. Docks.	Difference.	London Docks. +5 min.	St. Kath. Docks.	Difference.	London Docks. +5 min.	St. Kath. Docks.	Difference.	London Docks. +5 min.	St. Kath. Docks.	Difference.	London Docks. +5 min.	St. Kath. Docks.	Difference.	London Docks. +5 min.	St. Kath Docks.	Difference.
	h m	h m	m	h m	h m	m	h m	h m	m	h m	h m	m	h m	h m	m	h m	h m	m
1.	5 30	5 27	+ 3	6 35	6 42	− 7	7 50	7 56	− 6	8 35	8 41	− 6	11 15	11 21	− 6	11 15	11 18	− 3
	5 45	5 42	+ 3	6 45	6 53	− 8	8 20	8 16	+ 4	9 20	9 46	−26	......	......	......	11 45	11 58	−13
2.	6 5	6 8	− 3	7 25	7 28	− 3	9 5	9 12	− 7	10 15	10 21	− 6	12 15	12 3	+12	......	......	......
	6 20	6 23	− 3	7 35	7 48	−13	9 45	9 42	+ 3	11 5	11 12	− 7	12 20	12 23	− 3	12 10	12 7	+ 3
3.	7 0	6 57	+ 3	8 20	8 35	−15	10 45	10 47	− 2	11 25	11 38	−13	12 50	12 58	− 8	12 45	12 48	− 3
	7 15	7 7	+ 8	8 50	8 48	+ 2	11 30	11 27	+ 3	......	......	......	12 50	12 58	− 8	12 55	12 54	+ 1
4.	8 0	7 57	+ 3	9 25	9 37	−12	12 5	12 12	− 7	12 15	12 22	− 7	1 25	1 32	− 7	1 20	1 24	− 4
	8 15	8 7	+ 8	10 5	10 4	+ 1	......	......	......	12 40	12 48	− 8	1 30	1 42	−12	1 35	1 32	+ 3
5.	9 0	8 21	+39	10 45	10 53	− 8	12 35	12 42	− 7	1 15	1 13	+ 2	2 0	2 8	− 8	1 55	1 55	0
	9 20	8 57	+23	11 15	11 25	−10	1 0	1 3	− 3	1 20	1 25	5	2 10	2 12	− 2	2 10	2 9	+ 1
6.	10 5	10 12	− 7	12 5	12 12	− 7	1 25	1 32	− 7	1 50	2 2	−12	2 25	2 38	−13	2 25	2 28	− 3
	10 25	10 27	− 2	......	......	......	1 50	1 42	+ 8	1 55	2 3	− 8	2 35	2 41	− 6	2 45	2 53	− 8
7.	11 5	11 13	− 8	12 25	12 31	− 6	2 25	2 22	+ 3	2 35	2 32	+ 3	2 50	3 2	−12	3 10	3 5	+ 5
	11 35	11 37	− 2	12 50	1 8	−18	2 30	2 28	+ 2	2 40	2 42	− 2	3 5	3 13	− 8	3 30	3 23	+ 7
8.	12 5	12 25	−20	1 25	1 33	− 8	2 50	3 8	−18	3 5	3 7	− 2	3 35	3 33	+ 2	3 45	3 37	+ 8
	......	......	......	1 45	1 58	−13	3 5	3 13	− 8	3 5	3 13	− 8	3 35	3 47	−12	4 5	3 57	+ 8
9.	12 35	12 36	− 1	2 15	2 27	−12	3 30	3 33	− 3	3 30	3 32	− 2	3 55	3 57	− 2	4 20	4 13	+ 7
	1 5	1 8	− 3	2 35	2 46	−11	3 30	3 34	+ 1	3 35	3 41	− 6	4 15	4 17	− 2	4 35	4 33	+ 2
10.	1 30	1 42	−12	3 25	3 18	+ 7	3 50	3 57	− 7	3 55	3 57	− 2	4 25	4 32	− 7	4 50	4 48	+ 2
	2 10	2 15	− 5	3 30	3 31	− 1	3 50	4 2	−12	4 0	3 52	+ 8	4 40	4 39	+ 1	5 10	5 16	− 6
11.	2 25	2 33	− 8	3 55	4 3	− 8	4 25	4 17	+ 8	4 25	4 21	+ 4	5 0	4 54	+ 6	5 25	5 32	− 7
	2 45	2 57	−12	4 10	4 8	+ 2	4 30	4 33	− 3	4 25	4 32	− 7	5 10	5 17	− 7	5 50	6 8	−18
12.	3 15	3 32	−17	4 35	4 32	+ 3	4 55	5 2	− 7	4 55	4 48	+ 7	5 35	5 23	+12	6 15	6 17	− 2
	3 35	3 47	−12	4 35	4 44	− 9	5 5	5 7	− 2	4 55	4 52	+ 3	5 50	5 57	− 7	6 35	6 27	+ 8
13.	4 15	4 22	− 7	5 5	5 14	− 9	5 20	5 26	− 6	5 20	5 22	− 2	6 15	6 13	+ 2	7 5	6 58	+ 7
	4 20	4 26	− 6	5 15	5 15	0	5 35	5 41	− 6	5 30	5 26	+ 4	6 55	7 2	− 7	7 40	7 38	+ 2
14.	4 50	4 55	− 5	5 45	5 48	− 3	6 0	6 6	− 6	5 55	6 2	− 7	7 25	7 28	− 3	8 5	8 9	− 4
	5 5	5 7	− 2	5 50	5 58	− 8	6 10	6 12	− 2	6 0	5 58	+ 2	8 5	8 7	− 2	8 55	8 57	− 2
15.	5 40	5 47	− 7	6 20	6 27	− 7	6 35	6 37	− 2	6 40	6 45	− 5	8 35	8 47	−12	9 20	9 27	− 7
	5 45	5 47	− 2	6 10	6 27	−17	6 45	6 52	− 7	7 15	7 21	− 6	9 40	9 37	+ 3	10 15	10 21	− 6
16.	6 20	6 31	−11	6 50	7 0	−10	7 20	7 22	− 2	7 50	8 8	−18	10 15	10 17	− 2	10 30	10 42	−12
	6 25	6 27	− 2	6 50	7 3	−13	7 20	7 42	−22	8 40	8 52	−12	10 50	10 53	− 3	11 15	11 13	+ 2
17.	7 0	7 7	− 7	7 20	7 31	−11	8 35	8 41	− 6	9 25	9 49	−24	11 20	11 23	− 3	11 35	12 40	− 5
	7 5	7 11	− 6	7 45	7 42	+ 3	9 20	9 34	−14	10 25	10 36	−11	11 45	11 50	− 5	12 5	12 11	− 6
18.	7 55	8 7	−12	8 20	8 45	−25	10 20	10 23	− 3	11 5	11 12	− 7	12 5	12 15	−10	......	......	......
	7 45	7 57	−12	8 55	8 58	− 3	11 20	11 7	+13	11 55	11 41	+14	......	......	......	12 25	12 36	−11
19.	8 45	8 47	− 2	9 35	10 2	−27	11 35	11 47	−12	......	......	......	12 55	1 6	−11	1 '0	12 58	+ 2
	8 45	8 46	− 1	10 20	10 19	+ 1	12 5	12 3	+ 2	12 10	12 15	− 5	12 45	12 43	+ 2	1 30	1 17	+13
20.	9 45	9 57	−12	11 15	11 19	− 4	......	......	......	12 40	12 44	− 4	1 25	1 26	− 1	1 55	1 57	− 2
	10 5	10 7	− 2	11 50	11 47	+ 3	12 25	12 27	− 2	12 55	1 1	− 6	1 40	1 49	− 9	2 15	2 15	0
21.	10 40	10 42	− 2	......	......	......	1 10	1 12	− 2	1 15	1 14	+ 1	2 15	2 13	+ 2	2 45	2 43	+ 2
	11 15	11 12	+ 3	12 15	12 17	− 2	1 20	1 22	− 2	1 30	1 30	0	2 25	2 29	− 4	3 5	3 8	− 3
22.	11 50	12 10	−20	12 50	12 42	+ 8	1 45	1 50	− 5	1 55	1 57	− 2	2 45	2 44	+ 1	3 30	3 25	+ 5
	......	......	......	1 0	1 7	− 7	2 5	2 2	+ 3	2 15	2 14	+ 1	3 5	3 18	−13	3 50	3 51	− 1
23.	12 15	12 11	+ 4	1 30	1 32	− 2	2 30	2 27	+ 3	2 35	2 37	− 2	3 40	3 48	− 8	4 15	4 15	− 0
	12 45	12 52	− 7	1 40	1 47	− 7	2 45	2 43	+ 2	2 50	2 51	− 1	3 55	3 57	− 2	4 40	4 43	− 3
24.	1 10	1 11	− 1	2 5	2 15	−10	3 5	3 9	− 4	3 10	3 17	− 7	4 25	4 28	− 3	5 0	5 5	− 5
	1 30	1 42	−12	2 20	2 28	− 8	3 20	3 15	+ 5	3 35	3 38	− 3	4 45	4 58	−13	5 35	5 36	− 1
25.	1 55	1 52	+ 3	2 40	2 53	−13	3 45	3 43	+ 2	4 0	4 3	− 3	5 10	5 7	+ 3	5 55	5 51	+ 4
	2 20	2 17	+ 3	3 0	3 2	− 2	3 50	4 3	−13	4 10	4 13	− 3	5 45	5 53	− 8	6 30	6 26	+ 4
26.	2 45	2 32	+13	3 25	3 33	− 8	4 20	4 26	− 6	4 30	4 33	− 3	5 55	5 48	+ 7	6 55	6 32	+23
	2 55	2 58	− 3	3 45	3 43	+ 2	4 35	4 32	+ 3	4 50	4 57	− 7	6 45	6 50	− 5	7 5	7 22	−17
27.	3 15	3 11	+ 4	4 10	4 15	− 5	4 55	5 8	−13	5 15	5 13	+ 2	6 40	6 44	− 4	7 25	7 27	− 2
	3 30	3 25	+ 5	4 20	4 26	− 6	5 10	5 13	− 3	5 30	5 37	− 7	7 50	7 50	0	8 10	8 8	+ 2
28.	3 45	3 46	− 1	4 45	4 48	− 3	5 40	5 43	− 3	5 50	6 8	−18	7 45	7 44	+ 1	8 25	8 26	− 1
	4 0	4 3	− 3	5 5	5 1	+ 4	5 55	5 57	− 2	6 30	6 53	−23	8 45	8 38	+ 7	9 5	9 17	−12
29.	4 25	4 28	− 3	5 30	5 27	+ 3	6 25	6 15	+10	7 0	7 10	−10	9 5	9 2	+ 3	8 5	8 13	− 5
	4 40	4 41	− 1	5 40	5 41	− 1	6 45	6 56	−11	8 5	8 14	− 9	10 10	9 57	+13	10 5	10 45	−40
30.	5 5	4 42	+23	6 10	6 9	+ 1	7 15	7 11	+ 4	8 15	8 23	− 8	10 20	10 27	− 7	10 45	11 22	−37
	5 25	5 22	+ 3	6 20	6 19	+ 1	7 55	8 2	− 7	9 25	9 37	−12	11 5	11 18	−13	11 5	11 46	−41
31.	5 50	5 53	− 3	6 50	7 2	−12	......	......	......	10 0	10 12	−12	......	......	......	11 30	......	......
	6 5	6 3	+ 2	7 10	7 12	− 2	......	......	......	10 50	11 5	−15	......	......	......	12 5	12 21	−16

Table B.

Showing a comparison of the observations of the Heights of High Water made at the London Docks, increased by five feet, and those at the St. Katherine Docks. The observations marked with an * appear doubtful.

Date. 1835.	July.			August.			September.			October.			November.			December.		
	London Docks. + 5 ft.	St. Kath. Docks.	Difference.	London Docks. + 5 ft.	St. Kath. Docks.	Difference.	London Docks. + 5 ft.	St. Kath. Docks.	Difference.	London Docks. + 5 ft.	St. Kath. Docks.	Difference.	London Docks. + 5 ft.	St. Kath. Docks.	Difference.	London Docks. + 5 ft.	St. Kath. Docks.	Difference.
	ft. in.	ft. in.	in.	ft. in.	ft. in.	in.	ft. in.	ft. in.	in.	ft. in.	ft. in.	in.	ft. in.	ft. in.	in.	ft. in.	ft. in.	in.
1.	26 6	26 6	0	26 7	26 8	− 1	25 1	25 1	0	23 7	23 8	− 1	25 2	25 2	0	26 8	26 10	− 2
	26 8	26 9	− 1	26 4	26 5	− 1	25 4	25 3	+ 1	24 8	24 10	− 2	......	......	......	27 3	27 2	+ 1
2.	26 9	26 9	0	26 3	26 3	0	24 3	24 4	− 1	23 7	23 6	+ 1	26 6	26 7	− 1	......	......	......
	26 1	26 7	− 6	26 6	26 1	+ 5	25 0	25 0	0	25 8	25 9	− 1	26 1	26 2	− 1	26 5	26 5	0
3.	26 1	26 8	− 7	26 3	26 4	− 1	24 7	24 6	+ 1	26 5	26 5	0	27 0	27 0	0	27 7	27 8	− 1
	25 4	25 5	− 1	25 6	25 7	− 1	25 6	25 5	+ 1	......	......	......	26 6	26 7	− 1	27 5	27 6	− 1
4.	25 9	25 9	0	25 3	25 2	+ 1	25 1	25 1	0	27 6	27 9	− 3	26 4	26 5	− 1	27 3	27 4	− 1
	25 5	25 5	0	25 4	25 5	− 1	......	......	......	26 1	26 1	0	26 4	26 6	− 2	26 9	26 10	− 1
5.	26 0	25 2	+10	25 4	25 5	− 1	26 6	26 6	0	27 5	27 4	+ 1	26 10	26 9	+ 1	27 1	27 0	+ 1
	25 2	25 11	− 9	26 0	25 9	+ 3	26 6	26 6	0	27 0	27 1	− 1	27 3	27 4	− 1	27 8	27 8	0
6.	25 8	25 8	0	25 4	25 6	− 2	27 8	27 9	− 1	28 1	28 2	− 1	27 6	27 7	− 1	27 2	27 4	− 2
	26 0	26 1	− 1	......	......	......	27 0	27 0	0	28 1	28 1	0	27 6	27 7	− 1	27 3	27 3	0
7.	26 4	24 6	2	26 1	26 2	− 1	27 3	27 4	− 1	27 11	28 0	− 1	26 8	26 8	0	27 6	27 6	0
	26 1	26 3	− 2	26 0	26 7	− 7	27 5	27 6	− 1	28 1	28 2	− 1	26 1	26 1	0	27 2	27 4	− 2
8.	26 0	26 1	− 1	27 10	27 8	+ 2	27 10	27 5	+ 5	27 11	27 11	0	26 1	26 2	− 1	27 1	27 1	0
	......	......	......	27 11	28 0	+ 1	27 8	27 10	− 2	27 10	27 9	+ 1	26 7	26 7	0	26 7	26 6	+ 1
9.	27 3	27 4	− 1	28 2	28 3	− 1	27 4	27 5	− 1	27 1	27 1	0	26 10	27 0	− 2	26 0	26 2	− 2
	26 11	26 10	+ 1	27 9	27 9	0	27 8	27 9	− 1	27 4	27 4	0	27 2	27 2	0	27 7	27 8	− 1
10.	27 2	27 3	− 1	27 9	27 8	+ 1	28 1	28 1	0	28 6	28 6	0	26 8	26 7	+ 1	25 3	25 4	− 1
	27 2	27 1	+ 1	27 6	27 5	+ 1	26 6	26 7	− 1	28 4	28 4	0	26 4	26 3	+ 1	25 5	25 6	− 1
11.	28 1	28 1	0	27 9	27 8	+ 1	25 6	25 7	− 1	27 0	27 3	− 3	25 1	25 1	0	24 6	24 6	0
	28 1	28 0	+ 1	27 8	27 10	− 2	26 8	26 8	0	27 3	27 4	− 1	25 8	25 3	+ 5	24 3	25 6	−15
12.	27 9	27 10	− 1	27 5	27 6	− 1	26 6	26 7	− 1	28 1	28 1	0	24 9	24 8	+ 1	24 6	24 8	− 2
	27 0	27 0	0	27 5	27 4	+ 1	26 5	26 5	0	25 10	25 9	+ 1	25 3	25 5	− 2	24 6	24 7	− 1
13.	27 4	27 7	− 3	27 6	27 7	− 1	26 2	26 4	− 2	25 9	25 9	0	23 2	23 2	0	24 0	24 2	− 2
	27 3	27 3	0	27 4	27 4	0	26 3	26 3	0	25 8	27 9	− 1	23 9	23 10	− 1	24 7	24 10	− 3
14.	27 6	27 7	− 1	26 5	26 5	0	25 3	25 3	0	25 8	25 7	+ 1	22 7	22 7	0	24 0	24 1	− 1
	27 2	27 2	0	26 6	26 7	− 1	25 1	25 4	− 3	25 9	25 8	+ 1	23 11	23 11	0	25 2	25 4	− 2
15.	27 0	27 1	− 1	25 6	25 6	0	24 1	24 3	− 2	23 2	23 1	+ 1	22 9	23 10	− 1	24 4	24 3	+ 1
	26 3	26 5	− 2	25 10	25 10	0	24 2	24 2	0	23 3	23 3	0	24 5	24 6	− 1	25 0	25 1	− 1
16.	26 5	26 6	− 1	24 10	24 10	0	23 4	23 4	0	22 3	22 3	0	24 9	24 7	+ 2	25 0	25 1	− 1
	26 5	26 6	− 1	24 11	24 10	+ 1	23 10	23 8	+ 2	23 3	23 4	− 1	25 7	25 6	+ 1	25 8	25 5	+ 3
17.	25 7	25 8	− 1	24 6	24 5	+ 1	22 11	22 10	+ 1	22 11	22 11	0	25 2	25 1	+ 1	25 3	25 3	0
	25 0	24 11	+ 1	24 1	24 0	+ 1	23 6	23 6	0	24 2	24 2	0	26 8	26 8	0	26 10	26 9	+ 1
18.	25 2	25 2	0	23 6	23 7	− 1	23 3	23 3	0	24 0	24 0	0	26 3	26 4	− 1	......	......	......
	24 7	24 8	− 1	23 11	23 5	+ 6	24 3	24 3	0	25 0	25 4	− 4	......	......	......	26 8	26 9	− 1
19.	24 3	24 0	+ 3	23 3	23 3	0	23 3	23 3	0	......	......	......	28 11	28 11	0	29 1	29 1	0
	24 3	24 3	0	24 0	23 10	+ 2	25 11	25 10	+ 1	25 0	25 1	− 1	28 3	28 4	− 1	29 4	29 3	+ 1
20.	23 4	23 4	0	24 3	24 3	0	......	......	......	26 3	26 4	− 1	27 11	27 11	0	29 4	29 3	+ 1
	24 5	24 5	0	24 4	24 6	− 2	25 3	25 5	− 2	26 4	26 6	− 2	27 3	27 4	− 1	29 0	29 1	− 1
21.	24 11	24 11	0	......	......	......	26 2	26 2	0	28 0	28 0	0	27 11	27 10	+ 1	28 4	28 5	− 1
	24 7	24 8	− 1	25 0	25 1	− 1	26 8	26 8	0	27 2	27 2	0	27 8	27 9	− 1	28 9	28 10	− 1
22.	25 7	25 7	0	25 11	25 9	+ 2	28 0	28 1	− 1	28 2	28 1	+ 1	28 3	28 1	+ 2	28 3	28 2	+ 1
	......	......	......	24 9	25 9	−12	27 7	27 7	0	28 1	28 1	0	28 0	28 0	0	28 3	28 4	− 1
23.	25 2	25 2	0	26 0	26 0	0	27 7	27 7	0	28 2	28 3	− 1	27 8	28 0	− 4	27 2	27 4	− 2
	25 8	25 7	+ 1	26 2	26 1	+ 1	26 7	26 9	− 2	28 0	28 1	− 1	29 0	29 0	0	27 11	27 11	0
24.	25 5	25 6	− 1	27 1	27 1	0	28 5	28 5	0	28 2	28 3	− 1	27 11	27 11	0	26 11	26 9	+ 2
	26 3	26 3	0	27 1	27 2	− 1	28 6	28 7	− 1	28 2	28 1	+ 1	27 5	27 6	− 1	27 0	27 1	− 1
25.	26 1	26 1	0	27 11	27 11	0	29 2	29 2	0	27 8	27 10	− 2	26 8	26 9	− 1	26 4	26 4	0
	26 11	27 0	− 1	27 8	27 9	− 1	28 4	28 5	− 1	28 10	28 11	− 1	27 1	27 2	− 1	26 9	26 10	− 1
26.	26 9	26 11	− 2	27 1	27 5	− 1	27 8	27 11	− 3	26 4	26 4	0	26 7	26 7	0	25 8	26 5	− 9
	27 4	27 5	− 1	27 4	27 4	0	28 3	28 2	+ 1	27 3	27 3	0	26 11	26 11	0	25 6	25 5	+ 1
27.	27 2	27 3	− 1	27 8	27 8	0	28 6	28 8	− 2	26 7	26 8	− 1	25 7	25 7	0	24 8	24 7	+ 1
	27 3	27 4	− 1	27 10	28 1	− 3	28 2	28 2	0	27 7	27 7	0	25 8	25 5	+ 3	25 4	25 5	− 1
28.	27 4	27 5	− 1	27 10	27 10	0	27 0	27 0	0	25 8	25 9	− 1	26 3	26 5	− 2	24 3	24 3	0
	27 6	27 8	− 2	28 0	28 0	0	27 7	27 8	− 1	26 0	26 2	− 2	26 8	26 10	− 2	22 8	22 8	0
29.	27 11	28 0	− 1	27 5	27 4	+ 1	24 6	26 5	−23	24 9	24 8	+ 1	25 2	25 3	− 1	24 10	24 8	+ 2
	27 1	27 2	− 1	27 6	27 6	0	25 2	25 1	+ 1	24 5	24 6	− 1	26 8	26 10	− 2	26 0	25 9	+ 3
30.	27 4	27 1	+ 3	27 0	27 0	0	26 1	26 1	0	24 10	24 10	0	26 2	26 1	+ 1	24 8	24 11	− 3
	27 2	27 2	0	27 3	27 4	− 1	25 3	25 5	− 2	25 6	25 8	− 2	26 5	26 3	+ 2	25 0	24 7	+ 5
31.	27 7	27 7	0	26 4	26 4	0	......	......	......	24 5	24 4	+ 1	......	......	......	24 6	......	......
	27 7	27 7	0	26 2	26 0	+ 2	......	......	......	26 8	26 8	0	......	......	......	24 10	24 9	+ 1

Table C.

Showing a comparison of the observed Times of High Water at the St. Katherine Docks, increased by five minutes, with the predicted Times given in the British Almanac. The observations marked with an * appear doubtful.

Date. 1835.	July.			August.			September.			October.			November.			December.		
	British Alman.	St. Kath. Docks. +5 min.	Error of Prediction.	British Alman.	St. Kath. Docks. +5 min.	Error of Prediction.	British Alman.	St. Kath. Docks. +5 min.	Error of Prediction.	British Alman.	St. Kath. Docks. +5 min.	Error of Prediction.	British Alman.	St. Kath. Docks. +5 min.	Error of Prediction.	British Alman.	St. Kath. Docks. +5 min.	Error of Prediction.
	h m	h m	m	h m	h m	m	h m	h m	m	h m	h m	m	h m	h m	m	h m	h m	m
1.	5 16	5 32	−16	6 25	6 47	−22	7 31	8 1*	−30	8 56	8 46	+10	11 29	11 26	+ 3	11 38	11 23	+15
	5 38	5 47	− 9	6 48	6 58	−10	8 12	8 21	− 9	9 46	9 51	− 5	......	......	......	12 2	12 3	− 1
2.	6 0	6 13	−13	7 10	7 33	−23	8 58	9 17	−19	10 37	10 26	+11	11 50	12 8	−18	......	......	......
	6 26	6 28	− 2	7 34	7 53	−19	9 46	9 47	− 1	11 17	11 17	0	12 26	12 28	− 2	12 22	12 12	+10
3.	6 57	7 2	− 5	8 2	8 40*	−38	10 35	10 52	−17	11 51	11 43	+ 8	12 47	1 3	−16	12 44	12 53	− 9
	7 22	7 12	+10	8 35	8 53	−18	11 20	11 32	−12	......	......	......	1 4	1 3	+ 1	1 2	12 59	+ 3
4.	7 46	8 2	−16	9 12	9 42	−30	11 59	12 17	−18	12 21	12 27	− 6	1 20	1 37	−17	1 19	1 29	−10
	8 9	8 12	− 3	9 56	10 9	−13	......	......	......	12 46	12 53	− 7	1 36	1 47	−11	1 37	1 37	0
5.	8 41	8 26	+15	10 39	10 58	−19	12 32	12 47	−15	1 11	1 18	− 7	1 51	2 13	−22	1 51	2 0	− 9
	9 13	9 2	+11	11 21	11 30	− 9	1 2	1 8	− 6	1 32	1 30	+ 2	2 8	2 17	− 9	2 10	2 14	− 4
6.	9 49	10 17	−28	12 1	12 17	−16	1 27	1 37	10	1 52	2 7	−15	2 23	2 43	−20	2 28	2 33	− 5
	10 23	10 32	− 9	......	......	......	1 50	1 47	+ 3	2 9	2 8	+ 1	2 36	2 46	−10	2 44	2 58	−14
7.	11 0	11 18	−18	12 35	12 36	− 1	2 9	2 27	−18	2 24	2 37	−13	2 51	3 7	−16	3 2	3 10	− 8
	11 32	11 42	−10	1 6	1 13	− 7	2 29	2 33	− 4	2 38	2 47	− 9	3 5	3 18	−13	3 20	3 28	− 8
8.	......	......	......	1 34	1 38	− 4	2 48	3 13	−25	2 53	3 12	−19	3 21	3 38	−17	3 40	3 42	− 2
	12 7	12 30	−23	1 59	2 3	− 4	3 7	3 18	−11	3 9	3 18	− 9	3 37	3 52	−15	3 54	4 2	− 8
9.	12 41	12 41	0	2 24	2 32	− 8	3 25	3 38	−13	3 24	3 37	−13	3 54	4 2	− 8	4 13	4 18	− 5
	1 9	1 13	− 4	2 45	2 51	− 6	3 40	3 39	+ 1	3 41	3 46	− 5	4 10	4 22	−12	4 30	4 38	− 8
10.	1 38	1 47	− 9	3 7	3 23	−16	3 55	4 2	− 7	3 52	4 2	−10	4 25	4 37	−12	4 47	4 53	− 6
	2 5	2 20	−15	3 25	3 36	−11	4 9	4 7	+ 2	4 5	3 57	+ 8	4 41	4 44	− 3	5 7	5 21	−14
11.	2 40	2 38	+ 2	3 43	4 8	−25	4 24	4 22	+ 2	4 20	4 26	− 6	4 58	4 59	− 1	5 27	5 37	−10
	2 54	3 2	− 8	4 3	4 13	−10	4 37	4 38	− 1	4 33	4 37	− 4	5 17	5 22	− 5	5 50	6 13	−23
12.	3 14	3 37	−23	4 22	4 37	−15	4 53	5 7	−14	4 45	4 53	− 8	5 40	5 28	+12	6 15	6 22	− 7
	3 37	3 52	−15	4 42	4 49	− 7	5 9	5 12	− 3	5 2	4 57	+ 5	6 4	6 2	+ 2	6 44	6 32	+12
13.	4 1	4 27	−26	4 58	5 19	−21	5 23	5 31	− 8	5 20	5 27	− 7	6 29	6 18	+11	7 12	7 3	+ 9
	4 23	4 31	− 8	5 13	5 20	− 7	5 41	5 46	− 5	5 39	5 31	+ 8	7 1	7 7	− 6	7 43	7 43	0
14.	4 45	5 0	− 5	5 30	5 53	−23	5 59	6 11	−12	5 55	6 7	−12	7 34	7 33	+ 1	8 18	8 14	+ 4
	5 4	5 12	− 8	5 47	6 3	−16	6 13	6 17	− 4	6 20	6 3	+17	8 12	8 12	0	8 51	9 2	−11
15.	5 26	5 52	−26	6 2	6 32	−30	6 31	6 42	−11	6 48	6 50	− 2	8 54	8 52	+ 2	9 21	9 32	−11
	5 48	5 52	− 4	6 19	6 32	−13	6 54	6 57	− 3	7 18	7 26	− 8	9 33	9 42	− 9	9 51	10 26	−35
16.	6 13	6 36	−23	6 37	7 5	−28	7 22	7 27	− 5	8 0	8 13	−13	10 10	10 22	−12	10 21	10 47	−26
	6 33	6 32	+ 1	6 57	7 8	−11	7 54	7 47	+ 7	8 46	8 57	−11	10 48	10 58	−10	10 52	11 18	−26
17.	6 50	7 12	−22	7 17	7 36	−19	8 36	8 46	−10	9 29	9 54	−25	11 20	10 28	− 8	11 24	12 45*	−81
	7 10	7 16	− 6	7 41	7 47	− 6	9 23	9 39	−16	10 14	10 41	−27	11 45	11 55	−10	11 55	12 16	−21
18.	7 32	8 12	−40	8 10	8 50	−40	10 11	10 28	−17	10 53	11 17	−24	12 8	12 20	−12	......	......	......
	7 54	8 2	− 8	8 45	9 3	−18	10 56	11 12	−16	11 24	11 46	−22	......	......	......	12 25	12 41	−16
19.	8 19	8 52*	−33	9 24	10 7	−43	11 29	11 52*	−23	......	......	......	12 29	1 11	−42	12 51	1 3	−12
	8 49	8 51	− 2	10 6	10 24	−18	12 2	12 8	− 6	11 56	12 17	−21	12 54	12 48	+ 6	1 18	1 22	− 4
20.	9 19	10 2*	−43	10 47	11 24	−37	......	......	......	12 23	12 49	−26	1 14	1 31	−17	1 42	2 2	−20
	9 51	10 12	−21	11 25	11 52	−27	12 28	12 32	− 4	12 49	1 6	−17	1 36	1 54	−18	2 10	2 20	−10
21.	10 24	10 47	−23	......	......	......	12 55	1 17	−22	1 10	1 19	− 9	1 58	2 18	−20	2 36	2 48	−12
	10 59	12 17	−18	11 58	12 22	−24	1 17	1 27	−10	1 28	1 35	− 7	2 20	2 34	−14	3 2	3 13	−11
22.	11 31	12 15	−44	12 27	12 47	−20	1 37	1 55	−18	1 44	2 2	−18	2 46	2 49	− 3	3 28	3 30	− 2
	......	......	......	12 55	1 12	−17	1 58	2 7	− 9	2 4	2 19	−15	3 10	3 23	−13	3 54	3 56	− 2
23.	11 59	12 16	−17	1 20	1 37	−17	2 17	2 32	−15	2 25	2 42	−17	3 36	3 53	−17	4 19	4 20	− 1
	12 28	12 57	−29	1 44	1 52	− 8	2 35	2 48	−13	2 47	2 56	− 9	4 1	4 2	− 1	4 42	4 48	− 6
24.	12 53	1 16	−23	2 3	2 20	−17	2 54	3 14	−20	3 5	3 22	−17	4 23	4 33	−10	5 7	5 10	− 3
	1 17	1 47	−30	2 23	2 33	−10	3 12	3 20	− 8	3 29	3 43	−14	4 49	5 3	−14	5 29	5 41	−12
25.	1 40	1 57	−17	2 42	2 58	−16	3 32	3 48	−16	3 51	4 8	−17	5 15	5 12	+ 3	5 52	5 56	− 4
	1 59	2 22	−23	3 3	3 7	− 4	3 51	4 8	−17	4 12	4 18	− 6	5 41	5 58	−17	6 15	6 31	−16
26.	2 18	2 37	−19	3 22	3 38	−16	4 11	4 31	−20	4 32	4 38	− 6	6 8	5 53	+15	6 43	6 37	+ 6
	2 38	3 3	−25	3 40	3 48	− 8	4 29	4 37	− 8	4 56	5 2	− 6	6 36	6 55	−19	7 12	7 37	−15
27.	3 0	3 16	−16	3 59	4 20	−21	4 49	5 13	−24	5 20	5 18	+ 2	7 6	6 49	+17	7 37	7 32	+ 5
	3 20	3 30	−10	4 15	4 31	−16	5 13	5 18	− 5	5 46	5 42	+ 4	7 40	7 55	−15	8 3	8 13	−10
28.	3 38	3 51	−13	4 33	4 53	−20	5 35	5 48	−13	6 13	6 13	0	8 17	7 49*	+28	8 27	8 31	− 4
	3 57	4 8	−11	4 51	5 6	−15	6 0	6 2	− 2	6 42	6 58	−16	8 54	8 43	+11	8 58	9 22	−28
29.	4 17	4 33	−16	5 13	5 32	−19	6 23	6 20	+ 3	7 16	7 15	+ 1	9 32	9 7	+25	9 28	8 18*	+70
	4 37	4 46	− 9	5 37	5 46	− 9	6 49	7 1	− 2	7 59	8 19	−20	10 10	10 2	+ 8	9 58	10 50*	−52
30.	4 58	4 47	+11	5 58	6 14	−16	7 20	7 16	+ 4	8 47	8 28	+19	10 45	10 32	+13	10 30	11 27*	−57
	5 20	5 27	− 7	6 20	6 24	− 4	8 5	8 7	− 2	9 30	9 42	−12	11 13	11 23	−10	11 3	11 51*	−48
31.	5 45	5 58	−13	6 40	7 7	−27	......	......	......	10 14	10 17	− 3	......	......	......	11 32	0 0	
	6 5	6 8	− 3	7 2	7 17	−15	......	......	......	10 56	11 10	−14	......	......	......	11 59	12 26	−27

Table D.

Showing a comparison of the observed Heights of High Water at the St. Katherine Docks, with the predicted Heights given in the British Almanac, increased by five feet. The observations marked with an * appear doubtful.

Date.	July.			August.			September.			October.			November.			December.		
1835.	British Alman. + 5 ft.	St.Kath. Docks.	Error of Prediction.	British Alman. + 5 ft.	St.Kath. Docks.	Error of Prediction.	British Alman. + 5 ft.	St.Kath. Docks.	Error of Prediction.	British Alman. + 5 ft.	St.Kath. Docks.	Error of Prediction.	British Alman. + 5 ft.	St.Kath. Docks.	Error of Prediction.	British Alman. + 5 ft.	St.Kath. Docks.	Error of Prediction.
	ft. in.	ft. in.	in.	ft. in.	ft. in.	in.	ft. in.	ft. in.	in.	ft. in.	ft. in.	in.	ft. in.	ft. in.	in.	ft. in.	ft. in.	in.
1.	27 1	26 6	+ 7	26 8	26 8	0	24 10	25 1	− 3	24 3	23 8	+ 7	25 4	25 2	+ 2	25 1	26 10	−21
	26 10	26 9	+ 1	26 4	26 5	− 1	24 6	25 3	− 9	24 6	24 10	− 4	......	......	......	25 3	27 2	−23
2.	26 7	26 9	− 2	25 10	26 3	− 5	24 5	24 4	+ 1	24 10	23 6*	+16	25 8	26 7	−11	......	......	......
	26 5	26 7	− 2	25 5	26 1	− 8	24 7	25 0	− 5	25 3	25 9	− 6	25 11	26 2	− 3	25 5	26 5	−12
3.	26 2	26 8	− 6	25 1	26 4	−15	25 0	24 6	+ 6	25 7	26 5	−10	26 3	27 0	− 9	25 9	27 8	−23
	25 10	25 5	+ 5	24 9	25 7	−10	25 5	25 5	0	......	......	......	26 6	26 7	− 1	26 1	27 6	−17
4.	25 7	25 9	− 2	24 8	25 2	− 6	25 10	25 1	+ 9	26 0	27 9*	−21	26 8	26 5	+ 3	26 4	27 4	−12
	25 4	25 5	− 1	24 10	25 5	− 7	......	......	......	26 4	26 1	+ 3	26 9	26 6	+ 3	26 6	26 10	− 4
5.	25 2	25 2	0	25 2	25 5	− 3	26 3	26 6	− 3	26 8	27 4	− 8	26 11	26 9	+ 2	26 9	27 0	− 3
	25 1	25 11	−10	25 7	25 9	− 2	26 8	26 6	+ 2	26 11	27 1	− 2	27 1	27 4	− 3	26 11	27 8	− 9
6.	25 2	25 8	− 6	26 0	25 6	+ 6	27 0	27 9	− 9	27 2	28 2	−12	27 2	27 7	− 5	27 1	27 4	− 3
	25 4	26 1	− 9	......	......	......	27 2	27 0	+ 2	27 5	28 1	− 8	27 3	27 7	− 4	27 2	27 3	− 1
7.	25 8	24 6	+14	26 6	26 2	+ 4	27 5	27 4	+ 1	27 6	28 0	− 6	27 3	26 8	+ 7	27 2	27 6	− 4
	26 0	26 3	− 3	26 11	26 7	+ 4	27 7	27 6	+ 1	27 6	28 2	− 8	27 3	26 1	+14	27 3	27 4	− 1
8.	26 5	26 1	+ 4	27 2	27 8	− 6	27 8	27 5	+ 3	27 5	27 11	− 6	27 2	26 2	+12	27 3	27 1	+ 2
	......	......	......	27 6	28 0	− 6	27 8	27 10	− 2	27 4	27 9	− 5	27 1	26 7	+ 6	27 1	26 6	+ 7
9.	26 11	27 4	− 5	27 8	28 3	− 7	27 8	27 5	+ 3	27 4	27 1	+ 3	27 0	27 0	0	27 0	26 2	+10
	27 3	26 10	+ 5	27 9	27 9	0	27 7	27 9	− 2	27 3	27 4	− 1	26 10	27 2	− 4	26 10	27 8	−10
10.	27 6	27 3	+ 3	27 9	27 8	+ 1	27 6	28 1	− 7	27 2	28 6	−16	26 8	26 7	+ 1	26 8	25 4	+16
	27 9	27 1	+ 8	27 9	27 5	+ 4	27 4	26 7	+ 9	27 0	28 4	−16	26 5	26 3	+ 2	26 6	25 6	+12
11.	27 11	28 1	− 2	27 9	27 8	+ 1	27 1	25 7	+18	26 10	27 3	− 5	26 2	25 1	+13	26 3	24 6	+21
	27 11	28 0	− 1	27 7	27 10	− 3	26 11	26 8	+ 3	26 7	27 4	− 9	25 10	25 3	+ 7	26 0	25 6	+ 6
12.	28 0	27 10	+ 2	27 5	27 6	− 1	26 8	26 7	+ 1	26 4	28 1*	−21	25 7	24 8	+11	25 9	24 8	+13
	28 0	27 0	+12	27 2	27 4	− 2	26 5	26 5	− 0	26 1	25 9	+ 4	25 3	25 5	− 2	25 6	24 7	+11
13.	27 10	27 7	+ 3	27 0	27 7	− 7	26 2	26 4	− 2	25 9	25 9	0	24 11	23 2	+21	25 3	24 2	+13
	27 7	27 3	+ 4	26 10	27 4	− 6	25 11	26 3	− 4	25 5	27 9*	−28	24 8	23 10	+10	25 1	24 10	+ 3
14.	27 4	27 7	− 3	26 6	26 5	+ 1	25 5	25 3	+ 2	25 1	25 7	− 6	24 6	22 7*	+23	25 1	24 1	+12
	27 1	27 2	− 1	26 1	26 7	− 6	25 1	25 4	− 3	24 8	25 8	−12	24 6	23 11	+ 7	25 2	25 4	− 2
15.	26 9	27 1	− 4	25 9	25 6	+ 3	24 8	24 3	+ 5	24 8	23 1	+19	24 8	23 4	+16	25 3	24 3	+12
	26 4	26 5	− 1	25 5	25 10	− 5	24 3	24 2	+ 1	24 2	23 3	+11	25 0	24 6	+ 6	25 4	25 1	+ 3
16.	26 0	26 6	− 6	25 1	24 10	+ 3	24 0	23 4	+ 8	24 0	22 3	+21	25 5	24 7	+10	25 7	25 1	+ 6
	25 8	26 6	−10	24 9	24 10	− 1	23 10	23 8	+ 2	24 1	23 4	+ 9	22 11	25 6	+ 5	25 10	25 5	+ 5
17.	25 4	25 8	− 4	24 4	24 5	− 1	23 9	22 10	+11	24 5	22 11	+18	26 5	25 1	+16	26 3	25 3	+12
	24 11	24 11	0	24 0	24 0	0	23 10	23 6	+ 4	24 11	24 2	+ 9	26 8	26 8	0	26 6	26 9	− 3
18.	24 7	25 2	− 7	23 9	23 7	+ 2	24 4	23 3	+13	25 5	24 0	+17	26 11	26 4	+ 7	......	......	......
	24 3	24 8	− 5	23 8	23 5	+ 3	24 10	24 3	+ 7	26 0	25 4	+ 8	......	......	......	26 11	26 9	+ 2
19.	24 0	24 0	− 0	23 8	23 3	+ 5	25 4	23 3*	+25	......	......	......	27 3	28 11*	−20	27 4	29 1	−21
	23 10	24 3	− 5	23 11	23 10	+ 1	25 10	25 10	0	26 5	25 1	+16	27 6	28 4	−10	27 7	29 3	−16
20.	23 9	23 4	+ 5	24 4	24 3	+ 1	......	......	......	26 11	26 4	+ 7	27 9	27 11	− 2	27 11	29 3	−16
	23 9	24 5	− 8	24 8	24 6	+ 2	26 5	25 5	+12	27 5	26 6	+11	28 0	27 4	+ 8	28 2	29 1	−11
21.	23 11	24 11	−12	......	......	......	26 11	26 2	+ 9	27 9	28 0	− 3	28 2	27 10	+ 4	28 3	28 5	− 2
	24 2	24 8	− 6	25 3	25 1	+ 2	27 4	26 8	+ 8	27 11	27 2	+ 9	28 3	27 9	+ 6	28 4	28 10	− 6
22.	24 7	25 7	−12	25 9	25 9	0	27 8	28 1	− 5	28 1	28 1	0	28 4	28 1	+ 3	28 4	28 2	+ 2
	......	......	......	26 4	25 9	+ 7	28 0	27 7	+ 5	28 3	28 1	+ 2	28 5	28 0	+ 5	28 2	28 4	− 2
23.	25 0	25 2	− 2	26 9	26 0	+ 9	28 2	27 7	+ 7	28 4	28 3	+ 1	28 4	28 0	+ 4	27 11	27 4	+ 7
	25 5	25 7	− 2	27 1	26 1	+12	28 4	26 9	+19	28 5	28 1	+ 4	28 2	29 0	−10	27 8	27 11	− 3
24.	25 11	25 6	+ 5	27 6	27 1	+ 5	28 4	28 5	− 1	28 6	28 3	+ 3	27 10	27 11	− 1	27 3	26 9	+ 6
	26 4	26 3	+ 1	27 9	27 2	+ 7	28 5	28 7	− 2	28 5	28 1	+ 4	27 5	27 6	− 1	26 10	27 1	− 3
25.	26 8	26 1	+ 7	27 11	27 11	0	28 5	29 2	− 9	28 4	27 10	+ 6	26 11	26 9	+ 2	26 5	26 4	+ 1
	27 1	27 0	+ 1	28 1	27 9	+ 4	28 4	28 5	− 1	28 1	28 11	−10	26 5	27 2	− 9	26 0	26 10	−10
26.	27 5	26 11	+ 6	28 2	27 5	+ 9	28 3	27 11	+ 4	27 10	26 4	+18	25 11	26 7	− 8	25 7	26 5	−10
	27 7	27 5	+ 2	28 3	27 4	+11	28 1	28 2	− 1	27 4	27 3	+ 1	25 5	26 11	−18	25 2	25 5	− 3
27.	27 9	27 3	+ 6	28 3	27 8	+ 7	27 9	28 8	−11	26 9	26 8	+ 1	24 11	25 7	− 8	24 10	24 7	+ 3
	27 10	27 4	+ 6	28 2	28 1	+ 1	27 4	28 2	−10	26 3	27 7	−16	24 7	25 5	−10	24 7	25 5	−10
28.	27 11	27 5	+ 6	28 1	27 10	+ 3	26 10	27 0	− 2	25 7	25 9	− 2	24 5	26 5*	−21	24 3	24 3	0
	27 10	27 8	+ 2	27 11	28 0	− 1	26 4	27 8	−16	25 1	26 2	−13	24 4	26 10*	−30	24 0	22 8*	+16
29.	27 10	28 0	− 2	27 7	27 4	+ 3	25 8	26 5	− 9	24 7	24 8	− 1	24 6	25 2	− 8	23 11	24 8	− 9
	27 9	27 2	+ 7	27 3	27 6	− 3	25 1	25 1	0	24 4	24 6	− 2	24 8	26 10*	−26	23 11	25 9*	−22
30.	27 8	27 1	+ 7	26 10	27 0	− 2	24 8	26 1	−17	24 3	24 10	− 7	24 9	26 2	−17	24 1	24 11	−10
	27 6	27 2	+ 4	26 5	27 4	−11	24 8	25 5	− 9	24 5	25 8	−13	24 11	26 3	−16	24 3	24 7	− 4
31.	27 3	27 7	− 4	25 11	26 4	− 5	......	......	......	24 8	24 4	+ 4	......	......	......	24 6	......	
	27 0	27 7	− 7	25 4	26 0	− 8	......	......	......	25 0	26 8	−20	......	......	......	24 10	24 9	+ 1

Table XXVII.

January.

Upper Transits, P.M.						Lower (Interpolated) Transits, A.M.					
Moon's Transit.	Moon's Parallax.	Interval.	Height.	Moon's Declination.	Equation of Time.	Moon's Transit.	Moon's Parallax.	Interval.	Height.	Moon's Declination.	Equation of Time.
h m		h m	ft. in.	°	m	h m		h m	ft. in.	°	m
31·3	57·5	11 12	17 0·8	S. 18·6	+10·1	30·5	58	11 10·5	18 1·2	S. 19·1	+ 9·8
1 30·5	58	10 58·6	17 7·2	S. 15	+10·1	1 32·6	57	10 58·6	18 3	S. 14·6	+10
2 27·6	57·2	10 46·1	16 6	S. 10	+10·3	2 32·9	57·5	10 44·4	18 5·6	S. 9·9	+ 9·9
3 31	57·1	10 37·6	15 10·1	S. 3·6	+ 9·8	3 29·6	57	10 36·6	16 10·3	S. 3·6	+ 9·8
4 29·3	56·9	10 31·8	14 7·2	N. 2·8	+10·5	4 27·5	56·5	10 31·3	15 10	N. 2·0	+10
5 30·6	56·5	10 34·1	13 1	N. 7·6	+ 9·9	5 29·2	56·6	10 33·2	14 6·4	N. 7·1	+ 9·7
6 33·8	56·7	10 50·1	12 1	N. 14	+10·2	6 26·8	56·6	10 48·1	12 10·9	N. 13·7	+ 9·8
7 30·3	56·9	11 18	13 0	N. 17·7	+ 9·6	7 29·5	56·6	11 16·8	12 10·2	N. 18·2	+10·2
8 32·9	56·6	11 39·3	13 5·1	N. 21·1	+10·1	8 29	56·8	11 40·7	13 8·6	N. 20·3	+ 9·7
9 34·1	57·4	11 44·8	14 11·6	N. 22·8	+10·1	9 23·8	57·4	11 46·8	14 9·3	N. 22·5	+ 9·9
10 34·2	57·3	11 40·4	16 1	N. 22·5	+10·3	10 25·7	57·2	11 41·4	15 10·6	N. 22·3	+10·2
11 33·5	57·7	11 26·2	17 1·1	N. 21·6	+10	11 32	57·5	11 28·5	16 8·5	N. 22	+ 9·8
Upper Transits, A.M.						**Lower (Interpolated) Transits, P.M.**					
30·4	57·3	11 14	17 6·1	N. 18·3	+10·2	32·4	57·4	11 14·2	17 1·6	N. 18·1	+10·2
1 32·1	56·9	11 0	17 7·9	N. 14·9	+10	1 31·8	57·4	10 59·2	17 3·8	N. 15·1	+10·1
2 30·7	57·3	10 44·8	17 10·5	N. 10·4	+ 9·9	2 30·5	57·2	10 48·2	16 11·1	N. 9·4	+10·3
3 29	57	10 37·1	16 5·8	N. 4·7	+10·1	3 30·6	57·1	10 37·2	15 10·8	N. 4·8	+10
4 30·4	56·6	10 30·5	15 6	S. 4·0	+10·4	4 29·8	56·8	10 31·6	14 7·5	S. 3·3	+10
5 29·8	57·2	10 33	14 2·1	S. 8·1	+ 9·8	5 30·3	56·6	10 35·1	13 2·3	S. 8·7	+10·1
6 28·9	56·8	10 48·6	12 10·5	S. 13·7	+ 9·6	6 30·1	56·7	10 50·1	12 11·1	S. 14	+10·2
7 33·1	56·8	11 18·4	12 6·7	S. 18·3	+10	7 25·9	57·3	11 16·2	12 8·7	S. 18	+10·1
8 35	57·4	11 39·5	13 8·8	S. 21	+ 9·9	8 26·7	57	11 38·5	13 5·6	S. 20·5	+10·1
9 32	57·8	11 44·2	14 10·5	S. 22·3	+10·1	9 31·9	57·5	11 44·8	15 1·6	S. 22·8	+ 9·9
10 29·6	57·5	11 40·3	15 6·2	S. 22·5	+ 9·8	10 34·4	57·9	11 36·7	16 5·3	S. 22·8	+10·3
11 27·3	57·5	11 28	16 2·8	S. 22	+ 9·7	11 33·6	58·1	11 24·8	17 5·8	S. 21·6	+10·3

February.

Upper Transits, P.M.						Lower (Interpolated) Transits, A.M.					
33·1	56·8	11 18·9	17 6	S. 10·1	+14·2	31·6	57·7	11 14·5	18 11·2	S. 10·1	+14·2
1 34·3	57·7	11 2·8	17 11·7	S. 2·8	+14·1	1 27	57·1	11 3·8	18 10·9	S. 3·5	+14·2
2 31·5	57	10 51	16 11·3	N. 2·7	+14·2	2 28·6	57·3	10 49·9	18 3·2	N. 2·1	+14·3
3 30·8	56·9	10 37·8	15 11·2	N. 8·6	+14·2	3 27·6	57·2	10 36·7	17 3·7	N. 8·5	+14·2
4 30·1	56·9	10 28	14 10·6	N. 13·5	+14·1	4 29·8	56·8	10 28·6	15 4·3	N. 13·6	+14·1
5 28·7	56·4	10 27·1	12 10·6	N. 18·2	+14·1	5 32·3	56·2	10 26·7	13 9·2	N. 18·8	+14·2
6 29·9	56·6	10 39·6	12 1	N. 21·3	+14·2	6 30·7	56·8	10 35·7	12 4·5	N. 21·1	+14·1
7 32·2	56·7	11 11·2	11 10·6	N. 22·5	+14	7 29·5	56·7	11 7·4	11 10·2	N. 21·9	+14·1
8 34·1	57	11 38·8	13 2·7	N. 22·8	+14·1	8 28	56·6	11 39	12 6·1	N. 23·4	+14·1
9 31·8	57·1	11 49·3	14 11·2	N. 21	+14·2	9 28	57	11 48·2	14 2·9	N. 21·6	+14·2
10 32·8	57·2	11 43·4	16 3·1	N. 19·1	+14·2	10 27·3	57·4	11 45·5	15 11·4	N. 18·7	+14·2
11 30·8	57	11 32·6	17 7·3	N. 14·5	+14·3	11 29·4	57·3	11 31·7	17 4·2	N. 15	+14·3
Upper Transits, A.M.						**Lower (Interpolated) Transits, P.M.**					
28·8	57·6	11 16·7	18 3·8	N. 9·8	+14·2	33·1	57·1	11 18·5	17 9·2	N. 9·3	+14·2
1 30·4	57	11 4·5	18 9	N. 3·6	+14·2	1 30·2	57·6	11 5	18 2·8	N. 3·9	+14·2
2 34	57·6	10 48·2	18 3·3	S. 2·4	+14·3	2 29·4	57·1	10 49·6	17 5·1	S. 2·5	+14·2
3 30	57·3	10 37·3	16 9·6	S. 7·4	+14·3	3 28·7	57·1	10 39	16 1	S. 7·2	+14·2
4 29·9	56·8	10 28·9	15 4	S. 13·8	+14·2	4 28·5	57·2	10 27·8	15 0	S. 13·9	+14·2
5 32·2	57	10 24·3	13 6·4	S. 18·7	+14·1	5 26·1	57·3	10 26·4	13 5·8	S. 18·2	+14·2
6 28·2	57	10 32	11 10·4	S. 20·8	+14·1	6 30·3	56·8	10 37·7	11 7·8	S. 21·2	+14·2
7 29·6	57·2	11 4·3	11 7·3	S. 22·9	+14·1	7 33·2	57·1	11 10·4	11 9·9	S. 22·3	+14·1
8 31·5	57·3	11 36·2	12 9·1	S. 23	+14·2	8 28·5	57·2	11 36·7	13 1·1	S. 22·8	+14
9 32·6	57	11 49·5	14 0·6	S. 21·7	+14·2	9 26·5	57·5	11 45·6	14 9	S. 22	+14·2
10 33·1	57·6	11 42·1	15 10·7	S. 18·2	+14·2	10 28·7	57·2	11 42·6	16 1	S. 19·5	+14·2
11 29·8	57·8	11 29·9	16 11	S. 15	+14·3	11 31·8	57·3	11 31	17 4	S. 14·1	+14·1

Table XXVII. (Continued.)

March.

Moon's Transit.	Moon's Parallax.	Interval.	Height.	Moon's Declination.	Equation of Time.	Moon's Transit.	Moon's Parallax.	Interval.	Height.	Moon's Declination.	Equation of Time.
Upper Transits, P.M.						**Lower (Interpolated) Transits, A.M.**					
h m		h m	ft. in.	°	m	h m		h m	ft. in.	°	m
31·8	57·7	11 17·5	17 7·6	N. 1·5	+ 8·5	31	57·4	11 19·1	18 4·1	N. 1·3	+ 8·5
1 32·2	57·1	11 4·5	17 8	N. 8·5	+ 8·2	1 29·8	57·9	11 3	19 0·8	N. 7·1	+ 8·8
2 31·3	57·5	10 48·3	17 2·6	N. 12·6	+ 8·5	2 31·6	56·8	10 50·8	17 2·9	N. 13·2	+ 8·4
3 30·1	56·7	10 33·5	15 10·9	N. 17·4	+ 9	3 31	57	10 32·2	16 6·2	N. 18·2	+ 8·4
4 31·4	56·7	10 20·7	14 0·1	N. 21·2	+ 8·5	4 28·3	56·6	10 21·9	14 8·3	N. 20·5	+ 8·7
5 29·9	56·5	10 16·2	12 7	N. 22·6	+ 8·4	5 30·6	56·3	10 17·2	13 0·5	N. 22·2	+ 8·8
6 31·8	56·4	10 31·9	11 3·4	N. 22·5	+ 8·1	6 33	56·4	10 31·6	11 0·1	N. 22·4	+ 8·6
7 30·8	56·7	11 7·4	11 2	N. 22·6	+ 8·5	7 32·1	56·6	11 8·8	11 2·3	N. 21·8	+ 8·4
8 31·5	56·6	11 43·3	12 9·3	N. 19·9	+ 8·6	8 30·2	57	11 40·4	12 6·7	N. 19·5	+ 8·5
9 32·3	56·9	11 53·1	14 10	N. 14·8	+ 8	9 32·8	57·2	11 49	14 6·1	N. 15	+ 8·3
10 29·5	57·5	11 45	16 9·4	N. 10·3	+ 8·3	10 35	57	11 47·4	16 3·1	N. .10	+ 8·6
11 29·4	57·2	11 35·6	17 8·8	N. 5·0	+ 8·8	11 31·5	57·8	11 32·2	17 7·6	N. 4·7	+ 9
Upper Transits, A.M.						**Lower (Interpolated) Transits, P.M.**					
32·8	58·1	11 18·1	18 8·3	S. 1·9	+ 8·6	28·8	57·5	11 21·3	18 2	S. 1·3	+ 8·8
1 30·2	57·6	11 4·1	18 11·2	S. 6·8	+ 8·9	1 31	57·9	11 3·3	18 6·2	S. 8	+ 8·4
2 31·9	57·4	10 48·5	17 10·1	S. 13·1	+ 8·7	2 30·2	57·8	10 48·2	17 7·2	S. 12·7	+ 8·6
3 35	57·2	10 32·6	16 5·8	S. 18·2	+ 8·8	3 29·6	57·2	10 33·8	16 3·9	S. 17·6	+ 9
4 30	56·9	10 19·4	14 7·4	S. 20·8	+ 8·9	4 30·4	56·6	10 19·3	14 7	S. 21·1	+ 8·3
5 30·5	56·7	10 16·1	12 11·3	S. 22·3	+ 8·6	5 30·2	57	10 15·5	13 1·2	S. 22·2	+ 8·7
6 32·4	56·5	10 28·4	11 4·6	S. 23·2	+ 8·8	6 29·8	56·6	10 30·6	11 10·5	S. 22·9	+ 8·1
7 31·3	57	11 4·7	11 1·8	S. 21·6	+ 8·4	7 27·9	57	11 5·7	11 9·9	S. 21·7	+ 8·3
8 32·5	56·8	11 42·5	12 6·6	S. 19·4	+ 8	8 29·4	56·8	11 39·6	12 10·5	S. 20·1	+ 8
9 27·8	57·5	11 50·7	14 3·7	S. 15·6	+ 8·5	9 32·9	57·3	11 49·5	14 11·6	S. 14·8	+ 8·3
10 26·9	57·2	11 43·9	15 9	S. 11·2	+ 9	10 33·1	57·5	11 45·5	16 9·2	S. 9·8	+ 8·1
11 29·6	57·9	11 32·9	17 4·1	S. 5·4	+ 8·4	11 28·5	57·9	11 30·4	17 10·5	S. 5·0	+ 8·6

April.

Moon's Transit.	Moon's Parallax.	Interval.	Height.	Moon's Declination.	Equation of Time.	Moon's Transit.	Moon's Parallax.	Interval.	Height.	Moon's Declination.	Equation of Time.
Upper Transits, P.M.						**Lower (Interpolated) Transits, A.M.**					
31·6	57·5	11 18	17 10·1	N. 13·1	− ·01	26·8	57·2	11 20·7	18 3·7	N. 12·1	+ ·04
1 31·4	57·1	11 2	17 4·5	N. 16·8	+ ·03	1 32·7	57·5	11 1	18 1·1	N. 17·1	0
2 31	57·5	10 42·3	16 9·7	N. 20·6	0	2 30	57	10 45	16 11·2	N. 20·3	+ ·01
3 33·1	56·9	10 25·6	15 8·6	N. 22·7	− ·03	3 27·3	57	10 29·3	15 11·9	N. 22	− ·01
4 34	56·8	10 13·3	14 3·3	N. 22·7	·02	4 28·4	56·7	10 15	14 2·2	N. 22·9	0
5 34·7	56·6	10 12·2	12 7·9	N. 22·1	0	5 28	57	10 12·2	12 8·1	N. 22·1	+ ·02
6 31	56·6	10 32	11 5·4	N. 20·2	− ·01	6 30·3	56·6	10 35	11 2·5	N. 18·9	− ·03
7 32·6	56·7	11 17·2	11 10	N. 15·6	− ·03	7 31·4	56·9	11 18·4	11 7·6	N. 16·2	+ ·01
8 32	56·9	11 46	13 4·4	N. 10·8	− ·03	8 30·3	57	11 43·3	13 3·8	N. 12	− ·01
9 27	57·1	11 53·2	15 1·7	N. 5·7	0	9 29·6	57·1	11 54·7	15 1·4	N. 5·1	− ·03
10 27·5	57	11 48·6	16 7·6	S. 0·7	− ·03	10 32·2	57·2	11 48·4	16 7·1	S. 0·4	− ·02
11 28·1	57·9	11 33	17 4·8	S. 7	− ·01	11 32·1	57·3	11 36	17 9·6	S. 7·5	− ·03
Upper Transits, A.M.						**Lower (Interpolated) Transits, P.M.**					
28	57·1	11 21·2	17 11	S. 11·5	+ ·05	32	58	11 16·5	18 2·8	S. 13	+ ·01
1 29·1	57·9	11 0·5	18 1·2	S. 17·4	− ·02	1 28·5	57·7	11 0·5	18 0·9	S. 16·5	+ ·02
2 28·4	57·8	10 42·6	17 3·3	S. 20·7	0	2 30·7	57·4	10 43·8	17 2·6	S. 20·7	0
3 31·1	57·3	10 26·9	16 0·5	S. 22	+ ·03	3 32·5	57·3	10 24·6	16 2·3	S. 22 6	+ ·03
4 31·6	57·3	10 14·1	14 3·3	S. 23·4	− ·02	4 30	57·2	10 13·9	14 11·2	S. 22·6	− ·03
5 31·8	57·1	10 12·5	12 8·1	S. 22·1	0	5 25·7	57	10 11·8	13 5·2	S. 22·2	− ·01
6 32·5	56·7	10 33·6	11 2·8	S. 19·9	− ·01	6 27·3	56·8	10 26·9	11 10·8	S. 20·5	0
7 31·4	57·1	11 11·9	11 4·2	S. 16	− ·01	7 27	57·2	11 9·7	11 11·3	S. 16·3	− ·02
8 27·2	57·1	11 46·2	12 8·5	S. 12	+ ·02	8 29·3	57	11 43·4	13 6·5	S. 10·8	− ·02
9 30·8	56·8	11 56·1	14 7·6	S. 6	− ·03	9 30·2	57·2	11 52·5	15 5	S. 5·1	− ·01
10 28	57·5	11 47·2	16 4·5	N. 0·6	0	10 29·1	57	11 48·8	16 8·9	N. 0·8	− ·01
11 30	56·6	11 38	17 0·1	N. 6·3	+ ·03	11 28·7	57	11 37	17 8·8	N. 7·0	− ·01

Table XXVII. (Continued.)

May.

Upper Transits, P.M.						Lower (Interpolated) Transits, A.M.					
Moon's Transit.	Hor. Par.	Interval.	Height.	Moon's Declination.	Equation of Time.	Moon's Transit.	Hor. Par.	Interval.	Height.	Moon's Declination.	Equation of Time.
h m	′	h m	ft. in.	°	m	h m	′	h m	ft. in.	°	m
28·6	56·9	11 17·2	17 0	N. 20·9	− 3·6	32·3	57·3	11 16·4	17 4	N. 20·4	− 3·7
1 31·2	57·1	10 53	17 0·4	N. 22·2	− 3·7	1 32	56·9	10 58	16 10	N. 21·9	− 3·6
2 31·3	57·1	10 39	16 7·3	N. 22·7	− 3·7	2 33·5	56·9	10 38·6	16 1·8	N. 23	− 3·7
3 28	57	10 26·4	15 10·5	N. 21·9	− 3·7	3 33·1	57	10 25·6	15 4·5	N. 22·1	− 3·7
4 27·8	57	10 15·7	14 8·5	N. 20·4	− 3·7	4 30·9	56·9	10 16·1	14 1·5	N. 19·6	− 3·6
5 30	56·9	10 18·6	13 6·6	N. 16	− 3·6	5 27·6	56·9	10 19·8	12 11·1	N. 16	− 3·7
6 29·2	57	10 40·4	12 8	N. 11·6	− 3·6	6 28·8	56·8	10 42·5	12 1·2	N. 12·6	− 3·6
7 27·4	57·1	11 18	12 10·1	N. 5·5	− 3·7	7 30·1	57	11 20·6	12 8	N. 5·5	− 3·7
8 29	56·9	11 47·4	13 11·2	S. 0·5	− 3·7	8 29·5	57·1	11 47·8	13 9·6	N. 0·5	− 3·7
9 29·9	57·6	11 50·8	15 3·8	S. 6·9	− 3·7	9 28·2	57	11 54·2	15 3·8	S. 7·3	− 3·7
10 31·1	57·1	11 46·7	16 1·7	S. 11·8	− 3·7	10 32·8	57·7	11 45·5	16 6·1	S. 11·9	− 3·7
11 32·5	57·6	11 29·9	16 11·7	S. 17·5	− 3·6	11 26·2	57·5	11 33·1	17 4	S. 16·4	− 3·6
Upper Transits, A.M.						Lower (Interpolated) Transits, P.M.					
30	57·5	11 15·8	17 5·1	S. 19·8	− 3·7	27·4	57·2	11 16·4	17 8	S. 20·3	− 3·7
1 27·3	57·3	10 59·5	17 2·8	S. 22	− 3·7	1 30	57·9	10 56·8	17 7·3	S. 22·3	− 3·7
2 27·5	57·6	10 39·2	16 5·6	S. 22·7	− 3·6	2 30·1	57·6	10 38·5	17 0·2	S. 22·6	− 3·7
3 32·8	57·3	10 24·8	15 5·1	S. 22·1	− 3·7	3 27·9	57·5	10 23·0	16 4·5	S. 22	− 3·7
4 33·4	57·5	10 17·3	14 2·6	S. 19·7	− 3·7	4 28·9	57·2	10 14·3	14 11·6	S. 20·3	− 3·7
5 30·3	57·5	10 18·3	12 9·9	S. 16·6	− 3·6	5 31·4	57·2	10 18·5	13 10·2	S. 15·5	− 3·7
6 27	56·9	10 40·4	11 6·7	S. 12·3	− 3·6	6 32·2	57·2	10 42·7	13 0	S. 11·6	− 3·6
7 27·8	57	11 22·4	12 0·8	S. 6·0	− 3·7	7 33·5	57·2	11 21·1	13 0·9	S. 5·5	− 3·7
8 30	57	11 45·8	13 3·2	N. 0·5	− 3·6	8 31·6	56·8	11 47·3	13 11·6	N. 0·9	− 3·7
9 32·5	56·7	11 53·7	14 9·1	N. 7·0	− 3·6	9 29·1	57·2	11 50·9	15 5·1	N. 6·3	− 3·7
10 33·7	57	11 47·4	16 2·6	N. 11·8	− 3·7	10 28·2	56·3	11 46·7	16 3	N. 11·9	− 3·6
11 29·5	57·2	11 32·9	16 10·2	N. 16·5	− 3·7	11 31·5	57·1	11 31·9	16 10·8	N. 17·0	− 3·6

June.

Upper Transits, P.M.						Lower (Interpolated) Transits, A.M.					
32·3	57·2	11 11·7	16 11·3	N. 22·8	+ ·1	29·7	56·9	11 13·8	16 4·7	N. 23	0
1 29	57·1	10 55·7	17 0·1	N. 22·6	− ·1	1 33·8	57	10 56·1	16 4·5	N. 21·7	+ ·3
2 30·1	56·9	10 40	16 8·4	N. 19·4	+ ·2	2 32·7	57·1	10 39·6	16 0·3	N. 19·4	+ ·2
3 31·6	56·7	10 29·6	16 1·8	N. 15·4	+ ·3	3 26·9	56·9	10 31·3	15 3·8	N. 16·3	0
4 30·9	57	10 24·5	15 0·6	N. 11·1	− ·1	4 30·7	56·7	10 25·1	14 1·1	N. 11·8	− ·1
5 29·3	56·8	10 32	13 11·1	N. 5·1	+ ·8	5 32·4	56·8	10 31·8	13 4·9	N. 4·7	+ ·6
6 29·5	57	10 52·9	13 4·1	S. 0·7	+ ·2	6 28·1	56·7	10 53·4	12 7	S. 0·7	+ ·1
7 31·7	57·2	11 24	13 2·1	S. 7·5	+ ·5	7 28·9	57	11 24·5	12 11·4	S. 7·1	+ ·2
8 33·3	57·2	11 43·7	13 11·2	S. 11·3	+ ·2	8 27·9	57·6	11 42	13 10·8	S. 12·0	0
9 34·9	57·5	11 48·6	14 10·8	S. 18·0	+ ·7	9 29·1	57·2	11 49·4	15 1·5	S. 16·6	+ ·2
10 30·1	58·4	11 40·8	15 9·4	S. 20·1	+ ·1	10 31·8	57·7	11 40·7	16 2·2	S. 20	+ ·4
11 32	57·8	11 26·7	16 5·4	S. 22·4	+ ·3	11 31·4	57·6	11 28	16 11·1	S. 22·3	+ ·3
Upper Transits, A.M.						Lower (Interpolated) Transits, P.M.					
30	56·8	11 12·2	16 0·6	S. 23·8	− ·1	30·1	57·8	11 11·5	17 5·8	S. 22·9	+ ·1
1 28·5	57·4	10 55·4	16 5·1	S. 22	0	1 29·4	57·4	10 54·9	17 4·8	S. 22·5	0
2 28·9	57·7	10 41	16 1·9	S. 18·9	+ ·4	2 28·5	57·3	10 37·3	17 1·9	S. 19·7	+ ·1
3 28·3	57	10 28·9	15 2·3	S. 17·1	− ·1	3 31·9	57·2	10 28·9	16 5·2	S. 15·6	+ ·4
4 27·7	57·2	10 25·2	14 4	S. 10·6	+ ·5	4 32·2	57·2	10 24·5	15 7·1	S. 11	+ ·2
5 27·4	56·9	10 31·1	13 1·9	S. 6·0	+ ·1	5 29·7	56·7	10 31·1	14 4·5	S. 5·3	+ ·1
6 31	57·1	10 54	12 7·6	N. 0·9	+ ·1	6 29·3	56·9	10 53·5	13 10	N. 0·9	+ ·3
7 33·8	56·9	11 25·9	12 7·6	N. 6·9	+ ·1	7 26·7	57·1	11 21	13 7·8	N. 7·4	+ ·4
8 30·5	56·9	11 45·2	13 7·2	N. 12·4	+ ·2	8 28·8	56·9	11 45	13 11·8	N. 12·1	+ ·2
9 28·8	57	11 50	14 9	N. 16·8	0	9 32·2	57·7	11 47·9	15 0	N. 17·6	+ ·3
10 31	57	11 42·6	15 10·2	N. 20·7	+ ·2	10 30·8	57·2	11 42·7	15 10	N. 20·2	+ ·4
11 32·7	57	11 28	16 7·6	N. 22·0	+ ·3	11 29·2	56·9	11 31·2	16 3	N. 22·2	0

Table XXVII. (Continued.)

July.											
Upper Transits, P.M.						Lower (Interpolated) Transits, A.M.					
Moon's Transit.	Hor. Par.	Interval.	Height.	Moon's Declination.	Equation of Time.	Moon's Transit.	Hor. Par.	Interval.	Height.	Moon's Declination.	Equation of Time.
h m	′	h m	ft. in.	°	m	h m	′	h m	h m	°	m
35·3	56·9	11 13·3	17 4·8	N. 18·9	+ 5·3	30·1	57	11 12·1	16 9·2	N. 19·9	+ 5·6
1 34·5	57·2	10 55·5	17 8·6	N· 15·3	+ 5·2	1 30·1	56·8	11 1	16 9·3	N. 16·2	+ 5·2
2 31·3	57	10 46	17 5·2	N. 10·8	+ 5·3	2 32·4	57	10 47·3	17 3·9	N. 10·3	+ 5·2
3 31·4	56·7	10 37·3	16 7·4	N. 4·8	+ 5·3	3 31·4	56·9	10 38·4	15 8·1	N. 4·6	+ 5·2
4 32·1	56·9	10 32·5	15 4·1	S. 1·1	+ 5·1	4 31·4	56·8	10 32·8	14 7·7	S. 1·5	+ 5·2
5 28·6	57·1	10 35·0	13 11·6	S. 7·6	+ 5·3	5 33	56·9	10 35·2	13 6·8	S. 7·6	+ 5·3
6 28·8	56·8	10 50·6	13 3	S. 13·3	+ 5·4	6 30·4	57·1	10 50·4	12 8·8	S. 12·6	+ 5·2
7 25·6	57·2	11 13·3	12 9·5	S. 17·7	+ 5·3	7 32·8	57	11 19·6	12 4·3	S. 17·6	+ 5·2
8 25·1	57·2	11 36·9	13 3·1	S. 20·3	+ 5·2	8 35·2	57·3	11 37·8	13 9·7	S. 21·3	+ 5·4
9 28·6	58	11 43	14 5·6	S. 22·4	+ 5·3	9 30·9	57·8	11 42·1	15 0·7	S. 22	+ 5·3
10 34·5	57·7	11 38·3	15 0·4	S. 22·7	+ 5·4	10 26·4	57·8	11 39·9	15 11·7	S. 23·3	+ 5·2
11 37·8	58	11 24·4	16 3·1	S. 21·8	+ 5·3	11 27·2	58	11 25·1	16 11·5	S. 22·4	+ 5·1
Upper Transits, A.M.						Lower (Interpolated) Transits, P.M.					
29·7	58	11 13·6	16 7·1	S. 19·7	+ 5·2	32·2	57·7	11 10·5	17 5·8	S. 19·2	+ 5·3
1 28	57·2	10 58·3	16 4·8	S. 16·5	+ 5·1	1 33·7	57·8	10 59·1	17 10·8	S. 15·7	+ 5·2
2 30·8	57·2	10 47·8	16 2·6	S. 10·9	+ 5·3	2 29·2	57·2	10 46·5	17 5·5	S. 11·2	+ 5·1
3 31·4	57·1	10 38·3	15 8·1	S. 5·0	+ 5·2	3 31·8	57	10 37	16 11·1	S. 4·2	+ 5·4
4 30·1	56·7	10 33·7	14 5·3	N. 1·7	+ 5·2	4 31	57	10 33	15 9	N. 1·8	+ 5·4
5 30·5	56·7	10 37·7	13 3	N. 6·7	+ 5·2	5 26·8	56·7	10 36	14 5·8	N. 7·0	+ 5·3
6 29·7	56·8	10 52·6	12 3·1	N. 12·4	+ 5·2	6 29·2	56·7	10 51·1	13 2·3	N. 13·3	+ 5·3
7 29·7	56·8	11 18·5	12 4	N. 17·3	+ 5·2	7 32·7	57	11 16·7	13 1·8	N. 17	+ 5·2
8 28·4	57	11 41·5	13 3·7	N. 19·5	+ 5·2	8 33·6	56·9	11 41·1	13 4	N. 21·4	+ 5·3
9 28·2	57·3	11 46·5	14 8	N. 22·1	+ 5·3	9 33·1	57·3	11 45·5	14 5·7	N. 22·2	+ 5·4
10 30·3	57·5	11 34	15 11·4	N. 22·6	+ 5·2	10 31·2	57·2	11 41	15 3·3	N. 22·9	+ 5·2
11 32·9	57·2	11 27·8	16 9	N. 22·1	+ 5·1	11 31·7	57	11 27·6	16 1·5	N. 21·4	+ 5·3
August.											
Upper Transits, P.M.						Lower (Interpolated) Transits, A.M.					
32	57·4	11 16·7	18 0·8	N. 11·2	+ 3·5	30·5	57·4	11 17·5	17 2·6	N. 10·4	+ 3·4
1 32·4	57·3	11 4·3	18 1·6	N. 4·2	+ 3·3	1 24·7	57·6	11 6	17 5·7	N. 5·4	+ 3·4
2 29·6	57·3	10 51·3	17 11	S. 1 0	+ 3·6	2 26·8	57	10 52·5	16 11·7	S. 1·2	+ 3·5
3 27·4	56·6	10 40·2	16 8·2	S. 7·1	+ 3·7	3 31·9	57·1	10 39·3	16 2	S. 6·8	+ 4·0
4 28·5	57	10 30	15 4·3	S. 13	+ 3·3	4 33	56·8	10 31·3	14 7·4	S. 13	+ 3·8
5 24·5	56·7	10 26·4	13 9·4	S. 17	+ 3·6	5 35·3	56·8	10 28·7	13 3·3	S. 18·1	+ 3·4
6 24·9	56·6	10 37·9	12 1·7	S. 20·4	+ 3·7	6 34	56·9	10 42·2	12 1	S. 21·3	+ 3·5
7 26·3	57·1	11 5·4	11 11·6	S. 22·5	+ 3·3	7 30·9	57·3	11 7·7	12 2·1	S. 22·6	+ 3·5
8 30·1	56·8	11 38	12 7	S. 23	+ 3·2	8 30·2	57	11 37·5	13 2·8	S. 22·6	+ 3·5
9 29·8	57·7	11 44·7	14 1·5	S. 21·7	+ 3·4	9 33·8	57·5	11 45·8	14 9·4	S. 22·2	+ 3·7
10 26·6	57·8	11 42	15 3·1	S. 19·8	+ 3·3	10 37	57·6	11 40·8	16 7·6	S. 18·4	+ 3·2
11 25·9	57·9	11 30·7	16 6·2	S. 15·7	+ 3·4	11 34·9	58·1	11 29·1	17 10·1	S. 15	+ 3·3
Upper Transits, P.M.						Lower (Interpolated) Transits, P.M.					
31·3	57·7	11 17·2	17 0·8	S. 10·7	+ 3·3	29·6	57·8	11 17	18 8	S. 10·3	+ 3·5
1 30·3	57·7	11 3·9	17 4·3	S. 5·2	+ 3·7	1 29·6	57	11 4·5	18 6·2	S. 5·4	+ 4·0
2 27·7	57	10 52·7	16 7·8	N. 0·6	+ 3·7	2 30·6	57·8	10 50	18 4·8	N. 1·5	+ 3·5
3 30·3	57	10 38·1	15 10·4	N. 7·2	+ 3·5	3 28·6	56·7	10 39·5	16 11	N. 6·7	+ 3·8
4 26·9	56·8	10 30·7	14 7·5	N. 12·4	+ 4·0	4 29·6	56·8	10 29·2	15 6·2	N. 13·4	+ 3·0
5 29·5	56·4	10 28·4	12 11·5	N. 17·9	+ 3·4	5 30·2	56·7	10 27·7	13 9·7	N. 16·8	+ 3·7
6 29·9	56·5	10 40·7	11 8·3	N. 20·5	+ 3·5	6 31	56·7	10 41·2	12 1·9	N. 20·8	+ 3·6
7 26·5	56·5	11 8·9	11 7·2	N. 21·6	+ 3·7	7 31·5	56·5	11 11·1	11 9·8	N. 22·3	+ 3·4
8 27	56·9	11 35·2	12 10·9	N. 23·1	+ 3·7	8 32·1	56·8	11 38 6	12 6·9	N. 23·1	+ 3·4
9 27·4	57	11 47·6	14 6	N. 21·9	+ 3·3	9 31	56·7	11 49·4	14 0·3	N. 21	+ 3·0
10 29·6	57	11 42·9	15 11·8	N. 19·5	+ 3·3	10 29·9	57·2	11 43·2	15 4·5	N. 19·2	+ 3·5
11 30·7	57·7	11 30·3	17 3·7	N. 15·3	+ 3·5	11 30·7	57·4	11 30·3	16 5·6	N. 16	+ 3·5

Table XXVII. (Continued.)

September.

Moon's Transit.	Hor. Par.	Interval.	Height.	Moon's Declination.	Equation of Time.	Moon's Transit.	Hor. Par.	Interval.	Height.	Moon's Declination.	Equation of Time.
Upper Transits, P.M.						Lower (Interpolated) Transits, A.M.					
h m	′	h m	ft. in.	°	m	h m	′	h m	ft. in.	°	m
30	57·8	11 19·7	18 8·5	S. 0·8	−5·4	30·6	57·4	11 21·4	17 10·6	0	− 5·2
1 29·8	57·4	11 4·5	18 7·7	S. 6·2	−5·3	1 34·4	57·7	11 1·3	18 1·3	S. 7·3	− 5·5
2 30·4	57·2	10 46·8	18 0·8	S. 12·5	−5·7	2 31·9	57·2	10 49·1	17 4·7	S. 11·5	− 4·6
3 26·2	57·5	10 34·4	16 8·9	S. 17	−4·9	3 31·7	56·8	10 35	16 5·2	S. 17·2	− 4·7
4 25·8	57·	10 22·2	15 0·9	S. 19·6	−4·6	4 33·2	57	10 20·5	14 9·3	S. 20·4	− 5·2
5 23·8	57·1	10 15·3	13 5	S. 22·3	−5·1	5 35·8	56·8	10 16·6	13 2·3	S. 22·4	− 5
6 26·7	56·7	10 27·3	11 7·8	S. 23·2	−5·4	6 34·5	57	10 30·6	11 6	S. 22·8	− 4·8
7 28·4	57·2	11 1·6	11 4·8	S. 22·1	−5·3	7 34·2	57·2	11 3·3	12 4·6	S. 22·3	− 5
8 27	57·1	11 38·1	12 7·9	S. 19·9	−5·3	8 33·9	57	11 40·7	13 3·5	S. 20·4	− 5·3
9 30·3	57·6	11 48	14 6·7	S. 16·1	−5·3	9 36·1	57·4	11 49·7	15 7	S. 15·5	− 5·5
10 31·5	57·2	11 46·8	16 0·4	S. 11	−5·8	10 30·9	57·6	11 45·6	17 2·2	S. 11·1	− 5·5
11 32·2	57·9	11 33·1	17 4·6	S. 5·1	−5·8	11 30·5	57·3	11 35·3	18 3·4	S. 6·2	− 5·1
Upper Transits, A.M.						Lower (Interpolated) Transits, P.M.					
32·8	57·1	11 20·7	17 9·4	N. 0·8	− 5·2	30·9	57·7	11 18·5	19 1·8	N. 1·6	− 5·8
1 29·1	57·4	11 6·2	17 10·2	N. 6·3	− 5·5	1 31·2	57·3	11 4·2	19 0·8	N. 6·9	− 5·1
2 27·4	57·7	10 50·8	17 5·9	N. 12	− 4·9	2 31·9	57·6	10 48·2	18 6·5	N. 12·9	− 5·6
3 29·5	56·8	10 34·5	16 1·4	N. 16·9	− 4·9	3 33·2	57	10 32·4	16 9·5	N. 16·7	− 5·2
4 29	56·8	10 21·4	14 8·7	N. 20·1	− 5	4 33	56·9	10 21·1	15 2·7	N. 20·2	− 5·6
5 25·6	56·4	10 17·3	13 0	N. 21·5	− 5	5 30·3	56·7	10 18·1	13 3·9	N. 22·1	− 5·3
6 31·8	56·6	10 31·4	11 7·1	N. 22·2	− 4·7	6 31	56·4	10 29	11 5·3	N. 23·3	− 5·2
7 27·6	56·9	11 5·5	11 7·2	N. 22	− 5·1	7 32·4	56·6	11 9·2	11 4·8	N. 22·1	− 5·3
8 26·7	56·7	11 39·5	12 10·5	N. 20·5	− 5·1	8 32·4	56·7	11 42·2	12 9·8	N. 19·8	− 5·4
9 29·1	56·7	11 51·8	14 10·8	N. 15·5	− 5·6	9 30·1	56·9	11 52·2	14 2·4	N. 16·8	− 5·1
10 28·3	57·5	11 45·2	16 9	N. 11·3	− 5·8	10 31·5	56·9	11 47·1	15 10·5	N. 11·1	− 6
11 30	56·9	11 37·3	17 9·1	N. 5·9	− 5·4	11 31·6	57·6	11 35·2	17 3·5	N. 5	− 5·6

October.

Moon's Transit.	Hor. Par.	Interval.	Height.	Moon's Declination.	Equation of Time.	Moon's Transit.	Hor. Par.	Interval.	Height.	Moon's Declination.	Equation of Time.
Upper Transits, P.M.						Lower (Interpolated) Transits, A.M.					
28·6	57	11 20·6	18 5·8	S. 11·5	−14·4	30·6	57·4	11 18·3	18 2·7	S. 11·7	−14
1 26·5	57·7	11 2	18 7·7	S. 16·5	−14·2	1 27·6	57·5	11 3·2	18 3·1	S. 15·8	−13·8
2 24	57·4	10 43·6	17 10·8	S. 19·8	−14	2 30·7	57·3	10 41·1	17 6·4	S. 20·5	−14
3 22·5	57·6	10 28	16 8·5	S. 22	−14·1	3 33·1	57·7	10 25·8	16 8·6	S. 22·4	−13·7
4 26·1	57·2	10 16	14 10·8	S. 23	−14	4 34·3	57·4	10 14·7	15 0·6	S. 22·7	−13·8
5 30·3	57·4	10 11·6	12 10·3	S. 22·2	−14	5 33·3	57·4	10 11·6	13 7·8	S. 21·9	−13·7
6 27·7	57	10 27	11 5·5	S. 20·3	−14·3	6 34·2	56·5	10 30·7	11 11·6	S. 20·7	−13·8
7 27·3	57·3	11 6·6	11 5·7	S. 16·9	−13·9	7 36·4	57·2	11 11·5	12 6·2	S. 16·1	−14·2
8 31·3	57	11 45·7	12 11·8	S. 12·8	−14·1	8 34·5	57	11 44·9	13 10·6	S. 12·8	−13·9
9 30·5	57·1	11 54	14 7	S. 5·9	−14·7	9 33·3	57	11 54·4	15 8·2	S. 5·9	−14·1
10 28·5	57·4	11 47·9	16 1·8	S. 0·5	−14	10 29·5	57	11 49·6	17 3·4	S. 0·5	−14·3
11 27·8	57	11 37·3	17 3·7	N. 5·0	−14·1	11 30·6	57·7	11 32·9	18 7·1	N. 5·2	−14·1
Upper Transits, A.M.						Lower (Interpolated) Transits, P.M.					
29·7	57·7	11 18·4	18 5·4	N. 11·1	−14·2	28·9	56·9	11 19·9	18 9·5	N. 11·2	−14
1 30·5	56·9	11 2·3	17 8·9	N. 16·8	−14	1 31·8	57·3	11 0·2	18 7·2	N. 16·5	−14·4
2 30·5	57·3	10 43	17 4·2	N. 20·5	−14·2	2 32·7	57·2	10 41·3	17 9·3	N. 20·6	−14·1
3 27·8	57	10 27·3	16 0·9	N. 21·5	−13·9	3 32·4	57·2	10 25·8	16 5·9	N. 21·9	−14
4 26·8	57·4	10 14·4	14 10·8	N. 23·2	−13·8	4 35·2	57	10 13·1	14 1·6	N. 23·4	−14·1
5 27·8	56·8	10 11·9	13 2·1	N. 22·2	−13·6	5 36·5	56·5	10 13·5	12 9·1	N. 21·6	−14
6 26	56·6	10 27·3	11 11·7	N. 20·6	−13·9	6 36·3	56·8	10 32·5	11 3·3	N. 20·1	−14·2
7 29·5	56·7	11 12·4	11 10·7	N. 16·9	−14·1	7 31·8	56·8	11 14	11 6	N. 17·1	−14
8 30·3	57	11 44·9	13 11·3	N. 12·6	−13·7	8 30·4	56·6	11 47·9	13 0·4	N. 12·6	−14
9 28·8	57	11 55·3	15 3·3	N. 7·1	−14	9 31·4	57·2	11 54·9	15 1·4	N. 6·3	−14·2
10 29·2	57·2	11 49·1	17 1·8	0	−12·2	10 32·3	57·1	11 48·9	16 7·1	0	−14
11 29	57·3	11 35·4	18 2·2	S. 5·5	−13·8	11 32	57·3	11 35	17 8	S. 6·4	−14·4

Table XXVII. (Continued.)

November.

Upper Transits, P.M.						Lower (Interpolated) Transits, A.M.					
Moon's Transit.	Hor. Par.	Interval.	Height.	Moon's Declination.	Equation of Time.	Moon's Transit.	Hor. Par.	Interval.	Height.	Moon's Declination.	Equation of Time.
h m	′	h m	ft. in.	°	m	h m	′	h m	ft. in.	°	m
31·5	56·9	11 14·9	17 11	S. 19·9	−14·5	33·3	57·6	11 12·7	18 0·5	S. 20·4	−14·5
1 28·9	57·2	11 0·3	17 9·2	S. 21·9	−14·5	1 31·7	57·5	10 55·5	18 0·5	S. 22·2	−14·2
2 28·3	57·3	10 38·2	16 9·1	S. 23	−14·1	2 29·1	57·5	10 38·1	16 10·9	S. 23	−14·4
3 29·8	57·3	10 22·8	15 8	S. 22·1	−14	3 28·2	57·2	10 23·7	16 8·2	S. 22·6	−14·2
4 28·4	57·3	10 14·6	14 7	S. 20·3	−14·4	4 31·1	57·1	10 15·1	15 2·4	S. 20·4	−14·5
5 27·2	57·2	10 16·8	12 10·3	S. 16·6	−14·2	5 36·9	57·1	10 18·3	13 10·8	S. 16·2	−14·3
6 29·4	56·9	10 41	11 9·1	S. 12·1	−14·2	6 32·1	57·2	10 40·4	13 4·2	S. 12·5	−14·5
7 29·4	57	11 16·3	11 11·8	S. 6·4	−14·3	7 29·3	57	11 19·6	13 0	S. 6·5	−14
8 27·2	57·4	11 46	13 6·1	S. 1·2	−14·5	8 30·1	56·9	11 46·2	14 3	S. 0·7	−14·3
9 29·7	57·1	11 53·3	15 0	N. 5·6	−14·3	9 27·3	57·4	11 51·9	15 9·7	N. 4·6	−14·6
10 32·2	57·7	11 44·5	16 4·5	N. 10·7	−14·3	10 26·7	57·2	11 47·1	16 9·3	N. 10·6	−14·5
11 31·5	57·1	11 33·6	16 11·1	N. 17·2	−14·3	11 29·9	57·2	11 32·6	17 6·5	N. 16·5	−14·4
Upper Transits, A.M.						Lower (Interpolated) Transits, P.M.					
35·6	57·1	11 14·7	17 8·8	N. 19·8	−14·4	30·2	57·4	11 15·9	18 2·2	N. 19·7	−14·4
1 33·1	57·3	10 56·8	17 5	N. 22·0	−14·3	1 32·1	56·8	10 57·4	17 3·1	N. 22·7	−14·2
2 30·9	57·1	10 40·5	16 11	N. 23·1	−14·4	2 33·5	57·1	10 39·1	16 10·9	N. 22·6	−14·4
3 26·8	56·7	10 26	16 1·4	N. 22·1	−14·3	3 34·2	56·9	10 24·4	15 7·3	N. 21·9	−14·1
4 30	56·5	10 14·6	14 9·3	N. 20·6	−14·5	4 35·1	56·8	10 14·1	14 0·5	N. 19·8	−14
5 31·4	56·9	10 16·9	13 5·4	N. 16·1	−14·2	5 28·1	56·8	10 17·5	12 11·7	N. 16·9	−14·2
6 28·3	51·1	19 37	12 10	N. 12·5	−14·4	6 31·1	56·6	10 43·4	11 9·4	N. 12·6	−14·4
7 29·3	56·6	11 21·7	12 10·3	N. 7·3	−14·5	7 33	57·2	11 24·7	12 7·6	N. 5·6	−14·1
8 28·4	57	11 47·7	14 1·2	N. 0·9	−14·6	8 31·5	56·9	11 50·3	13 10·8	N. 0·3	−14·5
9 28·6	57·5	11 54·1	16 0·7	S. 4·6	−14·7	9 31·7	57·4	11 53	15 8	S. 6·0	−14·4
10 30	57	11 47·9	16 10·4	S. 11·4	−14·3	10 27·6	57·5	11 46·9	16 9·1	S. 11·0	−14·4
11 32·7	57·8	11 30·5	17 11·9	S. 16·4	−14·6	11 29·5	57·2	11 33·6	17 9	S. 15·9	−14·5

December.

Upper Transits, P.M.						Lower (Interpolated) Transits, A.M.					
34·1	57·7	11 7·8	17 3·2	S. 23	− 3·3	26·8	57·3	11 13·5	17 8	S. 23·2	− 4
1 31·1	57·4	10 53·7	17 2	S. 22·2	− 3·6	1 31·3	57·3	10 53·8	17 9	S. 21·8	− 3·3
2 29·8	57·1	10 40·2	16 7·8	S. 19·2	− 3·1	2 32·6	57·4	10 38·8	17 5·8	S. 19·2	− 3·7
3 30·8	57·1	10 29·5	15 6·4	S. 15·4	− 3·2	3 27·5	57·6	10 27·9	16 11·6	S. 16	− 3·1
4 33·1	57·1	10 22·8	14 5	S. 11·2	− 3·5	4 27·9	56·7	10 23·1	15 9·6	S. 11·2	− 3·3
5 32·9	56·8	10 31·5	13 1·2	S. 4·4	− 3	5 30·8	57·1	10 29·5	14 11·3	S. 5·6	− 3·6
6 32·7	56·9	10 55·2	13 1·1	N. 1·0	− 2·3	6 31·8	57	10 52·1	13 8·8	N. 1·1	− 3·8
7 30·7	57·3	11 23·3	12 8·6	N. 6·7	− 3·4	7 30	57	11 24	13 11·1	N. 6·6	− 3·4
8 29·2	57	11 46·9	13 9·5	N. 12·3	− 3·9	8 29·7	57·2	11 44·5	14 6·6	N. 12·3	− 4
9 29·2	57·4	11 48·7	15 4·4	N. 16·8	− 3·4	9 27·9	57·7	11 46·5	15 10·2	N. 15·9	− 4
10 25·5	57·5	11 43·5	16 0·5	N. 19·7	− 3·2	10 32·6	57	11 41·7	16 4·4	N. 20·9	− 3·6
11 28·9	57·3	11 28·3	17 0·6	N. 21·7	− 3·4	11 35·2	57·2	11 28·5	16 11·5	N. 22	− 3·5
Upper Transits, A.M.						Lower (Interpolated) Transits, P.M.					
32	57	11 11·3	17 2·2	N. 23·6	− 4·1	33·2	57	11 13·3	17 1·6	N. 22·4	− 3·5
1 33·9	57·1	10 53·8	17 4·6	N. 21·9	− 3·2	1 29·2	57·2	10 57·5	16 11·4	N. 21·9	− 3·4
2 31	56·9	10 39·8	16 9·6	N. 19·1	− 3·4	2 25·3	57	10 42·2	16 3·2	N. 19·6	− 3·5
3 29	56·9	10 27·7	16 1·7	N. 16	− 3·4	3 29·2	56·4	10 29·5	15 0	N. 16·2	− 3·4
4 31·6	56·6	10 23·8	14 11·6	N. 11	− 2·9	4 32·2	57	10 26·3	14 4·3	N. 10·2	− 2·8
5 31·2	56·7	10 30	14 2·3	N. 6	− 4·2	5 28·6	56·6	10 31·9	12 9·2	N. 5·1	− 2·5
6 32·4	57	10 53·8	12 11	S. 1·5	− 2·8	6 29·3	56·8	10 52·8	12 6·7	S. 0·3	− 3·9
7 32·4	56·9	11 26·5	13 1·1	S. 6·8	− 4·1	7 31·6	57·3	11 24·2	13 0·5	S. 6·7	− 3·5
8 29·8	57·5	11 45·1	14 0·2	S. 12·3	− 4	8 29·7	57·2	11 44·9	13 11·1	S. 12·5	− 3·2
9 24·6	57·5	11 49·1	15 2·7	S. 16·1	− 4	9 31·9	57·4	11 48·9	15 4·4	S. 17·5	− 3·2
10 27·2	57·6	11 43·2	16 5	S. 19·8	− 3·9	10 32·8	58·1	11 41·4	16 2·9	S. 20·2	− 3·8
11 34·2	57·7	11 26·9	17 0	S. 22·7	− 3·4	11 26·6	57	11 29·7	17 4·8	S. 21·5	− 3·5

Table XXVIII.

Upper Transits, P.M.

Moon's Transit. P.M.	January.		February.		March.		April.		May.		June.	
	Diurnal inequality.	Moon's Declination.	Diurnal inequality.	Moon's Declination.	Diurnal inequality.	Moon's Declination.	Diurnal inequality.	Moon's Declination.	Diurnal inequality.	Moon's Declination.	Diurnal inequality.	Moon's Declination.
h m	feet.	°	feet.	°	feet.	°	feet.	°	feet.	°	feet.	°
30	−·41	S. 18·6	−·52	S. 10·1	−·75	N. 1·5	−·18	N. 13·1	−·33	N. 20·9	+·20	N. 22·8
1 30	−·14	S. 15	−·10	S. 2·8	−·82	N. 8·5	−·50	N. 16·8	−·11	N. 22·2	+·16	N. 22·6
2 30	−·06	S. 10	−·73	N. 2·7	−·38	N. 12·6	−·20	N. 20·6	+·05	N. 22·7	−·04	N. 19·4
3 30	−·22	S. 3·6	−·79	N. 8·6	−·41	N. 17·4	−·21	N. 22·7	+·09	N. 21·9	+·35	N. 15·4
4 30	−·56	N. 2·8	−·32	N. 13·5	−·39	N. 21·2	−·07	N. 22·7	+·19	N. 20·4	+·38	N. 11·1
5 30	−·60	N. 7·6	−·63	N. 18·2	−·43	N. 22·6	−·17	N. 22·1	+·21	N. 16	+·16	N. 5·1
6 30	−·51	N. 14	−·12	N. 21·3	−·11	N. 22·5	+·01	N. 20·2	+·32	N. 11·6	+·26	S. 0·7
7 30	−·62	N. 17·7	+·06	N. 22·5	−·15	N. 22·6	+·08	N. 15·6	+·28	N. 5·5	+·29	S. 7·5
8 30	−·17	N. 21·1	+·33	N. 22·8	0	N. 19·9	+·02	N. 10·8	+·20	S. 0·5	+·02	S. 11·3
9 30	−·07	N. 22·8	+·41	N. 21	+·07	N. 14·8	+·17	N. 5·7	+·10	S. 6·9	−·10	S. 18·0
10 30	+·03	N. 22·5	+·23	N. 19·1	+·27	N. 10·3	+·06	S. 0·7	+·16	S. 11·8	−·12	S. 20·1
11 30	+·30	N. 21·6	+·22	N. 14·5	+·07	N. 5	−·19	S. 7·0	−·06	S. 17·5	−·14	S. 22·4

	July.		August.		September.		October.		November.		December.	
30	+·25	N. 18·9	+·37	N. 11·2	+·40	0	+·02	S. 11·7	−·06	S. 19·9	0	S. 23
1 30	+·55	N. 15·3	+·17	N. 4·2	+·25	S. 7·3	+·34	S. 15·8	+·03	S. 21·9	−·12	S. 22·2
2 30	+·60	N. 10·8	+·54	S. 1·0	+·18	S. 11·5	+·19	S. 20·5	−·27	S. 23	−·14	S. 19·2
3 30	+·35	N. 4·8	+·17	S. 7·1	+·14	S. 17·2	+·05	S. 22·4	−·35	S. 22·1	−·35	S. 15·4
4 30	+·42	S. 1·1	+·29	S. 13	+·01	S. 20·4	+·01	S. 22·7	+·05	S. 20·3	−·48	S. 11·2
5 30	+·11	S. 7·6	+·17	S. 17	+·05	S. 22·4	−·48	S. 21·9	−·54	S. 16·6	−·69	S. 4·4
6 30	+·37	S. 13·3	+·08	S. 20·4	−·03	S. 22·8	−·19	S. 20·7	−·53	S. 12·1	+·02	N. 1·0
7 30	+·06	S. 17·7	+·16	S. 22·5	−·37	S. 22·3	−·44	S. 16·1	−·64	S. 6·4	−·49	N. 6·7
8 30	−·07	S. 20·3	−·24	S. 23	−·16	S. 20·4	−·42	S. 12·8	−·38	S. 1·2	−·26	N. 12·3
9 30	−·14	S. 22·4	−·22	S. 21·7	−·22	S. 15·5	−·55	S. 5·9	−·61	N. 5·6	+·03	N. 16·8
10 30	−·65	S. 22·7	−·31	S. 19·8	−·38	S. 11·1	−·45	S. 0·5	−·39	N. 10·7	−·28	N. 19·7
11 30	−·41	S. 21·8	−·41	S. 15·7	−·36	S. 6·2	+·43	N. 5·2	−·60	N. 17·2	−·08	N. 21·7

Upper Transits, A.M.

A.M.	January.		February.		March.		April.		May.		June.	
30	+·03	N. 18·3	+·28	N. 9·8	+·23	S. 1·9	−·08	S. 11·5	+·09	S. 19·8	−·67	S. 23·8
1 30	−·09	N. 14·9	+·71	N. 3·6	+·44	S. 6·8	+·22	S. 17·4	+·06	S. 22	−·42	S. 22
2 30	+·25	N. 10·4	+·62	S. 2·4	+·28	S. 13·1	+·24	S. 20·7	−·16	S. 22·7	−·59	S. 18·9
3 30	+·39	N. 4·7	+·07	S. 7·4	+·44	S. 18·2	+·10	S. 22	−·28	S. 22·1	−·65	S. 17·1
4 30	+·36	S. 4	+·13	S. 13·8	+·15	S. 20·8	−·12	S. 23·4	−·19	S. 19·7	−·39	S. 10·6
5 30	+·48	S. 8·1	+·21	S. 18·7	−·06	S. 22·3	−·18	S. 22·1	−·51	S. 16·6	−·63	S. 6·0
6 30	+·26	S. 13·7	−·11	S. 20·8	+·03	S. 23·2	−·14	S. 19·9	−·78	S. 12·3	−·44	N. 0·9
7 30	+·16	S. 18·3	−·35	S. 22·9	−·17	S. 21·6	−·38	S. 16	−·50	S. 6·0	−·47	N. 6·9
8 30	+·06	S. 21	−·06	S. 23	−·27	S. 19·4	−·50	S. 12	−·48	N. 0·5	−·27	N. 12·4
9 30	−·11	S. 22·3	−·49	S. 21·7	−·20	S. 15·6	−·46	S. 6	−·52	N. 7	−·16	N. 16·8
10 30	−·46	S. 22·5	−·15	S. 18·2	−·64	S. 11·2	−·20	N. 0·6	−·13	N. 11·8	−·08	N. 20·7
11 30	−·47	S. 22	−·48	S. 15	−·31	S. 5·4	−·53	N. 6·3	−·16	N. 16·5	+·03	N. 22·0

	July.		August.		September.		October.		November.		December.	
30	−·49	S. 19·7	−·60	S. 10·7	−·51	N. 1·6	−·07	N. 11·2	−·20	N. 19·8	− ·08	N. 23·6
1 30	−·75	S. 16·5	−·60	S. 5·2	−·54	N. 6·9	−·54	N. 16·5	−·29	N. 22	+ ·08	N. 21·9
2 30	−·63	S. 10·9	−·75	N. 0·6	−·44	N. 12·9	−·26	N. 20·6	−·07	N. 23·1	+ ·01	N. 19·1
3 30	−·61	S. 5·0	−·59	N. 7·2	−·39	N. 16·7	−·44	N. 21·9	+·06	N. 22·1	+ ·23	N. 16
4 30	−·53	N. 1·7	−·48	N. 12·4	−·24	N. 20·2	+·07	N. 23·4	+·27	N. 20·6	+ ·04	N. 11
5 30	−·57	N. 6·7	−·50	N. 17·9	−·32	N. 22·1	+·47	N. 21·6	+·12	N. 16·1	+ ·38	N. 6
6 30	−·51	N. 12·4	−·31	N. 20·5	−·03	N. 23·3	+·23	N. 20·1	+·55	N. 12·5	− ·13	S. 1·5
7 30	−·39	N. 17·3	−·17	N. 21·6	−·12	N. 22·1	−·06	N. 17·1	+·24	N. 7·3	−1·14	S. 6·8
8 30	−·05	N. 19·5	+·16	N. 23·1	+·04	N. 19·8	+·57	N. 12·6	+·19	N. 0·9	− ·05	S. 12·3
9 30	+·04	N. 22·1	+·22	N. 21·9	+·14	N. 16·8	+·18	N. 6·3	+·48	S. 4·6	− ·04	S. 16·1
10 30	+·33	N. 22·6	+·14	N. 19·5	+·41	N. 11·1	+·33	S. 0	+·13	S. 11·4	+ ·05	S. 19·8
11 30	+·21	N. 22·1	+·31	N. 15·3	+·07	N. 5·0	+·27	S. 6·4	+·44	S. 16·4	− ·17	S. 22·7

Table XXVIII. (Continued.)

Lower (Interpolated) Transits, A.M.

Moon's Transit, A.M.	January. Diurnal inequality.	January. Moon's Declination.	February. Diurnal inequality.	February. Moon's Declination.	March. Diurnal inequality.	March. Moon's Declination.	April. Diurnal inequality.	April. Moon's Declination.	May. Diurnal inequality.	May. Moon's Declination.	June. Diurnal inequality.	June. Moon's Declination.
	feet.	°	feet.	°	feet.	°	feet.	°	feet.	°	feet.	°
30	+·61	S. 19·1	+·86	S. 10·1	−·03	N. 1·3	+·28	N. 12·1	+·01	N. 20·4	−·33	N. 23
1 30	+·50	S. 14·6	+·83	S. 3·5	+·59	N. 7·1	+·24	N. 17·1	−·30	N. 21·9	−·45	N. 21·7
2 30	+·93	S. 9·9	+·56	N. 2·1	−·32	N. 13·0	−·08	N. 20·3	−·37	N. 23·0	−·68	N. 19·4
3 30	+·78	S. 3·6	+·52	N. 8·5	+·24	N. 18·2	−·05	N. 22·0	−·33	N. 22·1	−·54	N. 16·3
4 30	+·65	N. 2·0	+·16	N 13·6	+·12	N. 20·5	−·29	N. 22·9	−·33	N. 19·6	−·59	N. 11·8
5 30	+·82	N. 7·1	+·43	N. 18·8	+·08	N. 22·2	−·27	N. 22·1	−·45	N. 16·0	−·34	N. 4·7
6 30	+·28	N. 13·7	+·42	N. 21·1	−·32	N. 22·4	−·24	N. 18·9	−·26	N. 12·6	−·49	N. 0·7
7 30	+·48	N. 18·2	+·07	N. 21·9	−·15	N. 21·8	−·11	N. 16·2	+·08	N. 5·5	−·14	S. 7·1
8 30	+·17	N. 20·3	−·24	N. 23·4	−·13	N. 19·5	+·03	N. 12·0	+·06	N. 0·5	+·06	S. 12·0
9 30	−·03	N. 22·5	−·17	N. 21·6	−·28	N. 15·0	+·05	N. 5·1	+·15	S. 7·3	+·20	S. 16·6
10 30	−·08	N. 22·3	−·06	N. 18·7	−·54	N. 10·0	−·07	S. 0·4	+·17	S. 11·9	+·24	S. 20·0
11 30	−·07	N. 22·0	−·02	N. 15·0	−·09	N. 4·7	+·18	S. 7·5	+·35	S. 16·4	+·33	S. 22·3

	July.		August.		September.		October.		November.		December.	
30	−·31	N. 19·9	−·47	N. 10·4	−·44	S. 0·8	−·30	S. 11·5	+ ·07	S. 20·4	+ ·43	S. 23·2
1 30	−·36	N. 16·2	−·52	N. 6·4	−·31	S. 6·2	−·02	S. 16·5	+ ·28	S. 22·2	+ ·47	S. 21·8
2 30	+·47	N. 10·3	−·43	S. 1·2	−·48	S. 12·5	−·08	S. 19·8	− ·10	S. 23	+ ·70	S. 19·2
3 30	−·59	N. 4·6	−·26	S. 6·8	−·12	S. 17	+·31	S. 22	+ ·64	S. 22·6	+1·04	S. 16
4 30	−·30	S. 1·5	−·34	S. 13	−·09	S. 19·6	+·40	S. 23	+ ·72	S. 20·4	+ ·82	S. 11·2
5 30	−.22	S. 7·6	−·08	S. 18·1	+·16	S. 22·3	+·57	S. 22·2	+ ·68	S. 16·2	+1·11	S. 5·6
6 30	−·14	S. 12·6	+·12	S. 21·3	−·04	S. 23·2	+·35	S. 20·3	+1·07	S. 12·5	+ ·69	N. 1·1
7 30	−·37	S. 17·6	+·36	S. 22·6	+·72	S. 22·1	+·46	S. 16·9	+ ·38	S. 6·5	+ ·71	N. 6·6
8 30	+·30	S. 21·3	+·41	S. 22·6	+·32	S. 19·9	+·40	S. 12·8	+ ·30	S. 0·7	+ ·48	N. 12·3
9 30	+·40	S. 22·0	+·33	S. 22·2	+·60	S. 16·1	+·46	S. 5·9	+ ·24	N. 4·6	+ ·53	N. 15·9
10 30	+·43	S. 23·3	+·62	S. 18·4	+·78	S. 11	+·46	S. 0·5	+ ·09	N. 10·6	− ·04	N. 20·9
11 30	+·41	S. 22·4	+·81	S. 15	+·58	S. 5·1	+·67	N. 5	+ ·04	N. 16·5	− ·23	N. 22

Lower (Interpolated) Transits, P.M.

P.M.	January.		February.		March.		April.		May.		June.	
30	−·33	N. 18·1	−·31	N. 9·3	−·18	S. 1·3	+·21	S. 13·0	+·34	S. 20·3	+·25	S. 22·9
1 30	−·41	N. 15·1	+·19	N. 3·9	+·01	S. 8·0	+·20	S. 16·5	+·46	S. 22·3	+·45	S. 22·5
2 30	−·40	N. 9·4	−·28	S. 2·5	−·03	S. 12·7	+·21	S. 20·7	+·46	S. 22·6	+·60	S. 19·7
3 30	−·18	N. 4·8	−·67	S. 7·2	0	S. 17·6	+·25	S. 22·6	+·59	S. 22·0	+·36	S. 15·6
4 30	−·52	S. 3·3	−·24	S. 13·9	+·09	S. 21·1	+·49	S. 22·6	+·46	S. 20·3	+·90	S. 11·0
5 30	−·50	S. 8·7	−·02	S. 18·2	+·09	S. 22·2	+·44	S. 22·2	+·54	S. 15·5	+·62	S. 5·3
6 30	+·31	S. 14·0	−·32	S. 21·2	+·45	S. 22·9	+·44	S. 20·5	+·68	S. 11·6	+·76	N. 0·9
7 30	+·37	S. 18·0	+·02	S. 22·3	+·57	S. 21·7	+·27	S. 16·3	+·49	S. 5·5	+·56	N. 7·4
8 30	−·03	S. 20·5	+·33	S. 22·8	+·22	S. 20·1	+·27	S. 10·8	+·18	N. 0·9	+·14	N. 12·1
9 30	+·13	S. 22·8	+·40	S. 22·0	+·17	S. 14·8	+·26	S. 5·1	+·23	N. 6·3	+·03	N. 17·6
10 30	+·38	S. 22·8	+·15	S. 19·5	+·08	S. 9·8	+·15	N. 0·8	−·01	N. 11·9	−·09	N. 20·2
11 30	+·69	S. 21·6	−·09	S. 14·1	+·23	S. 5·0	+·15	N. 7·0	−·13	N. 17·0	−·31	N. 22·2

	July.		August.		September.		October.		November.		December.	
30	+·37	S. 19·2	+ ·98	S. 10·3	+·84	N. 0·8	+·27	N. 11·1	+·23	N. 19·7	− ·11	N. 22·4
1 30	+·74	S. 15·7	+ ·56	S. 5·4	+·69	N. 6·3	+·32	N. 16·8	−·46	N. 22·7	− ·35	N. 21·9
2 30	+·61	S. 11·2	+1·04	N. 1·5	+·68	N. 12	+·19	N. 20·5	−·05	N. 22·6	− ·60	N. 19·6
3 30	+·65	S. 4·2	+ ·17	N. 6·7	+·38	N. 16·9	+·08	N. 21·5	−·31	N. 21·9	− ·90	N. 16·2
4 30	+·81	N. 1·8	+ ·50	N. 13·4	+·37	N. 20·1	−·47	N. 23·2	−·36	N. 19·8	− ·57	N. 10·2
5 30	+·58	N. 7·0	+ ·35	N. 16·8	+·13	N. 21·5	−·24	N. 22·2	−·40	N. 16·9	−1·07	N. 5·1
6 30	+·31	N. 13·3	+ ·17	N. 20·8	−·18	N. 22·2	−·36	N. 20·6	−·50	N. 12·6	− ·50	S. 0·3
7 30	+·42	N. 17	+ ·02	N. 22·3	−·37	N. 22	−·46	N. 16·9	−·04	N. 5·6	− ·18	S. 6·7
8 30	−·08	N. 21·4	− ·28	N. 23·1	−·10	N. 20·5	−·33	N. 12·6	−·09	N. 0·3	− ·15	S. 12·5
9 30	−·22	N. 22·2	− ·34	N. 21	−·58	N. 15·5	−·05	N. 7·1	+·01	S. 6·0	− ·02	S. 17·5
10 20	−·35	N. 22·9	− ·47	N. 19·2	−·54	N. 11·3	−·29	0	−·95	S. 11·0	− ·18	S. 20·2
11 30	−·43	N. 21·4	− ·54	N. 16	−·56	N. 5·9	−·29	S. 5·5	+·26	S. 15·9	+ ·23	S. 21·5

Diurnal inequality in the height of high water at Liverpool from 13,327 observations. _ See Tables XXVIII and XXIX.

Upper Transits ___ *P. M.*
Do. ___ *A. M.*
Lower Interpolated Transits ___ *A. M.*
Do. ___ *P. M.*

The letters A.M., P.M. refer to the time of the moon's transit preceeding the time of high water.
The continuous lines have been laid down from Table XXIX. which is conjectural and formed by arbitrary alterations from Table XXVIII. The interval between these lines may be considered as the difference between the height of the morning and evening Tide for the middle of each month.

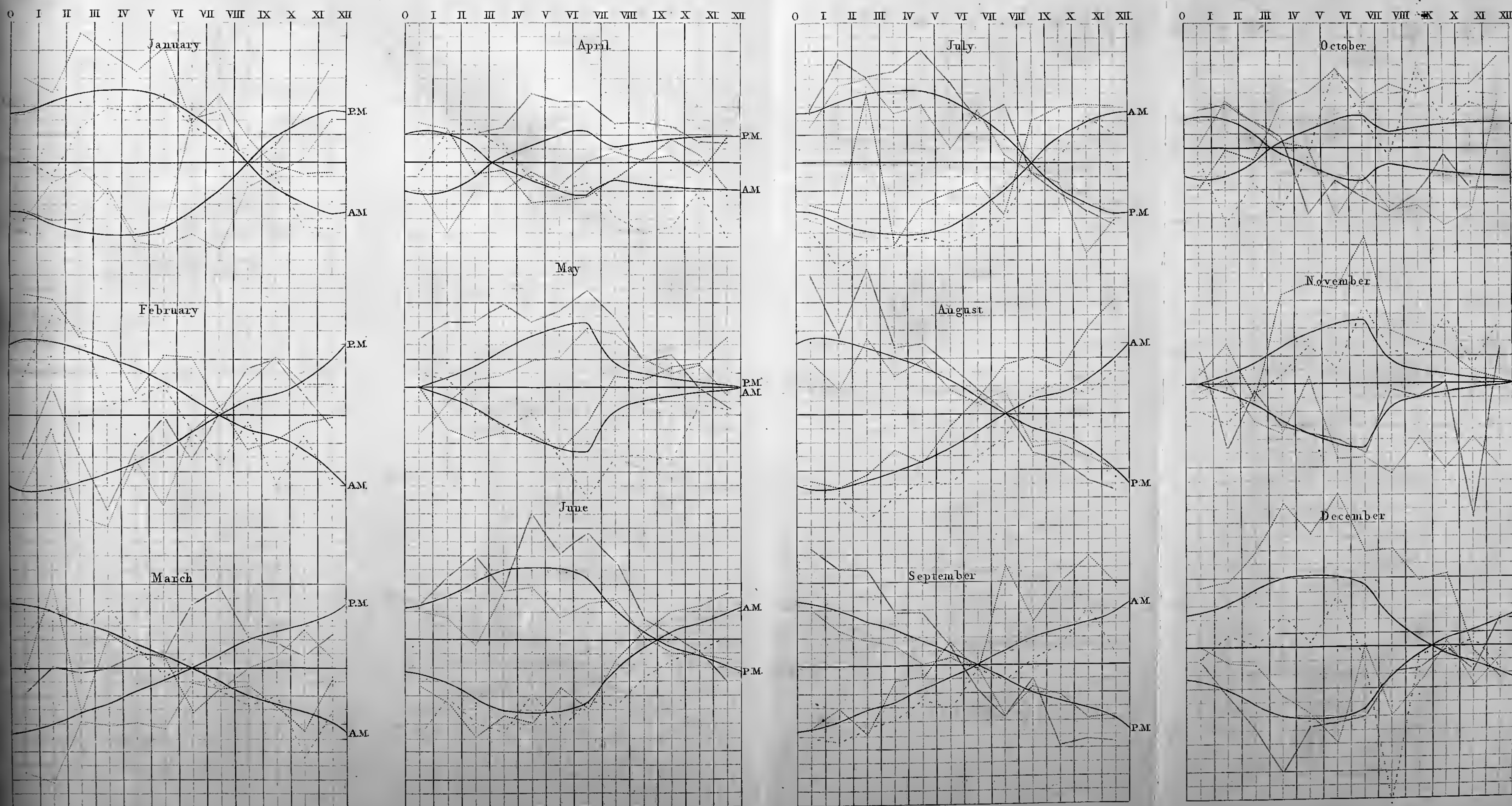


TABLE XXIX.

Conjectural, formed by interpolation and by arbitrary alterations from Table XXVIII.

Moon's Transit.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
h	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.
P.M. 0	−·35	−·50	−·45	−·20	·00	+·22	+·35	+·50	+·45	+·20	00	−·22
1	−·40	−·53	−·42	−·22	+·03	+·28	+·40	+·53	+·42	+·22	−·03	−·28
2	−·47	−·50	−·35	−·17	+·10	+·36	+·47	+·50	+·35	+·17	−·10	−·36
3	−·50	−·45	−·30	−·02	+·20	+·46	+·50	+·45	+·30	+·02	−·20	−·46
4	−·51	−·38	−·20	+·09	+·30	+·51	+·51	+·38	+·20	−·09	−·30	−·51
5	−·50	−·30	−·12	+·16	+·40	+·51	+·50	+·30	+·12	−·16	−·40	−·51
6	−·40	−·18	−·05	+·23	+·46	+·50	+·40	+·18	+·05	−·23	−·46	−·50
7	−·28	−·07	+·05	+·15	+·30	+·32	+·28	+·07	−·05	−·15	−·30	−·32
8	−·10	+·07	+·16	+·12	+·12	+·13	+·10	−·07	−·16	−·12	−·12	−·13
9	+·10	+·12	+·22	+·15	+·08	·00	−·10	−·12	−·22	−·15	−·08	00
10	+·24	+·19	+·28	+·18	+·04	−·09	−·24	−·19	−·28	−·18	−·04	+·09
11	+·33	+·21	+·36	+·19	+·02	−·15	−·33	−·21	−·36	−·19	−·02	+·15
A.M. 0	+·35	+·50	+·45	+·20	·00	−·22	−·35	−·50	−·45	−·20	00	+·22
1	+·40	+·53	+·42	+·22	−·03	−·28	−·40	−·33	−·42	−·22	+·03	+·28
2	+·47	+·50	+·35	+·17	−·10	−·36	−·47	−·50	−·35	−·17	+·10	+·36
3	+·50	+·45	+·30	+·02	−·20	−·46	−·50	−·45	−·30	−·02	+·20	+·46
4	+·51	+·38	+·20	−·09	−·30	−·51	−·51	−·38	−·20	+·09	+·30	+·51
5	+·50	+·30	+·12	−·16	−·40	−·51	−·50	−·30	−·12	+·16	+·40	+·51
6	+·40	+·18	+·05	−·23	−·46	−·50	−·40	−·18	−·05	+·23	+·46	+·50
7	+·28	+·07	−·05	−·15	−·30	−·32	−·28	−·07	+·05	+·15	+·30	+·32
8	+·10	−·07	−·16	−·12	−·12	−·13	−·10	+·07	+·16	+·12	+·12	+·13
9	−·10	−·12	−·22	−·15	−·08	00	+·10	+·12	+·22	+·15	+·08	00
10	−·24	−·19	−·28	−·18	−·04	+·09	+·24	+·19	+·28	+·18	+·04	−·09
11	−·33	−·21	−·36	−·19	−·02	+·15	+·33	+·21	+·36	+·19	+·02	−·15

IX. *Geometrical Investigations concerning the Phenomena of Terrestrial Magnetism. Second Series:—On the Number of Points at which a magnetic needle can take a position vertical to the Earth's surface. By* THOMAS STEPHENS DAVIES, *Esq., F.R.S. L. & E. F.R.A.S. Royal Military Academy, Woolwich.*

Received January 28,—Read February 4, 1836.

THE plan and objects of this series of papers have been so far explained already as to render it superfluous to enumerate them here.

At the close of my former paper* I have given the rectangular equation of the curve of verticity,—or that in any point of which a magnetic needle being placed, its line of direction would pass through the centre of the earth, and consequently be vertical to the horizon at the point where it cut the surface: but as the form and character of the curve could not be directly obtained from that equation, nor from any other into which it could be transformed; and as, moreover, the process by which they could be obtained required considerable preliminary investigations, I preferred to leave it in that state rather than give the partial and incomplete solution, which, in the midst of the deep domestic affliction that I was involved in, I must then have done. It is here, however, by pursuing a different course fully determined, as far, at least, as it is subservient to our physical problem: and I did not feel myself at liberty to insert in a paper on that subject any collateral inquiries, which, however interesting in a geometrical point of view, would be irrelevant to the immediate discussion professedly before me.

By transforming the rectangular equation (76.) of the curve of verticity into a polar one, I have shown that there are two values of the radius-vector, and only two, for every value of the polar angle; and a few of the consequences which seemed likely to facilitate our inquiry are deduced from it. The genesis of the curve, however, pointed out the necessity of a more complete examination of the magnetic curve itself: and it will appear that even in a geometrical view, and independently of any of its physical applications, this latter curve (the magnetical) is amongst the most elegant and interesting we possess. The method of investigation is, as far as I know, a new one: but it is one that in many cases, besides the present, may be very effective, and therefore valuable. Still as I could not lay down the principles of the method in this paper, so as to justify my processes, I have so modified it by a combination with the method of rectangular coordinates as fully to answer my present purposes. The method consists in taking as the variables in the equation of the curve, the *angles*

* Philosophical Transactions, 1835, p. 246, equation 76.

made by radiants drawn from two given points (in the present case the magnetic poles) with the line joining those two points: and it was suggested to my mind several years ago when considering a question proposed by Professor WALLACE in Professor LEYBOURN'S Mathematical Repository, viz., to "rectify the magnetic curve." The modification I have here used consists in the expression of the differential coefficients of a rectangular equation in terms of the polar angles, $\theta_{\prime}$ and $\theta_{\prime\prime}$ and the constants of the given equation: but I hope soon to complete a dissertation on coordination generally, and to give the necessary differential expressions that are requisite in the investigation of loci, plane, spherical, and solid; in which case several of the following processes may be considerably abbreviated.

I was compelled to employ the method here specified in consequence of the complicated form under which the rectangular and polar equations of the magnetic curve present themselves, being such as not to encourage the least hope of effecting my object by means of either of them, or by both of them conjointly.

From these investigations it appears, That both systems of branches, the convergent and the divergent, are comprised in the same angular equation of the magnetic curve already referred to, and deduced at page 238 of the Philosophical Transactions of last year: that the divergent branches on one side of the magnetic axis are continuous (algebraically and geometrically) of the convergent branches on the other, to the same parameter β: that the divergent branches are asymptotic, and the geometrical construction of the asymptote is very easy: that the continuous branches have the poles for points of inflexion, and that these are the only points of inflexion within finite limits, of the whole system: that the geometrical construction of a tangent at any point, that is the direction of a small needle whose centre is at that point, is always possible, and the process very simple: together with other properties not less interesting, though less easy to express in brief phraseology. An elegant curve is thus brought within the domain of geometry, which, when its properties are fully developed, will, I think, be second only to the conic sections themselves in point of mathematical interest: whilst its adaptation to at least one important physical inquiry will tend to enhance its value in the estimation of those who take an interest in such applications as are now, or may hereafter be, made of it, and even in this respect render it not inferior in point of value to any other loci except the conic sections, and perhaps the logarithmic curve*.

As both systems of branches of the magnetic curve are found to be involved in the same angular equation, and in the same rectangular one also by means of the double

* It is certainly a remarkable circumstance, that so few of our most elegant curves (geometrically considered) are capable of being rendered subservient to physical inquiries: for with the exceptions above mentioned, there is, besides the cycloid and the harmonic curve, with perhaps one or two of the spirals, scarcely one which could not be expunged from our geometry without any serious injury to physical science. This, together with the fact that in the dynamical problems which occur in physics, it is found to be generally most convenient to assume the time as the independent variable, has led some writers, too hastily as it appears to me, to conclude

sign of the radicals,—so also, as we should expect from the same principles being employed in both cases, in the equation of the curve of verticity the branches adapted to like poles and those adapted to unlike poles, are expressed in the same equation given in my former paper. It required, then, the previous separation of the two systems in the magnetic curve as a preliminary step to the separation of those in the curve of verticity. Such is the course I have pursued, but I have carefully abstained from the insertion of any properties of either curve except those which were essential in the determination of the number of points of *terrestrial verticity*. However, to avoid the long and complicated reductions into which I found the algebraical investigation was leading me, I have in one instance recurred to a geometrical method of investigation, founded on an elegant property of the magnetic curve, (first given by Professor LESLIE in his Geometrical Analysis, p. 400,) by means of which, and the genesis of the curve of verticity which it suggests, enables us to establish the required conclusion with ease and simplicity.

No apology is necessary, I conceive, for the introduction of new methods of investigating a problem in pure science, where those already existing are either insufficient or inconveniently operose in their application to that problem: nor yet for the employment of several different methods which in treatises on pure science are usually, and in good taste, kept distinct, when we are investigating a physical problem to which any one of them, taken separately, is inadequate. I readily and fully admit the desirableness and superior elegance of unity of mathematical method, even in physical investigations; and, doubtless, repeated efforts made by different geometers tends to the gradual formation of such united and systematic methods of development: nevertheless it is rarely the case that a unity in the full sense of the word can be brought into the mathematics of physical inquiries,—the unity that exists in the most perfect of them being more apparent than real. Mere symbolical notation does not constitute sameness of method. I have made no attempt of the kind in these inquiries, but have employed the language, methods, and notations, that seemed to me to be best adapted to obtaining the results after which the conditions and objects of the problem led me to search; or otherwise more apparent symmetry might have been easily given to the several investigations into which I have entered.

I have prefixed a few geometrical lemmas which were necessary to substantiate and facilitate the mode of reasoning here employed. Most of them are required in the present paper, the earlier one subservient to some of the others, and these for direct quotation. There is one indeed, (the sixth) which is not essential, but it is

that all consideration of curves may be advantageously banished from such inquiries. Such a plan may, indeed, furnish an elegant abstraction fitted for the higher order of minds and the highest degree of mathematical skill: but it would at the same time effectually preclude the possibility of elementary acquirement, and in very many cases that of original inquiry in *new* departments of physical science. The consequence would therefore in reality be, to retrograde the science instead of facilitating its progress. Curves, though but so few of them, enter into so many branches of philosophical investigation, that science would, save to one in a thousand, be rendered unintelligible by their abolition.

added on account of its connexion as to form of enunciation with the third, and from its admitting of a very neat analysis and construction.

The conclusion, I may, finally, add, at which I have arrived, is :—*That when two centres of magnetic force of equal intensity and opposite direction are situated anywhere within the earth, there are always two, and never more than two, points on its surface at which the needle can take a direction perpendicular to the horizon.*

GEOMETRICAL LEMMAS.

Lemma I. Local Theorem.—*If from two given points lines be inflected to meet, and have a given ratio, the locus of their intersection is a given circle.*

This proposition was known to the Greek geometers, and is employed by Eutocius in his preface to the Conics of Apollonius. The analysis and synthesis of it are given by Simson, in two different ways, in his Restoration of the Plane Loci, lib. ii. prop. ii. The latter of these is also given by Professor Leslie in his Geometrical Analysis, book iii. prop. 13, and is that generally employed by geometrical writers. The following one is, so far as I know, different from any that has been given: still, but for its better answering the purposes I have in view, I should not have inserted it here, as on no other account can it, perhaps, be entitled to such an appropriation.

Let T and U (Plate X. fig. 1.) be the given points, and T N, N U, a pair of corresponding lines in the given ratio. Bisect the interior and exterior angles T N U and U N K by the lines N C, N D meeting the line T U at C and D respectively. Then

$$TC : CU :: TN : NU,$$

and

$$TD : DU :: TN : NU.$$

Hence the sum, or the difference, of two lines and their ratio being given, the lines themselves are given, and hence the points C and D are given, and the line C D between them is given in magnitude and position.

Again, since the interior and exterior angles at N, which are together equal to two right angles, are bisected by N C and N D, the angle C N D is a right angle, and therefore also given in magnitude. And since C D is given, and the angle C N D is a right angle, the locus of N is a circle on C D as a diameter. Hence the following

Construction.—Divide the given line internally and externally in the given ratio in C D, and on C D describe a circle. This will, as is evident from the analysis, be the locus sought.

Corollary.—The line T U is divided harmonically in C and D; for from the two analogies above, we have

$$TC : CU :: TD : DU.$$

Scholium.—A similar division may be made, the points C and D lying on the other side of M, the middle of T U. This implies, however, an inversion of the antecedent and consequent of the terms of the ratio.

Fig. 1

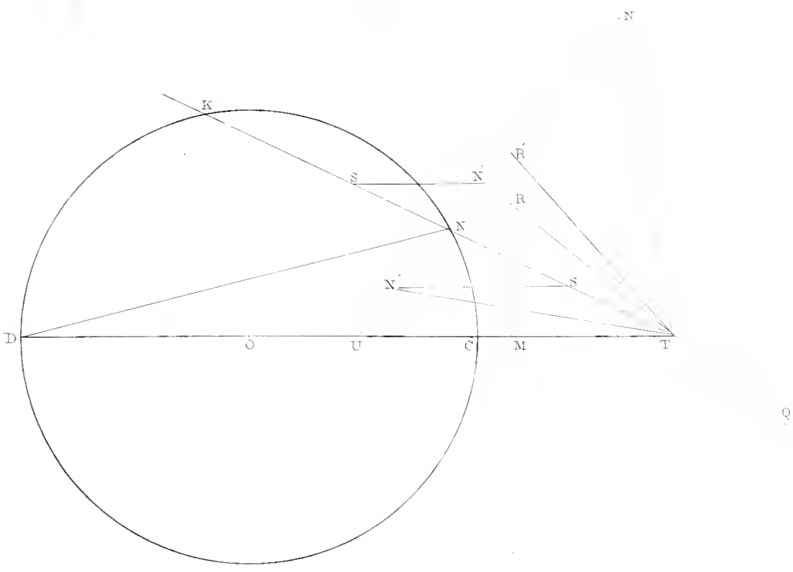


Fig. 2

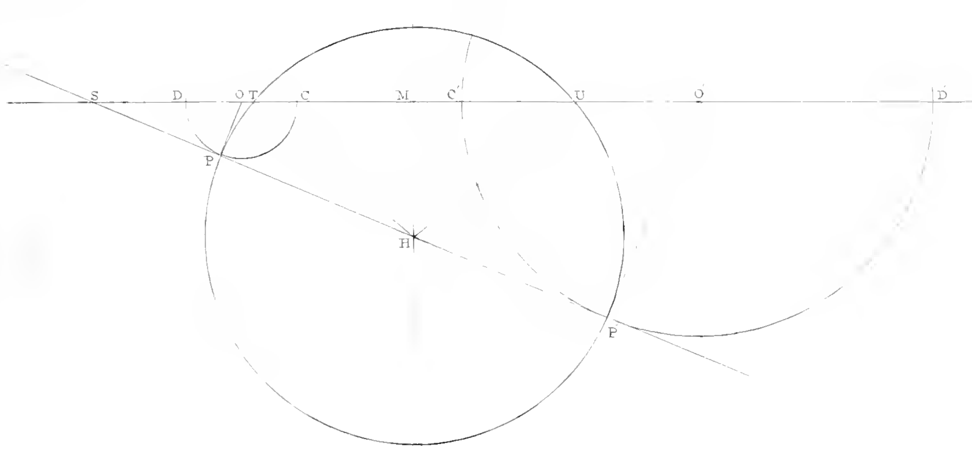
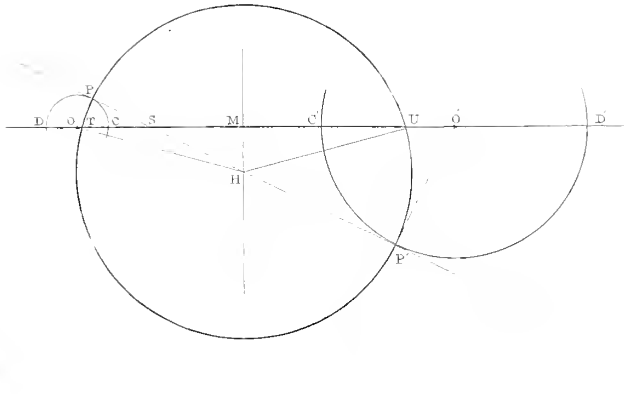


Fig. 3



LEMMA II. THEOREM.—*If lines be inflected to a point* without *the circle* T N U *from* T *and* U, *the ratio* T N′ : N′ U *will be* less *than the ratio* T N : N U ; *but if to a point within the circle, the ratio will be* greater. (Plate X. fig. 1.)

First. Let N′ be *without* the circle. This divides itself into two cases.

1. Where N′ is on the same side of M R (drawn from M at right angles to T U) as the points C and D are.

Join T N′, N′ U, and let N′ U cut the circle in N. Join also T N and draw N′ S parallel to T U, meeting T N in S, and produce T N to Q.

Then, by parallels, the angles Q T U, T N′ S are equal. And since N′ T U is less than a right angle, (for it is also an angle of the triangle T M R, of which T M R is a right angle, the point N′ being by hypothesis on the other side of R M from T,) the angle Q T U is greater than a right angle; and hence T N′ S is greater than a right angle, and consequently N′ S T is less than a right angle, and, *à fortiori*, less than T N′ S. The line T S is therefore greater than T N′.

But by similar triangles T N : N U : : T S : N′ U. Hence since the line T N′ is less than T S, the ratio T N′ : N′ U is less than the ratio T S : N′ U, and hence less than the ratio T N : N U.

2. Let N′ and C D be on opposite sides of M R. Join N′ U cutting M R in R′ and join T R′. Then the angles R′ T U, R′ U T are equal. But N′ T U is greater than R′ T U, and hence greater than N′ U T. The side N′ U of the triangle N′ T U is therefore also greater than N′ T. Hence the ratio N′ T : N′ U is a ratio of less inequality, whilst the ratio T N : N U is by hypothesis a ratio of greater inequality. The ratio T N′ : N′ U is therefore, in this case also, less than the ratio T N : N U.

Secondly. Let the point N′ lie *within* the circle. Produce U N′ to meet the circle at N and join N T. Draw N′ S parallel to T U, to meet T N at S.

Then it may be proved as before that T N′ is greater than T S. And by similar triangles T N : N U : : T S : N U′. But since T N′ is greater than T S, the ratio T S : N′ U, that is the ratio T N : N U, is less than the ratio T N′ : N U.

Scholium.—The antecedent of the lines in the expression of the ratio are considered to be drawn from the more distant point T: but these conditions will be reversed when the order of the terms is reversed.

LEMMA III. PROBLEM.—*From two given points* T *and* U (Plate X. figg. 2. and 3.) *to inflect lines to meet in a right line given by position, so that their ratio shall be the least or greatest possible.*

This problem is divisible into two cases according as the intersection S of the given line H S with the line T U drawn through the given points T and U, is on the same side of M with the antecedent or with the consequent of the lines which are in the required ratio. As, however, both cases are constructed by the same operation, it will be more convenient to give the analysis of them in juxtaposition by means of parallel vertical columns. Also for convenience of comparison, I shall employ the same letters in both cases, merely accentuating one set for the sake of distinction.

Let T U be produced to meet S H in S. Then it is obvious from the preceding lemmas that the problem is reducible to the description of two circles which shall touch the given line H S; and each divide the line T U internally and externally in the same ratio, or divide it harmonically; that is, in the one case U C : C T :: U D : D T, and in the other T C′ : C′ U :: T D′ : D′ U.

Suppose the points of contact P and P′ to be found. Draw M H from M the middle of T U perpendicular to T U, and let it meet H S in H. Draw the lines P O and P′ O′ from the points of contact perpendicular to H S; then O and O′ are the centres of the circles.

First case. Where the line T P is the antecedent, and the ratio the least possible.

By Leslie's Geom., vi. 7. $MT^2 = MC.MD$, that is,

$$MT^2 = (MS - SC)(MS - SD)$$
$$= MS^2 - MS(SC + SD) + SD.SC$$
$$= MS^2 - 2MS.SO + SD.SC.$$

But by the similar triangles SPO, SHM,

$$MS.SO = PS.SH;$$

and by the circle $SC.SD = SP^2$.

Hence

$$MT^2 = MS^2 - 2HS.SP + SP^2$$
$$= HS^2 - 2HS.SP + SP^2 - HM^2.$$

Hence

$$(HS - SP)^2 = MT^2 + MH^2 = HT^2.$$

That is $SP = HS - HT$.

Second case. Where the line T P′ is the antecedent, and the ratio the least possible.

By Leslie's Geom., ib. $MU^2 = MC'.MD'$, that is,

$$MU^2 = (SC' - SM)(SD' - SM)$$
$$= SC'.SD' - SM(SC' + SD') + SM^2$$
$$= SC'.SD' - 2SM.SO' + SM^2.$$

But by the similar triangles SP′O′, SHM,

$$MS.SO' = P'S.SH;$$

and by the circle $SC'.SD' = SP'^2$.

Hence

$$MU^2 = SP'^2 - 2HS.SP' + SM^2$$
$$= SP'^2 - 2HS.SP' + HS^2 - HM^2.$$

Hence

$$(SP' - HS)^2 = MU^2 + MH^2 = HU^2 = HT^2.$$

That is $SP' = HS + HT$.

Hence this

Construction.—Draw the perpendicular M H (from M) to T U, meeting H S in H. With centre H and distance H T or H U describe a circle cutting H S in P and P′. These are the points at which the ratios are those sought, as is too evident from the analysis to need a formal demonstration.

The ratio T P : P U is hence the *least*, and T P′ : P′ U the *greatest* that lines drawn from T and U to meet in the line H S, can possibly have; or the greatest and least, if the order of the terms of the ratio be changed. At H, midway between P and P′, they have a ratio of equality.

Lemma IV. Theorem.—*Not more than two pairs of lines can be inflected from the same two points* T *and* U *to meet in the same straight line* H S, *and have to one another a given ratio, the order of the terms of the ratio being given.*

For since the locus of all the intersections of all lines which can be so drawn is a circle, (Lemma I.) and a circle can cut a straight line in only two points, the truth of the proposition follows.

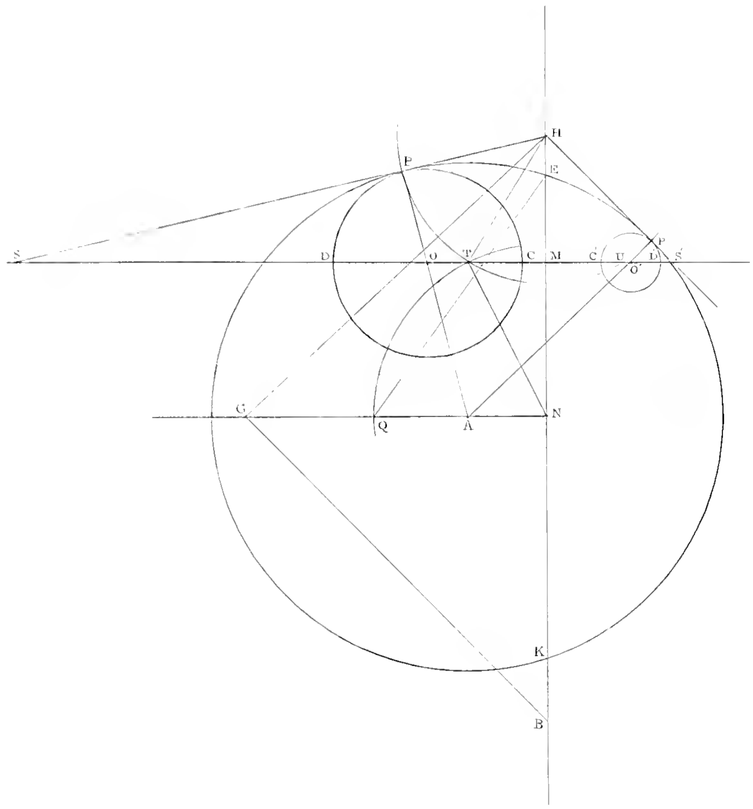


Fig. 5.

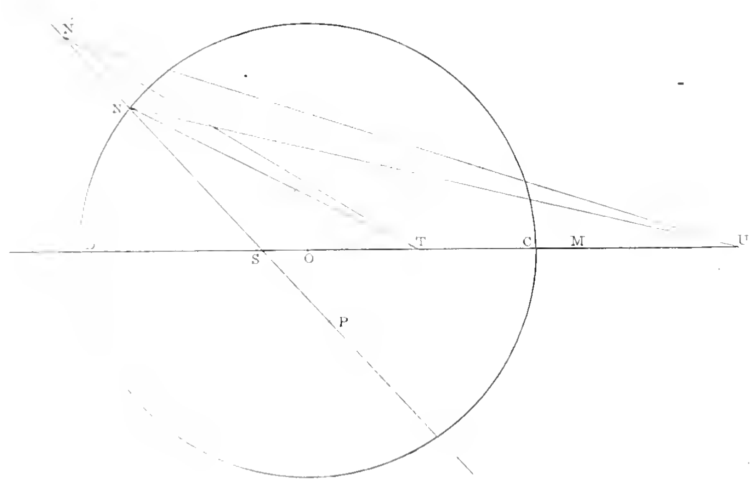


Fig. 4.

Fig. 6.

J. Basire, sc.

Lemma V. Theorem.—*If lines be inflected from two points in a given straight line to two given points, the pair which is more remote from the point at which the ratio is a minimum will have a greater ratio than those inflected from the point which is nearer. And when their order is changed, the ratio will be less.*

Let T, U be the points, and suppose T the point nearer to the given line N S to be the point from which the antecedent line is drawn. Let P be the point found in the last lemma, and N nearer to P than N′ is; then the ratio N′ T : N′ U is greater than the ratio N T : N U. (Plate XI. fig. 4.)

For divide the line T U harmonically in the points C and D in the ratio N T : N U, and describe the circle on C D, passing through N (Lemma I.).

Then since N is nearer to P than N′ is, the point N′ falls without the circle D N C; and hence (Lemma II.) the ratio N T : N U is less than N′ T : N′ U.—*Q. E. D.*

Lemma VI. Problem.—From two given points T, U *within* a circle to inflect lines to the circumference, so that they shall have the *greatest* or the *least* ratio possible. (Plate XI. fig. 5.)

Suppose the points to be found at P and P′; and draw the tangent P H meeting the perpendicular M H from M, the middle of the line joining the given points.

Then from the reasoning in Lemma II., the circle of ratios will touch the given circle in P and divide T U harmonically in C and D; and from Lemma IV.* we learn that H P = H T. Hence the problem is reduced to finding a point H in the line M H, from which tangents being drawn to the given circle R P E they will be equal to H T or H U.

Though A the centre of the given circle draw A N parallel to T U; then it is perpendicular to H M or E K, and E K is bisected in N. Then

$$H P^2 = K H \,.\, H E = H T^2 = T M^2 + M H^2$$
$$= T M^2 + (N H - N M)^2$$
$$= T M^2 + N H^2 \; 2 H N \,.\, N M + N M^2.$$

Hence $$2 H N \,.\, N M = T M^2 + N E^2 + K H \,.\, H E - K H \,.\, H E + N M^2$$
$$= T M^2 + M N^2 + N E^2$$
$$= T N^2 + N E^2.$$

Whence we have to form a rectangle whose area is $T N^2 + N E^2$, and one of whose sides is 2 N M; and the other side of the rectangle is the distance of H from N.

Construction.—With centre N and distance N T describe a circle cutting A N in Q. Join E Q, and make N G = E Q, and N B = 2 N M. Join B G, and draw G H perpendicular to it, cutting M H in H. Then with centre H and distance H T or H U, describe a circle cutting the given circle in P and P′, and these will be the points required.

* Since D P T touches the line H P and divides T U harmonically.

The demonstration is obvious from the analysis.

The problem admits of several other cases, but the same analysis and construction, *mutatis mutandis*, serves for them all.

LEMMA VII. THEOREM.—*If a ratio be one of greater inequality, the triplicate of that ratio is greater than the ratio itself; but if it be a ratio of less inequality, the triplicate ratio is less than the ratio itself. Also conversely, the subtriplicate of a ratio of greater inequality is less than the ratio itself; but of a ratio of less inequality, the subtriplicate is greater than the ratio itself.*

This is too obvious to need a formal proof here.

LEMMA VIII. PROBLEM.—*If a line* T U *be divided in any undetermined ratio, viz.* $r_{,} : r_{,,}$ *in* C *and* D, *and* O *be the middle of* C D; *and if the same line be divided in* Q *in the triplicate ratio of* $r_{,} : r_{,,}$; *it is required to find whether* Q *and* O *can ever coincide for any value of the ratio* $r_{,} : r_{,,}$. (Plate XI. fig. 6.)

Suppose they can; and let us first investigate the values of T O and T Q generally. Then if T U = 2 a, we have

$$\left.\begin{aligned} \mathrm{TC} &= \frac{2 a r_{,}}{r_{,,} + r_{,}} \\ \mathrm{DT} &= \frac{2 a r_{,}}{r_{,,} - r_{,}} \end{aligned}\right\}$$

Hence

$$\mathrm{DT} + \mathrm{TC} = \mathrm{DC} = \frac{4 a r_{,} r_{,,}}{r_{,,}^2 - r_{,}^2}$$

and

$$\mathrm{OC} = \frac{1}{2} \mathrm{DC} = \frac{2 a r_{,} r_{,,}}{r_{,,}^2 - r_{,}^2}$$

and therefore

$$\mathrm{TO} = \mathrm{CO} - \mathrm{CT} = \frac{2 a r_{,}^2}{r_{,,}^2 - r_{,}^2}$$

Again,

$$\mathrm{TQ} = \frac{2 a r_{,}^3}{r_{,,}^3 - r_{,}^3}$$

And since we have admitted the hypothesis of the equality of T O, T Q, we have

$$\frac{2 a r_{,}^3}{r_{,,}^3 - r_{,}^3} = \frac{2 a r_{,}^2}{r_{,,}^2 - r_{,}^2}$$

which reduces to

$$r_{,}^2 r_{,,}^2 = 0.$$

And this again to the two equations $r_{,}^2 = 0$ and $r_{,,}^2 = 0$; which indicates that it takes place at T and U, or when the ratios are infinitely great and infinitely small.

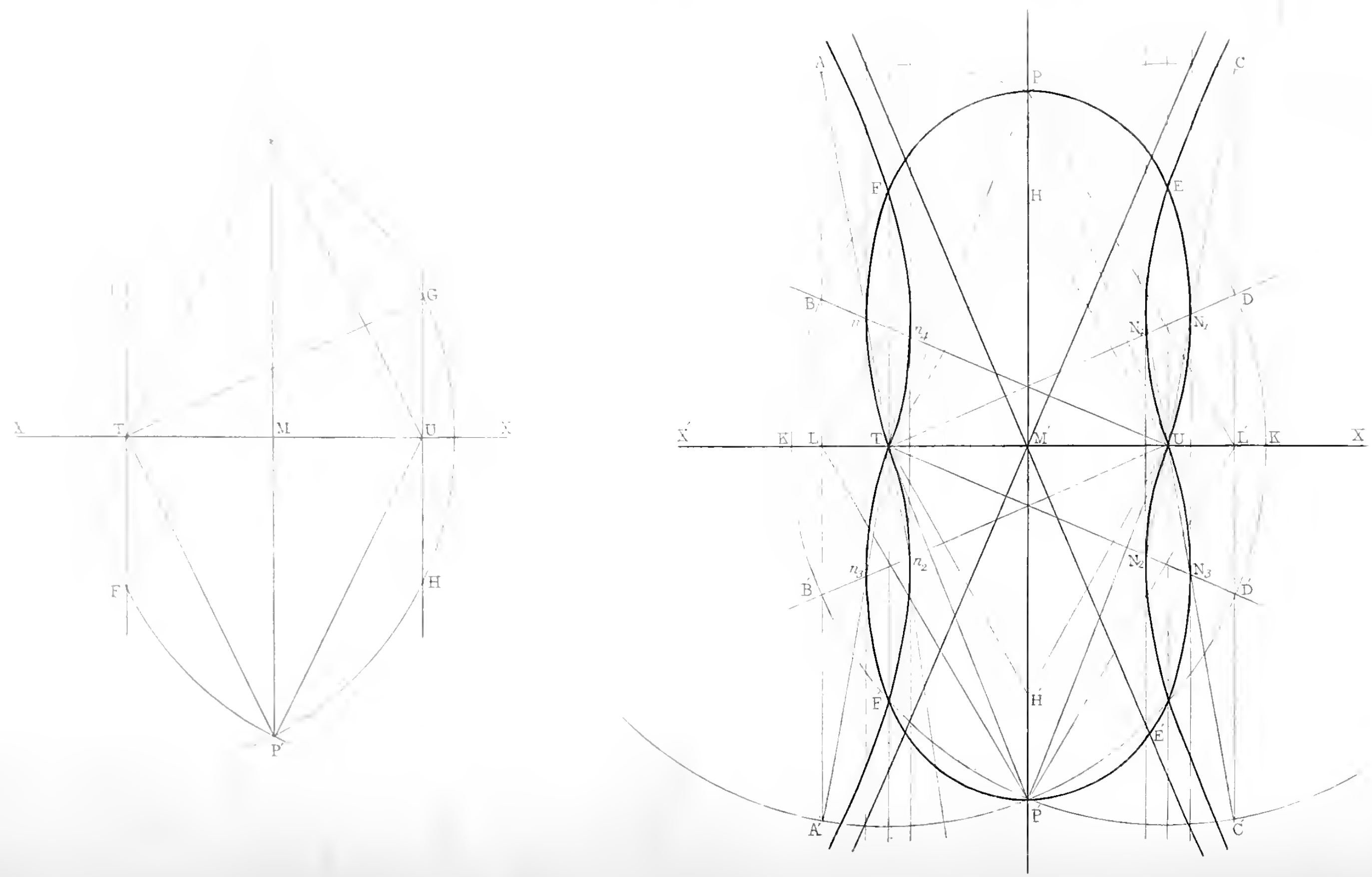


Fig. 10

J. Basire, sc

Lemma IX. Problem.—*To ascertain which is the greater,* T O *or* T Q.

Put T O − T Q = c; that is, $c =$

$$2a\left\{\frac{r_{\prime}^2}{r_{\prime\prime}^2-r_{\prime}^2}-\frac{r_{\prime}^3}{r_{\prime\prime}^3-r_{\prime}^3}\right\}=\frac{2\,a\,r_{\prime}^2\,r_{\prime\prime}^2}{(r_{\prime\prime}^2-r_{\prime}^2)(r_{\prime}^2+r_{\prime}r_{\prime\prime}+r_{\prime\prime}^2)}=\frac{2\,a\,r_{\prime}^2\,r_{\prime\prime}^2}{(r_{\prime\prime}-r_{\prime})(r_{\prime\prime}+r_{\prime})(r_{\prime}^2+r_{\prime}r_{\prime\prime}+r_{\prime\prime}^2)}.$$

Now whilst $r_{\prime\prime}$ is greater than $r_{\prime}$, that is whilst the distances T O, T Q are reckoned to the left of T, this is essentially positive, since all the factors except $r_{\prime\prime}^2 - r_{\prime}^2$ are essentially positive, however the quantities be reckoned; and hence the point O lies more remote from T than Q does. In precisely the same way it may be shown to be true when the points C and D, &c. are taken respectively to the right of the middle. Hence we may infer that, under all circumstances, except those determined in the last lemma, the quantity c is *finite*: and it may be easily shown to increase, as $r_{\prime}$ and $r_{\prime\prime}$ increase, *ad infinitum*.

XX.—*On the Points at which the Magnetic Needle takes a Position vertical to the Surface of the Earth.**

At the close of my last paper, art. xix., I stated that I had been unable to resolve equation (78.) into its simple or quadratic component factors, and was therefore unable by means of it to assign positively the number of points on the earth's surface at which the needle can take a vertical position. That difficulty may, however, be obviated by a different process from that which I then indicated; and as this new method fully meets all the objects, physical and geometrical, which led to the formation of that equation, any further discussion of it in that form may now be dispensed with. It will here be proved that on the hypothesis of two poles of equal intensity and of different kinds, *there never can be more than two points on the earth's surface at which the needle can take the position in question.*

For this purpose let us return to the equation (76.), and take the axis of x parallel to the axis of the terrestrial magnet T U (Plate XII. figg. 7, 8.), the centre of the earth O being still the origin of the coordinates. Draw O V perpendicular to T U, which will coincide in position with the axis of y. Denote the angles which O U and O T make with O V by $\alpha_{\prime\prime}$ and $\alpha_{\prime}$, the line O V itself in magnitude by b, and the current polar coordinates of the curve of contact (of the tangents from O to the magnetic curves whose common poles are U and T) by r and θ.

Then we have V T = $a_{\prime} = b\tan\alpha_{\prime}$, V U = $a_{\prime\prime} = b\tan\alpha_{\prime\prime}$, and $b_{\prime} = b_{\prime\prime} = b$; and likewise $x = r\sin\theta$, and $y = r\cos\theta$. Make these substitutions in (76.), viz. in

$$(b_{\prime}x - a_{\prime}y)^2\{(a_{\prime\prime}-x)^2+(b_{\prime\prime}-y)^2\}^3 = (b_{\prime\prime}x - a_{\prime\prime}y)^2\{(a_{\prime}-x)^2+(b_{\prime}-y)^2\}^3;$$

and there will result, after a few reductions too easy and obvious to need indication here, the following polar equation of the curve of contact:

* Continued from the Philosophical Transactions, 1835, p. 248.

$$\left.\begin{array}{l} r^2 \cos^2 \alpha_{,,} \sin^2 \overline{\theta - \alpha_{,}} \{r^2 - 2\, b\, r \sec \alpha_{,,} \cos \overline{\theta - \alpha_{,,}} + b^2 \sec^2 \alpha_{,,}\}^3 \\ \quad = r^2 \cos^2 \alpha_{,} \sin^2 \overline{\theta - \alpha_{,,}} \{r^2 - 2\, b\, r \sec \alpha_{,} \cos \overline{\theta - \alpha_{,}} + b^2 \sec^2 \alpha_{,}\}^3 \end{array}\right\} \quad (79.)$$

and this resolves at once into the two equations

$$\left.\begin{array}{l} \cos^2 \alpha_{,,} \sin^2 \overline{\theta - \alpha_{,}} \{r^2 - 2\, b\, r \sec \alpha_{,,} \cos \overline{\theta - \alpha_{,,}} + b^2 \sec^2 \alpha_{,,}\}^3 \\ \quad = \cos^2 \alpha_{,} \sin^2 \overline{\theta - \alpha_{,,}} \{r^2 - 2\, b\, r \sec \alpha_{,} \cos \overline{\theta - \alpha_{,}} + b^2 \sec^2 \alpha_{,}\}^3 \end{array}\right\} \quad . \quad . \quad (80.)$$

$$\text{and } r^2 = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (81.)$$

If now we extract the cube root of both sides of (80.), and resolve the quadratic for r, we shall obtain the following equation of the system of branches of the curve of contact:

$$r = b \cdot \frac{\begin{array}{l}\sec \alpha_{,,} \cos \overline{\theta - \alpha_{,,}} (\cos \alpha_{,,} \sin \overline{\theta - \alpha_{,}})^{\frac{2}{3}} \\ - \sec \alpha_{,} \cos \overline{\theta - \alpha_{,}} (\cos \alpha_{,} \sin \overline{\theta - \alpha_{,,}})^{\frac{2}{3}}\end{array} \pm \left\{\begin{array}{l} (\sec^2 \alpha_{,,} - 2 \sec \alpha_{,} \sec \alpha_{,,} \cos \overline{\theta - \alpha_{,}} \cos \overline{\theta - \alpha_{,,}} + \sec^2 \alpha_{,}) (\cos \alpha_{,} \cos \alpha_{,,} \sin \overline{\theta - \alpha_{,}} \sin \overline{\theta - \alpha_{,,}})^{\frac{2}{3}} \\ - \sec^2 \alpha_{,,} \sin^2 \overline{\theta - \alpha_{,,}} (\cos \alpha_{,,} \sin \overline{\theta - \alpha_{,}})^{\frac{4}{3}} - \sec^2 \alpha_{,} \sin^2 \overline{\theta - \alpha_{,}} (\cos \alpha_{,} \sin \overline{\theta - \alpha_{,}})^{\frac{4}{3}} \end{array}\right\}^{\frac{1}{2}}}{(\cos \alpha_{,,} \sin \overline{\theta - \alpha_{,}})^{\frac{2}{3}} - (\cos \alpha_{,} \sin \overline{\theta - \alpha_{,,}})^{\frac{2}{3}}} \quad (82.)$$

From this equation we learn the important fact, that *no more than two points* of the curve of contact can exist for each value of θ. To render it subservient to the completion of our object in this inquiry, it will be necessary to establish two other properties, viz. that the quantity under the radical is essentially positive, so as to render the curve real for all values of θ, and that of these values one is greater and the other less than $b \sec \theta$. The slightest attention, however, to the form of the expression will convince us that this would be a work of great labour if performed in a perfectly satisfactory manner; and that probably it would exceed the means at present in our possession for conducting such a discussion to a successful termination. It is fortunately as unnecessary as it is difficult, since by recurring to the genesis of the curve itself both these conclusions may be readily established; and as these are all that are essential to the present investigation, I do not think myself under any necessity to examine those characters of the curve which are mere matters of mathematical curiosity, even though some of them may be very readily obtained from the equation itself. Except, therefore, for facilitating some few steps of the succeeding course of inquiry, and for the establishment of the above-named general principle (the duality of the values of r for each value of θ), I shall rarely have occasion to again employ this equation, the objects of its introduction being hereby fully answered.

XXI.—By referring to art. XIX. (pp. 245, 246.) it will be obvious that the geometrical expression of the hypothesis belongs equally to the case of the convergent and divergent magnetic curves*; and hence the equations just obtained (81, 82.) must express both those cases. Moreover, it embraces the cases where N, O are on the

* These appropriate terms were first used by Professor LESLIE in his "Geometry of Curve Lines," p. 400, to designate the curves when the poles were respectively of different kinds and of the same kind. Professors ROBISON and PLAYFAIR had only considered the convergent curve; and I am not aware that any other author except Dr. ROGET has taken up the subject. See Library of Useful Knowledge, art. MAGNETISM, and Journal of Royal Institution, February 1831. Mr. BARLOW has followed LESLIE, Encyclopædia Metropolitana, p. 794.

same, and on different sides of the magnetic axis T U, as the slightest consideration will render obvious. In order, therefore, to separate these cases and adapt them to our immediate subject, we must recur to the magnetic curves themselves, and examine their particular characters with more care than has hitherto been done. We shall thus be enabled to establish the duality of the vertical points where the centres of force are, as assumed in the hypothesis, only two, and of equal intensity. The same method, it will readily appear after a little consideration, will establish analogous conclusions, whatever be the relative nature and intensities of the forces $F_{\prime}$ and $F_{\prime\prime}$ resident in the two centres T and U, should, at any future time, such an investigation be considered necessary, and prove that in no case can there be more than four such points on the earth's surface. In the present paper it will be shown, that could we imagine such an hypothesis to have any foundation in nature, the existence of two poles of the same kind and of equal intensities would in certain cases produce four points on the earth's surface, at which the needle would be vertical, and in others only two.

XXII.—*To trace the Magnetic Curve, and determine the Nature of its Branches and Singular Points.*

The equation of the curve* is

$$\cos \theta_{\prime} + \cos \theta_{\prime\prime} = 2 \cos \beta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (83.)$$

(A.) Let T and U be the poles; then since the equation is to be fulfilled by the cosines of $\theta_{\prime}$ and $\theta_{\prime\prime}$, we have the four following systems of equations, each of which fulfils the condition of (83.) for the same numerical value of $\theta_{\prime}$ and $\theta_{\prime\prime}$. (Plate XII. fig. 9.)

1. $\cos \theta_{\prime} + \cos \theta_{\prime\prime} = 2 \cos \beta.$
2. $\cos (- \theta_{\prime}) + \cos \theta_{\prime\prime} = 2 \cos \beta.$
3. $\cos (-\theta_{\prime}) + \cos (- \theta_{\prime\prime}) = 2 \cos \beta.$
4. $\cos \theta_{\prime} + \cos (- \theta_{\prime\prime}) = 2 \cos \beta.$

(B.) Hence the equation of the cosines determines the four points N_1, N_2, N_3, N_4 corresponding to the four forms of the equation just given respectively; and each of these points will trace out a branch of the system of curves as $\theta_{\prime}$, and consequently $\theta_{\prime\prime}$ is made to vary its actual angular value, cos β retaining the same value throughout the whole. When $\theta_{\prime}$ and $\theta_{\prime\prime}$ are both on the same side of the axis, that is both + or both −, the branches traced out are the convergent ones; but when on different sides, or one + and the other −, the branches are the divergent ones.

(C.) The four branches thus traced form two pairs of symmetrical portions, viz. N_1 is symmetrical to N_3, and N_2 to N_4.

(D.) When $\theta_{\prime}$ and $\theta_{\prime\prime}$ have interchanged their values, the four points will have at-

* See Philosophical Transactions, 1835, p. 238.

tained symmetrical positions with respect to the line T U, but inverted in respect to the extremities T and U, viz. the positions n_1, n_2, n_3, n_4 of the figure.

(E.) The latter system of points is symmetrical with the former, taken with respect to Y M Y′ at right angles to the magnetic axis, and passing through its centre M, viz. N_1 with n_1, N_2 with n_2, &c.

(F.) The points N_1 and N_3 trace out branches which are continuous from U to T, respectively above and below the magnetic axis.

(G.) The points N_4 and N_2 trace branches which diverge more and more from Y M Y′ as the angles approach to equality, since the angles T N_4 U and T N_2 U become more and more acute (being the differences of N_4 T U and N_4 U X, and of N_2 T U and N_2 U X, respectively), and when these are equal the points N_4 and N_2 become infinitely remote; that is, these are infinite branches.

(H.) For the same reason the points n_4 and n_2 trace a pair of infinite branches turned from the perpendicular Y M Y′ to the middle of the magnet, in the opposite direction to the former.

(I.) The branches are symmetrical to the magnetic axis and to the perpendicular through its centre, in the same cases in which the tracing points were severally symmetrical.—The general figure of the several systems of branches is represented in Plate XI. fig. 10.

(K.) No other points than N_1, N_2, N_3, N_4 fulfil the equation for the same angular values of $\theta_{\prime}$ and $\theta_{\prime\prime}$; and hence no other branches than these can exist.

(L.) Describe the parallelogram T P U P′, having T U for its diameter, and the angles at T and U above and below the line T U each equal to β. Through M (the centre of T U) draw the lines q_4 M Q_2 and q_2 M Q_4 parallel to the sides of the parallelogram. Then the infinite branches traced out by N_4, N_2, n_4 and n_2 have these lines for rectilinear asymptotes, whilst P and P′ are the vertices of the finite branches passing from T to U above and below the axis of the magnet. (Figg. 10, 11, 12.)

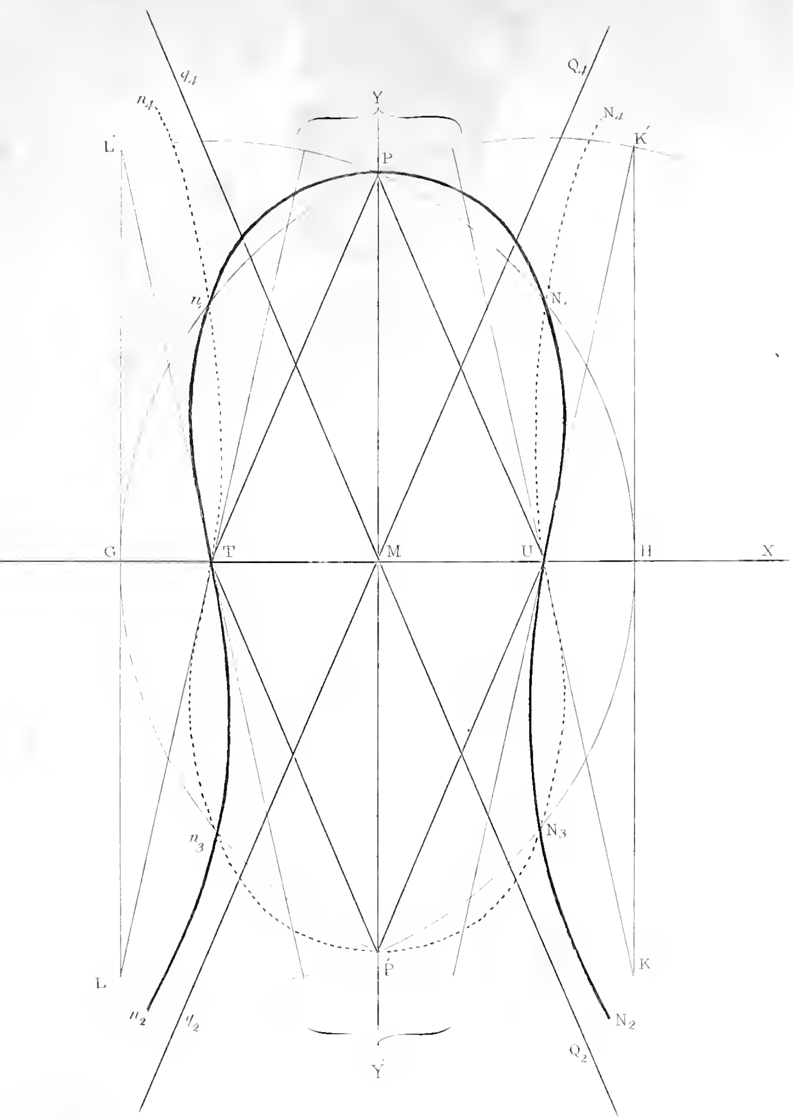
(M.) Moreover, if from centres T and U with radii T P, U P circles be described cutting the axis in G and H, and from these points lines be drawn parallel to P P′ uniting the circles in K K′ and L L′, then the parallelogram formed by drawing the radii through these points till they meet in Y and Y′, will have its sides tangents to all the branches of the curve that pass through T and U respectively in those points.

(N.) The points T and U are true points of inflexion of the branches, the convergent ones above being continuous of the divergent ones below the axis, and the convergent ones below the axis being continuous of the divergent ones above the axis, the first series n_2 T n_1 P N_1 T N_2 being marked in full line, and the second in dotted line. (Plate XIII. figg. 11, 12.)

(O.) If we conceive the plane of the curve to be a sphere of infinite radius*, then

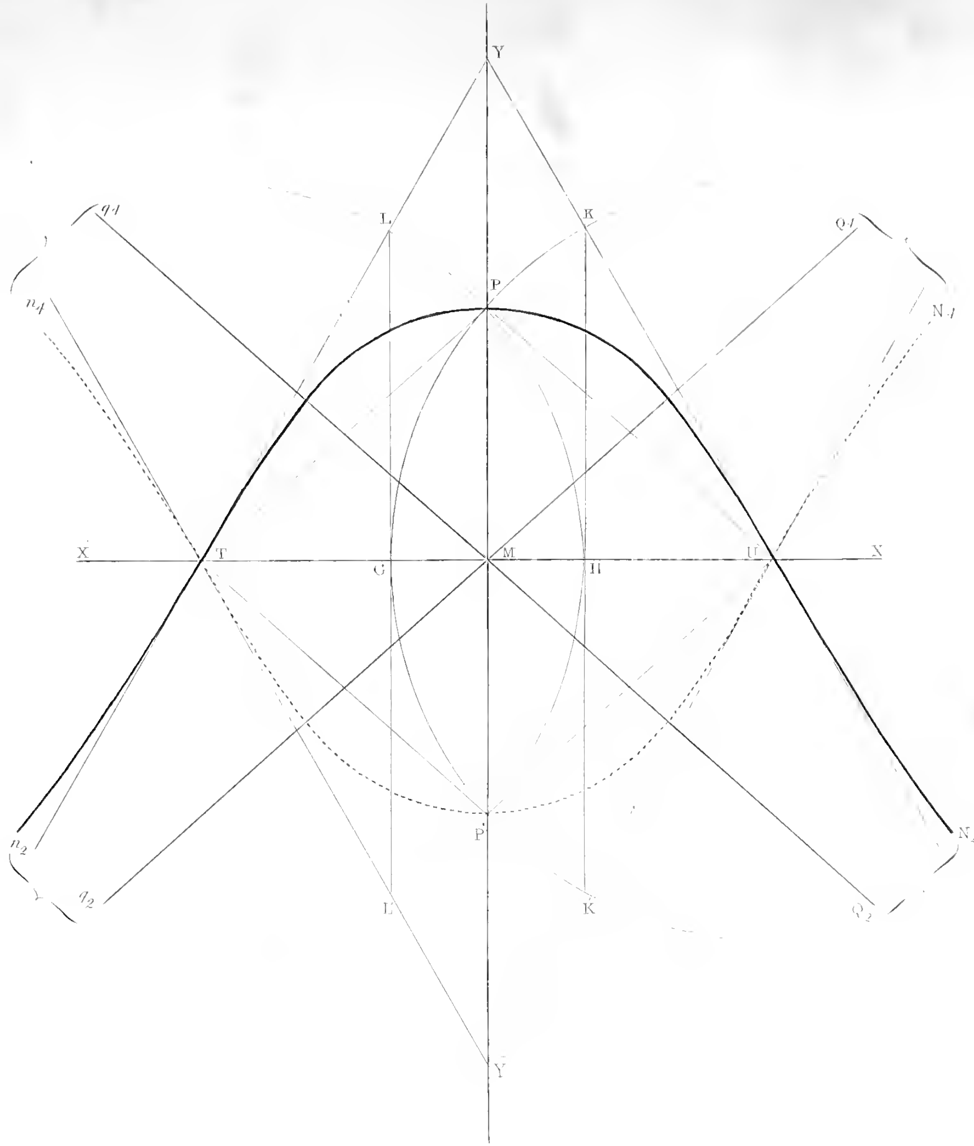
* The idea of considering the plane as an infinite sphere was first, I think, employed by the veteran geometer HACHETTE, for any real mathematical purpose except that of tracing a mere analogy between plane and spherical trigonometry, in his solution of VIETA's "Problem of Spherical Tangencies." This use of it is now

Fig. 11



Y
P
L
K
G
T
M
U
H
X
N₁
N₂
N₃
N₄
n₁
n₂
n₃
n₄
Q₁
Q₂
q₁
q₂

Fig. 12.



Y
P
L
K
G
T
M
U
H
X
N₁
N₂
n₁
n₂
Q₁
Q₂
q₁
q₂

the dotted and full-line branches meet at the opposite side of the sphere from M; and if we call this point M′, the point M′ is also a double point of inflexion, and the whole system of branches may in this sense be said to be continuous.

(P.) The following construction gives the vertices of the curve with respect to the line P M P′. (Plate XII. fig. 10.)

Describe an equilateral triangle L P L′, whose perpendicular is P M, and from centres T and U two circles P K P′ and P K′ P′. Through L and L′ draw lines parallel to P P′, meeting the circles in A, B, B′, A′, and C, D, D′, C′. Draw radii to the several points of intersection of the lines A A′ and C C′ with the circles; these will intersect in the points n_1, n_2, n_3, n_4 and N_1, N_2, N_3, N_4, which are the several vertices sought. The outer points n_1, n_3, N_1 and N_3 are those at which the vertices of the convergent branches are situated, and the inner ones n_2, n_4, N_2, N_4 are those of the divergent branches.

When the points L and L′ fall without the double segment P K P′ K, the construction fails, and there are no such points in the curve when this takes place. This obviously will be the case when L and L′ fall between the poles T and U*.

(Q.) The points in which the convergent and divergent branches intersect are found as follows:—(Figg. 10, 13.)

Draw through T and U the indefinite perpendiculars to the axis, and with centres T and U describe the circles P G H P′, P E F P′ cutting the perpendiculars in G, H, and E, F, respectively; then E, F, G, H are the points sought.

When T P is less than T U the construction fails, and the existence of such points becomes impossible. When T P = T U, the points coalesce in the poles, and the branches all *touch* there.

We shall now proceed to establish the truth of such of the preceding properties of the curve as are not immediately evident.

XXIII.—*The Vertices; or the Points at which the Tangent is perpendicular or parallel to the Axis.*

That it may be more easily effected, resume the general equation (38.), the poles being endowed with equal absolute intensities of force.

$$\frac{dy}{dx} = \frac{\frac{y}{r_{\prime}^3} - \frac{y}{r_{\prime\prime}^3}}{\frac{x+a}{r_{\prime}^3} - \frac{x-a}{r_{\prime\prime}^3}} = \frac{\frac{\sin\theta_{\prime}}{r_{\prime}^2} - \frac{\sin\theta_{\prime\prime}}{r_{\prime\prime}^2}}{\frac{\cos\theta_{\prime}}{r_{\prime}^2} + \frac{\cos\theta_{\prime\prime}}{r_{\prime\prime}^2}} = \frac{r_{\prime\prime}^2\sin\theta_{\prime} - r_{\prime}^2\sin\theta_{\prime\prime}}{r_{\prime\prime}^2\cos\theta_{\prime} + r_{\prime}^2\sin\theta_{\prime\prime}} . \quad . \quad . \quad (84.)$$

sufficiently familiar to the Continental geometers; but I am not aware that any further use of the principle has been made even by them. The application here made of the idea was suggested to my mind several years ago in considering the nature of infinite branches in a case analogous to the present one; and it seems to supply a desideratum, the want of which all writers on the higher geometry have felt when discussing the characters of certain particulars respecting curve lines.

* These considerations will be rendered subservient to an investigation of the state of the forces in what is commonly, though very improperly, called a "suturated" bar-magnet, which will hereafter be laid before the Royal Society.

But by the triangle T N U we have

$$r_{\prime\prime} \sin \theta_{\prime\prime} = r_{\prime} \sin \theta_{\prime},$$

which substituted in (84.) gives

$$\frac{dy}{dx} = \frac{\sin^3 \theta_{\prime} - \sin^3 \theta_{\prime\prime}}{\sin^2 \theta_{\prime} \cos \theta_{\prime} + \sin^2 \theta_{\prime\prime} \cos \theta_{\prime}} \quad \ldots \ldots \quad (85.)$$

First. That the tangent may be parallel to the magnetic axis, we must have

$$\sin^3 \theta_{\prime} - \sin^3 \theta_{\prime\prime} = 0,$$

which resolves itself into

$$\sin \theta_{\prime} - \sin \theta_{\prime\prime} = 0 \quad \ldots \ldots \quad (86.)$$

and

$$\sin^2 \theta_{\prime} + \sin \theta_{\prime} \sin \theta_{\prime\prime} + \sin^2 \theta_{\prime\prime} = 0. \quad \ldots \ldots \quad (87.)$$

The latter of these equations (87.) being imaginary, (for $\sin \theta_{\prime\prime} = \frac{-1 \pm \sqrt{-3}}{2} \sin \theta_{\prime}$,) the only points that exist where the property holds good are to be determined from the former (86.).

This equation may be fulfilled by the four following systems of values :

1. $\pm \theta_{\prime}$ and $\pm \theta_{\prime\prime}$; 2. $\theta_{\prime}$ and $\pi \mp \theta_{\prime\prime}$; 3. $\pi \mp \theta_{\prime}$ and $\theta_{\prime\prime}$; and 4. $\pi \mp \theta_{\prime}$ and $\pi \mp \theta_{\prime\prime}$.

But whichever of these we employ, it must be consistent with the equation of the curve itself; viz. with

$$\cos \theta_{\prime} + \cos \theta_{\prime\prime} = 2 \cos \beta.$$

1. The first of these obviously gives the points P and P′, and is consistent with the equation of the curve.

2. The fourth is virtually the same as the first, if we consider that in taking one supplement we should take all the supplements.

3. The second and third are incompatible with the equation of the curve itself.

There are hence only the two points P and P′ at which the needle can be parallel to the axis.

Secondly. That the tangent may be perpendicular to the axis, we must have

$$\sin^2 \theta_{\prime} \cos \theta_{\prime} + \sin^2 \theta_{\prime\prime} \cos \theta_{\prime\prime} = 0. \quad \ldots \ldots \quad (88.)$$

Expressing $\sin^2 \theta_{\prime}$, $\sin^2 \theta_{\prime\prime}$, and $\cos \theta_{\prime\prime}$ in terms of β and $\theta_{\prime}$ from the equation of the curve, and inserting the results in (88.), we shall obtain, after slight reductions,

$$\left.\begin{aligned} \cos \theta_{\prime} &= \cos \beta \pm \frac{\sin \beta}{\sqrt{3}}, \\ \text{and} \quad \cos \theta_{\prime\prime} &= \cos \beta \mp \frac{\sin \beta}{\sqrt{3}}. \end{aligned}\right\} \quad \ldots \ldots \quad (89.)$$

From these equations it appears that if β be less than $\frac{\pi}{3}$, there can be no point of the system at which the tangent is perpendicular to the axis, as in that case either $\cos \theta_{\prime}$ or $\cos \theta_{\prime\prime}$ would be greater than unity.

Fig. 14.

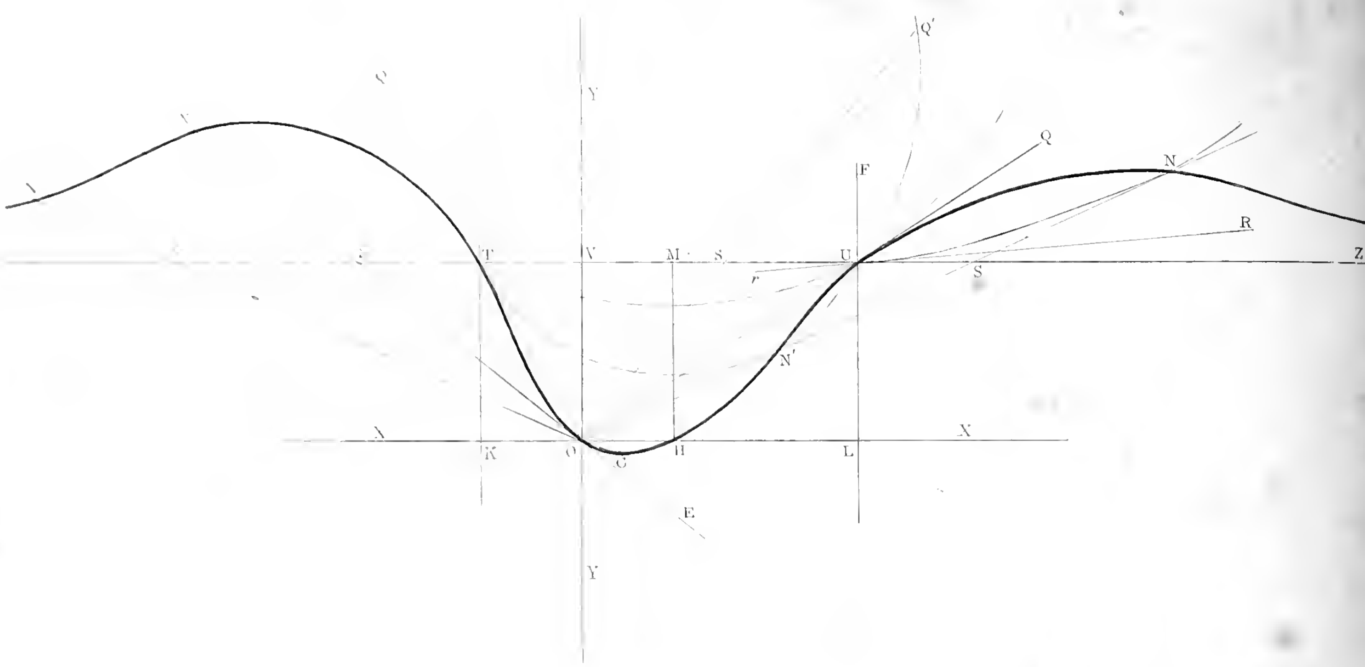
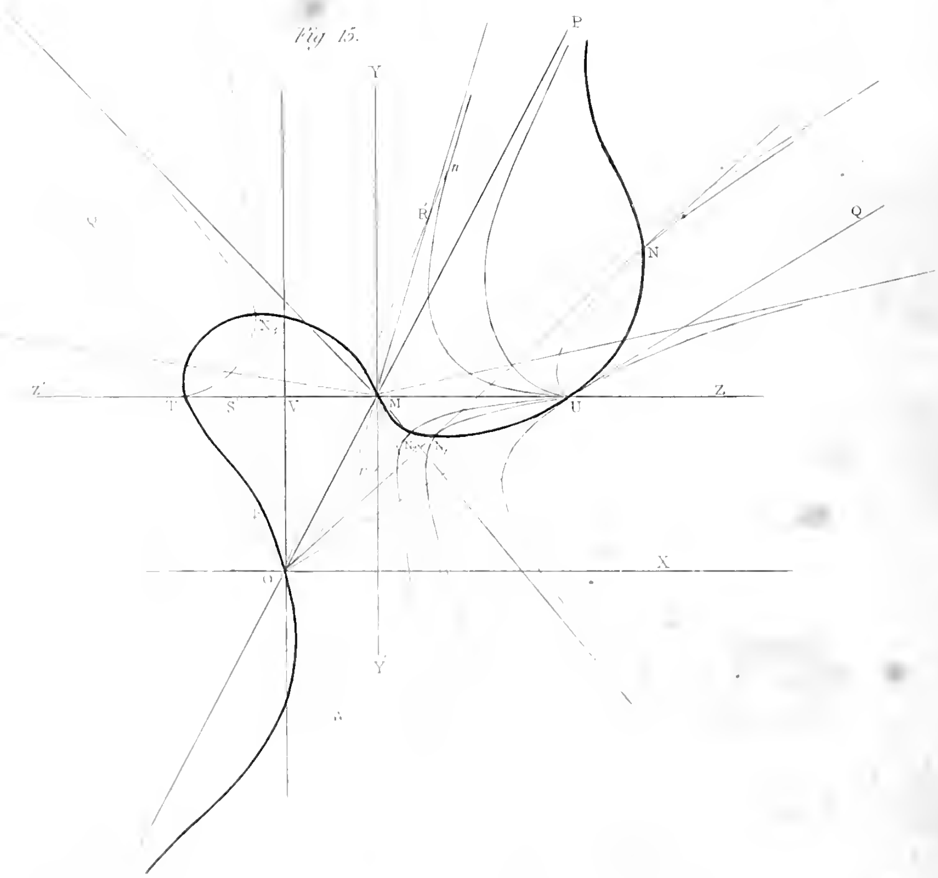


Fig. 15.



We may now establish the truth of the construction given at (P.) of art. XXII. for finding these points.

Let PT = radius = 1; then $PM = \sin\beta$, and $LN = \frac{\sin\beta}{\sqrt{3}}$. Hence $TL = \cos\beta - \frac{\sin\beta}{\sqrt{3}}$ and $LU = \cos\beta + \frac{\sin\beta}{\sqrt{3}}$ are the corresponding values of $\cos\theta_{,}$ and $\cos\theta_{,,}$ in equations (89.): and the intersection, therefore, of the lines BU and DT give one of the points in question.

In the same manner, by taking the other combinations of + and − as signs of $\theta_{,}$ and $\theta_{,,}$ in these equations, as signified in the opening of the last section (XXII. A.), the other points will be shown to be those given by the construction enunciated in (XXII. P.).

It is also clear that the limitation to which the construction is subjected is that expressed by the limitation of the equations themselves.

Moreover, the distribution of the points as to the particular branches of the curves to which they belong is properly made: for $T n_1$, $U n_1$ are the radiants belonging to values of $\theta_{,}$ and $\theta_{,,}$ on the *same side* of the axis. Hence n_1 is in the convergent curve. So, for the same reason, are n_3, N_1 and N_3. Again, since n_4 is found by the intersection of radiants $n_2 U$ and $n_3 T$, whose corresponding values of $\cos\theta_{,}$ and $\cos\theta_{,,}$ are estimated for $\theta_{,}$ and $\theta_{,,}$ *on different sides* of the axis, n_4 is in the divergent curve. So also, for the same reason, are n_1, N_4 and N_1 in the divergent branches.

XXIV.—*The Points of Intersection of the Finite with the Infinite Branches: and on the Asymptotes.*

1. The construction of the points of intersection has been given in (XXII. Q.), and it is thus proved. (Plate XIV. fig. 13.)

When in the construction of the points, the two points N_1 and N_4 coincide, the radiants $N_1 U$ and $N_4 U$ coincide also. But $N_4 U X = T U N_3 = T U N_1$. Hence when the radiants coalesce they form a line at right angles to TU.

Also in this case $\cos\theta_{,,} = 0$, and the equation of the curve becomes at this point

$$\cos\theta_{,} = 2\cos\beta.$$

But taking $r_{,} = 1$, $TM = \cos\beta$ and $TU = 2\cos\beta$; and if GT be drawn it is = 1. Hence the construction is true.

2. The construction of the asymptotes has been given in (XXII. L.); and its truth may be thus established.

To prove that $q_2 M Q_4$ and $q_4 M Q_2$ are Asymptotes to the Infinite Branches. (Plate XIV. figg. 11, 12.)

Since the point of the curve is found by the intersection of two parallel lines PT and $P' U$, it is infinitely distant; and the two radii $r_{,}$ and $r_{,,}$ themselves being infinite, are equal to one another. But the tangent to any point of the magnetic curve divides the axis in the ratio of $r_{\prime}^{3}$ to $r_{\prime\prime}^{3}$: and since these radii when infinite are equal, the tangent to the infinitely distant point of the curve bisects the magnet, or passes through M.

Now the line $M\,Q_4$ being parallel to T P and P′ U by construction passes through their common intersection; and dividing the axis T U in the ratio of $r_{\prime}^{3}$ to $r_{\prime\prime}^{3}$ is a tangent to the curve at that infinitely distant point. That is, $M\,Q_4$ is an asymptote to the branch $U\,N_4$ of the curve.

In the same way the other branches are shown to have severally the lines drawn, as already described, through M for rectilinear asymptotes.

XXV.—*The Points of Inflexion.*

In this case the second differential coefficient is equal to zero.

By (85.) we have

$$\frac{d\,y}{d\,x} = \frac{\sin^3\theta_{\prime} - \sin^3\theta_{\prime\prime}}{\sin^2\theta_{\prime}\cos\theta_{\prime} + \sin^2\theta_{\prime\prime}\cos\theta_{\prime\prime}}.$$

Also

$$\frac{x+a}{r_{\prime}} = \cos\theta_{\prime}, \text{ or } x = r_{\prime}\cos\theta_{\prime} - a,$$

and therefore

$$d\,x = \cos\theta_{\prime}\,d\,r_{\prime} - r_{\prime}\sin\theta_{\prime}\,d\,\theta_{\prime},$$

which, since by the triangle

$$r_{\prime} = \frac{2\,a\sin\theta_{\prime}}{\sin\overline{\theta_{\prime}+\theta_{\prime\prime}}}$$

is convertible into

$$d\,x = \frac{\begin{array}{l} -\,(\sin\theta_{\prime\prime}\sin\theta_{\prime}\sin\overline{\theta_{\prime}+\theta_{\prime\prime}} + \sin\theta_{\prime\prime}\cos\theta_{\prime}\cos\overline{\theta_{\prime}+\theta_{\prime\prime}})\,d\,\theta_{\prime} \\ +\,(\cos\theta_{\prime\prime}\cos\theta_{\prime}\sin\overline{\theta_{\prime}+\theta_{\prime\prime}} - \sin\theta_{\prime\prime}\cos\theta_{\prime}\cos\overline{\theta_{\prime}+\theta_{\prime\prime}})\,d\,\theta_{\prime\prime}\end{array}}{\sin^2\overline{\theta_{\prime}+\theta_{\prime\prime}}}. \quad \ldots \quad (90.)$$

Now, by the differential equation of the curve * we also have

$$d\,\theta_{\prime\prime} = -\frac{\sin\theta_{\prime}\,d\,\theta_{\prime}}{\sin\theta_{\prime\prime}}\,\dagger,$$

which converts (90.) into

$$d\,x = -2\,a\,.\,\frac{\sin^2\theta_{\prime\prime}(\sin\theta_{\prime}\sin\overline{\theta_{\prime}+\theta_{\prime\prime}} + \cos\theta_{\prime}\cos\overline{\theta_{\prime}+\theta_{\prime\prime}}) + \sin\theta_{\prime}\cos\theta_{\prime}(\cos\theta_{\prime\prime}\sin\overline{\theta_{\prime}+\theta_{\prime\prime}} - \sin\theta_{\prime\prime}\cos\overline{\theta_{\prime}+\theta_{\prime\prime}})}{\sin\theta_{\prime\prime}\sin^2\overline{\theta_{\prime}+\theta_{\prime\prime}}}\,.\,d\,\theta_{\prime}$$

or into

$$d\,x = -\,2\,a\,.\,\frac{\sin^2\theta_{\prime\prime}\cos\theta_{\prime\prime} + \sin^2\theta_{\prime}\cos\theta_{\prime}}{\sin\theta_{\prime\prime}\sin^2\overline{\theta_{\prime}+\theta_{\prime\prime}}}\,.\,d\,\theta_{\prime}. \quad \ldots \quad (91.)$$

In a similar manner from the equation $y = r\sin\theta_{\prime}$ we obtain

$$d\,y = -\,2\,a\,.\,\frac{\sin^3\theta_{\prime} - \sin^3\theta_{\prime\prime}}{\sin\theta_{\prime\prime}\sin^2\overline{\theta_{\prime}+\theta_{\prime\prime}}}\,d\,\theta_{\prime}. \quad \ldots \quad (92.)$$

But this is more readily obtained at once from a comparison of (85.) with (91.).

* Philosophical Transactions, 1835, p. 238.

† In this case, as also in the formation of the angular equation (85.), $\theta_{\prime\prime}$ is taken the supplement of the $\theta_{\prime\prime}$ in the differential equations; and hence the change of sign from — to +.

To obtain the second differential coefficient, first differentiate the numerator and denominator separately.

$$\left.\begin{aligned} d\{\sin^3\theta_, - \sin^3\theta_{,,}\} &= 3(\sin^2\theta_,\cos\theta_, d\theta_, - \sin^2\theta_{,,}\cos\theta_{,,}d\theta_{,,}), \\ \text{or, by the equation } d\theta_{,,} &= \frac{\sin\theta_, d\theta_,}{\sin\theta_{,,}}, \\ &= 3\sin\theta_,\{\sin\theta_,\cos\theta_, + \sin\theta_{,,}\cos\theta_{,,}\}\,d\theta_, \end{aligned}\right\} \quad . \quad . \quad (93.)$$

$$\left.\begin{aligned} d\{\sin^2\theta_,\cos\theta_, - \sin^2\theta_{,,}\cos\theta_{,,}\} &= (2\sin\theta_,\cos^2\theta_, - \sin^3\theta_,)\,d\theta_, + (2\sin\theta_{,,}\cos^2\theta_{,,} - \sin^3\theta_{,,})\,d\theta_{,,} \\ &= \sin\theta_,\{2\cos^2\theta_, - \sin^2\theta_,) - (2\cos^2\theta_{,,} - \sin^2\theta_{,,})\}\,d\theta_, \\ &= 3\sin\theta_,(\cos^2\theta_, - \cos^2\theta_{,,})\,d\theta_,. \end{aligned}\right\} (94.)$$

Hence from (91.), (93.), and (94.), omitting the constant factors, we have

$$\left.\begin{aligned} &= \sin\theta_,\sin\theta_{,,}\sin^2\overline{\theta_,+\theta_{,,}}\,.\,\frac{(\sin\theta_,\cos\theta_,+\sin\theta_{,,}\cos\theta_{,,})(\sin^2\theta_,\cos\theta_,-\sin^2\theta_{,,}\cos\theta_{,,})-(\cos^2\theta_,-\cos^2\theta_{,,})(\sin^3\theta_,-\sin^3\theta_{,,})}{(\sin^2\theta_,\cos\theta_, + \sin^2\theta_{,,}\cos\theta_{,,})^3} \\ &= \frac{\sin\theta_,\sin\theta_{,,}\sin^3\overline{\theta_,+\theta_{,,}}\,(\sin^2\theta_{,,}\cos\theta_, + \sin^2\theta_,\cos\theta_{,,})}{(\sin^2\theta_,\cos\theta_, + \sin^2\theta_{,,}\cos\theta_{,,})^3} \\ &= \frac{\sin\theta_,\sin\theta_{,,}\sin^3\overline{\theta_,+\theta_{,,}}\,(\cos\theta_, + \cos\theta_{,,})(1 - \cos\theta_,\cos\theta_{,,})}{(\sin^2\theta_,\cos\theta_, + \sin^2\theta_{,,}\cos\theta_{,,})^3} = 0. \end{aligned}\right\} (95.)$$

Since the denominator of this cannot become infinite, the condition is only fulfilled by the numerator $= 0$: and this gives the five following equations:

$$\left.\begin{aligned} &1. & \sin\theta_, &= 0 \\ &2. & \sin\theta_{,,} &= 0 \\ &3. & \sin^3\overline{\theta_,+\theta_{,,}} &= 0 \\ &4. & \cos\theta_,+\cos\theta &= 0 \\ \text{and } &5. & \cos\theta_,\cos\theta_{,,} &= 1. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (96.)$$

The first and second of these show that the poles themselves are true points of inflexion, and hence that the order of the branches as to continuity is, that the infinite are continuous of the finite branches on the opposite sides of the axis, as indicated by the full line and dotted line representations (Plate XIV. figg. 11, 12.), and as stated in (XXII.). It is evident that these conditions are consistent with the equation of the curve $\cos\theta_, + \cos\theta_{,,} = 2\cos\beta$; and, therefore, all the necessary conditions are thus fulfilled.

The third equation is fulfilled by the equation $\theta_, + \theta_{,,} = \pi$, which is also consistent with the equation of the curve. But this is the case when the radiants $r_,$ and $r_{,,}$ are parallel; and then the tangent, as has been already shown, is an asymptote. The view, then, which has been taken at (XXII. o.) of the infinite branches having points of inflection on the opposite side of the infinite sphere, is borne out by the analytical expression of the points of inflexion.

The third equation is, moreover, fulfilled also by $\theta_, + \theta_{,,} = 0$, $\theta_, = - \theta_{,,}$, which indicates the opposite branches to those just described: and hence the same remark may be made respecting them.

The fourth and fifth equations are *generally* inconsistent with the equation of the curve, and hence in all those cases imaginary. When, however, $\cos\beta = 0$, or $\beta = \frac{\pi}{2}$, the equation of the curve is consistent with this equation. The divergent branches in this case respectively coalesce with the magnetic axis itself, and the convergent ones by their continual expansion outwards have then come to coalescence with the axis produced; each through its whole length. In these cases, *any* point in the magnetic axis may be considered as a point of inflexion, and the tangent in all cases so taken makes an angle 0 or π with the axis.

When the middle M, however, of the axis, and its opposite point on the infinite sphere are taken, *any line* through them may be considered a tangent, as the asymptote, properly speaking, has then ceased to exist, or to be expressed by the equation. In other words, the direction of the curve at these points is become properly indeterminate. That the expressions themselves indicate this, will be made to appear in the next section.

XXVI.—*To find the Multiple Points and the Directions of their Tangents of the Magnetic Curve.*

At a multiple point we shall have, in consequence of (83.) and (85.), the three equations

$$\left.\begin{array}{ll} & 1.\ (\sin^3\theta_{\prime} - \sin^3\theta_{\prime\prime}) \sin\theta_{\prime} \sin\theta_{\prime\prime} \sin^2\overline{\theta_{\prime} + \theta_{\prime\prime}} = 0, \\ & 2.\ (\sin^2\theta_{\prime}\cos_{\prime} + \sin^2\theta_{\prime\prime}\cos\theta_{\prime\prime}) \sin\theta_{\prime} \sin\theta_{\prime\prime} \sin^2\overline{\theta_{\prime} + \theta_{\prime\prime}} = 0, \\ \text{and} & 3.\ \cos\theta_{\prime} + \cos\theta_{\prime\prime} = 2\cos\beta. \end{array}\right\} \quad \dots \quad (97.)^*$$

If these three equations be simultaneously fulfilled by the same values of $\theta_{\prime}$ and $\theta_{\prime\prime}$, the points indicated by those values will be multiple points. But this is the case with either of the values

$$\left.\begin{array}{ll} 1. & \sin\theta_{\prime} = 0, \\ 2. & \sin\theta_{\prime\prime} = 0, \\ 3. & \sin^2\overline{\theta' + \theta_{\prime\prime}} = 0, \end{array}\right\} \quad \dots\dots\dots\dots \quad (98.)$$

that is, at the poles and at the point of symptotism; that is, at the point on the infi-

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* Since

$$d\left(\frac{\frac{x}{z}}{\frac{y}{z}}\right) = \frac{\frac{y}{z}\,d\left(\frac{x}{z}\right) - \frac{x}{z}\,d\left(\frac{y}{z}\right)}{\frac{y^2}{z^2}} = \frac{y\,\frac{z\,dx - x\,dz}{z^2} - x\,.\,\frac{z\,dy - y\,dz}{z^2}}{\frac{y^2}{z^2}}$$

$$= \frac{yz\,dx - yx\,dz - xz\,dy + xy\,dz}{z\,y^2} = \frac{y\,dx - x\,dy}{y^2} = d\left(\frac{z}{y}\right),$$

nite sphere diametrically opposite to M. These results accord with the statements made in article XXII., and justify them.

To find the values of $\frac{dy}{dx}$ corresponding to these multiple points, we may proceed thus:

When $\sin\theta_{\prime} = 0$, we have $\cos\theta_{\prime} = \pm 1$; and $\cos\theta_{\prime\prime} = 2\cos\beta \mp 1$, the lower sign of which being impossible, so long as $\cos\beta$ is positive (which is our hypothesis) in these investigations; and though we might have so extended them as to include negative values of $\cos\beta$, yet nothing in point of generality would have been gained thereby, as only a reduplication of the branches of the curve would have resulted from it), and hence the only solution is $\cos\theta_{\prime\prime} = 2\cos\beta - 1$. But this final direction of $r_{\prime\prime}$ is the direction of the tangent at U: and as the same holds for $\cos(-\theta_{\prime\prime})$, there are two tangents at U equally inclined to the axis.

The value here given accords with the construction given (in XXII. M.). For (figg. 10, 12, 13.), taking P U as radius $= 1$, U H $=$ U T $-$ T H $= 2\cos\beta - 1$; and it is the cosine of the angle K U H by the construction. Hence it fulfils the condition of the equation and gives the tangents at the point U. In the same manner that construction gives the tangents at T.

The combination of the third equations of (97.) and (98.) may also be easily shown to coincide with the construction given for the asymptotes in (XXII. O.); but as the truth of that construction has already been proved at the close of XXIV., it is unnecessary to recur to it here.

But there occurs here a difficulty which is worthy of notice, but which is readily shown, however, to be only apparent. We have seen at (XXII. Q.) that there is another doubly symmetrical system of double points possible for values of β between

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it is quite clear that so long as we require only the differential fraction $\frac{\frac{x}{z}}{\frac{y}{z}}$, we may eleminate the denominators by the usual process *before* we commence the differentiation, even though they involve functions of the variable quantities that enter into an investigation. The same is true of factors not fractional: for

$$d\left(\frac{xz}{yz}\right) = \frac{yz(x\,dz + z\,dx) - xz(y\,dz + z\,dy)}{y^2 z^2}$$

$$= \frac{y z^2 dx - x z^2 dy}{y^2 z^2} = \frac{y\,dx - x\,dy}{y^2} = d\left(\frac{x}{y}\right).$$

It hence follows, that for seeking the second differential coefficient of the curve, we may eliminate by division or multiplication any common factor, integral, or fractional that enters into the numerator and denominator of the first differential coefficient, and hence that the process followed in (XXIII.) is legitimate. But when, on the contrary, each of the terms (numerator and denominator) of the first differential coefficient is to be equated to some other quantity, or to zero (as in finding the multiple points), then all the factors should, by the fundamental principles of algebraic equations, be retained in both. Hence the equations of condition (the first and second in (97.)) must retain *all* the factors which the process of first differentiation introduced into them. The want of due attention to this principle, simple and obvious as it is, has often led to very incomplete, and sometimes very erroneous, enumerations of the characters of certain curve lines.

certain limits; but our equations give no intelligence of their existence or character under the aspect we have yet viewed them.

The general expression of $\frac{dy}{dx}$ given in (85.) combined with (83.), viz.

$$\frac{dy}{dx} = \frac{\sin^3 \theta_{,} - \sin^3 \theta_{,,}}{\sin^2 \theta_{,} \cos \theta_{,} + \sin^2 \theta_{,,} \cos \theta_{,,}}$$

with $\cos \theta_{,} + \cos \theta_{,,} = 2 \cos \beta$

is not in a *rational form*, and therefore does in itself for all corresponding values of $\cos \theta_{,}$ and $\cos \theta_{,,}$ involve multiple values of tangent of inclination, $\frac{dy}{dx}$. There are, indeed, for each value of $\cos \theta_{,}$ four points in the curve, all whose tangents *ought to be* included in the expression of the values of $\frac{dy}{dx}$. These four values are

$$\left.\begin{aligned} \frac{dy}{dx} &= \frac{\pm \left\{ \sqrt{(1 - \cos^2 \theta_{,})^3} + \sqrt{\{1 - (2 \cos \beta - \cos \theta_{,})^2\}^3} \right\}}{(\cos \theta_{,} - \cos \beta)^2 - \frac{1}{3} \sin^2 \beta} \sec \beta \\ \text{and} \qquad \frac{dy}{dx} &= \frac{\pm \left\{ \sqrt{(1 = \cos^2 \theta_{,})^3} - \sqrt{\{1 - (2 \cos \beta - \cos \theta_{,})_1\}^3} \right\}}{(\cos \theta_{,} - \cos \beta)^2 - \frac{1}{3} \sin^2 \beta} \sec \beta \end{aligned}\right\} \quad . \quad (99.)$$

This method of proceeding, therefore, has the advantage (and in all cases where it is applied the same is true) of giving *directly* the several inclinations of the tangent to the axis of x or y corresponding to any assumed value of $\theta_{,}$: but for the *complete* and *certain* determination of the multiple points of the system, other and additional considerations, as stated in the prefatory remarks to this paper, are necessary. At present we may pass the subject over, as the only remaining multiple points have already been determined, as well as the conditions of their existence, in (XXII. Q.), and proved at the beginning of (XXIV.).

Finally. It was stated at the close of (XXV.), that when the curves themselves were the final ones, or coincided respectively with the magnetic axis, the tangent at M was indeterminate; and the statement is thus rendered evident.

When the curve coincides with the axis $\cos \beta = 0$, and the equation of the curve becomes $\cos \theta_{,} + \cos \theta_{,,} = 0$, or $\theta_{,} = \pi - \theta_{,,}$. Hence $\sin \theta_{,} = \sin \theta_{,,}$, and $\cos \theta_{,} = -\cos \theta_{,,}$. These values of $\theta_{,}$ and $\theta_{,,}$ inserted in (85.) give

$$\frac{dy}{dx} = \frac{\sin^3 \theta_{,} - \sin^3 \theta_{,}}{\sin^2 \theta_{,} \cos \theta_{,} - \sin^2 \theta_{,} \cos \theta_{,}} = \frac{0}{0} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (100.)$$

Moreover, if we take the successive differential coefficients to infinity, (as the value of n,) we still evidently have

$$\frac{d^n y}{dx^n} = \frac{0}{0}.$$

Hence there is no factor in (100.) which is determinate; or the inclination of the tangent to the axis is at that point *essentially indeterminate*.

All the properties of the magnetic curve that are essential to our future investigations of the physical problem, and none else, have now been fully stated and established. We shall proceed now to their application.

XXVII.—*On the Curve of Magnetic Verticity: or that in any Point of which a Magnetic Needle being placed, it will be directed towards a given Point.*

At each point of the curve of verticity, the tangent to the magnetic curve through that point is directed to the given point, this being the defining character of the curve of verticity. Its polar equations are given in (81.) and (82.), and before we proceed to employ the properties of the magnetic curve to the determination of the properties of this, it is necessary to make one or two remarks on those equations, and deductions from them.

In the first place, the general equation (79.) involves the fourth power of r, and therefore, generally indicates that a line drawn through *any other point* in the plane of the curve will have four values, either all real, two real, or all imaginary, since a transformation of coordinates does not alter its dimension, and therefore the number of its roots; and it is easy to see that in its general form, the separation of its roots would be impracticable, in the literal state of the component data. But as by taking the coordinates in the particular way that is there done, a loss of two dimensions has occurred, or, more properly speaking, a separation of the general equation into two others, the utmost simplification that can possibly arise from the mode of assuming the system of reference, has been here effected. It is very probable that this is the only way in which that separation could have been made; and hence there is little hope of further improvement in the process by the transformation of coordinates, at least so far as origin of r is concerned*.

The point O (Plate XIV. fig. 14.) is a quadruple point, whilst any line drawn through O can cut the curve in only two points besides O. The values of r can therefore, except at this point, be only two, whilst in it they are four. Such is the obvious

* By transposing the origin of θ to the line bisecting the angle TOU, (that is, putting $\alpha_{\prime\prime} = \varepsilon + \delta, \alpha_{\prime} = \varepsilon - \delta$, and hence $\overline{\theta - \alpha_{\prime\prime}} = \overline{\theta - \varepsilon} - \delta = \chi - \delta$, and $\overline{\theta - \varepsilon} + \delta = \chi + \delta$,) we obtain a result in some respects better adapted to our final purpose; but still as the equation so transformed offers insuperable obstacles to a *complete* discussion of the curve, and we have been otherwise able to effect without that aid, it is unnecessary to do more than allude to it here. The same may be said of the rectangular equation (76.) itself, when the origin is transposed to T, V, M, or U, and referred either to oblique coordinates coincident with the asymptotes, or to rectangular coordinates bisecting the angles of the asymptotes, or having one coincident with the magnetic axis itself; or again, referring the system to rectangular coordinates through O, one of which is parallel to the magnetic axis; and so on. By one or other of these I have been able to obtain a few properties of the curve, but by no one, nor by all of them together, to deduce anything approaching to a complete development of its properties, the form of its branches, or its singular points. Could it have been so effected, there is no question that it would be the more elegant mode of proceeding, viewed in reference to mathematical symmetry; and for that reason I have spent a good deal of time in attempting it; but after repeated failures, I am compelled to admit that, in the present case, "*fallere et fugere est triumphus.*"

interpretation of equation (79.), or of its component ones (80.) and (81.). The system is, then, in this case, *coincident with* that of the curve, whose values of r are two, and a circle of infinitely small radius. Still we are not entitled to say that the whole system is actually so composed, since by taking the origin of polar coordinates in any other way, we should not find the equations expressive of *that* condition fulfilled. It is essential to keep this principle in view (and it is too often overlooked) in the discussion of the properties of curve lines.

In the second place, let us ascertain if (81.) admits of any infinite values of r. In this case, the denominator is equal to zero, (since the numerator cannot become infinite), or,

$$(\cos \alpha_{,,} \sin \overline{\theta - \alpha_{,}})^{\frac{2}{3}} - (\cos \alpha_{,} \sin \overline{\theta - \alpha_{,,}})^{\frac{2}{3}} = 0. \quad . \quad . \quad . \quad . \quad . \quad (101.)$$

This is fulfilled by

$$\left.\begin{array}{l} \text{1st, } (\cos \alpha_{,} \sin \overline{\theta - \alpha_{,}})^{\frac{1}{3}} - (\cos \alpha_{,} \sin \overline{\theta - \alpha_{,,}})^{\frac{1}{3}} = 0 \\ \text{2nd, } (\cos \alpha_{,,} \sin \overline{\theta - \alpha_{,}})^{\frac{1}{3}} + (\cos \alpha_{,} \sin \overline{\theta - \alpha_{,,}})^{\frac{1}{3}} = 0. \end{array}\right\} \quad . \quad . \quad (102.)$$

and

Transpose and cube the first of these; then by expansion and aggregation, we find

$$\sin (\alpha_{,} - \alpha_{,,}) \cos \theta = 0,$$

or,

$$\theta = \pm \frac{\pi}{2}.$$

Hence the radius vector parallel to the magnetic axis is infinite, whilst θ is finite; and hence it is either an asymptote or parallel to an asymptote. To ascertain which, let us conceive the origin transferred to V (which, since it is only the *position* of a straight line we are seeking, we are entitled to do): then, since whether V be between the poles or beyond one of them, we have $\alpha_{,} - \alpha_{,,} = 0$ or $\alpha_{,} - \alpha_{,,} = \pi$, the equation $\sin (\alpha_{,} - \alpha_{,,}) \cos \theta = 0$ is fulfilled by the coefficient of the variable, that line is the asymptote. This will, however, also appear from other considerations in the next section.

Again, transpose and cube the second of equations (102.), then we obtain

$$\tan \theta = \frac{1}{2} (\tan \alpha_{,} + \tan \alpha_{,,}),$$

which indicates the radius vector through the centre M of the magnet, and which, since θ is finite, whilst r is infinite, the line O M is either an asymptote or parallel to one. The determination which would be the case, would, from the equations themselves, be not difficult but rather laborious; and hence we shall employ another method in the next section to show that it is itself the asymptote to two infinite branches of the divergent curve.

Thirdly. To ascertain whether there be any equal values of r. In this case the quantity under the radical symbol becomes equal to zero, or, which is the same thing,

$$\left.\begin{array}{l} (\sec^2 \alpha_{,,} - 2 \sec \alpha_{,,} \sec \alpha_{,} \cos \overline{\theta - \alpha_{,}} \cos \overline{\theta - \alpha_{,,}} + \sec^2 \alpha_{,}) (\cos \alpha_{,} \cos \alpha_{,,} \sin \overline{\theta - \alpha_{,}} \sin \overline{\theta - \alpha_{,,}})^{\frac{2}{3}} \\ \quad = \sec^2 \alpha_{,,} \sin^2 \overline{\theta - \alpha_{,,}} (\cos \alpha_{,,} \sin \overline{\theta - \alpha_{,}})^{\frac{4}{3}} + \sec^2 \alpha_{,} \sin^2 \overline{\theta - \alpha_{,}} (\cos \alpha_{,} \sin \overline{\theta - \alpha_{,,}})^{\frac{4}{3}} \end{array}\right\} \quad (103.)$$

which is fulfilled either by $\theta = \alpha_{,}$ or $\theta = \alpha_{,,}$. At the points T and U, then, the two values of r for such values of θ as lie to the right and left of these points coalesce, and the radius becomes a tangent to the branches of the curve that meet there. In the same way for values of θ between these points the branches of the curve coalesce, and the radii-vectores form at those points tangents also to these branches of the system. It also immediately appears that there are no other real values of θ which fulfil the condition, and hence there are no other such points besides those now determined.

By inserting these values of θ in (91.) we have in the two cases respectively, as we should anticipate, $r = b \sec \alpha_{,}$, and $r = b \sec \alpha_{,,}$ for the values of r.

XXVIII.—*The Branches of the Curve of Verticity corresponding to the Convergent System of Magnetic Branches.* (Plate XIV. fig. 14.)

Let T, M, U be, as before, the poles and the middle of the axis, and O the centre of the earth. Draw X′ X through O parallel to T U and O V, or Y′ Y perpendicular to it. Also draw M H parallel to O V, and the lines O T, O V, which produce to Q′ and Q respectively; and confining our attention to one side of the middle of the axis M, the corresponding branches may be thus investigated.

There is always one particular convergent curve to some parameter β which will touch the line O U at the point U, and which, since O and U are given points, is determinate and single (XXII. XXIII.). The same is true of O T; and the corresponding values are $\cos \beta = \sin^2 \frac{1}{2} \alpha_{,}$ and $\cos \beta = \sin^2 \frac{1}{2} \alpha_{,,}$.

Those curves only in which β enters as a larger angle, or in which $\cos \beta$ diminishes, can have tangents drawn to them from O. For if any line O S′ Q′ be drawn from O between O T and O U, or to cut the magnetic axis itself between the poles, the curve being wholly concave to O Y, it will *intersect* the curve at Q′; and since there are no points of inflexion (XXV.) between T and U in the branch U Q′, it cannot again meet the curve, and consequently cannot touch it. Hence only the branches of those convergent curves which depend on a parameter β greater than that already specified can be touched by lines from O.

No point of the convergent system of curves of verticity can therefore lie in the region Q′ T U Q.

Let any curve U N be taken to the right of the line O U and above the axis. Then since the curve is convex (its tangent U R at U making a less angle R U Z with the axis than O U Q) to the point O, a tangent can be drawn from O to a point N in it. And since there are no points of inflexion in the magnetic branch, there can be only one tangent so drawn to that branch.

The points of contact, or the points of this branch of the curve of verticity, are always at a finite distance from the line of the magnetic axis. For whilst the magnetic curve itself is finite, all its points are at a finite distance from the axis Z′ Z; and hence all the points of contact of lines from O to it, which constitute the curves of verticity, are also at finite distances from that line. Moreover, when the magnetic curve becomes infinite, it coincides with the axis Z′ Z (XXVI.), and hence the final tangent must also have a point of coincidence at an infinite distance from M with the line Z′ Z. Nor can the curve meet the line Z′ Z in any point to the right of U, and at a finite distance from it. Since, if it can, the convergent branch of the magnetic curve also meets the axis at that point. But the tangent to every finite branch of a convergent magnetic curve makes a finite angle with Z′ Z at U, and having no points of inflexion, it cannot meet the tangent again. But if it meet Z′ Z, it must have previously crossed its tangent at the point U. Hence the hypothesis of the curve of verticity meeting the axis Z′ Z at a finite distance involves a contradiction. The magnetic axis Z′ Z is therefore an asymptote to this branch of the curve of verticity.

Precisely the same circumstances take place in the branch lying above the axis and to the left of T. The convergent curve has therefore two asymptotic branches, the line of the magnetic axis being the rectilinear asymptote to them both; and no other points of the curve lie on the opposite side of that line from O.

In the next place, for the determination of the branch or branches of the curve lying on the same side of the magnetic axis with O:—

For the same reason as before, the line O Q′ cannot be a tangent to any one of the curves lying below the axis; and the first curve that can have a tangent is that whose tangent at U coincides with T O; and as the same line is a tangent to the curve above and to the curve below the magnetic axes at their common point, the branches above and below are *continuous* ones.

For any other position, as O N, there is always one convergent magnetic curve which can touch it, as at N′; and the two distances O N and O N′ are the two values of r, which correspond to any specified value of θ, as V O N, in the general polar equation of the curve (82.). The point N′ will hence trace out another branch of the convergent curve U N′ H (H being determined as already specified) corresponding to the asymptotic branch to the right of U, which branch will be finite, and comprised within the rectangle O V U L. Also, since by (82.) there are but two values of r corresponding to each value of θ, there are no other branches to the right of M H besides these two.

Divide T U produced in S′, in the triplicate ratio of T O : U O, (or $S'U^3 : S'T^3 ::$ O U : T U), then O S′ is a tangent to the curve which passes through O. No curve which passes more remotely from the axis than O can have a tangent drawn to it from O, since O is on the concave side of all such curves, and they have no points of inflexion. Nor can curves passing through T have tangents drawn to them, for reasons before given, till β has become such as to render O T a tangent at T. From that state till the curve passes through O, the point O lies on the convex side of them, and hence from O two tangents can be drawn to each individual curve, one on each side of O, but which coalesce in the single tangent at that point. The curve is hence continuous from U to O, and from T to O; and since they have at that point a coalescent tangent, they form continuous branches not interrupted at O, their point of union. The branch T O H is therefore also finite, and corresponds to the asymptotic branch to the left of T, as U N′ H did to the asymptotic branch to the right of U.

In all these cases, and for all values of θ from $\alpha_{\prime}$ to $-\frac{\pi}{2}$, and from α to $+\frac{\pi}{2}$, we have therefore found two values of r, and by equation (82.) these are all that can exist. Nor can any other tangents than those we have described be drawn to the *convergent* system of magnetic curves; *and hence it appears that the general equation* (82.) *applies to the convergent curves of verticity for no other than the values above assigned.*

XXIX.—*On the Branches of the Curve of Verticity corresponding to the divergent System of Magnetic Branches.*

Let O, T, M, U, and Z′ Z denote the same things as in XXVIII.; and draw the line O T, O U, and O M, which continue indefinitely. (Plate XIV. fig. 15.)

Then since each individual divergent curve has an asymptote passing through M (XXIV.), the line O M is an asymptote to some one curve; and as the asymptote is itself a tangent from O to that curve at a point infinitely distant, the corresponding point in the divergent branch of the curve verticity is itself that same point. Whence the line O M is an asymptote to the branch of the curve of verticity; and since the asymptote itself lies in the angle Y M U, the branch to which that asymptote belongs emanates from U, or otherwise the magnetic curve must have crossed the vertical M Y.

There is also one curve which can touch O U at U, determined, as before explained, by the value of β; and there can be no one drawn between this and the produced axis U Z, which admits of a tangent from O; for in that case the curve is concave to O, and has no points of inflexion in that branch.

Take some intermediate position, as O N; then since the point O is on the convex side of the curve U N, a tangent can be drawn; and only one for the curve is convex to M Y, and has no points of inflexion in its finite branch. Nor again, can it be touched in the other part of the branch by a tangent from O, since O is on the opposite side of the tangent U T at the point of inflexion U. Also, as this is the case for all positions of the asymptote within the angle P M U, the series of contacts will trace out a curve, commencing at O, and having O M P for an asymptote; and there is only one branch situated within the angle U M P.

Again, let the curves be continued, whose asymptotes lie within the angle P M Y, as M P′. Then any line from O to the *left* of M will cut the asymptote before it meets the curve, and hence cannot be a tangent to that curve. None of the points of the curve of verticity corresponding to the divergent branches of the magnetic curve are therefore situated within the angle P′ M Y.

Attending next to what takes place in the angle U M Y′, we see that the first curve that can have a tangent drawn from O is that which has O U for its tangent: for any other would lie on the opposite side of its asymptote from O.

For every position of O N there is always one single magnetic divergent curve which can touch it in the angle U M Y′ as at N′, each of which will be successively found by giving to β successive values infinitesimally near to each other. Also as these curves approach more and more towards asymptotism with M Y′, they approach in their course more nearly to the point M: till when M Y′ becomes the actual position of the asymptote, the curve is reduced to the single *point* M; and it has been shown (XXVI.) that any line, as M O, is a tangent at this point to that individual case of the curve. The curve of verticity which corresponds to the divergent magnetic branches in the angle U M Y′ is, then, finite, and comprised between the lines U O and U M within that angle.

Moreover, as the same line O U Q is a tangent to the infinite branch in the angle P M U and to the finite branch in the angle U M Y′, these two branches are the one continuous of the other.

Proceeding to the angle Y M T, a precisely similar series of circumstances takes place as in its opposite angle U M Y′. The branch has O T for a tangent at T; it proceeds gradually round till it arrives at M and *meets* the branch U N′ M at M. We should be led to expect, from the principle of the continuity of the same law holding at all points in the course of a locus, that the two branches which meet at M are continuous: but as we have no other property of the point M before us except the *indeterminateness* of the tangent to the magnetic curve at that point, we might hesitate, did any conclusion of importance hinge upon it, to affirm the continuity of those branches positively. But by transposing the origin of rectangular coordinates in equation (76.) to M, and investigating the number and position of the tangents at the origin, the question is settled in the affirmative. The process is, however, long and rather intricate; and as we have no occasion to employ the property in our present inquiries, it is unnecessary to give its investigation here.

In the same manner as in the angle Y M U, we may divide its opposite and the only remaining region T M Y′ into two parts T M P″ and P″ M Y′, and consider them in order *.

The first curve which can have a tangent drawn to it from O is that which has O T for its tangent: and as before, the branch thus generated having a common tangent with the branch above the axis, they will form a continuous curve at that point.

To all the curves whose asymptotes lie in the angle T M P″ there can be one tangent drawn, and only one: for the point O is on the convex side of the curve viewed in reference to the tangent T U at the point of inflexion T of the magnetic curve. These will trace out a branch terminating at some point between O and T, as R. Whilst the magnetic curves vary through the interval of their passing from R to O, the point O will be not only on the convex side of the curve with respect to T U, but also between the curve and its asymptote. In this region, then, two tangents can be

* See also Plate XV. fig. 16, where this part of the work is drawn to a larger scale.

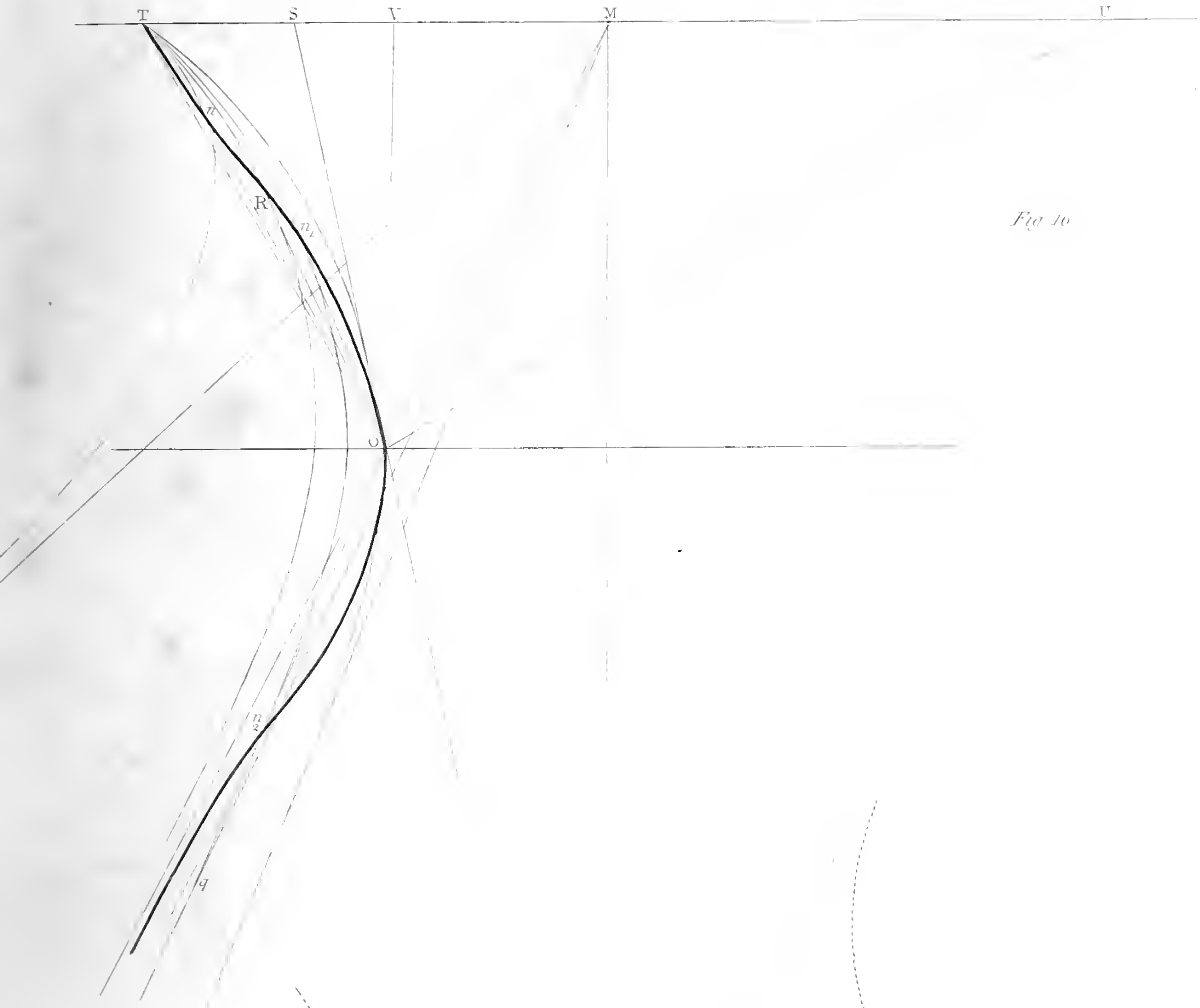
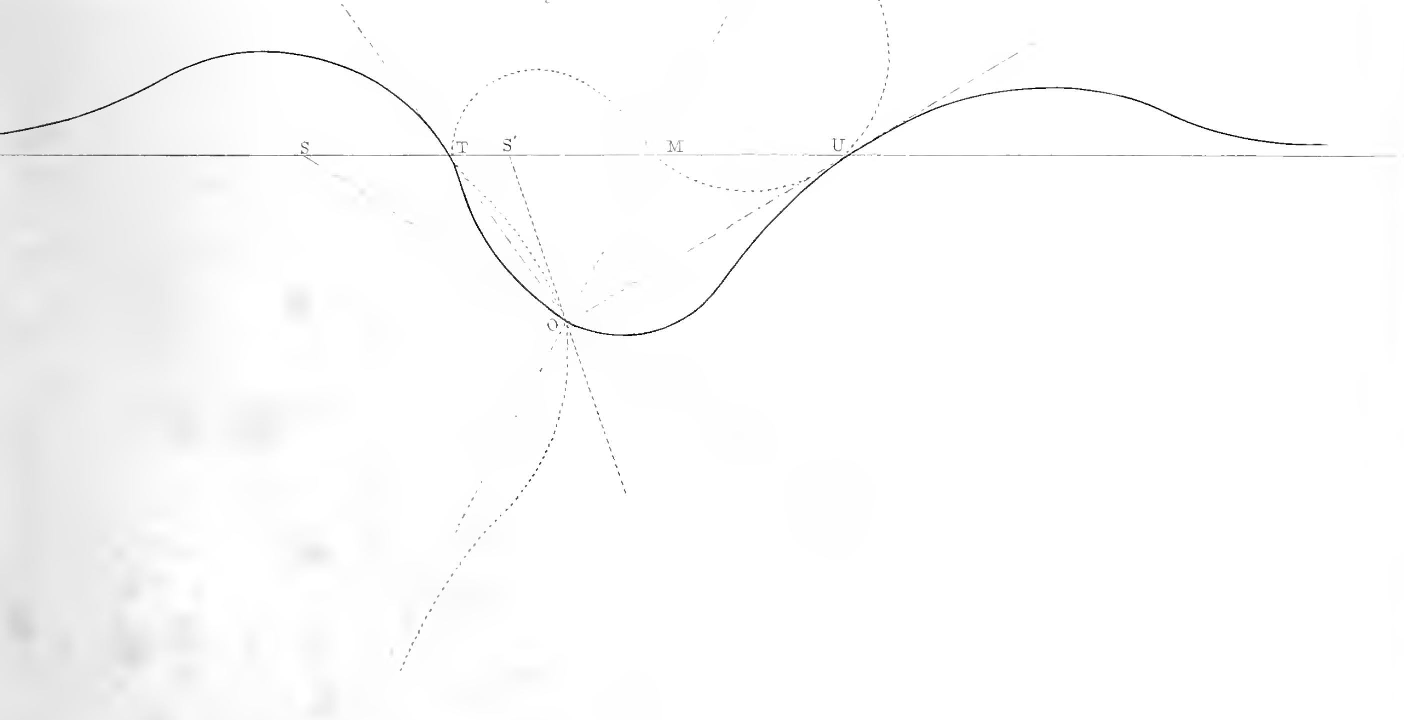


Fig. 16

Fig. 17



drawn from O to each curve, and trace out segments coalescing at O, where the two tangents coalesce in one, and the segments therefore also being continuous. Portions, then, of the curve of verticity will lie on both sides of M O in this region: one portion in the angle T M O between R and O, and the other in the angle O M Y′.

Now when the asymptote to the magnetic curve passes through O, we have seen that there is a tangent to it at R. But in the same case, M O P″ is itself a tangent to the same branch at a point infinitely distant. As the magnetic curves approach towards O, there will still be two tangents possible, the point of contact of one becoming continually nearer to O in the angle P″ M Y, and the other in the angle R M P″, till, as before shown, they coalesce continuously in O. The branch lying in the angle P″ M Y′, therefore, has M O P″ for a rectilinear asymptote, as the branch in the opposite region had M P.

It would probably be difficult to establish, from any considerations furnished by the properties of the magnetic curves, the utmost angular extent to which the infinite branch lying in the angle O M Q extended from the asymptote M O P″. But recurring to the fact furnished by equation (82.), that for every value of θ which gives one real value of r there is also a second real value; and as for all values of θ comprised between $\alpha_{,}$ and $\alpha_{,,}$, or within the angular region T O U, we have shown that there is one real value of r; and, with the exception of the angular region M O W, (W O W′ being the tangent to the magnetic curve at O,) we have established two values; it follows, that for completing the whole series, and fulfilling the conditions of (82.), there must be a second value of r for every direction which a line can take in the angle P″ O W′; or, which is the same thing, the infinite branch will touch the line O W′, but can never pass to the right of it; that is, it lies wholly in the angle P″O W′, and never meets the line O W′ again after it passes through O.

The course of each curve of verticity is thus fully made out: and it now appears that each is confined within specific and peculiar angular regions referred to lines drawn from O through the magnetic centres of force. Though in itself the divergent system is not required in the present physical problem, yet the separation of them as constituents of the equation (76.) was essential to enable us to ascertain the separate branches of the convergent one, which we have yet to discuss in its application to that problem. For the more complete and ready understanding of the whole system of branches, I refer to the figure of them as the representation of the complete equation (76.), the dotted branches representing the curve of verticity for the divergent branches of the magnetic curve, and the full-lined ones that for the convergent branches, to the consideration of which we shall hereafter return*. (Plate XV. fig. 17.)

* In the cases examined, the point O was without M H: but when it is in that line the divergent curve has its asymptotic branches both approaching the asymptotic on the same side of O, viz. on that on the opposite side of T U from O. The figure (Plate XVI. fig. 18.) will render further verbal detail unnecessary here.

XXX.—*Professor* LESLIE'S *Property of the Magnetic Curve, and a Genesis of the Curve of Verticity founded on it.*

If tangents be drawn from a given point in the magnetic axis to a series of magnetic curves, either convergent or divergent, the locus of the points of contact is a given circle.

For since the point S is given (Plate XVI. fig. 19.), the ratio S T : SU is given, and hence its subtriplicate N T : N U is also given; and the points T and U being also given, the conclusion follows from Lemma I.

The same conclusion also follows from our equation (76.), as in this case $b_{\prime}=b_{\prime\prime}=0$ (taking the axis of a parallel to T U), and the equation is converted at once into

$$\left.\begin{aligned} &y^{\frac{1}{3}}\, a_{\prime}^{\frac{2}{3}} \{(a_{\prime\prime}-x)^2+y^2\} - y^{\frac{1}{3}}\, a_{\prime\prime}^{\frac{2}{3}} \{(a_{\prime}-x)^2+y^2\}=0, \\ &\text{or } y^{\frac{1}{3}}=0, \text{ and } (a_{\prime}^{\frac{2}{3}}-a_{\prime\prime}^{\frac{2}{3}})(x^2+y^2) - a_{\prime}^{\frac{2}{3}} a_{\prime\prime}^{\frac{2}{3}} \{a_{\prime\prime}^{\frac{1}{3}}-a_{\prime}^{\frac{1}{3}}\} x + a_{\prime}^{\frac{2}{3}} a_{\prime\prime}^{\frac{2}{3}} \{a_{\prime}^{\frac{2}{3}}-a_{\prime\prime}^{\frac{2}{3}}\} = 0. \end{aligned}\right\} (104.)$$

The former of these is a foreign factor introduced, so far as the locus of a lower order than 76 is concerned, by the eliminations through which that equation was obtained: the latter is the equation of a circle whose centre is in the axis, but not in the form best adapted for use; which would be to refer it to M, and thereby make $-a_{\prime\prime}=a_{\prime}=a$. As, however, we only require it for constructive purposes and geometrical reasoning, it is unnecessary to examine the equation further; and, except as a verification by a particular case of our general equation (76.), would not have been noticed here. Where it is possible, such verifications, it is admitted on all hands, should be made.

Genesis of the curve of verticity.—Take any point S in the magnetic axis, and find two lines in the triplicate ratio of S T : T U*. Describe the circle D N C, which is the locus of lines inflected from T and U in this triplicate ratio, (Lemma I.,) and let it cut the line O S in N. N is a point in the curve.

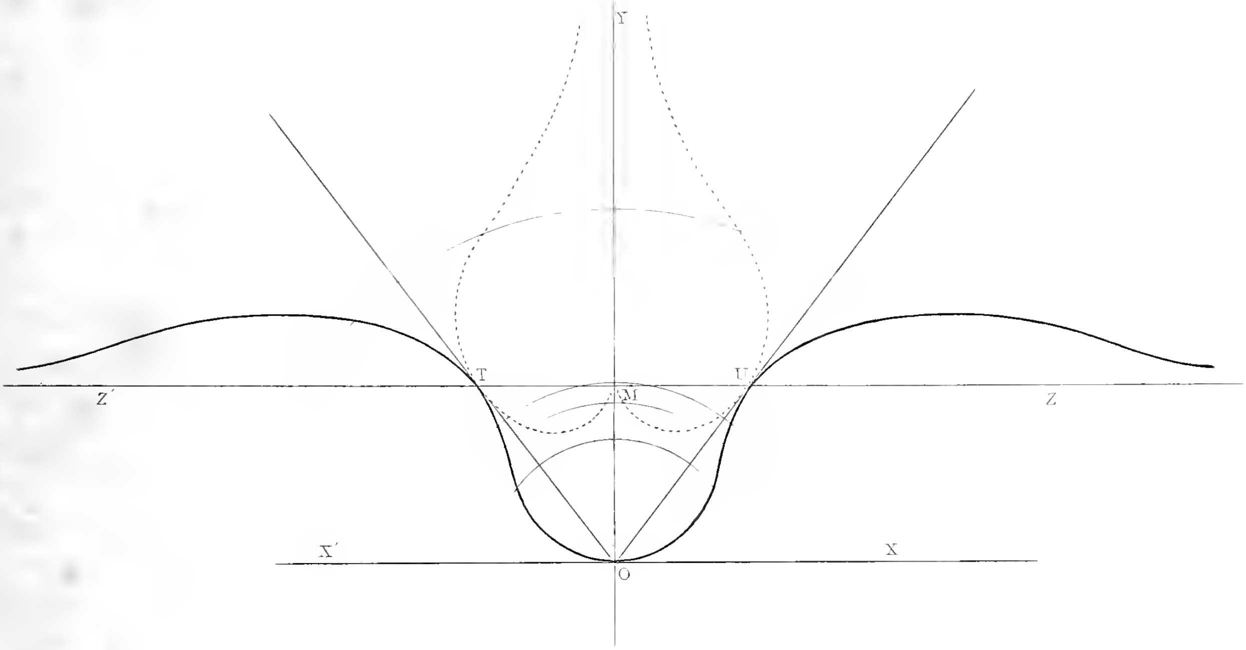
This does not enable us, however, to discriminate the branches themselves of the two classes of curves; nor, therefore, supersede the necessity of the preceding investigations.

XXXI.—*The Circle whose centre is* O *cannot cut the asymptotic branches of the convergent system of the Curve of Verticity in more than two points, one in each branch.*

Let N be a point in the curve of verticity where it cuts the earth's surface such that O N is the direction of the needle tending the centre of the earth. Draw the radiants N T and N U; and let N O intersect the magnetic axis in S. Then S T : S U is the triplicate of the ratio N T : N U, or of $r_{\prime} : r_{\prime\prime}$. Describe the circle D N C (Lemma I.), which is the locus of the point N corresponding to S; and with centre O and

* Dr. ROGET, Secretary of the Royal Society, has given a very elegant construction of this problem in the Journal of the Royal Institution for February 1831.

Fig. 18.


Y
T
U
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M
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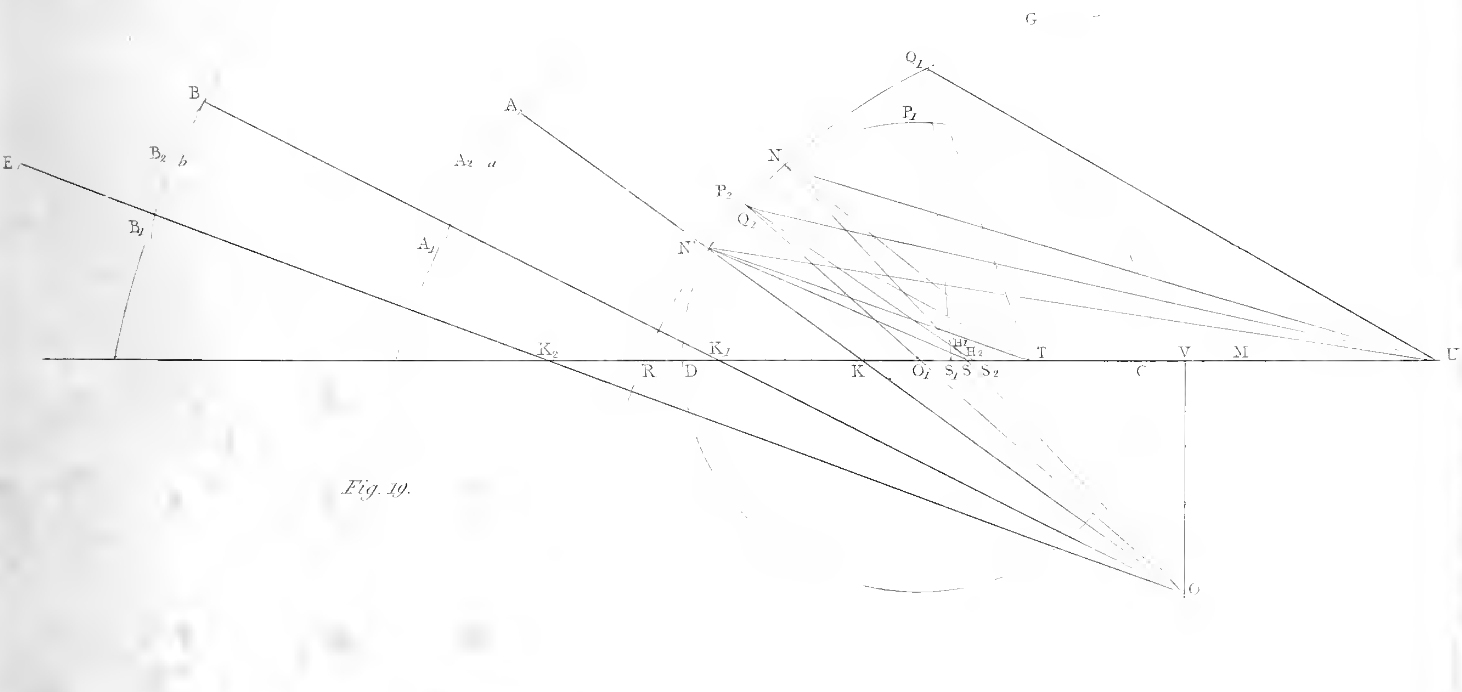
X
O


G
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B
A
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B₂ b
A₂ a
P₂
Q₂
B₁
A₁
N'
K₂
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K
O₁
S₁ S S₂
T
V
M
U
R
D
C
O

Fig. 19.

distance O N describe the circle R N G, which will be the magnetic meridian. Through the centres O and $O_{,}$ ($O_{,}$ being the centre of the circle D N C) draw O $O_{,}$, cutting the circles in Q_2 and P_2: for it will cut them both since it passes through both their centres.

Then (Lemma 8.) the point $O_{,}$ always lies to the left of S, and at a finite distance from it. Hence O P_2 is greater than O N, and hence the circle whose centre is O cuts that whose centre is $O_{,}$ in another point N. Moreover, the line O $O_{,}$ passing through both the centres, is perpendicular to their common chord N N′, and bisects both it and the arcs N′ Q_2 N and N′ P_2 N in Q_2 and P_2 respectively.

But since the line S $O_{,}$ is finite in all cases (Lemma 8.), the angles N′ O P_2 and P_2 O N are also finite, and hence $O_{,}$ K (K being the intersection of N′ O with the magnetic axis) is also finite, and, obviously, greater than S $O_{,}$.

There are, hence, three finite segments of the magnetic meridian in which the needle may be placed, distinct from one another, and each requiring a distinct consideration: and we proceed to prove that in whichever of them placed, except at N, the line of its natural direction will not pass through O, the centre of the earth.

1. *When* $Q_{,}$ *is taken in the arc of the circle* R N G *between* N *and* G, *as the position of a magnetic needle.* Then drawing $Q_{,}$T, $Q_{,}$U, we have, by Lemma 2., the ratio T $Q_{,}$: $Q_{,}$ U less than the ratio T N : N U; and hence, drawing the tangent at $Q_{,}$ to cut the magnetic axis at $S_{,}$, the ratio of $S_{,}$ T : $S_{,}$ U, which is the triplicate of this, is also less than the ratio S T : S U. The point $S_{,}$, therefore, lies to the left of S, or more remote from T than S is. Consequently $Q_{,}$ $S_{,}$ cuts N S on the side of the magnetic axis at $H_{,}$, opposite to the centre O of the magnetic meridian; and as these lines have once intersected they cannot intersect again, and hence the line $Q_{,}$ $S_{,}$ cannot pass through O; or, in other words, the needle at $Q_{,}$ in the arc N G cannot be vertical to the earth's surface.

2. *When* Q_2 *is taken in the arc* N′ Q_2 N *of the magnetic meridian.* Draw Q_2 T, Q_2 U, and the tangent at Q_2 cutting the magnetic axis in S_2. Then (Lemma 2.) the ratio Q_2 T : Q_2 U is greater than the ratio N T : T U; and hence the ratio S_2 T : S_2 U is also greater than S T : T U, these being the triplicates of those. The point S_2 falls, therefore, nearer to T than S does. The line of the needle's direction Q_2 S_2 at Q_2, therefore, cuts that at N, at a point H_2 on the side of the magnetic axis opposite to O, and hence, as in the last case, cannot pass through O, the centre of the earth.

3. *Neither can a needle placed at any point in the arc* R N *pass through* O. For, draw $N_{,}$ S. This is the direction of a needle at N′, and hence this does not pass through O. Join N′ O, cutting the magnetic axis in K. Then S K is a finite quantity, and hence the ratio K T : K U is less than S T : S U, and the point A of the curve of verticity corresponding to it has its radiants in a less ratio than N′ T : N′ N. That point, therefore, (Lemma 5.) must be more remote from the point of least ratio (which, obviously, from Lemma 3., lies on the opposite side of the axis T U, the angle T K O being acute), or beyond the point N.

With centre O, and distance O A, describe the circle A *a* A′, and with the ratio A T : A U, the circle $A A_2 A_1$; and, as in the former cases, these arcs will be finite. Join $A_{\prime}$ O, cutting the magnetic axis in $K_{\prime}$. Then also none of the needles either in $A_1 A_2 A$ or in A G, except that at A, will pass through O, and that at $A_{\prime}$ passes through $K_{\prime}$.

In the same way, by producing O $A_{\prime}$ to B till B T : B U is the triplicate of the ratio $K_{\prime}$ T : $K_{\prime}$ U, we shall have another point B in the curve of verticity; through which, with centre O and distance O B, describe a circle cutting the circle of ratios of B in the points B and $B_{\prime}$; and join $B_{\prime}$ O, cutting the magnetic axis in K_2. And repeat this process as far as may be necessary both as to construction and reasoning.

Then, since the distances T S, S K, K K_1, K_1 K_2, ... are all finite, and the distance T R also finite, a continued repetition of these processes will at length conduct us to a point K_n , either coincident with R, or more remote from T than R is. Let E be a corresponding point in the curve of verticity. Then the segment of the curve joining N and A must lie in the mixtilineal angle formed by the line A N′ and the arc $N' Q_2 N$; the segment A B is in the mixtilineal angle B $A_{\prime}$ A, and so on to E. But the arc of the magnetic meridian lies *wholly without* this series of angles, and hence cannot in any one point coincide with the segments of the curve which lies *within* them. The magnetic meridian, therefore, can only cut the asymptotic branch of the curve which lies to the left of T and above the axis, in one single point N.

In the same way, exactly, may it be shown that the magnetic meridian can only cut the other asymptotic branch to the right of U in one single point.

By processes of the same nature it may be proved that the finite branches of the convergent system can never be cut in more than two points by a circle whose centre is O; and that the same is true to the divergent system. But neither of these cases falls within the objects of the *physical problem* under consideration, it would be superfluous to enter upon them here; although for giving completeness to the *geometrical problem* such a discussion would be indispensable. However, after what has been done in the foregoing pages, this portion of it can present no difficulty to the geometer who may be disposed to follow it out, as the reasonings which I have employed in its solution, and which completely apply to all the cases, is essentially the same as that detailed in the case here discussed at length. It is only necessary to observe, that the positions of the finite and infinite branches in the two systems are so situated that, in the divergent system, all four branches, the two finite and the two infinite ones, *may be* cut by the same circle, or only the two infinite ones, depending upon the radius of that circle: whilst in the convergent system, which we have had occasion here to consider, only two of the branches, either the two infinite ones, or the two finite ones, or one of each, can be cut by the same circle.

XXXII.—The consequence of all this investigation, then, is, *that if we admit the hypothesis of two centres of magnetic force situated within the earth, there will be two,* *and only two, points on the earth's surface, at which the needle can take a position vertical to the horizon.*

Whether this be the number actually existing on the surface of our earth, we are not at present in a condition to determine. One such undoubtedly there is, and a second is probable, but its position has not been assigned; neither from any observations yet published, can it be even approximately determined, nor, therefore, its existence positively affirmed. I am not aware that any observations give reason to suspect the existence of more than these two; and hence, so far as we can judge from the data before us, the conclusion now obtained as a consequence of two magnetic centres of force, is consistent with the phenomena for which the hypothesis is required to account. *It is therefore a strong argument, in the present state of our actual knowledge of the phenomena of terrestrial magnetism, for the truth of that hypothesis.*

XXXIII.—No particular specification of the cases which can arise from the relative positions of the centre of the earth, and the coordinates of the centres of force, is here necessary; as, except in the case of the magnetic axis passing through the centre of the earth, no considerable simplification of the equation, nor, therefore, any essential variation in the form of the curve of verticity, so far as I have remarked, can arise. When O is in the line $y \, \mathrm{M} \, y_{\prime}$ (fig. 18.) the branches are symmetrical, it is true, and the form rather more simple than when it is in any other position; and as we remove O to points further on either side from that line, the curve becomes more and more *bizarre*, but still it retains the same *general features* as in its more simple case; and its branches have in all cases the same character, whatever be the coordinates of O with respect to the magnetic poles, and not situated in the same line with them.

If, moreover, we have determined two points on the surface of the earth at which the needle can become vertical, and describe the great circle passing through them, we know that the poles themselves are in this plane, and situated somewhere in the concave angle formed by drawing the radii from those points to the centre. As, however, four quantities are necessary to express the coordinates of those poles, and we have only two conditions given, the actual position of the points themselves cannot be determined from these data. The problem is hence, even when both points of verticity are known, still left *indeterminate*. Nevertheless, by combining these with other observations upon the dip and variation made at different, and still better at distant, places, the problem becomes capable of solution.

Again, if we could determine the points on the earth's surface at which the *intensity* is a maximum (in respect to its contiguous points, in all directions from it), we should obtain other conditions, which united with those of the two points of verticity, from which the positions of the magnetic poles might be determined. To the solution of this problem, the determination of maximum-intensity points, I shall next direct the attention of the Royal Society. The investigations are already completed, and I hope shortly to find leisure to put them into order; and shall only premise here, that as the results of the hypotheses of like and unlike poles are comprised in the same equation, it will, as was the case in the present discussion, require a peculiar mode of treatment to separate and define the curves of equal intensity belonging to each of them.

XXXIV.—It will be remarked, that in the equations of the magnetic curve, the final position of the needle, or that which it takes when its centre is coincident with either centre of force, is different from that which is exhibited by a needle acted on by an artificial magnet in all our experiments. This might at first sight seem to throw some doubt on the validity of the principle employed in deriving the equation of the magnetic curve; but a little reflection will convince us that the conditions of such experiments are different from those which obtain in the case before us.

In all our experiments, the length of the needle itself bears a finite ratio to the distance of its centre from the poles of the magnet upon which we experiment, and hence the action becomes mutual the two magnets, and the system of actions being thus rendered compound, its results must be expressed by a more complicated formula than in the case we have supposed, and from which our equations of the magnetic curve have been derived. This complexity of the conditions implies a corresponding complexity of the equation by which they are expressed; but uniform experience has shown that as we diminish the length of the needle, and increase its distance from the poles, the observed results are more nearly approximative to the results of the hypothesis from which we have started. In the case of the curves exhibited by iron filings strewed on a paper above a bar magnet, the approach of the observed curve to the calculated one is very close*. But in this case the length of the needle (or magnetized particle of the iron) is very small in comparison with the magnet and with its distance from the magnetic poles; and, moreover, has in itself so little magnetic intensity as to exert an insensible reciprocal influence on the state of the bar itself. This is precisely a miniature representation of the case of a small needle acted on by the terrestrial magnet, though the ratio of the particle of iron to the magnetic bar is many times greater than that of a needle to the terrestrial magnet; and hence the discrepancy between the observed and calculated result in the former case is many times greater than in the latter. No ground of exception to our plan of inquiry can therefore be found in this circumstance, but rather a confirmation of its validity in reference to the use we have made of it.

* LESLIE's Geometrical Analysis, p. 405.

Royal Military Academy, Woolwich,
January 27th, 1836.

X. *On Voltaic Combinations. In a Letter addressed to* MICHAEL FARADAY, *D.C.L. F.R.S., Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal & Imp. Acadd. of Science, Paris, Petersburgh, &c. By* J. FREDERIC DANIELL, *F.R.S., Prof. Chem. in King's College, London.*

Received January 26,—Read February 11, 1836.

MY DEAR FARADAY,

YOU know how deep an interest I have taken in your "Experimental Researches in Electricity," and how zealously I have availed myself of the opportunities, which you have ever kindly afforded me, of profiting by your oral explanation of such difficulties as occurred to me in the study of your last series of papers in the Philosophical Transactions. Having been early impressed with the conviction that the science of chemistry would date, from their publication, one of its great revolutions and eras of fresh impulse, I have been careful not only to store my own mind with the new facts and reasonings which they contain, but to impress them upon my pupils in my class room; and for this purpose I have been led to contrive some new apparatus and forms of experiments, by which the principles which you have promulgated have been verified, and, I think, in some instances demonstrated to advantage. In thus working in the mine which you have opened, you will be the last to be surprised if I should have moreover stumbled upon some threads of ore which you may have passed by, or temporarily abandoned, whilst following the main lode; and you will not be displeased that I venture to submit to your judgement whether there be enough of novelty or importance in the following observations to render them worthy of the attention of the Royal Society.

One result, I know, will gratify you; namely, that amongst the almost innumerable tests to which I have exposed your great discovery of the *definite chemical action of electricity*, I have found no fact to militate against it; and you will the more rejoice should I succeed in proving to you that, under the direction of this fundamental principle, I have been led to the construction of a voltaic arrangement, which furnishes a constant current of electricity for any length of time which may be required; and have thus been enabled to remove one of the greatest difficulties which have hitherto obstructed those who have endeavoured to measure and compare the different voltaic phenomena, viz. the variableness of the action of the common batteries.

You are aware of the vexatious accident by which I lost the original notes of my experiments during the last year, which will prevent my giving you such full details of some of the results as I could have desired, although I have been careful to repeat all those in which measures are of fundamental importance.

I shall first beg to direct your attention to an arrangement which I purpose to distinguish by the name of the *Dissected Battery*; it has answered an analogous purpose to me of one of those optical instruments, by looking through which a multitude of confused lines assume a regular disposition; and many detached facts, well known before, have become clearer and of new importance, from their connexion and comparison with each other by its means. The battery consists of ten glass cells, a section of one of which is represented in the accompanying figures.

(*a*, *b*, *c*, *d*), Plate VIII. fig. 1. is a foot of solid glass, containing a cavity (*e*, *f*, *g*, *h*), the upper part of which is fitted with a stopper (*g*, *h*). Through this stopper the stems of the two plates (*i*, *j*, *k*, *l*, *m*, *n*) pass into the lower part of the cavity, which is divided into two cells by the partition (*o*, *p*), and each of which contains mercury, into which the wires respectively dip. This arrangement admits of the plates being changed at pleasure with little difficulty. The plates may be connected together, or with the plates of other cells, by means of wires (*p*, *q*,) passing through the lateral holes (*t*, *u*) and dipping also into the cups of mercury. To the glass foot thus arranged a glass shade (*v*, *w*, *x*, *y*, *z*, *z*) is fitted by grinding, and constitutes a cell for the reception of the liquid. A graduated glass jar (A, B) may be suspended over either plate by means of a brass clip, proceeding from a rod placed by the side of the cell in the manner represented by fig. 2, which is a perspective drawing of a circular arrangement of ten such cells.

Fig. 3. represents the section of a cell which is adapted to the same purposes, but is less expensive in construction. It is supported in a perforated table (C, D) by its projecting rim (*v*, *w*, *y*, *z*), and the stems of the plates pass through the glass stopper (*a*, *b*, *c*, *d*) into the exterior mercury cups (*o*, *p*), by means of which all the necessary connexions may be made.

The active elements of the circuit which I adopted as standards of comparison were, for the metals, platinum and amalgamated zinc plates of the dimensions 3 inches by 1 inch; and for the electrolyte, water acidulated with sulphuric acid in the ratio of 100 parts by volume of the former to $2\frac{1}{4}$ of the latter (sp. gr. 1027·5), proportions which I adopted for the purpose of connecting some of my experiments with yours*.

This dilute acid was almost without local action upon the amalgamated zinc. When the plate was wholly immersed, its surface became, after some time, covered with bubbles of hydrogen gas, which strongly adhered to it; but after twenty-four hours it was found to have lost but a very small fraction of a grain of its weight. The force of heterogeneous adhesion, by which hydrogen and other gases are thus retained upon the surface of metals, I am disposed to regard as exerting a very important influence

* Experimental Researches, x. 1127. 1128.

Fig. 2.

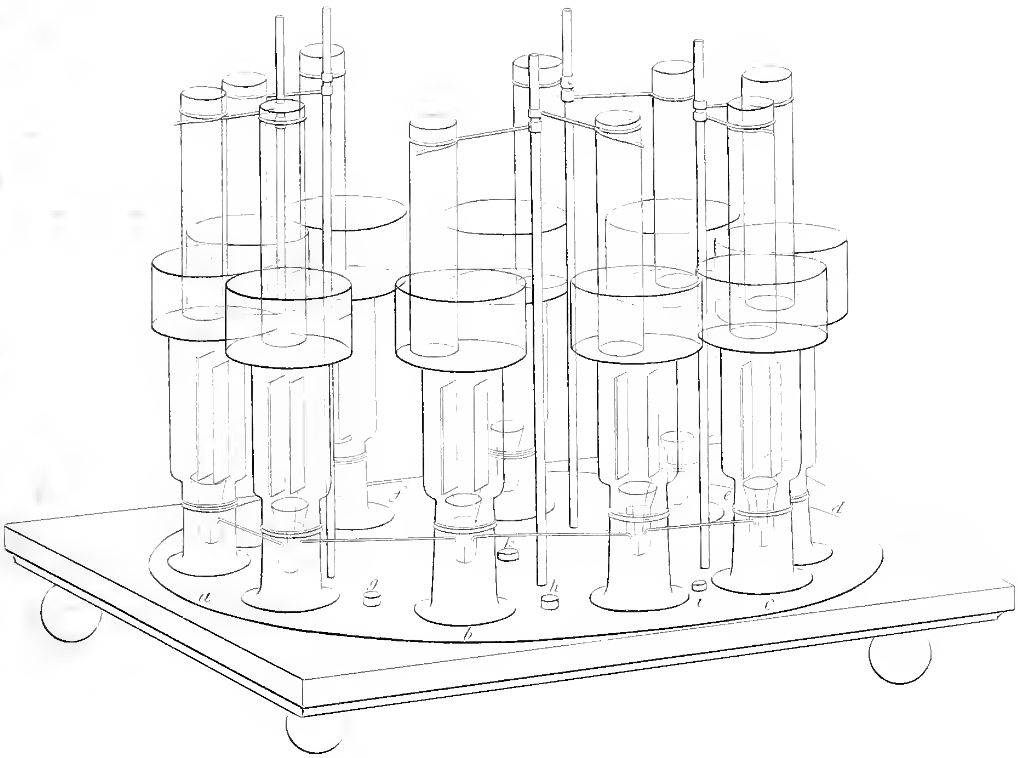


Fig. 1.

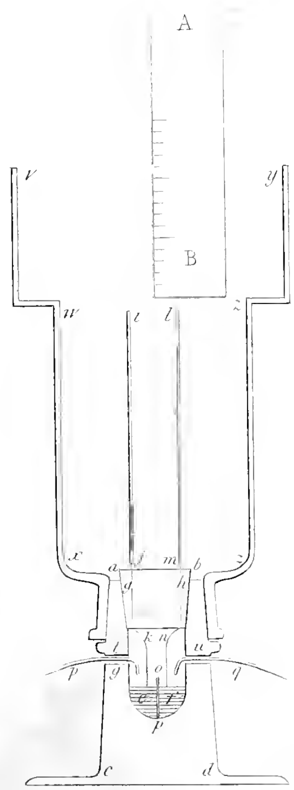
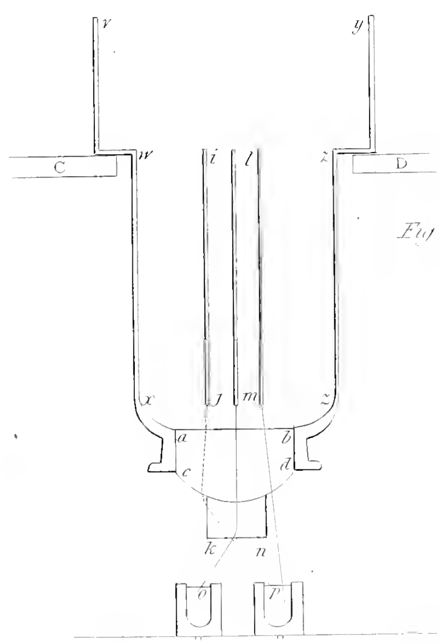


Fig. 3.



Dissected Battery.

Leopold Martin del.

both upon *local* and *current* affinity; and in the case before us I have little doubt that it is the affinity of the hydrogen thus tied down, as it were, to the surface of the plate which puts a stop to the decomposition of the water by the zinc. When a small quantity of nitric acid was added to the acidulated water, the same plate was entirely dissolved in a few hours without the extrication of any gaseous matter. It is probable that the elements of the nitric acid in this case acted by entering into combination with the hydrogen as it was evolved, and thus removing its opposing attraction. The decomposition also uninterruptedly proceeds when the hydrogen is evolved at a comparatively remote point in the voltaic circuit. The well-known energy of gases in what is called their *nascent state* may be referred to the same cause, namely, their adhesion to the surfaces against which they are evolved, by which their elasticity is counteracted; much in the same way, and by the same species of force, that the soluble gases are kept down by solution in water, and are enabled to exert their affinities for other forms of matter to advantage*.

Of the strength of the affinity of hydrogen in this state we have abundant evidence in the facility with which it deoxidates the oxide of copper, when sulphate of copper is exposed to it, and precipitates the metal upon the negative plate of a voltaic circuit. I shall also have occasion to prove to you in the course of this paper, that it is sufficient to deoxidate the oxide of zinc under similar circumstances; and we see at once the impossibility of the oxidation of the metal proceeding in the presence of an agent capable of abstracting oxygen from it when already combined.

When a metallic communication was made between the two plates of a cell in this its normal state, the evolution of hydrogen from the platinum was abundant, and at about the rate of three cubic inches in a quarter of an hour.

Before I proceed to describe the principal series of experiments which I instituted with this apparatus, it may perhaps be worth while to point out the facility with which, by its means, some of the principles of the voltaic battery may be demonstrated, which are the most elementary, but at the same time the most difficult to illustrate.

Taking one of the cells charged with water, acidulated with sulphuric acid, to which a small quantity of nitric acid has been added, its total inaction previous to the completion of the circuit is strongly contrasted with the torrents of gas which rise into the graduated jar placed over the platinum plate after the wires (*p*, *q*) are

* There is one well-known experiment which has given rise to much ingenious hypothesis, which may also receive its simplest explanation from the action of this force of heterogeneous adhesion, namely, the so-called metallization of ammonia. When mercury is made the negative electrode of a voltaic circuit in connexion with a strong solution of muriate of ammonia, hydrogen and ammoniacal gases are both evolved in contact with it: and not only does the adhesive force restrain the elasticity of the gases, but the latter also reacts upon the cohesion of the fluid metal, and causes it to expand and increase its volume in the manner which has been so often described. The results of the most careful examination of this amalgam by the first chemists are in entire conformity with this opinion.

brought into contact. If we take a second cell charged in the same way, and connect together the two zinc plates and the two platina, all is quiescent. No current can circulate, because the tendency of the generating plate in one cell to originate such a current in one direction of the circuit, is opposed by an equal tendency of the generating plate of the other to form one in the opposite direction. Substituting now, for the second cell, a third, charged with a solution of iodide of potassium and starch instead of the acidulated water, although it is easy to demonstrate that there is a similar tendency to form an opposing current, yet the current originating from the higher affinities of the first cell is sufficient to overcome that from the weaker affinities of the last cell, and iodine is abundantly precipitated upon the platinum plate of the latter, and immediately detected by the deep blue colour. Again, detaching the last cell, and connecting together its two plates in single circuit, its own current is established in the opposite direction, and the blue colour of the iodine speedily disappears from the platinum plate, under the influence of the hydrogen, which now takes its place. Thus your fundamental proof of the non-necessity of the contact of dissimilar metals for the establishment of an electric current is rendered very apparent and striking.

Recurring again to the two cells similarly charged with acidulated water, if instead of connecting them by their two platinum and their two zinc plates, we connect each platinum with the zinc plate of the other, the direction of the currents from both generating plates will coincide, and each, instead of opposing, will assist the other; and the completion of the circuit will be manifested by the evolution of gas in both the collecting jars.

I began my series of experiments by charging each cell with water acidulated with sulphuric acid alone, in the proportion which I have just stated; and upon connecting them in single circles I found that the amount of action of each, as measured by the quantity of hydrogen collected in their respective voltameters, in equal times, differed very materially, notwithstanding the apparent similitude of their circumstances. The difference between the highest and the lowest was nearly a third; and the inequality may probably be ascribed, partly to slight differences in the distances of the generating and conducting plates which it was not easy to avoid, and partly to differences in the amalgamated surfaces. Upon connecting them altogether in a single circular series, the inequalities disappeared: the amount of gas from each was equal, but was found to have fallen to that from the weakest cell.

The circular arrangement of the cells of the battery, fig. 2., admits of their being combined together in various ways with the greatest facility by means of small cups of mercury (*g*, *h*, *i*) placed at proper intervals. My next disposition was to connect all the platinum plates together by wires radiating from them to a central cup (*k*) of mercury, and all the zinc plates by wires dipping into a ring of the same metal placed in a groove (*a b c d e f*) surrounding the whole arrangement. In this state of things no action was of course manifest, for there was no complete circuit; but upon making a connexion by means of a wire between the central cup and the exterior circle of mercury, the current was enabled to circulate, and was manifested by the simultaneous evolution of gas from all the cells. The inequality of action became again apparent, and the differences between the cells was nearly the same, as when they were connected in separate single circuits.

The result of this arrangement was virtually the same as that of a single pair of platinum and zinc plates, exposing the same extent of surface as that of the ten pairs added together; and the whole quantity of force generated in each cell must have passed through the single wire which connected the centre with the circumference in the progress of its circulation.

By these two arrangements of the same elements of the battery, the relations of quantity and intensity in the circulating affinity are placed in a very striking point of view. Setting aside, as we may do in our present comparison, the inequalities to which I have just referred, the same quantity of force is generated and expended in the cells in both combinations: but when in series, no connecting wire conveys more than the quantity generated in one cell to the next; whilst, in the single circuit, the whole quantity generated in all the cells must pass through the central connecting wire. This difference of quantity may be manifested by the elevation of temperature occasioned in a fine platinum wire when made the connecting medium of the latter combination, whilst the same wire will remain cold when employed for the same purpose in the former. On the other hand, the intensity of the force, though small in quantity, derived from the repeated impulses of the disposition in series, is shown by its projecting itself in the form of a spark through a break in the conductor; whilst ten times the quantity of the same force is effectually arrested in the simple circle by the slightest disruption of the continuity of its metallic path for want of this accumulated energy.

I proceeded next to combine the cells together in pairs, two platinum plates being connected, and two zinc; and the five pairs were afterwards arranged in series by wires leading from each pair of zinc to the adjoining pair of platinum. The irregularity of action again disappeared; the amount of gas was equal in all the voltameters; slightly exceeded that from the single-series arrangement; but did not quite come up to the amount of the most energetic single cell. The arrangement was equivalent to a series of five plates of double the standard size, and the quantity which circulated was determined by the least efficient pair.

Leaving one pair of cells thus connected together, the others were again disunited, and recombined with it in single series; the effect being that of a plate of double size interposed in a compound circuit with eight single. The gas collected in each of the voltameters of the double cell was exactly half of that in the several voltameters of the single cells; proving that the double plate had been reduced in efficiency to the exact standard of the single plates by its combination with them. The regulating effect of the voltaic series, by which all irregularities of its elements are equalized, is strikingly exemplified in these experiments; and it is clearly manifest from them, as a primary law, that the circulating current must be uniform throughout its course.

In these arrangements, every cell had been a *generating* cell, and added something either to the quantity or intensity of the circulating force: I now proceeded to ascertain the effects of various *retarding* cells upon the elements of the battery. For this purpose I connected the cells together in single series, substituting a platinum plate for one of the zinc; and the obstacle reacted upon the whole series: the action was reduced by more than one third, and the quantities of gas collected from each generating cell was exactly equal to the hydrogen collected from the retarding cell. Repeating the experiment with a similar change in the next cell, the quantity of hydrogen in all the voltameters was equal, but reduced to little more than one tenth, and the current was entirely stopped by three retarding cells to seven generating cells.

When, instead of changing the zinc plate of one of the cells, I reversed its position in the series by turning the latter round, the more active opposition of the tendency in that cell to establish a contrary current was indicated by the decline of the quantity of gas in the nine voltameters of the regular cells to about one fourth; while it is remarkable that the quantity of hydrogen collected from the reversed zinc plate was considerably less. In several repetitions of this experiment this deficiency always occurred; and I also ascertained that the quantity of oxygen evolved from the corresponding platinum was the equivalent of the lesser quantity, and not of that which was evolved from the regular cells.

When one of the zinc plates was removed from the regular series, and replaced with a platinum plate which had been previously coated, by voltaic influence, with metallic copper, the phenomena were striking and instructive. No gas at first was evolved from the coppered plate, but it became slowly oxidated, and the progress of the oxidation could be traced by the gradual blackening of its surface. The oxide again was gradually dissolved, and the bright white surface of the platinum made its appearance, and oxygen gas began to be disengaged. At that moment the current received a check, which was quite appreciable by the voltameters.

As the progress of my experiments would require the use of an independent voltameter, I thought it desirable previously to determine its retarding power by comparison with that of a single retarding cell; and for this purpose I substituted one of your upright construction for one of the cells of the battery in single series: and I found, that notwithstanding its plates were only three inches long by six eighths broad, their nearer approximation counterbalanced their deficiency of surface; and the quantity of hydrogen which the instrument indicated was exactly equal to that in the voltameters of the retarding cell. The measure of the effects of different degrees of approximation in the plates of voltaic combinations was one of the objects of my experiments; but this I have for the present postponed, for reasons which will soon be apparent.

The next variation of the standard elements of the battery was to place conducting plates in connexion with one another on each side of the generating plates. This was easily effected by making both the plates, *ij*, *lm*, (fig. 3.) of platinum communicate with one mercury cup (p); while the amalgamated zinc plate placed between them communicated with the other cup (o). This arrangement was perfectly analogous in principle to the double or Wollaston plates of the common battery. Upon combining the cells thus arranged in single series, nearly as much gas was collected from each plate, on the opposite sides of the generating plates, as had been collected in an equal time from the single plates; the double surface of the platinum having enabled the single zinc plate to decompose very nearly a double portion of water.

During this experiment, and its frequent repetitions, I remarked that the bubbles of hydrogen arose not only from the surfaces of the platinum which were immediately opposed to the zinc, but from the opposite surfaces likewise; and when a little sulphate of copper was added to the liquid in the cells, both faces of the plates became coated with the reduced metal.

Almost the only serious difficulty with regard to your chemical theory of voltaic action which ever occurred to me, has arisen from the well-known case of voltaic protection, in which a small piece of zinc or iron has been found to defend from corrosion a surface of copper so many hundred times its own superficial dimensions. I was unable for a long time to understand how the hydrogen, which could only be the equivalent of the oxygen taken up by the former metals, became spread over so disproportionate a surface; and this effect seemed to me more intelligible upon the hypothesis of some condition of the copper, analogous to those produced by electrical or magnetic induction, than upon any known chemical principles. The following means of determining the question by experiment now occurred to me.

I took a silver plate, fifteen inches square, and, placing it in a shallow trough, covered it with the dilute sulphuric acid, to which a portion of sulphate of copper had been added. I then supported an amalgamated zinc wire of about one eighth of an inch in diameter, so as to allow one of its ends just to rest upon the centre of the plate. The instant the two metals came in contact, a circular spot of metallic copper was thrown down upon the silver, and rapidly spread itself in such a way that in a few hours it formed a well-defined circle of six inches in diameter. Particles of copper could be traced even beyond this; but none appeared to have reached the edges of the plate.

I varied this experiment by soldering to the centre of a silver plate, 8 inches by $5\frac{1}{2}$ inches, a small piece of amalgamated zinc $\frac{7}{8}$ths of an inch long by $\frac{3}{16}$ths wide, and placed it perpendicularly in a jar, and covered it with the solution of copper in acidulated water. The copper immediately began to precipitate itself upon the silver, in the form of an oval surrounding the zinc, and gradually extended itself equally on all sides. There was no extension of the copper on the upper side, to indicate that the hydrogen, the reducing agent, had been carried upwards by its levity; but it spread itself downwards and laterally at the same rate, till in a few hours it reached the edge of the plate. It then made its appearance upon the opposite surface, and ultimately both sides were completely coated; but the deposition decreased in thickness as it receded from the central zinc. This effect, in a voltaic combination, of a small generating surface upon a large conducting one, is strikingly contrasted with the result when we reverse the circumstances of the arrangement; for by causing a rod of platinum to rest by its end upon a large surface of amalgamated zinc, covered with the acidulated water, hydrogen of course escapes from the former metal; but the oxidation of the latter is so local, that a hole is eaten through it at the point of contact.

These experiments set the question at rest which they were meant to resolve; and I think, upon attentively considering the matter, that we may find the cause of this great diffusion of the hydrogen in that force of *heterogeneous adhesion* to which I have already referred the power of nascent gases, reacting upon, or acting in conjunction with, its elastic or self-repulsive force. Under the influence of these two forces the gaseous matter seems to extend itself upon the metal, much in the same way that a drop of volatile oil spreads itself over a large surface of water; and it seems not improbable that the efficiency of the conducting plates of voltaic arrangements may in part, if not wholly, depend upon the extent of surface which they afford for this diffusion of the gas, by which it is carried beyond the sphere of reaction upon the generating plates. To test the accuracy of this hypothesis, I reduced the width of the zinc plates in the battery; and beginning by halving, and then diminishing them to a quarter of their original dimensions, I ultimately found that amalgamated wires of one eighth of an inch diameter and three inches long, were as efficacious as the inch-wide plates; and that under their influence the conducting plates evolved as much gas in equal portions of time as when connected with the larger generating surfaces.

Amongst the numerous experiments which I have tried by the substitution of different metals, both as generating and conducting plates, for the normal plates of the battery, I must only detain you on the present occasion with the results of two. The first was the change of common zinc plates for the amalgamated, when strong local action took place, which, however, did not appear to interfere with the current affinity when the cells were connected in single series; for while the disengagement of hydrogen from the zinc was at the highest, as much gas was evolved from the platinum as when they were combined with the amalgamated metal.

The second was the substitution of wrought iron for the amalgamated zinc plates. I had expected, from theoretical speculations upon its low equivalent number, that iron would have proved a very efficient generating metal; and my surprise was great upon discovering that its action in the battery was almost null. A slight local action took place upon the plates themselves, but the evolution of gas from the platinum was scarcely apparent, and quite immeasurable. This being the case, I converted them into conducting plates by removing the platinum, and opposing to them amalgamated zinc plates; and I was no less surprised to find that in their new office they greatly exceeded in efficiency the platinum plates themselves; the quantity of gas collected in the voltameters in series being nearly double in amount to that from the battery in its normal state.

I repeated this experiment with fresh iron plates, and ascertained that previous opposition to the platinum had greatly contributed to this energetic action of the first set: the disengagement of gas from the second but little exceeded that from the normal plates.

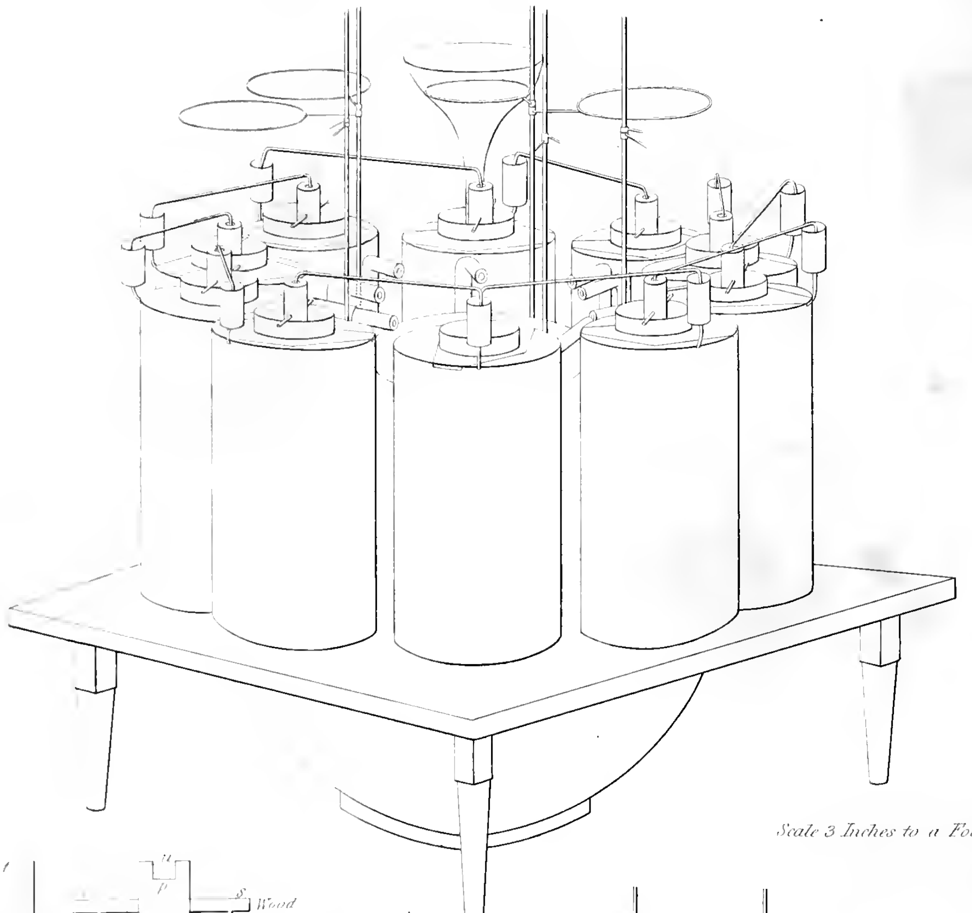
I am inclined to believe that we may again find the explanation of these phenomena in the attraction of heterogeneous adhesion; and I think it probable that the hydrogen gas adhering with less force to the surface of the iron than to that of platinum, is more readily carried out of the sphere of action when evolved in the process of circulation upon the former than upon the latter. The efficacy derived from the previous combination of the iron with platinum may be ascribed to the cleansing of its surface, or possibly to some difference of mechanical structure developed in this particular position.

My attention was next turned to the effects of certain changes in the condition of the electrolyte employed in the excitation of the battery; and now it was that I began to experience those, apparently capricious, changes in the force of the current which soon obliged me to give up all hope of rendering the results comparable with one another in the present form of its construction. When I added to the acidulated water a volume of nitric acid equal to that of the sulphuric acid employed, the quantity of hydrogen evolved from the conducting plates of the cells was greatly diminished, and was irregular; but when the voltameter was substituted for one of the cells, the quantity of hydrogen which it indicated was at first nearly treble that from the battery in its normal state. This action, however, speedily declined by a quantity which was quite appreciable at intervals of five minutes. By breaking the connexion of the circuit this energy was partially recovered, and that even during the short cessation of action which arose from the mere turning of the voltameter. When the charge of nitric acid was increased, the evolution of hydrogen from some of the cells was wholly stopped, whilst in others it continued to a small but unequal amount; the quantity of the circulating current was at the same time increased, but rapidly declined as the action continued.

My efforts were now turned to discover the cause of this perplexing variation; but I shall not trouble you with the numberless experiments which I made upon the subject, any further than may be necessary to elucidate the steps by which I was ultimately led to its detection. I allowed the battery to exhaust itself by leaving it in connexion for thirty hours, at the expiration of which time scarcely any gas was evolved in the voltameter. I then removed the platinum plates and substituted fresh plates of the same metal; and when the connexions were made, the action was renewed with nearly the same activity as at the first; the falling off, however, was rather more rapid than before. Even iron plates when substituted for the exhausted platinum renewed the action with great effect.

The decline thus seemed to depend upon some state taken up by the conducting plates, and this I endeavoured to remove in various ways. I polished them with rotten stone; I heated them red hot; and I boiled them in a strong solution of potassa, but without any decided effect. Boiling them, however, in nitric or muriatic acids completely restored them. Thus, frequently renewing the plates but retaining the old charge of acidulated water, the first action was always very brisk, but lasted for shorter and shorter periods, till the decline at last became almost immediate. Upon examining the plates after a long-protracted action of this kind, I found a roughness upon their edges and faces which conveyed the idea of a corrosion of the platinum. A more attentive investigation proved this to arise from a deposition upon them of metallic zinc; and upon leaving the battery in connexion for eighteen hours the coating increased to such a degree that it could be detached in large flakes. The precipitated metal was deposited chiefly upon the surface opposed to the zinc plate, but in some had extended considerably on the opposite side. It had mostly a beautiful mammillated appearance; but in one or two instances a crystalline structure was visible with a lens. I ascertained the weight of zinc thus deposited upon a plate by removing it with an acid, and found it 27·86 grains. The metal was pure, and when detached did not dissolve in the dilute acid till touched with a piece of platinum, when abundance of hydrogen gas was immediately given off from the latter. A similar deposition, in equal abundance, was formed upon iron when the conducting-plates were made of that metal. It originated doubtless from the oxide of zinc formed by the action of the battery at the generating-plate, and reduced by the *nascent* hydrogen on the conducting-plate; and its varying quantity and accumulation was amply sufficient to account for the variations and ultimate annihilation of the circulating force. In some instances, indeed, the casing of the platinum was so entire as to have the complete effect of one zinc surface opposed to another. This local deoxidating power of the hydrogen is exerted upon every oxide within its reach, and is more or less injurious to the action of the battery according as the resulting deposition upon the plates is more or less capable of generating a counter current by its reoxidation. The most injurious of all precipitations must, of course, be that of zinc upon the conducting-plate; and it must take place with a rapidity proportioned to the quantity of oxide held in solution by the electrolyte. In the common construction of the voltaic battery the solution of the metal is very rapid, on account of the local as well as the current action, and the deoxidating action must very soon be established; and the reason of the incrustation of zinc never having been remarked, is doubtless the extreme facility with which it is redissolved upon breaking the circuit. By its close contact with the copper it forms simple voltaic circles, and is under circumstances the most favourable for its removal. The momentary recovery of the strength of the

Fig. 2.



Scale 3 Inches to a Foot.

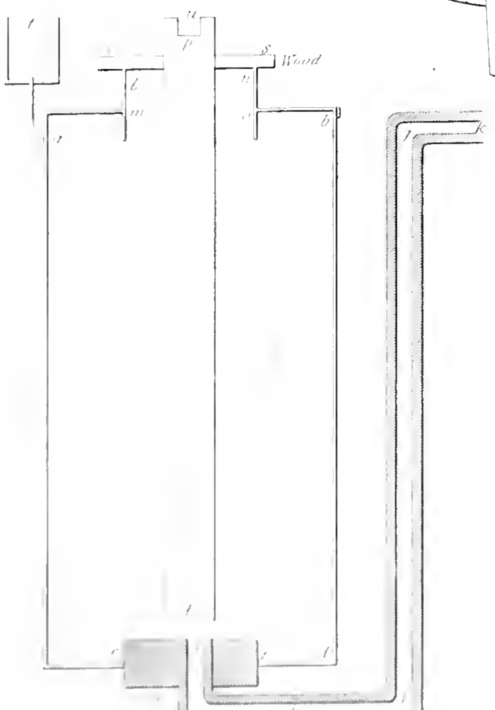
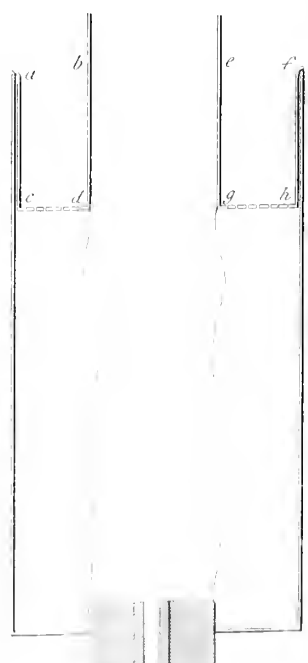


Fig. 3.



battery by breaking the metallic contact is now perfectly intelligible; and we can no longer be at a loss to understand all the phenomena of Ritter's secondary piles, the effects ascribed to polarity by M. Becquerel, and the experiments of Marianini and A. De la Rive upon this interesting subject.

I have remarked in the dissected battery, that when any oxide of copper was present in the solution, it was thrown down in preference to the oxide of zinc; and that plates which had become covered with the former metal were, in some degree, protected from depositions of the latter. Upon adding, however, sulphate of copper in any considerable quantity to the liquid in the cells, notwithstanding the amalgamation of the zinc, there was local action enough upon that metal to disengage hydrogen; which, in however small a quantity, was sufficient to commence the precipitation of the copper upon it. Single circles were thus immediately formed by the two metals, and local action increased to such a degree as speedily to cover the zinc with reduced copper. The addition of nitrate of mercury is not liable to the same objection; and when I made use of this salt in the cells, the platinum plates soon became covered with metallic mercury in such quantities as ultimately to fall from them in drops. I did not carry this experiment far enough to ascertain whether the oxide of zinc would ultimately have become reduced with the oxide of mercury: but on one occasion, when some oxide of copper was present, the platinum plate became incrusted with a beautiful white soft amalgam; which upon analysis proved to be an amalgam of copper.

Notwithstanding the unfavourable effect upon the circulating force of this secondary deoxidating power of the *nascent* hydrogen, where the ultimate result is the deposition of a solid body having a strong affinity for the oxygen of the electrolyte, there can be no doubt that the removal of the hydrogen by combinations which do not give rise to such precipitation greatly promotes the efficiency of the current: and I have had abundant opportunities of confirming your explanation of the good effects of the addition of nitric acid to the battery-charge by its abstraction of the hydrogen*. It is, moreover, quite certain, that not only does the oxygen of the acid act in this way as a *dehydrogenating* agent, but the nitrogen also; for by neutralizing the exhausted charge of one of the cells of the battery with carbonate of soda, I not only obtained abundance of ammonia by distillation with lime, but a copious precipitate of ammonio-muriate of platinum with a solution of the chloride of that metal.

Having thus detailed, I hope sufficiently, the progressive steps by which I was led to the discovery of the cause of the variations and decline of the force of the voltaic battery, as well as of certain principles upon the application of which I have relied for the counteraction of its injurious effects, I will not detain you with any account of the less successful attempts which I made for that purpose, but proceed at once to describe a voltaic combination which I trust you will think worthy of the name of the Constant Battery.

Fig. 1. of Plate IX. represents a section of one of the cells, ten of which are shown

* Experimental Researches (1021.).

in connexion at fig. 2.: (*a b c d*) is a cylinder of copper six inches high and three and a half inches wide; it is open at the top (*a b*) but closed at the bottom, except a collar (*e f*) one and a half inch wide, intended for the reception of a cork into which a glass siphon-tube (*g h i j k*) is fitted. On the top (*a b*) a copper collar corresponding with the one at the bottom rests by two horizontal arms. Previously to the fixing of the cork siphon-tube in its place, a membranous tube formed of a part of the gullet of an ox is drawn through the lower collar (*e f*) and fastened with twine to the upper (*l m n o*); and when tightly fixed by the cork below, forming an internal cavity to the cell communicating with the siphon-tube in such a way as that when filled with any liquid to the level (*m o*) any addition causes it to flow out at the aperture (*k*). In this state, for any number of drops allowed to fall into the top of the cavity, an equal number are discharged from the bottom. (*p q*) is a rod of cast zinc amalgamated with mercury, six inches long and half an inch diameter, supported on the rim of the upper collar by a stick of wood (*r s*) passing through a hole drilled in its upper extremity: (*t*) is a small cup for the reception of mercury, by which, and the cavity (*u*) at the top of the zinc rod, various connexions of the copper and zinc of the different cells may be made by means of wires proceeding from one to the other.

In fig. 2. the ten cells are represented as connected in single series, the zinc of one with the copper of the next. They stand upon a small table in a circle with the apertures of the siphon-tubes turned inwards, surrounding a large funnel communicating with the basin underneath for the reception of any liquid which may overflow. A smaller funnel is supported over the internal cavity of each cell by a ring sliding upon rods of brass placed between each pair of cells. One of these only is shown in the drawing to avoid the crowding of the sketch.

In the construction of this battery, I have availed myself of the power of reducing the surface of the generating plates to a *minimum*, the effective surface of one of the amalgamated zinc rods being less than ten square inches, whilst the internal surface of the copper cylinder to which it is opposed is nearly 72 square inches. My principal objects have been, to remove out of the circuit the oxide of zinc, which has been proved to be so injurious to the action of the common battery, as fast as its solution is formed, and to absorb the hydrogen evolved upon the copper without the precipitation of any substance which might deteriorate the latter.

The first is completely effected by the suspension of the rod in the interior membranous cell, into which fresh acidulated water is allowed slowly to drop from the funnel suspended over it, and the aperture of which is adjusted for the purpose; whilst the heavier solution of the oxide is withdrawn from the bottom at an equal rate by the siphon-tube. When both the exterior and interior cavities of the cell were charged with the same diluted acid, and connexion made between the zinc and the copper by means of a fine platinum wire $\frac{1}{200}$th of an inch in diameter, I found that the wire became red hot, and that the wet membrane presented no obstruction to the passage of the current.

The second object is attained by charging the exterior space surrounding the membrane with a saturated solution of sulphate of copper instead of diluted acid: upon completing the circuit the current passed freely through this solution; no hydrogen made its appearance upon the conducting plate, but a beautiful pink coating of pure copper was precipitated upon it, and thus perpetually renewed its surface.

When the whole battery was properly arranged and charged in this manner, no evolution of gas took place from the generating or conducting plates, either before or after the connexions were complete; but when a voltameter was included in the circuit, its action was found to be very energetic. It was also much more steady and permanent than that of the ordinary battery; but still there was a gradual, but very slow, decline, which I traced at length to the weakening of the saline solution by the precipitation of the copper, and the consequent decline of its conducting power.

To obviate this defect, I suspended some solid sulphate of copper in small muslin bags which just dipped below the surface of the solution in the cylinders; which gradually dissolving as the precipitation proceeded, kept it in a state of saturation. This expedient fully answered the purpose, and I found the current perfectly steady for six hours together. This arrangement I have since improved by placing the salt in a perforated colander of copper fixed to the upper collar.

Fig. 3. represents a section of this additional arrangement. (*a c f h*) is the colander with its central collar (*b d e g*), which rests by a small ledge upon the rim of the cylinder. The membrane is drawn through the collar, and turning over its edge is then fastened with twine.

After this alteration, the effective length of the zinc rods exposed to the action of the acid was found to be no more than four inches and a quarter. In ascertaining the powers of the battery in single series, the voltameter was the same that I have already described; the specific gravity of the solution of sulphate of copper was found to be 1198·5, and I commenced my experiments with the standard sulphuric acid, specific gravity 1027·3.

With this charge, after the circuit had been completed for ten minutes, the mean quantity of mixed gases taken at intervals of five minutes for two hours was 2·1 cubic inches, the results never varying more than 0·1 cubic inch from one another. The battery was then left in connexion, without the voltameter, for two hours, and again examined for three intervals of five minutes, when the mean quantity of gases was found as before. It was again left in connexion for two hours, and re-examined with the same result.

Upon adding nitric acid to the solution of sulphate of copper, I found that an injurious effect was produced; and that the mean quantity of gas in five minutes was lowered to 1·1 cubic inch: at this rate of action the battery, however, remained steady for six hours.

Returning to the original solution of sulphate of copper for the exterior cavity of the cells, I doubled the strength of the dilute acid for the interior; adding, for this purpose, $4\frac{1}{2}$ measures of oil of vitriol to 100 measures of water, sp. gr. 1056·2, and found the mean quantity of gas evolved in the voltameter increased to 3·8 cubic inches per five minutes. I then removed the meter, and connected the circuit, leaving the battery in action for four hours. Upon replacing the meter it was ascertained to be still working at the same rate. The addition of nitric acid to the solution of the sulphate again reduced the rate to 2·1 cubic inches.

I now made trial of an addition of nitric acid to the interior liquid of the cell, adding an equal volume to that of the sulphuric acid, and restoring the solution of the neutral sulphate to the exterior division. At the first impression, an increased effect seemed to be produced, and the action for the first quarter of an hour was as high as 4·2 cubic inches per five minutes; but it ultimately settled down, and remained at the former amount without the nitric acid, of 3·8 cubic inches.

Here I may remark, that at the first immersion of the rods the effect of the battery is always a little higher than it afterwards settles at: and this I found to depend upon the evolution of a small quantity of hydrogen from a residue of local action upon the zinc, which adhered to its surface, and slightly impeded the action of the acid. These bubbles scarcely ever rose to the surface, and after the first quarter of an hour the battery took up a steady rate of action. By gently agitating the rods in the acid, the action would momentarily return to its original amount, but always settled down to the lower rate.

How very small this local action is will be seen from the details of the following series of experiments, which I shall give at length. Upon this occasion I weighed the ten zinc rods, which were the same which I had used from the first, and were reduced to nearly half their original circumference, that I might judge how nearly the zinc oxidated would be equivalent to the volume of gases evolved: their weight was $2\frac{1}{4}$ lbs. and 13 grains (15,763 grains). I trebled the strength of the acid, adding $6\frac{3}{4}$ measures of oil of vitriol to 100 measures of water, sp. gr. 1079·4. I sometimes made use of a large voltameter, the plates of which were 3 inches by 1 inch, and which, having a bent tube, admitted of my receiving the gases into a large graduated jar, and sometimes of the same small meter which I had hitherto employed. The results are contained in the following Table.

Time.	Quantity of Gas.	Rate per quarter of an Hour.	Rate per five Minutes.
Large Voltameter.			
h m	cubic inches.	cubic inches.	cubic inches.
11 24			
11 39	16	16	5·3
11 45			
11 60	30	14	4·6
12 3			
12 18	44	14	4·6
12 19			
12 34	58	14	4·6
12 49	71	13	4·3
Small Voltameter.			
12 51			
12 56	......	......	4·6
12 58			
1 3	......	......	4·6
Hydrogen removed by agitating the Rods.			
1 6			
1 11	......	......	4·9
Large Voltameter.			
1 14			
1 29	85	14	4·6
1 31			
2 1	27	13·5	4·5
2 31	54	13·5	4·5
3 1	82	14	4·6
Small Voltameter.			
3 2			
3 7	......	......	4·8

Thus the total quantity of mixed gases collected amounted to 186 cubic inches, which, being corrected for pressure, were equal to 188·48. The zinc rods were removed from the cells, rinsed in water, and carefully dried, when they were found to have lost 933 grains.

Now adopting the data for computation, as laid down in your Tenth Series (1126.), of 100 cubic inches of the mixed gases being equivalent to 12·68 grains of water, and taking the equivalent number of water as 9, and that of zinc as 32·5, we have the following proportions:

Mixed Gases.		Water.				
cub. inch.		grs.		cub. inch.		grs.
100	:	12·68	::	188	:	23·84,

and

Equiv. Water.		Equiv. Zinc.				
				grs.		grs.
9	:	32·5	::	23·84	:	86·1;

showing the quantity of zinc equivalent to 188 cubic inches of the mixed gases to be 86·1 grains; differing from the quantity actually consumed in each cell only 7·2 grains, or 2·3 for each equivalent. The waste of metal, indeed, must have been even smaller than this; for at the bottom of the cells I found a small quantity of amalgam, which had fallen off the rods, and which, at the time, it was not convenient to collect, but which, if added to the weight, would have materially diminished the deficiency.

For this quantity of zinc, 166·5 grains of metallic copper must have been precipitated on each copper cylinder; for not a bubble of hydrogen made its appearance upon them, and 6623 grains of crystallized sulphate of copper must have been consumed in the whole battery, or only 377 grains short of a pound avoirdupois.

Upon examining the cells after the course of experiments, the fresh-precipitated copper had a most beautiful appearance, being of a bright pink colour. It was not only deposited upon the sides and bottom of the cylinders, but upon the under surface also of the colanders. In the angle formed by the junction of the bottoms with the sides and at the contact of the membrane with the collars of the cullenders, it was collected in the largest quantities, and had a very distinct mammillated structure. At these points it is probable that the diffusion of the hydrogen had been impeded, and it had consequently accumulated in greatest abundance.

The charge of sulphate of copper has been left for weeks together in the battery, only taking care to keep the bottom of the colanders covered with the solid salt; but the acidulated water was withdrawn from the interior tubes every morning by means of a siphon, and a fresh charge substituted. It was generally found very slightly tinged with the sulphate, which would have been injurious to the zinc rods.

The only disadvantage that I am aware of in this new construction of the battery, is the unavoidable distance of the generating from the conducting metal, which is five or six times greater than in the double Wollaston plates; and this I sought to obviate as much as possible by improving the conducting power of the interposed electrolyte. I made trial of sulphuric acid of four times the standard strength, or 9 volumes of oil of vitriol to 100 of water, sp. gr. 1105·4; and this I worked for six hours continuously, and found the result very steady; 5·0 cubic inches with the large voltameter, and 5·5 by the smaller, per five minutes of time.

Taking into consideration the great precipitation of the sulphate of copper, I am doubtful whether the solution could be kept in a state of saturation at much beyond this rate of work; and in cases where it may be desirable to maintain a constant action for a great length of time, it may not be desirable to carry it even so far; but this, with many other questions, I must leave for future examination.

Being now desirous to bring the *constant battery* into more immediate comparison with one of the usual construction, I took a trough with ten Wollaston plates four inches square, which had been but little used, and carefully cleaned and fitted a double case of copper to the last zinc, to make the series complete. I charged it with a mixture in the proportions of 100 water, $2\frac{1}{4}$ oil of vitriol, and 2 nitric acid, and set it to work with the small voltameter. The following Table contains the results measured at intervals of five minutes.

Time. h m	Quantity of Gas. cubic inches.	Rate per 5 min. cubic inches.	Time. h m	Quantity of Gas. cubic inches.	Rate per 5 min. cubic inches.
10 22			11 24	1·3	1·3
10 27	2·5	2·5	11 29	2·2	0·9
10 32	4·6	2·1	11 34	3·0	0·8
10 34	Voltameter refilled.		11 39	3·55	0·55
10 39	2·6	2·6	11 44	4·1	0·55
10 44	4·4	1·8	11 49	4·5	0·4
10 46	Voltameter refilled.		11 54	4·9	0·4
10 51	2·8	2·8	11 59	5·25	0·35
10 56	4·9	2·1	12 0	Voltameter refilled.	
10 57	Voltameter refilled.		12 15	0·9	0·3
11 2	2·4	2·4	12 30	1·7	0·26
11 7	4·2	1·8	12 45	2·25	0·26
11 8	Voltameter refilled.		1 0	2·75	0·25
11 13	1·8	1·8	1 58	Battery left in connexion.	
11 18	3·2	1·4	2 13	0·1	0·03
11 19	Voltameter refilled.				

This series of observations displays in a striking manner the peculiar irregularities of the common voltaic battery, and entirely agrees with those which I had previously made with the dissected battery; the energy of the first contact declining even in the first five minutes a fourth or a third; recovering itself again by one minute's rest, and again declining: even with five such intervals of interrupted action, falling off permanently one half in the first hour; in the next hour of unceasing action rapidly falling to one tenth; and after four hours' connexion almost entirely ceasing.

If we compare the surfaces of the generating and conducting plates in each cell of the two batteries, we shall find that of the zinc in the Wollaston battery 32 square inches, whilst in the constant battery it does not exceed $7\frac{1}{2}$ inches. If we calculate only the interior surface of the copper of the former, that is also 32 inches; but if both surfaces be efficient, (as I believe, though not to the same amount,) then it amounts to 64 inches. It can only, of course, be the interior surface of the copper cylinders of the *constant battery* which assists the action; and reckoning $5\frac{1}{2}$ inches in depth to be efficient, it amounts to $65\frac{1}{2}$ inches; to which if we add a small quantity for the bottom and the underside of the colander, the amount of surface but little exeeeds the former; but then it is all disposed to the greatest advantage. Under these circumstances the power of the constant battery is double that of the common battery at its first impulse, and can be maintained for any length of time in an invariable condition.

I shall trouble you, on the present occasion, with but one comparison more; and that is, of the efficiency of the two batteries under a retarding force. The charge which I used for the *constant battery* was only the double acid (sp. gr. 1056); but nevertheless I found that, with the opposition of three voltameters, the amount of gases in each was 0·9 cubic inch per five minutes; whilst a fresh set of Wollaston plates, with a new charge in the same proportions as before, gave only 0·32 cubic inch in the first five minutes: so that the superiority of the former, under these circumstances, was even greater than before.

I will only add, that I have kept six inches of platinum wire, $\frac{1}{200}$th of an inch diameter, permanently red hot for a considerable length of time when the battery was merely connected in single series, and that the spark between charcoal-points is remarkably beautiful; and shall reserve for a future communication, if you will permit it, some observations upon the different modes of combining the plates, for which it affords great facilities, and the different important applications to which, from the invariableness of its action, it is applicable.

My principal object in these researches has been the attainment of this constancy; but, in addition, this new combination will be found, I think, to possess advantages which will secure to it a more general application than I at first contemplated.

First, the abolition of all local action by the facility of applying amalgamated zinc:

Second, the trifling expense of replacing, the zinc rods when worn out (for they are easily cast and fitted in the laboratory); and the total absence of any wear of the copper:

Third, the non-necessity of employing nitric acid, and the substitution of the cheaper materials, sulphate of copper and oil of vitriol; to which I may add, the absence of any annoying fumes:

And, fourth, the facility and perfection with which all metallic communications may be made, and different combinations of the plates arranged.

Hoping that I may not have wearied you by these details,

I remain, dear FARADAY,

Your very faithful friend,

J. F. DANIELL.

King's College,
23rd January, 1836.

XI. *Additional Observations on Voltaic Combinations. In a Letter addressed to* Michael Faraday, *D.C.L. F.R.S., Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal & Imp. Acadd. of Science, Paris, Petersburgh, &c. By* J. Frederic Daniell, *F.R.S., Prof. Chem. in King's College, London.*

Received April 14,—Read April 21, 1836.

My dear Faraday,

THE Council of the Royal Society having done me the honour to order the publication of my observations upon "Voltaic Combinations" in the Philosophical Transactions, I should wish to add the results of some further researches, which may render the account of the *constant voltaic battery* more complete and practically useful.

My great object in this combination was to obtain an invariable current of force sufficient to effect chemical decompositions, after overcoming the resistance necessary to register its quantity by the voltameter; and having succeeded in this, it seemed to me almost a matter of indifference to the solution of the various important questions to which it might be applied, whether the quantity were large or small. I quickly, however, discovered that the battery might be rendered not only perfectly steady in action but very powerful; and that it would be extremely efficient and convenient for all the purposes to which the common voltaic battery is usually applied. I set myself therefore to perfect its construction with this view.

Before I state the results I wish to direct your attention to some observations which affect the construction and application of your voltameter, being convinced that anything which may tend to facilitate the use or establish the correctness of that most important instrument will prove of substantial benefit to electro-chemical science. In my previous communication (p. 112) I stated that I had found that the plates of a voltameter only six eighths of an inch wide evolved the same amount of gases as two platinum plates one inch wide, and of the same height, in one of the cells of the dissected battery, when the former was substituted in the circuit for the latter; and I imagined that "their nearer approximation had counterbalanced their deficiency of surface." The results of the experiments recorded in the Table (p. 121), also show that there was no difference in the indications of the same small meter and those of a larger, the plates of which were three inches by one inch when they were alternately used. The plates of the larger instrument were moveable, and admitted of adjustment to different distances from each other; and by these means I ascertained that no alteration, within the distance of the generating and conducting plates of the battery, produced any difference in the results.

This rendered the non-influence of variation of surface still more remarkable; and wishing to push the observation to the utmost, I reduced the plates of a voltameter to the width of one eighth of an inch, retaining the same height of three inches, and still found its efficacy unimpaired. I next thrust two platinum wires one tenth of an inch diameter through a cork closing the mouth of a glass tube, exposing about two inches of each to the dilute acid without any diminution of effect, the battery generating in all these experiments at the rate of 2·7 cubic inches per five minutes.

When I even covered the wires with a resinous cement so as to have only one fourth of an inch of each exposed, the gases evolved in the same time amounted to 2·3 cubic inches. I ultimately coated the wires entirely with cement, and carefully bared their mere points with a file, when they still gave off the gases at the rate of 0·8 cubic inch per five minutes; and in this case the currents of the gases instead of rising at once from the points of the horizontal wires seemed to be projected forward into the liquid with some force. This independence of the results upon the metallic surfaces of the voltameter is curiously contrasted with the paramount influence of the surface of the conducting plates of the cells of the battery, but is probably owing to the absence of any active chemical affinity assisting or retarding the main current which circulates.

In the prosecution of my experiments I now began to perceive that it was by no means necessary to attend so closely to the supply of fresh acid to the battery as at first seemed advisable; and I ascertained that after an uninterrupted action of five hours, without its renewal, the voltameter only indicated a decline from 2·7 to 2·4. It being of great consequence to note to what extent the constancy of the action might be independent of the exact adjustment of the acid, I left the battery in connexion for twenty-four hours, at the expiration of which time 0·9 cubic inch of the mixed gases was collected in the voltameter in a quarter of an hour, or 0·3 cubic inch per five minutes. The acid was found to be almost perfectly saturated; a very small drop of dilute ammonia occasioning an instantaneous precipitation of oxide of zinc in the solution, which had a specific gravity of 1·276. In this state three quarters of a fluid ounce of fresh dilute acid was poured upon the top of the solution in each cell (the total charge of each cell being about 5½ fluid ounces), when the action not only rose to its original amount of 2·7 cubic inches, but to 4·2 cubic inches, at which rate of work it kept perfectly steady without any further renewal of the acid for four hours.

This increase of action I could only refer to the superior conducting power of the solution of sulphate of zinc; and two important points were thus indicated: 1st, that the conducting power of the electrolyte in the battery might be increased with great advantage; and 2ndly, that the quantity of the circulating force was still more independent of the surface of the generating metal than my experiments had yet shown it to be. The length of the zinc rods exposed to the action of the fresh acid, which must have floated upon the saline solution in this experiment, could not much have exceeded one inch, and yet they proved perfectly efficient. I immediately confirmed this conclusion by shortening the zinc rods to one fourth of their original length, and found no diminution of the power of the battery when charged with fresh acid.

I next charged the battery with acid of the same strength as that which I employed in the voltameters, viz., eight parts of water to one part of oil of vitriol by measure (specific gravity 1136), and obtained at once a steady action of 11· cubic inches per five minutes; this was therefore the mixture which I employed in all my subsequent experiments.

This rate of action must obviously require some attention to the state of the solutions in the battery, when perfect steadiness is required to be maintained for a long period; as nearly 25 grains of oxide of zinc are formed, and 154 grains of sulphate of copper decomposed in each cell per hour. Nevertheless it will remain constant for an hour and a half even without any change of acid; and the addition every now and then of one fluid ounce of fresh acid will maintain the action for any desired time, provided the colanders be kept well supplied with the salt of copper.

Considering the great advantage which had been derived from the extension of the surface of the conducting metal of the battery, I now wished to ascertain whether this might be carried further; and for this purpose I caused some cells to be fitted up in the interior with ten small plates, one inch wide, extending from the bottom to the colander, and converging from the interior circumference towards the centre. In this way they just reached to the membranous tube, and the surface of the copper was more than trebled. I thought it possible that advantage might be thus derived, not only from the great extension of surface, but from the approximation of a part of the conducting plate to the generating rod.

My first experiment was made with five plain cells and five ribbed, working into separate voltameters, and I found the products equal, inclining, if anything, in favour of the former. The amount of gases from each was, however, only 5·5 cubic inches per five minutes, or one half of that from the whole series of ten cells. When the ribbed and the plain cells were connected together in one series, the amount in one meter was, as before, 11 cubic inches.

It now seemed evident that a series of five did not confer *intensity* enough upon the current to enable its whole quantity to overcome the resistance of the conducting fluid between the two metals at the existing distance; and this being the case with the plain cells, no increase of quantity could be expected to manifest itself from those with extended surfaces. When the latter were joined with the former, the smaller surface of course governed the whole series according to the established law. I therefore completed the number of the ribbed cells to ten, and setting them to work against an equal number of plain, the product of the latter was, as before, 11· cubic inches, but that of the former did not exceed 8·5. I have not yet been able to satisfy myself, whether this unexpected difference, in the opposite direction to what I expected, is dependent upon the construction of the cells or upon some accidental circumstance which I have not been able to trace; but the result may certainly be taken to prove that no advantage arises from the extension or approximation of the conducting metal which I have described.

The increase of the number of the battery series requires for convenience a different arrangement from that which I described in my last communication; and I now place the cells in two parallel lines of ten each, upon a long table, the siphon-tubes arranged opposite to each other, and hanging over a small gutter placed between the rows, to carry off the refuse solution when it is necessary to change the acid; and as the uniformity of action may be completely maintained by the occasional addition of a small quantity of fresh liquid, I have been able to dispense with the cumbrous addition of the dripping funnels. This arrangement admits with facility of any combination of the plates which may be desired.

I proceeded now to connect the cells together in pairs; the zinc rod of each ribbed cell being in communication with that of a plain cell, and the copper with the copper. The ten pairs were then connected in a series of ten: the product of this combination was 17 cubic inches per five minutes, or exactly double that of the single-ribbed cells.

Considered in a theoretical point of view, these experiments seem to me to lead to the conclusion that the most perfect voltaic combination would consist of a solid sphere of a generating metal, surrounded by a hollow sphere of a conducting metal, with a stratum of intervening electrolyte perpetually renewed, and the metals communicating by a wire defended from the electrolyte by a glass tube covering that portion which it would be necessary should pass through it. In such a hypothetical arrangement, the resistance of the electrolyte would increase directly as the distances of the two spheres, or as the thickness of the stratum; while, supposing this resistance overcome, the quantity of force set in circulation would increase as the square of that distance from the centre, or as the surface of the exterior sphere. The number of a series required to give the necessary impulse would consequently only increase as the simple distance, while the advantage would increase as the square.

The rod of zinc within the cylinder of copper is probably the nearest practical approximation which can be made to such an arrangement; but the soundness of this deduction might doubtless be tested by varying the diameters of the cylinders.

The battery which I have now described, consisting of twenty cells, will, I think, be found amply sufficient for all the purposes of demonstration and investigation. It is competent to keep eight inches of platinum wire $\frac{1}{100}$th inch permanently red hot in the open air; and the amount of work which it is able to perform renders it even an economical source of the purest oxygen for laboratory purposes.

To facilitate this application, I have fitted up a cell by inclosing a platinum plate, instead of the zinc rod, within the membranous tube, which is closed at the upper end by a glass tube bent in a convenient form to deliver the disengaged gas under a receiver. When this cell is included in the circuit of double cells, the hydrogen is absorbed as before by the oxide of copper; but the oxygen is evolved at the rate of 84 cubic inches per hour.

I shall conclude these observations with the result of an experiment which places the secondary action of the affinities of the disengaged gases in a striking point of view. The mixed gases collected in a voltameter in five minutes were found to amount to 17 cubic inches; the oxygen collected in an equal time, when the hydrogen was absorbed, was 7 cubic inches; which are equivalent to 21 cubic inches of the mixed gases: and the hydrogen collected when the oxygen was absorbed (as is readily effected by reversing the connexions of the cell which I have just described) amounted to 16 cubic inches, equivalent to about 24 cubic inches of the mixed gases. Thus the removal of the hydrogen from the sphere of attraction allowed of an increase of action equal to 4 cubic inches, while the increase from the like removal of the oxygen was very nearly double; a difference which is probably referable to the equivalent combining volumes of the two gases. This observation opens a new field of inquiry of great interest, upon which, with your permission, I hope to have the pleasure of addressing you at no very distant period.

I remain, my dear FARADAY,

Yours very faithfully,

J. F. DANIELL.

King's College,
6*th April*, 1836.

XII. *Researches on the Tides.—Fifth Series. On the Solar Inequality and on the Diurnal Inequality of the Tides at Liverpool. By the Rev.* William Whewell, *F.R.S., Fellow of Trinity College, Cambridge.*

Received February 23,—Read March 3, 1836.

Sect. 1. *Present State of the subject.*

1. THE great success with which recent researches on the Tides have been attended, has encouraged me to attempt some further advances in this subject. The laws of the semimenstrual inequality of the times were shown by Mr. Lubbock, from the London observations, to agree very closely with the equilibrium-theory: and this result has been confirmed by the examination of observations made at many other places. I have shown, from the Liverpool observations, that the semimenstrual inequality of the heights presents a still more complete agreement with the equilibrium-theory*; and by the help of Mr. Lubbock's discussions of the London and of the Liverpool observations, I have shown, in the Second and Fourth Series of these Researches, that the inequalities depending on the changes of lunar parallax and declination may be very well represented by the equilibrium-theory, with certain modifications, which are far from inconsistent with the best mechanical views we can at present form of the laws of the motion of fluids.

The most obvious points which now remain requiring still to be made out and explained, are the diurnal inequality, and the solar inequalities of the time and height of high water.

2. The *Diurnal Inequality* of the tides is that which makes the tide of the morning and evening of the same day at the same place, differ both in height and time of high water, according to a law depending on the time of the year. This is called the diurnal inequality, because its cycle is a day.

The existence of such an inequality in the heights of high water has often been noticed by seamen and other observers, as I have stated in the First Series of these Researches†. But its reality has only recently been confirmed by regular and measured observations, and its laws have never been correctly laid down. Its existence appeared very palpably in the curves which I constructed in order to examine the results of the tide observations made by the coast-guard in June 1834, and also in the curves drawn by the self-registering tide-gauge erected near Bristol; although the Sheerness machine did not exhibit it, in consequence of the tide at that place

* Fourth Series: Philosophical Transactions, 1836, page 1.

† Philosophical Transactions, 1833, page 221.

being a compound of two others. But this inequality had never been obtained in numbers till the recent discussion of the Liverpool tides. Under Mr. Lubbock's direction, Mr. Dessiou has obtained it from the observations of Mr. Hutchinson. Mr. Bywater, who has calculated Tide-Tables for Liverpool for the present year, has also obtained this inequality from his own calculations, (suggested, as he states, by the remarks made in the First Series of these Researches,) and has introduced it, for the first time, into published tables; and I have just learnt from Mr. Bunt, who is at present employed in forming Tide-Tables for Bristol, that he has also obtained the general form of this inequality, agreeing, on the whole, with the other results, although with some discrepancies. These arise, I conceive, principally from the shortness of the series of observations which he had at his command (less than two years). The tide observations now going on at these ports, when hereafter discussed as the preceding ones have been, will, I have not any doubt, lead to the production of tide-tables possessing a degree of accuracy which, a little while ago, would have been considered unattainable.

3. It is natural for us to wish to refer the effects of the diurnal inequality, so far as they have yet been obtained, to the equilibrium-theory. I will say a few words on this subject.

The general relation of these results to the equilibrium-theory it is not difficult to see. When the moon is south of the equator, the equilibrium-tide corresponding to her upper transit at any place having southern latitude, would be greater than the tide corresponding to her lower transit: when she is north of the equator the contrary would be the case. When she is in the equator the two tides are equal. Now in one lunation she moves in an orbit inclined to the equator. Hence, while she moves from the sun to the sun again, (that is, while the time of her upper transit passes through the whole twenty-four hours from noon to noon again,) the tide which corresponds to her upper transit will, during one lunation, be greater during half the period, and less during the other half, than the tide of the next half-day; and at two particular times in the lunation the difference will vanish. The times of moon's transit for which the diurnal difference vanishes, correspond to the times when the moon is in the equator, and are therefore different at different seasons of the year. Now from the general correspondence of the phenomena of the tides with the equilibrium-theory, we may expect that the circumstances of the diurnal inequality will be the same as those which have been described; but that the time when the diurnal inequality vanishes will not be the time when the moon is in the equator, but some time afterwards.

4. By the statements above referred to on this subject it appears that the time at which the diurnal difference vanishes is when the moon's transit takes place about $9^h\ 30^m$ in January, and two hours earlier in each succeeding month, taking the general average of the facts. Now in the middle of January the sun is $4^h\ 30^m$ from the vernal equinox, and hence the moon is 5^h beyond the equinox when the diurnal inequality

vanishes. The same result would be obtained from the other months, since the sun's right ascension increases on an average two hours in each month*. Hence the evanescence of the diurnal inequality which, in the equilibrium-spheroid, would take place when the moon is in the equator, does not take place till she has described 5^h of right ascension after that time, and this requires six days and a quarter.

5. In the inequalities hitherto considered, which were the effects of the sun, and of changes of the moon's parallax and declination, the circumstances of the tide agree with those of the equilibrium-spheroid one day and a half or two days previous. It appears that such an interval suffices for the forces, when near their maximum, to accumulate and transmit their effects to Liverpool; and after this interval the diminution of the actual forces overbalances the increase arising from their continued action. But in this case of the diurnal inequality we find that above six days are required for this accumulation and transfer. The inequality vanishes six days after its cause vanishes, and in the same way it reaches its maximum six days after the producing force is greatest. On a little consideration we shall not be surprised at the great time required to bring this inequality to its full magnitude. The semi-diurnal tides, alternately greater and less, which are transmitted from the Southern Ocean to Liverpool, may be compared to oscillations of the ocean, and these are augmented by the action of the forces occurring at intervals equal to those of the oscillations. Hence the oscillations go on increasing for a considerable period after the forces have gone on diminishing, and reach their maximum almost a week after the forces have passed theirs.

6. In January the tide at Liverpool, which follows the moon's upper transit by about eleven hours, is *less* than the following tide, when the moon is near the sun, and consequently when the moon is south of the equator. This agrees with the equilibrium-tide depending upon the position of the moon at the time of transit. But it is clear that the tide which reaches Liverpool does in fact come from the Southern Ocean; and hence the equilibrium-tide for the time of the moon's upper transit at Liverpool will be *greater* than the following one, when the moon is near the sun in January. Hence the tide at Liverpool is not that which corresponds to the equilibrium-tide *at* the time of transit, but to the equilibrium-tide about either twelve or thirty-six hours *earlier*. The latter is probably the right conclusion, and agrees with the inference already obtained from the effects of the solar forces, that the tide agrees with an equilibrium-tide thirty-seven hours and a half previous to the moon's transit.

8. The succeeding sections of this paper will be devoted to the investigation of the *Solar Inequalities* at Liverpool. By carefully eliminating the lunar effects, which the preceding researches enable us to do, I have determined, as I conceive beyond dis-

* In order to detect the diurnal inequality, the observations have been classed by calendar months; but as this inequality depends mainly on the moon's declination, it would probably have been obtained more distinctly if the observations had been classed according to the moon's declination, distinguishing north and south declination, and also increasing and decreasing declination.

pute, the approximate circumstances of the solar correction for the *height*. I have also obtained, though by no means with the same certainty, evidence of the existence and laws of the solar inequality of the *times*. These inequalities, as thus discovered, exhibit the same general agreement with the equilibrium-theory which has been disclosed in all the inequalities hitherto detected*.

9. But though the equilibrium-theory thus seems to suggest and express the laws of the various inequalities of the tides, I would by no means be understood to rate this theory above its true value. It is not the true theory, but a very inaccurate and insufficient substitute for it, which we are compelled to adopt in consequence of the extremely imperfect state of the mathematical science of hydrodynamics. The tides are a problem of the motion, not of the *equilibrium* of fluids; and we can never fully explain the circumstances of the phenomena till the problem has been solved in its genuine form. This solution is perhaps not beyond the powers of modern mathematics, but it has certainly never yet been given. LAPLACE's solution, besides being obtained by means of a precarious assumption, rests upon several arbitrary hypotheses, fatal to it even as a first approximation; and, I believe it will be found, leaves out of consideration an essential portion of the forces. To obtain any useful result, the question must be taken up afresh and treated in another manner.

10. I hope some mathematician will be found able and willing to execute this task. But in the mean time I may be permitted to observe, that what has been already done in the discussion of tide observations, and in bringing to light the empirical laws of the phenomena, has entirely altered the position of this branch of science with respect to the mathematical theory. A little while ago the theory was in advance of observation; at present observation is in advance of theory. A very few years since, the equilibrium-theory and the Laplacian theory were in a condition to assign laws regulating the changes of the times and heights under given astronomical circumstances, and it had not been shown from observation whether these laws were obeyed. We can now state what the agreement and disagreement is between such theoretical laws and the facts; and we call upon the mathematician to substitute for these two theories, both confessedly false, some other, which shall come nearer to the true state of the case, and, by that means, nearer to the laws of the phenomena. The performance of this task is requisite for the completion of the Newtonian theory of the universe.

§ 2. *On the Effect of the Moon's Declination on the Tides at Liverpool*†.

11. In order to obtain the Solar Inequality, it was necessary to eliminate the effect of the variations of the lunar tidal forces; the results of the two sets of forces being combined in the tides when tabulated according to months, as is done in Mr. LUB-

* While I write this, I am informed by Mr. BUNT that he has obtained the law of the solar inequality of the times at Bristol. The agreement with my results is very remarkable.

† In the calculations of this Memoir, I have been assisted by Mr. NAYLER of Queen's College in this University.

BOCK's Tables I., II., III.* In these tables the parallax may be already considered as reduced to its mean value; for each number is the result of about ninety observations, distributed through nineteen years, in which time the moon's perigee moves twice round the ecliptic; and hence the deviations above and below the mean will balance each other. But it is otherwise with the declination; for since the mean moon may be considered as moving in the mean elliptic, a certain time of the year, with a certain hour of moon's transit, implies a certain declination; for the moon's time of transit added to the sun's right ascension is the moon's right ascension, which of course determines the declination. Hence the variations of the numbers in Tables II. and III. are those which arise from the varying forces of the mean moon and of the sun; and the greater part of the variation arises from the change of the declination of the moon. This part must be eliminated, in order to bring into view the solar effect; and we are able to perform this elimination by means of Table I., since that Table contains also the mean declination of the moon for each set of observations. In each set, the number of observations being large, and the limits of declination small, the mean correction for the lunar declination may be taken to be identical with the correction for the mean lunar declination, although the correction varies nearly as the square of the declination. But in order to apply this correction, we must have a table of the effect of every degree of declination; whereas Mr. LUBBOCK's Tables XII. and XVI. only contain the effect for every 3° of declination. It is obvious also, by inspection of this table, that it is affected by many casual irregularities which must be got rid of by interpolation. Among these irregularities, however, I do not include the apparent difference of the effect of north and south declination, which is shown in Tables XI. and XV.; since it will be seen by comparison with Table X. that the greater part of this difference arises from the effect of parallax; for the changes of parallax and declination so nearly recur in the same cycle, that their effects are not insulated in a period of nineteen years. The remainder of the difference arises from the solar inequality of which we are in search.

12. I have therefore laid down Mr. LUBBOCK's Tables XII. and XVI., and interpolated them by means of curves, and have thus obtained two new declination tables for the *Times* and the *Heights*. These tables I shall designate as Declination Tables (W. T.) and (W. H.) respectively; meaning by the former letter to distinguish them from Mr. LUBBOCK's Tables XII. and XVI. They may be used in calculating the time and height of high water at Liverpool (the table for the height being altered by a constant if the measures are not taken from Mr. HUTCHINSON's zero). When so applied, they take the place of Mr. LUBBOCK's Tables XXV. and XXVI., from which they differ—in including the semimenstrual inequality, in being interpolated independently, in being given for the middle of each hour instead of the beginning, and for every degree of declination instead of every three degrees. These Tables will be found at the end of this Memoir, namely,

* Philosophical Transactions, 1835, p. 282.

Declination Table (W. T.). *To be used in predicting the Time of high water at Liverpool, for the mean parallax.*

Declination Table (W. H.). *To be used in predicting the Height of high water at Liverpool, for the mean parallax.*

When these tables are used in predicting the tides, the times and heights found by these must be corrected for parallax by Mr. LUBBOCK's Tables XXIII. and XXIV., or by the formulæ given in my Memoir on the Empirical Laws of the Tides in the Port of Liverpool. I will insert, at the end of this paper, Tables of the correction for parallax given by these formulæ.

§ 3. *On the Solar Inequality of the Heights of High Water at Liverpool.*

13. By means of the Declination Tables (W. T.) and (W. H.) we can reject from Mr. LUBBOCK's Tables II. and III. the part which depends on the moon's declination, and we thus have the remainder, which is the solar correction as far as it can be obtained from the observations of Mr. HUTCHINSON. This is what I have done in the following calculations (Table A.). The third column of each table, marked (W. H.), contains the height due by the declination table; the fourth column, marked (L. III.), contains the observed height as given by Mr. LUBBOCK's Table III.; and the column (Diff. H.) is the excess of the latter, and is therefore the residual height due to the solar effect. In like manner the columns marked (W. T.), (L. II.) and (Diff. T.) contain the *Intervals* due to the moon's declination, the observed intervals, and the residual solar effect.

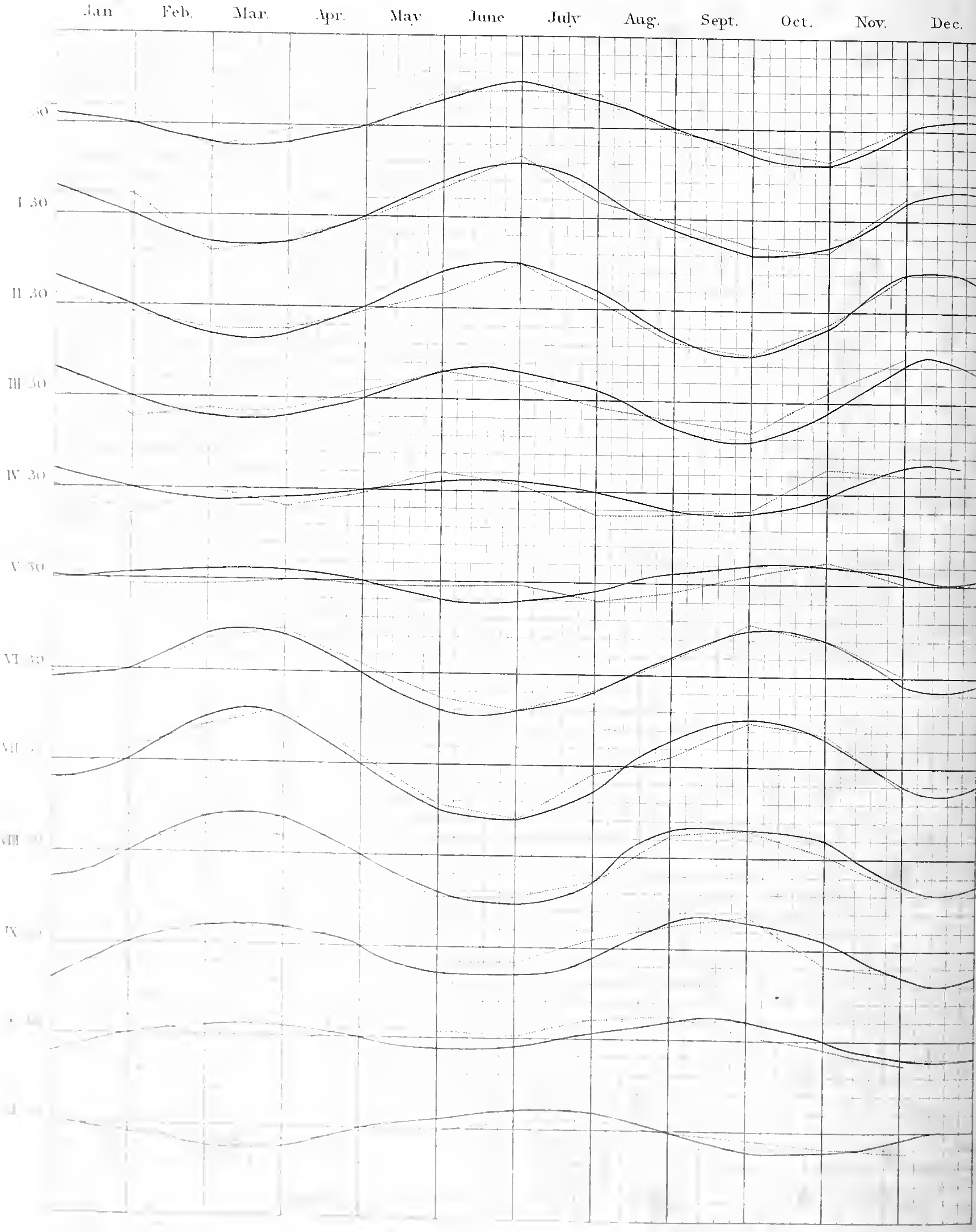
I shall consider the solar effect on the *Heights* in the first place, since its law is more manifest. It may be expected, like the other tidal inequalities, to be resolvable into a non-periodic and a periodic part. In order to effect this, I take the mean of each monthly column of differences, and subtract it from each number in the column. In this manner I obtain Table (B. H.).

In order to trace the law of the periodical part, which exists in the remainders, if at all, I lay down these remainders by coordinates, as in Plate XVII.

14. If we draw lines through the dots belonging to each hour of the moon's transit, it becomes manifest that these remainders really result from a solar inequality. For the curves (the dotted curves in Plate XVII.) have all the same general form, having a reference to an annual cycle. Thus there are, for all of them, a maximum or minimum in March, another in July, another in October, and another in December. Hence the effect of lunar declination has been nearly or altogether eliminated; for this effect is a maximum for different hours at different times of the year, as appears by the reason of the case, and as is shown in Mr. LUBBOCK's Tables IV. and V.

It appears also by the curves, that the effects of the changes of the solar force are the greatest for the hours of moon's transit, $1^h\ 30^m$ and $7^h\ 30^m$, and are in these two cases the opposite of one another, which agrees with the nature of the case; for the

SOLAR CORRECTION OF HEIGHTS. PERIODIC PARTS. LIVERPOOL.



[illegible] time too large. The positive ordinates are measured downwards.

J. Basire sculp.

height of the solar tide is added to that of the lunar in the former case (at spring-tides), and subtracted in the latter (at neap-tides).

15. Thus the general course of the phenomena shows the existence of the solar inequality; but we shall trace its law more distinctly by taking the suggestions of the equilibrium-theory.

If h, h', be the height of the solar and lunar tides, ϕ the angular distance of the sun and moon, y the compound tide, we have

$$y = \sqrt{\{h^2 + h'^2 + 2\,h\,h' \cos 2\,(\phi - \alpha)\}},$$

as shown on former occasions; α being a quantity which is to be determined so as to accommodate the equilibrium-theory to the actual case.

Let h undergo any change so as to become $h + \Delta\,h$, $\Delta\,h$ being small. Then y becomes approximately

$$y + \Delta\,y = y + \frac{d\,y}{d\,h}\,\Delta\,h = y + \frac{h + h' \cos 2\,(\phi - \alpha)}{\sqrt{\{h^2 + h'^2 + 2\,h\,h' \cos 2\,(\phi - \alpha)\}}}\,\Delta\,h.$$

The quantity $\Delta\,h$ varies according to the different season of the year, and depends principally on the sun's declination, the changes of solar parallax being too small to affect much the amount of solar tidal force. The curve which expresses the changes of $\Delta\,h$ has, as appears in the figures, a maximum at the end of March, soon after the time when the sun is in the equator, and when, consequently, his tidal force is the greatest. It cuts the axis in May, soon after the sun has his mean effective declination: it has a minimum in July, soon after his greatest declination, at which time his force is least. The mean effect recurs in the end of August: there is another maximum in the end of September. About November, December, and January, there is another mean and another minimum, arising from the mean declination in November and the greatest declination in December. Thus the general course of the value of the correction for a given value of ϕ agrees with the equilibrium-theory. The want of perfect regularity in the form of the curves is due partly to the combination of the effects of solar parallax and solar declination. According to the theory, the greatest amount of the correction for solar declination is about one tenth, and the greatest amount of the correction for solar parallax about one twentieth, of the whole solar tide.

16. For a given season of the year, if we follow the changes of ϕ through twelve hours, we easily see from the formula that Δy has a continuous series of values, among which are a maximum and a minimum value. Hence the curves in Plate XVII. ought to be such that the ordinates for each month, taken in the successive hour-lines, form a continuous series. The subtraction of the non-periodical part, which we have performed, will not affect this continuity, since this part is constant for the month.

Hence in correcting the original curves of Plate XVII., so as to get rid of irregularities, we must endeavour to make them conform to these two conditions:—that the ordinates for the same month shall form a continuous series, with a maximum and minimum; and, that the curves for the different hours shall be similar to each other, and have their maxima and minima at the same seasons. This latter condition may perhaps be slightly modified, so that α may be somewhat different for different values of Δh.

The curves drawn with full lines in Plate XVII., are drawn under these conditions, the ordinates being those contained in Table (C. H.). Their agreement with the original curves is such as to entitle us to consider them as correct interpolations, for the coincidence is almost complete in the cases where the corrections are the largest; as the hours $0^h\ 30^m$, $1^h\ 30^m$, $2^h\ 30^m$, $6^h\ 30^m$, $7^h\ 30^m$, $8^h\ 30^m$; and there are no material discrepancies except in the lines $4^h\ 30^m$ and $10^h\ 30^m$; and even in these the difference is only a displacement of a maximum of four inches by about a month. The observations, therefore, prove that there is a solar correction of the heights which follows, nearly, the law suggested by the equilibrium-theory. The greatest amount of the periodical part of this correction is about half a foot *plus* and *minus*; and to this must be added the non-periodical part, which at some seasons amounts to one fifth of a foot.

17. We may expand the formula above given for Δy into a non-periodical and a periodical part. We have

$$\Delta y = \frac{h + h' \cos 2(\phi - \alpha)}{\sqrt{\{h^2 + h'^2 + 2hh' \cos 2(\phi - \alpha)\}}} \cdot \Delta h,$$

$$= \frac{h'}{\sqrt{(h^2 + h'^2)}} \frac{c + \cos 2(\phi - \alpha)}{\sqrt{\{1 + \frac{2c}{1 + c^2} \cos 2(\phi - \alpha)\}}} \cdot \Delta h,$$

Making $c = \frac{h}{h'}$. Expanding and omitting c^2, &c.

$$\Delta y = \frac{h'}{\sqrt{(h^2 + h'^2)}} \Big(c + \cos 2(\phi - \alpha)\Big) \Big(1 - c \cos 2(\phi - \alpha)\Big) \Delta h$$

$$= \frac{h'}{\sqrt{(h^2 + h'^2)}} \Big\{c + \cos 2(\phi - \alpha) - c \cos^2 2(\phi - \alpha)\Big\} \Delta h$$

$$= \frac{h'}{\sqrt{(h^2 + h'^2)}} \Big\{\frac{c}{2} + \cos 2(\phi - \alpha) - \frac{c}{2} \cos 4(\phi - \alpha)\Big\} \Delta h$$

Hence the periodical part is

$$\frac{h' \Delta h}{\sqrt{(h^2 + h'^2)}} \Big\{\cos 2(\phi - \alpha) - \frac{c}{2} \cos 4(\phi - \alpha)\Big\}.$$

This vanishes for two values of ϕ, which are a little less than 90° or 6^h from each other when c is small. In Table (B. H.) it appears that the periodical solar correction vanishes for two values of the hour-angle which are nearly 6^h from each other in each month. Hence we may suppose that the periodical part involving $\cos 2(\phi - \alpha)$ is alone sensible. This vanishes when $\cos 2(\phi - \alpha) = 6^h$ or 18^h, $\phi = 3^h + \alpha$ or $9^h + \alpha$. Hence we find that in January and September α is $2^h\ 30^m$; in March, April, June, July, October α is about 2^h; in August and November it is $1^h\ 30^m$; in December it is 3^h.

These differences in the value of α may arise from the lunar declination not being completely eliminated in our previous calculation, the interpolation of the Table being slightly inexact. But it is by no means improbable that they are the discrepancies, arising from mechanical principles, which exist between the tides of water in motion and the results of the equilibrium-hypothesis. The general agreement of the equilibrium-theory with the facts (assuming $\alpha = 2^h$) is near enough to be very remarkable. It may be possible, by additional care and labour, to bring the solar interpolated curves nearer to the observations, preserving the requisite conditions: but I conceive that enough has been done to establish the general law.

§ 4. *On the Solar Inequality of the Time of High Water at Liverpool.*

18. The solar inequality of the time must be found in exactly the same manner as the solar inequality of the heights. By interpolation of Mr. LUBBOCK's Table XVI., I have obtained Declination Table (W. T.); and by comparing the time of high water due to lunar declination by this Table with the observed times as given in column (L. II.) of the calculations, I obtain the residual quantities in the columns (Diff. T.), which should exhibit the solar correction of the times.

I then find the mean of the column for each month, and subtract it from every number in the column, in order to separate the non-periodical from the periodical part of the residual quantities. The means and the remainders are exhibited in Table (B. T.). The remainders were laid down by coordinates, but I have not thought it necessary to give these curves.

The points thus found being joined by continuous lines, I had a series of curves which ought to exhibit the solar inequality. These lines were less obviously regular than those which we obtained by a similar treatment of the heights; but they were still apparently free from any lunar effect, which would have given a maximum passing successively from one month to another; and I could trace a solar cycle in them; namely, the curves for $1^h\ 30^m$, $2^h\ 30^m$, and $3^h\ 30^m$ had a maximum about April, and a minimum about August, while the curves for $5^h\ 30^m$, $6^h\ 30^m$, $7^h\ 30^m$ had these features inverted, and the rest of the curves had small ordinates only.

19. Let us compare the laws of the phenomena of which we thus catch a glimpse with the laws according to the equilibrium-theory. We have, on that hypothesis,

$$\tan(\theta' - \lambda') = -\frac{h \sin 2(\varphi - \alpha)}{h' + h \cos 2(\varphi - \alpha)} = t \text{ suppose,}$$

where θ' is the interval of the tide and moon's transit.

Now let h become $h + \Delta h$, and θ' become $\theta' + \Delta\theta'$, we have, approximately,

$$\tan(\theta' - \lambda') + \frac{d \cdot \tan(\theta' - \lambda')}{d\theta'} \Delta\theta' = t + \frac{dt}{dh} \Delta h$$

$$\sec^2(\theta' - \lambda') \cdot \Delta\theta' = \frac{dt}{dh} \Delta h$$

$$\Delta \theta' = \frac{1}{1+t^2} \cdot \frac{d\,t}{d\,h} \Delta h$$

$$= - \frac{h' \sin 2(\phi - \alpha)}{h^2 + h'^2 + 2\,h\,h' \cos 2(\phi - \alpha)} \cdot \Delta h.$$

Hence for a given value of Δh, that is, for a given month, $\Delta \theta'$ goes through a cycle which has a minimum and a maximum, when $\cos 2(\phi - \alpha) = - \frac{2\,h\,h'}{h^2 + h'^2}$. If $\frac{h'}{h} = 3$, which has been found to be nearly the value by other phenomena, the two values of $2(\phi - \alpha)$ are 144° and 216°, or 10^h and 17^h nearly; and if in this case, α be $- 1^h\ 30^m$, the maxima and minima will agree with $\phi = 3^h\ 30^m$, $\phi = 7^m\ 0^m$, which appears best to represent the phenomena.

20. After drawing the lines described in Art. 18, I drew interpolated curves for the hours $1^h\ 30^m$, $2^h\ 30^m$, $3^h\ 30^m$, and $5^h\ 30^m$, $6^h\ 30^m$, $7^h\ 30^m$; the general agreement of the course of the interpolated and the original curves appeared to me to be such as to show that the errors arose from a solar inequality, following in its general changes the law given by the equilibrium-theory. I have given the corrections upon this supposition in Table (C. T.). The displacement of the zero points and maxima, and the want of proportionality in the maximum values, which appear in some of the lines, I have admitted, because such modifications may arise from my not having got rid of the whole of the non-periodical effect. Such irregularities may also arise from there being still some vestige of the lunar declination correction not got rid of by the processes which I have employed: but this, if it exists, must be very small, and I do not think there can be any doubt as to the general form of the solar correction of the time. I have, however, abstained from filling up the Table, as not thinking my present materials sufficient to enable me to do so with any confidence.

Declination Table (W. T.). To be used in predicting the *Time* of high water at Liverpool for the mean parallax.

Moon's Transit.	0° Decl.	1° Decl.	2° Decl.	3° Decl.	4° Decl.	5° Decl.	6° Decl.	7° Decl.	8° Decl.	9° Decl.	10° Decl.	11° Decl.	12° Decl.	13° Decl.
h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m
0 30	11 20	11 20	11 20	11 20	11 20	11 20	11 20·5	11 20	11 20	11 19·5	11 19	11 19	11 18	11 18·5
1 30	11 5	11 5	11 5·5	11 5	11 5	11 5	11 5	11 5	11 4	11 4	11 4	11 3·5	11 3	11 2·5
2 30	10 51	10 51	10 51	10 51	10 51	10 51	10 50	10 50	10 50	10 49·5	10 49	10 49	10 47·5	10 47·5
3 30	10 39	10 39·5	10 39·5	10 39·5	10 39·5	10 39·5	10 39	10 39	10 38·5	10 38	10 38	10 37	10 36·5	10 36
4 30	10 31	10 31·5	10 31·5	10 31·5	10 31	10 31·5	10 31·5	10 31	10 30	10 29	10 28·5	10 28	10 27·5	10 27
5 30	10 35	10 35	10 35	10 34·5	10 34·5	10 34·5	10 34	10 34	10 33·5	10 32·5	10 32	10 31	10 30	10 28·5
6 30	10 54	10 54	10 54	10 53·5	10 53	10 52·5	10 52	10 51·5	10 51	10 50	10 49	10 48	10 47	10 46
7 30	11 24	11 24	11 24	11 24	11 24	11 24	11 23·5	11 23	11 23	11 22	11 22	11 20·5	11 20	11 19
8 30	11 47	11 47	11 47	11 47	11 47	11 47	11 47	11 47	11 47	11 47	11 46	11 46	11 45	11 44·5
9 30	11 53·5	11 53·5	11 53·5	11 53·5	11 53·5	11 53·5	11 53	11 53	11 53	11 52·5	11 52	11 52	11 52	11 51·5
10 30	11 46	11 46	11 46	11 46	11 46	11 46	11 46	11 46	11 46	11 46	11 46	11 46	11 46	11 46
11 30	11 35	11 35	11 35	11 35	11 35	11 35	11 35	11 35	11 35	11 34·5	11 34·5	11 34·5	11 34	11 34
	14° Decl.	15° Decl.	16° Decl.	17° Decl.	18° Decl.	19° Decl.	20° Decl.	21° Decl.	22° Decl.	23° Decl.	24° Decl.	25° Decl.	26° Decl.	27° Decl.
0 30	11 17·5	11 17	11 16·5	11 16	11 15	11 14	11 14	11 13	11 12·5	11 12	11 11·5	11 11	11 10	11 9
1 30	11 2	11 1·5	11 1·5	11 0·5	10 59·5	10 59	10 58·5	10 58	10 57·5	10 57	10 56	10 54	10 53	10 52
2 30	10 47	10 46·5	10 43·5	10 44	10 44	10 43	10 42	10 41	10 40	10 39	10 37·5	10 37	10 36	10 34
3 30	10 35	10 34	10 33	10 32·5	10 32	10 31	10 29·5	10 28	10 27	10 26	10 25	10 24	10 22	10 21
4 30	10 26·5	10 25	10 24	10 23	10 22	10 21·5	10 20	10 18·5	10 17·5	10 16	10 14	10 12·5	10 11	10 9
5 30	10 27	10 26	10 25	10 23·5	10 22·5	10 21·5	10 19	10 18	10 17	10 15	10 13·5	10 12·5	10 11·5	10 10
6 30	10 44·5	10 43·5	10 42·5	10 41	10 39	10 37·5	10 36	10 35	10 33	10 32	10 31	10 29	10 27	10 25
7 30	11 18	11 18	11 17	11 15	11 13·5	11 12·5	11 11	11 10	11 8·5	11 7	11 6	11 4	11 2·5	11 0·5
8 30	11 44·5	11 44	11 43	11 42	11 42	11 41	11 40·5	11 40	11 39	11 38	11 37	11 36	11 34	11 33·5
9 30	11 51	11 51	11 50	11 50	11 49	11 49	11 48·5	11 48	11 47·5	11 47	11 46·5	11 46	11 45	11 44
10 30	11 45·5	11 44·5	11 44·5	11 44	11 43	11 43	11 42·5	11 42	11 41·5	11 40	11 39·5	11 39	11 38	11 37·5
11 30	11 33·5	11 32·5	11 32·5	11 32	11 32	11 31·5	11 30	11 29	11 28·5	11 28	11 27·5	11 27	11 26	11 25

Declination Table (W. H.). To be used in predicting the ***Height*** of high water at Liverpool for the mean parallax.

The heights in feet from Mr. Hutchinson's zero.

Moon's Transit.	0° Decl.	1° Decl.	2° Decl.	3° Decl.	4° Decl.	5° Decl.	6° Decl.	7° Decl.	8° Decl.	9° Decl.	10° Decl.	11° Decl.	12° Decl.	13° Decl.
h m	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.
0 30	18·3	18·3	18·3	18·3	18·3	18·3	18·3	18·3	18·3	18·25	18·25	18·2	18·15	18·1
1 30	18·3	18·3	18·3	18·3	18·3	18·3	18·3	18·3	18·3	18·25	18·25	18·2	18·15	18·1
2 30	17·65	17·65	17·65	17·65	17·65	17·65	17·65	17·65	17·65	17·6	17·6	17·55	17·5	17·5
3 30	16·6	16·6	16·6	16·6	16·6	16·55	16·55	16·55	16·55	16·5	16·5	16·5	16·45	16·4
4 30	15·2	15·2	15·2	15·2	15·2	15·2	15·15	15·15	15·1	15·1	15·05	15·05	15·0	14·95
5 30	13·95	13·95	13·9	13·9	13·9	13·9	13·85	13·8	13·75	13·75	13·75	13·7	13·65	13·65
6 30	13·0	13·0	13·0	13·0	12·95	12·95	12·9	12·9	12·85	12·8	12·75	12·7	12·65	12·6
7 30	12·95	12·95	12·95	12·95	12·95	12·9	12·	12·9	12·85	12·8	12·75	12·7	12·65	12·6
8 30	13·95	13·95	13·9	13·9	13·85	13·85	13·85	13·8	13·75	13·75	13·7	13·65	13·65	13·55
9 30	15·35	15·35	15·35	15·35	15·35	15·35	15·35	15·35	15·3	15·3	15·25	15·25	15·2	15·15
10 30	16·8	16·8	16·8	16·8	16·75	16·75	16·75	16·75	16·7	16·7	16·65	16·6	16·55	16·5
11 30	17·7	17·7	17·7	17·7	17·65	17·65	17·65	17·65	17·65	17·65	17·6	17·55	17·55	17·45
	14° Decl.	15° Decl.	16° Decl.	17° Decl.	18° Decl.	19° Decl.	20° Decl.	21° Decl.	22° Decl.	23° Decl.	24° Decl.	25° Decl.	26° Decl.	27° Decl.
0 30	18·05	17·95	17·9	17·8	17·7	17·65	17·5	17·4	17·3	17·2	17·1	17·0	16·8	16·7
1 30	18·05	17·95	17·9	17·8	17·7	17·65	17·5	17·4	17·3	17·2	17·1	17·0	16·8	16·7
2 30	17·45	17·4	17·35	17·25	17·2	17·1	17·0	16·9	16·85	16·75	16·65	16·55	16·4	16·3
3 30	16·35	16·35	16·3	16·25	16·25	16·2	16·15	16·1	16·0	15·95	15·9	15·8	15·7	15·6
4 30	14·95	14·9	14·9	14·8	14·75	14·75	14·65	14·6	14·55	14·5	14·4	14·35	14·25	14·15
5 30	13·6	13·55	13·5	13·45	13·4	13·35	13·25	13·2	13·1	13·05	12·95	12·9	12·8	12·75
6 30	12·55	12·5	12·45	12·35	12·25	12·2	12·1	12·0	11·9	11·8	11·7	11·5	11·35	11·2
7 30	12·5	12·5	12·4	12·35	12·25	12·2	12·1	12·0	11·9	11·75	11·65	11·5	11·35	11·2
8 30	13·5	13·45	13·4	13·3	13·25	13·2	13·15	13·05	12·95	12·9	12·8	12·75	12·65	12·55
9 30	15·1	15·05	15·0	14·95	14·9	14·85	14·75	14·7	14·6	14·5	14·4	14·3	14·2	14·1
10 30	16·45	16·4	16·35	16·3	16·25	16·2	16·1	16·0	15·85	15·75	15·6	15·45	15·3	15·1
11 30	17·4	17·35	17·25	17·2	17·1	17·0	16·9	16·8	16·7	16·6	16·45	16·3	16·15	16·0

Parallax Table (W. T.). To be used in correcting the *Time* of high water at Liverpool, predicted for mean parallax 57′.

Moon's Transit.	54′.	55′.	56′.	57′.	58′.	59′.	60′.	61′.
h m	m	m	m	m	m	m	m	m
0 30	9	6	3	0	− 3	− 6	− 9	−12
1 30	7	5	2	0	− 2	− 5	− 7	− 9
2 30	5	3	2	0	− 2	− 3	− 5	− 6
3 30	3	2	1	0	− 1	− 2	− 3	− 3
4 30	1	1	0	0	0	− 1	− 1	− 1
5 30	1	1	0	0	0	− 1	− 1	− 2
6 30	4	3	1	0	− 1	− 3	− 4	− 5
7 30	9	6	3	0	− 3	− 6	− 9	−12
8 30	13	8	4	0	− 4	− 8	−13	−17
9 30	14	9	5	0	− 5	− 9	−14	−19
10 30	13	9	4	0	− 4	− 9	−13	−18
11 30	11	7	4	0	− 4	− 7	−11	−15

Parallax Table (W. H.). To be used in correcting the *Height* of high water at Liverpool, predicted for mean parallax 57′.

H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.
−1·2	−0·8	−0·4	0	+0·4	+0·8	+1·2	+1·6

Table (A).

Calculation of the Differences between the Time and Height of High Water at Liverpool, as due to the Moon's Declination, and as shown by Mr. Hutchinson's Observations, for every month of the year.

January.							
Moon's Transit.	Declination.	(W. H.) Height due to Declination.	(L. III.) Observed Height.	Diff. H.	(W. T.) Time due to Declination.	(L. II.) Observed Time.	Diff. T.
h m	°	feet.	feet.		h m	h m	m
0 30	18	17·70	17·47	−·23	11 15	11 13·7	−1·3
1 30	15	17·95	17·74	−·21	11 1	10 59·5	−1·5
2 30	10	17·60	17·52	−·08	10 48	10 47·0	−1·0
3 30	5	16·55	16·08	−·47	10 40	10 37·1	−2·9
4 30	5	15·15	15·14	−·01	10 31	10 31·1	+ ·1
5 30	8	13·75	13·19	−·06	10 33	10 33·8	+ ·8
6 30	14	12·55	12·61	+·06	10 44	10 49·3	+5·3
7 30	19	12·20	12·38	+·18	11 12	11 16·1	+4·1
8 30	19	13·20	13·56	+·36	11 41	11 37·8	−3·2
9 30	23	14·50	14·95	+·45	11 47	11 45·0	−2·0
10 30	22	15·85	15·98	+·13	11 41	11 38·5	−2·5
11 30	22	16·70	16·73	+·03	11 28	11 34·2	+6·2

TABLE (A). Continued.

Moon's Transit.	Declination.	(W. H.) Height due to Declination.	(L. III.) Observed Height.	Diff. H.	(W. T.) Time due to Declination.	(L. II.) Observed Time.	Diff. T.
February.							
h m	°	feet.	feet.		h m	h m	m
0 30	10	18·25	18·05	—·20	11 19	11 18·3	— ·7
1 30	5	18·30	18·04	—·26	11 5	11 8·2	+3·2
2 30	5	17·65	17·69	+·04	10 50	10 49·7	— ·3
3 30	8	16·55	16·73	+·18	10 39	10 37·3	—1·7
4 30	14	14·95	15·20	+·25	10 26	10 27·3	+1·3
5 30	18	13·40	13·39	—·01	10 22	10 26·3	+4·3
6 30	21	12·00	11·96	—·04	10 35	10 37·9	+2·9
7 30	21	11·95	11·78	—·17	11 10	11 8·0	—2·0
8 30	23	12·90	12·79	—·11	11 38	11 37·4	— ·6
9 30	22	14·60	14·47	—·13	11 47	11 51·0	+4·0
10 30	19	16·20	15·96	—·24	11 43	11 44·0	+1·0
11 30	15	17·35	17·39	+·04	11 32	11 31·5	— ·5
March.							
0 30	5	18·3	18·38	+·08	11 20	11 19·1	— ·9
1 30	8	18·25	18·49	+·24	11 4	11 3·9	— ·1
2 30	13	17·5	17·63	+·13	10 47·5	10 49·4	+1·9
3 30	16	16·3	16·31	+·01	10 33	10 33·3	+ ·3
4 30	21	14·6	14·47	—·13	10 19	10 20·3	+1·3
5 30	22	13·1	13·01	—·09	10 17	10 16·2	— ·8
6 30	22	11·9	11·42	—·48	10 33	10 30·2	—2·8
7 30	22	11·85	11·31	—·54	11 8·5	11 5·8	—2·7
8 30	20	13·1	12·68	—·42	11 40	11 40·7	+ ·7
9 30	15	15·05	14·62	—·43	11 50·5	11 50·3	— ·2
10 30	10	16·65	16·54	—·11	11 46	11 44·9	—1·1
11 30	6	17·65	17·67	+·02	11 35	11 33·4	—1·6
April.							
0 30	12	18·15	18·01	—·14	11 19	11 19·4	+ ·4
1 30	17	17·8	17·88	+·08	11	11 1·1	+1·1
2 30	20	17	17·01	+·01	10 42	10 43·4	+1·4
3 30	22	16·05	15·98	—·07	10 27	10 30·4	+3·4
4 30	23	14·45	14·44	—·01	10 15	10 14·2	— ·8
5 30	22	13·1	12·89	—·21	10 17	10 12·8	—4·2
6 30	20	12·1	11·44	—·66	10 36	10 31·8	—4·2
7 30	16	12·4	11·71	—·69	11 16·5	11 8·5	—8·0
8 30	11	13·65	13·28	—·37	11 45	11 44·0	—1·0
9 30	6	15·35	15·07	—·28	11 53	11 54·3	+1·3
10 30	5	16·75	16·61	—·14	11 46	11 48·2	+2·2
11 30	7	17·65	17·60	—·05	11 35	11 36·0	+1·0
May.							
0 30	20	17·5	17·33	—·17	11 14	11 16·3	+2·3
1 30	22	17·3	17·15	—·15	10 57	10 57·9	+ ·9
2 30	23	16·75	16·56	—·19	10 39	10 38·6	— ·4
3 30	22	16	15·75	—·25	10 27	10 25·1	—1·9
4 30	20	14·65	14·48	—·17	10 20	10 16·0	—4·0
5 30	16	13·5	13·33	—·17	10 25	10 18·8	—6·2
6 30	12	12·65	12·35	—·30	10 47	10 41·9	—5·1
7 30	7	12·85	12·58	—·27	11 23	11 20·6	—2·4
8 30	5	13·85	13·75	—·10	11 46·5	11 46·9	+ ·4
9 30	7	15·35	15·01	—·34	11 53	11 52·5	— ·5
10 30	12	16·55	16·28	—·27	11 46	11 46·9	+ ·9
11 30	17	17·2	17·01	—·19	11 32	11 31·2	— ·8

Table (A). Continued.

Moon's Transit.	Declination.	(W. H.) Height due to Declination.	(L. III.) Observed Height.	Diff. H.	(W. T.) Time due to Declination.	(L. II.) Observed Time.	Diff. T.
June.							
h m	°	feet.	feet.		h m	h m	m
0 30	23	17·2	16·73	−·47	11 12	11 13·4	+1·4
1 30	22	17·3	16·84	−·46	10 57	10 55·5	−1·5
2 30	20	17	16·74	−·26	10 42	10 39·8	−2·2
3 30	16	16·3	15·81	−·49	10 33	10 29·6	−3·4
4 30	11	15·05	14·69	−·36	10 28	10 24·9	−3·1
5 30	6	13·85	13·75	−·10	10 34	10 31·6	−2·4
6 30	5	12·95	13·07	+·12	10 52·5	10 52·9	+ ·4
7 30	8	12·8	13·09	+·29	11 22·5	11 24·6	+2·1
8 30	12	13·6	13·86	+·26	11 45	11 44·0	−1·0
9 30	17	14·95	14·93	−·02	11 50	11 49·2	− ·8
10 30	20	16·1	15·91	−·19	11 42	11 41·8	− ·2
11 30	22	16·7	16·57	−·13	11 28	11 28·9	+ ·9
July.							
0 30	19	17·6	17·08	−·52	11 14	11 13·5	− ·5
1 30	16	17·9	17·14	−·76	11 1	10 57·8	−3·2
2 30	11	17·55	16·84	−·61	10 49	10 45·2	−3·8
3 30	6	16·55	16·29	−·26	10 39	10 37·6	−1·4
4 30	5	15·15	14·96	−·19	10 31	10 33·1	+2·1
5 30	8	13·75	13·83	+·08	10 33	10 34·2	+1·2
6 30	13	12·6	12·87	+·27	10 45	10 51·6	+6·6
7 30	18	12·25	12·72	+·47	11 13	11 12·2	−0·8
8 30	21	13·05	13·39	+·34	11 40	11 38·9	−1·1
9 30	22	14·6	14·64	+·04	11 45·5	11 48·5	+3·0
10 30	23	15·75	15·61	−·14	11 40	11 39·6	− ·4
11 30	22	16·7	16·52	−·18	11 28	11 27·2	− ·8
August.							
0 30	11	18·2	17·68	−·52	11 19	11 17·5	−1·5
1 30	6	18·3	17·96	−·34	11 5	11 4·5	− ·5
2 30	5	17·65	17·37	−·28	10 51	10 51·6	+ ·6
3 30	7	16·55	16·46	−·09	10 39	10 35·4	−3·6
4 30	13	14·95	15·03	+·08	10 27	10 30·2	+3·2
5 30	17	13·45	13·46	+·01	10 23	10 28·7	+5·7
6 30	21	12	12·00	·00	10 35	10 38·4	+3·4
7 30	22	11·85	11·80	−·05	11 8	11 8·6	+ ·6
8 30	25	12·7	12·82	+·12	11 36	11 37·4	+1·4
9 30	22	14·6	14·34	−·26	11 47·5	11 46·9	− ·6
10 30	19	16·2	15·84	−·36	11 43	11 42·4	− ·6
11 30	16	17·25	16·99	−·26	11 32	11 30·1	−1·9
September.							
0 30	4	18·3	18·30	·00	11 20	11 20·4	+ ·4
1 30	7	18·3	18·39	+·09	11 10	11 3·0	−7·0
2 30	12	17·5	17·89	+·39	10 48	10 48·8	+ ·8
3 30	17	16·25	16·50	+·25	10 32·5	10 34·0	+1·5
4 30	20	14·65	14·94	+·29	10 20	10 21·3	+1·3
5 30	22	13·1	13·19	+·09	10 17	10 16·9	− ·1
6 30	23	11·8	11·63	−·17	10 32	10 32·1	+ ·1
7 30	22	11·85	11·78	−·07	11 8	11 4·8	−3·2
8 30	20	13·1	12·86	−·24	11 40	11 39·3	− ·7
9 30	16	15	14·78	−·22	11 50	11 50·3	+ ·3
10 30	11	16·6	16·38	−·22	11 46	11 46·4	+ ·4
11 30	6	17·65	17·69	+·04	11 35	11 35·2	+ ·2

TABLE (A). Continued.

Moon's Transit.	Declination.	(W. H.) Height due to Declination.	(L. III.) Observed Height.	Diff. H.	(W. T.) Time due to Declination.	(L. II.) Observed Time.	Diff. T.
October.							
h m	°	feet.	feet.		h m	h m	h m
0 30	11	18·2	18·52	+·32	11 19	11 19·1	+ ·1
1 30	16	17·85	18·28	+·43	11 1	11 1·7	+ ·7
2 30	20	17	17·62	+·62	10 42	10 42·3	+ ·3
3 30	22	16	16·48	+·46	10 27	10 26·7	− ·3
4 30	23	14·45	14·76	+·31	10 15	10 14·7	− ·3
5 30	22	13·1	13·15	+·05	10 16	10 12·2	−3·8
6 30	20	12·1	11·70	−·40	10 36	10 29·1	−6·9
7 30	17	12·3	11·95	−·35	11 15	11 11·0	−4·0
8 30	13	13·55	13·37	−·18	11 45	11 45·1	+ ·1
9 30	7	15·35	15·13	−·22	11 53	11 54·0	+1·0
10 30	4	16·75	16·83	+·08	11 46	11 48·7	+2·7
11 30	7	17·65	17·92	+·27	11 35	11 35·3	+ ·3
November.							
0 30	20	17·5	17·95	+·45	11 14	11 18·4	+4·4
1 30	22	17·3	17·73	+·43	10 57·5	10 58·7	+1·2
2 30	23	16·75	16·99	+·24	10 38	10 39·1	+1·1
3 30	22	16	16·01	+·01	10 27	10 24·1	−2·9
4 30	20	14·65	14·50	−·15	10 20	10 14·7	−5·3
5 30	16	13·5	13·33	−·17	10 24·5	10 17·1	−7·4
6 30	13	12·6	12·28	−·32	10 45	10 40·4	−4·6
7 30	7	12·85	12·62	−·23	11 23	11 21·3	−1·7
8 30	5	13·85	13·95	+·10	11 47	11 47·9	+ ·9
9 30	6	15·35	15·61	+·26	11 53	11 53·8	+ ·8
10 30	12	16·55	16·73	+·18	11 46	11 46·7	+ ·7
11 30	17	17·2	17·50	+·30	11 32	11 32·9	+ ·9
December.							
0 30	23	17·2	17·25	+·05	11 12·5	11 12·9	+ ·4
1 30	21	17·4	17·28	−·12	10 58	10 54·8	−3·2
2 30	19	17·1	16·80	−·30	10 43	10 40·8	−2·2
3 30	16	16·3	15·89	−·41	10 33	10 28·7	−4·3
4 30	11	15·05	14·95	−·10	10 28	10 23·9	−4·1
5 30	8	13·75	13·82	+·07	10 33	10 30·6	−2·4
6 30	5	12·95	13·06	+·11	10 52·5	10 52·5	− ·0
7 30	7	12·85	13·21	+·36	11 23	11 23·4	+ ·4
8 30	12	13·6	14·07	+·47	11 45	11 45·7	+ ·7
9 30	17	14·95	15·35	+·40	11 50	11 48·2	−1·8
10 30	20	16·05	16·38	+·33	11 42·5	11 42·2	− ·3
11 30	21	16·8	17·14	+·34	11 29	11 27·7	−1·3

TABLE (B. II.). The Lunar Effect rejected.

Residual quantities. *Heights.*

☽'s Transit.	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.
Means	+·01	−·05	−·14	−·21	−·20	−·15	−·12	−·16	+·02	+·12	+·09	+·10
	Remainders.											
h m												
0 30	−·24	−·15	+·22	+·07	+·03	−·32	−·40	−·36	−·02	+·20	+·36	−·05
1 30	−·22	−·21	+·38	+·29	+·05	−·31	−·64	−·18	+·07	+·31	+·34	−·22
2 30	−·09	+·09	+·27	+·22	+·01	−·11	−·49	−·12	+·37	+·50	+·15	−·40
3 30	−·48	+·23	+·15	+·14	−·05	−·34	−·14	+·07	+·23	+·36	−·08	−·51
4 30	−·02	+·30	+·01	+·20	+·03	−·21	−·07	+·24	+·27	+·19	−·24	−·20
5 30	−·07	+·04	+·05	−·00	+·03	+·05	+·20	+·17	+·07	−·07	−·26	−·03
6 30	+·05	+·01	−·34	−·45	−·10	+·27	+·39	+·16	−·19	−·52	−·41	+·01
7 30	+·17	−·12	−·40	−·48	−·07	+·44	+·59	+·11	−·09	−·47	−·32	+·26
8 30	+·35	−·06	−·28	−·16	+·10	+·41	+·46	+·28	−·26	−·30	+·01	+·37
9 30	+·44	−·08	−·29	−·07	−·14	+·13	+·16	−·10	−·24	−·34	+·17	+·30
10 30	+·12	−·19	+·03	+·07	−·07	−·04	−·02	−·20	−·24	−·04	+·09	+·23
11 30	+·02	+·09	+·16	+·16	+·01	+·02	−·06	−·10	+·02	+·15	+·21	+·24

TABLE (C. II.). The Remainders in Table (B. II.) corrected by Interpolation.

Periodical and non-periodical part of the Solar Effect, in *tenths of feet.*

☽'s Transit.	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.
h m												
0 30	− 1	0	+ 2	+ 2	0	− 3	− 5	− 3	0	+ 3	+ 4	0
1 30	− 3	0	+ 3	+ 3	0	− 4	− 6	− 3	+ 1	+ 4	+ 3	− 2
2 30	− 3	0	+ 3	+ 3	0	− 4	− 5	− 2	+ 3	+ 5	+ 2	− 4
3 30	− 3	0	+ 2	+ 2	0	− 3	− 3	− 1	+ 3	+ 4	+ 1	− 5
4 30	− 2	0	+ 1	+ 1	0	− 1	− 1	0	+ 2	+ 2	0	− 3
5 30	0	0	− 1	− 1	0	+ 2	+ 2	+ 1	− 1	− 2	− 2	− 1
6 30	+ 1	0	− 4	− 4	0	+ 4	+ 4	+ 2	− 2	− 5	− 4	+ 1
7 30	+ 2	0	− 5	− 5	0	+ 5	+ 6	+ 3	− 3	− 5	− 3	+ 2
8 30	+ 3	0	− 4	− 4	0	+ 4	+ 5	+ 3	− 3	− 3	− 2	+ 3
9 30	+ 4	0	− 2	− 2	0	+ 3	+ 3	+ 2	− 3	− 3	− 1	+ 3
10 30	+ 2	0	− 1	− 1	0	+ 1	0	0	− 2	− 2	+ 0	+ 2
11 30	0	0	+ 2	+ 2	0	− 1	− 2	− 2	0	+ 2	+ 2	+ 1
Non-periodical	0	−1	− 2	− 2	−2	− 2	− 1	− 1	0	+ 1	+ 1	+ 1

SOLAR TABLE (W. II.). Correction of the Heights for the Sun's Effect, in *tenths of feet.*

☽'s Transit.	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.
h m												
0 30	− 1	− 1	0	0	− 2	− 5	− 6	− 4	0	+ 4	+ 5	+ 1
1 30	− 3	− 1	+ 1	+ 1	− 2	− 6	− 7	− 4	+ 1	+ 5	+ 4	− 1
2 30	− 3	− 1	+ 1	+ 1	− 2	− 6	− 6	− 3	+ 3	+ 6	+ 3	− 3
3 30	− 3	− 1	+ 0	0	− 2	− 5	− 4	− 2	+ 3	+ 5	+ 2	− 4
4 30	− 2	− 1	− 1	− 1	− 2	− 3	− 2	− 1	+ 2	+ 3	+ 1	− 2
5 30	0	− 1	− 3	− 3	− 2	0	+ 1	0	− 1	− 1	− 1	0
6 30	+ 1	− 1	− 6	− 6	− 2	+ 2	+ 3	+ 1	− 2	− 4	− 3	+ 2
7 30	+ 2	− 1	− 7	− 7	− 2	+ 3	+ 5	+ 2	− 3	− 4	− 2	+ 3
8 30	+ 3	− 1	− 6	− 6	− 2	+ 2	+ 4	+ 2	− 3	− 2	− 1	+ 4
9 30	+ 4	− 1	− 4	− 4	− 2	+ 1	+ 2	+ 1	− 3	− 2	0	+ 4
10 30	+ 2	− 1	− 3	− 3	− 2	− 1	− 1	− 1	− 2	− 1	+ 1	+ 3
11 30	+ 0	− 1	0	0	− 2	− 3	− 3	− 3	0	+ 1	+ 3	+ 2

Table (B. T.). The Lunar Effects rejected.

Residual Quantities.—*Times.*

☽'s Transit.	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.
Means.........	+·2	+·9	−·5	−·6	−1·4	−·8	+·1	+·5	−·5	−·8	−1·0	−1·5
	Remainders.											
h m	m	m	m	m	m	m	m	m	m	m	m	m
0 30	−1·5	−1·6	− ·4	+1·0	+3·7	+2·2	− ·6	−2·0	+ ·9	+ ·9	+5·4	+1·9
1 30	−1·7	+2·3	+ ·4	+1·7	+2·3	− ·7	−3·3	−1·0	−6·5	+1·5	+2·2	−1·7
2 30	−1·2	−1·2	+2·4	+2·0	+1·0	−1·4	−3·9	+ ·1	+1·3	+1·1	+2·1	− ·7
3 30	−3·1	−2·6	+ ·8	+4·0	− ·5	−2·6	−1·5	−4·1	+2·0	+ ·5	−1·9	−2·8
4 30	− ·1	+ ·4	+1·8	− ·2	−2·6	−2·3	+2·0	+2·7	+1·8	+ ·5	−4·3	−2·6
5 30	+ ·6	+3·4	− ·3	−3·6	−4·8	−1·6	+1·1	+5·2	+ ·4	−3·0	−6·4	− ·9
6 30	+5·1	+2·0	−2·3	−3·6	−3·7	+1·2	+6·5	+2·9	+ ·6	−6·1	−3·6	+1·5
7 30	+3·9	−2·9	−2·2	−7·4	−1·0	+2·9	− ·9	+ ·1	−2·7	−3·2	− ·7	+1·9
8 30	−3·4	−1·5	+1·2	− ·4	+1·8	− ·2	−1·2	+ ·9	− ·2	+ ·9	+1·9	+2·2
9 30	−2·2	+3·1	+ ·3	+1·9	+ ·9	− ·0	−2·9	−1·1	+ ·8	+1·8	+1·8	·3
10 30	−2·7	+ ·1	− ·6	+2·8	+2·3	+ ·6	− ·5	−1·1	+ ·9	+3·5	+1·7	+1·2
11 30	+6·0	−1·4	−1·1	+1·6	+ ·6	+1·7	− ·9	−2·4	+ ·7	+1·1	+1·9	+ ·2

Table (C. T.). Periodical part of the Solar Effect.

The remainders in Table (B. T.) corrected by interpolation.

☽'s Transit.	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.
h m												
0 30												
1 30	−2	0	+2	+3	+2	0	−3	−4	−2	0	+2	0
2 30	−2	0	+3	+3	+2	0	−3	−2	0	+2	+2	0
3 30	−3	0	+3	+4	+3	0	−4	−4	0	+1	+1	0
4 30												
5 30	+3	0	−3	−4	−3	0	+4	+4	0	−4	−4	0
6 30	+4	0	−4	−5	−4	0	+5	+5	0	−5	−5	0
7 30	+4	0	−4	−5	−4	0	+2	+2	0	−3	−3	0
8 30												
9 30												
10 30												
11 30												

Solar Table (W. T.). Correction of the Times for the Sun's Effect.

For this correction we may use Table (C. T.), omitting the correction for the numbers there omitted till further investigations have made the Table more correct: and introducing also the interpolated nonperiodical correction, viz.

Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.
	m		m	m	m		m		m	m	m
0	+1	0	−1	−1	−1	0	+1	0	−1	−1	−1

XIII. *On the Action of Light upon Plants, and of Plants upon the Atmosphere.* By

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Received November 3,—Read December 17, 1835.

THE researches of PRIESTLEY, INGENHOUSZ, SENEBIER, ELLIS, and above all of the younger SAUSSURE, have long put us in possession of the leading facts appertaining to the influence of light upon the green parts of plants; and Professor DECANDOLLE has embodied the substance of all that had been ascertained on this subject, up to the year 1831, in his admirable work on Vegetable Physiology. But there appear, by the confession of this latter naturalist, to remain certain subordinate questions respecting this same function, which, though perhaps occasionally touched upon by the above-cited experimentalists and by others, can scarcely be said to have as yet obtained a satisfactory reply.

The first of these questions relates to the *nature* of the influence which, in the cases alluded to, is assignable to light. As this agent often produces chemical changes by its direct action upon inorganic bodies, decomposing saline solutions, discolouring oils, and reducing metallic oxides, so it may be supposed to operate directly upon the air, and to possess the power of decomposing carbonic acid, when this substance is presented to it within the pores of the vegetable tissue. And, on the other hand, as light appears to be a specific stimulus to the vital functions of animals, so it may be imagined to act in a similar manner on those of plants, thus enabling them to secrete from the carbonic acid presented to them the carbon required for their nutrition.

Another point as yet undecided relates to the *extent* of the influence it exerts over the vegetable kingdom; or, in other words, the degree in which certain processes attributed to its presence are capable of counteracting others that are going on at all times, whether light be absent or not. Thus, although it may be conceded, as a fact already well established, that plants purify the air in the sunshine, it still remained to be proved by more decided experiments than had hitherto been instituted, whether the quantity of oxygen given out by them during the day exceeded that absorbed during the night; and moreover, supposing this latter question answered in the affirmative, whether the probable excess was likely to be such, as would afford a counterpoise to the effects produced by animal respiration, combustion, and the like*.

After considering therefore the *mode* in which light appears to affect the functions of plants, I shall naturally proceed to examine the *extent* of the changes produced by the latter upon the air through its influence.

Part I.—*On the Influence of Light upon Plants.*

If of the two modes of considering the operation of light above noticed we adopt the second, that is, if light be supposed to affect plants by a specific stimulus, such as it exerts on animals, and not in the first instance the air as a chemical agent, it ought to follow, that those portions of the spectrum which possess the strongest illuminating power, should exercise upon them the most powerful influence, and produce the most decided effects.

Senebier, however, has stated, that the green colour of leaves, which is supposed to be connected with the decomposition of carbonic acid and the evolution of oxygen, is produced most rapidly under the action of the violet ray†; and as the latter, from the feeble light and heat it communicates, seems almost inert, with reference to the functions *of animals*, such a circumstance, if substantiated, would seem strongly to favour the contrary hypothesis.

This latter view of the mode in which carbonic acid is decomposed within the vessels of the plant, would likewise be somewhat confirmed, if it should appear, that whilst the above process was most favoured by violet light, other functions, which are affected by the presence of this agent, but which evidently depend upon a process taking place in the vessels of the plant, are influenced in proportion to the luminousness of the ray; whilst, if the same law were found to prevail in both these cases, and if all the processes alluded to could be proved to go on most rapidly under the influence of the darkest and most refrangible portion of the solar spectrum, a curious difference between the *mode* of its operation upon the vegetable and animal kingdoms might then be suggested.

* I am aware it may be urged, that the quantity, of carbonic acid added, and of oxygen subtracted by these latter means within any moderate period of time, is in itself so small compared with the entire bulk of the atmosphere, that we must not argue, because the constitution of the latter has appeared to continue uniform ever since accurate methods have been devised for determining it, therefore that no change can be taking place insensibly in the proportions of its ingredients.

Still, however, when we recollect, how many ages have elapsed since the present races of animals were created, and how many more since the existence of others, which, although extinct, appear from the analogies of their structure to have carried on the same respiratory process which those now in existence fulfill, and therefore could not have endured an atmosphere much more highly charged with oxygen than the present one, we cannot help feeling, that nature must have some means at her disposal, by which the purity of the atmosphere is restored, and its constitution thus maintained without alteration.

† Mém. de Phys. Chim., tom. ii. p. 55.

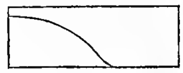
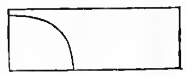
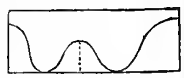
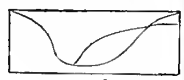
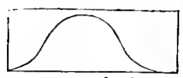
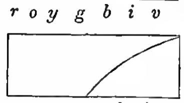
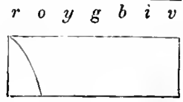
I felt therefore desirous of putting to the test of experiment the two following questions: 1st, whether the several solar rays act upon plants with equal or with different degrees of energy; and secondly, whether all the functions of plants that seem dependent on the presence of light are affected in the same ratio by similar rays.

Now the following functions are found to depend in great measure on the influence of light.

1. The decomposition of carbonic acid, and the consequent evolution of oxygen, already spoken of.
2. The green colour of leaves, and other analogous parts.
3. The expansion or unfolding of the leaves in certain species, the folding of which on the close of day constitutes what has been called "the sleep of plants."
4. The irritability belonging to certain other plants, such as the *Mimosa pudica.*
5. The exhalation of water from the leaves.
6. The absorption of the same by the roots.

The difficulty, however, of comparing the relative intensity of the light transmitted by the various coloured media, which were employed in my experiments, induced me to content myself with showing, that the effect of light upon plants corresponds with its illuminating, rather than with its chemical, or its calorific influence; and to wave the more difficult inquiry, whether its operation upon the vegetable kingdom exactly keeps pace with the increase in its own intensity. And in order to show that the former is the case, I will in the first place set down the order of sequence of the several media, with reference one to the other, in respect to their illuminating, their calorific, and their chemical power; stating at the same time, by means of a diagram, the rays intercepted and transmitted by each, as was determined for me by Professor Powell; and afterwards proceed to a statement of their respective influence upon plants.

With regard to the means adopted for estimating these points, I need perhaps only remark, that the relative illuminating power of the several media was ascertained by the number of thicknesses of wire gauze, which produced a certain definite degree of obscurity or indistinctness when interposed; their relative calorific power, by the number of degrees which a thermometer with a blackened bulb was raised in a given time by the light transmitted through each; their relative chemical influence, by the time required to reduce paper moistened with a solution of nitrate of silver to a certain standard point of discoloration. The figures in the annexed Table therefore must be understood to express nothing more than the *order of sequence,* and not to indicate the ratio between the several media in any of the above respects.

Nature of the media.	Its type.	Its illuminating influence.	Its calorific influence.	Its chemical influence.
Transparent glass	. . . .	7	7	7
Orange, No. 5.	 r y g b	6	6	4
Red, No. 4.	 r y g	4	5	0
Blue, No. 3.	 r y g	4	3	6
Purple, No. 2.	 r y g b v	3	4	6
Green, No. 1.	 r o y g b i v	5	2	3
Bottle containing a solution of ammonio-sulphate of copper, No. 6.	 r o y g b i v	2	1	5
Bottle containing port wine, No. 7	 r	1	3	0

Now it remained to be seen, with which of the above scales the power of occasioning a decomposition of carbonic acid in the vessels of the plant, and of forwarding those other functions of vegetable life which depend upon light, most nearly corresponded.

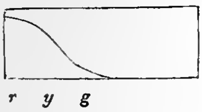
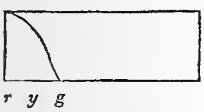
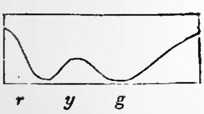
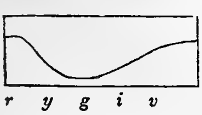
For this purpose, a certain number of fresh leaves, which presented in each case an extent of surface as nearly as possible equal, and had been previously ascertained to give out equal quantities of oxygen, were introduced severally into jars, filled with water impregnated with carbonic acid gas, placed on the surface of a pneumatic trough, and exposed for a certain time to the influence of the solar rays.

The jars, in which the leaves thus selected stood, were severally covered over by a wooden screen, which intercepted all light from the included jar, excepting in front, where a frame was fitted to it of a nature calculated to support, either a circular pane of glass, or a flat bottle of corresponding dimensions.

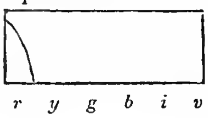
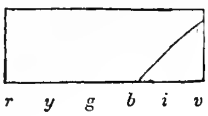
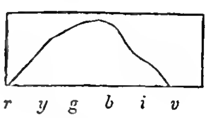
By fixing into the frame the various coloured media with which it was intended to operate, I was enabled to ascertain the influence which the light transmitted through each might exert upon the plant included.

From a variety of experiments, which it seems unnecessary to detail, in as much as they merely tend to confirm the statements of preceding observers, it appeared, that the kind of leaf selected made but little difference in the result. I therefore contented myself with selecting such, as could be procured most readily, and in the freshest condition.

The first plant operated on was the common Cabbage (*Brassica oleracea*) ; and as some of the results obtained in this instance appeared anomalous, in as much as they indicated an evolution of pure nitrogen, which I have detected in none of my other experiments, three or more trials with each kind of glass were in this instance had recourse to. The following were the results obtained.

Media through which the light was transmitted.	State of the weather.	Proportion between the whole quantity of gas obtained in the two jars.	Proportion per cent. between the oxygen and nitrogen in the two jars.	Proportion between the oxygen in the two jars.	Proportion between the nitrogen in the two jars.
Jar 1. Transparent window-glass Jar 2. Glass No. 5. (Orange.) Type 	*Exp.* 1. Bright sunshine.	Jar 1. 100 Jar 2. 80	O. 44 N. 56 O. 40 N. 60	100 77·5	100 86
	Exp. 2. Bright sunshine.	Jar 1. 100 Jar 2. 82	O. 33 N. 66 O. 33·4 N. 66·6	100 80	100 83
	Exp. 3.* Bright sunshine.	Jar 1. 100 Jar 2. 83	O. 0 N. 100 O. 0 N. 83		
Jar 1. Same as before. Jar 2. Glass No. 4. (Crimson-coloured.) Type 	*Exp.* 1. Bright sunshine with a few fleecy clouds.	Jar 1. 100 Jar 2. 45	O. 37·5 N. 62·5 O. 33·3 N. 66·6	100 40	100 48
	Exp. 2. Strong bright sunshine.	Jar 1. 100 Jar 2. 54	O. 39 N. 61 O. 38 N. 62	100 52·5	100 55
Jar 1. Same as before. Jar 2. Glass No. 3. (Dark blue.) Type 	*Exp.* 1. Sunshine with partial clouds.	Jar 1. 100 Jar 2. 57·5	O. 33 N. 66 O. 34·6 N. 65·4	100 56	100 56·5
	Exp. 2. Bright sunshine without clouds.	Jar 1. 100 Jar 2. 57·5	O. 33·3 N. 66·6 O. 30·2 N. 69·8	100 70·5	100 53·5
Jar 1. Same as before. Jar 2. Glass No. 2. (Purple.) Type 	*Exp.* 1. Sunshine without clouds.	Jar 1. 100 Jar 2. 47	O. 38·4 N. 61·6 O. 30 N. 70	100 35·4	100 51
	Exp. 2. Sunshine more obscured than in Exp. 1.	Jar 1. 100 Jar 2. 31·4	O. 25 N. 75 O. 22 N. 78	100 27·7	100 26·6

* N.B. The leaves in this latter case were quite fresh, and had not been previously immersed in water.

Media through which the light was transmitted.	State of the weather.	Proportion between the whole quantity of gas obtained in the two jars.	Proportion per cent. between the oxygen and nitrogen in the two jars.	Proportion between the oxygen in the two jars.	Proportion between the nitrogen in the two jars.
Jar. 1. Same as before. Jar. 2. Bottle No. 7. Filled with port wine. Type 	*Exp*. 1 & 2. Sunshine of different degrees of brightness.	Jar 1. 100 Jar 2. No Gas.			
Jar 1. Same as before. Jar 2. Bottle No. 6. Filled with the ammonio-sulphate of copper. Type 	*Exp*. 1. Sunshine of feeble intensity, though without dense clouds.	Jar 1. 100 Jar 2. 18	O. 47 N. 53 O. 37·8 N. 62·2	100 14·5	100 20·6
	Exp. 2. Bright and cloudless sunshine.	Jar 1. 100 Jar 2. 49	O. 32·7 N. 67·3 O. 37 N. 63	100 55	100 46
Jar 1. Same as before. Jar 2. Glass No. 1. (Green.) Type 	*Exp*. 1. Bright sunshine with occasional clouds.	Jar 1. 100 Jar 2. 32·8	O. 44 N. 56 O. 34·7 N. 65·3	100 26	100 38
	Exp. 2. Sunshine, for the most part bright.	Jar 1. 100 Jar 2. 25	O. 34 N. 66 O. 26·7 N. 73·3	100 18	100 25·6
	Exp. 3. Sunshine feeble and intermitting.	Jar 1. 100 Jar 2. no gas collected.			

The *second series* of experiments undertaken was with the leaves of the *Salicornia herbacea*, and the following were the results obtained. Temperature 65° FAHR.

Media through which the light was transmitted.	State of the weather.	Proportion between the whole quantity of gas obtained in the two jars.	Proportion per cent. between the oxygen and nitrogen in the two jars.	Proportion between the oxygen in the two jars.	Proportion between the nitrogen in the two jars.
Jar 1. Transparent glass .... Jar 2. No. 5. Glass (Orange). .	Feeble sunshine.	Jar 1. 100 Jar 2. 67	O. 25 N. 75 O. 16·4 N. 83·6	100 44	100 75
Jar 1. Transparent .... Jar 2. No. 4. (Red)..........	Feeble sunshine.	Jar 1. 100 Jar 2. 60	O. 10 N. 90 O. 0 N. 60	100 0	100 66·5
Jar 1. Transparent .... Jar 2. No. 3. (Blue)	Feeble sunshine.	Jar 1. 100 Jar 2. 20	O. 25 N. 75 O. 0 N. 20	100 0	100 26·7

Media through which the light was transmitted.	State of the weather.	Proportion between the whole quantity of gas obtained in the two jars.	Proportion per cent. between the oxygen and nitrogen in the two jars.	Proportion between the oxygen in the two jars.	Proportion between the nitrogen in the two jars.
Jar 1. Transparent .... Jar 2. No. 2. (Purple)........	Bright sunshine, with a few clouds.	Jar 1. 100 Jar 2. 16	O. 46 N. 64 O. 15 N. 85	100 5·25	100 21·4
Jar 1. Transparent .... Jar 2. No. 7. Bottle filled with port wine........	Bright sunshine.	Jar 1. 100 Jar 2. 0			
Jar 1. Transparent .... Jar 2. No. 6. Bottle (with ammonio-sulphate of copper)	Bright sun, though with a few clouds.	Jar 1. 100 Jar 2. 42	O. 33 N. 66 O. 0 N. 42	100 0	100 63
Jar 1. Transparent .... Jar 2. No. 1. Glass (Green) ..	Bright sunshine.	Jar 1. 100 Jar 2. 43	O. 46 N. 64 O. 0 N. 43	100 0	100 67

The *third series* undertaken was with common Sea Wrack (*Fucus digitatus*), immersed in water of temperature 65°, and the following were the results obtained.

Media through which the light was transmitted.	State of the weather.	Proportion between the whole quantity of gas obtained in the two jars.	Proportion per cent. between the oxygen and nitrogen in the two jars.	Proportion between the oxygen in the two jars.	Proportion between the nitrogen in the two jars.
Jar 1. Transparent glass .... Jar 2. No. 5. (Orange)	*Exp.* 1. Feeble sunshine.	Jar 1. 100 Jar 2. 31	O. 55·5 N. 44·5 O. 14·2 N. 85·8	100 7·9	100 59
	Exp. 2. Bright sunshine.	Jar 1. 100 Jar 2. 19·2	O. 60 N. 40 O. 44 N. 56	100 14	100 27
Jar 1. Transparent .... Jar 2. No. 4. (Red)	Bright sunshine.	Jar 1. 100 Jar 2. 10·8	O. 75 N. 25 O. 46 N. 54	100 6·65	100 23
Jar 1. Transparent .... Jar 2. No. 3. Glass (Blue)....	*Exp.* 1. Bright sunshine.	Jar 1. 100 Jar 2. 13	O. 69 N. 31 O. 31·5 N. 68·5	100 6	100 28·7
	Dull day.	Jar 1. 100 Jar 2. 7·3	O. not determined. O. 0 N. 7·3		
Jar 1. Transparent .... Jar 2. No. 2. Glass (Purple) ..	Feeble sunshine.	Jar 1. 100 Jar 2. 12·5	O. 60 N. 40 O. 8·8 N. 91·2	100 1·8	100 28·5
Jar 1. Transparent .... Jar 2. No. 6. Bottle (containing a solution of ammonio-sulphate of copper)	Bright sunshine.	Jar 1. 100 Jar 2. 8	O. 70 N. 30 O. 19 N. 81	100 2	100 21·7
Jar 1. Transparent .... Jar 2. No. 1. (Green)	*Exp.* 1. Weak sunshine.	Jar 1. 100 Jar 2. 7·6	O. 40 N. 60	100	100 7·6
	Exp. 2. Strong sunshine.	Jar 1. 100 Jar 2. 5·2	O. 65 N. 35	100	100 5·2

Bottle No. 7 with port wine tried without effect.

The *fourth series*, with leaves of *Tussilago hybrida*, in water of temp. 70°, gave the following results.

Media through which the light was transmitted.	State of the weather.	Proportion between the whole quantity of gas obtained in the two jars.	Proportion per cent. between the oxygen and nitrogen in the two jars.	Proportion be tween the oxygen in the two jars.	Proportion between the nitrogen in the two jars.
Jar 1. Transparent glass. Jar 2. Glass No 5. (Orange.)	Sultry day.	100 56	O. 45 N. 55 O. 41 N. 59	100 51	100 60
Jar 1. Transparent. Jar 2. Glass No. 4. (Red.)	Ditto.	100 49	O. 44·5 N. 55·5 O. 61 N. 59	100 67·5	100 39
Jar 1. Transparent. Jar 2. Glass No. 3. (Blue.)	Ditto.	100 41	O. 62 N. 38 O. 33 N. 66	100 22	100 72
Jar 1. Transparent. Jar 2. Glass No. 2. (Purple.)	Ditto.	100 10·7	O. 54 N. 46		
Jar 1. Transparent. Jar 2. No. 1. (Green.)	Ditto.	100 14	O. 53 N. 47 O. none, or barely any.		
Jar 1. Transparent. Jar 2. Bottle No. 6. containing the copper solution.	Ditto.	100 21	O. 50 N. 50 O. 13 N. 87	100 5·5	100 20·5

Fifth Series, with leaves of *Cochlearea Armoracia* immersed in water having a temperature of 72°, gave the following results.

Media through which the light was transmitted.	State of the weather.	Proportion between the whole quantity of gas obtained in the two jars.	Proportion per cent. between the oxygen and nitrogen in the two jars.	Proportion between the oxygen in the two jars.	Proportion between the nitrogen in the two jars.
Jar 1. Transparent glass .... Jar 2. No. 5. (Orange)	Bright sunshine. Th. 80°.	100 75	O. 57 N. 43 O. 54 N. 46	100 71	100 80
Jar 1. Transparent .... Jar 2. No. 2. (Blue)	Ditto.	100 24	O. 57 N. 43 O. 27 N. 73	100 11·4	100 40
Jar 1. Transparent .... Jar 2. No. 5. (Green)........	Ditto.	100 14·5	O. 57 N. 43 O. 20 N. 80	100 5·25	100 26·8
Jar 1. Transparent .... Jar 2. No. 4. (Red)..........	Ditto.	100 15	Not measured. O. 31 N. 69		
Jar 1. Transparent .... Jar 2. No. 3. (Purple) ...... Jar 3. No. 1. (Green)........ Jar 4. No. 6. (Bottle with copper solution) ..	Ditto.	100 36 20 12	O. 52 N. 48 O. 35 N. 65 O. 16 N. 84 O. 5 N. 95	100·0 24·2 6·2 1·2	100 48 35·2 22·5

N.B. In another experiment with No. 6. no gas at all was collected.

Sixth series, with sprigs of *Mentha viridis* immersed in water of the temperature of 74° Fahr.

Media through which the light was transmitted.	State of the weather.	Proportion between the whole quantity of gas obtained in the two jars.	Proportion per cent. between the oxygen and nitrogen in the two jars.	Proportion between the oxygen in the two jars.	Proportion between the nitrogen in the two jars.
Jar 1. Transparent glass	Cloudless	100	O. 59 N. 41	100	100
Jar 2. No. 5. (Orange)	sky.	70	O. 37 N. 63	44	108
Jar 3. No. 2. (Blue)	Thermo-	22·5	O. 8 N. 92	30	50·5
Jar 4. No. 6. (Bottle with copper solution)	meter 80°.	20·0	O. 4 N. 96	13·5	47·0
Jar 1. Transparent glass	Ditto.	100	O. 47 N. 53	100	100
Jar 2. No. 1. (Green)........		20	O. 7 N. 93	3	34
Jar 3. No. 3. (Purple)		20	O. 0 N. 100	0	40
Jar 4. No. 4. (Red)		30	O. 12 N. 88	7·7	49

Similar experiments made upon leaves of *Rheum Rhaponticum*, of *Allium ursinum*, and of various species of Meadow-grass, corroborated the same conclusions; as likewise did some on plants confined in atmospheric air, containing about six per cent. of carbonic acid, and exposed to these several media.

In the above experiments the proportion of oxygen was ascertained, by heating the air in a bent graduated tube with phosphorus, and observing the diminution of capacity thereby occasioned; two per cent. being allowed for the expansion caused in nitrogen by phosphorous vapour. This method, which I have always found to give very uniform results, I adopted in preference to that of exploding the gas with hydrogen, as being less troublesome and more expeditious, in a climate so damp as ours, than a process requiring the aid of electricity.

The constant presence of more or less nitrogen in the air emitted by the plant, is a circumstance which, although often before observed, deserves, nevertheless, here to be briefly adverted to.

Its quantity appeared to be relatively smaller in proportion to the intensity of the solar influence, being always least under transparent glass; and where the light transmitted was not energetic enough to cause any emission of oxygen at all, still some portion of nitrogen would frequently be given out. Perhaps this circumstance may admit of explanation, by considering the emission of gas from leaves, when exposed to light under water, as derived from two sources; the first, the disengagement of a portion of atmospheric air which it had previously absorbed, and whose place within the tissue of the plant is probably supplied, either by the water with which it is surrounded, or by the carbonic acid with which this water is impregnated; the latter, the emission of pure oxygen, derived from a decomposition of the carbonic acid in contact with it.

Hence in Experiment 1. with cabbage-leaves, where we obtained 100 parts of a gas consisting of oxygen 44, and nitrogen 56 parts, we may suppose that the leaves had emitted,

Of atmospheric air which had been previously absorbed	68 parts: consisting of	nitrogen	56
		oxygen	12
together with excess of oxygen . .	32	Excess of oxygen . .	32
Total of gas obtained	100	Total of oxygen . .	44

whereas, when orange-coloured glass No. 5. was employed, we obtained,

Of atmospheric air	60·5	Oxygen	12·6
Excess of oxygen	19·4	Excess of oxygen .	19·4
Total of gas . .	79·9	Total of oxygen .	32·0

The two most difficult cases to explain seem to be, first, the evolution of pure nitrogen, and secondly, that of the same gas accompanied with a smaller proportion of oxygen than that present in the atmosphere.

In the instance in which the former was observed, no incipient putrefaction could be suspected by way of accounting for its occurrence, for the plants were fresh and healthy; and the circumstance that gas is not disengaged at all in the dark, proves the evolution of nitrogen to be in both cases a process connected with the same kind of action as that to which the emission of oxygen is to be ascribed.

Perhaps the phenomenon may be better understood by reference to the experiments of the younger Saussure, which go to prove; that oxygen becomes fixed in the plant in a condition, such as renders it incapable of being withdrawn from the vegetable tissue by the air-pump, or by other mechanical means; that it there unites with the carbon, so as to bring the latter into a fit state for the plant to assimilate it; and that it is then again disengaged from its combination, by a process not unaptly compared by the late Professor Burnett to the digestion of animals.

Now for this latter function to be discharged, the stimulus of the more luminous rays may be requisite, whilst that of the duller portions of the spectrum may suffice for the mere respiration of the plant, or for the elimination of the residuary air. Hence when rays of the latter description are alone transmitted, the composition of the gas evolved may even indicate a smaller amount of oxygen than that present in atmospheric air, because a portion of this element had become combined with certain of the carbonaceous principles present in the vegetable tissue, or been fixed in some manner within the plant.

The other processes, enumerated as under the influence of solar light, appear to be subjected to the same law, as that by which we have seen the decomposition of carbonic acid in the green parts of plants to be regulated.

From a few experiments I have made on the secretion of green matter in the leaves, I should be led to infer, in contradiction to the results of Senebier, that the most luminous rays were most influential; the orange glass, whose chemical influence was as 4, whilst its illuminating power was as 6, quickly imparting to the primordial leaves of beans which had just appeared above ground a bright green hue, whereas under the ammonio-sulphate, whose illuminating power was as 2, whilst its chemical influence was as 5, they continued of a pale yellow, scarcely indeed of a shade darker than in another case where light was completely excluded.

I have made some experiments with similar results on the colours of flowers, the intensity or depth of which appeared also to depend on the brightness of the kind of light that had been allowed admission to them.

The irritability of the Sensitive Plant was likewise found to be dependent on the influence of bright rays, and not of those which act chemically. Six healthy sensitive plants were introduced in the beginning of August into an oblong box, with partitions so arranged, that each pot was in contact with a differently coloured light. In five weeks' time, that which had been exposed to the full light of the sun, transmitted through transparent glass, was still excitable, as was the one covered with the orange-coloured; but those which had received merely the portions of light transmitted through the copper solution, through port wine, and through a solution of green muriate of copper, as well as one which had been kept in entire darkness, lost all their irritability. Yet in each case the temperature was kept up to the same point by means of a hot-bed.

The exhalation of moisture from the leaves, and the absorption of it by the roots, are the last processes dependent on the action of light to which my attention has been directed. The results of my experiments on these two points confirm the same general inference as that to which the foregoing ones point; but having met with some apparent anomalies, I shall forbear at present to report the numerical results of the respective trials made with various glasses. It will be sufficient to lay before the Society a statement of the plan on which the experiments were conducted, and to particularize one or two which tend to show, that the processes above alluded to are probably dependent on the combined action of heat and light, coupled with those mechanical influences, which operate upon dead, as well as upon living organic matter.

The method whereby I proposed to estimate the degree, in which the exhalation of moisture depended on the quality of the light admitted to the plant, was in itself sufficiently simple. It consisted, in placing some plant growing in a pot, in a square tin vessel, the margin of which received in a groove a cucumber frame of sufficiently large dimensions to inclose it. All communication with the external air was cut off, by means of a little oil introduced into the groove into which the edges of the frame dipped, and the moisture exhaled was absorbed by concentrated sulphuric acid, placed in shallow earthen pots, along with the plant, in the interior of the tin vessel.

By weighing these vessels, just before the plant was introduced, and immediately after it had been taken out, I hoped to ascertain the amount of water that had been evolved, and after deducting from the sum total the quantity which had been previously found to be given off by the plant in the dark, I concluded that the remainder ought to represent the quantity to be set down to the action of light. But as it was impossible to command an uniform intensity of solar radiation during the whole period occupied by any one series of experiments, another plant of the same kind and size was placed under transparent glass; and from the comparative amount of moisture emitted by it, I calculated what might be the difference in the amount of solar influence during the period at which the experiments were carried on.

Now although the experiments conducted on the above plan in general tended to show, that the extrication of moisture, *cæteris paribus*, was most abundant in proportion to the intensity of the light admitted, (orange glass in general causing more moisture to be exhaled than red or green,) yet in some instances blue and purple glasses, and still more remarkably, bottles filled with the cupreous solution, would cause a more abundant exhalation than orange or even transparent glass. Here, however, another principle seems to come into play, namely, the influence of heat radiated from the surface of the screen. This I infer, first, because the quantity of water exhaled under the influence of the copper solution became greatest, when the state of the weather was such as to elevate the temperature of the liquid considerably above that of the surrounding atmosphere; and, secondly, because a bottle filled with water blackened with ink to such a degree, as to transmit just as much light, so far as could be measured by the eye, as that filled with the copper solution was found to do, caused an equally considerable amount of water to be evolved by the plant.

Thus I selected two plants of the Tree Mallow (*Lavatera arborea*), which, by a previous experiment, had been found to exhale in the open air equal quantities of moisture, and placed the one under a frame, into which were inserted the bottles of ink and water, and the other under one with the solution of ammonio-sulphate of copper. Both fluids soon acquired in the sun a temperature from 110° to 120° Fahr.; and at the end of two hours the sulphuric acid in connexion with each of the plants was successively weighed, and the increase found to be nearly uniform; that under the ink and water having gained 150 grains per hour, that under the cupreous solution 162 grains in the same time.

Now as water with the addition of a little ink is known to absorb the rays proceeding from all parts of the spectrum in an equal ratio, it follows, that the effect produced in either instance must be ascribed to the heat radiated, and not to any peculiar virtue of the violet extremity in stimulating the vegetative functions.

Yet it is curious that the presence of some light seems essential to the due continuance of this process. The same plants which had been employed in the preceding experiment were placed out in the sun on a bright day; one, as before, under the influence of the light transmitted through the cupreous solution, the other under a frame covered over with blue tiles, which, together with the liquid, soon became heated by the sun's rays to the temperature of 110° or 120° of Fahrenheit. At the end of a certain time the sulphuric acid contained in the tin vessel which had inclosed the plant exposed to the action of the violet end of the spectrum had gained at the rate of 159 grains in the hour, (which is within 3 grains of the amount obtained in the previous experiment,) whilst that in connexion with the one covered with the tiles had only increased by 32 grains.

Thus it would appear, that although heat assists the process, some degree of light is essential to its activity.

I was desirous, likewise, of ascertaining whether the brightest kind of light attainable by artificial means contributed in any degree to the process under consideration; Professor DECANDOLLE having found that the leaves of plants placed in a cellar became green on exposure to a strong light from lamps, and that their flowers even reversed their natural periods of opening, when the cellar was illuminated by night, and kept dark during the day.

In my own experiments, the light employed was that produced by a jet of mixed oxygen and hydrogen directed upon a ball of quick-lime, a kind of light, which I have found capable, like that from the sun, of passing through, and being concentrated by, a lens. Nevertheless, in two or three experiments, each lasting nearly an hour, in which the rays proceeding from the incandescent lime were directed towards, and thrown back upon the plant, by concave metallic reflectors, no increase in the quantity of moisture exhaled could be detected, beyond what the same individual had given out whilst in the dark.

The last function which it was left for me to consider, namely, the absorption of water by the roots, is so related to the preceding one, that it might almost be inferred *à priori* to be subject to the same laws.

It seems indeed evident, that, *cæteris paribus*, in proportion to the velocity with which the sap ascends, will the extremities of the roots absorb moisture from the ground; since, unless the former operation continued, the latter organs would very soon become fully charged with humidity, and thus the absorption be put a stop to.

In order to ascertain the quantity of water absorbed under different circumstances by the roots of plants, I made the following experiments.

Two small plants of *Helianthus annuus*, in pots marked A and B, were immersed in tin vessels nearly full of water, the height of which within was measured by glass tubes cemented into them below, and rising on the outside nearly to the top. These vessels were severally provided with tin covers, each of which had a circular aperture at its centre, through which the stem of the plant passed, and another small one at the side, through which water might be introduced. Being elsewhere closely attached to the vessel, little or no evaporation could take place from the surface of the included water.

Things being thus prepared, the two plants were placed for twenty-four hours in the open air, part of the time exposed to a bright sun, with the thermometer at 75° in the shade.

At the expiration of that time it was found that the vessels had lost, as nearly as could be ascertained, the same quantity of water, which amounted in each case to four ounces.

The next day, the thermometer being as on the preceding occasion, and the sun bright, sunflower A was placed under a frame glazed with orange-coloured glass the same as No. 5, and sunflower B under one glazed with blue glass as No. 2.

In the evening it was found, that the tin vessel containing plant A had lost three ounces of water, and that containing B one ounce. Now in another experiment with the same plants, the tin containing A, placed under a frame glazed with blue glass No. 2, had lost six ounces; that containing B, under one glazed with orange, No. 5, had lost $9\frac{1}{2}$. So that although the ratio between the two was very different (being in the one case as 1 to 3, in the second as 1 to $1\frac{1}{2}$), still in both there was a manifest superiority in the orange over the blue glass with respect to its power of producing absorption. Now orange-coloured glass seemed to act with about half the energy which belonged to transparent; for in another experiment upon the same plants, whilst the *former* had caused an absorption of four ounces, the *latter* had occasioned one of only two.

The same plants being next tried, one with a covering of transparent, the other with one of red glass, it was found that the former had absorbed $4\frac{1}{2}$, the latter 2 ounces, in equal times, the ratio being as 2·00 to ·89.

Hence the following may be stated as representing the relative amount of absorption in these several cases:

Under transparent	2·00
Under orange	1·00
Under blue, varying from	·33 to ·66
Under red	·89

Similar experiments were tried with Vines, and with the *Sagittaria sagittifolia*, which latter being an aquatic plant, continued for a longer period in a healthy condition when immersed in the water. But in either instance the same exception was found with respect to the influence of light on the rate of absorption, as had been observed in that of exhalation, those glasses which radiated most heat, appearing to act upon the plant with an energy quite disproportionate to their illuminating power.

The heat of the weather was very great during these experiments, the thermometer being frequently above 80°, and the liquor in the bottles often mounting as high as 105°, so that it would seem, that the heat radiated from the coloured liquid assisted in promoting absorption in the roots, as it appeared to have done in the former one in increasing the exhalation from the leaves. The same might have been the case, though in a lesser degree, when the blue or red glasses were the media employed, and thus certain irregularities observed in the results may perhaps be explained, by supposing that the joint action, of the light transmitted, and of the heat radiated from these screens, caused a greater exhalation from the leaves, and thereby produced a more abundant absorption by the roots to supply the deficiency.

Upon the whole, then, I am inclined to infer, from the general tenor of the experiments I have hitherto made, that both the exhalation and the absorption of moisture by plants, so far as they depend upon the influence of light, are affected in the greatest degree by the most luminous rays, and that all the functions of the vegetable economy, which are owing to the presence of this agent, follow in that respect the same law. And this is just what we ought to expect, if we suppose that light acts upon the vegetable, as it does upon the animal kingdom, in the character of a specific stimulus; for we all are sensible, that the vivifying influence of light upon ourselves is in proportion to its brightness; nor is it uninteresting to remark, that rays of the very description which most abound in solar light, are at once the most cheering to the animal creation, and the most conducive to the growth and well-being of the vegetable.

We have already seen, that even that most intense form of artificial light which is emitted from incandescent lime, produces no sensible influence upon plants; and we are reminded, that the same holds good with respect to animals, by the fact said to have been observed by the exhibitors of the oxyhydrogen microscope, namely, that animalcules of kinds which used to be speedily destroyed by the too stimulating action of solar light, appear to suffer much less from that now substituted, provided the water in which they are immersed does not become heated thereby.

Part II. *On the Action of Plants upon the Atmosphere.*

Having now considered the mode in which light may be supposed to operate upon plants, I shall next proceed to examine the extent of the changes wrought in the constitution of the atmosphere by the latter, under the influence of this agent.

I say the extent of the changes produced, because no one denies the nature of this operation, or questions the fact, that carbonic acid is really decomposed, and oxygen given out, by the green parts of plants under certain circumstances. But between the original views of Priestley, who saw in this process a counterpoise to the effects of animal respiration and the like, and those of Ellis, who did not admit it even as an equivalent to the opposite tendency of vegetable respiration, as carried on during the absence of solar light, a wide difference exists, and hence some fresh investigations seem necessary with reference to this question.

On perusing the account, which Mr. Ellis, in the Second Part of his Researches on Air, has given of the experiments by which he attempts to establish this latter opinion, several circumstances occurred to me, of a nature calculated to throw doubts upon their conclusiveness. I may mention, for example, in the first place, the smallness of the volume of air in which his plants were confined; in the second, the length of time that was suffered to elapse before the air underwent examination, owing to which it is even stated occasionally that the leaves had begun to fade and drop off; in the third place, the removal in some cases, and the neglect of a due supply in all, of that carbonic acid, the decomposition of which would have constituted the very source of the oxygen which it was expected to discover.

Accordingly I kept constantly in view these three essential conditions, and contrived an apparatus, in which the quantity of air should be so large, that the healthy functions of the plant might be as little as possible interfered with; in which the constitution of the air could be examined as frequently as I pleased; and in which a regular supply of carbonic acid could be kept up, without disturbing the plant, or suspending the progress of the experiment by its introduction.

The apparatus consisted of a large bell-glass jar, containing in one case 600, in the other 800 cubic inches of air*, and suspended by pulleys. Its edges dipped into quicksilver, contained in a double iron cylinder of corresponding dimensions to the jar, which being closed at bottom, constituted a well of about six inches in depth, calculated to receive a fluid, and to admit of the glass vessel moving freely in it. The inner margin of this hollow cylinder was cemented air-tight, according to circumstances, either to a plate or a pot of iron, upon which the plant operated on might be placed; and the jar was then let down upon it, until its edges were sunk a little beneath the surface of the mercury.

Thus all communication with external atmosphere was cut off, and the effect of the plant upon the air inclosed in the jar was readily measured, by simply pressing down the latter, and thus expelling a portion of its contents through a tube, communicating with its interior, and introduced at its outer extremity under a pneumatic trough, wherein the air might be collected and examined. By connecting this extremity with a vessel containing a measured quantity of carbonic acid, and raising the jar a little in the well of mercury, it was easy to draw in any proportion of that gas, with which it was thought proper that the plant should be supplied. A portion of the air was always tested, immediately after the introduction of every fresh portion of carbonic acid, and again after an interval of some hours, and the proportion of this gas and of oxygen present was carefully registered. The amount of carbonic acid was determined by a solution of potass, that of oxygen by the rapid combustion of phosphorus with a portion of it in a bent tube.

Such was the mode of procedure, when an entire plant became the subject of experiment; but some of the most satisfactory trials were with branches of certain shrubs, themselves too large to be admitted under the jar. These branches, without being detached from the parent trunk, were introduced through a hole in the centre of two corresponding semicircular plates of iron, which were cemented air-tight, to the inner margin of the iron cylinder on the one hand, and to the stem of the branch on the other. In this manner, when the jar came to be placed over them, and to dip beneath the surface of the mercury, the external air was as effectually excluded, as it had been when the whole of the plant was inclosed.

The results of several experiments conducted after this plan will be given in a tabular form; but it may be well in the first instance to specify one of the most satisfactory of those undertaken. In this case the jar itself contained about 600 cubic inches of air, and the plant experimented on was the common Lilac (*Syringa vulgaris*).

* Larger jars, containing from 1200 to 1300 cubic inches, were latterly employed.

The proportion of carbonic acid in the jar was each morning made equivalent to 5 or 6 per cent. by additions through the tube.

The first day no great alteration in the air was detected, but on the second day, by eight in the evening, the oxygen had risen to 26·5 per cent. In the morning it had sunk to 26, but by 2 P.M. it had again risen to no less than 29·75, and by sunset it had reached 30 per cent. At night it sunk one half per cent.; but the effect during the following day was not estimated, as the sickly appearance which the plant now began to assume induced me to suspend the experiment.

In a second trial, however, the branch of a healthy Lilac growing in the garden was introduced into the same jar, where it was suffered to remain until its leaves became entirely withered.

The first day the increase of oxygen in the jar was no more than 0·25 per cent., but on the second it rose to 25·0. At night it sunk to nearly 22 per cent., but the next evening it had again risen to 27 per cent. This was the maximum of its increase, for at night it sunk to 26, and in the morning exhibited signs of incipient decay. Accordingly in the evening the oxygen amounted only to 26·5; the next evening to 25·5; the following one to 24·75; and the one next succeeding it had sunk to the point at which it stood at the commencement, or to 21 per cent.

The reason of this decrease was, however, very manifest in the decay and falling off of the leaves; so that this circumstance does not invalidate the conclusion which the preceding experiments concur in establishing, namely, that in fine weather, at least so long as the plant continues healthy, it adds considerably to the oxygen of the air when carbonic acid is freely supplied.

In the last instance quoted, the exposed surface of all the leaves inclosed in the jar, which were about fifty in number, was calculated at no more than three hundred square inches, and yet there must have been added to the air of the jar as much as twenty-six cubic inches of oxygen, in consequence of the action of this quantity upon the carbonic acid introduced.

But there is reason to believe, that even under the circumstances above stated (which were more favourable to the due performance of the functions of the plant than those to which Mr. Ellis's were subjected,) the amount of oxygen evolved was much smaller than it would have been in the open air, for by introducing several plants into the same jar of air in pretty quick succession, I have succeeded in raising the amount of oxygen contained from 21 to 39 per cent., and probably had not even then attained the limit to which the increase of this constituent might have been brought.

How great then must be the effect of an entire tree in the open air under favourable circumstances! and we must recollect that, *cæteris paribus*, the circumstances will be favourable to the exertion of the vital energies of the plant, within certain limits at least, in proportion as animal respiration and animal putrefaction furnish to it a supply of carbonic acid.

Neither is this influence exerted exclusively by plants of any particular kind or description. I have found it alike in the monocotyledonous and the dicotyledonous, in such as thrive in sunshine and those which prefer the shade, in aquatic as well as in terrestrial, in cryptogamous and imperfect, such as Ferns and Algæ, as well as in those of a more complicated organization. How low in the scale of vegetable life this power extends is not yet exactly ascertained, but Professor Marcet of Geneva, in a late paper, has shown that it does not prevail amongst Fungi.

The disappearance of carbonic acid in my experiments always more than kept pace with the addition to the quantity of oxygen; but the shortness of time during which the plant could be retained in a sufficiently healthy condition, prevented my ascertaining, whether after the carbonic acid had been absorbed by it, a part was not at some subsequent period given out again unchanged.

A small portion might perhaps have been taken up by a thin film of water, which I was compelled to keep continually upon the surface of the mercury, in order to prevent the latter from destroying, by a disengagement of its vapour, the plant confined underneath it. This quantity, however, must have been inconsiderable compared with the amount introduced.

I shall now conclude, by placing in a tabular form some of the principal, or the more illustrative experiments which I have carried on, appending some remarks immediately suggested by the particular phenomena observed in each.

Experiments concerning the influence of plants on atmospheric air mixed with various proportions of carbonic acid; the plants being exposed to the sun, and confined in jars containing from 600 to 800 cubic inches of air, and which rested upon mercury covered by a thin film of water.

Experiment 1.

Date.	Circumstances of the experiment.	Proportion per cent. of carbonic acid added.	Absorption per cent. caused by solution of potash.	Residuum per cent. after burning phosphorus in a portion of the air.	Allowance for phosphorus vapour.	Calculated amount per cent. of nitrogen.	Calculated amount per cent. of oxygen.
April 10.	12 A. M. A small Cypress in a garden-pot was introduced into the jar, its stem being cemented air-tight into the hole in the two hemispherical iron plates, that fit the inner margin of the hollow cylinder	0	0	81	2	79	21
	6 P. M. After a cloudy and gloomy day, with only occasional gleams of sunshine	0	0	81	2	79	21
April 11.	8 A. M.	0	0	79	2	77	23
	8½	8	8				
	5 P. M. Stormy and cloudy day, much like that preceding it	0	0	79·5	2	77·5	22·5
April 12.	12 A. M. No sun during the morning, but a settled rain throughout	0	0	81·5	2	79·5	20·5
	1 P. M.	4	4				
April 13.	8 A. M.	0	0	82	2	80	20
	12 A. M. A fine bright day, with occasional storms	3	3				
	4 P. M.	0	1	79	2	77	23
April 14.	The unfavourable state of the weather induced me to suspend the experiment.						

Remarks.—The circumstances most worthy of remark in this experiment appear to be, 1st, the emission of 2 per cent. of oxygen the second day, when the proportion of carbonic acid in the air of the jar was too small to be detected by my apparatus; and 2ndly, the absorption of carbonic acid afterwards when no oxygen was evolved. In the first case we must suppose that the plant had imbibed from the atmosphere, previously to its confinement, the carbonic acid, which under the influence of sunshine it decomposed within the jar; in the second, that it absorbed a large quantity of carbonic acid, which in the unfavourable state of the weather it did not decompose. This latter supposition must be adopted as applicable to most of the succeeding experiments; for my apparatus was proved to be sufficiently perfect to prevent any such escape of carbonic acid, within a corresponding period of time, introduced into the jar, when no plant was present in it, and thereby to obviate any suspicion that it might arise from a defect in the union of the joints.

Experiment 2.

Date.	Circumstances of the experiment.	Proportion per cent. of carbonic acid added.	Absorption per cent. caused by solution of potash.	Residuum per cent. after burning phosphorus in a portion of the air.	Allowance for phosphorus vapour.	Calculated amount per cent. of nitrogen.	Calculated amount per cent. of oxygen.
April 19.	2 P. M. A dull day, with a tendency to rain. Stem and branches of a Persian Lilac (*Syringa persica*) introduced into the jar in the manner above described	9	9	81	2	79	21
	7 P. M.	0	2·75	78	2	76	24
April 20.	8 A. M. Air examined before the sun had acquired any power	0	2·75	80	2	78	22
	11 A. M.	3·25	6·00				
	7 P. M. After a dull day, with occasional storms until 2 P. M., when the sun broke out.............	0	4·25	80	2	79	21
April 21.	Observed the leaves to be altered and faded, and this, together with the unfavourable state of the weather, induced me to discontinue the experiment.						

Observations.—It appears from this experiment, that when a plant is in a perfectly healthy and fresh condition, it may add considerably to the amount of oxygen even in dull weather.

Experiment 3.

Date.	Circumstances of the experiment.	Proportion per cent. of carbonic acid added.	Absorption per cent. caused by solution of potash.	Residuum per cent. after burning phosphorus in a portion of the air.	Allowance for phosphorus vapour.	Calculated amount per cent. of nitrogen.	Calculated amount per cent. of oxygen.
April 26.	12 A. M. Dull day, with occasional gleams of sunshine, but no rain. A (*Pelargonium*) Geranium with its pot introduced into the jar	5	5	81	2	79	21·0
	7 P. M.	0	1	79·5	2	77·5	22·5
April 27.	8 A. M.	0	0	79·25	2	77·25	22·75
	The unfavourable state of the weather led me to discontinue this experiment.						

Experiment 4.

Date.	Circumstances of the experiment.	Proportion per cent. of carbonic acid added.	Absorption per cent. caused by solution of potash.	Residuum per cent. after burning phosphorus in a portion of the air.	Allowance for phosphorus vapour.	Calculated amount per cent. of nitrogen.	Calculated amount per cent. of oxygen.
April 29.	12 A. M. Dull and stormy day, with occasional gleams of sunshine. A Geranium with its pot introduced into the jar, occupying together a capacity of about 60 cubic inches	5	5	81	2	79	21
	6 P. M.	0	1	83·6	2	81·6	18·4
April 30.	10 A. M.	5	5	82·5	2	80·5	19·5
	5 P. M. After a bright day, with clouds and showers occurring occasionally	0	0	78·5	2	76·5	23·5
May 1.	8 A. M. Access of the morning sun prevented by an opake screen covering the jar	0	2				
	A diminution in the quantity of oxygen was detected, but its amount was not registered, as the apparatus was found not to be perfectly air-tight. The experiment was therefore suspended in order to repair the defect.						

Experiment 5.

Date.	Circumstances of the experiment.	Proportion per cent. of carbonic acid added.	Absorption per cent. caused by solution of potash.	Residuum per cent. after burning phosphorus in a portion of the air.	Allowance for phosphorus vapour.	Calculated amount per cent. of nitrogen.	Calculated amount per cent. of oxygen.
May 8.	12 A. M. Bright day. A Geranium in a pot was introduced into the jar	5	5	81	2	79	21·00
	1½ P. M.	0	2·5	79·75	2	77·75	22·25
	2½	0	2·25	79·25	2	77·25	22·75
	3½	0	0·05	79·00	2	77·00	23·00
May 9.	8 A. M. During the morning screened from the sun..................	0	0·0				
	10 A. M. Fine bright day	10	10·0				
	11 A. M.	0	9·50	82	2	80·00	20·00
	12 A. M.	0	6·25	82	2	80·00	20·00
	2 P. M.	0	6·5	80	2	78·00	22·00

Experiment 6.

Date.	Circumstances of the experiment.	Proportion per cent. of carbonic acid added.	Absorption per cent. caused by solution of potash.	Residuum per cent. after burning phosphorus in a portion of the air.	Allowance for phosphorus vapour.	Calculated amount per cent. of nitrogen.	Calculated amount per cent. of oxygen.
May 10.	Fine bright day. At 12 A. M. a fresh Geranium was introduced	10·5	10·5	81	2	79	21
	5½ P. M.	0	1·0	76	2	74	26

Experiment 7.

Date.	Circumstances of the experiment.	Proportion per cent. of carbonic acid added.	Absorption per cent. caused by solution of potash.	Residuum per cent. after burning phosphorus in a portion of the air.	Allowance for phosphorus vapour.	Calculated amount per cent. of nitrogen.	Calculated amount per cent. of oxygen.
May 11.	11 A. M. Dull and sultry day. Fresh Geranium introduced	8·5	8·5	81	2	79	21
	11 P. M.	0	1	81	2	79	21

Observations.—These two latter experiments show decidedly that the disappearance of carbonic acid is not at all in proportion to the disengagement of oxygen. In Experiment 6, in a bright sun, the Geranium, which absorbed 9·5 per cent. of carbonic acid, emitted 5 per cent. of oxygen; in Experiment 7. a similar plant, which absorbed 7·5 of the former in a dull day, seemed to have made no addition at all to the amount of oxygen in the jar.

Experiment 8.

May 13.	2 P. M. Bright sunshine all the day. A young Lilac (*Syringa vulgaris*) had its stem introduced under the jar, its roots being in a pot outside	8	8	81	2	79	21
	$7\frac{1}{2}$ P. M.	0	0·05	82·5	2	80·5	19
May 14.	8 A. M.	0	6·00	not estimated.			
	$8\frac{1}{2}$. Day as that preceding it......	2	8				
	8 P. M.	0	2	75·5	2	73·5	26·5
May 15.	9 A. M. Jar during the preceding part of the morning covered with a screen	0	3·5	76·0	2	74	26·0
	$9\frac{1}{2}$ A. M.	6·5	10·0				
	2 P. M.	0	3·0	72·25	2	70·25	29·75
	$8\frac{1}{2}$ P. M.	0	0·0	72·00	2	70·00	30·00
May 16.	8 A. M. Jar during the former part of the morning having been covered with a screen	0	2·75	72·50	2	70·50	29·50
	The plant beginning to look unhealthy was now removed.						

Observations.—The remarkable clearness of the sky, and the warmth of the weather during these three days, was peculiarly favourable to the experiments, and will account for the large quantity of oxygen obtained, which was equal to 8·5 per cent., or 8·5 × 6 = 51 cubic inches. Owing to the longer time that the plant continued in a state of health, the quantity of carbonic acid that disappeared did not so much exceed that of the oxygen given out as in the preceding experiments.

Experiment 9.

May 20	4 P. M. Day dull, but free from rain: in the evening a little sunshine. Branch of a Lilac, having on it about fifty healthy leaves, each leaf on an average presenting a surface of about six square inches (50 × 6 = 300 square inches to the whole)	5	5	81	2	79	21
	7 P. M.	0	3·3	80·75	2	78·75	21·25
May 21.	$7\frac{1}{2}$ A. M. Jar during the former part of the morning covered by an opake screen	0	2·25	80	2	78	22
	$11\frac{1}{2}$ A. M. Bright sunny day. Thermometer 65°..................	0	2·25	79·5	2	77·5	22·5
	$3\frac{1}{2}$	0	0·00	77	2	75·0	25·0

Experiment 9.—Continued.

Date.	Circumstances of the experiment.	Proportion per cent. of carbonic acid added.	Absorption per cent. caused by solution of potash.	Residuum per cent. after burning phosphorus in a portion of the air.	Allowance for phosphorus vapour.	Calculated amount per cent. of nitrogen.	Calculated amount per cent. of oxygen.
May 22.	8 A.M. Before uncovering the jar..	0	0·5	79·75	2	77·75	22·25
	9 A.M. Fine bright cloudless day. Thermometer 68°............	8·5	9·0	....	....	....	....
	1½ P.M....................	0	6·0	75·75	2	73·75	26·25
	5 P.M.	0	3·0	74·5	2	72·5	27·5
May 23.	8 A.M. Before the screen had been removed	0	2	75	2	73	27
	8 P.M. After a fine bright day	0	0	76	2	74	26
May 24.	8 A.M. Before removing the screen..	0	0	75·5	2	73·5	26·5
	4 P.M. After a fine bright day	0	0	76·5	2	74·5	25·5
May 25.	8 A.M. Before removing the screen..	0	2	76·75	2	74·75	25·25
	6 P.M. Fine bright day, like yesterday	0	0	77·25	2	75·25	24·75
May 27.	10 A.M.....................	0	1	80·75	2	78·75	21·25
	Experiment discontinued, as most of the leaves had dropped off, and the remainder looked decayed and withered. The decay of the leaves began to date from the 24th, at which time the amount of oxygen will be seen to have begun to diminish.						

Observations.—This experiment shows how erroneous a conclusion we might deduce, if we contented ourselves with examining the air after an interval of some days, and how soon, even under the most favourable circumstances, confinement interferes with the healthy functions of a plant.

Experiment 10.

Date.	Circumstances of the experiment.	Proportion per cent. of carbonic acid added.	Absorption per cent. caused by solution of potash.	Residuum per cent. after burning phosphorus in a portion of the air.	Allowance for phosphorus vapour.	Calculated amount per cent. of nitrogen.	Calculated amount per cent. of oxygen.
Aug. 20.	12 A.M. Day dull and cloudy, with a few occasional gleams of sunshine. Therm. 70°. A young Cedar with its pot was introduced under a jar having a capacity of about 800 cubic inches	7·5	7·5	81	2	79	21
	10 P.M......................	....	2·75	80	2	78	22
Aug. 21.	10 A.M. Day overcast, but with occasional gleams of sunshine. Thermometer 70°................	0	2·0	82·5	2	80·5	19·5
	10½	7·5	9·5	....	....	....	....
	7 P.M.	0	5·0	80	2	78	22·0
Aug. 22.	11 A.M. Windy day, but with a bright sun till about 4 P.M. Thermometer 70°................	0	4·0	80	2	78	22·0
	7 P.M.	0	0·0	78·75	2	76·75	23·25
Aug. 23.	8 A.M. Windy and cloudy day, with occasional showers, and few gleams of sunshine. Therm. 64°	0	2·0	80	2	78	22
	10 A.M.	7·5	9·5	80	2	78	22
	7 P.M.	0	4·0	78·50	2	76·50	23·50
Aug. 24.	10 A.M. Cloudy day, without sun. Therm. 66°	0	1	77·25	2	75·25	24·75
	12 A.M.	7·5	....	....	....	....	....
	7 P.M.	0	5	78·20	2	76·20	23·80
	Experiment discontinued.						

Experiment 11.

Date.	Circumstances of the experiment.	Proportion per cent. of carbonic acid added.	Absorption per cent. caused by solution of potash.	Residuum per cent. after burning phosphorus in a portion of the air.	Allowance for phosphorus vapour.	Calculated amount per cent. of nitrogen.	Calculated amount per cent. of oxygen.
Aug. 20.	12 A. M. Bright day, with occasional clouds. Therm. 72°. Another Cedar like the former, with its pot, was introduced into a jar containing about 1300 cubic inches of air; size of the plant being 8 inches in diameter, branches and all, and 16 inches from the pot to the topmost branch	7·50	7·50	....	....	79	21
	10 P. M.	....	2·00	80	2	78	22
Aug. 21.	10 A. M. Windy day, often overcast	....	1·75	79·50	2	77·50	22·50
	$10\frac{1}{2}$ A. M.	7·50	9·25	....	....	....	....
	7 P. M.	....	5·00	79·50	2	77·50	22·50
Aug. 22.	11 A. M. Windy day, but with a bright sun	....	2·50	77·0	2	75·00	25·00
	7 P. M.	0	1·0	75·5	2	73·5	26·5
Aug. 23.	8 A. M. Windy and cloudy day, as above stated	0	1·0	78·0	2	76	24·0
	10 A. M.	7·5	....	....	....	....	....
	7 P. M.	0	6·0	75·25	2	73·25	26·75
Aug. 24.	10 A. M. Cloudy day, without sun	0	5·0	77·25	2	75·25	24·75
	12 A. M.	2·5	7·5	....	....	....	....
	7 P. M. Experiment was here discontinued.	0	6·0	75·25	2	73·25	26·75

Observations.—These two latter experiments tend to show that the leaves of evergreens purify the air as well as those of deciduous plants, although they may not act upon the carbonic acid with the same energy as the latter do.

Experiment 12.

Date.	Circumstances of the experiment.	Proportion per cent. of carbonic acid added.	Absorption per cent. caused by solution of potash.	Residuum per cent. after burning phosphorus in a portion of the air.	Allowance for phosphorus vapour.	Calculated amount per cent. of nitrogen.	Calculated amount per cent. of oxygen.
May 27.	Bright sunny day. Therm. 60°. *Crassula lactea* with its pot introduced into the jar at 12 A. M.	11	11	....	....	79	21
	6 P. M.	0	6·5	81·25	2	79·25	20·75
May 28.	8 A. M. Jar previously covered with a screen	0	6·0	81·25	2	79·25	20·75
	5 P. M. After a bright day	0	6·5	80·00	2	78·00	20·00

Observations.—This experiment, and another made on another species of Crassula, show that these succulent plants do not act with much energy on the air, at least under confinement; but the following experiment evinces that even this tribe act in the same manner, though not with the same intensity.

Experiment 13.

Date.	Circumstances of the experiment.	Proportion per cent. of carbonic acid added.	Absorption per cent. caused by solution of potash.	Residuum per cent. after burning phosphorus in a portion of the air.	Allowance for phosphorus vapour.	Calculated amount per cent. of nitrogen.	Calculated amount per cent. of oxygen.
June 30.	12 A. M. Plant of *Mesembryanthemum verrucosum* introduced into jar. (N.B. No record of the weather: probably it was fine)	6·6	....	....	....	79	21
	8 P. M.	....	4·9	78·6	2·	76·6	23·4
July 1.	10 A. M.	....	2·0	....	....	....	....
	$10\frac{1}{2}$ A. M.	4·5	6·5	....	....	....	....

Experiment 13.—Continued.

Date.	Circumstances of the experiment.	Proportion per cent. of carbonic acid added.	Absorption per cent. caused by solution of potash.	Residuum per cent. after burning phosphorus in a portion of the air.	Allowance for phosphorus vapour.	Calculated amount per cent. of	
						nitrogen.	oxygen.
July 1.	5 P. M.	0	5·0	82·5	2	80·5	19·5
July 2.	1 P. M.	0	5·0	79·5	2	77·5	22·5
July 3.	12 A. M.	0	2·0	80·75	2	78·75	21·25
July 4.	12 A. M.	3·25	2·25	81·75	2	79·75	20·25
	8 P. M.	....	5·00	81·5	2	79·5	20·50
July 5.	1 P. M.	....	2·00	81·25	2	79·25	20·75
July 7.	12 A. M.	....	1·00	83·75	2	81·75	18·25

Observations.—Before the experiment was brought to a close, the plant had assumed an unhealthy appearance; yet the length of time during which it was confined, and the smallness of the change it produced upon the included air, show the more languid manner in which the functions of vegetable life are carried on through the medium of the metamorphosed leaves of this tribe of plants. A similar experiment was tried with a specimen of *Aloe mitræformis*, in which case also 2 per cent. was added to the amount of oxygen by its presence.

Experiment 14.

Date.	Circumstances of the experiment.	Proportion per cent. of carbonic acid added.	Absorption per cent. caused by solution of potash.	Residuum per cent. after burning phosphorus in a portion of the air.	Allowance for phosphorus vapour.	Nitrogen.	Oxygen.
Aug. 30.	12 A. M. Healthy Dahlia in a pot, containing one full-blown flower and twenty leaves, were introduced into the jar	7·5	....	....	....	79	21
	8 P. M. After a day, bright till 3 P. M. afterwards overcast	....	3·0	79·25	2	77·25	22·75
Aug. 31.	10 P. M. Bleak and boisterous weather throughout the day, with much rain. Therm. 65°	0	2·0	77·25	2	75·25	24·75
Sept. 1.	Weather cold, stormy, and bleak, but a bright sun occasionally. Thermometer 55° at 9 A. M.	7·5	....	....	....	....	....
	At 10 P. M.	....	2·0	78·25	2	76·25	23·75
Sept. 2.	Fine day but cloudy: sun at intervals bright. Therm. 62° at 10 P. M.	0	1·0	79·25	2	77·25	22·75
Sept. 3.	Dull stormy day at 9 A. M.	7·5	....	....	....	....	....
	At 4 P. M.	....	4·0	77·25	2	75·25	24·75
Sept. 4.	Fine day at 1 P. M.	....	0·0	75·50	2	73·50	26·50
	Experiment suspended, though the plant seemed to have suffered but very little from its confinement.						

Experiment 15.

Date.	Circumstances of the experiment.	Proportion per cent. of carbonic acid added.	Absorption per cent. caused by solution of potash.	Residuum per cent. after burning phosphorus in a portion of the air.	Allowance for phosphorus vapour.	Nitrogen.	Oxygen.
Aug. 30.	Weather reported above. Healthy Dahlia, with about as many leaves as the former one, but without any flower, was introduced into the jar at 12 A. M.	7·5	....	....	....	79	21
	8 P. M.	0	3·75	....	....	....	....
Aug. 31.	10 A. M.	0	3·75	82·25	2	80·25	20·75
	10 P. M.	0	2·00	82·75	2	80·75	19·25
Sept. 1.	10 A. M.	7·5	9·50	....	....	....	....
	10 P. M.	....	7·50	78·00	2	76·00	24·00
Sept. 2.	10 P. M.	....	5·00	79·00	2	77·00	23·00
Sept. 3.	4 P. M.	....	4·00	78·25	2	76·25	23·75
Sept. 4.	1 P. M.	....	1·00	78·00	2	76·00	24·00
	Experiment was here suspended, as I was on the point of leaving home.						

Observations.—These two last experiments show that even plants which are in flower may purify the atmosphere, the influence of the accompanying leaves more than counterbalancing that of the parts of fructification: and, indeed, a comparison of the results of the first experiment with those of the second seems to indicate, that the presence or absence of a flower does not make so great a difference as might have been anticipated.

In the following experiments a succession of different plants were introduced at intervals of from four to twelve hours, into a jar containing about 240 cubic inches of common air, together with generally about ten cubic inches of carbonic acid. The following Table will show the increase of oxygen caused.

Experiment 16.

Date.	Circumstances of the experiment.	Oxygen.	
		Proportion per cent.	Calculated number of cubic inches.
May 23.	Before any plant was introduced	21	50
	After a pot of *Sinapis alba* had continued in contact with the air during six hours of bright sunshine	25	60
	After a pot containing *Hydrangea hortensis,* only in leaf, had continued in contact with the air during twelve hours of bright sunshine	26·5	63·5
May 24.	After a pot, containing a *Crassula lactea,* had continued in contact with the air during four hours of bright sunshine..................	32·75	77
	After a pot of *Lepidium sativum* had continued in contact with the air during four hours of bright sunshine.........................	33·5	80·5
May 25.	After a pot containing a Geranium (Pelargonium) had continued in contact with the air during four hours' bright sunshine	36·5	87·0
	After the same Hydrangea as before had continued in contact with the air during five hours' bright sunshine.........................	33·25	80·0
May 27.	After a Geranium had continued in contact with the air during five hours' bright sunshine	37·0	88·0
	After a healthy Myrtle had continued in contact with the air during four hours' bright sun	39·0	93
May 28.	After a young but apparently healthy plant of the Sweet Pea (*Lathyrus odoratus*) had continued in contact with the air for four hours	33	78·5
May 30.	After a Geranium had continued in contact with the air for five hours	36·5	87·0

Observations.—Here, owing to an accident, the experiment was suspended; but it was carried far enough to show how much oxygen may be added to the air during the day by plants in a healthy condition. The increase was progressive until I employed the same plant a second time: it probably had suffered from its previous confinement, or from being passed backwards and forwards through the water of the pneumatic trough on which the jar rested. The only other plant which diminished the amount of oxygen was the Sweet Pea; all the others added somewhat to its quantity.

Experiment 17.

Capacity of jar 620 cubic inches (600 atmospheric air, 20 carbonic acid).

August 7.	Before any plant was introduced	21	126
	6 P.M. After a Geranium had continued in contact with the air during five hours' strong sun.....................................	23	138
August 8.	10 A.M. After a second Geranium had remained in contact with the air during the previous night and morning	22	132
	5 P.M. After a Myrtle had been in contact with the air during the period since the previous experiment made	24·5	147
August 9.	11 A.M. After a second Myrtle had continued in contact with the air ever since the last experiment	26	156
	5 P.M. A common garden Lettuce growing in a pot, having continued in contact with the air since the last experiment: day fine........	29	174
August 10.	11 A.M. A second Lettuce, having continued in contact with the air since the preceding experiment: day wet.....................	30	180
	5 P.M. After a Geranium had been introduced since the preceding experiment: day wet.......................................	35	210
August 11.	11 A.M. After a second Geranium had continued in contact with the air during the interval since the last experiment	28	168
	Finding a temporary difficulty in procuring healthy plants, I here suspended the experiments.		

Observations.—This latter series of experiments was undertaken in order to show, that even when the plants were confined in the air of the jar, by night, as well as by day, the balance was still greatly in favour of their purifying influence.

With respect to the comparative influence of similar plants in direct and in diffused light, I have as yet made only a few experiments, but the results of these few seem to indicate that even under the latter circumstances an increase in the amount of oxygen will be sometimes produced by them.

	Per cent.
Thus a jar, in which a Geranium had been confined, contained, after exposure for four hours to direct light, of oxygen	25
Whilst another, in which a similar plant had been placed for the same time in diffused light, contained	19·5
On the other hand,	
Two jars, containing Myrtles, after being placed in direct light for eight hours, had acquired of oxygen,	
No. 1.	23·75
No. 2.	26·00
Two similar jars with Myrtles in diffused light, exposed for the same time, contained,	
No. 1.	20·0
No. 2.	23·0
Two jars with *Polypodium dryopteris*, the one in direct, the other in diffused light, each contained of oxygen	22·0

In conclusion, it may perhaps be well to present in a tabular form the different questions which this inquiry embraces, pointing out how far each may be considered as answered, either by the experiments of preceding investigators, or by those detailed in the present memoir.

Scheme of Experiments.

Part I.—ON THE ACTION OF LIGHT UPON PLANTS.

I. Solar light.

A. direct.

1. In causing the leaves to emit oxygen, and to decompose carbonic acid . . . . . .
   - Immersed in water. Tried with 1. *Brassica oleracea*; 2. *Salicornia herbacea*; 3. *Fucus digitatus*; 4. *Tussilago hybrida*; 5. *Cochlearia Armoracia*; 6. *Mentha viridis*; 7. *Rheum rhaponticum*; 8. *Allium ursinum*; and several species of *Gramineæ*.
   - In atmospheric air. Tried with Geraniums.
2. To become green when etiolated. . . . Tried with Beans.
3. To maintain their irritability . . . . Tried with the Sensitive Plant (*Mimosa pudica*).
4. To exhale water by their leaves . . . . . . Tried with Vines, Dahlias, Helianthuses, *Lavatera arborea*, &c.
5. To absorb the same by their roots . . . . Tried with plants of *Helianthus annuus*, *Sagittaria sagittifolia*, Vines, &c.

B. diffused or reflected. Its influence compared with that of direct solar light in the above particulars. Tried with the Geranium, Myrtle, and Polypodium, so far as regards its relative influence in causing the emission of oxygen.

II. Artificial light, obtained A. from lamps. Tried by Professor Decandolle.
B. from incandescent lime. Tried by myself, but no influence detected.

Part II.—ON THE ACTION OF PLANTS UPON THE ATMOSPHERE.

I. Proportion between the effects attributable to their action during the night and during the day.

1. During fine weather, and in bright sunshine.

		Maximum increase per cent. of oxygen.
Of plants without flowers, and with leaves alone, viz.	Cupressus	2
	Cedrus	3·75
	Syringa vulgaris . .	8·75
	Ditto	6·50
	Pelargonium	2·00
	Ditto	5·00
	Crassula, 2 sp.	0·00
	Mesembryanthemum	2·40
	Dahlia	3·00
Of plants with flowers and leaves.	Dahlia	3·75
	Helianthus	1·00

2. During bad weather, or in diffused light. *Syringa persica* 3 per cent. Geranium, Myrtle, Fern, as noticed above.

II. Proportion between the carbonic acid absorbed and oxygen evolved. My experiments show that when plants are confined the former is always greatest at first; but this may not continue to be the case after a certain interval.

III. Greatest amount of oxygen that can be added to the air of a jar by the influence of a plant. My experiments show that at least 18 per cent. of oxygen may be so added.

IV. At what stage in the scale of vegetable life the function of purifying air stops.
- Probably where there cease to be leaves.—I have shown that it exists in dicotyledonous and monocotyledonous, in evergreens and deciduous, in terrestrial and aquatic plants, in the green parts of succulents as well as in ordinary leaves, in Algæ and in Ferns as well as in phanerogamous families.
- Prof. Marcet has shown that it does not take place in Fungi.

XIV. *Researches in the Integral Calculus.—Part* I. *By* H. F. Talbot, *Esq. F.R.S.*

Received and read March 10, 1836.

§ 1. *Brief Historical Sketch of the Subject.*

THE first inventors of the integral calculus observed that only a certain number of formulæ were susceptible of exact integration, or could be reduced to a finite number of terms involving algebraic, circular, or logarithmic quantities. When this result could not be attained, they were accustomed to develop the integral in an infinite series. But this method, although useful when numerical values are to be computed, is entirely inadequate, in an analytical point of view, to supply the place of the exact integral; for the progress of analysis has shown many instances of exact relation between different integrals which cannot by any means be inferred from the infinite series in which they are developed.

The first great improvement beyond this was made by Fagnani about the year 1714. This most acute and ingenious mathematician proposed the following question to the scientific world in an Italian journal*: "Given a biquadratic parabola whose equation is $x^4 = y$, and an arc of it, to find another arc, so that *their difference* may be rectifiable."

No answer appearing, he published a solution of the problem in the year 1715†, and extended it in a nearly similar manner to other curves whose equation is $x^n = y$, viz. to those cases where n equals one of the numbers $3, \frac{5}{2}, \frac{3}{5}, \frac{2}{5}, \frac{1}{3}, \frac{1}{4}$.

In the year 1718 and afterwards he published a variety of important theorems respecting the division into equal parts of the arcs of the lemniscate, and respecting the ellipse and hyperbola, in both of which he showed how two arcs may be determined of which the difference is a known straight line. These discoveries justify us in regarding Fagnani as the founder of a new and very curious branch of analysis.

Euler, who enriched almost every department of science with new discoveries, exhibited the complete algebraic integral of the equation

$$\frac{dx}{\sqrt{\alpha + \beta x + \gamma x^2 + \delta x^3 + \varepsilon x^4}} + \frac{dy}{\sqrt{\alpha + \beta y + \gamma y^2 + \delta y^3 + \varepsilon y^4}} = 0;$$

a remarkable theorem, which long continued to be the *ne plus ultra* of this branch of science, little success having attended the endeavours of mathematicians to arrive at results of greater generality.

* Giornale de' Letterati d'Italia, tom. xix. p. 438.

† tom. xxii. p. 229.

The excellent work of LEGENDRE* was destined to arrange, classify, and distinguish the properties of elliptic integrals which are implicitly contained in EULER's theorem above mentioned. In this treatise he has thoroughly examined the nature of these transcendents, and presented the results of his inquiries in a luminous and well-arranged theory.

The extensive tables which accompany his work will enable future mathematicians to make as frequent and convenient use of elliptic integrals as they have hitherto done of circular and logarithmic ones.

In the year 1828 Mr. ABEL, of Christiania in Norway, published a very remarkable theorem, which gives the sum of a series of integrals of the form $\int \frac{P\,dx}{\sqrt{R}}$, where P and R are entire functions of x, of the form $x^n + a\,x^{n-1} + b\,x^{n-2} + \ldots\ldots$; n being any whole positive number, and a, b, &c. constant coefficients.

This theorem extends much further than EULER's, in as much as the latter is limited to those forms of R which contain no higher powers of x than the fourth. It departs still more widely from EULER's theorem, in exhibiting the sum, not of two only, but of many integrals of the same form. And it must be observed that this plurality of terms is in general necessary; for if we give to the expression $\int \frac{dx}{\sqrt{R}}$ its utmost generality, it does not appear possible to find the sum of only two such integrals in finite algebraic, or logarithmic terms; but it is requisite to combine a greater number of them, below which number the problem cannot be reduced.

ABEL's theorem in general furnishes a multitude of solutions for each particular case of the problem: notwithstanding which it is possible to find other solutions which appear not to be comprised in his theorem, nor deducible from it†.

On the publication of this theorem the illustrious LEGENDRE, who at an advanced age still cultivated his favourite science with all the ardour of youth, was one of the first to feel its extent and importance. And accordingly, with a degree of zeal almost unequalled in the annals of science, he devoted a large portion of time to the verification and elucidation of the theorem by numerical examples. The result of these calculations was amply confirmatory of its truth, and it therefore undoubtedly stands upon the basis of rigorous demonstration.

There can be little doubt that the ingenious mathematician to whom we are indebted for this theorem would have arrived at fresh discoveries, of not inferior value,

* Exercices de Calcul Intégral. Paris, 1811. Traité des Fonctions Elliptiques. Paris, 1825.

† For instance, if $\frac{dx}{\sqrt{1+x^4}} + \frac{dy}{\sqrt{1+y^4}} = 0$, his theorem gives the integral $xy=1$; but, apparently, it does not give this other integral $y^2 = \frac{\sqrt{1+x^4}+x\sqrt{2}}{\sqrt{1+x^4}-x\sqrt{2}}$, which was discovered by FAGNANI (Produzioni Matematiche, vol. ii. p. 369.).

if a premature death had not terminated his career, to the irreparable loss of science, at the early age of twenty-seven.

Before concluding this slight historical sketch of the subject, I ought not to omit mention of a valuable recent memoir by M. POISSON*, in which he has considered various forms of integrals which are not comprehended in ABEL's formula.

It has already been stated that the integrals to which ABEL's theorem relates are those comprised in the general expression $\int \frac{P\,dx}{\sqrt{R}}$, where P and R are entirely polynomials in x. Next in order of succession to these there naturally presents itself the class of integrals whose general expression is $\int \frac{P\,dx}{\sqrt[3]{R}}$, where the polynomial R is affected with a cubic instead of a quadratic radical.

But ABEL's theorem has no reference to these, and consequently affords us no assistance in their solution. The same may be said with regard to the succeeding classes of integrals, $\int \frac{P\,dx}{\sqrt[4]{R}}, \int \frac{P\,dx}{\sqrt[5]{R}}$, and generally $\int \frac{P\,dx}{\sqrt[n]{R}}$. Still less does it enable us to find the sum of such integrals as $\int \phi\,(R)\,dx$, R being as before an entire polynomial†, and ϕ *any function whatever*. This is the problem to the solution of which the following pages will be dedicated.

I may be here permitted to mention, that ABEL's theorem was unknown to me until some years after its publication, and that these Researches were nearly completed before I was acquainted with it. I have, however, made no alteration in them, but have chosen to present the subject in the manner in which it originally occurred to me.

I am not aware that Mr. ABEL has left any memorial of the successive steps of reasoning by which he arrived at his theorem. Probably they were very different from those which I have employed, and therefore I have detailed at some length my method of investigation, beginning with the first rudiments of the theory at which I afterwards arrived.

§ 2.

It was remarked by the earliest inventors of the integral calculus, that there was a mutual dependence between the two integrals $\int y\,dx$ and $\int x\,dy$, so that if the one were given the other became known, by virtue of the equation

$$\int x\,dy + \int y\,dx = x\,y + C.$$

If therefore one of these forms happened to be more easy of integration than the other, they directed it to be substituted for it.

* CRELLE's Journal, vol. xii. p. 89. Berlin, 1834.

† By "polynomial" I here understand an expression containing at least two different powers of x.

There is, however, one case in which no alteration is produced by this substitution, and that is when the variable x is the same function of y that y is of x; or when $x = \phi y$, $y = \phi x$. For then the integral $\int y\, d x$ or $\int \phi x\, d x$ has the same form with $\int x\, d y$ or $\int \phi y\, d y$.

In this case therefore

$$\int \phi x\, d x + \int \phi y\, d y = x y + \text{C}.$$

This equation holds good whether $\int \phi x\, d x$ can be integrated in finite terms, or whether it cannot.

The equations $x = \phi y$ and $y = \phi x$, manifestly imply that a *symmetrical equation* exists between x and y, and its symmetry is the only requisite condition. In other respects it may be any whatever.

Notwithstanding the simplicity of this reasoning, it does not appear that any mathematician before Fagnani clearly perceived the important consequences which might be deduced from it. But he has obtained from it the following important theorem respecting the arcs of the hyperbola.

If x be the abscissa of a hyperbola whose principal semi-axis $= 1$, its arc

$$= \int d x \sqrt{\frac{e^2 x^2 - 1}{x^2 - 1}},$$

where e is the eccentricity, or the distance between the centre and focus.

Let y be another abscissa, so related to the former that

$$e y = \sqrt{\frac{e^2 x^2 - 1}{x^2 - 1}},$$

whence

$$e^2 x^2 y^2 = e^2 (x^2 + y^2) - 1.$$

This equation being symmetrical with respect to x and y, it follows that those letters may be permuted,

$$\therefore e x = \sqrt{\frac{e^2 y^2 - 1}{y^2 - 1}}.$$

Multiplying these equations respectively by $d x$ and $d y$, and then adding them,

$$e y\, d x + e x\, d y = d x \sqrt{\frac{e^2 x^2 - 1}{x^2 - 1}} + d y \sqrt{\frac{e^2 y^2 - 1}{y^2 - 1}}.$$

$$\therefore e x y + \text{C} = \int d x \sqrt{\frac{e^2 x^2 - 1}{x^2 - 1}} + \int d y \sqrt{\frac{e^2 y^3 \quad 1}{y^2 - 1}},$$

which is the theorem in question.

Since the arc of an ellipse may also be expressed by the formula $\int d x \sqrt{\frac{e^2 x^2 - 1}{x^2 - 1}}$, it may be asked whether the theorem applies to the ellipse, or to the hyperbola, or to both curves?

Let us therefore return to the equation

$$e^2 x^2 y^2 = e^2 (x^2 + y^2) - 1,$$

whence

$$-(x^2 + y^2) + x^2 y^2 = -\frac{1}{e^2}$$

$$(1 - x^2)(1 - y^2) = 1 - \frac{1}{e^2}.$$

In the ellipse the abscissæ x, y are necessarily both less than 1, and in the hyperbola they are both greater than 1. Therefore in either case the product $(1 - x^2)(1 - y^2)$ is a positive quantity, $\therefore 1 - \frac{1}{e^2}$ must be a positive quantity, which gives $1 > \frac{1}{e^2}$, or $e > 1$. This condition obtains in the hyperbola but not in the ellipse, therefore the theorem is not applicable to the latter. An analogous theorem, however, exists for the ellipse, which I shall not now stop to examine.

In imitation of the above proceeding, let us make the more general supposition

$$e y = \left(\frac{e^n x^n - 1}{x^n - 1}\right)^{\frac{1}{n}},$$

whence

$$e^n x^n y^n = e^n (x^n + y^n) - 1,$$

a symmetrical equation;

$$\therefore e x = \left(\frac{e^n y^n - 1}{y^n - 1}\right)^{\frac{1}{n}}.$$

Proceeding as before, we find

$$e x y + C = \int \left(\frac{e^n x^n - 1}{x^n - 1}\right)^{\frac{1}{n}} d x + \int \left(\frac{e^n y^n - 1}{y^n - 1}\right)^{\frac{1}{n}} d y$$

$$= S \int \left(\frac{e^n x^n - 1}{x^n - 1}\right)^{\frac{1}{n}} d x,$$

where the notation $S\int$ is employed to express with brevity the sum of two (or any number) of similar integrals.

The sums of many other integrals might be found in the same manner; but I proceed to more general inquiries.

§ 3.

The first idea of a more extended method occurred to me about fifteen years ago, when pursuing mathematical studies at Cambridge; and it was suggested by an attentive consideration of the process by which FAGNANI had rectified the hyperbola, as mentioned in the preceding section. The question occurred to me, whether it might not be possible to combine *three* integrals in a similar manner, by supposing two symmetrical equations to exist between three variables?

Since we have

$$x\,y\,z + \mathrm{C} = \int y\,z\,dx + \int x\,z\,dy + \int x\,y\,dz,$$

if we suppose any two equations to exist between the variables, then y and z are functions of x which assume definite values when x is given. Therefore also the product $y\,z$ is a function of x, which may be called $\varphi\,x$.

If now the two equations are symmetrical, it follows that the letters x, y, z may be permuted; which gives $x\,z = \varphi\,y$, and $x\,y = \varphi\,z$;

$$\therefore x\,y\,z + \mathrm{C} = \int \varphi\,x\,dx + \int \varphi\,y\,dy + \int \varphi\,z\,dz$$

$$= \mathrm{S}\int \varphi\,x\,dx.$$

It is evident that this reasoning may be extended to any number n of variables between which there exist $n-1$ *symmetrical* equations, which circumstance renders them all similar functions of each other.

Let r designate their product $x\,y\,z\ldots$, and therefore $\frac{r}{x} = y\,z\ldots$, or the product of all except x.

$$\therefore r + \mathrm{C} = \int \frac{r}{x}\,dx + \int \frac{r}{y}\,dy +, \&c.$$

But if $\frac{r}{x} = \varphi\,x$, we have by merely permuting the letters,

$$\frac{r}{y} = \varphi\,y, \frac{r}{z} = \varphi\,z, \&c.$$

Therefore

$$\left.\begin{aligned} r + \mathrm{C} &= \int \varphi\,x\,.\,dx + \int \varphi\,y\,.\,dy +, \&c. \\ &= \mathrm{S}\int \varphi\,x\,.\,dx. \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (A.)$$

This equation I first obtained in the year 1821, but not having leisure at that time to pursue the subject much further, I contented myself with making a note of it as being a subject that deserved to be further examined into. I afterwards found it to be the key, as it were, of the whole method.

In the year 1825 I resumed this investigation, and endeavoured, by the trial of various forms of symmetrical equations between the variables, to see whether this method would lead to new results, or whether, on the contrary, it would turn out to be a mere variation of the methods in common use.

I here give the results of some of these early trials, just as I find them in the original papers.

Ex. 1. Let the 2 symmetrical equations be

(1.) $x + y + z = a$

(2.) $x^2 + y^2 + z^2 = b^2$,

a and b^2 being constants.

These give

$$\phi x \text{ or } y z = \frac{a^2 - b^2}{2} - a x + x^2.$$

And theorem A. gives

$$(3.)\ x y z + C = \int \phi x \, d x + \int \phi y \, d y + \int \phi z \, d z.$$

Now here it is easy to verify the theorem, because $\int \phi x \, d x$ is known, viz.

$$\int \phi x \, d x = \frac{a^2 - b^2}{2} x - \frac{a}{2} x^2 + \frac{x^3}{3},$$

and similarly with respect to $\int \phi y \, d y$ and $\int \phi z \, d z$; $\therefore$ by addition,

$$S \int \phi x \, d x = \frac{a^2 - b^2}{2} (x + y + z) - \frac{a}{2} (x^2 + y^2 + z^2) + \frac{x^3 + y^3 + z^3}{3},$$

or, by help of equations (1.) and (2.),

$$S \int \phi x \, . \, d x = \text{const.} + \frac{x^3 + y^3 + z^3}{3}.$$

Although this result differs at first sight from that given by equation (3.),

$$S \int \phi x \, d x = \text{const.} + x y z,$$

yet it is easy to see that they are identical. For since

$$x + y + z = \text{const., by equation (1.),}$$

and

$$x^2 + y^2 + z^2 = \text{const., by equation (2.),}$$

it follows as a necessary consequence that

$$\frac{x^3 + y^3 + z^3}{3} = x y z + \text{const.},$$

which verifies the theorem in this case.

In the examples which follow next I shall suppose one of the given equations to be

$$x + y + z = 0.$$

Ex. 2. Let the other equation be $(x^2 - 1)(y^2 - 1)(z^2 - 1) = -1$, we find

$$\phi x, \text{ or } y z = -1 + \sqrt{\frac{1 + x^2 - x^4}{1 - x^2}}.$$

Ex. 3. Let $x^4 + y^4 + z^4 = 2 x y z$, we find

$$2 y z = 2 x^2 + x + \sqrt{4 x^3 + x^2} = 2 \phi x.$$

Ex. 4. Let $x^5 + y^5 + z^5 = -5$, we find

$$y z = \frac{x^2}{2} + \sqrt{\frac{1}{x} + \frac{x^4}{4}} = \phi x.$$

In each of these cases therefore we find the sum of three integrals of the form $\int \phi x \, . \, d x$ to be equal to $x y z + C$.

Before going further it may be well to adopt, for the sake of brevity, the following notation.

Let there be any number of variables, three for example; then the sum of their nth powers or $x^n + y^n + z^n$ may be briefly written $S\,x^n$, and similarly if $f\,x$ be any function of x, $S f x$ stands for the sum of $f x + f y + f z$. Also $S\,x\,y$ means $x\,y + x\,z + y\,z$. And in general

$$S f(x,y) = f(x,y) + f(y,x) + f(x,z) + f(z,x) + f(y,z) + f(z,y),$$

being the sum of all the permutations of the letters. A few examples will render this notation familiar.

Let there be 3 variables, then

$$S\,x = x + y + z$$
$$S\,x\,y = x\,y + x\,z + y\,z$$
$$S\,x^2\,y = x^2\,y + y^2\,x + x^2\,z + z^2\,x + y^2\,z + z^2\,y$$
$$S\,x^2\,y^2 = x^2\,y^2 + x^2\,z^2 + y^2\,z^2.$$

Let $r = x\,y\,z$.

$$S\,\frac{r}{x} = y\,z + x\,z + x\,y = S\,x\,y$$

$$S\,\frac{1}{x} = \frac{1}{r}\,S\,x\,y$$

$$S\,\frac{1}{x^2} = \frac{1}{r^2}\,S\,x^2\,y^2$$

$$S\,\frac{1}{x^n} = \frac{1}{r^n}\,S\,x^n\,y^n, \text{ \&c. \&c.}$$

Let there be 5 variables, u, v, x, y, z, then

$$S\,u\,v\,x\,y = u\,v\,x\,y + u\,v\,x\,z + u\,v\,y\,z + u\,x\,y\,z + v\,x\,y\,z, \text{ \&c. \&c. \&c.}$$

The greater the number of the variables, the greater is the advantage of this abbreviated notation.

To resume our examples:

Ex. 5.—Let $S\,x^2\,y^2 + a\,.\,x\,y\,z + b\,S\,x\,y = c$. Then supposing, as before, that $x + y + z = 0$, we find

$$2\,y\,z = 2\,x^2 - a\,x - b + \sqrt{(4\,c + b^2) + 2\,a\,b\,x + a^2\,x^2 - 4\,a\,x^3}.$$

By properly determining the constants a, b, c, this radical may be made to agree with any proposed cubic $\sqrt{x^3 + \alpha\,x^2 + \beta\,x + \gamma}$.

Ex. 6.—Let $x^6 + y^6 + z^6 = 0$, or $S\,x^6 = 0$. Here $y\,z$ or $\varphi\,x$ is an implicit function of x, only determinable by the solution of the cubic equation

$$(m - x^2)^3 = \frac{3}{2}\,x^2\,m^2,$$

where $m = y\,z$.

Notwithstanding the complicated nature of the function $\phi\, x$, we still have

$$\mathrm{S}\int \phi\, x \,.\, d\, x = x\, y\, z + \mathrm{C}.$$

But the most interesting result was obtained by combining the equations

$$x + y + z = 0$$

$$x\, y + x\, z + y\, z = -\frac{x^2\, y^2\, z^2}{4};$$

or, in the compendious notation, putting $x\, y\, z = r$,

$$\mathrm{S}\, x = 0 \qquad \mathrm{S}\, x y = \frac{-r^2}{4}:$$

whence

$$y\, z = \frac{2\sqrt{1 + x^4}}{x^2} - \frac{2}{x^2} = \phi\, x,$$

and by the general theorem A

$$x\, y\, z + \mathrm{C} = \mathrm{S}\int \phi\, x \,.\, d\, x. \qquad \text{(1.)}$$

But $\int \phi\, x \,.\, d\, x$ consists of two parts, of which the latter is $\int \frac{-2\, d\, x}{x^2} = \frac{2}{x}$. Therefore the sum of three such portions $= \frac{2}{x} + \frac{2}{y} + \frac{2}{z} = 2\,\mathrm{S}\frac{1}{x} = \frac{2\,\mathrm{S}\, x\, y}{r} = \frac{-r}{2}$; since by hypothesis $\mathrm{S}\, x\, y = \frac{-r^2}{4}$.

Hence if we put

$$\int \frac{\sqrt{1 + x^4}}{x^2} \,.\, d\, x = \int \psi\, x \,.\, d\, x,$$

equation (1.) becomes

$$r + \mathrm{C} = 2\,\mathrm{S}\int \psi\, x \,.\, d\, x - \frac{r}{2};$$

whence

$$\mathrm{S}\int \psi\, x \,.\, d\, x = \frac{3}{4}\, r + \text{const.}$$

Now this result is highly deserving of attention; for the integral which we have here called $\int \psi\, x \,.\, d\, x = \int \frac{\sqrt{1 + x^4}}{x^2} \,.\, d\, x$, is no other than the arc of an equilateral hyperbola whose abscissa is x, the equation of the curve being referred to its asymptotes. When I arrived at this result, I immediately perceived that (provided there were no error in the reasoning, of which I at first entertained some doubts,) it was an entirely new and undiscovered property of the hyperbola. I therefore proceeded to verify it by calculating numerical examples.

The theorem may be stated thus: If three abscissæ of an equilateral hyperbola verify the equations $\mathrm{S}\, x = 0$, $\mathrm{S}\, x\, y = \frac{-r^2}{4}$, the sum of the arcs subtended by those abscissæ $= \frac{3}{4}\, r +$ const. In order to eliminate the constant, the value of which was unknown, I supposed one of the abscissæ x to assume some other value x', and therefore a corresponding change to take place with respect to y and z (since they are functions of x). These new values may be called y' and z'.

At the same time the product $x\,y\,z = r$ is changed to $x'\,y'\,z' = r'$; and the first set of arcs, which may be denoted by A, are changed for a second set, which may be denoted by B.

Now the original equation gives, sum of arcs A $= \frac{3}{4} r + C$, and the changed equation gives, sum of arcs B $= \frac{3}{4} r' + C$; $\therefore$ by subtraction, sum of arcs A $-$ sum of arcs B $= \frac{3}{4}(r - r')$, in which result the constant is eliminated.

The accompanying figure 1. represents two opposite equilateral hyperbolas, with their asymptotes. C is the centre, and origin of the abscissæ $CX = x$, $CY = y$, $CZ = z$, of which the latter must be negative (supposing the two former positive), by reason of the equation $x + y + z = 0$. Therefore it belongs to the opposite hyperbola. If XP is the ordinate corresponding to the abscissa CX, the equation of the curve is $CX \cdot XP = 1$, and the arc subtended by CX is the infinite arc OP.

Fig. 1.

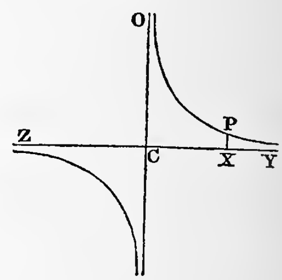
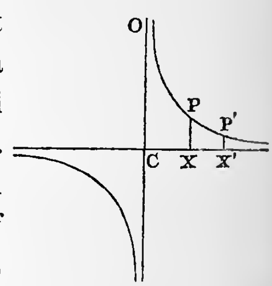


Fig. 2. represents the abscissa $CX = x$, both in its original and in its altered state when it has become $CX' = x'$. In the former case it subtends the infinite arc OP, and in the latter case the infinite arc OP′. But in taking the difference there remains the portion of abscissa XX′ subtended by the arc PP′, which is a finite quantity, and thus the embarrassing consideration of the infinite arcs is avoided. Now the sum of arcs A $-$ sum of arcs B $=$ sum of three limited arcs, of which PP′ is one, and the others subtend the portions of abscissæ YY′ and ZZ′. Denoting these arcs by K, we have this equation in finite terms:

Fig. 2.



$$\text{Sum of arcs K} = \tfrac{3}{4}(r - r').$$

Now in order to put this equation to the test of numerical computation, it is requisite to find three quantities that verify the equations

$$S\,x = 0 \qquad S\,x\,y = \frac{-r^2}{4}$$

Suppose, therefore,

$$x = 1$$
$$y = 1{\cdot}7535$$
$$z = -2{\cdot}7535,$$

whence

$$x\,y\,z = -4{\cdot}8281 = r.$$

The equations are satisfied by these values, and also by the following:

$$x' = 1{\cdot}1$$
$$y' = 1{\cdot}5826$$
$$z' = -2{\cdot}6826,$$

whence

$$x'\,y'\,z' = -4{\cdot}670 = r'.$$

[4.] $\therefore\ x' - x = 0{\cdot}1$

$y' - y = -0{\cdot}1709 \qquad r' - r = 0{\cdot}1581.$

$z' - z = 0{\cdot}0709$

Now we can, without difficulty, calculate the approximate value of the arc (P P′ in fig. 2.) subtended by the portion of abscissa $x' - x$ (X X′ in the figure). Calling this the arc (x), we have

$$\text{Arc } (x) = 0{\cdot}1351$$
$$\text{Arc } (y) = 0{\cdot}1817$$
$$\text{Arc } (z) = 0{\cdot}0715.$$

And according to the theorem we ought to have

$$\text{Sum of arcs} = \tfrac{3}{4}\,(r' - r) = 0{\cdot}118.$$

But in this example, arc (x) is to be accounted negative. Therefore we have

$$\text{Arc } (y) + \text{Arc } (z) = 0{\cdot}253$$
$$-\ \text{Arc } (x) = 0{\cdot}135$$
$$\text{Sum} = 0{\cdot}118$$

which is in accordance with the theorem.

Second example.—Suppose, as before,

$$x = 1$$
$$y = 1{\cdot}7535$$
$$z = -2{\cdot}7535,$$

whence

$$x\,y\,z = -4{\cdot}8281.$$

And also

$$x' = 2$$
$$y' = 0{\cdot}8875$$
$$z' = -2{\cdot}8875,$$

whence

$$x'\,y'\,z' = -5{\cdot}1253\,;$$

both of which systems of values satisfy the given equations of condition.

[5.] $\therefore\ x' - x = 1$

$y' - y = -0{\cdot}866 \qquad r' - r = -0{\cdot}297.$

$z' - z = -0{\cdot}134$

By calculation we find

$$\text{Arc } (x) = 1{\cdot}1319$$
$$\text{Arc } (y) = 1{\cdot}0443$$
$$\text{Arc } (z) = 0{\cdot}1350.$$

But in this example both arc (x) and arc (z) are negative. Therefore we have

$$- \text{Arc}\ (x) - \text{Arc}\ (z) = -\ 1{\cdot}267$$
$$+ \text{Arc}\ (y) = +\ 1{\cdot}044$$
$$\text{Sum} = -\ 0{\cdot}223$$

Also

$$r' - r = -\ 0{\cdot}297$$
$$\therefore \tfrac{3}{4}\,(r' - r) = -\ 0{\cdot}223$$

in accordance with the theorem.

I had at first some difficulty to perceive the reason why some of the arcs were to be considered negative rather than the others. This question was one of a novel nature, which had not hitherto occurred to analysts, and therefore no solution of it was to be met with in books. On the other hand, to leave so essential a point without any demonstration was unsatisfactory. But the following considerations appeared to afford an explanation of this fact.

In the first example, since $x + y + z = 0$, and both x and y are positive quantities, z must be negative. Therefore $x\,y$ is positive, but $x\,z$ and $y\,z$ are negative.

Now we have

$$\frac{y\,z}{2} = \frac{\sqrt{1 + x^4} - 1}{x^2},$$

which therefore must be negative: $\therefore$ also $\sqrt{1 + x^4} - 1$ must be negative. But this quantity would necessarily be positive if the radical had a positive sign. Therefore the radical must have a negative sign.

Reasoning in the same manner, because $\frac{x\,z}{2} = \frac{\sqrt{1 + y^4} - 1}{y^2}$, this radical has necessarily a negative sign; and because $\frac{x\,y}{2} = \frac{\sqrt{1 + z^4} - 1}{z^2}$, this radical has a positive sign. Attributing, therefore, these signs to the radicals, the three hyperbolic arcs are respectively,

$$-\int \frac{d\,x}{x^2} \sqrt{1 + x^4}, \qquad -\int \frac{d\,y}{y^2} \sqrt{1 + y^4}, \qquad +\int \frac{d\,z}{z^2} \sqrt{1 + z^4}.$$

On the other hand, during the change of x, y, z to x', y', z' respectively, we have seen that x and z increase while y diminishes (see equations [4.]); $\therefore$ $d\,x$ and $d\,z$ are to be accounted positive, and $d\,y$ negative. This consideration renders it necessary to write

$$[6.] \quad -\int \frac{d\,x}{x^2} \sqrt{1 + x^4}, \qquad +\int \frac{d\,y}{y^2} \sqrt{1 + y^4}, \qquad +\int \frac{d\,z}{z^2} \sqrt{1 + z^4}.$$

And thus the assertion that arc (x) is to be considered negative in this example is justified.

Second example.—Here the reasoning remains the same as far as regards the signs of the different radicals; but it appears from the equations [5.], that while x increases, both y and z diminish, $\therefore$ $d\,x$ is positive, and $d\,y$ and $d\,z$ are negative. Therefore the only difference between this example and the former respects the sign of $d\,z$. Therefore equation [6.] must be written

$$-\int \frac{d\,x}{x^2} \sqrt{1 + x^4}, \qquad +\int \frac{d\,y}{y^2} \sqrt{1 + y^4}, \qquad -\int \frac{d\,z}{z^2} \sqrt{1 + z^4},$$

which justifies the assertion, that in this example arcs (x) and (z) are to be accounted negative, and arc (y) positive.

Perhaps this reasoning may not be altogether free from objection: I wish it, therefore, to be remembered that I am not giving it here as being the most convenient method of determining the signs of the arcs, but merely as being the reasoning I employed at the time* when I first met with this theorem.

This theorem shows that three hyperbolic arcs may be determined in an infinity of ways, so that their sum may be an algebraic quantity. At the same time it shows that one of these arcs cannot be supposed always to be 0, so that FAGNANI's theorem respecting the sum of *two* arcs is not an instance or particular case of this. I have dwelt at some length on this theorem, because the theory of the conic sections has always been regarded as so important by mathematicians that any considerable addition to it is thought deserving of attention.

I now proceed to other results which presented themselves in the course of this inquiry.

Still continuing to suppose the variables to be three in number, it is allowable to suppose between them any two symmetrical equations whatever; and thence if we can deduce the value of $y\,z$ or $\varphi\,x$ in terms of x alone, we may apply the general theorem A.

Ex. 1. Let $\mathrm{S}\,x = a$, and $\mathrm{S}\,x\,y = \left(\frac{x\,y\,z}{2}\right)^2$, we find

$$\frac{y\,z}{2} = \frac{1}{x^2} + \frac{\sqrt{1 - x^4 + a\,x^3}}{x^2}.$$

Ex. 2. Let $\mathrm{S}\,x = a$, and $\mathrm{S}\,x\,y = \sqrt{2\,b\,.\,x\,y\,z}$, we find

$$y\,z = x^2 - \overline{a - b}\,.\,x + \sqrt{2\,b\,x^3 + (b^2 - 2\,a\,b)\,x^2}.$$

Ex. 3. Let $\sqrt{x} + \sqrt{y} + \sqrt{z} = \sqrt{a}$ (or $\mathrm{S}\sqrt{x} = \sqrt{a}$), and let $\mathrm{S}\,x = b$, we find

$$\sqrt{y\,z} = \frac{a - b}{2} - \sqrt{a\,x} + x = f\,x,$$

whence

$$y\,z = \varphi\,x = [f\,x]^2.$$

A great variety of different suppositions of this sort may be made; but if the resulting function $\varphi\,x$ should become too complicated, little practical advantage would be derived from the knowledge of its properties. I therefore thought of another method of obtaining this function, by means of what may be termed "changing the conditions." Thus let the original equations of condition be

$$x + y + z = 0, \quad \text{and} \quad x\,y + x\,z + y\,z = -1,$$

* 1825.

whence this equation results,

$$y z = x^2 - 1.$$

Now for x, y, z write their cubes (both in the original and in the resulting equation), and they become

$$[7.] \quad x^3 + y^3 + z^3 = 0$$
$$x^3 y^3 + x^3 z^3 + y^3 z^3 = -1$$

and

$$y^3 z^3 = x^6 - 1.$$

Taking the cube root of this, we have

$$y z = \sqrt[3]{x^6 - 1}.$$

Whence it follows that the sum of three integrals

$$\int d x \sqrt[3]{x^6 - 1} + \int d y \sqrt[3]{y^6 - 1} + \int d z \sqrt[3]{z^6 - 1} = x y z + C,$$

whenever x, y, z satisfy the two given equations of condition [7.], which may be briefly written

$$S x^3 = 0 \qquad S x^3 y^3 = -1.$$

Here we *changed the conditions*, by writing x^3 for x. We might have written x^n for x, and thereby obtained a more general result*. Even values of n must, however, be excluded, because the equation $x^n + y^n + z^n = 0$ would otherwise be impossible.

Ex. 4. Let $S x = a$, and $S \frac{1}{x} = \frac{1}{b}$, whence

$$y z = \frac{a b x - b x^2}{x - b}.$$

Now if we write for x, y, z, a, b, their square roots, these three equations become

$$\sqrt{x} + \sqrt{y} + \sqrt{z} = \sqrt{a},$$

$$\sqrt{\frac{1}{x}} + \sqrt{\frac{1}{y}} + \sqrt{\frac{1}{z}} = \sqrt{\frac{1}{b}},$$

and

$$\sqrt{y z} = \frac{\sqrt{a b x} - \sqrt{b} \cdot x}{\sqrt{x} - \sqrt{b}} = f x,$$

whence

$$y z = \varphi x = [f x]^2.$$

Many interesting theorems may be obtained by this method of "*changing the original conditions*," but these examples of it will suffice for the present.

I now perceived that the hypothesis upon which my method was grounded, viz. that $n - 1$ symmetrical equations existed between n variables, was the same thing as to suppose that these variables were the roots of an equation of n dimensions, *one of whose coefficients at least was variable*, the others being either constants, or functions of the variable one. This consideration introduced a great degree of clearness and

* Viz. the sum of three integrals, like $\int d x \sqrt[n]{x^{2n} - 1}$.

simplicity into the subject, besides facilitating in no ordinary degree the progress of research. For instance, suppose there are 3 variables, and let $x + y + z = p$, $x y + x z + y z = q$ and $x y z = r$, then x, y, z are the roots of the equation

$$u^3 - p u^2 + q u - r = 0,$$

where the variable u denotes *indifferently* either of the variables x, y, or z. This new letter u is only introduced for the sake of clearness, since we may equally well say that x, y, z are the roots of the equation

$$x^3 - p x^2 + q x - r = 0,$$

or of

$$y^3 - p y^2 + q y - r = 0, \text{\&c.}$$

This latter mode of expression is often more convenient. Now the function φx, which we wish to determine,

$$= y z = \frac{x y z}{x} = \frac{r}{x};$$

and since p, q are here supposed to be *given functions of* r, we may find the value of $\frac{r}{x}$ in terms of x, provided we can solve the algebraic equation

$$x^3 - p x^2 + q x - r = 0,$$

with respect to r.

Example. Let us resume the question concerning the sum of 3 arcs in the equilateral hyperbola. The equations of condition were

$$x + y + z = 0,$$

$$x y + x z + y z = \frac{-x^2 y^2 z^2}{4},$$

or

$$p = 0, q = \frac{-r^2}{4},$$

$\therefore x, y, z$ are the roots of

$$x^3 - \frac{r^2}{4} x - r = 0.$$

This equation (arranged according to the powers of r) is

$$\frac{-x}{4} \cdot r^2 - r + x^3 = 0,$$

or

$$r^2 + \frac{4}{x} \cdot r - 4 x^2 = 0,$$

whence

$$r = \frac{-2}{x} + 2\sqrt{x^2 + \frac{1}{x^2}},$$

and

$$\frac{r}{x} = \frac{-2}{x^2} + \frac{2\sqrt{1 + x^4}}{x^2} = \varphi x,$$

which agrees with the former result. But now we are able to point out with clearness the limits of the possibility of the theorem. For the cubic equation

$$x^3 - \frac{r^2}{4} \cdot x - r = 0$$

being compared with the form

$$x^3 - a\,x + b = 0,$$

must have impossible roots when $\frac{b^2}{4} > \frac{a^3}{27}$, or when $\frac{r^2}{4} > \frac{r^6}{64 \times 27}$, or $1 > \frac{r^4}{16 \times 27}$, or $16 \cdot 27 > r^4$. Hence it appears that there are impossible roots whenever r is less than

$$\sqrt[4]{16 \times 27} = 2\sqrt[4]{27} = \pm\, 4 \cdot 559.$$

Accordingly in our numerical examples it will be seen that the values* of r are not contained within these limits.

Another example. We have found (page 183. *Ex.* 3.) that the suppositions $\mathrm{S}\,x = 0$, $\mathrm{S}\,x^4 = 2\,r$, give

$$2\,\phi\,x = 2\,x^2 + x + \sqrt{4\,x^3 + x^2}.$$

Here x, y, z are roots of $x^3 + q\,x - r = 0$, and we easily find from the doctrine of equations that

$$\mathrm{S}\,x^4 = 2\,q^2, \quad \therefore 2\,q^2 = 2\,r, \quad \therefore q^2 = r.$$

Therefore $x + q\,x - q^2 = 0$. Solving this quadratic equation with respect to q we have

$$q = \frac{x}{2} + \frac{1}{2}\sqrt{x^2 + 4\,x^3}.$$

But since

$$x^3 + q\,x - r = 0, \ \frac{r}{x} = x^2 + q.$$

$$\therefore \frac{2\,r}{x} = 2\,x^2 + x + \sqrt{x^2 + 4\,x^3} = 2\,\phi\,x,$$

which agrees with the former result.

Another example. Let $x^3 + q\,x - r = 0$, which gives for the first condition

$$\mathrm{S}\,x = 0.$$

And let the second condition be

$$r^2 + c\,r = q^2 + a\,q + b,$$

a, b, c being constants. We find

$$\frac{r}{x} = \frac{x^2 + \frac{c}{2} \cdot x - \frac{a}{2} + \sqrt{\mathrm{X}}}{1 - x^2},$$

where X is a polynomial of 6 dimensions.

Now let the n variables $x, y, z \ldots\ldots$ be roots of $x^n - p\,x^{n-1} + \ldots\ldots \pm r = 0$, where I continue to denote the product of all the roots by r; we have still $\phi\,x = \frac{r}{x}$. Let the coefficients p, &c. &c. be replaced by their values in terms of r (which are supposed given, by means of $n - 1$ equations of condition). Then let the equation be arranged according to the powers of r, and the solution of it will give the value of r in terms of x, and therefore the value of $\frac{r}{x} = \phi\,x$.

* These were $r = -4 \cdot 83$, $r = -4 \cdot 67$, $r = -5 \cdot 13$.

Now if it be considered that this method extends to any number whatever of variables, and that the coefficients of the equation may be any functions of each other that we please to make them, it will appear at once how wide a field of inquiry here opens before us. It was the wish to reduce these extensive but rather complicated results to something like a clear and connected system which obliged me to defer the publication of them longer than I should otherwise have wished, by which means I lost the priority which at one time was in my power of announcing the existence of this new branch of analysis; for the results hitherto mentioned, together with many others, which for the sake of brevity I omit, were obtained in the years 1825 and 1826, and consequently two or three years previously to the publication of ABEL's theorem. And it will be observed that they comprise large classes of integrals which are not contained in his formula

$$\int \frac{P\,dx}{\sqrt{R}}.$$

Of this I have given an instance in the integral,

$$\int dx \sqrt[3]{x^6 - 1},$$

and the more general one,

$$\int dx \sqrt[n]{x^{2n} - 1}.$$

But an unlimited number of such forms may be found by the method I have pointed out of "changing the conditions" at first established between the variables. We may conclude therefore that if $x, y, z \dots\dots$ are the roots of an equation of n dimensions, having at least one variable coefficient, and if we can find the function $\varphi x = \frac{r}{x}$ in terms of x, we may thence deduce the algebraic sum of the n integrals,

$$\int \varphi x \,.\, dx + \int \varphi y \,.\, dy + \dots\dots$$

But the inverse problem still remains. *Given the function φx, or the integral $\int \varphi x \,.\, dx$, to find the equation*

$$x^n - p\,x^{n-1} + \dots\dots \pm r = 0,$$

of which $x, y, z \dots\dots$ must be roots, in order that $S \int \varphi x \,.\, dx$ may have an algebraic sum?

This is evidently the most important part of the subject, for in applying the method to practice the form of the function φx is given beforehand. That this research requires methods of its own will appear at once from a simple example.

Let $\int \sqrt{1 + x^n} \,.\, dx$ be the proposed integral, where n is any whole number. Let us first suppose

$$\sqrt{1 + x^n} = \varphi x = \frac{r}{x}.$$

This gives

$$r = x\sqrt{1 + x^n},$$

or

$$x^{n+2} + x^2 - r^2 = 0,$$

an equation, the product of whose roots must be $\pm r^2$. But by the hypothesis this product is always represented by r. Whence it follows that no solution of the required problem is effected by the supposition $\sqrt{1 + x^n} = \frac{r}{x}$. And at the same time we see that in order for any supposition to be successful, it is necessary that the resulting equation, arranged according to the powers of x, should have r for its last term.

Now let us remark, that if $S\int \psi x \,.\, dx$ has an algebraic sum, then $S\int (\psi x + x^a)\, dx$ has likewise an algebraic sum. For it equals the former sum, with the addition of $S\int x^a\, dx$, or $\frac{1}{a+1}\,(x^{a+1} + y^{a+1} + z^{a+1} + \ldots\ldots)$, which is an algebraic quantity. In the same way we see that $S\int (\psi x + m\,x^a + n\,x^b + \ldots\ldots)\, dx$ has an algebraic sum if m, n, a, b, &c. are constants, and if there are any number of such simple terms of the form $m\,x^a$. Hence if the proposed integral be $\int \psi\, x \,.\, d\,x$, and the supposition $\phi\, x$ or $\frac{r}{x} = \psi\, x$ does not succeed, we are led to try the suppositions

$$\frac{r}{x} = \psi\, x + x,\quad \frac{r}{x} = \psi\, x + x + x^2, \text{ \&c. \&c.}$$

Example. Let the integral $\int \psi\, x \,.\, d\,x = \int \sqrt{1 + x^n} \,.\, d\,x$ as before. Suppose $\sqrt{1 + x^n} = \frac{x}{2} + \sqrt{1 + r}$, whence we deduce

$$x^n - \frac{x^2}{4} - \sqrt{1 + r}\,.\,x - r = 0,$$

an equation which has n roots, whose product is r. We also find

$$1 + r = 1 + x^n - x\sqrt{1 + x^n} + \frac{x^2}{4},$$

whence

$$\frac{r}{x} = x^{n-1} + \frac{x}{4} - \sqrt{1 + x^n},$$

or

$$\phi\, x = x^{n-1} + \frac{x}{4} - \psi\, x.$$

And therefore since $S\int \phi\, x\, d\,x$ has an algebraic sum $= r +$ const., it follows that $S\int \psi\, x\, d\,x$ has an algebraic sum also, viz.

$$\text{S}\int x^{n-1}\,d\,x + \frac{1}{4}\,\text{S}\int x\,d\,x - (r + \text{const.})$$

or

$$\frac{1}{n}\,\text{S}\,x^n + \frac{1}{8}\,\text{S}\,x^2 - (r + \text{const.})$$

But if the form of the proposed integral $\int \psi\, x \,.\, d\,x$ is complicated, no doubt it would be difficult to find an equation like

$$\frac{r}{x} = \pm\,\psi\,x + m\,x^a + n\,x^b + \ldots\ldots,$$

such that when developed and arranged according to the powers* of x its last term should be r. Probably this is not possible in general. And yet the proposed integrals $\text{S}\int \psi\,x\,d\,x$ may have an algebraic sum. For hitherto we have tacitly supposed that this algebraic quantity, if it existed, was the product of the variables $= r +$ const., since we have derived all our reasonings from the theorem

$$x\,y\,z\ldots = \int y\,z\ldots\ldots d\,x + \int x\,z\ldots\ldots d\,y + \&\text{c}.$$

But it is evident that the algebraic sum may as well have any other form as the one in question. It may be a constant or any symmetrical combination of the variables. The foundation of our reasoning has therefore hitherto been too limited, and requires to be extended. Let us therefore direct our inquiries to the attainment of a more general method.

§ 4. *Exposition of a more general method.*

If $x, y, z \ldots\ldots$ are the roots of any equation,

$$x^n - p\,x^{n-1} + p'\,x^{n-2}\ldots\ldots = 0,$$

then not only the coefficients themselves p, p', p'', &c., but also all combinations of them, are symmetrical functions of the roots. Let v be a general symbol denoting any one of these coefficients or of these combinations. Then v may be considered either as a function of all the roots, or of only one of them. And in the latter case this root may be changed for any of the others without causing any alteration in the value of v.

Example. Let there be two variables x and y, roots of $x^2 - v\,x + 1 = 0$, which may be also written $y^2 - v\,y + 1 = 0$. Then v if considered as a function of both x and y, is equal to $x + y$, the sum of the roots. But if considered as a function of x alone, it is $= \frac{1+x^2}{x}$. And if considered as a function of y alone, it is $= \frac{1+y^2}{y}$.

$$\therefore \frac{1+x^2}{x} = \frac{1+y^2}{y},$$

* The coefficient of the highest power of x being always supposed $= 1$.

or x may be permuted for y. Hence also

$$\varphi \, . \, \frac{1+x^2}{x} = \varphi \, . \, \frac{1+y^2}{y},$$

φ being any function.

Quantities which $\left(\text{like } \varphi \, . \, \frac{1+x^2}{x} \text{ in this example}\right)$ are not changed in value by permuting the roots, may be termed "symmetrical functions" of the variables x, y, z, &c. or simply "symmetricals" of the equation whose roots are $x, y, z \ldots\ldots$ Thus the quantity $\varphi\left(\frac{1+x^2}{x}\right)$ is a symmetrical of the equation

$$x^2 - v\,x + 1 = 0.$$

But the quantity $\varphi\left(\frac{1-x^2}{x}\right)$ is not a symmetrical of it, because $\frac{1-x^2}{x}$ is not equal to $\frac{1-y^2}{y}$.

Ex. 2. Let x, y, z, be roots of $x^3 - v\,x + 1 = 0$, which may also be written $y^3 - v\,y + 1 = 0$, or $z^3 - v\,z + 1 = 0$, whence

$$v = \frac{1+x^3}{x} = \frac{1+y^3}{y} = \frac{1+z^3}{z},$$

whence also

$$\varphi \, . \, \frac{1+x^3}{x} = \varphi \, . \, \frac{1+y^3}{y} = \varphi \, . \, \frac{1+z^3}{z}.$$

Therefore the quantity $\varphi \, . \, \frac{1+x^3}{x}$ is a symmetrical of the equation $x^3 - v\,x + 1 = 0$: but $\varphi \, . \, \frac{1+x^2}{x}$ is not a symmetrical of it, because $\frac{1+x^2}{x}$ is not equal to $\frac{1+y^2}{y}$.

These things being premised, it is evident that the same quantity may be a symmetrical of one equation and not so of another. Therefore the problem arises: *Any quantity being given, to find the equation with respect to which it is symmetrical?*

Ex. 1. Let $\frac{1+x^2}{x}$ be the given quantity. Put $\frac{1+x^2}{x} = v$, v being a general symbol for any symmetrical quantity:

$$\therefore\ x^2 - v\,x + 1 = 0$$

is the required equation, and the indeterminate v is thereby determined to be the sum of the roots.

Ex. 2. Let $\frac{1+x^3}{x}$ be the given quantity. Put $\frac{1+x^3}{x} = v$:

$$\therefore\ x^3 - v\,x + 1 = 0$$

is the required equation, and $-v$ is thereby determined to be the sum of the products of every two roots *.

* *Another example.*—Let $\frac{1+x+x^2}{1-x}$ be the given quantity. Put it $= v$:

$$\therefore\ x^2 + (1+v)\,x + (1-v) = 0$$

is the required equation, and $(1+v)$ is determined to be the sum of the roots $x+y$, and $1-v$ their product $x\,y$. Whence, by eliminating v, we find the following relation between x and y: $x\,y - (x+y) = 2$. This example will be referred to hereafter.

Ex. 3. Let $x + \sqrt{1-x^2}$ be the given quantity. Put $x+\sqrt{1-x^2}=v$:

$$\therefore\ (v-x)^2 = 1 - x^2$$

$$2x^2 - 2vx + (v^2 - 1) = 0$$

$$x^2 - vx + \frac{v^2-1}{2} = 0$$

is the required equation, and v is determined to be the sum of its roots. Also $\frac{v^2-1}{2}$ is equal to their product xy. In other words, the equation must be such, that its roots x, y, answer the condition $xy = \frac{(x+y)^2-1}{2}$.

Ex. 4. Let $x + \sqrt{\frac{1}{x}}$ be the given quantity. Put $x + \sqrt{\frac{1}{x}} = v$:

$$\therefore\ (v-x)^2 = \frac{1}{x}$$

$$\therefore\ x^3 - 2vx^2 + v^2x - 1 = 0$$

is the required equation, which is thereby determined to be a cubic, the product of whose roots $= 1$, and v is found to be half the sum of the roots.

Case of exception.—It is essential to remark, that when the given quantity contains only one power of x, it cannot be a symmetrical. *Ex.* $\sqrt{1+x^n}$ cannot be a symmetrical; for if it could, we should have $\sqrt{1+x^n} = \sqrt{1+y^n} = \sqrt{1+z^n} =$ &c., whence $x = y = z =$ &c.; whereas we suppose the roots to be in general all different from one another. With this exception the required equation may be easily found in most cases by putting the given quantity, or $fx = v$. And if the roots of the equation thus found are denoted by $x, y, z \ldots\ldots$ it is an immediate consequence of the hypothesis that $fx = fy = fz =$ &c. Thus in the last example we have

$$x + \sqrt{\frac{1}{x}} = y + \sqrt{\frac{1}{y}} = z + \sqrt{\frac{1}{z}}.$$

Let us now suppose that $\mathrm{S}\,dx$, the sum of the differentials of the roots, or $dx + dy + dz$ &c., is multiplied by a symmetrical, that is, by one of the above-mentioned quantities fx. The product is $fx\,.\,dx + fx\,.\,dy + fx\,.\,dz +$ &c. But in consequence of the equality $fx = fy = fz =$ &c. the result is the same, if the first term is multiplied by fx, the second by fy, the third by fz, and so on. So that the product is $fx\,.\,dx + fy\,.\,dy + fz\,.\,dz +$ &c., which is our abbreviated notation $= \mathrm{S}fx\,.\,dx$.

$$\therefore fx\,.\,\mathrm{S}\,dx = \mathrm{S}fx\,.\,dx. \qquad (1.)$$

This theorem is of the greatest importance, and will be of constant use in the sequel. It must not be forgotten that it is only true when fx is a symmetrical, and therefore capable of being represented by v. Replacing fx by v, it becomes $v\,\mathrm{S}\,dx = \mathrm{S}\,v\,dx$. In this form it is self-evident, because v remains the same, however the letters $x, y, z, \ldots.$ are permuted.

More generally, if the quantity $S \psi x . d x$, which means $\psi x . dx + \psi y . dy + \psi z . dz +$ &c., is multiplied by $f x$, the product may be exhibited in the form

$$f x \psi x . d x + f y \psi y . d y + f z \psi z . d z + \&c.,$$

which in our notation is $S f x \psi x . d x$. Whence the theorem

$$f x S \psi x . d x = S f x \psi x . d x, \quad \ldots \ldots \ldots \ldots \quad (2.)$$

which may also be put in the self-evident form

$$v S \psi x . d x = S v \psi x . d x.$$

Equation (1.) is a corollary from (2.) when $\psi x = 1$.

These results may be comprised in a general rule, viz. that whatever be the nature of the differential $\psi x . d x$, if we multiply the sum of a series of such quantities, or $S \psi x . d x$, by $f x$ any function of x, the multiplication is effected by introducing $f x$ within the sign S, provided (and this is the essential condition) *that $f x$ is a symmetrical of that equation of which all the variables are roots.* It is upon this principle that the method which I am about to explain chiefly reposes.

Suppose $\int X . d x$ to be the proposed integral, X being any function of x. In the first place we have to determine the number of the other variables y, z, &c., and also the nature of the equation $x^n - p x^{n-1} + \&c. = 0$, of which they are roots. And this may in general be accomplished by the following process: *Assume* X *to be a symmetrical of this unknown equation,* or that $X = v$; then if this equation $X = v$ can be cleared of radicals, &c., (as in examples 1, 2, 3, 4,) it may be ultimately reduced to the form

$$x^n - p x^{n-1} + p' x^{n-2} \ldots \ldots = 0,$$

where p, p', p'', the coefficients, are either constants or functions of v. The index n of this equation determines the number of the variables.

Let Y be a quantity containing the variable y, in the same manner that X contains x, and let Z contain z in the same manner, and so on for the other roots. Then $v = X = Y = Z =$ &c., in consequence of the hypothesis that X is a symmetrical of this equation. Therefore the sum of the following series of differentials,

$$X . d x + Y . d y + Z . d z + \&c.,$$

is equal to $X (d x + d y + d z + \&c.)$

$$= X . S d x = v S d x.$$

Now $S d x = d p$, where p is the coefficient of the second term of the equation

$$x^n - p x^{n-1} + p' x^{n-2} \ldots \ldots = 0;$$

and, as we have before remarked, p is either a constant or a function of $v = \varphi . v$.

First let it be a constant: then

$$d p = S d x = 0,$$

which gives

$$X\,dx + Y\,dy + Z\,dz + \ldots\ldots = v\,S\,dx$$
$$= 0,$$

whence we deduce the very important consequence

$$\int X\,.\,dx + \int Y\,.\,dy + \int Z\,.\,dz + \ldots\ldots = \text{const.},$$

which is true, whatever be the nature of the function X, provided only that the coefficient p is constant.

Secondly, let $p = \varphi\,v$,

$$\therefore\; dp = d\,.\,\varphi\,v,$$
$$\therefore\; X\,.\,dx + Y\,.\,dy + Z\,.\,dz + \&c. = v\,.\,d\varphi\,v,$$
$$\therefore \int X\,.\,dx + \int Y\,.\,dy + \int Z\,.\,dz + \&c. = \int v\,.\,d\varphi\,v\,;$$

and therefore the sum of the integrals, or $S\int X\,.\,dx$, is known, whenever the formula $\int v\,d\varphi\,v$ is capable of integration; or, which is equivalent, when the form $\int \varphi\,v\,.\,dv$ is capable of integration. These consequences flow, with respect to the proposed integral $\int X\,.\,dx$, from the supposition that X is a symmetrical of the equation whose roots the variables are. But a much more general method is attainable, by putting the proposed integral under the form $\int \frac{X}{\psi\,x}\,.\,\psi\,x\,dx$, and then assuming the quantity $\frac{X}{\psi\,x}$ to be a symmetrical of the said equation *.

Therefore $\frac{X}{\psi\,x}$ may be represented by the general symbol v, and the proposed integral by $\int v\,.\,\psi\,x\,.\,dx$.

The series of differentials

$$X\,dx + Y\,dy + Z\,dz + \&c.$$

may therefore be written

$$v\,\psi\,x\,dx + v\,\psi\,y\,dy + v\,\psi\,z\,dz + \&c.$$
$$= v\,(\psi\,x\,dx + \psi\,y\,.\,dy + \&c.),$$

which we write

$$= v\,S\,\psi\,x\,.\,dx.$$

Therefore, whenever it happens that $S\,\psi\,x\,.\,dx = 0$, we have $v\,S\,\psi\,x\,.\,dx = 0$,

$$\therefore \int X\,dx + \int Y\,dy + \int Z\,dz + \&c. = \text{const.}:$$

* Under this new supposition the quantity X of course ceases, in general, to be a symmetrical of the equation.

and since ψx is arbitrary, this condition $S \psi x \, d x = 0$ may frequently be realized. But it will be observed, that every change in the form of ψx changes the equation between the variables. But if $S \psi x \, d x$ is not $= 0$, as it generally is not, it must be a quantity symmetrically composed with respect to all the variables, and therefore a function of v, since all the coefficients of the equation are functions of v, or constants. Therefore it may be represented by $d . \phi v$,

$$\therefore X \, d x + Y \, d y + Z \, d z + \&c. = v \, S \psi x \, d x$$
$$= v . d . \phi v$$

$$\therefore \int X \, d x + \int Y \, d y + \&c. = \int v \, d . \phi v :$$

and therefore the sum of the integrals is known in all those cases in which $\int v \, d . \phi v$ can be integrated.

The most direct and advantageous method of treating any proposed integral $\int X \, d x$, is to make one of the two suppositions above mentioned, viz. $X = v$, or $X = v . \psi x$. But the supposition $X = v + \psi x$, also, often leads to simple and satisfactory results. Our choice, however, is not limited to these forms, but may include others that are comprehended under the general formula $X = f(v, x)$, each of which may perhaps find its application in special cases.

This process, in all its generality, constitutes the method which I now propose. The use and application of it will be best shown by examples.

§ 5.

Direct integration of the formula $\int X . d x$, when that is possible, confirms and illustrates the above results, of which it will be convenient to adduce a few simple examples.

Let there be two variables x, y, roots of $x^2 - v x + 1 = 0$. Therefore the quantity $\frac{1 + x^2}{x} = \frac{1 + y^2}{y} = v$ is a symmetrical of this equation. And we find

$$x + y = v \quad \therefore d x + d y = d v \quad \text{. . . . . (1.)}$$

$$x y = 1 \quad \therefore \frac{d x}{x} + \frac{d y}{y} = 0 \quad \text{. . . . . (2.)}$$

$$x^2 + y^2 = v^2 - 2 \quad \therefore x \, d x + y \, d y = v \, d v ; \quad \text{. . . . . (3.)}$$

which results may be thus written:

$$S \, d x = d v \quad \text{. . . . . (1.)}$$

$$S \frac{d x}{x} = 0 \quad \text{. . . . . (2.)}$$

$$S \, x \, d x = v \, d v . \quad \text{. . . . . (3.)}$$

Multiply each of these equations by the equation $\frac{1+x^2}{x} = v$, and we find

$$\frac{1+x^2}{x} \mathrm{S}\, d\,x = v\, d\, v \quad \ldots \quad (1.)$$

$$\frac{1+x^2}{x} \mathrm{S} \frac{d\,x}{x} = 0 \quad \ldots \quad (2.)$$

$$\frac{1+x^2}{x} \mathrm{S}\, x\, d\,x = v^2\, d\, v. \quad \ldots \quad (3.)$$

But because $\frac{1+x^2}{x}$ is a symmetrical, we may, according to the general principle, introduce it within the sign S.

$$\therefore \mathrm{S}\, \frac{1+x^2}{x}\, d\,x = v\, d\, v \quad \ldots \quad (1.)$$

$$\mathrm{S}\, \frac{1+x^2}{x^2}\, d\,x = 0 \quad \ldots \quad (2.)$$

$$\mathrm{S}\, (1+x^2)\, d\,x = v^2\, d\, v. \quad \ldots \quad (3.)$$

The integrals of these equations are

$$* \mathrm{S} \int \frac{1+x^2}{x}\, d\,x = \frac{v^2}{2} + \text{const.} \quad \ldots \quad (1.)$$

$$\mathrm{S} \int \frac{1+x^2}{x^2}\, d\,x = \text{const.} \quad \ldots \quad (2.)$$

$$\mathrm{S} \int (1+x^2)\, dx = \frac{v^3}{3} + \text{const.} \quad \ldots \quad (3.)$$

And we propose to verify these three results by direct integration. First then we have

$$\int \frac{1+x^2}{x}\, d\,x + \int \frac{1+y^2}{y}\, .\, d\,y = \frac{x^2+y^2}{2} + \log x + \log y + \text{const.}$$

But

$$x\,y = 1 \therefore \log x + \log y = 0,$$

and

$$x^2 + y^2 = v^2 - 2.$$

$$\therefore \int \frac{1+x^2}{x}\, .\, d\,x + \int \frac{1+y^2}{y}\, d\,y = \frac{v^2}{2} + \text{const.} \quad \ldots \quad (1.)$$

Secondly we have

* It is indifferent whether we write $\mathrm{S}\int$ or $\int \mathrm{S}$, and we may remark that the signs $\mathrm{S} \int d$ may often be permuted. Thus if there are two variables,

$$\mathrm{S} \int d\,x = \int \mathrm{S}\, d\,x = x + y$$

$$d\,\mathrm{S}\, x = \mathrm{S}\, d\,x = d\,x + d\,y$$

$$\frac{1}{2} d\,\mathrm{S}\, x^2 = \mathrm{S}\, d \frac{x^2}{2} = x\,d\,x + y\,d\,y.$$

$$\left.\begin{aligned}\int\frac{1+x^3}{x^3}\,d\,x+\int\frac{1+y^2}{y^2}\,d\,y &= (x+y)-\left(\frac{1}{x}+\frac{1}{y}\right)+\text{const.}\\ &= (x+y)-\left(\frac{x+y}{1}\right)+\text{const.}\\ &= \text{const.}\end{aligned}\right\}\ .\ .\ \ (2.)$$

Thirdly we have

$$\int(1+x^2)\,d\,x+\int(1+y^2)\,d\,y=(x+y)+\frac{x^3+y^3}{3}+\text{const.}$$

$$=v+\frac{x^3+y^3}{3}+\text{const.}$$

But the formula makes it

$$=\frac{v^3}{3}+\text{const.}\ .\ .\ .\ .\ .\ .\ .\ .\ .\ .\ .\ .\ .\ .\ .\ (3.)$$

It is necessary therefore to show that these two results are in accordance, or that

$$\frac{v^3}{3}+\text{const.}=v+\frac{x^3+y^3}{3},$$

or that

$$x^3+y^3=v^3-3\,v+\text{const.}$$

This may be shown by multiplying together the equations

$$x^2+y^2=v^2-2,$$
$$x+y=v,$$

which gives

$$x^3+y^3+x\,y\,(x+y)=v^3-2\,v,$$

and since

$$x\,y=1,\ \text{and}\ x+y=v$$
$$x^3+y^3=v^3-3\,v$$

in accordance with the formula.

I will now apply the method to another example, which conducts to a new and interesting property of the cubic parabola. Since $\varphi\frac{1+x^2}{x}$ is a symmetrical of the equation $x^2-v\,x+1=0$, (as has been already remarked) we have, as a particular instance of this,

$$\sqrt{\frac{1+x^2}{x}}=\text{a symmetrical}=\sqrt{v}.$$

Multiply this equation by $S\,d\,x=d\,v$,

$$\therefore\sqrt{\frac{1+x^2}{x}}\,S\,d\,x=\sqrt{v}\,.\,d\,v,$$

or

$$S\sqrt{\frac{1+x^2}{x}}\,.\,d\,x=\sqrt{v}\,.\,d\,v.$$

The sign S being thus transposed because $\sqrt{\frac{1+x^2}{x}}$ is a symmetrical.

The integral of this last equation is

$$S\int\sqrt{\frac{1+x^2}{x}}\,.\,dx = \frac{2}{3}v^{\frac{3}{2}} + C,$$

which means that

$$[1.]\int\sqrt{\frac{1+x^2}{x}}\,.\,dx + \int\sqrt{\frac{1+y^2}{y}}\,.\,dy = \frac{2}{3}(x+y)^{\frac{3}{2}} + C,$$

provided that $xy = 1$. For since x, y, are roots of $x^2 - vx + 1 = 0$, $x + y = v$, a variable quantity, and $xy = 1$ is the only condition which the variables must satisfy. Now assume $x = u^2$, $y = t^2$, and the relation between the new variables will be $u^2 t^2 = 1$, or $ut = 1$; and equation (1.) becomes, when divided by 2,

$$\frac{1}{3}(u^2 + t^2)^{\frac{3}{2}} + C = \int\sqrt{1+u^4}\,.\,du + \int\sqrt{1+t^4}\,.\,dt,$$

whence the following theorem.

If u, t, two ordinates of the cubic parabola, are reciprocals, so that $ut = 1$, then the sum of the two corresponding arcs of the curve* $= \frac{1}{3}(u^2 + t^2)^{\frac{3}{2}} + \text{const.}$

The reader may wish to see this result also verified by direct integration. Since then $ut = 1$, let us write $\frac{1}{u}$ instead of t in the equation

$$\frac{1}{3}(u^2 + t^2)^{\frac{3}{2}} + C = \int\sqrt{1+u^4}\,.\,du + \int\sqrt{1+t^4}\,.\,dt,$$

and it becomes

$$\frac{1}{3}\left(u^2 + \frac{1}{u^2}\right)^{\frac{3}{2}} + C = \int\sqrt{1+u^4}\,.\,du - \int\sqrt{1+u^4}\,.\,\frac{du}{u^4},$$

or

$$\frac{1}{3}\frac{(1+u^4)^{\frac{3}{2}}}{u^3} + C = \int\sqrt{1+u^4}\,.\,du\left(1 - \frac{1}{u^4}\right),$$

which ought to be identically true, whatever be the value of u. To see that it is so in reality, we have only to differentiate the first part of the equation, and we find its differential to be

$$2\sqrt{1+u^4}\,.\,du - \frac{(1+u^4)^{\frac{3}{2}}}{u^4}\,.\,du$$

$$= \sqrt{1+u^4}\,.\,du\left(2 - \frac{1+u^4}{u^4}\right) = \sqrt{1+u^4}\,du\left(1 - \frac{1}{u^4}\right),$$

which is the differential of the second part of the equation.

Let us now show the application of the method to formulæ containing cubic radicals.

* The equation to the cubic parabola, whose coordinates are u, u', being $3u' = u^3$, it follows that the arc $= \int du\sqrt{1+u^4}$.

Resuming the former equations

$$x^2 - v x + 1 = 0, \quad v = \frac{1 + x^2}{x},$$

we have

$$\sqrt[3]{v} = \sqrt[3]{\frac{1 + x^2}{x}}.$$

Multiply this equation by S $d x$ ($= d v$)

$$\therefore \sqrt[3]{v} \,.\, d v = \sqrt[3]{\frac{1 + x^2}{x}} \,.\, \mathrm{S}\, d x.$$

But we may introduce $\sqrt[3]{\frac{1 + x^2}{x}}$ (since it is a symmetrical) within the sign S,

$$\therefore \sqrt[3]{v} \,.\, d v = \mathrm{S} \sqrt[3]{\frac{1 + x^2}{x}} \,.\, d x,$$

and integrating

$$\frac{3}{4} v^{\frac{4}{3}} + \text{const.} = \mathrm{S} \int \sqrt[3]{\frac{1 + x^2}{x}} \,.\, d x.$$

It is plain that the sum of two integrals of the form $\int \sqrt[n]{\frac{1 + x^2}{x}} \,.\, d x$ may be found by a similar process, provided always that $x y = 1$.

Resuming the last example we have

$$\sqrt[3]{v} = \sqrt[3]{\frac{1 + x^2}{x}}.$$

If we multiply this equation by S $\frac{d x}{x}$ instead of S $d x$, we have

$$\sqrt[3]{v} \,.\, \mathrm{S} \frac{d x}{x} = \sqrt[3]{\frac{1 + x^2}{x}}\, \mathrm{S} \frac{d x}{x} = \mathrm{S} \sqrt[3]{\frac{1 + x^2}{x}} \,.\, \frac{d x}{x} = \mathrm{S} \sqrt[3]{\frac{1 + x^2}{x^4}} \,.\, d x.$$

But S $\frac{d x}{x} = 0$ in this example *,

$$\therefore 0 = \mathrm{S} \sqrt[3]{\frac{1 + x^2}{x^4}} \,.\, d x$$

$\therefore$ integrating we find the sum of two integrals of the form $\int \sqrt[3]{\frac{1 + x^2}{x^4}} \,.\, d x$ is a constant, if $x y = 1$.

Since nothing tends more to elucidate a subject than a frequent recurrence to first principles, I will remark that this result also follows at once from the supposition $x y = 1$. For if we write $\frac{1}{x}$ for y,

$$\int \sqrt[3]{\frac{1 + x^2}{x^4}} \,.\, d x + \int \sqrt[3]{\frac{1 + y^2}{y^4}} \,.\, d y$$

becomes

$$\int \sqrt[3]{\frac{1 + x^2}{x^4}} \,.\, d x - \int \sqrt[3]{x^4 + x^2} \,.\, \frac{d x}{x^2}$$

$$= \int \sqrt[3]{\frac{1 + x^2}{x^4}} \,.\, d x - \int \sqrt[3]{\frac{1 + x^2}{x^4}} \,.\, d x = \int 0 = \text{const.}$$

* See page 200; or page 208, note.

We are now in possession of principles which enable us to attack the general problem, "To find an algebraic relation between n variables $x, y, z \ldots\ldots$, such that

$$\int \phi\,(\mathrm{X})\, d\,x + \int \phi\,(\mathrm{Y})\, d\,y + \int \phi\,(\mathrm{Z})\, d\,z + \&c. = \text{const.},$$

X being a polynomial of n dimensions with constant coefficients of the form

$$x^n - a\,x^{n-1} + b\,x^{n-2} + \&c. + h\,x + k,$$

and containing at least two distinct powers of x; and ϕ being any function whatever of the said polynomial.

It does not appear that any mathematician has hitherto proposed this problem. The principles of our method lead to the following solution:

Let X, or $x^n - a\,x^{n-1} + \ldots\ldots + h\,x + k = v$, v being a variable quantity susceptible of any value *.

$$\therefore x^n - a\,x^{n-1} + \ldots\ldots + h\,x + (k - v) = 0.$$

This equation has only one variable coefficient, viz. $(k - v)$. Therefore the values of its n roots depend upon v, so far, at least, that when v changes its value, each root (generally speaking) undergoes a corresponding change. Also the sum of the roots $x + y + z + \ldots\ldots = a$ is constant.

$$\therefore d\,x + d\,y + d\,z + \ldots\ldots = 0,$$

or

$$\mathrm{S}\,d\,x = 0.$$

Since $v = \mathrm{X}$, $\phi\,v = \phi\,\mathrm{X}$. Multiply this equation by $\mathrm{S}\,d\,x = 0$,

$$\therefore \phi\,v\,\mathrm{S}\,d\,x = \phi\,\mathrm{X}\,.\,\mathrm{S}\,d\,x$$

$$\therefore 0 = \phi\,\mathrm{X}\,.\,\mathrm{S}\,d\,x = \mathrm{S}\,\phi\,\mathrm{X}\,.\,d\,x$$

(because $\phi\,\mathrm{X}$ is a symmetrical of this equation)

$$\therefore \text{const.} = \mathrm{S}\int \phi\,\mathrm{X}\,.\,d\,x,$$

which therefore is the required solution of the problem.

Example.—Let $\int \sqrt[3]{x^3 + x + 1}\,.\,d\,x$ be the proposed integral. Assume $x^3 + x + 1 = v$,

$$\therefore x^3 + x + (1 - v) = 0.$$

* To suppose $\mathrm{X} = v$ is the same as to suppose X to be a symmetrical of the equation between the variables (as recommended at pages 198, 200). Whence also $\phi\,\mathrm{X}$ is a symmetrical of the same equation. The symbol v retains the same meaning as before, viz. that of a quantity independent of x, or which continues to have the same value when x is permuted for any other root of the equation. I shall give it this meaning throughout the present memoir.

The solution given in the text may be expressed in other words, by saying that any two of the variables, as for instance x and y, are mutually connected by the equation

$$x^n - a\,x^{n-1} + \ldots\ldots + h\,x = y^n - a\,y^{n-1} + \ldots\ldots + h\,y,$$

whence of course it follows that X, or $x^n - a\,x^{n-1} + \ldots\ldots + h\,x + k$, does not change its value when x is permuted for y, and therefore it may properly be denoted by v, according to the acceptation which we have hitherto given to that letter.

Attribute to v any numerical value, and let the three roots of the equation then be m, m', m''. And when v has some other value, let the roots be n, n', n''. So that while v has changed progressively from one value to the other, the root m has progressively changed its value to n, the root m' to n', and the root m'' to n''.

These things being thus understood, the meaning of the theorem is, that the value of the integral $\int d\,x \sqrt[3]{x^3 + x + 1}$ taken between the limits $x = m$, $x = n$,

+ its value between the limits m', n',
+ its value between the limits m'', n'',
= a constant.

If the question be viewed geometrically, since the roots of an equation are the intersections of a curve with its axis, a progressive change in the value of $(k-v)$, the absolute term, is equivalent to a displacement of the axis parallel to itself, in consequence of which all the intersections change their places simultaneously.

In the case of two variables, we have simply

$$X = x^2 - a\,x + b = v,$$

or

$$x^2 - a\,x + (b - v) = 0.$$

And if x, y, are the roots of this equation, the theorem becomes

$$\int \phi X . d\,x + \int \phi Y . d\,y = \text{const.},$$

ϕ being any function.

Now in this particular case the theorem admits of a very simple demonstration. For since $x + y = a$, $y = a - x$; and substituting this value in $Y = y^2 - a\,y + b$, it becomes $(a - x)^2 - a\,(a - x) + b = x^2 - a\,x + b$: also $d\,y$ becomes $-\,d\,x$.

$\therefore \phi\,(y^2 - a\,y + b)\,d\,y$ becomes $-\,\phi\,(x^2 - a\,x + b)\,d\,x$.

Therefore

$$\phi\,(x^2 - a\,x + b)\,d\,x + \phi\,(y^2 - a\,y + b)\,d\,y = 0,$$

or

$$\phi X . d\,x + \phi Y . d\,y = 0$$

$$\therefore \int \phi X . d\,x + \int \phi Y . d\,y = \text{const.},$$

which was to be demonstrated.

Let $X = x^n - a\,x^{n-1} + \ldots.$ as before, it may be shown upon the same principles that $S \int \phi X . x^m\,d\,x = \text{const.}$, provided $S\,x^m\,d\,x = 0$, or $S\,x^{m+1}$ is constant, that is to say, does not contain v; which depends on the relative values of m and n. Also we may obtain in a similar manner the solution of the following problem, viz.

$$S \int \phi \left(\frac{X}{X'}\right) d\,x = \text{const.},$$

where X is a polynomial of n dimensions, and X' another polynomial of not more than $n - 2$ dimensions. For, putting

$$\frac{X}{X'} = v,$$

x, y, &c. &c. are roots of

$$X - v X' = 0,$$

which is of the form

$$x^n - a x^{n-1} + (b - v) x^{n-2} + \&c. = 0,$$

where

$$S x = a = \text{const.},$$

and therefore

$$S d x = 0 \;\therefore S \varphi \left(\frac{X}{X'}\right) d x = \varphi \left(\frac{X}{X'}\right) S d x = 0,$$

$$\therefore S \int \varphi \left(\frac{X}{X'}\right) d x = \text{const.}$$

I will now add several examples, and I request the reader's attention to the *directness* with which their solutions are obtained by means of the foregoing principles. In the present paper I have avoided the use of transformations, except that of $x = u^n$, because they are unnecessary to the success of the method, and that I am here considering general principles rather than individual results.

§ 6. *Examples.*

Ex. 1. Let the proposed integral be

$$\int \frac{d x}{\sqrt{1 - x^3}}.$$

This is Mr. Lubbock's first example in his paper on Abel's theorem in the Philosophical Magazine*. The result which he finds is equivalent to this, that if x and y satisfy the equation

$$x y - (x + y) = 2,$$

then

$$\int \frac{d x}{\sqrt{1 - x^3}} + \int \frac{d y}{\sqrt{1 - y^3}} = \text{const.}$$

For the sake of comparison I will take this as the first example of my method, and supposing its solution to be unknown, proceed to investigate it as follows:

$\int \frac{d x}{\sqrt{1 - x^3}}$ may be put under the form

$$\int \frac{d x}{(1 - x) \sqrt{\frac{1 + x + x^2}{1 - x}}}.$$

Put

$$\frac{1 + x + x^2}{1 - x} = v$$

$$\therefore x^2 + (1 + v) x + (1 - v) = 0,$$

x and y must be the roots of this equation†,

* Vol. vi. p. 118.

† See the note in page 196.

$$\therefore x + y = -(1 + v) \quad x y = 1 - v$$

$$\therefore x y - (x + y) = 2,$$

which is the equation of condition found by Mr. LUBBOCK.

Again, the sum of the integrals

$$= \int \frac{dx}{1-x} \sqrt{\frac{1}{v}} + \int \frac{dy}{1-y} \sqrt{\frac{1}{v}}$$

$$= \int \sqrt{\frac{1}{v}} \left(\frac{dx}{1-x} + \frac{dy}{1-y} \right) = \int 0 = \text{const.}$$

because, since

$$x y - (x + y) = 2$$

$$(1 - x)(1 - y) = 1 + x y - (x + y) = 3$$

$$\therefore \log (1 - x) + \log (1 - y) = \log 3$$

$$\therefore \frac{dx}{1-x} + \frac{dy}{1-y} = 0$$

$\therefore$ sum of the integrals = const. *Q. E. D.*

Ex. 2. To find the sum of *three* integrals of the same form.

$\int \frac{dx}{\sqrt{1-x^3}}$ may be written $\int \frac{dx}{x} \sqrt{\frac{x^2}{1-x^3}}$. Put $\frac{x^2}{1-x^3} = \frac{1}{v}$,

$$\therefore x^3 + v x^2 - 1 = 0.$$

The three variables x, y, z, must be roots of this equation,

$$\therefore x y z = 1, \text{ and } x y + x z + y z = 0.$$

Here we have

$$\sqrt{\frac{x^2}{1-x^3}} = \sqrt{\frac{1}{v}}.$$

Multiply this by $\text{S} \frac{dx}{x} = 0$*,

$$\therefore \sqrt{\frac{x^2}{1-x^3}} \text{S} \frac{dx}{x} = 0.$$

But

$$\sqrt{\frac{x^2}{1-x^3}} \text{S} \frac{dx}{x} = \text{S} \sqrt{\frac{x^3}{1-x^3}} \cdot \frac{dx}{x} = \text{S} \frac{dx}{\sqrt{1-x^3}}$$

$$\therefore \text{S} \frac{dx}{\sqrt{1-x^3}} = 0 \quad \therefore \text{S} \int \frac{dx}{\sqrt{1-x^3}} = \text{const.}$$

* In any equation whose last term is constant, $\text{S} \frac{dx}{x} = 0$. For

$$\text{S} \frac{dx}{x} = \frac{dx}{x} + \frac{dy}{y} + \frac{dz}{z} + \&c.$$

$$= d \log x + d \log y + \&c.$$

$$= d \,.\, \log (x y z \ldots .)$$

$$= d \,.\, \text{const.} = 0.$$

$\therefore$ the sum of the three integrals is constant, provided that $xy + xz + yz = 0$, and that $xyz = 1$. It will be observed that the solution is simpler in the case of three integrals than of two.

Ex. 3. Supposing the relation between x, y, z the same as in the last example, to find the sum of three integrals of the form $\int \frac{x\,dx}{\sqrt{1-x^3}}$. Since

$$\sqrt{\frac{1}{v}} = \frac{x}{\sqrt{1-x^3}},$$

or in other words, since $\frac{x}{\sqrt{1-x^3}}$ is a symmetrical of the equation

$$x^3 + v\,x^2 - 1 = 0,$$

if it be multiplied by S $d\,x$ the result will be

$$\mathrm{S}\frac{x\,dx}{\sqrt{1-x^3}}.$$

Also

$$\mathrm{S}\,x = -v \quad \therefore \mathrm{S}\,dx = -dv$$

$$\therefore \mathrm{S}\frac{x\,dx}{\sqrt{1-x^3}} = \sqrt{\frac{1}{v}}\,\mathrm{S}\,dx = \frac{-dv}{\sqrt{v}}.$$

Therefore

$$\mathrm{S}\int\frac{x\,dx}{\sqrt{1-x^3}} = \text{const.} - 2\sqrt{v}.$$

Ex. 4. The same suppositions continuing, required the sum of three integrals of the form $\int\frac{x^2\,dx}{\sqrt{1-x^3}}$. As before,

$$\frac{x}{\sqrt{1-x^3}} = \sqrt{\frac{1}{v}}.$$

Multiply by S $x\,d\,x$,

$$\therefore \mathrm{S}\frac{x^2\,dx}{\sqrt{1-x^3}} = \sqrt{\frac{1}{v}}\,\mathrm{S}\,x\,dx = dv\sqrt{v},$$

(because the equation $x^3 + v\,x^2 - 1 = 0$ gives $\mathrm{S}\,x^2 = v^2 \;\therefore \mathrm{S}\,x\,dx = v\,dv$)

$$\therefore \mathrm{S}\int\frac{x^2\,dx}{\sqrt{1-x^3}} = \frac{2}{3}v^{\frac{3}{2}} + \text{const.}$$

But since $\int\frac{x^2\,dx}{\sqrt{1-x^3}}$ is a form which is readily integrable *per se*, it will naturally be asked whether the result of direct integration is the same as that given by our formulæ. This example therefore affords a convenient opportunity of showing the close accordance between this branch of the integral calculus and the theory of algebraic equations.

By direct integration,

$$\mathrm{S}\int\frac{x^2\,dx}{\sqrt{1-x^3}} = -\frac{2}{3}\left(\sqrt{1-x^3} + \sqrt{1-y^3} + \sqrt{1-z^3}\right) + \text{const.}$$

And by our method,

$$S\int \frac{x^2\,dx}{\sqrt{1-x^3}} = \frac{2}{3}v^{\frac{3}{2}} + \text{const.}$$

∴ it remains to verify that

$$\sqrt{1-x^3} + \sqrt{1-y^3} + \sqrt{1-z^3} = -v^{\frac{3}{2}}.$$

In order to demonstrate this, let us resume the original equation $x^3 + v\,x^2 - 1 = 0$, which gives $v\,x^2 = 1 - x^3$,

$$\therefore \sqrt{v}\,.\,x = \sqrt{1-x^3},$$

and similarly,

$$\sqrt{v}\,.\,y = \sqrt{1-y^3}$$

$$\sqrt{v}\,.\,z = \sqrt{1-z^3}$$

$$\therefore \sqrt{1-x^3} + \sqrt{1-y^3} + \sqrt{1-z^3} = \sqrt{v}\,(x+y+z)$$

$$= \sqrt{v}\,(-v) = -v^{\frac{3}{2}}. \quad \textit{Q.E.D.}$$

Ex. 5. $\int \frac{dx}{\sqrt{(x^3+x^2)} + \sqrt[3]{x^3+x^2}}$.

This is a function of the binomial $x^3 + x^2$, which being put $= v$, we have

$$x^3 + x^2 - v = 0.$$

The three roots of this equation are the variables that answer the problem.

Putting $\sqrt{v} + \sqrt[3]{v} = \varphi\,v$, the sum of the three integrals becomes

$$\int \frac{dx}{\varphi\,v} + \int \frac{dy}{\varphi\,v} + \int \frac{dz}{\varphi\,v} = \int \frac{1}{\varphi\,v}\,S\,dx\,;$$

but $S\,dx = 0$, because $x + y + z = -1$,

$$\therefore \text{ sum of integrals } = \int 0 = \text{const.}$$

Ex. 6. $\int dx\,\sqrt{1+x^n}$.

Here we cannot suppose $\sqrt{1+x^n} = v$ a symmetrical quantity, because that would amount to the supposition $\sqrt{1+x^n} = \sqrt{1+y^n} = \sqrt{1+z^n} = \&c.$, which implies that $x = y = z = \&c.$, whereas we suppose the roots to be in general all different from one another. This is the reason why it was remarked before, that it was requisite the polynomial should contain at least two distinct powers of x. When that is not the case, a second power of x must be introduced. There are several ways of doing this; the simplest is the following:

Put $\int dx\,\sqrt{1+x^n}$ under the form $\int x\,dx\,\sqrt{\frac{1+x^n}{x^2}}$. Assume $\frac{1+x^n}{x^2} = v$,

$$\therefore x^n - v\,x^2 + 1 = 0\,;$$

an equation of n dimensions, of which v is the only variable coefficient. The n roots of this equation answer the problem.

The sum of n integrals becomes

$$\int x\,dx\,\sqrt{v} + \int y\,dy\,\sqrt{v} + \&c. = \int \sqrt{v}\,.\,\mathrm{S}\,x\,dx.$$

If n is a number greater than 4, $\mathrm{S}\,x^2 = 0$:

$$\therefore \mathrm{S}\,x\,dx = 0 \quad \therefore \text{sum of } n \text{ integrals} = \text{const.}$$

If $n = 4$, $\mathrm{S}\,x^2 = 2\,v$, $\therefore \mathrm{S}\,x\,dx = dv$,

$$\therefore \text{sum of four integrals} = \int \sqrt{v}\,.\,dv = \frac{2}{3}v^{\frac{3}{2}} + \mathrm{C}.$$

If $n = 3$, $\mathrm{S}\,x^2 = v^2$, $\therefore \mathrm{S}\,x\,dx = v\,dv$,

$$\therefore \text{sum of three integrals} = \int v^{\frac{3}{2}}\,dv = \frac{2}{5}v^{\frac{5}{2}} + \mathrm{C}.$$

But when n is greater than 4, the equation has impossible roots, therefore the solution is imaginary. Although, as LEGENDRE has demonstrated*, these imaginary cases do not cease to have a real analytical meaning; the sum of two imaginary integrals forming a real integral in a manner analogous to that in which two imaginary roots of an equation form a real sum and product.

But we may avoid these imaginary solutions by putting $\int dx\sqrt{1 + x^n}$ in the form

$$\int dx\,(a + b\,x + c\,x^2 \ldots\ldots)\sqrt{\frac{1 + x^n}{(a + b\,x + c\,x^2 \ldots\ldots)^2}}.$$

Assuming, then,

$$\frac{1 + x^n}{(a + b\,x + c\,x^2 \ldots\ldots)^2} = v,$$

we may attribute to the polynomial any number of terms suitable to the exponent n†, and then it is in most cases possible to find such numerical values for the constant coefficients a, b, c, &c., that the resulting equation shall have all its roots real.

Each integral, then, has the form

$$\int dx\,(a + b\,x + c\,x^2 \ldots\ldots)\sqrt{v},$$

and the sum of all

$$= \int \sqrt{v}\,\mathrm{S}\,dx\,(a + b\,x + c\,x^2 \ldots\ldots),$$

where $\mathrm{S}\,dx\,(a + b\,x + c\,x^2 \ldots\ldots)$ = the aggregate of the partial sums

$$a\,\mathrm{S}\,dx + b\,\mathrm{S}\,x\,dx + c\,\mathrm{S}\,x^2\,dx + \ldots\ldots,$$

which is the differential of

$$a\,\mathrm{S}\,x + \frac{b}{2}\,\mathrm{S}\,x^2 + \frac{c}{3}\,\mathrm{S}\,x^3 + \ldots\ldots,$$

and may therefore be expressed in terms of v, since the quantities $\mathrm{S}\,x$, $\mathrm{S}\,x^2$, $\mathrm{S}\,x^3$, &c. are readily found in terms of v by the usual doctrine of algebraic equations.

* Fonctions Elliptiques, vol. iii. p. 326.

† In general the number of its terms may be $\frac{n}{2}$ or $\frac{n+1}{2}$.

Ex. 7. $\int dx \sqrt[3]{1 + x^n}$.

This may be put in the form

$$\int dx\,(a + b\,x \ldots\ldots) \sqrt[3]{\frac{1 + x^n}{(a + b\,x + \ldots.)^3}};$$

and putting

$$\frac{1 + x^n}{(a + b\,x + \ldots.)^3} = v,$$

the reasoning is the same nearly as in the preceding case. The same principles are applicable to the more general integral $\int dx \sqrt[m]{1 + x^n}$, m being a whole number.

These solutions give the algebraic sum of n integrals of the proposed form. But this number n may be reduced by various methods to a lower number, which is the minimum that the problem admits of: ex. gr. the lowest number of integrals of the form $\int dx\ \sqrt{1+x^4}$ which have an algebraic sum is *two*; of the form $\int dx\ \sqrt{1+x^5}$ is *three*; of the form $\int dx \sqrt{1+x^{10}}$ is likewise *three*, &c. &c., which subject I shall treat of in a subsequent section.

Ex. 8. $\int \frac{dx}{\sqrt[3]{x^3 - 1}}$.

First solution. Put $x^3 = t$, and the integral becomes

$$\frac{\frac{1}{3}\,t^{-\frac{2}{3}}\,dt}{\sqrt[3]{t - 1}} = \frac{1}{3} \cdot \frac{dt}{\sqrt[3]{t^3 - t^2}}.$$

Put $t^3 - t^2 = v$, or $t^3 - t^2 - v = 0$. The three roots of this equation answer the problem.

$$\therefore \text{ the sum of three integrals } = \int \frac{\frac{1}{3}\,\mathrm{S}\,dt}{\sqrt[3]{v}} = \int 0 = \text{const.}$$

(because $\mathrm{S}\,t = 1$, being the coefficient of the second term of the equation $t^3 - t^2 - v = 0$ taken negatively, whence $\mathrm{S}\,dt = 0$).

Second solution. Put $x^3 = t^2$, and the integral becomes

$$\frac{\frac{2}{3}\,t^{-\frac{1}{3}}\,dt}{\sqrt[3]{t^2 - 1}} = \frac{2}{3}\,\frac{dt}{\sqrt[3]{t^3 - t}}.$$

Put $t^3 - t = v$,

$$\therefore t^3 - t - v = 0,$$

and the roots of this equation answer the problem.

$$\therefore \text{ the sum of three integrals } = \int \frac{2}{3}\,\frac{\mathrm{S}\,dt}{\sqrt[3]{v}} = \int 0 = \text{const.}$$

(because $\mathrm{S}\,t = 0$ in the equation $t^3 - t - v = 0\ \therefore \mathrm{S}\,dt = 0$), and the sum of the integrals therefore reduces itself in this case also to a constant.

It will probably be satisfactory to the reader to see some one of these results verified by arithmetical computation. Let us therefore select this last example for that purpose.

§ 7. *Example of an arithmetical calculation of the sum of three Integrals.*

The preceding analysis shows that the sum of the three integrals

$$\int \frac{dx}{\sqrt[3]{x^3-1}} + \int \frac{dy}{\sqrt[3]{y^3-1}} + \int \frac{dz}{\sqrt[3]{z^3-1}} = \text{const.}$$

if $x^{\frac{3}{2}}, y^{\frac{3}{2}}, z^{\frac{3}{2}}$ are roots of the equation

$$t^3 - t - v = 0.$$

But the form of this equation shows that the sum of its roots $= 0$, the sum of the products of every two roots $= -1$, while the product of all the roots is a variable quantity $= v$; ∴ the quantities x, y, z must satisfy the two following equations,

$$x^{\frac{3}{2}} + y^{\frac{3}{2}} + z^{\frac{3}{2}} = 0$$

$$(xy)^{\frac{3}{2}} + (xz)^{\frac{3}{2}} + (yz)^{\frac{3}{2}} = -1.$$

And if they do so, we shall have

$$\int_x + \int_y + \int_z = \text{const.},$$

denoting by $\int_x$ the integral $\int \frac{dx}{\sqrt[3]{x^3-1}}$.

But in order to eliminate the constant, we may take three other variables x', y', z', satisfying the same two equations of condition, and thence deduce

$$\int_{x'} + \int_{y'} + \int_{z'} = \text{const.}$$

Whence by subtraction we eliminate the constant

$$\left(\int_x - \int_{x'}\right) + \left(\int_y - \int_{y'}\right) + \left(\int_z - \int_{z'}\right) = 0. \quad \ldots\ldots \quad [1.]$$

Now by the usual methods we find that the equations of condition are satisfied by the values

$$x = \cdot 352342$$
$$y = \cdot 917532$$
$$z = 1\cdot 057860,$$

and also by the values

$$x' = \cdot 392456$$
$$y' = \cdot 900227$$
$$z' = 1\cdot 065602^*.$$

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* These values give

$x^{\frac{3}{2}} =$	$-0\cdot 209149$	$x'^{\frac{3}{2}} =$	$-0\cdot 245862$
$y^{\frac{3}{2}} =$	$-0\cdot 878885$	$y'^{\frac{3}{2}} =$	$-0\cdot 854138$
$z^{\frac{3}{2}} =$	$1\cdot 088034$	$z'^{\frac{3}{2}} =$	$1\cdot 100000$
Sum $=$	0	Sum $=$	0

This verifies the first equation of condition. The squares of these quantities are

It remains therefore to try by actual calculation whether these values satisfy the equation [1.].

$$\int_x = \int \frac{dx}{\sqrt[3]{x^3 - 1}} = x + \frac{1}{3} \cdot \frac{x^4}{4} + \frac{1.4}{3.6} \cdot \frac{x^7}{7} + \frac{1.4.7}{3.6.9} \cdot \frac{x^{10}}{10} + \&c.$$

$$\int_{x'} = \int \frac{dx'}{\sqrt[3]{x'^3 - 1}} = x' + \frac{1}{3} \cdot \frac{x'^4}{4} + \frac{1.4}{3.6} \cdot \frac{x'^7}{7} + \frac{1.4.7}{3.6.9} \cdot \frac{x'^{10}}{10} + \&c.$$

$\therefore$ putting $x' - x = \Delta x$, $x'^4 - x^4 = \Delta (x^4)$ &c.

$$\int_{x'} - \int_x = \Delta x + \frac{1}{3} \frac{\Delta (x^4)}{4} + \frac{1.4}{3.6} \cdot \frac{\Delta (x^7)}{7} + \frac{1.4.7}{3.6.9} \cdot \frac{\Delta (x^{10})}{10} + \&c.$$

and since $\Delta x = \cdot 04011$ is a small quantity, we readily find the sum of the series $= \cdot 04083$. Treating the other variables in the same manner, the result obtained is

$$X = \int_{x'} - \int_x = \cdot 040834$$

$$Y = \int_{y'} - \int_y = \cdot 027526$$

$$Z = \int_{z'} - \int_z = \cdot 013315.$$

With regard to the signs, it appears that the integral X has a sign opposed to that of the other two. We find therefore finally,

$$\begin{aligned} Y + Z &= \cdot 040841 \\ X &= \cdot 040834 \\ \hline \therefore Y + Z - X &= \cdot 000007. \end{aligned}$$

On the other hand the formula gives $Y + Z - X = 0$, rigorously. Therefore the computation is only in error in the sixth place of decimals, which in consequence of the prolixity of these calculations may be considered to be a sufficient trial of its accuracy.

$x^3 = \cdot 043743$	$x'^3 = \cdot 060448$
$y^3 = \cdot 772439$	$y'^3 = \cdot 729552$
$z^3 = 1 \cdot 183818$	$z'^3 = 1 \cdot 210000$
Sum = 2	Sum = 2

Squaring the equation $x^{\frac{3}{2}} + y^{\frac{3}{2}} + z^{\frac{3}{2}} = 0$, we have

$$(x^3 + y^3 + z^3) + 2 (\overline{x y}|^{\frac{3}{2}} + \overline{x z}|^{\frac{3}{2}} + \overline{y z}|^{\frac{3}{2}}) = 0,$$

and substituting the value just found of $x^3 + y^3 + z^3 = 2$, we have

$$\overline{x y}|^{\frac{3}{2}} + \overline{x z}|^{\frac{3}{2}} + \overline{y z}|^{\frac{3}{2}} = -1,$$

which verifies the second equation of condition.

Note.—The integrals comprised in the formula $\int \frac{P\,dx}{\sqrt{R}}$ have been called ultra-elliptic by LEGENDRE. I think I have sufficiently shown that no line of distinction can be drawn between them and integrals in general; all of which, that are functions of a given polynomial, possess the property which was supposed to characterize the ultra-elliptic class.

PHILOSOPHICAL TRANSACTIONS.

XV. The Bakerian Lecture.—*On the Tides at the Port of London.*

By John William Lubbock, *Esq. F.R.S.*

Received June 9,—Read June 16, 1836.

THE discussions of tide observations which I have had the honour to lay before the Society on different occasions, have been instituted with reference to the transit of the moon immediately preceding the time of high water. The Tables which I have thus prepared for London and Liverpool, in order to serve for predicting the phenomena, answer the purpose for which they were intended, and may also afford some notions with respect to the laws of the phenomena, and to the degree of accuracy of which the inquiry is susceptible, impeded by the rude manner in which the observations are made, and by accidents. But when the discussion is instituted with reference to the transit immediately preceding the time of high water, the law of the variations in the interval between the moon's transit and the time of high water is obscured.

The discussion of nineteen years' observations of tides at the London Docks, which I now offer, has been made with reference to the moon's transit two days previous, and will, I trust, be viewed with interest, for it proves that the laws to which the phenomena are subject accord generally with the views propounded long since by Bernoulli.

Mr. Stratford states in the preface to the Nautical Almanac, that he employs manuscript tables for computing the time of high water at London Bridge, founded upon my principal Table III., given at page 401 of the Philosophical Transactions for 1831, and he states that this table has been reconstructed. But my table is founded upon so great a number of observations, and the law agrees with theory so remarkably, that Mr. Stratford's alterations cannot be important. I have reason to think that Mr. Stratford employs my corrections for the variations of the moon's parallax and declination, but that he deduced the *calendar-month inequality in the interval* by a process similar to that which I employed in the Philosophical Transactions for 1834, Part I. The nature of this inequality, which has not been yet understood, is clearly shown in this paper. When the moon's transit immediately preceding the time of high water is taken as the *argument*, this inequality arises chiefly from the variation in the interval between successive transits of the moon*.

I shall now endeavour to explain Bernoulli's solution of the problem, in order to render intelligible the comparisons between theory and observation which accompany this paper. Let us allow for an instant, that were the earth a perfect sphere covered throughout by a fluid, the fluid would assume the same form at any given instant as it would do if the forces then acting upon each particle were invariable in magnitude and direction. The actual approximation to this state of things is greater in the southern hemisphere than in northern latitudes, and on our coasts. Moreover, let us suppose that the tide-wave is subject to this law at the Cape of Good Hope, or in some region still more remote, and that it is propagated along the Atlantic Ocean and round our island, "according to the stamp first set upon it by the moon's pressure." Upon these suppositions, which are virtually those of Bernoulli, and which may be said to constitute the equilibrium-theory, it is easy to calculate the variations in the time and height of high water at any given place, if the time in which the tide-wave is propagated does not vary. The results which are contained in this paper are intended to assist in determining how far the phenomena accord with these suppositions.

The tide-wave travels from the Cape of Good Hope to Gibraltar in about twelve hours, from Gibraltar to Edinburgh in about twelve hours, and from Edinburgh to London in about the same time. I have shown that the *retard* at Brest is considerably less than at London; and there can be no doubt that at the Cape of Good Hope it is less than at Brest.

Bernoulli's theory may be considered as proceeding upon these principles. Bernoulli calculated tables for some of the corrections, but he did not explain with sufficient precision the manner in which these tables must be used. I allude here particularly to Bernoulli's parallax correction for the interval, p. 165. He says, "Pour se servir de cette table, il ne faudra plus qu'ajouter aux nombres des six dernières colonnes l'heure moyenne du port." But it is not sufficient to increase the argument of the table, which is the angular distance between the luminaries, by twenty degrees, as Bernoulli supposes, in order to accommodate the table to the reasoning in p. 161, where he says, "Et enfin on trouve une conformité exacte entre les deux points en question, en donnant un jour et demi au retardement des marées, c'est-à-dire, en supposant que l'état des marées est tel qu'il devroit être naturellement, un jour et demi plutôt." Although this supposition is admissible, at Brest, for example, and although Bernoulli's table would I think afford the true correction in the interval between the moon's transit and the time of high water, in the case of a perfect sphere covered by an ocean, by applying it to the transit of the moon *immediately*

* See Table XXIII.

preceding, still this is not the case actually, and no approximation even would be obtained to the true parallax or calendar-month inequality in this manner. Nor did BERNOULLI indicate the great difference in the *retard*, or *age of the tide*, at different places; and he appears to have attributed this retard* to the inertia of the water, an error which LAPLACE pointed out. The difficulty to which I have alluded in ascertaining the correct interval between a given transit of the moon and the time of high water does not influence so much the calculation of the heights, because the parallax and declination corrections for the height change very little with the moon's age.

In forming future discussions similar to that contained in this paper, it is desirable that they should be instituted with reference to the same transit of the moon, namely, with reference to the transit which precedes the time of high water at London by about 51 hours †; otherwise even the variations in the heights will not be immediately comparable with those here given, and the variations in the intervals will be very different. This may be seen by comparing the tables in this paper with those which I obtained formerly with Mr. DESSIOU's assistance, and in which the discussion of the same observations was instituted with reference to the transit immediately preceding the time of high water. The variations in the interval between two successive transits of the moon are, in fact, of the same order in amount as those in the interval between the moon's transit and the time of high water due to the variations in magnitude of the attractive forces; and when the interval between the time of high water and the moon's transit *immediately preceding* is considered, (at least on our coasts,) the variations from both these causes are mixed up together.

As the tide-wave travels northward to the coasts of Great Britain from the Cape of Good Hope, passing the French coast, the variations in the *interval* and in the height at Brest must be similar to those at London and Liverpool. My results ought, therefore, to agree with those which may hereafter be deduced from the observations made at Brest by order of the French Government, and not yet published. The Brest observations may, however, be rather more accurate than those to which I have had access; and as the tide is single, the diurnal inequality is perhaps there more distinct.

Although the imperfection of observations renders it indispensable to employ the average of a great number in order to deduce with safety any conclusions, this is equally required on account of the influence of what may be termed accidents, such as the winds and the varying pressure of the atmospheric column. M. DAUSSY has

* "Nous avons encore fait voir, que sans le concours des causes secondes les plus grandes marées devroient se faire dans les syzygies et les plus petites dans les quadratures. Cependant on a observé, que les unes et les autres se font un ou deux jours plus tard. Ce retardement est encore produit, si non pour le tout, au moins en partie par l'inertie des eaux qui doivent être mises en mouvement et qui ne sauroient obéir assez promtement aux forces qui les sollicitent, pour leur faire suivre les loix que ces forces demanderoient."—p. 158. I use the word *retard* after BERNOULLI.

† This is also desirable with reference even to the *establishment* of ports, for obvious reasons.

ascertained that at Brest the height of high water varies inversely as the height of the barometer, and that the ocean rises ·223 metre, or 8·78 inches, for a depression of ·0158 metre, or ·622 inch, in the barometer *.

In order to confirm this interesting result, and to ascertain the variation in the height of high water at Liverpool simultaneous with the variation of the atmospheric pressure, I requested Mr. Dessiou to calculate from our tables the heights and times of high water at Liverpool for the year 1784, and to compare the errors of the calculated heights and times with the heights of the barometer as recorded by Mr. Hutchinson for that year. The errors were divided into three categories:

	inches.		inches.
Those for a height of barometer between	29	and	29·50
———	29·50	and	30
———	30	and	30·50

	inches.				inches.
For	29·25,	mean of	148	results, we found the mean error in the calculated heights of high water...	−5·6
	29·76,	———	328		+ ·1
	30·16,	———	232		+4·5

Hence we may say roughly that at Liverpool a fall of one tenth of an inch in the barometer raises the tide an inch, *cæteris paribus*. The time of high water appeared not to be much affected.

The same errors in the calculated times and heights for 1784 were again classed, in order if possible to ascertain the effect of the wind, and the following results were obtained.

	Error in calculated Time of High Water.	Error in calculated Height of High Water.	Height of Barometer.	Number of Observations.
	m	inches.		
N.	+ ·1	+1·4	29·80	32
N.N.E.	− ·8	+8·9	29·83	19
N.E.	− ·8	+9·7	29·79	28
E.N.E.	+1·7	+9·1	29·77	16
E.	− ·8	+5·2	29·86	38
E.S.E.	−2·8	+ ·7	29·76	21
S.E.	−1·6	+1·0	29·91	141
S.S.E.	−4·2	+2·2	29·80	37
S.	−5·0	−4·7	29·60	4
S.S.W.	−3·2	−4·2	29·25	12
S.W.	−1·7	−3·0	29·57	45
W.S.W.	−4·2	−5·5	29·54	42
W.	−1·6	− ·4	29·73	87
W.N.W.	−2·9	+1·2	29·89	28
N.W.	−1·5	− ·3	29·38	72
N.N.W.	−3·9	−2·4	29·61	24

Hence it appears that north-easterly winds at Liverpool depress the tide, and south-

* See Connaissance des Temps, 1834.

westerly winds raise it. As northerly winds raise the barometer and southerly winds depress it, it will be difficult, if not impossible, to separate the effect of winds and that of the variation in the pressure of the atmosphere from each other.

I requested Mr. Dessiou to separate and class the errors of the predicted heights from Table D., in order to ascertain the variation in the height of high water at London simultaneous with the variation of the barometer during 1835.

	Barom.	Therm.				inches.
For	29·29	51·9,	mean of	68	results, we found the mean error in the calculated heights of high water...	+ 6·1
	29·77	59·0	———	282		+ 10·7
	30·19	56·9	———	351		+ 12·4

so that the water rises 6·3 inches for ·90 depression of the barometer.

I have found on several occasions when remarkably high tides have taken place at London, that they have been preceded by a very low barometer.

If these collateral inquiries relating to the influence of the wind and of the atmospheric pressure appear of sufficient importance to deserve a complete and satisfactory solution, much additional labour must be devoted to the accomplishment of this object. I find upon comparing the registers of the London and St. Katherine Docks, that the direction of the wind is scarcely ever noted to be the same on the same day; probably the direction is always fluctuating, and the discrepancy may then be accounted for by supposing that the observation is not made at the same moment at both places. The barometer admits of more precise observation; but if the tide originates at a very remote distance on the surface of the earth, the atmospheric pressure *there* has probably more influence upon the phenomena than the pressure in our vicinity. This difficulty is diminished by the circumstance that the great fluctuations of the barometer are not rapid, and that the variations in the pressure of the atmosphere are extremely extensive. It will still, however, I apprehend, be very difficult to distinguish between the effects arising from variations in the atmospheric pressure, and those arising immediately from the effect of wind, as I have before remarked.

When the discussion of the observations of the tides made at Liverpool was published, although the opinion of several persons whom I consulted was in favour of *apparent* solar time having been employed by Mr. Hutchinson in registering the observations, I was unable to arrive at any certain conclusion in this particular. But by comparing our predicted tides for Liverpool with the observations made there in February last, it seems beyond doubt that Mr. Hutchinson did employ *apparent* solar time, and our tables must be interpreted accordingly, for the mean error of our predicted times of high water for last February is only three minutes, while the equation of time for that month is much more considerable.

If the surface of the fluid assume the same form at any given instant as it would do if the forces then acting upon each particle were invariable in magnitude and direction, the variation of the height of the water, or distance of a particle at the surface of the ocean from the earth's centre, neglecting terms multiplied by the fourth power of the parallaxes, is proportional to

$$\frac{3\,m\,P^3}{2}\left\{\cos^2\zeta - \frac{1}{3}\right\} + \frac{3\,m'\,P'^3}{2}\left\{\cos^2\zeta' - \frac{1}{3}\right\}*,$$

where m is the mass of the luminary, P the horizontal parallax, and ζ the zenith distance, the unaccented quantities referring to the sun, and the accented quantities to the moon.

If α denote right ascension, δ declination, l geographical latitude, and μ sidereal time,

$$\cos\zeta = \cos\delta\cos l\cos(\mu - \alpha) + \sin\delta\sin l.$$

h, the height of the water,

$$= D - \frac{m\,R\,P^3}{2\,M}\left(1 - \frac{3}{2}\cos^2 l\right)(1 - 3\sin^2\delta)$$

$$+ \frac{3\,m\,R\,P^3}{4\,M}\cos^2 l\,\{\cos^2\delta\cos(2\,\mu - 2\,\alpha) + 2\sin 2\,\delta\tan l\cos(\mu - \alpha)\}$$

$$- \frac{m'\,R\,P'^3}{2\,M}\left(1 - \frac{3}{2}\cos^2 l\right)(1 - 3\sin^2\delta')$$

$$+ \frac{3\,m'\,R\,P'^3}{4\,M}\cos^2 l\,\{\cos^2\delta'\cos(2\,\mu - 2\,\alpha') + 2\sin 2\,\delta'\tan l\cos(\mu - \alpha')\},$$

M being the mass of the earth, and D a constant depending only on the zero line, from which the heights are reckoned.

At high water

$$\tan(2\,\mu - 2\,\alpha') = \frac{\dfrac{m\,P^3\cos^2\delta}{m'\,P'^3\cos^2\delta'}\sin(2\,\alpha - 2\,\alpha') - \dfrac{m\,P^3\sin 2\,\delta\tan l\sin(\mu-\alpha)}{m'\,P'^3\cos^2\delta'\cos(2\,\mu - 2\,\alpha')} - \dfrac{2\tan l\tan\delta'\sin(\mu - \alpha')}{\cos(2\,\mu - 2\,\alpha')}}{1 + \dfrac{m\,P^3\cos^2\delta}{m'\,P'^3\cos^2\delta'}\cos(2\,\alpha - 2\,\alpha')}.$$

If $\mu - \alpha' = \psi$, $\quad \alpha - \alpha' = \varphi \quad \dfrac{m\,P^3\cos^2\delta}{m'\,P'^3\cos^2\delta'} = A \quad \mu - \alpha = \psi - \varphi$

and if we neglect the difference in the interval for the morning and evening tides,

$$\tan 2\,\psi = \frac{A\sin 2\,\varphi}{1 + A\cos 2\,\varphi}:$$

ψ is the hour-angle of the moon at the time of high water, and is an angle differing† little from 0 or 180°.

If $E = \dfrac{3\,m'\,R\,P'^3}{4\,M}\cos^2 l\cos^2\delta'$,

considering only the arguments $2\,\mu - 2\,\alpha$ and $2\,\mu - 2\,\alpha'$,

* This amounts to supposing that the differential equation to the fluid surface is given by the equation

$$X\,d\,x + Y\,d\,y + Z\,d\,z = 0$$

so as to neglect the quantity $u'\,d\,x + v'\,d\,y + w'\,d\,z$ in the notation of M. Poisson, Traité de Mécanique, vol. ii. p. 669.

† This has reference to the case of a perfect sphere.

$$h = D + E\{A \cos(2\psi - 2\phi) + \cos 2\psi\}$$

$E\,A$ varies as $P^3 \cos^2 \delta$, E varies as $P'^3 \cos^2 \delta'$.

If (A) and (E) denote the values of those quantities for the mean parallaxes and declinations, that is, when $\delta = \delta' = 15°$, $P = \sin 8''\cdot 8$, $P' = \sin 57'$, the semimenstrual inequalities in the interval and in the height are given by the expressions

$$\tan 2\psi = \frac{(A) \sin 2\phi}{1 + (A) \cos 2\phi},$$

$$h = D + (E)\{(A) \cos(2\psi - 2\phi) + \cos 2\psi\};$$

D being a constant which depends only on the zero line, from which the height is reckoned. The value of (A) obtained from observations of the interval at different places should be the same, unless neap tides are transmitted with a different velocity from that of spring tides.

The difference in height between the morning and evening tide depends upon the angles $\psi - \phi$ and ψ; if this difference be called d h, considering only the quantities multiplied by P^3 and P'^3,

$$d\,h = B\{(A) \sin 2\delta \cos(\psi - \phi) + \sin 2\delta' \cos \psi\};$$

B being a constant. The quantities multiplied by P'^4 may also, perhaps, give some sensible term in the diurnal inequality; and the term $\frac{m' R P'^3}{2M}$ may give an inequality in the height depending upon the moon's parallax and independent of her age or time of transit. The diurnal inequality of the interval may be put in the form

$$d\,\psi = \frac{F \cos 2\psi}{1 + (A) \cos 2\phi}\left\{\frac{(A) \sin 2\delta}{\cos^2 \delta'} \sin(\psi - \phi) + 2 \tan \delta' \sin \psi\right\}$$

F being a constant.

The inequalities of the heights at different places depending upon the angles $2\psi - 2\phi$ and 2ψ are proportional to the quantity (E); so that if they have been obtained for any place P, they may be obtained for any other place P′, by multiplying the former by $\frac{(E')}{(E)}$. The inequalities in the interval are the same everywhere, according to the theory above explained; but in both cases the argument may require to be shifted.

The British Association for the Advancement of Science having placed at my disposal for the purpose a sum of money, I employed Mr. Jones and Mr. Russell, two excellent computers, to discuss nineteen years' observations made at the London Docks, with reference to the moon's transit two days previous, and the results have been arranged in the accompanying tables. I now proceed to compare these with theoretical results deduced from the preceding expressions.

First, with respect to the semimenstrual inequality. From the column headed "Mean" in Table II. it appears that

For the transit happening at $9^h\,30^m$ the interval is $3^h\,48^m\cdot 9$

———————————— 3 30 ——————— 2 25 ·1

$3^h\,48^m\cdot 9 - 2^h\,25^m\cdot 1 = 1^h\,23^m\cdot 8$, which, converted into space, $= 21°$ nearly.

$$\log \tan 21^\circ = \log (A) = 9{\cdot}5841774.$$

$$\frac{1}{(A)} = 2{\cdot}605.$$

When the discussion was instituted with reference to the transit immediately preceding the time of high water, the value of log (A) came out 9·5784858.

I find, moreover,

$$D = 16^{\text{ft}}{\cdot}69 \qquad (E) = 4{\cdot}43 \qquad \log (E) = {\cdot}6468993.$$

The semimenstrual inequality calculated from BERNOULLI's expression is very similar to the inequality deduced from observation. See Table XXVIII. and Plate XVIII.

The mean interval in the former discussion corresponded to the moon's transit at 2^{h}; now it corresponds to the moon's transit at 26^{m}. The constants now obtained differ so little from those obtained before, that the tables calculated by Mr. JONES, and given in my last paper*, are applicable, making the moon's transit at 30^{m} correspond to $\varphi = 0$, for the moon's transit at $1^{\text{h}}\ 30^{\text{m}}$ $\varphi = 15^\circ$, &c.

The calendar-month inequality is complicated in its nature; it results from the variations in the declinations of the luminaries, and in the sun's parallax. Table XXIX., calculated by Mr. JONES, offers a comparison in this respect between theory and observation†. The results in this table have been laid down in diagrams (see Plate XIX.), in order that the nature of the agreement may be better understood. The terms

$$\left\{1 - \frac{3}{2}\cos^2 l\right\}\left\{\frac{3\,m\,R\,P^3}{2\,M}\sin^2\delta + \frac{3\,m'\,R\,P'^3}{2\,M}\sin^2\delta'\right\},$$

occur in the expression for the height (see p. 222.), substituting of course for

$$1 - \frac{3}{2}\cos^2 l$$

a certain constant, to be determined from the observations, which amounts to introducing an inequality in the height

$$= C\{(A)\sin^2\delta + \sin^2\delta'\},$$

C being a constant; but these terms appear to be insensible.

The calendar month inequality in the height may also result partly from the fluctuations in the barometer. According to Mr. DANIELL‡, the following are the heights of the barometer in the different months of the year:

	inches.		inches.		inches.		inches.
January	29·921	April	29·881	July	29·874	October	29·774
February	30·067	May	29·898	August	29·891	November	29·776
March	29·843	June	30·020	Sept.	29·931	December	29·693

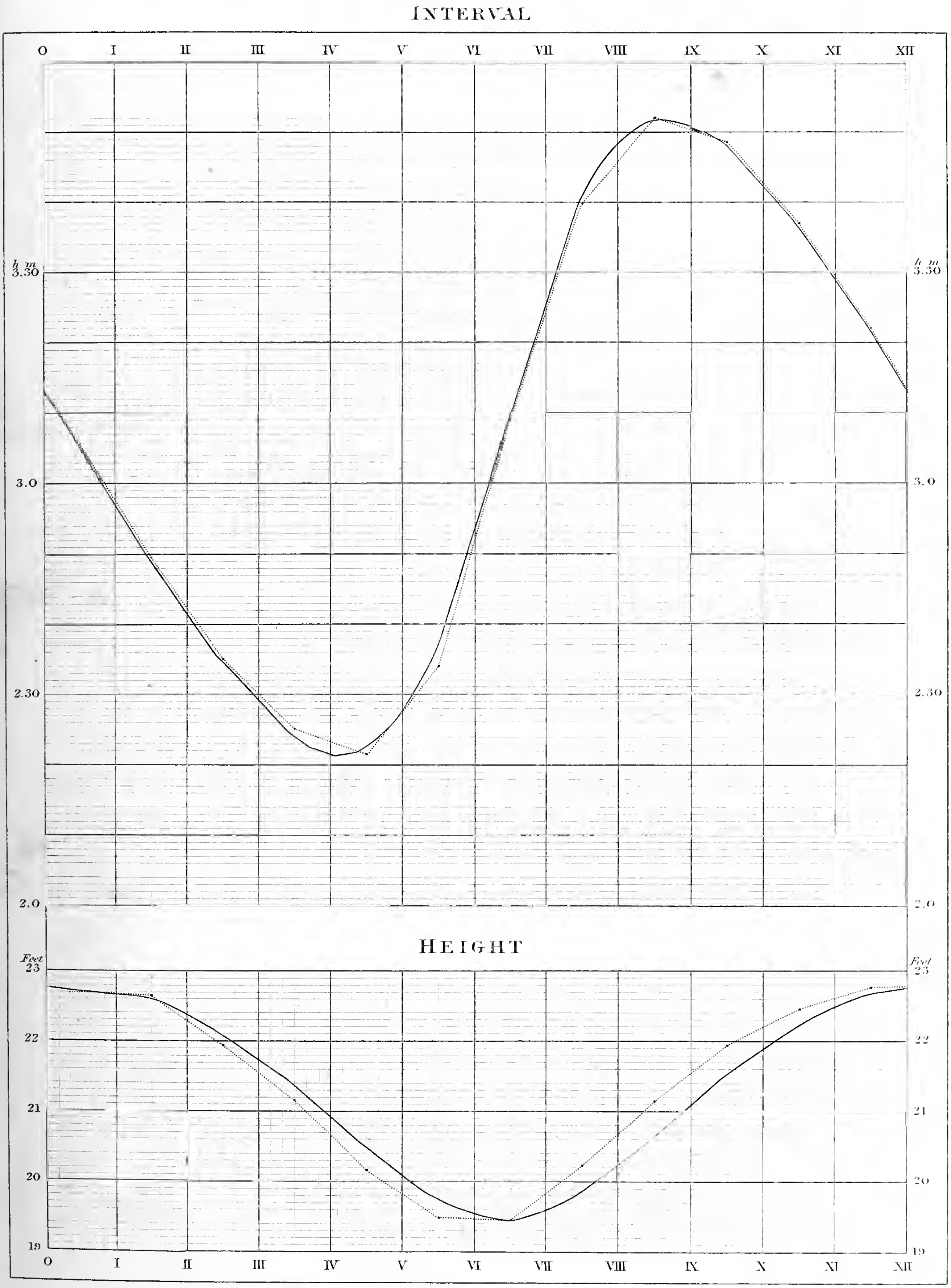
Hence, in order to arrive at the utmost precision in the comparison, it might, per-

* Philosophical Transactions, 1836, p. 58.

† The sun's declination is that for the middle of the month, and the moon's declination is given for each category in Table I.

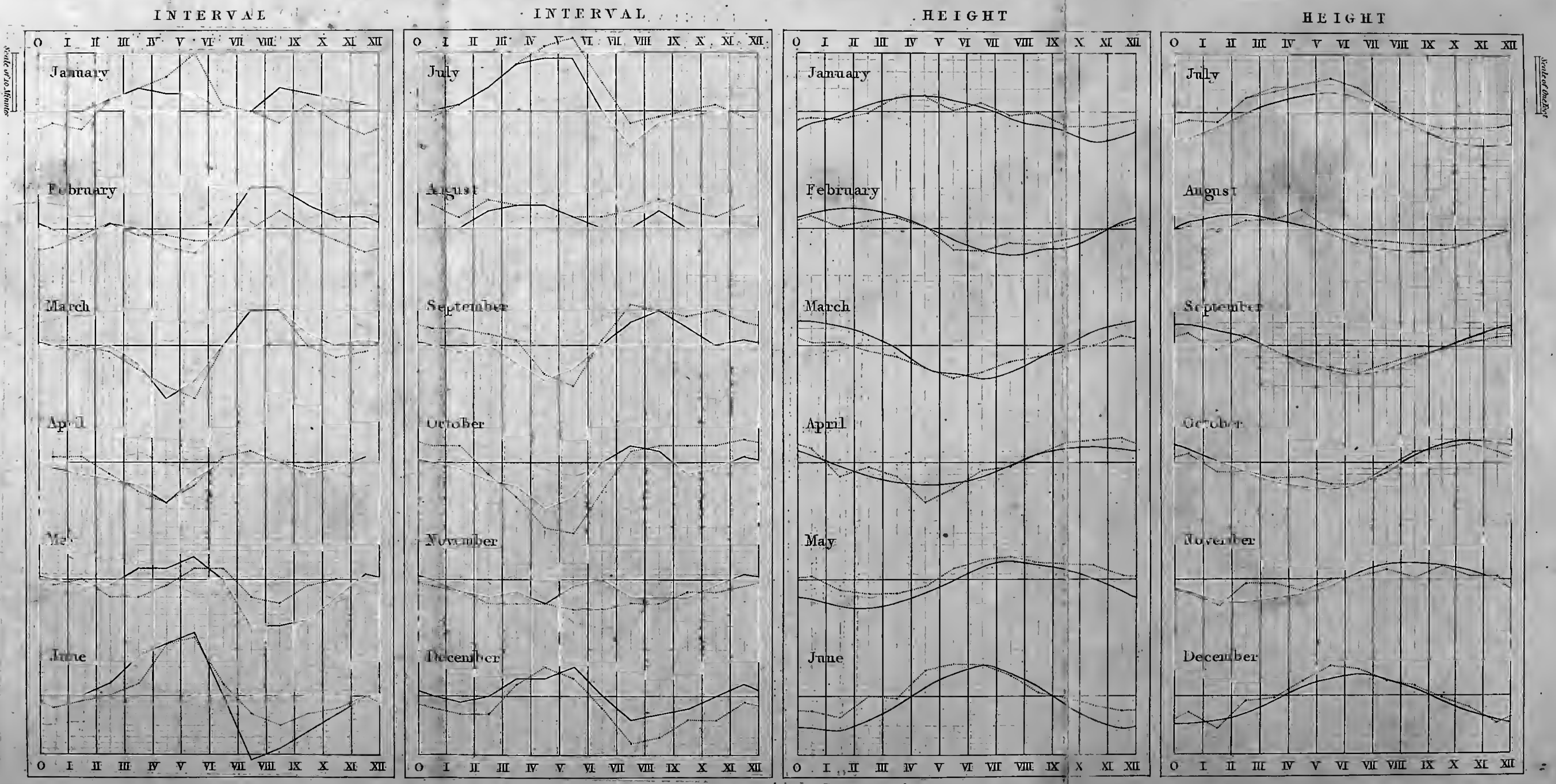
‡ Meteorological Essays.

Diagram showing a comparison between the Semi-menstrual correction at London, in the interval, and in the height, as deduced from theory and from observation. See Table XXVIII



In these curves the abscissa represents the time of the Moon's transit two day's previous. Observation .... Theory. ———

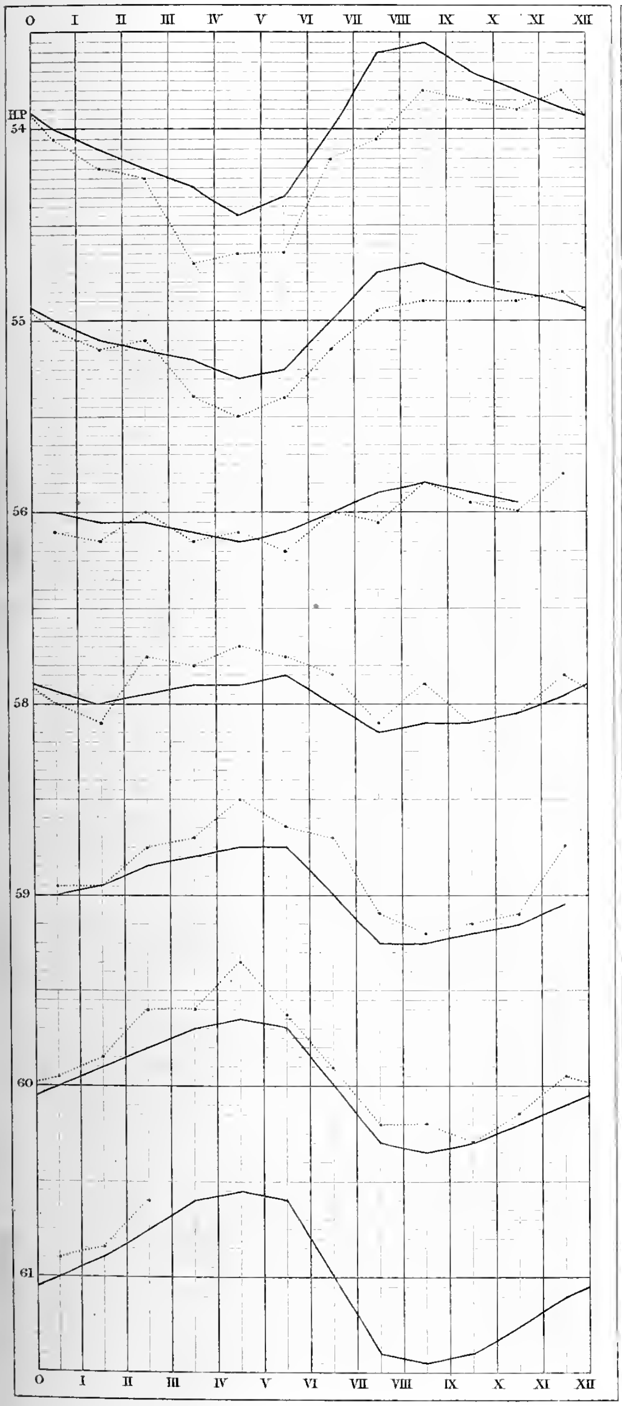
Diagram showing a comparison between the calendar month correction in the interval and in the height as deduced from theory and from observations at the London Docks _ See Table XXIX.



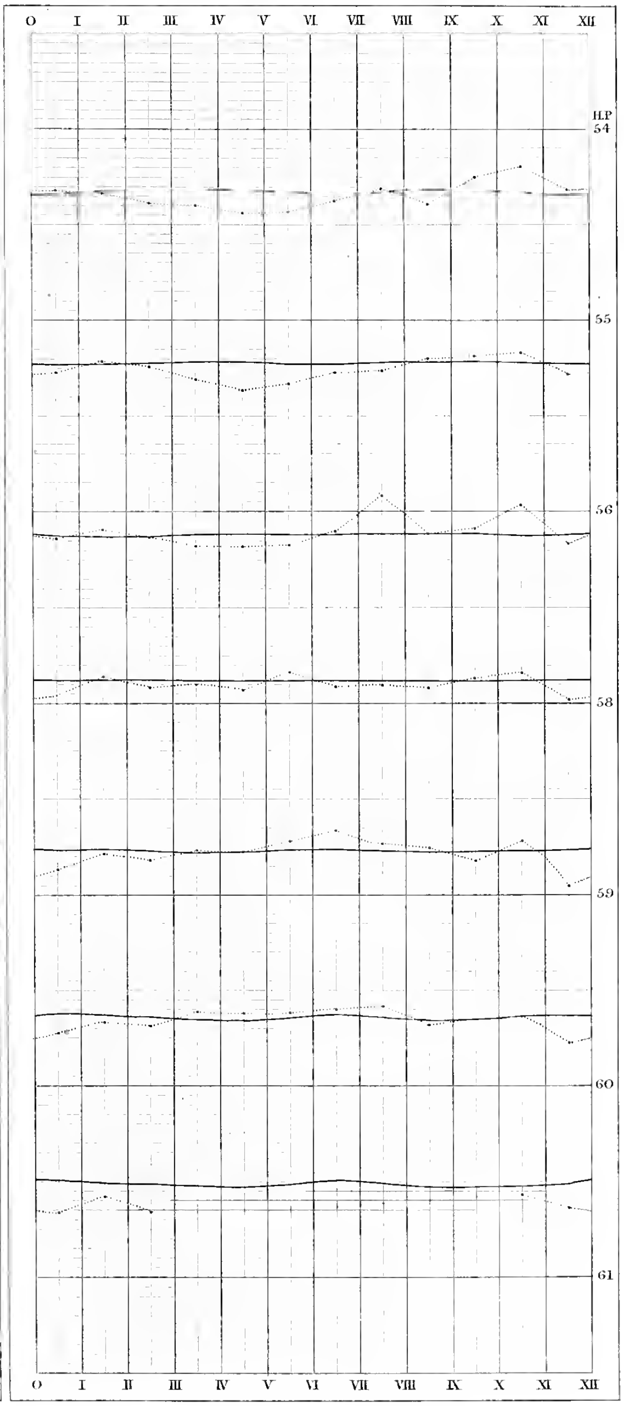
In these curves the abscissa represents the time of the Moon's transit two days previous Observation........ Theory.——

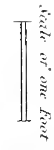
Diagram showing a comparison between the Moon's parallax correction in the interval and in the height as deduced from theory and from observations at the London Docks. — See Table XXX.

INTERVAL

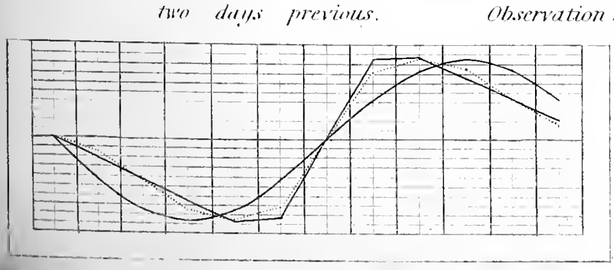


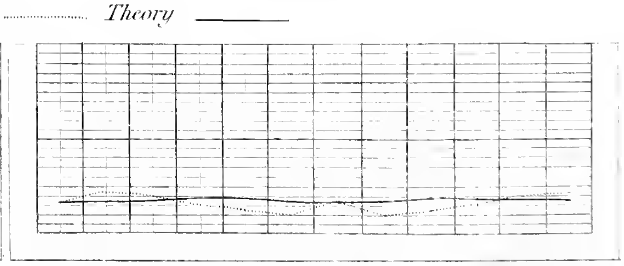
HEIGHT





In these curves the abscissa represents the time of the Moon's transit two days previous. *Observation* .... *Theory* ———





haps, be necessary to introduce a correction depending upon the height of the barometer, which may amount in the extreme case to nearly two inches at London. Another difficulty arises from the influence of the wind, in as much only as it is distinguished from that of the atmospheric pressure. This circumstance, and the rude nature of the observations, seem to render it very doubtful whether the refinements to which I have alluded would be attended with much advantage.

Table XXX. offers a comparison between the moon's parallax-correction in the interval and in the height, as deduced from theory and from observation. See Plate XX. In order to diminish the irregularities, and to employ the concourse of all the observations, I employed the following method: Let δP be the difference of parallax, or

$$\text{The parallax} - 57'.$$

I suppose the parallax correction to be proportional to δP; hence the correction for parallax $54'$ = three times the correction for parallax $56'$, and the total of the absolute corrections for parallaxes $54'$, $55'$, $56'$, $58'$, $59'$, $60'$, $61' = \frac{16}{3} \times$ the correction for parallax $54'$. Whatever be the law of the parallax-correction, it may certainly be considered as proceeding according to powers of δP; and the preceding hypothesis amounts to neglecting all the powers except the first. I now employ only the total of the corrections deduced from the discussions, and I multiply it by $\frac{3}{16}$, or the equivalent multiplier, in order to have the correction for $54'$. The following Table exhibits the results, which may each be considered as resulting from the average of about 800 observations.

Correction for H. P. 54′ at London.

Moon's Transit.	Interval. A.	Height. A.	Moon's Transit.	Interval. A.	Height. A.
h m	m	feet.	h m	m	feet.
0 30	− 1·4	−·56	6 30	− 5·0	−·72
1 30	− 2·7	−·65	7 30	+ 1·7	−·69
2 30	− 6·1	−·62	8 30	+ 3·9	−·67
3 30	−10·7	−·80	9 10	+ 4·3	−·60
4 30	−13·1	−·83	10 10	+ 2·5	−·59
5 30	−12·5	−·92	11 30	+ 0·3	−·49

The number of observations from which the preceding Table is deduced is so considerable, that it is impossible, I think, to admit in it any error of consequence. According to the expression for $\tan 2\psi$ in p. 222, the moon's parallax-correction in the interval is the same for $\varphi = 90 \pm \theta$, only with a contrary sign, and for the height it is the same. In the following Table I have endeavoured to detach all such part of the moon's parallax-correction (deduced from the observations) as is consistent with such an expression, from the residue, and I have placed in the next column the moon's parallax-correction calculated by Mr. Jones from the expression for $\tan 2\psi$ in p. 222.

φ.	Moon's Transit.	Correction for H. P. 54′.					
		Interval.			Height.		
		Observation. B.	Theory.	Residue. C.	Observation. B.	Theory.	Residue. C.
° °	h m	m	m	m			
0 or 180	0 30	0	0	− 1·4	− ·66	− ·66	+ ·10
15 — 195	1 30	− 1·5	− 2·1	− 1·2	− ·57	− ·66	− ·08
30 — 210	2 30	− 4·3	− 4·2	− 1·8	− ·60	− ·64	− ·02
45 — 225	3 30	− 7·5	− 6·5	− 3·2	− ·70	− ·62	− ·10
60 — 240	4 30	− 8·5	− 8·7	− 4·6	− ·75	− ·61	− ·08
75 — 255	5 30	− 7·1	− 8·4	− 5·4	− ·80	− ·64	− ·12
90 — 270	6 30	0	0	− 5·0	− ·66	− ·66	− ·06
105 — 285	7 30	+ 7·1	+ 8·4	− 5·4	− ·80	− ·64	+ ·11
120 — 300	8 30	+ 8·5	+ 8·7	− 4·6	− ·75	− ·61	+ ·08
135 — 315	9 30	+ 7·5	+ 6·5	− 3·2	− ·70	− ·62	+ ·10
150 — 330	10 30	+ 4·3	+ 4·2	− 1·8	− ·60	− ·64	+ ·01
165 — 345	11 30	+ 1·5	+ 2·1	− 1·2	− ·57	− ·66	+ ·08

B + C = A.

The residue of the interval may, I think, be represented by

$$\frac{\text{constant} \times \delta P}{1 + A \cos 2\varphi}$$

which is not inconsistent with theory. The *residue* of the height is small. The results of the preceding Table are displayed in diagrams at foot of Plate XX.

Table XXXI. offers a comparison between the moon's declination-correction in the interval and in the height, as deduced from Bernoulli's theory and from the observations.

It appears that the semimenstrual, declination and parallax-corrections are in accordance with the laws assigned to them by Bernoulli's theory, at least taken in the manner which I have attempted to define in this paper, and within the limits of the errors of the observations. This view of the question accords with that given by Laplace in the following passage of the Exposition du Système du Monde, p. 289: "Chacun de nos ports peut être considéré à cet égard, comme étant à l'extremité d'un canal, à l'embouchure duquel les marées partielles arrivent au moment même du passage des astres au méridien, et emploient un jour et demi à parvenir à son extrémité."

This view of the question is also adopted by Mr. Whewell; but I do not think with Mr. Whewell that the "retroposition of the tide in longitude and in time is affected by changes depending upon variations of the moon's force*." I think that what Mr. Whewell attributes to the change in λ', "the retroposition of the tide in longitude," is chiefly due to the variation in the intervals between the successive transits of the moon, which has hitherto been overlooked.

From the expression

$$\tan 2\psi = \frac{A \sin 2\phi}{1 + A \cos 2\phi}$$

* "On the Empirical Laws of the Tides in the Port of Liverpool." Philosophical Transactions, 1836, p. 22.

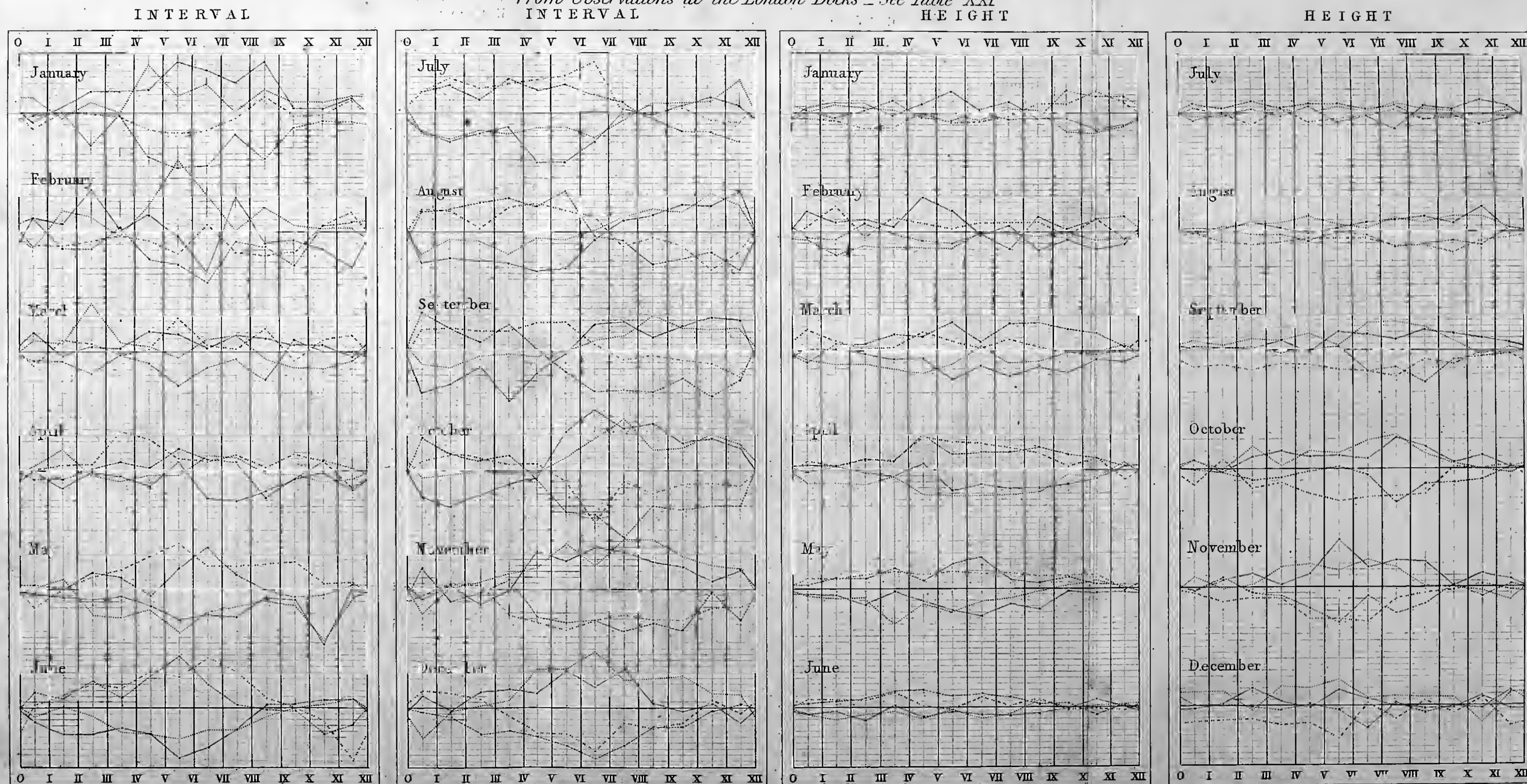
Intervals depending upon Upper Transits P.M. ————— ....

——— *D°.* ——— *A.M.* ——— - - - - - -

——— *Lower (Interpolated) Transits A.M.* ....

——— *D°.* ——— *P.M.* ————

Diagram showing the Diurnal inequality in the Interval and in the height for the middle of every month in the Year. from Observations at the London Docks _ See Table XXI



In these curves the abscissa represents the time of the Moon's transit two days previous *Observation* .... *Theory* ————

it is easy to deduce

$$d\,\psi = \frac{\cos^2 2\,\psi \sin 2\,\phi\, d\,A}{2\,(1 + A \cos 2\,\phi)^2} = \frac{\sin 2\,\phi\, d\,A}{2\,(1 + 2\,A \cos 2\,\phi + A^2)}.$$

The expression $d\,\psi = \frac{\sin 2\phi\, d\,A}{2}$ would agree with the empirical expressions which Mr. WHEWELL has suggested* for the variations in the interval; but the terms $2\,A \cos 2\,\phi + A^2$ in the denominator have a sensible influence on the value of $d\,\psi$.

Table XXI. shows the diurnal inequality in the interval and in the height, which is also laid down in Plate XXII. The diurnal inequality of the height at London is scarcely sensible; and when the observations are divided into so many categories, a sufficient number does not remain to afford a satisfactory average. I have given a comparison with theory of the diurnal inequality for the interval in Table XXXII.

In the comparison which I have instituted between BERNOULLI's theory and observation, it should be remembered that I have employed throughout the same constant (A) for all the interval and height corrections. But by assuming the form only of the corrections according to theory, and using various constants, expressions might perhaps be obtained which would represent the observations a little better. Such alterations, however, have not been suggested by theory, nor would they be attended with any practical utility. My intention in laying down the results in diagrams has been partly to exhibit the nature and extent of their irregularities, which would no doubt be diminished by employing a greater number of observations. If even the equilibrium-theory were complete and sufficient in the case of a perfect sphere, the form of the channel in which a derived tide-wave flows cannot fail to influence in a slight degree the form and magnitude of the different corrections. It is easy to see, for example, that the corrections for a derived tide-wave flowing through a channel bounded by perpendicular sides, would not be exactly the same as in a channel bounded by shelving coasts. May not the slight disagreement between the theory and observation curves for the semimenstrual inequality of the height in Plate XVIII. be accounted for in this manner? If spring tides and neap tides are propagated with different velocities, the value of the constant (A) resulting from observations at distant places will not be exactly the same.

In comparing the semimenstrual inequality as deduced from theory with that deduced from observation, I have supposed the declinations of the sun and moon to be equal to 15°, and the horizontal parallax of the moon equal to 57′. The corrections which might be required in consequence of deviations from this hypothesis, and which are given in Tables XXVIII. and XXIX., are so small, that they may be neglected, and the columns headed " Mean" in Tables II. and III. may be considered as affording the semimenstrual inequality. The moon's average declination corresponding to the totality of observations employed is 15°·2, and the moon's average horizontal parallax 57′·0. The agreement between the theory and observation curves in Plates XIX.,

* Philosophical Transactions, 1834.

XX., and XXI., is more striking for the heights than for the intervals. This, I apprehend, is partly owing to the circumstance that the corrections for the height vary but little with the moon's age, and are therefore less influenced by the particular moon's transit chosen to be the argument of the tables.

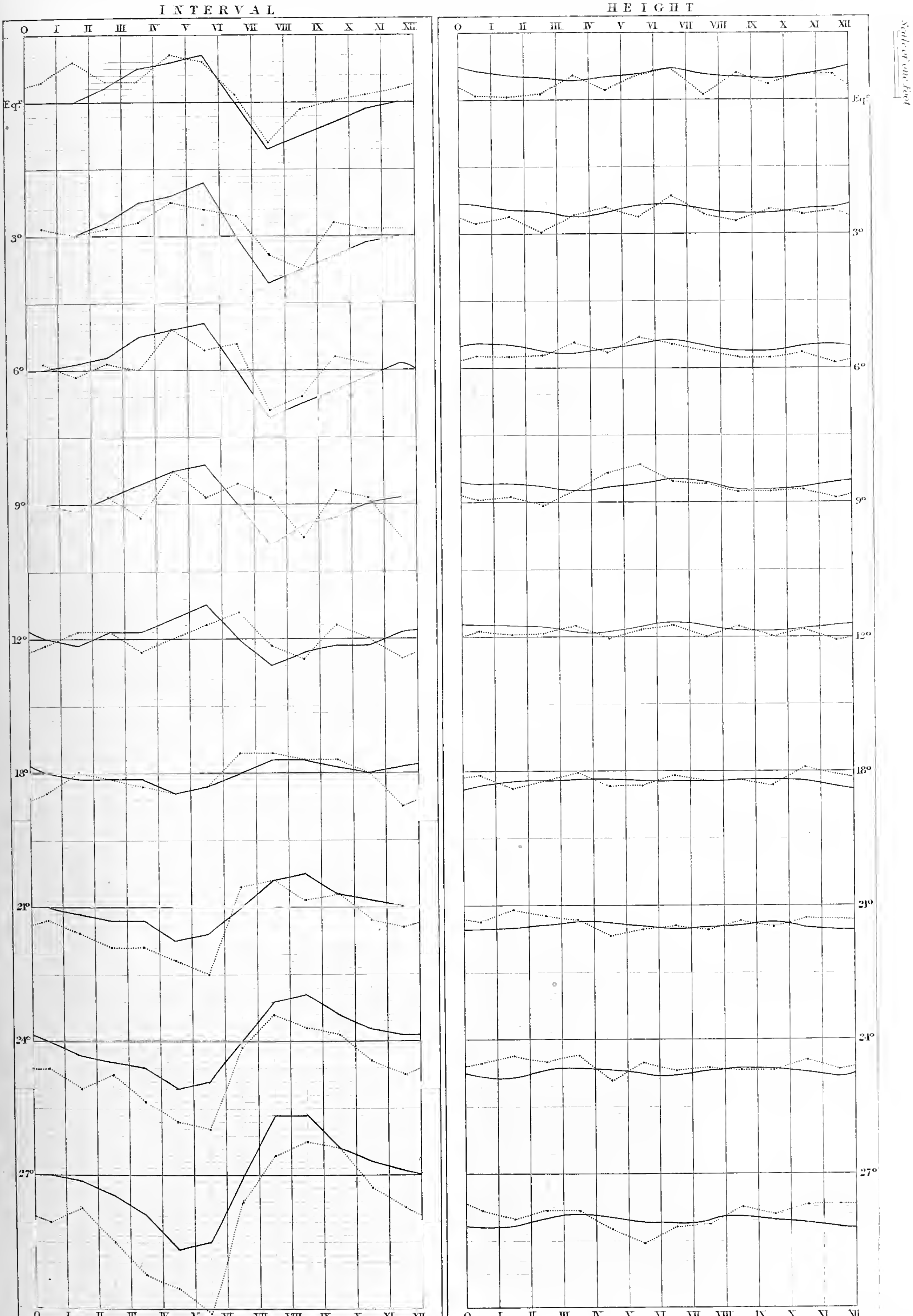
In future, for the port of London, the phenomena of the tides may safely be predicted by employing tables founded on Bernoulli's theory, having for their *argument* the fifth transit of the moon preceding the high water of which the time and height are required, and founded upon the constants (A), D and (E) given in pp. 223 and 224. Such tables are included in those contained in this paper, and calculated by Mr. Jones, which give the comparisons between theory and observation. The calendar-month correction is of course to be considered as included in those for the declinations and parallaxes of the luminaries.

In estimating how far the agreement between theory and observation is to be considered as complete, it must be recollected that it is impossible, even with the apparatus at the St. Katherine Docks, to determine the exact minute of high water; for the water almost always continues stationary for some time, without falling one tenth of an inch. At the London Docks they only attempt to register the time to the nearest five minutes.

Mr. Peirce informs me that he cannot account for the difference between the observations at the London and the St. Katherine Docks. The swell of the steamers cannot affect the gauge at the latter place, as it is completely sheltered from any motion of the surface of the river. I suspect occasional errors of transcription, particularly in the month of January last. Results of theory cannot offer a closer agreement with those of observation than observations at the same place, or at places separated by a short distance, do with each other. Tables A and B, which have been furnished me by Mr. Deacon, offer a comparison between the observations at the London and at the St. Katherine Docks. The observations have been brought up to the same standard by the addition of certain quantities; in order that if no source of error existed, they might present no difference.

The tables in this paper having reference to the interval between successive transits of the moon, furnish the means of shifting the argument approximately.

Diagram showing a Comparison between the Moon's declination correction in the interval and in the height as deduced from theory & from observations at the London Docks. ___ See Table XXXI



In these curves the abscissa represents the time of the Moon's transit two days previous. Observation .... Theory ______

Table A.

Showing a comparison of the observations of the Times of High Water made at the London Docks, increased by five minutes, and those at the St. Katherine Docks. The observations marked with an * appear doubtful.

Date. 1836.	January.			February.			March.			April.			May.			June.		
	London Docks. +5 min.	St. Kath. Docks.	Difference.	London Docks. +5 min.	St. Kath. Docks.	Difference.	London Docks. +5 min.	St. Kath. Docks.	Difference.	London Docks. +5 min.	St. Kath. Docks.	Difference.	London Docks. +5 min.	St. Kath. Docks.	Difference.	London Docks. +5 min.	St. Kath Docks.	Difference.
	h m	h m	m	h m	h m	m	h m	h m	m	h m	h m	m	h m	h m	m	h m	h m	m
1.	......	......	......	1 5	1 18	−13	12 15	12 28	−13	1 35	1 28	+ 7	1 45	1 47	− 2	2 45	2 43	+ 2
	12 25	12 33	− 8	1 20	1 33	−13	12 45	12 48	− 3	2 5	2 9	− 4	2 15	2 17	− 2	3 10	3 18	− 8
2.	1 5	1 8	− 3	1 50	1 43	+ 7	1 10	1 12	− 2	2 15	2 12	+ 3	2 25	2 33	− 8	3 35	3 37	− 2
	1 20	1 48	−28	2 20	2 13	+ 7	1 40	1 42	− 2	2 40	2 48	− 8	2 50	2 58	− 8	3 45	3 57	−12
3.	1 35	1 48	−13	2 35	2 33	+ 2	1 50	2 2	−12	2 45	2 48	− 3	3 0	3 3	− 3	4 20	4 28	− 8
	1 55	2 2	− 7	2 45	2 53	− 8	2 15	2 32	−17	3 20	3 32	−12	3 30	3 43	−13	4 35	4 43	− 8
4.	2 20	2 34	−14	3 10	3 17	− 7	2 45	2 43	+ 2	3 30	3 38	− 8	3 45	3 53	− 8	5 10	5 13	− 3
	2 30	2 47	−17	3 25	3 46	−21	3 5	3 13	− 8	3 50	4 2	−12	4 5	4 12	− 7	5 35	5 34	− 1
5.	2 45	3 12	−27	3 50	3 58	− 8	3 20	3 15	+ 5	4 5	4 12	− 7	4 30	4 33	− 3	6 15	6 17	+ 2
	3 5	3 24	−19	4 15	4 11	+ 4	3 35	3 51	−16	4 30	4 38	− 8	4 55	4 59	− 4		6 29	
6.	3 35	3 52	−17	4 20	4 18	+ 2	3 40	3 52	−12	4 50	4 58	− 8	5 20	5 24	− 4	7 15	7 17	− 2
	3 55	4 3	− 8	4 40	4 42	− 2	4 10	4 10	0	5 15	5 22	− 7	5 40	5 48	− 8	7 30	7 38	− 8
7.	4 5	4 28	−23	4 50	4 46	+ 4	4 45	4 23	+22	5 30	5 38	− 8	6 15	6 24	− 9	8 25	8 27	−12
	4 15	4 40	−25	5 20	5 23	− 3	4 50	4 50	0	5 50	6 6	−16	6 25	6 42	−17	8 45	8 42	+ 3
8.	4 25	5 12	−47	5 35	5 28	+ 7	5 5	5 13	− 8	6 15	6 15	0	7 20	7 27	− 7	9 35	9 37	− 2
	5 0	5 7	− 7	5 55	6 3	− 8	5 30	5 27	+ 3	6 35	6 37	− 2	7 40	7 47	− 7	9 50	9 43	+ 7
9.	5 5	5 48	−43	6 20	6 18	+ 2	5 45	5 42	+ 3	7 15	7 23	− 8	8 40	8 52	−12	10 30	10 43	−13
	5 35	5 53	−18	6 35	6 39	− 4	6 15	6 10	+ 5	7 45	7 46	− 1	9 25	9 21	+ 4	10 50	10 49	+ 1
10.	5 55	6 30	−35	6 50	6 48	+ 2	6 30	6 31	1	8 45	9 4	−19	10 10	10 23	−13	11 25	11 46	−21
	6 20	6 32	−12	7 25	7 29	− 4	6 45	7 9	−24	9 15	9 22	− 7	10 35	10 42	− 7	11 50	11 57	− 7
11.	6 35	7 1	−26	7 45	7 53	− 8	7 10	7 18	− 8	10 20	10 32	−12	11 20	11 27	− 7	......	......	......
	7 0	7 4	− 4	8 5	8 14	− 9	8 0	8 8	− 8	10 55	10 58	− 3	11 35	11 42	− 7	12 20	12 19	+ 1
12.	7 5	7 25	−20	8 15	8 38	−23	8 15	8 31	−16	11 45	11 52	− 7	......	......	......	12 30	12 32	− 2
	7 45	8 23	−38	9 45	9 43	+ 2	9 30	9 38	− 8	......	......	......	12 15	12 22	− 7	1 5	1 7	− 2
13.	8 15	9 4	−49	10 35	10 47	−12	10 20	10 17	+ 3	12 15	12 8	− 3	12 30	12 32	− 2	1 20	1 16	+ 4
	9 0	9 38	−38	11 20	11 28	− 8	10 50	10 53	− 3	12 45	12 52	− 7	1 5	1 8	− 3	1 40	1 50	−10
14.	9 25	10 19	−54	11 50	12 8	−18	11 55	12 15	−20	1 5	1 18	−13	1 15	1 12	+ 3	2 5	1 57	+ 8
	9 55	10 53	−58	......	......	......	......	......	......	1 25	1 28	− 3	1 30	1 41	−11	2 20	2 27	− 7
15.	10 40	11 11	−31	12 25	12 31	− 6	12 25	12 17	+ 8	1 40	1 44	− 4	1 50	1 53	− 3	2 25	2 42	−17
	11 15	11 29	−14	12 55	1 6	−11	12 45	12 42	+ 3	2 10	2 18	− 8	2 15	2 22	− 7	2 55	2 58	− 3
16.	11 55	12 9	−14	1 30	1 32	− 2	12 55	1 7	−12	2 20	2 24	− 4	2 25	2 31	− 6	3 10	3 13	− 3
	......	......	......	1 55	2 13	−18	1 35	1 41	− 6	2 45	2 57	−12	2 50	2 53	− 3	3 25	3 28	− 3
17.	12 35	12 41	− 6	2 35	2 32	+ 3	1 55	1 59	− 4	3 0	3 5	− 5	3 10	3 6	+ 4	3 50	3 57	− 7
	1 5	1 8	− 3	2 50	3 4	−14	2 25	2 39	−14	3 10	3 18	− 8	3 20	3 28	− 8	4 0	4 8	− 8
18.	1 35	1 37	− 2	3 20	3 27	− 7	2 45	2 53	− 8	3 25	3 24	+ 1	3 35	3 37	− 2	4 25	4 21	+ 4
	1 55	2 12	−17	3 30	3 37	− 7	2 55	2 57	− 2	3 45	3 51	− 6	3 55	3 51	+ 4	4 30	4 31	− 1
19.	2 30	2 28	+ 2	3 55	3 48	+ 7	3 10	3 13	− 3	3 55	3 56	− 1	4 15	4 17	− 2	4 55	4 57	− 2
	2 45	2 48	− 3	4 15	4 23	− 8	3 50	3 57	− 7	4 20	4 20	0	4 25	4 32	− 7	5 5	5 6	− 1
20.	3 20	3 27	− 7	4 20	4 18	+ 2	3 50	3 48	+ 2	4 25	4 32	− 7	4 40	4 43	− 3	5 35	5 36	− 1
	3 45	3 52	− 7	4 45	4 52	− 7	4 15	4 16	− 1	4 35	4 46	−11	4 50	5 2	−12	5 45	5 48	− 3
21.	4 5	4 5	0	4 55	4 57	− 2	4 20	4 18	+ 2	4 55	5 2	− 7	5 15	5 12	+ 3	6 25	6 27	− 2
	4 30	4 33	− 3	5 25	5 22	+ 3	4 30	4 47	−17	5 5	5 1	+ 4	5 20	5 27	− 7	6 40	6 32	+ 8
22.	4 45	4 41	+ 4	5 35	5 43	− 8	4 50	4 53	− 3	5 25	5 27	− 2	6 0	5 52	+ 8	7 25	7 25	0
	5 15	5 18	− 3	5 50	6 2	−12	5 10	5 16	− 6	5 45	5 47	− 2	6 5	6 6	− 1	7 35	7 32	+ 3
23.	5 20	5 12	+ 8	6 0	6 3	− 3	5 25	5 21	+ 4	5 55	5 56	− 1	6 40	6 47	− 7	8 15	8 17	− 2
	6 5	6 12	− 7	6 25	6 33	− 8	5 35	5 46	−11	6 20	6 28	− 8	7 5	7 7	− 2	8 25	8 42	−17
24.	6 15	6 18	− 3	6 35	7 1	−26	5 55	6 2	− 7	6 55	7 7	−12	7 40	7 42	− 2	9 15	9 21	− 6
	6 25	6 21	+ 4	6 50	7 7	−17	6 20	6 22	− 2	7 20	7 32	−12	8 0	8 8	− 8	9 40	9 57	−17
25.	6 35	6 31	+ 4	7 0	7 50	−50	6 20	6 18	+ 2	8 15	8 21	− 6	9 0	9 1	− 1	10 25	10 27	− 2
	7 20	7 23	− 3	7 35	8 13	−38	7 15	7 21	− 6	9 15	9 11	+ 4	9 25	9 32	− 7	10 55	10 58	− 3
26.	7 30	8 3	−33	8 5	9 2	−57	7 20	7 15	+ 5	9 50	9 46	+ 4	10 15	10 22	− 7	11 30	11 42	−12
	8 5	8 21	−16	9 5	9 37	−32	7 45	7 56	−11	10 25	10 27	− 2	10 50	10 41	+ 9	11 55	11 49	+ 6
27.	8 5	8 37	−32	9 35	9 48	−13	8 30	8 28	+ 2	11 10	11 10	0	11 25	11 27	− 2	......	......	......
	8 25	9 57	−92	10 25	10 27	− 2	9 35	9 23	+12	11 35	11 37	− 2	11 40	11 32	+ 8	12 30	12 32	− 2
28.	9 40	9 17	+23	11 0	11 7	− 7	10 20	10 32	−12	......	......	......	......	......	......	12 50	12 55	− 5
	9 45	10 28	−43	11 50	11 47	+ 3	10 55	11 9	−14	12 15	12 18	− 3	12 10	12 17	− 7	1 20	1 22	− 2
29.	10 20	11 33	−73	......	......	......	11 40	12 7	−27	12 30	12 33	− 3	12 25	12 32	− 7	1 40	1 44	− 4
	11 35	11 40	− 5	12 15	11 49	+26	......	......	......	1 0	12 57	+ 3	12 55	1 3	− 8	2 20	2 20	0
30.	11 35	11 32	+ 3	......	......	......	12 15	12 6	+ 9	1 10	1 12	− 2	1 15	1 20	− 5	2 40	2 38	+ 2
	11 25	11 51	−26	......	......	......	12 35	12 41	− 6	1 25	1 42	−17	1 40	1 53	−13	3 5	3 13	− 8
31.	.. ...	......	......	......	......	......	12 50	1 4	−14	......	......	......	1 55	2 2	− 7	......	......	......
	12 20	12 33	−13	......	......	......	1 10	1 21	−11	......	......	......	2 25	2 35	−10	......	......	......

Table B.

Showing a comparison of the observations of the Heights of High Water made at the London Docks, increased by five feet, and those at the St. Katherine Docks. The observations marked with an * appear doubtful.

Date. 1836.	January.			February.			March.			April.			May.			June.		
	London Docks. + 5 ft.	St. Kath. Docks.	Difference.	London Docks. + 5 ft.	St. Kath. Docks.	Difference.	London Docks. + 5 ft.	St. Kath. Docks.	Difference.	London Docks. + 5 ft.	St. Kath. Docks.	Difference.	London Docks. + 5 ft.	St. Kath. Docks.	Difference.	London Docks. + 5 ft.	St. Kath. Docks.	Difference.
	ft. in.	ft. in.	in.	ft. in.	ft. in.	in.	ft. in.	ft. in.	in.	ft. in.	ft. in.	in.	ft. in.	ft. in.	in.	ft. in.	ft. in.	in.
1.	......	......	......	25 10	25 4	+ 6	26 0	25 8	+ 4	27 6	27 6	0	27 10	27 8	+ 2	28 9	28 10	− 1
	25 2	25 2	0	26 3	26 4	− 1	26 4	25 11	+ 5	28 8	28 1	+ 7	28 8	28 8	0	28 5	28 5	0
2.	24 5	24 5	0	26 9	26 9	0	23 0	26 5	−41	28 9	28 9	0	29 2	29 0	+ 2	28 9	28 9	0
	25 0	24 8	+ 4	27 8	27 8	0	26 2	22 9	+45	28 3	28 4	− 1	30 9	30 8	+ 1	28 3	28 4	− 1
3.	24 6	24 7	− 1	28 3	28 0	+ 3	27 5	26 3	+14	28 10	29 0	− 2	29 3	29 2	+ 1	28 3	28 4	− 1
	25 11	26 0	− 1	28 4	28 5	− 1	28 5	28 6	− 1	28 11	28 11	0	29 4	29 5	− 1	27 0	27 1	− 1
4.	25 5	26 9	−16	28 2	28 3	− 1	28 5	28 3	+ 2	28 11	28 10	+ 1	29 2	29 2	0	28 1	28 1	0
	26 8	26 10	− 2	27 6	27 7	− 1	28 3	28 5	− 2	28 6	28 5	+ 1	28 6	28 6	0	26 5	26 4	+ 1
5.	26 10	27 0	− 2	27 2	27 3	− 1	28 0	28 5	− 5	28 4	28 5	− 1	28 5	28 5	0	27 3	27 4	− 1
	27 0	26 9	+ 3	27 4	27 6	− 2	28 10	28 10	0	28 1	28 1	0	27 9	27 9	0		25 7	
6.	26 6	27 1	− 7	26 11	26 10	+ 1	29 3	29 2	+ 1	27 10	27 10	0	27 11	28 0	− 1	26 8	26 9	− 1
	27 1	26 9	+ 4	28 8	28 8	0	29 7	29 4	+ 3	27 4	27 4	0	26 6	26 9	− 3	24 11	24 7	+ 4
7.	26 9	26 6	+ 3	28 2	28 3	− 1	28 11	28 10	+ 1	27 4	27 5	− 1	26 11	27 0	− 1	25 8	25 8	0
	26 6	25 11	+ 7	28 5	28 5	0	28 4	28 4	0	26 2	26 1	+ 1	25 3	25 3	0	25 6	25 5	+ 1
8.	26 0	26 1	− 1	27 2	27 4	− 2	28 5	28 5	0	27 10	27 9	+ 1	25 10	25 9	+ 1	25 8	25 9	− 1
	26 2	26 6	− 4	25 10	25 10	0	28 6	28 7	− 1	26 3	26 3	0	24 2	24 2	0	25 3	25 3	0
9.	26 3	26 1	+ 2	26 3	26 3	0	27 10	27 10	0	26 6	26 6	0	25 6	25 7	− 1	25 11	25 11	0
	26 1	25 9	+ 4	27 0	26 10	+ 2	27 7	27 6	+ 1	25 0	24 10	+ 2	24 0	23 11	+ 1	25 8	25 8	0
10.	25 10	26 6	− 8	26 6	26 8	− 2	27 7	27 8	− 1	25 6	25 6	0	25 8	25 8	0	25 8	25 7	+ 1
	26 4	25 4	+12	25 7	25 8	− 1	26 0	26 0	0	24 5	24 6	− 1	24 0	24 8	− 8	26 2	26 3	− 1
11.	25 3	26 2	−11	24 3	24 0	+ 3	25 6	25 7	− 1	26 0	26 0	0	25 7	25 8	− 1	......	......	......
	26 3	25 8	+ 7	29 3	29 1	+ 2	25 0	25 0	0	25 2	25 2	0	25 3	25 3	0	26 9	26 10	− 1
12.	26 0	26 5	− 5	22 6	22 5	+ 1	24 8	24 8	0	26 3	26 3	0	......	......	......	26 1	26 1	0
	26 4	25 2	+14	28 4	28 4	0	24 0	24 0	0	......	......	......	26 6	26 6	0	22 6	26 7	− 1
13.	25 2	25 0	+ 2	25 3	25 4	− 1	24 10	24 10	0	26 1	26 3	− 2	26 3	26 3	0	26 6	26 7	− 1
	25 5	24 11	+ 6	24 8	24 8	0	24 9	24 9	0	26 10	26 10	0	26 8	26 8	0	26 8	26 7	+ 1
14.	24 9	23 9	+12	26 0	26 1	− 1	22 1	21 10	+ 3	27 1	27 1	0	26 6	26 7	− 1	26 8	26 8	0
	23 10	25 3	−17	......	......	......	......	......	......	28 3	27 8	+ 7	27 3	27 5	− 2	27 1	27 0	+ 1
15.	25 6	26 1	− 7	25 10	25 10		27 0	27 0	0	27 5	27 6	− 1	26 11	26 10	+ 1	27 1	27 3	+ 1
	26 11	26 5	+ 6	26 11	26 10	+ 1	26 8	26 9	− 1	28 1	28 0	+ 1	27 1	27 2	− 1	27 4	27 3	+ 1
16.	28 0	28 1	− 1	26 8	26 8	0	26 1	26 1	0	27 10	27 10	0	27 1	27 1	0	27 2	27 1	+ 1
	......	......	......	27 7	27 7	0	28 3	28 1	+ 2	27 10	27 11	− 1	27 4	27 3	+ 1	26 11	27 0	− 1
17.	26 3	26 3	0	25 5	25 3	+ 2	28 2	28 2	0	28 1	28 1	0	27 0	27 0	0	27 1	27 0	+ 1
	28 7	28 8	− 1	28 6	28 8	− 2	24 3	24 3	0	27 9	27 9	0	27 0	26 11	+ 1	26 11	27 0	− 1
18.	27 6	27 6	0	29 9	29 7	+ 2	28 1	28 2	− 1	27 9	27 4	+ 5	26 8	26 8	0	26 8	26 10	− 2
	26 3	26 1	+ 2	30 1	30 0	+ 1	30 0	29 11	+ 1	27 8	27 7	+ 1	26 8	26 9	− 1	22 2	26 3	− 1
19.	27 10	27 9	+ 1	28 0	27 11	+ 1	27 11	27 10	+ 1	27 5	27 5	0	27 0	26 11	+ 1	26 8	26 3	+ 5
	29 2	27 3	+23	28 4	28 4	0	28 3	28 3	0	27 1	27 1	0	26 2	26 3	− 1	26 3	26 4	− 1
20.	27 3	27 6	− 3	28 3	28 2	+ 1	28 11	28 10	+ 1	26 7	26 8	− 1	25 11	25 11	0	26 3	26 3	0
	27 8	27 7	+ 1	28 0	28 1	− 1	28 6	28 6	0	25 3	25 4	− 1	25 9	25 7	+ 2	25 4	24 5	+11
21.	27 8	27 8	0	27 5	27 5	0	28 0	28 0	0	26 1	26 1	0	25 9	25 10	− 1	25 9	25 9	0
	27 11	27 9	+ 2	26 9	26 9	0	27 10	27 8	+ 2	26 3	26 4	− 1	25 8	25 8	0	25 3	25 4	− 1
22.	27 8	27 8	0	27 3	27 4	− 1	27 10	27 10	0	24 1	25 0	−11	25 1	25 0	+ 1	24 8	24 7	+ 1
	26 10	27 8	−10	26 6	26 8	− 2	27 0	27 1	− 1	25 3	25 3	0	24 2	24 3	− 1	24 6	24 5	+ 1
23.	26 3	26 7	− 4	26 10	26 10	0	27 8	27 8	0	25 6	25 5	+ 1	24 11	25 0	− 1	24 10	24 10	0
	23 3	24 7	−16	26 0	25 11	+ 1	25 4	25 4	0	24 1	24 0	+ 1	24 4	24 4	0	24 9	24 9	0
24.	26 5	26 5	0	25 11	24 11	+12	25 6	25 2	+ 4	24 6	24 7	− 1	25 0	25 0	0	25 5	23 5	+24
	27 4	27 4	0	25 0	24 4	+ 8	24 7	24 8	− 1	24 2	24 2	0	23 4	23 4	0	24 10	24 7	+ 3
25.	26 3	26 1	+ 2	24 5	24 5	0	25 5	25 6	− 1	23 7	23 7	0	24 4	24 4	0	25 7	25 6	+ 1
	24 8	24 7	+ 1	24 1	24 7	− 6	23 4	23 5	− 1	23 0	22 11	+ 1	24 2	24 1	+ 1	25 9	25 6	+ 3
26.	24 8	23 9	+11	24 8	24 2	+ 6	21 2	21 0	+ 2	24 1	24 0	+ 1	25 8	25 8	0	26 5	26 4	+ 1
	23 9	23 4	+ 5	24 2	24 5	− 3	22 6	22 5	+ 1	24 8	23 7	+13	24 9	24 9	0	26 7	26 7	0
27.	23 6	23 2	+ 4	23 5	28 6	−61	23 5	23 6	− 1	25 8	25 7	+ 1	25 10	25 10	0	......	......	......
	23 3	26 7	−40	24 3	24 3	0	22 9	22 9	0	24 5	24 5	0	25 7	25 7	0	26 9	26 8	+ 1
28.	24 8	24 9	− 1	24 9	24 10	− 1	24 11	24 10	+ 1	......	......	......	......	......	......	27 2	27 1	+ 1
	24 10	23 2	+20	24 9	24 9	0	23 2	23 2	0	25 3	25 4	− 1	26 7	26 8	− 1	27 4	27 2	+ 2
29.	23 3	24 5	−14	......	......	......	24 4	24 6	− 2	27 5	27 6	− 1	26 10	26 10	0	28 1	28 1	0
	21 5	25 0	− 7	25 8	24 6	+14	......	......	......	27 9	27 9	0	27 11	27 8	+ 3	27 9	27 9	0
30.	25 1	27 4	−27	......	......	......	25 3	25 3	0	27 3	27 11	− 8	27 3	27 3	0	28 3	28 4	− 1
	27 4	25 2	+26	......	......	......	26 4	26 5	− 1	27 7	27 8	− 1	28 3	28 1	+ 2	28 1	28 7	− 6
31.	......	......	......	......	......	......	25 3	25 6	− 3	......	......	......	28 3	28 3	0	......	......	......

Table C.

Showing a comparison of the observed Times of High Water at the St. Katherine Docks, increased by five minutes, with the predicted Times given in the British Almanac. The observations marked with an * appear doubtful.

Date.	January.			February.			March.			April.			May.			June.		
1836.	British Alman.	St. Kath. Docks. +5 min.	Error of Prediction.	British Alman.	St. Kath. Docks. +5 min.	Error of Prediction.	British Alman.	St. Kath. Docks. +5 min.	Error of Prediction.	British Alman.	St. Kath. Docks. +5 min.	Error of Prediction.	British Alman.	St. Kath. Docks. +5 min.	Error of Prediction.	British Alman.	St. Kath. Docks. +5 min.	Error of Prediction.
	h m	h m	m	h m	h m	m	h m	h m	m	h m	h m	m	h m	h m	m	h m	h m	m
1.	......	......	......	1 16	1 23	− 7	12 48	12 33	+15	1 51	1 33	+18	1 49	1 52	− 3	2 57	2 48	+ 9
	12 32	12 38	− 6	1 40	1 38	+ 2	1 15	12 53	+22	2 11	2 14	− 3	2 8	2 22	−14	3 23	3 23	......
2.	12 55	1 13	−18	2 1	1 48	+13	1 38	1 17	+21	2 28	2 17	+11	2 29	2 38	− 9	3 47	3 42	+ 5
	1 18	1 53	−35	2 20	2 18	+ 2	1 59	1 47	+12	2 42	2 53	−11	2 49	3 3	−14	4 13	4 2	+11
3.	1 40	1 53	−13	2 41	2 38	+ 3	2 18	2 7	+11	3 0	2 53	+ 7	3 11	3 8	+ 3	4 39	4 33	+ 6
	1 57	2 7	−10	3 0	2 58	+ 2	2 37	2 37	0	3 17	3 37	−20	3 35	3 48	−13	5 3	4 48	+15
4.	2 18	2 39	−21	3 18	3 22	− 4	2 57	2 48	+ 9	3 37	3 43	− 6	3 57	3 58	− 1	5 28	5 18	+10
	2 35	2 52	−17	3 35	3 51	−16	3 14	3 18	− 4	3 57	4 7	−10	4 19	4 17	+ 2	5 53	5 39	+14
5.	2 55	3 17	−22	3 53	4 3	−10	3 32	3 20	+12	4 15	4 17	− 2	4 43	4 38	+ 5	6 22	6 22	0
	3 14	3 29	−15	4 10	4 16	− 6	3 48	3 56	− 8	4 35	4 43	− 8	5 8	5 4	+ 4	6 50	6 34	+16
6.	3 31	3 57	−26	4 28	4 23	+ 5	4 4	3 57	+ 7	4 55	5 3	− 8	5 31	5 29	+ 2	7 18	7 22	− 4
	3 49	4 8	−19	4 47	4 47	0	4 20	4 15	+ 5	5 18	5 27	− 9	5 59	5 53	+ 6	7 49	7 43	+ 6
7.	4 8	4 33	−25	5 8	4 51	+17	4 39	4 28	+11	5 41	5 43	− 2	6 15	6 29	−14	8 25	8 42	−17
	4 28	4 45	−17	5 24	5 28	− 4	4 57	4 55	+ 2	6 6	6 11	− 5	6 54	6 47	+ 7	9 1	8 47	+14
8.	4 47	5 17	−30	5 44	5 33	+11	5 14	5 18	− 4	6 31	6 20	+11	7 29	7 32	− 3	9 35	9 42	− 7
	5 7	5 12	− 5	6 4	6 8	− 4	5 35	5 32	+ 3	6 55	6 42	+13	8 8	7 52	+16	10 8	9 48	+20
9.	5 27	5 53	−26	6 23	6 23	0	5 59	5 47	+12	7 27	7 28	− 1	8 50	8 57	− 7	10 39	10 48	− 9
	5 49	5 58	− 9	6 45	6 44	+ 1	6 21	6 15	+ 6	8 7	7 51	+16	9 32	9 26	+ 6	11 9	10 54	+15
10.	6 12	6 35	−23	7 8	6 53	+15	6 43	6 36	+ 7	8 57	9 9	−12	10 16	10 28	−12	11 39	11 51	−12
	6 34	6 37	− 3	7 30	7 34	− 4	7 6	7 14	− 8	9 46	9 27	+19	10 54	10 47	+ 7	12 4	12 2	+ 2
11.	6 58	7 6	− 8	7 56	7 58	− 2	7 35	7 23	+12	10 34	10 37	− 3	11 33	11 32	+ 1	......	......	......
	7 23	7 9	+14	8 29	8 19	+10	8 11	8 13	− 2	11 19	11 3	+16	12 3	11 47	+16	12 28	12 24	+ 4
12.	7 43	7 30	+13	9 7	8 43	+24	8 55	8 36	+19	11 54	11 57	− 3	......	......	......	12 48	12 37	+11
	8 7	8 28	−21	9 50	9 48	+ 2	9 43	9 43	0	......	......	......	12 28	12 27	+ 1	1 11	1 12	− 1
13.	8 39	9 9	−30	10 37	10 52	−15	10 37	10 22	+15	12 26	12 13	+13	12 48	12 37	+11	1 30	1 21	+ 9
	9 10	9 43	−33	11 24	11 33	− 9	11 25	10 58	+27	12 52	12 57	− 5	1 7	1 13	− 6	1 49	1 55	− 6
14.	9 42	10 24	−42	12 6	12 13	− 7	12 5	12 20	−15	1 19	1 23	− 4	1 27	1 17	+10	2 6	2 2	+ 4
	10 18	10 58	−40	......	......	......	......	......	......	1 42	1 33	+ 9	1 44	1 46	− 2	2 27	2 32	− 5
15.	10 58	11 16	−18	12 42	12 36	+ 6	12 38	12 22	+16	2 0	1 49	+11	1 59	1 58	+ 1	2 45	2 47	− 2
	11 35	11 34	+ 1	1 17	1 11	+ 6	1 9	12 47	+22	2 14	2 23	− 9	2 15	2 27	−12	3 3	3 3	0
16.	12 12	12 14	− 2	1 43	1 37	+ 6	1 36	1 12	+24	2 30	2 29	+ 1	2 32	2 36	− 4	3 24	3 18	+ 6
	......	......	......	2 6	2 18	−12	1 58	1 46	+12	2 47	3 2	−15	2 48	2 58	−10	3 38	3 33	+ 5
17.	12 48	12 46	+ 2	2 36	2 37	− 1	2 18	2 4	+14	3 2	3 10	− 8	3 4	3 11	− 7	3 55	4 2	− 7
	1 18	1 13	+ 5	2 58	3 9	−11	2 40	2 44	− 4	3 20	3 23	− 3	3 22	3 33	−11	4 13	4 13	0
18.	1 48	1 42	+ 6	3 17	3 32	−15	2 59	2 58	+ 1	3 35	3 29	+ 6	3 38	3 42	− 4	4 32	4 26	+ 6
	2 16	2 17	− 1	3 38	3 42	− 4	3 14	3 2	+12	3 48	3 56	− 8	3 54	3 56	− 2	4 47	4 36	+11
19.	2 39	2 33	+ 6	3 56	3 53	+ 3	3 33	3 18	+15	4 6	4 1	+ 5	4 10	4 22	−12	5 6	5 2	+ 4
	3 4	2 53	+11	4 15	4 28	−13	3 47	4 2	−15	4 22	4 25	− 3	4 27	4 37	−10	5 26	5 11	+15
20.	3 28	3 32	− 4	4 32	4 23	+ 9	4 2	3 53	+ 9	4 37	4 37	0	4 44	4 48	− 4	5 45	5 41	+ 4
	3 51	3 57	− 6	4 49	4 57	− 8	4 18	4 21	− 3	4 49	4 51	− 2	5 3	5 7	− 4	6 5	5 53	+13
21.	4 14	4 10	+ 4	5 5	5 2	+ 3	4 34	4 23	+11	5 3	5 7	− 4	5 21	5 17	+ 4	6 29	6 32	− 3
	4 36	4 38	− 2	5 21	5 27	− 6	4 48	4 52	− 4	5 19	5 6	+13	5 39	5 32	+ 7	6 56	6 37	+19
22.	4 57	4 46	+11	5 35	5 48	−13	5 3	4 58	+ 5	5 37	5 32	+ 5	6 2	5 57	+ 5	7 25	7 30	− 5
	5 18	5 23	− 5	5 54	6 7	−13	5 17	5 21	− 4	5 56	5 52	+ 4	6 24	6 11	+13	7 55	7 37	+18
23.	5 40	5 17	+23	6 11	6 8	+ 3	5 33	5 26	+ 7	6 19	6 1	+18	6 50	6 52	− 2	8 21	8 22	− 1
	6 3	6 17	−14	6 28	6 38	−10	5 51	5 51	0	6 41	6 33	+ 8	7 18	7 12	+ 6	8 47	8 47	0
24.	6 20	6 23	− 3	6 46	7 6	−20	6 9	6 7	+ 2	7 10	7 12	− 2	7 49	7 47	+ 2	9 17	9 26	− 9
	6 38	6 26	+12	7 5	7 12	− 7	6 28	6 27	+ 1	7 39	7 37	+ 2	8 25	8 13	+12	9 44	10 2	−18
25.	6 56	6 36	+20	7 27	7 55	−28	6 45	6 23	+22	8 18	8 26	− 8	9 3	9 6	− 3	10 17	10 32	−15
	7 15	7 28	−13	7 49	8 18	−29	7 9	7 26	−17	9 3	9 16	−13	9 40	9 37	+ 3	10 51	11 2	−11
26.	7 35	8 8	−33	8 21	9 7	−46	7 37	7 20	+17	9 44	9 51	− 7	10 17	10 27	−10	11 23	11 47	−24
	7 59	8 26	−27	8 56	9 42	−46	8 12	8 1	+11	10 26	10 32	− 6	10 48	10 46	+ 2	11 55	11 54	+ 1
27.	8 25	8 42	−17	9 40	9 53	−13	8 56	8 33	+23	11 5	11 15	−10	11 18	11 32	−14	......	......	......
	8 52	10 2	−70	10 24	10 32	− 8	9 44	9 28	+16	11 38	11 42	− 4	11 40	11 37	+ 3	12 30	12 37	− 7
28.	9 25	9 22	+ 3	11 8	11 12	− 4	10 28	10 37	− 9	......	......	... 0	......	......	......	12 59	1 0	− 1
	10 0	10 33	−33	11 47	11 52	− 5	11 13	11 14	− 1	12 8	12 23	−15	12 6	12 22	−16	1 28	1 27	+ 1
29.	10 37	11 38	−61	......	......	......	11 48	12 12	−24	12 33	12 38	− 5	12 32	12 37	− 5	1 53	1 49	+ 4
	11 16	11 45	−29	12 20	11 54	+24	......	......	......	12 55	1 2	− 7	12 57	1 8	−11	2 21	2 25	− 4
30.	11 49	11 37	+12	......	......	......	12 18	12 11	+ 7	1 13	1 17	− 4	1 19	1 25	− 6	2 50	2 43	+ 7
	12 20	11 56	+24	......	......	......	12 45	12 46	− 1	1 30	1 47	−17	1 41	1 58	−17	3 15	3 18	− 3
31.	......	......	......	......	......	......	1 9	1 9	0	......	......	......	2 6	2 7	− 1	......	......	......
	12 49	12 38	+11	......	......	......	1 32	1 26	+ 6	......	......	......	2 30	2 40	−10	......	......	......

Table D.

Showing a comparison of the observed Heights of High Water at the St. Katherine Docks, with the predicted Heights given in the British Almanac, increased by five feet. The observations marked with an * appear doubtful.

Date.	January.			February.			March.			April.			May.			June.		
1836.	British Alman. + 5 ft.	St. Kath. Docks.	Error of Prediction.	British Alman. + 5 ft.	St. Kath. Docks.	Error of Prediction.	British Alman. + 5 ft.	St. Kath. Docks.	Error of Prediction.	British Alman. + 5 ft.	St. Kath. Docks.	Error of Prediction.	British Alman. + 5 ft.	St. Kath. Docks.	Error of Prediction.	British Alman. + 5 ft.	St. Kath. Docks.	Error of Prediction.
	ft. in.	ft. in.	in.	ft. in.	ft. in.	in.	ft. in.	ft. in.	in.	ft. in.	ft. in.	in.	ft. in.	ft. in.	in.	ft. in.	ft. in.	in.
1.	......	......	......	25 9	25 4	+ 5	25 6	25 8	− 2	27 4	27 6	− 2	27 7	27 8	− 1	28 2	28 10	− 8
	25 0	25 2	− 2	26 1	26 4	− 3	26 1	25 11	+ 2	27 7	28 1	− 6	27 10	28 8	−10	28 2	28 5	− 3
2.	25 4	24 5	+11	26 6	26 9	− 3	26 6	26 5	+ 1	27 8	28 9	−13	28 0	29 0	−12	28 2	28 9	− 7
	25 9	24 8	+13	26 11	27 8	− 9	26 10	22 9	+49	27 10	28 4	− 6	28 1	30 8	−31	28 0	28 4	− 4
3.	26 1	24 7	+18	27 1	28 0	−11	27 2	26 3	+11	27 11	29 0	−13	28 2	29 2	−12	27 10	28 4	− 6
	26 4	26 0	+ 4	27 3	28 5	−14	27 6	28 6	−12	28 0	28 11	−11	28 3	29 5	−14	27 6	27 1	+ 5
4.	26 9	26 9	0	27 5	28 3	−10	27 8	28 3	− 7	28 1	28 10	− 9	28 2	29 2	−12	27 1	28 1	−12
	26 11	26 10	+ 1	27 6	27 7	− 1	27 10	28 5	− 7	28 2	28 5	− 3	28 0	28 6	− 6	26 7	26 4	+ 3
5.	27 1	27 0	+ 1	27 7	27 3	+ 4	27 11	28 5	− 6	28 0	28 5	− 5	27 9	28 5	− 8	26 1	27 4	−15
	27 2	26 9	+ 5	27 8	27 6	+ 2	28 0	28 10	−10	27 11	28 1	− 2	27 5	27 9	− 4	25 7	25 7	0
6.	27 3	27 1	+ 2	27 8	26 10	+10	28 0	29 2	−14	27 9	27 10	− 1	26 10	28 0	−14	25 1	26 9	−18
	27 4	26 9	+ 7	27 7	28 8	−13	27 11	29 4	−17	27 5	27 4	+ 1	26 4	26 9	− 5	24 9	24 7	+ 2
7.	27 4	26 6	+10	27 6	28 3	− 9	27 11	28 10	−11	26 11	27 5	− 6	25 9	27 0	−15	24 7	25 8	−13
	27 3	25 11	+16	27 4	28 5	−13	27 9	28 4	− 7	26 5	26 1	+ 4	25 3	25 3	0	24 1	25 5	−12
8.	27 2	26 1	+13	27 1	27 4	− 3	27 7	28 5	−10	25 10	27 9	−23	24 9	25 9	−12	24 5	25 9	−16
	27 0	26 6	+ 6	26 10	25 10	+12	27 3	28 7	−16	25 2	26 3	−13	24 6	24 2	+ 4	24 4	25 3	− 9
9.	26 11	26 1	+10	26 6	26 3	+ 3	26 11	27 10	−11	24 9	26 6	−21	24 3	25 7	−16	24 5	25 11	−18
	26 8	25 9	+11	26 3	26 10	− 7	26 0	27 6	−18	24 5	24 10	− 5	24 5	23 11	+ 6	24 6	25 8	−14
10.	26 5	26 6	− 1	25 10	26 8	−10	25 1	27 8	−31	24 4	25 6	−14	24 8	25 8	−12	24 8	25 7	−11
	26 2	25 4	+10	25 5	25 8	− 3	24 7	26 0	−17	24 5	24 6	− 1	24 11	24 8	+ 3	24 11	26 3	−16
11.	26 0	26 2	− 2	25 0	24 0	+12	24 3	25 7	−16	24 9	26 0	−13	25 3	25 8	− 5	......	......	......
	25 9	25 8	+ 1	24 8	29 1	−53	24 1	25 0	−11	25 1	25 2	− 1	25 6	25 3	+ 3	25 0	26 10	−22
12.	25 5	26 5	−12	24 6	22 5	+25	24 2	24 8	− 6	25 7	26 3	− 8	......	......	......	25 4	26 1	− 9
	25 2	25 2	0	24 7	28 4	−45	24 5	24 0	+ 5	......	......	......	25 8	26 6	−10	25 9	26 7	−10
13.	24 11	25 0	− 1	24 11	25 4	− 5	24 10	24 10	0	25 10	26 3	− 5	25 10	26 3	− 5	26 0	26 7	− 7
	24 10	24 11	− 1	25 4	24 8	+ 8	25 3	24 9	+ 6	26 3	26 10	− 7	26 1	26 8	− 7	26 3	26 7	− 4
14.	24 9	23 9	+12	25 10	26 1	− 3	25 8	21 10	+46	26 8	27 1	− 5	26 4	26 7	− 3	26 7	26 8	− 1
	25 0	25 3	− 3	......	......	......	......	......	......	26 11	27 8	− 9	26 7	27 5	−10	26 10	27 0	− 2
15.	25 4	26 1	− 9	26 4	25 10	+ 6	26 1	27 0	−11	27 0	27 6	− 6	26 9	26 10	− 1	27 1	27 0	+ 1
	25 9	26 5	− 8	26 10	26 10	0	26 7	26 9	− 2	27 1	28 0	−11	26 11	27 2	− 3	27 1	27 3	− 2
16.	26 2	28 1	−23	27 1	26 8	+ 5	26 10	26 1	+ 9	27 2	27 10	− 8	27 0	27 1	− 1	27 0	27 1	− 1
	......	......	......	27 5	27 7	− 2	27 1	28 1	−12	27 3	27 11	− 8	27 1	27 3	− 2	27 0	27 0	0
17.	26 9	26 3	+ 6	27 7	25 3	+28	27 4	28 2	−10	27 3	28 1	−10	27 1	27 0	+ 1	27 0	27 0	0
	27 0	28 8	−20	27 8	28 8	−12	27 7	24 3	+40	27 3	27 9	− 6	27 0	26 11	+ 1	26 11	27 0	− 1
18.	27 5	27 6	− 1	27 9	29 7	−20	27 8	28 2	− 6	27 3	27 4	− 1	27 0	26 8	+ 4	26 10	26 10	0
	27 10	26 1	+21	27 9	30 0	−27	27 8	29 11	−27	27 2	27 7	− 5	26 11	26 9	+ 2	26 8	26 3	+ 5
19.	27 10	27 9	+ 1	27 9	27 11	− 2	27 7	27 10	− 3	27 1	27 5	− 4	26 10	26 11	− 1	26 6	26 3	+ 3
	27 11	27 3	+ 8	27 8	28 4	− 8	27 6	28 3	− 9	26 11	27 1	− 2	26 8	26 3	+ 5	26 3	26 4	− 1
20.	27 11	27 6	+ 5	27 6	28 2	− 8	27 5	28 10	−17	26 9	26 8	+ 1	26 6	25 11	+ 7	26 1	26 3	− 2
	27 11	27 7	+ 4	27 3	28 1	−10	27 3	28 6	−15	26 6	25 4	+14	26 4	25 7	+ 9	25 10	24 5	+17
21.	27 10	27 8	+ 2	27 0	27 5	− 5	27 1	28 0	−11	26 3	26 1	+ 2	26 0	25 10	+ 2	25 8	25 9	− 1
	27 8	27 9	− 1	26 8	26 9	− 1	26 11	27 8	− 9	26 11	26 4	+ 7	25 8	25 8	0	25 6	25 4	+ 2
22.	27 5	27 8	− 3	26 6	27 4	−10	26 8	27 10	−14	25 7	25 0	+ 7	25 5	25 0	+ 5	25 3	24 7	+ 8
	27 2	27 8	− 6	26 1	26 8	− 7	26 4	27 1	− 9	25 3	25 3	0	25 1	24 3	+11	25 1	24 5	+ 8
23.	26 9	26 7	+ 2	25 10	26 10	−12	26 1	27 8	−19	24 10	25 5	− 7	24 10	25 0	− 2	25 0	24 10	+ 2
	26 3	24 7	+20	25 4	25 11	− 7	25 8	25 4	+ 4	24 6	24 0	+ 6	24 7	24 4	+ 3	24 10	24 9	+ 1
24.	26 0	26 5	− 5	25 0	24 11	+ 1	25 4	25 2	+ 2	24 2	24 7	− 5	24 6	25 0	− 6	24 9	23 5	+16
	25 8	27 4	−20	24 8	24 4	+ 4	24 11	24 8	+ 3	24 0	24 2	− 2	24 5	23 4	+13	24 10	24 7	+ 3
25.	25 3	26 1	−10	24 2	24 5	− 3	24 6	25 6	−12	23 10	23 7	+ 3	24 6	24 4	+ 2	25 0	25 6	− 6
	24 11	24 7	+ 4	23 11	24 7	− 8	24 1	23 5	+ 8	23 11	22 11	+12	24 9	24 1	+ 8	25 4	25 6	− 2
26.	24 6	23 9	+ 9	23 6	24 2	− 8	23 10	21 0	+34	24 2	24 0	+ 2	25 2	25 8	− 6	25 9	26 4	− 7
	24 1	23 4	+ 9	23 4	24 5	−13	23 7	22 5	+14	24 6	23 7	+11	25 5	24 9	+ 8	26 1	26 7	− 6
27.	23 10	23 2	+ 8	23 5	28 6	−61	23 7	23 6	+ 1	25 1	25 7	− 6	25 9	25 10	− 1	......	......	......
	23 7	26 7	−36	23 8	24 3	− 7	23 8	22 9	+11	25 7	24 5	+14	26 0	25 7	+ 5	26 7	26 8	− 1
28.	23 4	24 9	−17	24 1	24 10	− 9	24 3	24 10	− 7	......	......	......	......	......	......	27 1	27 1	0
	23 5	23 2	+ 2	24 6	24 9	− 3	24 7	23 2	+17	26 1	25 4	+ 9	26 3	26 8	− 5	27 5	27 2	+ 3
29.	23 8	24 5	− 9	......	......	......	25 0	24 6	+ 6	26 7	27 6	−11	26 8	26 10	− 2	27 9	28 1	− 4
	23 11	25 0	−13	24 11	24 6	+ 5	......	......	......	26 11	27 9	−10	27 1	27 8	− 7	28 0	27 9	+ 3
30.	24 4	27 4	−36	......	......	......	25 7	25 3	+ 4	27 5	27 11	− 6	27 4	27 3	+ 1	28 1	28 4	− 3
	24 10	25 2	− 4	......	......	......	26 0	26 5	− 5	27 7	27 8	− 1	27 7	28 1	− 6	28 2	28 7	− 5
31.	......	......	......	......	......	......	26 7	25 6	+13	......	......	......	27 11	28 3	− 4	......	......	......
	25 3	23 7	+20				27 0	23 5	+43			......	28 1	28 7	− 6	......	......	......

Table I.

Showing the Interval between the Apparent Solar Time of the Moon's Transit and the Time of High Water, and the Height of High Water at the London Docks (together with the Interval between the Moon's Transits), corresponding to the Apparent Solar Time of the Moon's Transit, in each month of the year, from 13,370 observations made at the London Docks between the 1st of January 1808, and the 31st of December 1826.

January.

Number of Observations.	Apparent Solar Time of Moon's Transit.	Interval between the Moon's Transit and the Time of high water.	Height of Tide.	Interval between Moon's Transits.	Mean of Moon's Declination.	Mean Hor. Par.
	h m	h m	ft. in.	h m	°	′
90	0 30·0	3 3·3	22 8·4	1 40·1	19	56·9
89	1 28·8	2 47·3	22 6·7	1 36·1	16	57·3
103	2 28·3	2 37·5	21 11·3	1 32·2	11	57·1
103	3 30·4	2 29·5	21 4·9	1 30·5	6	56·9
101	4 30·0	2 27·7	20 5·4	1 31·8	5	56·7
109	5 30·5	2 44·0	19 6·0	1 36·3	9	56·9
96	6 30·2	3 9·8	19 7·1	1 40·9	13	56·9
97	7 29·6	3 39·9	20 2·2	1 46·7	18	57·0
89	8 29·9	3 50·3	21 1·5	1 50·4	21	57·2
86	9 29·3	3 49·6	21 8·3	1 50·7	23	57·1
86	10 29·3	3 35·9	22 2·5	1 49·7	23	57·4
88	11 29·2	3 18·3	22 6·9	1 48·0	21	57·2

Sun's Declination S. 21°.

February.

Number of Observations.	Apparent Solar Time of Moon's Transit.	Interval between the Moon's Transit and the Time of high water.	Height of Tide.	Interval between Moon's Transits.	Mean of Moon's Declination.	Mean Hor. Par.
	h m	h m	ft. in.	h m	°	′
89	0 29·1	3 2·8	23 0·2	1 34·8	10	57·3
93	1 27·5	2 49·5	22 8·7	1 33·4	6	57·3
98	2 28·1	2 36·2	22 1·2	1 33·9	5	57·2
95	3 29·5	2 24·5	21 4·4	1 36·5	8	56·8
89	4 29·8	2 20·8	20 2·5	1 40·0	14	56·7
83	5 28·8	2 30·7	19 1·7	1 45·3	18	56·7
84	6 28·1	3 5·8	19 0·6	1 47·3	21	56·6
78	7 28·5	3 39·8	19 11·8	1 49·5	23	56·8
77	8 27·1	3 55·2	20 10·1	1 48·0	22	56·8
83	9 27·6	3 49·4	21 8·7	1 46·1	22	57·2
86	10 30·0	3 35·0	22 5·2	1 41·2	19	57·2
83	11 30·8	3 18·3	22 10·3	1 37·4	14	57·3

Sun's Declination S. 13°.

March.

Number of Observations.	Apparent Solar Time of Moon's Transit.	Interval between the Moon's Transit and the Time of high water.	Height of Tide.	Interval between Moon's Transits.	Mean of Moon's Declination.	Mean Hor. Par.
102	0 29·0	3 5·9	22 10·5	1 35·2	5	57·2
100	1. 30·5	2 49·4	22 8·8	1 37·8	8	57·2
96	2 29·8	2 34·1	21 10·4	1 40·9	13	56·9
92	3 28·7	2 21·2	20 11·9	1 45·0	17	56·8
93	4 29·0	2 14·0	19 9·5	1 46·9	23	57·2
86	5 29·2	2 24·7	18 11·6	1 48·2	22	56·5
90	6 28·6	3 7·7	18 11·9	1 47·6	23	56·8
89	7 28·5	3 45·1	19 10·8	1 45·2	22	56·8
98	8 29·2	3 57·7	20 11·3	1 42·2	19	57·1
92	9 29·4	3 49·0	21 10·3	1 39·4	16	57·4
101	10 29·2	3 35·8	22 5·5	1 36·6	11	57·5
98	11 29·0	3 21·6	22 11·5	1 35·5	6	57·1

Sun's Declination S. 2°.

April.

Number of Observations.	Apparent Solar Time of Moon's Transit.	Interval between the Moon's Transit and the Time of high water.	Height of Tide.	Interval between Moon's Transits.	Mean of Moon's Declination.	Mean Hor. Par.
91	0 29·4	3 7·0	23 0·7	1 43·8	13	57·2
88	1 27·9	2 50·8	22 5·7	1 47·0	17	57·4
91	2 28·7	2 33·4	21 10·8	1 48·3	20	57·0
82	3 28·8	2 20·2	20 11·8	1 49·4	22	57·0
86	4 27·8	2 13·5	19 6·0	1 46·9	23	57·2
86	5 28·1	2 28·3	19 0·9	1 44·2	22	56·6
89	6 28·2	3 8·7	19 3·6	1 40·7	20	57·0
94	7 28·8	3 41·4	20 1·8	1 36·4	16	56·8
97	8 29·5	3 52·2	21 3·7	1 34·2	11	57·0
101	9 29·9	3 48·1	22 3·3	1 34·3	6	57·1
98	10 30·1	3 37·3	22 10·3	1 36·3	5	57·3
97	11 30·2	3 22·2	23 2·2	1 39·9	7	57·5

Sun's Declination N. 10°.

May.

Number of Observations.	Apparent Solar Time of Moon's Transit.	Interval between the Moon's Transit and the Time of high water.	Height of Tide.	Interval between Moon's Transits.	Mean of Moon's Declination.	Mean Hor. Par.
91	0 30·1	3 4·6	22 10·3	1 50·6	20	57·5
86	1 30·1	2 49·3	22 5·9	1 51·5	22	57·6
89	2 29·9	2 32·1	21 8·6	1 48·6	23	57·2
87	3 29·6	2 22·3	20 11·5	1 45·5	22	57·0
95	4 29·3	2 20·6	20 1·0	1 40·3	20	56·8
94	5 29·1	2 35·5	19 8·1	1 35·6	17	56·6
99	6 29·0	3 10·1	19 9·2	1 32·2	12	56·5
104	7 29·3	3 37·1	20 7·6	1 32·2	7	56·7
103	8 30·2	3 48·1	21 5·3	1 33·2	5	56·8
100	9 30·1	3 47·6	22 2·6	1 37·4	7	57·1
92	10 28·1	3 38·1	22 8·3	1 42·7	12	57·3
97	11 27·7	3 22·5	22 10·3	1 47·2	17	57·5

Sun's Declination N. 19°.

June.

Number of Observations.	Apparent Solar Time of Moon's Transit.	Interval between the Moon's Transit and the Time of high water.	Height of Tide.	Interval between Moon's Transits.	Mean of Moon's Declination.	Mean Hor. Par.
85	0 28·3	3 4·1	22 7·1	1 45·1	21	57·3
87	1 30·1	2 48·7	22 3·7	1 45·6	22	57·4
87	2 30·4	2 35·2	21 11·3	1 40·9	20	57·4
92	3 30·1	2 27·1	21 1·8	1 34·8	16	57·0
99	4 30·1	2 31·0	20 7·2	1 32·8	11	57·0
100	5 29·7	2 43·5	20 0·6	1 31·1	7	56·8
104	6 30·8	3 11·3	19 11·7	1 32·0	5	56·6
97	7 31·0	3 36·8	20 7·1	1 35·8	8	56·4
95	8 30·4	3 47·2	21 2·5	1 41·4	12	57·0
88	9 29·6	3 46·4	21 11·2	1 46·6	17	57·1
85	10 28·7	3 34·9	22 3·6	1 49·3	20	57·2
83	11 28·2	3 22·5	22 6·3	1 51·0	22	57·3

Sun's Declination N. 23°.

July.

Number of Observations.	Apparent Solar Time of Moon's Transit.	Interval between the Moon's Transit and the Time of high water.	Height of Tide.	Interval between Moon's Transits.	Mean of Moon's Declination.	Mean Hor. Par.
86	0 30·1	3 5·0	22 8·2	1 41·6	20	57·3
89	1 27·9	2 51·4	22 6·1	1 36·3	16	57·1
106	2 29·1	2 39·3	22 2·5	1 33·2	11	57·3
101	3 30·9	2 33·3	21 6·4	1 31·9	6	57·1
103	4 29·6	2 32·8	20 8·2	1 32·5	5	56·9
107	5 29·6	2 45·8	20 1·1	1 36·4	8	57·1
101	6 28·8	3 12·8	19 10·3	1 40·9	13	56·9
96	7 28·5	3 37·7	20 3·4	1 45·1	17	56·9
88	8 28·9	3 50·9	20 11·9	1 49·2	20	57·0
88	9 29·2	3 48·7	21 8·1	1 50·8	23	57·0
83	10 28·9	3 38·8	22 2·3	1 49·5	23	57·1
91	11 29·3	3 21·7	22 6·2	1 45·7	22	57·2

Sun's Declination N. 21°.

August.

Number of Observations.	Apparent Solar Time of Moon's Transit.	Interval between the Moon's Transit and the Time of high water.	Height of Tide.	Interval between Moon's Transits.	Mean of Moon's Declination.	Mean Hor. Par.
98	0 30·0	3 9·8	22 9·8	1 34·7	11	57·1
94	1 30·1	2 51·4	22 7·7	1 32·2	7	57·0
98	2 28·7	2 40·6	22 1·4	1 33·5	4	57·0
100	3 29·1	2 29·2	21 4·2	1 36·0	8	56·8
97	4 29·8	2 25·6	20 5·8	1 40·1	13	56·7
90	5 29·2	2 35·8	19 5·8	1 45·0	18	56·6
99	6 29·9	3 11·0	19 3·4	1 48·3	21	56·8
88	7 30·0	3 43·0	19 11·6	1 51·3	22	57·2
92	8 28·5	3 56·7	20 10·6	1 50·1	23	57·1
89	9 29·6	3 52·4	21 8·4	1 47·6	22	57·3
89	10 30·5	3 39·4	22 3·0	1 43·2	19	57·2
92	11 29·8	3 26·0	22 8·6	1 38·4	16	57·3

Sun's Declination N. 14°.

Table I. (Continued.)

September.							October.						
Number of Observations.	Apparent Solar Time of Moon's Transit.	Interval between the Moon's Transit and the Time of high water.	Height of Tide.	Interval between Moon's Transits.	Mean of Moon's Declination.	Mean Hor. Par.	Number of Observations.	Apparent Solar Time of Moon's Transit.	Interval between the Moon's Transit and the Time of high water.	Height of Tide.	Interval between Moon's Transits.	Mean of Moon's Declination.	Mean Hor. Par.
	h m	h m	ft. in.	h m	°	′		h m	h m	ft. in.	h m	°	′
100	0 29·1	3 9·5	23 0·3	1 35·7	5	57·1	96	0 30·0	3 9·0	22 11·2	1 42·7	12	57·5
99	1 29·8	2 53·0	22 7·5	1 37·3	8	57·2	96	1 30·2	2 52·3	22 6·3	1 46·4	16	57·5
94	2 30·1	2 37·2	22 0·8	1 40·7	12	57·0	89	2 30·4	2 33·5	21 9·5	1 48·0	20	57·0
88	3 28·8	2 26·7	21 1·1	1 44·6	17	56·9	87	3 29·0	2 18·9	20 10·9	1 49·1	22	56·9
92	4 30·0	2 16·2	19 10·3	1 46·6	20	56·5	89	4 29·9	2 10·1	19 11·2	1 47·4	23	56·7
81	5 30·4	2 27·6	19 1·8	1 49·3	22	56·8	90	5 30·2	2 21·7	19 1·6	1 44·5	22	56·6
84	6 30·2	3 9·5	18 11·8	1 47·6	23	56·6	87	6 31·4	3 5·0	19 1·1	1 41·1	20	56·7
87	7 29·9	3 46·9	19 11·0	1 44·3	22	56·8	90	7 31·4	3 41·9	20 1·9	1 36·6	17	56·6
89	8 29·9	3 58·0	21 0·0	1 42·3	20	57·0	95	8 30·0	3 55·4	21 3·6	1 34·4	12	56·8
94	9 29·7	3 53·6	21 11·0	1 39·4	17	57·3	103	9 28·8	3 51·8	22 2·2	1 33·8	7	56·9
94	10 29·6	3 43·1	22 6·5	1 36·6	12	57·3	105	10 29·2	3 40·5	22 9·1	1 35·6	5	57·4
96	11 29·3	3 26·6	22 11·3	1 35·4	7	57·4	102	11 29·8	3 26·2	22 11·5	1 38·7	7	57·5
Sun's Declination N. 3°.							Sun's Declination S. 9°.						

November.							December.						
87	0 28·6	3 5·1	22 7·3	1 49·9	20	57·5	87	0 28·9	3 3·7	22 6·2	1 48·2	23	57·2
86	1 28·6	2 47·8	22 3·1	1 50·7	22	57·3	85	1 30·5	2 46·0	22 1·5	1 45·4	22	57·2
85	2 29·2	2 30·9	21 10·5	1 48·9	23	57·2	89	2 31·2	2 31·3	21 10·3	1 40·4	20	57·4
87	3 29·8	2 21·4	21 1·1	1 45·2	22	57·3	93	3 30·6	2 27·6	21 1·6	1 35·5	16	57·1
89	4 30·3	2 17·7	19 11·5	1 40·0	20	56·8	101	4 29·4	2 26·4	20 5·2	1 32·4	11	57·1
88	5 28·8	2 28·1	19 6·4	1 36·1	18	56·6	107	5 30·0	2 37·1	19 11·9	1 30·9	7	56·9
103	6 29·3	3 3·3	19 5·8	1 32·5	12	56·6	101	6 29·4	3 6·2	19 10·9	1 31·9	5	56·7
95	7 29·9	3 36·0	20 4·7	1 31·9	7	56·7	103	7 28·3	3 31·2	20 5·3	1 34·9	8	56·7
97	8 29·2	3 47·6	21 1·8	1 33·1	5	56·9	102	8 29·7	3 44·8	21 3·6	1 41·1	12	56·9
96	9 29·7	3 46·5	22 1·8	1 37·1	7	57·2	88	9 29·9	3 44·8	21 10·7	1 45·5	17	57·1
93	10 29·3	3 35·3	22 6·1	1 41·3	10	57·1	89	10 28·4	3 33·8	22 3·3	1 48·2	20	57·1
88	11 28·6	3 21·2	22 9·6	1 46·3	16	57·3	88	11 28·0	3 21·3	22 3·5	1 49·1	23	57·2
Sun's Declination S. 18°.							Sun's Declination S. 23°.						

The argument, Moon's Transit, is the fifth transit previous to the time of high water, therefore all the *intervals* must be increased by 48 hours. The fifth column contains the interval between the moon's transit in the second column and the fourth afterwards, both transits being the mean of the same as the number of observations given in the first column.

Table II. (Interpolated from Table I.)

Showing the Interval between the Apparent Solar Time of the Moon's Transit and the Time of High Water at the London Docks, for each month in the year.

Moon's Transit.	January.	February.	March.	April.	May.	June.	July.	August.	Sept.	October.	Nov.	Dec.	Mean.
h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m
0 30	3 3·3	3 2·6	3 5·6	3 6·9	3 4·6	3 3·7	3 5·0	3 9·8	3 9·2	3 9·0	3 4·7	3 3·4	3 5·7
1 30	2 47·1	2 48·9	2 49·5	2 50·2	2 49·3	2 48·7	2 51·0	2 51·4	2 53·0	2 52·4	2 47·4	2 46·1	2 49·6
2 30	2 37·3	2 35·8	2 34·1	2 33·1	2 32·1	2 35·3	2 39·2	2 40·4	2 37·2	2 33·6	2 30·8	2 31·6	2 35·1
3 30	2 29·6	2 24·5	2 21·0	2 20·1	2 22·3	2 27·1	2 33·3	2 29·1	2 26·5	2 18·7	2 21·4	2 27·6	2 25·1
4 30	2 27·7	2 20·8	2 14·2	2 14·0	2 20·8	2 30·1	2 32·8	2 25·6	2 16·2	2 10·1	2 17·7	2 26·4	2 21·4
5 30	2 43·9	2 31·4	2 25·3	2 29·6	2 36·0	2 43·6	2 45·9	2 36·1	2 27·4	2 21·6	2 28·8	2 37·1	2 33·9
6 30	3 9·7	3 6·9	3 8·6	3 9·7	3 10·5	3 10·9	3 13·3	3 11·1	3 9·4	3 4·1	3 3·7	3 6·5	3 8·8
7 30	3 40·0	3 40·2	3 45·4	3 41·6	3 37·2	3 36·4	3 38·2	3 43·0	3 46·9	3 41·3	3 36·0	3 31·6	3 39·8
8 30	3 50·3	3 54·9	3 57·6	3 52·2	3 48·1	3 47·1	3 51·0	3 56·8	3 58·0	3 55·4	3 47·7	3 44·8	3 52·0
9 30	3 49·6	3 48·8	3 48·9	3 48·1	3 47·6	3 46·3	3 48·6	3 52·3	3 53·6	3 51·6	3 46·5	3 44·8	3 48·9
10 30	3 35·7	3 35·0	3 35·6	3 37·3	3 37·6	3 34·7	3 38·6	3 39·5	3 43·0	3 40·3	3 35·2	3 33·6	3 37·2
11 30	3 18·1	3 18·5	3 21·4	3 22·2	3 21·8	3 22·0	3 21·5	3 26·0	3 26·4	3 26·1	3 20·9	3 20·8	3 22·1

Table III. (Interpolated from Table I.)

Showing the Height of High Water at the London Docks, corresponding to the Apparent Solar Time of the Moon's Transit, in each month of the year.

Moon's Transit.	January.	February.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.	Mean.
h m	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.
0 30	22·70	23·01	22·87	23·05	22·86	22·58	22·68	22·82	23·02	22·93	22·59	22·52	22·80
1 30	22·55	22·70	22·74	22·45	22·49	22·31	22·50	22·64	22·62	22·52	22·25	22·12	22·66
2 30	21·93	22·08	21·86	21·88	21·72	21·94	22·20	22·10	22·07	21·80	21·87	21·87	21·94
3 30	21·41	21·36	20·97	20·95	20·95	21·15	21·54	21·34	21·07	20·90	21·09	21·14	21·16
4 30	20·45	20·20	19·78	19·48	20·09	20·60	20·67	20·48	19·86	19·93	19·96	20·42	20·17
5 30	19·51	19·14	18·97	19·08	19·69	20·05	20·09	19·47	19·15	19·13	19·51	19·99	19·49
6 30	19·59	19·08	19·00	19·33	19·78	19·98	19·86	19·28	18·98	19·08	19·48	19·91	19·44
7 30	20·19	20·00	19·93	20·17	20·64	20·58	20·30	19·97	19·92	20·13	20·40	20·47	20·22
8 30	21·13	20·88	20·95	21·32	21·44	21·20	21·01	20·91	21·00	21·30	21·16	21·30	21·14
9 30	21·70	21·75	21·86	22·28	22·22	21·93	21·68	21·70	21·92	22·20	22·15	21·89	21·94
10 30	22·21	22·43	22·46	22·86	22·70	22·31	22·20	22·25	22·54	22·77	22·51	22·28	22·46
11 30	22·58	22·86	22·96	23·18	22·85	22·53	22·52	22·72	22·94	22·96	22·80	22·30	22·77

Table IV.

Showing the Difference in the Interval between the Apparent Solar Time of the Moon's Transit and the Time of High Water, and the Mean Interval, for every month in the year.

Moon's Transit.	January.	February.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.	Moon's Transit.
h m	m	m	m	m	m	m	m	m	m	m	m	m	h m
0 30	− 2·4	− 3·1	− 0·1	+ 1·2	− 1·1	− 2·0	− 0·7	+ 4·1	+ 3·5	+ 3·3	− 1·0	− 2·3	0 30
1 30	− 2·5	− 0·7	− 0·1	+ 0·6	− 0·3	− 0·9	+ 1·4	+ 1·8	+ 3·4	+ 2·8	− 2·2	− 3·5	1 30
2 30	+ 2·2	+ 0·7	− 1·0	− 2·0	− 3·0	+ 0·2	+ 4·1	+ 5·3	+ 2·1	− 1·5	− 4·3	− 3·5	2 30
3 30	+ 4·5	− 0·6	− 4·1	− 5·0	− 2·8	+ 2·0	+ 8·2	+ 4·0	+ 1·4	− 6·4	− 3·7	+ 2·5	3 30
4 30	+ 6·3	− 0·6	− 7·2	− 7·4	− 0·6	+ 8·7	+11·4	+ 4·2	− 5·2	−11·3	− 3·7	+ 5·0	4 30
5 30	+10·0	− 2·5	− 8·6	− 4·3	+ 2·1	+ 9·7	+12·0	+ 2·2	− 6·5	−12·3	− 5·1	+ 3·2	5 30
6 30	+ 0·9	− 1·9	− 0·2	+ 0·9	+ 1·7	+ 2·1	+ 4·5	+ 2·3	+ 0·6	− 4·7	− 5·1	− 2·3	6 30
7 30	+ 0·2	+ 0·4	+ 5·6	+ 1·8	− 2·6	− 3·4	− 1·6	+ 3·2	+ 7·1	+ 1·5	− 3·8	− 8·2	7 30
8 30	− 1·7	+ 2·9	+ 5·6	+ 0·2	− 3·9	− 4·9	− 1·0	+ 4·8	+ 6·0	+ 3·4	− 4·3	− 7·2	8 30
9 30	+ 0·7	− 0·1	0·0	− 0·8	− 1·3	− 2·6	− 0·3	+ 3·4	+ 4·7	+ 2·7	− 2·4	− 4·1	9 30
10 30	− 1·5	− 2·2	− 1·6	+ 0·1	+ 0·4	− 2·5	+ 1·4	+ 2·3	+ 5·8	+ 3·1	− 2·0	− 3·6	10 30
11 30	− 4·0	− 3·6	− 0·7	+ 0·1	− 0·3	− 0·1	− 0·6	+ 3·9	+ 4·3	+ 4·0	− 1·2	− 1·3	11 30

Table V.

Showing the Difference in the Height of High Water and the Mean Height, for every month in the year.

Moon's Transit.	January.	February.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.	Moon's Transit.
h m	feet.	feet.	feet.	feet.	feet	feet.	feet.	feet.	feet.	feet.	feet.	feet.	h m
0 30	− ·10	+ ·21	+ ·07	+ ·25	+ ·06	− ·22	− ·12	+ ·02	+ ·22	+ ·13	− ·21	− ·28	0 30
1 30	− ·11	+ ·04	+ ·08	− ·21	− ·17	− ·35	− ·16	− ·02	− ·04	− ·14	− ·41	− ·54	1 30
2 30	− ·01	+ ·14	− ·08	− ·06	− ·22	·00	+ ·26	+ ·16	+ ·13	− ·14	− ·07	− ·07	2 30
3 30	+ ·25	+ ·20	− ·19	− ·21	− ·21	− ·01	+ ·38	+ ·18	− ·09	− ·26	− ·07	− ·02	3 30
4 30	+ ·28	+ ·03	− ·39	− ·69	− ·08	+ ·43	+ ·50	+ ·31	− ·31	− ·24	− ·21	+ ·25	4 30
5 30	+ ·02	− ·35	− ·52	− ·41	+ ·20	+ ·56	+ ·60	− ·02	− ·34	− ·36	+ ·02	+ ·50	5 30
6 30	+ ·15	− ·36	− ·44	− ·11	+ ·34	+ ·54	+ ·42	− ·16	− ·46	− ·36	+ ·04	+ ·47	6 30
7 30	− ·03	− ·22	− ·29	− ·05	+ ·42	+ ·36	+ ·08	− ·25	− ·30	− ·09	+ ·18	+ ·25	7 30
8 30	− ·01	− ·26	− ·19	+ ·18	+ ·30	+ ·06	− ·13	− ·23	− ·14	+ ·16	+ ·02	+ ·16	8 30
9 30	− ·24	− ·19	− ·08	+ ·34	+ ·28	− ·01	− ·26	− ·24	− ·02	+ ·26	+ ·21	− ·05	9 30
10 30	− ·25	− ·03	·00	+ ·40	+ ·24	− ·15	− ·26	− ·21	+ ·08	+ ·31	+ ·05	− ·18	10 30
11 30	− ·19	+ ·09	+ ·19	+ ·41	+ ·08	− ·24	− ·25	− ·05	+ ·17	+ ·19	+ ·03	− ·47	11 30

Table VI.

Showing the Interval between the Apparent Solar Time of the Moon's Transit and the Time of High Water, the Height of High Water, and the Interval between the Moon's Transits at the London Docks, corresponding to the Apparent Solar Time of the Moon's Transit, for every minute of her Horizontal Parallax.

Hor. Par. 54′.

Number of Observations.	Moon's Transit.	Interval.	Height of Tide.	Interval of Transits.	Moon's Declination.
	h m	h m	ft. in.	h m	°
232	0 29·7	3 4·7	22 2·4	1 30·3	15·2
225	1 29·4	2 46·5	21 9·4	1 30·2	14·8
221	2 29·2	2 28·9	21 2·9	1 30·9	15·7
202	3 28·8	2 12·4	20 5·7	1 30·2	14·6
190	4 29·9	2 9·5	19 6·0	1 31·2	15·6
159	5 28·4	2 21·6	18 8·6	1 31·0	16·1
169	6 29·4	3 3·9	18 9·5	1 30·9	15·4
176	7 31·1	3 40·4	19 8·0	1 30·3	15·4
194	8 30·3	3 55·9	20 4·5	1 30·1	15·0
217	9 29·7	3 52·6	21 4·4	1 30·4	15·2
222	10 29·9	3 39·7	21 10·8	1 30·1	14·8
223	11 29·2	3 23·2	22 2·6	1 30·1	14·4

Sun's Declination 15°.

Hor. Par. 55′.

Number of Observations.	Moon's Transit.	Interval.	Height of Tide.	Interval of Transits.	Moon's Declination.
	h m	h m	ft. in.	h m	°
149	0 29·3	3 5·0	22 3·8	1 33·2	14·4
155	1 28·7	2 48·0	22 1·3	1 33·4	14·7
178	2 29·4	2 32·0	21 6·2	1 32·5	13·7
181	3 29·5	2 17·7	20 7·7	1 33·0	15·1
214	4 29·9	2 12·4	19 7·7	1 33·4	15·1
215	5 29·9	2 27·3	18 11·3	1 32·9	14·8
223	6 28·9	3 4·3	18 10·9	1 33·2	15·4
206	7 28·8	3 40·7	19 8·6	1 33·1	15·0
189	8 29·3	3 53·8	20 8·8	1 32·8	14·6
159	9 28·9	3 52·4	21 6·3	1 32·6	14·8
163	10 29·0	3 40·1	21 11·3	1 32·8	14·5
158	11 29·4	3 23·2	22 3·3	1 33·1	14·4

Sun's Declination 15°.

Hor. Par. 56′.

Number of Observations.	Moon's Transit.	Interval.	Height of Tide.	Interval of Transits.	Moon's Declination.
123	0 29·1	3 4·5	22 6·7	1 36·4	14·5
107	1 29·8	2 47·3	22 3·0	1 36·1	14·0
130	2 29·6	2 33·5	21 8·9	1 36·7	14·9
125	3 30·4	2 23·2	20 10·8	1 36·0	14·1
141	4 28·9	2 19·8	20 0·1	1 36·5	14·9
153	5 30·6	2 32·0	19 3·3	1 36·9	15·1
150	6 30·0	3 7·3	19 3·1	1 36·2	14·6
143	7 29·3	3 39·7	20 5·0	1 36·9	15·2
146	8 28·3	3 54·0	20 10·9	1 37·4	15·2
128	9 28·8	3 51·2	21 8·1	1 37·7	15·4
117	10 28·2	3 38·4	22 3·8	1 37·7	14·3
113	11 29·2	3 24·1	22 6·4	1 36·9	14·6

Sun's Declination 15 .

Hor. Par. 57′.

Number of Observations.	Moon's Transit.	Interval.	Height of Tide.	Interval of Transits.	Moon's Declination.
88	0 29·1	3 6·1	22 9·9	1 40·0	14·7
105	1 30·8	2 50 1	22 5·1	1 40·3	14·9
96	2 31·1	2 33·2	21 11·8	1 39·2	14·2
113	3 29·9	2 25·9	21 3·0	1 40·3	14·9
128	4 30·8	2 22·2	20 4·3	1 40·6	15·4
135	5 30·4	2 35·9	19 7·1	1 40·4	15·8
143	6 29·2	3 7·3	19 5·5	1 40·4	15·1
135	7 29·0	3 40·3	20 3·0	1 41·2	15·4
116	8 30·6	3 51·6	21 1·8	1 41·0	15·2
108	9 29·4	3 50·0	21 10·4	1 40·6	14·7
100	10 27·8	3 38·4	22 3·1	1 40·9	14·8
98	11 28·9	3 20·2	22 10·1	1 40·3	14·1

Sun's Declination 15°.

Hor. Par. 58′.

Number of Observations.	Moon's Transit.	Interval.	Height of Tide.	Interval of Transits.	Moon's Declination.
87	0 31·0	3 5·5	22 10·9	1 44·3	15·0
93	1 28·5	2 48·3	22 8·7	1 44·1	14·7
107	2 30·1	2 38·1	22 1·9	1 43·2	14·3
114	3 31·0	2 30·1	21 5·3	1 43·6	15·0
140	4 29·5	2 27·8	20 6·2	1 43·0	14·3
166	5 29·8	2 40·3	19 11·2	1 44·6	15·2
166	6 29·3	3 10·1	19 7·5	1 44·9	15·6
141	7 28·1	3 37·9	20 6·3	1 45·1	14·6
123	8 27·2	3 52·8	21 3·4	1 45·6	15·6
103	9 28·4	3 47·8	22 1·3	1 45·4	15·0
92	10 30·1	3 36·9	22 7·1	1 44·4	14·8
90	11 29·3	3 22·5	22 10·7	1 44·8	14·7

Sun's Declination 15°.

Hor. Par. 59′.

Number of Observations.	Moon's Transit.	Interval.	Height of Tide.	Interval of Transits.	Moon's Declination.
96	0 28·2	3 7·7	23 1·0	1 47·9	14·4
97	1 30·6	2 51·1	22 10·2	1 46·6	14·0
109	2 30·4	2 38·0	22 4·2	1 47·1	14·6
150	3 31·0	2 32·2	21 8·4	1 47·3	14·8
217	4 32·0	2 32·2	20 9·5	1 48·0	15·4
283	5 29·7	2 42·7	20 2·0	1 48·4	15·3
281	6 29·9	3 13·6	20 1·8	1 49·2	15·3
224	7 27·2	3 37·8	20 9·0	1 49·4	15·0
147	8 26·9	3 47·6	21 7·2	1 49·1	14·4
121	9 27·0	3 46·9	22 2·6	1 50·0	15·2
97	10 29·2	3 36·4	22 10·4	1 48·6	14·3
90	11 27·7	3 24·9	22 11·4	1 48·7	14·7

Sun's Declination 15°.

Hor. Par. 60′.

Number of Observations.	Moon's Transit.	Interval.	Height of Tide.	Interval of Transits.	Moon's Declination.
112	0 29·2	3 6·5	23 4·6	1 52·3	14·8
112	1 30·7	2 53·6	23 1·4	1 51·1	14·8
170	2 31·8	2 41·3	22 7·0	1 50·5	14·3
211	3 28·4	2 33·7	22 0·7	1 50·0	15·0
101	4 23·0	2 34·5	21 2·7	1 48·8	15·4
7	5 7·3	2 46·7	20 8·6	1 46·4	14·5
4	6 42·5	3 31·7	20 3·2	1 55·2	16·0
90	7 37·4	3 39·1	21 1·9	1 54·7	15·1
205	8 32·2	3 47·8	21 9·3	1 54·5	15·0
171	9 26·8	3 44·7	22 6·4	1 52·9	13·9
123	10 28·3	3 35·2	23 0·0	1 53·3	14·6
107	11 29·3	3 21·1	23 3·5	1 51·8	13·9

Sun's Declination 15°

Hor. Par. 61′.

Number of Observations.	Moon's Transit.	Interval.	Height of Tide.	Interval of Transits.	Moon's Declination.
214	0 29·7	3 8·0	23 6·1	1 55·8	14·5
196	1 28·1	2 53·6	23 3·2	1 55·1	14·8
112	2 22·7	2 43·0	22 9·0	1 53·1	15·3
5	3 8·0	2 34·4	21 10·6	1 55·6	18·6
2	8 57·0	3 49·5	22 5·0	1 60·0	16·5
98	9 38·1	3 43·4	22 9·8	1 58·2	15·5
184	10 30·4	3 34·8	23 1·8	1 57·4	14·4
217	11 29·4	3 20·9	23 4·9	1 57·1	14·6

Sun's Declination 15°.

Table VII. (Interpolated from Table VI.)

	H. P. 54′.		H. P. 55′.		H. P. 56′.		H. P. 57′.		H. P. 58′.		H. P. 59′.		H. P. 60′.		H. P. 61′.	
Moon's Transit.	Interval.	Height of Tide.	Interval.	Height of Tide.	Interval.	Height of Tide.	Interval.	Height of Tide.	Interval.	Height of Tide.	Interval.	Height of Tide.	Interval.	Height of Tide.	Interval.	Height of Tide.
h m	h m	ft.	h m	ft.	h m	ft.	h m	ft.	h m	ft.	h m	ft.	h m	ft.	h m	ft.
0 30	3 4·6	22·20	3 4·8	22·31	3 4·2	22·55	3 5·8	22·82	3 5·8	22·91	3 7·2	23·08	3 6·3	23·38	3 7·9	23·51
1 30	2 46·3	21·78	2 47·6	22·02	2 47·2	22·25	2 50·3	22·43	2 48·0	22·71	2 51·2	22·85	2 53·7	23·12	2 53·2	23·26
2 30	2 28·7	21·23	2 31·8	21·51	2 33·4	21·74	2 33·4	21·99	2 38·1	22·16	2 38·1	22·35	2 41·6	22·60	2 41·8	22·68
3 30	2 12·2	20·46	2 17·6	20·63	2 23·2	20·90	2 25·9	21·25	2 30·2	21·45	2 32·3	21·72	2 33·6	22·04		
4 30	2 9·5	19·50	2 12·4	19·64	2 20·6	20·00	2 22·3	20·37	2 27·9	20·51	2 32·0	20·82	2 35·1	21·14		
5 30	2 22·3	18·71	2 27·4	18·94	2 31·8	19·28	2 35·8	19·60	2 40·4	19·93	2 42·8	20·17				
6 30	3 4·3	18·80	3 5·0	18·92	3 7·3	19·26	3 7·7	19·46	3 10·4	19·63	3 13·6	20·15				
7 30	3 39·9	19·65	3 41·2	19·73	3 40·0	20·43	3 40·7	20·26	3 38·6	20·55	3 38·6	20·78	3 37·0	21·07		
8 30	3 55·9	20·37	3 53·9	20·74	3 54·2	20·93	3 51·5	21·14	3 53·0	21·33	3 47·8	21·64	3 47·7	21·75		
9 30	3 52·6	21·37	3 52·3	21·53	3 51·0	21·68	3 49·9	21·87	3 47·6	22·13	3 46·6	22·25	3 44·4	22·57	3 43·9	22·72
10 30	3 39·7	21·90	3 39·9	21·95	3 38·0	22·33	3 37·8	22·28	3 36·9	22·59	3 36·3	22·87	3 34·9	23·01	3 34·9	23·15
11 30	3 22·9	22·22	3 23·0	22·27	3 23·8	22·54	3 19·8	22·85	3 22·3	22·89	3 24·4	22·95	3 20·9	23·29	3 20·7	23·41

Table VIII.

Showing the Difference in the Interval between the Time of the Moon's Transit and the Time of High Water, and the Interval corresponding to fifty-seven minutes of the Moon's Horizontal Parallax.

Moon's Transit.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
h m	m	m	m	m	m	m	m	m
0 30	− 1·2	− 1·0	− 1·6	0	0·0	+ 1·4	+ 0·5	+ 2·1
1 30	− 4·0	− 2·7	− 3·1	0	− 2·3	+ 0·9	+ 3·4	+ 2·9
2 30	− 4·7	− 1·6	0·0	0	+ 4·7	+ 4·7	+ 8·2	+ 8·4
3 30	−13·7	− 8·3	− 2·7	0	+ 4·3	+ 6·4	+ 7·7	
4 30	−12·8	− 9·9	− 1·7	0	+ 5·6	+ 9·7	+12·8	
5 30	−13·5	− 8·4	− 4·1	0	+ 4·6	+ 7·0		
6 30	− 3·4	− 2·7	− 0·4	0	+ 2·7	+ 5·9		
7 30	− 0·8	+ 0·5	− 0·7	0	− 2·1	− 2·1	− 3·7	
8 30	+ 4·4	+ 2·4	+ 2·7	0	+ 1·5	− 3·7	− 3·8	
9 30	+ 2·7	+ 2·4	+ 1·1	0	− 2·3	− 3·3	− 5·5	− 6·0
10 30	+ 1·9	+ 2·1	+ 0·2	0	− 0·9	− 1·5	− 2·9	− 2·9
11 30	+ 3·1	+ 3·2	+ 4·0	0	+ 2·5	+ 4·6	+ 1·1	+ 0·9

Table IX.

Showing the Difference between the Height of High Water and the Height corresponding to fifty-seven minutes of the Moon's Horizontal Parallax.

Moon's Transit.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
h m	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.
0 30	− ·62	− ·51	− ·27	0	+ ·09	+ ·26	+ ·56	+ ·69
1 30	− ·65	− ·41	− ·18	0	+ ·28	+ ·42	+ ·69	+ ·83
2 30	− ·76	− ·48	− ·25	0	+ ·17	+ ·36	+ ·61	+ ·69
3 30	− ·79	− ·62	− ·35	0	+ ·20	+ ·47	+ ·79	
4 30	− ·87	− ·73	− ·37	0	+ ·14	+ ·45	+ ·77	
5 30	− ·88	− ·66	− ·32	0	+ ·33	+ ·57		
6 30	− ·66	− ·54	− ·20	0	+ ·17	+ ·69		
7 30	− ·61	− ·53	+ ·17	0	+ ·29	+ ·52	+ ·81	
8 30	− ·77	− ·40	− ·21	0	+ ·19	+ ·50	+ ·61	
9 30	− ·50	− ·34	− ·19	0	+ ·26	+ ·38	+ ·70	+ ·85
10 30	− ·38	− ·33	+ ·05	0	+ ·31	+ ·59	+ ·73	+ ·87
11 30	− ·63	− ·58	− ·31	0	+ ·04	+ ·10	+ ·44	+ ·56

Table X.

Showing the Interval between the Apparent Solar Time of the Moon's Transit and the Time of High Water, the Height of High Water, and the Interval between the Moon's Transits, at the London Docks, corresponding to the Apparent Solar Time of the Moon's Transit for every three degrees of her Declination north and south.

1° 30′ N. to 1° 30′ South Declination.

Number of Observations.	Moon's Transit.	Interval.	Height of Tide.	Interval of Transits.	Hor. Par.	Sun's Decl.
	h m	h m	ft. in.	h m	′	°
48	0 31·2	3 9·9	22 11·2	1 34·2	57·2	4·4
42	1 27·8	2 58·1	22 7·5	1 33·1	57·1	9·1
51	2 26·7	2 41·0	22 1·7	1 33·3	57·3	14·1
45	3 27·6	2 31·3	21 6·8	1 31·7	57·0	18·3
47	4 26·4	2 31·0	20 6·7	1 31·1	56·6	21·0
43	5 30·5	2 44·3	19 11·7	1 30·5	56·6	22·8
41	6 28·3	3 7·0	19 11·5	1 31·4	56·6	22·8
44	7 25·0	3 31·7	20 4·1	1 31·8	56·6	21·5
47	8 25·8	3 50·1	21 6·3	1 33·3	57·0	19·3
49	9 27·9	3 47·5	22 3·2	1 34·5	57·3	14·2
49	10 26·7	3 39·4	22 9·5	1 35·1	57·3	9·1
52	11 29·2	3 26·2	23 2·2	1 35·0	57·2	4·5

1° 30′ to 4° 30′ North Declination.

Number of Observations.	Moon's Transit.	Interval.	Height of Tide.	Interval of Transits.	Hor. Par.	Sun's Decl.
	h m	h m	ft. in.	h m	′	°
48	0 29·1	3 8·7	23 0·8	1 34·5	57·2	4·6
47	1 25·7	2 52·1	22 11·3	1 34·8	57·5	8·9
56	2 28·8	2 37·3	21 11·7	1 31·9	56·8	14·1
49	3 30·7	2 31·3	21 5·4	1 31·7	56·8	18·6
41	4 30·2	2 29·1	20 7·5	1 31·0	56·8	20·9
42	5 30·5	2 40·7	19 10·7	1 32·0	56·9	22·5
47	6 30·9	3 10·0	20 0·7	1 31·7	56·6	22·5
50	7 33·7	3 36·3	20 8·6	1 31·3	56·6	21·0
50	8 34·0	3 49·9	21 6·5	1 34·2	57·1	17·8
44	9 34·0	3 50·1	22 6·5	1 33·9	56·9	14·2
49	10 30·4	3 38·8	22 9·3	1 34·8	57·1	9·2
50	11 26·7	3 25·8	23 1·4	1 36·5	57·6	5·0

1° 30′ to 4° 30′ South Declination.

Number of Observations.	Moon's Transit.	Interval.	Height of Tide.	Interval of Transits.	Hor. Par.	Sun's Decl.
44	0 28·7	3 8·0	23 0·0	1 35·9	57·4	4·7
54	1 30·7	2 50·7	22 9·1	1 34·4	57·3	9·0
43	2 29·5	2 38·1	22 1·9	1 32·9	57·0	14·2
49	3 30·2	2 30·2	21 5·4	1 31·9	56·9	18·7
47	4 33·0	2 28·8	20 10·1	1 31·9	56·8	21·3
46	5 29·9	2 44·7	19 10·7	1 30·7	56·6	22·6
48	6 29·8	3 10·1	20 0·8	1 32·6	56·9	22·6
45	7 31·0	3 37·7	20 7·5	1 32·0	56·8	21·8
45	8 31·9	3 45·3	21 4·3	1 34·7	57·0	18·3
46	9 29·7	3 46·8	22 4·4	1 35·4	57·4	14·6
46	10 27·3	3 39·2	22 9·1	1 35·3	57·3	9·3
52	11 30·7	3 24·9	23 2·0	1 35·7	57·5	5·1

4° 30′ to 7° 30′ North Declination.

Number of Observations.	Moon's Transit.	Interval.	Height of Tide.	Interval of Transits.	Hor. Par.	Sun's Decl.
57	0 30·1	3 8·3	23 0·5	1 36·5	57·5	6·4
55	1 30·2	2 50·7	22 9·5	1 35·1	57·2	8·9
48	2 32·5	2 38·1	22 0·8	1 34·1	57·3	14·7
49	3 31·5	2 29·5	21 5·9	1 33·1	57·0	18·3
43	4 26·6	2 30·1	20 6·2	1 31·4	56·5	20·6
50	5 26·9	2 39·8	20 1·3	1 31·9	56·6	22·0
52	6 29·2	3 7·8	19 11·1	1 32·4	56·6	22·1
48	7 29·8	3 31·2	20 9·2	1 33·7	56·9	20·4
47	8 30·5	3 50·7	21 3·7	1 33·9	57·1	17·9
51	9 29·3	3 50·9	22 2·0	1 36·1	57·3	13·7
57	10 31·4	3 38·4	22 8·2	1 35·6	57·3	8·9
52	11 32·0	3 24·9	22 10·4	1 35·7	56·9	6·7

7° 30′ to 10° 30′ North Declination.

Number of Observations.	Moon's Transit.	Interval.	Height of Tide.	Interval of Transits.	Hor. Par.	Sun's Decl.
52	0 29·0	3 7·3	22 11·1	1 35·6	57·0	9·0
53	1 29·8	2 50·8	22 7·4	1 35·7	57·1	9·9
51	2 28·9	2 37·6	21 11·2	1 34·5	56·6	13·8
49	3 31·0	2 26·3	21 2·5	1 33·5	56·7	17·4
49	4 30·1	2 27·1	20 5·0	1 32·3	56·6	19·6
49	5 31·7	2 37·3	19 11·8	1 33·7	56·4	21·1
48	6 28·7	3 8·6	19 10·5	1 33·2	56·6	21·2
52	7 28·9	3 39·5	20 6·9	1 34·7	56·7	19·5
52	8 29·0	3 49·6	21 2·5	1 34·5	56·6	16·4
52	9 28·6	3 51·1	22 1·4	1 35·1	56·7	12·7
52	10 31·0	3 39·5	22 8·0	1 37·2	57·2	9·9
50	11 27·9	3 23·9	22 9·0	1 36·2	57·0	9·1

4° 30′ to 7° 30′ South Declination.

Number of Observations.	Moon's Transit.	Interval.	Height of Tide.	Interval of Transits.	Hor. Par.	Sun's Decl.
50	0 30·1	3 7·5	23 1·2	1 36·5	57·6	7·0
47	1 27·8	2 53·4	22 8·9	1 35·0	57·1	8·2
50	2 26·7	2 38·9	22 4·2	1 35·0	57·4	13·5
44	3 27·6	2 28·0	21 7·8	1 32·2	56·6	17·2
48	4 30·1	2 29·5	20 8·9	1 32·5	56·9	20·7
50	5 28·9	2 42·1	20 0·4	1 33·0	56·9	21·8
45	6 27·4	3 13·1	19 10·2	1 32·2	56·5	22·0
46	7 29·8	3 35·1	20 5·5	1 33·8	57·1	20·9
44	8 28·3	3 45·3	21 5·4	1 34·8	57·0	17·9
51	9 29·7	3 47·0	22 2·5	1 35·5	57·2	14·1
51	10 30·3	3 38·1	22 7·4	1 36·8	57·4	8·0
51	11 29·8	3 24·3	22 11·3	1 36·1	57·2	6·7

7° 30′ to 10° 30′ South Declination.

Number of Observations.	Moon's Transit.	Interval.	Height of Tide.	Interval of Transits.	Hor. Par.	Sun's Decl.
54	0 29·6	3 7·7	22 10·6	1 37·5	57·6	9·0
52	1 29·8	2 49·9	22 8·5	1 36·8	57·4	9·9
52	2 29·8	2 39·2	22 1·1	1 36·2	57·4	13·0
48	3 28·7	2 27·7	21 6·1	1 35·7	57·2	15·6
53	4 31·6	2 31·5	20 5·9	1 33·8	56·8	19·6
50	5 31·1	2 42·7	20 5·0	1 34·7	57·0	21·0
50	6 30·9	3 12·7	19 8·9	1 33·4	56·7	21·1
45	7 29·9	3 40·9	20 8·0	1 36·7	57·2	19·7
51	8 28·4	3 44·6	21 6·3	1 34·7	56·8	15·8
51	9 28·1	3 46·6	22 2·8	1 37·9	57·4	14·0
53	10 28·6	3 38·6	22 7·3	1 38·2	57·5	9·3
55	11 28·7	3 15·5	22 11·8	1 39·2	57·6	9·5

Table X. (Continued.)

10° 30′ to 13° 30′ North Declination.

Number of Observations.	Moon's Transit.	Interval.	Height of Tide.	Interval of Transits.	Hor. Par.	Sun's Decl.
	h m	h m	ft. in.	h m	′	°
60	0 28·4	3 6·1	22 11·0	1 38·3	57·2	11·6
59	1 29·8	2 52·5	22 6·8	1 37·7	57·3	12·0
57	2 28·9	2 38·5	22 0·3	1 36·6	56·8	11·7
59	3 28·3	2 27·1	21 3·1	1 35·6	56·8	14·9
55	4 29·4	2 23·7	20 4·4	1 35·3	56·9	17·7
55	5 30·1	2 37·5	19 8·2	1 32·8	56·4	19·5
55	6 28·9	3 10·0	19 7·9	1 34·2	56·5	19·1
52	7 28·4	3 36·4	20 4·4	1 36·8	56·7	18·2
56	8 29·7	3 47·7	21 2·2	1 35·9	56·6	15·8
58	9 30·3	3 49·5	22 1·6	1 38·3	57·1	13·0
52	10 29·5	3 38·0	22 6·1	1 38·2	57·0	11·3
59	11 29·6	3 23·0	23 0·0	1 38·1	56·9	12·0

13° 30′ to 16° 30′ North Declination.

Number of Observations.	Moon's Transit.	Interval.	Height of Tide.	Interval of Transits.	Hor. Par.	Sun's Decl.
	h m	h m	ft. in.	h m	′	°
60	0 31·2	3 6·2	22 9·8	1 39·3	56·9	15·5
64	1 29·7	2 49·9	22 6·1	1 39·4	56·9	14·2
59	2 28·4	2 35·7	22 0·5	1 38·0	56·7	13·3
62	3 27·4	2 27·6	21 0·8	1 38·7	56·6	12·8
63	4 31·2	2 21·8	20 2·7	1 36·9	56·4	16·0
66	5 28·9	2 38·0	19 6·8	1 37·9	56·6	16·7
62	6 28·2	3 3·9	19 4·5	1 37·2	56·2	16·9
65	7 28·5	3 39·9	20 2·1	1 37·1	56·5	15·4
72	8 29·3	3 52·9	21 2·3	1 39·0	56·6	13·5
61	9 30·3	3 48·5	22 0·5	1 38·5	56·6	13·0
71	10 28·3	3 38·6	22 4·9	1 39·4	56·7	14·1
67	11 29·5	3 25·4	22 9·0	1 40·4	57·0	16·0

10° 30′ to 13° 30′ South Declination.

Number of Observations.	Moon's Transit.	Interval.	Height of Tide.	Interval of Transits.	Hor. Par.	Sun's Decl.
57	0 27·9	3 8·1	23 0·6	1 40·9	57·6	12·1
61	1 28·8	2 53·1	22 7·7	1 39·7	57·8	11·8
59	2 30·6	2 36·8	22 2·6	1 38·4	57·4	13·2
63	3 29·4	2 26·8	21 5·4	1 37·1	57·0	14·5
53	4 30·1	2 24·7	20 4·4	1 36·8	57·0	17·5
52	5 29·0	2 42·8	19 10·2	1 37·2	57·0	19·4
52	6 27·4	3 10·0	19 9·2	1 37·0	57·0	19·8
57	7 28·6	3 38·5	20 4·0	1 37·7	57·1	18·0
59	8 29·7	3 50·1	21 5·6	1 38·2	57·2	15·3
55	9 32·8	3 49·3	22 0·2	1 40·4	57·5	12·1
54	10 28·9	3 37·7	22 8·3	1 40·2	57·5	12·3
54	11 29·3	3 20·8	22 6·3	1 40·7	57·7	11·9

13° 30′ to 16° 30′ South Declination.

Number of Observations.	Moon's Transit.	Interval.	Height of Tide.	Interval of Transits.	Hor. Par.	Sun's Decl.
58	0 28·8	3 8·4	22 11·2	1 42·5	57·6	15·0
63	1 27·9	2 53·4	22 8·3	1 43·3	58·0	13·7
63	2 32·6	2 38·1	22 0·2	1 41·5	57·7	13·4
61	3 31·5	2 29·6	21 3·8	1 41·1	57·4	13·2
71	4 30·7	2 26·2	20 5·9	1 37·1	57·1	14·7
66	5 30·3	2 38·7	19 8·0	1 39·5	56·9	17·0
64	6 31·4	3 10·3	19 7·7	1 38·6	57·0	16·7
59	7 29·2	3 37·5	20 6·2	1 40·9	57·1	15·4
60	8 29·4	3 51·0	21 2·9	1 42·5	57·5	14·2
59	9 29·4	3 45·5	22 1·0	1 41·6	57·6	13·4
64	10 28·9	3 37·4	22 6·5	1 41·8	57·5	13·8
68	11 29·3	3 23·6	22 10·6	1 43·3	57·9	15·2

16° 30′ to 19° 30′ North Declination.

Number of Observations.	Moon's Transit.	Interval.	Height of Tide.	Interval of Transits.	Hor. Par.	Sun's Decl.
92	0 29·5	3 5·1	22 8·4	1 42·2	56·8	18·9
92	1 30·4	2 50·4	22 1·5	1 40·5	56·5	17·4
95	2 30·1	2 34·6	21 10·0	1 40·1	56·5	15·2
94	3 29·4	2 23·7	21 0·2	1 40·6	56·5	13·3
94	4 28·9	2 19·1	20 0·8	1 40·2	56·4	11·6
92	5 29·0	2 34·8	19 5·6	1 40·1	56·3	11·6
105	6 30·2	3 10·8	19 4·8	1 40·2	56·4	11·0
93	7 29·6	3 44·1	20 0·3	1 40·8	56·4	12·6
101	8 27·7	3 53·8	21 0·4	1 41·2	56·4	13·6
84	9 29·9	3 51·0	21 9·5	1 42·0	56·6	15·4
88	10 29·4	3 38·5	22 6·3	1 42·6	57·2	18·5
87	11 29·0	3 22·3	22 7·1	1 42·1	56·8	18·9

19° 30′ to 22° 30′ North Declination.

Number of Observations.	Moon's Transit.	Interval.	Height of Tide.	Interval of Transits.	Hor. Par.	Sun's Decl.
64	0 28·0	3 5·2	22 7·6	1 47·7	57·5	19·0
64	1 27·7	2 48·3	22 7·5	1 47·5	57·6	18·6
71	2 28·9	2 31·3	21 11·1	1 46·0	57·3	15·6
65	3 31·9	2 21·4	21 0·2	1 46·4	57·0	11·8
76	4 28·7	2 16·4	19 9·4	1 44·8	56·8	11·8
74	5 27·6	2 28·7	19 2·3	1 46·0	57·0	10·7
70	6 28·9	3 11·3	19 4·6	1 46·7	57·1	11·7
71	7 29·5	3 42·8	20 0·8	1 47·7	57·2	10·9
64	8 29·9	3 52·1	21 0·7	1 49·2	57·6	13·0
68	9 28·7	3 47·9	21 10·3	1 48·7	57·7	15·2
66	10 30·3	3 35·3	22 4·5	1 49·0	58·0	18·3
65	11 28·8	3 22·6	22 7·6	1 48·8	57·7	19·9

16° 30′ to 19° 30′ South Declination.

Number of Observations.	Moon's Transit.	Interval.	Height of Tide.	Interval of Transits.	Hor. Par.	Sun's Decl.
92	0 30·1	3 3·8	22 10·7	1 46·2	57·8	19·0
80	1 28·2	2 51·7	22 5·8	1 45·5	57·7	18·3
95	2 28·0	2 37·3	21 11·9	1 44·4	57·6	16·2
86	3 29·2	2 28·7	21 4·1	1 45·5	57·7	13·1
94	4 30·4	2 23·9	20 3·1	1 43·8	57·3	12·0
88	5 30·2	2 37·3	19 4·1	1 44·5	57·2	11·4
93	6 29·7	3 9·8	19 5·3	1 44·3	57·1	11·6
99	7 30·7	3 40·8	20 4·5	1 45·3	57·3	12·0
89	8 30·9	3 54·1	21 1·7	1 46·1	57·5	13·5
84	9 27·2	3 48·7	21 10·5	1 46·2	57·6	14·6
91	10 31·2	3 36·8	22 5·8	1 47·5	57·9	17·7
75	11 28·7	3 16·9	22 9·8	1 47·4	58·1	19·3

19° 30′ to 22° 30′ South Declination.

Number of Observations.	Moon's Transit.	Interval.	Height of Tide.	Interval of Transits.	Hor. Par.	Sun's Decl.
67	0 29·8	3 5·9	22 7·7	1 45·4	57·1	19·9
65	1 30·7	2 46·5	22 4·6	1 46·7	57·5	18·2
69	2 30·0	2 31·8	21 9·6	1 43·8	56·9	15·0
68	3 29·4	2 22·9	20 10·6	1 43·0	56·8	13·9
79	4 30·0	2 16·3	19 11·9	1 42·6	56·5	11·5
68	5 30·7	2 28·1	19 3·4	1 42·4	56·6	11·2
75	6 28·6	3 6·9	19 0·1	1 41·8	56·2	10·5
71	7 28·6	3 42·5	19 10·1	1 41·9	56·3	12·1
77	8 30·6	3 53·8	20 10·9	1 43·2	56·4	12·9
68	9 30·6	3 50·2	21 7·8	1 43·9	56·7	15·6
65	10 27·1	3 36·4	22 2·1	1 41·9	56·6	17·8
72	11 29·8	3 21·1	22 6·7	1 45·7	57·1	19·9

Table X. (Continued.)

Number of Observations.	22° 30′ to 25° 30′ North Declination. Moon's Transit.	Interval.	Height of Tide.	Interval of Transits.	Hor. Par.	Sun's Decl.	Number of Observations.	Above 25° 30′ North Declination. Moon's Transit.	Interval.	Height of Tide.	Interval of Transits.	Hor. Par.	Sun's Decl.
	h m	h m	ft. in.	h m	′	°		h m	h m	ft. in.	h m	′	°
55	0 29·8	3 2·9	22 5·8	1 53·2	58·1	21·0	41	0 30·7	3 1·7	22 6·6	1 49·9	56·9	22·3
54	1 29·8	2 46·4	22 4·7	1 51·6	57·8	19·3	42	1 29·7	2 48·4	21 9·7	1 49·6	57·1	21·0
53	2 31·4	2 33·7	21 8·9	1 50·2	57·5	16·4	49	2 31·2	2 27·4	21 5·6	1 48·8	56·6	17·9
49	3 27·8	2 21·5	21 0·0	1 49·9	57·5	13·1	47	3 29·8	2 13·1	20 6·9	1 49·9	56·8	13·8
56	4 29·2	2 13·8	19 10·0	1 49·8	57·1	10·3	52	4 28·3	2 8·5	19 3·8	1 48·5	56·5	9·2
52	5 31·3	2 25·3	19 4·0	1 49·4	57·1	10·3	59	5 28·9	2 18·4	18 6·3	1 47·9	56·4	6·6
57	6 30·9	3 7·6	18 11·3	1 50·6	57·3	8·9	55	6 29·2	3 4·1	18 7·2	1 48·6	56·3	6·6
53	7 30·3	3 42·0	20 0·2	1 51·5	57·3	10·9	54	7 29·2	3 43·9	19 6·3	1 48·9	56·3	8·5
53	8 27·0	3 52·1	20 10·5	1 52·4	57·7	13·1	47	8 27·6	3 56·8	20 8·0	1 49·5	56·3	13·6
55	9 27·0	3 49·5	21 8·0	1 52·4	57·6	16·2	54	9 27·4	3 52·4	21 4·1	1 49·0	56·7	17·7
49	10 29·5	3 34·7	22 4·6	1 54·5	58·2	19·2	43	10 27·9	3 37·0	21 10·6	1 50·5	56·9	20·9
48	11 29·5	3 18·2	22 8·0	1 52·5	57·7	20·8	47	11 29·0	3 20·0	22 3·3	1 50·0	56·7	22·2
	22° 30′ to 25° 30′ South Declination.							Above 25° 30′ South Declination.					
56	0 29·4	3 4·1	22 6·5	1 46·8	56·9	20·9	40	0 29·8	2 57·9	22 6·4	1 53·9	57·3	22·3
55	1 31·8	2 41·7	22 3·2	1 45·3	56·5	19·3	44	1 29·1	2 44·8	22 0·2	1 54·0	57·7	21·0
53	2 30·1	2 30·6	21 7·7	1 44·9	56·6	16·4	47	2 30·5	2 27·0	21 5·9	1 52·3	57·3	17·7
53	3 31·7	2 17·3	20 10·9	1 44·9	56·6	13·0	49	3 27·1	2 14·4	20 8·4	1 52·4	57·3	14·0
56	4 29·8	2 10·0	19 7·8	1 43·6	56·2	10·8	53	4 29·0	2 6·5	19 9·4	1 52·7	57·2	9·2
60	5 30·7	2 27·1	19 1·9	1 44·3	56·6	9·3	53	5 25·9	2 14·9	18 8·7	1 51·7	56·8	6·3
56	6 31·1	3 7·0	19 1·3	1 43·7	56·2	9·9	57	6 28·9	3 0·6	18 9·9	1 52·9	57·0	6·7
56	7 29·9	3 45·0	19 10·8	1 44·3	56·2	10·5	51	7 28·7	3 39·5	19 7·5	1 53·7	57·1	9·0
59	8 27·3	3 56·1	20 7·2	1 45·1	56·3	12·9	47	8 27·9	3 56·4	20 8·7	1 55·9	57·6	13·9
54	9 29·7	3 47·5	21 5·5	1 44·7	56·3	16·2	49	9 28·8	3 49·8	21 6·4	1 55·4	57·7	18·2
51	10 27·6	3 35·0	21 11·4	1 45·2	56·3	19·4	44	10 31·0	3 35·9	22 2·0	1 55·5	57·9	20·5
51	11 27·4	3 20·5	22 0·7	1 45·2	56·5	21·1	43	11 29·0	3 20·1	22 5·0	1 55·6	57·8	22·3

Table XI. (Interpolated from Table X.)

Showing the Interval between the Apparent Solar Time of the Moon's Transit and the Time of High Water at the London Docks for every three degrees of her Declination north *and* south.

Moon's Transit.	0° Decl.	3° N. Decl.	6° N. Decl.	9° N. Decl.	12° N. Decl.	15° N. Decl.	18° N. Decl.	21° N. Decl.	24° N. Decl.	27° N. Decl.	Mean.
h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m
0 30	3 10·1	3 8·5	3 8·3	3 7·0	3 5·7	3 6·6	3 5·0	3 4·6	3 2·8	3 1·9	3 6·1
1 30	2 57·6	2 51·0	2 50·7	2 50·8	2 52·4	2 49·8	2 50·5	2 47·7	2 46·4	2 48·3	2 50·5
2 30	2 40·3	2 37·1	2 38·5	2 37·4	2 38·3	2 35·4	2 34·6	2 31·1	2 34·0	2 27·7	2 35·4
3 30	2 31·1	2 31·3	2 29·6	2 26·2	2 26·9	2 27·3	2 23·6	2 21·6	2 21·1	2 13·1	2 25·2
4 30	2 31·4	2 29·1	2 30·4	2 27·1	2 23·7	2 21·9	2 19·2	2 16·5	2 13·8	2 8·6	2 22·2
5 30	2 44·1	2 40·5	2 40·8	2 36·7	2 37·5	2 38·4	2 35·2	2 29·8	2 24·7	2 18·9	2 34·7
6 30	3 7·7	3 9·6	3 8·1	3 7·9	3 10·5	3 4·8	3 10·7	3 12·0	3 7·0	3 4·6	3 8·3
7 30	3 33·5	3 35·1	3 31·3	3 39·1	3 36·9	3 40·5	3 44·2	3 43·0	3 41·9	3 44·3	3 39·0
8 30	3 50·7	3 49·5	3 50·6	3 49·7	3 47·7	3 52·9	3 53·9	3 52·1	3 52·3	3 57·0	3 51·6
9 30	3 47·3	3 50·5	3 50·8	3 51·0	3 49·5	3 48·5	3 51·0	3 47·7	3 49·1	3 52·0	3 49·7
10 30	3 38·8	3 38·9	3 38·7	3 39·7	3 37·9	3 38·3	3 38·4	3 35·4	3 34·6	3 36·4	3 37·7
11 30	3 26·0	3 25·0	3 25·4	3 23·3	3 22·9	3 25·3	3 22·0	3 22·3	3 18·1	3 19·7	3 23·0
		3° S. Decl.	6° S. Decl.	9° S. Decl.	12° S. Decl.	15° S. Decl.	18° S. Decl.	21° S. Decl.	24° S. Decl.	27° S. Decl.	Mean.
0 30		3 7·6	3 7·5	3 7·6	3 7·5	3 8·0	3 3·8	3 5·9	3 2·9	2 57·9	3 6·5
1 30		2 50·9	2 49·8	2 49·9	2 52·8	2 52·9	2 51·3	2 46·7	2 42·1	2 44·5	2 49·9
2 30		2 38·0	2 38·3	2 39·2	2 36·9	2 38·6	2 36·9	2 31·8	2 30·6	2 27·1	2 35·8
3 30		2 30·2	2 27·8	2 27·6	2 26·8	2 29·8	2 28·6	2 22·9	2 17·6	2 14·0	2 25·6
4 30		2 28·6	2 29·5	2 31·3	2 24·7	2 26·3	2 23·9	2 16·3	2 10·0	2 6·5	2 22·9
5 30		2 44·7	2 42·4	2 42·3	2 43·2	2 38·6	2 37·2	2 27·8	2 26·8	2 16·8	2 36·4
6 30		3 10·2	3 14·2	3 12·3	3 12·2	3 9·6	3 9·9	3 7·8	3 6·3	3 1·3	3 9·2
7 30		3 37·4	3 35·2	3 40·9	3 39·0	3 37·8	3 40·5	3 43·0	3 45·0	3 40·1	3 39·2
8 30		3 45·1	3 45·6	3 44·7	3 50·1	3 51·0	3 54·0	3 53·8	3 56·3	3 56·5	3 50·8
9 30		3 46·8	3 47·0	3 46·5	3 49·6	3 45·4	3 48·6	3 50·3	3 47·5	3 49·6	3 47·9
10 30		3 38·7	3 38·2	3 38·3	3 37·4	3 37·2	3 37·1	3 35·7	3 34·4	3 36·2	3 37·2
11 30		3 25·1	3 24·3	3 15·1	3 20·6	3 23·4	3 16·5	3 21·0	3 19·8	3 19·8	3 21·2

Table XII.

Showing the Interval between the Apparent Time of the Moon's Transit and the Time of High Water at the London Docks, for every three degrees of her Declination north *or* south.

Moon's Transit.	0° Decl.	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.	Mean.
h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m
0 30	3 10·1	3 8·0	3 7·9	3 7·3	3 6·6	3 7·3	3 4·4	3 5·3	3 2·9	2 59·9	3 6·3
1 30	2 57·6	2 51·0	2 50·3	2 50·3	2 52·6	2 51·3	2 50·9	2 47·2	2 44·3	2 46·4	2 50·2
2 30	2 40·3	2 37·6	2 38·4	2 38·3	2 37·6	2 37·0	2 35·8	2 31·4	2 32·3	2 27·4	2 35·6
3 30	2 31·1	2 30·8	2 28·7	2 26·9	2 26·8	2 28·5	2 26·1	2 22·2	2 19·4	2 13·7	2 25·4
4 30	2 31·4	2 28·9	2 30·0	2 28·7	2 24·2	2 24·1	2 21·5	2 16·4	2 11·9	2 7·6	2 22·5
5 30	2 44·1	2 42·6	2 41·6	2 39·5	2 40·4	2 38·5	2 36·2	2 28·8	2 25·8	2 17·9	2 35·5
6 30	3 7·7	3 9·9	3 11·1	3 10·1	3 11·3	3 7·2	3 10·3	3 9·9	3 6·7	3 2·9	3 8·7
7 30	3 33·5	3 36·3	3 33·3	3 40·0	3 37·9	3 39·1	3 42·3	3 43·0	3 43·5	3 42·2	3 39·1
8 30	3 50·7	3 47·3	3 48·1	3 47·4	3 48·9	3 52·0	3 53·9	3 52·9	3 54·3	3 56·8	3 51·2
9 30	3 47·3	3 48·6	3 48·9	3 48·7	3 49·5	3 47·0	3 49·8	3 49·0	3 48·3	3 50·8	3 48·8
10 30	3 38·8	3 38·8	3 38·5	3 39·0	3 37·6	3 37·8	3 37·7	3 35·5	3 34·5	3 36·3	3 37·5
11 30	3 26·0	3 25·1	3 24·9	3 19·2	3 21·7	3 24·4	3 19·2	3 21·6	3 19·0	3 19·7	3 22·1

Table XIII.

Showing the Difference in the Interval between the Apparent Time of the Moon's Transit and the Time of High Water at the London Docks, and the Interval corresponding to fifteen degrees Declination, for every three degrees of the Moon's Declination north *and* south.

Moon's Transit.	0° Decl.	3° N. Decl.	6° N. Decl.	9° N. Decl.	12° N. Decl.	15° N. Decl.	18° N. Decl.	21° N. Decl.	24° N. Decl.	27° N. Decl.	Moon's Transit.
h m	m	m	m	m	m	m	m	m	m	m	h m
0 30	+ 3·5	+ 1·9	+ 1·7	+ 0·4	− 0·9	0	− 1·6	− 2·0	− 3·8	− 4·7	0 30
1 30	+ 7·8	+ 1·2	+ 0·9	+ 1·0	+ 2·6	0	+ 0·7	− 2·1	− 3·4	− 1·5	1 30
2 30	+ 4·9	+ 1·7	+ 3·1	+ 2·0	+ 2·9	0	− 0·8	− 4·3	− 1·4	− 7·7	2 30
3 30	+ 3·8	+ 4·0	+ 2·3	− 1·1	− 0·4	0	− 3·7	− 5·7	− 6·2	−14·2	3 30
4 30	+ 9·5	+ 7·2	+ 8·5	+ 5·2	+ 1·8	0	− 2·7	− 5·4	− 8·1	−13·3	4 30
5 30	+ 5·7	+ 2·1	+ 2·4	− 1·7	− 0·9	0	− 3·2	− 8·6	−13·7	−19·5	5 30
6 30	+ 2·9	+ 4·8	+ 3·3	+ 3·1	+ 5·7	0	+ 5·9	+ 7·2	+ 2·2	− 0·2	6 30
7 30	− 7·0	− 5·4	− 9·2	− 1·4	− 3·6	0	+ 3·7	+ 2·5	+ 1·4	+ 3·8	7 30
8 30	− 2·2	− 3·4	− 2·3	− 3·2	− 5·2	0	+ 1·0	− 0·8	− 0·6	+ 4·1	8 30
9 30	− 1·2	+ 2·0	+ 2·3	+ 2·5	+ 1·0	0	+ 2·5	− 0·8	+ 0·6	+ 3·5	9 30
10 30	+ 0·5	+ 0·6	+ 0·4	+ 1·4	− 0·4	0	+ 0·1	− 2·9	− 3·7	− 1·9	10 30
11 30	+ 0·7	− 0·3	+ 0·1	− 2·0	− 2·4	0	− 3·3	− 3·0	− 7·2	− 5·6	11 30
	0° Decl.	3° S. Decl.	6° S. Decl.	9° S. Decl.	12° S. Decl.	15° S. Decl.	18° S. Decl.	21° S. Decl.	24° S. Decl.	27° S. Decl.	
0 30	+ 2·1	− 0·4	− 0·5	− 0·4	− 0·5	0	− 4·2	− 2·1	− 5·1	−10·1	0 30
1 30	+ 4·7	− 2·0	− 3·1	− 3·0	− 0·1	0	− 1·6	− 6·2	−10·8	− 8·5	1 30
2 30	+ 1·7	− 0·6	− 0·3	+ 0·6	− 1·7	0	− 1·7	− 6·8	− 8·0	−11·5	2 30
3 30	+ 1·3	+ 0·4	− 2·0	− 2·2	− 3·0	0	− 1·2	− 6·9	−12·2	−15·8	3 30
4 30	+ 5·1	+ 2·3	+ 3·2	+ 5·0	− 1·6	0	− 2·4	−10·0	−16·3	−19·8	4 30
5 30	+ 5·5	+ 6·1	+ 3·8	+ 3·7	+ 4·6	0	− 1·4	−10·8	−11·8	−21·8	5 30
6 30	− 1·9	+ 0·6	+ 4·6	+ 2·7	+ 2·6	0	+ 0·3	− 1·8	− 3·3	− 8·3	6 30
7 30	− 4·3	− 0·4	− 2·6	+ 3·1	+ 1·2	0	+ 2·7	+ 5·2	+ 7·2	+ 2·3	7 30
8 30	− 0·3	− 5·9	− 5·4	− 6·3	− 0·9	0	+ 3·0	+ 2·8	+ 5·3	+ 5·5	8 30
9 30	+ 1·9	+ 1·4	+ 1·6	+ 1·1	+ 4·2	0	+ 3·2	+ 4·9	+ 2·1	+ 4·2	9 30
10 30	+ 1·6	+ 1·5	+ 1·0	+ 1·1	+ 0·2	0	− 0·1	− 1·5	− 2·8	− 1·0	10 30
11 30	+ 2·6	+ 1·7	+ 0·9	− 8·3	− 2·8	0	− 6·9	− 2·4	− 3·6	− 3·6	11 30

Table XIV.

Showing the Difference in the Interval between the Apparent Time of the Moon's Transit and the Time of High Water at the London Docks, and the Interval corresponding to fifteen degrees Declination, for every three degrees of the Moon's Declination north *or* south.

Moon's Transit.	0° Decl.	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.	Moon's Transit.
h m	m	m	m	m	m	m	m	m	m	m	h m
0 30	+ 2·8	+ 0·7	+ 0·6	0·0	− 0·7	0	− 2·9	− 2·0	− 4·4	− 7·4	0 30
1 30	+ 6·3	− 0·3	− 1·0	− 1·0	+ 1·3	0	− 0·4	− 4·1	− 7·0	− 4·9	1 30
2 30	+ 3·3	+ 0·6	+ 1·4	+ 1·3	+ 0·6	0	− 1·2	− 5·6	− 4·7	− 9·6	2 30
3 30	+ 2·6	+ 2·3	+ 0·2	− 1·6	− 1·7	0	− 2·4	− 6·3	− 9·1	−14·8	3 30
4 30	+ 7·3	+ 4·8	+ 5·9	+ 4·6	+ 0·1	0	− 2·6	− 7·7	−12·2	−16·5	4 30
5 30	+ 5·6	+ 4·1	+ 3·1	+ 1·0	+ 1·9	0	− 2·3	− 9·7	−12·7	−20·6	5 30
6 30	+ 0·5	+ 2·7	+ 3·9	+ 2·9	+ 4·1	0	+ 3·1	+ 2·7	− 0·5	− 4·3	6 30
7 30	− 5·6	− 2·8	− 5·8	+ 0·9	− 1·2	0	+ 3·2	+ 3·9	+ 4·4	+ 3·1	7 30
8 30	− 1·3	− 4·7	− 3·9	− 4·6	− 3·1	0	+ 1·9	+ 0·9	+ 2·3	+ 4·8	8 30
9 30	+ 0·3	+ 1·6	+ 1·9	+ 1·7	+ 2·5	0	+ 2·8	+ 2·0	+ 1·3	+ 3·8	9 30
10 30	+ 1·0	+ 1·0	+ 0·7	+ 1·2	− 0·2	0	− 0·1	− 2·3	− 3·3	− 1·5	10 30
11 30	+ 1·6	+ 0·7	+ 0·5	− 5·2	− 2·7	0	− 5·2	− 2·8	− 5·4	− 4·7	11 30

Table XV. (Interpolated from Table X.)

Showing the Height of High Water at the London Docks for every three degrees of the Moon's Declination north *and* south.

Moon's Transit.	0° Decl.	3° N. Decl.	6° N. Decl.	9° N. Decl.	12° N. Decl.	15° N. Decl.	18° N. Decl.	21° N. Decl.	24° N. Decl.	27° N. Decl.	Mean.
h m	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.
0 30	22·94	23·07	23·04	22·92	22·91	22·82	22·70	22·63	22·48	22·55	22·81
1 30	22·61	22·94	22·79	22·62	22·56	22·51	22·13	22·61	22·39	21·81	22·49
2 30	22·12	21·97	22·09	21·92	22·01	22·02	21·83	21·91	21·76	21·47	21·91
3 30	21·53	21·46	21·51	21·22	21·23	21·03	21·01	21·03	20·96	20·57	21·15
4 30	20·51	20·62	20·47	20·42	20·36	20·24	20·05	19·77	19·83	19·29	20·16
5 30	19·97	19·89	20·09	19·93	19·68	19·56	19·46	19·19	19·34	18·52	19·56
6 30	19·97	20·05	19·92	19·88	19·66	19·38	19·40	19·39	18·93	18·61	19·52
7 30	20·41	20·67	20·77	20·58	20·38	20·20	20·03	20·08	20·01	19·54	20·27
8 30	21·59	21·48	21·30	21·21	21·20	21·20	21·07	21·06	20·92	20·70	21·17
9 30	22·29	22·50	22·17	22·14	22·13	22·04	21·80	21·87	21·71	21·37	22·00
10 30	22·82	22·77	22·76	22·66	22·52	22·42	22·53	22·37	22·39	21·90	22·51
11 30	23·18	23·12	22·86	22·76	23·01	22·75	22·60	22·63	22·67	22·28	22·79
		3° S. Decl.	6° S. Decl.	9° S. Decl.	12° S. Decl.	15° S. Decl.	18° S. Decl.	21° S. Decl.	24° S. Decl.	27° S. Decl.	Mean.
0 30		23·00	23·10	22·88	23·04	22·93	22·89	22·64	22·54	22·53	22·92
1 30		22·76	22·72	22·71	22·63	22·67	22·47	22·39	22·27	22·01	22·52
2 30		22·14	22·32	22·09	22·22	22·05	21·97	21·80	21·64	21·50	21·98
3 30		21·45	21·62	21·49	21·44	21·33	21·33	20·89	20·93	20·65	21·27
4 30		20·87	20·74	20·51	20·37	20·50	20·26	19·99	19·65	19·77	20·32
5 30		19·89	20·02	20·43	19·84	19·67	19·35	19·29	19·17	18·70	19·63
6 30		20·07	19·87	19·76	19·77	19·65	19·44	19·01	19·10	18·83	19·54
7 30		20·61	20·46	20·67	20·35	20·53	20·37	19·87	19·90	19·65	20·28
8 30		21·33	21·47	21·54	21·47	21·25	21·13	20·90	20·63	20·76	21·21
9 30		22·37	22·21	22·25	21·99	22·09	21·91	21·64	21·46	21·54	21·97
10 30		22·77	22·62	22·62	22·69	22·55	22·47	22·20	21·97	22·16	22·49
11 30		23·17	22·94	22·93	22·52	22·88	22·82	22·57	22·03	22·42	22·75

Table XVI.

Showing the Height of High Water at the London Docks for every three degrees of the Moon's Declination north *or* south.

Moon's Transit.	0° Decl.	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.	Mean.
h m	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.
0 30	22·94	23·04	23·07	22·90	22·97	22·88	22·79	22·64	22·51	22·54	22·86
1 30	22·61	22·85	22·76	22·66	22·60	22·59	22·30	22·50	22·33	21·91	22·51
2 30	22·12	22·05	22·21	22·00	22·11	22·04	21·90	21·85	21·70	21·49	21·95
3 30	21·53	21·46	21·57	21·35	21·33	21·18	21·17	20·96	20·94	20·61	21·21
4 30	20·51	20·75	20·60	20·46	20·36	20·37	20·16	19·88	19·74	19·53	20·24
5 30	19·97	19·89	20·06	20·18	19·76	19·62	19·40	19·24	19·25	18·61	19·60
6 30	19·97	20·06	19·90	19·82	19·71	19·51	19·42	19·20	19·01	18·72	19·53
7 30	20·41	20·64	20·61	20·62	20·36	20·36	20·20	19·97	19·95	19·60	20·27
8 30	21·59	21·41	21·39	21·38	21·33	21·22	21·10	20·98	20·77	20·73	21·19
9 30	22·29	22·43	22·19	22·20	22·06	22·06	21·85	21·75	21·58	21·46	21·99
10 30	22·82	22·77	22·69	22·64	22·60	22·48	22·50	22·28	22·18	22·03	22·50
11 30	23·18	23·15	22·90	22·84	22·76	22·81	22·71	22·60	22·35	22·35	22·77

Table XVII.

Showing the Difference in the Height of High Water at the London Docks, and the Height corresponding to fifteen degrees Declination, for every three degrees of the Moon's Declination north *and* south.

Moon's Transit.	0° Decl.	3° N. Decl.	6° N. Decl.	9° N. Decl.	12° N. Decl.	15° N. Decl.	18° N. Decl.	21° N. Decl.	24° N. Decl.	27° N. Decl.	Moon's Transit.
h m	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	h m
0 30	+ 0·12	+ 0·25	+ 0·22	+ 0·10	+ 0·09	0	− 0·12	− 0·19	− 0·34	− 0·27	0 30
1 30	+ 0·10	+ 0·43	+ 0·28	+ 0·11	+ 0·05	0	− 0·38	+ 0·10	− 0·12	− 0·70	1 30
2 30	+ 0·10	− 0·05	+ 0·07	− 0·10	− 0·01	0	− 0·19	− 0·11	− 0·26	− 0·55	2 30
3 30	+ 0·50	+ 0·43	+ 0·48	+ 0·19	+ 0·20	0	− 0·02	0·00	− 0·07	− 0·47	3 30
4 30	+ 0·27	+ 0·38	+ 0·23	+ 0·18	+ 0·12	0	− 0·19	− 0·47	− 0·41	− 0·95	4 30
5 30	+ 0·41	+ 0·33	+ 0·53	+ 0·37	+ 0·12	0	− 0·10	− 0·37	− 0·22	− 1·04	5 30
6 30	+ 0·59	+ 0·67	+ 0·54	+ 0·50	+ 0·28	0	+ 0·02	+ 0·01	− 0·45	− 0·77	6 30
7 30	+ 0·21	+ 0·47	+ 0·57	+ 0·38	+ 0·18	0	− 0·17	− 0·12	− 0·19	− 0·66	7 30
8 30	+ 0·39	+ 0·28	+ 0·10	+ 0·01	0·00	0	− 0·13	− 0·14	− 0·28	− 0·50	8 30
9 30	+ 0·25	+ 0·46	+ 0·13	+ 0·10	+ 0·09	0	− 0·24	− 0·17	− 0·33	− 0·67	9 30
10 30	+ 0·40	+ 0·35	+ 0·34	+ 0·24	+ 0·10	0	+ 0·11	− 0·05	− 0·03	− 0·52	10 30
11 30	+ 0·43	+ 0·37	+ 0·11	+ 0·01	+ 0·26	0	− 0·15	− 0·12	− 0·08	− 0·47	11 30
	0° Decl.	3° S. Decl.	6° S. Decl.	9° S. Decl.	12° S. Decl.	15° S. Decl.	18° S. Decl.	21° S. Decl.	24° S. Decl.	27° S. Decl.	
0 30	+ 0·01	+ 0·07	+ 0·17	− 0·05	+ 0·11	0	− 0·04	− 0·29	− 0·49	− 0·40	0 30
1 30	− 0·06	+ 0·09	+ 0·05	+ 0·04	− 0·04	0	− 0·20	− 0·28	− 0·40	− 0·66	1 30
2 30	+ 0·07	+ 0·09	+ 0·27	+ 0·04	+ 0·17	0	− 0·08	− 0·25	− 0·41	− 0·55	2 30
3 30	+ 0·20	+ 0·12	+ 0·29	+ 0·16	+ 0·11	0	0·00	− 0·44	− 0·40	− 0·68	3 30
4 30	+ 0·01	+ 0·37	+ 0·24	+ 0·01	− 0·13	0	− 0·24	− 0·51	− 0·85	− 0·73	4 30
5 30	+ 0·30	+ 0·22	+ 0·35	+ 0·76	+ 0·17	0	− 0·32	− 0·38	− 0·50	− 0·97	5 30
6 30	+ 0·32	+ 0·42	+ 0·22	+ 0·11	+ 0·12	0	− 0·21	− 0·64	− 0·55	− 0·82	6 30
7 30	− 0·12	+ 0·08	− 0·07	+ 0·14	− 0·18	0	− 0·16	− 0·66	− 0·63	− 0·88	7 30
8 30	+ 0·34	+ 0·08	+ 0·22	+ 0·29	+ 0·22	0	− 0·12	− 0·35	− 0·62	− 0·49	8 30
9 30	+ 0·20	+ 0·28	+ 0·12	+ 0·16	− 0·10	0	− 0·18	− 0·45	− 0·63	− 0·55	9 30
10 30	+ 0·27	+ 0·22	+ 0·07	+ 0·07	+ 0·14	0	− 0·08	− 0·35	− 0·58	− 0·39	10 30
11 30	+ 0·30	+ 0·29	+ 0·06	+ 0·05	− 0·36	0	− 0·06	− 0·31	− 0·85	− 0·46	11 30

Table XVIII.

Showing the Difference in the Height of High Water at the London Docks, and the Height corresponding to fifteen degrees Declination, for every three degrees of the Moon's Declination north *or* south.

Moon's Transit.	0° Decl.	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.	Moon's Transit.
h m	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	h m
0 30	+ 0·06	+ 0·16	+ 0·19	+ 0·02	+ 0·09	0	− 0·09	− 0·24	− 0·37	− 0·34	0 30
1 30	+ 0·02	+ 0·26	+ 0·17	+ 0·07	+ 0·01	0	− 0·29	− 0·09	− 0·26	− 0·68	1 30
2 30	+ 0·08	+ 0·01	+ 0·17	− 0·04	+ 0·07	0	− 0·14	− 0·19	− 0·34	− 0·55	2 30
3 30	+ 0·35	+ 0·28	+ 0·39	+ 0·17	+ 0·15	0	− 0·01	− 0·22	− 0·24	− 0·57	3 30
4 30	+ 0·14	+ 0·38	+ 0·23	+ 0·41	− 0·01	0	− 0·21	− 0·49	− 0·63	− 0·84	4 30
5 30	+ 0·35	+ 0·27	+ 0·44	+ 0·56	+ 0·14	0	− 0·22	− 0·38	− 0·37	− 1·01	5 30
6 30	+ 0·46	+ 0·55	+ 0·39	+ 0·31	+ 0·20	0	− 0·09	− 0·31	− 0·50	− 0·79	6 30
7 30	+ 0·05	+ 0·28	+ 0·25	+ 0·26	0·00	0	− 0·16	− 0·39	− 0·41	− 0·76	7 30
8 30	+ 0·37	+ 0·19	+ 0·17	+ 0·16	+ 0·11	0	− 0·12	− 0·24	− 0·45	− 0·49	8 30
9 30	+ 0·23	+ 0·37	+ 0·13	+ 0·14	0·00	0	− 0·21	− 0·31	− 0·48	− 0·60	9 30
10 30	+ 0·34	+ 0·29	+ 0·21	+ 0·16	+ 0·12	0	+ 0·02	− 0·20	− 0·30	− 0·45	10 30
11 30	+ 0·37	+ 0·34	+ 0·09	+ 0·03	− 0·05	0	− 0·10	− 0·21	− 0·46	− 0·46	11 30

Table XIX.

Showing the Difference in the Height of High Water at the London Docks when the Moon's Declination is north or south.

Moon's Transit.	6° Declination.			21° Declination.		
	North.	South.	Difference.	North.	South.	Difference.
h m	feet.	feet.	feet.	feet.	feet.	feet.
0 30	+ ·13	+ ·11	− ·02	− ·28	+ ·04	+ ·32
1 30	+ ·19	+ ·14	− ·05	− ·21	− ·22	− ·01
2 30	− ·05	+ ·14	+ ·19	− ·21	− ·24	− ·03
3 30	+ ·21	+ ·34	+ ·13	− ·18	− ·13	+ ·05
4 30	+ ·13	+ ·34	+ ·21	− ·49	− ·40	+ ·09
5 30	+ ·35	+ ·49	+ ·14	− ·29	− ·35	− ·06
6 30	+ ·44	+ ·39	− ·05	− ·27	− ·33	− ·06
7 30	+ ·31	+ ·22	− ·09	− ·32	− ·31	+ ·01
8 30	+ ·11	+ ·22	+ ·12	− ·20	− ·33	− ·13
9 30	+ ·21	+ ·22	+ ·01	− ·27	− ·39	− ·12
10 30	+ ·25	+ ·19	− ·06	− ·05	− ·27	− ·22
11 30	+ ·10	+ ·20	+ ·10	− ·18	− ·34	− ·16

The preceding Table has been formed by considering the quantities given for 6° and 21° as arithmetic means between those given in Table XVII. for 3°, 6°, 9°, and 18°, 21°, and 24°, so that they may each be considered as resulting from the average of about 150 observations. This Table does not seem to confirm what is stated by Laplace, Méc. Cél. tom. v. p. 162. "L'action de la lune pour élever la mer à Brest, est plus grande lorsque sa déclinaison est australe, que lorsqu'elle est boréale."

Table XX.

Showing the Interval between the Apparent Solar Time of the Moon's Transit and the Time of High Water, and the Height of High Water at the London Docks (together with the Interval between the Moon's Transits), corresponding to the Apparent Solar Time of the Moon's Upper and Lower Transit, P.M. and A.M.

Moon's Transit.	Moon's Parallax.	Interval between the Moon's Transit and the Time of high water.	Height.	Interval of Transits.	Moon's Declination.	Moon's Transit.	Moon's Parallax.	Interval between the Moon's Transit and the Time of high water.	Height.	Interval of Transits.	Moon's Declination.
January.											
Upper Transits, P.M.						Lower (Interpolated) Transits, A.M.					
h m	′	h m	ft. in.	h m	°	h m	′	h m	ft. in.	h m	°
0 32·8	56·9	3 4·2	22 8·4	1 38·6	S. 19·1	0 26·6	57·4	3 4·0	22 8·8	1 41·2	S. 19·2
1 33·4	57·2	2 45·5	22 7·8	1 35·1	S. 15·1	1 25·4	57·2	2 48·1	22 6·0	1 36·7	S. 16·2
2 28·2	57·1	2 39·3	22 0·5	1 32·1	S. 9·7	2 27·1	56·6	2 31·9	22 0·1	1 30·6	S. 10·8
3 29·2	56·6	2 29·6	21 5·7	1 29·1	S. 4·3	3 26·7	57·2	2 29·4	21 8·1	1 31·4	S. 3·3
4 30·3	56·7	2 35·7	20 4·7	1 31·2	N. 1·4	4 30·3	56·3	2 20·3	20 4·5	1 30·6	N. 2·5
5 31·7	56·5	2 45·9	19 5·9	1 35·6	N. 8·7	5 29·7	56·9	2 34·3	19 4·4	1 36·0	N. 7·6
6 31·9	56·5	3 15·4	19 6·0	1 40·3	N. 13·9	6 28·5	57·0	3 0·3	19 7·3	1 40·2	N. 13·1
7 26·2	56·9	3 38·8	20 2·5	1 47·3	N. 17·5	7 30·2	56·8	3 36·3	20 0·8	1 45·3	N. 17·5
8 25·7	57·1	3 54·4	21 1·4	1 49·7	N. 20·5	8 32·9	57·0	3 42·2	21 1·1	1 50·6	N. 21·4
9 29·0	57·0	3 51·5	21 4·2	1 50·7	N. 22·9	9 29·4	57·1	3 46·7	22 1·0	1 49·9	N. 22·3
10 30·5	57·4	3 37·3	21 10·4	1 50·6	N. 23·1	10 25·6	57·4	3 35·4	22 5·5	1 49·2	N. 22·4
11 28·9	57·5	3 20·9	22 3·9	1 46·1	N. 21·8	11 28·3	57·1	3 16·9	22 10·3	1 45·3	N. 20·9
Upper Transits, A.M.						Lower (Interpolated) Transits, P.M.					
0 30·4	57·1	3 2·5	22 7·1	1 39·8	N. 18·8	0 30·8	56·3	3 1·2	22 9·2	1 40·9	N. 20·0
1 30·6	57·4	2 47·0	22 4·8	1 36·5	N. 15·0	1 26·0	57·2	2 48·6	22 8·8	1 35·9	N. 15·7
2 30·8	57·3	2 37·1	21 7·6	1 32·5	N. 10·9	2 27·1	57·3	2 41·1	22 1·4	1 33·8	N. 8·8
3 29·1	57·1	2 28·9	21 1·9	1 31·8	N. 3·4	3 31·9	57·0	2 32·8	21 3·9	1 30·3	N. 3·9
4 29·8	56·8	2 25·0	20 5·5	1 31·8	S. 2·4	4 29·5	57·1	2 31·5	20 7·3	1 33·7	S. 2·4
5 29·3	57·1	2 40·5	19 4·0	1 37·0	S. 8·2	5 31·0	56·9	2 52·6	19 9·2	1 36·8	S. 8·5
6 25·8	57·0	3 4·8	19 5·0	1 41·3	S. 12·7	6 34·4	57·0	3 18·8	19 7·1	1 41·8	S. 13·4
7 28·4	56·9	3 38·7	20 0·9	1 46·2	S. 17·9	7 33·5	57·5	3 46·1	20 5·0	1 48·4	S. 18·0
8 31·2	57·2	3 53·0	21 3·4	1 51·0	S. 21·1	8 29·9	57·3	3 58·8	21 0·3	1 50·3	S. 20·5
9 29·2	57·2	3 49·0	21 9·9	1 53·0	S. 23·0	9 29·7	57·1	3 50·0	21 7·5	1 50·4	S. 22·2
10 28·3	57·3	3 35·7	22 6·3	1 49·8	S. 22·5	10 32·6	57·4	3 35·5	21 10·9	1 49·4	S. 23·3
11 27·9	57·1	3 20·8	22 9·0	1 46·0	S. 21·6	11 31·9	56·9	3 19·7	22 3·7	1 42·8	S. 20·5
February.											
Upper Transits, P.M.						Lower (Interpolated) Transits, A.M.					
0 29·0	57·2	3 5·0	22 11·0	1 33·8	S. 10·4	0 31·4	57·3	2 59·9	22 10·4	1 34·0	S. 10·1
1 26·5	57·2	2 53·2	22 4·0	1 32·6	S. 5·0	1 28·7	56·9	2 46·5	22 7·1	1 32·1	S. 3·6
2 28·8	57·2	2 38·7	21 10·3	1 33·7	N. 2·3	2 28·8	57·1	2 33·3	22 2·6	1 33·3	N. 1·3
3 29·7	56·4	2 25·3	21 3·8	1 35·2	N. 8·4	3 27·5	56·7	2 23·8	21 1·7	1 36·5	N. 7·7
4 31·3	56·9	2 25·5	19 8·3	1 41·8	N. 14·3	4 28·1	56·3	2 16·1	20 1·0	1 39·6	N. 13·0
5 26·4	56·6	2 41·9	18 10·0	1 44·7	N. 17·9	5 31·4	56·4	2 26·6	18 11·6	1 45·0	N. 19·0
6 27·4	56·3	3 11·6	19 0·4	1 46·1	N. 20·7	6 31·6	56·6	2 59·0	19 0·6	1 47·6	N. 20·8
7 31·5	56·9	3 41·0	20 0·0	1 50·4	N. 22·8	7 25·9	56·8	3 36·9	19 7·8	1 49·9	N. 22·7
8 25·6	57·0	3 58·5	20 8·9	1 48·4	N. 22·6	8 25·0	56·7	3 51·9	21 0·2	1 47·8	N. 21·9
9 25·0	57·5	3 50·7	21 8·0	1 48·9	N. 21·9	9 28·3	57·0	3 43·3	21 9·4	1 45·6	N. 21·9
10 29·0	57·3	3 35·9	22 3·3	1 44·2	N. 18·4	10 30·4	57·3	3 36·0	22 7·7	1 41·6	N. 18·7
11 28·7	57·3	3 19·9	22 8·7	1 37·7	N. 15·3	11 31·4	57·6	3 19·6	23 2·4	1 39·0	N. 15·0
Upper Transits, A.M.						Lower (Interpolated) Transits, P.M.					
0 25·9	57·7	3 1·7	23 1·0	1 36·2	N. 10·7	0 30·4	57·0	3 4·6	23 4·0	1 35·4	N. 9·5
1 25·0	57·5	2 47·8	23 2·1	1 34·4	N. 4·6	1 29·5	57·6	2 50·3	22 10·1	1 34·3	N. 4·4
2 26·7	57·1	2 34·8	22 0·2	1 33·7	S. 1·9	2 28·3	57·4	2 43·4	22 3·6	1 35·2	S. 1·9
3 30·4	57·1	2 23·8	21 7·0	1 37·6	S. 7·7	3 30·2	57·1	2 24·5	21 5·0	1 37·0	S. 7·8
4 27·9	56·7	2 18·7	20 4·3	1 40·8	S. 13·8	4 32·8	57·0	2 23·5	20 8·9	1 42·3	S. 14·0
5 28·0	56·7	2 27·0	19 2·5	1 45·1	S. 18·0	5 28·9	57·0	2 30·1	19 6·1	1 46·3	S. 18·1
6 26·1	57·0	3 6·1	19 2·3	1 48·6	S. 20·8	6 30·7	56·5	3 1·9	19 0·0	1 47·1	S. 20·6
7 25·6	56·7	3 35·3	20 1·9	1 48·6	S. 22·2	7 31·0	57·0	3 46·3	20 1·2	1 49·3	S. 22·6
8 27·5	56·6	3 48·8	21 0·1	1 47·9	S. 23·2	8 29·9	56·8	3 52·8	20 6·6	1 48·2	S. 23·2
9 27·0	57·2	3 45·9	21 7·9	1 46·4	S. 21·8	9 30·2	57·2	3 47·3	21 7·3	1 45·6	S. 21·6
10 30·7	57·0	3 35·9	22 7·6	1 40·5	S. 19·0	10 30·0	57·0	3 31·8	22 1·3	1 40·9	S. 18·6
11 31·0	57·3	3 21·6	22 9·0	1 37·5	S. 14·4	11 32·0	56·8	3 12·9	22 9·0	1 35·5	S. 14·5

Tabe XX. (Continued.)

March.

Moon's Transit.	Moon's Parallax.	Interval between the Moon's Transit and the Time of high water.	Height.	Interval of Transits.	Moon's Declination.	Moon's Transit.	Moon's Parallax.	Interval between the Moon's Transit and the Time of high water.	Height.	Interval of Transits.	Moon's Declination.
Upper Transits, P.M.						Lower (Interpolated) Transits, A.M.					
h m	′	h m	ft. in.	h m	°	h m	′	h m	ft. in.	h m	°
0 31·9	57·1	3 7·2	22 8·5	1 35·3	N. 2·1	0 26·8	56·7	3 4·8	22 8·0	1 33·5	N. 0·9
1 33·4	56·7	2 49·9	22 5·5	1 36·3	N. 7·6	1 28·3	57·3	2 48·2	22 10·4	1 38·4	N. 6·7
2 25·6	57·0	2 43·3	21 8·3	1 42·2	N. 13·4	2 28·2	56·6	2 30·8	22 0·0	1 39·1	N. 12·9
3 28·5	56·6	2 22·7	20 8·4	1 44·3	N. 17·0	3 31·8	56·5	2 20·2	21 2·3	1 44·3	N. 17·5
4 28·2	56·4	2 15·0	19 5·1	1 46·0	N. 20·8	4 30·8	56·5	2 12·1	20 0·0	1 47·4	N. 20·8
5 29·8	55·5	2 24·2	18 9·0	1 47·5	N. 22·2	5 29·0	56·5	2 19·1	19 5·6	1 47·3	N. 21·6
6 27·8	56·9	3 9·0	19 0·6	1 47·8	N. 23·0	6 29·8	56·5	3 5·7	19 1·6	1 47·6	N. 23·4
7 26·4	56·7	3 46·5	19 7·8	1 44·9	N. 22·3	7 31·3	57·1	3 44·8	20 4·7	1 47·0	N. 21·7
8 28·1	57·2	3 56·5	20 8·7	1 42·8	N. 18·6	8 30·2	57·2	3 52·8	21 4·8	1 43·1	N. 19·9
9 30·4	57·3	3 46·1	21 10·0	1 39·5	N. 15·9	9 30·2	57·5	3 51·8	22 2·6	1 39·5	N. 15·2
10 27·8	57·6	3 33·3	22 4·9	1 37·0	N. 11·4	10 32·7	57·7	3 34·9	22 9·2	1 37·4	N. 10·2
11 27·9	58·0	3 19·9	23 0·6	1 37·8	N. 4·7	11 29·6	57·4	3 21·4	23 0·4	1 35·6	N. 4·7
Upper Transits, A.M.						Lower (Interpolated) Transits, P.M.					
0 30·5	57·0	3 3·1	23 2·7	1 34·4	S. 1·7	0 26·7	58·0	3 9·3	22 10·6	1 38·0	S. 0·8
1 28·9	57·4	2 51·6	22 10·6	1 39·0	S. 6·7	1 30·4	57·2	2 48·7	22 9·1	1 38·1	S. 7·5
2 26·3	56·9	2 36·1	21 11·1	1 40·8	S. 13·4	2 33·7	57·1	2 34·0	21 9·1	1 42·2	S. 13·3
3 26·7	56·9	2 21·1	21 1·0	1 46·2	S. 17·3	3 27·2	57·0	2 21·8	20 10·6	1 45·3	S. 17·1
4 28·4	57·0	2 14·4	20 1·4	1 47·6	S. 20·4	4 29·0	56·7	2 17·4	19 8·1	1 46·8	S. 20·4
5 27·2	56·9	2 29·9	19 1·5	1 48·6	S. 22·1	5 30·6	56·9	2 28·2	18 5·8	1 49·4	S. 22·8
6 27·3	56·7	3 7·6	19 0·1	1 47·7	S. 22·8	6 29·7	57·0	3 8·9	18 9·2	1 47·4	S. 23·0
7 26·3	56·6	3 43·9	20 0·6	1 44·8	S. 22·8	7 29·8	56·9	3 47·9	19 6·0	1 44·6	S. 21·7
8 27·6	57·1	4 1·4	20 11·7	1 41·3	S. 19·1	8 31·0	57·0	3 59·6	20 8·3	1 41·5	S. 19·7
9 26·4	57·4	3 48·1	21 10·0	1 39·8	S. 16·8	9 30·2	57·3	3 49·9	21 6·4	1 38·7	S. 15·7
10 26·1	57·5	3 38·0	22 4·1	1 36·5	S. 11·2	10 30·2	57·2	3 36·7	22 3·7	1 35·4	S. 10·0
11 29·3	55·8	3 24·1	22 11·5	1 33·1	S. 4·6	11 29·0	57·1	3 21·0	22 9·1	1 35·6	S. 4·7

April.

Moon's Transit.	Moon's Parallax.	Interval between the Moon's Transit and the Time of high water.	Height.	Interval of Transits.	Moon's Declination.	Moon's Transit.	Moon's Parallax.	Interval between the Moon's Transit and the Time of high water.	Height.	Interval of Transits.	Moon's Declination.
Upper Transits, P.M.						Lower (Interpolated) Transits, A.M.					
0 29·6	57·1	3 7·9	22 10·0	1 42·6	N. 12·7	0 29·6	56·6	3 5·4	23 0·5	1 42·5	N. 12·4
1 26·2	57·3	2 54·7	22 3·2	1 47·5	N. 17·0	1 27·7	57·0	2 49·4	22 6·6	1 44·8	N. 16·3
2 27·4	56·5	2 33·1	21 8·1	1 46·8	N. 20·0	2 27·9	56·9	2 33·8	22 1·0	1 48·6	N. 20·5
3 29·0	56·8	2 18·4	20 9·9	1 48·9	N. 22·2	3 28·3	56·9	2 21·7	21 1·3	1 48·8	N. 22·0
4 24·4	56·5	2 10·1	19 2·0	1 46·4	N. 22·4	4 29·7	56·4	2 14·3	20 0·9	1 46·2	N. 22·8
5 21·8	56·5	2 24·7	18 8·2	1 44·4	N. 22·9	5 31·0	56·7	2 34·0	19 3·9	1 44·5	N. 21·9
6 26·7	57·7	3 9·8	18 10·4	1 41·0	N. 19·4	6 29·6	57·0	3 11·2	19 7·7	1 40·8	N. 20·3
7 30·1	56·8	3 43·8	19 9·2	1 37·5	N. 16·8	7 30·0	56·9	3 42·9	20 5·7	1 36·6	N. 15·8
8 28·7	56·9	3 51·6	20 11·0	1 34·0	N. 11·1	8 27·0	57·3	3 47·3	21 7·9	1 36·0	N. 11·1
9 28·7	57·5	3 47·7	22 2·1	1 36·1	N. 5·9	9 28·3	57·1	3 47·0	22 6·0	1 34·3	N. 6·0
10 31·1	57·4	3 35·1	22 8·6	1 36·1	S. 0·9	10 27·9	57·8	3 39·0	23 0·3	1 37·9	S. 0·3
11 32·1	58·1	3 20·3	23 3·5	1 43·3	S. 7·7	11 29·2	57·4	3 22·7	23 2·7	1 39·0	S. 6·4
Upper Transits, A.M.						Lower (Interpolated) Transits, P.M.					
0 27·4	57·5	3 8·0	23 2·5	1 44·2	S. 12·1	0 31·3	57·7	3 6·7	23 1·0	1 46·0	S. 13·3
1 29·3	57·5	2 51·4	22 6·3	1 47·2	S. 16·4	1 28·2	57·8	2 47·5	22 6·5	1 48·7	S. 16·9
2 33·2	57·3	2 33·5	22 0·2	1 49·4	S. 20·8	2 26·8	57·2	2 33·5	21 10·4	1 48·7	S. 19·6
3 28·8	57·0	2 26·8	21 1·4	1 48·4	S. 21·7	3 28·9	57·2	2 19·3	20 10·2	1 50·1	S. 22·1
4 25·3	57·0	2 19·1	20 0·6	1 47·8	S. 22·9	4 31·1	56·9	2 11·0	19 7·0	1 47·3	S. 23·2
5 26·1	56·6	2 29·9	19 6·5	1 44·5	S. 22·8	5 33·5	56·5	2 32·8	18 8·8	1 43·5	S. 21·6
6 24·4	56·9	3 7·6	19 8·7	1 41·0	S. 20·5	6 32·2	56·5	3 6·0	19 1·0	1 40·1	S. 19·9
7 23·9	56·7	3 42·0	20 5·3	1 35·4	S. 16·1	7 31·6	56·6	3 37·0	19 10·5	1 36·1	S. 15·8
8 30·1	57·0	3 52·5	21 7·3	1 34·4	S. 11·9	8 31·8	56·6	3 48·7	21 0·3	1 32·3	S. 11·2
9 33·7	56·9	3 50·4	22 4·2	1 33·2	S. 5·1	9 28·3	56·9	3 47·1	22 0·2	1 33·8	S. 4·9
10 32·9	57·1	3 38·4	23 0·8	1 36·5	N. 1·1	10 28·6	57·0	3 37·3	22 8·6	1 35·2	N. 0·9
11 30·6	57·3	3 18·9	23 0·2	1 38·6	N. 6·6	11 29·1	57·0	3 19·3	23 2·9	1 39·2	N. 6·4

Table XX. (Continued.)

Moon's Transit.	Moon's Parallax.	Interval between the Moon's Transit and the Time of high water.	Height.	Interval of Transits.	Moon's Declination.	Moon's Transit.	Moon's Parallax.	Interval between the Moon's Transit and the Time of high water.	Height.	Interval of Transits.	Moon's Declination.
May.											
Upper Transits, P.M.						Lower (Interpolated) Transits, A.M.					
h m	′	h m	ft. in.	h m	°	h m	′	h m	ft. in.	h m	°
0 30·4	57·2	3 4·6	22 8·8	1 49·6	N. 20·5	0 30·4	57·4	3 5·3	22 10·9	1 50·3	N. 19·6
1 34·7	57·4	2 49·6	22 3·2	1 50·3	N. 22·0	1 26·0	57·5	2 50·3	22 8·5	1 51·1	N. 21·7
2 33·2	57·1	2 27·1	21 6·1	1 48·5	N. 23·3	2 28·4	57·0	2 35·1	21 11·0	1 48·0	N. 22·2
3 31·2	56·9	2 17·6	20 4·0	1 44·6	N. 22·0	3 30·3	57·0	2 24·4	21 3·5	1 45·0	N. 21·9
4 29·8	56·8	2 17·5	19 9·9	1 40·9	N. 20·0	4 29·2	56·9	2 19·8	20 2·5	1 40·6	N. 20·4
5 29·6	56·3	2 28·5	19 3·6	1 35·4	N. 16·6	5 29·8	56·6	2 39·4	20 1·3	1 35·5	N. 16·7
6 28·2	56·6	3 5·8	19 6·6	1 31·8	N. 11·9	6 27·1	56·7	3 16·5	20 4·3	1 34·0	N. 11·4
7 27·7	57·1	3 33·6	20 5·3	1 34·0	N. 5·4	7 28·1	56·6	3 39·1	20 9·1	1 31·0	N. 6·5
8 33·2	56·8	3 46·6	21 5·5	1 33·1	S. 0·5	8 30·2	57·3	3 48·3	21 8·9	1 35·6	S. 0·5
9 30·7	57·5	3 44·7	22 3·5	1 39·0	S. 6·8	9 31·4	57·1	3 46·2	22 5·0	1 37·9	S. 6·0
10 23·7	57·6	3 30·0	22 9·5	1 43·6	S. 11·4	10 30·6	57·6	3 36·7	22 9·5	1 43·9	S. 12·3
11 25·2	57·3	3 21·5	22 10·1	1 46·9	S. 16·3	11 27·9	58·0	3 23·0	22 10·9	1 49·6	S. 16·5
Upper Transits, A.M.						Lower (Interpolated) Transits, P.M.					
0 32·2	58·0	3 1·1	23 0·5	1 53·0	S. 20·5	0 28·0	57·5	3 4·2	22 9·1	1 50·0	S. 19·4
1 28·8	57·7	2 49·6	22 7·8	1 52·2	S. 22·4	1 30·7	57·7	2 47·9	22 4·4	1 52·2	S. 22·0
2 23·6	57·2	2 35·5	22 0·3	1 48·7	S. 22·7	2 35·5	57·6	2 29·1	21 4·4	1 49·0	S. 22·9
3 22·2	56·9	2 26·4	21 4·5	1 46·6	S. 22·7	3 34·7	57·3	2 20·8	20 10·2	1 45·8	S. 20·9
4 27·5	56·7	2 25·9	20 4·0	1 39·2	S. 20·0	4 31·1	56·8	2 18·0	19 10·4	1 40·6	S. 19·7
5 29·5	57·0	2 43·6	19 11·9	1 33·2	S. 16·4	5 27·5	56·6	2 29·8	19 3·6	1 35·7	S. 17·2
6 29·3	56·4	3 14·2	20 0·2	1 32·5	S. 12·6	6 29·6	56·3	3 6·2	19 3·7	1 30·8	S. 11·8
7 32·1	56·6	3 41·9	20 11·1	1 31·9	S. 5·9	7 29·6	56·6	3 32·5	20 4·3	1 31·9	S. 5·2
8 28·4	56·6	3 52·3	21 7·1	1 32·0	N. 0·4	8 28·6	56·4	3 46·6	21 0·8	1 32·4	0·0
9 28·6	56·6	3 52·5	22 5·0	1 35·8	N. 6·4	9 30·0	57·1	3 46·0	22 2·2	1 37·3	N. 6·5
10 29·9	57·0	3 38·3	22 11·2	1 41·6	N. 11·7	10 27·7	56·9	3 28·3	22 6·9	1 41·6	N. 11·8
11 28·6	57·6	3 23·4	22 9·7	1 47·9	N. 16·7	11 29·5	56·9	3 22·0	22 9·0	1 45·1	N. 16·9
June.											
Upper Transits, P.M.						Lower (Interpolated) Transits, A.M.					
0 32·2	57·5	3 1·5	22 5·6	1 49·6	N. 22·9	0 27·6	57·0	3 7·1	22 8·0	1 48·5	N. 22·7
1 28·7	57·2	2 49·2	22 1·3	1 45·2	N. 21·9	1 30·4	57·4	2 49·9	22 5·0	1 45·6	N. 22·0
2 31·1	57·3	2 33·6	21 9·9	1 40·6	N. 19·4	2 28·2	57·3	2 40·0	22 1·2	1 40·4	N. 19·9
3 33·4	57·1	2 22·8	21 0·4	1 35·4	N. 16·0	3 24·3	57·0	2 32·6	21 6·0	1 37·0	N. 16·2
4 32·0	57·0	2 26·1	20 5·0	1 32·4	N. 11·2	4 28·2	57·0	2 34·9	20 11·0	1 33·5	N. 11·6
5 29·3	57·0	2 38·3	20 0·2	1 32·6	N. 5·8	5 29·6	56·7	2 52·4	20 3·0	1 31·3	N. 5·7
6 29·0	56·6	3 6·8	19 8·9	1 31·5	S. 0·3	6 29·8	56·8	3 14·6	20 2·4	1 33·8	S. 0·3
7 30·4	57·0	3 32·6	20 5·6	1 37·2	S. 6·6	7 32·0	55·6	3 37·7	20 9·3	1 36·1	S. 6·7
8 28·9	57·2	3 45·6	21 3·7	1 42·2	S. 12·4	8 31·5	57·1	3 46·9	21 3·2	1 42·6	S. 12·1
9 31·3	57·1	3 46·1	21 11·0	1 46·9	S. 17·6	9 26·1	57·5	3 46·1	21 11·5	1 47·0	S. 16·4
10 28·7	57·4	3 35·4	22 4·1	1 50·6	S. 20·3	10 27·7	57·0	3 35·7	22 2·0	1 48·6	S. 20·0
11 22·1	57·7	3 25·4	22 6·7	1 52·7	S. 22·2	11 31·3	57·6	3 18·8	22 6·7	1 52·0	S. 22·3
Upper Transits, A.M.						Lower (Interpolated) Transits, P.M.					
0 24·2	57·3	3 5·9	22 8·7	1 49·8	S. 23·0	0 30·2	57·5	3 1·4	22 5·7	1 49·2	S. 22·8
1 30·8	57·7	2 50·8	22 5·2	1 45·7	S. 21·9	1 30·6	57·3	2 44·0	22 3·0	1 45·6	S. 21·8
2 35·6	57·7	2 38·5	22 0·2	1 41·0	S. 19·4	2 25·9	57·4	2 31·5	21 8·9	1 41·7	S. 20·0
3 35·9	57·1	2 30·5	21 4·0	1 35·5	S. 15·8	3 27·3	56·8	2 23·8	21 2·4	1 35·7	S. 16·5
4 32·9	56·8	2 36·7	20 7·0	1 32·0	S. 11·1	4 27·1	57·2	2 25·3	20 5·4	1 33·0	S. 10·8
5 32·1	56·5	2 49·5	20 0·2	1 29·6	S. 7·0	5 28·3	56·8	2 34·7	19 11·5	1 31·0	S. 5·7
6 31·8	56·5	3 20·1	20 2·2	1 31·4	N. 1·1	6 32·6	56·5	3 5·4	19 10·0	1 31·3	N. 0·6
7 31·2	56·4	3 42·7	20 7·3	1 33·9	N. 6·6	7 30·2	56·7	3 32·4	20 5·2	1 35·8	N. 6·6
8 30·0	56·9	3 48·9	21 4·1	1 41·1	N. 12·7	8 31·2	56·7	3 47·5	21 0·2	1 40·0	N. 12·0
9 26·4	56·7	3 46·9	21 10·3	1 45·6	N. 16·6	9 34·0	56·9	3 46·7	21 11·5	1 46·8	N. 17·0
10 28·0	56·9	3 33·0	22 3·5	1 48·4	N. 19·8	10 30·9	57·3	3 35·7	22 5·3	1 50·0	N. 20·3
11 31·3	57·1	3 12·6	22 5·5	1 50·8	N. 22·8	11 27·9	56·9	3 23·9	22 6·2	1 48·7	N. 21·5

Table XX. (Continued.)

Moon's Transit.	Moon's Parallax.	Interval between the Moon's Transit and the Time of high water.	Height.	Interval of Transits.	Moon's Declination.	Moon's Transit.	Moon's Parallax.	Interval between the Moon's Transit and the Time of high water.	Height.	Interval of Transits.	Moon's Declination.
July.											
Upper Transits, P.M.						**Lower (Interpolated) Transits, A.M.**					
h m	′	h m	ft. in.	h m	°	h m	′	h m	ft. in.	h m	°
0 30·3	57·3	3 1·5	22 7·5	1 42·2	N. 19·5	0 28·9	57·3	3 8·4	22 9·8	1 41·7	N. 19·3
1 29·5	57·0	2 46·4	22 5·3	1 35·7	N. 16·3	1 27·2	57·1	2 56·2	22 7·2	1 37·0	N. 16·1
2 28·3	57·1	2 36·5	22 2·5	1 33·8	N. 10·4	2 30·7	57·6	2 41·1	22 4·7	1 33·4	N. 11·2
3 29·5	57·1	2 29·7	21 7·2	1 31·9	N. 5·2	3 32·8	57·4	2 38·7	21 7·1	1 33·2	N. 5·2
4 27·8	57·0	2 30·0	20 8·3	1 32·8	S. 1·8	4 28·6	56·9	2 36·4	20 10·1	1 33·1	S. 2·1
5 28·1	57·2	2 42·8	20 2·7	1 37·9	S. 7·6	5 28·6	56·9	2 50·2	20 2·7	1 36·1	S. 6·8
6 31·6	56·9	3 9·8	19 10·8	1 41·7	S. 13·2	6 26·5	57·2	3 15·0	19 10·9	1 42·1	S. 12·5
7 31·9	57·3	3 37·5	20 5·8	1 46·9	S. 16·9	7 26·8	56·9	3 35·5	20 5·2	1 45·5	S. 16·7
8 28·6	57·3	3 52·3	21 0·8	1 50·7	S. 19·8	8 29·9	57·1	3 49·7	20 11·2	1 49·8	S. 20·4
9 29·0	57·0	3 50·5	21 8·3	1 50·8	S. 22·6	9 31·1	57·2	3 45·4	21 7·7	1 50·9	S. 22·9
10 28·5	57·0	3 40·8	22 1·8	1 49·0	S. 23·0	10 29·1	57·6	3 35·6	22 0·0	1 51·1	S. 22·9
11 30·8	57·4	3 26·9	22 7·0	1 46·1	S. 21·7	11 26·6	57·0	3 17·4	22 5·0	1 46·2	S. 21·8
Upper Transits, A.M.						**Lower (Interpolated) Transits, P.M.**					
0 31·7	57·3	3 7·9	22 9·0	1 41·2	S. 19·0	0 29·7	57·3	3 2·0	22 6·5	1 41·4	S. 20·0
1 26·5	57·3	2 57·6	22 6·1	1 38·0	S. 16·4	1 28·1	56·9	2 46·0	22 5·8	1 34·6	S. 15·0
2 28·6	56·8	2 43·3	22 0·0	1 31·9	S. 11·3	2 28·5	57·5	2 36·0	22 3·0	1 34·7	S. 11·1
3 28·9	57·2	2 38·0	21 7·4	1 32·0	S. 4·5	3 31·2	56·7	2 30·1	21 5·4	1 30·9	S. 4·6
4 29·8	56·8	2 38·5	20 6·1	1 32·3	N. 1·6	4 32·0	56·8	2 24·4	20 8·9	1 32·1	N. 1·3
5 31·6	56·7	2 52·6	19 11·7	1 35·7	N. 7·4	5 30·5	57·4	2 37·8	19 10·9	1 35·7	N. 7·9
6 26·4	56·8	3 20·5	19 9·8	1 39·9	N. 12·0	6 29·7	56·7	3 8·1	19 9·7	1 40·0	N. 12·9
7 28·7	56·6	3 37·7	20 2·1	1 44·0	N. 16·0	7 29·5	56·8	3 38·2	20 0·2	1 45·5	N. 17·2
8 31·7	56·6	3 51·1	20 11·2	1 48·0	N. 20·6	8 25·0	56·8	3 50·3	21 0·6	1 48·3	N. 20·1
9 31·0	57·0	3 49·2	21 7·6	1 50·8	N. 22·6	9 26·0	56·8	3 49·9	21 8·3	1 50·5	N. 22·3
10 28·6	57·0	3 37·4	22 2·2	1 49·1	N. 23·0	10 29·5	56·9	3 41·0	22 5·2	1 48·9	N. 23·0
11 31·0	56·9	3 19·5	22 5·4	1 44·9	N. 21·3	11 28·7	57·4	3 22·9	22 7·4	1 45·8	N. 22·5
August.											
Upper Transits, P.M.						**Lower (Interpolated) Transits, A.M.**					
0 30·7	57·3	3 5·9	22 9·3	1 35·7	N. 10·5	0 31·1	57·0	3 12·7	22 9·3	1 34·0	N. 11·1
1 29·5	56·9	2 50·4	22 8·5	1 31·9	N. 4·9	1 29·4	57·4	2 54·8	22 8·1	1 33·9	N. 5·6
2 29·7	57·3	2 38·8	22 2·8	1 34·6	S. 1·1	2 28·2	56·8	2 45·5	22 1·2	1 33·2	S. 1·5
3 30·2	57·0	2 25·3	21 6·0	1 36·7	S. 7·1	3 29·2	56·9	2 34·6	21 3·4	1 35·7	S. 7·1
4 32·2	56·8	2 24·9	20 8·5	1 41·1	S. 13·3	4 29·0	56·8	2 29·6	20 4·1	1 40·7	S. 12·9
5 33·5	56·6	2 36·3	19 6·6	1 46·7	S. 18·1	5 28·2	57·0	2 42·3	19 5·9	1 45·4	S. 17·2
6 30·4	56·8	3 11·4	19 3·9	1 48·2	S. 20·6	6 30·0	56·8	3 10·5	19 2·8	1 48·6	S. 21·2
7 31·7	57·3	3 46·1	20 2·7	1 51·9	S. 22·7	7 27·5	57·3	3 40·7	19 8·2	1 52·0	S. 22·3
8 31·3	57·1	4 0·9	21 1·2	1 49·7	S. 23·2	8 26·8	57·3	3 51·3	20 7·6	1 51·2	S. 22·6
9 30·1	57·0	3 55·5	21 11·6	1 46·1	S. 21·6	9 27·4	57·6	3 48·0	21 6·2	1 49·0	S. 21·9
10 27·0	57·5	3 46·4	22 5·6	1 43·9	S. 19·1	10 30·2	56·9	3 36·1	22 0·4	1 42·9	S. 20·0
11 23·5	57·5	3 32·1	22 10·5	1 39·7	S. 16·0	11 31·9	57·3	3 19·1	22 8·0	1 37·7	S. 15·6
Upper Transits, A.M.						**Lower (Interpolated) Transits, P.M.**					
0 26·9	56·9	3 14·5	22 9·8	1 34·2	S. 11·3	0 32·0	57·2	3 4·0	22 10·4	1 35·1	S. 11·4
1 29·4	57·0	2 54·7	22 5·8	1 32·3	S. 5·3	1 32·4	56·6	2 46·3	22 8·1	1 30·9	S. 4·8
2 28·0	56·8	2 41·1	21 10·8	1 32·8	N. 0·2	2 29·2	57·1	2 35·6	22 3·0	1 33·3	N. 1·1
3 28·3	56·8	2 33·3	21 2·4	1 36·3	N. 7·4	3 28·6	56·6	2 24·0	21 4·9	1 35·2	N. 7·9
4 28·5	56·7	2 30·2	20 4·7	1 39·3	N. 12·3	4 28·5	56·6	2 18·8	20 5·7	1 38·9	N. 12·2
5 28·1	56·6	2 38·9	19 4·4	1 44·0	N. 17·2	5 27·5	56·3	2 28·8	19 6·5	1 43·9	N. 17·7
6 32·8	56·9	3 15·0	19 2·1	1 48·9	N. 21·0	6 25·8	56·6	3 7·1	19 5·1	1 47·4	N. 20·8
7 35·8	57·0	3 42·7	19 9·7	1 50·7	N. 22·5	7 25·0	57·0	3 42·7	20 1·4	1 50·7	N. 22·2
8 31·8	56·8	3 55·9	20 8·3	1 49·0	N. 22·4	8 24·4	57·2	3 58·8	21 1·0	1 50·7	N. 22·7
9 32·3	57·3	3 49·6	21 4·4	1 47·8	N. 21·9	9 28·3	57·2	3 56·9	21 9·9	1 47·5	N. 22·3
10 34·5	57·0	3 31·9	22 1·8	1 42·6	N. 19·3	10 30·3	57·3	3 43·6	22 4·3	1 43·5	N. 19·2
11 32·7	57·2	3 22·0	22 7·7	1 37·5	N. 16·2	11 30·3	57·3	3 32·4	22 8·6	1 38·7	N. 15·9

Table XX. (Continued.)

September.

Upper Transits, P.M.						Lower (Interpolated) Transits, A.M.					
Moon's Transit.	Moon's Parallax.	Interval between the Moon's Transit and the Time of high water.	Height.	Interval of Transits.	Moon's Declination.	Moon's Transit.	Moon's Parallax.	Interval between the Moon's Transit and the Time of high water.	Height.	Interval of Transits.	Moon's Declination.
h m	′	h m	ft. in.	h m	°	h m	′	h m	ft. in.	h m	°
0 28·9	57·8	3 7·3	23 2·2	1 37·4	0·0	0 30·0	57·2	3 15·8	23 0·9	1 35·4	N. 0·8
1 32·1	57·2	2 49·7	22 8·2	1 37·4	S. 6·2	1 28·2	57·2	2 57·1	22 8·3	1 37·4	S. 6·7
2 32·5	57·3	2 35·8	22 2·8	1 42·3	S. 12·7	2 28·0	57·3	2 38·9	22 1·5	1 41·5	S. 12·0
3 33·3	57·2	2 25·5	21 1·9	1 46·1	S. 17·0	3 24·5	57·5	2 31·6	21 1·3	1 44·7	S. 16·1
4 33·8	56·6	2 15·1	20 1·0	1 47·1	S. 20·3	4 29·0	56·7	2 16·2	19 10·4	1 46·9	S. 20·4
5 33·7	57·0	2 29·0	19 2·4	1 49·8	S. 22·2	5 28·9	56·9	2 23·5	19 1·1	1 49·5	S. 22·3
6 33·8	56·5	3 13·6	19 5·2	1 47·2	S. 23·0	6 26·8	56·7	3 0·4	18 7·5	1 47·9	S. 22·9
7 34·7	56·8	3 51·3	20 5·6	1 45·0	S. 22·2	7 26·1	57·0	3 38·6	19 7·0	1 46·8	S. 22·4
8 34·5	56·8	4 4·0	21 5·4	1 41·6	S. 19·7	8 25·3	57·2	3 50·7	20 4·7	1 42·2	S. 20·1
9 34·0	57·7	3 56·8	22 1·2	1 40·6	S. 16·7	9 28·4	57·0	3 49·2	21 8·9	1 38·3	S. 17·0
10 32·8	56·9	3 45·8	22 5·9	1 34·8	S. 11·6	10 30·5	57·6	3 35·3	22 5·3	1 37·5	S. 11·2
11 31·9	57·4	3 29·5	22 11·9	1 35·0	S. 5·0	11 29·3	57·3	3 21·9	22 10·4	1 34·8	S. 6·3
Upper Transits, A.M.						Lower (Interpolated) Transits, P.M.					
0 30·5	57·3	3 12·5	22 8·9	1 35·5	S. 0·1	0 27·3	56·9	3 3·0	23 0·4	1 34·2	S. 0·2
1 31·3	56·8	2 56·1	22 4·3	1 36·7	N. 6·8	1 27·5	57·5	2 48·1	22 8·1	1 37·7	N. 6·2
2 31·4	57·0	2 40·0	21 8·7	1 40·4	N. 12·1	2 29·3	56·4	2 34·7	22 1·0	1 38·8	N. 12·3
3 28·6	56·2	2 29·8	20 10·4	1 43·4	N. 16·3	3 29·2	56·7	2 18·3	21 2·9	1 44·5	N. 17·4
4 31·7	56·5	2 20·8	19 6·9	1 46·6	N. 20·3	4 25·4	56·0	2 12·3	19 10·8	1 46·1	N. 20·0
5 34·3	56·7	2 33·8	18 9·6	1 49·3	N. 22·3	5 24·8	56·6	2 24·6	19 5·9	1 48·5	N. 22·2
6 35·6	56·6	3 13·8	18 8·0	1 47·7	N. 22·9	6 24·2	56·5	3 9·8	19 2·2	1 47·8	N. 22·7
7 32·5	56·9	3 46·3	19 6·1	1 45·7	N. 22·4	7 26·8	56·4	3 50·5	20 0·5	1 45·1	N. 22·5
8 30·7	57·0	3 57·1	20 10·2	1 42·2	N. 19·6	8 29·2	56·8	4 0·0	21 3·5	1 42·0	N. 20·0
9 31·3	57·1	3 51·7	21 8·4	1 39·1	N. 17·1	9 25·6	57·2	3 58·1	22 1·8	1 40·1	N. 16·0
10 28·1	57·6	3 41·2	22 5·8	1 37·6	N. 11·5	10 27·2	57·3	3 48·7	22 8·6	1 36·6	N. 12·7
11 26·8	57·1	3 22·8	22 10·2	1 34·5	N. 6·4	11 29·1	57·7	3 31·6	23 0·8	1 37·1	N. 5·6

October.

Upper Transits, P.M.						Lower (Interpolated) Transits, A.M.					
0 30·3	57·4	3 7·7	23 1·0	1 42·2	S. 11·9	0 27·9	57·9	3 15·3	23 0·9	1 44·1	S. 11·0
1 33·4	57·8	2 50·5	22 5·7	1 48·3	S. 16·4	1 26·0	57·7	2 56·4	22 6·2	1 46·8	S. 15·4
2 33·4	57·5	2 32·4	22 1·5	1 50·2	S. 19·8	2 30·2	57·0	2 35·3	21 7·3	1 47·9	S. 19·9
3 29·9	57·0	2 18·0	20 11·3	1 49·3	S. 21·9	3 30·5	57·0	2 21·0	20 9·1	1 49·8	S. 22·3
4 31·8	56·8	2 9·6	20 2·5	1 47·5	S. 22·8	4 28·5	56·9	2 8·3	19 11·0	1 48·3	S. 22·8
5 33·5	56·5	2 25·5	19 4·6	1 44·1	S. 22·0	5 25·8	56·7	2 12·8	19 2·1	1 45·7	S. 22·5
6 37·1	56·9	3 14·3	19 7·3	1 41·8	S. 20·5	6 27·6	57·0	2 54·9	18 11·6	1 42·4	S. 20·3
7 34·6	56·7	3 51·8	20 9·7	1 36·7	S. 17·0	7 27·8	56·4	3 29·3	19 6·4	1 36·3	S. 17·6
8 30·8	56·6	3 58·9	21 6·6	1 33·0	S. 11·7	8 28·9	56·6	3 49·3	21 0·4	1 33·5	S. 12·5
9 29·6	56·9	3 55·4	22 3·3	1 33·3	S. 6·8	9 25·1	56·6	3 45·7	21 11·1	1 34·0	S. 7·0
10 29·5	57·3	3 46·4	22 9·1	1 34·5	S. 0·3	10 28·1	57·1	3 34·7	22 7·0	1 34·5	S. 1·2
11 29·3	57·0	3 30·6	22 10·6	1 36·8	N. 5·6	11 30·6	57·8	3 19·9	22 11·8	1 39·7	N. 5·7
Upper Transits, A.M.						Lower (Interpolated) Transits, P.M.					
0 30·7	57·3	3 10·2	22 7·9	1 43·0	N. 11·8	0 30·9	57·2	3 4·2	22 11·0	1 41·9	N. 11·8
1 30·5	57·4	2 54·6	22 8·0	1 45·5	N. 15·5	1 31·0	57·0	2 46·1	22 5·5	1 45·0	N. 15·3
2 30·9	56·5	2 35·5	21 5·7	1 46·6	N. 20·2	2 26·8	56·9	2 30·4	22 0·4	1 47·3	N. 19·3
3 31·6	56·7	2 19·5	20 7·6	1 49·0	N. 22·0	3 24·2	56·8	2 17·4	21 3·4	1 48·5	N. 21·9
4 33·9	56·5	2 9·1	19 5·4	1 47·1	N. 23·1	4 29·7	56·5	2 8·5	20 1·6	1 46·8	N. 22·7
5 34·4	56·6	2 21·5	18 6·9	1 43·7	N. 22·4	5 27·2	56·4	2 26·6	19 4·3	1 44·4	N. 20·3
6 32·1	56·5	2 56·6	18 7·2	1 40·0	N. 19·7	6 28·9	56·5	3 13·6	19 2·7	1 40·5	N. 20·1
7 34·8	56·6	3 41·3	19 9·7	1 36·4	N. 17·0	7 28·6	56·7	3 47·4	20 7·5	1 37·1	N. 16·7
8 32·3	57·0	3 51·9	20 11·1	1 35·6	N. 12·2	8 28·0	57·0	4 3·1	21 7·6	1 35·6	N. 12·7
9 31·0	57·1	3 49·2	22 2·4	1 34·9	N. 6·2	9 28·9	56·9	3 56·0	22 3·4	1 32·8	N. 7·0
10 30·7	57·3	3 37·7	22 9·2	1 35·4	N. 1·0	10 28·6	58·0	3 45·7	22 11·9	1 38·9	N. 0·5
11 29·1	58·2	3 23·4	23 0·2	1 41·3	S. 5·3	11 30·3	57·1	3 29·3	22 11·6	1 37·5	S. 5·6

Table XX. (Continued.)

November.

Upper Transits, P.M.						Lower (Interpolated) Transits, A.M.					
Moon's Transit.	Moon's Parallax.	Interval between the Moon's Transit and the Time of high water.	Height.	Interval of Transits.	Moon's Declination.	Moon's Transit.	Moon's Parallax.	Interval between the Moon's Transit and the Time of high water.	Height.	Interval of Transits.	Moon's Declination.
h m	′	h m	ft. in.	h m	°	h m	′	h m	ft. in.	h m	°
0 24·5	57·8	3 2·9	22 9·7	1 52·2	S. 19·5	0 33·6	57·5	3 8·3	22 6·0	1 50·0	S. 20·3
1 22·1	57·4	2 49·1	22 3·3	1 51·7	S. 21·5	1 36·5	57·7	2 44·8	22 4·4	1 52·0	S. 22·1
2 27·8	57·3	2 32·6	21 11·1	1 49·3	S. 23·0	2 29·9	57·3	2 30·6	21 9·2	1 48·9	S. 22·7
3 34·6	57·2	2 23·5	21 4·4	1 45·5	S. 22·6	3 25·3	57·4	2 23·4	20 11·9	1 46·2	S. 22·1
4 36·5	57·0	2 21·6	20 4·5	1 39·2	S. 19·7	4 28·4	56·6	2 13·2	19 8·7	1 40·1	S. 21·0
5 30·1	56·7	2 37·4	19 10·8	1 36·2	S. 17·4	5 28·9	56·5	2 22·7	18 10·5	1 35·8	S. 17·4
6 29·0	56·4	3 7·5	19 10·9	1 32·1	S. 12·5	6 27·7	56·7	2 57·6	19 3·0	1 32·6	S. 13·0
7 30·8	56·6	3 44·8	20 8·0	1 30·7	S. 6·3	7 28·2	56·8	3 28·7	19 10·6	1 32·6	S. 6·2
8 27·8	56·9	3 55·7	21 1·7	1 33·0	N. 0·2	8 31·3	56·8	3 42·0	20 10·9	1 32·3	S. 0·8
9 29·9	56·8	3 51·8	22 2·2	1 35·7	N. 5·3	9 31·9	57·2	3 39·4	22 2·5	1 37·7	N. 6·0
10 28·7	57·2	3 39·5	22 5·7	1 42·5	N. 11·9	10 29·5	56·7	3 34·8	22 4·2	1 39·4	N. 11·4
11 27·7	56·9	3 24·9	22 7·1	1 44·2	N. 15·9	11 28·9	57·1	3 16·1	22 10·3	1 45·1	N. 16·4
Upper Transits, A.M.						Lower (Interpolated) Transits, P.M.					
0 28·8	57·1	3 4·0	22 6·0	1 49·2	N. 20·1	0 26·3	57·4	3 4·7	22 6·4	1 48·6	N. 19·5
1 31·3	57·3	2 48·1	22 0·0	1 50·6	N. 22·3	1 25·0	57·0	2 49·0	22 4·8	1 48·6	N. 21·7
2 32·7	57·3	2 31·0	21 7·9	1 48·7	N. 22·7	2 26·9	56·9	2 28·9	22 1·6	1 48·6	N. 23·6
3 31·8	57·3	2 16·8	20 10·6	1 43·8	N. 21·7	3 26·8	57·1	2 21·7	21 2·2	1 45·3	N. 22·2
4 29·1	57·0	2 12·3	19 5·0	1 40·5	N. 20·6	4 27·5	56·7	2 23·8	20 3·7	1 40·1	N. 20·0
5 29·2	56·4	2 21·6	19 0·0	1 35·9	N. 17·9	5 27·3	56·7	2 32·7	20 5·2	1 36·5	N. 17·3
6 31·2	56·6	3 0·0	18 10·6	1 32·3	N. 12·0	6 29·6	56·8	3 10·5	19 9·3	1 33·0	N. 12·2
7 30·0	56·6	3 31·9	20 2·1	1 31·7	N. 6·1	7 30·7	56·9	3 39·1	20 10·3	1 32·5	N. 5·7
8 28·2	56·8	3 42·4	21 0·3	1 32·8	N. 0·9	8 29·2	57·1	3 52·1	21 7·0	1 34·6	N. 0·8
9 26·7	57·6	3 41·6	22 0·6	1 38·6	S. 5·6	9 30·2	57·1	3 50·2	22 1·8	1 36·8	S. 6·6
10 28·8	57·0	3 32·3	22 6·3	1 40·4	S. 11·1	10 30·5	57·6	3 36·2	22 8·7	1 44·0	S. 11·8
11 30·7	57·7	3 17·5	22 10·1	1 49·3	S. 16·6	11 27·7	57·5	3 24·8	22 11·2	1 47·0	S. 15·8

December.

Upper Transits, P.M.						Lower (Interpolated) Transits, A.M.					
0 35·1	57·1	2 56·7	22 9·0	1 47·6	S. 22·5	0 25·2	57·5	3 4·1	22 6·3	1 49·5	S. 22·6
1 28·1	57·6	2 46·1	22 4·6	1 46·6	S. 22·5	1 30·4	57·0	2 44·1	22 1·7	1 45·8	S. 22·8
2 25·3	57·4	2 32·7	21 9·9	1 41·9	S. 20·1	2 34·4	57·3	2 26·1	21 10·1	1 39·6	S. 19·5
3 28·0	57·0	2 29·0	21 6·0	1 35·0	S. 16·9	3 32·2	57·3	2 23·5	21 0·6	1 36·4	S. 16·3
4 29·0	57·3	2 32·9	20 8·8	1 32·5	S. 11·0	4 30·8	56·8	2 21·6	20 5·3	1 32·1	S. 11·3
5 30·1	56·8	2 43·8	20 5·3	1 30·2	S. 5·1	5 31·2	56·9	2 31·1	19 11·3	1 31·1	S. 6·9
6 29·6	56·7	3 16·0	20 0·5	1 32·0	N. 0·3	6 27·6	56·7	2 56·7	19 6·3	1 31·1	S. 0·5
7 27·7	56·6	3 35·8	20 7·6	1 34·3	N. 6·0	7 27·6	56·3	3 24·6	20 5·7	1 34·1	N. 7·2
8 28·5	56·6	3 49·6	21 5·2	1 40·2	N. 12·3	8 28·9	56·9	3 41·5	21 1·8	1 39·8	N. 11·8
9 31·2	56·7	3 50·1	22 0·6	1 44·5	N. 17·2	9 26·3	57·1	3 42·0	21 10·4	1 45·3	N. 16·6
10 29·2	57·1	3 31·5	21 11·8	1 49·1	N. 20·3	10 27·2	56·7	3 33·4	22 2·7	1 46·0	N. 19·7
11 25·6	57·0	3 24·1	22 6·0	1 49·1	N. 22·0	11 28·9	57·1	3 21·7	22 6·5	1 50·6	N. 22·6
Upper Transits, A.M.						Lower (Interpolated) Transits, P.M.					
0 27·0	57·1	3 7·3	22 3·0	1 48·0	N. 22·4	0 29·6	57·1	3 5·5	22 5·8	1 47·7	N. 23·0
1 31·3	57·0	2 48·5	21 10·2	1 45·0	N. 22·9	1 32·0	57·2	2 45·3	22 2·0	1 44·4	N. 22·6
2 30·9	57·7	2 33·1	21 7·6	1 40·0	N. 19·8	2 33·6	57·3	2 33·8	22 1·7	1 40·2	N. 19·7
3 30·3	57·0	2 26·4	20 9·2	1 35·9	N. 15·6	3 32·5	57·1	2 30·7	21 2·2	1 34·9	N. 16·0
4 27·0	57·3	2 21·7	20 1·8	1 33·3	N. 11·6	4 30·5	56·9	2 28·9	20 4·5	1 31·7	N. 11·5
5 29·0	56·7	2 32·3	19 5·2	1 30·0	N. 6·3	5 29·0	57·2	2 43·4	20 3·3	1 32·6	N. 5·6
6 31·0	57·0	2 59·6	19 11·4	1 33·7	S. 0·4	6 29·1	56·4	3 9·6	20 0·3	1 30·9	S. 0·7
7 30·7	56·8	3 29·3	20 3·1	1 35·5	S. 6·7	7 26·7	57·1	3 37·3	20 5·1	1 36·9	S. 6·1
8 31·9	57·0	3 41·6	21 4·4	1 41·8	S. 12·4	8 29·8	57·1	3 45·4	21 3·0	1 42·5	S. 12·7
9 28·1	57·7	3 42·6	21 9·1	1 46·7	S. 16·5	9 33·3	57·0	3 43·7	21 10·3	1 45·7	S. 17·1
10 27·1	57·2	3 35·1	22 4·8	1 49·0	S. 19·5	10 30·7	57·2	3 34·0	22 5·7	1 49·1	S. 20·4
11 31·9	57·2	3 19·5	21 11·7	1 49·9	S. 22·9	11 25·6	57·6	3 19·2	22 2·4	1 51·7	S. 22·4

Table XXI.

Showing the Difference in the Interval between the Apparent Solar Time of the Moon's Transit and the Time of High Water, and the Mean Interval and the Difference between the Height of High Water and the Mean Height.

Upper Transits, P.M.									
Moon's Transit, P.M.	January.			February.			March.		
	Diurnal Inequality.		Moon's Declination.	Diurnal Inequality.		Moon's Declination.	Diurnal Inequality.		Moon's Declination.
	Interval.	Height of High Water.		Interval.	Height of High Water.		Interval.	Height of High Water.	
h m	m	feet.	°	m	feet.	°	m	feet.	°
0 30	+ 2·3	·00	S. 19·1	− 2·4	− ·14	S. 10·4	+ 1·8	− ·15	N. 2·1
1 30	− 0·9	+ ·12	S. 15·1	+ 3·7	− ·40	S. 5·0	+ 1·1	− ·23	N. 7·6
2 30	+ 1·8	+ ·09	S. 9·9	+ 2·6	− ·24	N. 2·3	+ 8·2	− ·23	N. 13·4
3 30	0·0	+ ·05	S. 4·3	− 0·4	− ·26	N. 8·4	+ 1·4	− ·30	N. 17·0
4 30	+ 8·0	− ·05	N. 1·4	+ 4·5	− ·28	N. 14·3	+ 0·8	− ·38	N. 20·8
5 30	+ 2·7	− ·01	N. 8·7	+ 11·9	− ·29	N. 17·9	− 1·1	− ·22	N. 22·2
6 30	+ 4·8	− ·14	N. 13·9	+ 6·0	− ·02	N. 20·7	+ 1·9	+ ·07	N. 23·0
7 30	0·0	+ ·07	N. 17·5	+ 0·2	− ·02	N. 22·8	+ 2·6	− ·23	N. 22·3
8 30	+ 3·7	+ ·03	N. 20·5	+ 3·9	− ·08	N. 22·6	− 1·1	− ·19	N. 18·6
9 30	+ 1·8	− ·34	N. 22·9	+ 1·0	− ·02	N. 21·9	− 2·7	− ·03	N. 15·9
10 30	+ 1·7	− ·35	N. 23·1	+ 0·6	− ·15	N. 18·4	− 2·8	− ·03	N. 11·4
11 30	+ 2·5	− ·25	N. 21·8	+ 0·5	− ·13	N. 15·3	− 1·9	+ ·10	N. 4·7
	April.			May.			June.		
0 30	+ 0·9	− ·22	N. 12·7	+ 0·1	− ·12	N. 20·5	− 1·5	− ·11	N. 22·9
1 30	+ 3·4	− ·22	N. 17·0	+ 1·7	− ·17	N. 22·0	+ 0·2	− ·21	N. 21·9
2 30	− 0·8	− ·24	N. 20·0	− 4·1	− ·16	N. 23·3	− 1·5	− ·11	N. 19·4
3 30	− 1·9	− ·14	N. 22·2	− 4·6	− ·60	N. 22·0	− 4·1	− ·07	N. 16·0
4 30	− 0·8	− ·41	N. 22·4	− 3·3	− ·26	N. 20·0	− 4·3	− ·21	N. 11·2
5 30	− 0·7	− ·44	N. 22·9	− 7·3	− ·39	N. 16·6	− 5·1	− ·04	N. 5·8
6 30	+ 2·3	− ·43	N. 19·4	− 3·7	− ·21	N. 11·9	− 3·7	− ·21	S. 0·3
7 30	+ 2·1	− ·40	N. 16·8	− 2·8	− ·16	N. 5·4	− 3·9	− ·12	S. 6·6
8 30	− 0·6	− ·38	N. 11·1	− 1·8	− ·03	S. 0·5	− 1·4	+ ·12	S. 12·4
9 30	− 0·6	− ·08	N. 5·9	− 2·8	+ ·06	S. 6·8	− 0·1	− ·02	S. 17·6
10 30	− 2·0	− ·13	S. 0·9	− 8·8	+ ·13	S. 11·4	+ 0·5	+ ·04	S. 20·3
11 30	− 1·4	+ ·12	S. 7·7	− 1·5	+ ·01	S. 16·3	+ 1·3	+ ·05	S. 22·2
	July.			August.			September.		
0 30	− 3·4	− ·06	N. 19·5	− 3·7	− ·05	N. 10·5	− 2·2	+ ·16	0·0
1 30	− 4·6	− ·06	N. 16·3	− 1·1	+ ·07	N. 4·9	− 2·8	+ ·08	S. 6·2
2 30	− 2·9	·00	N. 10·4	− 1·7	+ ·13	S. 1·1	− 0·9	+ ·19	S. 12·7
3 30	− 3·6	+ ·05	N. 5·2	− 3·8	+ ·16	S. 7·1	− 0·4	+ ·15	S. 17·0
4 30	− 2·6	·00	S. 1·8	− 0·9	+ ·26	S. 13·3	− 1·0	+ ·28	S. 20·3
5 30	− 2·5	+ ·12	S. 7·6	− 1·2	+ ·12	S. 18·1	0·0	+ ·07	S. 22·2
6 30	− 4·2	+ ·03	S. 13·2	+ 0·1	+ ·04	S. 20·6	+ 1·7	+ ·43	S. 23·0
7 30	− 1·4	+ ·17	S. 16·9	+ 2·4	+ ·23	S. 22·7	+ 2·5	+ ·47	S. 22·2
8 30	+ 1·5	+ ·07	S. 19·8	+ 3·9	+ ·17	S. 23·2	+ 5·7	+ ·37	S. 19·7
9 30	+ 1·8	+ ·02	S. 22·6	+ 3·2	+ ·27	S. 21·6	+ 3·7	+ ·13	S. 16·7
10 30	+ 1·9	− ·04	S. 23·0	+ 6·3	+ ·24	S. 19·1	+ 3·4	− ·07	S. 11·6
11 30	+ 5·6	+ ·06	S. 21·7	+ 4·4	+ ·15	S. 16·0	+ 3·6	+ ·04	S. 5·0
	October.			November.			December.		
0 30	− 1·2	+ ·15	S. 11·9	− 4·0	+ ·19	S. 19·5	− 5·2	+ ·24	S. 22·5
1 30	− 0·9	− ·02	S. 16·4	+ 0·4	− ·03	S. 21·5	− 0·5	+ ·25	S. 22·5
2 30	− 0·2	+ ·37	S. 19·8	+ 1·2	+ ·03	S. 23·0	+ 0·4	− ·09	S. 20·1
3 30	− 0·7	+ ·04	S. 21·9	+ 2·6	+ ·34	S. 22·6	+ 1·3	+ ·33	S. 16·9
4 30	− 0·5	+ ·30	S. 22·8	+ 3·5	+ ·40	S. 19·7	+ 6·6	+ ·30	S. 11·0
5 30	+ 2·3	+ ·28	S. 22·0	+ 8·6	+ ·29	S. 17·2	+ 6·7	+ ·46	S. 5·1
6 30	+ 5·5	+ ·47	S. 20·5	+ 3·9	+ ·43	S. 12·5	+ 9·7	+ ·13	N. 0·3
7 30	+ 8·6	+ ·59	S. 17·0	+ 8·6	+ ·31	S. 6·3	+ 4·8	+ ·19	N. 6·0
8 30	+ 3·4	+ ·23	S. 11·7	+ 8·2	+ ·01	N. 0·2	+ 5·0	+ ·15	N. 12·3
9 30	+ 3·8	+ ·08	S. 6·8	+ 5·3	+ ·03	N. 5·3	+ 5·4	+ ·15	N. 17·2
10 30	+ 6·0	− ·01	S. 0·3	+ 4·0	− ·03	N. 11·9	+ 2·2	− ·30	N. 20·3
11 30	+ 4·3	− ·08	N. 5·6	+ 3·4	− ·21	N. 15·9	+ 2·2	+ ·21	N. 22·0

Table XXI. (Continued.)

Upper Transits, A.M.

Moon's Transit, A.M.	January. Diurnal Inequality. Interval.	January. Diurnal Inequality. Height of High Water.	January. Moon's Declination.	February. Diurnal Inequality. Interval.	February. Diurnal Inequality. Height of High Water.	February. Moon's Declination.	March. Diurnal Inequality. Interval.	March. Diurnal Inequality. Height of High Water.	March. Moon's Declination.
h m	m	feet.	°	m	feet.	°	m	feet.	°
0 30	− 2·7	− ·10	N. 18·8	− 2·1	+ ·09	N. 10·7	− 2·4	+ ·36	S. 1·7
1 30	0·0	− ·14	N. 15·0	− 2·2	+ ·43	N. 4·6	+ 1·9	+ ·13	S. 6·7
2 30	− 0·1	− ·29	N. 10·9	− 1·7	− ·11	S. 1·9	+ 1·1	+ ·01	S. 13·4
3 30	− 0·8	− ·26	N. 3·4	− 0·6	+ ·23	S. 7·7	− 0·5	+ ·06	S. 17·3
4 30	− 2·7	·00	S. 2·4	− 1·5	+ ·12	S. 13·8	+ 0·3	+ ·31	S. 20·4
5 30	− 3·2	− ·17	S. 8·2	− 3·6	+ ·05	S. 18·0	+ 5·7	+ ·13	S. 22·1
6 30	− 3·0	− ·15	S. 12·7	+ 1·3	+ ·14	S. 20·8	+ 0·7	+ ·03	S. 22·8
7 30	− 0·7	− ·09	S. 17·9	− 3·3	+ ·16	S. 22·2	+ 0·2	+ ·18	S. 22·8
8 30	+ 2·6	+ ·14	S. 21·1	− 3·9	+ ·16	S. 23·2	+ 3·9	+ ·05	S. 19·1
9 30	− 0·7	+ ·13	S. 23·0	− 3·2	− ·07	S. 21·8	− 1·5	− ·02	S. 16·8
10 30	− 0·4	+ ·33	S. 22·5	+ 1·0	+ ·20	S. 19·0	+ 1·9	− ·08	S. 11·2
11 30	+ 2·1	+ ·18	S. 21·6	+ 3·3	− ·12	S. 14·4	+ 2·5	·00	S. 4·6
	April.			May.			June.		
0 30	+ 0·2	+ ·15	S. 12·1	− ·30	+ ·19	S. 20·5	+ 0·6	+ ·13	S. 23·0
1 30	+ 1·0	+ ·07	S. 16·4	+ ·01	+ ·17	S. 22·4	+ 2·3	+ ·12	S. 21·9
2 30	+ 1·0	+ ·17	S. 20·8	+ ·21	+ ·24	S. 22·7	+ 4·2	+ ·12	S. 19·4
3 30	+ 6·6	+ ·15	S. 21·7	+ ·34	+ ·30	S. 22·7	+ 3·5	+ ·26	S. 15·8
4 30	+ 5·2	+ ·55	S. 22·9	+ ·55	+ ·23	S. 20·0	+ 6·1	+ ·02	S. 11·1
5 30	+ 1·9	+ ·47	S. 22·8	+ ·78	+ ·30	S. 16·4	+ 5·1	− ·03	S. 7·0
6 30	+ 1·4	+ ·44	S. 20·5	+ ·40	+ ·24	S. 12·6	+ 8·4	+ ·19	N. 1·1
7 30	+ 2·6	+ ·36	S. 16·1	+ ·40	+ ·26	S. 5·9	+ 6·0	+ ·02	N. 6·6
8 30	+ 0·3	+ ·29	S. 11·9	+ ·43	+ ·18	N. 0·4	+ 1·8	+ ·14	N. 12·7
9 30	+ 2·9	+ ·03	S. 5·1	+ ·47	+ ·21	N. 6·4	+ 0·1	− ·05	N. 16·6
10 30	+ 1·8	+ ·19	N. 1·1	+ ·07	+ ·23	N. 11·7	− 2·3	− ·01	N. 19·8
11 30	− 3·1	− ·17	N. 6·6	+ ·12	− ·04	N. 16·7	− 8·9	− ·08	N. 22·8
	July.			August.			September.		
0 30	+ 3·3	+ ·08	S. 19·0	+ 3·7	− ·01	S. 11·3	+ 3·4	− ·28	S. 0·1
1 30	+ 5·9	− ·02	S. 16·4	+ 3·1	− ·17	S. 5·3	+ 3·5	− ·25	N. 6·8
2 30	+ 3·9	− ·21	S. 11·3	+ 0·3	− ·23	N. 0·2	+ 3·1	− ·33	N. 12·1
3 30	+ 4·7	+ ·06	S. 4·5	+ 4·0	− ·17	N. 7·4	+ 3·1	− ·23	N. 16·3
4 30	+ 5·7	− ·16	N. 1·6	+ 4·7	− ·11	N. 12·3	+ 4·5	− ·26	N. 20·0
5 30	+ 6·1	− ·11	N. 7·4	+ 3·5	− ·12	N. 17·2	+ 4·5	− ·32	N. 22·3
6 30	+ 8·6	− ·04	N. 12·0	+ 2·4	− ·11	N. 21·0	+ 1·0	− ·35	N. 22·9
7 30	− 0·2	+ ·07	N. 16·0	− 2·3	− ·24	N. 22·5	− 1·6	− ·45	N. 22·4
8 30	− 0·1	− ·10	N. 20·6	− 1·0	− ·24	N. 22·4	− 0·9	− ·16	N. 19·6
9 30	+ 0·7	− ·06	N. 22·6	− 2·2	− ·36	N. 21·9	− 1·7	− ·24	N. 17·1
10 30	− 1·6	− ·01	N. 23·0	− 6·6	− ·14	N. 19·3	− 2·2	− ·04	N. 11·5
11 30	− 1·7	− ·07	N. 21·3	− 3·4	− ·10	N. 16·2	− 4·5	− ·07	N. 6·4
	October.			November.			December.		
0 30	+ 1·4	− ·27	N. 11·8	− 1·0	− ·10	N. 20·1	+ 3·0	− ·28	N. 22·4
1 30	+ 2·3	+ ·15	N. 15·5	+ 0·7	− ·24	N. 22·3	+ 2·8	− ·26	N. 22·9
2 30	+ 2·1	− ·32	N. 20·2	+ 0·7	− ·19	N. 22·7	+ 1·6	− ·23	N. 19·8
3 30	+ 1·1	− ·24	N. 22·0	− 4·4	− ·18	N. 21·7	− 1·2	− ·34	N. 15·6
4 30	+ 1·1	− ·42	N. 23·1	− 5·4	− ·55	N. 20·6	− 4·5	− ·30	N. 11·6
5 30	− 2·1	− ·53	N. 22·4	− 6·9	− ·53	N. 17·9	− 4·5	− ·57	N. 6·3
6 30	− 8·9	− ·49	N. 19·7	− 4·9	− ·61	N. 12·0	− 7·4	+ ·04	S. 0·4
7 30	− 2·0	− ·44	N. 17·0	− 4·5	− ·18	N. 6·1	− 2·5	− ·22	S. 6·7
8 30	− 3·7	− ·42	N. 12·2	− 5·2	− ·11	N. 0·9	− 3·5	+ ·09	S. 12·4
9 30	− 2·3	+ ·01	N. 6·2	− 5·2	− ·06	S. 5·6	− 2·4	− ·12	S. 16·5
10 30	− 2·2	− ·01	N. 1·0	− 3·1	+ ·02	S. 11·1	+ 1·2	+ ·13	S. 19·5
11 30	− 2·9	+ ·06	S. 5·3	− 3·2	− ·04	S. 16·6	− 0·8	− ·33	S. 22·9

Table XXI. (Continued.)

Lower (Interpolated) Transits, A.M.

Moon's Transit, A.M.	January.			February.			March.		
	Diurnal Inequality.		Moon's Declination.	Diurnal Inequality.		Moon's Declination.	Diurnal Inequality.		Moon's Declination.
	Interval.	Height of High Water.		Interval.	Height of High Water.		Interval.	Height of High Water.	
h m	m	feet.	°	m	feet.	°	m	feet.	°
0 30	− 0·4	+ ·02	S. 19·2	+ 2·2	− ·10	S. 10·1	− 1·6	− ·21	N. 0·9
1 30	− 0·1	− ·08	S. 16·2	− 2·8	− ·12	S. 3·6	− 1·8	+ ·12	N. 6·7
2 30	− 5·8	+ ·05	S. 10·8	− 2·7	+ ·13	N. 1·3	− 3·7	+ ·12	N. 12·9
3 30	− 0·5	+ ·22	S. 3·3	+ 0·8	− ·04	N. 7·7	− 0·5	+ ·26	N. 17·5
4 30	− 7·3	− ·07	N. 2·5	− 4·4	− ·17	N. 13·0	− 2·1	+ ·23	N. 20·8
5 30	− 9·6	− ·14	N. 7·6	− 5·4	− ·20	N. 19·0	− 5·8	+ ·49	N. 21·6
6 30	− 9·1	+ ·03	N. 13·1	− 8·6	− ·04	N. 20·8	− 2·8	+ ·14	N. 23·4
7 30	− 3·6	− ·12	N. 17·5	− 1·6	− ·28	N. 22·7	− 1·2	+ ·44	N. 21·7
8 30	− 7·8	− ·09	N. 21·4	− 2·5	+ ·23	N. 21·9	− 4·8	+ ·45	N. 19·9
9 30	− 2·9	+ ·39	N. 22·3	− 5·7	+ ·06	N. 21·9	+ 1·9	+ ·35	N. 15·2
10 30	− 1·4	+ ·27	N. 22·4	+ 1·1	+ ·20	N. 18·7	0·0	+ ·29	N. 10·2
11 30	− 1·6	+ ·29	N. 20·9	+ 1·5	+ ·34	N. 15·0	− 0·1	+ ·08	N. 4·7
	April.			May.			June.		
0 30	− 1·6	− ·01	N. 12·4	+ 0·8	+ ·06	N. 19·6	+ 2·7	+ ·08	N. 22·7
1 30	− 1·4	+ ·08	N. 16·3	0·0	+ ·19	N. 21·7	+ 1·3	+ ·11	N. 22·0
2 30	+ 0·2	+ ·18	N. 20·5	+ 2·7	+ ·18	N. 22·2	+ 4·4	+ ·14	N. 19·9
3 30	+ 1·3	+ ·13	N. 22·0	+ 2·1	+ ·35	N. 21·9	+ 1·7	+ ·29	N. 16·2
4 30	+ 0·3	+ ·59	N. 22·8	− 0·9	+ ·11	N. 20·4	+ 5·1	+ ·30	N. 11·6
5 30	+ 3·9	+ ·25	N. 21·9	+ 3·5	+ ·42	N. 16·7	+ 8·9	+ ·20	N. 5·7
6 30	+ 1·7	+ ·32	N. 20·3	+ 7·0	+ ·52	N. 11·4	+ 3·8	+ ·22	S. 0·3
7 30	+ 1·3	+ ·30	N. 15·8	+ 2·4	+ ·14	N. 6·5	+ 0·8	+ ·18	S. 6·7
8 30	− 4·8	+ ·39	N. 11·1	− 0·1	+ ·10	S. 0·5	− 0·3	+ ·05	S. 12·1
9 30	− 1·2	+ ·24	N. 6·0	− 1·3	+ ·18	S. 6·0	− 0·6	+ ·06	S. 16·4
10 30	+ 1·3	+ ·18	S. 0·3	− 0·8	+ ·08	S. 12·3	− 4·5	− ·14	S. 20·0
11 30	+ 0·3	+ ·04	S. 6·4	+ 0·7	+ ·07	S. 16·5	− 2·8	+ ·02	S. 22·3
	July.			August.			September.		
0 30	+ 3·2	+ ·14	N. 19·3	+ 3·2	− ·05	N. 11·1	+ 6·6	+ ·06	N. 0·8
1 30	+ 4·6	+ ·09	N. 16·1	+ 3·3	+ ·03	N. 5·6	+ 3·8	+ ·05	S. 6·7
2 30	+ 2·0	+ ·20	N. 11·2	+ 4·8	− ·04	S. 1·5	+ 1·3	+ ·03	S. 12·0
3 30	+ 5·5	+ ·08	N. 5·2	+ 5·4	− ·07	S. 7·1	+ 4·2	− ·06	S. 16·1
4 30	+ 3·7	+ ·15	S. 2·1	+ 4·1	− ·16	S. 12·9	+ 0·1	− ·01	S. 20·4
5 30	+ 4·8	+ ·13	S. 6·8	+ 6·8	·00	S. 17·2	− 3·4	+ ·04	S. 22·3
6 30	+ 3·0	+ ·06	S. 12·5	− 0·6	− ·05	S. 21·2	− 6·9	− ·34	S. 22·9
7 30	+ 1·8	+ ·16	S. 16·7	− 1·4	− ·25	S. 22·3	− 6·7	− ·28	S. 22·4
8 30	− 1·3	− ·08	S. 20·4	− 5·3	− ·23	S. 22·6	− 7·0	− ·53	S. 20·1
9 30	− 3·1	− ·05	S. 22·9	− 4·6	− ·15	S. 21·9	− 4·6	− ·16	S. 17·0
10 30	− 3·2	− ·19	S. 22·9	− 3·4	− ·22	S. 20·0	− 7·6	− ·10	S. 11·2
11 30	− 5·0	− ·09	S. 21·8	− 6·4	+ ·07	S. 15·6	− 4·7	− ·07	S. 6·3
	October.			November.			December.		
0 30	+ 5·7	+ ·14	S. 11·0	+ 3·9	− ·07	S. 20·3	− 0·7	·00	S. 22·6
1 30	+ 2·8	− ·05	S. 15·4	− 1·9	+ ·16	S. 22·1	− 1·9	+ ·02	S. 22·8
2 30	+ 1·8	− ·19	S. 19·9	− 0·4	− ·10	S. 22·7	− 4·8	·00	S. 19·5
3 30	+ 2·4	− ·13	S. 22·3	+ 1·5	− ·17	S. 22·1	− 4·0	− ·07	S. 16·3
4 30	− 1·8	− ·04	S. 22·8	− 4·4	− ·34	S. 21·0	− 4·9	+ ·03	S. 11·3
5 30	− 6·9	+ ·02	S. 22·5	− 5·7	− ·63	S. 17·4	− 6·4	− ·04	S. 6·9
6 30	− 7·6	− ·10	S. 20·3	− 5·3	− ·23	S. 13·0	− 8·7	− ·38	S. 0·5
7 30	− 11·1	− ·56	S. 17·6	− 7·1	− ·44	S. 6·2	− 6·4	+ ·04	N. 7·2
8 30	− 6·0	− ·25	S. 12·5	− 5·8	− ·27	S. 0·8	− 3·1	− ·13	N. 11·8
9 30	− 6·5	− ·22	S. 7·0	− 6·9	+ ·03	N. 6·0	− 3·1	− ·01	N. 16·6
10 30	− 6·0	− ·17	S. 1·2	− 0·5	− ·16	N. 11·4	− 0·5	− ·05	N. 19·7
11 30	− 6·0	+ ·02	N. 5·7	− 5·1	+ ·06	N. 16·4	+ 0·6	+ ·24	N. 22·6

Table XXI. (Continued.)

Lower (Interpolated) Transits, P.M.

Moon's Transit, P.M.	January.			February.			March.		
	Diurnal Inequality.		Moon's Declination.	Diurnal Inequality.		Moon's Declination.	Diurnal Inequality.		Moon's Declination.
	Interval.	Height of High Water.		Interval.	Height of High Water.		Interval.	Height of High Water.	
h m	m	feet.	°	m	feet.	°	m	feet.	°
0 30	− 1·9	+ ·07	N. 20·0	+ 2·1	+ ·32	N. 9·5	+ 3·0	+ ·01	S. 0·8
1 30	+ 0·8	+ ·16	N. 15·7	+ 1·3	+ ·14	N. 4·4	− 0·7	+ ·02	S. 7·5
2 30	+ 3·4	+ ·15	N. 8·8	+ 7·2	+ ·20	S. 1·9	+ 0·7	− ·04	S. 13·3
3 30	+ 3·4	− ·06	N. 3·9	0·0	+ ·06	S. 7·8	+ 0·4	− ·14	S. 17·1
4 30	+ 3·9	+ ·15	S. 2·4	+ 2·8	+ ·58	S. 14·0	+ 3·2	− ·12	S. 20·4
5 30	+ 8·3	+ ·36	S. 8·5	− 0·9	+ ·35	S. 18·1	+ 2·6	− ·49	S. 22·8
6 30	+ 7·0	+ ·03	S. 13·4	− 5·4	− ·08	S. 20·6	+ 0·5	− ·23	S. 23·0
7 30	+ 4·9	+ ·18	S. 18·0	+ 5·7	+ ·10	S. 22·6	+ 2·6	− ·43	S. 21·7
8 30	+ 8·5	− ·10	S. 20·5	− 2·1	− ·33	S. 23·2	+ 2·0	− ·28	S. 19·7
9 30	+ 0·3	− ·07	S. 22·2	− 1·5	− ·14	S. 21·6	+ 1·0	− ·33	S. 15·7
10 30	+ 0·4	− ·32	S. 23·3	− 3·2	− ·32	S. 18·6	+ 1·1	− ·16	S. 10·0
11 30	+ 2·1	− ·29	S. 20·5	− 6·2	− ·23	S. 14·5	− 0·6	− ·20	S. 4·7
	April.			May.			June.		
0 30	+ 0·2	+ ·05	S. 13·3	− 0·9	− ·12	S. 19·4	− 2·3	− ·11	S. 22·8
1 30	− 3·2	+ ·08	S. 16·9	− 1·2	− ·12	S. 22·0	− 4·6	− ·07	S. 21·8
2 30	− 0·3	− ·05	S. 19·6	− 1·8	− ·29	S. 22·9	− 4·5	− ·23	S. 20·0
3 30	− 1·0	− ·12	S. 22·1	− 1·1	− ·04	S. 20·9	− 3·2	+ ·02	S. 16·5
4 30	− 2·9	+ ·12	S. 23·2	− 2·7	− ·21	S. 19·7	− 4·5	− ·18	S. 10·8
5 30	+ 1·6	− ·36	S. 21·6	− 5·2	− ·39	S. 17·2	− 8·3	− ·10	S. 5·7
6 30	− 4·9	− ·23	S. 19·9	− 4·1	− ·47	S. 11·8	− 6·7	− ·14	N. 0·6
7 30	− 5·2	− ·32	S. 15·8	− 4·6	− ·27	S. 5·2	− 3·9	− ·15	N. 6·6
8 30	− 3·4	− ·32	S. 11·2	− 1·4	− ·35	0·0	+ 0·3	− ·10	N. 12·0
9 30	− 1·2	− ·24	S. 4·9	− 1·6	− ·04	N. 6·5	+ 0·8	− ·02	N. 17·0
10 30	− 0·3	− ·13	N. 0·9	− 9·0	− ·10	N. 11·8	+ 1·2	+ ·12	N. 20·3
11 30	− 3·1	+ ·06	N. 6·4	+ 0·1	− ·09	N. 16·9	+ 1·4	·00	N. 21·5
	July.			August.			September.		
0 30	− 3·1	− ·14	S. 20·0	− 5·3	+ ·05	S. 11·4	− 7·0	·00	S. 0·2
1 30	− 4·8	− ·02	S. 15·0	− 4·5	+ ·02	S. 4·8	− 5·5	+ ·04	N. 6·2
2 30	− 3·4	+ ·03	S. 11·1	− 5·0	+ ·14	N. 1·1	− 2·7	+ ·01	N. 12·3
3 30	− 2·6	− ·07	S. 4·6	− 5·3	+ ·05	N. 7·9	− 8·3	+ ·15	N. 17·4
4 30	− 8·5	+ ·10	N. 1·3	− 6·7	− ·03	N. 12·2	− 3·8	− ·03	N. 20·0
5 30	− 8·3	− ·18	N. 7·9	− 6·3	+ ·05	N. 17·7	− 0·5	+ ·30	N. 22·2
6 30	− 5·0	− ·05	N. 12·9	− 1·5	+ ·16	N. 20·8	+ 4·2	+ ·24	N. 22·7
7 30	+ 0·2	− ·28	N. 17·2	+ 1·9	+ ·21	N. 22·2	+ 4·9	+ ·17	N. 22·5
8 30	− 0·2	+ ·11	N. 20·1	+ 2·7	+ ·25	N. 22·7	+ 2·0	+ ·31	N. 20·0
9 30	+ 1·0	+ ·06	N. 22·3	+ 4·4	+ ·14	N. 22·3	+ 4·1	+ ·28	N. 16·0
10 30	+ 2·3	+ ·23	N. 23·0	+ 4·2	+ ·41	N. 19·2	+ 5·1	+ ·20	N. 12·7
11 30	+ 1·0	+ ·12	N. 22·5	+ 6·5	·00	N. 15·9	+ 4·9	+ ·13	N. 5·6
	October.			November.			December.		
0 30	− 4·6	− ·01	N. 11·8	− 1·7	− ·08	N. 19·5	+ 2·0	− ·04	N. 23·0
1 30	− 6·0	− ·05	N. 15·3	+ 0·7	+ ·12	N. 21·7	− 0·3	+ ·06	N. 22·6
2 30	− 4·1	+ ·20	N. 19·3	− 2·6	+ ·23	N. 23·6	+ 2·7	+ ·30	N. 19·7
3 30	− 2·4	+ ·30	N. 21·9	− 0·1	+ ·04	N. 22·2	+ 3·2	+ ·08	N. 16·0
4 30	− 1·6	+ ·19	N. 22·7	+ 6·3	+ ·22	N. 20·1	+ 2·5	− ·04	N. 11·5
5 30	+ 6·3	+ ·21	N. 20·3	+ 4·9	+ ·81	N. 17·3	+ 6·6	+ ·28	N. 5·6
6 30	+10·2	+ ·15	N. 20·1	+ 7·0	+ ·32	N. 12·2	+ 3·5	+ ·11	S. 0·7
7 30	+ 6·7	+ ·52	N. 16·7	+ 6·3	+ ·46	N. 5·7	+ 6·5	·00	S. 6·1
8 30	+ 7·9	+ ·37	N. 12·7	+ 4·6	+ ·43	N. 0·8	+ 0·6	− ·05	S. 12·7
9 30	+ 4·3	+ ·10	N. 7·0	+ 3·7	·00	S. 6·6	− 0·8	− ·05	S. 17·1
10 30	+ 5·1	+ ·23	N. 0·5	+ 1·1	+ ·21	S. 11·8	+ 0·5	+ ·19	S. 20·4
11 30	+ 3·3	+ ·01	S. 5·6	+ 3·3	+ ·13	S. 15·8	− 2·7	− ·09	S. 22·4

Table XXII.

Showing the Interval between the Moon's Transit in the first column and the fourth succeeding for each month of the year.

Moon's Transit.	January.	February.	March.	April.	May.	June.	July.	August.	Sept.	October.	Nov.	Dec.	Mean.
h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m
0 30	1 40·1	1 34·8	1 35·2	1 43·9	1 50·6	1 45·1	1 41·6	1 34·7	1 35·7	1 42·7	1 49·9	1 48·2	1 41·9
1 30	1 36·0	1 33·4	1 37·8	1 47·2	1 51·5	1 45·6	1 36·2	1 32·2	1 37·3	1 46·4	1 50·7	1 45·4	1 41·6
2 30	1 32·1	1 34·0	1 40·9	1 48·4	1 48·6	1 40·9	1 33·2	1 33·5	1 40·7	1 48·0	1 48·9	1 40·5	1 40·8
3 30	1 30·5	1 36·5	1 45·1	1 49·3	1 45·5	1 34·8	1 31·9	1 36·0	1 44·7	1 49·1	1 45·2	1 35·5	1 40·3
4 30	1 31·8	1 40·0	1 46·9	1 46·8	1 40·2	1 32·8	1 32·5	1 40·1	1 46·6	1 47·4	1 40·0	1 32·4	1 39·8
5 30	1 36·3	1 45·3	1 48·2	1 44·1	1 35·6	1 31·1	1 36·4	1 45·1	1 49·3	1 44·5	1 36·0	1 30·9	1 40·2
6 30	1 40·9	1 47·4	1 47·5	1 40·6	1 32·2	1 32·0	1 41·0	1 48·3	1 47·6	1 41·2	1 32·5	1 31·9	1 40·3
7 30	1 46·7	1 49·5	1 45·1	1 36·3	1 32·2	1 35·9	1 45·2	1 51·3	1 44·3	1 36·7	1 31·9	1 35·0	1 40·8
8 30	1 50·4	1 47·9	1 42·1	1 34·2	1 33·2	1 41·4	1 49·3	1 50·1	1 42·3	1 34·4	1 33·1	1 41·1	1 41·6
9 30	1 50·7	1 45·9	1 39·4	1 34·3	1 37·4	1 46·6	1 50·8	1 47·6	1 39·4	1 33·8	1 37·1	1 45·5	1 42·4
10 30	1 49·7	1 41·2	1 36·6	1 36·3	1 42·8	1 49·3	1 49·5	1 43·2	1 36·6	1 35·6	1 41·3	1 48·2	1 42·5
11 30	1 48·0	1 37·3	1 35·5	1 39·9	1 47·3	1 51·0	1 45·7	1 38·4	1 35·4	1 38·7	1 46·4	1 49·1	1 42·7

Table XXIII.

Showing the Difference in the Interval between the Moon's Transit in the first column and the fourth succeeding for each month of the year, and the Mean of all.

Moon's Transit.	January.	February.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.	Moon's Transit.
h m	m	m	m	m	m	m	m	m	m	m	m	m	h m
0 30	− 1·8	− 7·1	− 6·7	+ 2·0	+ 8·7	+ 3·2	− 0·3	− 7·2	− 6·2	+ 0·8	+ 8·0	+ 6·3	0 30
1 30	− 5·6	− 8·2	− 3·8	+ 5·6	+ 9·9	+ 4·0	− 5·4	− 9·4	− 4·3	+ 4·8	+ 9·1	+ 3·8	1 30
2 30	− 8·7	− 6·8	+ 0·1	+ 7·6	+ 7·8	+ 0·1	− 7·6	− 7·3	− 0·1	+ 7·2	+ 8·1	− 0·3	2 30
3 30	− 9·8	− 3·8	+ 4·8	+ 9·0	+ 5·2	− 5·5	− 8·4	− 4·3	+ 4·4	+ 8·8	+ 4·9	− 4·8	3 30
4 30	− 8·0	+ 0·2	+ 7·1	+ 7·0	+ 0·4	− 7·0	− 7·3	+ 0·3	+ 6·8	+ 7·6	+ 0·2	− 7·4	4 30
5 30	− 3·9	+ 5·1	+ 8·0	+ 3·9	− 4·6	− 9·1	− 3·8	+ 4·9	+ 9·1	+ 4·3	− 4·2	− 9·3	5 30
6 30	+ 0·6	+ 7·1	+ 7·2	+ 0·3	− 8·1	− 8·3	+ 0·7	+ 8·0	+ 7·3	+ 0·9	− 7·8	− 8·4	6 30
7 30	+ 5·9	+ 8·7	+ 4·3	− 4·5	− 8·6	− 4·9	+ 4·7	+10·5	+ 3·5	− 4·1	− 8·9	− 5·8	7 30
8 30	+ 8·8	+ 6·3	+ 0·5	− 7·4	− 8·4	− 0·2	+ 7·7	+ 8·5	+ 0·7	− 7·2	− 8·5	− 0·5	8 30
9 30	+ 8·3	+ 3·5	− 3·0	− 8·1	− 5·0	+ 4·2	+ 8·4	+ 5·2	− 3·0	− 8·6	− 5·3	+ 3·1	9 30
10 30	+ 7·2	− 1·3	− 5·9	− 6·2	+ 0·3	+ 6·8	+ 7·4	+ 0·7	− 5·9	− 6·9	− 1·2	+ 5·7	10 30
11 30	+ 5·3	− 5·4	− 7·2	− 2·8	+ 4·6	+ 8·3	+ 3·0	− 4·3	− 7·3	− 4·0	+ 3·7	+ 6·4	11 30

Table XXIV.

Showing the Interval between the Moon's Transit in the first column and the fourth succeeding for every minute of the Moon's Horizontal Parallax.

Apparent Solar Time of Moon's Transit.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
h m	h m	h m	h m	h m	h m	h m	h m	h m
0 30	1 30·3	1 33·2	1 36·4	1 40·0	1 44·3	1 47·9	1 52·3	1 55·8
1 30	1 30·2	1 33·4	1 36·1	1 40·3	1 44·1	1 46·6	1 51·1	1 55·1
2 30	1 30·9	1 32·5	1 36·7	1 39·2	1 43·2	1 47·1	1 50·5	1 53·1
3 30	1 30·2	1 33·0	1 36·0	1 40·3	1 43·6	1 47·3	1 50·0	
4 30	1 31·2	1 33·4	1 36·5	1 40·6	1 43·0	1 48·0	1 48·8	
5 30	1 31·0	1 32·9	1 36·9	1 40·4	1 44·6	1 48·4		
6 30	1 30·9	1 33·2	1 36·2	1 40·4	1 44·9	1 49·2		
7 30	1 30·3	1 33·1	1 36·9	1 41·2	1 45·1	1 49·4	1 54·7	
8 30	1 30·1	1 32·8	1 37·4	1 41·0	1 45·6	1 49·1	1 54·5	
9 30	1 30·4	1 32·6	1 37·7	1 40·6	1 45·4	1 50·0	1 52·9	1 58·2
10 30	1 30·1	1 32·8	1 37·7	1 40·9	1 44·4	1 48·6	1 53·3	1 57·4
11 30	1 30·1	1 33·1	1 36·9	1 40·3	1 44·8	1 48·7	1 51·8	1 57·1

Table XXV.

Showing the Difference in the Interval between the Moon's Transit in the first column and the fourth succeeding for every minute of the Moon's Horizontal Parallax, and that for Parallax 57′.

Apparent Solar Time of Moon's Transit.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
h m	m	m	m	m	m	m	m	m
0 30	− 9·7	− 6·8	− 3·6	0	+ 4·3	+ 7·9	+12·3	+15·8
1 30	−10·1	− 6·9	− 4·2	0	+ 3·8	+ 6·3	+10·8	+14·8
2 30	− 8·3	− 6·7	− 2·5	0	+ 4·0	+ 7·9	+11·3	+13·9
3 30	−10·1	− 7·3	− 4·3	0	+ 3·3	+ 7·0	+ 9·7	
4 30	− 9·4	− 7·2	− 4·1	0	+ 2·4	+ 7·4	+ 8·2	
5 30	− 9·4	− 7·5	− 3·5	0	+ 4·2	+ 8·0		
6 30	− 9·5	− 7·2	− 4·2	0	+ 4·5	+ 8·8		
7 30	−10·9	− 8·1	− 4·3	0	+ 3·9	+ 8·2	+13·5	
8 30	−10·9	− 8·2	− 3·6	0	+ 4·6	+ 8·1	+13·5	
9 30	−10·2	− 8·0	− 2·9	0	+ 4·8	+ 9·4	+12·3	+17·6
10 30	−10·8	− 8·1	− 3·2	0	+ 3·5	+ 7·7	+12·4	+16·5
11 30	−10·2	− 7·2	− 3·4	0	+ 4·5	+ 8·4	+11·5	+16·8

Table XXVI.

Showing the Interval between the Moon's Transit in the first column and the fourth succeeding for every three degrees of Declination.

Moon's Transit.	0° Decl.	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.	Mean.
h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m
0 30	1 34·2	1 35·2	1 36·5	1 36·6	1 39·6	1 40·9	1 44·2	1 47·2	1 50·0	1 51·9	1 41·6
1 30	1 33·1	1 34·6	1 35·0	1 36·2	1 38·4	1 41·4	1 43·0	1 47·1	1 48·4	1 51·8	1 40·9
2 30	1 33·3	1 32·4	1 34·5	1 35·3	1 37·5	1 39·7	1 42·3	1 44·9	1 47·6	1 50·5	1 39·8
3 30	1 31·7	1 31·8	1 31·8	1 34·6	1 36·4	1 39·9	1 43·1	1 44·7	1 47·4	1 51·1	1 39·2
4 30	1 31·1	1 31·5	1 32·2	1 33·0	1 36·0	1 37·0	1 42·0	1 43·7	1 46·2	1 50·6	1 38·3
5 30	1 30·5	1 31·3	1 32·4	1 34·2	1 35·0	1 38·7	1 42·3	1 44·2	1 46·9	1 49·8	1 38·5
6 30	1 31·4	1 32·2	1 32·3	1 33·3	1 35·6	1 37·9	1 42·2	1 44·3	1 47·2	1 50·8	1 38·7
7 30	1 31·8	1 31·7	1 33·7	1 35·7	1 37·2	1 39·0	1 43·0	1 44·8	1 47·9	1 51·3	1 39·6
8 30	1 33·3	1 34·4	1 34·4	1 34·6	1 37·1	1 40·7	1 43·7	1 46·2	1 48·7	1 52·7	1 40·6
9 30	1 34·5	1 34·6	1 35·8	1 36·5	1 39·3	1 40·0	1 44·1	1 46·3	1 48·5	1 52·2	1 41·2
10 30	1 35·1	1 35·0	1 36·2	1 37·7	1 39·2	1 40·6	1 45·0	1 45·4	1 49·8	1 53·0	1 41·7
11 30	1 35·0	1 36·1	1 35·9	1 37·7	1 39·4	1 41·9	1 44·6	1 47·2	1 48·8	1 52·8	1 41·9

Table XXVII.

Showing the Difference in the Interval between the Moon's Transit in the first column and the fourth succeeding for every three degrees of Declination, and that for Declination 15°.

Moon's Transit.	0° Decl.	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.	Moon's Transit.
h m	m	m	m	m	m	m	m	m	m	m	h m
0 30	− 6·7	− 5·7	− 4·4	− 4·3	− 1·3	0	+ 3·3	+ 6·3	+ 9·1	+ 11·0	0 30
1 30	− 8·3	− 6·8	− 6·4	− 5·2	− 3·0	0	+ 1·6	+ 5·7	+ 7·0	+ 10·4	1 30
2 30	− 6·4	− 7·3	− 5·2	− 4·4	− 2·2	0	+ 2·6	+ 5·2	+ 7·9	+ 10·8	2 30
3 30	− 8·2	− 8·1	− 8·1	− 5·3	− 3·5	0	+ 3·2	+ 4·8	+ 7·5	+ 10·2	3 30
4 30	− 5·9	− 5·5	− 4·8	− 4·0	− 1·0	0	+ 5·0	+ 6·7	+ 9·2	+ 13·6	4 30
5 30	− 8·2	− 7·4	− 6·3	− 4·5	− 3·7	0	+ 3·6	+ 5·5	+ 8·2	+ 11·1	5 30
6 30	− 6·5	− 5·7	− 5·6	− 4·6	− 2·3	0	+ 4·3	+ 6·4	+ 9·3	+ 12·9	6 30
7 30	− 7·2	− 7·3	− 5·3	− 3·3	− 1·8	0	+ 4·0	+ 5·8	+ 8·9	+ 12·3	7 30
8 30	− 7·4	− 6·3	− 6·3	− 6·1	− 3·6	0	+ 3·0	+ 5·5	+ 8·0	+ 12·0	8 30
9 30	− 5·5	− 5·4	− 4·2	− 3·5	− 0·7	0	+ 4·1	+ 6·3	+ 8·5	+ 12·2	9 30
10 30	− 5·5	− 5·6	− 4·4	− 2·9	− 1·4	0	+ 4·4	+ 4·8	+ 9·2	+ 12·4	10 30
11 30	− 6·9	− 5·8	− 6·0	− 4·2	− 2·5	0	+ 2·7	+ 5·3	+ 6·9	+ 13·9	11 30

TABLE XXVIII.

Showing a Comparison between the Semimenstrual Correction at London in the Interval and in the Height, as deduced from theory and observation. See Plate XVIII.

Moon's Transit.	Interval. ψ + constant.		Height. h.	
	Theory.	Observation.	Theory.	Observation.
h m	h m	h m	feet.	feet.
0 0	3 13·5		22·76	
0 30	3 5·3	3 5·7	22·77	22·80
1 0	2 57·0		22·70	
1 30	2 49·5	2 49·6	22·58	22·66
2 0	2 41·3		22·35	
2 30	2 35·0	2 35·1	22·09	21·94
3 0	2 29·0		21·73	
3 30	2 25·0	2 25·1	21·35	21·16
4 0	2 23·0		20·90	
4 30	2 21·5	2 21·4	20·47	20·17
5 0	2 28·0		20·10	
5 30	2 36·0	2 33·9	19·75	19·49
6 0	2 51·0		19·58	
6 30	3 9·0	3 8·8	19·47	19·44
7 0	3 24·0		19·64	
7 30	3 41·0	3 39·8	19·85	20·22
8 0	3 47·0		20·25	
8 30	3 52·0	3 52·0	20·63	21·14
9 0	3 50·0		21·10	
9 30	3 48·0	3 48·9	21·50	21·94
10 0	3 43·0		21·89	
10 30	3 37·0	3 37·2	22·22	22·46
11 0	3 29·0		22·47	
11 30	3 22·0	3 22·1	22·66	22·77

The argument in the preceding Table is the apparent solar time of the Moon's transit two days previous to the tide required. The constants employed in the calculation are

$$\log(A) = 9{\cdot}5841774 \quad D = 16{\cdot}69 \quad \log(E) = 0{\cdot}6468993.$$

See p. 224.

TABLE XXIX.

Showing the Calendar-month Inequality, as deduced from BERNOULLI's theory and from observation. See Plate XIX.

Moon's Transit.	January.					February.					March.					Moon's Transit.
	d ψ		d h		Moon's Declination.	d ψ		d h		Moon's Declination.	d ψ		d h		Moon's Declination.	
	Theory.	Observation.	Theory.	Observation.		Theory.	Observation.	Theory.	Observation.		Theory.	Observation.	Theory.	Observation.		
h m	m	m	feet.	feet.	°	m	m	feet.	feet.	°	m	m	feet.	feet.	°	h m
0 30	0	− 2	−·49	−·10	19	0	− 3	+·08	+·21	10	0	0	+·32	+·07	5	0 30
1 30	0	− 3	−·36	−·11	16	− 2	− 1	+·16	+·04	6	− 1	0	+·25	+·08	8	1 30
2 30	+ 2	+ 2	−·13	−·01	11	+ 1	+ 1	+·16	+·14	5	0	− 1	+·10	−·08	13	2 30
3 30	+ 4	+ 4	+·03	+·25	6	0	− 1	+·10	+·20	8	− 3	− 4	−·11	−·19	17	3 30
4 30	+ 3	+ 6	+·10	+·28	5	− 3	− 1	−·09	+·03	14	− 9	− 7	−·36	−·39	23	4 30
5 30	+ 3	+10	+·08	+·02	9	− 4	− 2	−·28	−·35	18	− 6	− 9	−·49	−·52	22	5 30
6 30	0	+ 1	+·01	+·15	13	0	− 2	−·45	−·36	21	0	0	−·58	−·44	23	6 30
7 30	0	0	−·24	−·03	18	+ 7	0	−·54	−·22	23	+ 6	+ 6	−·49	−·29	22	7 30
8 30	+ 4	− 2	−·43	−·01	21	+ 7	+ 3	−·47	−·26	22	+ 6	+ 6	−·26	−·19	19	8 30
9 30	+ 3	+ 1	−·53	−·24	23	+ 4	0	−·45	−·19	22	+ 2	0	−·06	−·08	16	9 30
10 30	+ 2	− 2	−·66	−·25	23	+ 2	− 2	−·29	−·03	19	0	− 2	+·16	·00	11	10 30
11 30	+ 1	− 4	−·60	−·19	21	+ 2	− 4	−·07	+·09	14	+ 1	− 1	+·29	+·19	6	11 30
	Sun's Decl. 21°, and Par. 8″·94.					Sun's Decl. 13°, and Par. 8″·90.					Sun's Decl. 3°, and Par. 8″·84.					

Table XXIX. (Continued.)

Moon's Transit.	April. d ψ. Theory.	April. d ψ. Observation.	April. d h. Theory.	April. d h. Observation.	April. Moon's Declination.	May. d ψ. Theory.	May. d ψ. Observation.	May. d h. Theory.	May. d h. Observation.	May. Moon's Declination.	June. d ψ. Theory.	June. d ψ. Observation.	June. d h. Theory.	June. d h. Observation.	June. Moon's Declination.	Moon's Transit.
h m	m	m	feet.	feet.	°	m	m	feet.	feet.	°	m	m	feet.	feet.	°	h m
0 30	0	+ 1	+·22	+·25	13	0	− 1	−·17	+·06	20	0	− 2	−·24	−·22	21	0 30
1 30	− 2	+ 1	+·04	−·21	17	− 1	0	−·28	−·17	22	0	− 1	−·29	−·35	22	1 30
2 30	− 3	− 2	−·12	−·06	20	− 1	− 3	−·30	−·22	23	+ 2	0	−·14	·00	20	2 30
3 30	− 4	− 5	−·24	−·21	22	+ 2	− 3	−·22	−·21	22	+ 7	+ 2	+·11	−·01	16	3 30
4 30	− 7	− 7	−·31	−·69	23	+ 2	− 1	−·07	−·08	20	+ 9	+ 9	+·36	+·43	11	4 30
5 30	− 3	− 4	−·32	−·41	22	+ 4	+ 2	+·10	+·20	17	+11	+10	+·55	+·56	7	5 30
6 30	0	+ 1	−·22	−·11	20	0	+ 2	+·34	+·34	12	0	+ 2	+·67	+·54	5	6 30
7 30	0	+ 2	−·02	−·05	16	− 8	− 3	+·42	+·42	7	−11	− 3	+·55	+·36	8	7 30
8 30	0	0	+·20	+·18	11	− 8	− 4	+·41	+·30	5	− 9	− 5	+·34	+·06	12	8 30
9 30	− 2	− 1	+·34	+·34	6	− 7	− 1	+·34	+·28	7	− 6	− 3	+·06	−·01	17	9 30
10 30	− 1	0	+·38	+·40	5	− 3	0	+·20	+·24	12	− 2	− 2	−·14	−·15	20	10 30
11 30	+ 1	0	+·37	+·41	7	+ 1	0	−·02	+·08	17	0	0	−·29	−·24	22	11 30
	Sun's Decl. 10°, and Par. 8″·76.					Sun's Decl. 19°, and Par. 8″·70.					Sun's Decl. 23°, and Par. 8″·66.					

Moon's Transit.	July. d ψ. Theory.	July. d ψ. Observation.	July. d h. Theory.	July. d h. Observation.	July. Moon's Declination.	August. d ψ. Theory.	August. d ψ. Observation.	August. d h. Theory.	August. d h. Observation.	August. Moon's Declination.	September. d ψ. Theory.	September. d ψ. Observation.	September. d h. Theory.	September. d h. Observation.	September. Moon's Declination.	Moon's Transit.
0 30	0	− 1	−·14	−·12	20	0	+ 4	+·30	+·02	11	0	+ 3	+·46	+·22	5	0 30
1 30	+ 1	+ 1	+·05	−·16	16	0	+ 2	+·41	−·02	7	− 1	+ 3	+·39	−·04	8	1 30
2 30	+ 4	+ 4	+·26	+·26	11	+ 3	+ 5	+·44	+·16	4	0	+ 2	+·26	+·13	12	2 30
3 30	+ 8	+ 8	+·41	+·38	6	+ 4	+ 4	+·35	+·18	8	− 3	+ 1	+·02	−·09	17	3 30
4 30	+ 9	+11	+·48	+·50	5	+ 4	+ 4	+·22	+·31	13	− 6	− 5	−·18	−·31	20	4 30
5 30	+ 9	+12	+·50	+·60	8	+ 2	+ 2	·00	−·02	18	− 4	− 7	−·36	−·34	22	5 30
6 30	0	+ 5	+·42	+·42	13	0	+ 2	−·16	−·16	21	0	+ 1	−·45	−·46	23	6 30
7 30	− 6	− 2	+·20	+·08	17	0	+ 3	−·21	−·25	22	+ 4	+ 7	−·36	−·30	22	7 30
8 30	− 2	− 1	−·01	−·13	20	+ 3	+ 5	−·24	−·23	23	+ 6	+ 6	−·18	−·14	20	8 30
9 30	− 1	0	−·22	−·26	23	0	+ 3	−·19	−·24	22	+ 3	+ 5	+·02	−·02	17	9 30
10 30	0	+ 1	−·27	−·26	23	0	+ 2	−·03	−·21	19	0	+ 6	+·26	+·08	12	10 30
11 30	0	− 1	−·25	−·25	22	0	+ 4	+·10	−·05	16	+ 1	+ 4	+·41	+·17	7	11 30
	Sun's Decl. 21°, and Par. 8″·66.					Sun's Decl. 14°, and Par. 8″·70.					Sun's Decl. 4°, and Par. 8″·76.					

Moon's Transit.	October. d ψ. Theory.	October. d ψ. Observation.	October. d h. Theory.	October. d h. Observation.	October. Moon's Declination.	November. d ψ. Theory.	November. d ψ. Observation.	November. d h. Theory.	November. d h. Observation.	November. Moon's Declination.	December. d ψ. Theory.	December. d ψ. Observation.	December. d h. Theory.	December. d h. Observation.	December. Moon's Declination.	Moon's Transit.
0 30	0	+ 3	+·13	+·13	12	0	− 1	−·42	−·21	20	0	− 2	−·77	−·28	23	0 30
1 30	− 1	+ 3	−·05	−·14	16	− 2	− 2	−·53	−·41	22	− 1	− 3	−·70	−·54	22	1 30
2 30	− 3	− 2	−·24	−·14	20	− 3	− 4	−·56	−·07	23	0	− 3	−·53	−·07	20	2 30
3 30	− 4	− 6	−·37	−·26	22	− 2	− 4	−·47	−·07	22	+ 3	+ 2	−·27	−·02	16	3 30
4 30	− 8	−11	−·45	−·24	23	− 4	− 4	−·34	−·21	20	+ 3	+ 5	−·01	+·25	11	4 30
5 30	− 6	−12	−·46	−·36	22	− 1	− 5	−·22	+·02	18	+ 5	+ 3	+·15	+·50	7	5 30
6 30	0	− 5	−·37	−·36	20	0	− 5	+·05	+·04	12	0	− 2	+·25	+·47	5	6 30
7 30	+ 3	+ 2	−·21	−·09	17	− 3	− 4	+·14	+·18	7	− 4	− 8	+·13	+·25	8	7 30
8 30	+ 2	+ 3	+·04	+·16	12	− 3	− 4	+·15	+·02	5	− 3	− 7	−·04	+·16	12	8 30
9 30	− 2	+ 3	+·19	+·26	7	− 3	− 2	+·09	+·21	7	− 2	− 4	−·32	−·05	17	9 30
10 30	− 1	+ 3	+·26	+·31	5	− 1	− 2	·00	+·05	10	0	− 4	−·53	−·18	20	10 30
11 30	+ 1	+ 4	+·24	+·19	7	+ 1	− 1	−·23	+·03	16	+ 2	− 1	−·70	−·47	23	11 30
	Sun's Decl. 9°, and Par. 8″·84.					Sun's Decl. 18°, and Par. 8″·90.					Sun's Decl. 23°, and Par. 8″·94.					

The sun's parallax was taken from Delambre's Tables for the middle of the month. The numbers given in the column headed "Observation" may each be considered as resulting from the mean of from 80 to 100 observations.

Table XXX.

Showing the Moon's Parallax Correction, as deduced from Bernoulli's theory and from observation. See Plate XX.

Moon's Transit.	H. P. 54′.				H. P. 55′.				H. P. 56′.				H. P. 57′.				Moon's Transit.
	d ψ.		d h.		d ψ.		d h.		d ψ.		d h.		d ψ.		d h.		
	Theory.	Observation.	Theory.	Observation.	Theory.	Observation.	Theory.	Observation.	Theory.	Observation.	Theory.	Observation.	Theory.	Observation.	Theory.	Observation.	
h m	m	m	feet.	feet.	m	m	feet.	feet.	m	m	feet.	feet.	m	m	feet.	feet.	h m
0 30	0	− 1	−·66	−·62	0	− 1	−·45	−·51	0	− 2	−·23	−·27	0	0	·00	·00	0 30
1 30	− 2	− 4	−·66	−·65	− 2	− 3	−·45	−·41	− 1	− 3	−·23	−·18	0	0	·00	·00	1 30
2 30	− 4	− 5	−·64	−·76	− 3	− 2	−·44	−·48	− 1	0	−·23	−·25	0	0	·00	·00	2 30
3 30	− 6	−14	−·62	−·79	− 4	− 8	−·42	−·62	− 2	− 3	−·21	−·35	0	0	·00	·00	3 30
4 30	− 9	−13	−·61	−·87	− 6	−10	−·42	−·73	− 3	− 2	−·21	−·37	0	0	·00	·00	4 30
5 30	− 8	−13	−·64	−·88	− 5	− 8	−·44	−·66	− 2	− 4	−·22	−·32	6	0	·00	·00	5 30
6 30	0	− 3	−·66	−·66	0	− 3	−·45	−·54	0	0	−·23	−·20	0	0	·00	·00	6 30
7 30	+ 8	− 1	−·64	−·61	+ 5	+ 1	−·44	−·53	+ 2	− 1	−·22	+·17	0	0	·00	·00	7 30
8 30	+ 9	+ 4	−·61	−·77	+ 6	+ 2	−·42	−·40	+ 3	+ 3	−·21	−·21	0	0	·00	·00	8 30
9 30	+ 6	+ 3	−·62	−·50	+ 4	+ 2	−·42	−·34	+ 2	+ 1	−·21	−·19	0	0	·00	·00	9 30
10 30	+ 4	+ 2	−·64	−·38	+ 3	+ 2	−·44	−·33	+ 1	0	−·23	+·05	0	0	·00	·00	10 30
11 30	+ 2	+ 3	−·66	−·63	+ 2	+ 3	−·45	−·58	+ 1	+ 4	−·23	−·31	0	0	·00	·00	11 30

	H. P. 58′.				H. P. 59′.				H. P. 60′.				H. P. 61′.				
0 30	0	0	+·24	+·09	0	+ 1	+·49	+·26	0	+ 1	+·75	+·56	0	+ 2	+1·01	+·69	0 30
1 30	0	− 2	+·24	+·28	+ 1	+ 1	+·48	+·42	+ 2	+ 3	+·73	+·69	+ 2	+ 3	+0·99	+·83	1 30
2 30	+ 1	+ 5	+·23	+·17	+ 3	+ 5	+·47	+·36	+ 4	+ 8	+·72	+·61	+ 5	+ 8	+0·97	+·69	2 30
3 30	+ 2	+ 4	+·22	+·20	+ 4	+ 6	+·45	+·47	+ 6	+ 8	+·70	+·79	+ 8		+0·95		3 30
4 30	+ 2	+ 6	+·22	+·14	+ 5	+10	+·45	+·45	+ 7	+13	+·69	+·77	+ 9		+0·94		4 30
5 30	+ 3	+ 5	+·23	+·33	+ 5	+ 7	+·46	+·57	+ 6		+·71		+ 8		+0·97		5 30
6 30	0	+ 3	+·24	+·17	0	+ 6	+·49	+·69	0		+·75		0		+1·01		6 30
7 30	− 3	− 2	+·23	+·29	− 5	− 2	+·46	+·52	− 6	− 4	+·71	+·81	− 8		+0·97		7 30
8 30	− 2	+ 2	+·22	+·19	− 5	− 4	+·45	+·50	− 7	− 4	+·69	+·61	− 9		+0·94		8 30
9 30	− 2	− 2	+·22	+·26	− 4	− 3	+·45	+·38	− 6	− 6	+·70	+·70	− 8	− 6	+0·95	+·85	9 30
10 30	− 1	− 1	+·23	+·31	− 3	− 2	+·47	+·59	− 4	− 3	+·72	+·73	− 5	− 3	+0·97	+·87	10 30
11 30	0	+ 3	+·24	+·04	− 1	+ 5	+·48	+·10	− 2	+ 1	+·73	+·44	− 2	+ 1	+0·99	+·56	11 30

Table XXXI.

Showing the Moon's Declination Correction in the Interval and Height, as deduced from Bernoulli's theory and from observation. See Plate XXI.

Moon's Transit.	0° Declination.					3° Declination.					6° Declination.					Moon's Transit.
	Sun's Declination.	d ψ		d h		Sun's Declination.	d ψ		d h		Sun's Declination.	d ψ		d h		
		Theory.	Observation.	Theory.	Observation.		Theory.	Observation.	Theory.	Observation.		Theory.	Observation.	Theory.	Observation.	
h m	°	m	m	feet.	feet.	°	m	m	feet.	feet.	°	m	m	feet.	feet.	h m
0 30	4·4	0	+ 3	+·43	+·06	4·6	0	+ 1	+·41	+·16	6·7	0	+ 1	+·37	+·19	0 30
1 30	9·1	0	+ 6	+·38	+·02	8·9	0	0	+·37	+·26	8·5	+ 1	− 1	+·33	+·17	1 30
2 30	14·1	+ 2	+ 3	+·32	+·08	14·1	+ 2	+ 1	+·31	+·01	14·1	+ 2	+ 1	+·27	+·17	2 30
3 30	18·3	+ 5	+ 3	+·28	+·35	18·6	+ 5	+ 2	+·26	+·28	17·8	+ 5	0	+·22	+·39	3 30
4 30	21·0	+ 6	+ 7	+·32	+·14	21·1	+ 6	+ 5	+·31	+·38	20·6	+ 6	+ 6	+·27	+·23	4 30
5 30	22·8	+ 7	+ 6	+·39	+·35	22·5	+ 8	+ 4	+·40	+·27	21·9	+ 7	+ 3	+·36	+·44	5 30
6 30	22·8	0	+ 1	+·47	+·46	22·5	0	+ 3	+·47	+·55	22·0	0	+ 4	+·41	+·39	6 30
7 30	21·5	− 7	− 6	+·39	+·05	21·4	− 7	− 3	+·37	+·28	20·6	− 7	− 6	+·33	+·25	7 30
8 30	19·3	− 5	− 1	+·31	+·37	18·1	− 5	− 5	+·30	+·19	17·9	− 5	− 4	+·26	+·17	8 30
9 30	14·2	− 3	0	+·30	+·23	14·4	− 3	+ 2	+·29	+·37	13·9	− 3	+ 2	+·25	+·13	9 30
10 30	9·1	− 1	+ 1	+·36	+·34	9·2	− 1	+ 1	+·35	+·29	8·4	− 1	+ 1	+·31	+·21	10 30
11 30	4·5	0	+ 2	+·41	+·37	5·0	0	+ 1	+·39	+·34	6·7	+ 1	+ 1	+·35	+·09	11 30

	9° Declination.					12° Declination.					15° Declination.					
0 30	9·0	0	0	+·29	+·02	11·8	0	− 1	+·16	+·09	15·2	0	0	·00	·00	0 30
1 30	9·9	− 1	− 1	+·26	+·07	11·9	− 1	+ 1	+·15	+·01	13·9	0	0	·00	·00	1 30
2 30	13·4	+ 1	+ 1	+·22	−·04	12·4	+ 1	+ 1	+·13	+·07	13·3	0	0	·00	·00	2 30
3 30	16·5	+ 3	− 2	+·18	+·17	14·7	+ 1	− 2	+·10	+·15	13·0	0	0	·00	·00	3 30
4 30	19·6	+ 5	+ 5	+·21	+·41	17·6	+ 3	0	+·12	−·01	15·3	0	0	·00	·00	4 30
5 30	21·0	+ 6	+ 1	+·27	+·56	19·4	+ 5	+ 2	+·16	+·14	16·9	0	0	·00	·00	5 30
6 30	21·1	0	+ 3	+·33	+·31	19·4	0	+ 4	+·20	+·20	16·8	0	0	·00	·00	6 30
7 30	19·6	− 6	+ 1	+·26	+·26	18·1	− 4	− 1	+·15	·00	15·4	0	0	·00	·00	7 30
8 30	16·1	− 3	− 5	+·19	+·16	15·5	− 2	− 3	+·11	+·11	13·8	0	0	·00	·00	8 30
9 30	13·3	− 2	+ 2	+·20	+·14	12·5	− 1	+ 2	+·11	·00	13·2	0	0	·00	·00	9 30
10 30	9·6	0	+ 1	+·25	+·16	11·8	− 1	0	+·14	+·12	14·0	0	0	·00	·00	10 30
11 30	9·3	+ 1	− 5	+·29	+·03	12·0	+ 1	− 3	+·15	−·05	15·6	0	0	·00	·00	11 30

Table XXXI. (Continued.)

Moon's Transit.	18° Declination.					21° Declination.					Moon's Transit.
	Sun's Declination.	d ψ		d h		Sun's Declination.	d ψ		d h		
		Theory.	Observation.	Theory.	Observation.		Theory.	Observation.	Theory.	Observation.	
h m	°	m	m	feet.	feet.	°	m	m	feet.	feet.	h m
0 30	18·9	0	− 3	−·20	−·09	19·4	0	− 2	−·38	−·24	0 30
1 30	17·8	− 1	0	−·19	−·29	18·4	− 1	− 4	−·34	−·09	1 30
2 30	15·7	− 1	− 1	−·14	−·14	15·3	− 2	− 6	−·28	−·19	2 30
3 30	13·2	− 1	− 2	−·12	−·01	12·8	− 2	− 6	−·26	−·22	3 30
4 30	11·8	− 3	− 3	−·14	−·21	11·6	− 5	− 8	−·38	−·49	4 30
5 30	11·5	− 2	− 2	−·16	−·22	11·0	− 4	−10	−·32	−·38	5 30
6 30	11·3	0	+ 3	−·17	−·09	11·1	0	+ 3	−·34	−·31	6 30
7 30	12·3	+ 2	+ 3	−·16	−·16	11·5	+ 4	+ 4	−·31	−·39	7 30
8 30	13·5	+ 2	+ 2	−·13	−·12	13·0	+ 5	+ 1	−·28	−·24	8 30
9 30	15·0	+ 1	+ 3	−·13	−·21	15·4	+ 2	+ 2	−·27	−·31	9 30
10 30	18·1	0	− 0	−·17	+·02	18·0	+ 1	− 2	−·32	−·20	10 30
11 30	19·1	+ 1	− 5	−·21	−·10	19·9	0	− 3	−·38	−·21	11 30
	24° Declination.					27° Declination.					
0 30	21·0	0	− 4	−·58	−·37	22·3	0	− 7	−·79	−·34	0 30
1 30	19·3	− 2	− 7	−·54	−·26	21·0	− 1	− 5	−·77	−·68	1 30
2 30	16·4	− 3	− 5	−·46	−·34	17·8	− 3	−10	−·67	−·55	2 30
3 30	13·0	− 4	− 9	−·44	−·24	13·9	− 6	−15	−·62	−·57	3 30
4 30	10·5	− 7	−12	−·44	−·63	9·2	−11	−17	−·61	−·84	4 30
5 30	9·8	− 6	−13	−·50	−·37	6·4	−10	−21	−·70	−1·01	5 30
6 30	9·4	0	− 1	−·54	−·50	6·6	0	− 4	−·75	−·79	6 30
7 30	10·7	+ 6	+ 4	−·49	−·41	8·7	+ 9	+ 3	−·67	−·76	7 30
8 30	13·0	+ 7	+ 2	−·44	−·45	13·8	+ 9	+ 5	−·61	−·49	8 30
9 30	16·2	+ 4	+ 1	−·44	−·48	18·0	+ 4	+ 4	−·64	−·60	9 30
10 30	19·3	+ 2	− 3	−·50	−·30	20·7	+ 2	− 2	−·71	−·45	10 30
11 30	21·0	+ 1	− 5	−·58	−·46	22·2	+ 1	− 5	−·79	−·46	11 30

Table XXXII.

Showing a Comparison between the Diurnal Inequality in the Interval, as deduced from theory and observation.

The numbers in the column headed "Theory" have been calculated by Mr. Russell from the expression for d ψ in p. 223, making the constant $F = 10$.

Observation and theory agree in this respect, that there is no difference between the diurnal inequality for the upper and lower transits, and that it recurs after six months with a contrary sign. I deduced the numbers in the column headed "Observation" upon these suppositions from those given in Table XXI. *before I had seen Mr. Russell's calculations.* The agreement is satisfactory with the expression derived from the equilibrium-theory; but in order to ascertain clearly the law of the diurnal inequality, I think it would be desirable to employ a greater number of observations.

Moon's Transit. P.M.	January.		February.		March.		April.		May.		June.		Moon's Transit. P.M.
	Observation.	Theory.	Observation.	Theory.	Observation.	Theory.	Observation.	Theory.	Observation.	Theory.	Observation.	Theory.	
h m	m	m	m	m	m	m	m	m	m	m	m	m	h m
0 30	0	0	0	0	0	0	0	0	0	0	0	0	0 30
1 30	+ 1	+ 1	+ 1	0	0	0	0	0	0	− 1	0	− 1	1 30
2 30	+ 3	+ 1	+ 3	0	+ 1	0	0	− 1	− 1	− 2	− 2	− 2	2 30
3 30	+ 3	+ 2	+ 3	0	+ 1	− 1	− 1	− 2	− 1	− 3	− 3	− 3	3 30
4 30	+ 4	+ 2	+ 3	0	+ 1	− 1	− 1	− 3	− 2	− 4	− 4	− 3	4 30
5 30	+ 5	+ 3	+ 4	+ 1	+ 1	− 1	− 2	− 4	− 4	− 5	− 5	− 4	5 30
6 30	+ 5	+ 4	+ 4	+ 3	+ 1	0	− 2	− 3	− 5	− 4	− 6	− 4	6 30
7 30	+ 4	+ 5	+ 3	+ 4	+ 0	+ 2	− 2	− 1	− 5	− 3	− 5	− 4	7 30
8 30	+ 4	+ 4	+ 3	+ 3	+ 1	+ 2	− 2	0	− 4	− 2	− 4	− 3	8 30
9 30	+ 5	+ 3	+ 3	+ 2	+ 1	+ 1	− 1	0	− 3	− 2	− 3	− 3	9 30
10 30	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	10 30

Table XXXIII.

This Table is intended to show that the deviations from the H. P. 57′ corresponding to the column headed "Mean" in Table II., have no sensible influence, so that the column in question may be considered as affording the Semimenstrual Inequality.

Moon's Transit.	Mean Par. (a).	Mean of (a) = (b).	a − b.	Corrections for (a − b) in d ψ.	Corrections for (a − b) in d h.	Moon's Transit.
h m	′		′	m	feet.	
0 30	57·1	57′·0	+ ·3	·00	+ ·07	0 30
1 30	57·3		+ ·3	·00	+ ·07	1 30
2 30	57·1		+ ·1	+ ·10	+ ·02	2 30
3 30	57·0		·0	·00	·00	3 30
4 30	56·9		− ·1	+ ·30	− ·02	4 30
5 30	56·7		− ·3	+ ·60	− ·07	5 30
6 30	56·7		− ·3	·00	− ·07	6 30
7 30	56·8		− ·2	− ·60	− ·07	7 30
8 30	57·0		·0	− ·30	− ·02	8 30
9 30	57·1		+ ·1	·00	·00	9 30
10 30	57·3		+ ·3	− ·10	+ ·02	10 30
11 30	57·3		+ ·3	·00	+ ·07	11 30

Table XXXIV.

This Table is intended to show that the deviations of the Moon's Declination from 15°, corresponding to the column headed "Mean" in Table II., have no sensible influence.

Moon's Transit.	Mean Decl. (a)′.	Mean of (a)′ = (b)′.	a′ − b′.	Corrections for (a′ − b′) in d ψ.	Corrections for (a′ − b′) in d h.	Moon's Transit.
h m	°		°	m	feet.	h m
0 30	14·7	15°·2	− ·5	·00	+ ·02	0 30
1 30	15·0		− ·2	·00	+ ·01	1 30
2 30	15·1		− ·1	+ ·03	·00	2 30
3 30	15·2		·0	·00	·00	3 30
4 30	15·6		+ ·4	+ ·27	− ·02	4 30
5 30	15·8		+ ·6	+ ·20	− ·03	5 30
6 30	15·6		+ ·4	·00	− ·02	6 30
7 30	15·5		+ ·3	− ·20	− ·03	7 30
8 30	15·2		·0	− ·27	− ·02	8 30
9 30	15·3		+ ·1	·00	·00	9 30
10 30	14·8		− ·4	− ·03	·00	10 30
11 30	14·7		− ·5	·00	+ ·01	11 30

Table XXXV.

Showing the Correction d ψ for the Sun's Parallax in the different months of the year, according to Bernoulli's theory.

	January. December.	February. November.	March. October.	April. September.	May. August.	June. July.	
Moon's Transit.	8″·94	8″·90	8″·84	8″·76	8″·70	8″·66	Moon's Transit.
h m	m	m	m	m	m	m	h m
0 30	0	0	0	0	0	0	0 30
1 30	− 1	− 1	0	0	0	0	1 30
2 30	− 1	− 1	0	0	+ 1	+ 1	2 30
3 30	− 2	− 2	0	0	+ 2	+ 2	3 30
4 30	− 3	− 2	− 1	+ 1	+ 3	+ 3	4 30
5 30	− 3	− 2	− 1	+ 1	+ 3	+ 3	5 30
6 30	0	0	0	0	0	0	6 30
7 30	+ 3	+ 2	+ 1	− 1	− 3	− 3	7 30
8 30	+ 3	+ 2	+ 1	− 1	− 3	− 3	8 30
9 30	+ 2	+ 2	0	0	− 2	− 2	9 30
10 30	+ 1	+ 1	0	0	− 1	− 1	10 30
11 30	+ 1	+ 1	0	0	0	0	11 30

Table XXXVI.

Showing the Correction d *h* for the Sun's Parallax in the different months of the year, according to Bernoulli's theory.

	January. December.	February. November.	March. October.	April. September.	May. August.	June. July.	
Moon's Transit.	8″·94	8″·90	8″·84	8″·76	8″·70	8″·66	Moon's Transit.
h m	feet.	feet.	feet.	feet.	feet.	feet.	h m
0 30	+ ·09	+ ·06	+ ·03	– ·03	– ·05	– ·08	0 30
1 30	+ ·08	+ ·05	+ ·03	– ·03	– ·05	– ·08	1 30
2 30	+ ·06	+ ·04	+ ·02	– ·02	– ·04	– ·06	2 30
3 30	+ ·03	+ ·02	+ ·01	– ·01	– ·02	– ·03	3 30
4 30	– ·01	·00	·00	·00	·00	+ ·01	4 30
5 30	– ·07	– ·05	– ·02	+ ·02	+ ·03	+ ·05	5 30
6 30	– ·08	– ·05	– ·03	+ ·03	+ ·06	+ ·09	6 30
7 30	– ·07	– ·05	– ·02	+ ·02	+ ·03	+ ·05	7 30
8 30	– ·01	·00	·00	·00	·00	+ ·01	8 30
9 30	+ ·03	+ ·02	+ ·01	– ·01	– ·02	– ·03	9 30
10 30	+ ·06	+ ·04	+ ·02	– ·02	– ·04	– ·06	10 30
11 30	+ ·08	+ ·05	+ ·03	– ·03	– ·05	– ·08	11 30

Index to the Tables.

In all the Tables the Interval is to be increased by two days, the argument being the transit two days previous.

Table I., showing the Interval between the Apparent Solar Time of the Moon's Transit and the Time of High Water, and the Height of High Water at the London Docks (together with the Interval between the Moon's Transits), corresponding to the Apparent Solar Time of the Moon's Transit, in each month of the year, from 13,370 observations made at the London Docks between the first of January 1808 and the 31st of December 1826.

Table II. (Interpolated from Table I.), showing the Interval between the Apparent Solar Time of the Moon's Transit and the Time of High Water at the London Docks, for each month in the year.

Table III. (Interpolated from Table I.), showing the Height of High Water at the London Docks, corresponding to the Apparent Solar Time of the Moon's Transit, in each month of the year.

Table IV., showing the Difference in the Interval between the Apparent Solar Time of the Moon's Transit and the time of High Water, and the Mean Interval, for every month in the year.

Table V., showing the Difference in the Height of High Water and the Mean Height, for every month in the year.

Table VI., showing the Interval between the Apparent Solar Time of the Moon's Transit and the Time of High Water, the Height of High Water, and the Interval between the Moon's Transits at the London Docks, corresponding to the Apparent Solar Time of the Moon's Transit, for every minute of her Horizontal Parallax.

Table VII. (Interpolated from Table VI.)

Table VIII., showing the Difference in the Interval between the Time of the Moon's Transit and the Time of High Water, and the Interval corresponding to fifty-seven minutes of the Moon's Horizontal Parallax.

Table IX., showing the Difference between the Height of High Water and the Height corresponding to fifty-seven minutes of the Moon's Horizontal Parallax.

Table X., showing the Interval between the Apparent Solar Time of the Moon's Transit and the Time of High Water, the Height of High Water, and the Interval between the Moon's Transits, at the London Docks, corresponding to the Apparent Solar Time of the Moon's Transit for every three degrees of her Declination north and south.

Table XI. (Interpolated from Table X.), showing the Interval between the Apparent Solar Time of the Moon's Transit and the Time of High Water at the London Docks for every three degrees of her Declination north *and* south.

Table XII., showing the Interval between the Apparent Solar Time of the Moon's Transit and the Time of High Water at the London Docks, for every three degrees of her Declination north *or* south.

Table XIII., showing the Difference in the Interval between the Apparent Solar Time of the Moon's Transit and the Time of High Water at the London Docks, and the Interval corresponding to fifteen degrees Declination, for every three degrees of the Moon's Declination north *and* south.

Table XIV., showing the Difference in the Interval between the Apparent Solar Time of the Moon's Transit and the Time of High Water at the London Docks, and the Interval corresponding to fifteen degrees Declination, for every three degrees of the Moon's Declination north *or* south.

Table XV. (Interpolated from Table X.), showing the Height of High Water at the London Docks for every three degrees of the Moon's Declination north *and* south.

Table XVI., showing the Height of High Water at the London Docks for every three degrees of the Moon's Declination north *or* south.

Table XVII., showing the Difference in the Height of High Water at the London Docks, and the Height corresponding to fifteen degrees Declination, for every three degrees of the Moon's Declination north *and* south.

Table XVIII., showing the Difference in the Height of High Water at the London Docks, and the Height corresponding to fifteen degrees Declination, for every three degrees of the Moon's Declination north *or* south.

Table XIX., showing the Difference in the Height of High Water at the London Docks when the Moon's Declination is north or south.

The variation of the interval and in the height in Tables X. to XIX. inclusive, is partly due to the change of the sun's declination, which is given in Table X. but not afterwards repeated, because interpolation for the even half-hour is not required.

Table XX., showing the Interval between the Apparent Solar Time of the Moon's Transit and the Time of High Water, and the Height of High Water at the London

Docks (together with the Interval between the Moon's Transits), corresponding to the Apparent Solar Time of the Moon's Upper and Lower Transits, P.M. and A.M.

Table XXI., showing the Difference in the Interval between the Apparent Solar Time of the Moon's Transit and the Time of High Water, and the Mean Interval and the Difference between the Height of High Water and the Mean Height. This Table has been formed by Interpolation from Table XX., in order to ascertain the amount of the Diurnal Inequality.

The moon's parallax in Table I. and Table X. is throughout very nearly, but not exactly 57′, and the moon's declination in Table VI. is very nearly, but not exactly 15°. In strictness the interval and the height ought to have been brought up to what they would have been upon those suppositions. I have neglected the small quantities which would have been thus introduced on account of their minuteness, and on account of the great additional labour they would have occasioned.

Table XXII., showing the Interval between the Moon's Transit in the first column and the fourth succeeding Transit for each month of the year.

Table XXIII., showing the Difference in the Interval between the Moon's Transit in the first column and the fourth succeeding Transit for each month of the year and the Mean of all.

Table XXIV., showing the Interval between the Moon's Transit in the first column and the fourth succeeding for every minute of the Moon's Horizontal Parallax.

Table XXV., showing the Difference in the Interval between the Moon's Transit in the first column and the fourth succeeding Transit, for every minute of the Moon's Horizontal Parallax, and that for Parallax 57′.

Table XXVI., showing the Interval between the Moon's Transit in the first column and the fourth succeeding Transit for every three degrees of Declination.

Table XXVII., showing the Difference in the Interval between the Moon's Transit in the first column and the fourth succeeding for every three degrees of Declination, and that for Declination 15°.

When the moon's transit is at 2^h P.M., for example, α' is greater than α, ψ is negative, tan $2\,\psi$ is negative, and ψ the variable quantity to be *added* to the apparent solar time of the moon's transit or the interval (in the perfect sphere) is negative.

Table XXVIII., showing a Comparison between the Semimenstrual Correction at London in the Interval and in the Height as deduced from theory and observation. See Plate XVIII.

The quantities in the following Tables, deduced from Bernoulli's equilibrium theory, are taken from the Tables calculated by Mr. Jones, and given in my paper in the Philosophical Transactions, 1836, p. 58, with the exception of the sun's parallax correction, which influences in a slight degree the calendar-month inequality. This correction, omitted before, is now given in Tables XXXIV. and XXXV.

The theory correction has been calculated by Mr. Jones with the following constants: $\log(A) = 9{\cdot}5841774$ $\quad D = 16^{ft}{\cdot}69$ $\quad \log(E) = {\cdot}6468993$.

Table XXIX., showing the Calendar-month Inequality, as deduced from BERNOULLI's theory and from observation. In making this comparison the inequality is supposed to arise from the corrections d ψ and d h due to the declinations of the sun and moon and to the sun's parallax, the moon's parallax being $57'$ throughout. See Plate XIX.

Table XXX., showing the Moon's Parallax Correction, as deduced from BERNOULLI's theory and from observation. In this comparison the declinations of the sun and moon are supposed equal to 15° throughout. The actual declinations are given in Table VI. for each category, in order to show that this supposition is admissible. See Plate XX.

Table XXXI., showing the Moon's Declination Correction in the Interval and Height, as deduced from BERNOULLI's theory and from observation. See Plate XXI. The quantities in this Table are influenced by the sun's declination, which is given for each category in Table X.

The parallax and declination corrections have been calculated by Mr. JONES from the expressions

$$\tan 2\,\psi = \frac{A \sin 2\,\varphi}{1 + A \cos 2\,\varphi} \qquad h = D + E\,\{A \cos\,(2\,\psi - 2\,\varphi) + \cos 2\,\psi\}.$$

Table XXXII., showing a Comparison between the Diurnal Inequality in the Interval, as deduced from theory and observation.

Table XXXIII., showing that the deviations from the H. P. $57'$, corresponding to the column headed "Mean" in Table II., have no sensible influence; so that the column in question may be considered as affording the semimenstrual Inequality.

Table XXXIV., showing that the deviations in the Moon's Declination from 15°, corresponding to the column headed "Mean" in Table II. have no sensible influence.

Table XXXV., showing the Correction d ψ for the Sun's Parallax in the different months of the year, according to BERNOULLI's theory.

Table XXXVI., showing the Correction d h for the Sun's Parallax in the different months of the year, according to BERNOULLI's theory.

Conclusion.

The expressions which we have employed in calculating from theory the semimenstrual, parallax, and declination corrections, are virtually those of BERNOULLI. These expressions are in a form well adapted for computation, so that nothing would have been gained by employing expressions less exact.

The approximate expression which Mr. WHEWELL deduced empirically from my former discussion of the London Dock observations for the moon's parallax correction of the interval is

$$P' = (P - p)\,\{B + B \sin (2\,\varphi - 2\,\beta)\}\,*.$$

* Philosophical Transactions, 1834, p. 37.

The second term agrees approximately with BERNOULLI's theory, as Mr. WHEWELL remarked. I consider that the first term was due to the variation in the interval between the moon's transits, and it has vanished, or nearly so, in the present discussion, because we have employed a different transit. I account in the same manner for the first term in the moon's declination correction of the interval, which Mr. WHEWELL deduced empirically from my former discussion, and which has also vanished in the present discussion for the same reason. This term perplexed me formerly in comparing the results I obtained from BERNOULLI's theory with those I obtained from observation*, being far too great to be attributed to errors in the observations, or in the mode of their discussion, so that I ventured to express an opinion that BERNOULLI's theory was insufficient. I discovered the true origin of this term about a year ago†. I conceive that the comparisons which accompany this paper establish the accuracy of BERNOULLI's theory nearly in as great a degree with respect to all the other corrections as with respect to the semimenstrual inequality, so that little remains to be gained by treating the problem more rigorously.

One point at least, however, I think deserves further elucidation. The mass of the moon which would result immediately from the constant (A), which I have deduced from the London and the Liverpool observations, is greater than that which has been derived by other methods. LAPLACE appears to have arrived at a similar conclusion from the Brest observations; but the arguments which he has used in order to remove this difficulty do not seem free from obscurity.

* See Philosophical Transactions, 1834, p. 144.

† See London and Edinburgh Philosophical Magazine, December 1835.

ERRATUM.

Page 224, *for* $\phi = 15°$ *read* $\phi = -15°$.

XVI. *Report of Magnetic Experiments tried on Board an Iron Steam-Vessel, by Order of the Right Honourable the Lords Commissioners of the Admiralty. By* Edward J. Johnson, *Esq. Commander R.N. Accompanied by Plans of the Vessel, and Tables showing the Horizontal Deflection of the Magnetic Needle at different Positions on board, together with the Dip and Magnetic Intensity observed at those Positions, and compared with Observations made on shore with the same Instruments. Addressed to* Charles Wood, *Esq. M.P. &c. &c., and communicated by Captain* Beaufort, *R.N. F.R.S. Hydrographer to the Admiralty, by Command of the Right Honourable the Lords Commissioners of the Admiralty.*

Received February 16,—Read March 10, 1836.

London, January 16th, 1836.

Sir,—In pursuance of orders from the Lords Commissioners of the Admiralty, I proceeded to Ireland, taking with me the necessary instruments for ascertaining the deviation of the magnetic needle produced by the local attraction of an iron steam-vessel, together with others for determining the dip, magnetic intensity, &c.

Every facility for pursuing the inquiry directed by Their Lordships was afforded by Messrs. Laird, of Liverpool, who built the "Garryowen;" and the City of Dublin Steam Packet Company (through Charles W. Williams, Esq.) liberally offered the use of the above-named vessel, on the river Shannon, for the purpose of trying the necessary experiments; and I must not omit to mention in this place my obligation to Professor Barlow, from whom I received many valuable hints relative to the application of his correcting-plate previous to leaving London.

With a view to perspicuity I have divided the accompanying Report into sections.

§ 1. *Description of the Method of Investigation.*

There being no wet dock at Limerick, nor in its vicinity, suitable for swinging the Garryowen, she was put under my directions in Tarbert Bay, on the 19th of October, and the following method of investigation was pursued.

All the compasses intended for use were carefully examined, and the caution of removing all iron from the person during the observations was strictly attended to.

Having fixed a station (X) on the south-west side of Tarbert Bay, a mile distant from the vessel, from whence the cone of a very remarkable mountain* in the County of Clare, and distant about nineteen miles, could be distinctly seen. I observed the

* "Dicomede."

bearing of it with the azimuth compass, that was afterwards used at the position A on board the Garryowen.

The magnetic meridian at the station X being determined by this compass, a distant object on the land was noted in the line of that meridian, and the theodolite in the succeeding observations was placed in the position of the same compass, so that in the simultaneous observations between A and X the bearings may be considered commensurate with those that would have been obtained by one compass only.

The preliminary observations being complete, I directed the Garryowen to be taut moored in the line between the station X and the cone of the mountain, in which line of direction was also a remarkable heap of stones on Kilkerran Point, so that the vertical wire of the theodolite (placed at station X) bisected these objects, and likewise the instrument on the forecastle of the vessel*. These objects on the land served also as excellent marks to ascertain if the vessel drifted from her proper position, for which purpose thwart marks were likewise fixed upon.

In order to have the conditions respecting the position of the iron on board always the same during the experiments, the Garryowen was moored with extra anchors and hawsers, so that her own bower anchors were at the bows, and their chain cables in a given place. The tiller, the crane, and boats' davits (being made of iron) were also secured in certain definite positions; and I am induced to mention these particulars, as it will be seen by the experiments how essential their observance is to accuracy.

Anchors having been laid out, and the necessary preparations made for warping the vessel's head to the required points, I thought it desirable previously to fixing the compasses in certain positions on board, and swinging the vessel an entire revolution, to ascertain the amount of local attraction when the Garryowen's head was in a direction where, in the generality of cases, the deviation had been found to approach its maximum, so as in some degree to guide me in the selection of a place for the principal observations, or that to which my chief attention was directed, viz. a position for placing a steering-compass, and that where the effect of Professor Barlow's correcting-plate might be tried.

With this view the vessel's head was warped to the true magnetic west, and the deviation ascertained in various parts of her.

At a position (No. 5) near the fore hatchway, and 5 feet above the deck, the deviation of the marked or north end of the needle was . . . 16° E.

Near the centre of the vessel before the large funnel, and 5 feet above the deck, the deviation was . . . . 26° 20′ E.

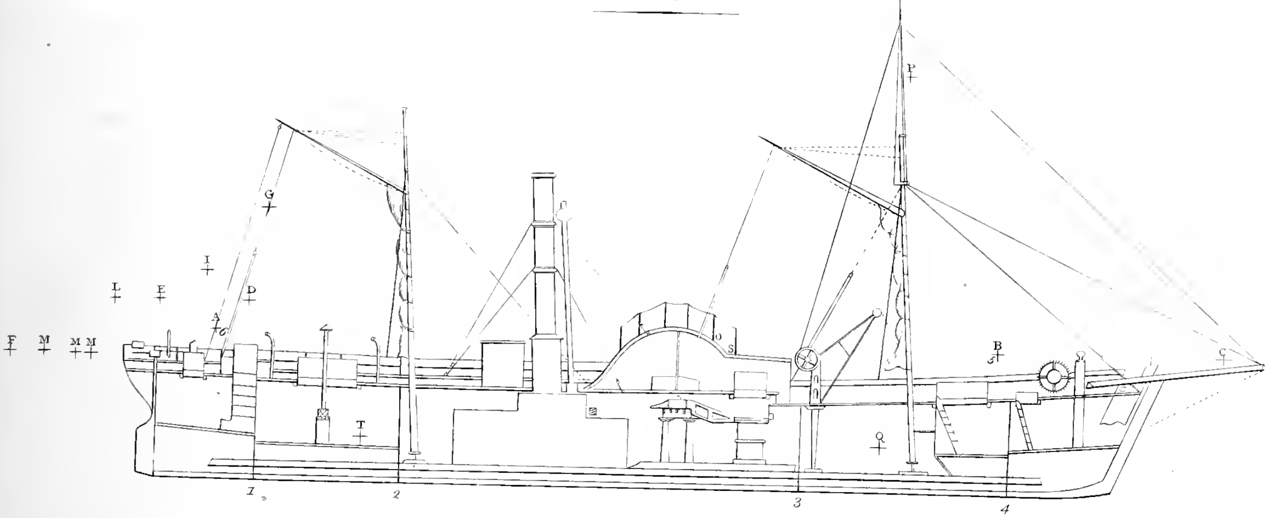
* This instrument was the graduated circle of a surveying-compass, to which I applied cross vanes for the purpose of observing the true magnetic direction of the vessel's head, a plan which I found more convenient than using a sextant for that purpose while warping round from point to point.

PLAN & SECTION OF THE GARRY-OWEN IRON STEAM VESSEL

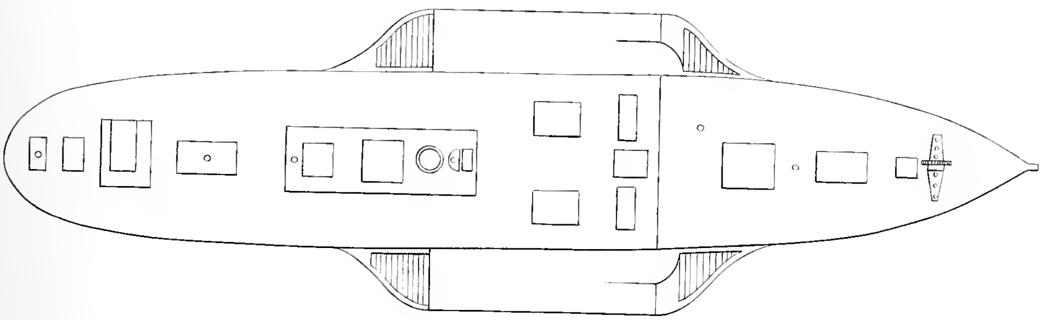
of 281 Tons Burthen.

belonging to the City of Dublin Steam Packet Company

Built by Mess.rs Laird of Liverpool, 1833.



1.2.3.4. Four Watertight Bulk Heads of Wrought Iron ¼ in. thick.



WEIGHT OF IRON.

Total Weight of Iron including Hull, Machinery, Anchors Cables &c. &c	180 Tons
Weight of Iron in Hull	95 "
D.o ... Engines	40 "
D.o ..... Shafts & Wheels	12 "
D.o .. Boilers	30 "
D.o Chimney	1 20
D.o Anchors & Cables	1 . 10

Stem 14 feet long × 4 feet wide
Beams 4 In deep. 4 In wide bound with Iron Plates
All Iron used in Hull and Boilers is Malleable

DIMENSIONS OF VESSEL &c.

Length on Deck 130 ft		*Beam*	21 6
D.o Keel 122 3		*Depth*	11 . 0

38 Double Frames amidships of Angle Iron 3 In wide × 3 deep × ½ In.

17 Single Frames forward of	3 × 3 × 5/8
22 D.o aft	3 × 3 × 5/16
Diameter of Chimney	3 feet
Height of D.o	28

Draught of Water Forward 5 . 3 — Aft . . 5 . 3

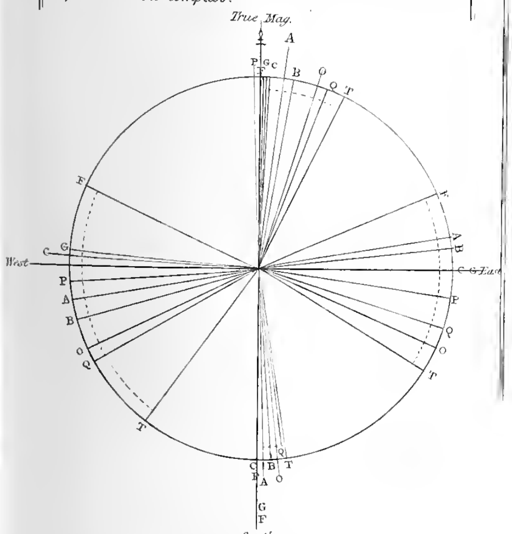
TWO ENGINES = 85 Horse power.

Diameter of Cylinder	ft. 3 . 0
Diameter of Wheel	ft 15 . 6

Engine makes 27 strokes p. minute

DIAGRAM. 1.

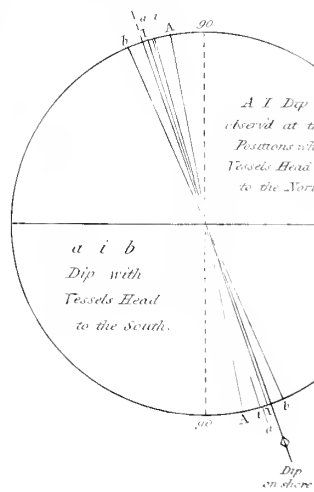
representing the deviation of several Compasses when placed in different parts of the Vessel when her head was in the true Magnetic direction of the Cardinal points of the Compass.



The letters A.B.C.&c. refer to the positions of the Compasses on board, and the adjoining lines shew the direction of the Vessels' Head, as indicated by the Compasses at those positions.

DIAGRAM. 2.

representing the comparative Dip of the Magnetic Needle on Tarbert Island and that observed at three positions on Board the "Garry Owen" in Tarbert Bay when the Vessels' Head was to the true Magnetic North and South.



In Diagram 2 the letters A.I. and a. i. b. refer to the positions of the Dipping Needle on board, and the lines adjoining denote the Dip observed there

And at a position (No. 6) $11\frac{1}{2}$ feet before the tafrail, and $5\frac{3}{4}$ feet above
the deck, the deviation was (when the boats' davits were out) . . 11° 40′ E.
Ditto (when the boats' davits were swung in-board) . . . . 6° 20′ E.*

The preceding facts showing the influence of small portions of iron in the vicinity of the compass, may be worthy the attention of the practical navigator; for it will be seen by the results obtained at a position not far from the binnacle, that the difference of deviation amounted to no less than 5° 20′ under the mere circumstance of swinging the quarter-boat's davits in-board from their usual position where the boats are hoisted up, to that place in which they would be secured in stormy weather at sea.

Bearing in mind the practical question of placing a steering-compass as near to a convenient part of the deck as possible, and not considering the local attraction developed by the preceding observations at No. 5. and No. 6. excessive, I fixed on positions near to them for placing the azimuth compasses†, viz. A and B.

The positions A and B were therefore selected with reference to the nearer portions of iron; the nearest iron to the pivot of one position being 5 feet $9\frac{5}{8}$ inches, and that to the pivot of the other 6 feet, except the painted wire of the forecastle skylight, which I afterwards discovered to be iron, and which was 4 feet 1 inch from the compass at B.

The exact distances of particular masses of iron from each compass will be found in Table X., page 288; and the relative position of the several compasses, according to measurement, will be readily understood by referring to the letters in the longitudinal section of the vessel (Plate XXIII.).

It is necessary to remark in this place, that simultaneous observations could not be made at all the positions on board, as the compasses would then have been too near to each other; therefore in referring to the letters denoting the place of each instrument, it is to be understood that the observations were made at different times, except where otherwise expressed. It must also be understood that the deflections at C, F, P, T, O, Q were observed with common compasses.

Mr. William Laird, Jun., who accompanied me to Ireland, kindly undertook to superintend the observations at Station X.; and to him I am also indebted for very accurate plans of the Garryowen, and likewise for noting the time during the observations for the magnetic intensity.

Having instructed the engineer of the vessel so far in the use of the theodolite as to enable him to observe the horizontal angle which the position A on board subtended with the magnetic meridian as the vessel was warped round, he was likewise

* Although the deviation was less when the davits were swung in-board, yet the directive power of the needle would be different, and it is the difference of deviation that is here to be remarked.

† These compasses were made by Mr. Gilbert, their pedestals being so contrived by him that Professor Barlow's correcting plate could be conveniently turned to the different points, and raised or depressed as occasions might require by means of brass tubes and screws.

stationed at X; and the necessary signals were arranged for the simultaneous observations between that station and A on board*.

Mr. Bingham of the Garryowen willingly attended to swinging the vessel, and also observed the direction of her head at the position G. These arrangements left me at liberty to make the more important observations at A and B, to keep a check with the instrument on the forecastle as to the direction of the vessel's head, and occasionally to overlook the men who were watching the other compasses.

The necessary preparations having been made, the Garryowen was warped round to all the points of the compass, but not till after frequent interruptions, sometimes for several days together, by rain and gales of wind.

The bearings of the cone of the distant mountain from the positions A and B, as well as the simultaneous bearings between A and X, were observed when the vessel's head was at each point, and from these bearings the deviation of the compass produced by the local attraction of the vessel was deduced.

These observations are registered in Table II., and the repetition of them during another revolution of the vessel is inserted in Table III.

Table I.

Simultaneous Observations made with Nine Compasses in different parts of the Vessel, the Bell being struck as the signal for observation.

Date.	True magnetic direction of vessel's head.	Direction of vessel's head by compass F.	Deviation at F.	Direction of vessel's head by T.	Deviation at T.	Direction of vessel's head by G.	Deviation at G.	Direction of vessel's head by A.	Deviation at A.	Direction of vessel's head by O.	Deviation at O.
Nov. 5.	North.	North.	0° 0′	N. 26° 0′ E.	26° 0′	N. 1° 0′ E.	1° 0′	N. 8° 20′ E.	8° 20′	N. 16° 30′ E.	16° 30′
Therm. 52° I.	N.E.	N. 33 0 E.	12 0	N. 74 30 E.	29 30	N. 45 0 E.	0 0	N. 55 0 E.	10 0	N. 72 0 E.	27 0
Barom. 29·8.	East.	N. 67 0 E.	23 0	S. 59 30 E.	30 30	East.	0 0	N. 80 40 E.	9 20	S. 67 0 E.	23 0
Nov. 4.	S.E.	S. 64 30 E.	19 30	S. 30 15 E.	14 45	S. 45 0 E.	0 0	S. 39 30 E.	5 30	S. 35 0 E.	10 0
Therm. 56°.	South.	S. 1 0 E.	1 0	S. 10 30 E.	10 30	S. 1 0 E.	1 0	S. 1 0 E.	1 0	S. 5 0 E.	5 0
Barom. 29·7.	S.W.	S. 65 0 W.	20 0	S. 8 30 W.	36 30	S. 45 0 W.	0 0	S. 33 0 W.	12 0	S. 26 0 W.	13 0
	West.	N. 66 30 W.	23 30	S. 35 30 W.	54 30	N. 85 0 W.	5 0	S. 80 0 W.	10 0	S. 63 15 W.	26 45
	N.W.	N. 32 0 W.	13 0	N. 30 0 W.	15 0	N. 40 0 W.	5 0	N. 38 0 W.	7 0	N. 64 0 W.	19 0
		Direction of vessel's head by Q.	Deviation at Q.	P.	at P.	B.	at B.	C.	at C.		
Nov. 5.	North.	N. 18° 0′ E.	18 0	N. 1 24 E.	1 24	N. 9 40 E.	9 40	N. 1 24 E.	1 24		
Therm. 52° I.	N.E.	N. 56 15 E.	11 15	N. 47 48 E.	2 48	N. 56 0 E.	11 0	N. 43 36 E.	1 24		
Barom. 29·8.	East.	S. 73 8 E.	16 52	S. 81 33 E.	8 27	N. 82 30 E.	7 30	East.	0 0		
Nov. 4.	S.E.	S. 36 33 E.	8 27	S. 42 12 E.	2 48	S. 41 0 E.	4 0	S. 42 12 E.	2 48		
Therm. 56°.	South.	S. 9 51 E.	9 51	South.	0 0	S. 5 0 E.	5 0	South.	0 0		
Barom. 29·7.	S.W.	S. 14 3 W.	30 57	S. 41 30 W.	3 30	S. 27 30 W.	17 30	S. 45 0 W.	0 0		
	West.	S. 59 3 W.	30 57	S. 84 23 W.	5 37	S. 73 30 W.	16 30	N. 86 30 W.	3 30		
	N.W.	N. 32 21 W.	12 39	N. 47 48 W.	2 48	N. 40 30 W.	4 30	N. 36 33 W.	8 27		

* The time was noted as a check to any mistake in the identity of the observations; but I have not thought it necessary to prolong the tables by inserting it.

Table II.

Showing the Deviation of the Horizontal Needle (or Local Attraction) at the positions A and B on board the Garryowen, as deduced from the true magnetic Bearing of the Cone of a distant mountain and the bearings of it observed on board, when the vessel's head was at each point of the Compass, together with the Deviation, deduced from Simultaneous Bearings.

Barometer.	Thermometer.	True magnetic direction of the vessel's head.	Observations at position A on board.			Simultaneous bearings from A on board with the station X on shore.			Observations at position B.		
			True magnetic bearing of the cone of Dicomede.	Bearings of the cone of Dicomede from A.	Deviation or local attraction at A.	True magnetic bearings of A from X.	Bearings of the station X from A.	Deviation or local attraction at A.	True magnetic bearing of the cone of Dicomede.	Bearing of the cone from B.	Deviation or local attraction at B.
	°	North.	N. 64° 50′ E.	N. 74° 15′ E.	9° 25′ +	N. 66° 10′ E.	S. 75° 30′ W.	9° 20′ +	N. 64° 50′ E.	N. 74° 15′ E.	9° 25′
		N. by E.	....	73 20	8 30	65 50	76 30	10 40	....	75 30	10 40
		N.N.E.	....	74 50	10 0	65 30	74 20	8 50	....	75 0	10 10
		N.E. by N.	....	75 30	10 40	65 15	74 40	9 25	....	Not observed.	
		N.E.	....	{79 0 / 75 10[a]}	{=14 10 / =10 20[a]}	64 50	75 15	10 25	....	75 30	10 40
		N.E. by E.	....	74 10	9 20	64 40	74 50	10 10	....	74 45	9 55
		E.N.E.	....	73 45	8 55	64 15	72 40	8 25	....	75 0	10 10
29·9	52	E. by N.	....	74 45	9 55	64 5	72 50	8 45	....	74 50	10 0
		East.	....	74 0	9 10	63 55	72 20	8 25	....	74 20	9 30
		E. by S.	....	73 10	8 20	63 30	71 30	8 0	....	73 45	8 55
		E.S.E.	....	72 5	7 15	63 15	70 10	6 55	....	73 10	8 20
		S.E. by E.	....	70 25	5 35	63 15	69 25	6 10	....	71 30	6 40
		S.E.	....	69 25	4 25	63 5	67 20	4 15	....	69 0	4 10
		S.E. by S.	....	67 0	2 10	63 10	64 20	1 20+	....	67 0	2 10+
		S.S.E.	....	65 0+	0 10+[b]	63 15	62 50	0 25–	....	64 30	0 10–
		S. by E.	....	61 30–	3 20–[c]	63 8	62 20	0 48	....	60 20	4 30[e]
		South.	....	58 50	6 0	63 20	60 10	3 10	....	58 20	6 30
		S. by W.	....	56 20	8 30	63 35	55 0	8 35	....	54 50	10 0
		S.S.W.	....	54 40	10 10	63 45	53 20	10 25	....	52 30	12 20
		S.W. by S.	....	52 0	12 50	64 0	51 40	12 20	....	49 20	15 30
		S.W.	....	51 30	13 20	64 12	50 45	13 27	....	Cone not seen for paddle-box.	
		S.W. by W.	....	51 20	13 30	64 35	50 20[d]	14 15	....	46 45	18 5
		W.S.W.	....	50 40	14 10	64 55	50 10[d]	14 45	....	44 30	20 20
		W. by S.	....	52 0	12 50	65 20	52 20[d]	13 0	....	48 10	16 40
30·1	55	West.	....	54 0	10 50	65 35	54 40	10 55	....	52 40	12 10
		W. by N.	....	57 50	7 0	65 45	60 0	5 45	....	56 50	8 0
		W.N.W.	....	61 0	3 50–	66 3	62 50	3 13	....	60 50	4 0–
		N.W. by W.	....	63 40	1 10–	66 8	65 50	0 18–	....	66 0	1 10+
		N.W.	....	67 45	2 55+	66 20	69 40	3 20+	....	68 35	3 45
		N.W. by N.	....	70 0	5 10	66 25	72 20	5 55	....	71 50	7 0
		N.N.W.	....	71 40	6 50	66 26	73 30	7 4	....	74 20	9 30
		N. by W.	....	73 10	8 20	66 27	75 40	9 13	....	76 0	11 10
		North.	N. 64 50 E.	N. 74 15 E.	9 25	N. 66 15 E.	S. 76 20 W.	10 5	....	74 10	9 20
			Fires extinguished.			The time of each observation was noted by watches regulated to the same time, as a check on the observation.—Fires extinguished.			Fires extinguished.		

Note.—Oct. 21. Moderate breezes from S.S.W.——Oct 27. Moderate breezes from the north, with slight showers.

[a] Repeated Oct. 27, and found to be 75° 10′. [b] Vessel sheering more than 1°. [c] Hawser broke.
[d] Station X not seen through the compass vanes, but a mark was placed on the paddle-box, and ascertained to be in a line with it was observed.
[e] Hawser broke.

After vibrating the needles during the above observations, I observed that both the compasses at A and B were occasionally embarrassed in their movements.

Table III.

Repetition of Observations at A and B, showing the Discrepancies which occurred in the Deflections of the Horizontal Needle at that position.

Date.	Barometer.	Thermometer.	True magnetic direction of vessel's head.	Simultaneous observations at X.	Simultaneous observations at A.	Local attraction at A.	True magnetic bearing of cone of mountain.	Bearings of the cone from A.	Deviation or local attraction at A.	True magnetic bearing of Dicomede from B.	Bearings of the cone of Dicomede from B.	Deviation or local attraction at B.
		°	North.	N. 65° 30′ E.	S. 76° 0′ W.	10° 30′	N. 64° 50′ E.	N. 73° 20′ E.	8° 30′	N. 64° 50′ E.	N. 74° 10′ E.	9° 20′
			N. by E.	65 25	75 50	10 25	....	74 30	9 40	....	73 30	8 48
			N.N.E.	65 10	77 55	12 45	....	73 20	8 30	....	73 30	8 40
			N.E. by N.	64 45	75 30	10 45	....	73 30	8 40	....	75 40	10 50
			N.E.	64 32	76 20	11 48	....	74 30	9 40	....	74 30	9 40
			N.E. by E.	64 13	74 50	10 37	....	74 10	9 20	....	75 0	10 10
			E.N.E.	63 53	72 30	8 37	....	72 30	7 40	....	75 20	10 30
Oct. 29.	29·9	57[a]	E. by N.	63 35	72 30	8 55	....	73 30	8 40	....	74 30	9 40
			East.	63 15	72 0	8 45	....	73 20	8 30	....	71 20[b]	6 30[b]
			E. by S.	63 7	70 50	7 43	....	72 55	8 5	....	73 0	8 18
			E.S.E.	62 55	70 0	7 5	....	72 30	7 40	....	74 0	9 10
			S.E. by E.	62 45	69 50	7 5	....	71 10	6 20	....	73 20	8 30
			S.E.	62 40	68 40	6 0	....	69 30	4 40	....	70 30	5 40
			S.E. by S.	62 37	65 50	3 13	....	67 0	2 10	....	68 50	4 0+
			S.S.E.	62 39	63 10	0 31+	....	65 10	0 20+	....	64 25	0 25–
			S. by E.	62 35	60 50	1 45–	....	62 20	2 30–	....	61 0	3 50
			South.	62 45	59 10	3 35	....	60 10	4 40	....	59 20	5 30
			S. by W.	63 30	55 0	8 30	....	55 40	9 10	....	56 30	8 20
Oct. 30.	30	52	S.S.W.	63 30	54 20	9 10	....	54 40	10 10	....	52 40	12 10
			S.W. by S.	63 45	51 40	12 5	....	53 30	11 20	....	52 50	12 0
			S.W.[d]	64 0	50 10	13 50	....	....	....	....	50 20	14 30
			S.W. by W.	64 28	54 40	9 48	....	[c] .... (Cone of mountain not seen.)	...... [c]	B from X. N. 64 40	X from B. S. 48 0 W.	16 40[c]
			W.S.W.	65 10	52 30	12 40	....	....	....	65 10	48 30	16 40
			W. by S.	65 45	52 20	13 35	....	....	....	65 30	50 30	15 0
			West.	66 8	55 20	10 48	....	....	....	65 35	52 30	13 0
Oct. 31.	30·1	50	W. by N.	66 15	58 20	7 55	....	....	....	65 30	54 20	11 10
			W.N.W.	66 15	61 20	4 55	....	....	....	65 20	60 0	5 20–
			N.W. by W.	66 30	66 0	0 30–	....	....	....	65 20	66 20	1 0+
			N.W.	66 32	68 30	1 58+	....	....	....	65 15	67 30	2 15
			N.W. by N.	66 30	72 0	5 30	....	....	....	65 10	71 40	6 30
			N.N.W.	66 30	73 0	6 30	....	....	....	65 5	73 0	7 55
			N. by W.	66 15	71 50	5 35	....	....	....	64 58	72 0	7 2
			North.	66 15	69 40	3 25	....	....	....	65 0	71 40	6 40

Fires lighted.—Steam up.

Note.—Oct. 29. Moderate breezes and cloudy weather.——Oct. 31. Moderate breezes from the east: hazy weather.

[a] 79° in engine-room. [b] Vessel's head E. 2½° S.

[c] The cone of the distant mountain here became obscured in mist, so that it was necessary to resort to simultaneous observations with the station X on the south-west side of Tarbert Bay.

Table IV.

Simultaneous Bearings between position I and the station X on the south-west side of Tarbert Bay, showing the Deflection of the Horizontal Needle at I.

Date.	Baro-meter.	Thermo-meter.	True magnetic direction of the vessel's head.	Simultaneous bearings from I.	Simultaneous bearings from X.	Deviation or local attraction at I.
		°		° ′	° ′	° ′
Nov. 12.	30·5	47	North.	N. 66 28 E.	S. 68 10 W.	1 42
			N. by E.	66 15	66 40	0 25+
			N.N.E	66 3	65 10	0 53−
			N.E. by N.	65 50	63 40	2 10
			N.E.	65 32	62 40	2 52
			N.E by E.	65 10	61 30	3 40
			E.N.E.	64 50	60 40	4 10
			E. by N.	64 35	59 40	4 55
Nov. 11.	30·4	46	East.	64 15	60 0	3 45
			E. by S.	63 30	59 45	3 45
			E.S.E.	63 20	59 25	3 55
			S.E. by E.	63 2	58 55	4 7
			S.E.	63 5	59 20	3 45
			S.E. by S.	63 0	59 40	3 20
			S.S.E.	62 15	59 0	3 15
			S. by E.	62 55	59 40	3 15
			South.	63 4	60 10	2 54
Nov. 13.	30·5	47	S. by W.	63 35	61 40	1 53
			S.S.W.	63 53	62 20	1 33
			S.W. by S.	64 5	Station not seen.	
			S.W.	64 20	64 0	0 20−
			S.W. by W.	64 35	66 0	1 25+
			W.S.W.	64 45	66 30	1 45
			W. by S.	65 12	67 30	2 18
			West.	65 35	Station not seen.	
Nov. 13.	30·5	46	W. by N.	65 40	70 10	4 30
			W.N.W.	65 50	71 30	6 0
			N.W. by W.	66 3	71 40	5 37
			N.W.	66 8	71 40	5 32
			N.W. by N.	66 16	70 50	4 34
			N.N.W.	66 14	70 10	3 56
			N. by W.	66 12	68 0	1 48
Nov. 14.	30·4	40·8	North.	66 5	67 50	1 45

Fires extinguished.

Note.—Nov. 12. Fine clear weather.——Nov. 11. Cold clear weather, wind E. Nov. 12. Moderate breezes from the east.

From the prevalence of wet and stormy weather it was impracticable to repeat or verify these observations.

TABLE V.—Horizontal Deflections of the Magnetic Needle observed at seven different parts of the Garryowen.

…meter.	Thermometer.	True magnetic direction of the vessel's head.	Direction of vessel's head by compass on bowsprit C.	Deviation or local attraction at C.	Direction of vessel's head by compass near gaff G.	Deviation or local attraction at G.	Direction of vessel's head by compass on stage L.	Deviation or local attraction at L.	Direction of vessel's head by compass on poop E.	Deviation or local attraction at E.	Direction of vessel's head by compass on fore part of poop D.	Deviation or local attraction at D.	Direction of vessel's head by compass in iron sphere S.	Deviation or local attraction at S.	Direction of vessel's head by compass in binnacle.	Deviation or local attraction at binnacle.
	°			° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	°	° ′	° ′
		North.	r*N. 1° 24′ E. N. 1 0 E.	1 12	r 0 30 E. North.	0 15	N. 2 0 W.	2 0	N. 1 30 E. r N. 1 0 E.	1 15	N. 5 0 E. r N. 4 30	4 45	N. 9 0 E. r N. 6 0 E.	7 30	N. 17 0 E. r 10 0	13 30
		N. by E.	N. by E.	0 0	N. 11 0 E.	0 30	N. 5 10 E.	6 5	10 0	1 15	17 0	5 45	15 0	3 45	28 0	16 45
		N.N.E.	N. 23° 30′ E.	1 0	21 30	1 0	11 0	11 30	16 30	6 0	27 30	5 0	22 30	0 0	not observed.	
·5	47	N.E. by N.	N.E. by N. ⅛ N.	1 24	33 0	0 45	17 30	16 15	25 30	8 15	39 30	5 45	31 30	2 15		
		N.E.	r N.E. ¼ N. N.E. ¼ N.	2 48	42 0	3 0	r 26 10 r 24 0	19 55	r N. 34 40 E. 34 0	10 40	52 0 r 50 0	6 0	N. 41 0 E. r 41 0	4 0	N. 59 0 E. r 64 0 E.	16 30
		N.E. by E.	N.E. by E. ¾ N.	2 48	54 30	2 15	32 30	23 45	43 0	13 15	62 0	5 45	50 0	6 15	N. 76 0 E.	19 45
		E.N.E.	E.N.E. ¼ N.	2 48	65 0	2 30	38 0	29 30	52 30	15 0	74 30	7 0	60 0	7 30	N. 87 0 E.	19 30
		E. by N.	E. by N. ¼ N.	2 48	74 30	4 25	46 30	32 15	62 0	16 45	N. 85 30 E.	4 30	67 0	11 45	S. 82 0 E.	19 15
		East.	r E. 2° N. East.	1 0	N. 86 0 E. r N. 89 0 E.	2 30	r N. 55 30 E. 55 30	34 30	N. 72 0 E. r 72 30	17 45	S. 82 30 E. r 83 0	7 15	N. 76 0 E. r 76 0	14 0	S. 71 0 E. r 71 0	19 0
		E. by S.	E. by S. 1° S.	1 0	S. 81 0 E.	2 15	65 0	13 45	N. 85 0 E.	16 15	S. 71 0 E.	7 45	N. 88 E.	13 15	S. 62 0 E.	16 45
		E.S.E.	E.S.E. ⅛ S.	1 24	71 15	3 45	N. 74 30 E.	7 0+	S. 84 30 E.	17 0	60 30	7 0	S. 84 0 E.	16 30	not observed.	
		S.E. by E.	S.E. by E. ⅛ S.	1 24	59 30	2 15	N. 84 0 E.	27 45	S. 72 30 E.	16 15	46 30	9 45	S. 72 0 E.	15 45		
·4	46	S.E.	r S.E. 1° S. S.E. 2° S.	1 30	r 46 30 47 30	2 0	S. 83 30 E. r S. 83 6 E.	38 30	57 0 r 61	14 0	40 30 r 40 30	4 30	65 0 r 61 0	18 0	S. 42 0 E.	3 0
		S.E. by S.	S.E. by S. 2° S.	2 0	35 45	2 0	67 6	23 21	46 0	12 15	31 0	2 45	50 0	16 15	not observed.	
		S.S.E.	S.S.E. ⅛ S.	1 24	23 0	0 30	53 6	30 36	33 0	10 30	23 0	0 30	38 0	15 30		
		S. by E.	S. by E.	0 0	11 30	0 0	33 0	21 45	20 0	8 45	13 50	2 35	S. 25 0 E.	13 45		
		South.	r S. 1° W. S. 1° 30′ W.	1 15	r S. 2 30 E. S. 1 30 E.	2 0	r S. 6 30 E. 1 30	4 0	S. 4 0 E. r 3 30	3 45	S. 2 0 E. r 2 30 E.	2 30	S. 13 E. r S. 9 E.	11 0	S. 9 0 E.	9 0
		S. by W.	S. by W. 1° S.	1 0	S. 11 30 W.	0 15	S. 21 30 W.	10 15	S. 10 30 W.	0 45	S. 6 50 W.	4 25	South.	11 15	South.	11 15
		S.S.W.	S.S.W. ⅛ S.	1 24	23 0	0 30	42 30	20 0	25 30	3 0	17 0	5 30	S. 15 30 W.	7 0	S. 6 0 W.	16 30
		S.W. by S.	S.W. by S. ⅛ S.	1 24	32 30	1 15	63 0	29 15	40 30	6 45	26 0	7 45	30 0	3 45	S. 14 0 W.	19 45
		S.W.	r S.W. ⅛ S. S.W. ⅛ S. r S.W. ¼ S.	2 6	r S. 45 W. 44 30 r 44 0	0 30	r S. 78 0 W. 76 0	32 0	53 30 54 50	9 10	35 30 r 36 0	9 15	44 0 r 45 0 r 42 0	1 20	S. 20 0 W. r 23 0	23 30
·5	47	S.W. by W.	S.W. by W. 1° S.	1 0	58 0	1 45	W. 0 30 N.	34 15	69 0	12 45	48 0	8 15	66 0	9 45	34 0	22 15
		W.S.W.	W.S.W.	0 0	68 20	0 50	N. 78 0 W.	34 30	S. 82 0 W.	14 30	59 0	8 30	S. 79 0 W.	11 30	41 0	26 30
		W. by S.	W. by S. 1° W.	1 0	81 0	2 15	N. 68 0 W.	33 15	N. 84 30 W.	16 45	71 30	7 15	N. 85 0 W.	16 15	51 0	27 45
		West.	r W. ⅛ N. W. ¼ N.	2 6	N. 89 0 W. r N. 89 0 W.	1 0	N. 58 0 W. r 60 0	31 0	N. 73 30 W. 72 30	17 0	S. 83 30 W. r 84 0	6 15	N. 69 0 W. r 65 10	22 55	S. 58 0 W. r 62 0	30 0
		W. by N.	W. by N. ¼ N. northerly.	3 0	N. 82 30 W.	3 45	N. 50 0 W.	28 45	N. 61 40 W.	17 5	N. 83 30 W.	4 45	55 0	23 45	S. 77 0 W.	24 15
		W.N.W.	N.W. by W. ½ W.	5 37	67 0	0 30	41 30	26 0	50 30	17 0	68 30	1 0	43 30	24 0	W. 2 0 N.	24 30
		N.W. by W.	N.W. ⅜ W.	4 12	54 0	2 15	35 0	21 15	41 40	14 35	56 30	0 15	34 0	22 15	N. 79 0 W.	22 45
·5	46	N.W.	N.W. ½ N. r N.W. ½ N.	6 0	r 44 30 42 30	2 30	r 27 0 27 30	17 45	32 30 32 30	12 30	43 0 r 42 30	2 15	20 0 r 24 30	22 45	N. 52 0 W. r 59 0	10 30
		N.W. by N.	N.W. by N. ½ N.	5 37	31 0	2 45	20 0	13 45	23 30	10 15	30 30	3 15	16 0	17 45	41 0	7 15
		N.N.W.	N.N.W. ½ N. northerly.	3 0	20 30	2 0	13 0	9 30	14 30	8 0	17 30	5 0	N. 6 30 W.	16 0	12 0	10 30
		N. by W.	N. by W. ¼ N.	2 48	r 11 0 13 0	0 45	6 15	5 0	5 30	5 45	13 0	1 45	N. 1 0 E.	12 15	N. 2 0 W.	9 15
·4	48	North.	N. ¼ E. r N. ⅛ E. r N. ¼ E.	2 16	N. 0 30 E. r N. 0 30 N.	0 0	N. 0 30 W. r N. 1 0 W.	0 45	N. 2 0 E. r N. 1 40 E.	1 50	N. 6 30 E. r N. 3 30 E.	5 0	N. 9 0 E.	9 0	N. 11 0 E. r N. 5 0	8 0

* r signifies that the observation was repeated; and it will be observed that at some of the positions nearest to the iron-work (such as the binnacle) considerable discrepancies occurred.

§ 2. *Results of Experiments.*

The principal results are arranged in a tabular form, with plans of the Garryowen (iron steam-vessel), so as to present at one view the relative positions of the compasses in different parts of the vessel; and the results obtained (both as relates to the horizontal deflections of the magnetic needle, and also as respects the dip and intensity) on board, compared with those observed with the same instruments on Tarbert Island. And I must here express my thanks to Professor CHRISTIE for his suggestions on observations to be made on the dip and intensity at two positions on board, the one near the head, and the other near the stern (A and B), where the centre of the dipping-needle was to occupy as nearly as possible the same position as the pivot of the azimuth-compass, with which the horizontal deflections were observed.

From having noticed a considerable embarrassment in the movement of the compasses, to which my attention was more particularly directed, viz. those at A and B, I was not entirely unprepared for the discrepancies that appeared between the first and second series of observations.

It is necessary to remark in this place, that during the second revolution of the vessel the fires were lighted and the steam was 'up'; but in considering the differences that appear, both as relates to the deviation deduced from the simultaneous observations and that obtained by the bearings of the cone of the distant mountain (see Table II.), and likewise the differences, which are still greater, in the results obtained during the second revolution of the vessel, especially towards the north (see Table III.), I am not enabled to attribute such differences to the circumstance of the fires being lighted; nor am I prepared to place them to the account of the sheering of the vessel, as they far exceed any limits that might reasonably be allowed for such a possible circumstance; but the freedom of motion of the needles might possibly have been affected by the local influence, or rather influences, in the vessel, that is, by the proximity of certain masses of iron*, probably imbued with magnetic powers acting in various directions, and consequently weakening the directive power of the needles and embarrassing their movements, notwithstanding that the compasses were at the several distances from these masses, which are noted in Table X., page 288.

It was my intention to have put that part of THEIR LORDSHIPS' orders relating to the trial of Professor BARLOW's correcting-plate in execution at the position A on board; but the results obtained there did not hold out a probability of its successful application, the discrepancies already alluded to being such as to prevent "the plane of no deviation" being accurately determined; and consequently the true position for fixing the plate (according to the law determined by Professor BARLOW) could not be

* The iron davits and stanchions abaft, and the spindle of the wheel, are here particularly alluded to.

obtained with sufficient exactness, from the cause I have already mentioned, viz. the too near proximity of certain masses of iron.

In most cases "the plane of no deviation" has been found in the direction of the keel; there are, however, exceptions to this; and the little approximation to such a line that may be deduced from the observations at A, shows that in the Garryowen at that position the line of no deviation would be oblique to the keel.

Comparing the results obtained at positions A and B, and referring to the plan of the vessel, which shows the variety of positions in which malleable iron is placed,—such as in the beams, ribs, sheathing, rails, stanchions, &c., in all of which magnetic influences might exist, I determined to remove the compasses further from the deck, and consequently further from the particular portions of the iron work which appeared to me to affect them.

These observations, while they proved that the distance of 5 or 6 feet from any iron work was not sufficient for placing a compass at A and B on board the Garryowen, they at the same time clearly demonstrated the necessity of observing the comparative effect produced on the dip and magnetic intensity by the vessel; for although the horizontal deflections of the needle did not appear to be excessive at position A, yet it was evident that its directive power was affected.

The very unfavourable weather that prevailed prevented me from making all the observations on the dip and intensity that I desired, or those suggested, as both the vertical and horizontal vibrations, as well as the dip, should be determined with the vessel's head at the true magnetic north and south, and likewise at the north and south points indicated by the needle at the position on board; besides which similar observations should be made with the vessel's head at east and west, the object being to ascertain how far the directive power of the needle may be affected at certain parts of the vessel. The observations on the dip and intensity which I was enabled to make are registered in the following Tables VI. and VII.

Table VI.

Register of the Dip of the Magnetic Needle observed at three positions on board the Garryowen, and compared with that observed on Tarbert Island.

	Face of instrument.	At Tarbert Island, Nov. 16, 1835. Poles of needle direct. Reading of needle. Lower end.	Upper end.
Face of needle to face of instrument.	East.	° ′ 70 20 70 20	° ′ 70 15 70 15
	West.	71 25 71 30	71 20 71 25
	North.	89 30 W. or 31′ W. of 90°.	89 25 E. or 35′ E. of 90°.
	South.	89 25 E. or 35′ E.	89 30 W. or 30′ W.
Face of needle reversed.	East.	71 15 71 15	71 15 71 10
	West.	70 30 70 25	70 25 70 30
	North.	89 35 E. or 25′ E.	89 30 W. or 30′ W.
	South.	89 35 W. or 25′ W.	89 15 E. or 45′ E.

Barom. 30·1, Therm. 46°.

Face of instrument.	At position A on board, Nov. 2, 1835. Poles of needle direct. Vessel's head north. Reading of needle. Lower end.	Upper end.	Vessel's head south. Reading of needle. Lower end.	Upper end.	At position B, Nov. 6. Poles of needle direct. Vessel's head north. Reading of needle. Lower end.	Upper end.	Vessel's head south.* Reading of needle. Lower end.	Upper end.	At position I, Nov. 13. Poles of needle direct at I. Vessel's head north. Reading of needle. Lower end.	Upper end.	Vessel's head south. Reading of needle. Lower end.	Upper end.
East.	78 0 vibrating to 78 15	78 5	73 25 vibrating to 73 45	73 15 vibrating to 73 45	Weather unfavourable.		66 45 vibrating to 67 30	66 30 vibrating to 67 15	72 30 vibrating to 73 15	72 45	74 45 vibrating to 74 30	74 50 ....
West.	78 30 vibrating to 79 0	78 40	74 0 vibrating to 74 40	73 40 vibrating to 74 40	Not observed.		68 30	68 40	73 30 vibrating to 74 0	73 40	74 15	74 20 ....

Position A: Barom. 29·8, Therm. 49°.—A slight motion in the vessel.

Position B: Barom. 29·9, Therm. 44°.

Position I: Barom. 30·5, Therm. 46°.

Arithmetical mean of the dip observed on board the Garryowen, with the face of the instrument at east and west, and with the face of the needle to the face of the instrument.

At position A (abaft). Vessel's head north.	Vessel's head south.	At position B (forward). Vessel's head north.	Vessel's head south.	At position I (abaft). Vessel's head north.	Vessel's head south.
78° 25′ 0″	73° 54′ 0″	Not observed.	67° 31′ 40″	73° 16′ 40″	74° 0″

Arithmetical mean of the dip observed on Tarbert Island.

Poles of needle direct, with the face of the instrument to the east and then to the west, and with the face of the needle to the face of the nstrument .... 70° 51′ 15″.

Ditto, with the face of the needle reversed .... 70° 50′ 37″.

Between each observation the needle was lifted on the Ys.

* During these observations the vessel sheered 2° to the east of south.

Table VII.

Vertical Vibrations for Intensity observed at Tarbert Island and on board the Garry-owen.

At Tarbert Island, Nov 19.—Barom. 30, Therm. 52°. Poles of needle direct.

Face of needle to face of instrument	Face of instrument	Reading of needle. Lower end.	Reading of needle. Upper end.	Arc.	Time.
		° ′	° ′	°	h m s
West.		71 25	71 25	24½	12 52 1
		......	......	...	24
		......	......	...	49·5
		......	......	...	14·5
		......	......	...	38·5
		......	......	...	4
		......	......	...	29
		......	......	...	54
		......	......	...	19
		......	......	...	43·5
		71 0	71 10	7½	12 56 8
		Time of 100 vibrations 4m 7s.			
East.		70 25	70 25	23½	12 8 57·5
		......	......	...	20·5
		......	......	...	45·5
		......	......	...	10
		......	......	...	35·5
		......	......	...	0·5
		......	......	...	25·5
		......	......	...	51
		......	......	...	15·5
		......	......	...	40
		......	......	6	12 13 5
		Time of 100 vibrations 4m 7s·5.			

Moderate breezes from the west. Dark cloudy weather.

At position A on board, Nov. 2.—Barom. 29·9, Therm. 52°. Poles of needle direct.

Face of instrument	Vessel's head north. Lower end.	Upper end.	Arc.	Time.	Vessel's head south. Lower end.	Upper end.	Arc.	Time.
	° ′	° ′	°	h m s	° ′	° ′	°	h m s
Face of instrument west.	78 45	78 30	23½	4 42 7·5	73 30	Do.	24	2 52 7·5
	......	......	...	28	to		...	26
	......	......	...	49·5	74 30	Do.	...	47·5
	......	......	...	11	......	......	...	9
	......	......	...	30·5	......	......	...	29·5
	......	......	...	52	......	......	14	51·5
	......	......	...	13	......	......	...	12·5
	......	......	...	33·5	......	......	...	34
	......	......	7½	50·5	......	......	...	55
	......	......	...	11	......	......	...	16
	......	......	4½	4 45 31·5	......	......	8	2 55 37·5
	Time of 100 vibrations 3m 24s.				Time of 100 vibrations 3m 30s.			
	At position A on board, Nov. 6.—Barom. 29·9, Therm. 48°.							
Face of instrument west.	......	......	...	......	73 50	Do.	24	3 7 3·5
	......	......	...	......	to		...	22·5
	......	......	...	......	74 0	Do.	...	44
	......	......	...	......	......	......	...	5
	......	......	...	......	......	......	...	26·5
	......	......	...	......	......	......	...	48·5
	......	......	...	......	......	......	...	10
	......	......	...	......	......	......	...	30·5
	......	......	...	......	......	......	...	52
	......	......	...	......	......	......	...	13
	......	......	...	......	......	......	8	3 10 34
					Time of 100 vibrations 3m 30s·5.			

Calm and cloudy.

At position I on board, Nov. 13.—Barom. 30·5, Therm. 46°. Poles of needle direct.

Face of instrument		Vessel's head south. Lower end.	Upper end.	Arc.	Time.
	The state of the weather prevented the observations at I, with the vessel's head north, from being made.	° ′	° ′	°	h m s
Face of instrument west.		74 0	74 50	24	2 46 57
		to	......	...	17·5
		74 30	......	...	40
		......	......	...	4
		......	......	...	26
		......	......	...	49
		......	......	...	11·5
		......	......	...	34
		......	......	...	57
		......	......	...	20
		......	......	7	2 50 42·5
		Time of 100 vibrations 3m 45s·5.			
Face of instrument east.		74 15	74 20	24	3 17 58
		......	......	...	18·5
		......	......	...	41·5
		......	......	...	4·5
		......	......	...	27
		......	......	..	50·5
		......	......	...	13·5
		......	......	...	36
		......	......	...	59
		......	......	...	22
		......	......	5¾	3 21 44·5
		Time of 100 vibrations 3m 46s·5.			

Calm and cloudy.

It will be sufficient in this place to notice that the dip on Tarbert Island was 70° 51′ with the face of the instrument to the east and west, and that at the position A on board the Garryowen, when her head was to the true magnetic north, the dip was 78° 25′, and when her head was to the south the dip was 73° 54′. The dip observed at several positions is inserted in Table VI., and the vertical vibrations for intensity in Table VII.; but further comment is unnecessary until the observations shall have been repeated when the vessel is aground, and her head in the directions noted above.

The horizontal vibrations for intensity were obtained with Hansteen's needle on Tarbert Island, but the state of the weather prevented me using that instrument on board.

During the several revolutions of the vessel I availed myself of the opportunity to ascertain the deviation in different parts of her, so far at least as common compasses were capable of developing it, and the observations at P, L, G, Q, T, C must be scrutinized only with reference to the nature of these instruments*; for although good of their kind, they were too heavy to be influenced by those minute changes that would have been indicated by delicately constructed instruments.

Several of these observations were simultaneous, the bell of the vessel being struck as the signal for observation when her head was steady upon the required point.

A platform, which was so constructed as to project over the stern on a level with the tafrail, enabled me to observe the deflections of a compass in that position, which was at the distance of 13½ feet from any kind of iron; and a temporary stage or poop, which was also erected at my request, 8 feet above the deck, afforded me the means of ascertaining the deviation in several positions at that elevation, as the vessel was warped round.

Some of these observations indicate that the deflections of the needle had reference rather to the nearer portions of iron than to the position and distance of the centre of the mass; for instance, the difference of positions of E and D (at which were placed small needles without the incumbrance of cards) with respect to the probable centre of the mass, will not account for the difference of deviations observed.

§ 3. *Several distinct positions reviewed from the results obtained, with reference to placing a Steering-Compass.*

The observations at F and M on the platform over the stern show that no advantage would be gained by placing a compass there; and this remark is applicable to several of the other positions tried, as will be readily seen by the results inserted in Tables I. and V.

There were other positions, however, which held out better hopes, from the deviation observed there being less than that which had been ascertained to exist in some

* The exact positions of these compasses are inserted in Table X. page 288; and their relative places will be clearly understood by referring to the longitudinal section of the vessel.

of His Majesty's ships. These positions were G*, P, C†, and, by the last series of observations, I, may be added: the three former, however, were inconvenient situations, and it therefore became necessary to try the latter (I) more particularly, by observing the dip and intensity as well as the horizontal deflections, with a view to ascertain how near a compass might be approached towards the deck with propriety, for the discrepancies observed at D, I attribute to the proximity of the boats' davits.

It will be proper to notice in this place some particulars which bear upon the question as relates to three parts of the vessel, viz. forward, amidships, and abaft; it being here understood that the place spoken of for the compass is above the mass of iron in these parts.

1st, *Forward.*—The forecastle regarded as a position for a compass is not favourable in a practical point of view ‡, as it would be in the way of a variety of work; and in small vessels, during gales of wind, as the men cannot always keep their stations there, it would be insecure.

The anchors, chain-cables, &c. being in the immediate vicinity, would be disadvantageous as relates to the local attraction, and with respect to motion (and in an iron vessel magnetism) it is not a favourable position §.

2nd, *Amidships.*—The centre, or rather immediately above the centre as respects length and breadth, would be a desirable place as to motion, and probably as to magnetism, if the large funnel were made of copper instead of iron; that is, if we may consider the vessel as an entire magnet; but against these premises is one of considerable consequence, viz. that the connecting-rods, shafts, cross-heads, &c. of the machinery are moveable quantities, and therefore it would be improper to place the compass within the range of their influence.

Between the foremast and the great funnel, the crane, being made of iron, and also the crank and cylinder-hatches, renders such a position unfavourable.

3rd, *Abaft.*—From the position of the great funnel, and the disposition of a variety of smaller portions of iron above the deck of the Garryowen, the quarter-deck (and of course above the mass of iron) appears to me to be the most eligible position for a steering-compass.

* Previously to the compass G being placed on the cross-plank (which was supported by two wooden ladders) the chain peak halyards were unrove, and rope was substituted; and after the first revolution of the vessel two small blocks on the gaff having been discovered to have iron pins, they were removed, so that the small bolt for the peak downhaul only remained, and this was 4 feet above the compass.

† After the first series of observations with compass C (which was mounted on glass legs) the large hoop of iron which was on the outer end of the bowsprit, about 4 feet from the compass, was removed, but the iron pin for the sheave block, which was nearly the same distance, still remained, as it was found inconvenient to displace.

‡ I am aware that it has been suggested to steer steam-vessels on the forecastle, by leading their tiller-ropes forward; but the above remarks have reference only to placing the compass.

§ These remarks do not apply to the bowsprit, as the results at C are so curious as to require further experiments, especially when contrasted with those over the stern.

Taking into consideration the situation of particular portions of the iron-work, and also the magnetic effect of the head and stern of the vessel, (which is described at page 282,) about one seventh the length of the vessel from the stern is an advantageous place on board the Garryowen.

The elevation above the deck should be such as to remove the needle from the separate actions of particular portions of the iron-work, so that if possible the little irregularities might disappear, and the joint effects of all the iron in the vessel be resolvable into one force, the power of which might be discovered, and perhaps controlled. If that of simple iron, by Professor Barlow's correcting-plate, and if that of the pole of a magnet, it might be useful to ascertain how far another magnet in a given position was capable of correcting the deflections.

How near the deck may be approached with security has yet to be determined. At the position I, on board the Garryowen, (which is $13\frac{1}{2}$ feet above the deck,) the horizontal deflections are inconsiderable, and the dip and intensity, so far as the very unfavourable weather permitted them to be observed, warrant the belief that a nearer approach might be made. This, however, can only be proved by experiment; and the remarks at page 276, and also at page 286, respecting the necessary experiments, are strictly applicable in this place.

Had the weather permitted, the effect of Professor Barlow's correcting-plate would have been tried at position I; but with the continuance of rain and gales of wind there was no possibility of proceeding further with any chance of that accuracy which the nature of the service directed by Their Lordships demanded.

§ 4. *Observations on the magnetic effect of the Head and Stern of the Garryowen.*

From the very remarkable deflections observed at the positions F and L*, and subsequently at M, I determined to ascertain the difference of effect which the head and stern of the vessel might produce on the compass. For this purpose I placed several magnetic needles on the small quay at the south-west side of Tarbert Bay; and among them was a dipping-needle, adjusted in the magnetic meridian, with the face of the instrument to the east, and indicating nearly the dip that I had previously observed on Tarbert Island. All the needles were placed out of the reciprocal influence of each other, and assumed their respective magnetic meridians.

Having taken my station at one of the compasses, Mr. William Laird and the engineer observed the others. A line was then passed from the vessel to the quay, for the purpose of measuring the several distances; and everything being in readiness, I directed the Garryowen to be warped from the south-east quarter, with her head towards the instruments on the quay. It is essential to remember that the unmarked ends (or those which pointed to the south) of all the needles were nearest to the vessel, and at the distance of 214 feet they retained their natural directions in their

* See Tables I. and V.

respective magnetic meridians *; but when the head of the vessel approached to the distance of 189 feet towards needle O, and 169 feet from needle D, the "unmarked ends" of both distinctly indicated *easterly* deviation, and at the distance of 81 feet the dipping-needle indicated a *decrease* of the northern dip.

As the vessel continued advancing with her head towards the instruments, the deflections of the horizontal needles to the *eastward* increased, and the northern dip *decreased*; so that on the near approach of the head the dip had decreased $1\frac{3}{4}°$, and the *easterly* deviation of the horizontal needles amounted to from 5 to 7 degrees.

The vessel was then swung round, so that her stern was placed as nearly as was practicable in the positions that her head had previously occupied, and precisely opposite effects on the several needles were clearly developed; viz. the unmarked ends (or those which pointed to the south) of the horizontal needles were deflected to the *westward*, and the northern dip was *increased*.

These experiments were repeated on the following day with similar results as to the direction of deflection to those obtained on the 17th of November, the whole of which will be found registered in the following Tables.

* The horizontal needles used in this experiment were suspended on pivots in the usual way, so that it is probable a very delicately constructed instrument, with a different suspension, such as is used for determining the diurnal variation, would have developed a deflection at a greater distance.

TABLES VIII. and IX.

Showing the Deflection both of the Horizontal and Dipping Needle when the Garry-owen was warped towards them, first with her Head nearest to the Instruments, and then with her Stern.

III. Nov. 17, 1835.—Barom. 30·1, Therm. 51°.

...agnetic ...tion in ... vessel ...ached.	Distance of nearest part of vessel from dipping-needle on quay.	Vessel's HEAD nearest to the instruments. Reading of dipping-needle.	Distance from D.	Reading of needle D.	Distance from O.	Reading of needle O.	Vessel's STERN nearest to the instruments. Distance of nearest parts of vessel from dipping-needle.	Reading of dipping-needle.	Distance from D.	Reading of needle D.	Distance from O.	Reading of needle O.
°	feet. 214 from stem	° ′ 70 30	feet. 208	South.	feet. 223	South.	feet. 214 from tafrail.	° ′ 70 30	feet. 208	South.	feet. 223	South.
	182	70 30	169	0° 15′ E.	189	0° 15′ E.	182	70 30	169	0° 10′ W.	189	0° 5′ W.
13 E.	81	70 20	76	0 45 E.	90	0 50 E.	81	70 50	76	0 30 W.	90	0 45 W.
	42	69 45	29	3 30 E.	51	2 10 E.	42	71 15	29	2 40 W.	51	2 30 W.
	22	68 45	9	7 5 E.	31	5 56 E.	22	72 0	[a]	16 30 W.	31	7 5 W.

Wind from the west, rainy weather.

[a] Tide carried stern nearer to compass, but distance was not measured.

. Nov. 18, 1835.—Barom. 29·9, Therm. 52°.

...tic ...f ...ch.	Dipping-needle. Distance of nearest part from instrument.	Reading of needle. HEAD nearest.	Reading of needle, STERN nearest.	Needle D. Distance from D.	Reading of needle, HEAD nearest.	Reading of needle, STERN nearest.	O. Dist. from O.	Reading of needle, HEAD nearest.	Reading of needle, STERN nearest.	E. Dist. from E.	Reading of needle, HEAD nearest.	Reading of needle, STERN nearest.	V. Dist. from V.	Reading of needle, HEAD nearest.	Reading of needle, STERN nearest.
	feet. 276	° ′ 70 25	° ′ 70 25	feet. 256	South.	South.	feet. 265	South.	South.	feet. 262	South.	South.	feet. 273	South.	South.
	185	70 25	70 25	166	0° 5′ E.	0° 10′ W.	175	South.	South.	172	0° 25′ E.	0° 15′ W.	184	South.	0° 15′ W.
	78	70 10	70 35	59	1 30 E.	0 50 W.	68	0° 50′ E.	0° 45′ W.	65	1 30 E.	1 0 W.	76	1° 15′ E.	0 50 W.
E.	47	69 50	70 40	26	2 30 E.	4 10 W.	37	2 10 E.	2 50 W.	34	4 0 E.	4 55 W.	45	3 30 E.	3 0 W.
	36	69 30	70 50	16	5 30 E.	15 0 W.[a]	26	3 50 E.	6 30 W.	23	9 0 E.	13 0 W.	35	5 45 E.	5 25 W.
	29	68 50	70 50		6 0 E.	31 50 W.[a]	19	4 5 E.	9 50 W.				26	7 35 E.	6 15 W.

Moderate breeze, with showers of rain.

[a] Tide forced vessel near to instrument.

In these observations the instruments were placed on a small quay on the west side of Tarbert Bay. Breadth of the S.E. end of quay $33\frac{1}{2}$ feet. Height of quay above high water 3 feet. Iron tafrail above water line 11 feet. Vessel's draught of water 5 feet 3 inches. Stern of vessel above water line $8\frac{1}{2}$ feet.

In considering the contents of these Tables, especially as respects the *amount* of deflection, it is requisite to bear in mind that the needles, being so placed on the quay as to avoid the influence of each other, were at different distances from the vessel, and not in the same line of bearing; consequently, when the vessel approached very near to the quay with her head or stern, the true magnetic bearing of those parts from each instrument was necessarily different: hence the difference of the *amount* of deflection.

As an iron ball, or disc, when made to approach a magnetic needle from the south-east, would produce a different deflection when its centre was above the equator of the dipping-needle to that which would occur when it was below, viz. in one case the deflection would be to the eastward, and in the other to the westward, due consideration was given to that circumstance in the examination of the results; and the following Table, showing the elevation of the vessel's stem and stern, as related to the level of the needles, and likewise the depression of the keel below their level, may be useful in reviewing this subject.

The observations having been made from about an hour and a half before to an hour after high water, a mean level has been taken in the construction of this Table; and it is to be understood that the tide neither rose three feet above nor fell three feet below this mean level during the experiments in question.

Part of vessel nearest to the instruments.	Dipping-needle.		Compass O.		D.		E.		V.	
	Upper part of stern.	Keel below axis.	Upper part of stern.	Keel below pivot.	Upper part of stern.	Keel below pivot.	Upper part of stern.	Keel below pivot.	Upper part of stern.	Keel below pivot.
Head.	1 foot below axis.	12¾ feet.	0½ feet below pivot.	14¼ feet.	4½ feet above pivot.	9 feet.	4½ feet above pivot.	9 feet.	4½ feet above pivot.	9 feet.
Stern.	Iron rails over stern 3½ feet above axis.	12¾ feet.	Iron rails over stern 2 feet above pivot.	14¼ feet.	Iron rails over stern 7 feet above pivot.	9 feet.	Iron rails over stern 7 feet above pivot.	9 feet.	Iron rails over stern 7 feet above pivot.	9 feet.

It will be noticed, that as the stern approached the instruments the iron rails over the tafrail of the vessel were above the level of the compass O; and when the head approached, the upper part of the stem was rather below that level; but with respect to the other needles D, E, V, both the upper parts of the stem and stern were above their level.

The above remarks, however, only relate to those portions of the vessel that approached nearest to the instruments; but in looking to the probable centre of the entire mass of iron in the Garryowen, and the different heights of the compasses on the quay, it is impossible to attribute the opposite deflections which occurred to the difference of elevation of the centre of the mass that could take place by the rise and fall of the tide during the observations.

The conclusion, therefore, which I have come to is, that the deflections alluded to were caused by the magnetic influence of the iron in the vessel; the combined effect of that about the bows representing the marked end of a magnet, (or that which would point to the north,) and that about the stern the unmarked end, (or that which would point to the south,) the effects on the different needles being precisely similar to those which would have occurred had a magnet been placed in the position of the vessel.

As both the bower-anchors were at the bows when the Garryowen approached the quay, magnetic effects may have been produced by their shanks and iron stocks; but at the distance of 189 feet and 169 feet, where the instruments clearly developed their respective deflections, (see Tables VIII. and IX.) the result must, in my opinion, be attributed to the combined magnetic effect of the iron in those parts of the vessel which were nearest to the instruments.

From the nature of the results I obtained on board the Garryowen, it would be advisable, as opportunities occur, to ascertain if similar effects are produced on the magnetic needle by other iron vessels, especially as respects the polarity of their heads and sterns, both before they have been put in motion through the water, and afterwards: this too might lead to other observations relative to the little oxidation that is reported to have taken place in the iron steam-vessel on the coast of Africa.

As in the construction of iron vessels, hammering the numerous rivets might elicit magnetic influences, it would be well to note, by compass, the direction of their heads and sterns when building, with a view of ascertaining whether (in combination with the former circumstance) any distinct magnetic properties indicated by those parts are due to the line of direction of the vessel with respect to the magnetic meridian.

The head of the Garryowen, when building, was west-north-west.

§ 5. *General Conclusions deduced from the experiments already made, with Notes on those which are requisite to be tried.*

The general conclusions relating to practical purposes that may be deduced from the experiments already tried are:

1st, That the ordinary place for a steering-compass on board ship is an improper position for it on board an iron steam-vessel.

2nd, That the binnacle-compass, in its usual place on board the Garryowen, is so much in error as not to be depended upon.

3rd, That in selecting a position for a steering-compass on board iron steam-vessels, it is requisite that it should be placed, as far as may be practicable, not only above the general mass of iron *, but also above any smaller portions of iron in its vicinity, or that such portions should be removed †.

* The great funnel is not alluded to here; but should that prove to be an impediment, copper might easily be substituted.

† The boat's davits, iron stanchions, and rails about the quarter-deck are particularly alluded to, it being easy to substitute wood for the former and copper for the latter.

4th. That it never should be placed on a level with either the ends of horizontal of perpendicular bars of iron.

5th. That the extreme ends * of an iron vessel are unfavourable positions, owing to magnetic influences, and that the connecting-rods, shafts, &c. of the machinery, being moveable quantities, renders the centre objectionable, independently of the position of the great iron funnel.

6th. That no favourable results were obtained by placing the compass below the deck, nor on a stage over the stern.

7th. That at the positions G, $20\frac{1}{2}$ feet above the quarter-deck, and I, $13\frac{1}{4}$ feet above the same level, and about one seventh the length of the vessel from the stern, the deflections of the horizontal needle were less than those which have been observed in some of His Majesty's ships; but in order to prove whether this or a nearer approach to the deck may be fixed upon as a proper place for a steering-compass, the following experiments should be tried.

The horizontal deflections at the different points as before, both with the fires lighted and extinguished†. The dip and magnetic intensity to be ascertained both when the vessel's head is at the true magnetic north and south, and likewise when it is at the north and south indicated by the needle at the position of observation on board; and further, that the vertical and horizontal vibrations should be determined when the vessel's head is towards the east and west; and it would tend to accuracy if these were obtained when the vessel was aground.

It would be desirable to repeat some of the experiments after the keel of the vessel has remained some time in the line of the magnetic meridian, and afterwards to compare them with those which might be obtained after her head had been kept to the east and west for the same length of time.

The vibrations of the needle, and a few bearings that could be quickly and accurately obtained after the vessel has been in motion through the water, would be desirable. The observations which were commenced on a needle placed in the centre of an iron sphere (3 feet in diameter) should be completed.

As it is known that iron when heated to a red colour attains an extraordinary magnetic power, it would be advisable to observe whether a very delicately constructed needle suspended in the position where the compass is to be placed, be affected at such a distance from the furnace on board, and this should be accomplished when the vessel is aground.

Similar experiments to those tried on the quay in Tarbert Bay might be repeated with advantage after the vessel has been in motion through the water, and needles ascertained to be of different intensities might be used.

Besides ascertaining the horizontal deflections of the needle produced by the local

* This remark does not apply to the bowsprit, as the curious results obtained at C, which have been already noticed, show that further observations are requisite.

† Especial notice of the compass on the bowsprit should be taken during the several revolutions of the vessel.

attraction of the vessel, the object of some of the experiments above enumerated (and which suggested themselves to me during the course of my inquiry) is to determine whether the causes that affect the direction of the magnetic needle on board an iron steam-vessel are constant or variable; and whether, should they prove to be the latter, that variation may amount to such a quantity as would be seriously detrimental in a practical point of view; or to prove that under ordinary circumstances the directive power will be sufficient for all the purposes required.

Some striking facts respecting the local attraction of ordinary steam-vessels having come under my immediate notice, I have considered it my duty to detail them at the end of the report of experiments, and have added some practical observations on placing compasses on board steam-vessels generally, to which I beg you will call Their Lordships' attention.

Hoping that the results which have been already obtained will not only be advantageous as a guide to future experiments, but also that they will prove useful in the practice of navigation, I have now to request that you will be pleased to lay this Report, with the accompanying Tables, before my Lords Commissioners of the Admiralty.

I have the honour to be, Sir,

Your very humble Servant,

E. J. JOHNSON,

Commander R.N.

To Charles Wood, Esq. M.P.,
Secretary to the Admiralty, &c. &c. &c.

Table

Table X.

Position of the different Compasses on board, with their distances from particular parts of the Iron-work.

Letter or Mark of Compass.	Position.	Distance of the Pivot from particular parts of the iron-work.
A.	Quarter-deck.	Above the deck, 5 feet 9 inches. From nearest iron beam, 5 feet 11⅜ inches. From nearest end of spindle of wheel 6 feet 6 inches, and 3 feet 3 inches above level. From iron tiller, 8 feet 2 inches. From iron rails abeam, 9 feet. From nearest part of davits when out, 10 feet 8¾, when inboard, 7 feet 1⅛ inch. From the great funnel, 39 feet 7 inches. From tafrail, 11 feet 5½ inches.
B.	Forecastle.	Above the deck, 5 feet 11 inches. From nearest iron beam, 6 feet 1½ inch. From iron abeam, 11 feet 2 inches. From chain cables, 6 feet on both sides. From iron windlass, 7 feet 5 inches. From pea of anchors, 10 feet 8½ inches. From the stem, 19 feet 8¼ inches. From great funnel, 55 feet 4½ inches. From end of crane, 18 feet 1 inch. From iron wire of skylight, 4 feet 1 inch.
C. on glass legs.	On bowsprit.	From the iron ring at the end of bowsprit, 4 feet 6 inches. From iron above the stern, 6 feet 7 inches. From iron cat head, 11 feet 3 inches.
D.	On the fore part of the temporary poop.	Above the deck, 8 feet 5¾ inches. From tafrail, 15 feet 9 inches. From the large funnel, 34 feet 8 inches. From nearest part of davits when out, 10 feet 11 inches.
E.	On the after part of the temporary poop.	From the deck, 8 feet 5 inches. Horizontal line to tafrail, 3 feet 3 inches. From nearest iron on tafrail (oblique), 7 feet 4½ inches. From spindle of wheel, 8 feet. From great funnel, 47 feet 10 inches.
F.	On the stage level with the tafrail.	From iron rails over the tafrail, 13 feet 6½ inches. From nearest part of davits, 18 feet 2 inches.
G.	On a plank 4 feet below the main gaff end.	Above the deck, 20 feet 7 inches. From iron bolt in gaff end, 4 feet 8 inches. From the great funnel, 32 feet. From the iron rails above the tafrail, 24 feet 6 inches.
I.	On the centre of the temporary poop.	Above the deck, 13 feet 4½ inches. Horizontal line to tafrail, 10 feet. From the great chimney, 40 feet. From the nearest part of davits, 13 feet 9 inches.
L.	On the poop projecting over the stern.	Horizontal line to perpendicular of tafrail, 3 feet 9 inches. From tafrail, 6 feet 4 inches. From davit, 21 feet 3 inches. From the great funnel, 54 feet 3 inches.
M.	On the stage over the stern level with the tafrail.	First position, 9 feet 11½ inches from tafrail. Second position, 5 feet 8 inches from tafrail. Third position, 4 feet from tafrail.
O.	Between the paddle-boxes.	From the great funnel, 22 feet 3 inches. From the iron rails between the paddle-boxes, 7 feet. From the nearest part of the crane, 12 feet.
P.	Two thirds up the foretopmast.	Above the deck, 40 feet 2 inches. From crane, 29 feet 3 inches. From the iron cap of the mast, 6 feet 11 inches. From the iron crosstrees, 12 feet 6 inches.
Q. on glass legs.	In the fore-hold.	From foremost bulkhead, 9 feet 6 inches. From aftermost bulkhead, 9 feet 6 inches. From nearest iron beam above, 4 feet 7½ inches. From ditto below, 4 feet 7½ inches. From sides of vessel, 9 feet 10 inches. From spindle of crane, 8 feet 4 inches, larboard side.
S.	In the iron sphere amidships.	Above the deck, 7 feet. From great funnel, 24 feet 5 inches. From iron rails on paddle-boxes, 10 feet 2 inches. From nearest part of crane, 9 feet.
T.	In the cabin.	From foremost iron bulkhead, 4 feet 7 inches. From iron stove abaft, 4 feet 7 inches. From iron beams above, 5 feet 4 inches. From ribs below, 5 feet 4 inches.
		From the sides of the vessel, 9 feet 11 inches.

XVII. *Researches on the Tides.—Sixth Series. On the Results of an extensive system of Tide Observations made on the coasts of Europe and America in June* 1835. *By the Rev.* William Whewell, *M.A., F.R.S., Fellow of Trinity College, Cambridge.*

Received June 2,—Read June 16, 1836.

Sect. I. *Introduction.*

1. I HAVE already, in communications to the Society, urged the importance which belongs to simultaneous tide observations made at distant places; and I have also stated some of the steps which have been taken in consequence of representations to this effect. Observations were made and continued for a fortnight in June 1834, at the coast-guard stations in Great Britain and Ireland; and I have given an account of some of the results of these observations in a paper already printed in the Transactions*. Being encouraged by the general interest taken in the subject, and by the desire to promote this branch of knowledge manifested by those who had officially the means of doing so, especially by Captain Beaufort, the Hydrographer of the Admiralty, I solicited a repetition of the coast-guard tide observations in June 1835, and also ventured to recommend that a request should be made to other maritime nations, to institute simultaneous tide observations on their coasts. The British observations were undertaken with the same readiness as before by Captain Bowles, the Chief Commissioner of the Coast-Guard Service. The proposal for the foreign observations was entertained and promoted with great zeal by the Board of Admiralty; and the Duke of Wellington, at that time Foreign Secretary of State, being applied to, to forward the scheme, His Grace fully acceded to the application, and made requests to foreign governments to join in the undertaking, in a manner which procured from them the most cordial and effective cooperation. Through the ambassadors of the maritime powers of Europe, and through A. Vail, Esq., the Chargé d'Affaires of the United States, who entered into this design with great interest, arrangements were made, and directions circulated, for simultaneous tide observations from the 8th to the 28th of June. These observations were made, for the most part with great care, under the direction of intelligent officers and men of science.

2. The chain of places of observation extended from the mouth of the Mississippi, round the Keys of Florida, along the coast of North America, as far as Nova Scotia; and from the Straits of Gibraltar, along the shores of Europe, to the North Cape of Norway. The number of places of observation was twenty-eight in America, seven in Spain, seven in Portugal, sixteen in France, five in Belgium, eighteen in the

* Part I. for 1835, p. 83.

Netherlands, twenty-four in Denmark, and twenty-four in Norway; and observations were made by the coast-guard of this country at 318 places in England and Scotland, and at 219 places in Ireland. Among the persons who superintended these observations on an extensive scale, I have profited in an especial manner by the labours of M. Möll, who directed and arranged those made in the Netherlands; M. Tegner, who has performed various reductions on the Danish observations, besides superintending a large portion of them; and M. Beautems-Beaupré, who has for some years been occupied with valuable hydrographical labours on the coasts of France. In several other cases in which the observations have been conducted in a very accurate and scientific manner, I do not find it stated, in the communications which contain the registers, under whose general direction the operations were carried on. The names of the particular observers will be found in the Tables appended to this memoir. I have not used the whole of the observations sent; as some, from the situation of the places, or from other causes, could not be made subservient to my general purpose. For instance, I have for the present omitted some, on account of their manifestly irregular character; others, because, being made at some distance up the course of a river, they gave no information respecting the tides of the ocean. Such data as these last mentioned may still be of use to myself or other investigators on some future occasion.

3. I now proceed to give some account of the general character of these observations, the mode employed in reducing them, and the information which they supply with respect to the phenomena of the tides.

The observers were directed to record the times of high water and of low water, and the height of the surface at each of these times, measured from a fixed point. The time was to be correctly ascertained by the best method which circumstances afforded; and where there was no pier or other permanent scale for the heights, a pole was to be erected. Other contrivances, intended to obviate peculiar difficulties, need not here be described. The high-water observations were to be considered as the most important.

These directions were for the most part faithfully and effectually followed. The observations at different places, made under very different circumstances and by persons of different classes, have, as might be expected, very various degrees of merit; but the general relations, both of accordance and discrepance, among the observations, convince me that in almost every instance they were conducted with care and fidelity. In many of the foreign observations the labour employed in order to obtain accurate results has been immense; and the persons under whose care they have been carried on are men of eminent scientific attainments. On our own coasts, the nature of the service to which the observers belonged led in many cases to the use of ruder methods; but the processes employed were mostly well selected according to the circumstances, and were applied with great practical sagacity. I cannot avoid repeating, with respect to the observations of June 1835, what I have already stated with respect to those of June 1834, that they reflect great credit both upon the intelligence and the punctuality of the officers and men of the coast-guard service.

4. Having had my views seconded by the favour and exertions of so many persons of various ranks and countries, it became me to turn to the best advantage the large mass of materials thus collected. It will, however, be seen on consideration, that the arrangement and reduction of this collection was beyond the powers of an individual. The effective places of observation being about five hundred, there were one thousand tides observed every day for twenty days; and as, for each tide, even taking high water only, the time and height were to be considered, I had forty thousand numbers to deal with as the basis of any calculations by which I might deduce general results from this large experiment.

I found in this, as in other similar instances, the Admiralty ready to assist me. Captain Beaufort kindly allowed Mr. Dessiou, of the Hydrographer's Office, to perform my calculations, as far as the business of the office left him time; but this being quite insufficient for my purpose, Lord Auckland, at that time First Lord of the Admiralty, did me the favour of complying with my suggestion that two additional clerks should be engaged, who might carry on these calculations; and Earl Minto, on his accession to the same office, readily agreed to retain these calculators in the same employment till it should be completed. These gentlemen, Mr. D. Ross and Mr. H. Boddy, have, under Mr. Dessiou's superintendence, performed the calculations, by which I have been enabled to draw from the tide observations of June the inferences which are the subject of this paper.

5. One of my principal objects was to fix with precision the form of the *cotidal lines*, by which the motion of the tide-wave is exhibited, and to which I had already attempted to make an approximation*. For this purpose the times of high water were treated as follows.

At each place the differences between the time of high water and the time of a preceding transit of the moon (which differences I call the *Lunitidal Intervals*) were taken for the whole series of observations. Next, these lunitidal intervals were laid down as the ordinates of a curve, the time of the moon's transit after the sun's being the abscissa. In this manner I had, for each place, a curve, which represented (in the way so frequently referred to by Mr. Lubbock and myself) the semimenstrual inequality of the lunitidal intervals, affected by the various errors and peculiarities of the observations. The inspection of these curves afforded me the means of judging of the best mode of combining them so as to get rid of local and casual anomalies. From these curves also the *mean lunitidal interval*, or *corrected establishment* of each place, was readily obtained. For this purpose a curve was drawn by the eye which should pass *among* the points representing the observations, and should retain, as much as possible, the general form of the semimenstrual curve. The intervals being

* Philosophical Transactions, 1833, Part I.

freed from gross irregularity by this graphical correction, the mean interval was taken, making allowance for parallax and declination.

6. This mean lunitidal interval, or *corrected establishment* of each place, differs from the *vulgar establishment*, or time of high water corresponding to new and full moon; for the time of high water at syzygy is affected by the semimenstrual inequality belonging to the moon's position one or two days earlier, and is therefore later by about 30^m than the mean interval would give it. In my former paper on Cotidal Lines I used the statements of the vulgar establishment at each place; in this, I shall employ the corrected establishment, as a more fixed element; for it is as yet uncertain how far the semimenstrual inequality differs at different places. On this account the cotidal lines for $0^h\ 30^m$, $1^h\ 30^m$, $2^h\ 30^m$, $3^h\ 30^m$, &c., which I shall now obtain, represent nearly the cotidal lines for 1^h, 2^h, 3^h, 4^h, &c. of my former charts.

7. The mean lunitidal interval would be the mean of the greatest and least intervals, if the time of high water were not affected by the moon's declination and parallax; but in consequence of these circumstances a correction of the mean is requisite.

In June 1835, if there had been no corrections for the moon's parallax and declination, the least interval at London would have been on the 16th, the greatest on the 23rd, each 44^m from the mean. But, in fact, the least interval was on the 15th, and was 4^m greater than it would have been without the corrections; and the greatest interval was on the 22nd, and was 9^m greater than it would have been without the corrections. Hence the mean of the observed intervals was $6\frac{1}{2}^m$ greater than it would be if declination and parallax did not affect it. If we use the Liverpool tables in the same way, we find the least interval, on the 14th, 1^m less than without the corrections; the greatest interval, on the 21st, 15^m greater than without the corrections. Hence the mean of the observed greatest and least intervals is 7^m larger than the true mean.

On this account I have found the mean lunitidal interval for each place by reading off the greatest and least ordinates of the curves of observation, graphically corrected as above, and by subtracting 7^m from the mean of these ordinates. The tables containing the result of this operation will be given in the sequel. In these tables the first and second columns contain the least and greatest lunitidal intervals: the third column is the difference of these two: the fourth column, the *reduction**, is the half-difference *minus* 7^m; and this added to the least interval gives the *corrected establishments* in the fifth column.

8. In order to use the corrected establishments thus found for the purpose of drawing cotidal lines, they must be reduced to a common origin of time by adding the west longitude (expressed in time), or subtracting the east longitude. In the Tables of Lunitidal Intervals, the sixth column contains the *longitude*, and the seventh the *Greenwich time* of the corrected establishment.

* When the semimenstrual inequality is unusually small, as in many places on the coast of America, I have used the half-difference *minus* 6^m for the reduction.

9. But there is also another correction necessary in order that the series of establishments thus obtained may rightly express the continued motion of the tide-wave. It is to a certain extent optional whether we will take the lunitidal interval resulting from the moon's transit next preceding, or next but one preceding; but when we pass from one transit to another in going through a series of places, we disconnect the establishments as representing the motion of the same tide-wave.

Thus, let there be two places on the same meridian, and on the afternoon of a certain day let it be high water at these places at two and at three o'clock; then the tide-wave is one hour in passing from one place to the other. But let the times of the moon's transit on this day, in the morning and afternoon, be $2^h\ 24^m$ and $2^h\ 48^m$ respectively; the tide at 3^h is referred to the P.M. transit immediately preceding at $2^h\ 48^m$, and the lunitidal interval is $0^h\ 12^m$; but the tide at 2^h is necessarily referred to the A.M. transit, because the P.M transit happens after the tide: hence the lunitidal interval here is $14^h - 2^h\ 24^m$, or $11^h\ 36^m$. But if the cotidal lines were drawn according to these intervals, $11^h\ 36^m$ and $12^h\ 12^m$, they would give a difference of 36^m only, instead of 60^m.

Such discrepancies will be removed, and the lunitidal intervals reduced to a connected series, so as to give a consistent series of cotidal lines, if we diminish each lunitidal interval in the ratio of $12^h\ 24^m$ (the interval of two lunar transits) to 12^h, that is, if we subtract 1^m for every half hour. Thus, in the above case, the lunitidal interval $11^h\ 36^m$ will become $11^h\ 13^m$, which, compared with $0^h\ 12^m$, or $12^h\ 12^m$, gives 59^m for the time employed in the passage of the tide-wave from the one place to the other. The corrected establishment thus further corrected (and reduced to Greenwich time) I call the *cotidal hour* in the tables of intervals.

The observations being estimated, grouped, and reduced by the above methods, I proceeded to combine them, so as to obtain from them systems of cotidal lines, and other information.

Sect. II. *On the form of the Cotidal Lines.*

10. The above reductions gave me the *cotidal hour*, or mean interval of time at which the tides follow the moon's transit, along the whole coast of America, from Florida to Nova Scotia, and along the oceanic coast of Europe from Gibraltar to the North Cape of Norway. The cotidal hours being laid down along the coasts, and lines drawn through the places where the same hour occurs, in such a manner as to be consistent with a possible motion of the tide-wave, we have the *cotidal lines*.

I have already, in the memoir already referred to *, endeavoured to discover the general form of such lines, both for the ocean at large and for the coasts of the British Isles in particular; and I have now to consider how far my new materials enable me to correct my first attempt. For this purpose the observations now before me are highly valuable, and their inaccuracy is scarcely of any moment. That they

* Essay towards a First Approximation to a Map of Cotidal Lines.

are real and simultaneous observations at a sufficient number of places along the coasts, gives them an immense superiority over the statements which I was formerly compelled to use, and which were for the most part only estimated results, founded upon imperfect observations or none, and often deduced by erroneous methods of estimation.

It is not surprising, therefore, that the differences between the form of the lines now obtained and my former maps should be considerable. At the same time I may observe, that all my views of the general course of the tide-wave have been confirmed by the present examination.

11. With regard to the general character of the corrections which I have had to introduce into my maps, I may state this as one circumstance: the cotidal lines make very acute angles with the shore, and run for great distances nearly parallel to it. I had already, to a certain extent, pointed out that the cotidal lines must have a shape of this kind. "They are convex," it was observed *, "in the direction of their motion, the ends near the shore being held back by the smaller velocity in shallower water, and other resistances." But it is necessary to exaggerate very much this feature in their shape, in order to make them conform to our observations, so that the lines near the shore are made near and almost parallel to each other. In this way the velocity of the tide-wave, which is, of course, to be estimated in a direction nearly perpendicular to the cotidal lines, is very much less near the shore than it is in the open ocean: perhaps we may even consider the velocity of the tide-wave in littoral regions as a quantity of a different order, and governed by different laws, from its velocity in the open ocean: but of this we may speak more distinctly hereafter.

One consequence of this form of the cotidal lines is, that though on a large extent of coast the direction and velocity of the progress of the tide-wave are marked clearly enough, in smaller portions the rate and even the direction of this progress may rapidly and repeatedly change. The cotidal line leaving the shore at so small an angle, may easily catch it again where it projects a little, and thus we have *points of divergence* and of *convergence* of the cotidal lines †.

For example, on the coast of America (see Table I.) the progress of the tide from Cape Hatteras is both southward to Cape Fear, Charlestown, Savannah, and St. Augustine, and northward to Delaware and New York; Cape Hatteras being a point of divergence. But at Newport, still further to the north-east, we find the tide again an hour earlier than New York, and even earlier than at Delaware Breakwater; so that between Cape Hatteras and Newport there must be a point of convergence. To the east of this, again, there is a point of divergence, and the hour of the tide becomes rapidly later as it travels into the bays of Massachusetts, Boston, and Fundy.

In the same manner, on the coast of Spain (see Table II.) the 2^h line touches the shore near Cadiz; it also touches at Cascaes near Lisbon, the tide-hour at interme-

* Philosophical Transactions, 1833, p. 231. † Ibid., p. 153.

diate places being as late as $2\frac{1}{2}^h$; and in the Bay of Biscay the hour at Santander is later than at Bilboa, though the latter place is further east.

In Ireland the $4\frac{1}{2}^h$ line runs along the whole coast of Munster, touching it in many places, and the 5^h line runs along the remaining west and south coast of the island at no great distance.

12. Another circumstance which I may notice in the corrected form of these lines, and which results from the same tendency, is, that the hour-lines which are earlier than the littoral ones spread over the general surface of the ocean more widely, and catch the projecting points of land sooner, than had been supposed. Thus the line of $10\frac{1}{2}^h$ nearly touches Cape Hatteras on the coast of America, and compels us to extend the 10^h and 11^h lines considerably to the west.

13. We may observe also that this expansion of the oceanic and compression of the intervals of the littoral cotidal lines, necessarily give an extremely complex form to the former, since they must in some degree accommodate themselves to all the land which surrounds them. Thus, as we have seen, the $10\frac{1}{2}^h$ hour-line nearly touches Cape Hatteras. It also extends from the eastern to the western coast of the Atlantic. But its course must be very sinuous, for the vulgar establishment at the Bermudas is 7^h 18^m*, which places the 11^h cotidal line nearly there. In these and similar cases it is probable that there are, as I have formerly suggested, "detached spaces within which the tides are later than in the surrounding seas, occupied by converging *rings* or *loops* of cotidal lines."

14. As there are large tracts of coast along which the tide-hour exhibits no steady progression, there are, on the other hand, points where it changes very rapidly. These are generally promontories. Thus on the coast of America we have a rapid change in passing round the projection formed by Nantucket and other islands. On the coast of France, in passing round Cape La Hague and Barfleur, the tide-hour advances from 6^h to 9^h. In the same manner on the opposite coast of England the 7^h and 8^h cotidal lines both touch St. Alban's Head in Dorsetshire, and the 9^h and 10^h lines both touch St. Catherine's Point in the Isle of Wight. The tide in passing round the north coast of Scotland and the Orkneys appears to undergo a comparatively rapid increase of the establishment from about 6^h on the western to 12^h on the eastern coast.

15. But the most rapid of the changes which thus occur in passing round promontories are those which are accompanied by a *meeting of tides*, arriving in opposite directions along two different channels; as the tides on the east coast of Ireland, which arrive both from the north and from the south; and the tides in the eastern part of the English Channel, which are derived through the Straits as well as up the Channel. I have already remarked that two tide-waves travelling in opposite directions along the same channel will make the tide-hour nearly constant along a considerable tract of coast, while it varies rapidly at the extremities of this tract†. I

* Philosophical Transactions, 1833, Part I. p. 172.

† Ibid. 1835, Part I. p. 87.

remarked that we find an exemplification of such a case in the tides of the south coast of England, from the Isle of Wight to the Land's End, as observed at the coast-guard stations in June 1834. At the period of writing that paper the observations of the south coast only had been reduced. I can now state that we have a much more remarkable example of the same fact in the tides on the east coast of Ireland. The rapid change of the tide-hour in passing round the northern and southern extremities of this coast is very remarkable, and may be seen in Tables III. and IV. Thus in passing round Rachlin Island and Fair Head, which form the north-eastern point of Ireland, through the narrow strait left by the Mull of Cantire, the tide-hour advances suddenly from $6\frac{1}{2}^h$ to $10\frac{1}{2}^h$. In the same manner in passing round Carnsore Point, from the south to the east coast of the county of Wexford, the tide-hour advances from $5\frac{1}{2}^h$ to $10\frac{1}{2}^h$, and 11^h in a very short distance.

Also when such *hinges* of the tide are once passed, the hour is nearly constant along the whole of the coast, as we have seen that it ought to be from general considerations. Thus all the way from Arklow in the south to Glenarm and Larne in the north of the eastern side of Ireland, the tide-hour at exposed points of the coast is from $10\frac{1}{2}^h$ to 11^h; and a little later in bights, as the Bay of Dublin and the mouth of the Boyne. The "meeting of the tides" may be considered as extending over the whole of this space. In like manner, as I have already stated*, the sea from the Isle of Wight to the Downs is affected (at least as to its tide-hour) both by the channel tide and by that of the German Ocean. Hence the cotidal lines in such cases will cease to extend across the channel, and will become nearly parallel to the shore, as we see the 10^h line on the east coast of Ireland, and the 10^h line on the south coast of England. The lines assume this form by the successive hour-lines projecting more and more in the middle of the channel, as an ellipse may become two parallel lines by retaining its minor axis, and increasing its major axis indefinitely.

16. There is another very curious circumstance connected with these cases of the meeting of tides. In those parts where the tide-hour increases most rapidly (or in other words where the tide-wave travels most slowly) the times of high water are subject to extreme irregularities. This is remarkably seen in the curves which I have used to represent the observations of such places. The lines for Rachlin Island, Ballycastle, Ballintoy, exhibit the most extraordinary irregularities in their course both in June 1834 and 1835. The greatest and least lunitidal intervals at Rachlin Island in June 1835 differ by no less than *five hours and a half*; and there are instances of this interval differing two hours and a half in *two successive tides*. This appears to be partly due to the effect of the diurnal inequality of which we shall have to speak, but still it shows how liable the tide at this place is to the influence of irregularities. And I may observe that this peculiarity in the tides of this place explains the apparent inconsistencies which I formerly noticed in the statements

* Philosophical Transactions, 1835, Part I. p. 89.

respecting these tides*. Knowing the anomalies which prevail in this neighbourhood, I do not now doubt that Captain Mudge's statements are all entirely correct.

Anomalies, but much smaller in amount, may be noticed at Cahore Point in Wexford, at the bays in the neighbourhood of St. Alban's Head, and at Freshwater in the Isle of Wight. I may observe that the occurrence of such irregularities, at the extremity of the space within which one tide is modified by another, is easily explicable. A difference of height or of wind, from one half-day to another, may cause one tide to affect the other much more or less; and thus the mixture of tides, which so entirely alters the tide-hour, may, at these limits, take place very inconstantly, and to a very variable amount.

Sect. 3. *On a Second Approximation to a Map of Cotidal Lines, and especially of those of the German Ocean.*

17. By means of the observations and reductions above described, I have constructed a map of the cotidal lines which pass near the shores of Europe, and a map for the German Ocean and the British Isles in particular, which are given with this paper. By reference to these maps, and by comparison of them with the Tables of Establishments which I have also given, the reader will see the general results of the observations, and their evidence.

He will also see in one of the maps the difference between this second approximation and the first approximation, which I formerly published. The cotidal hours which I have used in this case, however, correspond to the correct establishment, and not to the vulgar establishment, or time of high water at syzygy, which I used in my former essay. But it is easy to make allowance for this difference; for the correct establishment, at London and Liverpool, is very nearly half an hour smaller than the vulgar establishment, and for our purpose may for the present be considered as exactly so at all places. And hence the $1\frac{1}{2}^{h}$ cotidal line of my present map represents the 2^{h} line of the former one, and so on for the rest.

The correct establishment, which is the mean of the lunitidal intervals, may also be considered as the interval at which the high water follows the moon's transit at the highest spring tides and lowest neaps, for these correspond to the mean lunitidal interval.

I have not presented with this paper a map of the cotidal lines of the coast of North America, formed on the new materials; but I may observe that my former map is here considerably in error. The XI. hours cotidal line should strike Cape Hatteras; and the tides diverge from this both to the north and south, as has already been stated in art. 12.

The general views concerning the form of the cotidal lines already stated in Sect. 2, might be used in improving the form of the lines belonging to other places, as well as those to which the recent observations belong. But as a few years will, it may be

* Philosophical Transactions, 1833, p. 182.

hoped, add considerably to our materials for a closer approximation to a map for the whole world, I will not now attempt this, except for the seas to which the observations immediately refer.

18. I have already pointed out the extreme difficulty of forming into a consistent and intelligible scheme the tides of the German Ocean*. But as we have now a connected series of observations along the whole of its coast, we must make the attempt.

The obvious difficulties may be thus stated. Calling the coast from Calais to the north point of Denmark, for the sake of distinctness, the German coast, and considering it as opposite to the British coasts, the series of tide-hours on the two opposite coasts run thus from south to north.

British coast.	X.	XI.	XII.	XI.	X.	IX.	VIII.	VII.	VI.	V.	IV.	III.	II.	I.	XII.
		A				D		B							C
German coast.	11.	12.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	no tide.

Since the tide-wave in most parts of this series moves in opposite directions on the opposite sides of the sea, it is clear that the parts cannot be represented by any motion of a wave along a channel. Nor will it answer well to suppose the wave to run from C to A along the British coast, and back from A to C along the German coast; for the intervals of the lines would, on this supposition, diminish much in passing from the space C B to B A, and increase much again in passing from A B to B C; besides which this view does not take into account the disappearance of the tides on the coast of Denmark, and the connexion of the tides of Holland with those of France.

It appears that we may best combine all the facts into a consistent scheme, by dividing this ocean into two *rotatory* systems of tide-waves; one occupying the space from B to C, that is, from Norfolk and Holland to Norway; and the other the space from A to B, between the Netherlands and England. In the former space the cotidal lines may be supposed to revolve round the point C, where there is no tide; for it is clear that at a point where all the cotidal lines meet, it is high water equally at all hours, that is, the tide vanishes. In the space A B we must suppose similarly a tideless centre, as D, about which the cotidal lines revolve.

This mode of conceiving the progress of the tide does not differ essentially from the hypothesis of a progress from C to A and back from A to C, as already mentioned: for on such a hypothesis the motion might be conceived to be resolved into two rotatory systems, the wave being supposed to pass from VI. to 7. and from 6. to VII., instead of passing from VI. to VII. and from 6. to 7. But this is in reality no difference; for the change really is, that the ridge of the wave passes from the position VI. 6. to VII. 7.; which is equally well represented by either supposition.

This hypothesis of two rotatory systems in the German Ocean is recommended by its giving the most consistent and probable relations among the cotidal lines and

* Philosophical Transactions, 1833, p. 188.

the intervening spaces, as may be seen by reference to the chart; and I have therefore adopted it as the best approximation I can now obtain to the form of these lines.

This theory is, indeed, nothing more than a representation of the facts of the case; yet it gives a view of the mechanism of the tides of the German Ocean different from any which has hitherto been suggested. The southern rotatory system, which exists between the coast of Suffolk and the Netherlands, may be conceived to be kept in constant circulation by impulses received from the adjacent tides, that is, an impulse at 6^h on the coast of Norfolk, and an impulse at 12^h on the coast of Belgium. Thus it resembles a watch or clock, which is kept in continual motion by a sustaining force applied at intervals. The larger rotatory system, lying between the east coast of Scotland and England, and the coast of Germany and Denmark, does not, like the other, return into itself. We may conceive that in this case the tide-wave is turned aside by the opposing coast of Norfolk and Germany, so as to be thrown back upon itself in the neighbourhood of the coasts of Jutland after an interval of six hours. This would explain the vanishing of the tide in that region; for a tide at 12^h combined with a tide at 6^h are equivalent to no tide at all; the high water of the one filling up the low water of the other.

19. Besides this completion of our view of the tides of the German Ocean, our new materials give us the course of the tide-wave on the coast of Norway, which I had not previously ascertained. It appears that the 9^h cotidal line, which must pass somewhere near the Orkneys, also touches the opposite coast of Norway at Stavanger and Tananger; and as we find the hours go on to 12^h, both in proceeding southwards to Cromarty on the one coast and to the Naze on the other, we appear to be entitled to conclude that the 9^h, 10^h, and 11^h lines extend across the ocean here. But Stavanger is a point of divergence from which the tide also travels northwards; for it is $9^h\ 43^m$ at Bergen, $10^h\ 4^m$ at Christiansund, $11^h\ 22^m$ at Andænes in the Lofoden Isles, and $1^h\ 30^m$ at Tromsöe, in latitude 69° 38′. We may judge the 2^h line to be not far from the North Cape. And we have thus a tolerably complete view of the cotidal lines of the European seas.

We may observe that here also the tides of islands appear to be later than those of the surrounding seas, so as to compel us to make the cotidal lines form loops and rings. The tide-hour at Lerwick, on the east coast of Shetland, is $10^h\ 41^m$, though the islands appear to lie between the 6^h and 9^h lines.

Sect. IV. *Height of the Tide.*

20. The range of the tide, that is, the height of high water above low water, is very different at different places, and is affected by circumstances which it is very difficult to analyse. It is, however, clear, that the configuration of the coast exercises a very considerable influence upon the amount of this range. Thus the range is very much increased in deep inbends of the shore which are open in the direction of the tide-wave, as the Bristol Channel and the Gulf of Avranches; and much diminished at

promontories under certain circumstances. Thus at the south-east point of Ireland, (at Arklow, Glynn, and Cahore,) the greatest range is not more than three feet, while at a little distance along the coast each way it becomes twelve or thirteen feet: and this small amount of the tide on one side of the channel is the more remarkable, because it is just opposite the enormous range which occurs in the Bristol Channel. In order to exhibit the succession of facts of this kind, I have drawn out Table X., in which the greatest and least range at each place of observation in June 1834 and 1835 are recorded. The agreement of the two years with one another in the cases in which observations have been made in both, shows that these observations are entitled to considerable confidence. It may be observed, moreover, that the formulæ which have been obtained from the best discussions of tide observations do not lead us to expect a complete coincidence of the range in the two years. By the Liverpool tables it results (from the corrections for lunar declination and parallax) that the highest high water in June 1835 would be three feet one inch above the mean high water, while in June 1834 the greatest high water would only be two feet above the mean; and thus the greatest range at Liverpool would be two feet two inches more in June 1835 than in June 1834. It will be found that in our table the range of the tide is in almost all cases greater in 1835 than in 1834 by a quantity different according to the range itself.

21. I have also endeavoured in another manner to represent to the eye the course followed by the range of the tide. In a Map of the British Isles and the German Ocean I have drawn lines parallel to the coast, and expressing, by their number, the range of the tide; as many lines being drawn as there are *yards* in that range. An inspection of this map will make apparent several curious circumstances in the change of magnitude which the tide undergoes in its progress.

By reference either to the table or to the map, it will be seen that the range, which is 16 feet at the Scilly Isles, becomes 13 and 12 feet on the coast of Devonshire and Dorsetshire, and retains this value, with no great change, (proceeding outside the Isle of Wight,) to Selsey: it then increases, so that at Brighton the range is 18 or 19 feet, and at Eastbourne 21, which it is also at Dungeness, and not much less at Dover. At Dunkirk it is 16 feet French, and on the coast of Belgium it is about 4·5 French metres, or 15 feet. But in going along the coast of Holland eastward, it diminishes from 4 Dutch ells, its value near Flushing, to 2·3 ells at Ameland. On the coast of Denmark this diminution goes on: the tide is 10 Danish feet at the mouth of the Elbe; but in going north it becomes 5·6 feet at the point called Bleavand's Huk, or the Horn; 2·7 feet at Nyminde Gab; and only 1·5 foot at Agger, in the inbend of the Skaggerrack, which leads to the Baltic. In this neighbourhood we may conceive the tides to vanish, and hence I have here placed a pole or centre about which the tide-wave revolves, as I have already explained. When we pass this point, and advance northwards along the coast of Norway, the tide again assumes a considerable magnitude. At Tananger it is only 1 foot 9 inches English; at Skeudesnaes 2 feet

1 inch; at Christiansund 6 feet 8 inches; at Lofoden 7 feet 7 inches; and at Tromsöe, in latitude 69° 38′, it is 8 feet 8 inches. At Peterhead, on the coast opposite Norway, it is 12 feet.

I shall not here attempt to reduce these changes to any general rules, but shall proceed to another branch of our results.

Sect. V. *The Diurnal Inequality.*

22. The Diurnal Inequality of the tides is only now beginning to be attended to as it deserves; for it is a regular change, considerable in its amount, and almost universal in its prevalence. It would be easy to enumerate many actual cases in which the safety or loss of a ship has been determined by this inequality. Though the existence of such an inequality in particular places has long been known, its laws have been misunderstood: for example, it has been supposed always to affect the morning and evening tides in opposite ways, which is only an accidental and local expression of its rule. Mr. Lubbock* has published the mode in which he has obtained it for Liverpool, while Mr. Bywater, who has introduced it into his Tide-Tables for that port, and Mr. Bunt, who is constructing Tide-Tables for Bristol, have also collected this inequality from observations. But the connexion of the inequality, as it exists in different parts of the world, was never brought into view till the discussion of the European and American observations of last June. The laws which the inequality follows when thus considered on an extensive plan appear to me to be very curious, as they result from this examination of the facts; and I now proceed to explain them.

23. The inequality is most clearly seen in the heights of high water. I exhibited the results in curves, by erecting a series of ordinates at equal distances to represent the heights of the successive high waters above a fixed point at each place; and the curves which were thus produced showed, in most places, a series of parallel zigzags (the tides being alternately higher and lower); and these curves were so regular, and so exactly accompanied each other, as to prove both the goodness of the observations and the existence of the diurnal inequality. This was the case, in the most marked manner, on the coast of America, where scarcely any exception occurred. Next to this, the inequality was conspicuous, especially during a portion of the series of observations, on the coasts of Spain and Portugal; then on the west coast of France, the coast of Cornwall, and parts of the west coast of Ireland: on the shores of the German Ocean, although the operation of the inequality was obvious, it was less steady and regular.

24. The diurnal inequality depends upon the moon being north or south of the equator; its maximum *corresponds* to (but is not necessarily simultaneous with) the moon's greatest declination; and the period of its vanishing corresponds in like manner with the time of the moon passing the equator. Between periods corre-

* Philosophical Transactions, 1836, Part I., page 57.

sponding to two such passages, the inequality increases from 0 to a maximum, and decreases to 0 again; after which it again increases.

The curves which represent the heights do, in fact, exhibit such alternate increase and diminution of the diurnal inequality: and the inquiry naturally occurs, After how long a time does the moon's position show its effect in the diurnal inequality? In the case of Liverpool it appears, as I have pointed out *, that the diurnal inequality expresses the effect of the forces (upon the equilibrium-spheroid) as they existed six days previously. It is important to know whether this interval is the same in other places.

25. It is very far from being the same, and its changes are very curious. In June 1835 the moon had her greatest south declination on the 12th; her declination vanished on the 19th, early in the morning; and her greatest north declination was on the 26th. On the American coast, the diurnal inequality, as shown by the zigzag form of the curves, followed these changes, not at an interval of days, but almost simultaneously. The curve is strongly indented from the 10th to the 15th: the indentations at most of the places die away on or about the 18th; they then reappear, slipping over one tide, so as to throw the greatest tide from an odd to an even tide, or the reverse; and increase to their greatest magnitude again about the 26th. On that side of the Atlantic, therefore, the difference of the lunar forces on the two successive half-days appears to be felt almost instantaneously. But when we come to the European shore the result is very different. On the coasts of Spain and Portugal, and on the coast of France as far eastward as Cherbourg, the diurnal inequality is very steady and well marked, but it only appears to begin about the 9th or 10th, increases till the 16th or 17th, then decreases, and vanishes on the 21st or 22nd, after which it again increases. Thus the moon's crossing the equator on the 19th is not felt in its effects till two or three days afterwards. In like manner, on the coast of Cornwall, and on the west coast of Ireland, the inequality is well marked till the 21st or 22nd, after which it vanishes, and reappears irregularly only. As we advance further in the direction of the progress of the tide, we find the epoch of the diurnal inequality to be later and later, although the inequality, and therefore its epochs, are less clearly marked. Thus at Cowes, Portsmouth, and Hayling Island, the inequality begins on the 13th and vanishes again on the 23rd; on the east coast of Scotland, and of the North of England, in like manner, it appears on the 12th or 13th; but it seems to pass over a tide, which is equivalent to its vanishing, as early as the 21st. In the German Ocean, however, its course is not very intelligible; for though it appears very marked in the Danish observations, from the 12th to the 22nd, it misses one tide on the 18th. As the Danish tides will be seen by the map to arrive by two different paths, one of which is half a day longer than the other, it is easy to explain this change in the regular alternation of the tides, by supposing that the tide which comes from Scotland was predominant at one period of the lunation, and that which

* Philosophical Transactions, 1836, Part I., page 97.

arrives along the coast of the Netherlands predominant at another period. The short series of observations which we have now before us, does not by any means enable us to determine how far this change in the influence of the two tide-waves is constant and regular. On the coast of the Netherlands, also, this inequality seems to offer a peculiarity; for it vanishes on the 24th, but increases again without missing a tide. In the northern part of Norway it increases from the 12th, vanishes on the 20th, and exists but irregularly afterwards.

The evidence of these statements is seen most clearly by an inspection of the curves of which I have spoken; and the eye catches from these the course of the facts far more distinctly than from any numbers. But it is not necessary to publish all these curves, and I have therefore only annexed a specimen in Plate XXVII., and, for the rest, stated the results of them in numbers in Table XI. The means there given are obtained by a graphical interpolation, such as I have already described, and the other columns exhibit the effects which are mainly due to the diurnal inequality.

26. In these tables the differences of heights are arranged according as the tide occurs A.M. or P.M. But it will be seen at once that this is not, in fact, the circumstance on which the distinction depends; for at most of the places the P.M. tides are greatest till about the 12th, then the A.M. tides are the greatest till the 18th, and afterwards the P.M. tides are again the highest. Hence we see that it is impossible to give the law of this inequality, as is sometimes attempted, by saying that at one season of the year the A.M. tides are greatest, and at another season the P.M. tides are greatest. The real rule, on the coast of America, is, that the tide which follows the superior transit of the moon when she has south declination, and the inferior transit when she has north declination, is the greatest. And hence we see that the sign of this inequality in the tables must change when we come to the half-day without a tide in each semi-lunation, as it will be seen, by inspecting the tables, that it does: for if the tide which happens at $11^h\ 50^m$ A.M. today be the one which follows a superior transit, the tide which happens at $0^h\ 20^m$ P.M. tomorrow will also follow a superior transit; and therefore the + sign of the diurnal inequality must pass from the A.M. to the P.M. column.

On the west coasts of Portugal, Spain, France, and Ireland, and in the South-west of England, the rule is the same, except that we must state *two days after* the moon's crossing the equator to the south as the times when the inferior transit gives an increase to the next succeeding tide, and *vice versâ*. Thus on the coast of Cornwall the P.M. tide was greater from the 9th to the 19th (the day of full moon), because the moon had gone south of the equator on the 4th, and the P.M. tide followed the inferior transit. On the 20th the A.M tides began to follow the inferior transit, and the sign of the inequality would on this account change; but as the moon went north of the equator on the 19th, the tide following the *superior* transit must become the greatest on the 21st, that is, the P.M. tide: and thus the P.M. tides continue the greatest almost all through the month, as has been stated for Plymouth and other places on various occasions. We now see that this is merely an accidental result of the true rule.

27. The different epoch of the diurnal inequality in different parts of the world is a very curious fact; and the more so, since it is inconsistent with the mode hitherto adopted of explaining the circumstances of the tides by conceiving a tide-wave to travel to all shores in succession. In accordance with this view the tide on the shores of America had been considered as identical with the tide on the coasts of Spain and Portugal, which occurs about the same moment; nor does it appear easy to imagine the form of the tide-waves so that this shall not be the case. Yet we find that the tides on these two sides of the Atlantic cannot be identical in all respects; for on the 9th, 10th, and 11th of June, when the diurnal inequality was great in America, it was nothing in the West of Europe; and on the 18th and 19th, when this inequality had vanished in America, it was great in Europe. It would seem as if the tidal phenomena on this side of the Atlantic corresponded to an epoch (of the equilibrium-theory) two or three days later than the same phenomena in America; and we may perhaps add, that different kinds of phenomena do not appear to travel at the same rate. And thus the equilibrium-theory, though it may explain the general form of the inequalities, cannot give their epochs and amounts by any possible adjustment of constants.

I may add, that the notion of the progress of the tide-wave from south to north in the Atlantic is still further involved in difficulties by its appearing that at the Cape of Good Hope the diurnal inequality showed itself most clearly on the 17th, 18th, and 19th of June; that is, as late as in Spain and Portugal. This appears by observations undertaken at my request by Sir John Herschel; and though these observations, made under very inconvenient circumstances, are not very regular, there can, I think, be no doubt of the reality of the feature to which I have referred.

28. The diurnal inequality appears also, but not so generally, in the curves which represent the times: nor is this difference always in the same direction. Thus on the coast of America, at some places the P.M. tides are later than the mean, and the A.M. earlier, for a great part of June 1835, while at other places the reverse is the case: and the same peculiarity occurs on other coasts.

Though this circumstance appears at first sight anomalous, it is not difficult to explain it, at least hypothetically. The alteration of the time of high water by means of the diurnal inequality results, not only directly from the change of position of the equilibrium-tide, which of course affects all places alike, but also indirectly, from the diurnal inequality of the height; for tide-waves of different heights may both travel with different velocities, and have different spaces to describe: and thus the consequent change of time may either tend to make it sooner or later. If the evening tide be two feet higher than the morning tide, it may on that account travel faster along that part of the channel which they have in common; but then, if the shore be very shallow, an addition of two feet may make the water advance many hundred yards further; and thus, on this account, the time of high water would be later. The diurnal inequality of the heights, therefore, will depend upon local circumstances, not only for its quantity, but for its sign.

It appears by the observations that the diurnal inequality of the times is the most clearly marked in situations where the mixture of two tides ends; as at the north-east point of Ireland, where the tide following the A.M. transit of the moon is later than the mean; at the south-east point of Ireland, where the tide following the A.M. transit is the earlier; at Ostend; at Havre; on the coast of Denmark, where this diurnal inequality amounts to half an hour. The diurnal inequality is also very large in places where the tide has to run far inland, as in the Sound of Christiania in Norway, and in the Zuyder Zee in Holland. At Amsterdam the difference resulting from this inequality appears to be an hour; in the neighbourhood of Christiania it is larger still, but with great anomalies.

Sect. VI. *On the Semimenstrual Inequality.*

29. The amount of the semimenstrual inequality of the time of high water is very different at different places, so far as the evidence of the observations now before us shows; and though these are of too rude a kind to give the amount of the difference, they are sufficient, I think, to prove its existence; especially when coupled with the consideration of a reason for the difference, namely, that the spring tides being higher than the neaps, the tides of the two kinds may travel with velocities which at different places have different relations. Thus I conceive that I have here a confirmation of the opinion which I deduced from the observations of June 1834, that there is a *local* semimenstrual inequality in addition to the general one*. But I do not conceive that this series offers any very decisive proof of my former conjecture, that the semimenstrual inequality is less at promontories than in bays, or that it becomes less and less as the tide-wave advances. The changes of this inequality are not obviously explicable. On the coast of North America the amount of the difference of the greatest and least lunitidal intervals is small, being generally less than 80^m, and at Newport as low as 56^m. On the coast of Portugal at several places this difference is extraordinarily small, so as almost to throw doubt on the accuracy of the observations: at Pera in Algarve it is only 42^m, and at Lagos Bay only 24^m, while at Peniche it is 130^m. On the greater part of the French coast it ranges with great steadiness from 80^m to 100^m, except at the little harbour of Abrevrak, where it is 125^m. At Torr Head (in the north-east of Ireland) we have this difference 146^m, and at Rachlin Island (North of Ireland) it is four hours, even after the graphic correction; but these are cases of extreme irregularity. On many parts of the south coast of England it is small (about 70^m to 74^m), as at Exmouth, Weymouth, St. Alban's Head, St. Lawrence, Swanage Bay, Brighton, and Hastings.

* Philosophical Transactions, 1835, p. 85.

The amount of the semimenstrual inequality of height also varies. In general the greatest range, as will be seen by Table X., is twice or twice and a half the smallest; but this rule is far from universal. And many of the cases which appear to approach to this rule, really deviate from it when allowance is made for the diurnal inequality. Thus on the coast of America, Mount Desert Island, the whole amount of the semimenstrual inequality of high water is about three feet in a tide of thirteen feet, thus reducing the smallest range to eleven; but the diurnal inequality reduces it further to eight feet.

The column headed "Mean" in Table XI. exhibits not only the amount but the law of the semimenstrual inequality of the heights, so far as it is given by the observations of June 1835. It is not likely, however, that so short a series can be of much value for this purpose.

Sect. VII. *General Remarks, and Tables.*

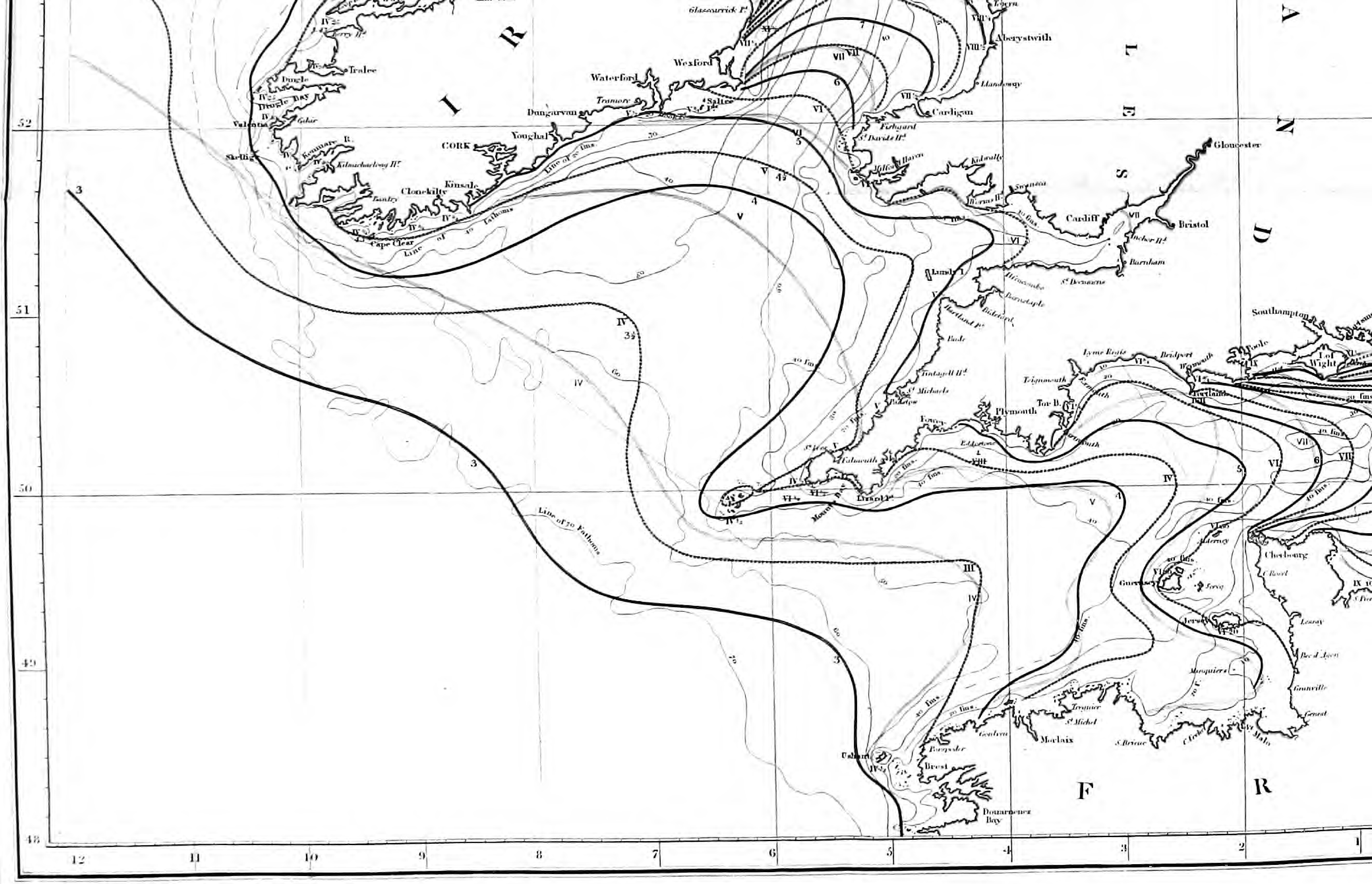
30. The preceding are the principal conclusions which offer themselves as resulting from the tide observations of June 1835. I trust that they will be considered of some value, especially when taken in connexion with the further researches to which they direct us. The form of the cotidal lines, and the progress of the tide-range in going along the shore, are points of considerable interest; but perhaps the most important consequence of this investigation is the prominence it gives to the diurnal inequality. We have here a regular change of the height of the tide, which in many places is not less than the difference of spring and neaps, which operates every day, but which has never yet been introduced into tide tables, and of which the law is not yet precisely known. It is of great importance, both to the theory of the tides and to the purposes of navigation, that this diurnal inequality should be fully analysed. The perplexity produced by the difference of its epoch on the coasts of America and of Europe, may perhaps be removed by the examination of observations at intermediate places. With this view I shall, as soon as I have the means, discuss observations made at Bermuda, and at Halifax in Nova Scotia; and it would be of use also to have observations at Iceland, at the Cape of Good Hope, and on the coast of Africa. It may be observed that observations would be available for this purpose if they gave the height of high water merely, without the time, a kind of observation made with little difficulty and trouble.

31. I shall now give a list of the Tables and Maps which are the results of the series of tide observations of June 1835, according to the preceding discussions.

Tables I. to IX. The correct establishments and cotidal hours of the places at which the most useful observations were made in June 1835.

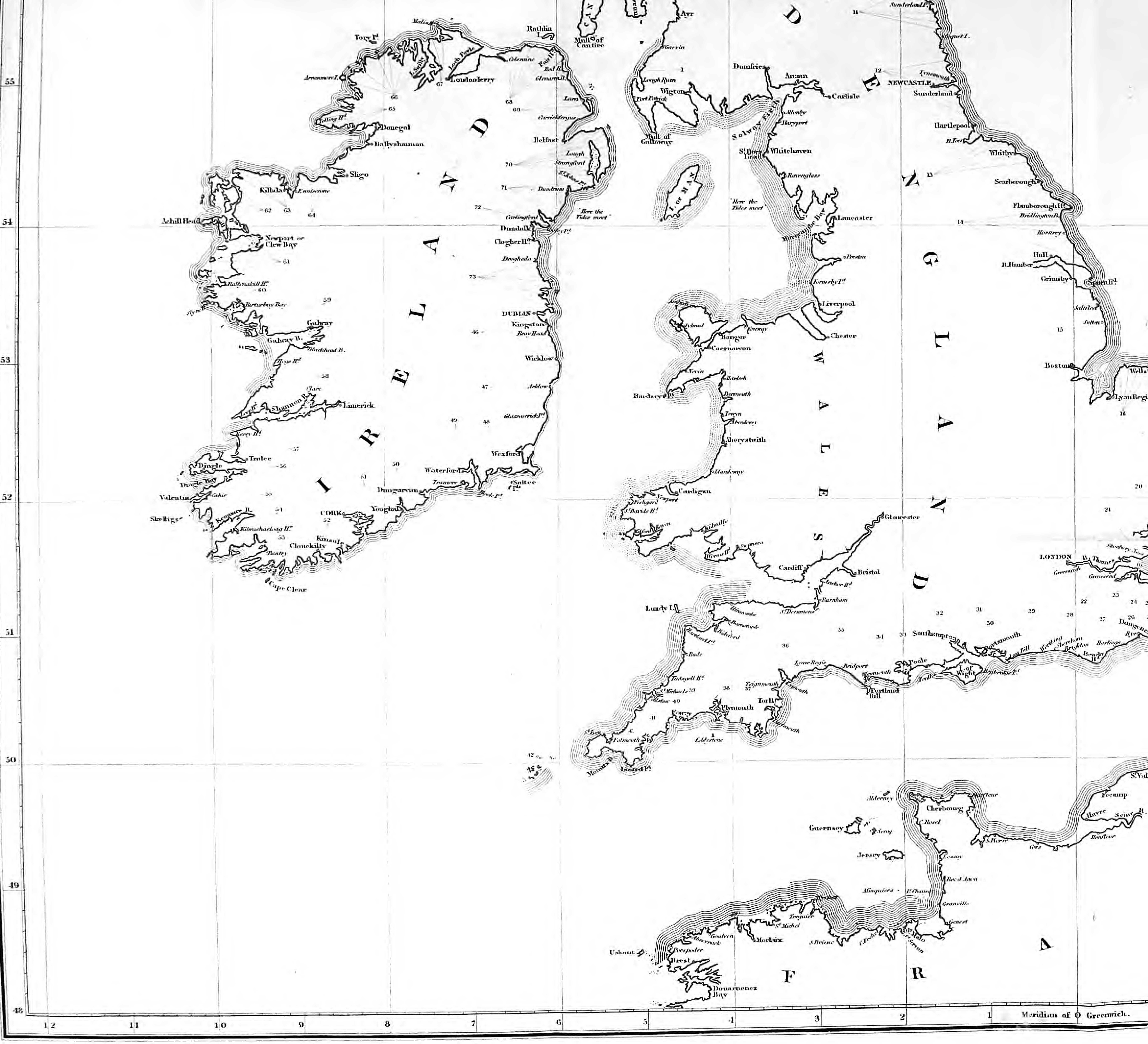
Several sets of observations have been omitted in this list, not because they were less carefully or skilfully made, but because on various accounts it was not desirable to combine them with the others.


NORWAY
ORKNEY ISLANDS
WESTERN ISLANDS
SCOTLAND
ENGLAND
IRELAND
WALES
JUTLAND
HOLLAND
GERMANY
ABERDEEN
PERTH
EDINBURGH
NEWCASTLE
DUBLIN
LONDON
AMSTERDAM
CORK
Line of 50 Fathoms
Line of 20 Fathoms


I R
E S
N D
L A
F R
Tralee
Dingle
Dingle Bay
Valentia
Skellig
Kenmare R.
Bantry
Clonakilty
Kinsale
Cape Clear
CORK
Youghal
Dungarvan
Tramore
Waterford
Wexford
Glascarrick Pt
Aberystwith
Cardigan
Fishguard
St Davids Hd
Milford Haven
Swansea
Cardiff
Gloucester
Bristol
Barnham
Lundy I.
Ilfracombe
Barnstaple
Bideford
Hartland Pt
Bude
Tintagell Hd
St Michaels
Padstow
St Ives
Falmouth
Lizard Pt
Mounts Bay
Plymouth
Fowey
Eddystone
Tor B.
Teignmouth
Exmouth
Dartmouth
Lyme Regis
Bridport
Weymouth
Portland
Poole
I. of Wight
Southampton
Cherbourg
Alderney
Guernsey
Jersey
Minquiers
Granville
St Malo
Morlaix
St Brieuc
Brest
Ushant
Douarnenez Bay
Line of 20 fms.
Line of 40 Fathoms
Line of 70 Fathoms
52
51
50
49
48
12
11
10
9
8
7
6
5
4
3
2
1

Jas. C. Walker Sculpt.


ORKNEY ISLANDS
WESTERN ISLANDS
THE MINSH
LITTLE MINSH
SCOTLAND
NORWAY
IRELAND
ENGLAND
WALES
NETHERLANDS
GERMANY
JUTLAND
St. Kilda
Stromness
Duncansby Head
Thurso
Wick
Dornoch Firth
Murray Firth
Inverness
Elgin
Peterhead
ABERDEEN
Montrose
PERTH
Firth of Tay
Firth of Forth
EDINBURGH
Dunbar
Berwick
Dumbarton
Ardrossan
Ayr
CANTIRE
Mull of Cantire
Rathlin I.
Londonderry
Donegal
Ballyshannon
Sligo
Killala
Achill Head
Newport or Clew Bay
Galway
Galway B.
Limerick
Shannon R.
Tralee
Dingle
Valentia
Skelligs
CORK
Youghal
Dungarvon
Waterford
Wexford
Saltee I.
Wicklow
Kingston
DUBLIN
Dundalk
Belfast
Clonakilty
Kinsale
Cape Clear
Dumfries
Carlisle
Wigton
Mull of Galloway
Solway Firth
Whitehaven
St. Bees Head
Lancaster
Liverpool
Chester
Carnarvon
Bangor
Bardsey I.
Aberystwith
Cardigan
Cardiff
Bristol
Gloucester
Lundy I.
NEWCASTLE
Sunderland
Hartlepool
Whitby
Scarborough
Flamborough Hd.
Hull
R. Humber
Grimsby
Boston
Lynn Regis
Cromer
Yarmouth
Harwich
LONDON
Margate
Dover
Dungeness
Southampton
Portsmouth
Calais
Ostend
Antwerp
Flushing
Hague
AMSTERDAM
Groningen
Emden
Heligoland
Ringkiobing
Eggersound
Christiansand
Arendal
Over and Lemon Shoals



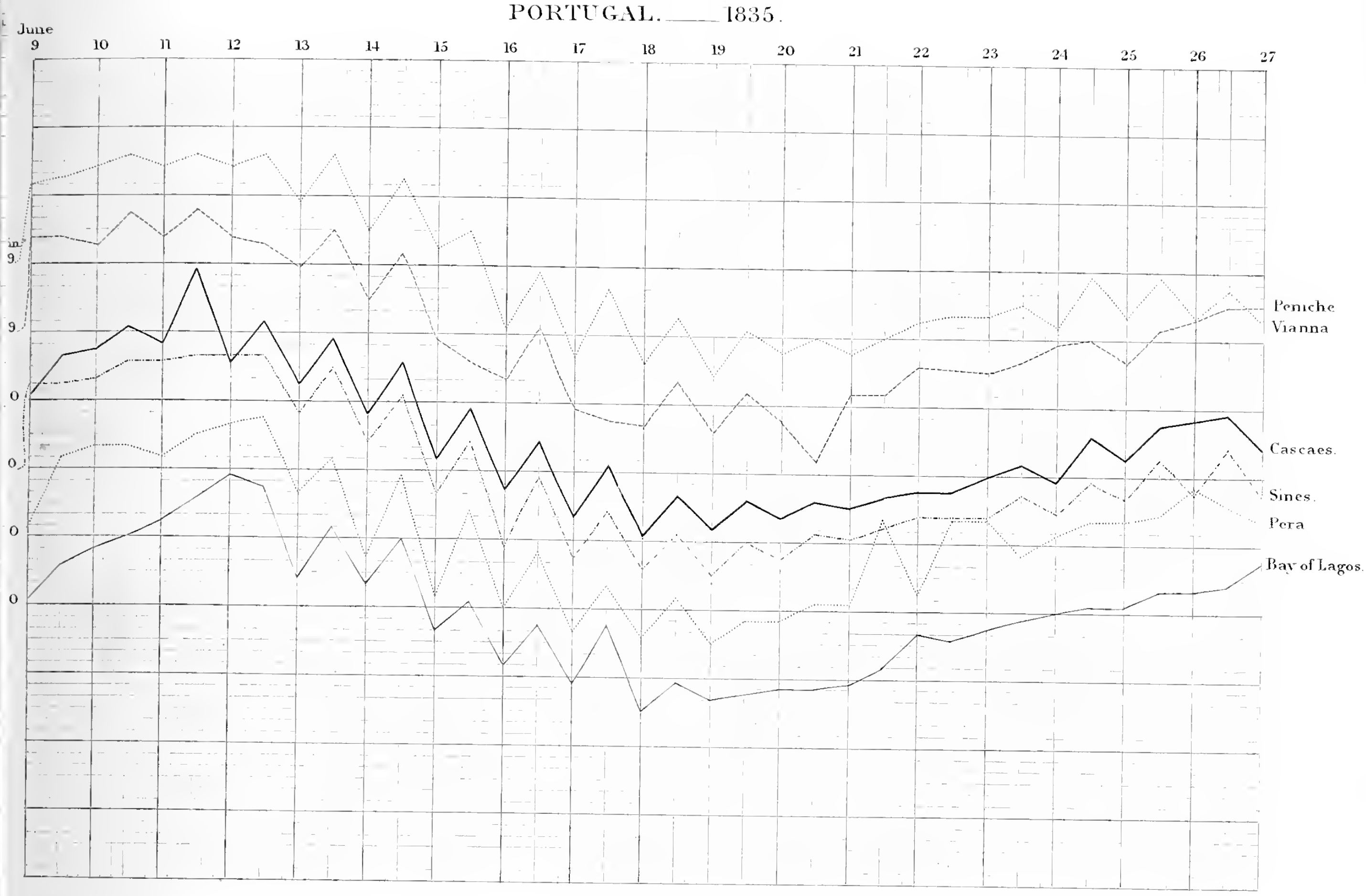
IRELAND
ENGLAND
WALES
FRANCE
Tory Id
Rathlin I.
Mull of Cantire
Ayr
Londonderry
Lough Foyle
Coleraine
Donegal
Ballyshannon
Sligo
Killala
Achill Head
Newport or Clew Bay
Belfast
Carrickfergus
Larne
Dundalk
Clogher Hd
Drogheda
Dumfries
Annan
Carlisle
Wigton
Mull of Galloway
Solway Firth
Whitehaven
Ravenglass
I. of MAN
Lancaster
Preston
Liverpool
Chester
Bangor
Caernarvon
Bardsey I.
Aberystwith
Cardigan
NEWCASTLE
Sunderland
Hartlepool
Whitby
Scarborough
Flamborough Hd
Hull
R. Humber
Grimsby
Boston
DUBLIN
Kingston
Wicklow
Arklow
Wexford
Waterford
Saltee Is.
Dungarvan
Youghal
CORK
Kinsale
Clonakilty
Cape Clear
Galway
Galway B.
Shannon R.
Limerick
Tralee
Dingle
Dingle Bay
Valentia
Skelligs
Glocester
Bristol
Cardiff
Lundy I.
LONDON
Southampton
Portsmouth
Poole
I. of Wight
Portland Bill
Plymouth
Lizard Pt
Cherbourg
Guernsey
Jersey
Granville
Morlaix
Brest
Ushant
Douarnenez Bay
Fecamp
Havre
Meridian of Greenwich.
55
54
53
52
51
50
49
48
12
11
10
9
8
7
6
5
4
3
2
1


CHART of the BRITISH ISLES,
To accompany Mr. Whewell's Paper on the TIDES.
SIXTH SERIES OF RESEARCHES.
The LINES marked thus drawn parallel to the Coast express in YARDS the RANGE of the Tide at Springs, that is, the height of high above low water, as given by the observations of June 1834 and 1835.
The LINES thus are from other authorities.
The numbers 1 to 75 refer to the coast guard stations, as stated in Table X.
GERMAN OCEAN
NETHERLANDS
JUTLAND
LONDON
Yarmouth
Harwich
Ramsgate
Dover
Dungeness
Calais
Boulogne
Dieppe
Havre
Ostend
Flushing
Antwerp
Hague
AMSTERDAM
Groningen
Emden
Heligoland
Greenwich.

J. Basire sculp.

HEIGHTS OF HIGH WATER.

PORTUGAL. ___ 1835.



AMERICA. ___ 1835.

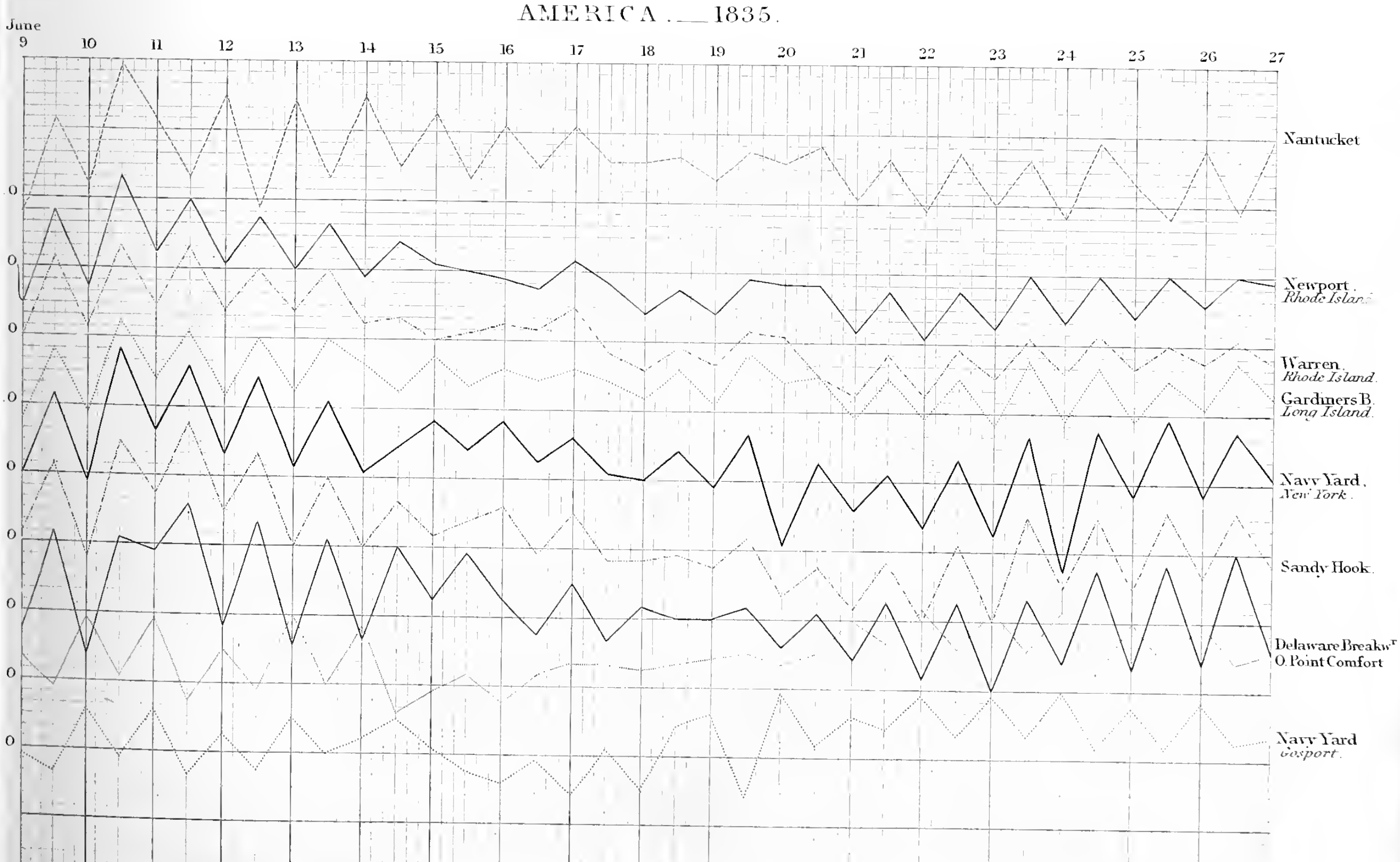


Table X. The Greatest and Least Ranges of the Tide at the places of observation selected as above.

In this Table I have inserted, for the foreign stations, the names of the directors of the observations, and of the observers; also the latitudes and longitudes of the places when those have been given along with the observations. For the British stations, I have given the range from the observations of June 1834 as well as 1835; and the names of the Inspecting Commanders of the districts of the Coast Guard, under whose direction the observations were made in 1835.

Table XI. Semimenstrual and Diurnal Inequalities of the Height of High Water at several places of observation.

In this list, those places are taken at which the diurnal inequality is most distinct and regular.

Plate XXIV. Map of the British Isles and coasts of the German Ocean, showing the *cotidal lines* (according to the correct establishment).

Plate XXV. The same Map, showing the *range of the high tides* at each point of the shore (in yards).

Plate XXVI. Map of the *coasts of Europe*, showing the *cotidal lines* according to the correct establishment.

Plate XXVII. Diagram exhibiting examples of the curve of the heights of high water, affected by the diurnal inequality, which has different epochs at different places.

The materials upon which the above Tables and Maps are founded are deposited in the Hydrographer's Office in the Admiralty; and I will give a list of them, since they may be of use in future investigations on the subject. They are,

The original Registers of the Coast Guard Observations in June 1834.

The original Registers of the Coast Guard Observations in June 1835.

The Registers of the Observations made in June 1835, transmitted to the Admiralty from North America, Portugal, Spain, France, Belgium, the Netherlands, Denmark, and Norway.

Founded upon these there are Tables containing

The Times of High Water arranged in order for each place;

The Lunitidal Intervals calculated from the Times;

The curves of Lunitidal Intervals for most of the places of observation, and for several groups of places, in order to obtain Tables I. to IX. by graphical interpolation;

The Heights of High Water arranged in order for each place;

The Curves of High Water for most of the places of observation, in order to obtain Table XI. by graphical interpolation.

The *mean* Lunitidal Intervals have also been calculated by addition for most of the places; but as I have not used these, I have not given them.

London, June 11, 1836.

LUNITIDAL INTERVALS. JUNE 1835.

Table I.

Coast of North America.

	Least Interval.	Greatest Interval.	Difference.	Reduction.	Corrected establishment.	Long. W.	Corr. Estab. Greenwich Time.	Cotidal Hour.
	h m	h m	m	m	h m	h m	h m	h m
c b Thomson's Island. Key West Florida	9 15	10 45	90	38	9 53	5 57	3 50	3 30
b St. Augustine	7 35	8 45	70	29	8 4	5 26	1 30	1 14
a Savannah	7 38	8 53	75	31	8 9	5 24	1 33	1 17
b Charlestown	6 57	7 57	60	24	7 21	5 20	0 41	0 26
a Cape Fear River	6 47	7 54	67	27	7 14	5 12	0 26	0 12
c Cape Hatteras	4 55	6 45	110	48	5 43	5 4	10 47	10 36
c c Gosport. Virginia Navy Yard	8 15	10 0	105	45	9 0	5 8	2 8	1 50
b Delaware Breakwater	7 7	8 15	68	28	7 35	5 0	0 35	0 20
b Sandy Hook	7 5	8 18	73	30	7 35	5 0	0 35	0 20
c b Old Point Comfort	7 50	9 19	89	37	8 27	5 0	1 27	1 10
a New York	8 8	9 18	70	29	8 37	4 56	1 33	1 16
b Newport	7 17	8 13	56	22	7 39	4 45	0 24	0 9
b Warren	7 35	8 48	73	30	8 5	4 36	0 41	0 25
c b Gardiner's Bay	9 17	10 48	91	38	9 55	4 37?	2 32	2 12
c a Cape Cod	10 50	12 15	85	35	11 25	4 38	4 3	3 40
a Province Town	10 55	12 12	77	32	11 27	....	......	......
a Boston	11 0	12 15	75	31	11 31	4 43	4 14	3 51
c a Portland	10 35	12 0	85	35	11 10	4 40	3 50	3 28
c a Mount Desert Island	10 35	12 0	85	35	11 10	4 32	3 42	3 20
a Portsmouth	11 0	12 13	73	30	11 30	4 44	4 14	3 51
a Gloucester	11 35	12 35	60	24	11 59	....	......	......
a E. Port Maine	10 40	12 0	180	33	11 13	....	......	......
b Nantucket	12 8	13 26	78	33	12 31	4 40	5 11	4 46

At the places marked *a* the curves are regular but very flat. At those marked *b* the curves are more broken from tide to tide, but the general course tolerably regular. At Cape Hatteras a sudden increase of the interval after June 18. At Newport, Warren, Gardiner's Bay, Gosport, an increase of the interval June 21 P.M.

The reduction made by subtracting 6^{m} from the mean, except at the places marked *c*, where 7^{m} is subtracted.

Key West, Florida has a diurnal inequality, which at its maximum (June 9 and 24) amounts to $2\frac{1}{2}^{h}$.

Nantucket has a tide-hour much later than the surrounding seas.

I add here the following observations which I have received from Sir John Herschel, made by him and Mr. Maclear.

Cape of Good Hope.

	Least Interval.	Greatest Interval.	Difference.	Reduction.	Corrected establishment.	Long. E.	Corr. Estab. Greenwich Time.	Cotidal Hour.
	h m	h m	m	m	h m	h m	h m	h m
Simon's Bay	1 50	4 0	130	58	2 48	1 20	1 28	1 22
Table Bay	1 25	3 48	143	64	2 29	1 20	1 9	1 6

Table II.

Coast of Spain, Portugal, France, Belgium.

	Least Interval.	Greatest Interval.	Difference.	Reduction.	Corrected establishment.	Long. W.	Corr. Estab. Greenwich Time.	Cotidal Hour.
	h m	h m	m	m	h m	m	h m	h m
Ceuta	1 4	2 55	111	48	1 55	21	2 16	2 12
Algesiras	0 45	2 38	113	49	1 34	21	1 55	1 52
Cadiz	1 2	2 32	90	38	1 40	25	2 5	2 2
Pera Algarve	1 13	1 55	42	21	1 34	33	2 7	2 4
Lagos Bay	1 55	2 19	24	12	2 7	35	2 42	2 38
Sines	1 25	2 51	86	36	2 1	35	2 36	2 32
Cascaes	0 50	2 22	92	39	1 29	35	2 4	2 1
Peniche	0 56	3 6	130	58	1 54	37	2 31	2 27
Bar of Oporto	1 50	3 25	95	40	2 30	35	3 5	3 2
Vianna	1 28	2 38	70	28	1 56	35	2 31	2 27
Camarinas	1 40	3 18	98	42	2 22	36	2 58	2 55
Ferrol	1 45	3 28	103	44	2 29	32	3 1	2 58
Santander	2 40	4 35	115	50	3 30	16	3 46	3 43
Bilboa	2 25	3 35	70	28	2 53	12	3 5	3 2
Ushant	0 48	2 25	97	41	1 29	20	1 49	1 46
Brest	2 48	4 17	89	37	3 25	18	3 43	3 36
Abrevrak	6 30	8 35	125	55	7 25	..	....	....
Lambrille (L'Isle de Sein)	3 3	4 30	87	36	3 39	..	....	....
Brehat (Isle)	4 53	6 15	82	34	5 27	12	5 39	5 28
St. Servan, Chaussey (Isle), Granville	5 7	6 55	108	47	5 54	6	6 0	5 48
Cherbourg	6 55	8 25	90	38	7 33	6	7 39	7 24
Barfleur	8 10	9 35	85	35	8 45	5	8 50	8 32
Havre	8 50	10 37	107	46	9 36	0	9 36	9 17
Dieppe	10 5	11 35	90	38	10 43	4E.	10 39	10 16
Cayeux	10 15	11 45	90	38	10 53	6	10 47	10 25
Boulogne	10 15	12 2	107	46	11 1	6	10 55	10 33
Calais	10 50	12 28	98	42	11 32	7	11 25	11 2
Dunkirk	11 15	12 50	95	40	11 55	12	11 43	11 19
Chenal de Port de Nieuport	11 20	12 55	95	40	12 0	11	11 49	11 25
Fort d'Ostend	11 35	13 22	107	46	12 21	12	12 9	11 44
Blankenberg	11 50	13 47	117	51	12 41	13	12 28	10 3
Rade de S^te^ Marie	14 55	16 35	100	43	15 38	16	15 54	13 23
Antwerp	15 18	17 57	99	42	16 0	18	16 48	14 26

Table III.

West and North Coast of Ireland.

	Least Interval.	Greatest Interval.	Difference.	Reduction.	Corrected establishment.	Long. W.	Corr. Estab. Greenwich Time.	Cotidal Hour.
	h m	h m	m	m	h m	m	h m	h m
56 *d*. . 56 *e* Sibyl Head	2 52	4 22	90	38	3 30	41	4 11	4 4
56 *f*. . 58 *a* Shannon Mouth	3 35	5 3	88	37	4 12	39	4 51	4 43
58 *c*. . 58 *g* Clare Coast	3 48	5 27	89	37	4 25	38	5 3	4 54
59 *b*. . 59 *g* Galway Coast	3 37	5 19	102	47	4 24	40	5 0	4 51
60 *a*. . 60 *c* Slyne Head, &c.	3 38	5 40	122	54	4 32	41	5 13	5 4
61 *a*. . 61 *e* Mishen, &c.	4 6	5 56	110	48	4 54	..	....	....
61 *c* Inisbofin	4 9	5 31	82	34	4 43	42	5 25	5 16
61 *e* Achilbeg	4 15	5 55	100	43	4 58	41	5 39	5 29
61 *f* Keel, Achil	4 0	6 7	127	56	4 56	42	5 38	5 28
62 *a* Elly Beg	3 55	6 1	126	56	4 51	..	....	....
62 *d* Blacksod Bay	4 0	6 35	155	70	5 10	41	5 51	5 41
62 *e* Ballygloss	4 2	6 6	124	55	4 57	38	5 35	5 25
63 *a*. . 63 *c* Killala Bay	4 20	6 4	104	45	5 5	37	5 42	5 32
64 *c*. . 64 *dd* Sligo Bay	4 40	6 25	105	45	5 25	36	6 1	5 50
64 *e*. . 65 *c* Donegal Bay	4 25	6 5	100	43	5 8	35	5 43	5 33
65 *d*. . 66 *c* Teelin Head, &c.	4 30	6 12	102	44	5 14	35	5 49	5 39
66 *f*. . 67 *b* Dunaff Head, &c.	5 2	6 28	86	36	5 38	30	6 8	5 57
67 *c* Malin Head	4 45	6 29	104	45	5 30	30	6 0	5 49
68 *h* Port Balinkae	5 13	7 5	112	49	6 2	..	....	....
68 *a* Port Rush	5 4	7 20	136	61	6 5	27	6 32	6 20
68 *e* Rachlin	6 0	10 0	240	113	7 53	25	8 18	8 2
68 *f* Torr Head	8 44	11 10	146	66	9 50	24	10 14	9 54
68 *c* Glenarm	9 55	11 15	80	33	10 28	23	10 51	10 30
68 *g*. . 69 *h* Larne, &c.	....	....	....	..	....	..	....	10 32

N.B. In the British observations the numbers refer to the districts of the coast-guard, and the letters *a*, *b*, *c*, &c. to the stations of each district; according to the list given in Table X.

61 *a*. Mishen or Mishoe (qu. same place?) differs 1^h in 1834 and 1835.

62. Dulaugh differs 48^m mean of 1834 and 1835.

62 *d*. Blacksod Bay irregular: differs in 1834 and 1835.

63 *e*. Lachen anomalous (flat).

63 *f* 64 *a*. Anomalous (flat).

64 *b*. Pulogherry anomalous.

64 *a*. Inniscrone very flat.

67 *d* 67 *e*. In Loch Foyle.

68 *b* 68 *c* 68 *d*. Extremely irregular.

Table IV.

South and East Coast of Ireland.

	Least Interval.	Greatest Interval.	Difference.	Reduction.	Corrected establishment.	Long. W.	Corr. Estab. Greenwich Time.	Cotidal Hour.
	h m	h m	m	m	h m	m	h m	h m
56 *d*. . 56 *e* Sibyl Head	2 52	4 22	90	38	3 30	41	4 11	4 4
55 *c*. . 56 *c* Dingle Bay, &c.	3 0	4 35	95	40	3 40	40	4 20	4 13
55 *b* Ballinskillings Bay	3 4	4 42	98	42	3 46	41	4 27	4 20
53 *h*. . 54 *b* Bantry Bay	3 14	5 5	111	48	4 2	39	4 41	4 33
52 *g*. . 53 *g'* Cape Clear, &c.	3 35	5 17	102	44	4 19	37	4 56	4 47
52 *a*. . 52 *f* Kinsale	......	......	....	....	4 29	34	5 3	4 54
51 *a*. . 51 *h* Cork, &c.	3 57	5 32	95	40	4 37	33	5 10	5 1
49 *f*. . 50 *e* Youghal, &c.	......	......	....	....	4 38	31	5 9	5 0
48 *e*. . 49 *e* Waterford, &c.	4 12	5 38	86	36	4 48	28	5 16	5 6
48 *a*. . 48 *d* Carnsore, &c.	4 40	6 15	95	40	5 20	25	5 45	5 34
47 *b*. . 47 *f* Cahore, &c.	6 35	8 0	85	35	7 10	25	7 35	7 24
47 *a* Arklow	9 15	11 30	135	60	10 15	24	10 39	10 19
46 *b*. . 46 *e* Bray	10 30	12 30	120	53	11 23	24	11 47	11 24
46 *a*. . 46 *a'* Dublin	10 30	12 7	97	41	11 11	25	11 36	11 14
73 *b*. . 73 *m* Lambay Island, &c.	10 20	12 3	103	44	11 4	24	11 28	11 6
73 *a* Boyne Mouth	11 0	12 50	110	48	11 48	25	12 13	11 49
72 *f* Clogher Head	10 25	11 55	90	38	11 3	25	11 28	11 6
70 *k* Portaferry	11 32	13 12	100	43	12 15	23	12 38	0 14
71 *a*. . 72 *e* Carlingford Station, &c.	10 12	11 49	97	41	10 53	23	11 16	10 54
70 *a*. . 70 *i* Donaghadee, &c.	10 10	11 43	93	39	10 49	22	11 11	10 49
68 *g*. . 69 *h* Larne, &c.	9 47	11 28	101	43	10 30	23	10 53	10 32

Table V.

West Coast of England.

	Least Interval.	Greatest Interval.	Difference.	Reduction.	Corrected establishment.	Long. W.	Corr. Estab. Greenwich Time.	Cotidal Hour.
	h m	h m	m	m	h m	m	h m	h m
42 *a*. . 42 *d* Scilly Isles	3 30	5 6	96	41	4 11	25	4 36	4 28
43 *a* Portreath	3 50	5 17	87	36	4 26	21	4 47	4 38
43 *c* Padstow	4 17	5 50	93	39	4 56	20	5 16	5 6
44 *a* Clovelly	4 27	6 0	93	39	5 5	17	5 22	5 12
44 *b* Barnstaple	......	......	....	....	......	16		
44 *c* Ilfracombe	4 38	6 22	104	45	5 23	16	5 39	5 28
45 *a* Portheinion	4 45	6 30	105	45	5 30	15	5 45	5 34
44 *d* Lynmouth	4 53	6 33	100	43	5 36	15	5 51	5 40
45 *b*. . 45 *c* Tenby	6 40	8 25	105	45	6 25	19	6 44	6 31

Table VI.

North and East Coast of Britain.

	Least Interval.	Greatest Interval.	Difference.	Reduction.	Corrected establishment.	Long.	Corr. Estab. Greenwich Time.	Cotidal Hour.
	h m	h m	m	m	h m	m	h m	h m
1 *a* Isle of Witham......	10 83	11 23	110	48	11 21	18 W.	11 39	11 17
1 *b* Cairn Ryan	10 28	11 24	116	51	11 19	19	11 38	11 16
1 *c* Port Logan, C. G. S.	10 22	11 5	103	44	11 6	19	11 25	11 3
2 *a* Lerwick	10 7	12 5	118	52	10 59	3	9 9	8 51
3 *a* Stromness..........	8 5	10 21	136	61	9 6	14	9 20	9 2
4 *a* Scrabsters (Thurso)..	7 45	9 9	84	35	8 20	14	8 22	8 6
5 *a* Cromarty	11 9	12 31	82	34	11 43	16	11 59	11 36
5 *b*.. 6 *c′* Elgin, &c...........	11 18	13 0	102	44	12 2	12	12 14	11 50
6 *c* Fraserburgh	11 10	12 46	96	41	11 51	8	11 59	11 35
6 *f* Rattry Head........	......	......	....	....	......	....	......	......
7 *a* Peterhead	12 10	13 40	90	38	12 48	8	12 56	31
7 *b*.. 8 *a* Aberdeen	12 25	14 15	110	48	13 13	8	13 21	55
8 *b* Johnshaven	12 38	14 27	109	47	13 25	9	13 34	1 7
8 *c*.. 8 *g* Montrose, &c.......	1 0	2 35	95	40	1 40	10	1 50	1 47
8 *h* Broughty Ferry	1 38	3 23	105	45	2 23	11	2 34	2 29
9 *a* St. Andrews........	1 3	2 38	95	40	1 43	11	1 54	1 51
9 *h* Elie (Fife)	0 47	2 25	98	42	1 29	11	1 40	1 37
10 Newhaven	1 24	2 48	84	34	1 58	11	2 9	2 5
10 *a*.. 10 *c* Dunbar, &c.........	1 16	2 58	102	44	2 0	10	2 10	2 6
10 *d*.. 10 *e* Berwick, &c.	1 30	3 15	105	45	2 15	8	2 23	2 19
11 *b*.. 11 *c* Holy Island, &c.	1 49	3 43	114	50	2 39	7	2 46	2 41
11 *d* Craster...........	......	......	....	....	......	....	......	......
11 *e* Alnmouth..........	......	......	....	....	......	....	......	......
12 *a* Blyth	2 14	3 55	101	43	2 57	5	3 2	2 56
12 *b*.. 12 *e* Sunderland, &c.	2 32	4 22	110	48	3 20	5	3 25	3 18
13 *a* Coatham	......	......	....	....	......	....	......	......
13 *b* Redcar	......	......	....	....	......	4	......	......
13 *c*.. 14 *a* Whitby, &c.........	3 2	4 40	98	42	3 44	2	3 46	3 39
14 *b* Filey.............	......	......	....	....	......	....	......	......
14 *c* Flamborough	3 5	4 22	77	31	3 36	0	3 36	3 29
14 *c′*.. 14 *f* Bridlington, &c.	3 46	5 32	106	46	4 32	0	4 32	4 23
16 *b*.. 16 *d* Wells, &c.	5 25	7 1	96	41	6 6	4 E.	6 2	5 50
17 *c*.. 18 *a* Cromer, &c.........	6 6	7 35	89	37	6 43	6	6 37	6 24
18 *b* Caistor	......	......	....	....	......	....	......	......
18 *b′* Yarmouth..........	8 26	9 51	85	35	9 1	8	8 53	8 35
18 *c* Gorlestone	......	......	....	....	......	....	......	......
18 *d* Corton	8 15	10 11	116	51	9 6	8	8 58	8 40
18 *e* Lowestoft..........	......	......	....	....	......	8	......	......
18 *f* Kessingham	8 57	10 27	90	38	9 35	8	9 27	9 8
18 *g* Southwold	9 18	10 58	100	43	10 1	7	9 54	9 34
19 *d* Orfordness	10 28	11 58	90	38	11 6	7	10 59	10 37
19 *f*.. 21 *c* Harwich, &c.	11 15	12 43	88	37	11 52	5	11 47	11 24

TABLE VII.

South Coast of England.

	Least Interval.	Greatest Interval.	Difference.	Reduction.	Corrected Establishment.	Long. W.	Corr. Estab. Greenwich Time.	Cotidal Hour.
	h m	h m	m	m	h m	m	h m	h m
42 *d*. . 42 *a* Scilly Isles	3 30	5 6	96	41	4 11	25	4 36	4 28
41 *b*′. . 40 *f* Mount's Bay	3 29	5 23	114	50	4 19	22	4 41	4 32
40 *e*. . 39 *h* Fowey, &c.	4 3	5 35	92	39	4 42	19	5 1	4 52
39 *g*. . 39 *a* East Looe, &c.......	4 14	5 58	104	45	4 59	18	5 16	5 6
38 *f*. . 37 *g*′ Plymouth Sound, &c.	4 24	5 56	92	39	5 3	17	5 20	5 10
37 *f*. . 37 *e* Prawle Head, Salcomb	4 36	6 12	96	41	5 17	15	5 32	5 21
37 *d*. . 37 *a* Torquay, &c.	5 0	6 30	90	38	5 38	14	5 52	5 41
36 *e*. . 36 *b* Teignmouth, &c.....	5 18	6 40	82	34	5 52	14	6 6	5 54
36 *a*. . 35 *c* Exmouth, &c.	5 13	6 25	72	29	5 42	14	5 56	5 45
34 *d*. . 33 *d* Weymouth Bay	6 17	7 27	70	28	6 45	10	6 55	5 42
33 *e* Kimmeridge Bay....	5 57	8 0	123	54	6 51	9	7 0	6 48
33 *d* St. Alban's Head....	6 18	7 28	70	28	6 46	8	6 54	6 40
33 *d* Swanage Bay	8 17	9 31	74	30	8 47	8	8 55	8 37
33 *c* Studland Bay	7 40	10 4	144	65	8 45	8	8 53	8 36
33 *d*. . 33 *a* Christchurch Bay....	8 5	9 53	108	47	8 52	7	8 59	8 48
32 *d* Lymington	11 14	13 0	106	46	12 0	6	12 6	11 52
30 *e*. . 29 *l* Portsmouth, &c.	10 55	12 15	80	33	11 28	5	11 33	11 10
31 *e* St. Lawrence	9 51	11 5	74	30	10 21	5	10 26	10 3
31 *c* Bembridge	10 35	12 1	86	36	11 11	5	11 16	10 54
29 *h* Selsea Bill	10 38	12 6	88	37	11 15	3	11 18	10 55
29 *h*. . 28 *m* Selsea to Brighton ..	10 18	11 31	73	29	10 47	2	10 49	10 27
28 *l*. . 28 *d* Rottingdean to Cuckmere	10 32	11 52	80	33	11 5	1	11 6	10 44
28 *c*. . 28 *a* Burling Gap to South Bourne	10 40	11 50	70	28	11 8	1 E.	11 7	10 45
27 *m*. . 27 *f* Gully Hill to Madox								
27 *i* Hastings	10 17	11 30	73	29	10 46	3 E.	10 43	10 21
27 *d*. . 26 *n*	....	....	..	..	....	4		
26 *n*. . 26 *l*′ Dungeness	10 16	11 46	90	38	10 54	4 E.	10 50	10 26
26 *k*. . 26 *a* Sutherland, Dover ..	10 22	11 53	91	38	11 0	5 E.	10 55	10 33
25 *n* Northend, Deal......	11 2	12 32	90	38	11 40	6	10 36	11 13
25 *h* Ramsgate	10 16	12 0	104	45	11 1	6	10 55	10 33
25 *g* Broadstairs	10 40	12 35	115	50	11 30	6	11 24	11 1

33 *b*, 33 *a*, 32 *f*, 32 *e*, 32 *c* rejected as imperfect or anomalous.

Table VIII.

Coasts of the Netherlands and Denmark.

	Least Interval.	Greatest Interval.	Difference.	Reduction.	Corrected Establishment.	Long. E.	Corr. Estab. Greenwich Time.	Cotidal Hour.
	h m	h m	m	m	h m	m	h m	h m
Westkapelle	0 20	1 48	88	37	0 57	13	0 44	0 42
Zwin or Sluice Dupe	0 33	1 57	84	35	1 8	14	0 54	0 52
Flushing	0 40	2 20	100	43	1 23	14	1 9	1 6
Browershaven	1 22	2 48	86	36	1 58	15	1 43	1 39
Goederede	1 35	3 5	90	38	2 13	15	1 58	1 54
Hellvoetsluys	2 25	3 55	90	38	3 3	16	2 47	2 41
Delflandshoofden	1 5	2 20	75	30	1 35	17	1 18	1 15
Brielle	2 5	3 25	80	33	2 38	16	2 22	2 17
Katwyk	1 20	3 5	105	45	2 5	17	1 48	1 44
Newdiep	5 55	7 55	120	53	6 48	20	6 28	6 14
Tor Schelling	7 48	9 20	92	39	8 27	21	8 6	7 49
Ameland	9 15	10 30	75	30	9 45	23	9 22	9 3
Rottum	10 0	11 25	85	35	10 35	26	10 9	9 48
Denmark.								
Norderpiep	11 48	13 25	97	41	12 29	36	11 53	11 30
Meldorf	12 20	14 0	100	43	13 3	37	12 26	0 0
Tonningen	12 40	14 10	90	38	13 18	36	12 42	0 15
Pelworm	12 35	14 15	100	43	13 18	35	12 43	0 16
Suder Oog	....	....	..	..	....	34		
Volterwick	12 7	13 50	103	44	12 51	35	12 16	11 50
Ording	11 35	13 15	100	43	12 18	35	11 43	11 18
Westerland (W. side of Sylt)	11 45	13 0	75	30	12 15	35	12 40	0 15
List (E. side of Sylt)	12 55	14 32	97	41	13 36	32	13 4	0 37
Wyck	12 57	14 30	93	39	13 36	35	13 1	0 34
Dagabül	12 50	14 30	100	43	13 33	35	12 58	0 31
Bongsiel	12 40	14 15	95	40	13 20	35	12 45	0 18
Amrum	12 13	14 7	114	50	13 3	33	12 30	0 4
Hoyer Canal	14 2	15 42	100	43	14 45	34	14 11	1 41
Hoyer	13 55	15 30	95	40	14 35	34	14 1	1 32
Sudwesthorn	13 12	14 55	103	44	13 56	32	13 24	0 56

The following are taken from Mr. Tegner's "Resultat," (sent along with the observations,) subtracting 30^m from his establishment, obtained by taking the mean from the 9th to the 18th of June.

	Latitude.				Corrected establishment.	Long. E.	Corr. Estab. Greenwich Time.	Cotidal Hour.
	° ′				h m	m	h m	h m
Helgoland	54 11½	....	..	..	11 44	31	11 13	10 50
Sönderhoe	55 20¾	....	..	..	14 21	34	13 47	1 18
Nordby	55 27	....	..	..	15 2	34	14 28	1 58
Blaavands-Huk	55 34	....	..	..	13 43	32	13 11	0 44
Nyminde Gab	55 47	....	..	..	14 40	33	14 7	1 38
Torskminde	56 20½	....	..	..	15 33	32	15 1	2 30
Agger	56 45	....	..	..	16 8	33	15 35	3 3
Hirtshals	57 35½	....	..	..	16 27	40	15 47	3 14
Skagen	57 42½	....	..	..	17 55	42	17 13	4 37

Table IX.

Coast of Norway, &c.

	Least Interval.	Greatest Interval.	Difference.	Reduction.	Corrected Establishment.	Long.	Corr. Estab. Greenwich Time.	Cotidal Hour.
	h m	h m	m	m	h m	m	h m	h m
[Scilly Isles	3 30	5 6	96	41	4 11	24 W.	4 35	4 27
Sibyl Head	2 52	4 22	90	38	3 30	40 W.	4 10	4 2
Blacksod Bay	4 0	6 35	94	40	4 40	41 W.	5 21	5 11
Donegal Bay	4 25	6 5	100	43	5 8	35 W.	5 43	5 33
Malin Head	4 45	6 29	104	45	5 30	30 W.	6 0	5 48
Scrabsters	7 45	9 9	84	35	8 20	14 W.		
Stromness	8 5	10 21	136	61	9 6	14 W.	9 20	9 1
Lerwick]	10 7	12 5	118	52	10 59	4 W.	11 3	10 41
Norway, going North.								
Tananger	8 45	10 13	88	37	9 22	24 E.		
Stavanger	9 8	10 55	107	46	9 54	24 E.	9 30	9 12
Skudesnæs	9 12	10 58	106	46	9 58			
Kumlesand. Kersford	9 14	10 36	82	34	9 48	22	9 26	9 8
Bergen	9 55	11 10	75	30	10 25	22 E.	10 3	9 43
Runde Ist	9 33	11 50	137	61	10 34	23 E.	10 11	9 50
Christiansund	10 0	11 42	102	44	10 44	31 E.	10 13	9 51
Froyen Ist. Point Fitteren	10 14	11 56	102	44	10 58	34 E.	10 24	10 4
Munkholm	10 30	12 13	103	44	11 14	44 E.	10 30	10 10
Vœroe	11 45	13 21	106	46	12 31	45 E.	11 36	11 12
Andænes. Lofoden	12 8	13 36	88	37	12 45	60 E.	11 45	11 22
Tromsoe	0 32	2 10	98	42	1 14	75 E.	2 29	2 27
Going South.								
Stavanger	......	......	....	....	9 54	....	......	9 12
Tananger	......	......	....	....	9 22			
Lindesnæs	1 36	3 50	134	60	2 36	28 E.	2 8	2 3
Christiansund	3 0	5 16	136	61	4 1	34 E.	3 27	3 19
Oxsoe	2 55	5 5	130	58	3 53			
Arendal	3 9	5 9	120	53	4 2	37 E.	3 25	3 17
Ostre Rusoer	2 48	5 12	144	65	3 53			
Jomfruhland	3 30	5 40	130	58	4 28			
Frederikswærn	3 15	5 37	142	64	4 19	41 E.	3 38	3 29
Langesund	3 30	5 20	110	48	4 18			
Talöern	3 30	5 56	146	66	4 36			
Frederikstadt	3 48	6 0	132	59	4 47			
Swelwigen	4 0	6 24	144	65	5 5			
Christiania	4 44	6 34	114	50	5 34	44 E.	4 50	4 39

In Greenland the high water at full and change is from 12 to 2. (Purdy, Memoir to accompany a Chart of the Northern Ocean, p. 24.)

Table X.

Greatest and Least Range of the Tide in June 1834 and 1835.

Scotland to the Thames.

Station.	Inspecting Commander.	1834. Date.	1834. Greatest Range.	1834. Date.	1834. Least Range.	1835. Date.	1835. Greatest Range.	1835. Date.	1835. Least Range.
			ft. in.		ft. in.		ft. in.		ft. in.
1 *a* Isle of Whithorn (Wigtonshire)....	Comm^r J. C. Bennet.	20 P	19 11	15 A	9 5	11 A	20 7	20 A	6 4
1 *b* Cairn Ryan..........	———	21 A	8 11	16 A	6 0	13 P	9 5	21 A	4 8
1 *c* Port Logan..........	———	....	12 5	15 A	8 8	12 A	13 3	20 A	7 4
2 *a* Lerwick (Shetland)....	Lieut. W. H. Brand,	....	6 0	....	3 3	13 P	6 5	17 P	2 4
3 *a* Stromness (Orkney) ..	Lieut. Ch. Jobson.	21 P	9 10	14 A	4 4	10 P	10 7	19 P	3 6
4 *a* Scrabsters (Thurso)....	Mr. G. Culmer.	....	....	....	....	11 P	15 3	20 A	5 8
5 *a* Cromarty...........	Mr. J. Prosser.	22 A	12 6	14 A	7 0	12 P	13 2	....	4 11
5 *b* Burghead............	———	....	11 0	....	6 8	12 P	12 5	19 A	5 3
5 *c* Lossiemouth.........	———	....	10 10	....	6 5	12 P	12 0	....	5 5
6 *a* Buckie.............	Mr. T. Blake.	21 P	10 8	....	5 10	12 A	11 3	....	4 6
6 *a'* Cullen.............	———	....	....	....	....	10 P	11 3	20 P	3 5
6 *b* Portsoy...........	———	....	....	....	....	12 P	11 2	18 P	4 0
6 *b* Sandend...........	———	22 A	10 7	16 A	6 2				
6 *c* Banff.............	———	21 P	10 10	14 A	6 1	....	11 0	19 A	4 9
6 *c* Gardenstone........	———	....	....	....	....	....	11 0	....	4 8
6 *d* Pennan............	———	21 P	10 6	15 A	6 2				
6 *e* Fraserburgh..........	———	22 A	10 6	14 A	6 1	....	11 1	....	4 8
6 *f* Rattray Head........	———								
7 Aberdeen............	Mr. T. Richmond.	22 P	11 3	16 A	7 4				
7 *a* Peterhead...........	———	21 A	10 4	....	5 8	....	12 0	20 A	5 4
7 *b* Colliestown..........	———	....	11 9	14 A	7 0	....	12 10	19 P	4 11
7 *c* Bethelvie............	———	8 P	11 1	17 A	8 0	13 A	12 10	18 A	5 0
7 *d* The Don (near Aberdeen)	———	22 A	11 10	14 A	8 0	12 P	12 10	20 A	5 11
7 *e* Cove Bay............	———	....	12 4	16 A	7 5	11 P	12 7	19 A	5 7
7 *f* Muchals............	———	21 A	12 3	....	5 7	11 A	13 4	21 A	6 2
8 *a* Katerline............	Mr. D. F. Wilson.	22 A	13 0	14 A	7 10	12 P	13 10	19 A	6 0
8 *b* Johnshaven..........	———	....	13 4	....	7 10	....	14 3	....	6 1
8 *c* Uzon.............	———	....	13 9	....	8 6	11 P	14 2	....	6 3
8 *d* Red Castle..........	———	....	14 5	....	8 10	12 P	14 8	....	6 8
8 *e* Auckmithie..........	———	22 P	15 1	13 A	9 8	11 P	15 2	....	6 10
8 *f* Arbroath............	———	22 A	14 3	16 A	8 11	12 P	15 1	....	6 7
8 *g* Westhaven..........	———	....	14 6	....	9 0	....	15 6	....	6 10
8 *h* Broughty Ferry......	———	....	14 6	....	9 6	....	15 0	....	7 3
9 *a* St. Andrews........	Lieut. H. Randall.	....	14 9	14 A	9 0	12 P	15 6	21 A	7 7
9 *b* Elie Fife............	———	....	16 1	14 P	10 7	....	16 3	19 P	9 0
10 Newhaven..........	Comm^r J. J. Arrow.	....	16 4	14 A	10 1	....	17 10	20 P	8 7
10 *a* North Berwick......	———	....	15 2	16 A	9 6	....	16 1	19 A	7 0
10 *b* Dunbar............	———	....	14 4	15 A	8 8	11 P	15 9	19 P	7 9
10 *c* Redheugh..........	———	....	11 8	14 A	6 0				
10 *d* Burnmouth..........	———	....	14 2	....	8 10	12 P	15 0	19 A	6 11
11 *a* Berwick upon Tweed..	Comm^r J. C. Hudson.	....	14 0	16 A	8 8	12 P	15 0	....	6 8
11 *b* Holy Island..........	———								
11 *c* Newton...........	———	....	14 6	14 A	8 9	....	14 6	....	6 4
11 *d* Craster Haven........	———	....	14 5	....	8 9	10 P	10 5	19 P	6 3
11 *e* Alnmouth..........	———	22 P	11 7	....	7 8				
12 *a* Blyth Haven........	Mr. J. W. Cuff.	22 A	14 9	....	8 11	12 P	15 3	19 A	6 10
12 *b* North Shields........	———	....	13 11	....	8 9				
12 *c* Sunderland..........	———	....	14 4	16 A	8 10				
12 *d* Hawthorn Hive......	———	....	14 3	14 A	8 11	....	15 8	21 P	7 3
12 *e* Black Hales..........	———	....	15 2	15 P	8 5	....	15 10	....	7 6
13 *a* Coatham............	Comm^r J. Kains.	....	15 6	16 A	9 8				

Table X. (Continued.)

Station.	Inspecting Commander.	1834. Date.	1834. Greatest Range. ft. in.	1834. Date.	1834. Least Range. ft. in.	1835. Date.	1835. Greatest Range. ft. in.	1835. Date.	1835. Least Range. ft. in.
13 *a* Redcar	Commr J. Kains.	....	15 6	....	9 7				
13 *c* Staithes	———	22 P	17 0	16 P	11 3				
13 *d* Whitby	———	22 A	15 10	14 A	9 8	11 A	16 6	19 A	7 6
13 *e* Robin Hood's Bay	———	....	14 11	....	9 3	12 P	15 11	....	6 11
14 Bridlington	Mr. W. Dean.	....	16 5	....	10 6	11 P	17 0	17 P	7 11
14 *a* Scarborough	———	....	15 5	....	9 9	12 P	16 11	19 A	7 1
14 *b* Filey	———	....	14 1	15 P	9 6	11 A	16 0	22 P	7 5
14 *c* Flamborough Head	———	....	16 0	15 A	10 0	11 P	17 11	22 A	7 0
14 *d* Ulrome	———	....	16 9	14 A	9 10	....	17 4	19 A	7 5
14 *e* Hornsey	———	....	16 6	....	10 7	12 P	18 0	....	7 9
14 *f* Sandlemere	———	....	16 10	....	10 8	10 A	18 6	....	8 4
14 *g* Grimsby	Commr P. Christie.								
14 *h* Clay Hole	———								
15 *a* Sutton	———								
15 *b* Hob Hole	———								
16 *a* Hunstanton	———	....	....	....	....	12 A	21 10	19 P	10 2
16 *b* Burnham	Commr R. C. Curry.	....	....	....	....	12 A	11 5	17 P	3 8
16 *c* Wells	———	22 A	16 8	14 A	8 6	11 P	18 4	20 P	7 0
16 *d* Morston	———	....	16 7	15 P	10 9				
16 *e* Brancaster	———	....	....	....	....	12 P	19 8	20 A	13 10
17 *a* Weybourne	Commr J. B. M'Hardy.	....	....	....	....	13 P	15 11	19 P	8 2
17 *b* Sherringham	———	....	17 0	14 A	11 0				
17 *c* Cromer	———	....	14 9	14 P	8 5	....	14 11	22 P	6 10
17 *d* Sydestrand	———								
17 *e* Mundesley	———								
17 *f* Bacton	———								
17 *g* Happisburgh	———	....	12 9	13 A	7 4				
17 *g* Hasbrough	———								
17 *h* Palling	———	....	9 11	14 P	6 1				
18 Yarmouth	Commr Spencer Smyth.	....	....	....	....	13 A	6 11	20 P	2 0
18 *a* Winterton	———	....	....	....	....	....	8 6	19 P	2 10
18 *b* Caister	———								
18 *c* Gorleston Pier	———								
18 *d* Corton	———	20 P	6 2	16 P	4 0	11 P	6 11	....	3 1
18 *e* Lowestoft	———	21 A	6 11	14 P	3 10	12 P	6 10	20 A	2 6
18 *f* Kessingland	———	21 P	5 11	13 A	4 3	13 A	6 10	17 A	2 6
18 *g* Southwold	———	22 A	7 2	14 P	4 6	11 A	6 5	19 P	2 10
19 *a* Mismere Haven	Commr Charles Moore.								
19 *b* Sizewell Gap	———								
19 *c* Aldborough	———								
19 *d* Orfordness	———	....	9 0	15 A	6 0	13 A	9 3	....	5 3
19 *e* Orford Haven	———	....	9 9	16 A	6 5	13 P	9 10	....	6 0
19 *f* Woodbridge	———	9 A	9 10	17 A	6 2	....	10 10	....	5 4
20 *a* Watton Gap	Commr G. S. Dyer.	22 A	12 2	14 P	8 5				
20 *b* Harwich Harbour	———	....	11 6	15 P	7 10	10 A	12 0	....	6 0
20 *c* Clacton	———	....	13 9	....	7 9				
20 *d* St. Osyth Tower	———	22 P	15 1	14 P	9 9	....	14 7	....	7 10
20 *e* St. Osyth Stone Point	———	....	....	....	....	13 A	15 7	....	8 4
20 *e'* Brightlingsea Creek	———								
20 *f* Richmond West Mersea	———								
21 *a* Bradwell (Essex)	Commr G. A. Halsted.	22 P	15 5	16 P	11 0	13 A	15 4	19 P	8 6
21 *b* Tillingham	———								
21 *c* Crouch River	———								
21 *c* Burnham	———								
21 *d* Kennett Head	———								
21 *e* Shoebury Ness	———								
21 *f* Southend	———								

Table X. (Continued.)

Thames to Scilly Islands.									
Station.	Inspecting Commander.	1834.				1835.			
		Date.	Greatest Range.	Date.	Least Range.	Date.	Greatest Range.	Date.	Least Range.
			ft. in.		ft. in.		ft. in.		ft. in.
22 *a* Bugsby's Hole	Comm^r Thomas Bushby.	22 P	19 11	16 P	15 5				
22 *b* Woolwich	———	....	20 3	....	15 7	13 A	20 11	19 P	13 3
22 *c* Erith	———	....	18 11	....	14 7	....	19 6	21 P	11 6
22 *d* Greenhithe..........	———	....	19 1	15 P	14 7	....	20 4	19 P	12 2
22 *e* Gravesend	———	....	18 5	....	13 8	....	19 5	....	12 5
22 *f* Cliff Creek..........	———	....	18 6	....	13 2	....	19 0	....	12 3
22 *g* Yantlett Creek	———	....	16 10	16 P	12 2	....	16 10	....	10 4
22 *h* Colemouth Creek	———	22 A	17 11	14 P	11 0				
22 *i* Rainham............	———	....	17 8	16 P	12 5				
22 *k* Haven Hole	———	....	....	....	....	11 P	18 5	....	10 10
23 *a* The Bathurst, Queensb.	Comm^r W. Kelly.	....	17 0	....	12 2	....	17 5	....	10 1
23 *b* Sheerness	———	22 P	16 10	....	12 3				
23 *c* Eastend Lane........	———								
23 *d* Hensbrook	———	....	....	....	....	....	17 4	....	9 4
23 *e* Warden Point........	———								
23 *f* Leysdown	———	....	15 10	....	11 2	15 A	17 1	....	9 6
23 *g* Shellness	———	....	....	....	....	13 A	16 8	20 A	8 5
24 *a* Fountain Hard, up Stangate Creek ..	Comm^r R. Barton.	....	15 0	15 A	10 3				
24 *b* Milton..............	———								
24 *c* Elmley Ferry	———								
24 *d* Conyer Creek........	———								
24 *e* The Forester, E. Swale	———	22 A	19 0	....	11 9	12 A	17 8	20 P	9 7
24 *f* The Beresford, Faversham Creek	———								
24 *g* Sandgate	———	22 A	20 8	15 A	14 10				
24 *h'* Seasalter Cliff	———								
24 *h* Seasalter, C. G. S.....	———								
24 *k* Whitstable Harbour ..	———								
24 *k'* Tankerton	———	22 P	16 0	17 A	11 3				
24 *l* Swale Cliff..........	———	....	16 0	16 P	11 0				
24 *m* Herne Bay..........	———	....	16 0	....	9 10				
24 *n* Bishopstone	———	22 A	16 6	....	9 9	13 A	17 6	21 A	8 0
24 *o* Reculver............	———	....	15 8	....	9 8	....	16 5	....	8 0
25 *a* St. Nicholas, C. G. S.	Comm^r S. Helland.	....	13 0	14 P	11 0	14 A	14 0	18 P	8 4
25 *b* Epple Bay	———	....	13 9	....	9 6	12 A	14 4	19 P	7 8
25 *c* Westgate	———	....	13 9	....	9 8	....	14 4	....	7 9
25 *d* Margate............	———	22 P	13 7	16 P	10 0				
25 *e* Newgate............	———	....	....	....	....	13 A	13 11	....	7 8
25 *f* Kingsgate	———	....	13 8	....	9 7	12 P	13 10	....	8 6
25 *g* Broadstairs..........	———	....	14 2	15 A	11 8	27 A	13 11	20 A	10 2
25 *h* Ramsgate	———	....	15 8	....	11 0	11 P	15 7	22 P	10 3
25 *i* Pegwell Bay	———	....	14 6	....	10 10	....	14 5	19 P	9 1
25 *k* North Shore	———	....	....	....	....	....	11 6	20 A	7 6
25 *l* Shingle End	———	....	....	....	....	12 P	17 1	19 P	8 8
25 *l'* Westbrook..........	———	....	....	....	....	11 P	14 1	....	8 0
25 *m* No. 2 Battery, Southdown	———	21 P	16 2	14 P	11 8	....	16 6	....	9 4
25 *n* No. 1 Battery, near Deal	———	22 A	16 10	....	11 11	11 A	17 5	....	9 7
25 *o* Northend, Deal	———	....	16 8	15 A	12 0	13 A	17 7	....	9 9
25 *p* Walmer	———	21 P	17 10	14 P	12 4	12 P	18 1	20 A	9 11
25 *q* Kingsdown..........	———	22 A	17 2	15 A	12 6				
25 *r* St. Margaret's Bay ..	———	....	17 10	....	12 11				

Table X. (Continued.)

Station.	Inspecting Commander.	1834. Date.	1834. Greatest Range. ft. in.	1834. Date.	1834. Least Range. ft. in.	1835. Date.	1835. Greatest Range. ft. in.	1835. Date.	1835. Least Range. ft. in.
25 *s* Cornhill Station	Commr S. Helland.								
26 *a* Casemates, Dover	Commr J. Sherer.	8 A	18 6	16 A	12 0				
26 *b* Townshend Battery	———	22 P	17 11	15 A	14 2				
26 *c* Lydden Point	———	22 A	20 0	....	14 3				
26 *d* Eastware Bay, Pelter Brig	———								
26 *e* No. 3 Tower	———								
26 *f* Folkstone	———								
26 *g* Sluice	———								
26 *g'* Sandgate	———	....	....	....	....	11 A	22 9	19 A	12 3
26 *h* Shore Cliff	———	22 A	21 6	15 A	14 0	....	22 2	20 A	11 6
26 *i* Fort Twiss	———	10 P	22 0	....	14 3	....	22 3	....	12 10
26 *k* Fort Sutherland	———	22 A	20 0	16 A	14 6	13 A	22 0	....	12 2
26 *l* Fort Moncrieff	———	22 P	19 9	....	13 11				
26 *m* No. 23 Tower, Dymchurch	———	21 P	22 8	15 A	15 0	14 A	20 0	....	12 6
26 *n* No. 24 Tower	———	22 P	21 3	15 P	14 10	10 P	22 4	....	11 8
26 *o* No. 27 Tower	———	21 P	22 6	15 A	14 0	12 A	22 5	....	11 10
26 *p* Littlestone	———	22 A	24 4	14 P	16 0	13 A	21 7	....	10 3
26 *q* Romney	———	....	....	....	....	....	21 10	....	7 7
26 *r* No. 2 Battery	———	....	....	....	....	12 A	23 4	19 P	12 6
26 *s* No. 1 Battery, Dungeness	———	....	....	....	....	....	23 6	20 A	12 8
26 *t* Redoubt, Dungeness	———	....	....	....	....	13 P	23 6	....	13 0
26 *u* No. 3, Dungeness	———	....	....	....	....	11 P	21 6	....	5 0
26 *u'* Lydd Station	———								
26 *w* Jury's Gap	———								
26 *x* Camber	———	22 A	22 5	16 A	12 8				
27 *a* Enchantress, C. G. S.	Commr Dawson Mayne.	....	....	....	....	....	24 7	20 A	13 6
27 *c* Rye Bay, 31 Tower	———	....	23 4	15 A	15 5				
27 *d* Winchelsea	———	....	....	....	....	12 P	23 0	....	11 9
27 *f* Maddocks, C. G. S.	———	....	....	....	....	12 A	23 5	....	7 6
27 *g* Farlight	———								
27 *h* Ecclesbourne	———								
27 *i* Hastings	———								
27 *k* Priory Station	———	....	....	....	....	11 P	22 11	....	12 3
27 *m* Gulley Hill	———	22 A	22 10	15 A	16 6	....	23 5	21 A	12 10
28 *a* Eastbourne	Commr James Morgan.	....	....	....	....	9 P	22 8	20 A	11 2
28 *b* Hollywell	———	....	....	....	....	11 A	21 9	....	11 6
28 *c* Berling Gap	———	....	20 3	....	14 0	12 A	21 2	....	11 0
28 *d* Crow Link Gap	———	....	19 7	....	13 11	10 P	20 4	....	11 6
28 *e* Cuckmere	———	....	....	....	....	....	20 0	20 P	10 0
28 *f* Blatchington	———								
28 *g* Newhaven	———								
28 *i* Rottingdean	———								
28 *k* Blackrock	———	22 A	18 0	15 A	14 6				
28 *l* Brighton	———	....	18 8	....	13 6	12 A	19 4	18 P	9 2
28 *m* Hove	———								
28 *o* Shoreham, C. G. S.	———								
28 *o'* Entrance to Shoreham Harbour	———	21 P	14 6	14 A	11 8	11 P	14 11	19 A	9 6
29 *a* Lancing	Commr John F. Appleby.	22 A	18 0	16 A	11 10	11 A	18 9	20 A	9 0
29 *b* Worthing	———	....	17 10	15 A	12 5				
29 *c* Kingstown	———	....	17 4	....	11 9				
29 *d* Littlehampton	———	20 P	17 0	....	11 5	13 P	18 4	18 P	9 8
29 *e* Elmer	———	22 P	16 5	....	10 11				

Table X. (Continued.)

Station.	Inspecting Commander.	1834. Date.	1834. Greatest Range.	1834. Date.	1834. Least Range.	1835. Date.	1835. Greatest Range.	1835. Date.	1835. Least Range.
			ft. in.		ft. in.		ft. in.		ft. in.
29 *f* Bognor	Commr John F. Appleby.	22 A	16 8	16 A	11 2				
29 *g* Pagham	———	....	16 3	15 A	10 8				
29 *h* Selsey	———	....	14 10	16 A	10 1	12 P	15 8	20 A	8 1
29 *i* Thorney, C. G. S.	———								
29 *l* Chichester Harbour	———	22 P	12 11	15 A	8 2	13 A	13 6	19 A	7 5
29 *l* Near Chichester Harbour	———	....	13 2	....	8 4				
30 *a* Hayling Island	Commr G. C. Blake.	....	13 3	....	8 10	12 P	13 10	20 A	6 8
30 *b* Langstone Harbour	———	20 P	13 1	....	8 0	13 P	13 7	....	7 0
30 *c* Southsea Castle	———								
30 *d* Portsmouth Dockyard	———	22 P	13 0	....	7 11	....	13 8	....	6 6
30 *f* Hill Head	———	....	12 0	....	7 0	12 P	12 10	....	5 8
30 *f* Stokes Bay	———	....	....	....	....	10 A	11 0	....	6 2
30 *g* Hamble Station	———	....	12 4	14 P	6 3	13 P	12 10	....	5 8
31 *a* Cowes Roads	Commr Charles Deare.	....	....	....	....	12 P	13 0	21 A	6 1
31 *b* Ryde	———	20 P	12 1	14 A	8 1	....	13 0	....	6 1
31 *c* Bembridge	———	22 A	11 4	15 A	7 10	11 P	11 8	20 A	6 4
31 *d* Sandown	———	22 P	10 10	15 P	7 11	10 A	12 2	19 A	6 4
31 *e* St. Lawrence	———	22 A	9 6	15 A	6 0	11 A	9 6	20 P	5 2
31 *f* Atherfield Rocks	———	....	....	....	....	10 P	7 4	....	2 6
31 *g* Freshwater	———	....	7 3	....	3 2	....	7 6	21 A	2 4
31 *h* South Yarmouth	———	....	7 6	....	4 1				
31 *i* Newton Harbour	———	....	....	....	....	12 A	8 3	....	3 0
32 *a* Southampton	Commr Geo. Bissett.	....	....	....	....	....	11 9	18 A	1 2
32 *b* Lepe	———	22 P	10 10	15 A	6 4	13 P	11 9	20 A	5 1
32 *c* Pitts Deep	———	....	8 9	....	6 4	12 P	8 5	21 A	5 6
32 *d* Lymington	———	....	....	....	....	27 A	9 0	19 A	5 1
32 *e* Hurst Castle	———								
32 *f* Barton Cliff	———	22 A	5 8	14 A	2 6	11 P	7 0	21 P	3 6
33 *a* Bourne Bottom	Commr Samuel Meredith.	22 P	5 4	15 A	3 9				
33 *b* Poole Harbour	———	22 A	5 2	....	1 9				
33 *c* Flag Head	———	....	....	....	....	....	6 5	21 A	1 2
33 *c* Studland Bay	———								
33 *d* Swanage Bay	———	22 P	3 10	14 P	2 0	10 P	6 4	21 P	1 9
33 *d'* St. Alban's Head	———	....	....	....	....	....	7 0	19 P	1 4
33 *e* Kimmeridge Bay	———	21 P	6 8	15 P	2 4	....	7 3	19 A	1 6
34 *a* Lulworth Cove	Commr Ch. Knight.	....	6 6	16 A	2 3	11 P	7 2	20 A	1 3
34 *b* Osmington Mills	———	22 P	6 10	....	3 3	....	7 3	18 P	3 2
34 *c* Weymouth	———	....	6 8	17 A	2 8	9 P	7 0	20 A	2 4
34 *d* Portland Castle	———	....	5 11	16 P	2 4	12 P	7 4	....	2 1
34 *e* Fleet	———								
34 *f* Langton, C. G. S.	———								
35 *a* Abbotsbury	Commr Henry Boteler.								
35 *b* Burton	———	7 P	12 2	14 A	5 7		12 7	21 A	1 3
35 *c* Bridport Harbour	———	21 P	13 0	15 A	6 4	10 P	13 6	20 P	3 8
35 *d* Chidcock	———	....	....	....	....	11 P	13 4	20 A	5 0
35 *e* Lyme Cobb Station	———	....	13 2	15 P	9 8	....	13 1	19 P	5 1
35 *f* Axmouth	———	22 P	12 10	16 P	6 5	....	13 4	20 P	5 8
35 *g* Beer, C. G. S.	———	....	13 6	14 P	7 9	10 P	12 11	18 A	5 11
35 *h* Branscombe	———	21 P	12 1	16 P	7 0	11 P	13 4	20 A	5 5
36 *a* Sidmouth	Commr Wm. Usherwood.	....	....	....	....	....	13 6	....	5 6
36' *a* Weston	———	22 P	14 0	17 A	5 4	....	13 7	21 A	5 4
36 *b* Budleigh Sallerton	———	22 A	13 9	14 P	6 6	....	12 9	19 A	4 8
36 *c* Exmouth	———	20 P	13 8	16 P	7 6	....	11 11	21 A	5 0
36 *d* Dawlish	———	....	....	....	....	12 P	14 1	20 A	5 8
36 *e* Teignmouth Harbour	———	22 A	12 10	15 P	7 5	....	13 0	....	6 3
37 *a* Babbacombe	Commr J. T. Talbot.	21 P	13 1	15 A	7 9	11 P	14 4	19 A	5 7

Table X. (Continued.)

Station.	Inspecting Commander.	1834. Date.	1834. Greatest Range.	1834. Date.	1834. Least Range.	1835. Date.	1835. Greatest Range.	1835. Date.	1835. Least Range.
			ft. in.		ft. in.		ft. in.		ft. in.
37 *a′* Torquay	Commr W. Usherwood.	21 P	14 4	16 P	8 2				
37 *b* Paignton	Commr J. T. Talbot.	21 P	14 4	16 P	8 1	11 P	14 6	21 A	5 7
37 *b′* Brixham Quay	———	22 P	13 4	15 A	8 0				
37 *c* Dartmouth	———	21 P	14 6	16 P	8 7				
37 *d* Torcross, Start Bay	———	....	13 10	15 P	7 9	12 P	15 0	20 A	6 4
37 *e* Prawles Head	———	....	16 2	16 P	9 2	....	15 1	....	7 1
37 *f* Salcombe	———	....	15 8	16 A	9 0	....	15 9	....	3 6
37 *g* Hope Cove	———	21 A	15 2	14 A	9 4	11 P	16 3	20 P	7 10
37 *h* Challabro	———	11 A	15 9	16 A	10 0	....	16 4	20 A	7 11
38 *a* Mothercombe	Commr C. Basden.	21 P	15 9	16 P	9 5	....	16 3	....	7 11
38 *b* Yealme	———	....	15 11	15 A	9 11	....	16 5	....	7 11
38 *c* Bovisand	———	22 A	15 0	14 P	9 5	12 P	16 4	....	7 10
38 *d* Stonehouse Point	———	22 P	15 7	16 A	10 2	....	16 9	20 P	8 8
38 *e* Cawsand	———	21 P	15 6	15 A	9 9	9 A	16 8	20 A	8 0
38 *f* Port Wrinkle	———	....	15 4	15 P	9 0	12 P	16 5	....	7 8
39 *a* East Looe	———	....	15 4	15 A	9 9	11 P	16 0	....	7 10
39 *b* Polperra	Commr George Pearce.	....	15 5	....	9 9	....	16 2	....	8 0
39 *c* Polruan	———	....	15 7	....	9 11	....	16 4	....	8 1
39 *d* Polkerris	———	....	15 9	....	9 10	....	16 3	20 P	8 1
39 *e* Porthpean	———	....	15 6	....	10 0	....	16 3	....	8 1
39 *f* Mevagissey	———	22 A	15 6	16 P	9 10	....	16 2	....	7 11
39 *g* Gorran Haven	———	....	15 5	15 A	10 3	12 P	16 5	....	8 0
39 *h* Port Lowe	———	21 P	15 2	....	9 9	11 P	16 0	....	7 11
40 *a* Gerran's Bay	Commr R. S. Triscott.	....	15 1	....	9 11	....	16 1	....	8 2
40 *b* St. Mawe's, C. G. S.	———	....	16 2	14 A	9 10	12 P	16 0	....	8 2
40 *c* Helford Harbour	———	....	....	....	....	....	15 10	....	8 1
40 *d* Coverack	———	....	14 6	15 A	9 5	11 P	15 9	....	8 0
40 *e* Cadgwith Cove	———	22 P	15 0	15 P	9 2	....	15 8	20 A	9 1
40 *f* Mullion Cove	———	22 A	16 3	....	9 8	....	16 1	19 P	7 7
41 *a* Prussia Cove	Commr Digby Marsh.	21 P	14 5	14 P	9 9	....	16 0	20 P	7 10
41 *b* Mousehole	———	....	15 0	15 A	9 0	12 P	16 11	....	8 2
41 *b′* Penzance Pier	———	....	15 4	16 A	10 3	11 P	16 6	....	8 3
41 *c* Sennen Cove	———								
42 *a* St. Mary's, Scilly	Mr. Charles Steele.	....	15 7	14 P	9 9	12 A	17 2	19 A	8 1
42 *b* St. Agnes	———	21 A	15 1	15 A	11 9	....	16 8	19 P	7 8
42 *c* Tresco, Scilly	———	21 P	15 10	14 P	10 5	10 A	17 7	20 P	8 4
42 *d* St. Martin's	———	....	16 0	....	9 11	11 P	17 6	....	8 6
North-west Coast of Cornwall and Devon.									
41 *d* Pendeen Cove	Mr. D. Williams.	21 P	18 5	15 P	11 10	11 A	19 10	18 P	10 0
41 *e* St. Ives	———	....	19 8	15 A	11 8	11 P	22 0	20 A	9 4
43 *a′* St. Agnes	———	22 A	21 0	14 P	11 9	....	23 4	20 P	10 2
43 *a* Portreath	———	21 P	20 6	15 A	12 10	....	21 8	20 P	10 6
43 *b* Newquay	———	....	23 4	15 P	13 10				
43 *c* Padstow	———	....	20 8	14 A	13 10	....	23 3	19 P	10 9
43 *d* Boscastle	———	....	22 6	15 A	13 5				
43 *e* Port Isaac	———	22 A	21 9	....	12 11	....	24 5	20 A	12 5
44 *a* Clovelly	Mr. J. Lister.	21 P	24 0	....	14 8	....	26 4	....	12 9
44 *b* Greysand Hill, near Banstaple	———	22 P	23 9	14 P	15 5	....	24 4	20 P	12 9
44 *c* Ilfracombe	———	....	26 4	15 A	21 6	....	28 6	....	13 8
44 *d* Lymouth	———	21 A	29 4	16 P	17 8	12 P	31 10	....	15 0
45 *a* Portheinion	Commr J. C. Fitzgerald.	21 P	25 10	15 A	15 3	11 P	27 2	20 A	13 6
45 *b* Tenby	———	....	24 7	14 P	11 9	12 A	25 9	....	5 4
45 *c* Newquay	———	....	11 0	15 P	3 9	11 P	15 5	19 P	6 0

Table X. (Continued.)

Coast of Ireland.

Station.	Inspecting Commander.	1834. Date.	1834. Greatest Range.	1834. Date.	1834. Least Range.	1835. Date.	1835. Greatest Range.	1835. Date.	1835. Least Range.
			ft. in.		ft. in.		ft. in.		ft. in.
46 *a* Dublin	Comm^r W. Nearne.	....	....	....	....	10 P	13 11	18 P	6 3
46 *a* Kingstown Harbour	———	20 P	11 3	15 A	7 4	....	11 6	17 P	6 1
46 *a* Kingstown	———								
46 *b* Bray	———	....	10 5	14 A	7 11	....	10 5	18 P	6 5
46 *c* Greystones	———	....	9 10	15 A	6 7	....	9 8	20 A	1 3
46 *d* Five Mile Point	———	21 P	9 3	15 P	4 11	....	9 1	17 P	4 0
46 *e* Wicklow Harbour	———	20 P	7 6	15 A	5 1	....	8 0	18 P	3 7
46 *e'* Long Rock	———	....	....	....	....	....	6 7	....	2 6
47 *a* Jack's Hole Point	———	....	....	....	....	....	4 9	17 P	1 8
47 Adrigole	———	21 P	9 6	15 P	6 2				
47 *b* Arklow	Lieut. F. S. Boileau.	21 P	3 7	12 P	1 6	10 A	2 11	16 P	0 1
47 *c* Kilmichael	———	....	4 4	....	2 8	11 P	3 8	18 P	1 8
47 *d* Ballymoney	———	....	3 0	13 P	0 6	11 A	3 8	....	0 6
47 *e* Glynn	———	....	3 2	13 A	0 10	11 A	3 5	....	0 2
47 *f* Cahore	———	....	3 0	15 P	0 6	....	3 6	19 P	0 2
47 *g* Blackwater	———	21 A	6 1	17 A	1 8				
48 Wexford	Mr. Thomas Dunlop.	....	....	....	....	10 P	5 9	16 P	1 0
48 *a* Curracloe	———	....	6 4	15 A	1 5				
48 *b* Rosslare	———	8 A	5 2	16 P	2 0	13 P	5 2	20 P	1 10
48 *c* Ballyglory	———	9 P	7 9	....	3 10	11 P	6 6	18 A	4 0
48 *d* Carnsore	———	21 P	7 10	....	4 5	10 P	8 6	19 A	4 0
48 *e* Kilmore Station	———	....	10 9	17 A	5 5	12 A	11 8	....	6 7
48 *f* Lough Bar	———	7 P	8 7	16 P	4 11	10 P	7 3	18 A	4 7
49 Waterford Harbour	Lieut. Charles Bagehot.	....	....	....	....	....	12 11	19 P	7 1
49 *a* Feathard Station	———								
49 *a'* Waterford Station	———	22 P	13 4	16 A	8 8	11 P	13 8	....	7 5
49 *b* Duncannon, Lumsden's Bay	———								
49 *c* Dunmore Station	———	....	....	....	....	....	12 11	20 P	6 3
49 *d* Ballymacan	———	21 P	10 5	16 P	6 10	....	12 0	19 A	6 0
49 *e* Tramore Station	———	9 P	11 1	18 A	2 8	11 A	13 0	20 P	6 8
49 *f* Boumahon	———	....	....	....	....	12 A	12 6	19 A	6 11
50 *a* Helwick Head	Comm^r H. E. Atkinson.	21 A	12 3	16 A	8 2				
50 *b* Ardmore	———	21 P	12 0	16 P	8 2				
50 *c* Youghall	———	....	11 9	....	8 5				
50 *d* Knockadoon	———								
50 *e* Ballycotton									
51 *a* Ballycrooneen	Comm^r Sir R. Hagon.	22 A	10 2	14 P	6 0	11 P	12 9	20 A	6 10
51 *a* Ballyrobin Point	———	....	12 2	16 A	6 9				
51 *b* Poor Head	———	21 P	12 6	15 P	8 0	11 P	12 4	....	6 6
51 *c* Roche Lighthouse	———	....	11 11	14 A	7 11	10 P	12 3	19 A	6 9
51 *d* East Ferry Station	———	....	13 3	16 P	8 9	12 P	13 2	20 P	6 11
51 *e* Cove of Cork	———	22 P	11 7	....	8 4	11 P	12 8	....	4 4
51 *f* City of Cork	———	21 P	13 2	14 P	9 3	....	13 7	19 P	7 9
51 *g* Crosshaven	Comm^r Thomas Greene.	22 P	11 9	15 A	8 3	....	12 7	19 A	7 1
51 *h* Robert's Cove	———	21 P	11 11	15 A	8 1	10 P	11 0	19 A	6 9
52 Sandy Cove	———	....	11 4	16 A	7 10				
52 *a* Oyster Haven	———	22 P	11 9	15 A	7 9	11 P	12 0	20 P	6 6
52 *b* Upper Cove	———	21 P	11 3	14 A	7 10	11 A	12 1	19 A	6 7
52 *c* Old Head Kinsale	———	22 P	11 10	15 P	8 4	11 P	12 7	19 P	2 6
52 *d* Howe Strand	———	21 P	11 5	15 A	7 6	....	11 10	20 A	6 5
52 *e* Courtmasherry	———	....	10 9	14 A	7 9	12 P	12 5	....	6 7
52 *f* Barry's Cove	———	9 P	11 3	15 P	8 6				

TABLE X. (Continued.)

Station.	Inspecting Commander.	1834. Date.	1834. Greatest Range.	1834. Date.	1834. Least Range.	1835. Date.	1835. Greatest Range.	1835. Date.	1835. Least Range.
			ft. in.		ft. in.		ft. in.		ft. in.
52 Ring Bar	Comm^r Thomas Greene.	21 A	10 2	15 A	5 8				
52 *g* Dunny Cove	———	....	9 10	....	5 5	11 P	11 1	19 P	6 4
52 *h* Dirk Cove	———	20 P	10 6	....	6 5	....	11 1	20 P	5 10
53 *a* Mill Cove	Comm^r W. Finlaison.	21 A	9 8	13 A	8 8	....	10 9	19 A	5 10
53 *b* Glandore	———	21 P	10 8	15 P	7 2	12 A	11 0	19 A	6 0
53 *c* Castle Townsend	———	21 A	10 5	....	7 4	11 P	10 10	20 A	5 3
53 *d* Barlogne	———	21 P	10 0	....	6 10	11 P	11 2	19 A	5 8
53 *e* Baltimore	———	....	9 6	16 P	6 7	....	11 1	....	5 6
53 Skull	———	....	9 10	15 P	6 5				
53 *g* Long Island	———	....	....	....	....	....	10 5	20 P	5 5
53 *g* Crookhaven	———	....	9 3	....	6 6	....	10 4	....	5 4
53 *h* Dunmanus	———	....	10 1	13 P	5 0	10 P	10 5	21 A	5 2
54 Bluehill, near Bantry	Lieut. A. Evanson.								
54 Collieries, Berehaven	———	....	....	....	....	10 P	10 6	19 P	0 6
54 Whitehorse Station, Bantry Bay	———	....	....	....	....	11 P	10 6	18 P	5 1
54 *a* Castleton	———	21 A	9 8	14 P	5 11	....	10 6	....	5 0
54 *a* Black Ball Station	———	22 A	9 7	....	6 2				
54 *b* Garnish	———	....	....	....	....	10 P	10 5	19 P	5 0
54 *c* Kilmichalog	———	20 P	10 7	15 P	6 3				
54 *d* Ballychroon	———	....	10 5	14 P	6 0				
54 *e* Whiddy Island	———	22 A	10 1	....	6 1				
55 *a* Whitestrand	Comm^r John Monday.	21 P	10 11	....	4 6				
55 *b* Ballinskelligs	———	....	9 9	....	6 0	11 P	11 0	19 P	4 9
55 *c* Port Magee, W. entrance to Valentia	———	20 P	10 7	15 P	6 2	....	11 5	20 A	5 5
55 *c* East end of Valentia	———	22 A	10 7	....	6 6	10 P	11 10	20 A	4 11
55 *d* Kells	———	20 P	9 2	....	2 6	11 P	12 0	19 P	5 3
56 Dunquin	———	22 A	13 8	14 P	8 4				
56 *a* Minard	Lieut. John Bowie.	20 P	11 0	14 P	6 1	11 P	12 4	19 P	5 1
56 *b* Dingle	———	21 P	10 8	16 A	5 11	....	12 1	....	5 7
56 *c* Ventry	———	....	11 1	15 P	6 7	11 A	11 11	....	5 5
56 *d* Ferriter's Cove	———	....	12 3	14 A	6 11	11 P	13 0	19 A	5 2
56 *e* Smerwick Harbour	———	21 A	11 11	15 P	6 11				
56 *e'* Ballydavid	———	....	....	....	....	11 A	13 0	19 P	5 7
56 *f* Brandon Bay	———	20 P	12 6	....	7 4	11 P	13 5	19 A	6 11
57 *a* Castle Gregory	Comm^r W. Shepheard.	22 A	13 4	14 P	7 11	....	14 0	21 A	6 8
57 *b* Barrow, C. G. S.	———	20 P	13 6	....	7 5	....	14 11	20 A	6 1
57 *c* Ballyheize	———	....	....	....	....	....	14 0	19 A	7 0
57 *d* Cashen River	———	21 P	12 6	....	8 6	9 P	14 0	19 P	6 0
57 *e* Beal, River Shannon	———	....	13 10	....	8 7	12 P	14 11	18 P	6 9
58 *a* Dunbeg	Comm^r G. E. Marshall.	20 P	13 5	15 A	7 4				
58 *a* Kilrush	———	....	13 9	....	8 4	11 P	15 0	19 A	6 7
58 *b* Kilcradane	———	....	13 3	15 P	6 4	....	14 5	18 P	6 9
58 *c* Kilkee	———	22 A	14 2	14 P	8 7	11 A	14 8	18 P	5 6
58 *d* Killard	———	20 P	13 4	15 P	7 8	11 P	14 7	18 P	5 2
58 *e* Seafield	———	....	13 3	14 P	7 11	....	14 2	19 A	6 2
58 *f* Freagh	———	19 P	12 6	....	8 0	10 P	13 7	19 P	7 6
58 *g* Liscannor	———	21 P	12 10	15 A	8 5	11 A	13 9	19 A	6 7
59 *a* Fairhill	Lieut. W. B. White.	20 P	14 2	15 P	4 3				
59 *b* North side of Arran	———	....	13 9	15 A	8 0	11 P	14 6	19 P	6 3
59 *c* Ballyonughan	———	....	14 0	14 P	8 1	....	15 4	18 P	6 9
59 *d* Newharbour	———	....	14 3	15 P	7 8	....	15 8	20 A	6 4
59 *e* Barna	———	....	13 8	14 P	8 4	....	15 5	....	6 0
59 *f* Costello Bay	———	....	13 5	....	7 10	....	13 3	19 P	6 5
59 *g* Lettermore	———	....	13 9	....	7 11	....	15 3	....	5 7
60 *a* Innislaken Island	Mr. John Andrews.	8 P	15 0	15 A	8 0	12 A	14 8	18 P	6 9

Table X. (Continued.)

Station.	Inspecting Commander.	1834.				1835.			
		Date.	Greatest Range.	Date.	Least Range.	Date.	Greatest Range.	Date.	Least Range.
			ft. in.		ft. in.		ft. in.		ft. in.
60 *b* Mannin Bay	Mr. John Andrews.	20 P	12 2	15 P	6 11	11 P	13 3	18 P	5 7
60 *c* Cleggan	———	21 P	12 2	14 P	6 10	....	13 3	19 P	5 6
60 *d* Killery	———	20 P	12 0	....	6 8	....	13 1	18 P	5 4
61 *a* Innishen...........	———	21 A	12 4	17 A	7 3	10 P	13 9	....	4 8
61 *b* Old Head	Lieut. Joseph Irwin.	21 P	10 8	16 P	8 1				
61 *c* Innisbofin	———	....	12 6	14 P	7 0	11 P	13 3	20 A	5 4
61 *d* Mynish	———	20 P	12 0	14 P	6 10	11 P	13 3	20 A	5 3
61 *e* Achill Beg	———	21 A	11 7	15 A	6 8	....	13 1	19 A	5 0
61 *f* Keel	———	22 P	11 6	15 P	7 0	12 P	12 3	20 A	5 1
62 Ballycroy	Lieut. John Nugent.	20 P	11 4	14 P	6 6	....	12 5	18 P	4 8
62 Dulaugh...........	———	....	....	....	....	10 P	12 7	19 P	5 0
62 Elly Bay Neptune	———	22 P	11 1	16 A	6 4	....	12 6	19 A	4 5
62 Bellmullet	———	....	9 11	....	5 0				
62 *a* Bullsmouth..........	———	20 P	11 6	14 P	6 7	....	10 6	20 P	3 2
62 *b* Doohooma	———	22 A	11 8	17 A	6 0	11 P	12 0	....	4 6
62 *d* Blacksod Station	———	20 P	11 1	16 A	6 3	....	12 7	....	4 10
62 *e* Ballyglass	———	....	10 8	14 P	6 7	....	11 8	19 P	3 6
63 *a* Doonkeeghan........	Lieut. W. Sterne.	....	10 6	....	6 0	....	11 9	18 A	4 11
63 *aa* Portaclog	———	22 P	10 4	15 A	5 8	....	11 10	19 P	4 8
63 *b* Port Terlin..........	———	8 P	10 11	16 P	5 1	12 P	11 6	....	4 8
63 *c* Bealderig	———	20 P	10 7	14 P	6 4	11 P	12 2	20 P	2 3
63 *d* Ballycastle	———	....	11 0	16 P	6 6				
63 *e* Lacken	———	21 A	11 2	15 A	5 6	11 P	12 4	19 A	4 9
63 *f* Kilcummin	———	20 P	11 1	16 P	6 0	....	12 5	20 A	3 11
63 *g* Ross	———	....	11 2	15 A	6 10	....	12 3	19 P	4 11
64 *a* Inniscrone	Lieut. H. J. Clifford.	22 A	10 8	14 P	6 3	....	10 11	21 A	4 6
64 *b* Pulloghery	———	....	....	....	....	....	11 9	20 A	0 7
64 *c* Pullendiva	———	20 P	11 1	....	6 1	10 P	11 11	19 P	4 11
64 *cc* Portavad...........	———	21 P	10 9	16 P	6 8	....	11 11	20 P	4 11
64 *d* Raughly	———	22 P	11 2	14 P	6 6	11 P	12 4	19 P	4 10
64 *dd* Sligo Harbour	———	....	11 1	13 P	5 11	....	12 6	....	5 1
64 *e* Mullaghmore	———	21 A	11 4	16 P	6 5	....	13 1	....	5 0
64 *f* Ballyshannon	———	21 P	10 11	14 P	7 1				
64 Port New	———	20 P	10 11	16 P	6 2				
65 *a* Dooran	Comm^r H. Layton.	....	11 7	14 P	6 9	12 P	11 9	....	3 6
65 *b* Trybane............	———	....	12 1	16 P	6 2	10 P	12 3	....	4 11
65 *c* Killybegs	———	21 P	11 3	14 P	6 10	12 P	12 0	....	4 11
65 *c* Teelin Harbour, East..	———	22 P	10 9	....	7 1	....	12 2	....	5 2
65 *c* Teelin Harbour, West	———	21 A	11 1	....	6 8	11 P	12 4	....	4 11
65 *d* Malinbeg	———	22 P	11 2	15 A	6 1	....	12 3	21 A	4 7
65 *e* Port Nov	———	22 P	10 10	16 P	6 2	11 P	12 3	19 P	4 8
65 *g* Daurus	———	....	11 0	13 A	6 8	11 P	12 2	....	4 8
66 Curran's Point	Comm^r W. B. Dobson.	....	11 6	16 P	6 8	10 A	12 2	20 P	5 0
66 Downing's Bay	———	....	13 2	14 P	5 8	12 A	11 6	20 A	5 9
66 *a* Rutland Island	———	21 A	10 8	14 A	4 6				
66 *b* Guidore	———	21 P	11 0	16 P	6 4	10 P	12 6	19 P	4 8
66 *c* Port Ballynash	———	....	10 7	15 P	5 5	11 P	10 8	18 A	4 10
66 *d* Sheephaven	———	22 P	11 5	....	5 7	....	12 7	20 P	3 11
66 *f* Crowris	———	21 P	13 2	....	6 11	....	12 10	19 P	4 8
66 *g* Rathmullen	Comm^r Charles Bosden.	....	13 0	14 P	7 5	....	13 9	....	4 11
67 *a* Dunree Fort	———	22 P	12 5	15 P	7 1	....	13 3	....	4 5
67 *b* Dunaff Head	———	21 P	11 5	....	5 6	....	11 5	18 A	4 10
67 *c* Malin Head	———	....	10 3	14 P	5 9	....	11 6	19 P	3 5
67 *d* Couldaff Glebe	———								
67 *e* Moville	———	22 P	6 9	15 A	3 8	....	7 2	....	2 3
68 *a* Port Rush	Comm^r E. W. Gilbert.	21 P	5 7	....	2 9	....	6 5	....	1 5
68 *b* Ballintrae	———	22 A	6 1	15 P	2 4	....	5 11	20 P	0 10

TABLE X. (Continued.)

Station.	Inspecting Commander.	1834. Date.	1834. Greatest Range.	1834. Date.	1834. Least Range.	1835. Date.	1835. Greatest Range.	1835. Date.	1835. Least Range.
			ft. in.		ft. in.		ft. in.		ft. in.
68 *b* Ballintry	Commr E. W. Gilbert.	21 A	4 6	15 A	1 7	....	4 5	18 P	0 7
68 *c* Glenarm	——	....	....	....	....	11 A	6 4	19 P	3 11
68 *d* Ballycastle	——	7 A	3 9	....	1 6	11 P	3 8	20 A	1 4
68 *d* Ballycastle, C. G. S.	——	....	....	....	....	10 P	11 8	19 P	5 4
68 *e* Rathlin Island	——	22 P	3 2	....	1 3	12 P	3 6	17 P	0 11
68 *f* Torr Head	——	17 P	5 7	11 A	2 5	10 A	4 10	19 P	1 6
68 *g* Cushendon	——	16 A	6 3	7 A	3 10	12 P	5 2	20 A	3 5
68 *h* Cushendall	——	21 A	5 9	15 A	3 5	11 A	4 1	19 A	4 0
69 *a* Garrow Point	Commr Douglas Cox.	....	6 2	16 P	4 10	9 A	6 9	18 P	3 10
69 *a* Belfast	——	8 P	9 7	17 P	7 1	11 P	10 3	20 A	6 7
69 *c* Larne	——	21 A	7 11	14 A	5 11	11 A	8 5	19 A	5 1
69 *d* Ballygally	——	....	7 3	....	5 2	12 A	7 5	19 P	4 7
69 *e* Port Muck	——	....	8 3	15 A	6 3	11 A	8 6	19 A	5 5
69 *f* Black Head	——	8 P	9 2	14 P	5 0	13 P	8 9	20 A	1 11
69 *g* Carrickfergus	——	21 A	9 10	15 A	7 0	11 A	9 10	....	6 1
69 *h* White House	——	20 P	9 8	13 P	6 10	....	10 2	18 A	2 10
70 Strangford	——	....	....	....	....	10 P	5 8	20 P	3 6
70 *a* Hollywood	Commr Charles Smith.	21 A	9 8	15 A	7 3				
70 *a'* Crawford's Burn	——	....	9 5	....	7 3	12 A	10 0	20 P	5 8
70 *b* Bangor	——	....	9 6	....	6 10	10 A	10 2	17 P	6 0
70 *c* Grimsport	——	19 P	9 6	....	6 10	....	10 7	18 P	5 10
70 *c'* Orlock Hill	——	21 A	10 1	....	7 3	....	10 8	21 A	4 6
70 *d* Donaghadee	——	....	11 4	....	8 0	....	11 8	19 P	7 2
70 *e* Millisle	——	....	12 0	....	8 7	....	12 10	17 P	7 4
70 *f* Ballywater	——	....	12 6	....	8 9	11 A	13 4	20 P	8 0
70 *g* Ballyhalbert	——	20 P	13 1	....	9 1	....	13 6	19 P	8 2
70 *h* Cloughy Bay	——	....	13 8	....	9 4	....	14 8	20 P	8 6
70 *i* Tarra Bay	——	21 A	14 0	....	9 10	....	15 10	19 P	8 8
70 *k* Portaferry	——	19 P	10 7	14 P	8 8	12 A	11 2	18 P	6 2
71 *a* Gun's Island	Commr Henry Ellis.	20 P	14 1	15 A	9 4	....	15 5	20 A	8 6
71 *b* Ardglass	——	....	14 6	....	10 4	10 P	15 6	....	8 7
71 *c* St. John's Point	——	22 A	14 9	....	9 6	....	16 2	19 P	7 4
71 *d* Newcastle	——	20 P	15 1	14 P	11 0	11 A	15 6	21 A	8 0
71 *e* Annalong	——	....	14 2	15 A	10 0	11 P	15 4	19 A	8 7
71 *f* Lee Stone	——	21 P	13 4	15 A	7 6	11 A	15 11	20 P	12 0
71 *g* Cranfield	——	20 P	14 0	17 A	11 0	11 P	15 5	19 A	8 1
72 *a* O'Meath	Commr Edw. Handfield.	22 P	14 10	15 A	10 8	....	15 9	19 P	9 4
72 *a* Carlingford Station	——	21 A	14 5	15 P	10 8				
72 *b* Greenore Point	——	....	14 3	15 A	10 0	10 P	15 3	....	8 11
72 *c* Cooley Point	——	21 P	15 4	....	10 3	....	15 4	20 P	8 6
72 *c* Giles Quay	——	21 A	16 1	....	9 7	11 P	15 9	20 A	8 3
72 *d* Soldier's Point	——	....	13 3	....	6 11	....	14 0	21 A	8 7
72 *e* Dunany Point	——	20 P	14 10	....	9 7	....	15 7	19 A	8 4
72 *f* Clogher Head	——	21 A	13 9	14 P	5 11	....	15 2	18 A	2 9
73 *a* Mouth of the Boyne	Commr Thomas Ross.	21 A	9 11	15 A	7 11	10 P	9 6	20 P	6 7
73 *b* Nannywater	——	20 P	13 8	....	9 0	11 P	14 4	20 A	7 3
73 *c* Balbriggan	——	....	13 6	....	8 11	....	14 4	....	7 11
73 *d* Skerries	——	....	....	....	....	11 A	13 0	18 P	9 2
73 *e* Rush	——	....	....	....	....	11 P	12 6	20 P	8 2
73 *f* Lough Shinney	——	21 P	14 0	14 A	9 11				
73 *g* Rogerstown	——								
73 *h* Portrane	——	21 P	12 4	15 A	8 6	11 A	13 11	20 A	7 3
73 *i* Lamboy Island	——	22 A	12 6	....	8 2	10 P	13 5	20 A	6 8
73 *k* Malahide	——	....	....	....	....	11 A	12 0	....	7 0
73 *l* Baldoyle Creek	——	....	....	....	....	....	10 5	18 P	7 5
73 *m* Howth Harbour	——	21 A	12 0	....	8 0	11 P	13 1	20 A	7 0

TABLE X. (Continued.)

Coast of America.

Honourable MAHLON DICKERSON, Secretary of the Navy, United States.

Station.	Observers.	Latitude N.			Longitude W.			Date.	Greatest Range.		Date.	Least Range.	
		°	′	″	°	′	″		ft.	in.		ft.	in.
Eastport (Maine)	Jery Burgin, Inspector.	44	54	0	66	56	0	11 P	22	10	21 A	14	8
Mount Desert Island ..	Henry S. Jones.	44	9	0	68	31	0	....	13	4	22 P	8	1
Portland	John Williams.	44	39	16	70	20	30	....	12	2	21 A	7	0
Portsmouth Navy Yard	Jos. R. Jarvis, Lieut. United States Navy.	43	4	44	70	45	0	10 P	10	4	20 A	6	1
Gloucester	John Webber.	42	36	0	70	42	0	10 P	12	8	21 P	6	9
Boston Navy Yard....	Commodore John Downes, Duncan Bradford, Professor of Mathematics, Henry French, passed Midshipman.	42	20	0	71	4	9	....	14	8	22 A	10	11
Cape Cod	Richard Ainsworth.	42	2	6	70	4	0	....	12	6	21 A	7	3
Province Town	Major James D. Graham, United States Corps of Topographical Engineers.	42	2	45	70	13	0	....	12	6	22 A	7	1
Nantucket	William Coffin.	41	16	12	70	7	42	12 A	2	6	....	0	11
Newport............	Col. J. G. Totten, Engineers, assisted by Lieut. Child, Artillery.	41	29	0	71	21	14	10 P	6	0	21 A	2	6
Warren	Lieut. Joel Abbot, United States Navy.	41	44	0	71	15	15	....	6	8	20 A	2	7
Gardiner's Bay	M'Perry, Master Commander, United States Navy.	41	4	0	72	5	0	....	3	5	21 A	1	5
New York Navy Yard..	Commodore C. G. Rigeby, Commander M. F. Mix.	40	42	40	74	1	8	....	6	6	20 A	1	6
Sandy Hook	Josiah Tattnall, Lieut. U.S. Navy.	40	28	0	74	1	0	....	7	1	....	2	7
Delaware (Breakwater)	A. R. Hetzel, 2nd Infantry.	38	57	0	75	10	0	10 P	6	4	20 P	3	0
Old Point Comfort....	C. H. Kennedy, Lieut. United States Navy.	37	0	0	76	22	10	10 P	3	9	21 A	1	10
Gosport Navy Yard ..	William P. S. Sanger, Engineer.	36	50	50	76	18	47	11 P	4	5	21 A	2	1
Cape Hatteras	Isaac S. Farrow, and Joseph C. Jennett.	35	14	0	75	30	0	9 P	5	6	19 A	2	0
Cape Fear River	J. Dimeck, Capt. Artillery.	33	48	0	78	9	0	10 P	6	11	20 A	2	7
Charleston	W. H. Pettes, Lieut. Artillery.	32	44	0	80	1	0	11 P	7	11	....	3	6
Savannah	C. S. Merchant, Capt. Artillery.	32	2	0	81	3	0	10 P	8	5	....	1	5
St. Augustine........	F. L. Dancy, Lieut. Artillery.	29	48	30	81	35	0	10 P	6	7	21 A	3	1
Key West	F. L. Dade, Brevet Major, United States Army.	24	29	0	81	55	0	13 A	2	6	21 P	1	6
Tampa Bay..........	R. A. Lantzinger, Major, United States Army.	28	5	0	83	18	0	15 P	3	3	17 P	0	8
Pensacola Navy Yard..	W. Chauncey, commanding Navy Yard, W. K. Latimer, Master Commandant, and Nahum Warren, Sailing Master.	30	32	0	87	12	0	13 A	2	3	20 A	0	10
Mobile Point	F. S. Belton.	30	13	0	88	21	0	11 A	2	1	....	0	8
Fort Wood..........	John M. Creylar, Assistant Surg., United States Army.	29	15	0	89	35	0	13 P	2	7	20 P	0	2
Fort Pike	John Mountfort, Major, Artillery.	28	0	0	89	0	0	....	1	8	21 A	0	0

Table X. (Continued.)

Coast of Portugal.

Baron de Sa da Bandeira, Minister and Secretary of State for the Marine Department. A Commission consisting of Major Gen. Jose Xavier Bressan Leite, Col. Marino Miguel Franzini, Capt. João de Fontes Pereira de Mello, Capt. Antonio Lopez da Costa Almeida, and Joze de Mello de Govea Prego.

Station.	Observers.	Greatest Range.	Least Range.
		ft. in.	ft. in.
Oporto			
Vianna	—— Carvalho.	10 3	4 3
Peniche	Captain Sá.	10 5	4 6
Cascaes	Captain Leotte.	10 0	4 6
Sines	Captain Nieira.	9 0	3 10
Pera	Lieutenant Rego.	10 6	4 2
Bay of Lagos		10 9	4 0

Coast of Spain.

Count Toreno.

Station.	Observers.	Greatest Range.	Least Range.
		Spanish ft. in.	Spanish ft. in.
Bilboa	Henry Thompson, Second Master, Saracen.	13 5	4 1
Santander	Jozé M. Chrum.	14 3	6 4
Ferrol	Captain Antonio Doral.	13 5	5 8
Camariñas	Angel Valdez.	12 0	5 6
Cadiz	Captain de Puonto, Luis de Caig.	11 1	4 9
Algesiras	Andres Ortiz.	3 5	1 5
Ceuta	Gorge P. Lasso de la Vega.	3 5	1 6

Coast of France.

M. Beautemps Beaupre'.

Station.	Observers.	Greatest Range.	Least Range.
		French ft. in.	French ft. in.
Dunkerque		16 5	9 5
Calais			
Boulogne			
Cayeux			
Dieppe			
Havre			
Lambrille		17 8	7 8
Barfleur	De Lamisse.	17 3	8 3
Cherbourg	D'Aboville.	17 3	7 5
Granville		37 6	16 7
Chausey		30 4	20 2
St. Servan	P. Trehouart.	34 7	14 5
Bréhat	A. D. Protet.	30 32	13 5
Abrevrack	Jaouen.	22 04	10 05
Ile d'Ouessant	Duchou.	19 0	8 7
Brest	Escande.	19 6	9 1

Coast of Belgium.

Station.	Observers.	Greatest Range.	Least Range.
		French Metres.	French Metres.
Fort d'Ostend	A. Kempynck.	4·10	2·95
Blankenberg	J. A. Claeys.	4·17	3·71
Rade St. Marie	D. T. A. Nuerveus.	2·35	1·80
Anvers	T. Sams.	4·67	2·80
Chenal du Port de Nieuport	A. Kempynck.	4·85	2·65

TABLE X. (Continued.)

Coast of the Netherlands.

Dr. G. MOLL, General Inspector.

Station.	Observers.	Greatest Range.	Least Range.
		Dutch Ells.	Dutch Ells.
Spaarndam	I. Kros.		
Zwanenbury	P. de Leeuw.		
Amsterdam	C. Aleywyn.		
Rottum	A. van Rhyn.	2·70	1·62
Ameland	Fenning.	2·30	1·40
Ter Schelling	J. H. Hofmeister.	2·10	1·30
Nieuwdiep	W. H. Sahernis.	1·34	·80
Kykduin	A. E. Thierens.	1·57	·91
Petten	G. Tabuis.	1·87	1·40
Katwyk	J. R. Cambier.	2·02	1·02
Delflandschehoofden	J. R. Loutman.	1·88	1·15
Brielle	A. A. Bouricius.	1·76	·93
Hellevoetsluis		2·34	1·11
Goederede	J. Aulladig.	2·81	2·36
Brouwershaven	V. H. Tulleken.	3·21	2·77
Westkapelle	Byl desroe.	3·97	2·18
Flushing		4·31	2·75
Zwin or Sluice deep		4·30	3·18

Coast of Denmark.

Major-Gen. CHRISTENSEN. Superintendent, Captain TEGNER.

Station.	Observers.	Latitude N.	Longitude E.	Greatest Range.	Least Range.
		° ′	° ′	Danish Feet.	Danish Feet.
Altona	Superintendent, Christensen.	53 32½	9 57	8·20	4·89
Pin Aue	Captain Christensen.	53 40½	9 32½	9·82	6·77
Gluckstadt	———	53 47	9 25	9·97	6·52
Brunsbuttel	———	53 54	9 1½	9·85	6·14
Meldorf	———	54 5½	9 1½	10·05	7·67
Tonningen	Superintendent, Major Lund.	54 18¼	8 56½	9·76	6·42
Stein Schleuse	———	54 21	9 17	7·81	5·18
Vollerwick	———	54 17	8 44½	10·58	6·76
Ording	———	54 21	8 36½	9·82	6·83
Pelvorm	Superintend., Capt. Petersen.	54 30	8 42	11·25	5·75
Seesand	———	54 31½	8 21½		
Amrum	———	54 38¼	8 23½	8·58	5·17
Wyck	———	54 42	8 35	7·83	5·08
Sonderhoe	Superintendent Tegner.	55 0	....	4·98	2·69
List	Superintendent, Nissen.	55 1	8 26	6·33	3·32
Blaavands Huk	Superintendent, Tegner.	55 34	8 5	5·63	2·48
Nyeminde Gab	Superintendent, Skibsted.	55 47	8 10¾	2·68	·96
Torskminde	———	56 20½	8 7½		
Agger	———	56 45	8 12	1·49	·50
Hals	Superintendent, Bluhme.	56 59	10 20		
Frederikshaven	Superintendent, Skibsted.	57 25¾	10 32½	1·46	·13
Skagen	———	57 42½	10 35½		
Hirtshals	———	57 35¼	9 58	1·35	·31
Helgoland		....	....	11.37	

TABLE X. (Continued.)

Coast of Norway.

Station.	Superintendent and Observer.	Latitude. N.	Longitude. E.	Greatest vertical rise.	Least vertical rise.
		° ′	h m	English ft. in.	English ft. in.
Tromsöe	Superintendent and Observer, Lieutenant Due.	69 30	1 15	8 8	3 11
Andænes (Lofoden)	Lieutenant Hagerup.	69 30	1 0	7 7	2 7
Væroe	Superintendent and Observer, Lieutenant Rynning.	67 44	47	8 5	3 4
Froyen Ist (Point Fitteren)	Superintendent, Commodore Ferry, Observer, Captain Sheen.	63 40	33	6 8	2 11
Munkholm	Superintendent, Commodore Terry, Observer, Captain Erbe.	63 26	42	8 11	4 2
Christiansund	Superintendent, Shive, Observer, J. H. Bryhn.	63 6½	31	6 8	2 9
Runde Ist (Skotholm)	Superintendent, Shive, Observer, W. Lorange.	62 22	23	6 0	2 2
Kumlesund (Rorsfjord)	Superintendent, Lund, Observer, A. W. Bergh.	60 10½	20	3 9	2 4
Bergen	Superintendent, Lund, Observer, G. A. Dirks.	60 24	21	4 6	1 11
Skudesnæs	Superintendent, Smith, Observer, Pedorsen.	59 8	21	2 1	10
Stavanger	Superintendent, Smith, Observers, Ctausen and Haaland.	58 58⅓	24	3 5	1 7
Tananger	Superintendent, Smith, Observer, G. Mousen.	58 56	22	1 9	5
Lindesnæs	Superintendent, Shive, Observer, Ole Gulliksen.	57 58	23		
Christiansund	Observer and Superintendent, O. W. Erichsen.	58 8	32	1 1	2
Oxsöe	Superintendent, Shive, Observer, C. Bergh.	58 3¾	32	1 1	3
Arendal	Superintendent, Shive, Observer, Astaksen.	58 27	35	1 0	3
Ostre Rusöer	Superintendent, Shive, Observer, Hauge.	58 42½	37	1 4	4
Jomfruhland	Superintendent, S. Lous, Observer, Grung.	58 51	39	1 4	4
Langesund	Superintendent, Shive, Observer, Molbach.	58 59	39	1 2	4
Fræderikswærn	Superintendent and Observer, S. Lous.	58 59	41	1 3	4
Valöerne	Superintendent, Captain S. Lous, Observer, Lieutenant Bull.	59 2	44	1 3	5
Frederikstadt	Superintendent, Shive, Observer, Kock.	59 12	44	2 1	3
Horten	Superintendent and Observer, Winge.	59 24½	42		
Svelvigen	Superintendent, Shive, Observer, Brenmehl.	59 36	42	1 2	4

Table XI.

Semimenstrual and Diurnal Inequality of High Water in June 1835.

Coast of America.

	Eastport.			Mount Desert Island.			Portsmouth.			Boston.		
	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.
1835.	ft. in.	in.	in.	ft. in.	in.	in.	ft. in.	in.	in.	ft. in.	in.	in.
June 9.	3 11	−11	+ 9	2 2	−10	+10	2 5	−11	+ 6	1 9	− 8	+ 5
10.	4 3	−14	+13	2 3	− 8	− 8	2 6	− 8	+12	2 3	− 8	+12
11.	4 4	−11	+ 9	2 3	− 8	+12	2 7	− 9	——	2 4	− 9	——
12.	4 3	——	−11	2 3	——	−10	2 7	+ 7	−11	2 3	+ 7	−12
13.	4 0	+12	− 8	2 2	+12	− 7	2 6	+10	− 7	2 1	+ 9	− 7
14.	3 7	+ 9	− 8	2 0	− 3	− 7	2 4	+ 9	− 7	1 10	+ 9	− 5
15.	2 10	+ 7	− 7	1 9	+ 6	− 7	2 1	+ 6	− 7	1 6	+ 7	− 7
16.	2 2	+ 4	− 2	1 5	+ 4	− 5	1 10	+ 4	− 3	1 3	+ 3	− 4
17.	1 8	+ 6	− 2	1 2	+40	− 2	1 7	+ 4	− 4	1 0	+ 5	− 3
18.	1 3	+ 1	+ 1	0 11	− 1	+ 1	1 5	− 2	− 1	0 9	0	0
19.	1 0	− 2	+ 1	0 8	− 2	0	1 3	− 2	+ 4	0 7	− 1	+ 4
20.	0 10	+ 4	+ 7	0 6	+ 6	+ 6	1 1	+ 3	+ 3	0 6	− 2	+ 2
21.	0 10	− 4	+ 6	0 5	− 5	+ 8	0 11	− 5	+ 4	0 4	− 8	+ 6
22.	0 11	− 9	+ 7	0 4	− 4	− 1	1 0	− 7	+ 7	0 4	− 8	+ 7
23.	1 0	− 5	+ 8	0 4	− 3	+10	1 1	− 6	+ 6	0 4	− 6	+ 6
24.	1 2	− 8	+ 7	0 4	− 5	+10	1 1	− 7	+ 6	0 5	− 7	+ 8
25.	1 3	− 6	+10	0 4	− 3	+ 1	1 2	− 7	+ 9	0 7	− 7	+ 7
26.	1 5	− 7	+ 9	0 5	− 5	——	1 3	——	− 8	0 9	− 8	
27.	1 8	− 9	....	0 7	+11	....	1 4	+ 8	....	1 1	+ 5	
28.												

	Cape Cod.			Province Town.			Newport.			Warren.		
June 9.	1 8	−10	+ 6	2 2	− 9	+ 7	1 2	− 8	+ 6	1 5	− 5	+ 6
10.	2 3	− 9	+13	2 6	−	+16	1 6	− 9	+ 9	1 9	− 7	+ 6
11.	2 4	−11	+ 6	2 8	−10	+ 8	1 8	− 5	+ 5	1 11	− 5	+ 5
12.	2 3	−14	——	2 6	——	−11	1 7	− 6	+ 3	1 10	− 5	+ 3
13.	2 1	+11	− 9	2 4	+13	− 6	1 5	− 5	+ 4	1 8	− 4	+ 5
14.	1 10	+ 9	− 8	2 2	+12	− 6	1 3	− 4	+ 3	1 5	− 2	+ 1
15.	1 8	+ 6	−12	1 11	+ 8	− 8	1 1	0	0	1 2	− 2	——
16.	1 5	+ 6	− 2	1 8	+ 3	− 6	1 0	——	− 2	1 1	+ 2	+ 2
17.	1 1	+ 4	− 6	1 6	+ 6	− 5	0 10	+ 4	+ 1	0 11	+ 7	0
18.	0 8	+ 1	0	1 3	− 1	− 1	0 8	− 3	+ 2	0 10	− 3	+ 2
19.	0 5	− 1	0	1 2	− 2	+ 1	0 7	− 2	+ 5	0 9	− 1	+ 5
20.	0 3	+ 4	+ 5	1 0	+ 2	+ 3	0 6	+ 4	+ 4	0 8	+ 5	− 1
21.	0 3	− 6	+ 6	0 11	+ 4	+ 5	0 5	− 3	+ 4	0 7	− 4	+ 3
22.	0 3	− 7	+10	0 10	− 8	+ 8	0 5	− 4	+ 3	0 8	− 4	+ 3
23.	0 4	− 5	+ 9	0 10	− 6	+ 7	0 7	− 4	+ 5	0 9	− 3	+ 3
24.	0 4	− 7	+ 9	0 10	− 6	+ 8	0 8	− 4	+ 4	0 11	− 4	+ 3
25.	0 6	− 8	+ 8	0 11	− 7	+ 7	0 9	− 4	+ 3	0 11	− 3	+ 1
26.	0 7	− 7	....	1 1	− 6	....	0 10	− 3	+ 1	0 11	− 2	+ 2
27.	0 10	+ 6	....	1 3	+ 9	....	0 11	0	....	0 10	− 1	
28.												

Table XI. (Continued.)

	Gardiner's Bay.			New York.			Sandy Hook.			Delaware.		
	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.
1835.	ft. in.	in.	in.	ft. in.	in.	in.	ft. in.	in.	in.	ft. in.	in.	in.
June 9.	1 2	− 4	+ 5	1 5	− 5	+ 7	1 5	− 3	+ 7	1 4	− 7	+ 9
10.	1 8	− 9	+ 6	1 9	−10	+12	1 9	− 9	+ 7	1 6	−13	+ 7
11.	1 10	− 5	+ 3	2 0	− 4	+ 7	2 0	− 3	+ 9	1 7	+ 4	+12
12.	1 10	− 8	+ 2	1 11	− 7	+ 7	1 11	− 5	+ 6	1 7	− 9	+ 9
13.	1 9	− 6	+ 4	1 9	− 7	+ 5	1 8	− 8	+ 6	1 8	−13	+ 5
14.	1 8	——	− 4	1 8	− 7	——	1 5	− 5	+ 3	1 8	−12	+ 4
15.	1 7	+ 2	− 2	1 7	+ 3	− 2	1 4	− 2	——	1 7	− 4	+ 5
16.	1 6	+ 1	− 1	1 6	+ 4	− 2	1 3	+ 4	− 3	1 5	——	− 6
17.	1 5	+ 2	0	1 4	+ 3	− 3	1 1	+ 5	− 2	1 2	+ 4	− 5
18.	1 4	− 2	+ 3	1 3	− 3	+ 3	0 11	− 1	+ 1	1 1	+ 1	− 1
19.	1 3	− 2	+ 7	1 1	− 2	+ 8	0 9	− 1	+ 5	1 0	0	+ 3
20.	1 2	+ 2	+ 4	0 11	−10	+ 5	0 7	− 4	+ 3	0 11	− 4	+ 3
21.	1 2	− 3	+ 5	0 10	− 3	+ 4	0 6	− 4	+ 5	0 10	− 5	+ 6
22.	1 1	− 3	+ 4	0 9	− 5	+ 7	0 5	− 5	+ 6	0 9	− 7	+ 6
23.	1 2	− 4	+ 6	0 9	− 6	+10	0 8	− 8	+ 8	0 10	−10	+ 6
24.	1 3	− 4	+ 5	0 11	−14	+ 9	0 11	− 5	+ 6	0 11	− 6	+ 9
25.	1 3	− 4	+ 3	1 2	− 4	+ 8	1 1	− 7	+ 6	1 1	− 9	+ 8
26.	1 4	− 3	+ 5	1 4	− 6	+ 4	1 1	− 5	+ 5	1 2	− 9	+10
27.	1 5	− 2	....	1 6	− 5	....	1 2	− 4	....	1 2	− 7	
28.												

	Old Point Comfort.			Gosport.			Cape Hatteras.			Cape Fear River. (Fort Johnston.)		
June 9.	0 5	+ 3	+ 7	0 7	+ 6	+ 8	2 4	+21	+14	1 9	− 5	+ 9
10.	0 6	− 5	+ 4	0 9	− 4	+ 3	2 3	− 5	+ 6	1 8	− 9	+11
11.	0 8	− 7	+ 6	0 11	− 6	+ 5	2 2	−20	+10	1 5	−17	+ 8
12.	0 9	− 3	+ 4	1 0	− 3	+ 3	2 1	−10	+16	1 2	− 9	+ 9
13.	0 9	− 9	+ 3	1 0	− 6	0	1 11	−15	+ 9	0 11	− 9	+ 7
14.	0 9	− 7	+ 7	1 0	——	− 7	1 9	−15	+20	0 7	− 8	+ 4
15.	0 11	——	− 1	1 1	− 2	+ 1	1 6	− 4	+ 4	0 5	− 6	+12
16.	0 11	+ 4	− 1	1 3	+ 2	− 2	1 3	− 5	+ 3	0 5	+ 3	——
17.	0 11	− 3	− 2	1 4	+ 3	− 4	1 0	− 4	+ 7	0 4	+ 9	0
18.	0 9	0	0	1 3	+ 3	− 7	0 10	− 2	——	0 3	+ 7	+ 3
19.	0 7	0	0	1 0	− 7	+ 8	0 9	− 9	+ 1	0 2	+ 3	+ 7
20.	0 5	+ 3	+ 3	0 9	− 9	+ 2	0 8	+10	+ 3	0 1	− 5	+ 2
21.	0 2	− 2	+ 3	0 7	− 2	+ 2	0 7	+ 5	+ 5	0 2	− 3	+ 9
22.	0 1	− 3	+ 4	0 5	− 4	+ 4	0 7	− 1	+ 6	0 3	− 6	+ 8
23.	0 1	− 3	+ 4	0 4	− 3	+ 4	0 8	− 8	+ 6	0 4	− 7	+ 8
24.	0 2	− 4	+ 6	0 5	− 4	+ 5	0 8	− 9	+ 6	0 5	− 6	+ 6
25.	0 3	− 3	+ 3	0 6	− 2	+ 4	0 9	− 7	+ 6	0 6	− 6	+ 6
26.	0 4	− 4	+ 2	0 7	− 5	+ 1	0 10	−10	+ 9	0 7	− 8	+ 7
27.	0 6	− 1	....	0 9	− 1	....	0 10	− 4	....	0 7	− 7	
28.												

TABLE XI. (Continued.)

	Charleston. (Fort Moultrie.)			Savannah.			St. Augustine. (Fort Marian.)					
	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.
1835.	ft. in.	in.	in.	ft. in.	in.	in.	ft. in.	in.	in.	ft. in.	in.	in.
June 9.	1 10	−13	+11	2 7	− 7	+ 7	1 6	− 8	+ 9			
10.	2 0	−11	+11	2 6	− 7	+ 9	1 7	− 7	+ 9			
11.	1 11	−11	+10	2 5	− 7	+ 8	1 5	− 6	+ 8			
12.	1 6	−11	+ 9	2 3	− 9	+ 9	1 4	−12	+ 8			
13.	1 2	− 9	+10	2 0	− 9	+ 7	1 2	−11	+ 6			
14.	0 10	− 7	+ 4	1 7	− 7	+ 4	0 11	− 6	+ 3			
15.	0 9	− 7	——	1 5	− 6	——	0 9	− 7	——			
16.	0 9	+ 9	+ 5	1 3	+ 5	+ 2	0 8	+ 3	− 3			
17.	0 8	+ 9	+ 1	1 1	+ 7	+ 2	0 7	+ 5	0			
18.	0 7	+16	+ 4	0 11	+ 2	+ 6	0 6	+ 1	+ 1			
19.	0 7	+ 2	+ 3	0 9	+ 3	+ 7	0 5	+ 1	+ 1			
20.	0 6	− 8	+ 2	0 8	− 9	+ 4	0 6	− 2	− 2			
21.	0 7	− 3	+ 9	0 11	− 8	+ 6	0 8	+ 1	+ 7			
22.	0 8	− 2	+10	1 4	− 5	+ 7	0 9	+ 1	+ 6			
23.	0 10	− 5	+ 7	1 5	− 8	+ 6	0 10	− 5	+ 6			
24.	0 10	− 5	+ 9	1 4	− 6	+ 8	0 11	− 7	+ 6			
25.	0 11	− 8	+ 6	1 3	− 8	+ 6	0 10	− 5	+ 2			
26.	1 0	− 9	+ 7	1 2	− 8	+ 9	0 8	− 7	+ 7			
27.	1 0	−11	....	0 10	− 7	....	0 5	− 2				
28.												

Portugal.

	Peniche.			Vianna.			Cascaes.			Sines.		
June 9.	3 2	+ 2	+ 1	3 2	+ 1	0	3 3	——	+ 1	3 2	+ 1	0
10.	3 5	− 2	+ 3	3 5	0	+ 1	3 9	− 1	+ 1	3 4	− 1	+ 1
11.	3 6	− 2	+ 3	3 6	− 1	+ 1	4 1	− 4	+ 9	3 7	0	0
12.	3 6	− 1	− 1	3 6	− 1	+ 2	4 0	− 6	+ 3	3 7	+ 1	+ 3
13.	3 3	− 4	+ 5	3 4	− 5	+ 4	3 8	− 5	+ 5	3 3	− 5	+ 6
14.	2 11	− 5	+ 6	3 1	− 7	+ 5	3 2	− 5	+ 7	2 9	− 4	+ 7
15.	2 5	− 6	− 6	2 6	− 3	+ 4	2 8	− 6	+ 6	2 3	− 7	+ 6
16.	1 10	− 6	+ 7	1 10	− 9	+ 5	2 2	− 5	+ 5	1 7	− 8	+ 7
17.	1 3	− 4	− 4	1 4	− 8	+ 6	1 10	− 6	+ 5	1 2	− 5	+ 5
18.	1 0	− 4	+ 5	1 0	− 5	+ 4	1 6	− 5	+ 3	0 11	− 4	+ 4
19.	0 10	− 3	+ 5	0 10	− 5	+ 3	1 5	− 3	+ 2	0 10	− 4	+ 2
20.	0 10	− 1	− 9	0 10	− 2	+ 2	1 6	− 2	+ 1	0 11	− 2	+ 1
21.	1 0	+ 2	− 1	0 11	− 2	0	1 7	− 1	0	1 1	− 1	+ 1
22.	1 4	+ 3	——	1 2	+ 2	——	1 9	0	− 1	1 4	+ 1	——
23.	1 6	− 1	0	1 5	− 1	0	1 11	——	+ 2	1 6	− 1	+ 1
24.	1 9	+ 2	+ 2	1 6	− 5	+ 4	2 2	3	+ 3	1 8	− 3	+ 2
25.	1 11	− 3	+ 2	1 8	− 4	+ 3	2 5	− 2	+ 2	1 11	− 3	+ 3
26.	2 1	+ 3	+ 6	1 7	− 3	+ 3	2 9	+ 2	+ 3	2 0	− 3	+ 5
27.	2 0	+ 6	....	1 4	− 1	....	2 7	− 2	....	2 0	− 3	

Table XI. (Continued.)

	Pera.			Bay of Lagos.								
	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.
1835.	ft. in.	in.	in.	ft. in.	in.	in.	ft. in.	in.	in.	ft. in.	in.	in.
June 9.	3 0	——	0	2 6	− 5	− 1						
10.	3 4	0	− 1	2 10	0	0						
11.	3 6	− 4	0	3 0	+ 3	+ 7						
12.	3 5	+ 3	+ 5	3 0	+11	+ 9						
13.	3 1	− 5	+ 5	2 11	− 6	+ 4						
14.	2 6	−10	+ 8	2 8	− 4	+ 7						
15.	2 0	−10	+ 8	2 2	− 6	+ 3						
16.	1 6	− 6	+ 6	1 8	− 6	+ 4						
17.	1 1	− 5	+ 5	1 3	− 4	+ 8						
18.	0 10	− 3	+ 4	0 11	− 5	+ 1						
19.	0 9	− 3	0	0 9	1	0						
20.	1 0	− 1	0	0 9	+ 1	0						
21.	1 3	− 2	+11	1 0	− 1	0						
22.	1 7	− 4	+ 7	1 5	+ 3	0						
23.	1 11	+ 5	− 3	1 9	0	——						
24.	2 2	——	+ 1	2 0	0	0						
25.	2 5	− 1	− 1	2 2	− 1	+ 1						
26.	2 6	+ 3	0	2 4	0	0						
27.	2 7	− 3	....	2 6	+ 3							

Spain.

	Santander.			Ferrol.			Camarinas.			Cadiz.		
June 9.	4 0	——	+ 2	4 0	− 3	− 1	3 4	0	+ 1	1 11	0	− 8
10.	4 2	− 1	+ 2	4 5	+ 3	0	3 9	− 3	+ 2	3 2	+ 4	− 2
11.	4 7	− 2	+ 2	4 6	− 3	+ 2	3 11	− 3	+ 2	3 4	+ 3	− 5
12.	4 5	− 3	+ 3	4 5	− 5	+ 4	3 10	− 4	+ 6	3 2	+ 3	− 6
13.	4 3	− 5	+ 3	4 1	− 5	+ 6	3 6	− 5	+ 7	2 11	+ 2	+ 5
14.	3 9	− 2	+ 7	3 5	− 6	+ 7	3 2	− 6	+ 9	2 6	− 4	+ 7
15.	3 1	− 7	+ 4	2 10	− 7	+ 8	2 7	− 7	+ 8	2 0	− 6	+ 6
16.	2 4	− 4	+ 7	2 2	− 7	+ 9	2 0	−10	+ 8	1 3	− 6	+ 3
17.	1 5	− 8	+ 6	1 6	− 7	+ 8	1 5	− 8	+ 7	0 9	− 5	+ 5
18.	1 2	− 6	+ 4	1 1	− 6	+ 6	1 0	− 7	+ 6	0 5	− 4	+ 4
19.	0 10	− 5	+ 7	0 11	− 4	+ 4	0 8	− 5	+ 4	0 3	− 3	+ 3
20.	0 9	+ 1	+ 1	1 1	− 3	+ 4	0 6	− 2	+ 5	0 5	− 3	+ 2
21.	1 1	0	——	1 2	0	——	0 7	0	——	0 8	0	+ 1
22.	1 6	0	+ 1	1 4	− 1	+ 1	0 10	0	+ 1	0 10	+ 1	− 1
23.	1 10	0	− 1	1 9	− 1	+ 2	1 3	− 1	+ 2	1 1	——	− 1
24.	2 4	− 1	+ 2	2 3	− 5	+ 2	1 7	− 2	+ 2	1 5	+ 3	− 1
25.	2 10	0	+ 1	2 6	0	+ 2	1 10	− 3	+ 3	1 8	− 2	+ 2
26.	2 11	− 1	+ 1	2 5	− 6	+ 5	1 10	− 6	+ 3	1 11	− 2	+ 3
27.	2 9	− 2	+ 3	2 2	− 4	....	1 9	− 3	....	1 10	− 5	

Table XI. (Continued.)

France.

	Lambrielle, Ile de Sein.			Barfleur.			Cherbourg.			Granville.		
	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.
1835.	ft. in.	in.	in.	ft. in.	in.	in.	ft. in.	in.	in.	ft. in.	in.	in.
June 9.	4 7	− 1	+ 1	4 0	+ 1	+ 1	4 3	− 1	+ 1	8 1	− 1	+ 1
10.	5 2	− 2	+ 1	4 4	− 2	+ 2	4 10	− 1	+ 2	9 7	− 2	+ 3
11.	5 8	+ 3	0	4 7	− 2	+ 2	5 2	− 1	+ 2	10 6	− 1	+ 5
12.	5 9	0	+ 1	4 6	− 3	+ 2	5 1	− 4	+ 8	10 6	− 4	+ 3
13.	5 2	− 5	+ 4	4 4	− 2	+ 3	4 10	− 3	+ 3	9 10	− 4	+ 3
14.	4 4	− 7	+ 3	3 9	− 4	+ 2	4 3	− 5	+ 4	8 9	− 7	+ 3
15.	3 3	− 6	+ 5	3 3	——	− 3	3 5	− 5	+ 3	7 0	− 4	+ 3
16.	2 5	− 4	+ 9	2 7	+ 2	− 6	2 6	− 6	——	4 9	− 4	+ 6
17.	1 7	− 6	− 5	1 10	+ 2	− 1	1 11	+ 5	− 3	2 9	− 4	+ 5
18.	0 11	− 4	− 3	1 1	+ 2	− 2	1 4	+ 4	− 3	1 3	− 4	——
19.	0 8	− 3	+ 2	0 9	+ 1	+ 2	0 10	+ 1	0	0 6	+ 4	− 4
20.	0 10	− 1	+ 1	0 8	− 2	+ 1	0 8	− 1	0	0 5	0	− 3
21.	1 1	——	− 1	0 11	0	+ 1	1 0	0	0	0 11	+ 3	− 2
22.	1 6	+ 1	0	1 4	− 1	+ 2	1 6	0	+ 2	2 0	0	− 1
23.	2 2	0	0	2 1	0	+ 1	2 4	− 1	0	3 4	+ 1	+ 2
24.	2 11	0	+ 2	2 8	− 2	+ 2	3 0	− 2	+ 4	4 7	− 2	+ 6
25.	3 2	− 3	− 3	3 0	− 3	+ 1	3 4	− 2	+ 2	5 9	− 3	+ 6
26.	3 3	− 12	+ 6	3 2	− 3	+ 9	3 6	− 4	+ 13	6 6	− 9	+ 12
27.	3 3	0	....	3 2	− 3	....	3 6	− 1	....	6 10	− 5	....
28.												

	Chausey.			St. Servan.			Brehat.			Abervrack.		
June 9.	8 2	− 1	+ 2	10 4	− 1	+ 3	7 4	− 2	+ 2	4 8	− 1	+ 1
10.	10 1	− 1	+ 4	10 6	+ 1	− 1	8 3	− 1	+ 4	5 4	− 2	+ 2
11.	10 9	− 5	+ 4	10 7	− 1	− 1	8 11	− 2	+ 3	5 9	− 2	+ 3
12.	10 11	− 4	+ 3	10 5	+ 1	+ 7	8 11	− 3	+ 3	5 8	− 3	+ 4
13.	10 4	− 4	+ 3	9 10	0	− 3	8 5	− 3	+ 9	5 3	− 3	+ 5
14.	9 3	− 5	+ 3	8 9	− 3	+ 6	7 6	− 5	+ 5	4 6	− 4	+ 5
15.	7 7	− 6	+ 5	7 2	− 6	+ 3	6 0	− 5	+ 5	3 9	− 7	+ 4
16.	5 6	− 6	+ 5	5 0	− 5	+ 8	4 6	− 9	+ 5	2 7	− 14	——
17.	3 8	− 5	+ 5	2 6	− 7	+ 2	2 11	− 6	+ 4	1 7	− 7	+ 6
18.	2 3	− 6	——	1 7	− 1	——	1 7	− 5	+ 2	0 10	− 4	+ 3
19.	1 7	+ 2	− 3	1 1	+ 2	− 3	1 1	——	− 2	0 4	− 4	+ 1
20.	1 4	+ 1	− 3	1 0	− 11	− 2	1 0	+ 1	− 1	0 5	− 2	+ 1
21.	1 9	+ 3	− 1	1 4	+ 3	− 1	1 5	+ 3	− 1	0 9	0	0
22.	2 10	0	0	2 6	0	+ 7	2 3	0	+ 1	1 6	0	0
23.	4 2	0	0	4 0	− 2	+ 1	3 5	+ 1	+ 1	2 5	0	− 1
24.	5 5	− 3	+ 5	5 2	− 2	+ 9	4 5	− 2	+ 4	3 0	+ 5	− 5
25.	6 4	− 4	+ 3	6 0	− 3	+ 1	5 1	− 2	+ 1	3 3	+ 3	− 7
26.	7 0	− 6	+ 11	6 9	0	+ 6	5 6	− 7	+ 11	3 5	+ 7	− 2
27.	7 6	− 5	....	7 1	− 5	...	5 10	+ 8	....	3 5	+ 2	
28.												

TABLE XI. (Continued.)

Netherlands.

	Sluischegat of Zwin.			Ulissengen.			Westkappel.			Goede Reede.		
	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.
1835.	ft. in.	in.	in.	ft. in.	in.	in.	ft. in.	in.	in.	ft. in.	in.	in.
June 9.	1 0	0	0	1 6	+ 1	——	1 2	0	0	——	——	− 1
10.	1 1	0	+ 1	1 8	− 2	+ 1	1 4	——	+ 1	1 7	− 3	+ 1
11.	1 2	+ 1	0	1 9	+ 1	0	1 5	0	0	1 5	0	0
12.	1 2	+ 1	0	1 10	+ 1	0	1 5	+ 1	− 1	1 6	+ 1	− 1
13.	1 3	+ 1	− 1	1 10	+ 1	− 1	1 5	+ 1	− 2	1 7	+ 2	− 2
14.	1 2	+ 2	− 2	1 9	0	− 2	1 4	+ 3	− 3	1 7	+ 2	− 1
15.	1 0	+ 1	− 2	1 7	+ 1	− 1	1 2	+ 1	− 2	1 6	+ 1	− 2
16.	0 8	+ 1	− 2	1 4	+ 1	− 4	0 11	+ 1	− 3	1 4	+ 2	− 3
17.	0 6	+ 2	0	1 1	+ 2	0	0 8	+ 3	0	1 3	+ 3	− 1
18.	0 3	0	− 4	0 11	− 0	− 3	0 6	0	− 4	1 2	+ 2	− 4
19.	0 1	+ 6	− 3	0 10	+ 6	− 2	0 3	+ 7	− 2	1 2	+ 8	− 2
20.	0 0	− 2	− 1	0 9	− 2	+ 1	0 2	− 2	− 1	1 1	0	− 1
21.	0 0	+ 2	− 2	0 10	+ 1	− 3	0 4	0	− 1	1 2	0	− 3
22.	0 3	+ 3	− 2	1 1	+ 4	− 2	0 8	+ 3	+ 3	1 4	+ 4	− 1
23.	0 5	+ 5	+ 1	1 4	+ 3	− 1	0 10	+ 3	0	1 5	——	+ 3
24.	0 7	——	− 3	1 5	——	− 3	0 11	− 2	——	1 7	− 2	− 4
25.	0 10	0	+ 1	1 8	0	0	1 2	− 1	0	1 10	− 2	+ 1
26.	1 2	+12	− 3	2 0	+14	− 2	1 6	+ 9	− 4	2 1	+12	− 3
27.	1 5	− 1	....	2 3	− 4	....	1 10	− 4	....	2 5	− 5	
28.												

	Hellevoetsluis.			Brielle.			Delflandsche Hoofden.			Katwijkaan Zee.		
June 9.	1 3	− 3	+ 1	1 5	− 4	− 1	1 4	− 2	0	1 5	0	0
10.	1 3	− 1	0	1 4	+ 4	− 1	1 4	− 1	0	1 4	− 1	+ 1
11.	1 3	0	0	1 3	− 1	− 1	1 4	− 1	+ 1	1 4	− 1	+ 1
12.	1 3	+ 1	− 1	1 3	+ 1	+ 1	1 5	+ 1	0	1 5	+ 1	+ 2
13.	1 4	+ 2	− 1	1 3	+ 2	− 1	1 7	+ 3	+ 1	1 5	+ 2	+ 2
14.	1 4	+ 4	− 3	1 4	+ 2	− 3	1 7	0	0	1 5	+ 4	− 2
15.	1 3	+ 2	− 2	1 2	+ 3	− 2	1 7	− 1	− 1	1 4	+ 3	− 3
16.	1 2	+ 2	− 2	1 2	+ 2	− 2	1 7	+ 5	− 3	1 3	+ 4	− 3
17.	1 2	+ 3	0	1 2	+ 3	+ 7	1 6	+ 3	− 2	1 3	+ 4	− 2
18.	1 1	0	− 3	1 2	− 0	− 4	1 6	− 1	− 1	1 3	+ 2	− 3
19.	1 1	+ 7	− 4	1 1	+ 7	− 2	1 5	+ 8	− 2	1 3	+ 5	− 3
20.	1 0	+ 1	+ 1	1 1	− 1	0	1 3	+ 1	− 3	1 2	0	0
21.	1 0	——	+ 2	1 1	+ 1	——	1 2	+ 2	+ 2	1 3	+ 1	− 2
22.	1 2	− 4	+ 5	1 2	− 3	+ 4	1 3	+ 4	+ 3	1 5	——	+ 4
23.	1 3	0	+ 4	1 4	+ 2	+ 4	1 4	+ 2	+ 5	1 7	− 1	+ 2
24.	1 6	− 4	− 7	1 6	− 2	− 5	1 6	+ 2	− 2	1 9	− 2	− 4
25.	1 11	− 3	− 2	1 10	− 1	+ 2	1 9	+ 2	+ 2	1 10	+ 1	+ 1
26.	2 7	+ 8	− 1	2 2	+12	− 6	2 2	+12	− 7	2 2	+11	− 8
27.	3 3	− 9	....	2 8	−10	....	2 6	− 8	....	2 6	−10	
28.												

Table XI. (Continued.)

	Hondsbosschen.											
	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.
1835.	ft. in.	in.	in.	ft. in.	in.	in.	ft. in.	in.	in.	ft. in.	in.	in.
June 9.	1 6	− 3	0									
10.	1 6	0	0									
11.	1 6	− 1	0									
12.	1 6	0	0									
13.	1 5	+ 2	0									
14.	1 5	+ 4	− 2									
15.	1 4	+ 2	− 2									
16.	1 4	+ 3	− 3									
17.	1 4	+ 5	− 1									
18.	1 4	+ 2	− 3									
19.	1 4	+ 9	+ 2									
20.	1 5	− 1	− 1									
21.	1 6	0	——									
22.	1 7	− 2	+ 1									
23.	1 8	+ 3	+ 3									
24.	1 10	− 2	− 3									
25.	2 0	+ 1	0									
26.	2 3	+ 8	− 6									
27.	2 7	−11										
28.												

Danish Coast.

	Altona.			Pin Aue.			Gluckstadt.			Meldorf.		
	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.
June 9.	·7	+ ·1	− ·1	1·0	+ ·2	− ·2	1·0	+ ·2	− ·2	1·0	·0	+ ·1
10.	·9	+ ·2	− ·1	1·2	+ ·2	− ·1	1·2	+ ·2	− ·1	1·3	+ ·2	·0
11.	1·0	+ ·3	− ·2	1·3	·0	− ·2	1·3	·0	− ·2	1·4	·0	− ·1
12.	1·3	·0	·0	1·4	·0	·0	1·4	·0	·0	1·6	·0	·0
13.	1·4	− ·2	·0	1·5	− ·2	+ ·2	1·4	− ·2	+ ·2	1·7	− ·2	+ ·1
14.	1·6	− ·4	+ ·6	1·6	− ·3	+ ·6	1·6	− ·3	+ ·3	1·9	− ·3	+ ·3
15.	1·7	− ·3	+ ·5	1·8	− ·3	+ ·7	1·7	− ·2	+ ·6	2·0	− ·4	+ ·3
16.	1·6	+ ·4	+ ·3	1·7	− ·7	+ ·3	1·6	− ·7	+ ·3	1·9	−1·0	+ ·4
17.	1·3	− ·8	+ ·5	1·5	− 10	+ ·9	1·3	− ·8	+1·0	1·7	− ·9	+ ·8
18.	1·2	−1·1	− ·2	1·4	− 11	+ ·0	1·1	−1·0	+ ·2	1·5	−1·4	+ ·1
19.	1·2	+ ·7	+1·9	1·3	+1·1	+1·11	1·1	+ ·9	+2·1	1·4	− ·6	+2·0
20.	1·1	....	− ·3	1·2	− ·6	+ ·5	1·1	− ·3	+ ·8	1·3	− ·4	+1·0
21.	1·1	+ ·2	− ·2	1·2	− ·1	+ ·3	1·1	·0	+ ·5	1·3	+ ·6	+ ·7
22.	1·1	·0	+ ·3	1·2	——	+ ·5	1·1	+ ·6	——	1·4	+ ·6	+ ·7
23.	1·0	+ ·4	+ ·1	1·2	+ ·6	+ ·2	1·1	+ ·6	+ ·2	1·4	+ ·2	+ ·4
24.	·9	+ ·4	+ ·4	1·1	+ ·6	+ ·2	1·2	+ ·5	+ ·3	1·5	+ ·0	+ ·2
25.	·8	− ·8	+ ·2	1·2	− ·8	+ ·1	1·2	− ·8	+ ·3	1·7	− ·0	+ ·4
26.	1·2	− ·3	+1·2	1·6	− ·4	+ ·8	1·6	− ·4	+ ·9	2·2	− ·6	+ ·3
27.	1·8	−1·0	—	2·7	− ·4	....	2·3	− ·4	....	2·6	+ ·1	
28.												
29.												

Table XI. (Continued.)

	Stein Schleuse.			Vollerwick.			Ording.			Pelworm.		
	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.
1835.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.
June 9.	·9	+ ·2	− ·1	1·2	+ ·3	+ ·2	1·1	·0	+ ·2	·5	·0	+ ·1
10.	1·1	+ ·1	·0	1·5	+ ·0	− ·1	1·3	·0	·0	·8	·0	+ ·1
11.	1·2	·0	− ·1	1·6	+ ·1	− ·1	1·4	·0	− ·1	1·0	− ·1	·0
12.	1·4	·0	+ ·1	1·7	·0	·0	1·5	·0	·0	1·1	·0	− ·3
13.	1·4	− ·4	+ ·2	1·7	− ·3	+ ·1	1·5	− ·2	+ ·1	1·1	− ·3	− ·9
14.	1·5	− ·2	− ·2	1·7	− ·2	+ ·4	1·6	− ·3	+ ·3	1·1	....	+ ·2
15.	1·5	− ·2	+ ·5	1·7	− ·4	+ ·3	1·6	− ·3	+ ·4	1·2	− ·3	+ ·3
16.	1·5	− ·7	+ ·3	1·7	+ ·1	+ ·4	1·5	− ·8	+ ·5	1·1	− ·7	+ ·4
17.	1·5	− ·7	+ ·8	1·6	− ·9	+ ·9	1·2	− ·6	+ 1·0	1·0	− ·6	+ ·9
18.	1·5	− 1·2	− ·2	1·5	− 1·3	− ·1	1·0	− ·9	+ ·2	·8	− ·9	+ ·3
19.	1·5	+ ·2	+ 1·5	1·5	·0	+ 1·5	1·0	+ ·5	——	·7	+ ·6	− ·4
20.	1·6	− ·5	+ ·6	1·5	− ·9	+ ·6	·9	——	+ ·6	·7	− ·4	+ ·8
21.	1·6	....	− ·3	1·4	− ·5	+ ·5	1·0	− ·1	+ ·7	·7	− ·1	+ 1·0
22.	1·6	+ ·6	+ 1·0	1·4	+ ·6	+ ·8	1·1	+ ·8	+ ·9	·7	+ ·9	+ 1·1
23.	1·7	+ 1·0	+ ·4	1·4	+ ·2	+ ·4	1·2	+ ·2	+ ·4	·9	+ ·5	+ ·4
24.	1·8	+ ·2	·0	1·4	+ ·2	——	1·4	+ ·2	− ·5	1·1	+ ·3	− ·6
25.	2·0	− 1·2	+ ·6	1·6	− 1·8	+ ·6	1·6	....	+ ·4	1·4	+ ·6	− ·1
26.	2·2	− ·2	+ ·2	2·0	− ·2	+ ·6	2·0	− ·3	+ ·4	1·7	+ ·6	− ·1
27.	2·6	·0	....	2·4	·0	....	2·5	....	....	2·6	− 1·1	
28.												
29.												

	Amrum.		
June 9.	·8	− ·1	+ ·1
10.	·9	− ·1	·0
11.	1·1	− ·1	− ·1
12.	1·1	+ ·1	+ ·1
13.	1·1	− ·4	+ ·2
14.	1·1	− ·2	+ ·3
15.	1·0	− ·2	+ ·4
16.	1·0	− ·5	+ ·6
17.	1·1	− ·5	+ ·8
18.	1·1	− ·3	+ ·1
19.	1·1	·0	+ ·9
20.	1·1	− ·8	+ ·3
21.	1·2	− ·5	+ ·3
22.	1·3	+ ·3	+ ·7
23.	1·3	+ ·3	+ ·1
24.	1·4	——	·0
25.	1·5	− ·5	+ ·4
26.	1·7	·0	+ ·7
27.	2·1	− ·5	
28.			

Table XI. (Continued.)

East Coast of Scotland.

	Port Logan.			Lerwick.			Scrabsters.			Buckie.		
	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.
1835.	ft. in.	in.	in.	ft. in.	in.	in.	in.	in.	in.	ft. in.	in.	in.
June 9.	3 2	+ 3	− 2	2 5	− 2	− 1	3 2	− 1	0	3 1	− 1	+ 1
10.	3 6	+ 2	− 1	2 8	0	+ 1	3 10	0	0	3 5	0	0
11.	3 8	0	− 1	2 8	0	− 2	4 1	− 1	+ 1	3 6	0	0
12.	3 7	− 3	+ 2	2 9	− 2	——	4 1	− 3	+ 5	3 5	+ 1	− 1
13.	3 5	− 4	+ 5	2 9	0	+ 4	3 8	− 2	+ 1	3 3	0	——
14.	3 3	− 3	+ 3	2 9	+ 1	− 4	3 2	− 5	+ 9	2 11	+ 1	− 5
15.	2 10	− 6	+ 6	2 7	+ 2	− 2	2 10	− 7	——	2 7	+ 3	− 5
16.	2 6	....	....	2 6	+ 3	− 3	2 6	+11	− 4	2 2	+ 12	− 3
17.	2 2	....	....	2 3	+ 5	− 4	2 3	+ 8	− 15	1 9	+ 12	− 8
18.	1 7	....	....	2 1	+ 6	− 4	2 1	+ 8	− 5	1 4	+ 4	+ 1
19.	1 2	....	....	1 11	− 2	− 2	1 11	+ 3	− 6	1 1	+ 1	− 7
20.	1 3	....	....	1 11	+ 2	− 6	1 11	+ 6	− 7	1 2	+ 4	−11
21.	1 7	....	....	2 2	0	0	2 2	+ 1	+ 5	1 8	− 2	+ 3
22.	2 3	....	....	2 9	0	+ 3	2 9	+ 2	+ 1	2 5	− 1	+ 2
23.	2 7	....	....	3 0	− 2	+ 2	3 0	+ 1	− 2	2 7	− 3	− 4
24.	2 7	....	....	3 0	0	0	3 0	+ 0	+ 4	2 7	− 2	− 2
25.	2 7	....	....	3 0	− 4	+ 2	3 0	− 5	+ 1	2 8	− 2	+ 4
26.	2 5	....	....	2 9	− 1	+ 1	2 9	− 3	+ 8	2 7	+ 1	− 1
27.	2 2	....	....	2 5	− 1	....	2 6	− 1	....	2 5	+ 3	

	Cullen.			Fraserburg.			Banff.		
June 9.	3 1	− 1	+ 1	3 0	0	0	2 9	− 1	− 1
10.	3 3	+ 1	+ 1	3 2	0	0	3 0	+ 1	− 1
11.	3 3	− 1	0	3 2	0	+ 1	3 1	0	——
12.	3 3	——	0	3 1	− 1	0	3 1	− 2	0
13.	3 2	0	+ 1	3 1	0	+ 1	2 11	+ 1	+ 2
14.	3 0	0	− 3	2 11	+ 1	− 4	2 9	0	− 3
15.	2 9	+ 2	− 5	2 8	+ 2	− 5	2 4	+ 2	− 4
16.	2 5	+ 5	− 7	2 4	+ 5	− 4	2 1	+ 8	− 4
17.	1 11	+11	−10	1 10	+ 9	− 8	1 9	+11	−10
18.	1 6	+ 2	− 1	1 6	+ 2	− 1	1 5	+ 2	− 2
19.	1 3	+ 1	− 3	1 4	+ 2	− 4	1 2	+ 2	− 6
20.	1 1	− 3	− 3	1 4	+ 4	− 6	1 1	+ 4	− 8
21.	1 7	0	+ 4	1 8	0	+ 7	1 6	+ 2	+ 4
22.	2 3	− 2	+ 3	2 4	− 1	+ 3	2 2	− 2	+ 3
23.	2 6	− 3	− 1	2 7	− 2	+ 1	2 3	− 2	0
24.	2 6	− 2	+ 3	2 7	− 2	+ 3	2 3	− 1	+ 3
25	2 6	− 3	− 2	2 6	− 3	+ 1	2 2	− 2	0
26.	2 5	——	+ 4	2 5	——	− 2	2 1	——	− 1
27.	2 4	+ 4	....	2 2	+ 6	....	1 11	+ 8	

Table XI. (Continued.)

South Coast of Cornwall.

	St. Agnes, Scilly.			Mousehole.			Mullion.		
	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.
1835.	ft. in.	in.	in.	ft. in.	in.	in.	ft. in.	in.	in.
June 9.	4 1	− 1	0	3 7	− 1	0	3 5	+ 1	0
10.	4 7	− 1	+ 3	4 1	− 1	+ 4	3 7	0	− 1
11.	4 11	0	+ 1	4 6	− 1	+ 1	3 9	0	− 3
12.	4 11	− 2	+ 2	4 5	− 3	+ 2	3 11	+ 1	+ 6
13.	4 7	− 2	+ 3	4 2	− 3	+ 4	3 10	− 2	+ 4
14.	4 1	− 5	+ 3	3 9	− 5	+ 5	3 6	− 4	+ 5
15.	3 4	− 5	+ 4	2 11	− 5	+ 4	3 0	− 3	+ 5
16.	2 5	− 7	+ 5	2 2	− 6	+ 8	2 4	− 8	+ 6
17.	1 8	− 4	+ 9	1 7	− 5	+ 8	1 7	− 7	+ 8
18.	1 0	− 2	− 5	1 1	− 6	+ 3	1 1	− 9	+ 5
19.	0 6	+ 1	− 3	0 11	− 4	+ 4	0 9	− 5	+ 4
20.	0 8	——	+ 5	0 10	——	− 3	0 8	——	− 5
21.	1 1	+ 2	− 1	1 0	+ 2	− 3	0 10	+ 4	0
22.	1 6	0	0	1 5	0	0	1 2	− 4	0
23.	2 2	0	+ 2	2 0	0	+ 1	1 10	− 2	− 5
24.	2 8	+ 2	+ 1	2 6	0	0	2 7	− 1	+ 4
25.	3 0	− 4	− 4	2 9	− 6	− 2	3 0	− 3	0
26.	3 3	− 5	+ 9	3 0	− 1	+ 7	3 3	− 1	+ 2
27.	3 4	− 4	....	3 0	− 1		3 3	− 1	
28.									

	Gerran's Bay.			Mevagissy.			Polperra.		
June 9.	3 4	− 1	+ 1	3 5	0	+ 1	4 2	0	+ 1
10.	3 10	− 1	+ 2	3 11	− 1	+ 2	4 6	− 1	+ 3
11.	4 1	− 2	+ 1	4 1	− 1	+ 2	4 9	....	+ 3
12.	4 1	− 3	+ 4	4 1	− 4	+ 4	4 8	− 4	+ 5
13.	3 10	− 4	+ 6	3 11	− 4	+ 4	4 5	− 3	+ 6
14.	3 5	− 6	+ 7	3 6	− 6	+ 5	4 0	− 5	+ 7
15.	2 10	− 7	+ 6	2 10	− 7	+ 5	3 6	− 6	+ 7
16.	2 2	− 8	+ 7	2 1	− 8	+ 9	2 10	− 8	+ 8
17.	1 7	− 6	+ 7	1 6	− 5	+10	2 4	− 7	+ 9
18.	1 1	− 7	+ 4	1 0	− 6	+ 3	1 10	− 8	+ 2
19.	0 10	− 3	——	0 9	− 3	——	1 7	——	− 3
20.	0 10	+ 3	− 3	0 9	+ 3	− 3	1 7	+ 2	− 3
21.	1 0	+ 3	− 2	0 10	+ 4	− 1	1 9	+ 3	− 4
22.	1 4	0	+ 1	1 3	0	+ 2	2 3	− 2	+ 1
23.	2 0	0	+ 1	2 0	− 1	0	2 10	− 3	+ 5
24.	2 6	0	+ 1	2 7	0	+ 2	3 4	− 1	+ 1
25.	2 7	− 5	0	2 9	− 8	− 4	3 6	− 9	− 3
26.	2 8	− 2	+ 5	3 0	− 4	+ 7	3 6	− 3	+ 9
27.	2 8	+ 1	......	3 0	− 2	......	3 4	− 1	
28.									

Table XI. (Continued.)

North coast of Ireland.

	Raughly Point.			Rutland Island.					
	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.	Mean.	A.M.	P.M.
1835.	ft. in.	in.	in.	ft. in.	in.	in.	ft. in.	in.	in.
June 9.	4 1	+ 1	− 1	5 5	− 8	+ 12			
10.	4 5	0	+ 2	6 3	− 5	+ 3			
11.	4 6	− 2	+ 3	6 9	− 6	+ 18			
12.	4 5	− 4	+ 7	6 9	− 9	+ 4			
13.	4 3	− 9	+ 9	6 2	− 2	+ 1			
14.	3 10	− 10	+ 9	5 4	− 8	+ 14			
15.	3 4	− 12	+ 11	4 4	− 5	+ 3			
16.	2 7	− 9	+ 19	3 1	− 6	+ 14			
17.	2 0	− 13	+ 16	2 0	− 12	+ 13			
18.	1 8	− 11	——	1 5	− 14	——			
19.	1 7	+ 12	− 10	1 0	+ 23	− 6			
20.	1 8	+ 10	− 10	7	+ 5	− 12			
21.	2 0	+ 2	− 4	7	− 10	− 5			
22.	2 7	+ 3	+ 2	10	+ 7	+ 6			
23.	3 0	− 2	− 4	1 6	− 2	+ 7			
24.	3 3	+ 3	+ 5	2 0	+ 6	+ 4			
25.	3 2	− 6	+ 1	2 9	+ 15	+ 8			
26.	3 0	− 4	+ 9	3 4	0	+ 6			
27.	2 8	− 2	......	3 5	+ 12				
28.									

West Coast of Ireland.

	Minaird.			Port Magee.			Brandon.		
June 9.	4 7	− 1	+ 1	3 4	− 1	+ 2	4 2	− 11	0
10.	4 7	− 4	+ 1	3 10	− 2	+ 2	4 9	+ 2	− 1
11.	4 9	− 1	+ 1	3 11	− 1	+ 1	5 0	0	+ 1
12.	4 7	− 1	+ 2	3 9	− 3	+ 2	4 10	− 2	+ 2
13.	4 1	− 4	+ 3	3 6	− 4	+ 3	4 2	− 4	+ 4
14.	3 8	− 2	+ 4	3 1	− 3	+ 3	3 6	− 5	+ 7
15.	3 1	− 4	+ 3	2 7	− 3	+ 1	2 10	− 4	+ 5
16.	2 6	− 4	+ 6	2 0	− 4	+ 3	2 5	− 1	+ 5
17.	1 11	− 4	+ 1	1 6	− 3	+ 2	1 10	− 9	+ 10
18.	1 6	− 2	+ 2	1 1	− 1	+ 1	1 6	− 9	+ 8
19.	1 5	− 2	− 1	11	− 2	+ 4	1 2	− 5	——
20.	1 7	− 2	+ 3	1 0	− 2	——	1 2	+ 1	− 4
21.	1 9	——	− 1	1 1	0	0	1 7	+ 1	+ 2
22.	2 0	− 2	+ 2	1 6	0	+ 1	2 1	− 1	+ 1
23.	2 4	− 2	+ 2	2 1	− 2	0	2 5	− 2	− 2
24.	2 8	− 2	+ 3	2 6	+ 1	+ 3	2 11	+ 8	+ 6
25.	2 10	− 1	− 3	2 7	− 5	− 3	3 2	+ 1	+ 5
26.	3 0	0	+ 6	2 6	− 8	+ 2	3 2	+ 5	+ 6
27.	2 10	− 3	......	2 3	0	......	3 1	− 5	
28.									

Table XI. (Continued.)

	Ballyonughan.			Barna, Port of Galway.			Killery.		
	Mean.	A.M.	P.M.	Mean.	A.M.	P.M,	Mean.	A.M.	P.M.
1835.	ft. in.	in.	in.	ft. in.	in.	in.	ft. in.	in.	in.
June 9.	3 9	− 2	+ 2	4 4	+ 1	+ 1	4 1	− 3	+ 4
10.	4 8	− 2	+ 3	5 2	+ 2	+ 2	4 7	− 3	+ 6
11.	4 11	− 3	+ 3	5 6	− 2	+ 4	4 9	− 6	+ 4
12.	4 8	− 7	+ 4	5 4	− 7	+ 3	4 7	− 4	+ 4
13.	4 3	− 3	+ 4	4 11	− 3	+ 3	4 3	− 6	+ 6
14.	3 8	− 4	+ 5	4 4	− 3	+ 4	3 8	− 2	+ 3
15.	3 0	− 4	+ 7	3 9	− 1	+ 4	2 10	− 4	+ 5
16.	2 3	− 3	+ 5	2 11	− 2	− 5	2 4	− 4	+ 12
17.	1 7	− 4	+ 3	2 2	− 3	+ 2	1 9	− 6	+ 2
18.	1 0	− 2	+ 2	1 8	− 3	+ 1	1 4	− 4	+ 2
19.	11	——	− 3	1 6	− 2	——	1 4	− 4	+ 2
20.	10	+ 3	− 2	1 7	+ 4	− 2	1 6	——	− 3
21.	1 2	− 1	0	1 10	− 2	− 1	2 0	0	0
22.	1 8	0	+ 3	2 4	+ 2	+ 2	2 7	+ 2	− 4
23.	2 4	− 2	0	3 0	− 4	0	2 10	+ 2	+ 2
24.	2 9	+ 2	+ 2	3 3	+ 4	+ 6	3 2	0	+ 9
25.	3 0	−1 1	− 2	3 5	− 10	+ 1	3 3	− 1	− 2
26.	3 0	0	+ 3	3 5	+ 3	+ 5	3 1	+ 1	− 3
27.	2 10	− 2	......	3 2	0	......	2 9	+1 0	
28.									

	Achil Beg.			Dulaugh.			Doonkeeghan.		
June 9.	4 7	− 6	+ 13	4 4	− 2	+ 4	3 0	0	0
10.	4 10	− 7	+ 4	4 9	− 2	+ 8	3 4	− 2	+ 3
11.	4 11	− 7	+ 5	4 10	− 8	+ 7	3 5	− 4	+ 5
12.	4 10	− 4	+ 4	4 7	− 5	+ 6	3 4	− 7	+ 7
13.	4 5	− 5	+ 2	4 3	− 5	+ 8	3 0	− 8	+ 10
14.	3 9	− 5	+ 10	3 9	− 17	+ 9	2 6	− 8	+ 13
15.	3 3	− 6	+ 8	3 3	− 7	+ 10	1 11	− 10	+ 14
16.	2 9	− 7	+ 19	2 9	− 7	+ 10	1 5	− 10	+ 16
17.	2 3	− 6	+ 4	2 3	− 10	+ 7	1 0	− 3	+ 10
18.	1 9	− 6	......	1 10	− 9	+ 4	8	− 12	0
19.	1 7	+ 2	− 2	1 7	− 3	+ 5	6	+ 8	− 8
20.	1 6	+ 2	− 2	1 8	− 3	......	6	+ 7	− 6
21.	1 10	0	+ 1	2 9	0	+ 1	9	+ 3	− 2
22.	2 4	+ 2	+ 3	2 5	+ 3	+ 3	1 3	+ 4	0
23.	2 10	− 2	+ 3	2 11	− 3	+ 4	1 9	− 3	+ 1
24.	3 3	− 1	+ 5	3 3	− 2	+ 5	2 0	0	+ 3
25.	3 3	− 13	0	3 3	− 15	− 1	2 0	− 12	− 1
26.	3 1	− 1	+ 4	3 2	− 2	+ 5	1 10	− 4	+ 6
27.	2 10	− 1	......	2 10	− 3	......	1 6	− 4	
28.									

XVIII. *On the Powers on which the Functions of Life in the more perfect Animals depend, and on the Manner in which they are associated in the production of their more complicated results.* *By* A. P. W. Philip, *M.D. F.R.S. L. & E. &c.*

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IN considering the powers of life, I shall in the first place inquire into the seat, the functions and the nature of each of these powers; and then point out the manner in which they are associated in the production of their more complicated results*.

OF the powers of the living animal the simplest is that by which the motion of its various members is effected, and which essentially contributes to all its more complicated functions, the contractile power of the muscular fibre, the healthy action of which is not a state of uniform contraction but of a constant and generally rapid suc-

* The following paper comprehends the results of a task, not of a few months or years, but, with the exception of the time devoted to the more active duties of my profession, of the greater part of not a short life. As far as I can, to render what I have done useful, it is necessary that the various facts should be compared, and thus the inferences they afford ascertained. They are dispersed through so many publications, eleven papers in the Philosophical Transactions, published in the course of twenty years, an Inquiry into the Laws of the Vital Functions, a Treatise on the Influence of Minute Doses of Mercury in restoring the Functions of Health, my Gulstonian Lectures on the more obscure affections of the Brain, &c., that although they are frequently referred to, it has almost always been, more or less, under mistaken views; because the writer, in commenting on one part, has been unacquainted with others with which it is intimately connected, for in so protracted an investigation, few will take the trouble to keep pace with the inquirer.

It will appear from the references in the following paper how each of the various facts, dispersed through so many publications, has contributed to fill up the great outline of the laws that regulate the animal functions, which has been the object of my labours, and from which the topics of the day have never induced me to swerve. It is evident that this outline must be ascertained, before the functions of particular organs can be successfully investigated; all of which more or less depend on the general laws of our frame.

The following I believe is the first attempt which has been made to ascertain experimentally the seat, the functions and the nature of all the powers of the more perfect animal, and the various relations they bear to each other, by which, several and in some functions all of these powers being enabled to cooperate, their more complicated results are effected. In the latter part of the subject, I have found much care required in rendering the language sufficiently explicit; and if in any instance I have failed in this attempt, I hope it will in some degree be ascribed to the very complicated nature of the subject, arising from the great variety of facts on which the conclusions are necessarily founded.

It will be admitted with respect to the conclusions themselves, that the circumstance of the present inquiry embracing the whole of the subject is in favour of their accuracy; because in that case, and where, as in the present instance, all the parts are intimately connected, few inferences consequently resting on any single position, an error may betray itself in so many ways, that it can hardly escape detection.

cession of contractions and relaxations. Its permanent contraction we have reason to believe is always a state of disease*.

Many of the older physiologists supposed that all the powers of the living animal reside in the nervous system. HALLER was the first who, in a way that commanded general attention, maintained that the muscular power resides in the muscular fibre itself, and made experiments for the purpose of establishing this opinion.

His conclusions however were not generally admitted, and the principle, on which the point was argued, could lead to no decision. It was as easy to affirm as to deny that the remaining nervous influence is the cause of the power which exists in the detached muscle, for it was impossible to separate from the muscular fibre the minute extremities of the nerves with which it is blended, and to them it was alleged that it owes the power, which for a short time it retains after its separation from the brain, spinal marrow and larger nerves.

The only conclusive means of determining the question appeared to be an appeal to such experiments, as are capable of directly ascertaining whether the effect of the nervous influence on the muscular fibre be that of maintaining or, analogous to the effects of other stimulants, of exhausting its excitability.

It appears from the 32nd experiment, detailed in my *Inquiry into the Laws of the Vital Functions* †, that the latter is the case to a degree, that leaves little doubt respecting the result ‡; which was confirmed by other experiments, in which I found in many trials, that when the powers of the nervous system are destroyed by opium or tobacco, the loss of power in the muscles is not proportioned to the degree in which the powers of that system are impaired, but simply to the degree in which their contractions had been excited through it §; and that the removal of both the brain and spinal marrow, which we shall find are the only organs employed in the formation of the nervous influence, does not in any degree impair the action of the heart and vessels as long as the healthy state of the blood can be maintained by artificial respiration ||.

From the whole of these experiments it appears that the opinion of HALLER is correct, that the power of the muscular fibre is not derived from the nervous system, but resides in that fibre itself; a conclusion which we shall find of no small importance

* See a paper on the Nature of Sleep published in the Philosophical Transactions for 1833 and republished in my treatise *On the Nature of Sleep and Death*.

† In referring to my *Inquiry into the Laws of the Vital Functions* the reference is always to the third edition.

‡ When two sets of muscles of the same description were exposed to the action of the same artificial stimulant, and one of them at the same time to the effects of the nervous influence, it was found that the excitability of the latter was most rapidly exhausted; and this was sometimes the case to so great a degree, that in one instance the excitability of the muscles exposed to both was exhausted in half the time required for its exhaustion in those exposed to the artificial stimulant alone.

§ The fourth edition of my Treatise on Fevers and Inflammations.

|| Philosophical Transactions for 1815, and my *Inquiry into the Laws of the Vital Functions*, Part II.

in judging of the nature of the nervous influence, and consequently of other functions of the living animal beside the function of the muscular fibre.

The powers of the nervous system properly so called, which cooperate with the muscular fibre in all the more complicated functions, next demand our attention; and it will appear that there is no other branch of physiology in which the generally received opinions have been, and indeed still are, so much at variance with simple matter of fact.

That what in common language is called the nervous system embraces two distinct sets of organs is evident; because not only do the functions of the sensorial and nervous organs, properly so called, essentially differ in their nature; but, as we shall find, their localities also are different. Now it has generally been taught that the nervous functions, properly so called, only administer to those of the sensorial power; that they are limited to the conveyance of impressions to and from the sensorial organs, and to the excitement of the muscles of voluntary motion*.

I shall in the first place inquire into the nature of the functions of the nervous system properly so called, and then endeavour to ascertain to what parts of that system the powers on which those functions depend belong.

The mere structure of the parts might have led physiologists to suspect that the organs of this system possess other powers than those just enumerated. We find two distinct classes of nerves, to one of which the functions subservient to the sensorial powers evidently belong, and it has never been proved that the other at all partake of these functions. Besides, it had appeared from experiments relating to this second class of nerves, although their results were differently reported by different writers, that they must possess functions of a wholly different nature.

Such were the circumstances which called my attention to this, as it were superadded, class of nerves; and I think it will appear from the facts I am about to adduce, both what are their functions, which we shall find much more complicated than those of the former class, and why the results of the experiments just referred to have been so differently reported.

The peculiarity of structure relating to these nerves is, that, while all the former class proceed, either from the brain or spinal marrow, directly to the parts they influence or which influence them; they either enter or send branches which enter a chain of protuberances called ganglions, from which nerves are sent to the parts influenced by them. Hence they are termed ganglionic nerves, a term however, which has not been employed in a very strict sense; because, besides the ganglions just mentioned, which receive nerves from different parts of the brain and spinal marrow, there are other protuberances also termed ganglions, which are formed on particular

* See in the Report of the British Association for the Advancement of Science for 1833 a paper by Dr. Henry of Manchester; and a Dissertation on the state of Medical Science from the termination of the 18th century to the present time by Dr. Allison, Professor of the Institutes of Medicine in the University of Edinburgh, in the Cyclopædia of Practical Medicine, published in 1834.

nerves, but which appear to have no relation to any nervous filaments but those of the particular nerve to which they belong. It is therefore necessary that I should define the sense in which I use the terms ganglion and ganglionic nerve. By ganglion, I mean a nervous protuberance which receives nerves from different sources; and by ganglionic nerve, a nerve which either enters or sends branches to such ganglions, or proceeds from them, whether it have or have not any such protuberance belonging to itself. It may be stated however that there is reason to believe that all nerves, having such protuberances, contribute towards forming the ganglions in the sense in which I use the term*.

One of the most evident peculiarities of the ganglionic nerves, in the sense in which I use the term, is, that while the cerebral and spinal nerves supply the sensitive organs and the muscles of voluntary motion, the ganglionic nerves supply the muscles of involuntary motion and the other vital organs.

Haller, finding that the heart cannot be influenced through its nerves in the same way as a muscle of voluntary motion, was led to the conclusion that the former cannot be directly influenced through the nerves. But M. Le Gallois has shown that he was deceived in this inference, the heart being immediately subject to the influence of the spinal marrow; and the latter author further inferred from his experiments that the spinal marrow is not only capable of directly influencing the heart through its nerves, but that, through the same channel, it bestows on both the heart and blood-vessels all their powers; an inference refuted both by experiments already referred to, and others, an account of which appeared in the Philosophical Transactions for 1815 and has since been republished in my *Inquiry into the Laws of the Vital Functions*; and some of which were at the request of the Royal Society repeated with the same results by Mr. Clift, Mr. Clift's confirmation of them being published in the same volume of the Transactions.

The circumstance of the brain and spinal marrow only, as we shall find, influencing the heart under peculiar circumstances is probably the cause of the fact ascertained by Haller, that it cannot be excited through its nerves in the same way as a muscle of voluntary motion, an observation which applies to all muscles of involuntary motion, a want of attention to which has misled some physiologists†.

From the whole of the experiments which have been referred to, it appears, on the one hand, that neither the brain nor spinal marrow bestows any power on the heart or vessels; but, on the other, that each of the former organs is equally capable of directly influencing both, (the vessels even to their utmost extremities,) and that, not only by exciting their powers, but also by impairing and even wholly destroying them, according to the nature and power of the agent operating on the brain or spinal marrow;

* See a paper on the functions of the nervous system in the Philosophical Transactions for 1829, which was republished in my treatise *On the Nature of Sleep and Death*.

† See my reply to MM. Breschet and Milne-Edwards in the Philosophical Transactions for 1833, entitled "Some Observations relating to the Function of Digestion."

although, in their usual functions, the heart and vessels, like the other muscles of involuntary motion, obey neither of these organs, but agents peculiar to themselves*.

Thus it appeared that the ganglionic, like the cerebral and spinal nerves of motion, may administer towards the contraction of the muscular fibre, unless, what I conceive to be more probably the case although not yet ascertained, branches of the latter nerves are bound up in the same sheath with the ganglionic nerves, as we shall find there is reason to believe is the case with respect to the nerves of sensation. Physiology has been much indebted to the experiments of Sir Charles Bell, M. Majendie and Mr. Mayo, from which it appears that the nerves of motion and those of sensation, although often bound up in the same sheath, are distinct nerves having different origins.

What are the functions which are peculiar to the ganglionic nerves in the sense in which I use that term?

This question is answered respecting one of the most important of the vital functions, the process of secretion, in papers published in the Philosophical Transactions for 1815 and 1822, and republished in the last edition of my *Inquiry into the Laws of the Vital Functions.*

It appears from the experiments detailed in those papers that when part of the eighth pair of nerves in their passage along the neck is removed, or these nerves are divided and one end of either portion is raised from its place, the secretion of gastric juice soon begins to fail in its properties; and if the animal survives for a certain time, the contents of the stomach are found not only undigested but quite dry, proving that there had been no secretion from it whatever for some time.

From these experiments we also learn how it has happened that such various accounts of the effects in the stomach of dividing the eighth pair of nerves is given by different experimentalists; because it was found that digestion was more or less completely interrupted in proportion as the divided ends of the nerves were kept at a considerable distance from each other. Even when the distance was a quarter of an inch, provided the divided ends were no otherwise displaced than in consequence of the retraction of the nerve on its division, digestion, although more or less deranged, was not interrupted, a subject to which I shall have occasion to recur. Now as this was a point which never particularly demanded attention, accident must always have more or less influenced the result.

But secretion is not the only vital function that is influenced by the division and separation of the divided ends of the eighth pair of nerves in the neck. It appears from experiments detailed in a paper, published in the Philosophical Transactions for 1827 and republished in my treatise *On the Nature of Sleep and Death*, that, under such circumstances, all the assimilating functions are so deranged that in

* See two papers published in the Philosophical Transactions for 1815, and republished in my *Inquiry into the Laws of the Vital Functions.*

many parts of the lungs, in the space of fifteen or twenty hours, not a vestige of their healthy structure remains.

Such it appears are the effects on the stomach and lungs of depriving them of a considerable portion of the influence of the brain. They are organs well adapted for such observations. In the stomach we have certain means of judging of any considerable deviation in the process of secretion; and from the peculiar structure of the lungs, they are well adapted for observations on changes of structure. That the effects are proportioned to the degree in which the influence of the brain is withdrawn, appears from comparing those of dividing and separating the divided ends of one or both nerves.

It is not however to the brain alone that similar observations apply, for it was found that depriving the stomach and lungs of the influence of the spinal marrow is attended with the same effects. When the lumbar portion of this organ was destroyed, the functions of the stomach and lungs and the structure of the latter were as much impaired as by the division and separation of the divided ends of one of the eighth pair of nerves; and when the lower half of the spinal marrow was destroyed, as much, as by the division and separation of the divided ends of both those nerves*.

It thus appears that the powers on which the secreting and assimilating functions depend reside in the brain and spinal marrow, and equally in these organs; nor does either of them act through the other in influencing the vital organs, as the brain is found to do through the spinal marrow in influencing many of the muscles of voluntary motion, the heart and vessels in every part being equally influenced by agents acting either on the brain or spinal marrow, when the other has been removed, as while both with all their connections remain†.

The question which next presents itself is, how far are they assisted in these offices by the nerves, ganglions and plexuses?

In a paper, published in the Philosophical Transactions for 1833 and republished in my treatise *On the Nature of Sleep and Death*, I have entered into this question at great length, where such observations and experiments will be found, as far as I am capable of judging, as render the following inferences unavoidable. That the nerves, ganglions and plexuses in no degree contribute to the formation of the nervous influence; the spinal and cerebral nerves being merely the means of conveying the influence of the parts of the brain and spinal marrow from which they proceed, and of conveying to these organs the influence of impressions made on their extremities; while the ganglions and plexuses are only the means of combining the influence of all parts of the brain and spinal marrow, through all parts of which the organs of the nervous power properly so called, are distributed; the nerves proceeding from the ganglions and plexuses, being the means of conveying this combined influence to the muscles of involuntary motion and the other vital organs.

* See my *Inquiry into the Laws of the Vital Functions*, Part II.

† Ibid.

The question here arises, For what purpose is the influence of every part of the brain and spinal marrow thus combined to be bestowed on these organs?

This question is answered by the experiments just referred to, which prove that the influence of every part of the brain and spinal marrow is necessary to the due performance of the functions of secretion and assimilation; and by other facts to which I shall have occasion to refer, which prove the necessity of the muscles of involuntary motion being under the controul of the same power, on which these functions depend.

All of them, as we have just seen, fail when any considerable part of the influence either of the brain or spinal marrow is withdrawn, the failure of function being proportioned to the degree in which the influence of either is withdrawn, proving that the influence of every part of them is essential to the due performance of those functions*.

Important and extensive as these functions are, there is still another, hardly less so, dependent on the powers of the nervous system properly so called. Sir Benjamin Brodie† proved by direct experiment many years ago that animal temperature is under the influence of the nervous system, and various observations evince that a debilitated state of the brain is accompanied with a diminished temperature.

I made many experiments on this subject detailed in my *Inquiry into the Laws of the Vital Functions*, from which it appears that in this, as in all the other vital functions, the spinal marrow shares with the brain. If the power of either organ be impaired, the temperature sinks in precisely the same proportion as the secretions are deranged. A particular organ may be deranged by preventing its due supply of nervous influence, and there may be no general diminution of temperature. The due nervous influence is prevented reaching the particular organ, but there is no diminution of the power of the brain or spinal marrow. When, on the other hand, the power of either of these organs is impaired, there is an immediate diminution of temperature.

When the lower half of the spinal marrow was destroyed the animal shivered, and would probably soon have died of cold if it had not been kept in a high temperature, and even when the lumbar portion alone was destroyed, a considerable but less diminution of temperature ensued‡.

Thus it appears, from the whole of the facts which have been referred to, that on an influence derived from the brain and spinal marrow, and not from any part, but from the whole of these organs, the secreting and more immediately assimilating functions and the maintenance of animal temperature depend. This influence therefore performs a still more important part in the vital than in the sensitive functions. In the latter we find it acting only a subordinate part; while in the former it must be regarded as the great agent, to which all others employed are subservient.

* See papers which appeared in the Philosophical Transactions for 1815 and 1827, and my *Inquiry into the Laws of the Vital Functions*, Part II.

† See the Philosophical Transactions for 1812 and 1814.

‡ *Inquiry into the Laws of the Vital Functions*, Part II.

Has the nervous influence any immediate dependence on any of the other powers of the animal frame?

The muscular, we have seen, has no immediate dependence on the nervous power, the only power on which its immediate dependence can be supposed. In like manner the sensorial is the only power on which any immediate dependence of the nervous power can be supposed.

I made an extensive set of experiments, detailed in my *Inquiry into the Laws of the Vital Functions*, to which I shall soon have occasion to refer more particularly, from which it appears that all the functions of the nervous power properly so called survive the removal of the sensorial power, with the exception, of course, of those in which that power is associated with it. After the removal of the sensorial power the nervous influence is still capable of all its other functions. It is still capable of exciting the muscles both of voluntary and involuntary motion, of, for a short space of time, forming the secreted fluids, performing the various functions of assimilation, so far as to preserve the structure of parts where it would otherwise have been impaired, and, to a certain degree, of maintaining animal temperature. The nervous, like the muscular power, therefore, is an independent power, having its seat in its own organs, and having no other dependence on the other powers of the living animal than for the due structure of those organs.

Such are the powers of the nervous and muscular systems of the more perfect animals, and the seat and functions of these powers.

They possess however two other sources of power, for the sensorial power and the powers of the living blood have no immediate dependence on either of the former powers, or on each other.

That the only dependence of the sensorial power is for the maintenance of its organs, is evident on the most cursory review of the animal economy. The nature of the functions of that power alone evinces that the living animal possesses no others from which it can be derived; and that the powers of living blood have no direct dependence on its other powers, is proved by the fact, that the blood retains its vital properties after it is separated from the body*.

With respect to the locality of the latter powers, the powers of the living blood, it appears from the fact just stated, existing in itself, must be coextensive with the functions of secretion and assimilation. At first view it would appear that the functions of the sensorial power, like those of the living blood, pervade every part of the system; the power of sensation seems to pervade the whole frame. On observing the phenomena with more care however, we find the seat of the sensorial power confined to a small space, when we compare it with that of the nervous power properly so called, the organs of which, we have seen, pervade the whole of the brain and spinal marrow.

* See Mr. Hunter's experiments on the Blood, and the experiments detailed in the last chapter of the second part of my *Inquiry into the Laws of the Vital Functions*.

The nerves of sensation in which are included, of course, the nerves of the external senses, and the immediate organs of the sensorial powers are not parts of the same organ, but distinct parts, having different localities and performing functions of a wholly different nature; that is, the sensorium does not pervade the whole system, but belongs to particular parts. To what parts has never been correctly ascertained, but we know that in man they are confined to certain parts of the brain with little if any participation by the spinal marrow; although in some of the inferior animals the spinal marrow largely partakes of them, a proof that the sensorium is not as some have supposed confined to a physical point, but is of a considerable extent.

Our sensations are referred to certain parts of the body by experience alone. Hence the well-known facts that infants are not aware of the part of the body in which the cause of any sensation originates; and when a limb has been lost, at whatever part the separation is made, we continue to refer to the lost part sensations excited by causes affecting the nerves of the stump.

The function of the nerves of sensation has relation to the sensorial organs alone. The influence they convey is the means by which the sensorium is impressed by distant parts, and such is their only function.

The more perfect animals then possess four distinct powers, having no direct dependence on each other, but each we shall find indirectly dependent on the other three, namely for the maintenance of its organs.

I am now to inquire how far we can advance in determining the nature of these powers, how far they are peculiar to the living animal, or the same which operate in other parts of nature.

WE are in the habit of regarding life as a power of peculiar mystery, but do we find any other principle of action less mysterious? It is not the principle but its properties, which are the objects of our senses. A knowledge of the former is not merely beyond the limits, but the nature of our minds. Do we know more of the principle of electricity or gravitation than of life, or is there more uncertainty in noting the property of resistance to fermentation and congelation without any sensible peculiarity in the substances possessed of this property, than that of weight or light? It is not that the nature of life is more obscure than that of any other principle of action, all are equally so, but that its phenomena, being more varied and bearing less analogy to those of other principles than these bear to each other, are less familiar objects of contemplation.

The subject thus appears invested with an obscurity which does not belong to it, and the perplexity has been increased by vain attempts to remove it; attempts on principles having no relation to the laws by which the phenomena of life are regulated. What possible relation can the laws of mechanics, or any other principle which operates in the inanimate world, bear to the phenomena of life properly so called? It is as much a distinct principle as any of those which operate in that world, and the same method which leads to a knowledge of other sciences must guide us here. There are no means but a study of its phenomena by which we can attain a knowledge of life, that is, of its properties, the only knowledge we can attain of any principle of action. But if our object be to attain a correct knowledge of it, we must first determine with accuracy what are the phenomena of life; for, in the complicated functions of the living animal, it requires not a little patience, labour and circumspection to distinguish what part depends on vital powers properly so called; and what, on a modification of the powers of inanimate nature. Even the most cursory view must convince us that many of the functions of the living animal partake of the latter powers.

Respiration is performed, that is the air is drawn into and expelled from the lungs, by means which act on the same principle as the bellows. The blood in the circulation moves on the same principle as the water in a set of water-pipes. It obeys a propelling force, and is subjected to the same laws of gravitation. The motion of our limbs is effected by the same mechanical laws, by which bodies are put in motion in the external world. Here, as in inanimate nature, velocity can only be obtained by the sacrifice of power. Similar observations apply to the various processes of secretion and assimilation. We can trace in these processes, the same chemical laws which obtain in the laboratory of the chemist; but there is at the same time in all the foregoing functions something more in operation, analogous to which we find nothing in inanimate nature.

The force indeed by which the air is drawn in and expelled in respiration operates on the same principle as in the bellows; but the powers by which the machinery is worked are the contractile power of the muscular fibre, and the power of the nerves by which it is excited. The motion of the blood depends on the same principle as that of the water in its pipes, but it is the contractility of the muscular fibre which supplies the moving power. The same observation applies to the motion of the various members of our body.

In like manner in the processes which maintain the organs of all these functions, and effect the separation of those parts of them which have become useless, and therefore noxious, while we trace the same chemical laws which operate in other parts of nature, we can perceive that they are constantly modified by the powers peculiar to the living animal; for it is not only impossible by any chemical arrangement to produce the same results in inanimate nature, but even by the principles which regulate its phenomena, to trace all the steps by which they are effected. We can neither, for example, imitate the process by which the temperature of living blood is raised above that of the surrounding medium, nor, on the principles of the chemistry of inanimate nature, trace all its steps. No position can be more erroneous than that the chemical processes of the living animal depend alone on the same laws with those of inanimate nature. The properties of life are as peculiarly its own as the properties of gravitation.

I am now to attempt to draw the line of distinction between the powers, which the living animal possesses in common with inanimate nature, and those peculiar to itself.

With respect to its mere mechanical powers to which I have just had occasion to refer, there can be but one opinion, that they are powers common to the living animal and inanimate nature; but with respect to the powers we have been more particularly considering, all of which appear at first view to be powers peculiar to the former, the question is not so easily answered. Until it is answered, however, it is evident that we cannot draw the line which correctly separates the phenomena of life from those which result from other principles of action, a line essential to an accurate view of the properties, that is, to a knowledge, of that principle.

The question which I am here to consider, then, is, how far are the sensorial, nervous and muscular powers and the powers of living blood peculiar to the living animal, or possessed by it in common with inanimate nature?

IT requires but little consideration to answer the question respecting the sensorial and muscular powers, and the powers peculiar to the living blood. Where do we find in inanimate nature a power which can be mistaken for any of them? But even the most cursory review of the functions, which, it appears from the experiments above referred to, are those of the nervous power properly so called, makes us pause. That the oxygen and carbon of the blood combine by the same agency as in the laboratory of the chemist, is a position too probable to be hastily dismissed; and if such be the case, to what other functions of the nervous influence will the same observation apply?

The following, it appears from experiments above referred to, comprehend the nervous functions properly so called.

1. The excitement of the muscles of voluntary motion in all their functions.
2. The excitement of the muscles of involuntary motion in some of their functions.
3. The maintenance of the processes on which animal temperature depends.
4. The formation of the various secreted fluids. And
5. The more immediate processes of assimilation by which the structure of our various organs is both effected and maintained.

Of these functions, the excitement of the muscles alone is the only one which may be supposed to be the effects of either a chemical or mechanical agent.

In all the healthy functions of life, however, in which the muscular power is employed, the stimulus which excites it, if we except the mere power of distension, appears to be of the former description. Even those stimulants, which maintain the functions of the alimentary canal, which, remotely depending on the stimulus of the food, may at first view be supposed to be the effect of a mechanical agent, appear to be wholly of a chemical nature. The ingesta will not excite a secretion of gastric juice unless they possess chemical properties of a certain description, and the muscular coat of the stomach is not duly excited unless the food has been converted into a healthy chyme, the formation of which, it appears from direct experiments, depends on the healthy state of the influence supplied by the brain and spinal marrow*. In like manner, the healthy action of the intestines, as appears from a thousand observations, can only be maintained when their healthy stimulant has been duly prepared by the chemical processes which take place in the duodenum; which also depend on the influence supplied by the brain and spinal marrow. It is evident that all the other functions just enumerated are of a chemical nature. It thus appears that all the nervous functions are chemical processes, and consequently that there may be an expectation of finding an agent in inanimate nature capable of them.

It was found that in proportion as the nervous influence, properly so called, is withdrawn, all these processes fail. It is evident therefore that on this influence the changes observed depend. Whatever therefore that influence may be, all its functions in their general nature are identical with the effects of the chemical agent, whatever that agent may be, which operates in inanimate nature. This step therefore appeared to be gained. Further reasoning however was unnecessary, because it was not difficult to submit the question to the test of direct experiment.

I was thus led to consider what power of inanimate nature it was most probable might be successfully substituted for the nervous influence.

An important point had been ascertained. It had been found that of all the powers of inanimate nature, voltaic electricity is most capable of the excitement of the muscular fibre, that is, of one of the functions of the nervous influence. This indeed went but a short way towards establishing the identity of the two powers, so many other stimulants being capable of exciting that fibre. It is not to be overlooked, however, that feeble as this argument is towards proving the identity of the nervous influence and voltaic electricity, it is powerful respecting the general nature of that influence; because on the supposition of the nervous influence being a vital power properly so called, we have here a vital power possessing a property in common with a thousand inanimate agents. Is there any unequivocal instance in which any of the properties of a vital principle, properly so called, is not essentially different from those of any of the principles of inanimate nature? On the whole, the property in question was sufficient to suggest the trial how far voltaic electricity is capable of the other functions of the nervous influence.

No hope of success of course could be entertained unless the artificial agent were employed under the same circumstances, under which the nervous influence operates; that is, while the structure of the organs is entire, and their vital properties unimpaired.

Under such circumstances I substituted it for the nervous influence in the various functions of secretion and assimilation with success. It was admitted by those who

* Philosophical Transactions for 1815 and 1822, and *Inquiry into the Laws of the Vital Functions*, Part II.

witnessed the results, that these functions were as effectually performed by it, as by that influence itself; and the experiments were afterwards publicly repeated both in London* and Paris†, in the latter on a great variety of animals, and in both instances with the same results. In the first of my papers published in the Philosophical Transactions for 1829 entitled *Some Observations relating to the Function of Digestion*, several circumstances are enumerated which it is necessary to keep in view in conducting such experiments.

Only one of the functions of the nervous influence now remained which had not been effected by voltaic electricity, the process by which animal temperature is maintained. For the purpose of determining how far it is capable of this function, it was judged the most satisfactory means to expose the living blood to its effects, both in its arterial and venous state. If voltaic electricity operate on the same principle as the nervous influence, it will raise the temperature of the former, but not of the latter, which has already undergone the operation of that influence. Such was found to be the case. The arterial blood immediately rose several degrees on coming into contact with the voltaic wires, but there was no increase of temperature in the venous blood, although, in both instances, the blood was subjected to them as it flowed from the vessels; it having appeared from previous experiments that the delay of even a few minutes, although no apparent change had taken place in the blood, and no elastic fluid had been disengaged from it, prevented any rise of temperature; so rapidly do some of the properties of living blood undergo a change after its removal from the vessels‡.

Such being the facts, I could no longer doubt that the nervous influence and voltaic electricity are powers of a similar nature, and it appeared to me that this would be most convincingly illustrated, by causing the nervous influence to pass through other conductors than the nerves; because such a fact would, independently of all others, prove that it is not a vital power properly so called, it being acknowledged on all sides that no such power admits of separation from the texture to which it belongs in the living animal.

With this view I made many vain attempts, and hardly escaped the ridicule of my associates for expecting that the nervous influence could exist in any texture but that to which it belongs in the living animal.

In the third edition of my Inquiry into the Laws of the Vital Functions, the reader

* The Journal of the Royal Institution of London for 1822. See also the London Medical and Physical Journal for May, 1820, vol. xliii. p. 385.

† De l'Influence du Système Nerveux sur la Digestion Stomachale; par MM. Breschet, D.M.P., Chef de Travaux Anatomiques de la Faculté de Médecine de Paris, &c.; H. Milne-Edwards, D.M.P.; et Vavasseur, D.M.P. (Mémoire lu à la Société Philomatique, la 2e Août, 1823.) Extrait des *Archives Générales de Médecine*, Aoû, 1823.

‡ See the second part of my *Inquiry into the Laws of the Vital Functions*, Experiments 80, 81, 82, 83, 84, and 85.

will find the circumstances detailed which led to the successful experiment, the result of which was publicly confirmed both in London and Paris; and those who in the first instance ridiculed my expectations, joined me in stating that such is the fact.

The cause of failure in my first experiments on this subject, was the circumstance of having made a wrong choice of the nerve on which I operated, which was a nerve of voluntary motion.

It will appear on reflection that this was a wrong choice. Before we can expect that the nervous influence can be made to pass through any other conductor than that to which it belongs in the animal body, there must exist a powerful cause soliciting it to some particular point. In a muscle of voluntary motion there can be no such cause. The nervous influence is not attracted to the muscle, it is sent to it by an act of the sensorium, carried into effect by the powers of the nervous organs, which are subjected to its influence; those organs which, on the one hand, prepare that influence, and those which, on the other, convey it when duly prepared*. The muscle is altogether passive till the influence is applied to it. But the case is wholly different with respect to many of the organs which contribute to the functions of the ganglionic system. We know from direct observation that in many of them, there is a cause continually operating, which solicits the nervous influence to them.

In these organs the living blood and nervous influence cooperate in the functions of secretion and assimilation; and it is an acknowledged fact, that when a determination of blood to secreting organs takes place, there is in the same proportion an increase of their secreted fluids, a result which cannot arrive without a corresponding supply of nervous influence. Thus we know, as indeed we had reason to expect, that the presence of the living blood in the secreting organs solicits a proportionable supply of that influence; and thus it was, that whereas, while I operated on the nerves of voluntary motion, my attempts were wholly fruitless, the very first attempt with the ganglionic nerves was crowned with success; nor, since the repetition of the experiments in London and Paris, has the fact been questioned.

If the facts I have stated be correct, we can have little doubt that the nervous influence is of a nature similar to the inanimate agent which was substituted for it; for to say nothing of the circumstance of the nervous influence being capable of existing in a texture different from that to which it belongs in the living animal, we cannot suppose that there are two distinct powers, the one of which is capable of all the effects of the other; or I would rather say, that such a supposition amounts to a contradiction in terms, because as it is acknowledged that we know nothing of any principle of action but by its properties, it necessarily follows, that by these alone it can be distinguished.

In discussing the nature of the nervous influence too much has been ascribed to electric tests, which are referred to as if they possessed a power equal to that of chemical

* See the second of my papers in the Philosophical Transactions for 1829.

tests. A correct chemical test will give evidence of what we are in quest of under all circumstances; and is therefore capable in all instances of detecting its presence, and consequently its absence also. This arises from there being but one counteracting power, that of affinity. If the affinity be stronger in the test than in any other substance, the effect of all other affinities is destroyed. We possess no such electric test, because here there may be other counteracting causes beside the power of affinity, opposing currents, for example. Besides, we know that the properties of electricity are so modified by the powers of life, as greatly to interfere with its relations to our tests. The electricity of the torpedo and other electric animals does not affect the common electrometer, yet no one has doubted its identity with the electricity of inanimate nature.

Although electric tests therefore give evidence of the presence of electricity, we cannot by their means prove its absence; a fact with which we should not have been acquainted, were it not, under certain circumstances, possible to prove the presence of electricity without their aid; that is, the presence of electricity may under certain circumstances be proved, where it is not indicated by any of the properties generally admitted to be peculiar to it.

Suppose it were said, for example, that we cannot admit that electricity is the agent in the combination of oxygen and carbon, because there is no test by which its presence can be detected; the reply of Dr. Faraday, I conceive, would be; we cannot at present, whatever we may do hereafter, make the electricity employed in effecting this combination evident to any of our tests; but I consider its presence as a necessary inference, because I have adduced facts which prove, either that electricity is the agent in such combinations, or that nature here deviates from the simplicity observed in all her other works. Either electricity is the agent in the combination in question, or there are two kinds of chemical affinity.

Under such circumstances can any other reply avail except either disproving the facts or pointing out the fallacy of the inference?

What I have done is strikingly illustrated by the late investigations of Dr. Faraday. It is more than twenty years since I found that voltaic electricity is capable of all the functions of the nervous influence; it now appears from the facts, on which he has founded his doctrine of electro-chemical equivalents, that electricity is the agent in all chemical processes*. According to the inferences of Dr. Faraday therefore, the experiments, which prove that the nervous influence is the agent in the functions we have been considering, all of which we have seen are chemical processes, are sufficient to prove its electric nature; and we are now also, on the other hand, furnished with direct proof that the brain is capable of collecting and applying, even according to the dictates of the will, the electric power.

Dr. Davy in his last paper on the torpedo† observes that, "when the brain has been

* Dr. Faraday's papers in the Philosophical Transactions for 1832, 1833, 1834, and 1835.

† Philosophical Transactions for 1834.

divided longitudinally, the fish has continued to give shocks; when the brain has been entirely extracted, the fish instantly lost this power, though the muscles generally continued to act powerfully;" from which it appears that the electric power is not like the muscular independent of the brain, but on the contrary immediately depending on it; proving that in this, as in all the other nervous functions, as appears from the facts stated in a paper which the Royal Society did me the honour to publish in 1829, and which was republished in my *Inquiry into the Nature of Sleep and Death*, the nerves are merely the passive, the brain, one of the active parts of the nervous system.

In the foregoing positions we here find, as in other similar instances, that when truth is once arrived at, other facts, beside those which led to it, arise to give it their aid.

Dr. Davy made no experiments to determine how far the spinal marrow, the only other part of the nervous system concerned in the formation of the nervous influence, partakes of the function in question. Were we to reason from the analogy afforded by all the other nervous functions properly so called, we should expect to find the spinal marrow sharing it equally with the brain. It is not unlikely that the removal either of the brain or spinal marrow would destroy this function, as is found to be the case with respect to the more complicated functions properly termed nervous; a point which can only be determined by an appeal to direct experiment; or, like the excitement of the muscles, it may belong to either organ separately, or, which is less probable, it may, being a function of volition, belong to the brain alone.

In addition to the foregoing statements I may refer to the success which has attended the employment of voltaic electricity in those diseases which depend on a deficient supply of nervous influence*.

That we may have a clear view of the line of distinction between the sensorial and nervous powers, a more particular consideration of the former is necessary.

THE following points we have seen are made out from the phenomena of every day's experience, that the organs of the sensorial power and the nerves of sensation, in which of course are included the nerves of the organs of the external senses, are distinct organs; the former being the immediate organs of those powers, the latter the organs which excite them.

We have just seen reason to believe that the influence conveyed by the nerves which excite the muscles and maintain the secreting and assimilating functions and the due temperature of the animal body, is a power which operates in inanimate nature; because, on the one hand, we have found such a power capable of all the functions of the nervous influence, properly so called; and, on the other, that this influence is capable of existing in other textures than those to which it belongs in the living animal, proving that it is not a vital power properly so called.

Are the properties of the influence conveyed by the nerves of sensation the same

* See my *Inquiry into the Laws of the Vital Functions*, Part III., and my *Treatise on Indigestion*, 7th Edition.

with those of any of the powers of inanimate nature, or can this influence exist in any texture but that to which it belongs in the living animal?

It is enough to say that its only property is that, by which it is enabled to co-operate with the immediate organs of the sensorial power. To such a property we not only find that there is nothing analogous in any of the properties of inanimate nature; but, as will more fully appear from what I am about to say, that the organs of the sensorial power are, in their healthy functions, unapproachable by any of its agents: it is therefore perhaps unnecessary to add, that we know of no texture in which the influence conveyed by the nerves of sensation can be supposed to exist, but that to which it belongs in the living animal.

The nerves of sensation therefore belong to the sensorial not to the nervous power. They convey an influence of a wholly different nature from that conveyed by the nerves of the latter power; and the only analogy which can be traced between their function and the operations of inanimate nature is, that it is excited by impressions received from the agents of the external world; to which we are indebted for all our knowledge of that world. The action of the immediate organs of the sensorial power, we have seen, being thus excited by one vital part acting on another, and by its vital properties alone, all analogy with the operations of inanimate nature here disappears.

While the other functions of the living animal are the results of inanimate agents acting on living parts or living parts on them, in the sensorial functions we see the effects of vital parts acting on each other and that by their vital properties alone. Hence the analogy between the former and the operations of inanimate nature, and, with the exception just pointed out, the total loss of all such analogy in the latter.

Whence is it possible to conceive that such analogy can arise except from the operation of some of the agents of the inanimate world? When the phenomena of the living animal are carefully compared with the preceding facts, it will be found that in every instance, in which any analogy between its functions and the operations of inanimate nature can be traced, the interference of such an agent may be detected.

WE have now considered individually the various powers of the more perfect living animal. We have found in it, beside the mechanical powers which, it will be admitted on all hands, it evidently possesses in common with inanimate nature, four distinct powers; three of them vital powers, properly so called, that is, powers having properties essentially different from those of the agents which operate in inanimate nature. In the fourth alone we recognise one of those agents; for we find it can exist in other textures than those to which it belongs in the living animal, and that we can substitute for it one of the powers of inanimate nature without deranging the functions of life.

All these powers are employed, although in a very different way, in the construction of two systems in a great degree distinct; the end of the one being the maintenance of our bodies, of the other, our intercourse with the world which surrounds us.

In the remaining part of this paper, I am to consider the various relations those powers bear to each other in the maintenance of the foregoing systems; and the way in which these systems themselves are so related, as to form the animal body into a whole, in which no part can be affected without tending more or less to influence every other.

IN order to ascertain the seat of the power on which muscular contractility depends, it was necessary in an early part of this paper to enter on the relation which subsists between the muscular and nervous systems; and it appears from what is there said, that the nervous influence, whether in its effects on the muscles of voluntary or involuntary motion, stands only in the relation of a stimulus or directly debilitating power to the muscular fibre, according to the manner in which its organs are impressed; a result, I may observe in passing, peculiarly in accordance with all the other facts which have been stated respecting the nature of that influence, because the same observation, we shall find, applies to all the agents of inanimate nature which are capable of influencing the muscular fibre.

The relation which next demands our attention is that which subsists between the organs of the nervous influence and the living blood.

The first thing, which here strikes us, is that the blood-vessels and nerves uniformly accompany each other; from which we are led to infer that they cooperate in functions of very general necessity.

The powers of the nervous system properly so called, we have seen, are all of a chemical nature. Of this nature therefore must be all processes in which they immediately cooperate. It is evident that where such powers are employed, to render them efficient, materials must be provided on which they may operate, and there must also of course be means by which these materials are duly exposed to their action.

The materials we find in the blood, the means, employed for the purpose of duly exposing them to the action of the nervous influence, in the capillary vessels, on which, the minute extremities of the nerves, (which we know, from numberless observations, are those parts of the nervous system by which its powers are immediately applied in the functions of secretion and assimilation, as well as the excitement of the muscular fibre,) are distributed. As the central are the only parts of the nervous system properly so called, employed in the formation of the nervous influence, the extremities of the nerves are the only immediate organs of its powers in all its functions.

The motion of the fluids in the capillary vessels, as appears from many experiments related in the Philosophical Transactions for 1815 and my *Inquiry into the Laws of the Vital Functions*, depends on a power which resides in themselves, in no degree depending on the power of the heart or arteries, except as far as is necessary for the due supply of blood to the latter, which form the reservoirs, from which the capillary vessels draw their supply. When in the newly dead animal a ligature is thrown round all the vessels attached to the heart and this organ is removed, the motion of the blood in the capillaries continues unimpaired, and only fails in proportion as the supply from the large arteries fails*; the cause of the emptiness of the latter some time after death.

By such means the materials on which the nervous influence operates are supplied and presented to it; and the means of supply, namely, the power of the heart and arteries, as well as that of the capillary vessels themselves, being, as we have seen, under the immediate influence of the same power which effects the chemical changes†, the supply is proportioned to the demand under the various conditions of the ever-changing functions; and under the same influence are the means of removal, whether of secreted fluids or solid parts become unfit for the purposes of life. Such are the circumstances above referred to, which render it necessary that the muscles, whether directly or indirectly, employed in these functions should be subjected to the same power on which depend the functions of secretion and assimilation, namely, all muscles of involuntary propulsion, that is, with a very few exceptions, all muscles of involuntary motion.

It appears from some lately ascertained facts that the secreted fluids are formed from the blood while still in its vessels, and not in the act of their separation by the secreting organs. That such must necessarily be the case appears from what has been said. The act of separation must be posterior to the changes effected by the chemical powers of the nervous influence. It is only while the blood is still in its vessels that it can be exposed to their operation; and we have reason to believe that it is only as the due changes have been effected, that is, only as the secreted fluid has acquired its due properties, that it applies the due stimulus to the vessels by which it is discharged: on the same principle that the due action of the intestines, by which they discharge their contents, is not excited if these contents have not acquired their due properties by the chemical processes which take place in the stomach and duodenum.

Such are the nature and functions of the nervous power, and its relations to the muscular power and the powers of the living blood. When we turn to the sensorial system, we find ourselves in a new world. Here voltaic electricity, which we so successfully substitute for the nervous influence, can do nothing. The immediate organs of the sensorial power, we have seen, are as it were hedged in and defended from contact with any of the agents of inanimate nature.

On the one hand, we find the nerves of sensation, which so far partake of the nature of the external world, that they are capable of receiving and propagating impressions from its agents, but in all other respects are allied to the organs with which they are associated. By their vital powers they influence the immediate organs of the sensorium, and the functions thence resulting are the effects of one vital organ influencing

* See a paper on the powers of circulation in the Philosophical Transactions for 1831 republished in my *Inquiry into the Nature of Sleep and Death*. See also my *Inquiry into the Laws of the Vital Functions*, Part II.

† See experiments detailed in the Philosophical Transactions for 1815 and 1822, and my *Inquiry into the Laws of the Vital Functions*, Part II.

another, and that by its vital properties alone; for it is evident that the properties operating here have nothing in common with those of any of the principles of inanimate nature. In the results consequently, we have seen all analogy with the phenomena of these principles, for the first time, lost; and necessarily so, none of the properties of the agents of that world being immediately employed in their production.

The nerves of sensation, it appears from what has been said, convey not the nervous influence properly so called. The influence they convey is of a nature essentially different from that by which the muscles are excited and the functions of secretion and assimilation maintained. They sufficiently partake of the nature of the sensorial organs to be capable of directly impressing them, and thus the latter receive all their impressions whether originating from without or within our own bodies.

On the other hand,—that is, that the sensorial organs may, without contact with any of the agents of the external world, impress those agents,—a more complicated machinery is required. The various nerves of sensation are the only means required for conveying impressions to these organs; but so simple an apparatus is not sufficient to convey to, and impress on, the materials of the external world, the dictates of volition. The powers of the nervous system are here called into operation by the sensorial powers, to which they are subjected; for it appears from many experiments, detailed in the Philosophical Transactions for 1815 and in my *Inquiry into the Laws of the Vital Functions*, that as the muscular is independent of the nervous power, but subjected to its influence, the nervous is independent of the sensorial power, but, in like manner, subjected to the influence of this power. In the case before us the nervous, influenced by the powers of the sensorial organs, supply a certain set of nerves with the stimulus which excites the muscles of voluntary motion, the immediate agents by which the materials of the external world are impressed.

I have had occasion to refer to the great variety of the phenomena of life, as one cause of their apparent obscurity. Such is their variety that we are at first view lost in attempting any arrangement or even enumeration of them. An essential step towards their arrangement, as appears from what has been said, is their division into those which are the immediate results of the cooperation of the principle of life with the principles of inanimate nature, and those which have no immediate dependence on the latter powers; for all our functions mediately or immediately depend on the operations of the agents of inanimate nature. All are more or less directly excited by impressions originating in their agency.

The most purely sensorial functions, our pleasures and pains, are as dependent, though more remotely, on the excitement maintained by them as the functions of the organs immediately impressed by them. Have not the excitements of memory as much originated in their impressions, as their more direct effects on the part impressed? And when the nature of our bodies and the circumstances in which we are placed are duly considered, what other result could be expected? Our organs, being composed of the same materials as the world which surrounds us, can only be directly influenced by agents of their own nature; and from that world, and by the medium of those organs, all the materials, not only of our acquired knowledge*, but of our enjoyments and our sufferings, are derived.

And as on the one hand, all our functions are more or less immediately excited by impressions made by the agents of the external world on organs composed of materials of their own nature, on the other, we have no power of influencing them, but through similar means. The only means of exciting our mental functions are the impressions of those agents on the organs of sense, and our only means of operating beyond our own bodies are through our organs of motion. Even when by our mental powers we influence those of other sentient beings, it is as much, though not so directly, by impressing the agents of the external world by the latter organs, as when we raise a weight or throw a stone.

SUCH is the general outline of the vital and sensitive systems; and the manner in which the various powers of the living animal are related in the formation of these systems. By the foregoing means, the nervous power maintains the vital functions properly so called; and the sensorial power is brought to cooperate with the powers of inanimate nature, powers which have no properties in common.

IT appears from the facts adduced in my paper on the Nature of Death, published in the Philosophical Transactions for 1834, that the vital and sensitive systems obey very different laws, the difference depending on the vast difference in the nature of the sensorial and nervous powers, the leading powers which pervade all their departments, and to which all their other powers are subservient.

These other powers, it appears from what has been said, are the same in both, namely, the muscular power and the powers of the living blood, and, in the sensitive system, the nervous power itself; for in this system all the other powers of the living animal are directly subjected to the sensorial power, while none of the powers of the vital system have any direct influence on it, their influence on the sensorial power being through the medium of its organs, the structure and wellbeing of which immediately depend on the vital powers.

In other respects also the laws of the two systems essentially differ. Nor will these differences surprise us, when it is recollected, as appears from the facts which have been stated, that while the leading power in the vital system is one of those powers which operate in the external world; that of the sensitive system not only possesses no properties in common with the agents of inanimate nature, but depends on a set of organs unapproachable in their healthy functions by any such agents.

When the facts adduced in the paper just referred to, and that on the Nature of

* We are born with the knowledge which is immediately essential to our existence. The infant knows as well how to suck and how to breathe as the adult. See my paper *On the Nature of Death.*

Sleep published in the Philosophical Transactions for 1833, are duly considered, it will appear that a principal cause of difference in the laws of these systems depends on the difference of the laws of excitability in the organs of their leading powers. In those of the leading power of the sensitive system, all degrees of excitement are followed by a rapid proportional exhaustion of excitability; so that the effect of the usual stimulants of life for a few hours, renders a state of inactivity essential to the maintenance of their health: while the exhaustion of the excitability of the organs of the leading power in the vital system by those stimulants, is the operation of many times as many years, the one determining the recurrence of sleep, the other the natural duration of life.

Thus it is that those, in whom, from habits of dissipation, extreme labour, or other causes, the excitability of the vital system is to a certain degree exhausted, but who as they approach middle life cease to be exposed to such causes, and during that portion of life, that is from thirty to fifty or fifty-five, feel little inconvenience from the effects of their early habits, there still being in the vital system sufficient excitability for the usual functions of life; after this period, when the defect of excitability begins to be felt sooner or later by all, feel the effects of its expenditure which had been so profuse in early life: many striking instances of which I have witnessed. Similar observations apply to long-protracted illness, severe misfortunes, or any other cause which at any period of life in a great degree, and for a considerable length of time, tend to exhaust the excitability of the vital system, although for a certain time the individual may enjoy his usual health after such causes have ceased to operate *.

The organs of the leading power in the vital system, as appears from the facts stated in my paper on the Nature of Death, possess at birth a high degree of excitability, a degree beyond that proportion which constitutes the firmest state of health—the cause, as there pointed out, of many of the most fatal diseases of infancy—which is by the operation of the usual stimulants of life gradually reduced till it bears a due proportion to those stimulants, by which the powers of the constitution are confirmed. At length from their continued operation the fault is a defect not a redundancy of excitability, to which every day necessarily adds, till they can no longer excite the organs on which that power depends; for in every instance the immmediate cause of absolute death, which is very different from what we call death, is the failure of that power †. And here as there are no means in the constitution, as in the case of the organs of the leading power in the sensitive system, of restoring the excitability of its organs, they at length finally cease to be excited. Thus it is that in almost all cases of great longevity we find that there has been little exposure during life to powerful causes of exhaustion of either body or mind, for we have seen that the nervous is immediately under the influence of the sensorial power; and that such instances are most frequent in the colder of the temperate climates, heat, on the one

* See what is said of the excitability of the two systems in my papers on the Nature of Sleep and Death.

† My paper on the Nature of Death. Philosophical Transactions for 1834.

hand, tending to exhaust excitability, and extreme cold, on the other, to render us less capable of excitement.

While considering the laws of excitability it is necessary to bear in mind an essential property of all those agents which are capable of calling it into action, and which has demanded less attention than its great importance in the treatment of disease demands. There is no agent capable of influencing either of the two systems into which the functions of the living animal arrange themselves, whether it be such as makes its chief impression on the mind or body, which is not capable of acting either as a stimulating or directly debilitating power according to the degree in which it is applied. There is none which may not be applied in so small a degree as to act as a stimulant, and in so great a degree as to act as a directly debilitating power. The most depressing passion in a comparatively small degree will excite, the most exciting in an excessive degree directly debilitate; and the same stimulus by which either the nervous or muscular fibre is directly excited, will by its excessive application directly deprive it of power. I know of no exception to this law. All medicines within their stimulant range excite, and unless the excitement exceeds the degree which produces no correspondent depression, (for such a degree of excitement is compatible with the laws of the vital though not with those of the sensitive system*,) it acts as a permanent tonic. All, beyond their stimulant range, act as directly, and although within that range, if of a certain intensity, as indirectly debilitating powers with respect to both systems†.

IT is evident from many facts, stated in my papers on the Nature of Sleep and Death, that each of the foregoing systems is a whole, which cannot be influenced in any one part without a tendency to be affected in all others; a property which perhaps more than any other influences the progress of their deviations from the healthy state; for every part more or less feeling the change effected in any one, if there be any from accidental causes more liable to disease than the rest, this part particularly feels the cause which operates on all; and, as I shall soon have occasion to point out more particularly, is even the means of diverting its effects from every other part. Thus it is that diseases of continuance become complicated, and that an affection, attended with little risk in the part first impressed by the offending cause, often becomes formidable by its secondary effects.

* My paper on the Nature of Sleep in the Philosophical Transactions for 1833.

† See what is said on this subject in my treatise *On the Influence of Minute Doses of Mercury in restoring the Functions of Health*, and my Gulstonian Lectures on the more obscure affections of the Brain, also in the recapitulation at the end of this paper. All my Treatises, to which I have occasion to refer, are more or less founded on the principles here recapitulated; and consequently in them more or less copious references to the facts, on which these principles rest, became necessary. In order to arrive at the conclusions of the present paper, it was necessary to state the whole of those facts with their various bearings, which I have done in as concise a manner as the requisite perspicuity appeared to admit of. In the less familiar parts of the subject, it requires some care to avoid being misunderstood.

The power which operates here, has been termed the sympathy of parts, the effects of which I have considered at length in a treatise on the more obscure diseases of the brain, being the Gulstonian Lectures delivered at the College of Physicians in 1835. I am now, after referring to its more prominent effects, to consider the nature of this function, and the powers on which it immediately depends.

AS it appears from the experiments above referred to that the organs of the sensorial and nervous powers, the leading principles of the two great systems the functions of which comprehend all the functions of life, although both belonging to the brain and spinal marrow, are distinct sets of organs; the one set being confined to a comparatively small portion of these organs, the other distributed through the whole of them, from the uppermost surfaces of the brain and cerebellum to the lowest portion of the spinal marrow; and as numberless observations evince that the immediate cause of sympathy exists in the central organs alone *, it follows that these systems must have different centres of sympathy, that if the different parts of each system sympathize, it cannot be through the same centre †. Now it appears from the phenomena of disease, compared with the results of the experiments just referred to, that each of the centres of these systems is often influenced with so little disturbance to the other, that disease of either system, especially when of a chronic nature, often spreads to distant parts of the system in question, without much affecting the other; a favourable result in the sensitive system, because it is only in proportion as the organs of the vital system are implicated that life is endangered; but in the vital system the most fruitful of all causes of obscurity, and that in diseases of the most formidable nature, to which many have fallen, and still fall a sacrifice; for so ill supplied are many of the vital organs with nerves of sensation, that in them diseases of sympathy often make a fatal progress without the state of the part originally affected having attracted attention, and without its restoration, that of the part secondarily, but more prominently, affected is impossible ‡. Thus also it is,—that is, in consequence of the one system often suffering with little disturbance to the other,—that extreme suffering not unfrequently continues for years without materially impairing the functions of life, the organs of suffering belonging to the sensitive system; while in other instances immediate danger presents itself with so little previous suffering, that even the medical attendant is unprepared for it.

* My Gulstonian Lectures.

† Ibid.

‡ The internal water in the head of children, for example, has, till within the last thirty years, been almost uniformly fatal, having been treated as an original affection of the brain. Dissection having now proved it to be a secondary affection depending on the state of the liver, there are few serious diseases in which the treatment is more uniformly successful, if it has not been allowed to arrive at its last stage. The original affection, which does not betray itself by any prominent symptom, being removed, its consequences yield to the means, which are powerless while it continues to operate. Other affections of the head, certain forms of pulmonary consumption, and many other diseases might be adduced as illustrating the same principles.

The latter evil can only be obviated by a careful study of the laws of sympathy in the vital system, and particularly by ascertaining what organs are most inclined to be affected by what others; for although the function of sympathy is, like other functions, influenced by causes peculiar to the individual, it is in a great degree regulated by principles, which more or less prevail in all*.

From the facts just referred to we easily perceive the cause of the sympathy by which every part of each of the foregoing systems is capable of influencing every other. Each is regulated by a leading principle, and in consequence of this, under an influence by which the affection of any one part tends to affect all others; because as all parts of each system both influence this principle, and are influenced by it, it necessarily follows that all must, through it,—that is, through the central organs of each system, which alone are the immediate organs of its leading principle,—feel the affections of each. Such, together with the laws I am now to consider, is the source of the function to which the term sympathy has been applied, a principle as I have just had occasion to observe, which perhaps more extensively than any other regulates the course of disease.

As each of the preceding systems is formed into a whole by its leading principle, the relations which these systems bear to each other have a similar effect with respect to the whole frame; for the affection of any one of its parts tends more or less, though much less powerfully than in the individual systems, to influence all others. The means by which the relation between the sensitive and vital systems, and consequently the most complicated functions are maintained, we are here to consider; to some of them I have already had occasion to refer.

WE have seen that the nervous power properly so called, the leading power in the vital system, is immediately under the influence of the sensorial power, the leading power in the sensitive system, and constitutes the medium through which all that part of our intercourse with the external world, by which the latter power influences it, is maintained. This therefore is the first bond of connexion to which I shall refer between the sensitive and vital systems. The second is the means by which the organs of both systems are maintained; for, as I have already had occasion to observe, the sensorial has a dependence on the vital system, for the maintenance of its organs, as the vital, we shall find, has a more remote dependence on the sensorial system, for the maintenance of its organs; the connexion thus established between them being increased by both systems equally depending for the maintenance of their organs on the muscular power and the powers of the living blood; both of which are in their turn subjected to the nervous, and the former certainly, and the latter, we have reason to believe, through the nervous, also to the sensorial power.

The sympathy which prevails through all parts of each system also contributes to the influence of these systems themselves on each other; because the state of the

* My Gulstonian Lectures.

parts secondarily affected in consequence of the power of sympathy, more or less influences both systems, all parts being more or less supplied with nerves from both.

But we have sufficient evidence in the phenomena of disease, compared with the results of the experiments referred to, that here, as in the instances just pointed out, the central organs of the sensitive, directly influence those of the vital system. A sympathetic pain it is well known referred to any part will at length produee actual inflammation of the part. Now while the pain alone exists, we know that the derangement, which produces it, is in the central organs alone of the sensitive system, and in no degree in the part to which it is referred; and we also know from the facts which have been stated that there is no channel through which this derangement can influence either the nerves or vessels of the part, but through the central organs of the vital system.

When the affection of the nerves or vessels of the part is the original disease, it influences the central organs of both systems by the actual disease of the part; but in the former casc there is no other channel of communication than that just referred to. The central organs of the sensitive, having no power over either the nerves or vessels, can only influencc them through the central organs of the vital system. Thus arises a double bond of connexion between the two systems, the central organs of the sensitivc system directly influencing those of the vital system, and the nerves of the sensitive system being necessarily influenced by all deviations from a state of health in whatever part, for all parts may be affected through the central organs of the vital system, the degree to which the effect in the sensitive system takes place being proportioned to that in which the part is supplied with nerves of sensation. As the central organs of the sensitive, directly influence those of the vital system; the latter, through the extremities of the different nerves with which the two sets of organs are associated, influcnce the former. Hence we have just seen the fatal obscurity of many diseases of those vital organs, which are ill supplied with this class of nerves; and as the more chronic the disease, the less it disturbs the sensitive nerves, it is in the more chronic cases that the obscurity is greatest, and consequently attended with the greatest risk.

Different parts of the central organs of the sensitive system correspond to different parts of the general frame. This is perhaps sufficiently proved by our being enabled by experience to refer our sensations to the seat of the cause which excites them; but in many of the inferior animals, where both the brain and spinal marrow partake of thc organs of the sensorial power, it may be proved by direct experiment, because after the removal of the brain we find the sensorial power lost only in those parts which derive thcir nerves from that organ.

But how comes it that the central organs of the vital system also have relation to certain parts of the gcneral frame, the nerves associated with these organs conveying, as appears from what has been said, their combined influence, which is bestowed alike on all vital organs?

It is a law of the animal economy, amply illustrated by the phenomena of disease, that when an impression influencing the system generally is, by previous debility or any other cause, directed to a particular part, its operation is diverted from all others. Now it appears from a thousand phenomena that the suffering of the sensitive system, referred to any particular part, is sufficient, under certain circumstances, in consequence of the influence of the central organs of the sensitive, over those of the vital system, to direct to it the effects of derangement excited in the latter. Thus even a diseased organ will often regain its healthy state, when the disease has spread to another, particularly if in the latter, it takes deeper root, if I may use the expression. It is a daily occurrence for a disease of function to be finally removed by a disease of structure being established in another organ. Hence the good effects of artificially exciting disease in external parts to relieve those more immediately essential to life; and the still more salutary effect, when the laws of our frame themselves produce the same effect, because here it is the uninfluenced result of those laws, whereas in the former case their tendency is constrained by artificial means. Thus for example it is that the inflammation of a gouty joint or other external disease often relieves the derangement of a vital organ, and that artificially repelling this effort of the constitution to save a vital part, has so often proved fatal.

On the facts that the central organs of the vital system directly influence the functions both of the vital nerves and of the vessels of every part, while those of the sensitive system have no direct influence on either, many of the phenomena of disease depend; because it is only in proportion as these nerves and the vessels of the part are influenced, that any disease of the part itself exists, and consequently that there is any tendency to derangement either of function or structure in the part; of function alone if the nerves alone are affected, of structure also as soon as the vessels partake of the disease. Hence it is that the tendency to change of structure, except where it takes place by imperceptible degrees, is, *cæteris paribus,* always proportioned to an inflammatory tendency which may be detected in the part, this tendency being the first indication that the vessels partake of the disease; and hence the importance of carefully watching and checking its approach, if the part be one essential to life, in all cases of deranged function of, or even of painful sensations referred to, particular parts.

EXTENSIVE as the foregoing relations of the vital and sensitive systems are, they are not the only ones. To determine the whole of them it is necessary to review the functions of the more perfect animals, and in particular correctly to ascertain the line of distinction between the functions of the two systems; in order to determine whether there be any beside those just pointed out in which they cooperate, and which consequently contribute to their dependence on each other.

I made many experiments with a view to draw the line of distinction between the vital and sensitive functions; and that the result might be the more certain, the attempt was made by two sets of experiments, conducted on different principles. By the one I attempted to ascertain what functions remain when the sensorial powers are withdrawn; by the other, what functions fail with the failure of the nervous powers; and the correspondence of the results of these sets of experiments tends to confirm the inferences from both *.

Much confusion had arisen from physiologists having neglected to ascertain this line. M. le Gallois, one of the most acute, soon found his difficulties from this cause such, that he was obliged to confess himself unable to proceed, and leave to his successors the task of removing them. He had adduced sufficient proof of the spinal marrow, to which the nerves of respiration belong, being capable of its functions independently of the brain; yet on the removal of a part of the brain, the medulla oblongata, respiration ceases. This difficulty he acknowledges he sees no means of removing, calling it "un des grands mystères de la puissance nerveuse, mystère qui sera devoilé tôt ou tard, et dont la découverte jettera la plus vive lumière sur le mechanisme des fonctions de cette merveilleuse puissance."

If the preceding facts be kept in view, it is evident without much consideration that none of the functions of the sensitive have any other dependence on the powers of the vital system, but for the due structure and wellbeing of their organs. The nature of the functions of the vital system here requires more consideration. They include respiration; circulation; those processes by which the secreted fluids are formed; those, namely the more immediately assimilating processes, by which our food is converted into the various organs of our bodies, and such parts of them as have become unfit for the purposes of life are separated and expelled, for all are in a state of change; and those by which the due temperature is maintained.

Does the sensitive cooperate with the vital system in any of these functions?

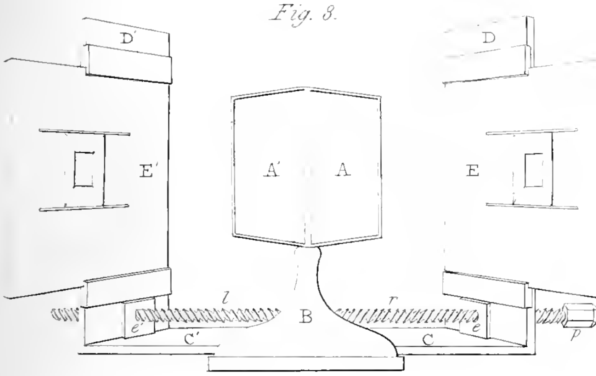
From the line of distinction, determined by the experiments just referred to, it appears that in one of them only is there such a cooperation.

I have in the last of my papers published in the Philosophical Transactions for 1829 considered at length the nature of respiration, and have, as far as I am capable of judging, adduced such facts as prove that the muscles employed in this function are, in the full sense of the word, muscles of voluntary motion. The first act in respiration is the impression made on the sensorium, the sensation excited by the want of fresh air in the lungs. We are enabled to supply it and remove the uneasiness, by exciting, through the nervous system properly so called, certain muscles subject to the will.

Respiration thus depending on the combined operation of both systems, is as effectually destroyed by a failure of the sensation which makes us will to inspire, as by that of the nervous or muscular power by which the will effects its object. Thus the difficulty of M. le Gallois disappears. It is true that the spinal marrow and its nerves are capable of their functions independently of the brain, and that the nerves employed in respiration are supplied by the spinal marrow, but in this function it is an act of

* *Inquiry into the Laws of the Vital Functions*, Part II.

Fig. 8.



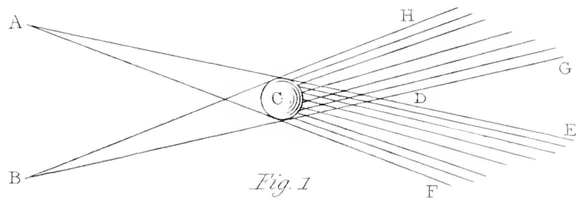


Fig. 1

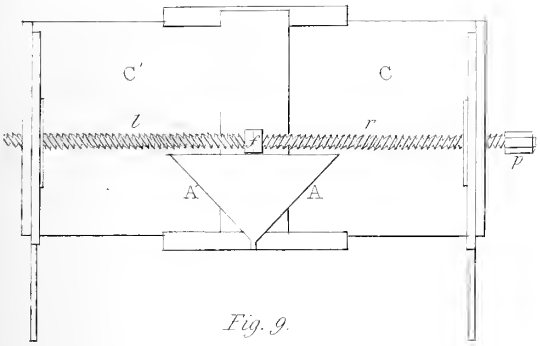


Fig. 9.

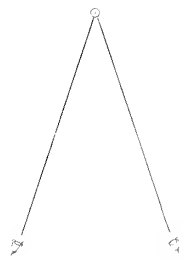
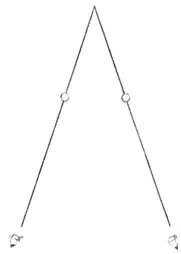


Fig 2



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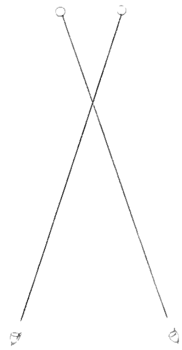

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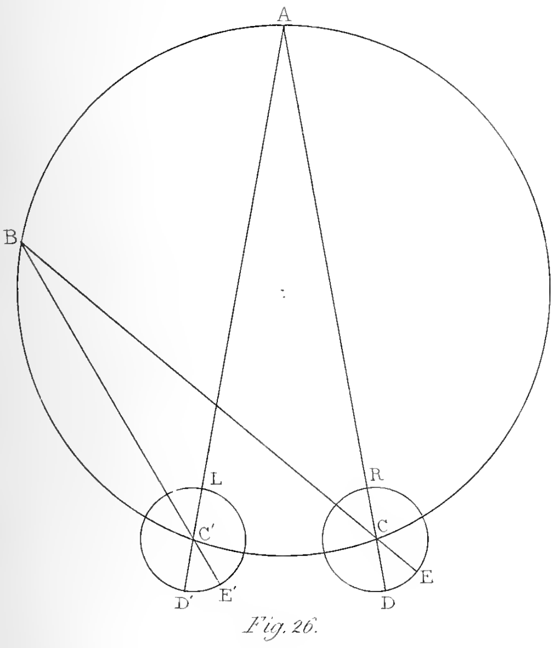


Fig. 26.

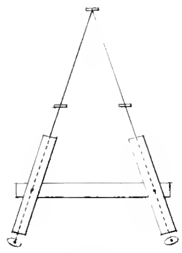


Fig 5.

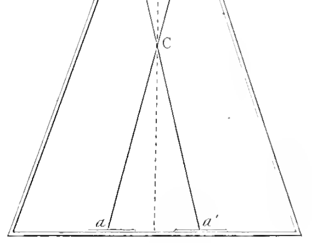


Fig. 6.

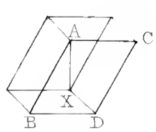
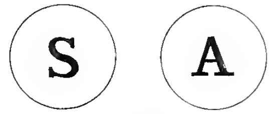


Fig. 22.



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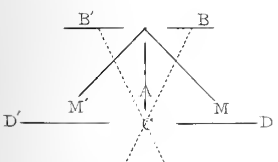

Fig. 21.

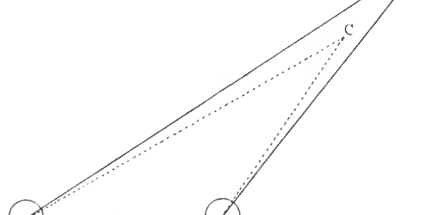

Fig. 24.

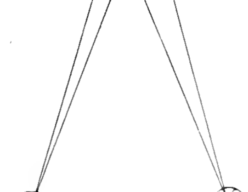

Fig. 7.

volition which excites them. They are quiescent till this act takes place. Hence it is that respiration ceases on the removal of the medulla oblongata, because by the removal of this part of the brain the power of sensation in all parts below the head and consequently of volition, as far as relates to those parts, is destroyed. Hence also the fact, above referred to, that the vital has a remote dependence on the sensitive system for the maintenance of its organs. If the muscles of respiration were not in the strictest sense muscles of voluntary motion, our powers of volition would in an essential respect be imperfect; for the due regulation of their action is essential in the formation of articulate sounds, the chief means by which our sensorial powers are enabled to influence those of other sentient beings.

In the papers on the Nature of Sleep and Death, published in the Philosophical Transactions for 1833 and 1834, I have pointed out how much the functions of the more perfect animal are influenced by this peculiarity of respiration, the only vital function, properly so called, in which the sensorial power cooperates; a circumstance which more generally perhaps than any other, which is equally of a local nature, influences the phenomena both of health and disease. In this function therefore we find a powerful bond of connexion between the sensitive and vital systems, and one, as appears from the papers just referred to, of the most extensive operation.

SUCH are the means by which the frame of the more perfect animal is formed into a whole, and the function of sympathy and its other more complicated functions above enumerated effected. A powerful connexion is established among all parts of each of the systems into which the functions arrange themselves, depending on each being regulated by a leading power which influences every part of the system to which it belongs, and in its turn is influenced by every part of it: and these systems themselves are intimately related in consequence of the nervous, the leading power in the vital system, by means of the control which the sensorial power exercises over it, being employed in the accomplishment of many of the sensitive functions, and the sensorial power, the leading power in the sensitive system, in one of the most important of the vital functions; by both systems not only depending for the maintenance of their organs on the same powers, but more or less directly on each other; by the powers common to both systems being under the influence of the leading principles of both; and by all affections of whatever part, whether original or sympathetic, necessarily influencing both its sensitive and vital nerves, and consequently the central organs of the system to which they belong.

FROM the whole of the facts referred to in the preceding paper, the great outline of the laws which regulate the functions of the more perfect animal is derived. The parts of which it consists, from the complicated nature of the subject, being very numerous, it is necessary, in order to place it in a clear point of view, concisely to recapitulate them; and as in the preceding paper, I commenced with the more simple, and was, by their intimate connexion with the more complicated powers, led to them; I shall in the recapitulation, that they may be viewed in both directions, begin with the more complicated, which by the same means will lead us to the more simple powers.

BESIDE the mechanical powers, of which the living animal evidently partakes in common with inanimate nature, it possesses, we have seen, four distinct powers, apparently peculiar to itself, having no direct dependence on each other, but each depending on the other three, for the maintenance of its organs; the sensorial, the nervous and the muscular powers, and the powers of the living blood.

By these powers are maintained the two systems into which the various functions arrange themselves, the vital and sensitive systems, the object of the one being the maintenance of our bodies, of the other, our intercourse with the external world.

THE organs of the sensorial power in man have their seat in the brain. They can be excited by no other means than the influence conveyed by the nerves of sensation, in the most extended sense of the expression, in every instance called into operation by impressions made on their extremities by agents which belong to inanimate nature, either existing within our own bodies, or making their impression from without; and, on the other hand, there are no means by which the sensorial organs can influence those agents but through the intervention of the powers of the nervous system properly so called. The nature of the sensorial power, we have seen, admits of no direct intercourse between its organs and the agents of inanimate nature, because it operates by properties, which have nothing in common with those of such agents; and as it can only receive impressions from the external world through the nerves of sensation, with which it is associated, it can only impress the agents of that world through the muscles of voluntary motion, excited by the nerves associated with them. Thus it is necessary, as we have by direct experiment found to be the case, that the organs of the nervous system should be placed under the control of the sensorial power. Through the same channel, we have seen, this power also, in some of their functions, controls the muscles of involuntary motion; and we have reason to believe, although the point has not been ascertained by direct experiment, all the powers of the living blood. And such, as appears from facts above referred to, is its influence on the nervous, and through it, on the muscular power, and we have reason to believe on the powers of the living blood, that it can not only excite, but impair and instantly destroy all these powers, according to the nature and power of the causes which influence its organs.

The circumstance of the muscular, as appears from facts above referred to, being the moving power of the blood in the vessels as well as the heart, greatly extends the influence of those powers which control it, namely the nervous power properly so called, and the sensorial power acting through it.

The only respects in which the sensorial power is related to the subject of this paper, are in the impressions it receives from the nerves of sensation, and the functions in which it cooperates with the nervous and muscular powers. Sensation and volition are the only sensorial powers employed in the maintenance of life.

While the organs of the sensorial power are thus capable of more or less directly influencing all the other organs of the living animal; they more or less feel in their turn, through the medium of the nerves of sensation, which, we have seen, convey an influence of wholly a different nature from that conveyed by the nerves associated with the organs of the nervous power properly so called, all changes effected in any part of our frame. By these means, this power constitutes the leading principle in the sensitive system, of which its organs form the central parts.

THE organs of the nervous power properly so called, have their seat equally in the brain and spinal marrow, and throughout all parts of them; and are excited, on the one hand, by the direct influence of the sensorial power, and on the other, by agents influencing the vital organs throughout every part of the frame; all of which, as in the case of the impressions made on the nerves of sensation, whether existing in our own bodies or making their impression from without, are agents of inanimate nature.

The immediate functions of the unaided nervous power are the excitement of the muscles of voluntary motion in all their functions, of the muscles of involuntary motion in some of their functions; and the immediate functions of this power in cooperation with the muscular power and the powers of the living blood, all the powers of both of which are directly subjected to its influence, are the formation of the secreted fluids, the maintenance of animal temperature and the various more immediately assimilating functions,—namely, the functions by which, on the one hand, our food is converted into our various organs, and, on the other, those parts of them which have become useless, are separated and expelled,—which renders it necessary that the muscles of involuntary motion as far as they cooperate in these functions, which with few exceptions include the whole of these muscles, should, as we have seen from direct experiment is the case, be under the immediate influence of the nervous power.

Neither the brain nor spinal marrow in the functions of the vital system acts through the other of these organs, as the brain is found to do through the spinal marrow in many of those of the sensitive system; each directly influencing every part.

The direct influence of the nervous power, it appears from what has been said, extends to all the functions of the system with the exception of those of the sensorial power, which it only influences through other functions. It directly influences, and is directly influenced by, all the vital functions properly so called, and hence constitutes the leading principle of the system to which they belong, therefore termed the vital system, of which its organs form the central parts.

THE circumstance of each of the foregoing systems being under the influence of a leading power, which is both capable of influencing and being influenced by every part of it, is the cause of that powerful sympathy which exists among all its parts, and which we have seen often essentially influences either system with but little disturbance to the other, on which many of the most important phenomena of disease depend.

THE muscular power, which has its seat we have seen in the muscular fibre itself, and the powers of the living blood, which have their seat in the blood itself, perform subordinate parts. They are equally employed in both systems for the maintenance of their organs. The latter supplies the materials endowed with the principle of life on which the nervous power operates in the formation of the secreted fluids, the maintenance of animal temperature and the various more immediate functions of assimilation; while the former, to which the vessels as well as the heart owe their power, supplies the means by which these materials are duly exposed to the operation of the nervous power, by which their necessary changes are effected; that is, to the influence of the extremities of the nerves, by which the nervous power operates in all these functions, as well as in the excitement of the muscles; for as the brain and spinal marrow, as we have seen proved by direct experiment, are the only parts of the nervous system employed in preparing the nervous influence; the minute extremities of the nerves are the only immediate organs of its functions; as the extreme parts of the sanguiferous system, the capillary vessels, are the organs by means of which the blood is immediately exposed to its influence.

That the capillary vessels may be, as little as possible, influenced by adventitious causes in functions of such importance in the animal economy, we find on the one hand, as appears from experiments above referred to, that the motion of their blood depends wholly on their own powers, the larger arteries which depend for their supply of blood on the heart, being only the reservoirs from which they draw their supply; and that, on the other, they are not controlled by the nervous influence through the medium of the heart, but receive this influence directly from its source: and so correct are these positions, that even the removal of the heart, if effected without considerable loss of blood, produces no immediate effect either on the action of the capillaries, or the control which the nervous power exercises over them.

SUCH are the individual powers of the living animal, their seat, the relation they bear to each other, and the manner in which their several functions are effected.

BUT the most complicated functions, it appears from what has been said, depend on the relations which subsist between the two systems themselves, into which the functions of all these powers are arranged.

They are related to each other, we have seen, by the nervous, the leading power in the vital system, in consequence of the control exercised over it by the sensorial, the leading power in the sensitive system, being employed in many of the functions of the latter; by the sensorial being employed in one of the most important of the vital functions, this peculiarity of respiration, for in no other of those functions is there any such cooperation, extensively influencing the phenomena both of health and disease; by both systems depending for the maintenance of their organs on the same powers, and more or less directly on each other; by the powers common to both systems, the muscular power and the powers of the living blood, being under the influence of the leading powers of both; and by all affections of whatever part necessarily influencing both its sensitive and vital nerves, and consequently the leading powers of both systems.

AS the various parts of each system are formed into a whole by all parts of each influencing and being influenced by its leading principle, so all parts of the animal body are formed into a whole, no part of which can be affected without tending more or less to affect all others, by the means just enumerated, by which these systems influence each other. Such are the foundations on which the laws of sympathy depend, a principle which, as I have endeavoured in a cursory way to point out, more than any other, influences the course of all deviations from a state of health.

THE functions of all the powers of the living animal, we have seen, are mediately or immediately excited by agents belonging to inanimate nature. Our organs are composed of the same materials with the external world, and can only be immediately impressed by agents of their own nature. It is true that the sensorial functions are the results of one vital part acting on another, the sensitive nerves on the immediate organs of the sensorial power; but the impression these nerves convey is in every instance received from the agents of inanimate nature. Here both the agent and the organs impressed are of the same general nature, being composed of similar materials, with our other organs. The peculiarity of the results depends on vital properties alone being employed in their production, whereas in all other functions of the living animal, the vital properties of the organ cooperate with the properties of the inanimate materials of which it is composed. Hence it is that its functions admit of being immediately excited by the agents of inanimate nature, which, having no properties in common with the only properties employed in the sensorial functions, cannot directly cooperate in their production.

Every agent capable of exciting any of the functions of the living animal, we have seen, acts as a stimulant or directly debilitating power, according to the degree in which it is applied. In the sensitive system their stimulant effect is always followed by a proportional exhaustion of excitability; in the vital system, only when the excitement exceeds a certain limit. I speak of a sensible exhaustion, an exhaustion beyond that produced by the usual stimulants of life, which, in the vital system, is too gradual to be perceived, and as far as relates to any particular stimulant employed within such limit, is so trifling that it may be safely overlooked. Hence it is that the vital system appears to possess an excitability which is not exhausted by stimulants except when applied in excess. It is essential, we have seen, in the treatment of disease to keep in view these properties of all agents capable of influencing the functions of life, that we may, as much as the nature of the case admits of, keep within the stimulant range of our remedies; and within that range, as far as possible, avoid the degree of excitement which produces sensible exhaustion of the vital organs.

WITH respect to the nature of the powers of the living animal which we have been considering, the sensorial and muscular powers and the powers peculiar to living blood we have found belong to the living animal alone, all their peculiar properties being the properties of life. The functions of life may be divided into two classes, those which are effected by the properties of this principle alone, and those, by far the more numerous class, which result from the cooperation of these properties, with those of the principles which operate in inanimate nature. The nervous power we have found to be a modification of one of the latter principles, because it can exist in other textures than those to which it belongs in the living animal, and we can substitute for it one of those principles without disturbing the functions of life.

Late discoveries have been gradually evincing how far more extensive, than was supposed, even a few years ago, is the dominion of Electricity. Magnetism, chemical affinity and (I believe, from the facts stated in the foregoing paper, it will be impossible to avoid the conclusion) the nervous influence, the leading power in the vital functions of the animal frame properly so called, appear all of them to be modifications of this apparently universal agent; for I may add, we have already some glimpses of its still more extensive dominion.

IN the preceding paper my objects have been to review the whole of the functions of the more perfect animal, to ascertain the nature of the powers on which they depend, the seat of each of these powers, the manner in which they are employed in effecting their several functions, and the manner in which they are associated in producing their more complicated results. Nothing in any part of the subject has been taken for granted, no position having been advanced without a reference to the observations or experiments on which it is founded.

I have here for the first time made an attempt which could not be done till all the facts on the subject had been ascertained, to point out the manner in which the different powers of the living animal influence each other, and thus conduce to their more complicated results; by which, being enabled to analyse these results, it might easily, were this the proper place, be shown, that we better see the operation of its different powers in the various deviations from a state of health, and can, under certain circumstances, better regulate the means of obviating them.

XIX. *Discussion of the Magnetical Observations made by Captain* Back, *R.N. during his late Arctic Expedition.* *By* S. Hunter Christie, *Esq. M.A. F.R.S. &c.*

Received and Read June 2, 1836.

PREVIOUS to Captain Back's departure in 1833 with the expedition for the relief of Captain Ross, he consulted me respecting the nature of the magnetical observations which I considered it desirable should be made in the regions he was likely to visit. I was fully sensible that, however available the expedition on which he had so nobly volunteered might be made to the cause of science, its primary object, and that to which all others must give place, was the relief of our gallant countrymen, at that time considered to be in imminent danger of perishing in the inhospitable regions which their enterprising spirit had led them to explore. I therefore considered it an object of the first importance, that whatever observations were to be made during the movements of the expedition should be so conducted as to cause as little delay as possible consistently with obtaining data for correct results, and also, that they should be made in the order of their importance. Compared with observations of the direction of the magnetic needle, both with reference to the meridian and to the vertical, other observations are of minor importance towards establishing anything like a theory of terrestrial magnetism. Considering that observations of the direction of the needle with reference to the meridian, though quite as important in a theoretical point of view as those with regard to the vertical, were necessarily called for in the conducting of the expedition, I, in the first instance, pointed out the observations which I considered necessary for determining the dip of the needle at the various stations where it might be practicable to make such observations; and I left the less important ones for the determination of the relative intensities of the absolute force acting upon the needle, which required more care, attention and assistance, to be made or not, according to the circumstances under which the expedition might be placed. Immediately on his return, Captain Back did me the favour to place all his magnetical observations at my disposal, and I feel that I should not do justice to the zeal and ability which that enterprising officer has displayed in the cause of science, if I did not, now that I have had leisure to reduce the observations, lay an account of them before the Royal Society.

I. *Observations of the Dip of the Magnetic Needle.*

The instrument employed for determining the dip was a small but very accurate one, by Dollond, furnished with two rectangular needles, each three inches in length.

This instrument is more fully described in the Appendix to Captain Back's Narrative. I have already stated that, in the mode of observing which I recommended to Captain Back, I had particular regard to the economy of time. Considering, therefore, that the operation of inverting the poles of the needle more than doubles the time required for observing without performing it, and, indeed, that it is one which can only be tolerated because generally necessary to counteract imperfections in the instrument employed, I proposed that, in general, this operation should be dispensed with. In order that the dip may be determined independently of this operation, it is necessary that the position of the centre of gravity of the needle employed, with reference to the axis of vibration, should be permanent, and that that position should be ascertained. Owing to the very short interval between the completion of the instruments and the departure of Captain Back, the observations made in London, from which, in conjunction with those to be made at the winter quarters of the expedition, I proposed determining this point, were not so satisfactory as might be desired; and I have consequently been under the necessity of having recourse to the latter alone. It is therefore necessary that in giving an account of the observations for the dip of the needle I should commence with those at Fort Reliance, the winter station.

Dip at Fort Reliance.—Of the two needles No. I. and No. II. belonging to the instrument, No. II. was reserved for determining the dip and intensity at the various stations, and was carefully preserved from all interference with its magnetic state; but with No. I. the dip at Fort Reliance was determined by observations made with its poles direct and likewise inverted. In the following Table are given the means of five observations made with this needle in each position of the instrument, and the mean results of two sets of observations.

Observations of the Dip at Fort Reliance with the Needle No. I.

Date.	Time.	Therm.	Poles of the needle direct.				Poles of the needle reversed.				Means.
			Face of the needle				Face of the needle				
			to the face of instrument.		reversed.		the face of instrument.		reversed.		
			Face of instrument		Face of instrument		Face of instrument		Face of instrument		
			East.	West.	East.	West.	East.	West.	East.	West.	
	h m										
1833. Oct. 10.	11 15 A.M. / 2 15 P.M.	41°·25	84° 19′·5	84° 17′·5	84° 54′	84° 25′	82° 34′·5	86° 9′·5	85° 47′·5	82° 12′	84° 19′ 56″
1834. May 22.	3 10 P.M. / 8 20 P.M.	47·0	85 27	83 27	83 21	85 00	81 1·5	83 44·5	85 38·5	82 0·5	83 42 30
			Mean dip at Fort Reliance.								84 1 13

In a paper "On Improvements in the Instruments and Methods employed in determining the Direction and Intensity of the Terrestrial Magnetic Force," published in the Philosophical Transactions for 1833, I have given the equations

$$\left.\begin{array}{l} w\,g\cos({}_{,}\theta + \gamma) - l\,m\sin({}_{,}\theta - \delta) = 0 \\ w\,g\cos({}'\theta - \gamma) - l\,m\sin({}'\theta - \delta) = 0 \end{array}\right\}*$$

for the determination of the dip, without inverting the poles of the needle, when the position of the centre of gravity of the needle is known. These equations, when the dip is known, determine the position of the centre of gravity. The form, however, in which I have given the expression for the determination of the dip,

$$\tan\delta = \frac{2 - (\cot{}'\theta - \cot{}_{,}\theta)\cot\gamma}{\cot{}'\theta + \cot{}_{,}\theta},$$

is not so convenient for computation as another which it may be made to assume. The above equations may be put in the form,

$$\mathrm{M}\sin({}'\theta - \delta) - \cos({}'\theta - \gamma) = 0 \quad \ldots\ldots\ldots \quad (1.),$$

$$\mathrm{M}\sin({}_{,}\theta - \delta) - \cos({}_{,}\theta + \gamma) = 0 \quad \ldots\ldots\ldots \quad (2.);$$

where M will represent the ratio of the static momentum of the magnetic force acting upon the needle, to the static momentum of its weight, about the axis of motion; δ representing the inclination of the direction of the terrestrial magnetic force to the horizon, or the dip; γ the angle which the line joining the centres of gravity and motion makes with the magnetic axis of the needle; $'\theta$ and $_{,}\theta$ the angles which that axis makes with the horizon, when the centre of gravity is above the axis of motion, and when it is below that axis. From these equations we obtain

$$\mathrm{M}\{\sin({}'\theta - \delta) + \sin({}_{,}\theta - \delta)\} - \{\cos({}'\theta - \gamma) + \cos({}_{,}\theta + \gamma)\} = 0,$$

$$\mathrm{M}\{\sin({}'\theta - \delta) - \sin({}_{,}\theta - \delta)\} - \{\cos({}'\theta - \gamma) - \cos({}_{,}\theta + \gamma)\} = 0.$$

Consequently,

$$\mathrm{M}\,.\cos\frac{{}'\theta - {}_{,}\theta}{2}\,.\sin\left\{\frac{{}'\theta + {}_{,}\theta}{2} - \delta\right\} - \cos\frac{{}'\theta + {}_{,}\theta}{2}\cos\left\{\gamma - \frac{{}'\theta - {}_{,}\theta}{2}\right\} = 0,$$

$$\mathrm{M}\,.\sin\frac{{}'\theta - {}_{,}\theta}{2}\,.\cos\left\{\frac{{}'\theta + {}_{,}\theta}{2} - \delta\right\} - \sin\frac{{}'\theta + {}_{,}\theta}{2}\sin\left\{\gamma - \frac{{}'\theta - {}_{,}\theta}{2}\right\} = 0;$$

or, putting $\mathrm{S} = \frac{{}'\theta + {}_{,}\theta}{2}$ and $\mathrm{D} = \frac{{}'\theta - {}_{,}\theta}{2}$,

$$\mathrm{M}\cos\mathrm{D}\sin(\mathrm{S} - \delta) - \cos\mathrm{S}\cos(\gamma - \mathrm{D}) = 0 \quad \ldots\ldots\ldots \quad (3.),$$

$$\mathrm{M}\sin\mathrm{D}\cos(\mathrm{S} - \delta) - \sin\mathrm{S}\sin(\gamma - \mathrm{D}) = 0. \quad \ldots\ldots\ldots \quad (4.).$$

Hence we have

$$\tan\mathrm{S}\,.\tan(\mathrm{S} - \delta) = \tan\mathrm{D}\,.\cot(\gamma - \mathrm{D}) \quad \ldots\ldots\ldots\ldots \quad (5.);$$

a most convenient equation for computing the value of δ, that of γ being known; or, *vice versâ*, for determining the value of γ from that of δ, to which purpose I shall now apply it, with reference to the observation made at Fort Reliance with the needle No. II.

In the following Table are given the means of five observations made with the

* Philosophical Transactions, 1833, p. 345.

needle No. II. in each position of the instrument, and the resulting mean values of the angles $'\theta$ and $_{\prime}\theta$.

Observations for the Dip at Fort Reliance with the Needle No. II.

Date.	Time.	Therm.	Face of the needle to the face of instrument. Face of instrument East. $_{\prime}\theta$.	Face of the needle to the face of instrument. Face of instrument West. $'\theta$.	Face of the needle reversed. Face of instrument East. $'\theta$.	Face of the needle reversed. Face of instrument West. $_{\prime}\theta$.	Mean values. $'\theta$.	Mean values. $_{\prime}\theta$.
1833. Oct. 9.	h m 2 15 P.M. – 5 40 P.M.	39°	84° 17′	86° 42′	86° 35′ 5	81° 21′·5	86° 38′ 45″	82° 49′ 15″
1834. May 21.	3 30 P.M. – 6 45 P.M.	49·6	83 55·5	86 10	86 8	81 59	86 9 0	82 57 15
			Means of the two sets of observations ……				86 23 53	82 53 15

Taking 84° 1′ as the dip at Fort Reliance, substituting this for δ, and the values in the preceding Table for $'\theta$ and $_{\prime}\theta$, in the equation (5.), we obtain $\gamma = 16° 29' 19''$. By means of this value of γ and the observed values of $'\theta$ and $_{\prime}\theta$, the value of δ, or the dip, is determined at the several stations at which observations were made with the needle No. II., assuming that the position of the axis of the needle was permanent in all these observations. The following Table contains: the means of Captain Back's observations of the direction of the needle, five being made in each position of the instrument, and both the lower and upper reading of the needle being registered; the deduced mean values of $'\theta$ and $_{\prime}\theta$; and the value of δ, or the dip, determined by substituting these mean values of $'\theta$ and $_{\prime}\theta$, and the above value of γ in the equation (5.).

Place of observation.	Latitude North.	Longitude West.	Date.	Observed dip of the needle. Face of the needle to face of instrument. Face of instrument East. $_{\prime}\theta$.	Observed dip of the needle. Face of the needle to face of instrument. Face of instrument West. $'\theta$.	Observed dip of the needle. Face of the needle reversed. Face of instrument East. $'\theta$.	Observed dip of the needle. Face of the needle reversed. Face of instrument West. $_{\prime}\theta$.	Mean values. $'\theta$.	Mean values. $_{\prime}\theta$.	Deduced value of δ or dip.
			1833.							
New York …………	40° 42′ 7″	74° 1′ 15″	April 1.	72° 24′·7	75° 28′·7	73° 47′	73° 16′	73° 37′ 51″	72° 50′ 21″	72° 49′ 18″
Montreal …………	45 29 34	73 42 27	April 19.	77 12	78 28	78 58	76 36·4	78 43 00	76 54 12	77 6 27
Fort Alexander……	50 36 49	96 21 25	June 10.	78 56·25	79 46·25	80 12·9	78 24·4	79 59 35	78 40 19	78 53 37
Cumberland House	53 57 33	102 21 46	July 6.	79 00	82 21	83 23·25	78 27·5	82 53 37	78 43 45	79 29 59
Isle à la Crosse…..	55 25 25	107 54 36	July 17.	79 45	82 2·9	82 40	77 53·5	82 21 28	78 49 15	79 28 24
Fort Chipewyan …	58 42 32	111 19 00	July 31.	80 27	82 38·5	84 14·5	80 6·5	83 26 30	80 16 45	81 00 39
Fort Resolution ..	61 10 26	113 45 00	Aug. 9.	81 38·5	85 11·5	85 29·5	80 7·5	85 20 30	80 53 00	82 3 9
Fort Reliance ……	62 46 29	109 0 39	Oct. 9.	84 17	86 42	86 35·5	81 21·5	86 38 45	82 49 15	84 3 19
			1834.							
			May 21.	83 55·5	86 10	86 8	81 59	86 9 00	82 57 15	83 58 44
Musk-Ox Rapid …	64 40 51	108 8 10	July 2.	85 10	87 36·25	87 35·6	84 28·75	87 35 56	84 49 23	85 53 32
Rock Rapid ………	65 54 18	98 10 7	July 23.	85 27	89 41	89 40·5	86 49	89 40 45	86 8 00	87 39 34
Point Beaufort……	67 41 24	95 2 16	July 31.	86 2	89 24·5	89 45·5	87 39	89 35 00	86 50 30	88 3 15
Montreal Island …	67 47 27	95 18 15	Aug. 2.	86 0	89 19	88 14	87 25·5	88 46 30	86 42 45	87 35 49
Point Ogle ………	68 13 57	94 58 1	Aug. 12.	89 28·5	91 45·5	88 56·5	87 34	90 21 00	88 31 15	89 24 12
Fort Reliance ……	…………	……………	Oct. 9.	84 2	86 30·5	86 27	83 1·5	86 28 45	83 31 45	84 31 24

In this and subsequent Tables I have given the results to the nearest second; not that I, by any means, consider that the dip has been in this, or that it is in any case determined to anything like such a degree of accuracy; but because such results rendered testing the accuracy of calculation more convenient; and that having, in subsequent calculations, employed the actual results obtained, I have thought it right to give them in all cases.

In order to show the care which Captain Back took, in making the observations, to be as free as possible from the effects of any particular local influence, it is necessary that I should give his notes on the situations, with reference to surrounding objects, of the spots on which the observations were made; and it will be proper that I should do so previous to making any remarks on the results contained in the foregoing Table.

New York. "The observations were made in the garden of the British consul under a temporary shed, erected for the purpose, and distant from the house eighty paces."

Montreal. "The observations were made under a tent on the island of St. Helen's, in the St. Lawrence, one thousand yards from the city of Montreal. There was no iron near; the roofs of the houses in the city are most of them covered with tin."

Fort Alexander. There is no note appended to the observations, on the position of the instrument here; but, in his Narrative, Captain Back makes a remark on some discrepancies in the dip of the needle, which he attributes to the influence of a distant thunder storm*.

Cumberland House. "The observations were made in a tent about two hundred yards from the house."

Isle à la Crosse. "The observations were made in a tent forty paces from the stockades of the fort."

Fort Chipewyan. "The observations were made in a tent to the westward of the fort."

Fort Resolution. "This set was made in a tent placed inside of the stockades of the fort, but quite free from the influence of any iron."

It is necessary I should remark, that in this set of observations the differences in the observed angles were considerable, with the face of the needle reversed and the face of the instrument west. Six observations were made in this position; they were as follow: 79° 00′; 79° 17′·5; 79° 55′; 80° 5′; 80° 12′·5; 80° 17′·5. The result given in the eighth column of the Table is the mean of the last four: I rejected the first two, because the others are very accordant and indicate no disposition in the needle to return to its first position. On the subject of these observations Captain Back has made this remark: "At the time the needle changed from 79° 15′ to 80° (lower readings) the weather was more than commonly gloomy, and some few drops of rain fell." During the time of making this whole set of observations "the weather was gloomy, dark, and overcast, with light rain at intervals:" so that the discrepancy here noticed was most probably owing to some change in the electric state of the atmosphere.

* Narrative of the Arctic Land Expedition, p. 41.

Fort Reliance. "These observations were made in my tent, which had a leather circular lodge or wigwam attached to it, in which was a small fire. The ground on which the stand was placed was gravel, about one hundred yards from the lake."

Rock Rapid. "These observations were made as before in the tent, the theodolite stand being placed on shingle, at the foot of a high gneiss rock."

Point Beaufort. "The observations made as usual; the stand on shingle at the base of a gneiss rock three to four hundred feet high."

Montreal Island. "The stand was placed on firm sand about sixty yards from some low rocks, and a sail was put over the tent to afford more shade."

Point Ogle. "The stand was firmly fixed in sand and shells, of which the beach was composed, thirty paces from the beach, which was packed with ice."

Independently of the accuracy with which the observations were made, and of the precautions taken to exclude the effects of any particular local influence upon the needle, it is evident that the correctness of the results deduced from them in the foregoing Table must depend upon the permanence of the angle γ, and upon a correct value having been assigned to that angle. It is therefore necessary to inquire whether any tests can be applied to these observations which may indicate the extent of the errors by which these results may be affected.

Values being assigned to γ and δ in the equations (1.) and (2.) or (3.) and (4.), these equations furnish measures of the terrestrial magnetic intensity, viz.

$$M = \frac{\cos({}'\theta - \gamma)}{\sin({}'\theta - \delta)}, \qquad \text{or } M = \frac{\cos({}_{,}\theta + \gamma)}{\sin({}_{,}\theta - \delta)} \quad . \; . \; . \; . \; . \; . \; . \quad (6.);$$

$$M = \frac{\cos S}{\cos D} \cdot \frac{\cos(\gamma - D)}{\sin(S - \delta)}, \text{ or } M = \frac{\sin S}{\sin D} \cdot \frac{\sin(\gamma - D)}{\cos(S - \delta)} \quad . \; . \; . \; . \; . \; . \quad (7.)$$

In order, however, that the values of M, thus determined, should be as little as possible affected by errors of adjustment of the instrument or needle, it is necessary that the centre of gravity of the needle should be distant from the axis of motion, and that the angle γ should be large*. The needle with which these observations were made not having been constructed for this purpose, but with the view of determining the variations in the terrestrial intensity by means of its times of vibration, its centre of gravity was made to coincide as nearly as possible with its axis of vibration. In determining therefore the values of M from the equations (6.) by means of it, we are not to expect very close approximations to the values derived from other principles. Admitting this, however, the results ought not in any case to be quite discordant. In the following Table I have given the values of M deduced from the equations (6.), assuming the value of γ to be 16° 29′ 19″; and in the following column, for the purpose of comparison, the relative values of M deduced from the times of vibration of the needle No. II. in the plane of the meridian†.

* Philosophical Transactions, 1833, p. 348.

† The observations from which these are deduced are given in a subsequent part of this paper.

Place of observation.	Relative intensity $M = \frac{\cos('\theta - \gamma)}{\sin('\theta - \delta)}$, or $M = \frac{\sin S . \sin(\gamma - D)}{\sin D \cos(S - \delta)}$	Relative intensity, or value of M deduced from the time of vibration of the needle No. II.
New York	38·41	8·140936
Montreal	16·60	8·3 ?
Fort Alexander	23·25	8·569610
Cumberland House	6·76	8·409142
Isle à la Crosse	8·12	8·278406
Fort Chipewyan	9·23	8·324042
Fort Resolution	6·29	8·612853
Fort Reliance	8·26702	8·26702
Musk-Ox Rapid	10·73	8·169870
Rock Rapid	8·20	8·327742
Point Beaufort	10·90	8·041422
Montreal Island	14·80	8·154152
Point Ogle	16·82	8·277498
Fort Reliance, Oct. 9, 1834.	10·03	8·269714

Making every allowance for the want of adaptation of the needle to this method of determining the relative intensities, for errors of observation in determining the times of vibration of the needle, and for any disturbing causes affecting these observations, such differences are here exhibited in the results obtained by the two methods, at New York, Montreal, Fort Alexander, Montreal Island, and Point Ogle, as can only be accounted for by errors in the assumed value of the angle γ, and clearly indicate a want of permanence in that angle. It therefore becomes necessary to inquire what changes in the angle γ will account for these discrepancies, and how far the dip may be affected. For this purpose either γ or δ must be eliminated from the equations (3.) and (4.), and the value of the other determined in terms of M.

Putting these equations in the form

$$\left.\begin{array}{l} M^2 \cos^2 D . \sin^2 (S - \delta) = \cos^2 S . \cos^2 (\gamma - D) \\ M^2 \sin^2 D . \cos^2 (S - \delta) = \sin^2 S . \sin^2 (\gamma - D) \end{array}\right\},$$

we obtain immediately

$$M^2 \{\sin^2 S . \cos^2 D - \cos^2 S . \sin^2 D\} . \sin^2 (S - \delta) = \sin^2 S . \cos^2 S - M^2 \cos^2 S . \sin^2 D,$$

$$M^2 \{\sin^2 S . \cos^2 D - \cos^2 S . \sin^2 D\} . \cos^2 (S - \delta) = M^2 \sin^2 S . \cos^2 D - \sin^2 S . \cos^2 S.$$

Putting $P = M . \frac{\sin D}{\sin S}$, and $Q = M . \frac{\cos D}{\cos S}$, these equations become

$$M^2 \sin '\theta \sin {}_{'}\theta \sin^2 (S - \delta) = \frac{1}{4} \sin^2 2 S (1 - P^2) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8.),$$

$$M^2 \sin '\theta \sin {}_{'}\theta \cos^2 (S - \delta) = \frac{1}{4} \sin^2 2 S (Q^2 - 1) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9.);$$

whence,

$$\tan (S - \delta) = \sqrt{\left\{\frac{1 - P^2}{Q^2 - 1}\right\}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10.)*$$

The equation (5.) gives

$$\cot (\gamma - D) = \frac{Q}{P}\sqrt{\left\{\frac{1 - P^2}{Q^2 - 1}\right\}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11.);$$

or, putting $P_{,} = \frac{1}{P}$ and $Q_{,} = \frac{1}{Q}$,

$$\cot (\gamma - D) = \sqrt{\left\{\frac{P_{,}^2 - 1}{1 - Q_{,}^2}\right\}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11_{,}.)$$

Assuming that the values of M deduced from the time of vibration of the needle at the several stations are the correct values, and substituting them in the equation (10.), I have computed the values of $S - \delta$, $\gamma - D$, and thence determined the dip and the angle γ at these stations. The results are arranged in the following Table.

Place of observation.	Value of M deduced from the time of vibration of the needle No. II.	Value of $\gamma - D$ deduced from the foregoing value of M.	Value of $S - \delta$ deduced from the foregoing value of M.	Resulting value of γ.	Resulting value of δ.	Value of δ deduced from the constant value $\gamma = 16\ 29' 19''$.	Difference between the two values of δ.
		° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″
New York	8·140936	3 21 55	2 1 37	3 45 40	71 12 29	72 49 18	+1 36 49
Montreal	8·3 ?	7 43 10	1 26 41	8 37 34	76 21 55	77 6 27	+0 44 32
Fort Alexander	8·569610	5 46 7	1 13 54	6 25 45	78 6 3	78 53 37	+0 47 34
Cumberland House	8·409142	18 1 36	1 2 7	20 6 31	79 46 34	79 29 59	−0 16 35
Isle à la Crosse	8·278406	15 0 21	1 5 37	16 46 28	79 29 45	79 28 24	−0 1 21
Fort Chipewyan	8·324042	13 24 54	0 56 54	14 59 47	80 54 44	81 00 39	+0 5 55
Fort Resolution	8·612853	19 43 10	0 45 6	21 56 55	82 21 39	82 3 9	−0 18 30
Fort Reliance, October 9, 1833, and May 21, 1834	8·267020	14 44 0	0 37 34	16 29 19	84 1 0	84 1 0	0 0 0
Musk-Ox Rapid	8·169870	11 26 22	0 27 16	12 49 39	85 45 24	85 53 32	+0 8 8
Rock Rapid	8·327742	14 56 30	0 14 35	16 42 53	87 39 48	87 39 34	−0 0 14
Point Beaufort	8·041422	11 5 49	0 13 5	12 28 4	87 59 40	88 3 15	+0 3 35
Montreal Island	8·154152	8 26 47	0 16 25	9 55 17	87 27 13	87 35 49	+0 8 36
Point Ogle	8·277498	7 35 38	0 4 3	8 30 31	89 22 5	89 24 12	+0 2 7
Fort Reliance, Oct. 9, 1834	8·269714	12 20 15	0 35 23	13 48 45	84 24 52	84 31 24	+0 6 32

The differences between the values of γ in the above Table, deduced from the equation (11.), and its assumed value, are certainly in many cases considerable; and the dip also, in some, differs considerably from that previously deduced. It is, however, to be remarked with regard to these differences, that the above values of γ are deduced from the assumption that no other sources of error existed in the instrument than the want of permanence in the axis of the needle itself. Those who have been most in the habit of making observations for determining the dip, will be best able

* Either of the equations (8.) or (9.) gives a convenient expression for the calculation of $S - \delta$, viz. $\sin (S - \delta) = \frac{\sin 2S}{2M}\sqrt{\frac{1 - P^2}{\sin '\theta \sin_{,}\theta}}$, or $\cos (S - \delta) = \frac{\sin 2S}{2M}\sqrt{\frac{Q^2 - 1}{\sin '\theta \sin_{,}\theta}}$; but the equation (10.) is even more so. It might appear that the equation (10.) would be better adapted for logarithmic computation, if put in the form $\tan (S - \delta) = \sqrt{\frac{(1 + P) \cdot (1 - P)}{(Q + 1) \cdot (Q - 1)}}$; but this is not the case, since the computation which would determine P determines P^2 by simply doubling the logarithm; and besides this, the same opening of the table by which Q^2 would be found, gives the logarithm of $Q^2 - 1$.

to judge how far this is likely to be the case in any dipping instrument; I can only say that in the observations which I have myself made, and in those of others which I have examined, I have rarely considered that the observed angles could be relied upon, as being absolutely those which the axis of the needle would make with the horizon, if perfectly free, in any assumed position; and even when a mean of several observations has been taken, I have in general felt that it might differ from the truth by several minutes. I do not therefore consider that all the discrepancies in the relative terrestrial intensities can properly be attributed to the want of permanence in the axis of the needle, though no doubt some, those which are the greatest, are principally attributable to this cause. It may, however, be proper to inquire what errors in the observed values of $'\theta$ and $_{,}\theta$ will account for these discrepancies, as we shall thus be enabled to judge whether they may not in some cases be ascribed to this cause. It however unfortunately happens that in this inquiry a knowledge of the dip itself is absolutely necessary; and it is only by assuming that which has been determined by means of the values $'\theta$ and $_{,}\theta$, now supposed to be erroneous, that we can determine the changes requisite in the values to account for the discrepancies in the relative intensities.

The equations (1.) and (2.) give immediately,

$$\cot {'\theta} = \frac{\mathrm{M} \cos \delta - \sin \gamma}{\mathrm{M} \sin \delta + \cos \gamma} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12.)$$

$$\cot {_{,}\theta} = \frac{\mathrm{M} \cos \delta + \sin \gamma}{\mathrm{M} \sin \delta + \cos \gamma}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13.)$$

Assuming then the values of M already given as deduced from the times of vibration of the needle, the dips as first deduced from the observations, and the constant value $\gamma = 16° \, 29' \, 19''$, the values of $'\theta$ and $_{,}\theta$ in the following Table are deduced from these equations.

Place of observation.	Value of M deduced from the time of vibration of the needle No. II.	Dip first deduced or value of δ.	Value of $'\theta$ computed from the equation (12.).	Observed value of $'\theta$.	Resulting error in the observed value of $'\theta$.	Value of $_{,}\theta$ computed from the equation (13.).	Observed value of $_{,}\theta$.	Resulting error in the observed value of $_{,}\theta$.
New York..............	8·140936	72° 49′ 18″	76° 21′ 23″	73° 37′ 51″	+2° 43′ 32″	72° 53′ 48″	72° 50′ 21″	+0° 3′ 27″
Montreal	8·3 ?	77 6 27	80 10 10	78 43 00	+1 27 10	76 43 16	76 54 12	−0 10 56
Fort Alexander	8·569610	78 53 37	81 41 53	79 59 35	+1 42 18	78 19 54	78 40 19	−0 20 25
Cumberland House...	8·409142	79 29 59	82 17 38	82 53 37	−0 35 59	78 51 53	78 43 45	+0 8 8
Isle à la Crosse.........	8·278406	79 28 24	82 18 34	82 21 28	−0 2 54	78 49 54	78 49 15	+0 0 39
Fort Chipewyan	8·324042	81 00 39	83 40 48	83 26 30	+0 14 18	80 12 29	80 16 45	−0 4 16
Fort Resolution	8·612853	82 3 9	84 32 23	85 20 30	−0 48 7	81 9 59	80 53 00	+0 16 59
Musk-Ox Rapid	8·169870	85 53 32	88 6 16	87 35 56	+0 30 20	84 32 58	84 49 23	−0 16 25
Rock Rapid	8·327742	87 39 34	89 39 9	89 40 45	−0 1 36	86 9 13	86 8 00	+0 1 13
Point Beaufort.........	8·041422	88 3 15	90 4 7	89 35 00	+0 29 7	86 27 28	86 50 30	−0 23 2
Montreal Island	8·154152	87 35 49	89 38 5	88 46 30	+0 51 35	86 4 9	86 42 45	−0 38 36
Point Ogle	8·277498	89 24 12	91 13 34	90 21 00	+0 52 34	87 42 22	88 31 15	−0 48 53

The differences here shown between the observed and computed values of the angle $_{,}\theta$ are, with a few exceptions, within the limits of the errors of dip observations; but those of the angle $'\theta$ are by no means so, excepting in a few instances. Upon the whole we must therefore conclude, that the discrepancies which appear between the values of the terrestrial intensity, as deduced from the times of vibration of the needle, and from the observed angles of inclination to the horizon, are principally attributable to a want of absolute permanence in its axis of motion. With respect to the amount of the change in this axis, it is to be observed, that in this needle the centre of gravity was nearly coincident with the axis; and, consequently, that a very minute derangement of the axis would cause a considerable change in the value of the angle γ. We must not therefore infer, in consequence of the differences in the values of this angle exhibited in the preceding Table, that the needle itself received any serious injury during the expedition. To such it could not be liable, being, during the whole expedition, under the personal charge of Captain BACK, who, aware of the importance of preserving this and other needles as nearly as possible in the same state, carried them himself with the same care as he bestowed on the chronometers.

Having now fully discussed the observations which Captain BACK made with the dipping needle, it is proper that I should state how far I consider that the dip is determined by them at the several stations. If we refer to the Table at p. 384, it will be seen that, excepting the New York, Montreal, and Fort Alexander observations, the differences between the dips as determined by means of the constant value assumed for the angle γ, and as determined from the relative terrestrial intensities, deduced from the times of vibration of the same needle, are generally much within the limits of the errors to which observations of the dip, with our present instruments, are generally found liable. I think therefore that we are to consider the results not only as entitled to the confidence which is generally given to those deduced from observations carefully made with good instruments, but that the differences in the last column of the table, in general, fairly exhibit the amount of the error in each case, by which the result may be affected. In the cases of New York, Montreal, and Fort Alexander this is so considerable that much uncertainty must attach to the results determined by means of the constant value assumed for the angle γ. With respect to the dip at these places as determined by means of the intensity, it may be considered that the same degree of uncertainty may not attach. It is, however, singular that the results at New York and Fort Alexander in this case differ more widely from preceding determinations than in the other. Sir JOHN FRANKLIN* determined the dip at New York in 1825 to be 73° 27′ 3″, which differs only 37′ 45″ from the dip determined from Captain BACK's observations by means of the constant value of γ, but differs 2° 14′ 34″ from the result obtained by means of the intensity. At Fort Alexander Sir JOHN FRANKLIN's determination of the dip in 1825 was 78° 47′ 8″, differing only − 5′ 29″ from the result from Captain BACK's observations in the first case, but + 41′ 5″ from the result in the second. In December 1822 Captain SABINE determined the dip at New York to be† 73° 5′ by means of a MEYER's needle, and

* Narrative of a Second Expedition to the Shores of the Polar Sea, Appendix, p. cxxxvi.

† An Account of Experiments to determine the Figure of the Earth, &c., p. 474.

72° 55′* by the times of vibration in the meridian and at right angles to it, which differ but little from the first determination from Captain BACK's observations.

Looking to the geographical positions of some of the stations, Rock Rapid, Point Beaufort, Montreal Island, and Point Ogle, we should be led to infer that the small difference in the dip at Rock Rapid, Point Beaufort, and Montreal Island, and the considerable difference between the dip at the two latter stations and that at Point Ogle, must be due to errors of observation, or to some local cause influencing the direction of the needle. The results at all the other stations would lead us to expect, at Point Beaufort and Montreal Island, a dip but little under 89°; and it will be seen that the observations for determining the intensity by the times of vibration of horizontal needles, indicate that such should be nearly its amount.

Independently of the reduction of Captain BACK's observations, and ascertaining the degree of reliance which might be placed on the results I have deduced, I had another and more general object in the long discussion into which I have entered, that of pointing out the kind of tests which may be applied to dip observations, in order to ascertain whether the observations are consistent with themselves, and by this means whether the results deduced from them are worthy of confidence. I have not yet had leisure to enter upon such an inquiry with many observations; but in some which I have examined I find far greater discrepancies in values of the angle γ than the greatest which I have noticed in the present instance. On some future occasion I may perhaps enter more fully upon such an inquiry, but for the present I shall leave it, and proceed with Captain BACK's observations.

II. *Observations of the Variation of the Magnetic Needle.*

As these observations are published in Captain BACK's Narrative, it would be superfluous to introduce them here in the form of a table, particularly as they are given in a subsequent table, in which I have instituted a comparison between results deduced from them, in conjunction with the dip observations which I have detailed, and certain theoretical results, with the view of ascertaining how far these observations tend to support the theory. I shall therefore here simply refer to the table at p. 390 for the variation at the different stations of observation.

III. *Comparison of the Observations of the Dip and Variation of the Needle with theoretical results.*

On the hypothesis of two magnetic poles symmetrically situated in a diameter of the earth and near to its centre, the poles of verticity and of convergence will coincide, and the tangent of the dip will be equal to twice the tangent of the magnetic latitude. Although, viewing all the phenomena on the whole surface of the earth, such an hypothesis is clearly inadequate to their explanation, it is interesting to inquire how far it may be consistent with those observed on a limited portion. For such an inquiry,

* An Account of Experiments to determine the Figure of the Earth, &c., p. 476.

the observations made by Captain Back are peculiarly well adapted, since in no case has so direct a progress been made, to such an extent, towards the magnetic pole, whether we consider that point as the point of convergence of magnetic meridians, or that at which the direction of the force is vertical. Assuming then the coincidence of these points, I had proposed inquiring whether the differential of the dip and the differential of the magnetic latitude, taking the increments in observations not very distant from each other to express these differentials, had the ratio which the theory gave, and had indeed made the requisite computations and comparisons; but on further consideration it appeared to me that this comparison was not so good a test of the theory as the more direct one of the dip itself with the magnetic latitude, since small errors in the dip would become very sensible in the value of the differential, and if these errors happened to conspire at two stations, this value might be doubled or reduced one half. I do not propose in the first instance giving the detail of that comparison, but it may be proper to mention the general nature of the results.

If λ represents the magnetic latitude, and δ the dip, then the equation

$$\tan \delta = 2 \tan \lambda$$

gives

$$\frac{d\,\delta}{d\,\lambda} = \frac{3 \cos 2\,\delta + 5}{4}.$$

In comparing the observations with this formula, I took the sum of the dips at two stations as the value of $2\,\delta$. The first observations which I employed in this comparison were those at Musk-Ox Rapid and Rock Rapid; and here the agreement was so marked that I certainly anticipated a general close agreement of the observations with the theory. This, however, was the only instance of that agreement which we ought to expect in such cases, if a theory be correct.

As the magnetic polar distances of two stations are determined from their geographical position and the observed variations of the needle by the solution of two spherical triangles, it would be superfluous to point out the course of calculation which I adopted; but it is necessary that I should state the nature of the comparison which I propose instituting between the results of theory and observation.

If φ is the magnetic polar distance of one station, and $\varphi_{,}$ that of another, the dip at the two stations being δ and $\delta_{,}$, then if the theory and the observations be both correct we shall have

$$\tan \delta \, . \tan \varphi = 2 \quad \text{and} \quad \tan \delta_{,} \tan \varphi_{,} = 2 \quad . \; . \; . \; . \; . \; . \; . \quad (15.);$$

and we may judge, by the approximation of these products in all cases to the number 2, of the degree of coincidence between the theory and the observations.

In the third column of the following Table are given the distances of the several stations from the magnetic pole of convergence, whose position is determined from the variations in the second column at the respective stations, combined as indicated in the first column; in the fourth column, the dip of the needle at these stations, as already determined from the constant value of the angle γ; in the fifth column, the numerical value of the product $\tan\delta . \tan\varphi$; and in the sixth column, the differences between this product and the number 2, that is, the error of the theoretical result. In the three following columns are given, first, the value of $\frac{d.\delta}{d.\lambda}$, deduced from the observations; secondly, the value of $\frac{3\cos 2\delta + 5}{4}$, to which the former should, according to the theory, be equal; and thirdly, the difference between the values of these fractions, or the error of the theoretical result. In this Table I have not included the observations at Point Ogle, because, from the greatly diminished horizontal force acting upon the needle, I do not consider that the variation could have been determined with any precision; indeed, the amount of the daily variation, in such a position, would, supposing that the variation could be accurately determined at any instant, render the hour at which the observation was made a matter of the first importance, the position of the magnetic pole resulting from an observation made at one period of the day being necessarily very different from that deduced from observations made at others *. This remark would in a great degree apply to the observations at Point Beaufort and Montreal Island, but these places are more conveniently situated with respect to Rock Rapid, for a comparison of the observations, than Point Ogle: indeed, the variation at Rock Rapid would assign a position to the south of Point Ogle for that of the magnetic pole, which position is quite at variance with the observations at the latter station.

* This is a consideration which does not appear to have occurred either to Sir John Ross or Captain James Ross in assigning a position to the magnetic pole. Taking a mean of Sir John Ross's daily variations at Victory Harbour for the month of April 1830, we have the variation at noon 100° 53′ W., and at midnight 85° 22′ W.; giving a diurnal variation of more than 15°. Assuming the dip here 88° 55′, as determined by Captain James Ross, the distance of the pole of verticity from Victory Harbour would be 2° 9′ 57″ nearly: consequently the situation of the pole at midnight would be 35′ 5″, or rather more than 40 miles distant from its position at noon. It appears to have been considered that the *true* position of the magnetic pole has been determined within much narrower limits than such an interval. Taking this view of the subject, it may be an inquiry worth entering upon, to ascertain whether the extent of the diurnal variations observed by Captain Foster at Port Bowen corresponds to the same orbit, if I may use the expression, of the magnetic pole on the earth's surface as that observed by Captain Back at Fort Reliance; whether these correspond with the orbit which would result from the diurnal variation given by Sir John Ross; and also whether the several times of the maxima and minima are in accordance with the same motion, whether uniform or not, of the pole in one orbit. This is an inquiry upon which my present engagements do not admit of my now entering, but I propose doing so as soon as I have the requisite leisure.

Places from the observations at which the position of the pole of convergence is determined *.	Observed variation of the needle.	Magnetic polar distance.	Dip.	Value of $\tan \delta . \tan \varphi$.	Error, or value of $\tan \delta . \tan \varphi - 2$.	Value of $\frac{d . \delta}{d . \lambda}$.	Value of $\frac{3 \cos 2 \delta + 5}{4}$.	Error, or value of $\frac{d . \delta}{d . \lambda} - \frac{3 \cos 2 \delta + 5}{4}$.
Fort Alexander	15° 16′ E.†	28° 22′ 43″	78° 53′ 37″	2·7519	+ 0·7519	·2874	·5527	− ·2653
and Cumberland House	19 14 E.†	26 16 11	79 29 59	2·6630	+ 0·6630			
Cumberland House …	19 14 E.†	22 20 28	79 29 59	2·2173	+ 0·2173	−·1316	·5499	− ·4183
and Isle à la Crosse …	23 19 E.†	22 8 26	79 28 24	2·1897	+ 0·1897			
Isle à la Crosse …	23 19 E.†	32 13 34	79 28 24	3·3931	+ 1·3931	·6921	·5431	+ ·1490
and Fort Chipewyan …	25 30 E.†	30 0 16	81 0 39	3·6504	+ 1·6504			
Fort Chipewyan …	25 30 E.†	21 19 2	81 0 39	2·4668	+ 0·4668	·6244	·5325	+ ·0919
and Fort Resolution …	29 15 E.†	19 38 56	82 3 9	2·5575	+ 0·5575			
Fort Chipewyan ..	25 30 E.†	10 6 50	81 0 39	1·1276	− 0·8724	·7044	·5325	+ ·1719
and Fort Resolution …	37 20 E.	8 38 6	82 3 9	1·0878	− 0·9122			
Fort Resolution …	37 20 E.	9 4 53	82 3 9	1·1449	− 0·8551	·7521	·5220	+ ·2301
and Fort Reliance …	35 19 E.	6 28 11	84 1 0	1·0770	− 0·9230			
Fort Reliance ..	35 19 E.	7 13 11	84 1 0	1·2087	− 0·7913	·9148	·5116	+ ·4032
and Musk-Ox Rapid ..	44 24 E.	5 30 14	85 53 32	1·3417	− 0·6583			
Musk-Ox Rapid …	44 24 E.	7 52 46	85 53 32	1·9271	− 0·0729	·5205	·5047	+ ·0158
and Rock Rapid …	29 16 E.	4 29 4	87 39 34	1·9188	− 0·0812			
Rock Rapid …	29 16 E.	2 21 16	87 39 34	1·0059	− 0·9941	·1868	·5021	− ·3153
and Point Beaufort ..	6 00 W.‡	0 14 28	88 3 15	0·1239	− 1·8761			
Rock Rapid …	29 16 E.	2 29 56	87 39 34	1·0677	− 0·9323	−·0288	·5025	− ·5313
and Montreal Island …	2 00 W.	0 19 30	87 35 49	0·1352	− 1·8748			
Fort Alexander …	15 16 E.†	27 55 38	78 53 37	2·7003	+ 0·7003	·5096	·5411	− ·0315
and Fort Resolution …	29 15 E.†	21 43 42	82 3 9	2·8546	+ 0·8546			
Fort Alexander …	15 16 E.†	23 9 47	78 53 37	2·1794	+ 0·1794	·5909	·5411	+ ·0498
and Fort Resolution …	37 20 E.	17 49 1	82 3 9	2·3021	+ 0·3021			
Fort Reliance …	35 19 E.	10 53 26	84 1 0	1·8357	− 0·1643	·7017	·5079	+ ·1938
and Rock Rapid …	29 16 E.	5 41 56	87 39 34	2·4415	+ 0·4415			

By an inspection of the numbers in the sixth column of this Table, which indicate the errors of the theoretical result, it will be seen that, with very few exceptions, there is not that accordance between the observations and the theory which, for the establishment of the theory, we ought to look for; and that they rather indicate that although the theory may be true as a first approximation, yet it requires considerable modification to render it accordant with the observations. The numbers in the ninth

* I have not included the observations at New York and Montreal in this table, on account of the uncertainty which appears to attend the determination of the variation at those cities. The variation at New York, according to the Admiralty Chart, is 2° 30′ W. Captain Bayfield found the variation at Vercheres, about twenty miles below Montreal, to be 10° 30′ W.; and we may from this assume 10° W. as the variation nearly at Montreal. These variations would give the place of the pole of convergence 7° 56′ from New York, and 3° 11′ from Montreal. Such a result is so totally at variance with all others, that it would be absurd to institute any comparison between conclusions drawn from it and any theoretical results. There can, I think, be no doubt that either the variation at New York is more than 2° 30′ W., or that at Montreal less than 10° W., or that each of these is erroneous, the one in defect, the other in excess. This is one among many instances of the very vague determination of an important element in terrestrial magnetism at places where we might expect that it would have been determined with considerable precision; and very forcibly points out the necessity of more accurate observations of the variation than we at present possess.

† Variations observed in 1825 by Sir John Franklin. Captain Back did not observe the variation at those stations in the present expedition. The other variations are those observed by Captain Back in this expedition.

‡ In the variations which Captain Back sent me, he has marked uncertain against this. The same uncertainty must apply to the variation at Montreal Island and Point Ogle. At Montreal Island the morning observation gave the variation 2° 43′ E., and the afternoon one 6° 42′ W.; the mean 2° W. At Point Ogle the variation by the morning observation was 1° 52′ E., and by the afternoon one 1° 46′ W.; "the sun's bearing at noon was first 180°, and after tapping the compass 183° 30′."

column point to the same conclusions. It must however be borne in mind that the variations at Fort Alexander, Cumberland House, Isle à la Crosse, and Fort Chipewyan, which determine the positions of the pole of convergence in these cases, are those observed by Sir John Franklin in 1825, and that the dips at these places, which determine the situation of the pole of verticity, are those resulting from the observations of Captain Back in 1834: so that these observations cannot be considered as strictly comparative. This objection cannot, however, be urged against the observations from Fort Resolution to the sea, which give results equally discordant with the theory.

We have already seen that the dip at the several stations, as determined from the constant value of the angle γ, differs, and in some cases considerably, from that deduced from the value of that angle determined by means of the relative intensities. It may therefore be satisfactory to inquire whether, by employing the dips obtained from these values of the angle γ, we shall obtain results more accordant with theory. In the following Table I have arranged the results in the same form as in the preceding, the variations and magnetic polar distances employed being those there given.

Places from the observations at which the position of the pole of convergence is determined.	Observed variation of the needle.	Magnetic polar distance.	Dip.	Value of $\tan\delta.\tan\varphi$.	Error, or value of $\tan\delta.\tan\varphi-2$.	Value of $\frac{d.\delta}{d.\lambda}$.	Value of $\frac{3\cos 2\delta+5}{4}$.	Error, or value of $\frac{d.\delta}{d.\lambda}-\frac{3\cos 2\delta+5}{4}$.
Fort Alexander	15° 16′ E.	28° 22′ 43″	78° 6′ 3″	2·5815	+ 0·5815	·7944	·5552	+ ·2392
and Cumberland House	19 14 E.	26 16 11	79 46 34	2·7366	+ 0·7366			
Cumberland House ...	19 14 E.	22 20 28	79 46 34	2·2786	+ 0·2786	−1·3975	·5485	− 1·9460
and Isle à la Crosse ...	23 19 E.	22 8 26	79 29 45	2·1944	+ 0·1944			
Isle à la Crosse ...	23 19 E.	32 13 34	79 29 45	3·4005	+ 1·4005	·6375	·5309	+ ·1066
and Fort Chipewyan ..	25 30 E.	30 0 16	80 54 44	3·6101	+ 1·6101			
Fort Chipewyan ...	25 30 E.	21 19 2	80 54 44	2·4396	+ 0·4396	·8683	·5317	+ ·3366
and Fort Resolution ...	29 15 E.	19 38 56	82 21 39	2·6621	+ 0·6621			
Fort Chipewyan ...	25 30 E.	10 6 50	80 54 44	1·1152	− 0·8848	·9795	·5317	+ ·4478
and Fort Resolution ..	37 20 E.	8 38 6	82 21 39	1·1322	− 0·8678			
Fort Resolution ...	37 20 E.	9 4 53	82 21 39	1·1917	− 0·8083	·7863	·5198	+ ·2665
and Fort Reliance ...	35 19 E.	6 28 11	84 24 52	1·1596	− 0·8404			
Fort Reliance ...	35 19 E.	7 13 11	84 24 52	1·2953	− 0·7047	·7823	·5110	+ ·2713
and Musk-Ox Rapid ...	44 24 E.	5 30 14	85 45 24	1·2987	− 0·7013			
Musk-Ox Rapid ...	44 24 E.	7 52 46	85 45 24	1·8653	− 0·1347	·5616	·5047	+ ·0569
and Rock Rapid ...	29 16 E.	4 29 4	87 39 48	1·9220	− 0·0780			
Rock Rapid ...	29 16 E.	2 21 16	87 39 48	1·0076	− 0·9924	·1567	·5022	− ·3455
and Point Beaufort ...	6 00 W.	0 14 28	87 59 40	0·1201	− 1·8799			
Rock Rapid ...	29 16 E.	2 29 56	87 39 48	1·0695	− 0·9305	− ·0965	·5027	− ·5992
and Montreal Island ...	2 00 W.	0 19 30	87 27 13	0·1275	− 1·8725			
Fort Alexander ...	15 16 E.	27 55 38	78 6 3	2·5156	+ 0·4844	·6872	·5432	+ ·1440
and Fort Resolution ...	29 15 E.	21 43 42	82 21 39	2·9713	+ 0·9712			
Fort Alexander ...	15 16 E.	23 9 47	78 6 3	2·0304	+ 0·0304	·7968	·5432	+ ·2536
and Fort Resolution ...	37 20 E.	17 49 1	82 21 39	2·3962	+ 0·3962			
Fort Reliance ...	35 19 E.	10 53 26	84 24 52	1·9673	− 0·0327	·6258	·5072	+ ·1186
and Rock Rapid ...	29 16 E.	5 41 56	87 39 48	2·4456	+ 0·4456			

Although the differences here shown between the results of theory and those deduced from the observations are in some cases less than in the preceding Table, yet in others they are greater; and the comparison does not upon the whole show a nearer coincidence. If this want of coincidence is to be attributed to errors in the observations, I think that the two comparisons which I have instituted indicate errors in the observed variations rather than in the dips of the needle, which have been deduced from the observations. The character of the differences between the theory and the observations, independently of their magnitude, strongly corroborates this conclusion. In all the results deduced from the observations at stations from Fort Alexander to Fort Resolution, in deducing which the variations observed by Sir JOHN FRANKLIN in 1825 have necessarily been employed, these differences indicate that the pole of convergence is more remote from the place of observation than the pole of verticity; whereas, in those deduced from observations at stations from Fort Resolution to Point Beaufort, in deducing which both the dip and variation are derived from Captain BACK's observations, these differences indicate precisely the reverse. If, however, we are not to suppose considerable errors in the observations of either the dip or variation of the needle, this comparison clearly indicates that the theoretical magnetic pole of verticity does not coincide with the pole of convergence, even when the positions of these points are deduced from observations made at very limited distances from them.

Having compared the results deduced from Captain BACK's very interesting observations with some of those derived from theory, I might now proceed to point out the actual positions which these observations, variously combined, would assign to the northern magnetic pole of convergence, and also those which, according to this theory, they would give to the pole of verticity. As, however, I propose investigating this subject with reference not only to Captain BACK's but also to other observations made in the same regions, I will reserve this branch of the inquiry for another communication.

IV. *Intensity of the Terrestrial Magnetic Force.*

The observations which were made by Captain BACK for the determination of the magnetic intensity were of two kinds, viz. by the time of vibration of a needle in the plane of the magnetic meridian, and by the times of vibration of three needles suspended horizontally, according to HANSTEEN's method.

I shall first discuss the observations which were made with the needle vibrated in the plane of the magnetic meridian. This needle is that with which the observations for the determination of the dip without the reversion of the poles was made, No. II. belonging to the dipping instrument, in which instrument its times of vibration were determined. The times of vibration were determined with the face of the instrument east and with its face west, the face of the needle being towards the face of the instrument; and also, where time admitted, similar observations were made with the needle reversed on its axis. In some cases two sets of observations were made in each position of the needle. The following Table contains the results of the observations which were made at various stations, and, in all cases, on the same spot as the corresponding observations for the dip, the two sets of observations having been made consecutively.

Place at which the observations were made.	Date.	Time of day nearly.	Face of the needle to the face of the instrument. Face of the instrument East. Semi-arc of vibration. Begin^g.	East. Semi-arc Ending.	East. No. of vibrations.	East. Time of vibrating.	West. Semi-arc of vibration. Begin^g.	West. Semi-arc Ending.	West. No. of vibrations.	West. Time of vibrating.	Mean time of one vibration.	Therm., mean of, at beginning and ending.	Face of the needle reversed. Face of the instrument East. Semi-arc of vibration. Begin^g.	East. Semi-arc Ending.	East. No. of vibrations.	East. Time of vibrating.	West. Semi-arc of vibration. Begin^g.	West. Semi-arc Ending.	West. No. of vibrations.	West. Time of vibrating.	Mean time of one vibration.	Therm., mean of, at beginning and ending.	Mean time of one vibration in the meridian.	Therm. Mean.
	1833.	h m	°	° ′		s	°	° ′		s	s		° ′	° ′		s	° ′	° ′		s	s		s	°
London	Feb. 9.	1 30 P.M. – 3 00	43	1 20	80	109·5	30	2 15	80	108·5	1·3625	......	40 0	7 30	80	112·5	34 30	5 0	80	112·0	1·4031	......	1·3828	50
New York	April 1.	3 30 P.M. – 4 00	30	3 0	70	91·0	30	3 0	70	89·0	1·2857	69°	Observations were not made in this position of the needle.											
Fort Alexander ...	June 10.	1 13 P.M. – 2 8	30	2 0	110	136·0	30	2 0	108	135·0	1·2432	70·5	32 0	2 0	90	112·0	30 0	2 0	110	141·0	1·2631	72·25	1·25315	71·4
Cumberland House	July 6.	2 42 P.M. – 4 15	30	2 0	106	135·0	30	2 0	102	128·0	1·2643	59·5	(30 0	2 0	112	189·0)*	30 0	2 0	120	152·0	1·2667	66	1·26505	62·75
Isle à la Crosse ...	July 17.	1 28 P.M. – 2 30	30	2 0	64	83·0	(30	2 0	72	150·0?)*	1·2969	73·5	(30 0	2 0	74	475·0!)*	30 0	2 0	79	99·0	1·2532	75·0	1·2750	74·25
Fort Chipewyan...	July 31.	2 45 P.M. – 4 00	30	2 0	90	117·0	30	2 0	100	130·0	1·3000	95·0	30 0	2 0	89	110·0	30 0	2 0	84	105·0	1·2430	95·0	1·2715	95·0
Fort Resolution...	Aug. 9.	3 48 P.M. – 4 42	30	2 0	110	135·0	30	2 0	96?	120·0	1·2387	65·6	30 0	2 0	100	125·0	30 0	2 0	110	140·0	1·2613	64·0	1·2500	64·8
Fort Reliance	Oct. 9.	2 9 P.M. – 5 00	30	4 0	80	103·0	30	5 0	80	101·0	1·2750	44·0	30 0	3 0	80	101·0	30 0	5 0	70	90·0	1·2741	39·8	1·27455	41·9
	1834.																							
Fort Reliance	May 21.	3 17 P.M – 6 30	40	4 0	80	102·6	30	2 0	80	102·9	1·2844	49·13	40 0	3 30	80	101·2	40 0	5 0	80	102·0	1·2700	50·12	1·2772	49·625
Fort Reliance	Oct. 9.	11 52 A.M. – 2 00 P.M.	40	3 30	80	101·5	40	4 0	80	103·0	1·27813	27·62	40 0	3 0	70	89·0	40 0	4 0	60	76·5	1·27321	32·0	1·27567	29·81
Musk-Ox Rapid	July 2.	3 33 P.M. – 3 50	40	3 0	70	89·8	40	1 15	60	77·5	1·28726	64·0	Observations were not made in this position of the needle.											
Rock Rapid	July 23.	2 46 P.M. – 3 00	40	3 0	40	51·2	40	2 0	50	64·0	1·2800	86·75	Observations were not made in this position of the needle.											
Point Beaufort ..	July 31.	3 5 P.M. – 3 30	40	2 30	100	129·5	40	4 0	100	130·0	1·2975	72·5	Observations were not made in this position of the needle.											
Montreal Island...	Aug. 2.	1 59 P.M. – 2 21	40	3 20	100	128·7	40	4 30	100	129·0	1·2885	74·12	Observations were not made in this position of the needle.											
Point Ogle	Aug. 12.	1 43 P.M. – 3 39	42	5 0	80	101·5	40	3 40	80	101·0	1·26563	53·0	40 0	2 10	70	90·0	40 0	4 30	60	77·0	1·28451	55·12	1·27507	54·06
	1836.																							
London	Feb. 12.	0 49 P.M. – 1 8	36	2 0	90	127·0	35	7 0	80	117·0	1·4368	44·0	Observations were not made in this position of the needle.											

* These observations, for reasons which I shall assign, have not been included in the means.

To this Table of the direct results of the observations, it is necessary that I should add a few remarks of Captain Back's, on circumstances connected with the observations, and of my own, on the observations themselves.

At Montreal, Captain Back remarks, that "from some mistake of the assistant the observations were so confusedly set down as to be useless:" and afterwards, that "not having an assistant to take time, &c. I adopted this plan;" that is, the time at which the needle commenced vibrating being noted, the vibrations were counted until the semi-arc of vibration was reduced to 2° or 3°, and the time again noted; so that, in each case, the time could not well be determined more accurately than to the nearest second. He subsequently instructed his servant William Mally in the manner of noting the time by chronometer; and although, as I am sorry to have to notice, there are, in the early observations, manifest errors in the time, yet this person appears, from the greater detail in the observations at Fort Reliance and subsequently (the arc and time being each noted at every tenth vibration), to have given very efficient assistance in this operation. In the foregoing Table I have marked with an asterisk those observations in which I consider that an error must have been made, either in the time or the number of vibrations, and which I have consequently omitted in taking the mean; and it will be proper that I should point out more particularly the results which these observations give.

At Cumberland House, with the face of the instrument east and the needle reversed on its axis, 112 vibrations appear to have been made in $3^{m}\ 9^{s}$, giving $1^{s}{\cdot}6875$ as the time of one vibration, which is so greatly at variance with the other results, that no conclusions could possibly be drawn from a mean in which it should enter. We may conceive that an error of ten vibrations in counting, or of one minute in the time, may easily have been made, but neither supposition would give a result at all in accordance with the others. It is scarcely conceivable that any change in the terrestrial intensity, whether arising from atmospheric or any other influence, could have caused so great an increase in the time of vibration; it is, however, proper to give Captain Back's remarks on the weather, which I shall do after noticing other observations which have been omitted in taking the means.

At Isle à la Crosse, with the face of the instrument west and the face of the needle to the face of the instrument, it appears that 72 vibrations were made in $2^{m}\ 30^{s}$, the times at beginning and ending being $8^{h}\ 56^{m}\ 30^{s}$ and $8^{h}\ 59^{m}\ 00^{s}$, by chronometer $13^{m}\ 20^{s}$ slow of Greenwich time. This interval would give $2^{s}{\cdot}083$ for the time of vibration. If we suppose an error of one minute to have been made in the time, then $1^{s}{\cdot}25$ would be the time of vibration, a result by no means improbable; but I consider that the safest course is to reject the observation. With the face of the needle reversed and the face of the instrument east, the times at beginning and ending were $9^{h}\ 50^{m}\ 30^{s}$ and $9^{h}\ 58^{m}\ 25^{s}$. We may conceive that here in transcribing 56^{m} may easily have been mistaken for 50^{m}; and this would reduce the interval in which seventy-four vibrations were made to $1^{m}\ 55^{s}$, and the time of one vibration to 1s·554; but even this is a most improbable result. In the subjoined Table I give the times of beginning and ending the vibrations, with Captain BACK's remarks on the weather, &c. at the times of observing.

<table>
<tr><th rowspan="3">Place of observation.</th><th colspan="2">Face of the needle to the face of the instrument.</th><th colspan="2">Face of the needle reversed.</th></tr>
<tr><th colspan="2">Face of the instrument</th><th colspan="2">Face of the instrument</th></tr>
<tr><th>East.</th><th>West.</th><th>East.</th><th>West.</th></tr>
<tr><td>Cumberland House.</td><td>Time commencing $10^h 22^m 25^s$.
Time concluding 10 24 40.
Wind S.W. by compass.
Weather squally, with rain; cloudy.</td><td>Time commencing $10^h 50^m 22^s$.
Time concluding 10 52 30.
Wind S.W. by compass.
Weather squally, with light rain; overcast.</td><td>Time commencing $9^h 47^m 32^s$.
Time concluding 9 50 41.
Wind S.W. by compass.
Weather squally, with rain; dark clouds, but no thunder.</td><td>Time commencing $9^h 18^m 59^s$.
Time concluding 9 21 31.
Wind S.W. by compass.
Weather calm and sultry, with distant thunder.</td></tr>
<tr><td rowspan="2">Isle à la Crosse.</td><td>Time commencing $8^h 37^m 00^s$.
Time concluding 8 38 23.
Weather overcast, calm.
Needle sluggish.</td><td>Time commencing $8^h 56^m 30^s$.
Time concluding 8 59 00.
Weather clearer; the sun seen at intervals.</td><td>Time commencing $9^h 50^m 30^s$.
Time concluding 9 58 25.
Weather clearer; sun out.</td><td>Time commencing $9^h 28^m 00^s$.
Time concluding 9 29 39.
Weather clearer.</td></tr>
<tr><td colspan="4">The needle was decidedly sluggish though carefully wiped.</td></tr>
</table>

At Rock Rapid it appears that there was considerable difficulty in counting more than forty or fifty vibrations of the needle, its motion being extremely unsteady, and occasionally it came to a dead stop. I have selected the two sets of vibrations in which it continued vibrating for the longest time. In two other sets, in similar positions, it made only thirty-two vibrations in the one case, and thirty-eight in the other: Captain BACK has a remark on these observations in his Narrative*. It is difficult to conceive any local cause for this tendency of the needle to come so much quicker to rest than in other cases, and I cannot but attribute it to the influence of the sun†. The observations were made as usual in a tent, but the height of the thermometer clearly indicates that the sun must have had considerable influence even under this screen. This view of the cause of the needle so soon coming to rest at Rock Rapid, is corroborated by the fact that, at Montreal Island "a sail was put over the tent to afford more shade," and there "the needle was particularly lively, and vibrated smoothly until it finally rested," the number of vibrations being one hundred. Whatever may have been the cause which thus affected the vibration of the needle, the circumstance must throw some uncertainty on the measure of the intensity deduced from its time of vibration.

Although I have found it necessary to make these remarks on Captain BACK's observations, it cannot, I trust, be supposed that I attribute the errors which I have pointed out to want of care or attention on the part of that most enterprising officer, for no one can appreciate more highly than I do the zeal which he manifested for the promotion of scientific research, by undertaking to make these observations on such an expedition as that on which he so nobly volunteered. I attribute these errors solely to Captain BACK's destitution of assistance; and I cannot but regret that it

* Narrative of the Arctic Land Expedition, p. 360.

† Philosophical Transactions, 1826, p. 219; 1828, p. 379.

should not have been afforded him from that service, the Royal Navy, which has furnished, and still continues to furnish, abundant instances of zeal and intelligence in the prosecution of scientific inquiry.

In order to deduce the relative magnetic intensities at the several stations from the results in the foregoing Table, it will first be necessary that a correction should be applied for the difference of temperature at which the observations were made. Immediately that I received the needles belonging to Captain Back's instruments, I instituted experiments for the determination of the correction to be applied, for this purpose, to the observations with each needle. As the method I adopted was the same with all the needles, I consider that it will be better to give all the experiments with their results in one view, and shall therefore defer giving these experiments and deducing the intensities at the different stations from the preceding observations until I have given an account of the observations which were made by Captain Back with horizontal needles, for the same purpose.

A small apparatus, on the plan first suggested by Hansteen, was employed for determining the horizontal intensity. It was furnished with two cylindrical magnetical needles, and a brass one of the same form and weight to divest the suspending silk fibres of torsion. As I had always found that pointed needles, whether of the form of a double segment of a circle or of that of a lozenge, vibrated more quickly than rectangular needles of the same weight, I suggested the advantage of having such a needle. A needle of a lozenge form was in consequence added to the apparatus. The horizontal intensity was therefore, where time admitted of it, determined by the vibrations of these three needles, distinguished as No. 1, No. 3, and Lozenge Needle. The observations appear to have been made in the usual manner, by noting the arc and time at the commencement; the time at every ten vibrations; generally the time and vibration at which the arc was diminished one half, and the terminal arc of vibration. The vibrations were, in general, continued until the arc was reduced to two or three degrees; but it appears that only with one of the needles, No. 3, could the vibrations be counted, even in London, as far as three hundred. As the dip increased, the number of vibrations, within the same limits of arc, decreased, and was ultimately reduced, at Point Ogle, to forty or fifty. I do not consider that, in these observations, the time could have been determined with sufficient accuracy to admit of the application of the method adopted by Hansteen, of taking a mean of several intervals, or that much advantage would arise from the application of a correction for the arc, to the time of vibration. I have not therefore thought it necessary to give the observations at length, but still I have, in the following Table, given the times in all cases where the arc was noted, in order that such a correction might be applied, if thought necessary. This Table will, I consider, require little explanation. In each case, the number of vibrations, the corresponding arcs, where observed, and the times, are given in three consecutive columns; and where the vibrations are continued beyond one hundred, the arcs and corresponding times are given in two subsequent columns, in which those corresponding to 100, 150, &c. vibrations are opposite to 0 50, &c. in the first column.

Table of the Times of Vibration of Horizontal Needles at different Stations.

Needle No. 1.

Place at which the observations were made.	Date.	Time of day nearly.	No. of vibrations.	Arc.	Time.	Arc.	Time.	Time of one vibration.	Therm.
		h m		°	m s	°	m s	s	°
London, the Admiralty Garden.	1833. Feb. 7. Nos. 1&3. Feb. 9. Lozenge.	1 30 P.M.	0 50 80 90 100	20 15 ... 10 ...	0 5 3 48 6 1·5 6 46 7 31	... 6·5 5 ... 4·5	7 31 11 12 13 26 14 10 14 54	4·445	56·0
Montreal, Island of St. Helen's.	April 19.	No observations were made.							
Fort Reliance.	Oct. 11.	2 57 P.M.	0 50 80	20 11 7·5	0 46 7 12 11 3	6 3·75 2·75	13 36·5 29 1 23 52	7·7000	45·5
Fort Reliance.	1834. Feb. 8.	No observations were made.							
Fort Reliance.	May 21.	2 22 P.M.	0 40 90	20 ... 5	0 47 5 55 12 18·5	... 3	13 36 18 43·5	7·6893	51·0
Fort Reliance.	Oct. 9.	No observations were made.							
Musk-Ox Rapid.	July 2.	No observations were made.							
Rock Rapid.	July 23.	No observations were made.							
Point Beaufort.	July 31.	No observations were made.							
Montreal Island.	Aug. 2.	No observations were made.							
Point Ogle.	Aug. 12.	No observations were made.							
London, the Admiralty Garden.	1836. Feb. 12.	3 48 P.M.	0 90	20 ...	0 14 7 15	... 4	8 0 15 0	4·6632	45·0

Needle No. 3.

Place at which the observations were made.	Date.	Time of day nearly.	No. of vibrations.	Arc.	Time.	Arc.	Time.	Arc.	Time.	Time of one vibration.	Therm.
		h m		°	m s	°	m s	°	m s	s	°
London, the Admiralty Garden.	1833. Feb. 7. Nos. 1&3. Feb. 9. Lozenge.	2 0 P.M.	0 30 50 80 100	30 ... 20 16·5 15	0 51 3 3 4 31 6 42 8 9	15 ... ... 6·5	8 9 10 19·5 11 47 13 57 15 24·5	6·5 5 ... ... 2·75	15 24·5 17 35 19 1·5 21 12 22 39·5	4·3167	56·0
Montreal, Island of St. Helen's.	April 19.	4 00 P.M.	0 10 50 70 90	20 ... ... 10 ..	0 41 1 26·5 4 40·5 6 16·5 7 53·5	... ... 5	8 42 9 29·5 12 43 14 19 15 56·5	... ...	16 44·5 17 32·5 20 45·5 22 22 23 58·5	4·82857 *	56·5
Fort Reliance.	Oct. 11.	2 13 P.M.	0 60 90	20 10 7	0 4 7 35 11 20	6·25 3·25 2·75	12 35 21 16·5 23 45			7·4790	46·0
Fort Reliance.	1834. Feb. 8.	2 5 P.M.	0 50 70	20 10 ...	0 35·5 6 51 9 21	 2	13 5·5 19 9·8 21 49·5			7·4941	45·25
Fort Reliance.	May 21.	1 40 P.M.	0 30 50	20 ... 10	0 9 3 53 6 22	... 3	12 32·5 16 14·5			7·42692	50·5
Fort Reliance.	Oct. 9.	3 12 P.M.	0 50 60	20 10 ...	0 13 6 36 7 52	5 ... 2	12 56·3 19 17 20 33·5			7·6281	30·0
Musk-Ox Rapid.	July 2.	4 57 P.M.	0 10 40	20 ... 9	0 32·5 2 1·5 6 28·5	3 2	15 21 16 50·5			8·8909	71·0
Rock Rapid.	July 23.	0 20 P.M.	0 70	20 2·5	0 35·5 15 47·5					13·0286	82·0
Point Beaufort.	July 31.	5 14 P.M.	0 70	20 2	0 15 21 50					18·500	74·0
Montreal Island.	Aug. 2.	5 15 P.M.	0 20 60	20 10 2·25	0 9·5 5 45 16 56					16·775	67·5
Point Ogle.	Aug. 12.	4 50 P.M.	0 40 50	20 ... 2·5	0 11·5 15 28 19 19	20 2·5	0 24 15 23			mean s 22·7125	62·125
London, the Admiralty Garden.	1836. Feb. 12.	4 8 P.M.	0 70	20 ...	0 34 5 53	... 5	8 11 13 29			4·5588	45·25

Lozenge Needle.

Place at which the observations were made.	Date.	Time of day nearly.	No. of vibrations.	Arc.	Time.	Arc.	Time.	Time of one vibration.	Therm.
		h m		°	m s	°	m s	s	°
London, the Admiralty Garden.	1833. Feb. 7. Nos. 1&3. Feb. 9. Lozenge.	3 56 P.M.	0 30 70 90 100	30 20 ... 10 ...	0 12 1 8·5 2 22·5 2 59·5 3 18·5	... ... 5 ... 3	3 18·5 4 13·5 4 27·5 6 4·5 6 22·5	1·8525	50·0
Montreal, Island of St. Helen's.	April 19.	No observations were made.							
Fort Reliance.	Oct. 11.	3 45 P.M.	0 40 50	20 10·75 9	0 59·5 3 6 3 37·5	4 2	6 15 8 21	3·15357	46·0
Fort Reliance.	1834. Feb. 8.	2 50 P.M.	0 28 40	20 ... 10	0 42 ...... 2 48	... 2	5 57 7 25	3·14844	35·0
Fort Reliance.	May 21.	2 50 P.M.	0 60 100	20 5·5 1·75	0 15 3 25 5 31·5			3·1650	51·0
Fort Reliance.	Oct. 9.	3 25 P.M.	0 30 90	20	0 30 2 4·3 5 12·8	... 2	5 44 7 18·5	3·1423	30
Musk-Ox Rapid.	July 2.	No observations were made.							
Rock Rapid.	July 23.	No observations were made.							
Point Beaufort.	July 31.	No observations were made.							
Montreal Island.	Aug. 2.	5 47 P.M.	0 30 50	20 5·5 2·25	0 33 3 59·5 6 17			6·880	63·5
Point Ogle.	Aug. 12.	5 48 P.M.	0 20 40	20 ... 1·5	0 4·5 3 0 5 56·5	20 ... 1·5	0 1 2 58·5 5 55	mean s 8·8250	63·5
London, the Admiralty Garden.	1836. Feb. 12.	4 24 P.M.	0 80	20 ...	0 47 3 20	... 3·5	3 57 6 28	1·8944	44·75

* Time of 280 vibrations, from 10th to 290th vibration = 1352 seconds.

I shall not now make any remarks on the results contained in the foregoing Table, but proceed to describe the method I adopted for determining the correction necessary to be applied, in order to reduce the measures of intensity at different temperatures, to be derived from the times of vibration in this and the preceding Table, to measures of intensity at the same temperature.

Determination of the Correction for the Difference of Temperature.

Since I first pointed out the necessity of applying a correction for the difference in the temperature at which observations for determining the magnetic intensity may have been made *, such a correction has, in many instances, been applied; but its amount has seldom been determined from direct experiments on the individual needle employed in the observations from which the intensities were to be deduced. This I consider to be essential; for in the numerous experiments which I have made on the subject, I have found that, in different needles, there is a very considerable difference in the amount of this correction; and M. Kupffer's experiments † show that this amount depends upon the nature and the temper of the steel employed. I therefore proposed to determine this correction for each of the needles which had been employed by Captain Back. For this purpose I employed an earthenware vessel having a wooden bottom firmly fixed in it, to which the needle under trial could be securely attached by means of ivory pegs. Above this vessel was placed a stage, supporting a compass having a small trial-needle, 1·36 inch long, delicately suspended by a single *fibre* of silk. The time in which this needle performed 100 vibrations having been carefully determined by two trials, the needle under examination was fixed to the wooden bottom of the vessel below, but separated from the wood by narrow slips of glass. This needle was placed with its centre exactly below the centre of the trial-needle, its axis in the magnetic meridian and its marked end towards north; and the distance of the upper or vibrating needle from the lower was so adjusted that its marked end still pointed north, but with a greatly diminished force. A small thermometer was placed on the glass of the compass, to indicate any change of temperature that the vibrating needle might undergo; and another thermometer was placed in the vessel containing the needle under trial, with its bulb close to that needle, and not touching the bottom of the vessel. Water, of as low a temperature as could be obtained, without employing chemical means, was then poured into the vessel containing the needle under trial; the temperature of this needle and also of the vibrating needle having been noted, the time of vibration of the latter was determined by two successive trials, and the temperature of both needles was again ascertained. Water of a higher temperature was then poured into the vessel, and the whole well agitated; and as soon as the temperature became uniform, the foregoing observations of temperature and time of vibration were repeated. In this manner, the temperature of the needle was successively raised to the highest temperature at which observations had

* Philosophical Transactions, 1825, p. 61.

† Annales de Chimie, tom. xxx. p. 113.

been made by Captain Back; after which it was lowered, by as nearly as possible the same intervals, to the lowest at which the experiments began; the temperature and time of vibration being similarly determined in all cases. The needle under trial being removed, the time in which the trial-needle made 100 vibrations was again ascertained. The needle, in all cases, commenced vibrating from the same arc 35°; the time, however, of commencing the vibrations was reckoned from its first passing zero, and the time of completing each ten vibrations, at the same point, was noted, up to one hundred.

The following Table exhibits the observations that were made with the needle No. II. belonging to Captain Back's dipping instrument. When under the influence of this needle, the time of vibration of the trial-needle was so much increased that 50 vibrations were performed in nearly the same time as 100 had been made when it was uninfluenced; and the vibrations could not have been conveniently counted much beyond this number. In all cases, the time was determined by two observations. In this Table, Thermometer II. indicates the temperature of the needle No. II., and Thermometer N, nearly that of the trial-needle.

Trial-needle uninfluenced.			Trial-needle under the influence of the needle No. II.							
	First observation.	Last observation.								
Beginning: Therm. II.	60°·0		....	46°·2	60°·0	72°·6	84°·6	72°·0	59°·8	46°·2
Beginning: Therm. N.	57·2	60°·2	....	58·2	59·6	60·0	61·0	61·0	61·0	60·6
Time of 100 vibrations	147s·8	147s·8	Time of 50 vibrations	157s·2	155s·6	153s·8	152s·6	154s·0	155s·5	157s·1
	148·0	147·9		157·0	155·7	154·0	152·8	154·0	155·6	157·1
Ending: Therm. II.	60°·0		....	47°·8	59°·6	71°·6	82°·8	72°·0	60°·8	47°·0
Ending: Therm. N.	58·0	60°·2	....	59·0	60·0	60·6	62·0	61·0	60·8	60·4
Mean: Therm. II.	60·0	....	....	47·0	59·8	72·1	83·7	72·0	60·3	46·6
Mean: Therm. N.	57·6	60·2	....	58·6	59·8	60·3	61·5	61·0	60·9	60·5
Mean time of one vibration	1s·47875		....	3s·142	3s·113	3s·078	3s·054	3s·080	3s·110	3s·142

The first observation commenced at $1^h\ 0^m$ P.M. 5th March, and the last concluded at $4^h\ 35^m$ P.M.

If we call the terrestrial force acting upon the vibrating needle, M; the force with which the needle II. acts upon it, at any given temperature, m; the time of vibration of the trial-needle, when acted upon by the terrestrial force alone, T; and its time of vibration, when influenced by the needle II., that is, when acted upon by the difference of the two forces M and m, t; then we shall have

$$\frac{M - m}{M} = \frac{T^2}{t^2};$$

and

$$\frac{m}{M} = 1 - \frac{T^2}{t^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16.)$$

Substituting in this equation the observed times of vibration at different temperatures, we shall obtain the values of $\frac{m}{M}$, that is, the measures of the intensity of the needle II. corresponding to these temperatures. The following Table contains the values of $\frac{m}{M}$ thus deduced, and in the last column are given the differences in the value of $\frac{m}{M}$ corresponding to a change of one degree in the temperature of the needle No. II.

Therm. II.	Differences.	Therm. N.	Values of $\frac{m}{M}$.	Differences.	Difference in value of $\frac{m}{M}$ corresponding to difference of 1° in Therm. II.
47°·0		58°·6	·7784982		
	12°·8			·0041462	·0003239
59·8		59·8	·7743520		
	12·3			·0051603	·0004195
72·1		60·3	·7691917		
	11·6			·0036425	·0003140
83·7		61·5	·7655492		
	11·7			·0039416	·0003369
72·0		61·0	·7694908		
	11·7			·0045710	·0003907
60·3		60·9	·7740618		
	13·7			·0044364	·0003238
46·6		60·5	·7784982		
				Mean....	·0003515

There are here, certainly, considerable discrepancies in the differences in the value of $\frac{m}{M}$ corresponding to a difference of 1° in the thermometer II. These, I consider, have arisen in a great measure, if not wholly, from the thermometer II. not indicating, in all cases, precisely the temperature of the needle No. II.: and this is one of the principal difficulties which occurs in an inquiry into the effects produced on the intensity of magnets by changes of temperature. From experiments which I long since made*, and from others which I have made more recently, I consider that the differences in the values of $\frac{m}{M}$ corresponding to a change of 1° in the temperature of a magnet increase with the temperature; but the foregoing results can scarcely be considered to indicate such a law. For the purpose, however, at present in view, this is not of great importance; and with regard to the discrepancies I have noticed, I may remark, that if these have arisen from the cause I have assigned, their effect on the mean result will be extremely small; for in this case the errors in the results arising from errors in the divisors would tend to destroy each other. In order that the results should be accurate, it is necessary, either that the temperature of the trial-needle should be the same throughout the observations, or that a correction should be applied for the changes which may have taken place in that temperature. This correction might be determined by observations similar to the preceding, but in this manner the final correction would form an infinite series. In these observations the

* Philosophical Transactions, 1825, p. 63.

indications of the thermometer N were not constant, but, as the whole difference did not amount to 3°, I do not consider that this can materially affect the result. It is, however, probable that the difference in the temperature of the needle N did not even amount to this: for in consequence of the glass shade under which the needle vibrated being too small to contain a thermometer, that instrument was placed on the outside, and would therefore be more affected by small changes in the temperature of the room than the needle which was inclosed.

If we take the mean of the values of $\frac{m}{M}$ corresponding to the indications 59°·8 and 60°·3 of the thermometer II., we shall have ·7742069 as the value of $\frac{m}{M}$ corresponding to the temperature 60°·05 of the needle II. Adding ·0000175 to this value, for the difference 0°·05, we have ·7742244 for the value of $\frac{m}{M}$ at the temperature 60°. If now we consider the intensity of the needle II., or the value of $\frac{m}{M}$ at 60° Fahr. to be 1, we shall have,

$$\text{The increment in the value of } \frac{m}{M} \text{ for each } 1^\circ \text{ above or below } 60^\circ = \frac{\mp \cdot 0003515}{\cdot 7742244}$$
$$= \mp \cdot 0004540.$$

Let $I_{\prime}$ be the measure of the terrestrial magnetic intensity resulting from observation, at any station, of the time of vibration of the needle No. II., at the temperature $60^\circ \pm \theta$; and I the measure of the intensity at the temperature 60°: then

$$I_{\prime} = I \mp \cdot 000454\, I.\theta;$$

and

$$I = \frac{I_{\prime}}{1 \mp \cdot 000454\,\theta} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (17.\ \text{II.})$$

By means of this formula, the measure of the intensity with the needle II. at any temperature $60^\circ \pm \theta$ may be reduced to the measure of the intensity at the standard temperature 60° Fahr.

In order to determine the corrections to be applied, for difference of temperature, to the observations made with the horizontal needles No. 1, No. 3, and the Lozenge-needle, experiments similar to those which I have described were made with these needles. As these experiments were made consecutively, it is necessary that I should give them in the order in which they were made.

With the Lozenge-needle, two sets of experiments were made. In the first set, this needle occupied the same position precisely as that in which the needle No. II. had previously been placed. The results are given in the following Table:

	Trial-needle under the influence of the Lozenge-needle.			Trial-needle uninfluenced.
Beginning Therm. L.......	46°·0	79°·4	46°·0	
Beginning Therm. N.......	64·6	68·0	65·0	63°·0
Time of 100 vibrations....	174ˢ·9	174ˢ·4	174ˢ·6	147ˢ·4
	174·8	174·4	174·8	147·6
Ending.. Therm. L.......	48°·0	77°·4	47°·6	
Ending.. Therm. N.......	66·0	66·0	66·0	67°·6
Mean .. Therm. L.......	47·0	78·4	46·3	
Mean .. Therm. N.......	65·3	67·0	65·5	65·3
Mean time of one vibration ..	1ˢ·7485	1ˢ·744	1ˢ·747	1ˢ·475

The observations commenced at $7^h\ 8^m$ P.M., and concluded at $8^h\ 45^m$ P.M. 5th March.

Finding that in this position of the lozenge-needle the times of vibration of the trial-needle were so little affected by changes in the temperature of the former that a small error in these times would materially affect the results, I made a second set of observations, in which the lozenge-needle was raised ·84 inch, its distance from the trial-needle being reduced to 3·06 inches. The following Table contains the results of this set:

	Trial-needle uninfluenced.		Trial-needle under the influence of the Lozenge-needle.		
	First observation.	Last observation.			
Beginning Therm. L.......			46°·8	80°·0	49°·0
Beginning Therm. N.......	60°·0	61°·6	59·6	60·0	60·0
Time of 100 vibrations	147ˢ·8	147ˢ·8	216ˢ·8	215ˢ·4	216ˢ·8
	147·7	147·9	217·2	215·4	216·3
Ending.. Therm. L.......			48°·4	77°·0	50°·2
Ending.. Therm. N.......	59°·2	61°·2	60·0	61·0	60·2
Mean .. Therm. L.......	....	....	47·6	78·5	49·6
Mean .. Therm. N.......	59·6	61·4	59·8	60·5	60·1
Mean time of one vibration ..	1ˢ·478		2ˢ·170	2ˢ·154	2ˢ·1655

Note.—The first observation with the trial-needle uninfluenced commenced at $0^h\ 5^m$ P.M.; then followed the observations with that needle under the influence; 1st, of the lozenge-needle; 2nd, of needle No. 3; 3rd, of needle No. 1; and the last observation with the trial-needle uninfluenced concluded at $4^h\ 50^m$ P.M. 6th March.

Substituting the times of vibration in the first set of observations in the equation (16.), we have the following results:

Therm. L.	Differences.	Therm. N.	Values of $\frac{m}{M}$.	Differences.	Difference in value of $\frac{m}{M}$ corresponding to difference of 1° in Therm. L.
47°·0 78·4 46·8	° 31·4 31·6	65°·2 67·0 65·5	·2900090 ·2863406 ·2887895	·0036684 ·0024489	·0001168 ·0000775
				Mean....	·0000972

Taking the mean of the values of $\frac{m}{M}$ corresponding to the indications 47°·0 and 46°·8 of the thermometer L, we have ·2893993 as the value of $\frac{m}{M}$ corresponding to the temperature 46°·9 of the needle L. Subtracting from this ·0000972 × 13·1, or ·0012733, we have ·288126 for the value of $\frac{m}{M}$ at the temperature 60°. If we take the value of $\frac{m}{M}$ at the temperature 60° to be 1, we shall have,

The increment in the value of $\frac{m}{M}$ for each 1° above or below 60° = ∓ ·0003374.

Substituting the times in the second set of observations in the equation (16.), we obtain the following results:

Therm. L.	Differences.	Therm. N.	Values of $\frac{m}{M}$.	Differences.	Difference in value of $\frac{m}{M}$ corresponding to difference of 1° in Therm. L.
47°·6 78·5 49·6	° 30·9 28·9	59°·8 60·5 60·1	·5360947 ·5291772 ·5341648	·0069175 ·0049876	·0002239 ·0001726
				Mean....	·0001982

From these, taking a mean as before, we obtain ·5351298 as the value of $\frac{m}{M}$ corresponding to the temperature 48°·6 of the needle L; and subtracting ·0001982 × 11·4 from this value, we have ·5328704 for the value of $\frac{m}{M}$ at the temperature 60°. Taking the value of $\frac{m}{M}$ at the temperature 60° to be 1, we have,

The increment in the value of $\frac{m}{M}$ for each 1° above or below 60° = ∓ ·0003719.

The mean of the two results gives

The increment in the value of $\frac{m}{M}$ for each 1° above or below 60° = ∓ ·0003546.

We therefore obtain, for the correction of observations made with the Lozenge-needle, the equation

$$I = \frac{I_{\prime}}{1 \mp \cdot 0003546\,\theta} \quad . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \quad (17.\ L.)$$

In the experiments with Hansteen's cylindrical needle No. 3, that needle occupied the same position which the lozenge-needle had previously in the last set, the experiments with the two needles immediately following each other. The results obtained with this needle are given in the two following Tables:

	Trial-needle un-influenced. First observation.	Trial-needle un-influenced. Last observation.	Trial-needle under the influence of the needle No. 3.		
Beginning, Therm. 3			47·6	76·4	48·0
Beginning, Therm. N.	60·0	61·6	61·1	62·0	60·2
Time of 100 vibrations	147·8	147·8	205·8	204·2	205·7
	147·7	147·9	205·6	204·2	205·9
Ending, Therm. 3			49·0	75·0	50·0
Ending, Therm. N.	59·2	61·2	61·2	62·4	61·0
Mean, Therm. 3	....	....	48·3	75·7	49·0
Mean, Therm. N.	59·6	61·4	61·15	62·2	60·6
Mean time of one vibration	1·478		2·057	2·042	2·058

Therm. 3.	Differences.	Therm. N.	Values of $\frac{m}{M}$.	Differences.	Difference in value of $\frac{m}{M}$ corresponding to difference of 1° in Therm. 3.
48·3		61·15	·4837261		
	27·4			·0076128	·0002778
75·7		62·2	·4761133		
	26·7			·0081145	·0003039
49·0		60·6	·4842278		
				Mean....	·0002909

Adopting similar methods of reduction to the preceding, from these results we obtain, for the correction of the observations made with the needle No. 3, the equation

$$I = \frac{I_{,}}{1 \mp \cdot 0000052\,\theta} \quad \text{. . . . .} \quad (17.\ 3.)$$

The needle No. 3 being removed, its place was occupied by the needle No. 1, with which the following results were obtained:

	Trial-needle uninfluenced. First observation.	Trial-needle uninfluenced. Last observation.	Trial-needle under the influence of the needle No. 1.		
Beginning, Therm. 1			45·4	79·0	46·0
Beginning, Therm. N.	60·0	61·6	60·0	60·8	60·6
Time of 100 vibrations	147·8	147·8	202·4	200·1	201·9
	147·7	147·9	202·3	200·3	201·9
Ending, Therm. 1			47·0	77·0	49·0
Ending, Therm. N.	59·2	61·2	60·0	62·0	61·0
Mean, Therm. 1	......	......	46·2	78·0	47·5
Mean, Therm. N.	59·6	61·4	60·0	61·4	60·8
Mean Time of one vibration	1·478		2·0235	2·002	2·019

Thermometer 1.	Differences.	Thermometer N.	Values of $\frac{m}{M}$.	Differences.	Difference in value of $\frac{m}{M}$ corresponding to difference of 1° in Therm. 1.
46°·2	°	60°·0	·4664903		
	31·8			·0115205	·0003623
78·0		61·4	·4549698		
	30·5			·0091396	·0002997
47·5		60·8	·4641094		
				Mean......	·0003310

Again, making use of similar methods of reduction with these results, the equation for the correction of the observations made with the needle No. 1 becomes

$$I = \frac{I_{\prime}}{1 \mp \cdot 0007181\,\theta} \quad \ldots\ldots\ldots\ldots \quad (17.\ 1.)$$

Reduction of the Observations.

Having pointed out the methods which I adopted for determining the constants which enter into the equations requisite for the reduction of observations, made with these needles at different temperatures, to results at a standard temperature, and given the equations necessary for such reduction, I now resume the consideration of Captain Back's observations for the determination of the relative terrestrial magnetic intensities at different stations.

I propose first to determine these intensities from the observed times of vibration of the dipping needle No. II. in the plane of the meridian, and then to inquire whether the results derived from the times of vibration of the horizontal needles are in accordance with these.

In order to determine the intensities from the times of vibration in the meridian, it is necessary to divide the observations into two classes; first, those from London to Fort Reliance; and secondly, those from Fort Reliance to Point Ogle, on the coast of the Polar Sea, and thence to London.

Taking the reciprocal of the square of the time of vibration as the measure of the intensity at the temperature at which an observation was made, this measure is to be reduced by means of the formula (17. II.), in order to determine the measure of the intensity at a standard temperature. If t is the time of vibration of the needle at the temperature $60° \pm \theta$, and I the intensity at the standard temperature 60°, then

$$I = \frac{1}{t^2(1 \mp \cdot 000454\,\theta)} \quad \ldots\ldots\ldots\ldots \quad (18.\ II.)$$

Taking the times of vibration of the needle No. II. and the temperatures already given, the corresponding measures of the intensity at the different stations, as deduced from this equation, are given in the following Table, and likewise the relative intensities, that at London being taken as unity. In deducing the latter, two kinds of comparison were necessary. At New York the times of vibration were only determined with the face of the needle to the face of the instrument; and I have therefore compared these observations with those in London, where the needle was vibrated in the same position. At the other stations the observations were made with the face of the needle to the face of the instrument, and likewise reversed; and these observations are compared with the similar observations in London.

Place of observation.	Date.	Time of vibration of needle.	Thermometer.	Measure of intensity at temp. 60°.	Ratio of intensity to that at London.
	1833.	s	°		
London	February 9.	1·3625	50·0	·5362407	1·00000
New York	April 1.	1·28572	69·0	·6074147	1·13273
London	February 9.	1·3828	50·0	·5206021	1·00000
Fort Alexander	June 10.	1·25315	71·4	·6400997	1·22954
Cumberland House	July 6.	1·26505	62·75	·6256439	1·20177
Isle à la Crosse	July 17.	1·2750	74·25	·6191539	1·18930
Fort Chipewyan	July 31.	1·2715	95·0	·6283987	1·20706
Fort Resolution	August 9.	1·2500	64·8	·6413976	1·23203
Fort Reliance	October 9.	1·27455	41·9	·6105560	1·17279
	1834.				
	May 21.	1·2772	49·63	·6101570	1·17202
	October 9.	1·27567	29·81	·6061936	1·16441
			Mean ..	·6089689	1·16974

From these results it is very evident, that, from some cause or other, very possibly from having been subjected to a high temperature, the magnetism of the needle suffered a permanent change in the interval between the observations at Fort Resolution on the 9th of August, and those at Fort Reliance on the 9th of October. Judging from the results of the observations at Fort Reliance in May 1834, and those in October of the same year, it would not appear that the magnetism of the needle underwent any material change during that interval; for although the intensity, as deduced in this Table, is somewhat less in October than in May, yet, as will be seen in the Table which I shall immediately give, taking the vibration with the face of the needle to the face of the instrument alone, the intensity appears to be rather greater in October than in May. Whether the magnetism of the needle suffered any change in the interval between the observations in London and those at Fort Resolution, cannot be determined; but it is at least not probable that it did so from New York, and the result at that city would not lead us to suppose that it had previously. We may therefore consider that no exception can be taken to these results on this ground. With regard to the results at Fort Reliance, the observations that were made in London, subsequent to the expedition, indicate that they are considerably in defect, as will be seen by the comparison which I shall now make between the observations at Fort Reliance and those at stations subsequently visited.

In the observations which were made in the course of Captain Back's perilous voyage to the mouth of the Thlew-ee-choh,—which river, in justice to its discoverer and first navigator, should, henceforward, be named *the Back*,—the needle was, in consequence of other most pressing calls on his time, only vibrated with its face to the face of the instrument, excepting at Point Ogle, where a vexatious detention afforded more time for these observations. In comparing, therefore, the results from these observations with those at Fort Reliance, I have only taken those derived from observations made there in the same position of the needle. I have, however, deduced the measure of the intensity at Point Ogle both from the observations made in this position of the needle, and also from a mean of these and of those with the needle reversed on its axis. In the following Table are given:—the measures of the intensities at the temperature 60°; the relative intensities, taking the mean of the intensity at Fort Reliance before quitting, and on the return to that station, as unity; and likewise the relative intensities, taking that at London, on Captain Back's return, as unity.

Place of observation.	Date.	Time of vibration of needle.	Thermometer.	Measure of intensity at temp. 60°.	Ratio of intensity to that at Fort Reliance.	Ratio of intensity to that at London.
	1834.	s	°			
Fort Reliance	May 21.	1·28440	49·13	·603192		
	Oct. 9.	1·27813	27·62	·603416		
		Mean		·603304	1·00000	1·25450
Musk-Ox Rapid	July 2.	1·28726	64·0	·604584	1.00212	1·25717
Rock Rapid	July 23.	1·28000	86·75	·617855	1·02412	1·28473
Point Beaufort	July 31.	1·2975	72·5	·597388	0·99020	1·24220
Montreal Island	Aug. 2.	1·2885	74·12	·606212	1·00482	1·26055
Point Ogle	Aug. 12.	1·26563	53·0	·622317	1·03152	1·29404
	1836.					
London	Feb. 12.	1·4368	44·0	·480910	0·79713	1·00000
Fort Reliance	May 21. Oct. 9.	Mean { needle direct and reversed }		·608175	1·00000	
Point Ogle	Aug. 12.	1·27507	54·06	·613426	1·00863	1·26576

On the hypothesis of two magnetic poles not far removed from the centre of the earth, to which I have already adverted, if I represent the intensity of the force in the direction of the dip, then

$$I = \frac{\mu}{\sqrt{(3 \cos 2\delta + 5)}} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (19.);$$

where μ is a constant, whose value may be assumed in a comparison of the intensities corresponding to different values of δ, or may be determined from the observed values of I.

If we assume the intensity at the magnetic equator to be 1, then $\mu = 2\sqrt{2}$, and the equation becomes

$$I = \frac{2\sqrt{2}}{\sqrt{(3 \cos 2\delta + 5)}} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (20.)$$

In the Table which follows I have given:—the dip as already determined from Captain Back's observations; the intensities as deduced in the preceding Tables, taking the mean of the results at Point Ogle; the values of I deduced from the last equation; and, for comparison with these, the relative intensities, assuming the intensity at London to be the theoretical value so determined. I have also given the values of μ resulting from the equation

$$\mu = \text{I.} \sqrt{(3 \cos 2 \delta + 5)} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21.),$$

by assuming for I the ratio of the intensity to that at London, as deduced from the observations; and taking the mean of these values, (excluding that for London, on account of its incongruity with all the others,) as the value of μ in the equation (19.), I have deduced the values of I from that equation, and given the differences between these values and those resulting from the observations.

Place of observation.	Dip.	Ratio of intensity to that at London.	Computed value of I, equation (20.).	Value of I deduced from observation.	Difference.	Value of constant μ, equation (21.).	Difference between values of μ and mean.	Computed value of I, equation (19.) μ=1·7927.	Error of computed value of I.
London	69° 43′	1·0000	1·7166	1·7166	+·0000	1·6496			
New York	72 49	1·1327	1·7804	1·9445	+·1641	1·7995	+·0068	1·1285	—·0042
Fort Alexander ..	78 54	1·2295	1·8973	2·1107	+·2134	1·8330	+·0403	1·2025[1]	—·0270
Cumberland House	79 30	1·2018	1·9073	2·0630	+·1557	1·7822	—·0105	1·2088	+·0070
Isle à la Crosse ..	79 28	1·1893	1·9068	2·0416	+·1348	1·7642	—·0285	1·2085	+·0192
Fort Chipewyan..	81 1	1·2071	1·9306	2·0726	+·1420	1·7684	—·0243	1·2237	+·0166
Fort Resolution..	82 3	1·2320	1·9450	2·1150	+·1700	1·7917	—·0010	1·2328	+·0008
Fort Reliance....	84 1	1·2545	1·9682	2·1535	+·1853	1·8028	+·0101	1·2475	—·0070
Musk-Ox Rapid..	85 54	1·2572	1·9851	2·1581	+·1730	1·7915	—·0012	1·2580	+·0008
Rock Rapid	87 40	1·2847	1·9951	2·2055	+·2104	1·8214	+·0287	1·2645	—·0202
Point Beaufort ..	88 3	1·2422	1·9965	2·1324	+·1359	1·7598	—·0329	1·2654	+·0232
Montreal Island..	87 36	1·2606	1·9948	2·1639	+·1691	1·7874	—·0053	1·2643	+·0037
Point Ogle......	89 24	1·2799	1·9997	2·1971	+·1974	1·8103	+·0176	1·2674	—·0125
					Mean ..	1·7927			

Admitting the accuracy of the results by which the intensities at the American stations are connected with the intensity at London, the results in this Table are, I consider, quite conclusive against the correctness of the formula on which the value of I depends, and, consequently, against that of the hypothesis from which it is deduced, if applied to the whole of this extent of the earth's surface; the intensities deduced from observation at the American stations, as compared with the intensity at London, being, without any exception, considerably in excess of the theoretical results. But if we look to the values of μ in the sixth column, excluding that for London, we cannot fail to be struck by their remarkable agreement with each other; and I must candidly confess, that although, previously to deducing these results, I might expect something like a tendency to agreement, yet I by no means anticipated such an accordance as is here manifested. If we examine the differences between the values of μ and its mean value, excluding that for London, we shall see that only in one instance does this difference amount to one forty-fifth of the mean, and rarely to one sixtieth. Although a comparison of the observed and computed intensities must lead to the same conclusions, I have considered it would be satisfactory that such a comparison should be made. A most striking agreement is here manifest between the computed values of I and those deduced from observation, in every instance from New York to Point Ogle. It will be seen that the greatest error in the computed value of I, that corresponding to the observations at Fort Alexander, does not amount to a forty-fifth of the intensity; and if a mean of all the errors, without regard to their signs, be taken, it will be found that this mean error does not amount to the one hundred and fourth part of the mean of all the intensities. Such differences as are here indicated are quite within the limits of the errors to which observations of this nature, when carefully made, are liable, and are much less than are, generally, to be expected in any comparison of results with a formula that ought to represent them, when those results are deduced from the vibrations of a needle. It certainly does appear that the intensity did not in all cases increase with the dip; but it is probable that these discrepancies have arisen either from small errors in the observations, or from local causes affecting the intensity of the terrestrial force. Upon the whole, I think we are fully warranted in concluding that in the track of country embraced by Captain Back's observations, from New York to the Arctic Sea, the phenomena of terrestrial magnetic intensity are very correctly represented by the formula with which I have compared them.

I now proceed to discuss the observations made with horizontal needles for determining the intensity.

In order to determine the measures of the horizontal intensity resulting from the observed times of vibration of the three needles given in a preceding Table (p. 397), it is necessary to apply corrections for the difference of temperature, as indicated in the equations (17. 1.), (17. 3.), (17. L.); and in the following Table I have given the results so corrected:

Place of observation.	Needle No. 1.			Needle No. 3.			Lozenge-needle.		
	Time of one vibration.	Therm.	Measure of horizontal intensity at Temp. 60°.	Time of one vibration.	Therm.	Measure of horizontal intensity at Temp. 60°.	Time of one vibration.	Therm.	Measure of horizontal intensity at Temp. 60°.
	s	°		s	°		s	°	
London, Feb. 7, 1833	4·445	56·0	·0504675	4·36167	56·0	·0524378	1·8525	50·0	·290366
Montreal	No observations made.			4·82857	56·5	·0428001	No observations made.		
Fort Reliance, Oct. 11, 1833..	7·7000	45·5	·0166925	7·4790	46·0	·0177276	3·1535	46·0	·1000563
Fort Reliance, Feb. 8, 1834..	No observations made.			7·4941	45·25	·0176483	3·14844	35·0	·0999945
Fort Reliance, May 21, 1834..	7·6893	51·0	·0168047	7·42692	50·5	·0180258	3·1650	51·0	·0995105
Fort Reliance, Oct. 9, 1834..	No observations made.			7·6281	30·0	·0168793	3·1423	30·0	·1002096
Musk-Ox Rapid....	No observations made.			8·8909	71·0	·0127353	No observations made.		
Rock Rapid	No observations made.			13·0286	82·0	·0059707	No observations made.		
Point Beaufort	No observations made.			18·5000	74·0	·0029468	No observations made.		
Montreal Island....	No observations made.			16·7750	67·5	·00356986	6·880	63·5	·0211526
Point Ogle........	No observations made.			22·7125	62·13	·00194097	8·825	63·5	·0128561
London, Feb. 12, 1836	4·6632	45·0	·0455006	4·5588	45·25	·04769145	1·8944	44·75	·277152

If we compare the measure of the intensity in London previous to Captain Back's departure with its measure by the same needle on his return, it is evident that each needle must have lost intensity during the expedition. The loss of intensity by the needles No. 1 and No. 3 appears to have been nearly the same, but that by the lozenge-needle is proportionally less than one half that by either of the others. With two of the needles, No. 1 and the Lozenge, it is not possible to determine whether this diminution of intensity took place previous to the arrival at Fort Reliance or subsequent to the final departure from that station; but with the other, No. 3, it is clear that it lost intensity during the progress of the expedition from Fort Reliance to Point Ogle, and its return to the former station. The results do not indicate that any material change took place in the intensities of any of the needles during the residence at Fort Reliance. From the results with Nos. 1 and 3 it would appear that at Fort Reliance the horizontal intensity was rather greater in May than in the preceding October; but those with the lozenge-needle indicate the reverse; and this cannot be attributed to any loss of intensity in the needle itself, for the observations with that needle in the following October, on the return to Fort Reliance, give an intensity greater than any preceding.

If, in deducing the intensity in the direction of the dip from the horizontal intensity, we take the mean of the results in London, and the mean of those at Fort Reliance previous to quitting and on the return there, we shall have the following as standards for comparison:

	Mean measure of horizontal intensity.		
	Needle No. 1.	Needle No. 3.	Lozenge-needle.
At London......	·047984	·0500646	·283759
At Fort Reliance..	·0167486	·0173400	·100032

Assuming these and the measures of the horizontal intensity in the foregoing Table, corresponding to the respective stations, and dividing by the cosine of the dip, we obtain the results in the following Table for the measures of the intensity in the direction of the dip:

Place of observation.	Dip.	Needle No. 1.		Needle No. 3.		Lozenge-needle.	
		Measure of intensity.	Ratio of intensity to that at London.	Measure of intensity.	Ratio of intensity to that at London.	Measure of intensity.	Ratio of intensity to that at London.
London	69° 43′	·138417	1·00000	·144429	1·00000	·818545	1·00000
Montreal	77 6	....	....	·191714	1·32479		
Fort Reliance......	84 1	·160675	1·160803	·166348	1·15185	·959637	1·17237
Musk-Ox Rapid....	85 54	....	....	·178122	1·23337		
Rock Rapid	87 40	....	....	·146653	1·01547		
Point Beaufort	88 3	....	....	·086601	0·59965		
Montreal Island....	87 36	....	....	·085249	0·59029	·505126	0·61710
Point Ogle........	89 24	....	....	·185352	1·28344	1·22769	1·49984

There is but little agreement between these results and those obtained from the times of vibration of the needle No. II. in the plane of the meridian; and in some cases they are quite incongruous. If we suppose that the needles retained their intensities unimpaired from the time of the last observations at Fort Reliance until the observations were made in London, that is, if we take the results of the latter observations as standards for comparison, the agreement will in general be greater; but still similar incongruities will exist. The results thus obtained are given in the following Table:

Place of observation.	Needle No. 1.		Needle No. 3.		Lozenge-needle.	
	Measure of intensity.	Ratio of intensity to that at London.	Measure of intensity.	Ratio of intensity to that at London.	Measure of intensity.	Ratio of intensity to that at London.
London	·131253	1·00000	·137573	1·00000	·799485	1·00000
Fort Reliance........	·160675	1·22416	·161929	1·17704	·959637	1·20032
Musk-Ox Rapid......	....	....	·178122	1·29475		
Rock Rapid	....	....	·146653	1·06600		
Point Beaufort	....	....	·086601	0·62949		
Montreal Island......	....	....	·085249	0·61966	·505126	0·631815
Point Ogle..........	....	....	·185352	1·34730	1·22769	1·53560

Whatever may be the law according to which the terrestrial magnetic intensity may vary, it is evident that the results at Rock Rapid, Point Beaufort, and Montreal Island, in either of these Tables, cannot be in accordance with those at Fort Reliance, Musk-Ox Rapid, and Point Ogle; and it may be worth inquiring to what this discordance, and that between these results and those with the dipping needle No. II., are to be attributed. That there may have been errors in the observations of the times of vibration of the needles is very possible, but not to the amount that such discordances would indicate; and I therefore attribute the want of agreement in the results to the inefficiency of Hansteen's method for the determination of the absolute intensity in such cases, rather than to errors of this description. When the dip is great, a small error in its determination will introduce a large one in the determination of the absolute intensity from the horizontal; and to such errors I consider that these discordances may, in a great measure, be attributed. In order to determine the errors in the dip necessary to account for this want of agreement between the results obtained with the dipping needle No. II. and with the horizontal needles, let i be the measure of the horizontal intensity, and δ the dip at the place of observation, I the measure of the absolute intensity at London with the same needle, and M the ratio of the intensity at the place of observation to that at London, as determined by the time of vibration of the needle No. II., then

$$\cos \delta = \frac{i}{\mathrm{I\,M}} \quad \ldots \ldots \quad (22.)$$

In the following Table are given the values of δ deduced from this formula by substituting for M its values in the Table at p. 408, and for I its values in each of the two foregoing Tables:

Place of observation.	Observed Dip.	Needle No. 1.		Needle No. 3.		Lozenge-needle.	
		Computed dip $\cos\delta = \frac{i}{\mathrm{I\,M}}$ $\mathrm{I} = \cdot 138417.$	Difference of computed and observed dip.	Computed dip $\cos\delta = \frac{i}{\mathrm{I\,M}}$ $\mathrm{I} = \cdot 144429.$	Difference of computed and observed dip.	Computed dip $\cos\delta = \frac{i}{\mathrm{I\,M}}$ $\mathrm{I} = \cdot 818545.$	Difference of computed and observed dip.
	° ′	° ′ ″	′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″
Fort Reliance......	84 1	84 27 54	+ 26 54	84 30 29	+0 29 29	84 24 35	+0 23 35
Musk-Ox Rapid....	85 54	....	....	85 58 40	+0 4 40		
Rock Rapid	87 40	....	....	88 9 21	+0 29 21		
Point Beaufort	88 3	....	....	89 3 32	+1 0 32		
Montreal Island....	87 36	....	....	88 52 35	+1 16 35	88 49 31	+1 13 31
Point Ogle........	89 24	....	....	89 23 54	−0 0 6	89 17 49	−0 6 11
		$\mathrm{I} = \cdot 131253.$		$\mathrm{I} = \cdot 137573.$		$\mathrm{I} = \cdot 799485.$	
Fort Reliance......		84 9 43	+ 8 43	84 14 1	+0 13 1	84 16 34	+0 15 34
Musk-Ox Rapid....		....	....	85 46 38	−0 7 22		
Rock Rapid		....	....	88 3 51	+0 23 51		
Point Beaufort		....	....	89 0 43	+0 57 43		
Montreal Island....		....	....	88 49 14	+1 13 14	88 47 50	+1 11 50
Point Ogle........		....	....	89 22 6	−0 1 54	89 16 48	−0 7 12

The differences between the observed and computed dips at Fort Reliance, Musk-Ox Rapid and Point Ogle, particularly in the latter part of this Table, are quite within the limits of errors in the determination of the dip. The difference at Rock Rapid may possibly be considered to exceed this; but it is to be observed that the value of M in the Table, p. 408, is at this place manifestly in excess, and I have before remarked that the vibration of the needle No. II. appears to have been there influenced by some particular cause which would render the value of M, deduced from its time of vibration, doubtful. With regard to the differences at Point Beaufort and Montreal Island, these are much greater than we can attribute to the errors to which the observations for the dip could be liable, considering the care which Captain Back appears in all cases to have bestowed on these observations. All circumstances, however, indicate that the dips deduced from the observations at these two stations are less than the truth; and as there is one error which it is possible may have been made in registering the observations, it is proper that I should here advert to it. The value of the angle $'\theta$ at Point Beaufort is 89° 35′ (Table, p. 380); and if we suppose that this arc was read on the southern limb of the instrument, as was the case at Point Ogle, instead of the northern, as it had been previously at Rock Rapid, and as it is registered, then the true value of $'\theta$ would be 90° 25′. This value would give the dip at Point Beaufort 88° 28′ instead of 88° 3′. If we suppose the same error to have been committed at Montreal Island, then the value of $'\theta$ there would be 91° 13′ 30″, and the dip 88° 48′ 30″ instead of 87° 35′ 49″. These results, particularly that for Montreal Island, from a comparison with those at Rock Rapid and Point Ogle, and also with those in the foregoing Table, are certainly very probable, and the error I have indicated is one which may easily have occurred; but I must not omit to state, that on calling Captain Back's attention to the circumstance, and referring to the register of the observations, he could recall no circumstance that could induce him to think that in these observations the reading of the needle was beyond 90° on the northern limb, though he perfectly remembered remarking that such was the case at Point Ogle, and that the angle registered was that on the southern limb, and so marked.

I have already stated my opinion of the inapplicability of Hansteen's method of determining the intensity, to cases where the dip is great—but my objections to the method are by no means limited to such cases—and I may notice another source of error, besides that to which I have adverted, to which results deduced from the times of vibration of a horizontal needle are in these cases peculiarly subject,—local influence. The directive force acting upon the horizontal needle is, in these cases, greatly diminished, and consequently the relative effect of an extraneous force is, with little exception, increased; so that masses of rock which are magnetic, although without polarity, may, by their position, exert an influence on the time of vibration of a horizontal needle, which would be altogether insensible on that of the dipping needle. That the times of vibration of the horizontal needle at Point Beaufort may have been thus affected, is not altogether improbable. On the observations there Captain Back has this remark: "Instrument in perfect adjustment; stand on shingle at the base of a gneiss rock three or four hundred feet high." There is no remark on the position of this rock with respect to the needle, and I cannot now consult Captain Back on the subject; but that in particular positions such a mass would affect the horizontal needle, and even the dipping needle, is more than probable. At Montreal Island the stand was "placed on firm sand, about sixty yards from some low rocks"; so that here there does not appear any particular cause for suspecting local influence. It is, however, proper to give a remark of Captain Back's relative to these observations: "No. 3 vibrated slowly, and on the first trial stopped dead at 10°. Two kettles, twenty yards off, were then taken further away, and I took off my brace-buckles; it then vibrated regularly, but made a long rest at each extreme, as if disposed to remain there. We have only made twelve miles N.W. $\frac{3}{4}$ W. from the last place, so that one would imagine some local cause, such as the rock, to account for the difference in the interval of vibrations." But I must now close my remarks on the observations at these two stations, although I may, very unwillingly, leave some degree of uncertainty attached to the results deduced from them.

Whatever uncertainty may attach to the results of the observations at Point Beaufort and Montreal Island, every circumstance tends to confirm the correctness of the observations at the more important station, Point Ogle, and to indicate the very near approach which was here made to the northern pole of verticity. If any doubts could be entertained with regard to the amount of the dip at this station, as determined from the observations, they must be completely removed by Captain Back's remarks on the difficulty of adjusting the horizontal needles,—which, it will be borne in mind, were delicately suspended by fibres of silk,—and the uncertainty of their zero points, at Point Ogle, totally unconnected as these remarks are with the cause of this difficulty and uncertainty; and I cannot omit to give them precisely as they are appended to the observations: "This needle (No. 3) was extremely difficult to get into adjustment, and was so dull and heavy in its motion, that to have marked the precise time when it turned at each extremity would have required a fixed reading-glass. At the conclusion it settled within half a degree of its zero, but in five minutes after altered, without any apparent cause, 6°. I then made the following set (the second set of vibrations). This did not correspond with the former, neither did the needle settle at the same zero I began from, but returned to the first, and what I supposed to be the correct one." With respect to the lozenge-needle, he remarks: "There was a difference of no less than 22° between the north points of this and No. 3, though this agreed with Dollond's light needle for adjusting the dip instrument. As usual, it was more active, and, in this instance, far more regular (in its vibrations). Nevertheless another set was taken: the needle remained in perfect adjustment *."

If we contrast the difficulty here manifest, in determining the direction of the magnetic meridian by means of needles delicately suspended, with the facility with which its direction was, apparently, determined by Sir John Ross at Victory Harbour and Padliak, at which places he states that he ascertained the dip to be 89° 55′ and 89° 56′, we cannot but conclude that such results are greatly in excess. With such a dip, all determinate direction in a horizontal needle, arising from the force of terrestrial magnetism, however delicate might be the suspension of the needle, is quite out of the question; and if a horizontal needle, so situated with respect to the pole of verticity, had a determinate direction, it must have been due to the force exerted upon it by some mass in its vicinity. The dip at Victory Harbour, according to Captain James Ross's observations †, is 88° 55′, and at Padliak 89° 17′; but even with this amount of dip I do not consider that the direction of the magnetic meridian could be ascertained, with anything like precision, by means of a Kater's azimuth compass, however accurate might be its construction, and with whatever care it might be used. At Cape Isabella, where Captain James Ross observed the dip 89° 22′, the north point of a Kater azimuth compass was directed to the north-west, but "its action was uncertain to eight or ten degrees ‡." Captain Back remarked this sluggishness of the compass-needles from the time he quitted Rock Rapid; his own,—a small Kater's azimuth very probably, certainly one of very delicate suspension,—he remarks, "frequently remained wherever it was placed, without evincing the slightest tendency to recover its polarity §." I have referred to these circumstances of the two expeditions, because they all tend to show, quite independently of the agreement which I have noticed among the results deduced from the observations, that at Point Ogle, at

* For further remarks on these observations, see Captain Back's Narrative, p. 415.

† Philosophical Transactions, 1834, p. 52. ‡ Ibid., p. 49. § Narrative, p. 378.

least as near an approach was made to the northern pole of verticity as at Padliak or Cape Isabella; and because the observations at Rock Rapid, the last point where, probably, the direction of the magnetic meridian could be ascertained with something like precision, would assign a position to this pole different from that assigned to it by Captain James Ross. As, however, I propose on some future occasion, investigating the positions which the observations on the continent and in the archipelago of North America would assign to the poles of verticity and convergence, I shall not now pursue the subject further.

I cannot conclude this discussion of Captain Back's highly interesting observations without expressing my own obligation to him for having placed them at my disposal, and, at the same time, my sense of the zeal he has shown in the prosecution of scientific inquiry. The undivided responsibility which rested upon Captain Back during the expedition, and particularly during the progress from Fort Reliance to the sea, by a navigation of unparalleled danger, was of no ordinary kind; and it was scarcely to be expected that, under such circumstances, any other observations than those absolutely requisite for the prosecution of the voyage would have been made. We however find that during this perilous and unknown navigation, Captain Back availed himself of every opportunity for making observations, necessarily tedious in their nature, and requiring much care and attention, but which he considered requisite for the attainment of particular scientific objects; and I feel I should be committing injustice were I not to express that I consider science is greatly indebted to him for the zeal and ability he has manifested in its cause.

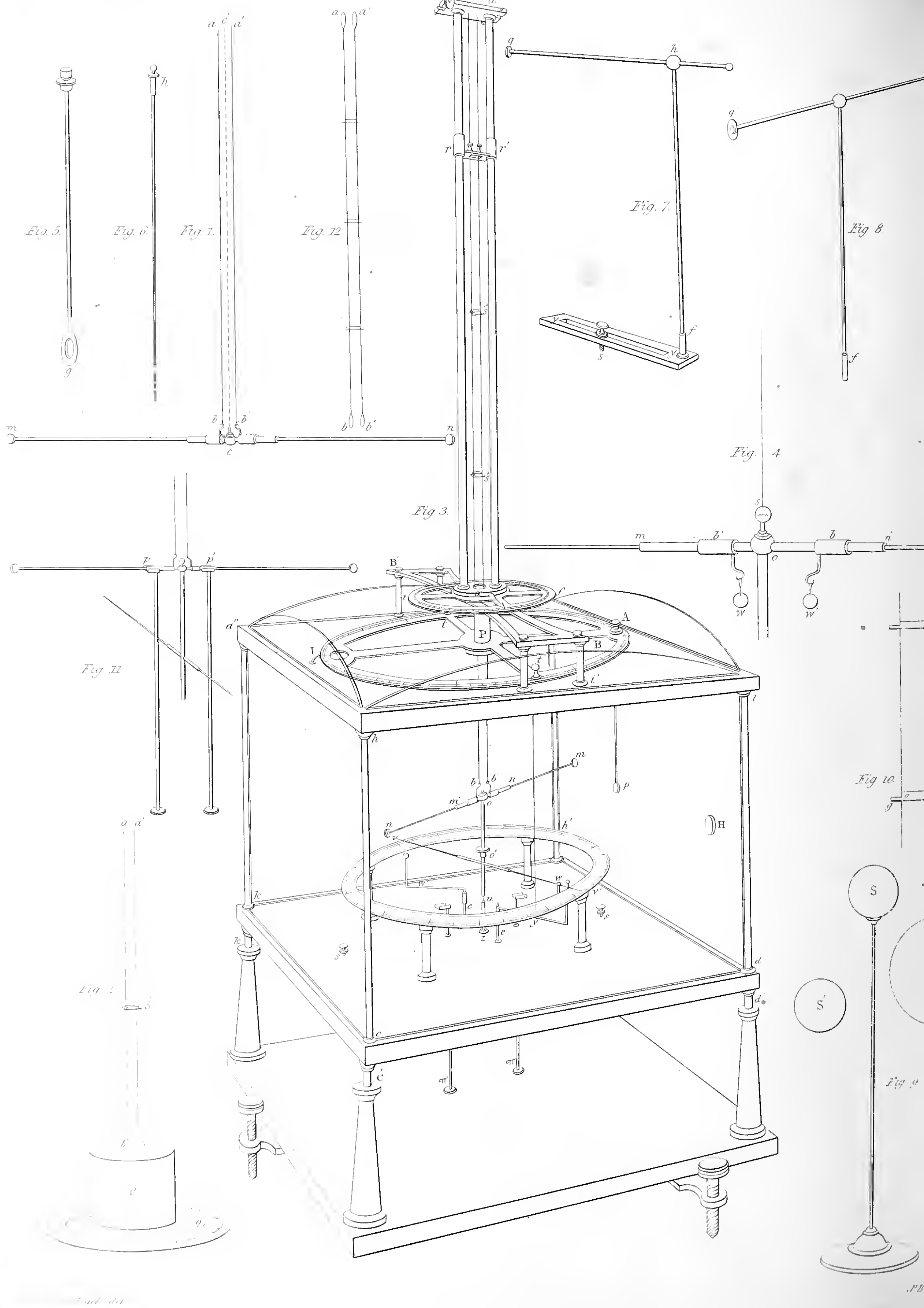
Fig. 5.
Fig. 6.
Fig. 1.
Fig. 12.
Fig. 7.
Fig 8.
Fig. 4
Fig 3.
Fig 11.
Fig 10.
Fig 9.

XX. *Inquiries concerning the Elementary Laws of Electricity. Second Series.*

By W. Snow Harris, *F.R.S. &c.*

Received May 11,—Read June 16, 1836.

1. HAVING frequently had occasion to employ the balance of torsion invented by Coulombe, on the indications of which so many important deductions in electricity rest, I have endeavoured at various times to free it from certain defects of a mechanical kind, and render it more completely available to the purposes of an electrometer. Some of the difficulties experienced by the experimentalist in the use of this valuable instrument had already engaged the attention of our talented countryman the late Professor Robinson of Edinburgh, who proposed an ingenious method of checking the swinging of the needle, generally set in motion whenever we turn the micrometer or electrify the insulated balls. In the course of these inquiries I was led to the construction of a new species of balance; it may be termed, from the peculiar mechanical principle on which it depends, a bifile balance. The reactive force of this instrument is not derived from any principle of elasticity, as in the balance of torsion, but is altogether dependent on gravity; it seems generally available in experimental physics, is extremely well adapted to the measurement of small forces of repulsion, and to researches in electricity and magnetism, and is easily converted into a common torsion balance when required, free from the difficulties before alluded to. A description of this new instrument, together with an account of some further researches into the elementary laws of electricity, may, I hope, be acceptable to the Royal Society.

2. If a needle $m\ n$, fig. 1, (Plate XXVIII.) be suspended by two equal and similar vertical filaments of silk without torsion, $a\ b$, $a'\ b'$, placed parallel to each other at equal distances from the centre $c'\ c$, and fixed at the points $a\ a'$, it is evident that its position of rest will be horizontal, and in the vertical plane passing through the two threads. Whenever, therefore, we turn the needle from this position about the imaginary axis $c\ c'$, the lines of suspension will become deflected from the vertical, so that the distance $c\ c'$ will be shortened. We have hence a reactive force derived from the weight of the needle, which becomes imparted, as it were, to the threads of suspension; since the centre of gravity of the mass will again tend to rest in its previous position, and will be in a similar condition to that of a body falling down a very small circular arc. If therefore the needle be freely abandoned to this reactive force, a vibratory motion will arise, by observing which we may determine by the formulæ for oscillating bodies the nature of the reactive force producing the oscillations.

3. With this view a cylinder of wood, P, fig. 2, of two inches in altitude and two inches diameter, was suspended by two parallel filaments of unspun silk, $a\,b$, $a'\,b'$, fixed in the points $b\,b'$ in a diameter of the upper surface of the cylinder at equal distances from its centre c, an index i being attached to it, for the purpose of observing on a graduated card $i\,g$ the duration and extent of the oscillations. The threads $a\,b$, $a'\,b'$ were suspended in a convenient frame, $s\,d\,e\,s$, Plate XXIX. fig. 13, sustained by a firm base A B, elevated on levelling screws; each thread, after passing through fine holes in a moveable bar $r\,r'$, was joined above to a strong piece of silk line $a\,a'$, continued through holes in the fixed bar $d\,e$, being finally attached to regulating pegs at d and e. By this arrangement the respective lengths of the threads of oscillation $a\,b$, $a'\,b'$ were readily adjusted, so as to cause the cylindrical weight P to hang parallel with the plane of the card $i\,g$, fig. 2. By changing the situation of the bar $r\,r'$, fig. 13, the lengths of the threads of oscillation could be varied; and by a succession of fine holes in the bar $r\,r'$ corresponding to holes in the cylindrical weight P, it was also easy to vary their distance apart.

4. Having obtained in this way a given length and distance between the threads, the centre of the cylinder c, fig. 2, was caused to hang immediately over the centre of the graduated circle $i\,g$, as shown by contact with the finely-pointed extremity of a vertical rod, passed through friction-corks in the central block D, fig. 13. Small stays, $s'\,s''\,s'''$, &c., of light reed or cork being now inserted between the threads at given distances (sect. 8. v.), in order to prevent them from collapsing; the index i was turned to an angle of 60°, and the weight P allowed to oscillate; the fine central point being subsequently depressed from beneath the base B B′, fig. 13.

Experiment A.—By carefully noting the rate of oscillation, the following results were immediately arrived at, viz.:

1°. The time of an oscillation is as the square root of the length of the threads of suspension divided by their distance apart, and is altogether independent of the weight of the oscillating body.

2°. The oscillations are isochronous at all angles.

5. From these results we may, by the general formula $n = \frac{P\,\pi^2\,a^2}{2\,g\,T^2}$, employed by Coulombe in his experiments on the torsion of wires, easily deduce the laws of the reactive force imparted to the threads.

In this formula, n is the force in terms of a unit of weight, = 1 grain, which, applied perpendicularly at the extremity of a lever of a unit of length, = 1 inch, will resist the reactive force imparted to the threads, when the cylinder c, figs. 2 and 13 has been turned about its axis, through an arc of 60°, whose chord is equal to the unit of length = radius. P is the weight of the cylinder c in terms of the unit of weight; a its radius; g the force of gravity, = 386 inches; T the time of an oscillation in seconds; π the ratio of the circumference to the diameter of the circle.

In applying this formula it is easy to perceive that the value of n will vary with

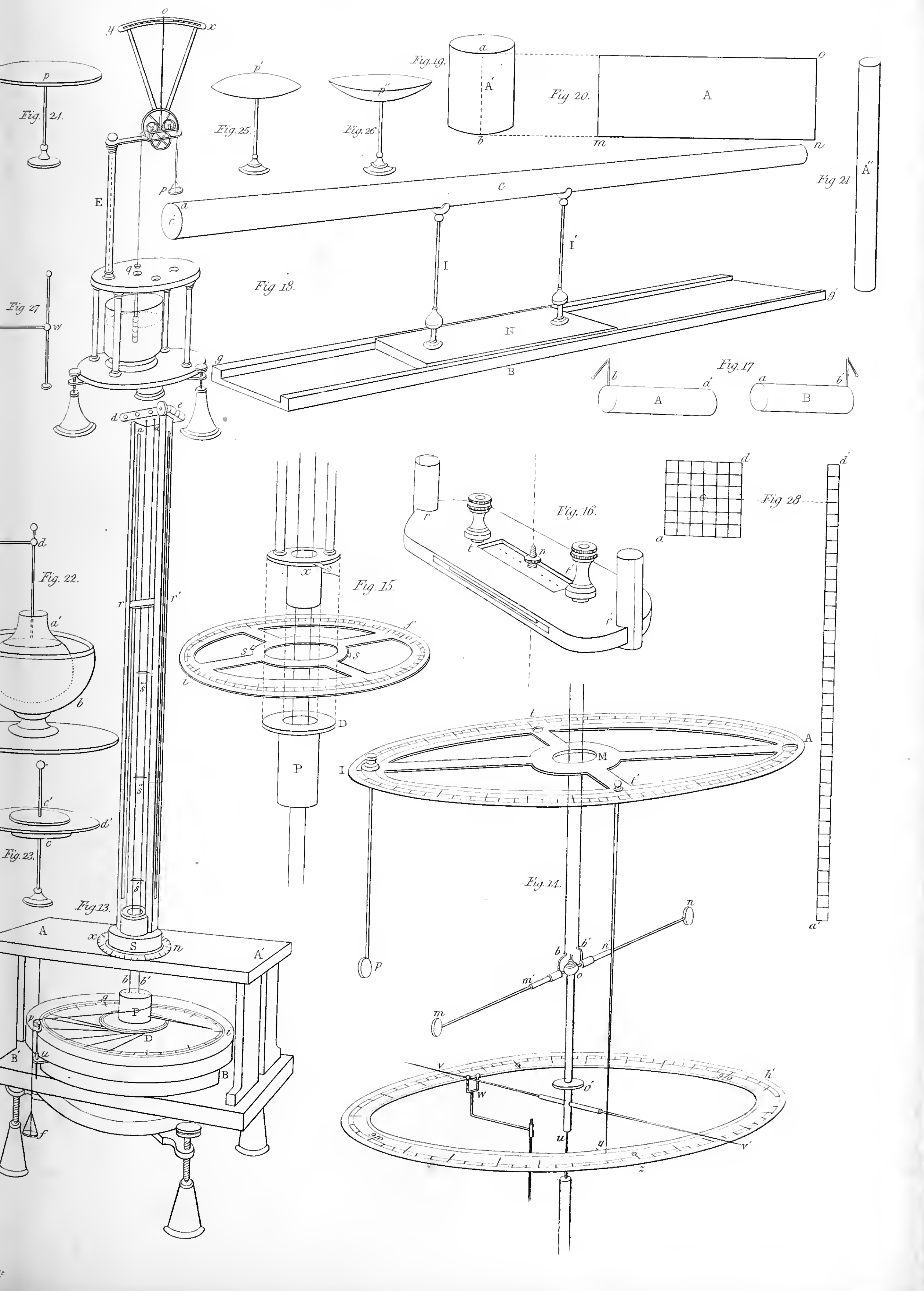
Fig. 13.
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Fig. 25.
Fig. 26.
Fig 27
Fig 28

the squares of the distances between the threads of oscillation divided by their lengths, and that, contrary to the law of torsion as deduced by COULOMBE, is as the weight of the cylinder P: hence we have $n \propto \frac{P\,d^2}{l}$; and since the oscillations of the cylinder are found by experiment to be isochronous at all angles (4.), we may conclude that n is also proportional to the angle of deflection of the threads, as shown by the index P i, fig. 13.

6. These results were verified experimentally in the following manner:

Experiment B.—A weight f was caused to act tangentially to the circumference of the cylinder P, fig. 13, by means of a slender filament of silk, $f\,p$ P, passing over an extremely delicate pully p. This pully was attached to a slide and socket u, fixed to the circular rim $u\,g\,i$, moveable about the interior block carrying the graduated card $p\,g\,i$; hence the line $f\,p$ P could be exactly set at right angles to the radius of deflection in all positions of the index i, and the precise weight determined requisite to balance the reactive force of the threads at any given angle, or otherwise by turning the whole frame of suspension $d\,s\,e$ in a circular socket formed in the transverse piece A A′ through any number of degrees, as measured by a graduated card $n\,x$ and index x, and preserving always the index P i of the cylindrical weight at zero, we arrive at the reactive force of the threads of suspension, at any required angle, in a similar manner.

The results of a series of experiments conducted in this way completely verified the above deductions, the weight requisite to maintain the index at an angle of 60° being as the weight of the cylinder P multiplied into the squares of the distances between the filaments of suspension divided by their length. It was also found to vary accurately with the angle of deflection, the attendant circumstances being unchanged.

7. The following Tables, abridged from a greater number of experiments than it is desirable to mention here, afford a sufficient practical evidence of the truth of these results. In these Tables the unit of weight is 1 grain, the unit of length 1 inch, the unit of time 1 second.

TABLE I.

Showing the Rate of Oscillation with different Lengths and Distances of the Threads, as in Sect. 4.

Weight of cylinder = 960 grains. Angle of oscillation 45°.

Length.	Distance.	Oscillations in 60″.	Time of ten oscillations by observation.	Time of one oscillation.
6	0·25	28·50	21	2·1
24		14·25	42	4·2
6	0·4	46	13·1	1·31
12		32·50	18·5	1·85
24		23	26·2	2·62
24	0·8	46	13·1	1·31

Similar results were obtained when the angle of oscillation was increased to 180° and upwards, as also when the weight of the cylinder P was varied from 960 to 480 and 240 grains respectively, the radius being in each constant. The rate of oscillation was taken with a valuable chronometer belonging to my friend Colonel HAMILTON SMITH, and by which portions of time so little as the one sixtieth part of a second could be well estimated.

TABLE II.

Showing experimentally the Weight in Grains requisite to resist the reactive Force of the Threads at an Angle of 60°, their Length and Distance apart being varied, as also the Altitude of the Cylinder P (Exp. B.).

Length.	Distance.	Weight in grains on a lever of 1 inch.		
		P = 960 grains.	P = 480 grains.	P = 240 grains.
6	0·25	2·675	1·325	0·66
24		0·67 +	0·325	0·15
6	0·4	7 +	3·525	1·75
12		3·55	1·775	0·885
24		1·75	0·875	0·425
24	0·8	6·85	3·425	1·750

The smaller weights employed in these experiments could not be considered as mathematically exact: they were, however, sufficiently accurate for the purposes required. They consisted of 10ths of grains, 20ths, 40ths, and 100ths. The numerical values in the above Table are those which resulted from the position of the index, so far as these small weights could determine; and it will be seen that the approximation to the values deducible from the preceding Table I., by means of the formula $n = \frac{P\,\pi^2 a^2}{2\,g\,l}$, are as near as could be expected from such an experiment.

TABLE III.

Showing the Weight in Grains, by Calculation and Experiment, required to balance the reactive Force of the Threads at various Angles of Deflection from 0 to 300°, the Threads being 24 inches in length and ·25 apart, and prevented from collapsing by seven small Stays, $s\ s'\ s''$, fig. 13, inserted between them at equal Distances (8.) (v.).

Weight of cylinder P = 960 grains.

Angle of deflection..	10	20	30	60	90	100	120	150	180	200	240	270	300
Force by formula ..	·115	·23	·34	·69	1·03	1·15	1·38	1·72	2·07	2·3	2·76	3·1	3·45
Force by experiment	·11 +	·22	·34 –	·67 +	1	1·1	1·35	1·7	2	2·25	2·725	3 +	3·425

The pulley *p*, fig. 13, employed in these experiments was extremely delicate. The small scale *f*, in which the weights were placed, weighed the ·1 of a grain, and was suspended by a filament of the thread of a silkworm; that part of it passing over the pulley being particularly slender and flexible. The ·01 of a grain was by this means rendered very sensible on the index. The approximations in this Table are sufficiently close to show that the reactive force of the threads is as the angle of deflection (Exp. A.).

8. Upon these data the electrometer represented in fig. 3 has been contrived, which the following description will, I hope, render sufficiently intelligible.

α. Fig. 3, $a'' d$ is a cubical box or cage of about 14 inches square and 10 inches high; its vertical faces consist of large panes of glass loosely inserted in the grooved edges of light mahogany pillars about ·2 of an inch in diameter; *h c*, *d l* represent two of the columns, and *d h* one of the glass faces. The upper edges of these panes project above, so as to be easily removed when requisite.

The base of the cage, *d k*, consists of a clamped square of well-seasoned mahogany; the boundary $a'' h l f$ of the upper part or roof is a stout framework of the same wood, through which the glass faces pass: the frame is firmly connected to the base by the intervention of the wood columns; each column is connected to the framework above by short brass shoulders fixed in their upper ends, and screwed into small plates of brass at the angular parts of the frame, as at the points *h l*; the horizontal grooves for the passage of the glass sides are continued through these plates in common with the woodwork of the frame; the base *k d* is secured beneath by similar shoulders fixed in the lower ends of the columns: these last pass freely through the base at the lower angles *k c d* into a flattened spherical nut, by which the whole is hove tight; each nut terminates in a cylindrical brass leg about an inch and a half in length and a quarter of an inch thick, as represented at $k' c' d'$. When the nuts are hove tight, the skeleton of the cage is very firm and complete: the cylindrical legs keep the instrument raised for about two inches above the table; they also serve to steady it on four pillars, as at $k' c' d'$, each five inches in length, and upon which the whole instrument is occasionally elevated for the purposes of experiment. The pillars last mentioned are united below to a clamped square of mahogany supported on levelling-screws, and have holes drilled into their upper ends for the reception of the cylindrical feet just mentioned.

β. An insulating needle *m n*, figs. 3 and 14, ten inches in length, is suspended within the cage by two vertical filaments of silk, *a b*, $a' b'$: it is connected with an index $v v'$, by means of a vertical rod *o u*, fixed to its centre. This index is about nine inches long, is set at right angles to the direction of the needle *m n*, and points out on a graduated circle, $v h' v'$, the angle of deflection from the point of rest. The graduated circle $v h' v'$, figs. 3 and 14, upon which the angular deflections of the needle are thus shown, is about nine inches in diameter: it is supported on four small pillars about an inch high, its centre being coincident with the central point at *u*.

γ. The needle *m n*, with its index, &c., is constructed in the following way: two light arms of glass, $m' n$, $n' m$, figs. 3 and 14, are inserted into the opposite ends of a small connecting joint, $m' n'$, about two inches or more in length, and ·2 of an inch in diameter, the whole forming one straight line. The connecting joint $m' n'$ consists of two pieces of light brass tube attached to opposite points of a small solid brass cylinder by short projecting studs: the central portion of the cylinder receives the extremity of the vertical rod *o u*. There are two sliders of brass on the connecting cylindrical tubes, which travel on them with a steady friction, and carry the hooks $b\ b'$ for the attachment of the suspension silks, which may be thus placed at any distance apart.

δ. A small circular area, *n*, of gold plate or light wood gilded, about ·4 of an inch in diameter and ·05 of an inch thick, with smoothed edges, is fixed to one extremity of the needle; and a small coated circle, *m*, of very thin varnished glass, or otherwise a similar disc, to the other. The coatings of the glass circle consist of circular pieces of gilt paper of about ·4 of an inch in diameter. This little element, when charged in the usual way with either electricity, maintains a very steady electrical state, not being liable to the variations which frequently arise in a simple insulated conductor; a property of great consequence in refined experiments, since in examining, from time to time, an insulated and similarly charged disc, *p*, figs. 3 and 14, or *q* and q', figs. 7 and 8, introduced within the cage, we thus obtain a more accurate measure of its electrical conditions than is usually arrived at by the electrization of a simple conductor. The small circular discs just mentioned are attached to the extremities of the needle *m n* by a cement of shell lac, a portion of the glass rod near the end being ground away or drawn out in the flame of a lamp for a short distance so as to receive them; and thus the plane of the disc coincides with the axis of the rod. When the disc is of gilded wood, a small hole is drilled into its edge, or a corresponding flattened groove cut out of it for about two thirds of the radius, by which a similar result is obtained. The glass arms of the needle sustaining the discs are each covered with a thin coating of shell lac or good sealing-wax, the glass being previously cleaned, and heated sufficiently to melt the wax.

ε. The vertical brass rod *o u*, carrying the index *v v*, is about 4½ inches long, and ·25 of an inch in diameter; its upper end terminates in a short shoulder; this shoulder passes through the centre of the connecting joint $m' n'$, and has a screw cut on it: there is a nut fitted to this screw, by which the two become completely united, and accurately set at right angles to each other.

ζ. The needle *m n*, with its index, &c., is suspended from the light frame $a\ x\ a'$, fig. 3: the suspension silks are connected by loops to the hooks $b\ b'$, and after passing through fine holes in the moveable plate $r\ r'$, and other holes in the fixed bar *a a* above it, are secured round the moveable pins $a\ a'$ for the convenience of adjustment.

η. The index $v\ v'$ is fixed horizontally to the rod *o u*, exactly at right angles to the direction of the needle *m n*. It consists of pieces of light reeds of straw inserted one on each side in thin spring tubes; these are fixed on the ends of a short wire of brass about 2 inches in length, accurately passed through the rod *o u*, about $4\frac{1}{2}$ inches below the needle, so as to project from it for about $\frac{1}{2}$ an inch or more. A perfectly straight line may be obtained in this way by constructing each arm of two or more pieces of straw reed, fitted at their extremities one within the other, so as to present a sort of tapering appearance, the last piece being cut to a point, after the manner of a common writing-pen. Immediately over this index there is a sort of flattened stage of sheet brass, fitted to the rod by a spring tube, for receiving small circular weights, by which the reactive force imparted to the threads may be occasionally increased (6.). The weight of the needle, with its index, &c., is about 480 grains.

The lower extremity of the index-rod at *u* has a conical hole drilled up it, which allows the whole to play freely about a central pivot fixed in the end of a cylindrical rod *u z*. This rod passes through a spring socket immediately in the centre of the base of the cage, so as to be easily elevated or depressed by means of a milled head beneath, in which it terminates.

ϑ. A little on one side of the central point *z*, but very near it, two other brass rods, *e e'*, pass through the base in a similar way to the former; one of these, *e*, carries a bent rod *y w*, fixed on it by a small spring socket, and terminating in a sort of fork, *w*, about ·5 of an inch in width. One of the arms of the index *v v'* may, by elevating or depressing the rod, be caused to pass through the fork, and thus the oscillations of the needle become completely checked in any point, or confined within a very limited number of degrees; the forked portions of the lever *w* are covered with some soft substance, so as to prevent, as much as possible, the index from rebounding. In this way by turning the rod *e* from beneath the base, the repulsive force acting on the needle is completely restrained, and gradually eased off to the point at which it becomes balanced by the reactive force of the threads. The opposite rod *e'* also carries a bent rod *e' w'*, by which we may at any time act on the other arm of the index, and thus obtain a very complete command over the oscillations of the needle. These two bent levers *w w'* may be changed to either side, or one of them only may be employed, according as it suits the convenience of the experimentalist.

ι. Two other rods, *π π*, fig. 3, pass with friction through the base of the cage, in a line perpendicular to that of the rods *e' e*; these carry two small stages temporarily fixed on the extremities of the rods by spring sockets, to which they are attached. The milled heads of these rods are seen projecting under the base. The stages on elevating the rods support and bear up the needle *m n*, at the time of suspending it, or when the instrument is not in use, or otherwise when it is required to be moved about from one place to another. When the rods are elevated the connecting joint *m' n'* of the needle is received between the perpendicular sides of the stages, as at *p p*, fig. 11; and thus it is at once supported and protected from damage, and its weight taken off the threads of suspension. The stages being temporarily fixed on the rods may be removed, when required, at the time of using the instrument, and the ends of the rods depressed below the level of the base.

κ. The upper part or roof $a'' l$ of the cage $a'' d$ is, as already stated (α.), a wood frame, inclosing a circular opening of about a foot in diameter; this opening is just covered by the circle A t I, which is about 13 inches in diameter, and ·3 of an inch thick. The circumference of this circle is about an inch wide, and is divided upon its upper surface in each quadrant from 0 to 90°. It has two cross pieces or arms, $t\,t'$, A I, figs. 3 and 14, at right angles to each other; through the intersection of these there is a large round hole, M, fig. 14, for receiving the hollow vertical pivot, P, figs. 3 and 15, by which the circle may be accurately turned about its centre. The openings between the cross arms are covered in by thin glass plates, secured to the brass underneath by the heads of small screws. These screws project over the edges of the plates, the glass being defended by a thin leather collar. The under surface of the circle A t I is ground flat, and is smeared with a little grease of some kind, where it bears on the framework beneath, in order to allow of motion about the central pivot with an easy friction: the angular quantity by which this circle is at any time turned, is shown by a small bent index, I, outside the circumference, fixed either at I or A*.

λ. One of the cross bars A I carries the insulated disc p, or an insulated coated disc of glass g, fig. 5. These discs are similar to those of the needle already described. Each disc is insulated on a slender varnished glass rod, moveable in a spring socket, fixed in a plug of wood, as represented in fig. 5, so as to be readily adjusted to a given length. The plugs A or I, figs. 3 and 14, fit accurately in corresponding holes passing through the arms of the brass circle; they are placed in two similar and opposite points of the diameter A I, their centres being 10 inches apart, or a distance just equal to the length of the needle. By this arrangement either of the discs, or any other body, may be introduced within the cage of the instrument at pleasure, and the action on the needle observed.

μ. There is a vertical index-rod, $t'\,y$, figs. 3 and 14, in the diameter $t\,t'$ 90° distant from the holes A I, pointing immediately over zero of the graduated circle $v\,v'$ when the needle is in its position of rest, and the two discs $n\,p$ just touch. This index is sustained in a spring tube fixed in a neat wood plug, h, fig. 6, so as to be readily adjusted to any length, and can be applied at the opposite side t, if required.

ν. Beside the discs n, p, m, above mentioned, we may, by raising one of the glass faces, occasionally place within the cage any other electrified body whose condition we wish to examine, or otherwise small insulated proof planes, such as $q\,q'$, figs. 7 and 8. These are insulated on slender rods of varnished glass, $q\,h$, $h\,f$, fig. 7, set by an intervening ball of wood at right angles to each other. The horizontal rods vary from 2 to 5 inches in length; the vertical insulators are from 4 to 5 inches in length;

* The circle A t I may be of wood, having a graduated circle of card-board fixed on it; the remaining graduated circles may be also of stout card-board, where economy is an object.

these last are cemented below into a neat brass ferule, *f*, so as to be easily inserted when in use into a spring socket, *y*, fig. 7, attached to a narrow plate of brass or wood *y y'*. This plate is temporarily fixed in the base of the cage, as at *s*, fig. 3, in a line perpendicular to the direction of the needle when at rest. A portion of the interior of the plate is removed, as at *y y'*, and a screw, *s*, figs. 7 and 3, terminating in a milled head passed through it into a nut fixed in the wood underneath; the whole may be thus accurately adjusted so as to bring the proof plate *q* just in contact with the disc of the needle, when the index *v v* is at zero.

Either of the discs above mentioned may be connected with an insulated charged body, S, fig. 9, through a round hole H drilled in one of the glass sides of the cage: this hole is about ·6 of an inch in diameter, and is placed so as to admit of the external body being connected with the back of the disc; the communication is effected through the intervention of a light wire, which we may suppose to join *p* S, figs. 3 and 9. The wire is supported horizontally in a neat ball of wood attached to the extremity of a vertical rod of glass, placed in the base of the cage in the way already described (ν.), fig. 7.

One of the extremities of the connecting wire terminates in a fine point to be inserted in the back of the fixed disc *p* opposed to the disc of the needle; and the other extremity either in a small knob or a point, as the circumstances of the experiment require.

ξ. The brass circle A *t* I turns freely about its centre under a light transverse piece of brass, B B', about 4 inches in width at its extremities, and ·3 of an inch in thickness: this piece is fixed on short vertical pillars *i i'*, &c., so as to be raised above the roof of the cage, the whole being secured by screws and nuts.

ο. The transverse piece B B' is accurately centered, and a circular opening drilled through the centre at *x* of about an inch or more in diameter; for the sake of lightness it is formed as represented in the figure. Immediately in the centre of this piece there is united to it a short plate of brass, D, fig. 15, three inches in diameter and three tenths of an inch thick. This plate has a drawn brass tube, P, of an inch bore, fixed underneath it, which, passing centrally through the frame B B', retains the circle A *t* I, figs. 3 and 14, in its place beneath. There is a similar plate and descending tube, *x*, figs. 3 and 15, placed immediately over this, carrying the frame *a x a'* for the suspension threads of the needle. The hollow tubes fit closely one within the other, as indicated in fig. 15; and the faces of contact of the two plates are ground fair together, so as to admit of the upper plate being turned upon the lower one without materially deranging its vertical position.

π. In order to estimate the angular quantity which the upper plate has been turned, there is a graduated circle, *i f*, figs. 3 and 15, of six inches in diameter, sustained without the edge of the fixed plate D by light arms projecting from an interior ring. This ring is accurately fitted to the circular edge of the fixed plate within, so as to admit of its turning upon it as a centre for a short distance, with some friction; by this the zero of the circle can be nicely set to any given point. The whole is then finally fixed in the required position by two small screws at $s\ s'$, fig. 15, passing through the interior ring against the edge of the plate within.

ϱ. Each quadrant of the graduated circle $i\ f$ is divided from 0 to 90°; and there is an index x projecting from the moveable plate above immediately over the divisions, indicating the angular quantity it has been turned, and consequently the amount of the deflective force impressed upon the needle $m\ n$ through its threads of suspension.

This arrangement, especially represented in fig. 15, enables us to retain the brass circle A I t and the frame $a\ x\ a'$ in their respective situations, without interfering with the required circular motions or the opening into the cage below for the threads of suspension (ζ.).

σ. The frame $a\ x\ a'$ may vary from one foot to three feet, according to the views of the experimentalist; it is slightly but firmly constructed of two light brass tubes, $x\ a$, $x\ a'$; these are received upon two short vertical rods, screwed into the plate x, and are united by a cross piece $a\ a'$ above. The extremities of this cross piece carry spring sockets for the two adjusting pegs $a\ a'$ already mentioned.

τ. The brass plate for regulating the length and distance of the suspension threads (β.) is steadied between two parallel pieces $r\ r'$ by means of small screws which tie them together; sufficient space being allowed for placing it accurately in the centre. The compressing pieces are united at each end by cross bars, and are soldered to short tubes which slide with friction on the vertical rods $x\ a$, $x\ a'$, as shown in fig. 16.

υ. The small stays $s\ s'$, fig. 3, inserted at given distances between the suspension threads, are formed and applied in the following manner. A short piece of finely grained cork, $g\ g'$, fig. 10, of the required length, about ·2 of an inch wide and ·1 of an inch in thickness, is cut nearly through at each extremity, and a horizontal portion $g\ a$, $g'\ a'$, removed, leaving a raised portion $a'\ a$ in the centre, exactly equal to the given distance between the threads. The filaments of suspension being each passed into the eye of a fine needle, are lightly smeared with a little oil, and run through the cork in the points of intersection of the depressed portion $g\ a$, $g'\ a'$, and the central straight line parallel to its sides. The stays will now hang with sufficient friction between the threads, and may be placed in the points required.

φ. In order to facilitate any required change in the sensibility of the instrument by varying the distance between the threads of suspension (5.), several sets are prepared at different distances apart, terminating above and below in small loops, as represented in fig. 12. The upper ends being passed into the eye of a fine needle, are easily run upwards through the holes in the plate $r\ r'$, fig. 3, at the given distances, and hooked on to the stout threads $a\ a'$ above, and to the hooks $b\ b'$ of the needle below.

9. When we wish to employ this instrument as a balance of torsion (1.), the needle, with its index, &c., is suspended by a metallic wire in the following way:—there is a small shoulder at the extremity of the rod $u\ o$, figs. 3 and 4, through which a small

hole is drilled, passing obliquely from the centre out at the side just below. The wire is passed through this hole, and its end finally secured by a small brass spherule *s*, fig. 4, accurately fitted to the end of the shoulder. When the brass spherule is gently forced into its situation, the wire is compressed, and becomes firmly united to the needle. Being thus secured below, it is continued through a fine central hole in the moveable plate $r\, r'$, fig. 3, and finally attached to an adjusting cylindrical peg at a'. It is completely stopped at any given length by a small conical screw and nut *n* fixed in the plate, fig. 16, and through which the wire passes; by tightening the nut the wire is compressed, and cannot therefore twist beyond a given point.

In order to balance the needle accurately, should it be required, the two sliders $b\, b'$, fig. 4, are turned so as to bring the suspension hooks underneath; this enables us to append to the hooks two small weights $w\, w'$, and place them at such respective distances from the centre as will accurately balance the needle *m n* in a horizontal position*.

The instrument thus prepared becomes, by means of the micrometer at *x*, a very complete torsion balance, possessing many important advantages; amongst these is the means of increasing or diminishing its susceptibility by changing the length of the wire, Coulombe having shown that the reactive force of a wire is in a simple inverse ratio of its length.

10. The reactive force obtained in the way just shown, by means of two parallel filaments of silk, is more perfect, in a great variety of instances, than the elastic force of torsion, and is besides very manageable. The deflection of the threads from the vertical may at all times be extremely little; and the angular deviation of the needle seldom greatly exceeds 90°, although by increasing the number of stays $s'\, s''$, &c., fig. 12, it may with safety be carried through the whole circle (7.); for many refined inquiries, however, the angle of deflection need not exceed 30°. We must, however, remark, that in this, as in the case of the torsion balance, an error may arise in large arcs by taking the arc itself as the measure of the distance between the opposed bodies *p n*, fig. 3, and the length of the lever at the extremity of which the force acts, as equal to radius or half the length of the needle: these errors, however, in the instrument we have been describing, sufficiently balance each other, or very nearly so; one of the factors of the moment of the force being the cosine of half the angle of deflection, and consequently less than radius, or the lever upon which the repulsion operates, whilst the arc taken to measure the distance is always greater than its chord, or the real distance.

11. It is easy to perceive that the reactive force imparted to the threads of suspension may be varied in any proportion, either by changing the position of the sliding bar $r\, r'$, by which their length is altered, or otherwise by varying their distance apart (9.); or finally, by the due adjustment of small circular weights placed on the

* A separate needle may be employed for this purpose, if desired.

small stage at o'; we may by either of these methods, taken separately or conjointly, vary the reactive power between very wide limits. In the instrument above mentioned a force so small as the $\frac{1}{20,000}$dth part of a grain for each degree is readily attained, and it may be diminished to the $\frac{1}{50,000}$dth.

12. The construction of this instrument having been completely described, a brief experimental explanation will suffice to make its practical application to the purposes of an electrometer easy and intelligible.

Let it, for example, be required to investigate the laws of the repulsive force exerted between the insulated bodies *p m*, figs. 3 and 14, under the variable conditions of distance, intensity of charge, and the like.

The instrument being elevated on its vertical pillars, as represented in fig. 3, is first levelled, and the central rod *o u* made, by a due adjustment of the plate $t\,t'$, fig. 16, to hang freely over the vertical pivot at *u*, at whatever angle the frame $a\,x\,a'$ may be turned: the disc *m* of the needle is now adjusted, so as just to touch the fixed disc *p* in every point, or nearly so, the forked lever *w* being employed to retain the index $v\,v'$ at zero. The fixed indexes I and $t'\,y$ are then also set at zero of their circles A *t i* and $v\,h\,v'$, the forked lever *w* arresting, without undue pressure, the discs *m* and *p* in contact, when the index $v\,v'$ is at zero. We now proceed to electrify the discs by means of any insulated charged body introduced within the cage, either through the circular hole H or under one of the glass sides, which can be raised for the purpose; this done, we turn the circle A *t* I through any given number of degrees in a direction *contrary* to that in which the needle would be repelled, the forked lever *w*, figs. 3 and 14, being gently eased away until the repulsive force becomes exactly balanced. In this case, as is evident, the distance between the repelling surfaces is measured by the number of degrees of the graduated circle $v\,h'\,v'$, intercepted between the vertical and horizontal indexes $t'\,y$, $v\,v'$, and the force itself by the number of degrees of the same circle contained between the final position of the index $v\,v'$ and zero. Suppose, for example, the repelling discs were actually circumstanced as in fig. 14, then their distance apart is expressed by the arc $y\,v'$, and the force, by the arc $z\,v'$, the point *z* being zero of the card. If the point *y* coincided with *z*, then both the distance and force would be expressed by the arc $z\,v'$. If the index $t'\,y$ be not employed, we estimate the distance of the bodies by means of the angular quantity indicated on the brass circle A *t* I, which must be in this case added to the angular deflection of the needle from zero. Let it now be required to examine the repulsive force at any other distance less than the former. We have only to move the circle A *t* I in the same direction as that in which the needle is repelled, and we immediately change the relation between the arcs $y\,v'$ and $z\,v'$, the receding of the needle being at the same time gently checked by the fork of the lever *w*. We thus eventually obtain a new distance and force, measured in terms of the same graduated circle.

In this kind of inquiry we may, as it is evident, obtain deflections of the needle from less than a single degree up to 360 degrees if requisite (7.) (Table III.), by turning the circle A t I either in a direction contrary to that in which the needle is repulsed, or otherwise in the same direction. We may in either case always estimate the distance of the repelling bodies independently of the vertical index $t\,y$, by adding or subtracting the angular turning of the circle A t I, to or from the angular deflection of the needle, according to the direction in which it has been moved.

If the disc m be connected with any external body, s, figs. 3 and 9, then the relative distances and forces may be varied by turning the micrometer index x a given number of degrees, either in the same or in a direction contrary to that in which the needle is repelled, which must be either added to, or subtracted from the total angular deviation of the index $v\,v'$, according as the direction is + or − in respect of the direction of the force of repulsion. Thus if, as in fig. 14, the deviation of the needle as expressed by the index $v\,v'$ amounted at first to 20°, and by subsequently turning the index x, figs. 3 and 15, by a quantity equal to 70°, in a direction contrary to that in which the needle was repelled, we had brought the index $v\,v'$ back to a distance of 10°, then the total force at 10° would be expressed by 10° + 70°, that is, 80°. If, on the contrary, we had turned the micrometer index x 30° in the same direction as that of the repulsion, and had by this advanced the index, say to a distance of 40°, then the total force at 40° would be expressed by 40° − 30°, that is, 10° only.

With respect to the distances of the electrified discs $p\,m$, they are always measured with sufficient accuracy by the arc contained between the vertical index $t\,y$ and horizontal index $v\,v'$, or by adding or subtracting the angular turning of the circle A t I.

13. There are one or two circumstances connected with the investigation of the laws of electrical action by means of this instrument, or the balance of torsion, requiring some attention. 1°. If any considerable time be employed in a particular experiment, the charged bodies will, under ordinary circumstances, lose some portion of their electricity. Coulombe endeavoured to avoid the error necessarily arising from this, by computing the amount of the electrical dissipation and applying a correction, and by some other methods incidental to the nature of the given inquiry. This process, however, is not always easy, or even safe; the dissipation of a charge being frequently a most uncertain operation; and a little more or a little less allowance for the assumed decrease of the charge makes an astonishing difference in delicate researches, the force by which a body loses its charge being as the square of the quantity of electricity. The only sure protection from this source of fallacy is to avoid it altogether, which can be always effected if we choose a proper season for experiment, and operate in a dry room moderately warmed by a common drying stove. It is surprising under these circumstances to see how completely the repelling bodies retain the charge; this, together with the facility of manipulating obtained by the instrument above mentioned, will be found to reduce indefinitely any error arising from dissipation. 2°. Coulombe found that some insulators did not insulate sufficiently, and were liable to become electrified; in consequence of this

his results were sometimes uncertain. We may also avoid this by exposing the insulators to the influence of small irons heated beyond redness, and prepared of various forms for the occasion, which at once deprives them of any electricity they may have taken up, and renders their insulating power sufficiently perfect. I have found slender glass rods covered with a varnish of coarse shell lac laid on warm, or otherwise good sealing-wax, become extremely perfect insulators when treated in this way.

14. The relation of the repulsive force between two bodies, as *p, m*, figs. 3 and 14, to the quantity of electricity with which each is charged, and to their distance apart, is of vital consequence to electrical investigations depending on the principle of repulsion. According to the experiments of M. COULOMBE, the total force between two insulated charged bodies, is as the quantity of electricity in each directly, and as the squares of the distances inversely*. Hence he supposes the action of each of the repelling bodies to enter into the composition of the result in such way, that the total force increases or diminishes with the electricity contained in either.

Thus the total force with which two bodies considered as points repel each other at any distance, D, is represented by $\frac{F}{D^2}$, or considering F as the product of two constants R R′, that is to say, of the force of each body, this expression becomes $\frac{R R'}{D^2}$. BIOT terms these constants R R′ the electrical reaction of the bodies to which they apply. Although this expression coincides in many cases with the results of experiment, it does not seem upon the whole sufficiently general. Having found many important exceptions to it, I was led to institute a series of experiments with a view of discovering the cases in which it might possibly fail.

Experiment C.—The results given in the following Table were deduced by repeatedly examining the repulsive force exerted between the electrified discs *p, m*, fig. 3, placed in a perfectly insulating atmosphere (13.) at various distances apart, and charged with equal and unequal quantities of electricity. The relative charges were obtained by the simple method resorted to by COULOMBE, viz. by touching one of the bodies with an insulated neutral body of the same dimensions, so as to abstract at any time one half the charge. In these experiments the quantity requisite to produce a repulsive force of 24° when equally divided between the two discs was taken as a unit of charge; the reactive force of the instrument being about the $\frac{1}{1500}$dth of a grain for each degree. I then proceeded to reduce the charge upon one of the discs in such given proportions as could be obtained by continually abstracting one half the quantity remaining on either disc. In deducing the results each disc was in turn made constant, and a mean result taken; this did not, however, upon the whole, greatly differ from that arrived at by one disc alone. The length of the threads of suspension was taken at 20 inches, and their distance apart ·25 of an inch,

* HAÜY's Philosophy, by GREGORY, p. 356 and 364.

† BIOT, Traité de Physique, tom. ii. p. 242.

the reactive force at 60°, being about the $\frac{1}{25}$th of a grain, that is, about the $\frac{1}{1500}$dth of a grain for each degree, as already mentioned.

Table IV.

Distances.		Forces with charges equally reduced.			Forces with charges reduced and unequal.					
Degrees.	Ratio of distance.	1 : 1	$\frac{1}{2}:\frac{1}{2}$	$\frac{1}{4}:\frac{1}{4}$	$1:\frac{1}{2}$	$1:\frac{1}{4}$	$1:\frac{1}{8}$	$\frac{1}{2}:\frac{1}{4}$	$\frac{1}{2}:\frac{1}{8}$	$\frac{1}{4}:\frac{1}{8}$
2	$\frac{1}{12}$	....	....	46	....	Attracted.	Attracted.	....	Attracted.	
3	$\frac{1}{8}$	....	....	36	....	62	30	55	29	16
6	$\frac{1}{4}$	....	85	18	94	46	22	30	16	8
9	$\frac{3}{8}$	125	42	8+	70	34	16−	14	8	4·5
12	$\frac{1}{2}$	94	23	4·5	47	23	10+	8+	5−	2+
18	$\frac{3}{4}$	44	10·5	2+	20	10	5	3+	2	....
24	1	24	6	1+	12	6	3	2	1+	....
48	2	6	1·5	....	3	1·5	....	....	....	....
72	3	3	....	....	1+	....	....	....	....	....
D	R	A	B	C	*a*	*b*	*c*	*d*	*e*	*f*

15. The above Table exhibits the following phenomena, which are not a little striking and important.

1st. It may be perceived, that in columns A and B, the discs being charged equally and to a given intensity, the forces vary, (with one or two exceptions only,) in an inverse ratio of the squares of the respective distances: in column A there is only one exception, viz. at the distance 9°. When, however, we begin to diminish the quantity on one of the discs, or charge them unequally, this law is only apparent up to a certain limit. Thus at the distance 12° and 6° of columns *a*, *b*, and *c*, as also at 12° and 9° of column A, 6° and 9° of column C, the law is in an inverse ratio of the simple distance, or nearly approaching to it, whilst within certain limits, and at other distances, the law of the force becomes irregular, and is apparently disturbed by some foreign influence. Similar results are more or less apparent in all the other columns where the quantities of electricity in the repelling bodies are unequal.

2nd. It is observable that the deviations from the law of $\frac{1}{d^2}$ are more apparent and decided, when the forces are more diminished, the inequality of the respective charges greater, and the distance less: under any or all of these conditions, the rate of increase of the repulsive forces diminishes, and the repulsion verges toward, and is at length superseded by, attraction, as may be seen by a slight inspection of the Table.

3rd. The quantities of electricity contained in either of the repelling bodies are not always proportional to the repulsive forces. Thus in columns B and *d* the respective quantities on one of the bodies are as 2 : 1, the quantity on the other remaining the same, $= \frac{1}{2}$; but the respective repulsive forces are nearly as 3 : 1, or at least approximate very closely to this ratio. The same thing is, in some instances, apparent in columns *b* and *d*, where the quantities are in the proportions of $1:\frac{1}{4}$, and $\frac{1}{2}:\frac{1}{4}$. We likewise observe in columns B and C that the respective quantities are as 2 : 1 on each of the discs; but the corresponding forces are not as 4 : 1, but nearly as 5 : 1 throughout.

16. Although these results may seem at first anomalous and unsatisfactory, they are still such as would be likely to arise out of the peculiar nature of electrical action, and may I believe, on due reflection, be found in complete accordance with its general laws. It may be shown, for example, that the force of induction in electricity is not confined to a charged and neutral body, but operates more or less freely, even between bodies *similarly* charged. Now whatever be the precise nature of the inductive process, it is present in every species of electrical action, although under certain circumstances its tendency to attraction may not be apparent, or be of a negative character.

Experiment D.—If, for example, two charged cylindrical conductors, A and B, fig. 17, have electroscopes, b b', connected with their distant ends, then, on approximating their near extremities a a', we shall observe a continual increase of force in the opposite ends b b' which will continue up to a certain limit according to the quantity of electricity with which the conductors are charged, and whether equally or unequally.

Now the inductive force of these cylinders on each other, as here evinced, may (in accordance with the doctrine of a single fluid, and merely *pour fixer les idées*,) be supposed to repel the electricity resident at their near extremities a' a, and cause it to retire towards the distant ends b' b. If the conductors A and B at the instant of this process be free to move, then the resistance to the increased accumulation towards the extremities b b' is attended by a mutual recession of the bodies, and they seem to repel each other. We do not however, under any circumstances, obtain really the whole repulsive force of which the quantities of electricity at first collected in the near extremities a a' would seem to be susceptible, since the operation of the inductive influence may be associated with a displacement of some of the agency on which the repulsion depends, and by which the quantity in the repelling ends is more or less diminished. If, however, the bodies be equally and highly charged, there may, in certain cases, be so little displacement at some distances, in comparison with the whole quantity accumulated, that the decrease of the force from this cause is of no great value. Supposing, however, the charges unequal, then the resistance in one body is greatly decreased, and there may arise so great a change in its repelling extremity, by the inductive influence, as to cause a very sensible diminution of the comparative repulsive force exerted at a given distance between the two bodies. If the disproportion of the respective charges be very great, then not only could all the free electricity of one of them, A, become displaced in its proximate extremity a', and so be actually reduced to a state of neutrality, but a further induction may arise, such as always occurs in the case of any other approximated neutral and charged surface, and which, although not so perfect as under the ordinary circumstances, owing to the accumulation of the free electricity toward the opposite extremity b, may still generate a very sensible attractive force; the tendency of the inductive process being, first to raise the anti-attractive state of the bodies to zero; secondly, to generate in them an actual attractive power. There is consequently no essential distinction in this action, whether it take place between two bodies each similarly charged, or between a charged and neutral body, or even between two bodies dissimilarly charged; the only difference being, that in the latter case the inductions commence at a point already beyond a limit, which may be called zero; in the two former they commence in the one case at zero, in the other at some point below it. I hope to lay before the Royal Society, at no very distant period, some new researches on electrical induction and attraction, which may possibly throw some further light on the nature of electrical forces. It is quite evident that the inductive process between two bodies similarly charged may become indefinitely modified by the various circumstances of quantity, intensity, distance of the charged bodies, and the like; giving rise to apparently complicated phenomena, as I think appear in the results of the experiments just given in Table IV. Thus in column A, where the charges on the discs are equal and of a given intensity, the resistance in one body to the inductive tendency of the other is at the given distances so great, that little or no attractive effect ensues, and the repulsive force proceeds according to a certain law; but in columns C, b, c, d, &c., where the resistance to electrical change by induction in one or both of the bodies is less, the decrease of the force is more sensible, and as the distances diminish, becomes more and more evident, until at last the repulsive force no longer increases, and is in some cases superseded by attraction. It is only, therefore, within certain limits that we should expect to find the results of experiment, in any instance, conformable to a regular law, even although the bodies be equally charged: thus in column B it may be perceived that the law of the force has become irregular, and as the distances further diminish, varies inversely as the simple distance. Such is also the case in columns C, c.

17. *Experiment* E.—This conclusion was further verified by operating at first with charges of a low intensity at small distances, viz. between 0 and 10°, and then by greatly increasing the sensibility of the instrument, examining the action of the same charges at distances more extended, or otherwise by taking such electrical intensities as showed, with a given reactive force, the march of the repulsion between very extensive limits, the discs being either equally or unequally charged. The results were in strict accordance with those already given in Table IV.: the law of the force, which at first was as $\frac{1}{d^2}$, became at a certain point irregular as the distances decreased, and after being as $\frac{1}{d}$ became in some cases again irregular, until at last the repulsion vanished altogether, and was superseded by attraction.

18. The deviations from a uniform law of action being thus evident, even with two

insulated and similar bodies equally charged, we may conclude that they always occur, to a greater or less extent, at some point or other, as the distance is varied. They will, however, as already observed, be most prominent under circumstances the most favourable to the inductive influence: and it is hence not surprising that the law of action should appear by experiment to be sometimes in an inverse ratio of the distance; at others in an inverse ratio of the squares of the distances, and in others not conformable to either; whilst in some instances we observe the singular phenomena of attraction at one distance and repulsion at another.

19. One condition favourable to the disturbances above mentioned, and which it is important to notice, is the inequality of the repelling bodies in respect of extension, an increase of extension being generally accompanied by an increased inductive susceptibility. Thus it may be observed, that in connecting an insulated sphere, S, fig. 9, or other body, with the fixed ball of the balance, we give it so much inductive power that it is only in very few cases we obtain a repulsive force varying as $\frac{1}{d^2}$, notwithstanding the intensity of the opposed discs is considerable: in this case the results will be often irregular, and the respective forces very frequently as 3 : 1; when the distances are as 2 : 1, in a great variety of cases the force will be found to vary as $\frac{1}{d}$.

Experiment F.—An experimental illustration of this may be seen in the following Table. In these experiments the fixed ball p, fig. 3, was connected with an insulated sphere of three inches diameter; the respective forces being the result of different charges, and being obtained by means of the micrometer circle, according to the method explained in section 12.

TABLE V.

Distance.	Force.	Distance.	Force.	Distance.	Force.	Distance.	Force.	Distance.	Force.	Distance.	Force.
4	18	4·5	110	10	65	13	78	17·5	117	20	150
8	8+	9	54	20	20	26	26	35	35	26	90
....	....	18	18	....	....	....	....	....	....	40	40
....	....	....	....	....	....	....	....	....	....	52	23+
A		B		C		D		E		F	

We here perceive, in accordance with the preceding phenomena, that in column A, where the intensities are not considerable, and the distances small, the force is as $\frac{1}{d}$, or very nearly so; whereas in column F, in which the intensities are considerable, and the distances great, we have the force nearly as $\frac{1}{d^2}$. In the other columns, B, C, D, E, the forces are nearly as 3 : 1 when the distances are as 2 : 1, except in one case in column B, where the force is again nearly as $\frac{1}{d}$ in the case of the distances being small, as at 9° and 4°·5. We also perceive in one instance, that where the distances are as 4 : 1, the forces are as 6 : 1, as in the distances 18° and 4°·5 of column B.

20. These facts are of great consequence to every species of electrical inquiry depending on any instrument whose indications are derived from repulsion. COULOMBE found, for example, that the balls of his balance were repelled with only one half the force at a given distance when the quantity of electricity in one of them was reduced to one half, and further concludes that the whole repulsive force expressed by $\frac{F}{D^2}$ diminishes for the same distance, D, as the absolute quantity of electricity in each of the repelling bodies considered as points. This principle he applied extensively, with a view of detecting the ratios of the quantities of electricity accumulated in charged bodies or in any given point of them. The electricity of the given point he considered as transferable to a small insulated disc, first applied to the body, and subsequently placed in his balance, the ball of the needle being already charged with a certain quantity of the same electricity. The insulated disc has been termed a proof plane: when this plane is placed upon any part of a charged body, it is supposed to be identical with an element of the surface, so far as relates to the distribution of the accumulated electricity; and hence, on removing it to the balance, it is assumed to operate just as the element would do under similar circumstances. Admitting, however, this identity, the indications of the instrument may not in all cases be directly proportionate to the quantity of electricity in the proof plane, since on the principles just explained, and as seen in Table IV., the respective quantities of electricity are not always as the repulsive forces: if the particular cases in which this happens be not first ascertained, we may possibly arrive at erroneous conclusions. It appears, for instance, from COULOMBE's researches with the proof plane, that the relative electrical capacities of a solid or hollow sphere and a circular plane of equal area, each side to each side, are as 2 : 1; that when electricity is accumulated on a globe, either hollow or solid, it is only found upon the exterior surface; hence in expanding the globe into a plane circular area of the same superficial extent, each side to each side, we double its capacity by giving it another exterior surface*; twice the quantity of electricity may therefore now be placed on it under the same intensity.

21. This point is of some importance to an exact electrical theory, and deserves attention. The experimental evidence in support of it is principally the following.

1°. When a small insulated body is plunged within a hollow sphere charged with electricity, it does not, on being again withdrawn, exhibit any electrical indication; whereas on touching the exterior surface, the insulated body becomes vigorously electrified.

2°. COULOMBE found, by means of his proof plane, that when a charged sphere had

* BIOT, Traité de Physique, tom. ii. p. 275.

been touched with an insulated circular plate, one of whose surfaces was equal to the exterior surface of the sphere, it exhibited only one third the reactive force which it evinced before the contact, and hence concluded that the quantity remaining on the globe after the contact, was only one third of the previous quantity; that consequently the charge had divided itself between the plate and sphere in the proportion of their assumed surfaces of action.

22. It may not be unimportant to examine the claims of this evidence, since it has necessarily considerable influence on the future progress of electricity.

In the detail of COULOMBE's experiments, given in the Traité de Physique of BIOT, the result of the contact of the plane and sphere does not appear to have been compared with any result of contact with a similar sphere; that is to say, with the electrical reaction of the original sphere when an equal sphere was substituted for the plate. According to his views, the reaction after *one* contact of the plate should equal the reaction after contact with a similar sphere, whose exterior surface was equal to the *two* surfaces of the plate. This experiment, after what has been just stated, is essential to an accurate result; since it is possible, that although the electrical reactions shown by the balance of repulsion may be nearly as 3 : 1, yet still the actual quantities of electricity may be only as 2 : 1 (15.). Not finding in any of the accounts of COULOMBE's inquiries which have come under my notice an experiment of this kind, it may be worth while to commence with this test of the theory. For this purpose I obtained two conducting spheres, each four inches in diameter, and a circular plate of eight inches in diameter, as represented by S, s', P, fig. 9.

Experiment G.—Having insulated and charged one of the spheres, S, as also the disc of the needle, with the same electricity, according to the method of experimenting adopted by COULOMBE, I proceeded to ascertain its electrical reaction by means of a tangent disc, and found the needle repelled 22°, the reactive force of the instrument being about the $\frac{1}{2000}$dth of a grain for each degree. The charged sphere was now touched with the insulated plate P, and its electrical reaction again observed. This being effected, I replaced the original charge on the sphere, so as to again obtain a force of 22°, and then repeated the experiment with the second insulated sphere *s*. The results are given in the following Table, in which it may be seen that the electrical reactions, after the respective contacts with the plate and sphere, the areas of which were equal; instead of being as 2 : 1, according to the theory, are nearly the same, whilst the subsequent forces at 22° distance, as compared with the reaction of the original charge of 22° at the same distance, are nearly as 3 : 1. This ratio we have found in several instances where the charges on the repelling discs are unequal and in the ratio of 2 : 1.

Table VI.

Reaction before contact at 22° distance.	Reactions after contact.			
	Reaction at 14°.		Reaction at 22°.	
	Plate.	Sphere.	Plate.	Sphere.
22°	14°−	14°+	7°	8°−
A	B		C	

In column A we have the force communicated to the proof plate at first. In column B the force after the respective contacts of the sphere and plate, as shown by the mere deflection of the needle. Column C shows the reaction at the original distance of 22°.

23. In order to obtain more readily a repetition of the original charge, I placed the insulated sphere immediately under the suspended plate of the electrometer described in my former paper *, and as represented for the cylindrical conductor in fig. 18. Having reproduced a given force, as indicated by this instrument, by repeatedly touching the sphere with a small transfer conductor charged to a sufficient intensity from the knob of a charged jar, the sphere was removed, and submitted to experiment. By this process the original electrical reaction of 22° could be easily obtained.

24. The result, therefore, arrived at by Coulombe's method of experiment may be classed with those cases in which the repulsive force exercised by the balance is not proportionate to the quantity of electricity. Thus, if we suppose, by way of further illustration, that in certain experiments similar to those detailed by Coulombe, the respective quantities of electricity communicated to the tangent disc had been really in the ratio of 2 : 1, but so circumstanced as to have been at first equal to and subsequently half the quantity with which the disc of the needle was charged; and suppose that the respective reactions had been taken at a distance of 24°, 18°, 12°, or 9°, with intensities corresponding to those given in columns B and *d* of Table IV., we might have then found the reactions in the ratio of 3 : 1, or nearly so; or supposing the proof plate, after both contacts, to have become charged to a *higher* intensity than the disc or ball of the needle, the respective quantities on the repelling bodies being in the proportions of $1 : \frac{1}{4}$ and $\frac{1}{2} : \frac{1}{4}$; that is, supposing the proof plane to have received, before the contact with the plate, *four times* as much electricity as existed on the ball of the needle, and after the contact only *twice* as much, then if the reactions were taken at a distance admitting of a sensible inductive disturbance (15.), the repulsive forces might still be in the ratio of 3 : 1, or nearly so, although the respective quantities of electricity on the proof plate producing the repulsion in each case should be really as 2 : 1. This is shown in columns *b* and *d* of Table IV., in which the forces corresponding to the distances 12°, 18°, and 24°, are nearly as 3 : 1, whilst the

* Philosophical Transactions for 1834, Part II. p. 215.

respective quantities are as 2 : 1. I have adverted to the results in Table IV. merely by way of illustration: it is however quite evident that with other intensities the coincidences alluded to might be more perfect, especially if, as in the experiment cited by M. Biot, we had, instead of a small disc, employed an insulated sphere of an inch in diameter for testing the respective electrical reactions: in this case, as I have already endeavoured to show (19.), the disturbing force of induction would be more powerful, one of the repelling bodies having greater extension (Table V.).

25. If, instead of previously charging the disc of the needle with the same electricity, it remains neutral, and the electricity taken up by the proof plane be equally distributed between them, we may frequently avoid the disturbances above mentioned, more especially if the electrical intensities and the distances are within certain limits. Thus we observe, in again referring to Table IV., that in columns A and B, where the quantities on each disc are as 2 : 1, the forces are in almost every instance as 4 : 1, the force being as the square of the quantity; a result which I obtained also from the attractive forces by another and very different kind of experiment*.

Experiment H.—With a view, therefore, of further verifying the preceding results, the former experiments, Table VI., were repeated in this way; that is to say, the electricity was distributed equally on the two discs and the square roots of the forces taken to designate the respective quantities: under these circumstances I found the electrical reactions of the charged sphere before and after contact both with the circular plate and with an equal sphere, in the ratio of 4 : 1; hence the quantity abstracted by the plate was equal to the quantity abstracted by the sphere, and just half the original quantity. I took in these experiments an electrical reaction of 16° distance as a unit of charge; having previously ascertained by experiment that with this intensity the forces varied as the square of the quantity, or very nearly so, when the quantity on the discs was reduced to one half; the reactive force of the instrument being about the $\frac{1}{2000}$dth of a grain for each degree, and the proof plate of an inductive capacity adequate to the purposes of the experiment (42.).

26. Although the above result seems sufficiently conclusive of the point under consideration, and is quite in keeping with the result arrived at in my former paper, viz. that the capacity of a sphere is the same as that of a circular plane of equal area into which we may suppose it to be expanded†: it may still not be altogether useless to verify it by another kind of experiment, equally conclusive and direct with the preceding.

Experiment I.—The discs *p m* of the balance, figs. 3 and 14, being brought into contact, and one of the insulated spheres above mentioned, S, fig. 9, connected with the fixed disc *p*, a charge was conveyed to it, which, on easing away the lever confining the needle, amounted to 48° at 48° distance. This being noted, the sphere S was touched with the insulated circular plate P, and the new position of the index from zero noted; the disc of the needle was then touched with a similar neutral disc, so

* Philosophical Transactions for 1834, p. 219. † Ibid. p. 235.

as to reduce the quantity contained on it to one half, and bring it to equality with the fixed disc, on the supposition that the plate had abstracted from the sphere one half the charge. The new distance of the index from zero was again observed, and the two discs again brought into contact, so as to equalize more completely the distribution between the discs, should any difference exist. Little difference, however, was apparent; hence the assumption that the plate had abstracted one half the electricity, was to a certain extent confirmed. Finally, the electrical reactions were observed at the original distance of 48°.

This process was repeated in substituting a similar sphere for the circular plate, and also a solid sphere of the same diameter. The following results were obtained: $f_1 f_2 f_3$ designate the reactions above mentioned, the electrical reaction at the original distance of 48° being denoted by f_3.

TABLE VII.

Reactive force of the instrument $\frac{1}{2000}$.

Reaction at 48° distance before contact.	Reactions after contact of plate.			Reactions after contact of sphere.			Reactions after contact of solid sphere.		
	f_1.	f_2.	f_3.	f_1.	f_2.	f_3.	f_1.	f_2.	f_3.
48°	39	22·5	12	38	23	12+	39	23	12
N.	a	b	M	a'	b'	M'	a''	b''	M''

27. It may be observed in this Table, that under whatever corresponding circumstances we compare the results after contact with the plate and spheres, whether previously to equalizing the electrical state of the discs, as in f_1, or subsequently, as in f_2, or otherwise after the equalization, as at f_3, the result is very nearly the same. I repeated these inquiries with the electrometer represented in fig. 18*, and found by the attractive force of a unit of charge on the suspended neutral plane, that whether an electrified sphere was subjected to the contact of a circular plate of equal area, or otherwise to the contact of a similar sphere, hollow or solid, the subsequent attractive forces were equal, and the quantities abstracted precisely one half the original quantity with which the first sphere was charged, or very nearly so, taking the square roots of the forces to represent the respective quantities. I found also in connecting the plate and sphere successively in any point with the fixed ball of the balance, and communicating to each the same quantity of electricity by means of a transfer plate, charged to a given intensity, that the electrical reactions were the same, as already shown (by another method of experiment) in my former paper†.

28. *Experiment* K.—This kind of experiment I further extended to cylinders, hexagonal and other prisms, and bodies of other forms; and find, as in the cases given

* Philosophical Transactions for 1834, p. 215. † Ibid. pp. 218, 232.

in my former inquiries*, that their electrical reactions or capacities are precisely the same as those of the plane areas, into which we may suppose them to be expanded. Thus the electrical capacity of the hollow cylinder A', fig. 19, open at both ends, is precisely the same as if it were cut through in the line *a b*, and expanded into a plane A, fig. 20, and conversely the capacity of the plate A, of inconsiderable thickness, is the same as that of an open cylinder A', fig. 19, or A'', fig. 21, into which we may suppose it to be turned without doubling over the edges, whether rolled in the direction of its length *m n* or breadth *n o*; now such could not possibly happen if their capacities were not the same, since the same quantity expanded on a double surface has a very different electrical reaction.

29. In comparing the capacities of a sphere and plate of equal area by the method just given (28.), we may place any charged body in connexion with the fixed ball of the balance,—such as a common cylindrical conductor,—since we have merely to discover the respective quantities abstracted. In every case of this kind the effect of contact with a plate and sphere of equal area will be found the same. We may also reverse the former experiment, and connect the plate with the fixed disc instead of one of the spheres, and so examine the decreased intensity of the plate after contact with the sphere or with a similar plate.

On referring to columns N; *b*, M; *b'*, M', &c. of Table VII., we observe one of those cases in which the force is nearly in an inverse ratio of the distance, the distance 23 and 48 being as 1 : 2 nearly, whilst the corresponding forces 23 and 12 are nearly as 2 : 1.

30. This simple induction of facts appears sufficiently conclusive; it clearly shows that a spherical conductor, either hollow or solid, and a plate of equal area, have the same electrical capacity, and that Coulombe's experiments are not opposed to such a conclusion.

31. We may now proceed to consider the case of an insulated body plunged within an electrified sphere, and this will necessarily lead to some further inquiries into the action of the proof plane, and to the conditions under which one substance receives electricity from another.

The curious fact that we do not abstract any portion of the charge accumulated on a hollow sphere by touching its interior surface with an insulated neutral disc placed wholly within it, will, on inquiry, be found little conclusive of the non-existence of electricity upon that surface; it may be experimentally shown that any insulated body plunged within an electrified shell, could not possibly take up a particle of electricity, even although a powerful accumulation *actually existed there*.

Experiment L.—Let a small sphere of glass *a a'*, fig. 22, made clean and dry, having a projecting neck at *a'* varnished with shell lac, be nearly filled with dry mercury, and let the whole be placed in a vessel *b b'*, also containing mercury, so as to give the glass an outer and inner coating; charge this system, and remove the charging

* Philosophical Transactions for 1834, pp. 232, 233.

wire *d* by an insulating handle *n d*, and subsequently the charged sphere *a a'*. Let the mercury contained within the sphere be now poured off; we have then a spherical body, upon the interior surface of which there is a powerful accumulation of free electricity. Insulate this charged sphere, and touch its interior surface with an insulated proof plate. The plate will not, on being again withdrawn, exhibit the least electrical indication, although electricity in a free state is *known* to exist upon its inner surface. If, however, we connect the proof plane with an insulated conducting rod projecting beyond the sphere, electricity will be freely taken up, as in the case of a body similarly circumstanced, and placed within a charged sphere of metal.

32. It is of no consequence to this experiment whether the glass sphere have subsequently to charging an external coating or not. Free electricity is everywhere diffused on its interior surface, and is easily communicable to any body capable of receiving it. The following experiment is sufficiently illustrative of this.

Experiment M.—Let a circular plate of glass *d d'*, fig. 23, be placed on a conducting plate *c*, whose diameter is about half that of the glass; place a similar plate *c'* upon its upper surface, immediately opposite the conducting plate *c* below, so as to give the glass two moveable coatings; charge this system by communicating electricity to the upper plate; remove the coatings, and place the glass on an insulating support; if the charged side be now touched with the proof plane, electricity will be freely taken up.

33. We have here a direct experimental fact, showing that the neutral state of the proof plate is by no means evidence of the non-existence of electricity upon the interior surface of a charged sphere, and therefore no influence of this kind can be logically deduced from the experiment in question (21.). Moreover, the theory itself assumes, upon certain principles in the 12th section of Newton, that the action of a spherical stratum of electricity upon any point placed within it is equal to zero: should electricity therefore *really* exist upon the interior surface of the sphere, it could not, if the theory be true, be imparted to a body placed wholly within it. The experiment therefore is in this point of view irrelevant; but if we take the experiment as a mere fact abstracted from hypotheses, it must necessarily be considered in connexion with other facts, such as those above stated (31.) (32.); in either case, however, it is clearly no evidence of the non-existence of electricity upon the interior surface of a charged shell.

34. The experiments last mentioned (L.), (M.) are instructive; they show that it is not only from the absence of electricity upon the interior surface of a charged shell that we fail to electrify a small insulated body placed within it, but that this result may also arise from incapacity of the given body itself to take up electricity under certain circumstances. Pursuing therefore these facts, unfettered by any hypothetical view of electricity, we are immediately led to examine under what conditions one body can receive electricity by communication from another, as in the case of touching a charged conductor with a small insulated disc of inconsiderable thickness; since

upon the indications of this instrument, termed a proof plane, all the experimental evidence of the theory of electrical distribution in charged bodies of various forms depends.

35. Without embarrassing the inquiry by any theoretical disquisition, let us study the phenomena as they immediately present themselves. There exists in electricity, as already observed, a peculiar kind of influence termed induction, not recognised in other invisible natural agencies, magnetism excepted; that is to say, the attractive force displayed by these wonderful powers is invariably accompanied by a previous change in the electrical or magnetic states of the attracting bodies. If, as in the case of electrical attraction, the forces thus generated, as it were, by induction, be sufficiently powerful to overcome all impediment to the free communication of electricity, these induced states are found to vanish, and a new disposition of the accumulation immediately takes place, and it is thus one body receives electricity from another. I hope at an early period to lay before the Royal Society some new phenomena in electricity calculated to throw further light on the operations of electrical induction and attraction, and from which it would appear that the attractive force between a charged and insulated body in a neutral state, is entirely dependent on the reciprocal inductive forces of which, under the given circumstances, both the bodies are susceptible, and not necessarily on the mere quantity existing on the charged body. The free electricity therefore which a small proof plane takes up from a charged body may not only depend on the quantity actually existing in any given point to which it is applied, but on the reciprocal inductive force of which the bodies are susceptible at such point of application. Should the inductive capacity of the proof plane become in any way affected by position, or by its thickness, or extension of any kind, or should the charged substance be itself more or less susceptible of induction in different points, then the inductive forces will be different, and the attraction generated between the charged body and the proof plane not always proportionate to the actual quantity of electricity present, as can be shown by many striking experiments. The proof plane would not under these circumstances take up in every situation a quantity of electricity proportionate to that of the element of the surface to which it is applied. That something of this kind takes place, may, I think, be made evident in the following way.

Experiment N.—Take three equal and similar circular metallic plates, p, p', p'', figs. 24, 25, 26. Let two of them, p', p'', be hollowed up into shallow spherical segments; insulate these bodies in the positions shown in the figures, the convexity of the segment p' being placed uppermost, and that of p'' downward; eharge these insulated conductors with the same quantity of electricity, which may be readily effected by the methods explained in my former paper*. Now the respective intensities, as measured by the connexion of the charged bodies with any electrometer, are all equal† (28.); hence there cannot possibly be any exception taken on account of a supposed double sur-

* Philosophical Transactions for 1834, p. 218. † Ibid. p. 233.

face of action, as respects the plate p; for, as already observed, if such really existed, the plate could not possibly evince the same intensity with the same quantity, as is shown by disposing the electricity on two plates instead of one, in which case the attractive force, as evinced by the electrometer, is in an inverse ratio of the square of the surface*; a result also demonstrable, in many instances, by means of the repulsive force communicated to the discs of the balance, fig. 3. Under these circumstances it is not illogical to conclude, that so far as surface is concerned, the electricity is upon the whole circumstanced much in the same way in each. Let a small proof plane be now applied to these charged bodies successively, in the similar points p, p', p'', and the respective electrical reactions observed. The greatest electrical reaction will be obtained on the convexity p', the next on the plane p, and the least on the concavity p''.

If in these experiments p, p', p'' represent the situation of the proof plane, we observe that in the concavity p'' it is most completely enveloped in the electrical molecules of the charged body; on the plane it is much less enveloped; on the convexity of the segment p' the particles of electricity are bent away from it, as it were, in all directions. Position alone therefore in respect of the other parts of the charged body may possibly influence the quantity taken up by the insulated plane. Now whatever tends to increase the inductive susceptibility of the proof plane, will bring the electrical reactions nearer a ratio of equality. Thus in giving the proof plane some considerable extension in the direction of its thickness, or otherwise in holding it by an insulated metallic wire, w, fig. 27, we increase its inductive force; in this case the differences in the electrical reactions with a given charge become less.

36. It follows from this, that could we employ a proof plane of perfect inductive susceptibility, we should actually arrive at equality in the reactions of the three bodies above mentioned. One method of effecting this is to make the proof plate a portion of a small coated element of glass, as represented in figs. 5 and 8. If a compound element such as this, and which has been already described (8, δ.), be substituted for the small insulated disc above mentioned, it will, after contact with the charged bodies p, p', p'', fig. 24, have an equal reaction imparted by each. Another method consists in extending the limits of the wire w, fig. 27; but this we do in connecting the bodies with the fixed disc of the balance in the way above mentioned (8, ν.); and we accordingly find that the repulsive forces imparted to the discs of the balance are equal.

37. *Experiment* O.—Should any doubts remain concerning the actual disposition of the electrical charge on the three bodies p, p', p'', figs. 24, 25, &c., we may substitute coated glass for these bodies, and charge them each equally by means of a small unit jar†. We have then, on removing the coatings from the charged side, or otherwise both the coatings, three strata of free electricity of the forms above mentioned. If any difficulty occur in obtaining two spherical segments of glass sufficiently similar, we may charge the opposite sides of the same segment successively. The temporary

* Philosophical Transactions for 1834, p. 219.

† Ibid. p. 217.

coatings both of the plate and segments may consist of sulphate of lime, neatly moulded to the glass, and subsequently gilded; they must be each of an equal surface. It will be also requisite for greater accuracy to obtain a glass plate of the same thickness as the spherical segment.

It is not therefore impossible for a proof plane, such as that employed by Coulombe, to exhibit unequal electrical reactions, and yet the distribution on the conductor to which it has been applied be uniform. Thus a tangent plane of inconsiderable thickness applied at the extremity *a* of a charged cylindrical conductor, *c*, fig. 18, may be in a more favourable position for the inductive action than if placed at the centre *c*, or in the centre of the circular plane *c'*, terminating either of its extremities.

38. In treating of the proof plane, philosophers have considered its action in more than one point of view. M. Biot states that the proof plane, in becoming assimilated with a superficial element of a charged body, will take up as much electricity upon each of its surfaces as exists upon the point to which it is applied; hence, on removal, it is charged with double that quantity*. M. Pouillet, on the contrary†, considers the proof plane to be, at the instant of contact, in precisely the same state as a superficial element of the same dimensions, and to be, on removal under the same electrical conditions, as a similar portion of the charged body would be placed if actually cut out of its surface; that is to say, according to his view, the electricity would be first collected on one side only, and would subsequently be expanded over both; each surface therefore has only half the quantity which the superficial element at first possessed.

39. Both these views of the state of the proof plane are evidently at variance with the phenomena above recorded (28.); this, it is true, is of no great consequence to Coulombe's results when he merely employs the proof plane to determine the ratios of the quantities of electricity distributed on a charged conductor; it has, however, still a material influence on the theory of electricity.

40. The experiments with the proof plane just mentioned (31.), (32.), together with the phenomena of repulsion so frequently alluded to in this paper (14.), necessarily lead us to investigate more rigorously the nature of its indications, in order to discover, if possible, the conditions under which it may fail to become charged, either with the same quantity as exists in the points touched, or otherwise in the ratio of the quantities.

The first notions which present themselves from our experience of ordinary electrical actions, would lead us to conclude that an electrified conductor of large dimensions could always charge a small body to saturation from any point of it, provided the electrical state of the touching body was such as would enable it to become so charged: this is indeed quite evident from the fact that a compound element su chas that already mentioned (8, δ.), and represented fig. 8, becomes charged equally at whatever point of a long electrified cylinder, *c*, fig. 18, it is applied, whether

* Traité de Physique, tom. ii. p. 271. † Physique Expérimentale, tom. i. Seconde Partie, p. 579.

at the centre *c* or at the extremity *a*; as also from the fact already noticed (36.), that whatever part of a charged conductor be connected with the fixed disc of the balance, or with any other species of electrometer, the instrument is equally affected. The proof plane, therefore, if it really exhibits the ratios of the quantities of electricity disposed in different points of a charged body, must depend materially on the resistance to an equal participation in the charge, arising from its little inductive susceptibility; it is hence very desirable to investigate experimentally the various effects which increased thickness, or extension of any kind, position, intensity of charge, and the like, may have on its indications. An experimental inquiry of this kind is, however, extremely difficult: we are, for example, open to all the sources of fallacy above described (15.), and illustrated by Table IV. Thus, if we examine the electrical reactions, having previously charged the disc of the needle with some given quantity of the same electricity, the subsequent quantities taken up by proof planes of various thicknesses, &c., may bear all sorts of proportions to this quantity; and since the law of action is under these circumstances not always regular (15.), we might have, in examining the respective forces at some given distance, *d*, very obscure and uncertain results. If, on the other hand, we neutralize the disc of the needle at each experiment, and diffuse the electricity taken up by the proof plane equally over each, we are still open to fallacy; since at small distances the force is sometimes in an inverse ratio of the simple distance, whilst at others it varies in an inverse ratio of the square of the distance (17.); and we have in some instances the electrical reaction so little that the discs do not separate, in consequence of the slight attractive force exerted between them at the point of contact, and which in some degree vitiates the result at a given distance, *d*.

41. *Experiment* P.—Notwithstanding these obstacles, I have endeavoured to obtain a good series of observations on the indications of insulated tangent planes of various thicknesses, applied to different points of a charged cylinder of about four feet in length, and 2·5 inches in diameter, terminating in plane circular faces. The experiments were conducted in the following way. The charged cylinder *c*, fig. 18, was placed on two slight insulators, I, I′, fixed on a small platform, N, supported beneath on four small rollers, so as to be moveable between the guide pieces *g′ g′* of the fixed base B. The extremity of the cylinder could be by this method brought immediately under the suspended plane *p* of the electrometer E*. When it was required to convey to the cylinder a charge of any given magnitude, the index of the instrument was adjusted from zero to a given number of degrees in the direction *o y*, by means of small weights placed in the cup at *q*. Electricity was then communicated to the cylinder *c* until the index came again to zero; and thus the distance *p a*, at which the attractive force operated on the suspended plane *p*, was always constant for any given charge. Now as the attractive force is as the square of the quantity of electricity on the charged body†, we have only to make the degrees at which we adjust the

* Philosophical Transactions for 1834, p. 215. † Ibid. p. 221.

index from zero vary in this ratio, and we obtain a double, treble, &c., quantity on the conductor, when the index is again brought to zero of the arc $x\ y$; a result which I further verified by actually placing the required quantities on the cylinder*; these methods were thus brought to check each other. Immediately the index was stationary at o; the charged cylinder was withdrawn from the electrometer, so as to avoid any possible determination of the charge toward the extremity, and the tangent plane immediately applied and examined by the method before mentioned (ν.), (8.). The cylinder was, at each successive experiment, again brought under the electrometer plane, and the electricity which had dissipated during the last experiment replaced; the tangent bodies employed, all exposed circular touching planes of half an inch in diameter; their extensions in the direction of the thickness were as follows: taking the letters $a\ b\ c$, &c., to represent the plates, and the unit of length = 1 inch, then $a = {\cdot}005$, $b = {\cdot}12$, $c = {\cdot}25$, $d = {\cdot}5$, $e = 1$, $g = 2$.

In addition to these, I tried a tangent plane backed by a wire of four inches in length, as represented in fig. 27, and also a compound element of coated glass = q, fig. 8, the coatings being areas of the same dimensions as the others, viz. ·4 of an inch in diameter, and of inconsiderable thickness. In observing these reactions the bodies were transferred to the balance, the disc of which was previously neutralized, and the electricity equally disposed upon the repelling bodies. The first deviation of the needle was then observed $= f$, and finally the force taken at $10° = \text{F}$, considered as a unit of distance. The square roots of the forces were then taken to designate the respective quantities of electricity, or the ratios of the quantities existing on the different points of the charged cylinder. The points of the cylinder C touched, were the centre E, the extreme end C', and the centre of the plane face terminating its extremity C'. Little difference being discoverable in the points between the centre and extremity, I have condensed the general results of this investigation within as short a space as possible: they are as follows. The letters $a\ b\ c$, &c., denote the different tangent bodies, the reactive force of the instrument being about the $\frac{1}{2000}$dth of a grain for each degree; f represents the first deflections, as also the distances of the repelling bodies; F the reactions taken at a given distance; $d = 10°$; C C' E denote the points touched.

Table VIII.

	Quantity of electricity = 1 = 32° of electrometer.															
	a.		*b.*		*c.*		*d.*		*e.*		*g.*		*p.*		*q.*	
	f.	F.	*f.*	F.	*f.*	F.	*f.*	F.	*f.*	F.	*f.*	F.	*f.*	F.	*f.*	F.
C . .	3+	1–	5	2·5	6·5	4	10	10	14	25	20	55	28	168	32	328
C' . .	4·5	2	7+	5	9	8	14	25	20	55	29	220	31	320	32	328
E . .	6	3·5	10	10	14	26	19	50	23	92	30	225	32	323	32	328

* Philosophical Transactions for 1834, p. 218.

Table IX.

	Quantity of electricity = ½ = 8° of electrometer.									
	d.		*e.*		*g.*		*p.*		*q.*	
	f.	F.	*f.*	F.	*f.*	F.	*f.*	F.	*f.*	F.
c . .	7	4	10	10	14	25	15	25	21 ·	81
c' . .	9	8	15	32	20	55	22	70	21	81
E . .	14	25	16	36	20	55	22	70	21	81

Supposing the given distance $d = 10°$ to be one of those (14.) which would admit of the repulsive forces being considered proportionate to the quantities of electricity in the respective points of the body touched, we have, in taking the square roots of the respective forces, at 10°, the following results.

Table X.

	Quantity of electricity = 1 = 32° of electr.							
	a.	*b.*	*c.*	*d.*	*e.*	*g.*	*p.*	*q.*
c . .	1—	1·6	2	3+	5	7·4	13	18
c' . .	1·4	2·2	2·8	5	7·4	15	18—	18
E . .	1·8	3+	5	7+	9·5	15	18—	18

Table XI.

	Quantity = ½ = 8° of electr.				
	d.	*e.*	*g.*	*p.*	*q.*
c . .	2	3+	5	5	9
c' . .	2·8	5·6	7·4	8+	9
E . .	5	6	7·4	8+	9

42. On examining these results we observe, 1°. In the horizontal column c, Table VIII., where the quantities of electricity are less considerable, the respective forces *f* F are in the first four cases, *a*, *b*, *c*, *d*, in an inverse ratio of the distances, or nearly so: thus we have for plate *b* force at 5° : force at 10° : : 5 : 2·5 : : 2 : 1. This law, however, begins at last to change, and become irregular when the quantity of electricity increases, a phenomenon already observed in Table IV. (14.). A similar result ensues in the horizontal column c', except that as the quantities of electricity are more considerable the law begins sooner to change, as at the plate *d*. The same result is seen in the column E; but here the quantity of electricity being greatly increased, the law changes still more early, as at the plate *c*. In Table IX. we observe similar effects; thus verifying in great measure the principles and conclusions we have already arrived at (14.). When, therefore, we begin to reduce these results so as to obtain the ratio of the quantities of electricity supposed to be distributed on the charged cylinder, we should expect to find them more or less disturbed by the variable inductive action between the repelling bodies. We accordingly perceive, on referring to Table X., that the ratio of the centre c to the extremity E is at first, with plates *a* and *b*, as 1 : 2, or nearly so; being a similar result to that obtained by Coulombe *. Under plate *c* this ratio tends to change, as more clearly shown in Table VIII.: in fact, it is at this point the law for the plate *c*, touched at the extremity, begins to

* Biot, tom. ii. p. 275.

vary, as seen in the same Table. When, however, the two columns C and E again vary together, or nearly so, the ratio of the quantities, as given by each of the discs, is about the same as before, until at last, where the induction upon the tangent plate is the most perfect, the ratio becomes one of equality, or nearly so. The same remarks apply in comparing the horizontal columns C' and E or C and C'.

43. In comparing Tables X. and XI. there is one important fact to be observed; viz. that we do not get the exact ratio of the respective quantities of electricity on the charged cylinder until we arrive at the most perfect inductions, as in the action of the plates *p* and *q*: here the reactions are as 2 : 1, or nearly so, whereas in comparing the similar plates and parts touched with the less powerful plates *d e g* the discrepancies are considerable, evidently showing that we have not really arrived at the true ratio of the quantities by means of these plates.

44. It is worthy of further inquiry, whether the proof plane be really identical with an element of the charged surface to which it is applied, or whether it be not in the condition of a neutral insulated body placed within an extremely small distance of a charged body, and subject to the same laws as subsist between two such bodies when placed under similar conditions, at more sensible distances, and at which electricity may be communicated. A rigorous examination of this question would probably elucidate many phenomena of electrical action at present involved in doubt: in the mean time it may not be unimportant to review such facts connected with this point as are already known.

45. It has been found, for example, that the attractive force between charged and insulated neutral bodies is less than when the latter are uninsulated; that perfect insulators are not sensibly attracted by electrified substances; and that, in every case of electrical attraction, the force is (as already observed,) proportionate to the previous induction of which the bodies are susceptible. In accordance with these facts a perfectly insulating disc reposes on a charged surface without becoming itself electrified; an insulated neutral conducting disc more or less so, in proportion to its thickness (42.); whilst an insulated plate whose inductive power is nearly perfect, is charged to equality with the point touched (36.).

I have found in the course of some recent inquiries that the attractive force between an electrified plane surface and an insulated disc of inconsiderable thickness, in a neutral state, is frequently in an inverse ratio of the distance between the two planes; the induction of which such a disc is susceptible being extremely limited; that on increasing the thickness the force also increases up to a certain point, where, under the given conditions of distance, quantity of electricity, and the like, the induction on the opposed surface remains nearly the same.

46. These facts, together with those already mentioned in the course of these inquiries, render it highly probable that the quantity of electricity taken up from the surface of a charged body by a small insulated disc of inconsiderable thickness may be greatly influenced by the position of the point of application, independently of the quantity of

electricity; so that the same quantity may possibly exist in two different points, and yet the proof plane become charged in a different ratio, the inductive power of the plate being different in these points. M. Biot, in the second volume of his Traité de Physique*, has given, from Coulombe's manuscripts, an account of the application of the proof plane to an electrified lamina of steel plate, eleven inches in length, one inch in width, and half a line thick. He states, that when the proof plane, which was an inch long and about a quarter of an inch wide, was applied beyond the extremity of the charged lamina, so as to touch its two opposite surfaces, the plate abstracted the electricity of both, and exhibited a reaction double of that which it showed when only applied at the extremity of one of the surfaces. Now it will be immediately perceived that in this application of the proof plane it was placed *entirely without* the charged body, the most favourable position possible for taking up the electricity of the plate; a circumstance which would greatly influence the result (35.). Should this ingenious experiment really prove the diffusion of a stratum of an invisible subtle fluid over the two surfaces of the plate, it would at the same time equally well demonstrate the existence of electricity upon the interior surface of a hollow body, as, for example, upon a hollow cylinder, into which we may suppose the steel plate in question to be formed, since we have shown (28.) that the intensities evinced by a given quantity of electricity are the same in both cases, and that consequently the distribution, so far as respects these two surfaces, must be similar.

47. Should the inductive susceptibility of the tangent disc be at any time, by its position in respect of the electrical particles, reduced to zero, or nearly so, it would then fail to become in any degree charged, and would be as inefficient as a plate of varnished glass or any other non-conductor, the inductive susceptibility of which is so little, that it will not, under ordinary circumstances, take up the least electricity on being applied to a charged body. Now it is not improbable that a small insulated plate plunged within a spherical charged shell is thus circumstanced: it may hence fail to become charged, even although electricity should really exist there, and which fact we have experimentally shown (31.). Similar effects would ensue in placing a neutral body under any other circumstances involving similar conditions, as in placing a very small conducting ball immediately between two large electrified globes. The small globe does not, under these circumstances, according to Coulombe, take up any free electricity.

48. It would be difficult, without the aid of induction, to explain in what way the mere position of a neutral body, in respect of the electrical particles by which it is surrounded, effects its power of absorbing electricity; and even with this we require a more extensive investigation of the phenomena than has yet appeared. That a change of position of the electrical particles of a charged surface in respect of each other, and of the body charged, is attended by important consequences, has been already shown by Volta, who observed the curious fact that the intensity of a charged

* p. 275.

plate becomes greatly diminished as its length is increased, although the *area* of the plate and the *quantity* of electricity remain the same; a subject which I have already treated of to some extent *. But in extending a plate in length, we elongate, as it were, the stratum of electricity resident about it, and thus place the electrical molecules, if such exist, in a new relation of position in respect of each other and of the general surface of the plate. If so apparently trifling a change as the extension of an electrified plate in length, the area not being diminished, is capable of diminishing the attractive force of the charge so much as to reduce it nearly in an inverse ratio of the length †, it is not unreasonable to suppose that the position of the proof plane, as respects the mass of the electrical molecules, may have an important influence on its indications.

Suppose, for example, the square plate *a d*, fig. 28, to be charged with electricity, and to a given intensity; imagine its area to consist of thirty-six equal squares, each an inch square, the side of the plate being six inches; then if we imagine the same area to become placed under the rectangle *a*' *d*', thirty-six inches in length, and only one inch in width, the thirty-six small squares will, as is evident, assume another arrangement in respect of each other. Any one square will be in contact with only two others, they will have, as it were, at least two sides free, whilst those at the ends *a*' *d*' will have three sides free. Now in the square *a d*, each of the smaller squares is placed between four others, except those at the edges, which have one side free, and those at the angles, as at *a* and *d*, which have two sides free.

It is always difficult, in treating of so incomprehensible an agency as electricity through the medium of effects, to avoid altogether certain hypothetical analogies and forms of expression: it will be however understood that in resorting to such analogies, they are to be considered merely as philosophical contrivances, introduced for the purposes of illustration, and not in any way subservient to an exclusive theory of electrical action.

Under this necessary limitation, let us imagine the distribution of the electricity upon the square *a d*, to be, in the absence of any other conducting body within the sphere of its influence, uniform. Then, as is proved by experiment ‡, the capacity of the area *a d* is increased when it becomes placed under the rectangle *a*' *d*'; we can hence place a greater quantity on the plate *a*' *d*', under the same intensity, than on the square *a d*. Imagine a proof plane to be applied to the square *a d* at the centre *c*, and to be in the state of any other insulated neutral body placed, under ordinary circumstances, very near it, without becoming identified with an element of the surface: then the same cause, whatever it be, which affected the capacity of the square *a d*, considered as a whole, may also affect the capacity of the proof plate considered as a whole. We may infer, for example, that at the centre *c* the electrical particles to which it is immediately applied are enveloped on all sides by other particles, and that hence none of the *sides* are free. When, however, we remove the plate to the

* Philosophical Transactions for 1834, p. 232. † Ibid. p. 233. ‡ Ibid. p. 232.

angle *a*, the particles are differently circumstanced, and have one or two sides free. Resting, therefore, on the previous fact, that the capacity of a conducting body, considered as a whole, and upon which we accumulate electricity, is affected by this circumstance, we might be led to conclude that the proof plate of small thickness should actually receive more electricity at the angle *a* than at the centre *c*, it being there less exposed to the opposing forces, whatever they be, which tend to contravene the induction. Now if we extend the thickness of the proof plane P, we place the distant points more without the influence of these forces; hence its inductive capacity becomes more perfect, so that when the thickness or other extension is sufficiently great to render the inductive capacity the same at the centre *c* as at the angle *a*, then the quantity of electricity abstracted is also the same, as is found by experiment (36.) (41.) Tables VIII. and IX.

49. It is somewhat doubtful therefore, whether we can really take the proof plane as an element of a charged body, since it forms no integral part of the surface, as is the case with the point touched. It may, however, be still open to the same influence as that which affects the capacity of the whole area to which it is applied; so that the disposition of electrified bodies to yield up their electricity at points or edges may as easily arise from the superior attractive force generated by a more perfect induction in external bodies, in the way just stated, as from an original concentration of the charge upon such points or edges. In short, we really know nothing of the actual distribution of electricity upon a charged surface, except through the medium of other bodies in some way applied to it. I have already endeavoured to show * that an electrified substance only gives off electricity by the influence of an attractive force, set up between it and some other substance: hence an electrified sphere or other body perfectly insulated in the best vacuum which can be obtained, under ordinary circumstances; will, if placed without the influence of any source of attraction, retain its electrical state for an indefinite period †. It is therefore not until we present a neutral conducting body to an insulated charged body that we begin to disturb the electrical distribution, which *may have* been previously uniform.

50. In the course of this and the preceding communication I have ventured occasionally to scrutinize the prevailing theories of electricity, and advert to the opinions entertained by many profound inquirers in this department of science. I would not, however, be thought insensible to their claims on our confidence. The researches of many distinguished philosophers on the Continent, together with those which have reflected so much honour on the science of our own country, must necessarily receive from every impartial mind the warmest admiration. It must not, therefore, be forgotten, that whilst detailing a series of facts carefully deduced by induction and ex-

* Philosophical Transactions for 1834, p. 242.

† Ibid. p. 244.

periment, I have no undue bias in favour of peculiar views of my own; my only object being, by new physical researches, to improve our acquaintance with one of the most subtle and powerful agencies in nature.

Plymouth,
April 12th, 1836.

XXI. *Note relative to the supposed Origin of the Deficient Rays in the Solar Spectrum; being an Account of an Experiment made at Edinburgh during the Annular Eclipse of* 15*th May* 1836. *By* James D. Forbes, *Esq. F.R.SS. L. & E. F.G.S. &c., and Professor of Natural Philosophy in the University of Edinburgh.*

Received May 25,—Read June 2, 1836.

The occurrence of the late solar eclipse, which was annular at Edinburgh, suggested and offered a simple method of conducting the following inquiry.

The deficiency of rays of light of certain definite degrees of refrangibility in the solar spectrum, was discovered by Dr. Wollaston*, but excited little attention until its rediscovery by M. Fraunhofer†. Since that period it has been a frequent subject of inquiry whence that deficiency proceeds. We first are tempted to suspect that the deficient rays may have been lost in passing through the prism used. But as in most cases the dark lines are the same, whatever be the material of the prism, and whatever the length of the path described by the light within it, this supposition is not tenable‡.

If these lines were all or principally owing either to the absorptive action of any matter which may exist in the planetary spaces, or to the effect of the earth's own atmosphere, we should have the same lines exhibited in the spectra of the fixed stars as in that of the sun. Sir David Brewster indeed states§ that he has been able to discover certain lines of the spectrum which are due to the action of the earth's atmosphere alone, since they are seen at small angular elevations of the sun above the horizon, and disappear when its altitude is greater. But these are not very numerous or important compared to the great mass of lines. Sir David Brewster having also observed that the lines produced by the absorption of nitrous acid gas upon artificial light are in many respects similar to those existing in the solar rays, is naturally led to attach considerable probability to the idea that the solar light is originally complete, and that the deficient rays have been stopped in passing through the sun's own atmosphere ‖. This atmosphere might be supposed to contain nitrous acid, or some similar gas, as a constituent.

* Philosophical Transactions, 1802.

† Memoirs of the Bavarian Academy; Schumacher's Astronomische Abhandlungen, 1823; Gilbert's Annalen, 1823.

‡ Rudberg, Bibliothèque Universelle, Janvier 1836.

§ Edinburgh Transactions, xii. 528.

‖ I do not know with whom the idea of the absorptive action of the sun's atmosphere originated. The

It occurred to me that an experiment might be made upon the light coming from different parts of the sun's surface which should decide this question. For supposing the sun to be surrounded by an atmosphere which his light must traverse, it is clear that the absorptive action must be greatest upon the light which reaches us from the edges of the sun (those points in whose horizon the earth appears), and least for that which traverses his atmosphere vertically (or to which the earth appears in the zenith). It results from this that the light derived from the extreme circumference of the solar disc might be expected to present more numerous and broader bands than when obtained from its whole surface, since the more complete spectrum derived from its central parts would fill up the gaps left in the spectrum of the lateral rays and conceal their deficiencies.

As the occurrence of the annular eclipse of the 15th of May suggested the inquiry, so it also afforded a very satisfactory mode of putting it to the test of experiment.

With a view to fix the aspect of the spectrum more accurately on my memory, I examined it the day before very carefully with the telescope of a theodolite placed about thirty feet from a vertical slit about one fiftieth or one sixtieth of an inch wide, upon which the solar rays were thrown by a heliostat. In front of the telescope was a flint-glass prism by DOLLOND, which exhibited the lines very satisfactorily. This apparatus was also arranged previously to the eclipse, and I satisfied myself that no minute changes of adjustment in the parts of the apparatus, such as the angle of incidence on the prism, the distance from the slit, the breadth and verticality of the slit, the quantity of light reflected by the heliostat, whether from single or double reflecting surfaces, &c., made any serious difference in the distinctness or general appearance of the spectral lines*. As the eclipse proceeded, and consequently the proportion

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editors of the London and Edinburgh Philosophical Magazine (December 1836) have, however, referred me to the mention of it in Sir JOHN HERSCHEL's writings, particularly his Elementary Treatise on Astronomy, from which I extract the following remarkable passage. "The prismatic analysis of the solar beam exhibits in the spectrum a series of fixed lines totally unlike those of any known terrestrial flame. This may hereafter lead us to a clearer insight into its origin. But before we can draw any conclusions from such an indication, we must recollect that previous to reaching us it has undergone the whole absorptive action of our atmosphere, as well as of the sun's. Of the latter we know nothing, and may conjecture everything. . . . . . It deserves inquiry whether some or all of the fixed lines observed by WOLLASTON and FRAUNHOFER, may not have their origin in our own atmosphere. Experiments made on lofty mountains or the cars of balloons on the one hand, and on the other with reflected beams, which have been made to traverse several miles of additional air near the surface, would decide this point. The absorptive effect of the sun's atmosphere, and possibly also of the medium surrounding it (whatever it be), which resists the motion of comets, cannot be thus eliminated." HERSCHEL's Astronomy, p. 212 *note*. See also his Essay on Light, Encyclopædia Metropolitana, art. 505. The object of the experiment now described is to show a method of elimination which applies, at least, to the sun's atmosphere.

* An erroneous impression seems to have been prevalent both as to the magnitude of the apparatus employed by FRAUNHOFER and as to the imperious necessity of these minute adjustments. Though the philosopher of Munich used a telescope of four inches aperture, and a prism of the same diameter, for observing the spectra of faint objects, such as stars, it appears that his map of the solar spectrum was made with a theodolite tele-

of *lateral* to *central* light increased, I continued to examine the whole length of the spectrum; but I particularly fixed upon three parts of it for more accurate comparison,—the neighbourhood of the line B in the red, the beautiful system of lines between E and *b* in the green, and the group marked G in the indigo. Notwithstanding the diminution of light I had no difficulty in pursuing my observations during the annular period; and at no time could I perceive any difference in the number, position, or thickness of the dark bands. I conceive that this result proves decisively that the sun's atmosphere has nothing to do with the production of this singular phenomenon.

Nor need this result surprise us. Spectra from artificial flames present bright and dark bands occasionally, without giving us any reason to suspect absorptive action; and the electric light presents its proper dark rays *. The solar light may also be primitively incomplete.

Had the weather proved unfavourable for viewing the eclipse, I intended to have tried the experiment by forming an image of the sun by using a lens of long focus, stopping alternately, by means of a screen, the exterior and central moiety of his rays, and restoring the remainder to parallelism by means of a second lens, then suffering these to fall upon a slit as before. The result of my experiment during the eclipse seemed however so decisive as to no *marked* change being produced at the sun's edges, that I have thought it unnecessary to repeat it.

As I do not intend to prosecute this subject at present, if the experiment just described should seem to the Royal Society worthy of being recorded, I should feel honoured by this slight communication receiving a place in the Philosophical Transactions.

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scope of only thirteen lines in diameter. His prisms, no doubt, were very perfect, but these may be replaced by hollow prisms filled with highly dispersive oils. I have frequently used oil of cassia, but on the present occasion preferred Dollond's flint glass, because the oil of cassia affected the distribution of colours in the spectrum, and was not otherwise very superior. I may observe in passing, that by viewing the direct solar light from a narrow slit by the *naked eye,* placed at some distance behind a good large hollow prism filled with oil of cassia, a very great number of the lines may be admirably seen. I take this opportunity of adding my anxious wish that Sir David Brewster should publish the details of his laborious experiments on the constitution of the solar spectrum, and his new maps of the solar lines.

* Above a year ago I made some careful experiments on the spectrum produced by the oxyhydrogen blowpipe directed upon lime. I was then unable to detect any irregularities of illumination.

Edinburgh, 21st May, 1836.

XXII. *A Comparison of the late Imperial Standard Troy Pound weight with a Platina copy of the same, and with other standards of authority. Communicated by Professor* Schumacher, *For. Memb. R.S., in a Letter to* F. Baily, *Esq., V.P. and Treas. R.S.*

Received June 9,—Read June 16, 1836.

1. Being desirous of obtaining an accurate copy of the English Imperial Standard Troy Pound, for an intended comparison of our weights therewith, I applied to the late Captain Kater, and he had the goodness to procure for me not only a copy made by Mr. Bate, exactly similar to those described in his paper *, but also a balance of Mr. Robinson, of the same dimension and construction as that used by himself in comparing the legal standard in the custody of the Clerk of the House of Commons. The copy of the Troy pound is of the same kind of brass as that used by Mr. Bate for the other copies sent by Captain Kater to different towns in Great Britain. It bears the stamp "T^{y} P^{d} 1824"; the same stamp, in fact, that was upon the pound No. 2.† which Captain Kater sent to Edinburgh. I shall designate this pound by the letter K. I received it, March 12, 1827, from the late Dr. Young. He had noted upon the cover of the box, "Imperial Troy pound: found by Captain "Kater to exceed the standard a very little, not more than ·006 grain."

2. Fearing that this comparison (giving only *one* limit, for it is not said how much the difference is *below* 0·006 gr.) might not be made with that care which I thought necessary for the use I intended to make of the copy, I wrote again to Captain Kater, begging him to send me a second copy compared more carefully with the standard. He kindly undertook the task, ordered for me at Mr. Robinson's a second copy made of brass, together with divisions by halves, and sent it me in the summer of 1828, with the following notice:

"York Gate, Regent's Park, 18th July, 1828.

" .... I carefully compared the Troy pound which Robinson made for you with two separate " pounds of my own, the errors of which I had well determined. These pounds I designate the *old* " *pound* and the *unmarked pound.* I will copy for your satisfaction the comparisons at length.

Date.	Old Pound.	Prof. S.'s Pound.
1828. June 10.	4·5	3·1
	5·0	3·9
	5·7	3·8
June 11.	4·5	3·0
Mean...	4·92	3·45
Deduct..	1·83	3·09
	3·09	0·36

The *old pound* is heavier than the imperial pound ·0122 gr. = 1·83 div. (1·5 div. being = ·01 gr.)

Prof. S.'s pound too heavy 0·36 div. = 0·0024 gr.

* Philosophical Transactions, 1826.

† Ibid., p. 12.

Date.	Unmarked Pound.	Prof. S.'s Pound.
June 13.	3·3 4·6 4·8 4·0	3·3 3·5 3·8 3·6
Mean...	4·17 3·00	3·55 3·17
Deduct..	7·17 4·00	0·38
	3·17	

N.B. ·02 gr. was placed in the opposite scale when the *unmarked pound* was counterpoised, and removed on replacing that pound with Prof. SCHUMACHER's pound.

The *unmarked pound* is heavier than the imperial pound ·0267 gr., or 4·0 divisions.

Prof. S.'s pound too heavy 0·38 div. = ·0025 gr.

"HENRY KATER."

The divisions to be deducted from the *unmarked* pound are, after an accurate calculation, 4·005 = 4·01. Of course ROBINSON's copy is, after the comparisons with the *unmarked* pound, too heavy 0·39 div. = 0·0026 gr., and by a mean of both pounds it is too heavy 0·0025 gr.

I call this new or second pound, made by ROBINSON, K^n, in order to distinguish it from the first pound, made by BATE, and compared for me by Captain KATER, which I have called K.

3. As soon as I received K^n, (Sept. 6, 1828,) I proceeded to compare it with K, and obtained the following comparisons, in which the divisions are already reduced to parts of the grain.

1828.	gr.	1828.	gr.
Sept. 7.	$K = K^n$ + 0·0191	Sept. 21.	$K = K^n$ + 0·0192
	+ 0·0204		+ 0·0209
	+ 0·0207	Sept. 22.	+ 0·0200
	+ 0·0182		+ 0·0216
	+ 0·0187		+ 0·0199
Sept. 21.	+ 0·0209		+ 0·0196
	+ 0·0193		+ 0·0207
	+ 0·0185		+ 0·0196
	+ 0·0188		+ 0·0216
	+ 0·0198		+ 0·0193
Mean of the three days $K = K^n + 0·0198$ gr. (20 comparisons.)			

4. This would give, assuming as zero Captain KATER's determination of K^n, (which he found too heavy 0·0025 gr.,) K too heavy 0·0223 gr. Therefore K, which, according to Captain KATER's first statement, should not exceed the standard more than 0·006 gr., exceeded it 0·022 gr. This discordance being far too considerable for the powers of ROBINSON's balances, and not suggesting such a difference of specific gravity between the two kinds of brass (BATE's and ROBINSON's) that might explain it, I sent K back to Captain KATER, with a request that he would compare it once more with his two pounds; explaining at the same time the cause of the additional trouble

I gave him. He had the goodness to comply with my wish. I received an answer, (out of which I extract the passage relating to the comparisons in 1827 and 1829,) with the following notice, which I subjoin entire.

"York Gate, Regent's Park, 31st May, 1829.

" .... I have the pleasure at length to send you the comparisons of your Troy pound with two " of mine. I am totally at a loss to conceive how this pound can have undergone the alteration you " mention; for as to an error of even the hundredth of a grain in the mean of two or three compari- " sons, it appears to me to be impossible. When I formerly tried it, it was merely with a view to see " that Mr. BATE had not made any great error, and I therefore made only two or three comparisons. " It is useless, however, to speculate further upon this.

" The comparisons I now send you have been made with all the care I could bestow on them; " and I think, taking the mean value of this pound and the one by ROBINSON I before examined, you " will be *very* near the truth.

" HENRY KATER.

" Comparisons of Professor SCHUMACHER'S Troy Pound, made by BATE, with Captain KATER'S two Troy Pounds.

Date.	Capt. K.'s Old Pound.	Prof. S.'s Pound.	Capt. K.'s Unmarked Pound.
1829.	Div.	Div.	Div.
Feb. 18.	0·6	1·5	1·9
19.	0·6	1·6	1·5
	0·6	1·9	1·6
	0·5	1·6	1·6
20.	0·4	1·1	1·3
	0·2	1·1	1·3
	0·9	2·9	2·7
21.	0·8	2·8	2·6
	1·1	3·4	3·7
22.	1·0	2·7	3·0
	1·1	3·3	3·0
	1·1	2·5	2·7
23.	1·2	2·9	2·4
	0·5	2·9	2·7
24.	1·1	3·3	2·8
26.	1·7	3·5	3·3
27.	1·7	3·8	3·2
Mar. 9.	0·6	1·8	1·2
	0·4	1·9	1·2
10.	0·3	2·2	2·1
Mean ..	0·79	2·44	2·29
	2·44		2·44
	+1·65		+0·15

One hundredth of a grain was found to be = 1·38 div.
Hence 1 div. = 0·00725 gr. (See below.)

Zero changed.

Professor SCHUMACHER'S pound heavier than the old pound 1·65 div.	=	·0119 gr.
Old pound heavier than the imperial pound		·0122
Professor SCHUMACHER'S pound heavier than the imperial pound		·0241
Professor SCHUMACHER'S pound heavier than the unmarked pound 0·15 div.	=	·0011
Unmarked pound heavier than the imperial pound		·0267
Prof. S.'s pound { By unmarked pound ..	+	·0278
Prof. S.'s pound { By old pound	+	·0241
Mean	+	·0259

"The above value of the divisions of the balance was determined by means of six different weights "of ·03 gr. each, and three of ·02 gr. each, one pound being in each scale.

Div.	Diff.
0·8	
	4·7
5·5	
	4·2
1·3	
	4·3
5·6	
	4·0
1·6	
	4·0
5·6	
	4·1
1·5	
	4·1
5·6	
	3·8
1·8	
	4·2
6·0	
	4·6
1·4	
	3·9
5·3	
Mean	4·17

$\frac{1}{3}$ = 1·39 = ·01 gr.

Div.	Diff.
1·9	
	2·8
4·7	
	2·6
2·1	
	2·9
5·0	
	3·1
1·9	
	2·2
4·1	
Mean =	2·72

$\frac{1}{2}$ = 1·36 = ·01 gr."

5. Captain Kater's last result, by which he found K too heavy 0·0259 gr., agrees with that found by me, by Robinson's pound, within 0·0036 gr., but differs from its first result 0·0299 gr. He seems inclined to explain it by an increase of weight in the mean time, and this was also my opinion when I sent the pound back to him. It had not then the brilliancy of polish which it had when it arrived; and I thought oxydation might have increased the weight; but when I had it back in the autumn of 1829, I compared it again with the weights inclosed in Robinson's balance, with which it had been compared upon its arrival in 1827, and there was no sensible difference from the first comparison *. It is therefore more probable that the first comparison made by Captain Kater with his own weights, being only intended to see that Mr. Bate *had not made any great error*, were not made with that care with which he compared it afterwards in 1829.

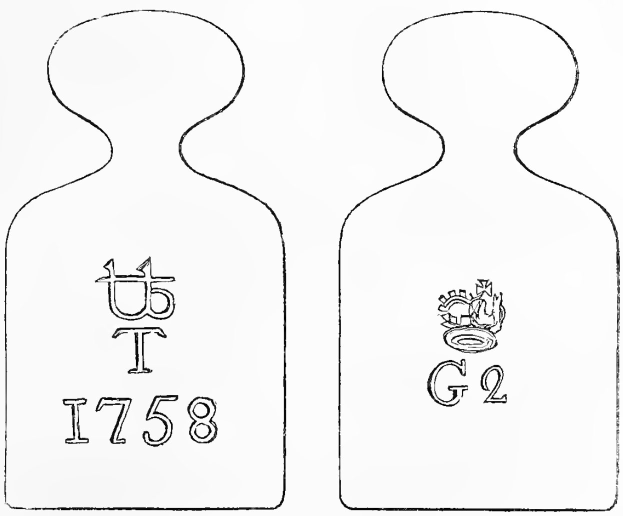
6. But before I got Captain Kater's answer, (of May 31, 1829,) I considered that a copy from two copies would hardly answer my purpose, and that, to obtain the accuracy at which I aimed, I should have a copy from the *original*, and that that copy, in order to preserve its accuracy, must not be made of a metal liable to oxydation, but of platina. I wanted at the same time more numerous comparisons than I could with any propriety charge my English friends with; and resolved to send one of my assistants (Captain Nehus, of the Royal Danish Engineers) to London, in order to make them. A platina pound was therefore ordered of Mr. Robinson.

Our Government applied to the English Government to obtain for Captain Nehus

* The brass weights of Robinson with which it was compared were of 5000 gr., 400 gr., 300 gr., (the rest were of platina,) and had a larger surface than Bate's pound. It is possible that the three brass weights of Robinson may in the mean time also have increased in weight by oxydation; but this increase ought to have been just equal to that of Bate's pound, as the comparisons in 1827 and 1829 gave the same result. Now it is not very probable that Robinson's weights should have been subjected to a smaller oxydation, and just in the inverse ratio of the surfaces smaller than Bate's pound. It is more probable that both weights have not in those two years suffered from oxydation.

free access to the Imperial Standard Troy Pound, and the permission to compare it with my copies; but this application proved unnecessary, because, before it could be made, the President of the Royal Society, Davies Gilbert, Esq., had the kindness to intercede with the Speaker of the House of Commons in Captain Nehus's behalf, and obtained permission to bring the Imperial Standard Troy Pound, upon his own responsibility, to the Apartments of the Royal Society in Somerset House, where it was deposited in the Council-Room: which place was assigned to Captain Nehus for his comparisons. Mr. Gilbert's kindness extended still further. The balance intended for these comparisons, and ordered of Mr. Robinson, not being ready when Captain Nehus arrived, he permitted him to use Ramsden's balance belonging to the Royal Society until Robinson's could be obtained. Captain Nehus experienced likewise during the course of his comparisons marks of uninterrupted kindness and attention from Mr. Hudson, then Assistant Secretary of the Royal Society.

7. As soon as the Imperial Standard Troy Pound was brought to Somerset House, Captain Nehus's first care was to make an accurate drawing of its shape and marks, measuring all its dimensions with the greatest care. The annexed drawing represents this pound in its actual dimensions; and is now, since the original has been destroyed by the calamitous fire that consumed the two Houses of Parliament in November 1834, the only thing remaining which can preserve an idea of it.



I give now Captain Nehus's comparisons, first made with Ramsden's balance, afterwards with Robinson's. The Imperial Standard Troy Pound is designated by U (Unit), my Platina copy by S.P. The difference of weight is expressed in parts of the scale, the value of which will be shown in the sequel (see pages 465 and 466.). The thermometers L and R were suspended in the box of the balance; L at left, R at right hand.

With Ramsden's Balance.

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
	pt.		°	°	°		pt.		°	°	°
1.	S.^{p} = U − 1·375	29·726	69·8	67·0	67·7	5.	S.^{p} = U − 1·900				
2.	S.^{p} = U − 0·500					6.	S.^{p} = U − 1·250				
3.	S.^{p} = U − 0·750					7.	S.^{p} = U − 0·875				
4.	S.^{p} = U − 0·350					8.	S.^{p} = U − 1·025	29·744	68·7	67·1	67·8
	1829, June 21.	Mean of 8 ...... S.^{p} = U − 1·003 pt.	Barom. 29·735	Att. Th. 69°·25	Th. L. 67°·05	Th. R. 67°·75					
9.	S.^{p} = U − 2·008	29·870	71·1	66·9	67·0	17.	S.^{p} = U − 0·675				
10.	S.^{p} = U + 0·033	29·883	69·3			18.	S.^{p} = U − 1·325				
11.	S.^{p} = U − 1·700	......	......	67·0	67·4	19.	S.^{p} = U − 0·800	......	......	67·3	67·9
12.	S.^{p} = U − 0·925					20.	S.^{p} = U − 1·617				
13.	S.^{p} = U − 1·850					21.	S.^{p} = U − 1·425				
14.	S.^{p} = U − 3·000					22.	S.^{p} = U − 1·250				
15.	S.^{p} = U − 0·275					23.	S.^{p} = U − 1·108				
16.	S.^{p} = U − 0·433					24.	S.^{p} = U − 0·250	29·904	68·4	67·2	68·0
	1829, June 23.	Mean of 16 ...... S.^{p} = U − 1·163 pt.	Barom. 29·886	Att. Th. 69°·6	Th. L. 67°·1	Th. R. 67°·57					
25.	S.^{p} = U − 1·050	29·992	75·9	67·8	67·8	33.	S.^{p} = U − 2·558				
26.	S.^{p} = U − 0·900					34.	S.^{p} = U − 1·950	......	......	68·5	68·0
27.	S.^{p} = U − 0·350					35.	S.^{p} = U + 0·200				
28.	S.^{p} = U − 1·350					36.	S.^{p} = U − 0·450				
29.	S.^{p} = U + 0·400					37.	S.^{p} = U − 1·200				
30.	S.^{p} = U − 1·350					38.	S.^{p} = U − 1·950				
31.	S.^{p} = U − 0·650					39.	S.^{p} = U − 0·175				
32.	S.^{p} = U − 1·550					40.	S.^{p} = U − 0·625				
	1829, June 24.	Mean of 16 ...... S.^{p} = U − 0·969 pt.	Barom. 29·992	Att. Th. 75°·9	Th. L. 68°·15	Th. R. 67°·9					
41.	S.^{p} = U − 2·375					46.	S.^{p} = U − 1·950				
42.	S.^{p} = U − 2·500					47.	S.^{p} = U − 0·150				
43.	S.^{p} = U − 0·650					48.	S.^{p} = U − 1·100				
44.	S.^{p} = U − 1·300					49.	S.^{p} = U − 0·150				
45.	S.^{p} = U + 0·400					50.	S.^{p} = U − 0·150	30·018	69·9	68·2	68·0
	1829, June 24.	Mean of 10 ...... S.^{p} = U − 0·992 pt.	Barom. 30·018	Att. Th. 69°·9	Th. L. 68°·2	Th. R. 68°·0					
51.	S.^{p} = U − 1·65	29·375	70·4	68·8	68·8	56.	S.^{p} = U − 2·60				
52.	S.^{p} = U − 2·25					57.	S.^{p} = U − 2·90				
53.	S.^{p} = U − 0·55					58.	S.^{p} = U − 1·50				
54.	S.^{p} = U − 2·10					59.	S.^{p} = U − 1·30				
55.	S.^{p} = U − 3·10					60.	S.^{p} = U − 2·00	29·388	70·6	69·2	69·6
	1829, June 28.	Mean of 10 ...... S.^{p} = U − 1·995 pt.	Barom. 29·381	Att. Th. 70°·5	Th. L. 69°·0	Th. R. 69°·2					
61.	S.^{p} = U − 2·60	29·660	65·5	66·5	66·8	66.	S.^{p} = U − 0·55	......	......	66·9	67·2
62.	S.^{p} = U − 1·85					67.	S.^{p} = U 0·00				
63.	S.^{p} = U − 1·40					68.	S.^{p} = U + 0·45				
64.	S.^{p} = U − 2·85					69.	S.^{p} = U − 2·80				
65.	S.^{p} = U − 1·20					70.	S.^{p} = U − 1·30	29·684	65·9		
	1829, June 29.	Mean of 10 ...... S.^{p} = U − 1·410 pt.	Barom. 29·672	Th. Att. 65°·7	Th. L. 66°·7	Th. R. 67°·0					
71.	S.^{p} = U − 2·65	29·583	65·0	65·2	65·2	76.	S.^{p} = U − 2·90	......	......	67·0	67·8
72.	S.^{p} = U − 2·45					77.	S.^{p} = U − 2·80				
73.	S.^{p} = U − 0·65					78.	S.^{p} = U − 2·55				
74.	S.^{p} = U − 2·55					79.	S.^{p} = U − 1·90				
75.	S.^{p} = U − 2·60					80.	S.^{p} = U − 1·15	29·494	65·5		
	1829, July 1.	Mean of 10 ...... S.^{p} = U − 2·22 pt.	Barom. 29·538	Att. Th. 65°·25	Th. L. 66°·1	Th. R. 66°·5					

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
81.	$S.^p = U - 1{\cdot}80$ pt.	29·554	63°·2	65°·0	65°·5	86.	$S.^p = U - 1{\cdot}85$ pt.		°	°	°
82.	$S.^p = U - 1{\cdot}50$					87.	$S.^p = U - 1{\cdot}15$				
83.	$S.^p = U - 0{\cdot}25$					88.	$S.^p = U - 1{\cdot}50$	....	....	65·4	65·8
84.	$S.^p = U - 1{\cdot}25$					89.	$S.^p = U - 0{\cdot}70$				
85.	$S.^p = U - 2{\cdot}25$					90.	$S.^p = U - 1{\cdot}90$	29·532	63·7		

1829, July 4. Mean of 10 ...... $S.^p = U - 1{\cdot}415$ pt. Barom. 29·543 Att. Th. 63°·45 Th. L. 65°·2 Th. R. 65°·65

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
91.	$S.^p = U - 1{\cdot}30$	29·530	63·2	64·0	64·1	96.	$S.^p = U - 1{\cdot}95$	....	....	65·2	65·4
92.	$S.^p = U - 1{\cdot}80$					97.	$S.^p = U - 2{\cdot}25$				
93.	$S.^p = U - 1{\cdot}35$					98.	$S.^p = U - 1{\cdot}35$				
94.	$S.^p = U - 1{\cdot}10$					99.	$S.^p = U - 1{\cdot}95$				
95.	$S.^p = U - 2{\cdot}35$					100.	$S.^p = U - 3{\cdot}30$				

1829, July 5. Mean of 10 ...... $S.^p = U - 1{\cdot}870$ pt. Barom. 29·530 Att. Th. 63°·2 Th. L. 64°·6 Th. R. 64°·75

With Robinson's Balance.

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
1.	$S.^p = U - 1{\cdot}60$					6.	$S.^p = U - 1{\cdot}85$				
2.	$S.^p = U - 1{\cdot}35$					7.	$S.^p = U - 2{\cdot}15$	....	....	69·4	69·8
3.	$S.^p = U - 1{\cdot}65$	....	....	69·5	69·8	8.	$S.^p = U - 0{\cdot}95$				
4.	$S.^p = U - 1{\cdot}70$					9.	$S.^p = U - 1{\cdot}30$				
5.	$S.^p = U - 1{\cdot}85$					10.	$S.^p = U - 1{\cdot}70$	29·462	68·8		

1829, June 28. Mean of 10 ...... $S.^p = U - 1{\cdot}610$ pt. Barom. 29·462 Att. Th. 68°·8 Th. L. 69°·45 Th. R. 69°·8

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
11.	$S.^p = U - 1{\cdot}00$	29·636	65·2	66·0	66·2	16.	$S.^p = U - 0{\cdot}25$				
12.	$S.^p = U - 0{\cdot}55$					17.	$S.^p = U - 0{\cdot}25$				
13.	$S.^p = U + 0{\cdot}30$					18.	$S.^p = U + 0{\cdot}40$				
14.	$S.^p = U - 0{\cdot}40$					19.	$S.^p = U - 2{\cdot}00$				
15.	$S.^p = U - 1{\cdot}65$	....	....	66·8	67·0	20.	$S.^p = U - 2{\cdot}15$	29·660	65·5	67·1	67·3

1829, June 29. Mean of 10 ...... $S.^p = U - 0{\cdot}755$ pt. Barom. 29·648 Att. Th. 65°·35 Th. L. 66°·6 Th. R. 66°·8

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
21.	$S.^p = U - 2{\cdot}75$	29·460	65·5	65·8	66·0	26.	$S.^p = U - 1{\cdot}35$				
22.	$S.^p = U - 1{\cdot}60$					27.	$S.^p = U - 1{\cdot}40$				
23.	$S.^p = U - 2{\cdot}65$	....	....	66·8	67·0	28.	$S.^p = U - 1{\cdot}30$				
24.	$S.^p = U - 2{\cdot}65$					29.	$S.^p = U - 1{\cdot}65$	....	....	67·0	67·1
25.	$S.^p = U - 2{\cdot}00$					30.	$S.^p = U - 1{\cdot}30$	29·406	66·0		

1829, July 1. Mean of 10 ...... $S.^p = U - 1{\cdot}865$ pt. Barom. 29·433 Att. Th. 65°·75 Th. L. 66°·5 Th. R. 66°·7

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
31.	$S.^p = U - 1{\cdot}65$	29·532	63·7	65·0	65·2	41.	$S.^p = U - 0{\cdot}90$	29·510	64·2		
32.	$S.^p = U - 0{\cdot}90$					42.	$S.^p = U - 1{\cdot}30$				
33.	$S.^p = U - 1{\cdot}00$					43.	$S.^p = U - 2{\cdot}25$				
34.	$S.^p = U - 1{\cdot}45$					44.	$S.^p = U - 1{\cdot}50$				
35.	$S.^p = U - 1{\cdot}20$					45.	$S.^p = U - 0{\cdot}75$				
36.	$S.^p = U - 1{\cdot}45$					46.	$S.^p = U - 0{\cdot}80$				
37.	$S.^p = U - 1{\cdot}45$					47.	$S.^p = U - 1{\cdot}45$				
38.	$S.^p = U - 0{\cdot}75$					48.	$S.^p = U - 0{\cdot}95$				
39.	$S.^p = U - 1{\cdot}45$					49.	$S.^p = U - 1{\cdot}50$				
40.	$S.^p = U - 0{\cdot}65$	....	....	65·1	65·3	50.	$S.^p = U - 1{\cdot}60$	29·513	64·2	65·2	65·4

1829, July 4. Mean of 20 ...... $S.^p = U - 1{\cdot}247$ pt. Barom. 29·518 Att. Th. 64°·0 Th. L. 65°·1 Th. R. 65°·3

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
51.	$S.^p = U - 1{\cdot}65$	29·596	63·8	64·8	65·0	61.	$S.^p = U - 1{\cdot}50$				
52.	$S.^p = U - 0{\cdot}65$					62.	$S.^p = U - 0{\cdot}95$				
53.	$S.^p = U - 0{\cdot}45$					63.	$S.^p = U - 1{\cdot}25$				
54.	$S.^p = U + 0{\cdot}10$					64.	$S.^p = U - 0{\cdot}80$				
55.	$S.^p = U - 0{\cdot}70$					65.	$S.^p = U - 1{\cdot}40$				
56.	$S.^p = U - 1{\cdot}15$					66.	$S.^p = U - 1{\cdot}45$				
57.	$S.^p = U - 1{\cdot}50$					67.	$S.^p = U - 1{\cdot}25$				
58.	$S.^p = U - 0{\cdot}75$					68.	$S.^p = U - 0{\cdot}50$				
59.	$S.^p = U - 0{\cdot}75$	....	....	65·2	65·4	69.	$S.^p = U - 1{\cdot}15$				
60.	$S.^p = U - 0{\cdot}60$					70.	$S.^p = U - 0{\cdot}80$				

1829, July 5. Mean of 20 ...... $S.^p = U - 0{\cdot}9575$ pt. Barom. 29·596 Att. Th. 63°·8 Th. L. 65°·0 Th. R. 65°·2

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
	pt. gr.		°	°	°		pt. gr.		°	°	°
71.	S.^{p} = U + 0·15 − 0·01	29·690	64·4	65·0	65·2	86.	S.^{p} = U − 0·10 − 0·01				
72.	S.^{p} = U − 0·50 − 0·01					87.	S.^{p} = U + 0·35 − 0·01				
73.	S.^{p} = U + 0·45 − 0·01					88.	S.^{p} = U + 0·05 − 0·01				
74.	S.^{p} = U − 0·95 − 0·01					89.	S.^{p} = U + 0·20 − 0·01				
75.	S.^{p} = U + 0·50 − 0·01					90.	S.^{p} = U + 0·70 − 0·01				
76.	S.^{p} = U + 0·10 − 0·01					91.	S.^{p} = U + 0·30 − 0·01	....	....	65·8	66·0
77.	S.^{p} = U 0·00 − 0·01	....	....	65·8	66·0	92.	S.^{p} = U + 0·05 − 0·01				
78.	S.^{p} = U − 0·45 − 0·01					93.	S.^{p} = U + 0·10 − 0·01				
79.	S.^{p} = U + 0·05 − 0·01					94.	S.^{p} = U − 0·10 − 0·01				
80.	S.^{p} = U − 0·20 − 0·01					95.	S.^{p} = U + 0·90 − 0·01				
81.	S.^{p} = U − 0·10 − 0·01					96.	S.^{p} = U + 0·30 − 0·01				
82.	S.^{p} = U 0·00 − 0·01					97.	S.^{p} = U − 0·25 − 0·01				
83.	S.^{p} = U + 0·50 − 0·01					98.	S.^{p} = U + 0·15 − 0·01				
84.	S.^{p} = U + 0·10 − 0·01					99.	S.^{p} = U + 0·10 − 0·01				
85.	S.^{p} = U − 0·10 − 0·01					100.	S.^{p} = U 0·00 − 0·01	29·750	65·0	65·9	66·1

1829, July 9. Mean of 30 ...... S.^{p} = U + 0·070 pt. − 0·01 gr. Barom. 29·720 Att. Th. 64°·7 Th. L. 65°·57 Th. R. 65°·77

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
	pt.						pt.				
101.	S.^{p} = U − 0·05	30·188	63·2	64·0	64·2	111.	S.^{p} = U + 0·05	....	....	64·8	65·0
102.	S.^{p} = U + 0·20					112.	S.^{p} = U + 0·35				
103.	S.^{p} = U + 0·10					113.	S.^{p} = U − 0·05				
104.	S.^{p} = U + 0·05					114.	S.^{p} = U + 0·25				
105.	S.^{p} = U + 0·30					115.	S.^{p} = U − 0·15				
106.	S.^{p} = U − 0·05	....	....	64·7	64·9	116.	S.^{p} = U − 0·05				
107.	S.^{p} = U + 0·25					117.	S.^{p} = U + 0·10				
108.	S.^{p} = U − 0·15					118.	S.^{p} = U + 0·35				
109.	S.^{p} = U − 0·30					119.	S.^{p} = U + 0·30				
110.	S.^{p} = U − 0·45					120.	S.^{p} = U − 0·20	30·170	64·0		

1829, August 1. Mean of 20 ...... S.^{p} = U + 0·0425 pt. Barom. 30·179 Att. Th. 63°·6 Th. L. 64°·5 Th. R. 64°·7

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
121.	S.^{p} = U 0·00	30·188	64·6	66·0	66·0	131.	S.^{p} = U − 0·20				
122.	S.^{p} = U − 0·15					132.	S.^{p} = U − 0·40				
123.	S.^{p} = U − 0·10					133.	S.^{p} = U − 0·20				
124.	S.^{p} = U − 0·15					134.	S.^{p} = U − 0·40				
125.	S.^{p} = U 0·00					135.	S.^{p} = U 0·00				
126.	S.^{p} = U − 0·35					136.	S.^{p} = U − 0·30	......	......	65·8	66·0
127.	S.^{p} = U − 0·40					137.	S.^{p} = U − 0·30				
128.	S.^{p} = U − 0·05	......	......	66·0	66·0	138.	S.^{p} = U − 0·90				
129.	S.^{p} = U − 0·05					139.	S.^{p} = U − 0·65				
130.	S.^{p} = U − 0·45					140.	S.^{p} = U − 0·40	30·184	64·6		

1829, August 2. Mean of 20 ...... S.^{p} = U − 0·2725 pt. Barom. 30·186 Att. Th. 64°·6 Th. L. 65°·93 Th. R. 66°·0

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
	pt. gr.										
141.	S.^{p} = U + 0·50 − 0·01	30·052	64·5	65·2	65·4	156.	S.^{p} = U − 0·50				
142.	S.^{p} = U + 0·60 − 0·01					157.	S.^{p} = U − 0·15				
143.	S.^{p} = U + 0·55 − 0·01					158.	S.^{p} = U − 0·60				
144.	S.^{p} = U + 0·15 − 0·01					159.	S.^{p} = U − 0·30				
145.	S.^{p} = U + 0·70 − 0·01					160.	S.^{p} = U − 0·25				
146.	S.^{p} = U + 0·15 − 0·01					161.	S.^{p} = U − 0·20	29·916	64·8	65·8	66·0
147.	S.^{p} = U + 0·60 − 0·01	......	......	65·8	66·0	162.	S.^{p} = U − 0·10				
148.	S.^{p} = U + 0·45 − 0·01					163.	S.^{p} = U − 0·25				
149.	S.^{p} = U − 0·60					164.	S.^{p} = U − 0·65				
150.	S.^{p} = U − 0·95					165.	S.^{p} = U − 0·35				
151.	S.^{p} = U − 0·65					166.	S.^{p} = U − 0·70				
152.	S.^{p} = U − 0·20					167.	S.^{p} = U − 0·10				
153.	S.^{p} = U 0·00					168.	S.^{p} = U − 0·95				
154.	S.^{p} = U − 0·75					169.	S.^{p} = U − 0·55				
155.	S.^{p} = U − 0·20	......	......	65·0	65·2	170.	S.^{p} = U − 0·70	29·882	65·0	66·0	66·1

1829, August 3. Mean of 30 ...... S.^{p} = U − $\frac{6·00 \text{ pt.} - 0·08 \text{ gr.}}{30}$ Barom. 29·950 Att. Th. 64°·77 Th. L. 65°·56 Th. R. 65°·74

= U − 0·20 pt. − 0·002667 gr.

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
	gr. pt.		°	°	°		gr. pt.		°	°	°
171.	S.P = U − 0·01 + 0·45	29·736	63·9	64·9	65·2	186.	S.P = U − 0·01 + 0·10				
172.	S.P = U − 0·01 + 0·35					187.	S.P = U − 0·01 + 0·55				
173.	S.P = U − 0·01 + 0·50					188.	S.P = U − 0·01 + 0·20				
174.	S.P = U − 0·01 + 0·40					189.	S.P = U − 0·01 + 0·40				
175.	S.P = U − 0·01 + 0·80					190.	S.P = U − 0·01 + 0·60				
176.	S.P = U − 0·01 − 0·25	......	......	65·0	65·2	191.	S.P = U − 0·01 + 0·55				
177.	S.P = U − 0·01 + 0·70					192.	S.P = U − 0·01 + 0·90	......	......	65·0	65·3
178.	S.P = U − 0·01 + 0·35					193.	S.P = U − 0·01 + 0·35				
179.	S.P = U − 0·01 + 0·75					194.	S.P = U − 0·01 + 0·35				
180.	S.P = U − 0·01 + 0·20					195.	S.P = U − 0·01 + 0·65				
181.	S.P = U − 0·01 + 0·20					196.	S.P = U − 0·01 + 0·15				
182.	S.P = U − 0·01 + 0·20					197.	S.P = U − 0·01 + 1·00				
183.	S.P = U − 0·01 + 1·10					198.	S.P = U − 0·01 − 0·05				
184.	S.P = U − 0·01 + 0·55	......	......	65·1	65·4	199.	S.P = U − 0·01 + 0·75				
185.	S.P = U − 0·01 + 0·55					200.	S.P = U − 0·01 + 0·70	29·710	63·8		

1829, August 4. Mean of 30 ...... S.P = U − 0·01 gr. + 0·4683 pt. Barom. 29·723 Th. Att. 63°·85 Th. L. 65°·0 Th. R. 65°·27

8. It remains now, for the purpose of obtaining the results of these comparisons, to determine the values of the parts or divisions of the graduated arc of the balance under the pressure of one troy pound in each scale, for RAMSDEN'S and ROBINSON'S balances, to reduce the indications of the barometer to absolute heights at 32° temperature, and to find the errors (if any) of the thermometers employed.

9. For determining the values of the divisions or parts of the graduated arc of RAMSDEN'S balance, the following observations were made, one troy pound being in each scale.

	grs. pts.		grs. pts.		grs. pts.		grs. pts.
June 21.	0·04 = 4·575	June 21.	0·04 = 4·620	June 21.	0·04 = 4·570	June 22.	0·04 = 5·750
21.	0·04 = 6·250	21.	0·04 = 4·550	22.	0·04 = 4·900	22.	0·04 = 4·375
Mean of the two days ...... 0·32 gr. = 39·590 parts, or 1 part = 0·00808 gr.							
The centre of gravity of the balance was altered.							
June 23.	0·04 = 6·738	June 24.	0·04 = 5·375	June 28.	0·04 = 4·950	July 4.	0·04 = 6·880
23.	0·04 = 6·033	24.	0·04 = 5·700	29.	0·04 = 6·550	4.	0·04 = 6·825
23.	0·04 = 6·575	24.	0·04 = 5·500	29.	0·04 = 5·325	5.	0·02 = 3·875
23.	0·04 = 7·279	24.	0·04 = 5·425	July 1.	0·04 = 6·850	5.	0·02 = 2·150
24.	0·04 = 6·138	28.	0·04 = 5·875	1.	0·04 = 5·525		
Mean of the seven days ...... 0·72 gr. = 109·568 parts, or 1 part = 0·00657 gr.							
The centre of gravity of the balance was altered.							
August 2.	0·02 = 2·800	August 2.	0·02 = 1·800	August 4.	0·02 = 1·300	August 4.	0·02 = 1·300
2.	0·02 = 2·200	4.	0·02 = 1·950	4.	0·02 = 1·500		
2.	0·02 = 1·100	4.	0·02 = 2·100	4.	0·02 = 1·400		
Mean of the two days ...... 0·20 gr. = 17·450 parts, or 1 part = 0·01146 gr.							

Of course we must employ, for the reduction of the comparisons, the following values of the parts:

June 21, 22.	1 part = 0·00808 gr.	RAMSDEN'S balance.
June 23, 24, 28, 29, July 1, 4, 5.	1 part = 0·00657	
August 2, 4.	1 part = 0·01146	

The centre of gravity of ROBINSON'S balance was never altered during the course of the observations, so that upon that account there seems no objection to employ the totality of the determinations of the values of the parts of the graduated scale for reducing all the observations made with it. All these determinations were made with one troy pound in each scale.

	gr. pts.		gr. pts.		gr. pts.		gr. pts.
June 28.	0·02 = 2·125	July 5.	0·02 = 1·800	July 11.	0·02 = 1·550	July 14.	0·02 = 1·550
28.	0·02 = 2·050	5.	0·02 = 1·825	11.	0·02 = 1·650	15.	0·02 = 1·500
29.	0·02 = 1·975	7.	0·02 = 1·575	12.	0·02 = 1·800	15.	0·02 = 1·500
29.	0·02 = 1·850	7.	0·02 = 1·650	12.	0·02 = 1·700	15.	0·02 = 1·600
July 1.	0·02 = 1·575	7.	0·02 = 1·750	12.	0·02 = 1·750	15.	0·02 = 2·050
1.	0·02 = 1·450	9.	0·02 = 1·725	12.	0·02 = 1·650	16.	0·02 = 1·700
4.	0·02 = 1·800	9.	0·02 = 1·725	12.	0·02 = 1·700	16.	0·02 = 1·500
4.	0·02 = 1·935	9.	0·02 = 1·650	14.	0·02 = 1·650	16.	0·02 = 1·600
4.	0·02 = 1·675	9.	0·02 = 1·775	14.	0·02 = 1·650	16.	0·02 = 1·900
5.	0·02 = 1·575	9.	0·02 = 1·750	14.	0·02 = 1·500		
Mean from June 28 to July 16 ...... 0·78 gr. = 66·735 parts, or 1 part = 0·01169 gr.							
July 30.	0·02 = 2·100	August 2.	0·02 = 1·875	August 4.	0·02 = 1·875	August 6.	0·02 = 1·900
August 1.	0·02 = 2·025	2.	0·02 = 1·950	4.	0·02 = 1·825	6.	0·02 = 1·925
1.	0·02 = 2·000	2.	0·02 = 1·950	5.	0·02 = 1·950		
2.	0·02 = 1·875	3.	0·02 = 2·075	5.	0·02 = 1·875		
Mean from July 30 to August 6 ...... 0·28 gr. = 27·200 parts, or 1 part = 0·01029 gr.							

Between the 16th of July and the 30th of that month there is a fortnight in which the balance has not been employed. As this interval may have affected the sensibility of the balance, it seems preferable not to take a mean of all the observations, but to divide them into two groups; one from June 28 to July 16, the other from July 30 to August 6. Indeed it appears, by inspection, that to 0·02 gr. in the last period belong more parts than in the former. We shall consequently employ the two values

from June 28 to July 16, 1 part = 0·01169 gr.
from July 30 to Aug. 6, 1 part = 0·01029 gr. } Robinson's balance.

10. The barometer used in the course of these comparisons was lent by Messrs. Troughton and Simms to Captain Nehus, because the instrument ordered before for that purpose was not finished when Captain Nehus arrived in England. He received it but some days before his return. Captain Nehus compared the instrument of Messrs. Troughton and Simms 19 times, from June 17 to June 27, with the barometer of the Royal Society, and found that 0·066 inch must be added to its indications in order to correspond with those of the instrument of the Royal Society.

This instrument, after the experiments were completed, was returned to the owners, and could of course not be immediately compared with my standard barometer of Buzengeiger, whose tube has an interior diameter of nearly 8 French lines. It was, however, compared with my standard by means of a mountain barometer of Dollond, which Captain Nehus brought over, and which had in London been compared with the instrument of Messrs. Troughton and Simms. According to these comparisons 0·057 inch must be added to its indications to reduce them to *absolute* heights. I have adopted this correction, which differs 0·009 inch from that given by the barometer of the Royal Society, because my standard is furnished with an apparatus serving to verify the position of the microscope with regard to the divided scale, and because in tubes of so large a diameter there can be no uncertainty in the value of the small correction for capillarity. For the comparison of the two pounds, it is of little consequence what correction is employed, + 0·066 inch or + 0·057 inch. It is unnecessary to add, that +0·057 inch involves the correction for capillarity and for the zero point of the scale; so that after having applied it, the heights given by the barometer may be considered as *absolute* heights.

11. The thermometers employed were small thermometers with ivory scales made by Messrs. Troughton and Simms, both suspended in the case of the balance near to the two ends of the beam. The thermometer marked L was at the left hand of the observer, the thermometer marked R at the right hand. I could determine only the corrections of the thermometer R, because the thermometer L was found to be broken when it arrived in Altona. Of course all its indications ought to be left out; and the temperature, during the observations, is only to be taken from the readings of the thermometer R.

For ascertaining its corrections, I employed two excellent standard thermometers; one the present of the late Mr. Troughton, which he had constructed expressly for me, the other the present of Professor Bessel, the corrections of which he had with great care determined, according to his method. Both standards have the Fahrenheit scale. The scale of the thermometer R being of ivory, I could not compare it with the standards in water, but I brought its bulb in contact with the bulb of the standards, and enveloped both bulbs in a thick cover of down. I found by

Troughton's standard at 69°·8 the corr. of R = − 0°·70, by 16 comp. made on 4 days.
at 63·3 ——— = − 0·61, by 16 comp. made on 7 days.
Bessel's standard .... at 64·2 ——— = − 0·64, by 10 comp. made on 5 days.

The correction given by Bessel's standard agrees within 0°·02 with that given by Troughton's standard. Upon these data I have constructed the following small Table of corrections to be applied to the thermometer R.

	Correction.		Correction.
63°	= − 0°·61	67°	= − 0°·66
64	= − 0·62	68	= − 0·68
65	= − 0·63	69	= − 0·69
66	= − 0·65	70	= − 0·70

12. It remains only to state how the observed heights of the barometer have been reduced to absolute heights at the temperature of 32°. I have for this purpose employed a table, which will be given in my "Jahrbuch für 1837," constructed upon the formula,

$$\text{Barometer reduction to } 32^\circ = h \, \frac{m\,(t - 32^\circ) - s\,(t - 62^\circ)}{1 + m\,(t - 32^\circ)},$$

where h denotes the height read off in English inches from the brass scale, which represents English inches at the temperature of 62°, m the expansion of the mercury in volume for 1° of Fahrenheit (= 0·0001001), and s the expansion of the brass scale in length for 1° of Fahrenheit (= 0·000010434). The temperature of the brass scale and of the mercury is supposed to be the same.

13. After having stated all the elements necessary for the reduction of the observations before given, I may now give them reduced in a general synopsis.

With Ramsden's Balance.

	No. of Comp.	Comparisons.	b.	t.		No. of Comp.	Comparisons.	b.	t.
1829.		gr.		°	1829.		gr.		°
June 21.	8	S.p = U − 0·00811	29·683	67·07	June 29.	10	S.p = U − 0·00927	29·630	66·34
23.	16	S.p = U − 0·00764	29·832	66·90	July 1.	10	S.p = U − 0·01459	29·498	65·84
24.	16	S.p = U − 0·00637	29·922	67·22	4.	10	S.p = U − 0·00930	29·507	65·01
24.	10	S.p = U − 0·00652	29·964	67·32	5.	10	S.p = U − 0·01229	29·495	64·12
28.	10	S.p = U − 0·01311	29·328	68·51					

Mean of 100 ...... S.p = U − 0·00940 gr.   b. 29·677   t. 66°·54

With Robinson's Balance.

	No. of Comp.	Comparisons.	b.	t.		No. of Comp.	Comparisons.	b.	t.
June 28.	10	S.p = U − 0·01882	29·413	69·10	July 9.	30	S.p = U − 0·00918	29·681	65·12
29.	10	S.p = U − 0·00882	29·607	66·14	Aug. 1.	20	S.p = U + 0·00044	30·141	64·07
July 1.	10	S.p = U − 0·02180	29·392	66·04	2.	20	S.p = U − 0·00281	30·146	65·35
4.	20	S.p = U − 0·01458	29·481	64·67	3.	30	S.p = U − 0·00473	29·910	65·09
5.	20	S.p = U − 0·01119	29·560	64·57	4.	30	S.p = U − 0·00518	29·686	64·64

Mean of 200 ...... S.p = U − 0·00815 gr.   b. 29·745   t. 65°·16

The means are taken according to the number of the comparisons of each day*: b is the absolute height of the barometer reduced to 32°, and t is the temperature of the air and weights as given by the Thermometer R; its indications being corrected by the Table before given (11.).

The general mean of all the 300 comparisons made (those made by Robinson's balance having double weight because they are double in number of those made by Ramsden's balance) is

$$\text{S.p} = \text{U} - 0{\cdot}00857 \text{ gr.} \qquad b = 29{\cdot}722, \quad t = 65^\circ{\cdot}62.$$

14. At the same time that Captain Nehus compared my platina copy S.p with the imperial standard, he compared also the first brass copy (K.) sent by Captain Kater (and compared, as before stated, several times by him with his two copies,) with the imperial standard. I begin by giving these observations as they were made, and shall afterwards reduce them in the same manner as the comparisons of the platina pound.

The same balances, the same barometer, and the same thermometers were used; as indeed they were at all the weighings made in Somerset House.

* If the mean result of several days be R, R′, R″, ..., and the corresponding numbers of comparisons made on each day n, n', n'', ..., the mean of all the results is $= \dfrac{\text{R}\,n + \text{R}'\,n' + \text{R}''\,n'' + \ldots\ldots}{n + n' + n'' + \ldots\ldots}$.

With Ramsden's Balance.

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
	pt.		°	°	°		pt.			°	°
1.	K = U + 5·19	29·748	68·7	66·8	67·0	5.	K = U + 5·08	......	......	66·9	67·1
2.	K = U + 1·68					6.	K = U + 4·03				
3.	K = U + 4·87					7.	K = U + 4·00				
4.	K = U + 3·40					8.	K = U + 4·33	......	......	67·0	67·4

1829, June 21. Mean of 8 ...... K = U + 4·0725 pt. Barom. 29·748 Att. Th. 68°·7 Th. L. 66°·9 Th. R. 67°·17

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
9.	K = U + 6·075	29·753	66·4	65·4	65·7	14.	K = U + 4·375				
10.	K = U + 4·125					15.	K = U + 5·600	......	......	66·0	66·4
11.	K = U + 4·825	......	......	65·8	66·0	16.	K = U + 4·000				
12.	K = U + 4·800					17.	K = U + 2·550	......	......	66·0	66·3
13.	K = U + 4·700	......	......	66·0	66·0	18.	K = U + 3·350	29·736	67·3		

1829, June 22. Mean of 10 ...... K = U + 4·440 pt. Barom. 29·745 Th. Att. 66°·9 Th. L. 65°·84 Th. R. 66°·08

With Robinson's Balance.

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
	gr. pt.						gr. pt.				
1.	K=U+0·03−0·40	29·812	63·6	64·2	64·5	17.	K=U+0·03+0·05				
2.	K=U+0·03−0·70					18.	K=U+0·03+0·10				
3.	K=U+0·03−0·05					19.	K=U+0·03−0·25				
4.	K=U+0·03−0·60					20.	K=U+0·03+0·25				
5.	K=U+0·03+0·40					21.	K=U+0·03+0·05				
6.	K=U+0·03+0·45					22.	K=U+0·03+0·65				
7.	K=U+0·03+1·20	29·713	63·9	64·6	64·6	23.	K=U+0·03+0·10				
8.	K=U+0·03+0·25					24.	K=U+0·03+1·45				
9.	K=U+0·03−0·10					25.	K=U+0·03+0·55	......	......	65·5	65·7
10.	K=U+0·03+0·05					26.	K=U+0·03+0·30				
11.	K=U+0·03+0·35					27.	K=U+0·03+0·10				
12.	K=U+0·03+0·20					28.	K=U+0·03+0·70				
13.	K=U+0·03+0·35					29.	K=U+0·03+0·05				
14.	K=U+0·03+0·75					30.	K=U+0·03+0·45				
15.	K=U+0·03+0·40	......	......	64·8	64·9	31.	K=U+0·03+0·20				
16.	K=U+0·03+0·55					32.	K=U+0·03+0·95	29·634	64·5	65·4	65·7

1829, July 7. Mean of 32 ..... K = U + 0·03 gr. + 0·2750 pt. Barom. 29·720 Th. Att. 64°·0 Th. L. 64°·9 Th. R. 65°·08

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
33.	K=U+0·03+1·65	29·800	65·4	66·0	66·2	40.	K=U+0·03+0·60				
34.	K=U+0·03+1·45					41.	K=U+0·03−0·15				
35.	K=U+0·03+1·00					42.	K=U+0·03−0·80				
36.	K=U+0·03+0·60					43.	K=U+0·03−0·55				
37.	K=U+0·03+0·70					44.	K=U+0·03 0·00				
38.	K=U+0·03−0·45					45.	K=U+0·03 0·00				
39.	K=U+0·03 0·00					46.	K=U+0·03+0·25	29·828	65·0	66·2	66·4

1829, July 9. Mean of 14 ...... K = U + 0·03 gr. + 0·3071 pt. Barom. 29·814 Att. Th. 65°·2 Th. L. 66°·1 Th. R. 66°·3

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
47.	K=U+0·03+1·10	29·481	64·2	65·0	65·2	55.	K=U+0·03−0·15	......	......	65·4	65·7
48.	K=U+0·03+0·20					56.	K=U+0·03+0·15				
49.	K=U+0·03−0·05					57.	K=U+0·03+0·70				
50.	K=U+0·03+0·50					58.	K=U+0·03+0·40				
51.	K=U+0·03+0·30					59.	K=U+0·03+1·00				
52.	K=U+0·03+0·70					60.	K=U+0·03−0·05				
53.	K=U+0·03+0·60					61.	K=U+0·03+0·60				
54.	K=U+0·03+0·45					62.	K=U+0·03+0·15	29·414	64·8	65·7	65·9

1829, July 11. Mean of 16 ...... K = U + 0·03 gr. + 0·4125 pt. Barom. 29·447 Att. Th. 64°·5 Th. L. 65°·37 Th. R. 65°6

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
63.	K=U+0·03+0·80	29·672	64·8	65·0	65·3	69.	K=U+0·03+0·10	......	......	66·0	66·2
64.	K=U+0·03+0·40					70.	K=U+0·03+0·25				
65.	K=U+0·03+0·25					71.	K=U+0·03+0·35				
66.	K=U+0·03+0·15					72.	K=U+0·03−0·20				
67.	K=U+0·03+0·55					73.	K=U+0·03+1·30				
68.	K=U+0·03+0·50					74.	K=U+0·03−0·05				

1829, July 30. Mean of 12 ...... K = U + 0·03 gr. + 0·3667 pt. Barom. 29·672 Att. Th. 64·8 Th. L. 65·5 Th. R. 65·75

15. These weighings give, using the same reductions as before for P, and taking the mean according to the number of comparisons:

	No. of Comp.	Comparisons.	*b.*	*t.*		No. of Comp.	Comparisons.	*b.*	*t.*
1829.		gr.		°	1829.		gr.		°
June 21.	8	K = U + 0·03292	29·698	66·51	July 9.	14	K = U + 0·03359	29·773	65·64
22.	10	K = U + 0·03589	29·700	65·43	11.	16	K = U + 0·03482	29·409	64·96
July 7.	32	K = U + 0·03321	29·683	64·45	30.	12	K = U + 0·03378	29·633	65·10
		Mean of 92 ...... K = U + 0·03389 gr.	29·646 *b.*	65°·09 *t.*					

This result differs considerably from that given by Capt. Kater by his last comparisons with his two weights, by which he found K = U + 0·0259 gr. (see § 5.). It is already there stated that K seems not to have undergone any change in the mean time; and its invariability is yet further proved by fifty comparisons made in October 1829 and February 1830 with K^n, by which I found

	No. of Comp.	Comparisons.		No. of Comp.	Comparisons.
1829. Oct. 7.	20	K = K^n + 0·02079	1830. Feb. 13.	12	K = K^n + 0·01916
1830. Feb. 11.	14	K = K^n + 0·01921	14.	4	K = K^n + 0·02106
		Mean of 50 ...... K = K^n + 0·01998 gr.			

This differs but 0·0002 grains, and of course nothing of importance, from the result obtained in 1828, when I found (see § 3.) K = K^n + 0·0198 grain.

16. After having compared K with the imperial standard troy pound, Capt. Nehus compared with it a brass pound which I had ordered at Mr. Robinson's. I denote this pound by S.^{b}. The comparisons were made with the same instruments as before specified, which served for all the weighings made in London. They are as follow.

With Robinson's Balance.

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
	gr. pt.		°	°	°		gr. pt.		°	°	°
1.	S.^{b}=U−0·01+0·25	30·225	64·2	65·0	65·2	11.	S.^{b}=U−0·01−0·30				
2.	S.^{b}=U−0·01 0·00					12.	S.^{b}=U−0·01−0·10				
3.	S.^{b}=U−0·01 0·00					13.	S.^{b}=U−0·01+0·15	......	......	65·9	66·0
4.	S.^{b}=U−0·01−0·35					14.	S.^{b}=U−0·01−0·15				
5.	S.^{b}=U−0·01−0·30					15.	S.^{b}=U−0·01+0·15				
6.	S.^{b}=U−0·01−0·15	.....	......	65·8	66·0	16.	S.^{b}=U−0·01−0·10				
7.	S.^{b}=U−0·01+0·20					17.	S.^{b}=U−0·01+0·60				
8.	S.^{b}=U−0·01−0·30					18.	S.^{b}=U−0·01+0·10				
9.	S.^{b}=U−0·01−0·20					19.	S.^{b}=U−0·01+0·25				
10.	S.^{b}=U−0·01−0·25					20.	S.^{b}=U−0·01−0·90	30·188	64·6		
	1829, August 2.		Mean of 20 ...... S.^{b} = U − 0·01 gr. − 0·070 pt.					Barom. 30·2065	Att. Th. 64°·4	Th. L. 65°·57	Th. R. 65°·73
21.	S.^{b}=U−0·01+0·30	29·585	63·0	64·2	64·5	31.	S.^{b}=U−0·01+0·65				
22.	S.^{b}=U−0·01−0·85					32.	S.^{b}=U−0·01+0·15				
23.	S.^{b}=U−0·01−0·25					33.	S.^{b}=U−0·01+0·40				
24.	S.^{b}=U−0·01−0·35					34.	S.^{b}=U−0·01−0·80	......	......	64·8	65·0
25.	S.^{b}=U−0·01 0·00					35.	S.^{b}=U−0·01+0·25				
26.	S.^{b}=U−0·01+0·15	......	......	64·8	65·0	36.	S.^{b}=U−0·01+0·10				
27.	S.^{b}=U−0·01+0·10					37.	S.^{b}=U−0·01+0·75				
28.	S.^{b}=U−0·01 0·00					38.	S.^{b}=U−0·01+0·55				
29.	S.^{b}=U−0·01+0·30					39.	S.^{b}=U−0·01−0·65				
30.	S.^{b}=U−0·01−0·50					40.	S.^{b}=U−0·01−0·10	29·940	63·8		
	1829, August 5.		Mean of 20 ...... S.^{b} = U − 0·01 gr. + 0·010 pt.					Barom. 29·7625	Att. Th. 63°·4	Th. L. 64°·6	Th. R. 64°·83

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
	gr. pt.						gr. at.				
41.	S.^{b}=U−0·01+0·30	30·036	63°·2	64°·2	64°·7	51.	S.^{b}=U−0·01−0·10		°	°	°
42.	S.^{b}=U−0·01+0·25					52.	S.^{b}=U−0·01+0·40				
43.	S.^{b}=U−0·01+0·40					53.	S.^{b}=U−0·01+0·35				
44.	S.^{b}=U−0·01 0·00					54.	S.^{b}=U−0·01−0·45				
45.	S.^{b}=U−0·01−0·55	......	......	64·8	65·0	55.	S.^{b}=U−0·01−0·25				
46.	S.^{b}=U−0·01−0·45					56.	S.^{b}=U−0·01 0·00				
47.	S.^{b}=U−0·01−0·05					57.	S.^{b}=U−0·01−0·00				
48.	S.^{b}=U−0·01+0·30					58.	S.^{b}=U−0·01−0·20				
49.	S.^{b}=U−0·01−0·25					59.	S.^{b}=U−0·01+0·45				
50.	S.^{b}=U−0·01−0·95					60.	S.^{b}=U−0·01 0·00				

1829, August 6. Mean of 20 ...... S.^{b} = U − 0·01 (gr.) − 0·040 (pt.) Barom. 30·036 Att. Th. 63°·2 Th. L. 64°·5 Th. R. 64°·85

These weighings give (using the same elements of reduction as before, and giving to *b* and *t* the significations already mentioned, as well as taking the means according to the number of observations), the following results :

	No. of Comp.	Comparisons.	*b*.	*t*.
1829.		gr.		°
August 2.	20	S.^{b} = U − 0·01072	30·167	65·09
5.	20	S.^{b} = U − 0·00990	29·727	64·20
6.	20	S.^{b} = U − 0·01041	30·000	64·22
Mean of 60		S.^{b} = U − 0·01034	29·965	64·50

17. These were the comparisons of my own pounds, which Captain Nehus made at Somerset House. He compared also a platina troy pound belonging to the Royal Society, and the brass pound of the Royal Mint, with the now lost imperial standard troy pound. The platina troy pound was made, upon the President's orders, by Mr. Cary *. I shall first give its comparisons with the imperial standard troy pound, premising only that I designate it by the letters RS.

With Robinson's Balance.

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
	gr. pt.						gr. pt.				
1.	RS=U−0·02+1·10	29·380	64°·8	65°·6	65°·9	11.	RS=U−0·02+0·80		°	°	°
2.	RS=U−0·02+1·10					12.	RS=U−0·02+0·85				
3.	RS=U−0·02+0·05					13.	RS=U−0·02+0·65				
4.	RS=U−0·02+1·35					14.	RS=U−0·02+0·50				
5.	RS=U−0·02+0·55					15.	RS=U−0·02+0·70	......	......	66·3	66·6
6.	RS=U−0·02+0·55					16.	RS=U−0·02+1·20				
7.	RS=U−0·02+1·55	......	......	66·0	66·2	17.	RS=U−0·02+0·60				
8.	RS=U−0·02+0·95					18.	RS=U−0·02+1·85				
9.	RS=U−0·02+0·30					19.	RS=U−0·02+0·70				
10.	RS=U−0·02+0·30					20.	RS=U−0·02+0·95	29·434	65·2	66·0	66·5
	1829, July 12.		Mean of 20 ...... RS = U − 0·02 (gr.) + 0·825 (pt.)				Barom. 29·407	Att. Th. 65°·0	Th. L. 66°·0	Th. R. 66°·3	
	pt.										
21.	RS = U − 0·00	29·810	66·0	67·0	67·2	28.	RS = U − 0·10				
22.	RS = U − 0·60					29.	RS = U 0·00				
23.	RS = U − 0·05					30.	RS = U − 0·50				
24.	RS = U 0·00					31.	RS = U − 0·35				
25.	RS = U + 0·10					32.	RS = U 0·00				
26.	RS = U − 0·35	......	......	67·5	67·8	33.	RS = U + 0·30				
27.	RS = U + 0·15					34.	RS = U − 0·30	29·832	66·8	67·8	68·1
	1829, July 14.		Mean of 14 ...... RS = U − 0·12143 (pt.)				Barom. 29·821	Att. Th. 66°·4	Th. L. 67°·4	Th. R. 67°·7	

* For the purpose of making this new platina pound, Mr. Cary was furnished with some of the platina which Dr. Wollaston had given to the Royal Society: but it seems that Mr. Cary did not employ *this* platina, but used some other kind; the reason for which has not been sufficiently explained.—F. Baily.

With Robinson's Balance.

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
	gr. pt.		°	°	°		pt.		°	°	°
35.	RS=U−0·02+1·25	29·932	66·8	67·5	67·8	43.	RS=U −0·20				
36.	RS=U−0·02+1·15					44.	RS=U −0·60				
37.	RS=U +0·05					45.	RS=U −0·05				
38.	RS=U −0·25					46.	RS=U −0·15				
39.	RS=U −0·25					47.	RS=U −0·05				
40.	RS=U −0·50	......	......	67·9	68·1	48.	RS=U −0·50	......	......	68·2	68·7
41.	RS=U −0·25					49.	RS=U −0·05				
42.	RS=U −0·40					50.	RS=U −0·60	29·902	67·0	68·1	68·6

1829, July 15. Mean of 16 ...... RS = U − 0·0025 gr. − 0·0875 pt. Barom. 29·917 Att. Th. 66°·9 Th. L. 67°·92 Th. R. 68°·3

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
51.	RS = U + 0·30	29·884	66·3	67·2	67·7	61.	RS = U − 0·20				
52.	RS = U − 0·40					62.	RS = U − 0·45				
53.	RS = U − 0·35					63.	RS = U 0·00				
54.	RS = U − 0·40					64.	RS = U − 0·25	......	......	67·9	68·1
55.	RS = U − 0·40					65.	RS = U − 0·20				
56.	RS = U − 1·10	......	......	67·6	68·0	66.	RS = U − 0·50				
57.	RS = U − 1·00					67.	RS = U − 0·30				
58.	RS = U − 0·60					68.	RS = U − 0·05				
59.	RS = U − 1·10					69.	RS = U − 0·50				
60.	RS = U − 0·10					70.	RS = U − 0·55	29·884	67·0	67·9	68·2

1829, July 16. Mean of 20 ...... RS = U − 0·4075 pt. Barom. 25·884 Att. Th. 66°·65 Th. L. 67°·65 Th. R. 68°·0

With Ramsden's Balance.

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
71.	RS = U + 1·30	30·224	63·2	64·8	65·0	86.	RS = U − 0·55				
72.	RS = U + 0·70					87.	RS = U + 0·15				
73.	RS = U + 0·80					88.	RS = U + 0·10				
74.	RS = U + 0·60					89.	RS = U 0·00				
75.	RS = U + 0·35					90.	RS = U + 0·25				
76.	RS = U + 0·10	......	......	65·0	65·1	91.	RS = U + 1·60				
77.	RS = U + 1·94					92.	RS = U + 0·20	......	......	65·4	65·6
78.	RS = U + 0·45					93.	RS = U + 0·80				
79.	RS = U − 0·15					94.	RS = U + 0·10				
80.	RS = U + 0·20					95.	RS = U + 0·45				
81.	RS = U + 0·65					96.	RS = U − 0·05				
82.	RS = U + 0·10					97.	RS = U 0·00				
83.	RS = U + 0·20					98.	RS = U + 0·05				
84.	RS = U + 0·10	......	......	65·1	65·2	99.	RS = U + 0·50				
85.	RS = U + 0·65					100.	RS = U − 0·05	30·225	64·2		

1829, August 2. Mean of 30 ...... RS = U + 0·381667 pt. Barom. 30·2245 Att. Th. 63°·7 Th. L. 65°·07 Th. R. 65°·22

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
101.	RS = U − 0·30	29·764	63·6	65·0	65·0	121.	RS = U + 0·60				
102.	RS = U − 1·50					122.	RS = U − 0·80				
103.	RS = U + 0·45					123.	RS = U 0·00				
104.	RS = U − 0·05					124.	RS = U + 0·45				
105.	RS = U 0·00					125.	RS = U − 0·50				
106.	RS = U + 1·25					126.	RS = U − 0·10				
107.	RS = U + 0·20					127.	RS = U − 0·70				
108.	RS = U + 0·15					128.	RS = U + 0·05	......	......	65·2	65·4
109.	RS = U − 0·20					129.	RS = U 0·00				
110.	RS = U − 0·05	......	......	65·0	65·2	130.	RS = U − 0·55				
111.	RS = U − 0·35					131.	RS = U − 0·10				
112.	RS = U − 0·50					132.	RS = U + 0·30				
113.	RS = U − 0·55					133.	RS = U − 0·35				
114.	RS = U − 0·05					134.	RS = U − 0·40				
115.	RS = U + 0·15					135.	RS = U − 0·10				
116.	RS = U − 1·00					136.	RS = U + 1·40	......	......	65·4	65·5
117.	RS = U + 0·35					137.	RS = U + 0·20				
118.	RS = U − 0·10	......	......	65·0	65·1	138.	RS = U − 1·35				
119.	RS = U + 1·00					139.	RS = U 0·00				
120.	RS = U + 0·50	29·742	64·0			140.	RS = U − 0·90	29·732	64·0		

1829, August 4. Mean of 40 ...... RS = U − 0·08625 pt. Barom. 29·746 Att. Th. 63°·87 Th. L. 65°·12 Th. R. 65°·24

These weighings give (using the same elements of reduction as before, giving to *b* and *t* the signification already explained, and taking the means according to the number of observations,) as follow:

1829.	No. of Comp.	Comparisons.	*b.*	*t.*	1829.	No. of Comp.	Comparisons.	*b.*	*t.*
		gr.		°			gr.		°
July 2.	20	RS = U − 0·01036	29·368	65·65	July 16.	20	RS = U − 0·00476	29·839	67·32
14.	14	RS = U − 0·00142	29·777	67·03	Aug. 2.	30	RS = U + 0·00437	30·186	64·59
15.	16	RS = U − 0·00352	29·871	67·62	4.	40	RS = U − 0·00099	29·709	64·61

Mean of 140 ...... RS = U − 0·00205 gr. 29·806 *b.* 65°·73 *t.*

The first four days Robinson's balance is used; the last two, Ramsden's balance.

18. The last pound which Captain Nehus compared with the now lost imperial standard troy pound, was that of the Royal Mint. It is marked "Ty Pd 1824", and the same that Captain Kater designates by No. 3.* Mr. (afterwards Sir John) Barton, of the Royal Mint, had the kindness to bring it to Somerset House, and it was there compared in his presence. The pound appeared in high preservation, and bore no marks of any oxydation whatever. It is needless to repeat that the same barometer and thermometers were used. The balance was that of Robinson. I designate this pound by the mark RM, since it belongs to the Royal Mint.

No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.	No.	Comparisons.	Barom.	Attached Therm.	Therm. L.	Therm. R.
	pt.			°	°		pt. gr.		°	°	°
1.	RM = U + 0·20	......	......	65·8	66·1	9.	RM = U + 0·90				
2.	RM = U + 0·65					10.	RM = U + 1·20	....	....	66·5	66·8
3.	RM = U + 0·75					11.	RM = U 0·00 + 0·01				
4.	RM = U + 0·60					12.	RM = U − 0·30 + 0·01				
5.	RM = U + 1·10					13.	RM = U + 0·45 + 0·01				
6.	RM = U + 0·55					14.	RM = U + 0·20 + 0·01				
7.	RM = U + 1·30					15.	RM = U − 0·05 + 0·01				
8.	RM = U + 0·20					16.	RM = U + 0·20 + 0·01	29·776	65·0	66·2	66·8

1829, July 30. Mean of 16 ...... RM = U + 0·496875 pt. + 0·00375 gr. Barom. 29·776 Att. Th. 65°·0 Th. L. 65°·17 Th. R. 66°·57

This gives, reduced for the mean of 16 comparisons,

RM = U + 0·00887 gr. 29·679 *b.* 65°·91 *t.*

Captain Kater found in 1824 †, RM = U + 0·0021 gr. The difference is 0·0068 gr.

19. I shall now put all the results obtained by Captain Nehus in London in one view.

No. of Comp.	Comparisons.	*b.*	*t.*
	gr.		°
92	K = U + 0·03389	29·646	65·09
300	S.p = U − 0·00857	29·722	65·62
60	S.b = U − 0·01034	29·965	64·50
140	RS = U − 0·00205	29·806	65·73
16	RM = U + 0·00887	29·679	65·91

* Philosophical Transactions, 1826, p. 12. 17, 18.

† Ibid., p. 18.

20. It would appear, from an inspection of these results, that the true weight of the now destroyed imperial standard troy pound might, even after its loss, be very well ascertained. There are five several pounds which were compared with it only six years ago with extraordinary care; these five pounds are still extant and in good preservation; the number of the comparisons exceeds 600, and these comparisons are made with excellent balances, and by a skilful and careful observer, who devoted, during several months, his whole time and attention to them. There remains, therefore, only (since each body weighed in air loses as much of its weight as the volume of air weighs, which is displaced by the body,) to add on both sides of the above 5 equations the weights of these displaced volumes of air: on one side the weight of the volume of air displaced by the copy, on the other side the weight of the volume of air displaced by the imperial standard troy pound; or to reduce (as it is generally called) these weighings to a vacuum. Indeed, if it had been possible to weigh the bodies in a vacuum, their weights would have sustained no losses, because there was nothing which they could displace, and the difference of weight indicated by the balance would have been their *true* difference of weight. This true difference of weight is evidently likewise obtained, when, by addition of the before-mentioned weights of the displaced volumes of air, the weighings made in air are corrected for the losses which the weights of the bodies necessarily suffered from the bodies being obliged to put aside the medium in which they were weighed: so that both modes of proceeding lead to the same result, viz. to the true difference of weight of the bodies.

Unhappily one of the most essential elements to calculate the volume of the now lost imperial standard troy pound, and of course to calculate the weight of an equal volume of air displaced by it, is still unknown; I mean *the specific gravity* of that pound. Even the metal of which it was made is uncertain. It is declared to be a *brass* pound by the Act of Parliament 5 George IV. chap. lxxiv. §. 4, as follows:

"And be it further enacted, That from and after the first day of May 1825, the "standard *brass* weight of one pound troy weight, made in the year 1758, now in the "custody of the Clerk of the House of Commons, shall be, and the same is hereby "declared to be, the original and genuine standard measure of weight: and that "such *brass* weight shall be, and the same is hereby declared to be, the unit or only "standard measure of weight, from which all other weights shall be derived, com-"puted and ascertained."

But as a law never pretends, nor can pretend, to decide upon the physical qualities of bodies, the expression *brass weight* must be understood as suggested to the legislators by those to whom the adjusting of the weights was committed, and states of course only *their* private opinion about the metal of which the pound was made; or, as this department fell under Captain Kater's care, we may consider the words *brass pound, brass weight,* as the expression of *his* private opinion. This is the more likely, because he seems to have considered it not only as brass, but as brass of the *same* specific gravity with the metal of the new pounds compared by him with the imperial standard. Indeed, he has nowhere noted (or, if he had noted, has nowhere published,) the state of the barometer and thermometer at the time of his comparisons; an omission which is only allowable when both weights are supposed to be of the same specific gravity and expansion *.

On the other side, Captain NEHUS, who had the imperial standard troy pound several months under his eyes, is decidedly of opinion that it was *not* of brass. He declares that its dark brown colour left no doubt that it must be of copper or bell-metal. Mr. ROBINSON, whose decision was called in aid, thought it (from the form of the rands, which the impression of the stamps had raised, and from the fine porosity of the surface,) to be of copper.

21. We have thus private opinion against private opinion, and the question still remains undecided. Under the impression that in the Parliamentary Reports about the year 1758, when the imperial standard troy pound was made, something more decisive respecting the metal of that pound might be found, I carefully perused the second volume of the "*Reports from Committees of the House of Commons.* Re-"printed (1803) by order of the House. Miscellaneous Subjects 1738—1765.—Folio." It contains two Reports from the Committees appointed to inquire into the original standards of weights and measures, both presented by Lord CARYSFORT: the first on May 26, 1758, the second on April 11, 1759. It appears that Mr. HARRIS, then Assay-Master of the Mint, presented to the first Committee three troy pounds made under his direction, whose weight was determined by a mean taken from the best old standards existing at that time, as described in the following words (p. 437 b): viz. "Therefore to ascertain the troy pound, according to the aforesaid experiments," (the mean taken from several standards,) "your Committee directed three several "troy pounds to be made under the direction of Mr. HARRIS, to be marked as "follows:"

[Here a rough sketch is inserted, showing the form of the imperial standard troy pound, and the marks stamped upon it.]

"and these have been accordingly made and adjusted with very curious and exact "scales of his at the Mint, and found to agree with the result of the experiments "made by the Committee: one of these weights is also produced herewith."

* Supposing BATE's metal, from which the new pounds were made, to be 8·0 specific gravity, and the imperial standard to be 8·1 (being a difference of only 0·1 in specific gravity from BATE's pounds), this would have required under common atmospheric circumstances (barom. 30·0, therm. 62°) a correction of 0·0109 grains to be applied to the comparisons, in order to obtain the true difference of weight. It is evident that Captain KATER, who gave his comparisons in *ten thousandth* parts of a grain, would not have neglected corrections which for so small a difference in the specific gravity affect the results even to *hundredths* of a grain, if he had not considered the metal of the old pound identical with that of the new pounds. But of this he could know nothing for certain.

This pound produced to the Committee was the now lost imperial standard troy pound, as appears from the 8th Resolution of the Committee (p. 439 a.), which proposes: "That the standard of weight ought to be the pound herewith delivered, "described in this Report, and made upon the examination and review of the several "present standard troy weights therein mentioned; and that the 12th part of the said "pound should be an ounce, the 20th part of such ounce a pennyweight, and the 24th "part of such pennyweight a grain."

This 8th Resolution of the Committee was agreed to by the House on the 2nd of June, 1758 (p. 463 a.). The pound itself was presented to the House (p. 456 b.*) in the preceding Session (April 11, 1759), and probably remained under the custody of the Clerk of the House of Commons until it was destroyed by the late fire. In all these statements there is not the least mention made of the metal of which it consisted, nor have I been able to find anything *decisive* about this point elsewhere in either of the Reports.

22. It appears, indeed, (p. 428 a.) that the standard weights of Guildhall were of *brass*; that the Charter of the Founders' Company, September 18, 1614, 12 James I., speaks of *brass* weights (p. 428 b.); that the weights in the Court of Receipt of the Exchequer were of *brass* (p. 447 b.). So that there is a great degree of probability that Mr. Harris, who adjusted his new weights upon those mentioned, made them also of brass: but this probability is somewhat lessened by the Report of the second Committee. This Committee took into its special consideration (p. 459 b.) the material of which the weights for the future were to be made; and it seems natural they should have decided for brass, if the new standard, already declared such by Parliament, (June 2nd, 1758,) or, what is the same, the now lost imperial standard troy pound, had been of brass: but they resolve only (p. 461 b.): "That models or patterns of measures of length and weights established as the genuine standards of "the kingdom should be made of *fine hard metal*, and deposited in the same place "and kept in the same manner as the trial-pieces for the coin, used on trials of the "Pix, are preserved in the Exchequer."

What is understood by *hard metal* appears (p. 460 a.) in the same Report, where the Committee decides, that "the smaller weights, from a pound through all its parts, "are most conveniently made of *copper, brass,* or other such hard metal;" where *copper* is even put in the first place as significative of the term *hard metal,* though, after the alphabetic order, *brass* ought to have preceded.

23. There is however, in the Report of this second Committee, (p. 457 a.) a statement which seems at first view to lead us to the knowledge at least of the metal, if not of the specific gravity of the lost standard. It is as follows: "Mr. Harris has "procured, by order of your Committee, two sets of the following multiples of the

* It seems that the pound was delivered to the House the 2nd of June 1758. "The Resolution of the former "Committee, agreed to by the House 2nd June 1758, ascertains the standard of weight to be the pound *then* "delivered to the House," &c. (p. 456 b.)

"standard pound, all of *fine brass*, which he will adjust with an apparatus, also "contrived on purpose, as soon as possible, viz.

" 2 pounds,
" 4 ditto,
" 8 ditto,
" 16 ditto,
" 32 ditto."

And we might consider this statement nearly as a proof that the imperial standard was also of the same *fine brass*, if the multiples might be supposed to be made *at the same time* with the standard; because it is highly probable that Mr. Harris, under whose direction the three single pounds and their multiples were made, would under these circumstances have made them all of *the same* metal. But it appears from the Report of the former Committee that the multiples *did not exist* when the three weights of one pound each were delivered to that Committee. For it states: "Your Committee intended to have had the parts and multiples of this troy pound "also made, but found that there were not instruments sufficiently exact for that "purpose; and the contriving and making of such would take up more time than "the probable continuance of this Session would permit: they have therefore *left it* "*for the future consideration of Parliament, whether anything of that nature should be* "*performed.*" (p. 437 b.)

The multiples were of course not even begun to be made when the pounds were already finished; and though it seems probable that Mr. Harris would as nearly as possible have made them of the same kind of metal as that of which the three single pounds were made, yet this produces only *a probability* that the imperial standard was of brass; but nothing proves that Mr. Harris could procure afterwards *the same* fine brass of which the three single pounds were formerly made. Now it is generally known that brass, as a compound metal, varies in density according to the different proportions of the compounding metals adopted by the makers. (I have found differences in specific gravity in different kinds of brass from 7·9 to 8·4.) Of course, if it was even *proved*, as it is not, that the imperial standard was of brass, nothing would be gained by that proof, if we could not at the same time ascertain the identical brass from which it was made.

24. This we should be able to do with a degree of probability bordering almost on certainty, if the other two single pounds, of the three presented to the Committee, could be traced, and were still existing. Indeed it is in the highest degree probable that these three single pounds, made all under Mr. Harris's direction, adjusted and presented at the same time by him, were also of the same identical metal: it remains only to ascertain what became of them. The 7th Resolution of the second Committee (p. 463 a.) states: "That it is the opinion of this Committee, that the yard mentioned "in the 2nd Resolution of the former Committee upon the subject of weights and "measures, agreed to by the House the 2nd June 1758, being the standard of length,

" and the pound mentioned in the 8th Resolution of the former Committee upon the " subject of weights and measures, agreed to by the House the 2nd June 1758, being " the standard of weight, ought to be deposited in the Court of Receipt of the Exche- " quer, and there safely kept under the Seals of the Chancellor of the Exchequer, and " of the Chief Baron, and the Seal of Office of the Chamberlains of the Exchequer, " and not to be opened but by the order and in the presence of the Chancellor of the " Exchequer and the Chief Baron for the time being."

If this pound, intended to be deposited in the Exchequer according to the wishes of the Committee, might be supposed to be a different pound from that presented to the House, (p. 457 b.) and of course one of the two remaining of the three that were presented to the former Committee, we might know (provided the Resolution was agreed to by the House) where to look for a pound of the identical metal of the lost standard; but the obvious meaning of the words seems to be, that the pound presented to the House in the last Session (i. e. the imperial standard now lost) should not be kept there, because the Committee thought it safer in the custody of the Exchequer. This is the more probable, because the following 9th Resolution (p. 463 a.) says expressly that the standard yard mentioned in the 2nd Resolution of the former Committee, agreed to by the House June 2nd 1758, was at the moment when the last Committee formed their Resolutions "now" *in the custody of the Clerk of the House*, which is not said of the standard pound, of which mention is made immediately after the yard, and which certainly would have been added (since they are precise in their expressions) if the pound had been at that moment really in the custody of the said Clerk. The Committee had probably taken it back, in order to have the multiples adjusted, which was not yet done, (see the 9th Resolution, where the words "when the same [the multiples] *are* adjusted" imply that meaning: it is also expressly said, p. 457, that Mr. HARRIS *will* adjust the multiples *as soon as possible*;) and which could not be done without having the use of the standard; and proposed by their 7th Resolution not to restore it to the Clerk of the House, but to deposit it, after having used it, at the Exchequer, where they meant also to transfer the yard, which was still in the custody of the Clerk.

It is not stated that the House agreed to this Resolution: on the contrary, it appears that it did not, because both yard and pound were, when Captain KATER compared them, and before that time, in the custody of the Clerk of the House of Commons, where they were originally deposited.

25. The following Resolutions (the 8th and 9th) of the second Committee might also possibly be understood as referring to one of the two remaining pounds of Mr. HARRIS. The 8th Resolution (p. 463 a.) proposes as the most effectual means to ascertain uniformity in measures of length and weights to be used throughout the realm, "To appoint certain persons at one particular office, with clerks and workmen " under them, for the purpose only of sizing and adjusting, for the use of the sub- " jects, all measures of length, and all weights, being parts, multiples, or certain pro-

" portions of the standards, to be used for the future." To which the 9th Resolution (ibidem) adds: "That it is the opinion of this Committee, that a model or pattern of the " said standard yard mentioned in the 2nd Resolution of the former Committee, agreed " to by the House June 2, 1758, *and now in custody of the Clerk of the House,* and a model " or pattern of the standard pound, mentioned in the 8th Resolution of the former " Committee, agreed to by the House June 2nd, 1758, together with models or patterns " of the parts of the said pound now presented to the House, and also of the multiples " of the said pound mentioned in this Report (*when the same are adjusted*), should be " kept in the said office, in custody of the said persons to be appointed for sizing " weights and measures, under the seal of the Chief Baron of the Court of Exchequer " for the time being, to be opened only by order of the said Chief Baron in his pre- " sence, or the presence of one of the Barons of the Exchequer, on the application of " the said persons, for the purpose of correcting and adjusting, as occasion shall re- " quire, the patterns or models used at the said office for sizing measures of length " and weights delivered out to the subjects."

This model or pattern of the standard pound, to be kept in the proposed office, might be understood to design one of the remaining pounds of Mr. Harris; but if the Committee had had one of those pounds in view, they would probably have specified it as one of the three single pounds presented by Mr. Harris to the former Committee: moreover, the already alleged 1st Resolution of the second Committee (p. 461 b.) speaks of models or patterns of measures of length and weights as "to " be made." They were of course *not yet made,* and therefore cannot be supposed to design the two remaining pounds of Mr. Harris, which *were already made* and adjusted, and presented as such in the preceding year (1758) to the former Committee.

To repeat in few words the results of these inquiries, it appears,

1°. That the Reports of the Committees in 1758 and 1759 do not specify the metal of which the imperial standard troy pound was made.

2°. That there is a probability that it was made of brass, according to Captain Kater's opinion.

3°. That if it was even certain that it was of brass, we ought to know of what kind of brass it was made, as the specific gravity of that metal varies according to its composition.

4°. That it is in the highest degree probable that the two remaining pounds which Mr. Harris presented to the Committee in 1758, were of the identical metal of the standard, so that if we could discover if these pounds still exist, and where they exist, the requisite specific gravity of the lost imperial standard troy pound might be ascertained.

5°. That the Reports of the Committees in 1758 and 1759 contain nothing by which we can learn what has become of these two remaining pounds.

26. Although the Reports of the Committee which presented the imperial standard troy pound to the House contain nothing from which we can ascertain its specific

gravity, there appears from another quarter a new prospect to arrive at a knowledge of this most important point. The Report from the Select Committee of the House of Lords, appointed to consider the petition of the Directors of the Chamber of Commerce and Manufactures, established by royal charter in the City of Glasgow, taking notice of the Bill intituled "*An Act for ascertaining and establishing Uniformity of Weights and Measures*," and praying their Lordships to give the matter of their petition due consideration, and that they will introduce into the Bill such parts of the petition as shall to their Lordships appear likely to prove beneficial; together with *the Minutes of Evidence* taken before the said Committee, 1823, (Ordered by the House of Commons to be printed, March 2, 1824,) Folio, contains, p. 14, under the Minutes of Evidence, the examination of Dr. KELLY (May 31, 1823): where, upon the query, "What was effected with regard to the weights and measures by the Committee of 1758?" Dr. KELLY answers: "They ordered three several troy pounds "to be adjusted, under the direction of Mr. HARRIS, the then Assay-Master of the "Mint. One of these was placed in the custody of the Clerk of the House of Commons; another was left with Mr. HARRIS, and is that now in the possession of "Mr. BINGLEY; and the third was, I understand, delivered to Mr. FREEMAN, weight-"maker to the Mint, the Exchequer, and the Bank of England, who used it as his "standard, and it is still so employed by his successor, Mr. VANDOME."

There is moreover on the same page 14 the following note:

"This weight [Mr. BINGLEY's pound] was produced to the Committee, at a subsequent meeting, by Mr. BINGLEY, who said it had formerly belonged to Mr. HARRIS "when he held the situation of Assay-Master. There was a memorandum on the lid "of the box in which it was kept, stating that Mr. HARRIS had made from it the "pound weight which was placed in the custody of the Clerk of the House of Commons by direction of the Committee of 1758, and which is called commonly the "Parliamentary pound."

If Dr. KELLY's statements be exact, as there is no doubt they are, and Messrs. BINGLEY's and VANDOME's pound be *really* the two remaining weights of the often-mentioned three which Mr. HARRIS presented to the Committee of 1758*, we can still either determine, with the highest degree of probability, the specific gravity of the lost imperial standard troy pound, or know with certainty that all hope to arrive at this knowledge is lost. It will be only requisite to ascertain with the greatest care the specific gravities of both pounds, the one in the possession of Mr. BINGLEY, and the other in the possession of Mr. VANDOME. If the specific gravity of both is found *the*

* There is an easy and obvious verification of the fact. If Mr. BINGLEY's and Mr. VANDOME's pounds be indeed the two remaining pounds of 1758, they must have the *same* stamps which the standard had, of which an exact representation, taken by Captain NEHUS in 1829, is subjoined to this paper. The Report of the Committee of 1758 says expressly (p. 437 b.), "Three several troy pounds to be made under the direction of "Mr. HARRIS, to be *marked* as follows:" here the representation of the stamps of the standard roughly cut in wood is added, which, according to the words of the Report, must be common to them all.

same, we might from that circumstance draw the highly probable conclusion, that the three single pounds of Mr. Harris, according to my hypothesis, were really made of the same identical metal; and the specific gravity of the two remaining pounds might with safety be considered as that of the lost standard. If, on the contrary, the two remaining pounds prove to be of *different* specific gravities, the hypothesis that all three were made of the same metal is evidently erroneous; and nothing can be inferred from the specific gravity of either of the two remaining. For in this case the metal of the lost standard may have been,

1. Identical with that of Mr. Bingley's pound, or
2. Identical with that of Mr. Vandome's pound, or
3. A different metal from that of both these pounds.

Now as there is no metal of which we know, except that of the two remaining pounds, that may be considered as identical with that of the lost standard, it is evident that if this also cannot be considered as being so, all hope is lost of arriving at the knowledge of the specific gravity of the late imperial standard troy pound.

27. It may be worth while to express in numbers the uncertainty that remains about the *true* weight of the lost standard pound as the case now stands; that is, without knowing if it was of brass or copper, and without having a precise knowledge of its specific gravity. To do this it will be necessary to state the formulæ and the numeric values used by me in the computation of its true difference of weight with all the pounds that have been compared with it; or, what is the same, in the reduction to a vacuum of all the weighings made. I have adopted both the formulæ and the numeric values contained in M. Bessel's excellent paper on the reduction of weighings in the *Astron. Nachrichten*, vol. vii. p. 373.

The specific gravity of a body is the quotient of its density divided by the density of that substance, which is considered as unity: as such, pure water is here adopted. But since both these densities vary with the temperature,—because the same invariable quantity of matter which the body contains is always distributed over its volume, variable with the temperature, so that generally speaking (the exception which pure water affords will immediately be noticed) the body has at a higher temperature less density than at a lower,—we must fix a certain temperature at which the body as well as the water is to be considered. It is not necessary that this fixed temperature should be the same for the body and the water, its choice for both being quite arbitrary.

For bodies, the most natural seems to be that of one of the fixed points of the thermometer; and the temperature of melting ice (Fahrenheit 32°, Réaumur and Centigrade 0°) is here adopted. For pure water, it is known that there is a maximum of its density, which takes place at nearly 39° Fahr.; and this maximum of density, or the density of pure water under the temperature of nearly 39° Fahr., is by preference adopted as unity.

Now as the densities of two bodies are in the direct ratio of their masses, and in the inverse ratio of their volumes, we can express the specific gravity of a body as the quotient of its mass, divided by the mass of pure water taken at its greatest density contained in a volume equal to that which the body occupies at 32° FAHR.: or, what is the same, as the quotient of its mass, divided by the mass of pure water which the body displaces; the water having the temperature of nearly 39° FAHR., and the body that of 32° FAHR.

If we denote the specific gravity of the body, thus understood, by Δ, the mass of the body by M, and the ratio of one of its dimensions under the temperature of the melting ice, and under that of the weighing, by ... 1 : R, the space which it occupies at the temperature which it has when it is weighed is $= \frac{M}{\Delta} R^3$, and it displaces a mass of air equal to $\frac{M}{\Delta} R^3 q$, where q denotes the specific gravity of the air at the moment of the weighing.

For the weights employed, let δ, m, r denote the same things as Δ, M, R for the body. We have consequently, when the body and the weights put upon the balance are in equilibrio, the equation

$$M\left(1 - \frac{R^3 q}{\Delta}\right) = m\left(1 - \frac{r^3 q}{\delta}\right) \qquad (1)$$

whereby M, or the absolute weight of the body, is easily obtained, viz.

$$M = m + M\frac{R^3 q}{\Delta} - m\frac{r^3 q}{\delta} \qquad (2)$$

or, as m may in the second number of the equation be generally substituted for M,

$$M = m + m\frac{R^3 q}{\Delta} - m\frac{r^3 q}{\delta} \text{ nearly.} \qquad (3)$$

Should a case occur in which this substitution would affect the last place of decimals, we may employ, either the exact equation, derived immediately from (1)

$$M = m\,\frac{1 - \frac{r^3 q}{\delta}}{1 - \frac{R^3 q}{\Delta}} \qquad (4)$$

or put the value of M, found by the equation (3) as coefficient of $\frac{R^3 q}{\Delta}$, into the equation (2).

28. In order to obtain the specific gravity, the body is weighed in air, and also when immersed in pure water. In the last case, as in the former, the *weights* are still in air. These two operations give, if we denote by

Q . . . the specific gravity of the water * at the temperature which it has when the body is immersed in it, and by

m', r', q', R$'$ . . . the values of m, r, q, R at the weighing in water †,

the following two equations, viz.

$$\text{for weighing in air,}\quad M\left(1-\frac{R^3 q}{\Delta}\right)=m\left(1-\frac{r^3 q}{\delta}\right) \qquad (1)$$

$$\text{for weighing in water,}\ M\left(1-\frac{R'^3 Q}{\Delta}\right)=m'\left(1-\frac{r'^3 q'}{\delta}\right) \qquad (5)$$

whence, by eliminating M, we obtain

$$\Delta=\frac{m\,R'^3 Q\left(1-\frac{r^3 q}{\delta}\right)-m' R^3 q\left(1-\frac{r'^3 q'}{\delta}\right)}{m\left(1-\frac{r^3 q}{\delta}\right)-m'\left(1-\frac{r'^3 q'}{\delta}\right)} \qquad (6)$$

or, if for brevity's sake we put $\frac{r^3 q}{\delta}=a$, $\frac{r'^3 q'}{\delta}=a'$,

$$\Delta=\frac{m\,R'^3 Q\,(1-a)-m' R^3 q\,(1-a')}{m\,(1-a)-m'\,(1-a')} \qquad (7)‡$$

If only the first power of a is taken into consideration, which (with the exception of elastic fluids) can cause no perceptible error, we have the approximate formula

$$\Delta=\frac{m}{m-m'}R'^3 Q-\frac{m'}{m-m'}R^3 q+\frac{m\,m'}{(m-m')^2}(R'^3 Q-R^3 q)\,(a-a') \qquad (8)$$

or, because R and R$'$ are nearly equal to 1, and q so small that it may be neglected, we may put $R'^3 Q - R^3 q = Q$, and obtain

$$\Delta=\frac{m}{m-m'}R'^3 Q-\frac{m'}{m-m'}R^3 q+\frac{m\,m'}{(m-m')^2}Q\,(a-a') \qquad (9)‡$$

29. It remains now to determine the numeric values to be used for these reductions, and to give Tables that make the application of the formulæ more easy.

* The unity adopted for specific gravities being pure water at its maximum of density, Q is of course the density of pure water at the temperature T, divided by its greatest density, or

$$Q=\frac{\text{density of pure water at the temperature T.}}{\text{density of pure water at the temperature of nearly 39° F.}}$$

T is the common temperature of the water in which the body is immersed, and of the body immersed in it.

† The weight of the body immersed in water (m') is evidently different from its weight in air (m), and the temperatures of the water and of the air, at the moment when the body is weighed in water, will generally be different from the temperature of the air for the moment when the body is weighed in air. The values of r, q, R depend on these temperatures, and will consequently generally be different in both cases, so that they ought to be distinguished by a particular notation with accents: r', q' depends on t' (= common temperature of the air and the weights when the body is weighed in water), and R$'$ depends on T (= common temperature of the water and the body immersed in it).

‡ When the atmospheric circumstances are the *same* at the weighing in water as they were at the weighing in air, or, in other words, if $b' = b$, and $t' = t$, it is not necessary to know the specific gravity of the weights em-

M. Bessel supposes that atmospheric air, at the temperature of melting ice, and under the pressure of 29·922 English inches (= 0·76 metre) of mercury, has the specific gravity of

$$\frac{13 \cdot 59606}{10475 \cdot 6} = \frac{1}{770 \cdot 488}$$

where the numerator is the specific gravity of mercury according to Brisson's experiments calculated by Hällström, and the denominator the ratio of the density of air to that of mercury found by MM. Biot and Arago. This gives for q, or for the height of the barometer expressed in English inches and reduced to the density of mercury at the temperature of melting ice, and for t, the temperature of the air expressed in Fahrenheit degrees,

$$q = b \cdot \frac{1}{770 \cdot 488} \cdot \frac{1}{29 \cdot 922} \cdot \frac{1}{1 + (t - 32^\circ)\, 0 \cdot 0020833} = b \cdot \frac{1}{23054 \cdot 39 [1 + (t - 32^\circ)\, 0 \cdot 0020833]} \quad (10)$$

Supposing weights of brass whose specific gravity = 8 (the correction for the actual specific gravity of the brass weights differing from 8 is easily applied, as will immediately be shown), and taking the linear expansion of brass for one degree of Fahrenheit's scale = 0·000010436, we have for this metal

$$r^3 = [1 + (t - 32^\circ)\, 0 \cdot 000010436]^3$$

and consequently,

$$\frac{r^3 q}{\delta} = a = b \cdot \frac{[1 + (t - 32^\circ)\, 0 \cdot 000010436]^3}{23054 \cdot 39 [1 + (t - 32^\circ)\, 0 \cdot 0020833]} \cdot \frac{1}{8}$$

Table I., here following, contains the logarithm of the coefficient of b in this formula, which coefficient we shall denote by α; so that $a = b\,\alpha$; the argument of which is the temperature of the air (the temperature of the weights being supposed equal to that of the air), or t in Fahrenheit degrees. If the body is weighed in water, it is evident that α' must be taken with the argument t' (= temperature of

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ployed for that purpose, nor even of what metal they are. Indeed, a depends on b and t, and a' on b' and t'; consequently if $b' = b$, and $t' = t$, we have also $a' = a$, and the fraction $\frac{m\,R'^3\,Q\,(1 - a) - m'\,R^3\,q\,(1 - a')}{m\,(1 - a) - m'\,(1 - a')}$ has the common factor in the denominator, and numerator $(1 - a)$, which consequently disappears, and reduces it to

$$\Delta = \frac{m\,R'^3\,Q}{m - m'} - \frac{m'\,R^3\,q}{m - m'}$$

The same result is obtained by the equation (9), in which in this case

$$\frac{m\,m'}{(m - m')^2} \cdot Q\,(a - a') = 0$$

so that we obtain as before

$$\Delta = \frac{m\,R'^3\,Q}{m - m'} - \frac{m'\,R^3\,q}{m - m'}$$

If the atmospheric circumstances are nearly the same in both weighings, the precise knowledge of the specific gravity of the weights employed has little influence, and always less in proportion as b' is nearer to b, and t' nearer to t.

the air at that moment), and that b', or the height of the barometer when the body is weighed in water, must be employed. We thus obtain

$$\frac{r'^3 q'}{\delta} = a' = b' \alpha'$$

If the brass, of which the weights are made, has a specific gravity $= \delta$, different from that assumed ($= 8$), α must be first multiplied by 8, and afterwards divided by δ. This comes to the same as applying to the numbers of Table I. the correction c; c being $= \log 8 - \log \delta$, or $c = 0{\cdot}90309 - \log \delta$.

30. We come now to $\frac{R^3 q}{\Delta}$. If the linear expansion of the weighed body for one degree of Fahrenheit's scale be denoted by e, we have $R^3 = [1 + (t - 32^\circ)\, e]^3$. For q we have already the equation (10), in which, by making

$$\beta = \frac{1}{23054{\cdot}39\,.\,[1 + (t - 32^\circ)\, 0{\cdot}0020833]}$$

we have $q = b\,\beta$: β is evidently the specific gravity of atmospheric air at the temperature t, and under the pressure of one English inch of mercury, the mercury being reduced to its density at the temperature of the melting ice. We thus obtain

$$\frac{R^3 q}{\Delta} = b\,\beta\,.\,\frac{[1 + (t - 32^\circ)\, e]^3}{\Delta}.$$

Table II. contains the logarithms of β. Its argument is the temperature of the air in Fahrenheit degrees at the moment of the weighing.

Tables III. IV. V. contain the logarithms of $[1 + (t - 32^\circ)\, e]^3$ for brass, copper, and platina. They suppose

For brass $e = 0{\cdot}000010436$
For copper $e = 0{\cdot}000009541$
For platina $e = 0{\cdot}000005000$ and $0{\cdot}000005050$

Table I.—For Brass weights the specific gravity of which = 8.

t.	log α.		t.	log α.		t.	log α.		t.	log α.		t.	log α.		t.	log α.	
32°	4·73416	89	41°	4·72621	87	50°	4·71841	85	59°	4·71076	85	68°	4·70324	83	77°	4·69585	81
33	4·73327	89	42	4·72534	87	51	4·71756	86	60	4·70991	84	69	4·70241	82	78	4·69504	81
34	4·73238	89	43	4·72447	87	52	4·71670	85	61	4·70907	83	70	4·70159	83	79	4·69423	81
35	4·73149	88	44	4·72360	87	53	4·71585	86	62	4·70824	84	71	4·70076	82	80	4·69342	81
36	4·73061	89	45	4·72273	87	54	4·71499	85	63	4·70740	84	72	4·69994	82	81	4·69261	81
37	4·72972	88	46	4·72186	86	55	4·71414	85	64	4·70656	83	73	4·69912	82	82	4·69180	80
38	4·72884	88	47	4·72100	87	56	4·71329	84	65	4·70573	83	74	4·69830	82	83	4·69100	81
39	4·72796	87	48	4·72013	86	57	4·71245	85	66	4·70490	83	75	4·69748	81	84	4·69019	80
40	4·72709	88	49	4·71927	86	58	4·71160	84	67	4·70407	83	76	4·69667	82	85	4·68939	80
41	4·72621		50	4·71841		59	4·71076		68	4·70324		77	4·69585		86	4·68859	

Log α is taken with the temperature of the air in Fahrenheit degrees at the moment of the weighing. If the brass weights are of any other specific gravity $= \delta$, a correction c must be applied to the numbers of the table: or $c = 0{\cdot}90309 - \log \delta$.

TABLE II.—Containing the Logarithms of β.

t.	log β.		t.	log β.		t.	log β.		t.	log β.		t.	log β.		t.	log β.	
32°	5·63725	91	41°	5·62918	89	50°	5·62126	87	59°	5·61348	86	68°	5·60584	84	77°	5·59833	83
33	5·63634	90	42	5·62829	88	51	5·62039	87	60	5·61262	85	69	5·60500	84	78	5·59750	82
34	5·63544	90	43	5·62741	89	52	5·61952	87	61	5·61177	85	70	5·60416	84	79	5·59668	83
35	5·63454	90	44	5·62652	88	53	5·61865	87	62	5·61092	85	71	5·60332	84	80	5·59585	82
36	5·63364	89	45	5·62564	88	54	5·61778	86	63	5·61007	85	72	5·60248	83	81	5·59503	82
37	5·63275	90	46	5·62476	88	55	5·61692	86	64	5·60922	85	73	5·60165	83	82	5·59421	82
38	5·63185	89	47	5·62388	87	56	5·61606	86	65	5·60837	85	74	5·60082	83	83	5·59339	81
39	5·63096	89	48	5·62301	88	57	5·61520	86	66	5·60752	84	75	5·59999	83	84	5·59258	82
40	5·63007	89	49	5·62213	87	58	5·61434	86	67	5·60668	84	76	5·59916	83	85	5·59176	81
41	5·62918		50	5·62126		59	5·61348		68	5·60584		77	5·59833		86	5·59085	

Log β is taken with the temperature of the air in FAHRENHEIT degrees at the moment of the weighing.

TABLE III.—Containing the Logarithms of $R^3 = [1 + (t - 32°)\,e]^3$ for *Brass*, assuming $e = 0·000010436$.

t.	log R³.	t.	log R³.	t.	log R³.	t.	log R³.	t.	log R³.	Proportional parts.	
32°	0·0000000	43°	0·0001496	54°	0·0002991	65°	0·0004486	76°	0·0005981		
33	0·0000136	44	0·0001632	55	0·0003127	66	0·0004622	77	0·0006117	0°·1	14
34	0·0000272	45	0·0001768	56	0·0003263	67	0·0004758	78	0·0006253	0·2	27
35	0·0000408	46	0·0001904	57	0·0003399	68	0·0004894	79	0·0006389	0·3	41
36	0·0000544	47	0·0002040	58	0·0003535	69	0·0005030	80	0·0006525	0·4	54
37	0·0000680	48	0·0002176	59	0·0003670	70	0·0005166	81	0·0006661	0·5	68
38	0·0000816	49	0·0002311	60	0·0003806	71	0·0005302	82	0·0006796	0·6	82
39	0·0000952	50	0·0002447	61	0·0003942	72	0·0005438	83	0·0006932	0·7	95
40	0·0001088	51	0·0002583	62	0·0004078	73	0·0005673	84	0·0007068	0·8	109
41	0·0001224	52	0·0002719	63	0·0004214	74	0·0005709	85	0·0007204	0·9	122
42	0·0001360	53	0·0002855	64	0·0004350	75	0·0005845	86	0·0007340		

Log R^3 is taken with the temperature of the air in FAHRENHEIT degrees when the body is in the air, and with the temperature of the water when the body is immersed in water.

TABLE IV.—Containing the Logarithms of $R^3 = [1 + (t - 32)\,e]^3$ for *Copper*, assuming $e = 0·000009541$.

t.	log R³.	t.	log R³.	t.	log R³.	t.	log R³.	t.	log R³.	Proportional parts.	
32°	0·0000000	43°	0·0001367	54°	0·0002734	65°	0·0004102	76°	0·0005468		
33	0·0000124	44	0·0001491	55	0·0002859	66	0·0004226	77	0·0005593	0°·1	12
34	0·0000249	45	0·0001616	56	0·0002983	67	0·0004350	78	0·0005717	0·2	25
35	0·0000373	46	0·0001740	57	0·0003107	68	0·0004474	79	0·0005841	0·3	37
36	0·0000497	47	0·0001864	58	0·0003232	69	0·0004599	80	0·0005966	0·4	50
37	0·0000621	48	0·0001989	59	0·0003356	70	0·0004723	81	0·0006090	0·5	62
38	0·0000746	49	0·0002113	60	0·0003480	71	0·0004847	82	0·0006214	0·6	75
39	0·0000870	50	0·0002237	61	0·0003608	72	0·0004971	83	0·0006338	0·7	87
40	0·0000994	51	0·0002361	62	0·0003729	73	0·0005096	84	0·0006462	0·8	99
41	0·0001119	52	0·0002486	63	0·0003853	74	0·0005220	85	0·0006587	0·9	112
42	0·0001243	53	0·0002610	64	0·0003977	75	0·0005344	86	0·0006711		

Log R^3 is taken with the temperature of the air in FAHRENHEIT degrees when the body is in the air, and with the temperature of the water when it is immersed in water.

TABLE V.—Containing the Logarithms of $R^3 = [1 + (t - 32^\circ)\, e]^3$ for *Platina.*

Assuming $e = 0{\cdot}000005000$.

t.	log R³.	t.	log R³.
32°	0·0000000	60°	0·0001824
33	0·0000065	61	0·0001889
34	0·0000130	62	0·0001954
35	0·0000195	63	0·0002019
36	0·0000261	64	0·0002085
37	0·0000326	65	0·0002150
38	0·0000391	66	0·0002215
39	0·0000456	67	0·0002280
40	0·0000521	68	0·0002345
41	0·0000586	69	0·0002410
42	0·0000651	70	0·0002475
43	0·0000717	71	0·0002540
44	0·0000782	72	0·0002606
45	0·0000847	73	0·0002671
46	0·0000912	74	0·0002736
47	0·0000977	75	0·0002801
48	0·0001042	76	0·0002866
49	0·0001107	77	0·0002931
50	0·0001173	78	0·0002996
51	0·0001238	79	0·0003061
52	0·0001303	80	0·0003127
53	0·0001368	81	0·0003192
54	0·0001433	82	0·0003257
55	0·0001498	83	0·0003322
56	0·0001563	84	0·0003387
57	0·0001629	85	0·0003452
58	0·0001693	86	0·0003517
59	0·0001759		

Assuming $e = 0{\cdot}000005050$.

t.	log R³.	t.	log R³.
32°	0·0000000	60°	0·0001842
33	0·0000066	61	0·0001908
34	0·0000132	62	0·0001974
35	0·0000197	63	0·0002040
36	0·0000263	64	0·0002106
37	0·0000329	65	0·0002171
38	0·0000395	66	0·0002237
39	0·0000461	67	0·0002303
40	0·0000526	68	0·0002369
41	0·0000592	69	0·0002435
42	0·0000658	70	0·0002500
43	0·0000724	71	0·0002566
44	0·0000790	72	0·0002632
45	0·0000855	73	0·0002698
46	0·0000921	74	0·0002764
47	0·0000987	75	0·0002829
48	0·0001053	76	0·0002895
49	0·0001119	77	0·0002961
50	0·0001184	78	0·0003027
51	0·0001250	79	0·0003093
52	0·0001316	80	0·0003158
53	0·0001382	81	0·0003224
54	0·0001448	82	0·0003289
55	0·0001513	83	0·0003355
56	0·0001579	84	0·0003421
57	0·0001645	85	0·0003486
58	0·0001711	86	0·0003552
59	0·0001777		

Proportional parts.

0°·1	7
0·2	13
0·3	20
0·4	26
0·5	33
0·6	40
0·7	46
0·8	53
0·9	59

Log R^3 is taken with the temperature of the air in FAHRENHEIT degrees when the body is in the air, and with the temperature of the water when the body is immersed in water.

31. We shall add to these tables a Table for the logarithms of Q, which is used when, by weighing in water, the specific gravity of a body ($= \Delta$) is to be determined. The table M. BESSEL has given is that of HÄLLSTRÖM, the result of his experiments and calculations published in *Vetenskaps Academiens Handlingar* för år 1823, and reprinted in POGGENDORFF'S *Annalen der Physik,* vol. i. p. 168. HÄLLSTRÖM found the density of pure water, between the limits of 0° and +32° of the centigrade thermometer,

$$= 1 + 0{\cdot}000052939\,\tau - 0{\cdot}0000065322\,\tau^2 + 0{\cdot}00000001445\,\tau^3$$

where τ denotes the degrees of the centigrade thermometer. The unity is here the density of pure water at the temperature of melting ice. This formula gives the greatest density of pure water $= 1{\cdot}00010824$, and the temperature at which it occurs $= + 4^\circ{\cdot}108$ centigrade. We have consequently,

$$Q = \frac{1}{1{\cdot}00010824}\,(1 + 0{\cdot}000052939\,\tau - 0{\cdot}0000065322\,\tau^2 + 0{\cdot}0000000144\,\tau^3)$$

and the logarithms of these values of Q are given by M. Bessel in the *Astron. Nach.*, vol. vii. p. 376.

Ten years later M. Hällström resumed the subject, and adding to his experiments those of MM. Muncke and Stampfer, made since his first determination, gave a new Table for the density of pure water in the *Vetenskaps Academiens Handlingar* för år 1833. The whole paper is translated in Poggendorff's *Annalen der Physik*, vol. xxxiv. p. 220 et seq. M. Hällström finds for the *volume* of pure water between the limits of 0° and + 30° of the centigrade thermometer*,

$$1 - 0{\cdot}000057590\,\tau + 0{\cdot}0000075611\,\tau^2 - 0{\cdot}000000035100\,\tau^3$$

where τ denotes degrees of the centigrade thermometer; and where the volume of pure water at the temperature of melting ice is considered as unity. If we call the volume ν, we obtain hence, for Fahrenheit's degrees (denoted by t), the formula

$$\nu = 1 - 0{\cdot}0000319945\,(t-32°) + 0{\cdot}00000233367\,(t-32°)^2 - 0{\cdot}00000000601848\,(t-32°)^3,$$

which gives the minimum of volume (for $t = 39°{\cdot}047$) $= 0{\cdot}99988832$, and consequently the maximum of density $= 1{\cdot}0001117$. Now the density being $= \frac{1}{\nu}$, we obtain

$$Q = \frac{1}{1{\cdot}0001117.(1 - 0{\cdot}0000319945\,(t-32) + 0{\cdot}00000233367\,(t-32)^2 - 0{\cdot}00000000601848\,(t-32)^3)}.$$

Agreeably to this equation the values of Q, whose logarithms are given in the following Table, are calculated; which Table contains also the values of ν as above stated, as well as of D, together with their logarithms: D being the density of pure water at the temperature t (in Fahrenheit degrees), the density at 32° being = 1.

* There are errors of the press, or oversights in calculation, in the original memoir of M. Hällström, repeated in the translation, which I have corrected here. The equation for ν should be the arithmetic mean (Poggendorff, p. 246.) of the four equations which M. Hällström calls I. V. VI. IX. Now we have

(I.) p. 228. $\nu = 1 - 0{\cdot}000049976\,\tau + 0{\cdot}0000062453\,\tau^2 - 0{\cdot}000000007645\,\tau^3$

(V.) p. 238. $\nu = 1 - 0{\cdot}000060835\,\tau + 0{\cdot}0000081037\,\tau^2 - 0{\cdot}000000048282\,\tau^3$

(VI.) p. 239. $\nu = 1 - 0{\cdot}000059269\,\tau + 0{\cdot}0000076816\,\tau^2 - 0{\cdot}000000037159\,\tau^3$

(IX.) p. 244. $\nu = 1 - 0{\cdot}000060280\,\tau + 0{\cdot}0000082138\,\tau^2 - 0{\cdot}000000047313\,\tau^3$

The arithmetic mean of these four equations is

$$\nu = 1 - 0{\cdot}000057590\,\tau + 0{\cdot}0000075611\,\tau^2 - 0{\cdot}000000035100\,\tau^3,$$

as above stated, and not

$$\nu = 1 - 0{\cdot}000057577\,\tau + 0{\cdot}0000075601\,\tau^2 - 0{\cdot}000000035091\,\tau^3,$$

as M. Hällström has it. The Table (p. 247.) likewise which he has calculated upon his formula for ν and D, has several inaccuracies.

TABLE VI.—Containing the logarithms of Q; and also the values of ν and D, and their logarithms.

t.	log Q.		ν.		log ν.		D.		log D.	
32°	9·9999515		1·0000000		0·0000000		1·0000000		0·0000000	
33	9·9999644	+129	0·9999703	−297	9·9999871	−129	1·0000297	+297	0·0000129	+129
34	9·9999753	109	0·9999453	250	9·9999762	109	1·0000547	250	0·0000238	109
35	9·9999841	88	0·9999249	204	9·9999674	88	1·0000751	204	0·0000326	88
36	9·9999911	70	0·9999090	159	9·9999604	70	1·0000910	159	0·0000396	70
37	9·9999960	49	0·9998976	114	9·9999555	49	1·0001024	114	0·0000445	49
38	9·9999990	30	0·9998907	69	9·9999525	30	1·0001093	69	0·0000475	30
39	0·0000000	+10	0·9998883	−24	9·9999515	−10	1·0001117	+24	0·0000485	+10
40	9·9999991	−9	0·9998903	+20	9·9999524	+9	1·0001097	−20	0·0000476	−9
41	9·9999964	27	0·9998967	64	9·9999551	27	1·0001033	64	0·0000449	27
42	9·9999917	47	0·9999074	107	9·9999598	47	1·0000926	107	0·0000402	47
43	9·9999852	65	0·9999224	150	9·9999663	65	1·0000776	150	0·0000337	65
44	9·9999768	84	0·9999417	193	9·9999747	84	1·0000583	193	0·0000253	84
45	9·9999666	102	0·9999652	235	9·9999849	102	1·0000348	235	0·0000151	102
46	9·9999545	121	0·9999930	278	9·9999970	121	1·0000070	278	0·0000030	121
47	9·9999407	138	1·0000249	319	0·0000108	138	0·9999751	319	9·9999892	138
48	9·9999250	157	1·0000609	360	0·0000265	157	0·9999391	360	9·9999735	157
49	9·9999077	173	1·0001009	400	0·0000438	173	0·9998991	400	9·9999562	173
50	9·9998885	192	1·0001451	442	0·0000630	192	0·9998549	442	9·9999370	192
51	9·9998676	209	1·0001933	482	0·0000839	209	0·9998067	482	9·9999161	209
52	9·9998449	227	1·0002454	521	0·0001066	227	0·9997547	520	9·9998934	227
53	9·9998206	243	1·0003015	561	0·0001309	243	0·9996986	561	9·9998691	243
54	9·9997945	261	1·0003615	600	0·0001570	261	0·9996386	600	9·9998430	261
55	9·9997668	277	1·0004254	639	0·0001847	277	0·9995748	638	9·9998153	277
56	9·9997374	294	1·0004931	677	0·0002141	294	0·9995071	677	9·9997859	294
57	9·9997064	310	1·0005646	715	0·0002451	310	0·9994357	714	9·9997549	310
58	9·9996737	327	1·0006399	753	0·0002778	327	0·9993605	752	9·9997222	327
59	9·9996394	343	1·0007189	790	0·0003121	343	0·9992816	789	9·9996879	343
60	9·9996035	359	1·0008016	827	0·0003480	359	0·9991990	826	9·9996520	359
61	9·9995660	375	1·0008880	864	0·0003855	375	0·9991128	862	9·9996145	375
62	9·9995270	390	1·0009780	900	0·0004245	390	0·9990230	898	9·9995755	390
63	9·9994864	406	1·0010715	935	0·0004651	406	0·9989296	934	9·9995349	406
64	9·9994443	421	1·0011686	971	0·0005072	421	0·9988328	968	9·9994928	421
65	9·9994006	437	1·0012693	1007	0·0005509	437	0·9987323	1005	9·9994491	437
66	9·9993554	452	1·0013734	1041	0·0005961	452	0·9986285	1038	9·9994039	452
67	9·9993088	466	1·0014809	1075	0·0006427	466	0·9985213	1072	9·9993573	466
68	9·9992607	481	1·0015918	1109	0·0006908	481	0·9984107	1106	9·9993092	481
69	9·9992112	495	1·0017061	1143	0·0007403	495	0·9982968	1139	9·9992597	495
70	9·9991602	510	1·0018238	1177	0·0007913	510	0·9981795	1173	9·9992087	510
71	9·9991077	525	1·0019447	1209	0·0008438	525	0·9980591	1204	9·9991562	525
72	9·9990539	538	1·0020689	1242	0·0008976	538	0·9979354	1237	9·9991024	538
73	9·9989987	552	1·0021963	1274	0·0009528	552	0·9978085	1269	9·9990472	552
74	9·9989421	566	1·0023269	1306	0·0010094	566	0·9976785	1300	9·9989906	566
75	9·9988841	580	1·0024607	1338	0·0010674	580	0·9975454	1331	9·9989326	580
76	9·9988248	593	1·0025976	1369	0·0011267	593	0·9974091	1363	9·9988733	593
77	9·9987642	606	1·0027375	1399	0·0011873	606	0·9972700	1391	9·9988127	606
78	9·9987023	619	1·0028805	1430	0·0012492	619	0·9971278	1422	9·9987508	619
79	9·9986391	632	1·0030265	1460	0·0013124	632	0·9969826	1452	9·9986876	632
80	9·9985746	645	1·0031755	1490	0·0013769	645	0·9968346	1480	9·9986231	645
81	9·9985088	658	1·0033274	1519	0·0014427	658	0·9966836	1510	9·9985573	658
82	9·9984418	670	1·0034822	1548	0·0015097	670	0·9965299	1537	9·9984903	670
83	9·9983736	682	1·0036398	1576	0·0015779	682	0·9963734	1565	9·9984221	682
84	9·9983042	694	1·0038003	1605	0·0016473	694	0·9962141	1593	9·9983527	694
85	9·9982335	707	1·0039636	1633	0·0017180	707	0·9960521	1620	9·9982820	707
86	9·9981617	−718	1·0041296	+1660	0·0017898	+718	0·9958874	−1648	9·9982102	−718

32. After having given all the elements and formulæ necessary for the reduction of the weighings, we may now state in numbers the uncertainty that remains about the true weight of the lost standard pound; respecting which at this moment we do not know whether it was made of brass or of copper (though the probability inclines

for brass), and of whose specific gravity, even if we assume it of brass, we are equally ignorant.

We will previously state the following results of the weighings given in § 19. in a form adapted for the subsequent calculations.

m.	log *b.*	*t.*	Δ.	$\log \frac{1}{\Delta}$.	*c.*
K = 5760·03389 gr.	1·47197	65°·09	7·994	......	+0·00033
S.p = 5759·99143	1·47308	65·62	21·1874	8·67392	
S.b = 5759·98966	1·47661	64·50	8·228	......	−0·01220
RS = 5759·99795	1·47430	65·73	21·1874	8·67392	
RM = 5760·00887	1·47245	65·91	7·994	......	+0·00033

The column marked with Δ contains the specific gravities of the pounds compared with U. Of these, the specific gravities of K and S.p only have been found by weighing in water*. The specific gravity of S.b is the mean of the specific gravities of two other brass weights made by Mr. Robinson, supposing that his brass was always nearly of the same specific gravity, which is indeed a precarious supposition, but may be adopted until S.b itself be weighed in water. The specific gravities of RS and RM are unknown. I have, until they be determined, supposed RS of the same specific gravity with S.p, and RM of the same specific gravity with K. Indeed K and RM are both made by Mr. Bate, and probably at the same time, so that it seems allowable to suppose that they are of the same metal.

The column *c* contains the correction for the three brass pounds to be applied to the log α found in Table I., on account of their specific gravities differing from 8·0, which is the specific gravity supposed in Table I. This correction is $= 0{\cdot}90309 - \log \delta$ (see § 29.).

33. We may now calculate the reduction to a vacuum for these weighings, on two hypotheses; assuming U,

1°. to have been of brass, with the specific gravity = 8·0;

2°. to have been of copper, with the specific gravity = 8·788.

In the first hypothesis, the logarithm of the quantity $\beta \frac{r^3}{\delta} = \alpha$, may for U be taken immediately out of Table I. Likewise the log of the quantity $\beta \frac{R^3}{\Delta}$ may for the three brass pounds be taken out of Table I., on applying to the log α the correction *c*, stated at the bottom of that Table. For the two platina pounds, $\beta \frac{R^3}{\Delta}$ must be calculated separately, which is also the case with $\beta \frac{r^3}{\delta}$ if we assume U to have been of copper. Log β is taken out of Table II.

* S.p itself has not yet been weighed in water. Its specific gravity is determined by weighing in water another weight of the same platina, which Mr. Robinson made for me, for that purpose.

The formula (3) becomes, after having put for q its value $b\,\beta$,

$$M = m + m\,b\,\beta\,\frac{R^3}{\Delta} - m\,b\,\beta\,\frac{r^3}{\delta},$$

which, it must be remembered, is only an approximate formula, the exact formula being

$$M = m + M\,b\,\beta\,\frac{R^3}{\Delta} - m\,b\,\beta\,\frac{r^3}{\delta}.$$

In general the logarithm of $m\,b\,\beta\,\frac{R^3}{\Delta}$ will be identical with the logarithm of $M\,b\,\beta\,\frac{R^3}{\Delta}$ when we use logarithms with five decimals, which give even more accuracy than the weighings can pretend to: but should log M (M being found by the first equation) differ in the fifth decimal from log m, we must use the value of M obtained by the first equation and put it in the latter, in the term $M\,b\,\beta\,\frac{R^3}{\Delta}$, in order to obtain a result for M as exact as may be found with logarithms of five decimals.

Reduction of the weighings of K.

U supposed of Brass.

$\log m = 3{\cdot}76042$
$\log b = 1{\cdot}47197$
(Table I.) $\log a = 4{\cdot}70566$
$c = 0{\cdot}00033$
$9{\cdot}93838$
$m\,b\,\beta\,\frac{R^3}{\Delta} = 0{\cdot}86772$

$\log m = 3{\cdot}76042$
$\log b = 1{\cdot}47197$
$\log a = 4{\cdot}70566$
$9{\cdot}93805$
$m\,b\,a = 0{\cdot}86706$

$m\,b\,\beta\,\frac{R^3}{\Delta} - m\,b\,a = 0{\cdot}86772 - 0{\cdot}86706 = +0{\cdot}00066$
$m = 5760{\cdot}03389$
$M = 5760{\cdot}03455$

The logarithm of M is 3·76042, the same as that of m, so that it is not necessary to repeat the calculation with log M.

U supposed of Copper.

$m\,b\,\beta\,\frac{R^3}{\Delta}$ as before $= 0{\cdot}86772$

$\log m = 3{\cdot}76042$
$\log b = 1{\cdot}47197$
(Table II.) $\log \beta = 5{\cdot}60829$
(Table IV.) $\log r^3 = 0{\cdot}00041$
$\log \frac{1}{\delta} = 9{\cdot}05611$
$9{\cdot}89720$
$m\,b\,\beta\,\frac{r^3}{\delta} = 0{\cdot}78922$

$m\,b\,\beta\,\frac{R^3}{\Delta} - m\,b\,\beta\,\frac{r^3}{\delta} = 0{\cdot}86772 - 0{\cdot}78922 = +0{\cdot}07850$
$m = 5760{\cdot}03389$
$M = 5760{\cdot}11239$ nearly.

The logarithm of M is 3·76043, being by one unity in the fifth decimal greater than log m. Therefore, by using log M instead of log m, we obtain

$\log M\,b\,\beta\,\frac{R^3}{\Delta} = 9{\cdot}93839$ $\qquad M\,b\,\beta\,\frac{R^3}{\Delta} = 0{\cdot}86774$

$M\,b\,\beta\,\frac{R^3}{\Delta} - m\,b\,\beta\,\frac{r^3}{\delta} = 0{\cdot}86774 - 0{\cdot}78922 = +0{\cdot}07852$
$m = 5760{\cdot}03389$
Correct value of $M = 5760{\cdot}11241$

Reduction of the weighings of S.^{p}.

U supposed of Brass.

$\log m = 3{\cdot}76042$

$\log b = 1{\cdot}47308$

(Tab. II.) $\log \beta = 5{\cdot}60784$

(Tab. V.) $\log R^3 = 0{\cdot}00022$

$\log \frac{1}{\Delta} = 8{\cdot}67392$

$9{\cdot}51548$

$m\,b\,\beta\frac{R^3}{\Delta} = 0{\cdot}32770$

$\log m = 3{\cdot}76042$

$\log b = 1{\cdot}47308$

(Tab. I.) $\log \alpha = 4{\cdot}70522$

$9{\cdot}93872$

$m\,b\,\alpha = 0{\cdot}86840$

$$m\,b\,\beta\frac{R^3}{\Delta} - m\,b\,\alpha = 0{\cdot}32770 - 0{\cdot}86840 = -0{\cdot}54070$$

$m = 5759{\cdot}99143$

$M = 5759{\cdot}45073$ nearly.

The logarithm of M is 3·76038, four unities in the fifth decimal less than log m. We obtain by using, as before, log M instead of log m,

$\log M\,b\,\beta\frac{R^3}{\Delta} = 9{\cdot}51544 \qquad M\,b\,\beta\frac{R^3}{\Delta} = 0{\cdot}32767$

$$M\,b\,\beta\frac{R^3}{\Delta} - m\,b\,\alpha = 0{\cdot}32767 - 0{\cdot}86840 = -0{\cdot}54073$$

$m = 5759{\cdot}99143$

Correct value of $M = 5759{\cdot}45070$

U supposed of Copper.

$m\,b\,\beta\frac{R^3}{\Delta}$ as before $= 0{\cdot}32770$

$\log m = 3{\cdot}76042$

$\log b = 1{\cdot}47308$

$\log \beta = 5{\cdot}60784$

(Table IV.) $\log r^3 = 0{\cdot}00042$

$\log \frac{1}{\delta} = 9{\cdot}05611$

$9{\cdot}89787$

$m\,b\,\beta\frac{r^3}{\delta} = 0{\cdot}79044$

$$m\,b\,\beta\frac{R^3}{\Delta} - m\,b\,\beta\frac{r^3}{\delta} = 0{\cdot}32770 - 0{\cdot}79044 = -0{\cdot}46274$$

$m = 5759{\cdot}99143$

$M = 5759{\cdot}52869$ nearly.

The logarithm of M is 3·76039, three unities in the fifth decimal less than log m. We obtain by using, as before, log M instead of log m,

$\log M\,b\,\beta\frac{R^3}{\Delta} = 9{\cdot}51545 \qquad M\,b\,\beta\frac{R^3}{\Delta} = 0{\cdot}32768$

$$M\,b\,\beta\frac{R^3}{\Delta} - m\,b\,\beta\frac{r^3}{\delta} = 0{\cdot}32768 - 0{\cdot}79044 = -0{\cdot}46276$$

$m = 5759{\cdot}99143$

Correct value of $M = 5759{\cdot}52867$

Reduction of the weighings of S.^{b}.

U supposed of Brass.

$\log m = 3{\cdot}76042$

$\log b = 1{\cdot}47661$

(Table I.) $\log \alpha = 4{\cdot}70615$

$9{\cdot}94318$

$c = 0{\cdot}01220$

$9{\cdot}93098$

$m\,b\,\beta\frac{R^3}{\Delta} = 0{\cdot}85306$

$\log m = 3{\cdot}76042$

$\log b = 1{\cdot}47661$

$\log \alpha = 4{\cdot}70615$

$9{\cdot}94318$

$m\,b\,\alpha = 0{\cdot}87736$

$$m\,b\,\beta\frac{R^3}{\Delta} - m\,b\,\alpha = 0{\cdot}85306 - 0{\cdot}87736 = -0{\cdot}02430$$

$m = 5759{\cdot}98966$

$M = 5759{\cdot}96536$

The logarithm of M is 3·76042, the same as that of m, so that it is not necessary to repeat the calculation with log M.

U supposed of Copper.

$m\,b\,\beta\frac{R^3}{\Delta}$ as before $= 0{\cdot}85306$

$\log m = 3{\cdot}76042$

$\log b = 1{\cdot}47661$

(Table II.) $\log \beta = 5{\cdot}60880$

(Table IV.) $\log r^3 = 0{\cdot}00040$

$\log \frac{1}{\delta} = 9{\cdot}05611$

$9{\cdot}90234$

$m\,b\,\beta\frac{r^3}{\delta} = 0{\cdot}79862$

$$m b\beta\frac{R^3}{\Delta} - m\beta\frac{r^3}{\delta} = 0{\cdot}85306 - 0{\cdot}79862 = +0{\cdot}05444$$

$m = 5759{\cdot}98966$

$M = 5760{\cdot}04410$

The logarithm of M is 3·76042, the same as that of m, so that it is not necessary to repeat the calculation with log M.

Reduction of the weighings of RS.

U supposed of Brass.

$\log m = 3{\cdot}76042$
$\log b = 1{\cdot}47430$
(Tab. II.) $\log \beta = 5{\cdot}60775$
(Tab. V.) $\log R^3 = 0{\cdot}00022$
$\log \frac{1}{\Delta} = 8{\cdot}67392$
$9{\cdot}51661$

$\log m = 3{\cdot}76042$
$\log b = 1{\cdot}47430$
(Tab. I.) $\log a = 4{\cdot}70512$
$9{\cdot}93984$
$m\,b\,a = 0{\cdot}87064$

$$m\,b\,\beta \frac{R^3}{\Delta} = 0{\cdot}32856$$

$$m\,b\,\beta \frac{R^3}{\Delta} - m\,b\,a = 0{\cdot}32856 - 0{\cdot}87064 = -\,0{\cdot}54208$$

$m = 5759{\cdot}99795$
$M = 5759{\cdot}45587$ nearly.

The logarithm of M is 3·76038, four unities in the fifth decimal less than log m. We obtain by putting, as before, log M in the place of log m,

$$\log M\,b\,\beta \frac{R^3}{\Delta} = 9{\cdot}51657 \qquad M\,b\,\beta \frac{R^3}{\Delta} = 0{\cdot}32852$$

$$M b \beta \frac{R^3}{\Delta} - m b \beta \frac{r^3}{\delta} = 0{\cdot}32852 - 0{\cdot}87064 = -\,0{\cdot}54212$$

$m = 5759{\cdot}99795$
Correct value of $M = 5759{\cdot}45583$

U supposed of Copper.

$m\,b\,\beta \frac{R^3}{\Delta}$ as before $= 0{\cdot}32856$

$\log m = 3{\cdot}76042$
$\log b = 1{\cdot}47430$
(Table II.) $\log \beta = 5{\cdot}60775$
(Table IV.) $\log r^3 = 0{\cdot}00042$
$\log \frac{1}{\delta} = 9{\cdot}05611$
$9{\cdot}89900$

$$m\,b\,\beta \frac{r^3}{\delta} = 0{\cdot}79251$$

$$m b \beta \frac{R^3}{\Delta} - m\,b\,\beta \frac{r^3}{\delta} = 0{\cdot}32856 - 0{\cdot}79251 = -\,0{\cdot}46395$$

$m = 5759{\cdot}99795$
$M = 5759{\cdot}53400$ nearly.

The logarithm of M is 3·76039, three unities in the fifth decimal less than log m. We obtain by putting, as before, log M in the place of log m,

$$\log M\,b\,\beta \frac{R^3}{\Delta} = 9{\cdot}51658 \qquad M\,b\,\beta \frac{R^3}{\Delta} = 0{\cdot}32853$$

$$M b \beta \frac{R^3}{\Delta} - m\,b\,\beta \frac{r^3}{\delta} = 0{\cdot}32853 - 0{\cdot}79251 = -\,0{\cdot}46398$$

$m = 5759{\cdot}99795$
Correct value of $M = 5759{\cdot}53397$

Reduction of the weighings of RM.

U supposed of Brass.

$\log m = 3{\cdot}76042$
$\log b = 1{\cdot}47245$
(Tab. I.) $\log a = 4{\cdot}70497$
$c = 0{\cdot}00033$
$9{\cdot}93817$

$\log m = 3{\cdot}76042$
$\log b = 1{\cdot}47245$
$\log a = 4{\cdot}70497$
$9{\cdot}93784$
$m\,b\,a = 0{\cdot}86664$

$$m\,b\,\beta \frac{R^3}{\Delta} = 0{\cdot}86730$$

$$m\,b\,\beta \frac{R^3}{\Delta} - m b a = 0{\cdot}86730 - 0{\cdot}86664 = +\,0{\cdot}00066$$

$m = 5760{\cdot}00887$
$M = 5760{\cdot}00953$

The logarithm of M is 3·76042, the same as that of m, so that it is not necessary to repeat the calculation with log M.

U supposed of Copper.

$m\,b\,\beta \frac{R^3}{\Delta}$ as before $= 0{\cdot}86730$

$\log m = 3{\cdot}76042$
$\log b = 1{\cdot}47245$
(Table II.) $\log \beta = 5{\cdot}60760$
(Table IV.) $\log r^3 = 0{\cdot}00042$
$\log \frac{1}{\delta} = 9{\cdot}05611$
$9{\cdot}89700$

$$m\,b\,\beta \frac{r^3}{\delta} = 0{\cdot}78886$$

$$m\,b\,\beta \frac{R^3}{\Delta} - m\,b\,\beta \frac{r^3}{\delta} = 0{\cdot}86730 - 0{\cdot}78886 = +\,0{\cdot}07844$$

$m = 5760{\cdot}00887$
$M = 5760{\cdot}08731$ nearly.

The logarithm of M is 3·76043, by one unity in the fifth decimal greater than log m. We obtain by using, as before, log M instead of log m,

$$\log M\,b\,\beta \frac{R^3}{\Delta} = 9{\cdot}93818 \qquad M\,b\,\beta \frac{R^3}{\Delta} = 0{\cdot}86732$$

$$M b \beta \frac{R^3}{\Delta} - m\,b\,\beta \frac{r^3}{\delta} = 0{\cdot}86732 - 0{\cdot}78886 = +\,0{\cdot}07846$$

$m = 5760{\cdot}00887$
Correct value of $M = 5760{\cdot}08733$

34. We may now put, in one view, the results thus obtained, using only four decimals, which is enough for the accuracy, of which the operation of weighing is capable. I have only used five decimals in the reductions, in order to have the fourth decimal not affected by the calculation.

The reductions in § 33. give for the absolute weight of the five several pounds, compared with U, the following values, expressed in grains troy: the first when we suppose U of brass with the specific gravity = 8·0, the second when we suppose U of copper with the specific gravity = 8·788.

	U of Brass.	U of Copper.
Absolute weight of K	= 5760·0346	= 5760·1124
Absolute weight of S.^{p}	= 5759·4507	= 5759·5287
Absolute weight of S.^{b}	= 5759·9654	= 5760·0441
Absolute weight of RS	= 5759·4558	= 5759·5340
Absolute weight of RM	= 5760·0095	= 5760·0873

If now the lost imperial standard troy pound should be restored by these five pounds, it must be made,

if U was of Brass,		if U was of Copper,	
0·0346 gr.	lighter than K	0·1124 gr.	lighter than K
0·5493	heavier than S.^{p}	0·4713	heavier than S.^{p}
0·0346	heavier than S.^{b}	0·0441	lighter than S.^{b}
0·5442	heavier than RS	0·4660	heavier than RS
0·0095	lighter than RM	0·0873	lighter than M.

The uncertainty of course that remains about the absolute weight of the lost standard is, by comparison with

My brass pound, made by BATE, . . .	denoted by K	= 0·0778 gr. troy.
My platina pound, ...	denoted by S.^{p}	= 0·0780
My brass pound, made by ROBINSON, .	denoted by S.^{b}	= 0·0787
The Royal Society's platina pound, . .	denoted by RS	= 0·0782
The Royal Mint brass pound, ...	denoted by RM	= 0·0778

or nearly 0·08 grain by all of them.

35. Nor is this uncertainty brought within much smaller limits if we adhere to the most probable hypothesis, and suppose U of brass. The specific gravity of brass being a compound metal, varies very much according to its composition. I have known brass to vary from 7·9 to nearly 8·5 specific gravity*. In the same manner as before we may find the absolute weight of S.^{p}, if we assume seven different specific gravities for U from 7·9 to 8·5, proceeding by 0·1.

* I have recently met with a piece of cast brass (intended for a pendulum bar) the specific gravity of which is *less* than 7·4.—F. BAILY.

We thus obtain for the absolute weight of $S.^p$, if U had its specific gravity,

= 7·9 .... 5759·4397 grains troy.
= 8·0 .... 5759·4507 ——
= 8·1 .... 5759·4614 ——
= 8·2 .... 5759·4719 ——
= 8·3 .... 5759·4821 ——
= 8·4 .... 5759·4921 ——
= 8·5 .... 5759·5018 ——

There remains consequently (if even we suppose U of brass) an uncertainty about its absolute weight = 5759·5018 − 5759·4397 = 0·0621 gr., or about 0·06 gr.

In fact, though we have five different pounds in excellent preservation, and compared with the lost standard with the greatest care and the best instruments, and though the number of these comparisons surpasses 600, there remains an uncertainty of 0·08 gr., or at least 0·06 gr., as to its real weight; and this solely on account of its specific gravity and expansion not being known. It is to be hoped that no pound will in future ever be declared a legal standard, unless these elements (the knowledge of which is indispensable even for a single comparison with a good balance) are previously determined with the greatest possible precision. A standard pound is intended for the purpose of obtaining from it accurate copies; and it therefore involves a contradiction if those elements are not well ascertained.

N.B. The formula, for the "Barometer reduction" in page 467, should have the sign − prefixed to it.

XXIII. *On the Brain of the Negro, compared with that of the European and the Orang-Outang. By Dr.* Frederick Tiedemann, *Professor of Anatomy and Physiology in the University of Heidelberg, and Foreign Member of the Royal Society.*

Received and Read June 9, 1836.

I TAKE the liberty of presenting to the Royal Society a paper on a subject which appears to me to be of great importance in the natural history, anatomy, and physiology of Man; interesting also in a political and legislative point of view. Celebrated naturalists, Camper*, Soemmerring†, and Cuvier‡, look upon the Negroes as a race inferior to the European in organization and intellectual powers, having much resemblance with the Monkey. Naturalists of less authority§ have exaggerated this opinion. Were it proved to be correct, the Negro would occupy a different situa-

* Ueber den natürlichen Unterschied der Gesichtszüge im Menschen. Berlin, 1792. 4to.

† Ueber die körperliche Verschiedenheit des Mohren vom Europäer. Mainz, 1784. 8vo. Soemmerring says at the end of his paper (p. 32), "From all that has been said, it does not appear unfair to conclude that in general the African Negroes resemble the genus Simia more than the Europeans.

‡ Le Règne Animal, tom. i. p. 95. Paris, 1817. "La race négre est confinée au midi de l'Atlas; son crâne comprimé, et son nez écrasé, son museau saillant et ses grosses lèvres, la rapprochent manifestement des singes; les peuplades qui la composent sont toujours restées barbares."

§ An Account of the regular Gradation in Man, and in different Animals and Vegetables. By Charles White. London, 1799. 4to. He says in the conclusions deducible from the facts and observations stated in the second part of this essay, p. 83:

"Taking the European man as a standard of comparison on the one hand, and the tribe of Simiæ on the other, and comparing the classes of mankind with the standards and with each other, they may be so arranged as to form a pretty regular gradation in respect to the differences in the bodily structure and economy, the European standing at the head, as being furthest removed from the brute creation.

"That the African, more especially in those particulars in which he differs from the European, approaches to the Ape.

"That the characteristics which distinguish the African from the European are the same, differing only in degree, as those which distinguish the Ape from the European."

Histoire Naturelle du Genre Humain, par F. F. Virey, tom. iii. p. 436. Paris, 1824. "De l'orang-outang il faudroit remonter au Hottentot, puis aux négres, plus intelligents, et enfin à l'homme blanc. Les singes semblent être aussi la racine du genre humain."

Lectures on Physiology, Zoology, and the Natural History of Man, by W. Lawrence. London, 1819. Mr. Lawrence, after he has given the characters of the Ethiopian variety, as observed in the genuine Negro tribes, says, p. 363, "In all the particulars just enumerated, the Negro structure approximates unequivocally to that of the Monkey. It not only differs from the Caucasian model, but is distinguished from it in two respects; the intellectual characters are reduced, the animal features enlarged and exaggerated. This inferiority of organization is attended with corresponding inferiority of faculties; which may be proved, not so much by the unfortunate beings who are degraded by slavery, as by every fact in the past history and the present condition of Africa."

tion in society from that which has so lately been given him by the noble British Government.

I propose in this treatise to examine more minutely the most important part of this doctrine, namely, the structure of the brain, the noblest part of the human body, in reference to its functions. A comparison between the brain of the Negro and that of the European and the Orang-Outang, hitherto muchn eglected, appeared to me most worthy of attention. I shall first of all try to answer the following two questions.

1st, Is there any important and essential difference between the structure of the brain of the Negro and that of the European? and

2ndly, Has the brain of the Negro more resemblance to that of the Orang-Outang than the brain of the European?

Should our researches induce us to answer these questions in the affirmative, we should then have reason to consider the opinion given above as true, and founded in nature. Should we be able to prove the falsity of this opinion, we should then be allowed to consider it as a mere literary fancy.

Comparison alone will enable us to answer these two questions. In order to do which we must first of all consider the size, weight, and dimensions of the objects to be compared. I have taken the materials for such a comparison from my researches on the brain and skull of Man and lower animals, for which purpose I have consulted the most celebrated anatomical museums, both on the Continent and in Great Britain.

We begin our researches with the comparison of the size of the brain of the European and that of the Negro, by answering the following question: Has the Negro the same quantity of brain as the European? We must first of all determine the weight and dimensions of the brain of the European, then that of the Negro, and compare them together.

Observations on the Weight of the Brain of Europeans.

The opinions of anatomists on the size and the weight of the human brain in general, and that of the European in particular, as to its absolute and relative weight and bulk compared with that of the body, are very uncertain. The old opinion of ARISTOTLE*, PLINY†, GALEN, and others, for many centuries regarded as correct, namely, that the human brain is absolutely and relatively larger than that of any other animal, is erroneous, and not founded on anatomical researches.

The brain of the Elephant‡ and Whale§ is absolutely much larger than the human

* Hist. Animal., lib. i. cap. 13. "Pro magnitudine sua homo habet maximum cerebrum."

† Hist. Animal., lib. ii. cap. 49. "Homo habet cerebrum portione maximum."

‡ The brain of an African Elephant seventeen years old, examined by PERRAULT (Descr. Anatom. d'un Eléphant, Mém. de l'Académie des Sciences de Paris, tom. iii. par. 3, p. 135), weighed 9 lbs.; was 8 inches long, 6 inches broad. The brain of an Asiatic Elephant weighed, according to ALLEN MOULINS (An Anatomical Account of an Elephant, p. 37. London, 1682. 4to,), 10 lbs.

According to my honoured friend Sir ASTLEY COOPER, the brain of an Elephant dissected by him weighed 8 lbs. 1 oz. 2 grs. (avoirdupois).

§ The brain of a Whale 75 feet long (*Balæna mysticetus*) weighed, according to RUDOLPHI, (Handbuch der brain. Although the human brain is considerably larger than that of any other animal, except the Elephant and Whale, even than the brain of animals much larger than Man, such as the Horse, the Zebra, Stag, Camel, Lion, Tiger, Bear, &c.; nevertheless, relatively to the size of his body, he has not the largest brain. Pozzi* has shown that many small birds (for instance, the Sparrow,) have, in comparison to the size of their body, a larger brain than Man. Daubenton, Haller†, Blumenbach, and Cuvier found the brain of some of the smaller Apes, of the Rodentia, and singing-birds, relatively to the size of the body, larger than in Man. We must seek for the cause of his superiority therefore, not merely in the greater bulk of his brain in comparison to that of his body, but regard must also be had to the size of his brain with respect to the bulk and thickness of the cerebral nerves, and likewise to the degree of perfection in its structure. Soemmerring‡ was the first to show that the human brain, in comparison to the size and thickness of the nerves, is larger than that of any other animal, even the Elephant and Whale, both of which have an absolutely larger brain than Man. Blumenbach's, Ebel's, Cuvier's, Treviranus's, and my own researches §, have sufficiently corroborated this. It is also satisfactorily shown that the organization of the human brain is far superior to that of any other animal, not even excepting those Apes which have the closest resemblance to Man.

Most anatomists, Vesal, Realdus Columbus, Bauhin, Highmore, Collins, and others, as well as those who have paid particular attention to the anatomy of the brain, Willis, Ridley, Vieussens, Tarin, Vicq-d'Azyr, have taken no notice of the weight of the human brain, resting content with what Aristotle has said upon that subject. What other celebrated anatomists, Piccolhomini, Schneider, Bartholin, Pozzi, Arlet, Haller, Meckel, Soemmerring, Portal, Cuvier, Joseph and Charles Wenzel, and Mascagni, have said on the weight of the brain is very unsatisfactory. They made use of different weights, without mentioning them; they neither take any notice of the size and weight, nor of the age and sex of the bodies, the brains of which they examined; and, lastly, they weighed far too few to draw any general conclusion. The note proves sufficiently the truth of this statement ||.

Physiologie, Band ii., Abth. 2, Seite 11,) 5 lbs. $10\frac{1}{4}$ oz., and measured $8'' \, 7\frac{1}{2}'''$ in length; that of a Narwhal (*Monodon monoceros*), 17 to 18 feet long, only 2 lbs. 3 oz., and was $6'' \, 3'''$ long.

* Observatio Anatomica de Cerebro, an sit in homine proportione majus, quam in aliis animalibus (Commentar. Bononiens., tom. ii. p. 1.). "Felis, canis, gallus, et pleraque animantium cerebrum habent portione minus, quam homo; qui id tamen universe affirmant de omnibus, videant in passere, ne fallantur."

† De Partium Corporis Humani præcipuarum Fabrica et Functionibus, tom. viii. p. 6. "Simiæ quædam minores, ut mures et animalia minora, videntur cerebrum habere potius ad corpus universum majus."

‡ De Basi Encephali, p. 17. "Homo ratione habita nervorum omnia hucusque animalia nota magnitudine cerebri superat."

§ Icones Cerebri Simiarum et quorumdam Mammalium rariorum. Heidelbergæ, 1821. fol.

|| Piccolhomini mentions first the weight of the brain (Anatomicæ Prælectiones, lib. v. lect. 2. Romæ, 1586. fol.). "Cerebrum humanum, quatuor aut quinque libras æquans pondere, maximum est."

In order to determine the weight of the human brain, I have weighed a number of brains, male and female, of different ages. In most cases I have also noticed the height of the body, according to the old measure of the Parisian Academy, as well as the weight and state of the body. I divided the brain from the spinal marrow, where the corpora pyramidalia, after their crossing, begin, and ascend upon the medulla oblongata. I separated the nerves at their entrance through the foramina of the skull. The serous or tunica arachnoidea and pia mater were then carefully removed. I made use of the apothecary or troy weight *.

Joh. Riolan fil. (Anthropogr., lib. iv. p. 385. Parisiis, 1626.). "Cum in statera humanum cerebrum librarem deprehendi trium librarum mercatoris pondus æquasse, quæ quatuor libris medicis respondent."

J. Loesel (Scrutinium Renum. Regiomonti, 1642. 4to.). "Cerebrum exemptum hominis strangulati, et ad lancem examinatum exæquabat libras quatuor medicas et totidem uncias."

C. V. Schneider (De Catarrhis, lib. iii. p. 592. Wittenbergæ, 1660. 4to.). "Ego etiam autor fui, ut cerebrum juvenis hominis, qui capite plexus erat, diligenti libraretur examini. Corpus erat sanissimum, validissimum; illud cerebrum pendebat tres libras medicas et octo uncias."

M. Sennert (De Cerebro. Wittenbergæ, 1662. 4to.). "The brain of a man weighed four pounds."

Th. Bartholin (Anatome, p. 468. Lugd. Batav., 1686.). "Cerebri humani magnitudo insignis est, nempe ad librarum quatuor vel quinque pondus."

Pozzi. "The brain of a young man weighed 3 lbs. $8\frac{1}{2}$ oz., and his body 112 lbs. 6 oz."

According to Arlet, (Mém. de Montpellier, p. 47. 1746,) the weight of the brain is equal to four pounds.

T. F. Meckel found the weight of the human brain of an adult 3 lbs. $\frac{1}{2}$ oz. $\frac{1}{2}$ drachm.

Soemmerring (De Corporis Humani Fabrica, tom. iv. p. 38. Trajecti ad Mœnum, 1798,) says, "Cerebrum et cerebellum, resecta medulla spinali statim pone nervum lingualem medium pondo sunt librarum duarum ad tres libras, sunt enim alia cerebra pondere librarum duarum, et unciarum quinque cum dimidia, alia librarum trium, et unciarum trium cum tribus quartis."

Portal (Anatomie Médicale, tom. iv. p. 30. Paris.). "Le cerveau, le cervelet et la moëlle allongée bien lavés et leurs vaisseaux vides de sang, pèsent dans l'adulte trois livres."

Josephus et Carolus Wenzel (De Penitiori Structura Cerebri Hominis et Brutorum, p. 267. Tubingæ, 1812, fol.). "Pondus encephali humani, quale id de quinto vitæ anno ad summam usque hominis senectutem plerumque invenitur, pondus viginti quatuor millium granorum non superat (= 4℔ 2℥). Totius cerebri pondus inter viginti et viginti duo millia (3℔ 5℥ 5ʒ 20 gr. et 3℔ 9℥ 6ʒ 40 gr.)."

Mascagni (Prodromo della Grande Anatomia, p. 78. Firenze, 1819. fol.). "Nell'uomo il cervello da libbre quattro ariva a libbre quattro e mezzo e anche piu."

Alex. Monro Hamilton (The Anatomy of the Brain. Edinburgh, 1831. 8vo.) has undertaken some valuable investigations on the weight of the human brain. He found, nearly averaging, the adult male encephalus, in the Scot's head, 3 lbs. 8 oz. troy: about one brain of seven is found about 4 lbs. troy; the female encephalus is heavier, 3 lbs. 4 oz. One of a hundred female brains is found of 4 lbs.

* 1 pound = 12 ounces; 1 ounce = 8 drachms; 1 drachm = 60 grains.

I. Male Bodies.

	Age.	Height of the body.	State of the body.	Weight of the body.	Weight of the brain.	The weight of the brain compared with that of the body.
		′ ″ ‴		lbs. oz. dr. gr.	lbs. oz. dr. gr.	
1.	New-born child	0 18 6	Lean	6 2 6 50	1 2 3 30	as 1 : 5·15
2.	The same	0 20 2	Well nourished	7 3 2 8	1 1 1 10	1 : 6·63
3.	Boy two years old	....	The same	28 5 0 0	1 11 3 0	1 : 14·58
4.	Boy two years and a half old	....	The same	......	2 2 1 0	
5.	Boy three years old	....	The same	41 2 0 0	2 3 2 28	1 : 18·008
6.	Boy six years old	....	......	......	2 11 5 0	
7.	Boy fifteen years old	4 6 0	Well nourished	100 7 0 3	4 6 0 0	1 : 24·75
8.	Young man seventeen years old	....	......	......	4 2 1 0	
9.	Man twenty-two years old	....	......	......	4 2 1 0	
10.	Man twenty-eight years old	....	Thin	108 11 4 44	3 11 2 0	1 : 27·67
11.	Man thirty years old	....	Very thin	......	3 11 7 0	
12.	Man thirty-one years old	....	Lean	100 10 1 22	3 10 5 0	1 : 25·95
13.	The same	....	......	136 7 0 0	3 10 1 0	1 : 35·53
14.	Man thirty-two years old	5 1 6	Muscular	162 9 0 0	4 7 5 0	1 : 35·11
15.	The same	5 2 0	The same	169 8 2 0	4 7 0 0	1 : 37·02
16.	Man thirty-two years old	....	Well nourished	148 0 0 0	3 2 0 20	1 : 46·68
17.	Man thirty-three years old	....	Lean	......	3 9 4 0	
18.	Man thirty-five years old	....	......	......	3 2 7 0	
19.	Man thirty-six years old	....	Muscular	166 2 7 19	4 0 5 30	1 : 8·97
20.	Man thirty-eight years old	....	The same	160 7 6 50	4 4 0 6	1 : 36·54
21.	Man thirty-eight years old	....	The same	162 0 4 57	4 6 6 0	1 : 35·51
22.	Man thirty-nine years old	5 1 6	Very muscular	173 1 4 0	4 4 3 0	1 : 39·66
23.	Man forty years old	5 0 6	Not muscular	133 0 6 0	4 4 2 0	1 : 30·56
24.	Man about forty years old	....	......	......	3 11 2 0	
25.	The same	....	Very muscular	185 9 0 0	4 1 2 40	1 : 45·18
26.	Man forty-six years old	5 1 6	Lean	107 3 4 0	3 10 1 0	1 : 27·91
27.	Man forty-six to fifty years old	....	Well nourished	164 7 6 20	3 7 4 0	1 : 45·42
28.	Man fifty years old	5 0 2	Thin	132 8 4 35	3 10 7 5	1 : 33·96
29.	Man about fifty years old	5 0 2½	Muscular	181 8 2 0	4 1 0 10	1 : 44·47
30.	Man about fifty years old	....	Well nourished	141 1 0 0	3 8 1 40	1 : 37·76
31.	Man fifty-five years old	5 5 6	Very muscular	182 1 7 37	4 5 1 30	1 : 41·09
32.	Man sixty-one years old	....	Lean	......	3 7 4 0	
33.	Man sixty-four years old	5 2 0	Well nourished	157 0 0 0	3 11 4 0	1 : 39·66
34.	Man sixty-four years old	5 4 6	Lean	141 0 5 0	4 0 4 0	1 : 34·89
35.	Man eighty-two years old	....	The same	124 10 7 30	3 2 3 0	1 : 39·06

II. Female Bodies.

	Age.	Height of the body.	State of the body.	Weight of the body.	Weight of the brain.	The weight of the brain compared with that of the body.
36.	New-born child	0 17 3	Lean	4 11 0 0	0 9 3 0	as 1 : 6·29
37.	The same	0 18 5	Well nourished	7 2 0 0	1 0 4 40	1 : 6·83
38.	Girl three years old	....	......	......	2 2 3 0	
39.	Girl five years old	....	......	......	2 4 1 50	
40.	Girl eight years and eight months old	3 4 6	Well nourished	49 0 2 51	3 5 5 0	1 : 14·13
41.	Girl thirteen years old	....	The same	63 2 6 23	3 6 2 30	1 : 17·93
42.	Girl sixteen years old	....	The same	......	3 10 2 0	
43.	Girl about twenty years old	....	The same	......	3 8 6 0	
44.	Woman twenty-five years old	....	The same	......	2 11 0 0	
45.	Woman about thirty-four years old	4 10 6	Well nourished	133 7 0 0	3 7 2 0	1 : 37·06
46.	Woman about thirty years old	....	Lean	......	3 11 0 0	
47.	Woman about thirty years old	....	Well nourished	123 4 2 25	3 7 0 0	1 : 34·42
48.	Woman thirty-eight years old	4 11 6	Muscular	153 6 5 35	3 5 0 20	1 : 44·89
49.	Woman forty-eight years old	....	......	......	2 8 5 50	
50.	Woman about fifty years old	....	Well nourished	134 6 2 57	3 4 0 40	1 : 40·27
51.	Woman sixty years old	....	The same	135 11 0 0	3 5 5 0	1 : 39·18
52.	Woman about eighty years old	....	......	......	2 9 1 0	

From these observations we may draw the following conclusions:

1. The weight of the brain of an adult male European varies between 3 lbs. 2 oz. and 4 lbs. 6 oz. The brain of men who have distinguished themselves by their great talents is often very large. The brain of the celebrated CUVIER weighed 3 lbs. 11 oz. 4 dr. 40 grs. avoirdupois, or 4 lbs. 11 oz. 4 dr. 30 grs. troy weight. The brain of the celebrated surgeon DUPUYTREN weighed 4 lbs. 10 oz. troy weight. The brain of men endowed with but feeble intellectual powers is, on the contrary, often very small, particularly in congenital idiotismus. The brain of an idiot fifty years old weighed only 1 lb. 8 oz. 4 dr., and that of another forty years of age weighed but 1 lb. 11 oz. 4 dr.

2. The female brain is lighter than that of the male. It varies between 2 lbs. 8 oz. and 3 lbs. 11 oz. I never found a female brain that weighed 4 lbs. The brain of a girl, an idiot, sixteen years old, weighed only 1 lb. 6 oz. 1 dr. The female brain weighs on an average from four to eight ounces less than that of the male; and this difference is already perceptible in a new-born child.

3. The brain arrives, on an average, to its full size towards the seventh or eighth year. SOEMMERRING* says erroneously that the brain does not increase after the third year. GALL and SPURZHEIM, on the other hand, are of opinion that the brain continues to grow till the fourteenth year. The brothers WENZEL † have shown that the brain arrives at its full growth about the seventh year. This is confirmed by HAMILTON's researches.

4. DESMOULINS ‡ is of opinion that the brain decreases in size in old people. From this circumstance he explains the diminution of the functions of the nervous system and intellectual powers. The truth of this assertion has not as yet been determined. The brothers WENZEL § and HAMILTON || deny it.

It is remarkable, that the brain of a man eighty-two years old was very small, and weighed but 3 lbs. 2 oz. 3 dr.; and the brain of a woman about eighty years old weighed but 2 lbs. 9 oz. 1 dr. (see preceding Tables). I have generally found the cavity of the skull smaller in old men than in middle-aged persons. It appears to me therefore probable, that the brain really decreases in old age, only more remarkably in some persons than in others.

5. There is undoubtedly a very close connexion between the absolute size of the brain and the intellectual powers and functions of the mind. This is evident from

* Tabula Baseos Encephali Pueri trium annorum, p. 13. Francoforti ad M., 1799. fol.

† De Penitiori Cerebri Structura, p. 266.

‡ De l'Etat du Système Nerveux sous ses Rapports de Volume et de Masse dans le Marasme non sénile; Journal de Physique, par DUCROTAY DE BLAINVILLE, Juin 1820, t. lxx. p. 442. Suite des Recherches, ibid., Fevr. 1821, t. xcii., p. 165.

§ l. c. p. 267. "In senectute pondus cerebri non notabiliter minui videtur; et cum hoc ipsum etiam in magnitudinis ratione locum habeat, diminutio efficaciæ cerebri in senectute cum aliqua æqua notabili massæ voluminisque ipsius adtenuatione haud necessario conjuncta esse videtur."

|| l. c. p. 5. "It is extremely doubtful whether the cranial contents usually diminish in old age. The vulgar opinion that they do, rests on no adequate evidence, and my induction would rather prove the negative."

the remarkable smallness of the brain in cases of congenital idiotismus, few much exceeding in weight the brain of a new-born child. Gall, Spurzheim, Haslam, Esquirol, and others have already observed this, which is also confirmed by my own researches. The brain of very talented men is remarkable, on the other hand, for its size.

Anatomists* differ very much as to the weight of the brain, compared with the bulk and weight of the body; for the weight of the body varies so much, that it is impossible to determine accurately the proportion between it and the brain. The weight of an adult varies from 100 to 800 lbs., and changes both in health and when under the influence of disease, depending in great measure on nutrition. The weight of the brain, although different in adults, remains generally the same, unaltered by the increase or diminution of the body. Thin persons have therefore, relative to the size of the body, a larger brain than stout people.

From my researches I have drawn the following conclusions.

1. The brain of a new-born child is relatively to the size of the body the largest; the proportion is 1 : 6.

2. The human brain is smaller in comparison to the body the nearer man approaches to his full growth. In the second year the proportion of the brain to the body is as 1 : 14; in the third, 1 : 18; in the fifteenth, 1 : 24. In a full-grown man between the age of twenty and seventy years, as 1 : 35 to 45. In lean persons the proportion is often as 1 : 22 to 27; in stout persons as 1 : 50 to 100, and more.

3. Although Aristotle has remarked that the female brain is absolutely smaller than the male, it is nevertheless not relatively smaller compared with the body; for the female body is in general lighter than that of the male. The female brain is for the most part even larger than the male, compared with the size of the body.

The different degree of susceptibility and sensibility of the nervous system seems to depend on the relative size of the brain as compared with that of the body. Children and young people are more susceptible, irritable, and sensible than adults, and have a relatively larger brain. Thin persons are more susceptible than stout. In diseases which affect the nourishment of the body the susceptibility increases as the patients grow thinner. The susceptibility and sensibility decreases, on the other hand, with persons recovering from a long illness, gradually as they regain their strength. The degree of sensibility in animals is also in proportion to the size of the brain. Mammalia and birds have a larger brain and are more susceptible than amphibious animals and fishes. I propose to go into this subject on another occasion, as it would at present take me too far from my immediate object.

* Haller (De Partium Corporis Humani præcipuarum Fabrica et Functionibus, t. viii. p. 16.) says, "Ego in puero sex annorum, pondus librarum 2, drachmarum 28 cum scr. reperi, quæ ratio, cum ægre 50 libr. ejusmodi puer æquat, fuerit fere $\frac{1}{23}$. In Pozziano exemplo fuit proxime $\frac{1}{30}$. In Arleti altero fuit omnino $\frac{1}{25}$, in altero $\frac{1}{35}$, et si cerebri pondus rotundo numero 4 libras expresseris, hominis vero adulti libris 140, circa $\frac{1}{35}$ calculus fere subsistet."

Cuvier (Anat. Comparée, tom. ii.) says that the relative weight of brain is = 1 : 22 — 35.

Weight of the Brain of a Negro.

Camper's assertion, that the facial angle is smaller in the Negro than in the European, has led many anatomists to the supposition that the Negro has a less quantity of brain than the European. There are but few observations on the weight of the brain of the Negro, and these do not agree with this supposition.

The brain of a Negro boy fourteen years old, weighed, according to Soemmerring*, 2 lbs. 10 oz. 3 dr. avoirdupois, or 3 lbs. 6 oz. 6 dr. troy weight. The brain of another handsome tall Negro, about twenty years of age, weighed 2 lbs. 13 oz. 4 dr. avoirdupois, or 3 lbs. 9 oz. 4 dr. troy weight†. My honoured friend Sir Astley Cooper gave me the following account on the weight of the brain of a Negro.

"The weight of the brain of a large Negro was 3 lbs. 1 oz., or 49 oz. The general weight of the brain of man is from 37 to 52 oz."

The Negro whose brain I have examined and drawn, was a short thin man, twenty-five years of age, hardly five feet high. He died at Liège of the smallpox, and was dissected there by my son-in-law, Professor Fohmann, and my son Henry. The brain and the spinal marrow were sent to me preserved in alcohol. The brain, separated from the spinal marrow below the medulla oblongata, weighed 2 lbs. 3 oz. 2 dr.

We can also prove, by measuring the cavity of the skull of Negroes and men of the Caucasian, Mongolian, American, and Malayan races, that the brain of the Negro is as large as that of the European and other nations.

Researches on the Size and Capacity of the Cavity of Skulls.

In order to determine the capacity of the cavity of the skull I pursued the following method.

1. I weighed the skull with or without the lower jawbone.
2. I then filled the cavity of the skull with dry millet-seed, through the foramen occipitale magnum. The skull was then weighed again, carefully filled.
3. I then deducted the weight of the empty skull from that of the filled one, and so obtained the capacity of the cavum cranii.

The following Tables record the results obtained from a number of Negro, European, Mongolian, American, and Malayan skulls, weighed in this manner.

I only weighed those skulls of whose authenticity I was convinced, and I have mentioned the collections and museums where they are to be found, so as to enable any one to convince himself of the truth and correctness of my researches.

* Ueber die Verschiedenheit des Negers vom Europäer, S. 19.

† Soemmerring himself allows that the brain is not always so large in Europeans.

I. Æthiopian Race.

A. Male Skulls.

	Names of the different Tribes.	Anatomical Collection.	Capacity of the cavum cranii. oz.	dr.	gr.
1.	Eboes, or Ibos, Negro of Congo. Died at Sierra Leone	Anatomical Museum of Dr. Knox at Edinburgh	54	2	33
2.	Old Caffre	Camper's Anatomical Museum at Groningen	43	7	0
3.	Negro	St. Thomas's Hospital, London	42	6	30
4.	Eboes. Negro	Collection of Dr. Knox	42	2	37
5.	Negro	Guy's Hospital, London	42	0	23
6.	Negro	St. Thomas's Hospital	41	6	37
7.	Native of Madagascar	Phrenological Society, Edinburgh	40	5	30
8.	Negro	Soemmerring's Anatomical Museum	40	5	6
9.	Negro of Loango	Camper's Collection	40	0	20
10.	Negro	St. Thomas's Hospital	39	6	33
11.	Hottentot	Collection of Mr. South, London	39	6	21
12.	Negro of Guinea	Camper's Anatomical Museum	39	2	0
13.	Bosjes man	Mr. South's Collection	38	7	5
14.	Negro of North America	Anatomical Museum at Groningen	38	4	0
15.	Caffre	Mr. South's Collection	37	5	59
16.	Negro eleven years old	Groningen Anatomical Collection	37	5	0
17.	Negro	St. Bartholomew's Hospital, London	37	3	35
18.	Negro of Surinam	Anatomical Museum at Heidelberg	37	2	30
19.	Negro	St. Bartholomew's Hospital	37	2	11
20.	Negro	The same	37	1	22
21.	Negro	St. Thomas's Hospital	37	0	1
22.	Ashantee Negro	Hunterian Museum, London	36	5	32
23.	Bosjesman	Phrenological Society, Edinburgh	36	3	56
24.	Negro of Angola	Camper's Collection	36	4	20
25.	Negro	Guy's Hospital, London	36	1	32
26.	Negro	Heidelberg Anatomical Museum	35	7	0
27.	Negro	The same	35	6	40
28.	Native of Mozambique	Camper's Museum	35	4	0
29.	Negro of Guinea..	The same	35	3	0
30.	Negro	The same	35	3	0
31.	Young Negro	The same	35	0	0
32.	Negro of Mozambique	Mr. South's Collection	34	6	0
33.	Negro of Curaçao	Groningen Anatomical Museum	34	4	0
34.	Negro of Cheribon	The same	33	3	0
35.	Bosjesman	Phrenological Society, Edinburgh	32	6	48
36.	Young Negro	Camper's Collection	32	0	0
37.	Young Negro of Madagascar	The same	32	0	0
38.	Negro	St. Bartholomew's Hospital	31	5	16

B. Female Skulls.

			oz.	dr.	gr.
39.	Negress	Camper's Museum	31	4	0
40.	Old Hottentot woman	The same	31	0	0
41.	Negress	Guy's Hospital, London	24	7	39

By these Tables it is clear that the cavum cranii of Negro women is smaller than that of the men; consequently they have an absolutely smaller brain, like the European women.

II. Caucasian Race.

A. Male Skulls of European Nations.

	Names of the different Nations.	Anatomical Collection.	Capacity of the cavum cranii. oz.	dr.	gr.
1.	Cossack, from the Don	Soemmerring's Museum	57	3	56
2.	Native of Piedmont	Mr. South's Collection, London	49	2	4
3.	Turk	Camper's Museum at Groningen	49	0	0
4.	Irishman	Presented to me by Mr. Hart of Dublin	48	4	30
5.	Scotchman	Presented to me by Dr. Handyside of Edinburgh	47	7	52
6.	Swiss	Camper's Museum	47	0	0
7.	Swede	Soemmerring's Museum	46	7	45
8.	Dutchman	Camper's Museum	46	0	0
9.	German	Heidelberg Museum	45	6	45
10.	Englishman	Camper's Museum	45	5	11
11.	Frenchman, formerly a grenadier	Heidelberg Museum	45	0	0
12.	Dutchman, very talented	Camper's Museum	45	0	0
13.	German	Heidelberg Museum	44	7	34
14.	Russian	Camper's Collection	44	5	0
15.	Englishman	Bartholomew's Hospital	44	4	15
16.	Frenchman	Soemmerring's Museum	44	2	42
17.	German	Heidelberg Museum	44	0	33
18.	Hungarian	The same	44	0	4
19.	German	The same	43	7	6
20.	The same	The same	43	6	58
21.	The same	The same	43	3	11
22.	Turk	Soemmerring's Museum	43	1	46
23.	German, native of Prussia	Camper's Museum	43	2	0
24.	Native of Finnland	Heidelberg Museum	42	7	4
25.	Cossack, from the Don	Soemmerring's Museum	42	6	45
26.	Dutchman	Camper's Museum	42	5	0
27.	Norwegian	The same	42	3	40
28.	German	Heidelberg Museum	42	1	15
29.	Dutchman	Camper's Museum	42	6	0
30.	The same	The same	42	5	0
31.	Swede	The same	42	0	0
32.	German, native of Holstein	The same	41	7	0
33.	Pole	Camper's Museum	41	7	0
34.	German	Heidelberg Museum	41	6	6
35.	Frenchman, native of Paris	Camper's Museum	41	6	0
36.	Frenchman	Soemmerring's Museum	41	3	52
37.	Native of Estland	The same	41	2	4
38.	Frenchman, from Angoumois	The same	41	1	20
39.	Swede	Camper's Museum	41	0	0
40.	The same	The same	40	7	0
41.	Dutchman	The same	40	7	0
42.	Jew, from Friesland	The same	40	6	0
43.	Dutchman	The same	40	4	0
44.	Hungarian	The same	40	4	0
45.	Swiss	The same	40	3	40
46.	German	Heidelberg Mueum	40	1	31
47.	Portuguese	Soemmerring's Museum	40	0	36
48.	Native of Finland	Heidelberg Museum	39	7	0
49.	Pole	Camper's Museum	39	7	0
50.	Native of Estland	Soemmerring's Museum	39	5	51
51.	Dutchman, from Scheveningen	Camper's Museum	39	4	0
52.	German	Heidelberg Museum	39	3	33
53.	Dutchman, talented man	Camper's Museum	39	2	0
54.	Turk	Soemmerring's Museum	39	3	5
55.	German	Heidelberg Museum	39	1	55

	Names of the different Nations.	Anatomical Collection.	Capacity of the cavum cranii. oz.	dr.	gr.
56.	Russian	Camper's Museum	39	1	0
57.	German	Heidelberg Museum	39	0	50
58.	Englishman	Camper's Museum	38	6	0
59.	Dutchman	The same	38	6	0
60.	Servier, from Cassava	Soemmerring's Museum	38	5	3
61.	Frenchman, from Lyons	Camper's Museum	38	4	0
62.	Russian, formerly a grenadier	Heidelberg Museum	38	4	0
63.	Portuguese	Camper's Museum	38	3	0
64.	Spaniard	The same	38	0	0
65.	Dane	The same	37	3	0
66.	Russian, from Petersburg	Soemmerring's Museum	37	2	2
67.	Dutchman	Camper's Museum	37	0	0
68.	Russian	Heidelberg Museum	36	7	50
69.	Swede	Camper's Museum	36	6	0
70.	German	Heidelberg Museum	36	2	28
71.	Neapolitan	Camper's Museum	36	0	0
72.	Cossack	The same	35	2	0
73.	Dutchman	The same	35	0	0
74.	Jew, from Amsterdam	The same	34	6	0
75.	Frenchman, from Paris	The same	34	2	0
76.	Dutchman	The same	32	6	0
77.	Prussian	The same	32	6	0
	B. Male Skulls of Asiatic Nations.				
1.	Russian, from Orenburg	Soemmerring's Museum	41	5	6
2.	Werschandier, on the other side of the Taurian Mountains	The same	40	5	8
3.	Armenian	Royal College of Surgeons, Dublin	40	2	0
4.	Arabian	Camper's Museum	40	2	0
5.	Native of Ceylon	Presented to the Edinburgh Phrenological Society by Mr. Lyon	38	7	20
6.	Hindoo	Presented to the Edinburgh Phrenological Society by Dr. Murray Patterson	38	5	54
7.	Circassian	Camper's Museum	38	3	0
8.	Native of Ceylon	Presented to the Edinburgh Phrenological Society by Mr. Lyon	38	2	0
9.	Georgian	Camper's Museum	38	0	0
10.	Hindoo	The same	37	0	0
11.	Parsee	Presented to me by Dr. Mackintosh of Edinburgh	36	7	30
12.	Hindoo	Soemmerring's Museum	36	3	0
13.	Hindoo	Edinburgh Phrenological Society	36	2	32
14.	Birmanese soldier	Mr. South's Collection	36	0	54
15.	Georgian	Camper's Museum	36	0	0
16.	Native of Ceylon, from the Vedah tribe	Presented to the Phrenological Society at Edinburgh by Mr. Lyon	35	0	16
17.	Georgian	Camper's Museum	34	6	0
18.	Parsee	Edinburgh Phrenological Society	33	1	49
19.	Hindoo	The same	32	2	24
20.	Circassian	Camper's Museum	33	0	0
21.	Hindoo	The same	32	6	0
22.	The same	Edinburgh Phrenological Society	32	1	4
23.	Native of Ceylon	Camper's Museum	31	0	0
24.	Hindoo Brahmin	Presented to the Edinburgh Phrenological Society by Dr. George Mackenzie	27	6	30

It is very remarkable that the capacity of the skull of the Hindoos is very small; which has been likewise observed by PATTERSON *.

* Monthly Review, December 1823, p. 286.

C. Male Skulls of African Nations.

	Names of the different Nations.	Anatomical Collection.	Capacity of the cavum cranii. oz.	dr.	gr.
1.	Skull of an Egyptian mummy	Hunterian Museum at London	44	6	11
2.	Native of Egypt	Soemmerring's Museum	44	5	38
3.	Mameluke	The same	37	2	58
4.	Native of Egypt	Camper's Museum	35	5	0

D. Female Skulls.

	Names of the different Nations.	Anatomical Collection.	oz.	dr.	gr.
1.	Irish	Presented to me by Mr. Hart	39	5	30
2.	German woman	Heidelberg Museum	38	6	12
3.	Dutch woman	Camper's Museum	38	0	0
4.	German woman	Heidelberg Museum	37	7	35
5.	The same	The same	37	3	30
6.	Dutch woman	Camper's Museum	35	0	0
7.	The same	The same	34	0	0
8.	German woman	Heidelberg Museum	33	4	30
9.	The same	The same	33	4	11
10.	The same	The same	33	1	21
11.	Dutch woman	Camper's Museum	31	4	0
12.	The same	The same	30	4	0

By this Table is proved what I have stated on the weight of the brain of women.

III. Mongolian Race.

A. Male Skulls.

	Name of the Tribes.	Anatomical Collection.	Capacity of the cavum cranii. oz.	dr.	gr.
1.	Esquimaux	Phrenological Society, Edinburgh	49	1	22
2.	Lapponian	Anatomical Collection at Heidelberg	48	2	25
3.	Scull of Tyloolick, an Esquimaux attached to one of Capt. Parry's Expeditions	Guy's Hospital	44	6	0
4.	Native of Hudson's Bay	St. Thomas's Hospital	42	2	40
5.	Kalmuc	Anatomical Museum at Groningen	42	0	0
6.	Native of Kamtschatka	The same	42	0	0
7.	Kalmuck	Soemmerring's Anatomical Museum	41	2	44
8.	Baskier	Anatomical Museum at Groningen	40	7	0
9.	Chinese	The same	40	2	2
10.	Native of Greenland	Camper's Anatomical Museum at Groningen	39	4	0
11.	Chinese	Hunterian Museum	38	3	3
12.	The same	Phrenological Society, Edinburgh	37	1	57
13.	Native of Labrador	Camper's Museum	37	0	0
14.	Chinese	St. Thomas's Hospital	35	7	16
15.	Native of Greenland	Dublin Surgical College	33	6	43
16.	Esquimaux	Groningen Anatomical Museum	33	6	0
17.	Native of Labrador	St. Thomas's Hospital	30	1	16
18.	Native of Nootka Sound	Christ College, Oxford	25	0	18

B. Female Skulls.

	Name of the Tribes.	Anatomical Collection.	oz.	dr.	gr.
19.	Esquimaux	Phrenological Society, Edinburgh	35	2	23
20.	Native of Greenland	Dublin College of Surgeons	31	0	43

IV. American Race.

	Name of the Tribes.	Anatomical Collection.	Capacity of the cavum cranii. oz.	dr.	gr.
	A. Male Skulls.				
1.	Botocudo	Anatomical Collection at Frankfurt	59	0	0
2.	North American Indian of the Algonquin tribe	Presented to Guy's Hospital by B. Harrison, Esq.	48	4	0
3.	Flat-headed Indian from the banks of the Columbia river	Guy's Hospital	45	6	0
4.	Peruvian from Arica	Presented to the Phrenological Society by T. Steel	45	5	35
5.	Red Indian from Newfoundland	Collection for Natural History of the University at Edinburgh	42	3	35
6.	Skull of Tooe-too, a chief of the Cherokees	College of Surgeons at Edinburgh	41	3	47
7.	Auraucanian Indian	Edinburgh Phrenological Society	41	3	42
8.	Flat-headed Indian from the Columbia river	Presented to Guy's Hospital by Mr. Harrison	40	7	30
9.	Skull of a Shenockor Chinock Indian*, from the burying place at the mouth of the river Walamet	Presented to the Collection of Natural History of the University of Edinburgh by Dr. M. Gairdner	40	5	22
10.	Native of the country on the north-western shore of lake Superior, a Chipaway chief	College of Surgeons, Edinburgh	40	2	57
	Chinock Indian	Edinburgh University	40	2	10
11.	Carib of St. Vincent. (This man was a good botanist.)	St. Thomas's Hospital	39	4	30
12.	Peruvian of Huacha, an Indian town near Lima	Phrenological Society, Edinburgh	38	2	32
13.	Inka of Illo in Peru	St. Thomas's Hospital	37	6	45
14.	Flat-headed Indian	Guy's Hospital	37	2	0
15.	Indian from Chili	Phrenological Society, Edinburgh	37	1	38
16.	Peruvian from the neighbourhood of Arica	The same	36	7	39
17.	Flat-headed Indian	Guy's Hospital	36	7	10
18.	Native of Antigua	Presented to Bartholomew's Hospital by Dr. Conquest	35	3	57
19.	Peruvian	Phrenological Society, Edinburgh	35	1	47
20.	Mexican of Oaxaca	Soemmerring's Museum	34	3	0
21.	Botocudo boy	Anatomical Collection at Frankfurt	32	4	55
22.	Native of South America	Hunterian Museum	31	0	44
23. 24.	Toway Indian, taken 400 miles above the mouth of the Missouri river	Anatomical Collection at Frankfurt	26	1	44
	B. Female Skulls.				
25.	Shenock Indian	Presented to the University of Edinburgh by Dr. M. Gairdner	40	5	22
26.	Red Indian	Edinburgh University	38	7	41
27.	Botocudo	Anatomical Museum at Frankfurt	31	6	41

* The nation of Indians who inhabit the north side of the mouth of the Columbia river flatten the head immediately after birth by a compress fastened on the forehead.

V. Malayan Race.

	Name of the Tribes.	Anatomical Collection.	Capacity of the cavum cranii. oz.	dr.	gr.
	A. Male Skulls.				
1.	Native of the island Huaheine	Presented to Guy's Hospital by Mr. Samuel Stutchbury, the naturalist of the Pacific Pearl Company	49	1	45
2.	Native of Java, from Cheribon	Soemmerring's Museum	48	5	42
3.	Native of the island Huaheine	Presented to Guy's Hospital by Mr. S. Stutchbury	47	0	41
4.	Native of Java	Heidelberg Museum	46	3	0
5.	Native of New Holland	Presented to St. Bartholomew's Hospital by M. Langstaff	45	7	46
6.	Native of Java	Heidelberg Museum	44	7	0
7.	Native of the island Tahiti (named Otaheite by Capt. Cook)	Presented to Guy's Hospital by Mr. S. Stutchbury	43	6	39
8.	Native of the island Raiatea (called Ullietea by Capt. Cook)	The same	43	6	29
9.	Native of the island Amana	The same	43	4	31
10.	Native of the island Madura	Heidelberg Museum	43	2	0
11.	Native of New Zealand	St. Thomas's Hospital	42	4	50
12.	Native of New South Wales	Presented to St. Bartholomew's Hospital by Mr. Hodgson	41	7	31
13.	The same	The same	41	4	56
14.	Native of New Zealand	Guy's Hospital	41	2	53
15.	Native of Celebes, from Macassar	Heidelberg Museum	41	2	0
16.	Native of Java	Camper's Museum	41	0	40
17.	Native of the island Eimeo	Presented to Guy's Hospital by the Pacific Pearl Company	40	4	46
18.	Native of Java	Camper's Museum	40	1	0
19.	Native of the island Rurutu (Oheitersa of Capt. Cook)	Presented to Guy's Hospital by the Pacific Pearl Company	39	7	16
20.	Native of the Sandwich Islands	Edinburgh Phrenological Society	38	7	29
21.	Native of Amboyna	Soemmerring's Museum	38	6	58
22.	Native of the Philippine Islands	Edinburgh Phrenological Society	38	4	26
23.	Native of the island Madura	Camper's Museum	38	3	0
24.	Native of Sumatra	The same	38	0	0
25.	Native of New Holland	Edinburgh Phrenological Society	37	4	23
26.	Native of Macassar	Soemmerring's Museum	36	6	52
27.	Native of New Holland	Hunterian Museum	36	6	40
28.	Native of Java	Camper's Museum	36	4	40
29.	Native of Nubaschenka	Anatomical Museum at Frankfurt	36	3	0
30.	Native of Madura	Camper's Museum	36	2	0
31.	Skull of a Lascar	The same	36	1	40
32.	Native of Van Diemen's Land	Christ College, Oxford	35	1	21
33.	Native of New South Wales	The same	35	0	30
34.	New Zealander	Dublin Royal College of Surgeons	34	4	43
35.	Native of Botany Bay	St. Thomas's Hospital	34	3	32
36.	Native of the island Nukahiva	From the Expedition of Capt. Kotzebue, in Soemmerring's Museum	33	6	28
37.	Native of Java	Soemmerring's Museum	31	4	52
38.	Skull of a Dayak, a native of Borneo	The same	30	5	0
	B. Female Skulls.				
39.	Native of the island Huaheine	Presented to Guy's Hospital by the Pacific Pearl Company	37	5	0
40.	Native of the island Rajatea	The same	34	0	2
41.	Native of New Holland	Edinburgh Phrenological Society	32	4	31
42.	Native of Java	Heidelberg Anatomical Museum	22	5	0
43.	A Lascar woman	Edinburgh College of Surgeons	19	2	49

It is evident from the comparison of the capacity of the cavum cranii of the Negro with that of the European, Mongolian, American, and Malayan, that the cavity of the skull of the Negro, in general, is not smaller than that of the European and other human races. The result of HAMILTON's * researches is the same. I hope this will convince others that the opinion of many naturalists, such as CAMPER, SOEMMERRING, CUVIER, LAWRENCE, and VIREY, that the Negro has a smaller skull and brain than the European, is ill founded, and entirely refuted by my researches. The mistaken notion of these naturalists arose from the application of CAMPER's facial line and facial angle on a few skulls of negroes living on the coasts †; who, according to credible travellers, are the lowest and most demoralized of all the Negro tribes; the miserable remains of an enslaved people, bodily and spiritually lowered and degraded by slavery and ill treatment. I look upon CAMPER's facial line and facial angle as very unsatisfactory in determining the capacity of the skull, the size of the brain, and the degree of intellectual powers.

The general characters and marks of the Ethiopian race, as given by naturalists, cannot be received as universal, nor are they strictly applicable to the greater number of the Negro tribes in the high lands of the interior of Africa. These characters are, the skin black; the hair black and woolly; the skull compressed laterally; the forehead low, depressed, slanting, and narrow; the cavity of the cranium smaller, and reduced both in its circumference and in its transverse diameters; the eyes prominent; great development of the face, and projection towards its lower part; the cheekbones prominent; the jaws narrow; the superior incisor teeth oblique; the chin retracted; the nose broad, thick, and flat; the lips, particularly the upper one, thick and projecting. This is the countenance of the Mozambique and Guinea Negroes, but it is not the feature of the natives of the high lands of Africa. The truth of this assertion is fully attested by the latest African travellers. WINTERBOTTOM ‡ says of the tribes of Timmanu and Soosoo Negroes, in the mountainous districts of

* l. c. p. 5. He says: "The common doctrine, that the African brain, and particularly that of the Negro, is greatly smaller than the European, is false. By a comparison of the capacity of two Caffre skulls, male and female, and of thirteen Negro crania, (six male, five female, and two of doubtful sex,) the encephalus of the African was found not inferior to the average size of the European."

† CAMPER has determined the facial angle of the Negro from the skull of a slave from Angola, and has given a plate of it, plate i. fig. 1.

LAWRENCE has given (plate viii.) a drawing of a Negro skull, of which he says (p. 363): "In such a skull as that represented in the eighth plate, which indeed has been particularly selected, because it is strongly characterized, no person, however little conversant with natural history or physiology, could fail to recognise a decided approach to the animal form. This inferiority of organization is attended with corresponding inferiority of faculties, which may be proved, not so much by the unfortunate beings who are degraded by slavery, as by every fact in the past history and present condition of Africa."

I must confess that I cannot call the figure a good one; and amongst some hundreds of Negro skulls I saw not one of so bad a form.

‡ Account of the Native Africans in the Neighbourhood of Sierra Leone, vol. i. pp. 184, 198. London, 1803.

Sierra Leone: "The sloping contracted forehead, small eyes, depressed nose, thick lips, and projecting jaws, with which the African is usually caricatured, are by no means constant traits; on the contrary, every gradation of countenance may be met with, from the disgusting picture too commonly drawn of them, to the finest set of European features."

TUCKEY * says the same of the Jalaffs or Oualafs; MEREDITH † of the Fantees; ADAMS ‡ and BOWDICH § of the Ashantees, the Dahomeys, and the Negroes of the banks of the river Chamba: they have good features, neither broad nor flat noses, nor thick lips. The Mandingos on the banks of the rivers Gambia, Joliba, the higher Senegal, and Niger, as also the Foulahs or Fullahs, and Fellatahs in the interior of Africa, in Bondu, Timboctoo, Housan, Sudan, Bornoo and Kaschna, vary but little, according to MUNGO PARK ||, DENHAM, and CLAPPERTON ¶, excepting in colour, from the Europeans. Their skin is not so black as that of the Negroes on the coast of Guinea, and their black hair is not so woolly, but long, soft, and silky. They have neither broad flat noses, thick lips, nor prominent cheekbones; sloping contracted forehead, nor a skull compressed from both sides, which most naturalists consider as the universal characteristics of a Negro. Most of them have well-formed skulls, long faces, handsome, even Roman or aquiline noses, thin lips, and agreeable features. The Negresses of these nations are as finely formed as the men, and are, with the exception of their colour, as handsome as European women.

SOMERVILLE, BARROW**, LICHTENSTEIN††, and BURCHELL‡‡, have shown that the Caffres and Bachapins, or Betchuanas, have the same form of skull, and the same high forehead and prominent nose as Europeans.

Credible travellers and accurate observers confirm also what the celebrated BLUMENBACH §§ said thirty years back, "that the exterior of Negroes gradually approaches to that of other races, and acquires by degrees their fine features."

Spinal Cord and Medulla Oblongata of the Negro.

The form and structure of the well preserved spinal cord of the Negro HONORÉ accord in every way with that of the European. It is divided anteriorly and pos-

* Narrative of the Expedition to explore the River Zaire. London, 1818.

† An Account of the Gold Coast of Africa. London, 1812.

‡ Remarks on the Country extending from the Cape Palma to the River Congo. London, 1823.

§ Mission from Cape Coast Castle to Ashantee, with a Statistical Account of that Kingdom, and Geographical Notices of other parts of the Interior of Africa. London, 1819.

|| Travels in the Interior Districts of Africa. London, 1799.

¶ Travels in Interior Parts of Africa. London, 1820. CLAPPERTON's Second Travels in the Interior of Africa, from Badagry to Soccatu. London, 1829.

** Southern Africa, vol. i. ch. 3.

†† Travels, ch. 18.

‡‡ Travels in the Interior Districts of South Africa. London, 1820.

§§ Beyträge zur Naturgeschichte. Th. 1. S. 73. Goettingen, 1806. Decas Craniorum, ii. p. 13. "Specimina teriorly, by the longitudinal fissure, into two equal parts. On both sides are situated the lateral longitudinal fissures which divide the spinal marrow into a posterior and anterior part. From these cords the posterior and anterior roots of the spinal nerves take their origin.

On the fore part of the medulla oblongata are the two corpora pyramidalia, and on the outside of these the corpora olivaria. On the posterior surface are the corpora restiformia, entering the cerebellum. Between them is the fourth ventricle (ventriculus quartus, calamus scriptorius,) and the striæ medullares. The following are the dimensions according to the old measurement of the Parisian Academy.

The medulla oblongata and the spinal cord from the pons Varolii to the end of the latter, measure 14 inches 11 lines. The breadth of the medulla oblongata below the pons Varolii is 10 inches $\frac{1}{2}$ line; the breadth of the medulla oblongata at the part where the corpora pyramidalia cross each other, $5\frac{2}{3}$ lines. The breadth of the spinal cord on the vertebræ cervicales superiores was $5\frac{1}{3}$ lines; on the vertebræ cervicales inferiores $6\frac{2}{3}$ lines; on the middle pectoral vertebræ, $4\frac{1}{2}$ lines; on the inferior pectoral vertebræ $5\frac{1}{3}$ lines.

In order to compare the spinal cord of the Negro with that of the European, I measured it in a man 5 feet 8 inches high, and in a woman 5 feet high.

	Man.		Woman.	
	in.	lines.	in.	lines.
Length of the medulla oblongata and of the spinal cord . .	17	3	14	10
Breadth of the medulla oblongata below the pons Varolii .		11		$10\frac{1}{3}$
Breadth of the medulla oblongata at the part where the corpora pyramidalia cross each other		$6\frac{1}{4}$		$5\frac{1}{2}$
Breadth of the spinal cord on the vertebræ cervicales superiores		$5\frac{1}{2}$		5
Breadth of the spinal cord on the vertebræ cervicales inferiores		$6\frac{3}{4}$		$6\frac{1}{3}$
Breadth of the spinal cord on the middle pectoral vertebræ .		5		$4\frac{1}{3}$
Breadth of the spinal cord on the inferior pectoral vertebræ		$5\frac{2}{3}$		$5\frac{1}{4}$

Hence there is no remarkable difference between the medulla oblongata and spinal cord of the Negro and that of the European, except the difference arising from the different size of the body.

Cerebellum of the Negro.

The cerebellum of the Negro, in regard to its outward form, fissures and lobes, is exactly similar to that of the European, as is shown in Plate XXXII. *k. k.* and

tria craniorum præstantissimam exhibent seriem, maxime si cum tribus istis Æthiopum craniis comparantur, quæ priore Decade exhibui, utpote quæ luculenter demonstrant, genuinos Æthiopes, si craniorum formam spectes, non minus certe, imo vero magis passim inter se ipsos ab invicem differre, quam nonnulli eorum a multorum Europæorum capitis forma differunt."

Plate XXXIII. *b*: its internal structure and the arrangement of the substantia corticalis and medullaris are also the same. In order to show that there is no difference in size and dimensions, with the exception arising from the different size of the body, I give two Tables of the dimensions of the Negro and European cerebellum.

I measured the brains of the following individuals of the Ethiopian race:

1st. Of a Negro whose brain I received from Liège. Plates XXX. XXXI. XXXII. XXXIII.

2nd. Of a Negro preserved in the Museum of Comparative Anatomy, in the Jardin du Roi, at Paris.

3rd. Of a Bosjes woman, dissected by Cuvier*, which I found in the Museum of Comparative Anatomy. Plate XXXIV. This woman was only 4 feet 6 inches 7 lines in height, according to the measure of the Parisian Academy. I give also the dimensions of the brain of a Negro mentioned by the brothers Wenzel.

Table I.

Dimensions of the Cerebellum and Nodus Encephali of Negroes.

	Negro Honoré.	Brain of a Negro preserved in the Museum of Comparative Anatomy at Paris.	Brain of the Bosjes woman called Hottentot Venus.	Brain of a Negro measured by the brothers Wenzel.
	in. lin.	in. lin.	in. lin.	in. lin.
Breadth or transverse diameter of the cerebellum	3 4	3 3½	3 2½	4 1
Longitudinal diameter of the cerebellum in the middle	2 4	2 5	2 4½	2 6
Breadth of the nodus encephali between the nervi quinti	1 1½			
Longitudinal diameter of the nodus encephali in the middle	0 10⅓			

Table II.

Dimensions of the Cerebellum and Nodus Encephali of Europeans.

	Males.						Females.		
	in. lin.	in. lin.	in. lin.	in. lin.	in. lin.	in. lin.	in. lin.	in. lin.	in. lin.
Greatest breadth of the cerebellum	4 3	3 11	3 10	3 8	3 7	3 6	3 6	3 5	3 3
Longitudinal diameter of the cerebellum in the middle	2 7	2 6	2 5½	2 5	....	....	2 5	2 4	....
Breadth of the nodus encephali between the nervi quinti	1 4	1 3	1 2	1 1½	1 1	....	1 1½	1 1¼	1 0⅔
Longitudinal diameter of the nodus encephali in the middle	1 1	1 0⅓	1 0	1 0	....	....	0 11	0 10	

* Cuvier (Extrait d'Observations, faites sur le cadavre d'une femme connue sous le nom de Venus Hottentotte; Mémoires du Muséum de l'Histoire Naturelle, tom. iii. p. 266.) has given a description of the individual publicly exhibited in London and Paris under the name of the Hottentot Venus.

The Brain (Cerebrum) of the Negro.

The Negro brain (Plates XXX. XXXI. XXXII. XXXIII.) which I dissected, as well as both the Negro brains which I had the opportunity of seeing in the Museum of Comparative Anatomy at Paris, have for the most part the same form as the European brain. The brain is divided by a deep fissure into two hemispheres. In this fissure appears the corpus callosum, or the commissura magna cerebri, which unites both hemispheres. The anterior portion of the hemispheres is something narrower than is usually the case in Europeans. This is particularly remarkable in the brain of the Bosjes woman (Plate XXXIV.). The length and height of the hemispheres do not visibly differ from that of the European; their breadth only is something less. This is evident from the comparison of the two Negro brains measured by me, of a Negro brain examined by the brothers Wenzel, of the brain of a Bosjes woman, and of the brains of seven European men and six women, the dimensions of which are given in the following Tables:

Table I.

Dimensions of the Cerebrum of Negroes.

	Brain of the Negro Honoré.	Brain of a Negro in the Jardin du Roi à Paris.	Brain of the Bosjes woman, the Hottentot Venus.	Brain of a Negro measured by the brothers Wenzel.
	in. lin.	in. lin.	in. lin.	in. lin.
Length of the cerebrum	5 10	5 $11\frac{2}{3}$	5 10	6 1
Greatest breadth of the cerebrum	4 6	4 $9\frac{2}{3}$	4 $4\frac{1}{2}$	5 0
Height of the cerebrum	2 11	3	2 11	

Table II.

Dimensions of the Cerebrum of European Males.

	in. lin.	in. lin.	in. lin.	in. lin.	in. lin.	in. lin.	in. lin.
Length of the cerebrum	7 2	6 2	6 1	6 1	6 $\frac{1}{2}$	6 0	5 9
Greatest breadth of the cerebrum	5 6	5 5	5 3	5 2	5 2	4 9	4 8
Height of the cerebrum	3 10	3 9	3 7	3 5	3 1	2 11	2 10

Table III.

Dimensions of the Cerebrum of European Females.

	in. lin.	in. lin.	in. lin.	in. lin.	in. lin.	in. lin.
Length of the cerebrum	6 4	6 3	6 1	5 10	5 8	5 3
Greatest breadth of the cerebrum	5 6	5 4	5 3			
Height of the cerebrum	2 11	2 $10\frac{1}{2}$	2 9	2 7		

Each hemisphere is subdivided into three lobes,—the anterior, middle, and posterior lobes,—similar to the European brain. The posterior lobes cover the cerebellum entirely, and project considerably over it. The whole external surface of the hemispheres is covered by a great number of gyri, separated by deep sulci. The gyri are particularly large on the anterior part of the hemispheres of the brain of the Bosjes woman. It is remarkable that the gyri and sulci of the hemispheres show more symmetry than is usually found in European brains. This is particularly visible in the brain of the Bosjes woman. On the middle of the basis of the brain (Plate XXXII.) are the pons Varolii, nodus encephali, or tuber annulare, the crura or pedunculi of the cerebrum, the eminentiæ candicantes or corpora albicantia, the gray tuberculum, and the hypophysis cerebri, or the glandula pituitaria, which on the whole are very much the same, as in the European brain. The pedunculus of the hypophysis in the brain of the Negro HONORE′ (*e.* Plate XXXIII.) was somewhat thicker and larger than in the European. The hypophysis cerebri (*f.* Plate XXXIII.) was somewhat smaller. I cannot say whether this is always the case in the brain of the Negro.

In reference to the internal structure of the brain of the Negro, it is composed, like the brain of the European, of two substances, the outer gray, or cortical substance, and the internal white fibrous or medullary substance, as has been observed by several anatomists; but they do not agree on this point.

J. F. MECKEL * says the gray substance is of a darker colour than in the European brain, and also the medullary substance is not so white, but yellowish gray or light brown.

J. G. WALTER † found the medullary substance in the Negro just as white as in the European: the cortical substance, on the other hand, darker, of a grayish brown colour, which he attributed to the darker colour of the blood in the Negro. CAMPER, BONN ‡, and SOEMMERRING § found likewise the medullary substance just as white in the Negro as in the European.

SOEMMERRING says that he examined three perfectly fresh Negro brains without finding any difference in the colour of the cortical substance from that of the European. FL. CALDANI ||, on the contrary, found the gray substance in the brains of two Negroes darker than in Europeans. RUDOLPHI ¶ has remarked the same in the brain of a Mulatto. I can say nothing regarding the colour of the medullary and cortical

* De la Diversité de Couleur dans la Substance Médullaire de Nègres; Histoire de l'Académie de Berlin, 1753, p. 97. Du Cerveau des Négres, ibid., 1757, p. 69.

† Epistola Anatomica ad virum illustrem W. HUNTERUM de Venis Oculi, p. 20. Berolini, 1778. "In Æthiope meo dissecto color omnium partium medullarium cerebri, cerebelli et medullæ oblongatæ erat perfecte albus. Substantia vero corticalis cerebri, quæ in Europæis cineritii coloris est, in hoc Æthiope paulo obscurioris, hoc est ex cineritio bruni coloris est. Hæc permutatio coloris mihi oriri videtur a sanguine, qui ad substantiam corticalem fertur."

‡ Descriptio Thesauri Ossium morbosorum Hoviani, p. 133. "Medulla cerebri, oblongata atque spinalis Æthiopissæ intus albissima erat."

§ Vom Körperlichen Unterschied des Negers, S. 18.

|| Congettura sopra Uso della glandola Timo, con alcuni altri Discorsi, p. 38. Venezia, 1808.

¶ Lehrbuch der Physiologie, B. ii. Abth. 1. S. 15.

substance, as the Negro brain which I dissected had been preserved some time in alcohol.

Meckel and Walter * found the texture of the medullary substance of a Negro brain firmer than usual, as tough and firm as it is sometimes found in the brain of insane persons. Neither Soemmerring, Camper, Bonn, Caldani, Rudolphi, nor I, found this.

In the internal structure of the brain of the Negro I did not observe any difference between it and that of the European; it seems to me therefore superfluous to give a minute description of it. Plate XXXIII. gives an exact representation of a vertical dissection of the brain of the Negro Honoré, and proves the accuracy of my statement. The corpora quadrigemina, the valve of Vieussens, the aqueduct of Sylvius, the pineal gland or the conarium, the optic thalami, with the corpora geniculata, the corpora striata, the corpus callosum, or commissura maxima cerebri, and also the commissura anterior and posterior of the brain, resemble entirely the same parts in the brain of the European.

The glandula pinealis, of a light red colour, of a conical form, lying upon the corpora quadrigemina, was fixed to the posterior commissura by a medullary lamella, and was connected by two processes of medullary substance, the pedunculi conarii, with the inner side of the thalami optici. In the centre of the pineal gland was a fine sandy substance of a yellow colour, called the lapilli glandulæ pinealis, or the acervulus. Soemmerring likewise found this sandy substance in all the Negro brains which he dissected.

The fornix, with the crura anteriora, arising from the corpora albicantia, the medullary lamellæ of the septum lucidum, the crura fornicis posteriora, with the fimbria medullaris, the pedes hippocampi majores, with their round undulated and serrated extremity, and the hippocampi minores, occupying the interior of the floor of the posterior cornu of the lateral ventricle, showed no difference from that of the European brain. The lateral ventricles of the brain, two irregular-shaped cavities occupying the central parts of the three lobes of each hemisphere, consisted of three cornua, as in the European brain. The anterior cornu ran forwards and inwards in the substance of the anterior lobe of each hemisphere. The middle or inferior cornu descended to and terminated in the middle lobe. The posterior cornu ran backwards and inwards into the posterior lobe of the brain, and ended in a rounded extremity. Within each lateral ventricle was a plexus choroideus.

Are the Nerves of the Negro thicker and larger than those of the European?

Soemmerring was the first who compared the size of the brain with the thickness of the nerves. He says that the nerves on the basis of the brain are somewhat thicker

* "Deprehendi substantiam medullarem cerebri in hoc Æthiope duriorem, quam ordinariè in Europæis esse solet, et ferè tantæ tenacitatis, ut in nonnullis hominibus mente captis."

in the Negro than in the European. This difference seemed to him particularly remarkable in the olfactory and optic nerves, and in the nervi quinti. This difference is not visible in the nerves of the brain of the Negro HONORE' (Plate XXXII.); they are quite as small as the nerves in European brains: nor did I find any difference in the brain of the Bosjes woman, nor in the two Negro brains in the Museum of Comparative Anatomy at Paris. We cannot, therefore, allow that the Negro brain is smaller than that of the European compared with the size of the nerves, or that the nerves of the Negro are thicker than those of the European.

Has the Brain of the Negro more resemblance to the Brain of the Orang-Outang than that of the European?

The Monkeys have in their outward form and inward structure the greatest reremblance to Man. GALEN* remarked their great similarity. TYSON† was the first who dissected the brains of an African Orang-Outang, and of a Jocko or Chimpanzee, and says he found no difference between them and the human brain. His own words are: "The brain is reputed the more immediate seat of the soul itself; one would be apt to think that since there is so great a disparity between the soul of a man and a brute, the organ likewise in which it is placed should be very different too; though by comparing the brain of our Pygmie with that of a man, and examining with the greatest exactness each part in both, it was very surprising to me to find so great a resemblance of the one to the other, that nothing could be more."

BUFFON‡, relying on TYSON's researches, says, "Le cerveau de l'Orang-Outang est absolument de la même forme et de la même proportion, et il ne pense pas; y a-t-il une preuve plus évidente, que la matière seule, quoique parfaitement organisée, ne peut produire ni la pensée ni la parole, qui en est le signe, à moins qu'elle ne soit animée par un principe superieur?"

I showed, several years back, by dissecting the brains of some species of the genus Simia§, as well as the brain of the Asiatic Orang-Outang‖, that the opinion of TYSON and BUFFON is erroneous, and that the brain of Monkeys, and even of the Orang-Outang, differs very much from the human brain. The brain of the Monkey and the Orang-Outang differs as follows from the human brain.

1. The brain is absolutely and relatively smaller and lighter, shorter, narrower, and lower than the human brain.

2. The brain is smaller in comparison to the size of the nerves than in man.

3. The hemispheres of the brain are, relatively to the spinal marrow, medulla ob-

* De Administrationibus Anatomicis, lib. i. c. 2. "Simia inter universa animantium genera, tum visceribus, tum musculis, tum arteriis, tum nervis, simillima homini est, quod et ossium forma."

† The Anatomy of a Pygmy. London, 1699. 4°.

‡ Histoire Naturelle, tom. xiv. p. 61.

§ Icones Cerebri Simiarum et quorumdam Mammalium rariorum. Heidelbergæ, 1821. Fol.

‖ The brain of the Orang-Outang compared with that of Man. Zeitschrift für Physiologie. 1827. B. 2. S. 17.

longata, the cerebellum, corpora quadrigemina, the thalami optici, and corpora striata, smaller than in Man.

4. The gyri and sulci of the brain are not so numerous as in Man.

Any one may eonvince himself of the truth of these assertions by examining my plates of the brain of Apes. I give here exact plates of the surface and basis of the brain of the Asiatic Orang-Outang (Plate XXXV. figg. 1, 2.), and of the African Orang-Outang, or Chimpanzee (Plate XXXV. figg. 3, 4.). This last plate represents the brains of those animals contained in the Hunterian Museum in London*. The hypophysis and the origin of several nerves are wanting. By comparing the Negro brain with those of the Orang-Outang, we shall find the same difference as between the brain of the European and the Orang-Outang, The only similarity between the brain of the Negro and that of the Orang-Outang is, that the gyri and sulci on both hemispheres are more symmetrical than in the brain of the European. It remains, however, to be proved whether this symmetry is to be found in all Negro brains, which I very much doubt. The size and quantity of the brain of the Negro varies as much as the European from that of the Orang-Outang. I measured the capacity of the cavum cranii of a full-grown Asiatie Pongo, and found that it only held 11 oz. 7 dr. The brain of this Pongo was therefore much smaller than is usual, even in congenital idiotism.

Conclusions drawn from these Anatomical Researches.

I. The brain of a Negro is upon the whole quite as large as that of the European and other human races. The weight of the brain, its dimensions, and the capacity of the cavum cranii prove this fact. Many anatomists have also incorrectly asserted that Europeans have a larger brain than Negroes.

II. The nerves of the Negro, relatively to the size of the brain, are not thicker than those of Europeans, as Soemmerring and his followers have said.

III. The outward form of the spinal cord, the medulla oblongata, the cerebellum, and cerebrum of the Negro, show no important difference from that of the European.

IV. Nor does the inward structure, the order of the cortical and medullary substance, nor the inward organization of the interior of the Negro brain show any difference from that of the European.

V. The Negro brain does not resemble that of the Orang-Outang more than the European brain, except in the more symmetrical distribution of the gyri and sulci. It is not even certain if this is always the case. We cannot therefore coincide with the opinion of many naturalists, who say that the Negro has more resemblance to Apes than Europeans, in reference to the brain and nervous system. It is true that many ugly and degenerate Negro tribes on the coast show some similarity in their outward form and inward structure to the Ape; for instance, in the greater size of the bones of the face, the projecting alveoli and teeth, the prominent cheek-bones, the

* The drawings were taken from these specimens by permission of the Board of Curators.

recession of the chin, the flat form of the nose-bones, the projecting and strong lower jaw, the position of the foramen occipitale magnum, the relative greater length of the ossa humeri and the bones of the foramen, the flat foot, and in the length, breadth, shape, and position of the os calcis.

Such are the similarities with the Ape mentioned by those authors who have paid more particular attention to the growth and anatomy of the Negro, as Camper, Soemmerring, Cuvier, White, Lawrence, and Virey. These points certainly distinguish many Negro tribes from the Europeans, but they are not common to all the Negroes of the interior of Africa; the greater number of which are well made, and have handsome features.

Some Remarks on the Intellectual Faculties of the Negro.

The brain is undoubtedly the organ of the mind. It is the part of our body which gives us the consciousness of our own existence, and through which we receive the impressions made upon the external senses, conducted to the brain by the nerves. Here the perceptions are compared and combined so as to produce ideas. In this organ we think, reason, desire, and will. In short, the brain is the instrument by which all the operations called intellectual are carried on. It is proved by facts and observations that animals partake of feelings, sensations, and intellectual faculties in a higher degree, and approach more nearly to mankind in proportion as their brain resembles more the human brain. An intimate connexion between the structure of the brain and the intellectual faculties in the animal kingdom cannot be doubted. As the facts which we have advanced plainly prove that there are no well-marked and essential differences between the brain of the Negro and European, we must conclude that no innate difference in the intellectual faculties can be admitted to exist between them. This has been denied by philosophers*, naturalists†, and travellers, who assert that the Ethiopian race is naturally inferior to the European in intellectual and moral powers. The data upon which such an opinion is based are either erroneous suppositions and false deductions from anatomy and physiology, or superficial observations on the intellectual and moral faculties of the Negroes, made by partial or prejudiced travellers. Very little value can be attached to these researches, when we consider that they have been made for the most part on poor and unfortunate Negroes in the Colonies, who have been torn from their native country and their families, and carried into the West Indies, and doomed there to a perpetual slavery and hard labour in the sugar plantations. Such is the nature

* Hume (Essays, vol. i. p. 21, Nat. M. p. 512.) and Meiners.

† Lawrence (l. c. p. 493.) says, "I deem the moral and intellectual character of the Negro inferior, and decidedly so, to that of the European; and as this inferiority arises from a corresponding difference of organization, I must regard it as his natural destiny." Mr. Lawrence speculates only on the inferior character of the Negro; he has given us no proof of the lower organization of the Negro's brain.

of the researches of THUNBERG *, LONG †, JEFFERSON ‡, ESTWICK, CHATELUX, and others. Many of them deny that the Negro is a reasonable being, and they say that all Negroes are vicious, malignant, perverse, treacherous, and faithless. They observe, that the understanding of the Negro is not capable of improvement, that their temper and disposition are incorrigible, and that they are incapable of civilization. Some have even believed the falsely supposed natural inferiority of the intellectual and moral faculties of the Ethiopian race, to be an excuse for slavery.

The character of the Negroes, as described by such authors, is the result of slavery and inhuman treatment, to which they are exposed in the colonies, as RAMSAY §, BECKFORD ||, DICKSON ¶, HAWKER **, T. CLARKSON ††, F. NEWTON ‡‡, G. PINCHANT §§, and the official documents |||| laid before the House of Commons, have sufficiently proved. The behaviour of the Negroes in a state of slavery accords with the treatment they receive from their white masters. This is asserted on the authority of BEATTIE, IMLAY ¶¶, B. EDWARDS ***, and others. The disposition of the poor Negro slaves is in general distrustful and cowardly; for so degrading is the nature of slavery, that the fortitude of the mind is lost, and its free agency restrained. A very keen observer ††† says: "The feelings of the Negroes are extremely acute. According to the manner in which they are treated they are gay or melancholy, laborious or

* THUNBERG says, "It may indeed be alleged that the inhabitants of the warmer climates have a dull torpid brain, and are less keen and sharp than the Europeans. They have a power of thinking, but not profoundly, and consequently conversation among them is rather trifling. They are in general idle, sleepy, heavy, and lascivious. To these qualities the heat of the climate itself inclines them; and without insulting the dark brown inhabitants of the East Indies, one may truly say that there is a greater difference between them and the Europeans than between them and the Monkeys."

† The History of Jamaica, tom. ii. pp. 335, 374. London, 1774.

‡ Notes on the State of Virginia, p. 232. London, 1787. JEFFERSON, speaking of the Negroes, says, "Comparing them by their faculties of memory, reason, and imagination, it appears to me that in memory they are equal to the Whites, in reason much inferior, as I think one could scarcely be found capable of tracing and comprehending the investigation of Euclid; and that in imagination they are dull, tasteless, and anomalous. Indeed it may be reckoned unfair to compare the capacity of Africans with that of Europeans, who have been so long civilized; but it cannot be reckoned so in comparing them to the American Indians."

§ Essay on the Treatment and Conversion of African Slaves. London, 1784.

|| Remarks upon the Situation of the Negroes in Jamaica, p. 84. London, 1788.

¶ Letters on Slavery, p. 20. London, 1789.

** Sermon. London, 1789.

†† Essay on the Slavery and Commerce of the Human Species.

‡‡ Thoughts upon Slavery.

§§ Notes on the West Indies.

|||| The horrors of the Negro Slavery existing in our West Indian Islands, irrefragably demonstrated from official documents recently presented to the House of Commons. London, 1805. 8°.

¶¶ A Topographical Description of the Western Territory of North America. London, 1793. 8°.

*** The History, Civil and Commercial, of the British West Indies. London, 1819.

††† Histoire des Antilles, p. 483.

slothful, friends or enemies. When well fed and not maltreated, they are contented, joyous, ready for every enjoyment; and the satisfaction of their mind is painted in their countenance. But when oppressed and abused they grow peevish, and often die of melancholy. Of good and bad treatment they are extremely sensible, and against those who injure them they bear a mortal hatred. On the other hand, when they contract an affection to a master there is no office, however hazardous, which they will not boldly execute to demonstrate their zeal and attachment."

The original good character of the Negro tribes on the Western Coast of Africa has been corrupted and ruined by the horrors of the slave trade, since they have unfortunately become acquainted with Europeans *. The introduction of brandy and other spirits, and the immorality, dissipation, cruelty, rapacity and fraud of the slave traders, have made the Negroes indolent, cunning, dissolute, and thievish.

This has been satisfactorily proved by many travellers, more particularly by Captain Ph. Beaver†. The slave trade alone is the principal cause of the slothfulness of the Negroes on the coasts‡, as the Committee for the African Institution allow, when they say, "How can it be expected that men should addict themselves to the arts of agriculture and commerce, whilst the labourers in both are themselves the greatest articles of trade, and form the chief exports of the country? What adequate motive can be found for toiling to improve their domestic comforts or their possessions, by men who are in constant danger of being hurried into perpetual exile? It is needless to take into account the many vices adverse to industry which are generated by this traffic; for it is enough to keep men indolent that no fruit of their labour can be secure to them for a moment."

It is proved, that since the slave trade has been greatly impeded by the acts of the Parliament for abolishing this infamous, dishonourable traffic, and since kidnapping on the Gold Coast has been much diminished, and personal security proportionately increased, the natives have become more diligent. This has manifested itself by increased industry. From those improvements may be inferred the unspeakable and innumerable benefits which must accrue to Africa from a total abolition of the traffic in slaves.

The Negro tribes of the interior parts of Africa are a far superior people to those

* Although the slave trade existed in the time of the Phœnicians, the old Egyptians, Carthaginians, Romans, and Saracens, it did not reach its full extent till the beginning of the sixteenth century, introduced on the western coast of Africa by the Portuguese Alonzo Gonzales. The Spaniards brought Negroes in the year 1502 to St. Domingo, and in the year 1510 to Peru. The slave trade was legal in the time of the Emperor Charles V., Pope Leo X., Queen Elizabeth, and Louis XIII., under the pretence that the Negroes are not Christians.

† African Memoranda, relative to an attempt to establish a British Settlement in the Island of Boulan. London, 1805.

‡ Report read to the General Meeting on the 15th July 1807, p. 34.

on the coasts. They are active, diligent, and industrious. We learn from all those travellers who have lately explored the interior of Africa, as Mungo Park, Golbery, Lucas, Hornemann, Burchell, Denham, Clapperton and others, that there already exists in districts remote from the coast a considerable degree of industry, and that no small progress has been made in several of the useful arts. It is also remarkable, that though these gentlemen travelled in various directions, and from points of the continent widely remote from each other, they all found the same striking contrast between the interior and the coast.

"The Negroes in general," says Mungo Park, "are considered by the whites of the coast as an indolent and inactive people: I think without reason. The nature of the climate is indeed unfavourable to great exertion; but surely a people cannot justly be denominated habitually indolent, whose wants are supplied, not by the spontaneous productions of nature, but by their own exertions. Few people work harder, when occasion requires, than the Mandingoes."

Lucas, Denham, and Clapperton say the same of the Soosoos, Fulahs, Felletas, the inhabitants of Sudan, Fezzan, Born, Houssa, Kashna, and Beghharmi. The productions of these countries are, different kinds of grain, garden-fruits, tobacco, indigo, cotton, beeswax, honey, gums, and woods used in dyeing. But of all these productions, which can only be obtained by cultivation and labour, the natives only grow sufficient for their own immediate use, as they have but few opportunities of turning to advantage the superfluous produce of their labour. They have vast herds of cattle, and occupy themselves with breeding horses and camels. In their great towns there are many mechanics, smiths, weavers, dyers, tanners, ropemakers, potters, and even goldsmiths and silversmiths. Mungo Park says: "But perhaps their ingenuity is most conspicuously displayed in working their native gold; for not only are they well acquainted with the preparation and use of an alkaline salt to assist in liquefying the metal, but in ornaments which they make from it; such as bracelets, necklaces, and ear-drops, to adorn their females, in which they display a variety of taste and an elegance of fancy that might excite admiration even among the best artists of Europe."

We may see by this that the Negroes in their native land are by no means indolent and inactive, or incapable of industry. They may be considered, on the contrary, as an industrious people.

The moral character and disposition of those Negroes who are not degenerated and ruined by slavery is in general very good. They are naturally affectionate, and ardently attached to their children, parents, friends, and countrymen. Their feelings of honesty, humanity, generosity, and gratitude are very acute. Their dispositions and manners are gentle, benevolent, and amiable. The Negro tribes are very hospitable towards each other as well as towards strangers. Travellers are heartily welcome to partake of whatever the family board affords. The little which they have they will divide with the poor, without any other motive than that of pure compassion for the indigent. In short, many of the Negroes possess a natural goodness of heart, warmth of affection, nobleness of character, and mildness of disposition; and it cannot be denied that they excel many Europeans who are most violent against them. ADANSON*, PROYART†, WINTERBOTTOM‡, GOLBERY§, TROTTER, TUCKEY, DEMANET||, MUNGO PARK, LUCAS, DENHAM, and CLAPPERTON, have mentioned many anecdotes truly honourable to the moral character of the Negro; and CLARKSON, FALCONBRIDGE, GRANDVILLE, NISBET¶, PINKART, RAMSAY, SHARP, WILBERFORCE, and other philanthropists, have collected and distributed them amongst the people ** in England, in order to give them a favourable opinion of the poor oppressed Negroes.

The intellectual faculties of the Negroes do not in general seem to be inferior to those of the European and other races. Such of them as are not bodily and morally degraded by slavery and oppression, have a pleasing and open expression of countenance, and are of a gay and cheerful turn. They exhibit proofs of good natural capacity, good sense, wit, and penetration. The truth of this statement is most fully confirmed by the accounts given by credible travellers. BARBAT†† says, "The blacks are, for the most part, men of sense and wit enough; of a sharp ready apprehension, and an excellent memory, beyond what is easy to imagine; for though they can neither read nor write, they are always regular in the greatest hurry of business and trade, and seldom in confusion."

But I will not lengthen this treatise with many extracts in proof of my opinion. I refer only to the works of two learned and respected men, my venerable friend BLUMENBACH‡‡, and Bishop GREGORY§§, both defenders of the intellectual powers of the Negroes. They have mentioned many instances of Negroes who made a certain progress in the liberal arts and sciences, and distinguished themselves as clergymen||||,

* Voyage en Senegal, pp. 31, 118.

† Histoire de Cango, pp. 59, 73. Paris, 1776.

‡ Account of the Native Africans in the Neighbourood of Sierra Leone. London, 1789.

§ Fragment d'un Voyage en Afrique, tom. ii. p. 391. Paris, 1802.

|| Histoire de l'Afrique Française, tom. ii. p. 3.

¶ Capacity of Negroes for Religious and Moral Improvement. London, 1789.

** An Abstract of the Evidence delivered before a Select Committee of the House of Commons in the Years 1790 and 1791, p. 91. London, 1801.

†† Description of the Coasts of North and South Guinea, in CHURCHILL'S Collection, vol. v. p. 235.

‡‡ Beyträge zur Naturgeschichte, Th. i. S. 73. Gottingen, 1806.

§§ De la Litterature des Nègres; ou Recherches sur leurs Intellectuelles, leurs Qualités morales et leur Littérature; suivies des Notices sur la Vie et les Ouvrages des Nègres, qui se sont distingués dans les Sciences les Lettres et les Arts. Paris, 1808. 8°.

|||| The preacher JAC. ELISA JOH. CAPITEIN, who studied at Leyden, and got his degree in the year 1742. Dissertatio Politico-theologica de Servitute Libertati Christianæ non contraria.

The Wesleyan Methodist preacher MADOCK.

philosophers*, mathematicians†, philologians‡, historians §, advocates, medical men ||, poets ¶, and musicians. Many Negroes have distinguished themselves by their talents in military tactics and politics **.

Not all Negro tribes can be called barbarous, nor have they all remained in a wild and barbarous state, as many historians and naturalists have too hastily asserted. Mungo Park, Denham, and Clapperton mention large towns, as Houssa, Kocka, Kaschne, Sego, and others, in which there are schools established for the education of youth: there is even a certain kind of literature amongst them, and men who study the Koran and some Arabian works. We must say with Robinson of all the tribes of the Ethiopian race, as well as of all other human races, "Whatever their tints may be, their souls are still the same."

The principal result of my researches on the brain of the Negro, is, that neither anatomy nor physiology can justify our placing them beneath the Europeans in a moral or intellectual point of view. How is it possible, then, to deny that the Ethiopian race is capable of civilization? This is just as false as it would have been in the time of Julius Cæsar to have considered the Germans, Britons, Helvetians, and Batavians incapable of civilization. The slave trade was the proximate and remote

* Ant. Wilh. Amo, who got the degree of Doctor of Philosophy at Wittenberg. Diss. Philosophica de Humanæ Mentis απαθεια s. sensionis ac facultatis sentiendi in mente humana absentia et earum in corpore nostro organico ac vivo præsentia, aut A. G. Amo, Guinea-Afro. Wittenbergæ, 1734.—Diss. Philosophica continens ideam distinctam eorum, quæ competunt vel menti vel corpori nostro vivo vel organico.

† Hannibal, Lieutenant-General and Director of the Engineers in the time of the Czar Peter I. and his son, who made the plan of the harbour and fortress Cherson in the time of Potemkin.

Lislet Geoffroy, an officer in the Engineers in the Isle of France, was elected Corresponding Member of the Parisian Academy on account of his good meteorological observations.

Thom. Fuller, of Virginia, distinguished by his arithmetical talents.

Benj. Bannacker, Negro from Maryland. He edited at Philadelphia an Almanac for the year 1794, concerning the motions of the sun and moon, the true places and aspects of the planets.

‡ Don Juan Latino, Professor of the Latin language at the University of Sevilla.

§ Ignatius Sancho, esteemed by Garrick and Sterne. Letters of the late Ignatius Sancho, an African. London, 1784. 3rd. edit.

Gustav. Vassa, who lived in London. He has given the interesting narrative of the Life of Olaudah Equiano, or Gustavus Vassa, written by himself. London, 1791. 3rd edit.

Othello, Negro from Baltimore, who has written a paper against Negro slavery.

|| James Derham, physician in New Orleans, of whom the celebrated Dr. Rush says: "I have conversed with him upon most of the acute and epidemic diseases of the country where he lives, and was pleased to find him perfectly acquainted with the modern simple mode of practice in those diseases. I expected to have suggested some new medicines to him, but he suggested many more to me."

¶ Francis Williams, who wrote some good Latin poems.

Phillis Wheatley, Negro servant at Boston, wrote poems on various subjects, religious and moral. Walpole, 1803.

** The unfortunate Toussaint Louverture, who imitated Napoleon in St. Domingo.

Denham and Clapperton give interesting portraits of Bello, the sultan of Kaschna, and of the sultans of Loggun, Kouka, and of the general Barca Ghana in Bournos.

reason of the innumerable evils which retarded the civilization of the African tribes. Great Britain has achieved a noble and splendid act of national justice in abolishing the slave trade. The chain which bound Africa to the dust, and prevented the success of every effort that was made to raise her, is broken. Hayti and the colony of Sierra Leone can attest that free Negroes are capable of being governed by mild laws, and require neither whips nor chains to enforce submission to civil authority.

Explanation of the Plates.

Plate XXX.

Upper surface of the brain of a male Negro, æt. 25.

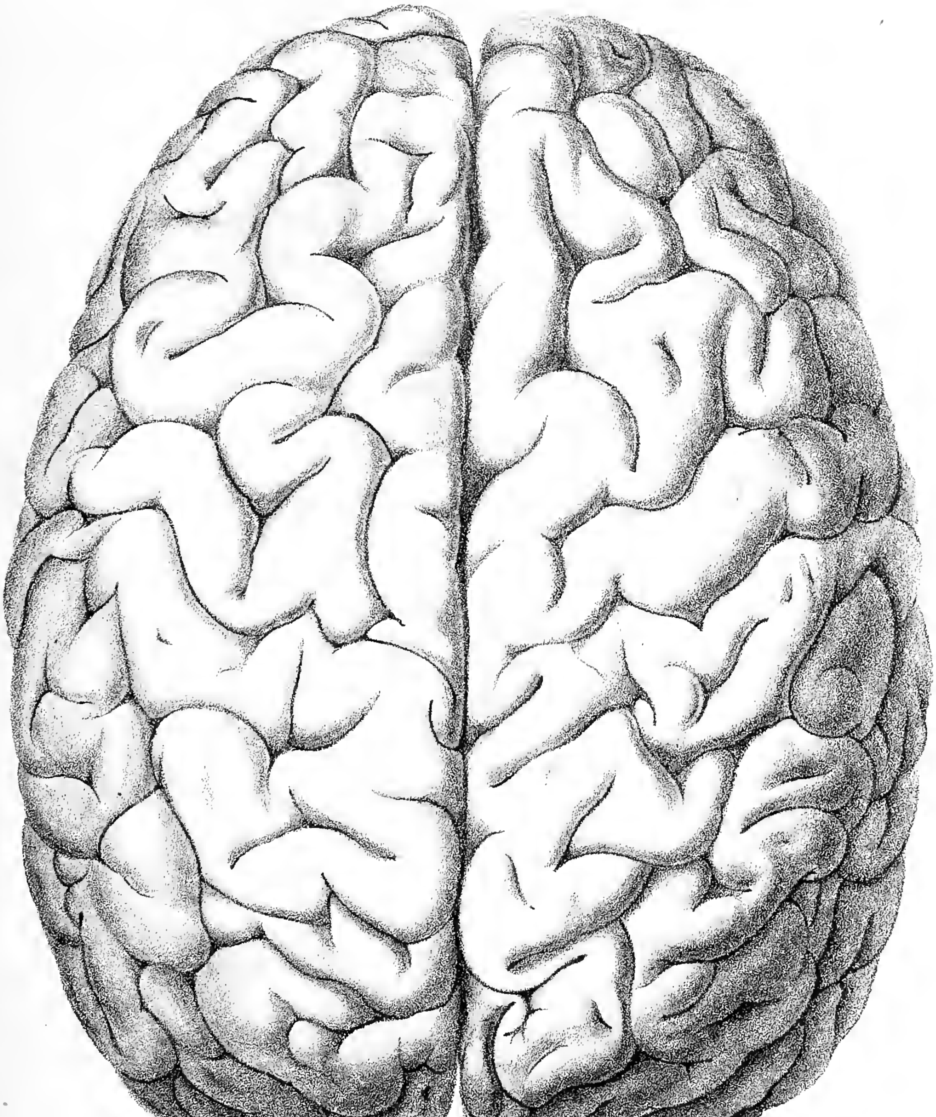
Plate XXXI.

Side view of the same brain.

Plate XXXII.

Base of the same brain.

a. Anterior lobes of the cerebrum.
b. Middle lobes.
c. Posterior lobes.
d. Grey tubercle, or floor of third ventricle.
e. Infundibulum, or pedunculus of
f. The pituitary gland, or hypophysis cerebri.
g. Eminentiæ candicantes, or corpora albicantia.
h. Crura cerebri.
i. Pons Varolii.
k. Cerebellum.
l. Floccus cerebelli.
m. Corpora pyramidalia.
n. Corpora olivaria.
o. Section made at the termination of the medulla oblongata.
1. Olfactory nerves.
2. Optic nerves.
3. Motores oculorum.
4. Pathetici.
5. Trigeminal nerves.
6. Abducentes.
7. Facial nerves.
8. Auditory nerves.



Male Negro.

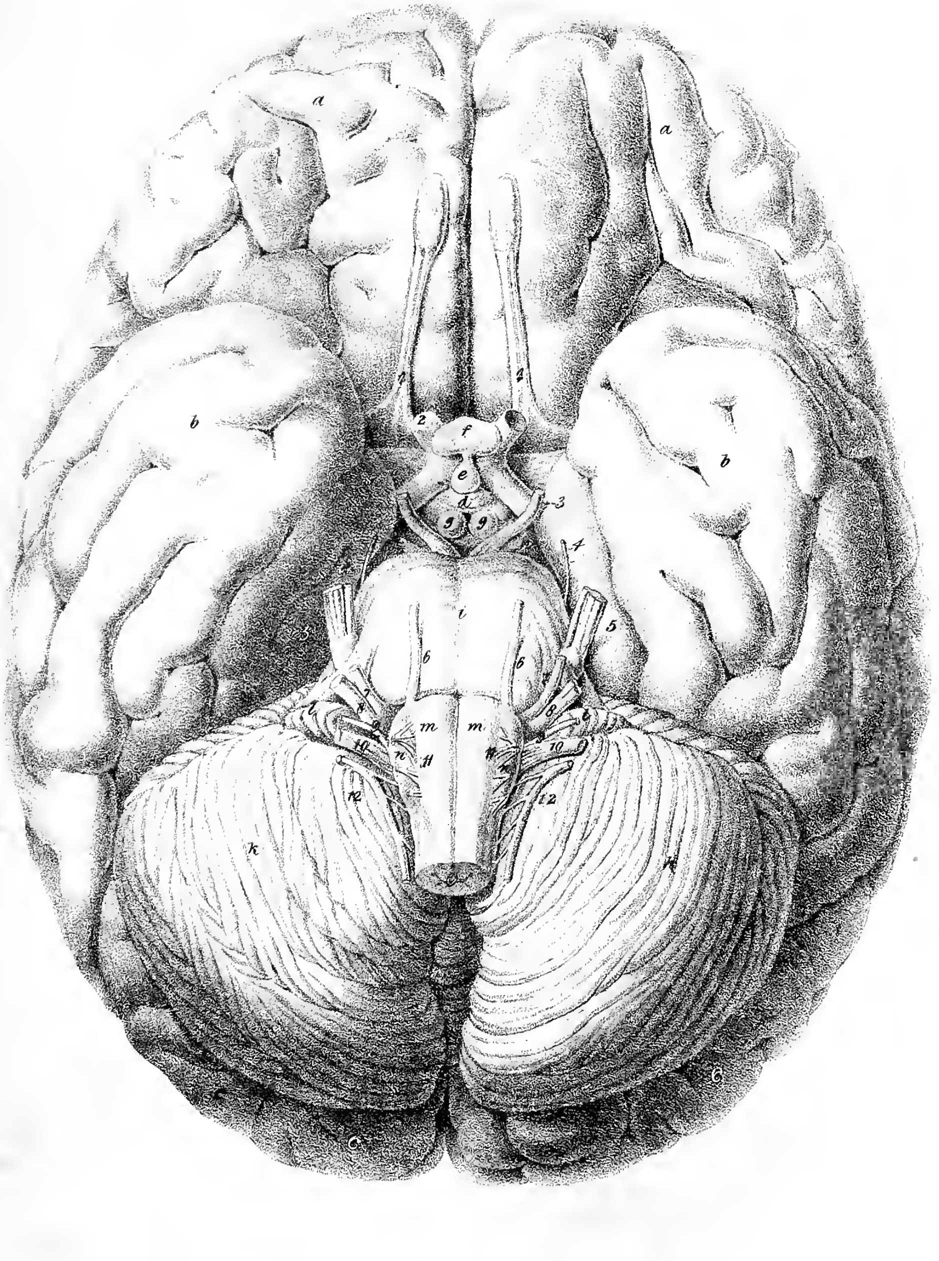
H. Tiedemann ad nat. del.

J. Basire lith.

Male Negro.

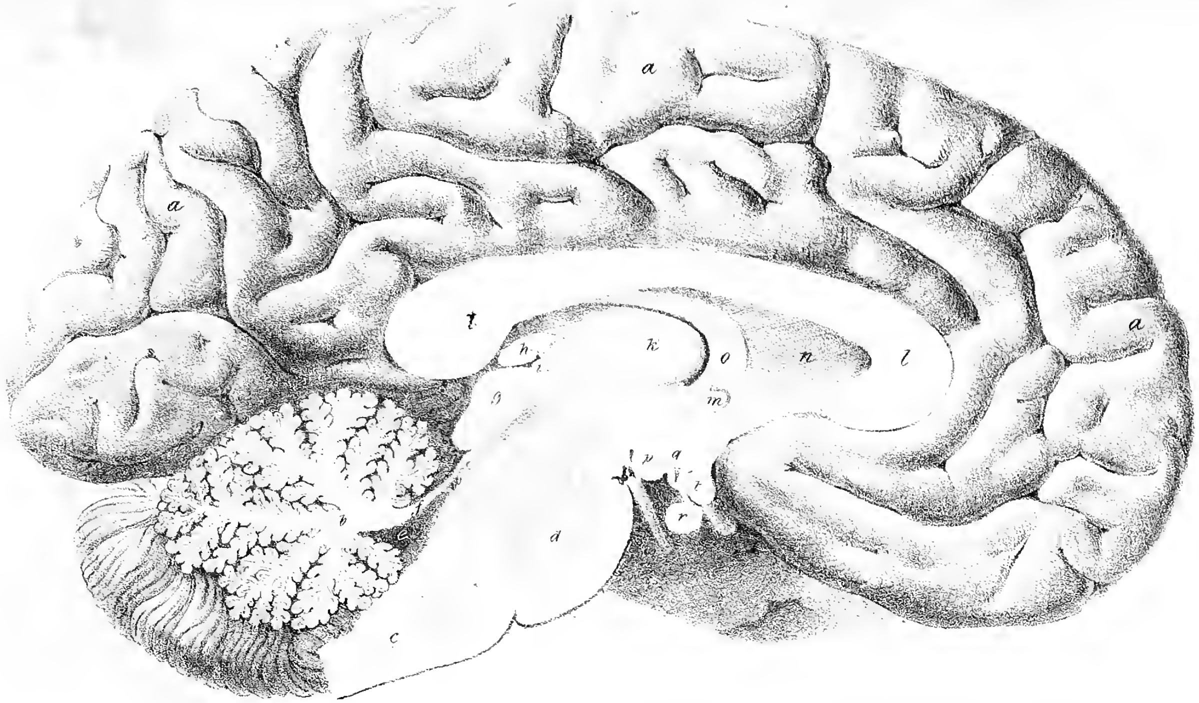
F. Tiedemann ad nat. del.

J. Basire lithog.



Male Negro.

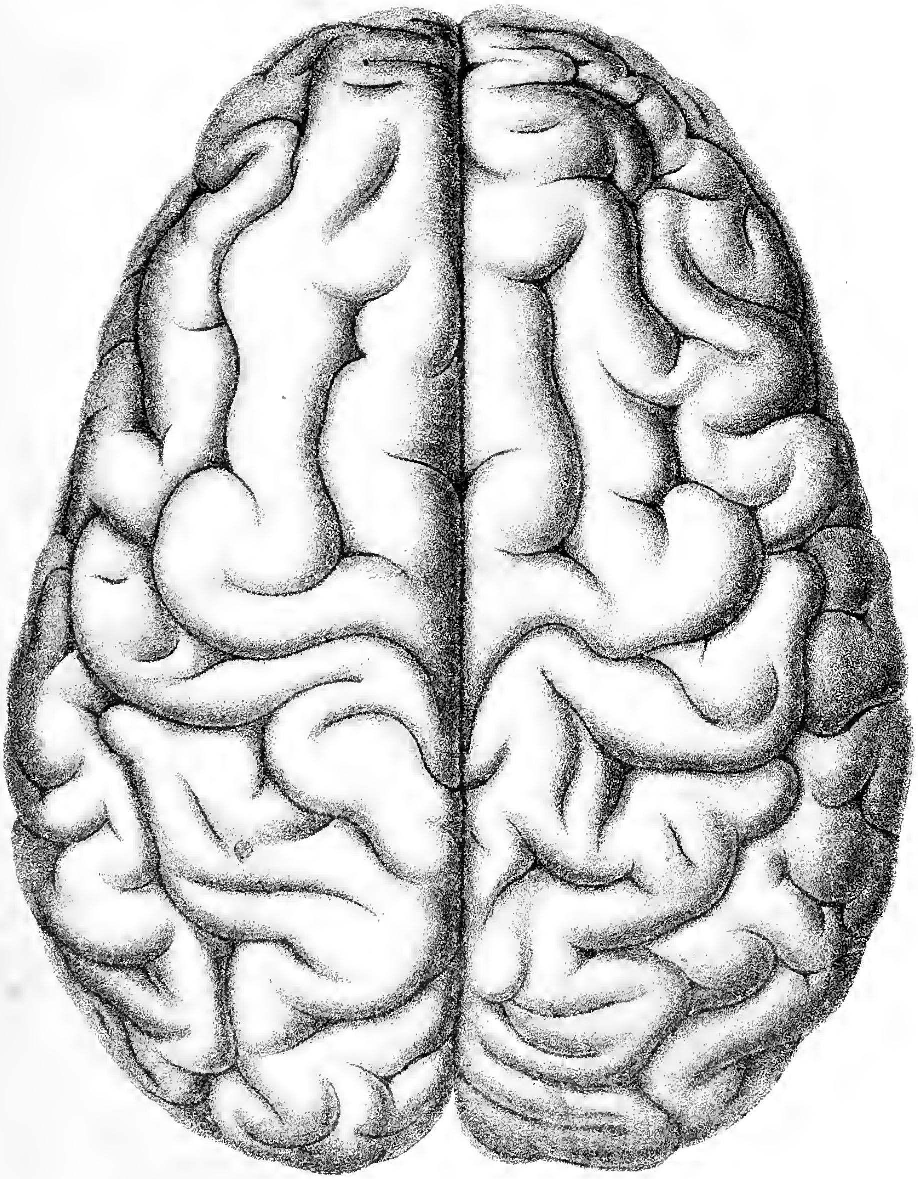
F. Wagner del.



Male Negro.

F. Wagner de nat. del.

J. Basire lithog.



Bosjes Woman

Fig. 1.

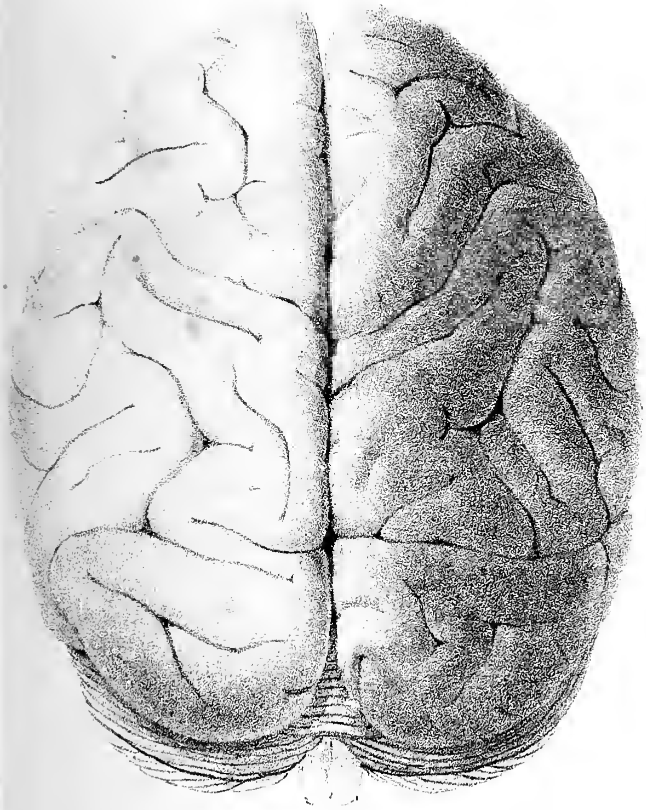
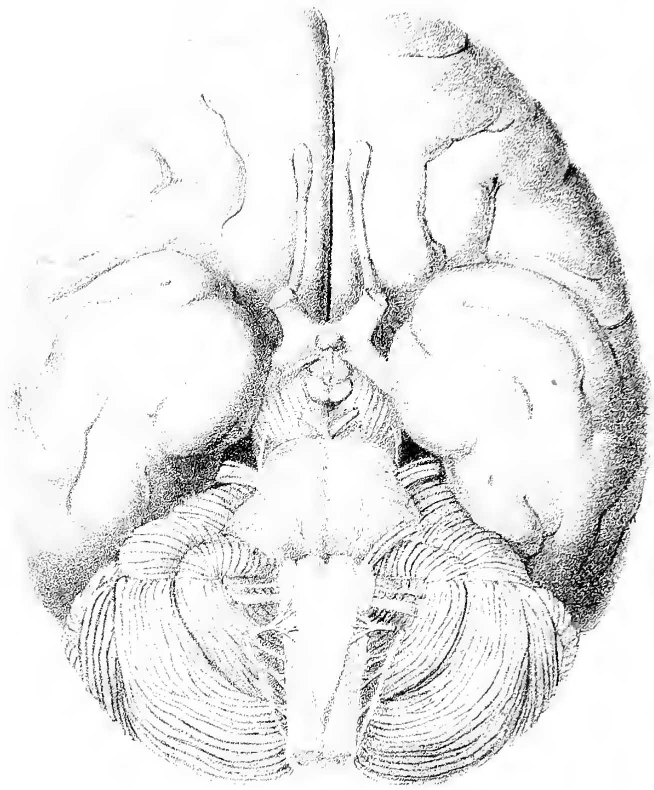


Fig. 2.



Simia Satyrus.

Fig. 3.

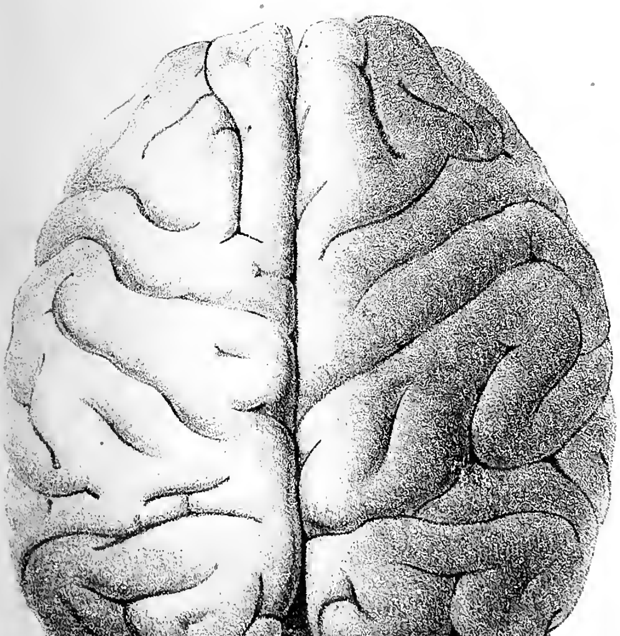
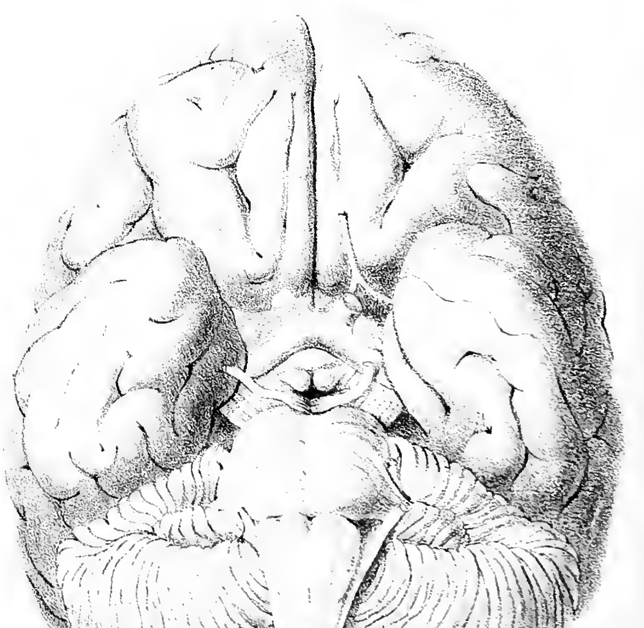


Fig. 4.



Simia Troglodytes.

...mann, ad nat. del.

9. Glosso-pharyngeal nerves.
10. Par vagum.
11. Lingual.
12. Accessory nerves.

Plate XXXIII.

Mesial surface of a vertical longitudinal section of the brain of the male Negro.

a. Mesial convolutions.
b. Cut surface of cerebellum.
c. Cut surface of grey substance in the medulla oblongata.
d. Pons Varolii.
e. Fourth ventricle.
f. Valve of Vieussens.
g. Corpora quadrigemina.
h. Pineal gland.
i. Posterior commissure.
k. Soft commissure.
l. Great commissure.
m. Anterior commissure.
n. Septum lucidum.
o. Fornix.
p. Eminentia candicans.
q. Infundibulum.
r. Pituitary gland.
s. Origin of motor oculi nerve.
t. Decussation of optic nerve.

Plate XXXIV.

Brain of a Bosjes woman.

Plate XXXV.

Brains of the Orang-Utan and Chimpanzee.

Fig. 1. Upper surface of the brain of a young Orang (*Simia Satyrus*, Erxl.).
2. Base of the same.
3. Upper surface of the brain of a young Chimpanzee (*Simia Troglodytes*, Blum.).
4. Base of the same.

XXIV. *On the Respiration of Insects. By* George Newport, *Esq. Member of the Royal College of Surgeons, and of the Entomological Society of London. Communicated by* P. M. Roget, *M.D. Sec. R.S.*

Received and Read June 16, 1836.

Respiration.

It has been long proved by many physiologists that insects produce the same changes in the atmosphere during respiration as other animals. Reaumur, Bonnet, Scheele, Huber, Edwards, Audouin, and others, have all shown that the results of the respiration of atmospheric air by insects are the production of carbonic acid gas, and the loss of oxygen; but these results vary in degree in different genera,—in the different states of the same insects,—and at different periods of the year. My object, therefore, in this paper will be to show the relative quantity of air consumed by different tribes of insects in their different states,—the power which particular insects have of supporting existence in different media,—and the relation which this power and the consumption of air bear to the comparative volume of the structures concerned.

The life of an insect has been considered by naturalists to have three distinct periods, the larva, the pupa, and the perfect state; but each of these periods, in so far as the functions of the different structures of the body are concerned, although tending only to the production of the perfect individual, is in itself a distinct condition. Thus the respiration, circulation, temperature, food, and locality of the insect are in general all different in the different states. In the earliest period of the larva state the respiration is much feebler than when the animal has nearly arrived at its full size, and the circulation of its blood is much quicker; but the relative quantity of its food is much greater, in proportion to its bulk, in the latter than at the earlier period, and its power of generating heat increases as it approaches to its adult condition. In the pupa state also there is a change in all these functions. In many genera the insect ceases to eat; its circulation becomes slower than at any other period; its respiration is greatly diminished in frequency and volume; and its power of generating and of maintaining a temperature of body above that of the surrounding medium, which every individual insect constantly preserves when in a state of activity, is now almost suspended. In the perfect, or imago, state there are other changes in these functions. The respiration again increases in frequency and volume; the power of generating and of maintaining heat is very much augmented; the circulation is more rapid than at any other period, while the necessity for a constant supply of food is often less urgent than in the larva state. Hence it is evident that much caution is necessary in drawing conclusions from our observations on the function of respiration in insects in their different states, and that where quantity of air is concerned the relative volume of the organs of respiration must not be forgotten.

Parts concerned in Respiration.

The parts more immediately concerned in respiration are the tracheæ and spiracles, the first of which undergo very great changes during the transformations of the insect. The next are the muscles and the nerves distributed to them.

1. *The Tracheæ.*

The tracheal vessels in the larva of *Sphinx ligustri*, L., consist, as described by Lyonet and others in the *Cossus ligniperda*, of a series of tubes conveying air like the bronchial tubes and tracheæ of other animals; but instead of being composed of only one set communicating with the mouth, as in vertebrated animals, they are multiplied in number, and are arranged along each side of the body of the insect near the middle line, between the dorsal and ventral layers of longitudinal muscles. There are nine sets of tracheal tubes on each side, corresponding to the nine outlets or spiracles. The first is situated in the second segment, behind the head, and the remaining eight are in the fifth and succeeding segments to the last or anal one. The different sets are connected together by two longitudinal tracheæ, one on each side of the body, and communicate by very short tubes from these tracheæ with the spiracles.

The structure of the tracheal tubes has been described by Swammerdam, Sprengel, and others. The tracheæ are formed, as described by Sprengel, of two membranes, an external serous, and an internal mucous, inclosing between them a spirally convoluted elastic fibre [Plate XXXVI. fig. 1. *a.*], which gives them the appearance of the tracheæ of other animals. The external or serous membrane [*b*] is very loosely attached around the middle or spiral structure. The whole mucous or internal lining is continuous with the cuticle or external covering of the body, and is thrown off and renewed whenever the insect changes its skin, as noticed by Swammerdam, DeGeer, Lyonet, and Bonnet; although Sprengel seems to consider this as not a true membrane, but as only forming a means of connexion between the coils of the middle or spiral fibre. I have seen this lining membrane of the tracheæ thrown off with the skin of the larva at every period of its change in almost every order of insects, but more particularly in the genera *Apis*, *Papilio*, and *Scarabæus*, and am satisfied that it is a distinct structure, and is not merely that portion of the membrane which lines the entrance of the tubes which is thus thrown off, but the complete lining membrane of the ramified tracheæ.

Each set of vessels consists of from eight to ten or twelve tubes, which originate in a bundle from the longitudinal tracheæ, and distribute their branches over the stomach and other viscera, sending minute anastomosing ramifications to every part of the body, even into the substance of the brain and nerves. The longitudinal tracheæ communicate freely with each other across the body, both at the anterior and posterior part of the insect, and along the whole dorsal and ventral surfaces, by small ramifications of the tracheæ from each side meeting and anastomosing together. At the anterior part of the body there is also a large tracheal tube communicating between the tracheæ of the sides. It is situated in the second segment, and extends into the base of the first, where it gives off, immediately behind the brain, four principal branches, which are distributed forwards over the brain and head. Two of these go to the antennæ, and the others to the anterior and upper part of the head. There is also a large branch from the under surface of the longitudinal trachea in the second segment, which communicates across the under surface of the segment with a corresponding one from the opposite side. This is the general arrangement of the tracheæ, particularly in the larva state of insects. The larva of the Bee has the tracheal vessels very small, but freely communicating around the body, as was shown by SWAMMERDAM. The same insect in its perfect state has these communications still existing, but the whole of the principal tracheæ are then developed into large vesicles or bags. This is the case even with those tracheæ which traverse the under surface of the abdomen, although the tubes of communication are not obliterated [Plate XXXVI. fig. 2. *g*.]. All volant insects in their perfect state have the respiratory organs of the same vesicular structure.

The vesicles, as was shown by SWAMMERDAM * and SPRENGEL, are covered with innumerable punctured spots, which are only perceptible under a good microscope, and when attentively examined exhibit somewhat the appearance of perforations. MARCEL DE SERRES and STRAUS DURCKHEIM deny the existence of spiral fibre in these vesicles, but SUCKOW and BURMEISTER † are of opinion that it really does exist, and I am myself disposed to maintain the same view. Indeed when we remember that the vesicles exist only in the perfect insect, and are only dilated tracheæ, and that in tracheæ the existence of spiral fibre is undoubted, it surely cannot be questioned that it exists also in the vesicles, although probably in a very attenuated state, and almost atrophied. The nature of the punctured spots in the vesicles is of some interest, since I am not aware that they have been distinctly observed until after the insect has passed into the perfect state. BURMEISTER, who contends for the existence of spiral fibre in the vesicles, conceives that these spots are occasioned by the rupture of the spiral fibre during development, and are formed by the interspaces between the portions of ruptured fibre. That this cannot be the case is proved by the existence of these spots in some of the tracheæ which communicate directly with vesicles, and have not been dilated, in which the spiral fibre is distinctly seen to be unbroken; and also by the circumstance of their not being in a regular series, or in the course of the fibres in the vesicles, but distributed thickly and irregularly over the surface of the whole vesicle, and by their existing between two parallel fibres, and even in the

* Biblia Naturæ, Plate XXIX. f. 10.

† Manual of Entomology, translated by SHUCKARD, 1836, p. 181.

substance of fibres, as I have distinctly seen them in the vesicles of the male Humble-bee, *Bombus terrestris*, STEPH. Besides this, I have also observed them terminating in an abrupt and remarkable manner in the dilatation of the large tracheæ in the same insect; and I have also seen them in some of the larger tracheæ themselves, as was observed and figured by SWAMMERDAM in *Oryctes nasicornis*, STEPH. The existence of these punctured spots so universally in perfect volant insects, leads us to inquire into their probable use. After many careful examinations I am disposed to believe that these spots are only partial perforations of the vesicular structures,—that they do not pass through the internal or mucous lining,—and perhaps are little cells or receptacles in the coats of the vesicles, in which the circulatory fluid can be most freely submitted to the action of the air in the vesicles through the delicate mucous lining, as in the minute terminal air-cells in the respiratory organs of vertebrated animals.

When the tracheal vessels have become developed into pulmonary sacs, those of each segment may be considered as analogous to, and as only a repetition of, the tracheal structures in Vertebrata. A very large proportion of the ramifications from each centre or spiracle in the larva of the Sphinx and other Lepidoptera is distributed over the alimentary canal. Those which are given to the œsophagus and stomach extend from the second to the tenth segment, while those which go to the duodenum are from the eleventh, and those to the colon and cæcum from the twelfth and thirteenth. The minute branches of these vessels pass between the fibres of the muscular coat of the alimentary canal, and are distributed upon the mucous coat, between it and a structure which I believe has not hitherto been described, the *adipose coat*, which lies between the mucous and muscular coats, and into which the ultimate ramifications of the tracheæ are extended. This layer is very distinct in the alimentary canal of *Cerura vinula*, STEPH., particularly in the colon and cæcum [Plate XXXVI. fig. 3. *a. b.*]. All the secretory and generative organs are furnished with minute anastomosing branches in abundance, even the dorsal vessel itself, and the ovarial tubes. They are distributed through the limbs, even to the extremities of the tarsi in the perfect insect, and through the antennæ and eyes.

The development of the air-vessels into sacs or bags in volant insects begins to take place a little before the insect changes into the pupa state. In all larvæ which undergo a complete metamorphosis, when passing into the pupa state, the respiratory organs are distinctly tracheal, without any dilatations; and this is more strictly the case in those insects which afterwards have the largest vesicles, as in the *Scarabæi*, *Lucani*, *Lepidoptera*, and *Hymenoptera*. In lepidopterous insects the tracheæ do not appear to undergo any marked change until about the time when the insect has ceased feeding. Those which are the first dilated in the Sphinx are from the second and fifth spiracles. These, with the anterior portions of the longitudinal tracheæ, become a little dilated soon after the insect has entered the earth, and is forming the cell in which it is to undergo its transformation. In the Butterfly (*Vanessa urticæ*, STEPH.), which does not enter the earth but suspends itself in the open air, the change begins to take place while the insect is spinning its thread for this purpose. When it has remained a few hours at rest, preparatory to undergoing its transformation, the tracheæ become enlarged; and at about the period when the change to the pupa state takes place the insect appears to make several powerful respiratory efforts, accompanied with much muscular exertion, and these are continued at intervals until its old skin becomes fissured, and is gradually thrown off. The tracheal vessels of the fifth and sixth segments, at this period of changing to the pupa state, begin to assume the vesicular form, and become more and more dilated during the first few days after the change. In the Sphinx, and those insects which pass the winter in the pupa state, there appears to be an interval, or cessation in the development of the tracheæ, as well as of all the other structures, during hybernation; but when the changes have again taken place in the spring the development continues until the respiratory organs occupy a very large proportion of the body of the insect; so that when the insect has arrived at the perfect state, the longitudinal tracheæ in the thorax are exceedingly large canals, leading to, and communicating with, the roots of the wings in the thorax and the air-bags in the abdomen. There are four of these air-bags on each side of the abdomen in the Sphinx and other Lepidoptera. The largest are close to the posterior part of the trunk or thorax, the others gradually decrease in size as they approach the anus. In the male Humble-bee, *Bombus terrestris*, Steph., the anterior vesicles are exceedingly large, and form, with those which follow them, a series of very freely communicating respiratory cavities, while a nearly similarly free communication exists between the two sides of the body in the transverse tracheæ, which are dilated into a series of funnels which communicate with each other across the body by their apices [Plate XXXVI. fig. 2. *g*.].

In those insects which undergo their changes in the open air without entering the earth, and which pass but a few days in that condition, there is no interval or period of suspension of development. In the common Nettle Butterfly, *Vanessa urticæ*, which during the summer undergoes its changes in at most fourteen days, and very often, if the season be favourable, in eight or nine, the changes in the respiratory organs have distinctly begun to take place about *two hours* after the insect has suspended itself for transformation. Meckel has observed that the air-sacs are found in the inseet soon after it has entered the pupa state; but I have found that the dilatation of the tracheæ, whieh are developed into these sacs, commences very much earlier. On examining the insect about *half an hour* before it changes to a pupa, I have always found the whole of the tracheæ a little distended, particularly those in the under surface of the thorax, from the first two pairs of spiracles, but their distribution to the stomach and intestines has continued as regular as in the active larva. It is at the actual moment of transformation that all the changes take place most rapidly. The efforts which the insect makes at that time appear very much to affect the condition of the respiratory organs. When the insect has fissured and thrown off its old skin as a larva, there is a cessation of its efforts for a few seconds. It then makes a few slow but very powerful respirations, during which, as in every forced inspiration, the abdominal segments are much distended, after which the longitudinal layers of muscles of these segments become very much contracted, and the segments themselves shortened. While this is taking place the circulatory fluid contained in the vessels of the abdomen is propelled forwards, and the wings, which at the moment of slipping off the old skin are scarcely so large as hemp-seeds, are distended at their base, and at each inspiration of the insect are gradually enlarged by the propulsion of circulatory fluid into them, and are carried down over the lateral and under surface of the trunk, and the ventral surface of the first two segments of the abdomen. This is exactly what takes place in the *Sphinx ligustri*, as well as in *Vanessa urticæ*. From the fact of all the tracheæ being enlarged immediately after the insect has changed to the pupa state, it seems not improbable that this enlargement is occasioned by the closing of the spiracles, and the expansion of the air within the tracheæ during the powerful respiratory efforts of the insect in effecting its transformation,—that it results from the recession of the circulatory fluid from the vessels of the abdomen into the partially developed wings taking off pressure at the instant from the tracheal tubes, which then become distended by the natural elasticity of the air contained within them.

Professor CARUS attributes the development of the air-bags and dilatation of the tracheæ entirely to the closing of the spiracles and the expansion of the contained air, which he thinks is *increased in quantity* * during the development of the insect. But it seems more probable that the formation of air-bags is occasioned simply by a continuance of the same cause, the elasticity of the contained air, which produces the enlargement of the tracheæ in the first instance, and that this enlargement or dilatation keeps pace with the gradually decreasing size of the digestive organs, since the spiracles are not permanently closed during the pupa state, but are in constant action, except during the period of complete hybernation. It is at the actual moment of transformation that the anterior pair of large vesicles begins to be formed, and the tracheæ in the next segment are a little more enlarged. The antennæ, which just before the change were coiled up within the sides of the head, are now extended along the sides and abdomen, and the tracheal vessels within them may be readily examined. If the antenna be separated from the pupa while soft and transparent, it will be seen that the trachea within it extends from its base to its apex [Plate XXXVI. fig. 5. *a. b.*]. It is a continuation of a large trachea that comes from the first spiracle, and crossing the segment above the œsophagus and dorsal vessel, sends off its cruciform branches immediately behind the brain, at the back part of the head. There are four branches given off at that point, as before noticed; the two external ones are those which supply the antennæ. The main tracheal vessel of the antenna at this period of the insect is very small, but afterwards becomes much enlarged.

* Introduction to Comparative Anatomy, translated by GORE, 1827, vol. ii. p. 167.

It passes along the antenna nearest its under surface, and in the *Vanessa urticæ*, Steph., gives off laterally thirty-four pairs of minute branches, one pair to each segment.

In the twelve segments which constitute the club or apex of the antenna, the trachea becomes very minute, and in the last four segments of the club it is divided into numerous ramifications [Plate XXXVI. fig. 6. *b*.]. About *an hour* after the change to the pupa state the tracheæ from the first spiracle, which ramified over the œsophagus, are enlarged to about double their original size, and instead of continuing of a pencillated structure, are almost of equal diameter throughout, and are beginning to be detached from the œsophagus, which is becoming narrower. At *seven hours* these changes have all been carried further, and the air-cells in the abdomen are much larger. At *twelve hours*, besides the gradual enlargement of the tubes, the chief thing observable is the diagonal direction of the tracheæ from the seventh spiracles, which supply the pyloric extremity of the stomach, which proves that the stomach is gradually becoming shorter previous to the detachment of these tracheæ, which subsequently takes place. *Eighteen* hours after the change all the longitudinal and thoracic tracheæ, with those of the head, are still further enlarged, and the tracheæ from the third pair of spiracles, which are given to the cardiac extremity of the stomach, are partly detached from that organ, and are more dilated than any of the others. The tracheæ from the ninth spiracles, which are given to the colon, are beginning also to assume a vesicular form. The stomach is still supplied with tracheæ from six spiracles. At *twenty-four hours* the changes are still advancing. At *thirty-six hours* the tracheal branches distributed to the different ganglia are enlarged; and at *forty-eight hours* the development of these parts is so far advanced that nearly all the tracheæ in the body have become a little dilated, and this dilatation continues until the insect has become perfect. The only difference between the development of this insect and the Sphinx, or those which undergo their metamorphoses in the earth and remain in the pupa state during the winter, is in the rapidity of the changes; and even this difference exists only in those diurnal insects which are developed in the beginning and middle part of the summer, since in those individuals which are produced late in the season, and consequently remain in pupa through the winter, all the circumstances are precisely similar. The real use of the pulmonary sacs, which are found in all volant insects, appears to be, as supposed by John Hunter, to enable the individual to alter its specific gravity at pleasure by enlarging its bulk, and thus render it better able to sustain itself on the wing with but little muscular effort. That this is the real use of the sacs may further be inferred from their non-existence in the larva or infant condition of the insect, and from their almost entire absence in all insects which are destined to live entirely on the ground. This opinion is further supported by the fact that they are most developed, relatively to the size of the individual, in those insects which sustain the longest and most powerful flight, as Hymenoptera, Lepidoptera, some of the winged Coleoptera, the Hemiptera and Lucani, while in none of these insects in the larva state is there anything approaching to sacs, but the respiratory organs are purely tracheal. The common Humble-bee, which has the largest pulmonary vesicles of any insect in the perfect state, has its respiratory organs in the larva state exceedingly small and pencillated. But further proof of the vesicles being for the sole purpose of lightening the body is to be found in the male *Lucanus cervus*, LINN. In that insect the large and apparently heavy mandibles and head, instead of being filled with solid muscles, are filled almost entirely with a string of vesicles, which are developed from the sides of some large tracheæ that extend through the mandibles. By this beautiful provision of nature these apparently heavy and unwieldy structures are rendered extremely light, while their solid exterior fits them for all the purposes of strength and defence required by the insect.

2. *The Spiracles.*

The *structure* of the spiracles, the orifices of respiration, is somewhat complicated. In the larva of *Sphinx ligustri*, L., the spiracles are oval apertures closed externally by valves, which open perpendicularly in their long axis, like the iris in feline Mammalia. They are placed on a level with the external or cuticular surface of the body, and are formed by a series of converging fibres, edged, as in the iris, by circular ones [Plate XXXVI. fig. 6.], and guard the entrance of the spiracles. At a little distance within this valve the spiracle is considerably enlarged, and there is situated a second valve, of a more complicated structure. The anterior half of this second valve [Plate XXXVI. fig. 7. *a*.] is of a darker colour and firmer substance than the outer valve. Its inner surface, or that which looks towards the viscera, is concave, and its margin crescent-shaped, and it is not acted on by any muscles. The posterior portion of the valve [fig. 7. *b*] is thick, moveable, and of a dark colour, and closes on the anterior half like a cushion or pad. This portion is acted on by a remarkable muscle, the *retractor valvulæ* [fig. 7. *c*], composed of five distinct fasciculi of fibres uniting in a common tendon, and by their conjoined action opening the valve, just as the *levator palpebræ* elevates the eyelid in Man and other animals. The tendon into which the converging fasciculi of this muscle are inserted, passes diagonally upwards and backwards, and is inserted into a little elevation in the common tegument of the body. A few circular fibres surround the edges of the inner valve [fig. 7. *d*.] of the spiracle, and constitute the sphincter muscle which closes the valve. It is among these fibres that the retractor muscle originates. The sphincter muscle and valve are still further acted upon by another muscle, which may be considered the great constrictor muscle of the spiracle, *retractor spiraculi* [fig. 7. *e*.]. This muscle originates from the *posterior margin of insertion* at the anterior ventral surface of each segment, at a little distance from the median line [Plate XXXVII. fig. 26.], and passing diagonally upwards and outwards terminates in a tendon, with which some of the fibres of the orbicular muscle are blended. The internal or proper valve of the spiracle appears to be continuous with the mucous lining of the tracheæ

[Plate XXXVI. fig. 7. *a. b.*]. CARUS has noticed a similar structure in the *Grylli*, but has not described it further than as resembling an eyelid*. He seems to have thought it simply a reduplication of the mucous membrane, but has taken no notice whatever of the muscles belonging to it.

The Muscles concerned in Respiration.

The muscles concerned in the function of respiration, besides those which properly belong to the spiracles, include those of each entire segment of the body. Every act of *inspiration* is of a mixed character, and is partly a voluntary effort of the animal, and partly dependent upon those combined laws of the animal economy which, depending upon each other for their continuance, constitute organic life. These laws have justly been designated the involuntary functions of the body. Every act of *expiration*, in the natural state of the animal, is more of an involuntary than of a voluntary character, and may be regarded simply as a disposition in the muscles concerned to regain their previous condition, which is intermediate between contraction and relaxation, and takes place independently of the will of the animal.

As every act of respiration thus consists of two distinct efforts, it necessarily requires the consentaneous action of all the muscles of the parietes of the abdomen and trunk in vertebrated, and of all the muscles of a segment of the body in invertebrated animals. This is really what takes place in respiration. In Man all the muscles of the chest and abdomen are in constant action; so are all the muscles of the segments in insects, whether the animal be awake or sleeping.

The muscles of a segment of the body in the larva of *Cossus ligniperda* have been minutely described by LYONET, but as I shall presently have occasion to notice the particular nerves which are distributed to them in the *Sphinx ligustri*, I must be permitted to describe those of the ventral surface of a segment in the larva of that insect

Those muscles which form distinct layers or sets, and act together, are generally inserted into slightly elevated ridges in the skin, while a single muscle, or the tendon of many muscles united together, is attached to a tuberculated elevation. The skin is there thicker than in other places, and thus affords a means of attachment. There are always three ridges for the insertion of muscles between two abdominal segments. The middle one is the largest, and affords both origin and insertion to the straight or longitudinal muscles, while the others in like manner afford origin and insertion to the oblique ones.

On removing the fat and viscera from the abdomen of the larva, the first layer which presents itself, and forms the interior parietes of the body, consists of many longitudinal fibres, like the *recti abdominales* of vertebrated animals. These muscles extend from the anterior part of the under surface of the second segment to the posterior part of the eleventh and twelfth; but it is only at the commencement of the

* CARUS, Comparative Anatomy, by GORE, 1827, vol. ii. p. 162.

sixth, which is in reality the commencement of the true abdomen, that they can properly be considered as recti muscles, since it is at this part where they begin to be most developed. While passing through the thoracic region they are thinner, narrower, and somewhat differently arranged. They are the most powerful of all the muscles of the abdomen, and are those which are most concerned in contracting and effecting the duplicature of the external teguments during the changes of the insect, and mainly assist in locomotion during the larva state. There are four sets of these muscles in the abdomen [Plate XXXVII. figg. 1. 1.], two on the dorsal and two on the ventral surface on each side of the nervous column and dorsal vessel. These muscles on the ventral surface are again divided into four sets, two on each side of the nervous column [figg. 1, 2.], and between which there is a slight interspace. The set which is situated on each side nearest the nervous column consists only of three narrow fasciculi of fibres, and may thence be called *recti minores* [fig. 2.]; while the other set, situated more externally in the segment, is broad and powerful, and consists of from twenty to twenty-five distinct fasciculi, and may thence be named *recti majores* [fig. 1.]. The attachments of these are different from those of the smaller recti. The larger recti are inserted into the middle ridge of attachment, which forms the margin of each segment [figg. 3, 3.], close to the insertion of the muscles of the preceding segment; while the smaller are inserted at about one fifth of a segment further back [fig. 4.] towards the tail, or extremity of the body, and pass over the ridge of attachment for the larger recti to their insertion in a corresponding part of the next segment. There is a small muscle which originates from the same line of attachment as the greater rectus, between it and the smaller, which goes to the stomach and attaches that viscus to the exterior tegument of the body, and may thence be called *retractor ventriculi* [fig. 5.]. There is one of these on each side the nervous column, from the fourth to the eleventh segment. On removing the recti we expose two layers of very thin fine muscles. The upper layer consists of nine distinct fasciculi of fibres, which pass backwards and outwards in a slightly diagonal direction [fig. 6.], but less so than those of the second layer [fig. 7.], which lies immediately beneath it. This consists of seven distinct fasciculi, which originate from the anterior margin of the segment close to the smaller recti and beneath the larger, and extend about half the breadth of the latter across the segment. They run backwards and outwards in a diagonal direction, and are attached below the recti as far as their outer margin [fig. 8.]. These layers of fibres, when in action, draw the outer anterior margin of the next segment towards the middle line of the body, and consequently when those of several segments on one side of the body act together they bring forwards the posterior portion of the body of the same side, and bend it in a semicircular direction. If these muscular layers on both sides of the body act together they draw forwards the posterior part of the body in a straight line. Immediately beneath these there is another diagonal layer of fibres, which originates close to the median line of the body [fig. 9.] beneath the nervous column, and a little anterior to the insertion of the smaller rectus. The origin of this set is exceedingly narrow, and resembles the origin of the oblique abdominal muscles from the crest of the pubis in Man. It is distinctly tendinous, and does not extend quite so far as the outer margin of the smaller rectus. This layer of muscular fibres begins to expand immediately above its origin, and is inserted beneath the greater rectus throughout nearly the whole extent of that muscle across the segment [fig. 10.]. When these oblique fasciculi are employed alone, they are evidently opposed to the latter, and draw the posterior part of each segment backwards and to the median line of the body; consequently they bend the anterior part of the body into the segment of an arch, and when both are in action they bend the anterior part of the body backwards. These muscular layers, from their form and direction, may be called the *first* [fig. 6.], *second* [figg. 7, 8.], and *third* [figg. 9, 10.] oblique. Beneath these there is another set of oblique fibres, which is formed of only two broad fasciculi. It originates from the anterior of the three ridges of attachment a little towards the inner edge of the smaller rectus [fig. 2.], and passing a little diagonally forwards and outwards beneath the great oblique, is attached to the third ridge of insertion, and may be called the *fourth oblique* [fig. 11.]. Beneath the posterior extremity of this set lies the *third* rectus, which is formed by three muscular fasciculi [fig. 12.], rather broader than those which constitute the second or smaller rectus, but running in the same direction longitudinally, and having the same origin and insertion. On removing the third rectus we arrive at the eighth layer of muscular fibres. This arises from the anterior ridge of attachment, and is formed by three rather broad fasciculi, which are partially crossed at their origin by the third rectus. This layer passes diagonally outwards, and is inserted into the third ridge as far as the outer margin of the great rectus and third oblique, which cover it, and may be called the *fifth oblique* [fig. 13.]. When this layer is removed we have exposed the triangular and transverse muscles. The *triangularis* [fig. 14.] consists of nine distinct muscular fasciculi, originating in a longitudinal series very near the median line of the body, alongside the nervous columns, and extending through the posterior half of the segment. The posterior of these fasciculi originates by three, and the one immediately preceding it by two, distinct heads or tendons, which, with the tendons of the other fasciculi of this set, indigitate with a set of short transverse muscular fibres [fig. 15.], ten in number, which occupy the median line beneath the nervous columns, and may be called the *transversus medius*. The fibres which form the triangular set pass backwards and outwards, with varying degrees of obliquity, and are inserted by strong tendons into the anterior transverse margin, or ridge of insertion [fig. 16.]. When this layer acts with its fellow of the opposite side it shortens the posterior half of the under surface of the segment; but when it acts singly, or in conjunction with the great or third oblique, it shortens that side of the segment, and bends that part of the body to the opposite side. It is a very powerful muscle in locomotion, and is probably also of great use in contracting the segment during transformation. The *transversi abdominales* [fig. 17.] are short, and consist of six rather broad and thick fibres, which form two sets, and originate at some distance from the median line, posteriorly to, and on the outer side of the tendons of the great oblique, and passing outwards are inserted into the integument about half way across the segment. They contract the under surface very powerfully, and bring the sides towards the median line. Anteriorly to these muscles, but further from the median line, is another set, the *abdominales anteriores* [fig. 18.], consisting of six short muscles, which are inserted into the inferior margin of the lateral surface of the body. The *abdominales laterales* arise in the posterior half of the segment by three great fasciculi of narrow tendons [fig. 19.], eight in the first, four in the second, and seven in the third. These tendons are formed from very powerful muscles, which interlace with each other, and are inserted at different distances of attachment posteriorly to the spiracle [fig. 20.]. These are the great muscles of the false feet, and are connected at their origin with other transverse muscles near the median line. When these muscles are removed there are two layers of fibres which arise from the anterior line of attachment in the posterior part of the segment. The inner layer, or *obliquus posterior* [fig. 21.], consists of nine small muscular fasciculi of fibres, which pass diagonally outwards, and are inserted into the skin at different distances beneath the lateral abdominal muscles. The other set consists also of nine distinct fasciculi [fig. 22.], which arise from the same margin in the lateral part of the segment, and after crossing over the last lateral abdominal, pass between it and the one immediately before, and are inserted into the integument: they may be called the *postero-laterales obliqui*. Besides these layers of muscular fibres there are four other sets which seem particularly concerned in the function of respiration. The first of these, the *transversus lateralis* [fig. 23.], arises tendinous from beneath the outer margin of the great ventral rectus, and passing upwards and outwards internal to the great longitudinal trachea, which it crosses, is gradually enlarged, and is inserted a little beneath the external margin of the dorsal rectus. The second *transversus lateralis* [fig. 24.] arises lower down than the first, and is inserted beneath the dorsal rectus at about half way across the segment. These muscular fasciculi appear to be directly concerned in contracting the segments in expiration. The other two muscles of respiration have been noticed when speaking of the spiracles. The first, *retractor spiraculi* [fig. 25.], as before noticed, originates from the third ridge of muscular insertion, at the anterior part of the segment, on the ventral surface, by a small tendon before the transverse abdominal muscles [fig. 26.]. It gradually increases in size, and passes upwards and obliquely outwards and backwards, and terminates in a tendon which is inserted, as before described, into the circular fibres which surround the spiracle. This muscle appears to be directly concerned in forcible expiration, during which it draws the spiracle inwards and downwards, and when the fibres which surround the spiracle and form the orbicular muscle act in conjunction with it, it assists in closing the spiracle. The other muscle directly concerned in the function of respiration, *retractor valvulæ*, is the immediate antagonist of the last, and has been described as one of the proper muscles of the spiracle [fig. 27.].

These are the muscles which more particularly claim our attention, in order that we may see what nerves are distributed to them, and consequently what nerves are most concerned in respiration.

Nerves concerned in Respiration.

The nerves concerned in respiration, like the muscles, include those of the whole segment. In a former paper I have particularly described those nerves which, from their distribution, were considered to be more especially concerned in respiration. My endeavour now is to trace these and the moto-sensitive nerves to their terminations in the different layers of ventral and lateral muscles.

The inverted position of the nervous cords in insects and other Invertebrata has not a little confounded the right understanding of the analogy which exists between the nervous cords of vertebrated and invertebrated animals, and has given an appearance of probability to the opinion entertained by some anatomists that the cords in Invertebrata are not analogous to the spinal cord of Vertebrata, but to the sympathetic system. Even some of those who now believe that these cords are really analogous to the cerebro-spinal system of the higher animals, can hardly reconcile this opinion with the fact of their being situated along the ventral instead of the dorsal surface of the body. The reason for this change of position of the cords in Invertebrata appears to be partly to protect the cords themselves, and partly that the nerves may be supplied to the limbs without having to travel round the sides of the body, and thereby be exposed to the hazard of injury, which they would be were they situated along the dorsal surface as in Vertebrata. But notwithstanding this change of position of the cords in Invertebrata,—since we now find that they are composed each of two tracts, as in vertebrated animals,—it is interesting to observe that these two cords, and the tracts of which they are composed, bear the same relative position to the viscera and to the exterior of the body as in Man and other Vertebrata. Thus the cord which runs along the ventral surface in Articulata has its motor tract nearest to the viscera (*a*), or most internal, the same as in the human subject; while the sensitive tract, which possesses the ganglia, lies along the under surface of the cord, and is nearest to the exterior of the body, just as the sensitive tract with its ganglia in Man lies nearest to the cutaneous or external surface. It will thus be seen that the two tracts maintain the same relative position with regard to each other, as well as to other parts of the body, in both divisions of the great kingdom of animated nature, whether the actual situation of the cords be along the dorsal or ventral surface of the body. This being the case, it leads us to consider the propriety of the terms *anterior* and *posterior tracts* or *columns*, as applied to the motor and sensitive tracts of the nervous system, and whether it would not be advisable entirely to abandon these terms, and designate the two columns *external* and *internal*, the sensitive the external, and the motor the internal column, since these terms would be strictly applicable to the situation or position of the columns in all classes of animals.

Perhaps it may at first appear doubtful whether this be really the case, since the large cerebral ganglia which give origin to the principal nerves of sense in Invertebrata are situated above the œsophagus towards the dorsal surface of the animal, while the cords themselves, and the ganglia which give origin to the nerves of motion and sensation simply, as well as those which supply the organs of manducation, are situated below it, along the ventral surface of the animal. Upon close examination it will be found that when the motor column in passing from the thorax to the head has arrived at the crura which descend from the brain on both sides of the œsophagus, it appears to wind round to the outer surface and unite with the base of the antennal nerves, where the column appears to terminate. It is in the median line above and between the two double cords that the transverse nerves originate (*b*), as described in my former paper; and these also unite with the nerves to the antennæ by very small filaments. This inclines me to consider them as forming part of one great system of nerves, which are more of an involuntary than of a voluntary character. Of this great system the sympathetic nerves doubtless form a part, of which the transverse nerves perhaps may be only a peculiar modification. It is a remarkable fact, that while the muscles of the wings are supplied with nerves which in every stage of the insect's existence originate by double roots, one of which is derived from the motor tract of the cords before it arrives at a ganglion, and the other both from a ganglion of the sensitive tract and from the motor tract which passes over it, they are also supplied with large nerves from the transverse series, as may be best seen in the larva, long before the organs unto which they are given are called into activity. But this is not the case with the muscles of the legs, which are supplied only with very minute filaments from the transverse series, in addition to their compound or moto-sensitive nerves. The reason for this difference in the distribution of the nerves to the wings and legs is clearly on account only of their difference of function, the wings being more directly concerned than the legs in the acts of respiration. Professor Müller* has recently thrown out some valuable hints with regard to the nature of these transverse nerves; he seems to consider them as peculiar nerves which combine the animal with the organic functions, not distinctly sympathetic nerves nor nerves of entirely voluntary motion. I am greatly inclined to lean to this opinion. It is evident that they are not simply the sympathetic system, because they are given so much to the muscles and tracheal vessels, while but very few filaments go to the viscera. I am less inclined to regard these nerves as the analogues of the true sympathetic, on account of their great size in certain insects, and because also it has been stated by De Serres† and Dr. Grant‡ that a series of ganglia exists on each side of the alimentary canal, which appears to be independent of the transverse nerves. I must acknowledge, however, that I have been unable to trace this series beyond the

* Müller's Archiv für Anatomie, &c., No. 1. 1835, pp. 81, 84.

† Paper on the Sympathetic Nerves of Insects.

‡ Lecture on Comparative Anatomy. Lancet, 1834, vol. ii. p. 515.

anterior lateral ganglia, which are contained within the head, or first segment, and are analogous to the superior cervical ganglia; and Professor Müller, in his paper upon the Sympathetic or Visceral Nerves of Insects, has expressed his doubts with regard to its existence as described by De Serres. Müller regards the nerve which I have described on a former occasion as the vagus, as analogous to the sympathetic; but there are many points which seem opposed to this opinion. The vagus, or recurrent nerve of Lyonet, is exceedingly small in almost every insect when compared with the size of the organs unto which it is distributed, especially when we compare those organs, and the nerves which supply them, with the corresponding parts of the human body. Besides which I have never been able to trace the nerve along the alimentary canal beyond the middle portion of the stomach, where it seems to be lost in the same manner as in Man and other Vertebrata.

With regard to the cords themselves, it was long ago suggested by Weber that the ganglia, which we now find to exist entirely in the sensitive tract in insects, are analogous to the intervertebral ganglia of Vertebrata. Hence the analogy between the spinal cord of Vertebrata and the abdominal cords in Invertebrata is very nearly proved. The very great analogy between the origin, course, and situation of the vagi nerves in Man, and the corresponding one in insects, clearly demonstrates the identity of the structures. Carus* has made some observations which lead us to consider whether the œsophagus and crop in some volant insects are not somewhat concerned in the function of respiration, since it is well known that every part of the alimentary canal is profusely supplied with tracheal vessels, and especially when we remember that the vagus is the chief nerve of the organs of respiration in Man. I shall therefore go more particularly into a description of this nerve in insects.

In all lepidopterous insects it has two distinct origins, one from each crus, which descends from the base of the cerebral ganglia or lobes of the brain. These origins are analogous to the two vagi nerves in Man, but instead of continuing separate and passing down one on each side of the œsophagus, they pass at first a little forwards and inwards, and unite above the palate, where they form a ganglion. Here also we have some analogy with the nerve in Man. The vagus nerve, after its junction with the spinal accessory, passes forwards and out of the skull through the foramen lacerum posterius, and there forms a slight enlargement almost precisely corresponding in situation to the point above the palate and pharynx, where the ganglia would have been situated on the nerves had the two vagi nerves in Man been united. From the ganglion thus formed by the union of the two roots of the vagus nerve in insects, the two approximated origins thus forming one trunk pass backwards along the median line of the œsophagus beneath the anterior portion of the dorsal vessel. A little behind the brain the vagus is united by filaments with the anterior lateral or cervical ganglia, which are analogous to the superior cervical of the sympathetic in Man. Here there is an analogy between the union of these ganglia and the vagus in insects, and the

* Comparative Anatomy, translated by Gore, vol. ii. p. 166.

corresponding ganglia and vagus in Vertebrata. The vagus nerve then passes along the median line of the œsophagus in close relation with the anterior or aortal portion of the dorsal vessel, which may be looked upon as the two carotids of Vertebrata united. The nerve lies between this portion of the vessel and the œsophagus, as between the carotids and lateral parts of the œsophagus in Vertebrata. A little before the vagus arrives at the cardiac portion of the stomach in insects it divides again into two parts, and very often at its point of division again forms a ganglion. This is the case in the *Meloë*, LINN., and some other genera. These two parts are divided into many others, which are distributed over the sides of the stomach, so that even in its ultimate distribution in insects the nerve still bears analogy to its distribution, and its division into many branches around the œsophagus and cardiac extremity of the stomach in Man and the higher Vertebrata. In *Crustacea* the nerve closely resembles that of insects, but approaches even nearer in its resemblance to that of Man. It is given almost entirely to the stomach, at the cardiac portion of which it forms a ganglion, and then divides into two branches, each division being subdivided into four portions, which are distributed around the stomach. Only a few filaments from these portions unite with some very fine nerves, which probably are the sympathetic, and which are given to the highly developed liver in these animals.

The minute distribution of the transverse and moto-sensitive nerves deserves particular attention. It is known that a ganglion exists in the sensitive tract of the cords in each segment, and that immediately anterior to this ganglion, on the dorsal surface of the cords, there is also a set of the transverse nerves (*c*). Each set of transverse nerves at the point of divergence from its longitudinal portion, which lies above and between the cords, forms a triangular plexus (*b*), in which the transverse fibres are observed to be continuous from one side of the body to the other, joined by the longitudinal ones, and thus form the plexus. The first branch from each set or plexus of transverse nerves is very small, and passes, as formerly described*, over the outer margin of the next ganglion (*a*), and then converging to the median line unites with its fellow of the opposite side to form the longitudinal tract (*d*), after each fibre has received a few filaments from the upper or motor surface of the cords. Hence these nerves are of mixed character, and contain some voluntary motor fibrils. The second branch of the transverse nerves (*e*) is given off on the inner side of the smaller rectus, and unites with the moto-sensitive nerve from the ganglion and motor tract of the cords (*f*). The transverse nerve then passes diagonally outwards and forwards over the smaller rectus, near the external margin of which it gives off its third branch (*g*); this passes at first forwards and outwards until it arrives at the insertion of the preceding small rectus. It then passes along the upper surface of the middle of the rectus, unto which it distributes minute branches, until it arrives at about the middle of the muscle. It then sends forward a small branch to the anterior extremity of the muscle (*h*), while its main trunk bends directly inwards to unite

* Philosophical Transactions, Part II., 1834, p. 410.

as will presently be shown, with a branch from the great moto-sensitive nerve. This union is exceedingly interesting, and proves that some of the nerves, at least, terminate in loops, which unite with portions of other nerves, according to the views of some of the German physiologists. The fourth branch also is an exceedingly interesting one, from its uniting in a similar manner with a branch from another moto-sensitive nerve. It is composed of fibres which are approximated to those of the transverse trunk, some of which passing from without inwards, and others from within outwards, form a little triangular plexus (*i*), similar to the one before described, and then unite to form the fourth branch of the transverse nerve. This branch passes directly forwards along the inner margin of the great rectus, and having arrived at the insertion of the muscle, it gives off a filament, which, dividing again into two portions (*j*), passes directly outwards, and is given to the greater recti at their insertion. The nerve then passes forwards to the external margin of the greater oblique, unto the lower portion of which it gives many filaments, and also to the second and third oblique, and to the triangularis, and then unites with the second, or inner division of the second pair of moto-sensitive nerves of the cord (*k*) in the preceding segment. The fifth branch of the transverse nerve passes off from the trunk a little more externally (*l*), and is given to the visceral surface of the greater rectus, and to some large tracheæ which are distributed over it. The transverse nerve then gives off a few small branches to the rectus, while its main trunk passes outwards until it arrives at a tuft of tracheal vessels which arise from the longitudinal trachea opposite to a spiracle (*m*). It then divides into two principal branches, one of which passes on each side of these tracheæ, giving off many branches. Some of these from the anterior branch pass inwards along the course of the tracheæ towards the alimentary canal, others forwards to the transverse lateral muscles, unto which they give filaments, and others upwards to the dorsal recti, unto which they are also distributed, while some of their ultimate branches appear to be given to the dorsal vessel. The posterior division of the nerve in like manner gives filaments to the tracheæ which arise opposite to the spiracle. A small branch joins with the trunk of the great moto-sensitive nerve (*n*), which crosses the trachea posterior to the spiracle. Another branch passes backwards, and dividing into several branches is given to the lateral oblique and lateral rectus muscles (*o*), and a fourth set passes onwards along the visceral surface of the dorsal rectus and to the dorsal vessel.

All the muscles unto which these nerves are distributed, besides being concerned in respiration, are necessarily concerned in the voluntary motions of the insect, and consequently it is necessary that they should be supplied with voluntary nerves, as well as with those just described, which are presumed to be of mixed function. This, it will be seen, is actually the case.

The first or chief pair of nerves from the moto-sensitive columns in each segment (*f*), is composed of one portion from the ganglion of the sensitive tract, and one from the motor which passes over it, and, after receiving a small filament from the transverse nerve (*e*), passes outwards over the smaller rectus, at the outer margin of which it gives off its first branch (*p*), which is directed backwards. It then passes between the third and fourth oblique, and gives off its second branch (*q*), which is directed forwards, and a little further outwards its third (*r*) and fourth (*s*), which are directed backwards. The trunk of the nerve then crosses the longitudinal trachea, and unites with a short nerve from the posterior division of the transverse nerve (*n*). It then divides into two branches (*t*), which pass upwards between the dorsal recti and oblique muscles, where they again divide into many branches. About midway across the dorsal recti some of the branches interweave with each other, and form a small plexus (*u*) before they are distributed to their proper muscles. It is the first two divisions of this great nerve which particularly claim our attention. The first division (*p*) passes backwards beneath the greater rectus, and divides again into two branches. The anterior one (*v*) is given to the four oblique muscles, and to the under surface of the rectus, which are thus shown to be supplied by two sets of nerves. The second division passes backwards and is again subdivided; the posterior division being given to the under surface of the smaller rectus, and to the lower portion of the great oblique, while its termination (*w*) is continuous with a portion of the second branch of the transverse nerves (*h*). Some of the branches of this nerve pass between the triangular and second oblique muscles (*x*), and are given to the latero-abdominal. The second branch of the main trunk of this nerve (*q*) passes obliquely forwards and outwards beneath the great oblique, and gives off first a small branch to the transverse abdominal muscles (*y*), and a little further on a second branch, one portion of which is also given to the transverse and latero-abdominal muscles (*z*), and another which passing more directly outwards supplies the latero-abdominal (31) and the oblique great constrictor of the spiracle (25), and divides behind the spiracle into two terminal portions, one of which is given to the retractor valvulæ (27), and the other, which passes forwards, to the transverse lateral muscles (24), which, as before stated, are supplied by two distinct sets of nerves. The divisions of this last portion of nerve are particularly interesting. Before dissecting these nerves I had conceived that the great constrictor of the spiracle and retractor valvulæ muscles were probably supplied by the transverse nerves, and hence was much surprised to find that they were supplied from the great moto-sensitive nerve from the ganglion of the cords, by which they are thus endowed with voluntary power and sensation. But upon reflection it will be seen that this ought really to be the case. To enable the insect to make a forcible expiration and close its spiracle, which is evidently an effort of the will, the great constrictor of the spiracle ought to be endowed with voluntary power. On the other hand, since the insect has a voluntary power of closing, it must also have a similar power of opening the orifice, and consequently the retractor valvulæ ought necessarily to be supplied from the same source. There are, however, a few filaments given from the transverse nerves to these muscles, and to the orbicular which surrounds the spiracle. The remaining portion of the trunk of this nerve passes forwards and outwards, crosses the retractor of the spiracle, and then gives off its third branch (32), which is almost immediately again divided, and sends one portion backwards to the transverse abdominal (17) and anterior abdominal (18), and the other forwards to the transverse lateral muscles (23). The remaining portion of the nerve continues its course over the transverse lateral, and terminates in the muscles of the back.

The second or oblique moto-sensitive nerve (33) from the cords and ganglia is much smaller than the first, the one we have just described. It passes diagonally outwards and backwards, and divides into two principal branches. The first passes outwards, and is given to the latero-abdominal muscles, which contract the diameter of the segment. One portion of the second branch supplies the triangular and transverse median muscles, while the other (*k*) passes downwards and outwards and unites with a portion of the third branch of the transverse nerves (*i*), as before stated.

From this distribution of the nerves it is evident that some of the muscles are supplied from two sources, and it can hardly be doubted that these have distinct functions. The remarkable fact that the transverse nerves appear almost exclusively to supply the tracheæ, while the moto-sensitive, which come from the motor tract and ganglia, and communicate volition and sensation, supply the muscles, even of the spiracles, cannot escape our observation as a striking proof that these nerves are of distinct functions. When we connect these facts with that of the longitudinal portion of the transverse nerves in each segment deriving a few filaments from the motor surface of the cords, and with that of filaments from the transverse nerves being distributed to some of the muscles in addition to nerves derived from the cords and ganglia, we can scarcely hesitate to assent to the opinion that while the transverse nerves connect the voluntary with the great organic functions of the body, they are more subservient to the latter than to the voluntary or animal powers.

The Manner in which Respiration is performed.

It has been shown that in every act of respiration in insects nearly all the muscles and nerves of each segment of the body are brought into consentaneous action, as the muscles of the chest and ribs in vertebrated animals, like which the insect is able to make either a forcible expiration, as during pain, and perhaps also during transformation, or can take a forcible inspiration at the instant of any sudden exertion. The manner in which the air is renewed in the trachea has excited some inquiry, but no satisfactory explanation of it has yet been given. Some have supposed that the dilatations and contractions of the dorsal vessel contribute towards it, others that simply the opening and shutting of the spiracles, the extension and contraction of the body, the presumed elasticity of the air-sacs, or the sliding of the segments one over the other, may be the means of effecting it*. But neither of these actions could alone induce a current, or succession of currents of air to be sent over the whole body,

* Carus, Introduction to Comparative Anatomy, vol. ii. p. 167, translated by Gore.

through the ramifying and anastomosing tracheæ, sufficient for the purposes of respiration, which is probably the result of several combined actions. The experimental fact observed by Reaumur, that the anterior pair of spiracles is the most important to the insect, connected with that of the gradual obliteration of the last two pairs during the changes to the perfect state, and the great extent to which the anterior spiracles are developed as the insect approaches that condition, lead us to conclude at once that it is chiefly through the anterior spiracles that respiration is performed. The action of the *retractores spiracula* muscles necessarily tends to contract the segments and close the spiracles, and thus, as it were, pump on the air through the longitudinal tracheæ towards the anterior or thoracic ones; while the relaxation of these muscles, and of the other oblique and longitudinal ones in each abdominal segment during the time the muscles of the thorax are in action, must naturally tend to enlarge the capacity of the body and induce an act of inspiration. Indeed we have full proof that respiration is performed by the alternate contraction and relaxation of the abdominal muscles in what takes place in Orthopterous, Hemipterous, and many Coleopterous insects. Carus has well remarked, that the abdominal segments, particularly in *Locusta*, are alternately elevated and depressed, like the ribs of Vertebrata; and every one must have observed the same thing in the larger *Bombi*, when fatigued, upon alighting after flight, and when excited. The contractions and relaxations of the muscles, and consequently the acts of inspiration and expiration, are then short and quick in proportion to the degree of excitement, which is sometimes so great that the whole abdomen is alternately extended and retracted just as the flanks and ribs of the racehorse alternate with each inspiration after a long and severely contested struggle on the course. In the *Gryllus viridissimus*, Linn., when excited I have counted thirty-seven contractions, corresponding to as many respirations, per minute, and these were precisely analogous to similar acts in Vertebrata. Thus several short contractions take place in regular succession at stated intervals, and these are followed by one more long and powerful than the rest. A slight pause then ensues, and the short contractions again commence, until they are followed at a certain interval by another long one, which is evidently a full inspiration, and takes place at no stated interval. When the insect is very much excited the interval between the long inspirations is greater, and the inspirations when made are more full and powerful. This view of the manner in which respiration is performed in insects is supported by the test of experiment.

If one of the larger moths be submerged in water, a few bubbles of air will be seen to arise from all the spiracles on each side of the body, but chiefly from the first, second, and third pairs. These bubbles diminish in size as we proceed towards the posterior extremity of the insect, the first and second pairs being the largest. During the changes of the insect these spiracles are greatly increased in size, and often considerably altered in form, while the spiracles and tracheæ in the posterior portion of the body are greatly diminished, and the anal pair which existed in the larva is entirely obliterated. The quantity of air expired through the different orifices must therefore be greater at the anterior than at the posterior ones. But if one of the larger Humble-bees (*Bombi*) be placed in water, and allowed to remain completely submerged until it has become nearly asphyxiated, the fact will then be still more apparent. At first there will be two large bubbles expired from the anterior spiracles, on each side of the trunk, at each longitudinal contraction of the abdominal segments, which corresponds to each expiration; but as the insect becomes more completely asphyxiated, the bubble of air will be only partially expelled from the spiracle, and again withdrawn at the entrance, without having escaped, at each longitudinal extension of the segments, which corresponds to each inspiration, while not a single bubble of air can be detected at the entrance of the posterior abdominal segments*. It is evident from this that it is chiefly through the anterior spiracles that respiration is performed; and it may from hence be inferred that the manner in which the insect prepares itself for flight is exactly like that of birds under similar circumstances. At the moment of elevating its elytra and expanding its wings the anterior pairs of spiracles are opened in the act of inspiration, and the air rushing into them passes into the tracheæ of the whole body, distending the air-bags and rendering the insect of less specific gravity, so that when the spiracles are closed at the instant when the insect endeavours to raise itself in the air, it is enabled to sustain a long and powerful flight with but little expenditure of muscular power. This is the condition of respiration in the perfect insect. In the pupa, and still more so in the larva, respiration is performed more equally by all the spiracles of the body, and less particularly by those of the thoracic segments. But even in these conditions of the insect the bubbles expired from the three anterior pairs of spiracles are the largest, and consequently these spiracles are the most important ones. It is thus evident that in the larva state the condition of respiration is but little advanced beyond that of the higher Vermes, and that it is only when the insect has passed through all its changes that its respiration is similar to that of the more perfect animals, which the insect then greatly resembles both in external and internal conformation.

The quantity and rapidity, or activity, of respiration appear to bear some relation to the muscular power of the insect in a state of activity. All volant insects, and among these particularly the *Hymenoptera*, respire with a greater rapidity in a given space of time, and degree of atmospheric temperature, than terrestrial insects, and in their larva condition much less than in their perfect. In the common Hive-Bee, *Apis mellifica*, LINN., I have counted from one hundred and ten to one hundred and sixty contractions of the abdominal segments per minute when the insect has been

* DEGEER appears to have made a similar observation, which he considered as expiration and inspiration, but the correctness of the opinion has been doubted by CARUS, vol. ii. p. 167. Mr. GOADBY has also noticed the contractions of the segments as analogous to acts of inspiration and expiration, but has not expressed an opinion respecting respiration being carried on chiefly through the anterior spiracles. See Medical Gazette, April 2, 1836.

much fatigued, while in the natural state, when the insect is undisturbed, the contractions of the segments, or acts of respiration, seldom amount to one half that number. In an extremely wild and irritable little bee, *Anthophora retusa*, STEPH., which dies from the most violent excitement and exhaustion in the course of an hour or two, after being captured and confined during summer, although plentifully supplied with food,—the acts of respiration are performed so rapidly that it is almost impossible to number them. On one occasion myself and a friend counted two hundred and forty in a minute.

The condition of respiration when an insect is recovering from a state of torpidity is very interesting. In the beginning of January, on a fine but cold windy day, upon examining one of my hives I found many bees which, having ventured abroad when the hive was disturbed, were lying torpid and completely motionless in a side box that was attached to the hive, when the temperature of the air was about 40° FAHR. I removed some of these to a room, the temperature of which was 60° FAHR., and they soon gave indications of reviviscence. The first visible signs of returning animation were slight twitchings of the tarsi, and feeble contractions of the abdominal segments, which gradually increased in frequency, but were at first very irregular. At two minutes after the first motions of the abdominal segments, and consequently after the acts of respiration were first perceived, the contractions gradually became more regular in their occurrence, and amounted to fifty-eight per minute. At the expiration of four minutes they amounted to sixty-three, at six minutes to seventy-two, at eleven minutes to eighty, at fifteen minutes to seventy-seven, at which time the insect began to move the whole of its limbs. At eighteen minutes they amounted to eighty-five, at twenty minutes to eighty-seven, at twenty-five minutes to eighty-four, at thirty-three minutes to one hundred and two, and at thirty-six minutes to one hundred and five; and when the insect was perfectly recovered, and had been for some time in a state of activity, the number of its respirations amounted to one hundred and sixty per minute. At thirty-three minutes the insect had regained its power of locomotion, and began to move about, and its respiration, which had then become more quiet and regular, was still more frequent than in a state of perfect health, when it seldom exceeds forty inspirations per minute.

It is in the pupa state that insects respire less frequently than in any other, and it is in this state that I have been able most distinctly to observe the action of the spiracles. When the insect has remained in the pupa state for a few weeks, in a low temperature, it passes into a complete state of hybernation, and its respiration, as I shall presently show, is almost entirely suspended; but when the insect has been kept in a temperature of 60° FAHR. or upwards, it respires very freely, and the action of the spiracles in the pupa of *Sphinx ligustri* may sometimes be observed by means of a microscope. There are in general about three contractions of the spiracles per minute, the intervals between which are very regular. A perfectly healthy and vigorous pupa always closes its spiracles whenever any irritating or obnoxious substance is brought into contact with them, which distinctly proves the possession of a voluntary power over the muscles connected with them, and which, as before shown, are supplied from the moto-sensitive columns.

Quantity of Respiration.

The very important fact established by Dr. Edwards in the higher animals, that a greater quantity of oxygen is required in the adult state in proportion to the capacity of the respiratory apparatus than in the earlier or infant condition of the animal, and that in a state of hybernation less even is required than in the infant state, is equally true as regards also the air-breathing Invertebrata, particularly the class of insects. The larva of *Ichneumon Atropos*, Steph., concealed within the body of the larva of *Sphinx ligustri*, Linn., and preying upon its substance, although provided with minute spiracles which lead into extremely delicate tracheæ distributed through its body, and doubtless maintaining a certain degree of respiration, requires not a twentieth part the amount of atmospheric air for its support which it requires in its perfect condition. The larvæ of the wild bank bees, *Anthophora retusa*, Steph., and *Eucera longicornis*, Steph., and of many others, confined in their cells in the earth; of the Sand Wasps and Spheges, buried deeply in the soil; of the *Cerambyces* and *Ptinidæ*, and other wood-boring insects; and of the *Scarabæi, Lucani, Tipulæ*, and *Muscæ*, many of which live in the most noxious and unaerated places, can exist for a very long time in situations in which the same insects in their perfect state would soon perish. Hence it is clear that a smaller quantity of air is required for the support of these insects while larvæ than when they have become perfect. The quantity of air required for the support of the same tribes of insects varies in like manner in the different species.

In the summer of 1832, at the suggestion of my friend Dr. Marshall Hall, I was led to inquire whether the quantity of respiration in insects bears any relation to the comparative irritability of the muscular fibre in the different genera and states of the same tribes of insects; and I was afterwards led to inquire more particularly into the exact amount of respiration in different insects, and different states of the same insects, for the purpose of ascertaining what relation, if any, subsists between the quantity of respiration and natural temperature of body in these animals. Although I was unable at that time to ascertain the exact amount of respiration in the different states and species, owing to various causes, such as the variations of the thermometer, the degree of excitement or quiescence of the insect, and consequently the uncertain amount of air consumed, and quantity of carbonic acid gas produced during the observation, yet the results gave a near approximation to the truth, and enabled me to form an opinion respecting the real amount in each observation when made under similar circumstances. The observations were made in the following manner. When a single specimen was employed the cubic bulk of the insect was first ascertained, and the insect was then confined in an accurately closed stoppered phial of known capacity, the time of day and temperature of the atmosphere being noted. After a certain period the stopper was carefully withdrawn under, and the phial was allowed to remain inverted in lime-water for about an hour. The quantity of carbonic acid gas, and consequently a near approximation to the real amount of respiration in a given time, was thus indicated by the absorption which took place,—proper allowance being made for the variations of the thermometer, and for other circumstances, which occurred during the period of observation. Although it must be evident that this mode of ascertaining the quantity of respiration is open to objections, it is sufficiently accurate to enable us to form a comparative view of the amount in different states and insects.

The quantity of respiration during a given period is very greatly influênced by the insects being either in a state of activity or quiescence, which explains the apparent discrepancy of some of the results, as shown in the accompanying Table.

Table I.

Exhibiting the quantity of carbonic acid gas excreted by different species of insects in their different states, and under different circumstances.

No.	Name of insect.	State.	Specimens.	Bulk.	Capacity of phial.	Hours inclosed.	Temperature.	Carbonic acid produced.	Remarks.
1.	Sphinx ligustri	Larva	1	0·13	1·96	5	69 to 71	0·430	During the day, soon after entering its last larva skin: August.
2.	Sphinx ligustri	Larva	1	0·15	3·16	5	69 to 71	0·431	During the day, in the same condition.
3.	Sphinx ligustri	Pupa	1	0·29	2·68	147	47·5 to 47·5	0·210	March 6, confined in the open air.
4.	Sphinx ligustri	Pupa	1	0·29	1·88	95	52·5 to 58	0·230	Confined in my sitting-room, March 6.
5.	Sphinx ligustri	Pupa	1	0·29	2·03	156	46 to 46	0·190	Confined in the open air, March 12.
6.	Sphinx ligustri	Pupa	1	0·29	1·94	156	46 to 46	0·190	Confined in the open air, March 12.
7.	Sphinx ligustri	Pupa	1	0·29	1·88	180	58 to 58	0·400	Confined in my sitting-room, Mar. 12.
8.	Sphinx ligustri	Pupa	1	0·29	2·34	194	61·5 to 59	0·345	March 25, has been long kept in high temperature.
9.	Sphinx ligustri	Pupa	1	0·29	1·88	194	61·5 to 59	0·310	Brought from exposure in open air to higher temperature.
10.	Sphinx ligustri	Pupa	1	0·28	1·89	210	48 to 48	0·235	Confined in open air on the ground, March 25.
11.	Papilio urticæ	Larva	3	0·09	1·14	12	70 to 66	0·120	Larvæ not full grown.
12.	Papilio urticæ	Larva	3	0·07	1·14	11	68 to 78	0·140	
13.	Papilio urticæ	Larva	3	0·07	1·14	7	65 to 74	0·110	
14.	Papilio urticæ	Larva	3	0·07	1·14	6½	75 to 69	0·140	Very active, July.
15.	Papilio urticæ	Pupa	5	0·10	1·96	48	67 to 78	0·130	Two days old.
16.	Papilio urticæ	Pupa	3	0·08	1·14	22	74 to 84	0·160	Three days old.
17.	Papilio urticæ	Pupa	3	0·08	1·14	10	74 to 69	0·050	During the night.
18.	Papilio urticæ	Pupa	3	0·08	1·14	10	69 to 69	0·040	During the day.
19.	Papilio urticæ	Pupa	3	0·08	1·14	10	67 to 67	0·041	During the night.
20.	Papilio urticæ	Perfect	3	0·07	1·14	16½	79 to 82	0·200	Four weeks old.
21.	Phalæna vinula	Pupa	1	0·11	2·34	257½	54 to 58	0·270	Just taken from its cocoon, March 6.
22.	Phalæna vinula	Pupa	1	0·11	2·68	194	61·5	0·363	Same specimen used, March 25.
23.	Phalæna vinula	Perfect	1	......	2·68	12	63 to 63	0·480	Active, April 23.
24.	Phalæna vinula	Perfect	1	......	2·68	12	63 to 63	0·490	More active, April 23.
25.	Bombus terrestris	Perfect	1	0·035	2·03	1	60	0·255	Just captured, active, April 6.
26.	Bombus terrestris	Perfect	1	0·055	2·03	1	60	0·345	Just captured and fed, very active.
27.	Bombus terrestris	Perfect	1	0·055	2·03	20	59	0·305	Resting during the whole observation.
28.	Bombus terrestris	Perfect	1	0·055	2·03	3½	60 to 59	0·120	Very slightly active.
29.	Anthophora retusa......	Perfect	1	0·023	1·88	1½	64	0·255	Just captured. May 6, active.
30.	Anthophora retusa......	Perfect	1	0·023	1·88	1½	66	0·330	Just captured. May 7, violently active.
31.	Geotropes vernalis......	Perfect	1	0·11	2·68	12	63 to 62	0·215	Active.
32.	Geotrupes vernalis......	Perfect	1	0·11	2·68	26	60	0·480	Active.
33.	Carabus cancellatus ...	Perfect	1	0·4	1·94	20	60 to 62	0·400	Moderately active.
34.	Carabus cancellatus ...	Perfect	1	0·4	1·94	54	60 to 60	0·430	Debilitated by fasting for three days.
35.	Chrysomela tenebricosa	Perfect	4	0·4	2·34	17½	58 to 57	0·340	March 9, active.
36.	Meloe violaceus.........	Perfect	1	0·4	2·34	25	63 to 63	0·190	A very small female, active.

Treviranus has very justly observed*, that the quantity, or I should rather say the activity of respiration in insects, is increased with the increase of atmospheric temperature. At the same time the law which has been established by Dr. Edwards with regard to the respiration of vertebrated animals, is equally applicable to insects, namely, that supposing the *activity* of respiration to be exactly of the same degree at two extremes of atmospheric temperature, say 32° Fahr. and 60° Fahr., there would be a greater quantity of oxygen consumed at the lower than at the higher temperature. It is necessary to bear all these circumstances in mind, and also the comparative size of the insect, in estimating the amount of its respiration. Thus the larvæ of Lepidopterous insects appear to respire a greater quantity of atmospheric air in a given time than the perfect insects or pupæ; but it must be remembered that they are in general very much larger in bulk, sometimes even double that of the perfect insect, and consequently consume a greater quantity of air. But if we examine larvæ which are of the same cubic bulk as their perfect insects, we shall find that, provided they continue in a state of activity, the respiration of the perfect insects will be much greater than that of their larvæ, as is shown in the observations on *Papilio urticæ*, Linn. in its different states, Table I., No. 11 to 20. It is there seen that the greatest amount of respiration is during the perfect state, and that the period when an insect which undergoes all its changes during a few weeks in the summer has the lowest degree of respiration, is about one or two days after it has entered its pupa state, as shown in the accompanying Table, No. 15. This is the condition of respiration during summer, when all the changes in the insect are continuous. The observations referred to in the Table were made upon larvæ which had not attained their full size, No. 12 to 14. But if a larva has arrived at its full size when the observation is made, it then appears to respire much more in a given time than the perfect insect. But this is not really the case. The observation is illusive, and arises, first, from the larva being almost always in a state of activity, and consequently having a more rapid consumption of oxygen, and next because it is frequently at least two thirds larger than the perfect insect. Thus the full-grown larva of *Odonestis potatoria*, Steph., is about ·26 of a cubic inch in bulk, while the perfect insect is not more than ·10. When the larva and perfect insect of this species were confined separately in glass stoppered phials of the same dimensions, 1·14, at the same temperature, 66° Fahr., the larva became asphyxiated in nine hours, while the perfect female was still living and vigorous at the expiration of twenty-four hours. In this case the larva was almost constantly in motion, while the perfect insect was quiet and resting. On a *primâ facie* view of this observation it would appear that the larva respires a greater quantity of air, compared with its bulk, in a given time, than the perfect insect. It is a well established fact, that among the higher animals respiration is at its minimum during sleep. It is neither so frequent nor so voluminous, and consequently there is less oxygen consumed. This has been long known with regard to the human species; but, as shown

* Lancet, vol. ii. 1835, p. 456.

by Dr. HALL*, is more decidedly the case in the hybernating Mammalia; and it is exactly like these in insects, in which, as will presently be shown, respiration is almost entirely suspended at certain periods.

It generally happens, that while we are making observations upon larvæ they are in constant activity, and consequently they then consume the greatest amount of oxygen; while the perfect insect, independently of its being two thirds smaller in bulk, is generally in a state of complete inactivity, at rest or sleeping, and then consumes only its smallest proportion of oxygen. Besides this, it is probable that the very confinement of the perfect insect in a given quantity of air, insulated from external currents and sudden changes of temperature, may induce a more complete state of rest, and thus be the means of reducing the respiration still lower than it would otherwise have fallen, and still further prevent the necessity for a renewal of atmospheric air in the phial. A female specimen of *Bombyx Caja*, whose cubic bulk was about ·09, confined in a phial of 1·14 capacity at a temperature varying from 63° to 71° FAHR., was still living and vigorous at the expiration of eighty-four hours; but during this time the insect was almost constantly in a state of inactivity. There is the same disposition in perfect butterflies (*Papiliones*) as in moths to become inactive when placed in confinement. After having been confined for a few minutes, and endeavouring to escape, they gradually become quiet, and their respiration is diminished. In order to prove distinctly that the quantity of respiration depends upon the degree of activity or quiescence of the individual insect, I confined a female *Bombus terrestris*, STEPH. (Table I. No. 28.), immediately after the insect was captured, in a glass-stoppered phial of about two cubic inches capacity, at a temperature of 60° FAHR. It continued in a state of violent activity, and in one hour evolved 0·345 of a cubic inch of carbonic acid gas; while the very same insect, when confined at nearly the same temperature (59° FAHR.) for twenty-four hours on the following day, during the whole of which time it was in a state of perfect rest, evolved only 0·305 of a cubic inch, which was not one twentieth part of the amount produced in a state of activity, although the insect had been fed immediately before commencing the observation.

The quantity of air deteriorated by an insect diminishes in proportion to the number of its respirations, and these diminish in frequency in proportion to the length of time it has remained in a state of quiescence. I had full proof of this during the above observation on the quantity of respiration of *Bombus terrestris*. Before noting the number of its respirations, the insect was allowed to remain at rest for about half an hour. At the expiration of that time the respirations were only fifty-eight per minute, and these were deep and laboured. At the expiration of one hundred and forty minutes, during the whole of which time the insect had remained at rest, its respirations were at the rate of only forty-six per minute: these were laborious and feeble, like those of an animal sinking into profound sleep. At the expiration of one hundred and eighty minutes, the respirations were no longer perceptible. Now in

* Philosophical Transactions for 1832, Part I.

this very insect, soon after it was captured, the number of its respirations, in a moderate state of excitement, amounted to from one hundred and ten to one hundred and twenty per minute. I have recently observed the same difference between the number of respirations in a state of activity and quiescence in a female specimen of *Sphinx ligustri* in the perfect state. After the insect had been considerably excited in flight, it respired at the rate of forty-two per minute; but when it had remained at rest about seventy-five minutes, its respiration had subsided to only fifteen per minute.

This state of quiescence or profound sleep is the condition into which most insects fall at the close of summer, and in which they remain in their hybernacula during winter, when, if they be not disturbed, respiration becomes almost entirely suspended. This is the state of true hybernation. LYONET has stated his belief that the respiration of pupæ is *entirely* suspended for a very great length of time during winter; but his experiments with the pupæ of *Sphinx ligustri*, which led him to this statement, and which were made by merely covering the spiracles with soap-water, and watching with a microscope for the rising of bubbles, do not seem sufficiently precise and accurate to warrant the conclusion. For the purpose of ascertaining this fact, I made a number of observations in the year 1829 upon the pupa of the Sphinx, and have since repeated them under different circumstances. There are different degrees of respiration at the same season of the year in pupæ of different insects, which appear to have reference to the conditions in which the insects are placed in their natural haunts. When the *Sphinx ligustri*, which passes its winter in the earth, is examined in October or November, it gives most decisive proofs of respiration in the production of carbonic acid gas; but this is much smaller in quantity than that which is produced at the same period in a given time, and under similar circumstances, by the pupa of *Pavonia minor*, STEPH., which passes its winter in the open air, and is more readily exposed to the varied influences of the seasons. In both these insects the quantity of respiration is diminished as the winter advances. In December and January, respiration has subsided to its lowest state, and can be detected only with great difficulty while the insect remains undisturbed, but it does not entirely cease; for if at that period the pupa be brought into a warm atmosphere of 45° FAHR. or upwards, it soon begins to respire more freely, and if placed in water or alcohol, a string of bubbles will be expired from the spiracles at each contraction of the segments; thus proving that a more powerful respiration is immediately induced when the insect begins to be aroused from its hybernating slumbers. It is only when the medium in which the insect is living is below 32° FAHR. that respiration is very nearly suspended. On the 1st of January 1836 I repeated my observations, which had originally been instituted in the winter of 1829. I removed four pupæ of *Sphinx ligustri* from the ground, in which they had remained undisturbed for several weeks, and placed them in glass-stoppered bottles, three in one bottle and one in another, and buried them in the earth about four inches below the surface. The temperature of the soil at that depth was 37°·5 FAHR., and of the atmosphere a few inches above the ground 42°·8 Fahr. For nearly a month before this the pupæ had been subject to the common influences of the season: there had been severe frost, the thermometer having sometimes sunk down to 20° Fahr. At the expiration of twenty-four hours the phials with the pupæ were removed from the ground, and their gaseous contents very carefully tested with pure lime-water, and gave the usual most unequivocal signs of respiration having taken place, but only in a slight degree. The temperature of the air a few inches above the ground was only 31° Fahr., that of the soil four inches beneath the surface 35°·2 Fahr.; so that it was clear the pupæ had continued to respire to within two or three degrees of the freezing-point, and perhaps even at very nearly that temperature. That the pupæ were still living and active, was proved by one of them once or twice contorting its abdomen when removed from the earth, before the stoppers of the bottles were withdrawn. The pupæ were then buried again as before, the temperature of the atmosphere and of the soil continuing respectively at 35°·2 and 31° Fahr. I then exposed another pupa in a stoppered phial on the surface of the ground for twenty-four hours. At eight o'clock on the following morning the thermometer stood at 16°·5 Fahr.; so that the pupa was then supporting a temperature of 15°·5 Fahr. below freezing. At the expiration of the twenty-four hours the temperature of the atmosphere had again risen to 31°·2 Fahr. and the phials were again examined. The temperature of the soil four inches below the surface was 33°·5 Fahr. In No. 3, which had been exposed on the surface of the ground, there was only the very faintest trace of carbonic acid gas, but sufficient to satisfy me that the pupa had respired. The phials Nos. 1 and 2 were then examined. In both there were incontestible proofs of the presence of carbonic acid gas; thus clearly indicating that the pupæ had respired freely at a temperature of 33°·5 Fahr., and in a slight degree even below 32° Fahr.

Although the pupæ employed on the above occasions had borne so low a temperature, they were not injured, since the whole of them have produced perfect insects*. That a pupa which has been constantly exposed, for forty-eight hours, to a temperature of five degrees below freezing does not become frozen, I am fully satisfied, having once made a very careful examination in order to ascertain this point. The pupa was taken from its exposed situation with a pair of forceps, in order that it might not be touched with the fingers, and have its temperature increased, and a horizontal incision was instantly made with a sharp scalpel through the posterior part of its body, which separated the dorsal from the ventral surface. All the parts immediately collapsed when exposed to the open air, and the muscles were almost as tense as during a state of activity; the fat was of its usual whiteness, and the dorsal vessel was exactly as it appears during any other period, excepting that it was a little contracted in diameter, but its contents were fluid. When the body of the pupa was cut through, the fluid flowed as usual; but I could not observe any motion of the dorsal vessel, nor any

* June 1836.

dilatation of the tracheæ; so that the circulatory and respiratory motions must have been very nearly, if not entirely, suspended. After the pupa had been held between my fingers for a few seconds, there was a slight contraction of the longitudinal muscles, resembling a respiratory effort, and I observed a motion commencing like the peristaltic motion among the viscera. I thought I could also observe a slight motion in the dorsal vessel. All this distinctly proved that the pupa had not been frozen. It is thus certain that a very great degree of cold can be borne by these insects without injury, and that during the time it is borne the respiration of the insect is very nearly suspended. But it is not merely a great degree of cold that can be borne by these insects without injury, but a great and sudden change of temperature, from a comparatively warm to a very cold atmosphere, as was shown in the observation on No. 3, above noticed; and even during that state the pupa will respire until the temperature has sunk below 32° Fahr. It is probable, that when the pupæ are remaining entirely undisturbed in their natural haunts, they respire much less, and that if the suspension of respiration really does take place, when it has once occurred it continues much longer than when they are removed from the soil and disturbed for the purpose of experimental observation, just as the sleep of the dormouse or bat will continue until the near approach of summer, although the animal is easily roused, and its respiration excited by external causes, even in the midst of winter. Yet it must be remarked, that when this takes place, whether the animal be one of the Mammalia or an insect which has arrived at its perfect state, it soon relapses again into its previous condition.

In order to ascertain the comparative amount of respiration in the same species of insect at the same period of the year in different degrees of temperature, I confined two pupæ of *Sphinx ligustri* in glass-stoppered bottles, inverted in a vessel of lime-water, and placed them on the ground in the open air, protected from the influence of the sun, and allowed them to remain for one hundred and fifty-six hours. During this time the temperature of the atmosphere was never lower than 35° Fahr., nor higher than 58° Fahr., being a range of 23°·6 Fahr. The temperature of the air at the time of inclosing the pupæ was at 46° Fahr., and it was exactly the same when the contents of the bottles were examined. The amount of carbonic acid gas produced by each of these pupæ was 0·19 of a cubic inch. This was between the 12th and 20th of March. At the time of inclosing these pupæ, I inclosed also another, which had previously been kept under precisely similar circumstances. This specimen was placed in my room, where the temperature during night was never lower than 45°, and during the day not higher than 60°, being a range of fifteen degrees. At the time of inclosing this pupa the temperature of the room was 58°, and it was at the same standard when the bottle was examined, which was at the expiration of a hundred and eighty hours. The quantity of carbonic acid gas amounted to 0·40 of a cubic inch, being nearly double the amount produced by either of the pupæ which were exposed to the open air; thus clearly proving that the relative quantity of respiration in insects, as Treviranus has recently remarked *, very much depends upon the temperature of the air inspired, and also on the state of quiescence or activity in which the insect has been living. I made also other trials with other pupæ. I inclosed one pupa which had remained during the whole winter in the open air, and one which had been kept for several weeks in my room, in two stoppered phials, when the temperature of the atmosphere was 61° Fahr., and allowed them to remain for one hundred and ninety-four hours. During this time the temperature of the atmosphere varied scarcely more than eight or ten degrees, and both phials were examined at a temperature of 59° Fahr. The first specimen, which had remained in my room, produced 0·345 of a cubic inch of carbonic acid gas, while the other, which had been brought from the open air, produced only 0·310; a difference of thirty five thousandths less in the insect which had been exposed. Hence it is clear that respiration is less perfectly performed in those insects which are only newly aroused from their state of hybernation than in those which have been long kept in a state of excitement.

The amazing difference which exists in the quantity of respiration of pupæ and of perfect insects is strikingly exemplified in *Cerura vinula*, Steph. (the Puss Moth). This insect, which it is well known is inclosed in a hard and impervious cocoon in the open air during its pupa state, is an admirable subject for experiment. On the 25th of March I inclosed a pupa, which had previously been several days removed from its cocoon, and consequently aroused from its hybernation, in a phial at a temperature of 61°·5 Fahr. At the expiration of one hundred and ninety-four hours it had produced only 0·363 of carbonic acid gas, which is considerably less than two thousandths per hour. On the 23rd and 24th of April I confined the same insect, twenty-four hours after it had escaped from the pupa state, and had become perfect, for twelve hours, at two separate times, when the temperature of the atmosphere was 63° Fahr. In one experiment it produced 0·480, and in the other 0·490 of carbonic acid, or at least forty thousandths per hour, and even during part of that time the insect was not in a state of activity.

In will thus be seen that the quantity of air deteriorated by an insect is regulated by various circumstances, independently of the natural habits of the species. When the pupa is in a state of complete hybernation, the respiration of the insect is at its minimum, while in the perfect insect, in a state of great activity, it is at its maximum. It is also evident that in making our observations the state of quiescence or activity, and the comparative bulk of the insect, in its different conditions, should be particularly attended to, or we may be led into errors whenever we attempt to compare the quantity of its respiration in its different states.

* As noticed in the Lancet, vol. ii. 1835, p. 456.

Duration of Life in different Media.

An important subject connected with respiration is the capability which insects possess of supporting existence for a certain time in different media. It is a task of great difficulty to ascertain the precise length of time which different insects can continue in noxious media without being destroyed. We are so little aware of all the circumstances which affect the respiration of insects, that it is almost impossible to ascertain the precise moment when they cease to respire, or become asphyxiated; and it is still more difficult to be certain at what period, when respiration is suspended, life becomes extinct. We must therefore, in our experiments upon this subject, assume certain data, from which the comparative duration of life, under certain circumstances, may be inferred. With this view I have assumed four data, which mark very distinct conditions of respiration. The first is that moment at which, when confined in the noxious medium, the insect, by the violence of its struggles and efforts to escape, begins to appear to respire with great difficulty, and is becoming asphyxiated. The second is the moment at which it can no longer be observed to give signs of life by moving its limbs or the segments of its body, and when it may fairly be supposed that respiration is entirely suspended. The third is that moment at which, after being removed from the noxious medium, and exposed to the open air, the insect begins again to revive, its reviviscence being indicated by motions of its limbs or other parts of its body. The fourth is marked by the period when the insect is so far sufficiently recovered as to be again capable of locomotion. By comparing these circumstances in different insects, we obtain a comparative knowledge of the state of respiration as affected by different media.

In the months of July and August 1832, I made a series of observations on different insects with these assumed data, and I then found that the order in which the vitality of insects appears to be affected in different media is as follows: hydrogen, water, carbonic acid, nitrous acid gas, chlorine, and cyanogen, as shown in the accompanying Table; and I have since repeated the observations on several species of insects with similar results.

Table II.

Exhibiting the Comparative Duration of Life of various Insects in different Media, expressed in Seconds, Minutes, and Hours.

Name of Insect.	Temperature of the atmosphere.	Atmospheric air.				Hydrogen.				Water.				Carbonic acid.				Mixed gases, Nitrous acid, Sulph. hydrogen, &c.				Chlorine.			
		Cubic bulk of insect.	Bulk of air.	Dying.	Motionless.	Dying.	Motionless.	Recovering.	Recovered.	Dying.	Motionless.	Recovering.	Recovered.	Dying.	Motionless.	Recovering.	Recovered.	Dying.	Motionless.	Recovering.	Recovered.	Dying.	Motionless.	Recovering.	Recovered.
	°			h m	h m	m s	h m s	m s	m	m s	m s	m s	m s	m	m s	m	m	m s	m s	m s	m	s	m	h	
ranea diadema	......	......	......	......	......	0 15	0 14 0	......	45																
eotrupes stercorarius	......	......	......	......	......	30 0	6 20 0	3 0	60	6 30	16 0	2 0	30 0	6	60 0	still	living.	0 30	2 0	......	30	20	2	12	
occinella 7-punctata	64	·03	·14	......	8 0	......	....	......	......	0 30	12 0	0 3	3 0												
occinella 7-punctata	......	......	......	......	......	......	....	......	......	0 30	9 0	......	3 0												
occinella 7-punctata	......	......	......	......	......	1 30	0 3 0	0 3	......	0 30	9 0	......	3 0												
hrysomela tenebricosa	......	......	......	......	......	4 0	0 14 0	0 30	6																
hrysomela tenebricosa	......	......	......	......	......	5 0	0 11 30	0 40	5	0 25	45 0	......	......	2	7 0	1	45	0 30	3 30	0 30	30	30	3		
hrysomela tenebricosa	......	......	......	......	......	6 0	0 11 30	1 0	3	......	......	......	......	2	45 0	......	......	1 0	3 30	30 0	35				
urculio..................	......	......	......	......	.....	10 0	0 12 0	1 0	7	0 30	13 0	10 0													
arabus emarginatus...	......	......	.. ...	......	......	1 0	0 4 0	1 0	2	......	......	......	......	1	2 30	2	10	1 0	3 30	15 0	25				
crida aptera (female)	61·5	·04	·14	0 35	1 37	0 25	0 3 0	0 30																	
crida aptera (male)...	61	·03	·14	0 45	2 48	0 20	0 2 0	5 0	15	2 0	2	......	20 0												
crida viridissima......	64·5	·10	1·14	......	7 15	0 10	0 3 15	1 0	14																
crida viridissima......	62	·10	1·14	......	7 0	0 10	0 3 0	0 30	8	0 5	3 0	0 30	0 12												
anessa urticæ (larva)	......	·02½	·14	4 0	7 0	0 30	0 2 30	......	3	1 0	6 0	0 10	20 0	5	0 18	......	60	5 0	0 30						
anessa urticæ (larva)	......	·02½	·14	1 0	2 5	1 0	0 3 0	......	3	1 30	6 0	......	20 0	5	20 0	......	60	5 0	0 30						
ombus subterraneus	......	......	......	......	......	0 15	0 2 45	0 30	7																
ombus subterraneus	......	......	......	......	......	0 30	0 5 0	0 30	10																
ombus sylvarum......	......	......	......	......	......	0 15	0 2 30	0 30																	
espa parietina.........	......	......	......	......	......	0 30	0 3 0	0 5	4																
elophilus tenax	......	......	......	......	......	1 0	0 4 45	......	1																

Some of these media affect respiration much sooner than others, which eventually are more fatal to the insect. Thus the larva of *Papilio urticæ*, Linn., gave indications of life much longer in carbonic acid gas than in hydrogen, but was much longer in recovering from the pernicious effect of it when again exposed to the open air; while, on the contrary, the perfect insect became motionless much sooner in carbonic acid than in hydrogen. The larvæ of this insect were also much longer in recovering from submersion in water than from confinement in hydrogen, from the effects of which they began to recover immediately they were exposed to the open air. I invariably found that if hydrogen be diluted with only a very small proportion of its volume of atmospheric air, it is capable of being respired by insects for many hours. Insects generally recover from the effects of confinement for several hours in hydrogen, or water, upon exposure to the open air, although they may appear to have been completely destroyed. These media exert no noxious influence whatever upon the insect, but asphyxiate exactly as they affect vertebrated animals, simply by the absence of oxygen. Water, however, appears to have a twofold effect upon the insect, first by the absence of oxygen, and next by its depriving the insect of its natural heat, and lastly by the great degree of cold, or further abstraction of heat, produced by the evaporation which takes place from the surface of the insect when again exposed to the air for recovery. This latter circumstance may perhaps account for the greater length of time which elapses before reviviscence takes place after confinement in water than after confinement in hydrogen. A larva of *Papilio urticæ*, Linn., which had been confined in hydrogen for more than twelve hours, began to revive in the course of two or three hours when again exposed to the open air, although it did not entirely recover its locomotive powers in a much longer period. A larva of the same species perfectly recovered in half an hour after being submerged in water for more than two hours. The larva of the common Drinker Moth (*Odonestis potatoria*, Steph.) perfectly recovered in one hour and a half, after being submerged for two hours and a half. The perfect female of the same species, after submersion for a similar length of time, perfectly recovered in less than an hour at a temperature of 73° Fahr. But this speedy reviviscence does not take place after confinement in carbonic acid, nitrous acid, and chlorine gas. Carbonic acid does not affect the respiration of insects so immediately as water, but ultimately it suppresses it much sooner; and although the individual gives signs of reviviscence rather sooner after confinement in carbonic acid than after submersion in water, it is much longer before it has completely recovered from its effects. Nitrous acid gas and chlorine seem at first to affect respiration about in the same degree, at least the symptoms are generally first apparent in about the same length of time. The first effects produced upon insects by these gases are generally observable in from five to ten seconds, although it is from fifteen to thirty seconds before the insect begins to make violent efforts to escape, while it seldom continues to give signs of animation for longer than from two to three minutes and a half. But the secondary effects of these gases are different.

Insects generally recover, although very slowly, after confinement in carbonic acid for a few minutes; but they very seldom recover after confinement in a mixture of sulphurous and nitrous acid gas, or chlorine, which appear to affect them as direct and specific poisons. The rapidity with which these gases affect the respiration of insects depends upon the peculiar habits or natural constitution of the species. Thus insects accustomed to inhabit the open atmosphere, Lepidoptera and Hymenoptera, are affected almost instantaneously and perish quickly by these gases, while those which are accustomed to inhabit noisome places, as the Carabi and Geotrupes, are either not affected, or recover from their effects much sooner. A specimen of *Geotrupes vulgaris*, Steph., when confined in a mixture of sulphurous and nitrous acid gas, was visibly affected in thirty seconds, and apparently completely asphyxiated in two minutes; but on being removed immediately to the open air, it was completely recovered in from twenty-five to thirty minutes. But an individual of the same species, when confined in chlorine, was powerfully affected in less than twenty seconds, and became completely motionless in two minutes; and although it was immediately removed to the open air, it hardly gave any signs of life for more than twelve hours afterwards, and even then it did not ultimately recover. We have thus a distinct proof of the poisonous nature of these gaseous bodies, and of their comparative virulence, and that the respiration of insects is affected by them in precisely the same manner as the respiration of vertebrated animals, the only apparent difference appearing to arise from a peculiar habit of body which resists their effects for a longer or shorter period. But the most deadly of all media is hydrocyanic acid in a state of vapour, admixed with atmospheric air. If an insect be confined over the fumes of hydrocyanic acid, it perishes almost instantaneously if the gas be powerful, but if only a small quantity be mixed with atmospheric air, the insect is paralysed for a time, but will ultimately recover. This difference between the effects of hydrocyanic acid gas and chlorine is very interesting. The instantaneous manner in which hydrocyanic acid gas, or rather cyanogen, destroys life and suppresses every act of respiration and volition, when respired by the insect, sufficiently proves that it cannot be by its admission into the circulation of the body, and that its being received into the system of tracheal tubes is sufficient to enable it to act upon the nervous system instantaneously. Even those insects which in every other medium are exceedingly tenacious of life, even in the deadliest, chlorine, perish in an instant in cyanogen. The insect dies in a tetanic state of contraction of all the muscles of the body. Chlorine, on the contrary, appears to kill by producing in the first place a rigid spasm of the respiratory organs, and a congested state of the mucous membrane, which renders respiration at first difficult and at length impossible.

From these circumstances I have been led to conclude that the manner in which the respiration of insects is affected by noxious media is the same as in Vertebrata, and that life is destroyed by them in precisely the same manner in both divisions of the great kingdom of animated nature.

Having thus considered the means which are employed in the function of respiration in insects, the manner in which it is performed, the quantity of respiration under different circumstances, and its duration in different media, it remains only to notice the relation which these bear to the volume of the structures concerned in the different states and species.

We have seen that in the larva state the respiratory organs in most species are very small, and that it is only in the perfect condition that they acquire the maximum of development. But in the pupa, or intermediate state, these organs are much larger than in the larva, yet the insect requires a much smaller quantity of air for its support in a given time. Hence it follows that although the organs concerned are of greater volume, the activity of respiration is diminished; so that the pupa is enabled to endure the effects of noxious media, or the privation of air, much longer than the larva, and the larva, as in the case of many Hymenopterous insects, longer than the perfect individual. The larva of an insect is analogous to the child, or new-born offspring of the mammiferous animal, and the analogy is the most perfect in its earlier condition. The pupa state bears a relation to the whole life of the insect similar to that which the hybernating condition bears to the life of the hybernating animal. In that state the volume and velocity of the circulation are diminished; the temperature of the body (which I hope to have the honour hereafter of proving to the Society is always higher than the surrounding medium in insects in a state of activity,) is then scarcely, if at all, above that of the atmosphere, and the respiration, as we have seen, is almost entirely suspended. This is exactly the condition of the hybernating Mammalia. When the insect awakes from its pupa state through the influence of external stimuli, its respiration, circulation, and temperature are all increased, and its capability of supporting existence in noxious media is diminished. It is gradually developed into the perfect animal, takes on itself the active duties of its existence, continues its kind, and dies. But even during this its perfect condition it occasionally passes into the hybernating state, which I shall consider more particularly on a future occasion.

Description of the Plates.

PLATE XXXVI.

Fig. 1. Part of a tracheal vessel of the larva of *Papilio brassicæ*, Linn., exhibiting the spiral fibre (*a*) and the external or serous membrane (*b*).

2. The respiratory organs contained within the abdomen of *Bombus terrestris* (Humble-Bee, magnified 10 diameters).

(*a*) Two large brown tracheæ, which pass through the petiole which connects the abdomen with the trunk, and are dilated in the first segment into the large vesicles (*f*): (*b*) small transverse connecting tube, which gives off a pair of small tracheæ, which, with two others from the large tracheæ, are developed into a very large, superior, transverse vesicle, which lies above the lateral ones in the abdomen: (*d, e*) two longitudinally directed tracheæ, which pass on each side of the œsophagus; (*e*) passes down to the lower part of the proventriculus, and distributes recurrent branches, (*d*) passes no lower than the anterior part of the proventriculus: (*g*) funnel-shaped transverse tracheæ, which pass beneath the muscles in the under surface of the body: (*i, k*) origins of similar vessels, which pass over the dorsal surface of the abdomen (*h*): (*i*) trachea to the duodenum or ventriculus, (*k*) to the small intestines, (*l*) to the colon and organs of generation.

Fig. 3. A portion of the colon and cæcum from the perfect insect *Phalæna vinula*, Linn., laid open to show the ramifications of the tracheæ in the adipose membrane (*a*), and that they do not ramify in the internal glandular or mucous membrane, a part of which is reflected separately (*b*). Magnified 10 diameters.

4. A portion of the mucous lining of the colon, studded with glands, and separated from the other structures. Magnified 24 diameters.

5. One of the antennæ of *Papilio urticæ*, Linn. removed from the insect immediately after changing to the pupa state, and viewed by transmitted light, and very highly magnified to show the beautiful distribution of the tracheal vessel.

6. The exterior spiracle with its converging fibres. Magnified 25 diameters.

7. The internal spiracle, with its valves, muscles, and nerves. Magnified 25 diameters. (*a*) part of the torn trachea; (*b*) the posterior valve; (*c*) the retractor valvulæ; (*d*) the circular sphincter muscle; (*e*) the retractor spiraculi.

PLATE XXXVII.

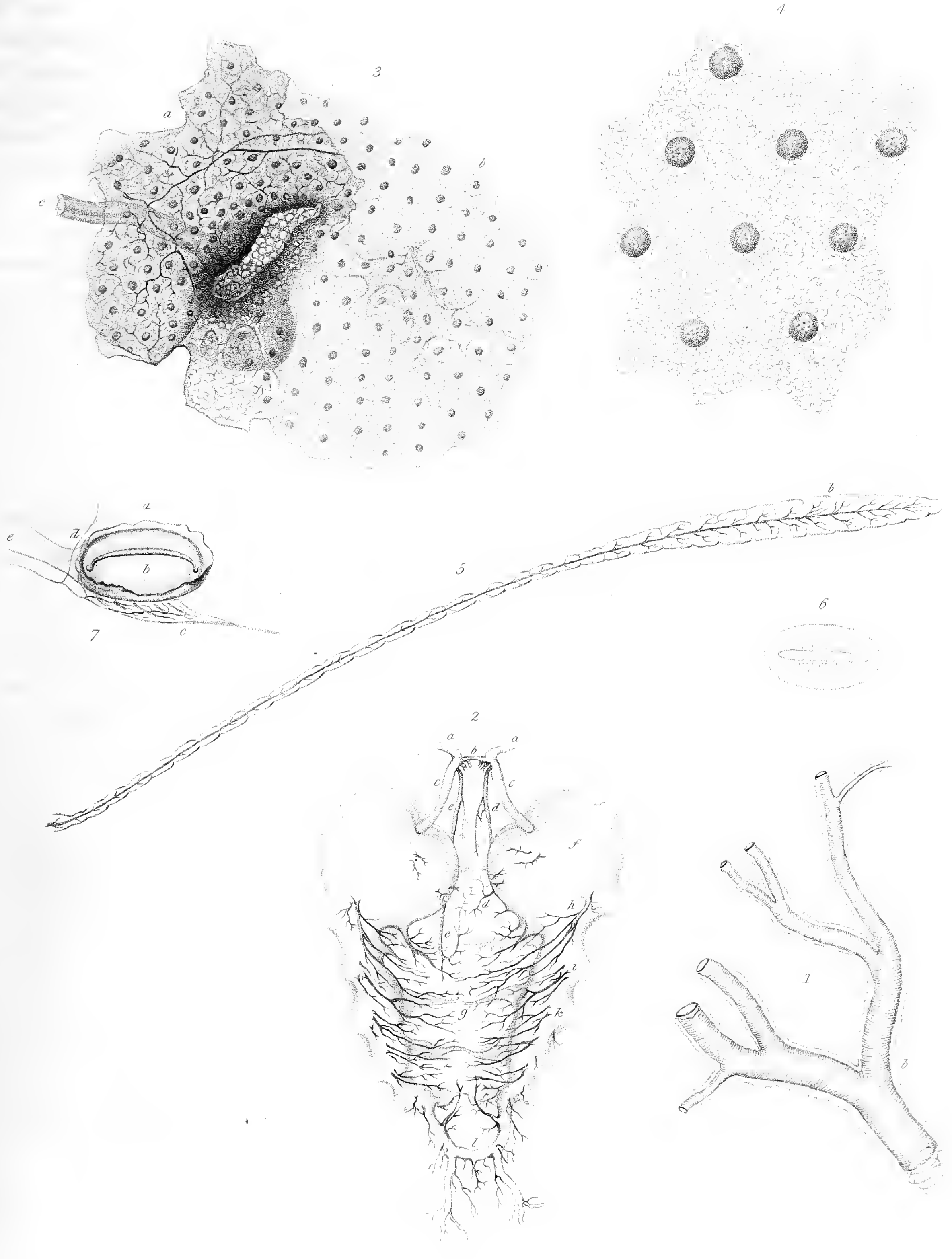
Represents the muscles, tracheæ, and nervous system of the posterior part of the body contained in the eighth, ninth, tenth, and eleven segments, magnified.

A. One half of the visceral surface of the eighth segment, with the muscles, nervous cords, and tracheæ *in situ*.

B. The visceral surface of the ninth segment, with the recti muscles of the right side of the body removed, and the recti and first and second oblique on the left.

C. The ventral surface of the tenth segment, with the larger and smaller recti and the four oblique muscles removed to show the transverse and spiracular muscles, and the third rectus.

D. The eleventh segment, containing the double ganglion and termination of the nervous columns.



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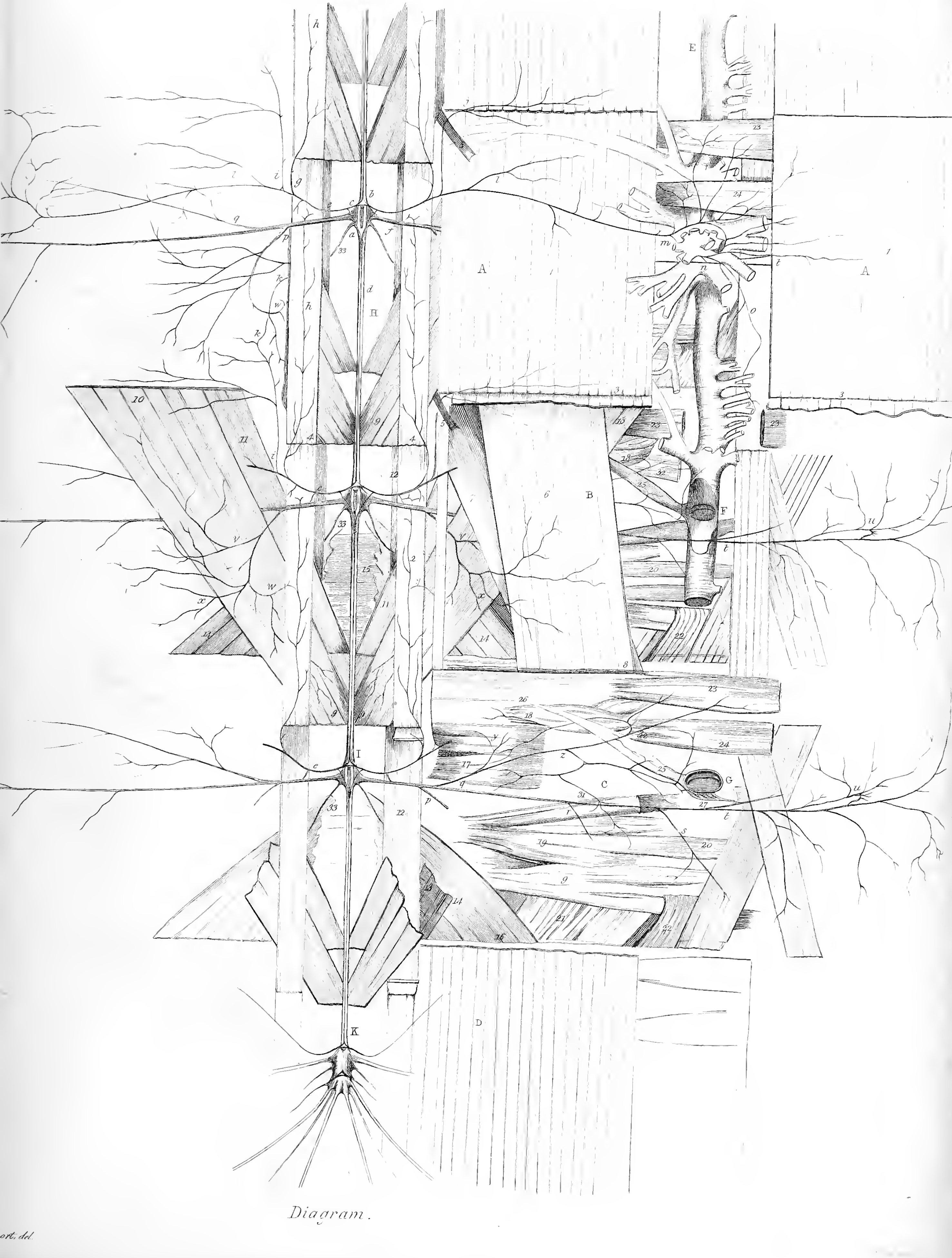


Diagram.

ort. del.

Muscles.

Fig. 1. 1. The great dorsal and ventral recti.
2. The smaller rectus.
3. The middle line of insertion for the attachment of the greater recti and three oblique.
4. The posterior or third line of insertion for the smaller recti and third oblique.
5. The retractor of the stomach.
6. The first oblique.
7, 8. The second oblique.
9, 10. The third oblique.
11. The fourth oblique.
12. The third rectus.
13. The fifth oblique.
14, 16. The triangularis muscle.
15. The transverse median muscles.
17. The transverse abdominal muscles.
18. The anterior abdominal.
19, 20. The lateral abdominal.
21. The sixth or posterior oblique.
22. The seventh or postero-lateral oblique.
23. The first transverse lateral muscle.
24. The second transverse lateral muscle.
25, 26. The retractor of the spiracle.
27. The retractor of the valve of the spiracle.

E. The longitudinal trachea of the right side, with its ramifications.
F. A portion of the trachea cut away to show the entrance of the spiracle.
G. The spiracle with the nerves and muscles belonging to it.

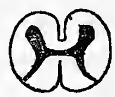
Nerves.

H, I, K. The nervous column of the segments, exhibiting the ganglia of the sensitive tract traversed longitudinally by the motor tracts of the cords which terminate upon the last or double caudal ganglion, by dividing each into two portions, one of which is given to each of the four terminal nerves. It exhibits also the transverse nerves, forming a triangular plexus above the cords anterior to each ganglion, and passing laterally on each side to distribute their branches on the visceral surface of the muscles.

a. The ganglion and transverse nerves.
b. The transverse plexus.

followed the anterior and lateral columns of the cord solely to the cerebrum, and the posterior columns exclusively to the cerebellum. Consequently the corpora restiformia or processus e cerebello ad medullam oblongatam have been described as consisting entirely of fibres from the posterior columns*.

The author having been able, by repeated dissections of the brain previously hardened by long-continued immersion in alcohol, to satisfy himself of the existence of certain fibres, which have hitherto escaped observation, ascending from the anterolateral columns of the spinal cord to the cerebellum, will proceed to detail their course and arrangement.

In order to execute this portion of his task with clearness, he feels it necessary to refer to the composition of the cord, as demonstrated by a transverse section. It will then be seen that the cineritious neurine deposited in the interior of the cord is arranged on each side, so as to form two semicircles, with their convexities opposed, and attached by a transverse bridge thus,  the posterior peaks alone reaching the surface of the cord. This last-mentioned arrangement of the gray matter, it will be seen, actually divides each side of the cord into two distinct columns. The posterior portion is the true *posterior column*, and the line of demarcation is distinct on the surface without a transverse section, in consequence of the posterior roots of the spinal nerves emerging at that point. All that portion of the cord which is anterior to this posterior lateral fissure not being divided in a similar way may be called the *antero-lateral* column.

From the antero-lateral column of the cord there are two sets of fibres ascending to the cerebellum, one from the anterior portion of the antero-lateral column, the other from the posterior. The posterior set of fibres are separated from the posterior columns by the posterior peaks of gray matter; and judging from the fact that the sensory division of the fifth pair of nerves is continuous with these fibres, it appears most probable they form part of the sensory tract. See Plate XXXVIII. fig. 2. I.

The anterior set, the *cerebellic fibres of the anterior columns* †, proceed from the front and sides of the cord, continuous therefore with the true motor tract. A portion of them may frequently be seen without dissection, and have been adverted to by several writers under the title of arciform filaments, though their termination in the cerebellum remained undiscovered. See fig. 1. E.

These *cerebellic fibres of the anterior columns*, opposite the decussation of the *py-*

* Mr. Herbert Mayo is, I believe, the only author who points out the fact that the restiform bodies are partly formed by some fibres from the posterior part of the cord, but anterior to the posterior lateral fissure, and therefore not solely by the true posterior columns. In the second edition of his Outlines of Physiology, p. 273, he says, "On cutting through and stripping down the corpus restiforme, it is found to carry with it the posterior lateral furrow."

† The fibres whose existence this paper is intended to demonstrate.

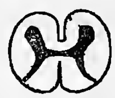
ramidal bodies, are separated from the posterior fibres of the antero-lateral columns, already described as ascending to the cerebellum, by fibres which occupy a place in the middle of the side of the cord. These fibres, which subsequently ascend through the pons Varolii to the cerebrum, and form the upper portion of the crus cerebri, have lately been described by Sir Charles Bell as a portion of the tract of sensation. The *cerebellic fibres of the anterior columns,* as they ascend to the cerebellum, pass principally below the olivary bodies, sometimes crossing the lower border of these bodies, while others which are deeper seated pass to the inner side of them. The whole, during their ascent to the cerebellum, cross to the outer side of the tract of sensation above referred to and sensory root of the fifth pair of nerves, and then, plunging into the substance of the corpus restiforme, interlace with the true posterior columns of the cord, and finally terminate in the cerebellum. See fig. 2.

These fibres, whose importance to the physiologist as proving unequivocally the existence of a complete communication between the motor tract of the spinal cord and the cerebellum need not be dwelt on, are most easily demonstrated in the following way: Let the posterior column be separated from the antero-lateral column, at the posterior lateral fissure, about two or three inches below the pons Varolii; and subsequently draw very carefully the posterior column, thus split from the anterior, up towards the cerebellum. The rent in the cord, which tears smoothly till it reaches the lower edge of the corpus restiforme, is there arrested by the *cerebellic fibres of the anterior columns,* unless too much force has been used, in which case they are easily torn through, and escape observation. They may be likewise shown by making the rent in the antero-lateral column itself, exactly at the centre of the lateral face of the cord, thus dividing the cord into two halves, an anterior and a posterior, and then continuing the rent in the same way. Again, these fibres may be shown by tearing up that portion of the front of the cord which is anterior to the anterior roots of the spinal nerves, which portion will be found, at the point where the anterior columns decussate, to split into three sets of fibres: one set of fibres cross to the opposite side; a second run to the inner side of the corpus olivare; a third set, which are not numerous, run below and to the outer side of the corpus olivare, and, ascending to the cerebellum, constitute a portion of the fibres in question.

If the sensory root of the fifth pair of nerves be traced through the pons Varolii, and the fibres which lie to the outer side of it in the medulla oblongata examined with care, they will be found connecting the anterior portion of the cord with the cerebellum; or, in other words, they will be found to be the *cerebellic fibres of the anterior columns.* The surface of these fibres is represented in fig. 1. E. as exposed by simply raising the pia mater, and carefully scraping the surface in a portion of the medulla previously hardened by alcohol. The deeper-seated are represented divided just at the point where they cross the sensory root of the fifth pair of nerves, which nerve is thus exposed to view in fig. 2. E. the corpus olivare having been raised from its natural position.

followed the anterior and lateral columns of the cord solely to the cerebrum, and the posterior columns exclusively to the cerebellum. Consequently the corpora restiformia or processus e cerebello ad medullam oblongatam have been described as consisting entirely of fibres from the posterior columns*.

The author having been able, by repeated dissections of the brain previously hardened by long-continued immersion in alcohol, to satisfy himself of the existence of certain fibres, which have hitherto escaped observation, ascending from the antero-lateral columns of the spinal cord to the cerebellum, will proceed to detail their course and arrangement.

In order to execute this portion of his task with clearness, he feels it necessary to refer to the composition of the cord, as demonstrated by a transverse section. It will then be seen that the cineritious neurine deposited in the interior of the cord is arranged on each side, so as to form two semicircles, with their convexities opposed, and attached by a transverse bridge thus,  the posterior peaks alone reaching the surface of the cord. This last-mentioned arrangement of the gray matter, it will be seen, actually divides each side of the cord into two distinct columns. The posterior portion is the true *posterior column*, and the line of demarcation is distinct on the surface without a transverse section, in consequence of the posterior roots of the spinal nerves emerging at that point. All that portion of the cord which is anterior to this posterior lateral fissure not being divided in a similar way may be called the *antero-lateral* column.

From the antero-lateral column of the cord there are two sets of fibres ascending to the cerebellum, one from the anterior portion of the antero-lateral column, the other from the posterior. The posterior set of fibres are separated from the posterior columns by the posterior peaks of gray matter; and judging from the fact that the sensory division of the fifth pair of nerves is continuous with these fibres, it appears most probable they form part of the sensory tract. See Plate XXXVIII. fig. 2. I.

The anterior set, the *cerebellic fibres of the anterior columns* †, proceed from the front and sides of the cord, continuous therefore with the true motor tract. A portion of them may frequently be seen without dissection, and have been adverted to by several writers under the title of arciform filaments, though their termination in the cerebellum remained undiscovered. See fig. 1. E.

These *cerebellic fibres of the anterior columns*, opposite the decussation of the *py-*

* Mr. Herbert Mayo is, I believe, the only author who points out the fact that the restiform bodies are partly formed by some fibres from the posterior part of the cord, but anterior to the posterior lateral fissure, and therefore not solely by the true posterior columns. In the second edition of his Outlines of Physiology, p. 273, he says, "On cutting through and stripping down the corpus restiforme, it is found to carry with it the posterior lateral furrow."

† The fibres whose existence this paper is intended to demonstrate.

ramidal bodies, are separated from the posterior fibres of the antero-lateral columns, already described as ascending to the cerebellum, by fibres which occupy a place in the middle of the side of the cord. These fibres, which subsequently ascend through the pons Varolii to the cerebrum, and form the upper portion of the crus cerebri, have lately been described by Sir Charles Bell as a portion of the tract of sensation. The *cerebellic fibres of the anterior columns,* as they ascend to the cerebellum, pass principally below the olivary bodies, sometimes crossing the lower border of these bodies, while others which are deeper seated pass to the inner side of them. The whole, during their ascent to the cerebellum, cross to the outer side of the tract of sensation above referred to and sensory root of the fifth pair of nerves, and then, plunging into the substance of the corpus restiforme, interlace with the true posterior columns of the cord, and finally terminate in the cerebellum. See fig. 2.

These fibres, whose importance to the physiologist as proving unequivocally the existence of a complete communication between the motor tract of the spinal cord and the cerebellum need not be dwelt on, are most easily demonstrated in the following way: Let the posterior column be separated from the antero-lateral column, at the posterior lateral fissure, about two or three inches below the pons Varolii; and subsequently draw very carefully the posterior column, thus split from the anterior, up towards the cerebellum. The rent in the cord, which tears smoothly till it reaches the lower edge of the corpus restiforme, is there arrested by the *cerebellic fibres of the anterior columns,* unless too much force has been used, in which case they are easily torn through, and escape observation. They may be likewise shown by making the rent in the antero-lateral column itself, exactly at the centre of the lateral face of the cord, thus dividing the cord into two halves, an anterior and a posterior, and then continuing the rent in the same way. Again, these fibres may be shown by tearing up that portion of the front of the cord which is anterior to the anterior roots of the spinal nerves, which portion will be found, at the point where the anterior columns decussate, to split into three sets of fibres: one set of fibres cross to the opposite side; a second run to the inner side of the corpus olivare; a third set, which are not numerous, run below and to the outer side of the corpus olivare, and, ascending to the cerebellum, constitute a portion of the fibres in question.

If the sensory root of the fifth pair of nerves be traced through the pons Varolii, and the fibres which lie to the outer side of it in the medulla oblongata examined with care, they will be found connecting the anterior portion of the cord with the cerebellum; or, in other words, they will be found to be the *cerebellic fibres of the anterior columns.* The surface of these fibres is represented in fig. 1. E. as exposed by simply raising the pia mater, and carefully scraping the surface in a portion of the medulla previously hardened by alcohol. The deeper-seated are represented divided just at the point where they cross the sensory root of the fifth pair of nerves, which nerve is thus exposed to view in fig. 2. E. the corpus olivare having been raised from its natural position.

Description of the Plate.

Plate XXXVIII.

Fig. 1. represents the medulla oblongata, pons Varolii, and a portion of the cerebellum.

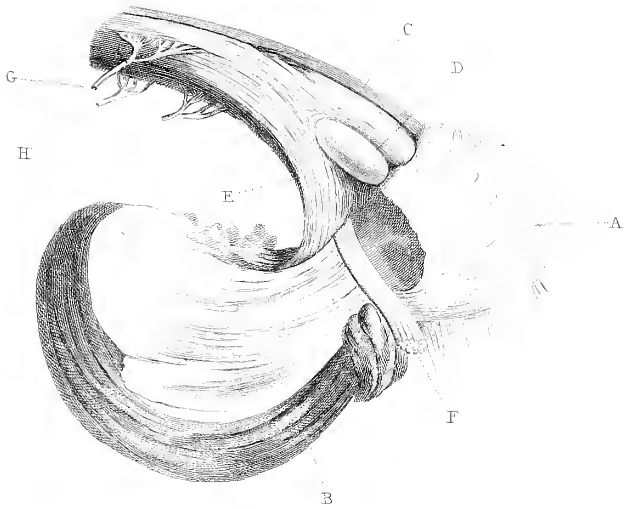
The following letters refer to the same parts in both figures.

- a. Pons Varolii.
- b. Cerebellum.
- c. Corpus pyramidale.
- d. Corpus olivare.
- e. The cerebellic fibres of the anterior columns running up to the cerebellum, and forming a layer on the corpus restiforme, or processus e cerebello ad medullam oblongatam.
- f. Sensory root of the fifth pair of nerves running through the pons Varolii, of which a section has been made to expose it.
- g. Anterior root of one of the spinal nerves.
- h. Posterior root of one of the spinal nerves.

Fig. 2. represents the medulla oblongata and pons Varolii.

The following letters refer alone to Fig. 2.

- c*. Decussation of the anterior columns.
- i. Fibres of the corpora restiformia derived from the posterior columns.
- k. Antero-lateral columns.
- l. Posterior columns, separated from the above by
- m. The posterior peaks of cineritious substance.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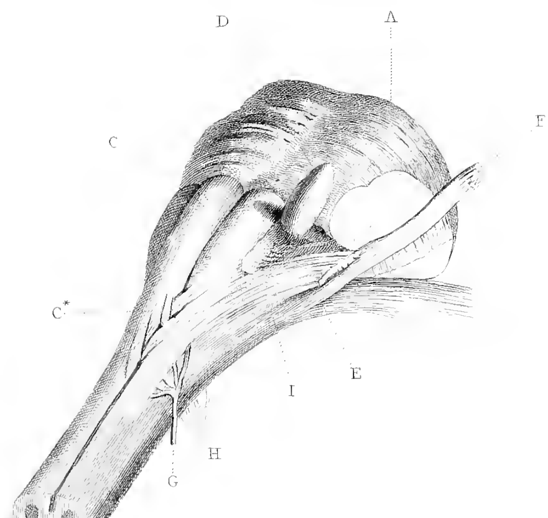
D
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C*
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XXVI. *On the Temperatures and Geological Relations of certain Hot Springs, particularly those of the Pyrenees; and on the Verification of Thermometers. By* James D. Forbes, *Esq. F.R.S., Professor of Natural Philosophy in the University of Edinburgh.*

Received February 17,—Read March 24, 1836.

If the chemistry of mineral waters has been as yet prosecuted to a very limited extent, notwithstanding the number of eminent analysts who have engaged in the research, much more has every other topic connected with their origin and nature been superficially treated*. The characters of springs of every kind are so important as to deserve minute and laborious research; and notwithstanding the partial essays of Von Buch and others, the whole subject remains in a state of confusion, and is involved in incongruities and contradictions †. The chief point to which the observations about to be described were directed, is the temperature of thermal springs; and, referring to this point alone, we might prove the almost total absence of exact data on the subject. Every traveller, to be sure, has measured the temperature of springs, but few have been aware of the difficulties which even this apparently simple inquiry involves.

We should have considered the accurate determination of the temperatures of thermal springs the first step towards a theory of their production ‡. The constancy of that temperature from day to day, from year to year, from century to century, would appear one of the most essential facts to determine; yet I am aware of scarcely a single published observation capable of being satisfactorily employed in such an inquiry. Not only are the errors of the instruments unknown, but the circumstances

* "Quiconque a sérieusement réfléchi sur cette matière, conviendra sans peine avec M. Alibert que la science des Eaux Minérales est, pour ainsi dire, à refaire."—Anglada, Traité des Eaux Minérales du Département des Pyrénées-Orientales, Pref.

† I have for a number of years been making collections respecting springs of all kinds, of which the present can be considered but as one division. Feeling, however, the doubtfulness of my ever completing this investigation, and the importance of stating completely all that relates to minute topographical details soon after the observations are made, I have lost no time in proceeding to the reduction of these Pyrenean experiments.

‡ "Depuis qu'il est assez généralement convenu que les sources thermales empruntent leur haute temperature à la chaleur propre des couches terrestres plus ou moins profondes d'où elles proviennent, l'étude des changements qu'elles peuvent éprouver a acquis une nouvelle importance. Il serait sans doute curieux de savoir si la cause chimique minéralisatrice de ces eaux dans laquelle on cherchait jadis l'explication de leur chaleur extraordinaire augmente d'intensité par le progrès du temps, ou si elle s'affaiblit; mais en tous cas, on n'aurait ainsi découvert qu'un fait local et sans portée; envisagé de l'autre manière, le phénomène, au contraire, se rattache aux plus grandes questions de la philosophie naturelle."—Arago.

under which the observations are made are liable to perpetual change. For instance, a spring in the state of nature may rise from rock directly, or from amongst debris. In the latter case, to fix the temperature is difficult, because it varies at different points; and it is nearly useless, because a year hence the circumstances of its efflux may be wholly changed. Again, in the more usual case of thermal waters being medicinally employed, it is frequently impossible (at least without much trouble) to reach the true source of the water, which is carried through pipes, conduits, and reservoirs before it is finally employed; and in this case the temperature is usually taken at the bath-cock, or at the 'buvette,' or drinking-cock, where consequently the water has been subjected to the variable cooling action of its intermediate transit. Thus, for example, at the great establishment of La Raillière at Cauteretz, in the Hautes Pyrénées, the water is cooled from 101°·9 to 99°·8 in passing through a short and well-inclosed stone conduit from the source to the 'buvette'; and in the neighbouring spring of the Mahourat, the spout from which the water flows, though in contact with the granite rock from which it rises, and, in common parlance, the true or real source, I found to give a temperature 0°·5 lower than I obtained a few feet further back by squeezing myself into an almost inaccessible cleft of the rock. Thus for the most part we have no assurance that two travellers have observed the same spring at the same point; and hence identity of name by no means infers comparability, even supposing the instruments perfect. The frequent alterations in the thermal establishments render a specific description of the locality still more indispensable. Where the mineral water is not applied to use, we have a new difficulty in the recognition of a spring by the mere description of locality. That hot springs should ever be so abundant as to render this possible might seem improbable; I have had occasion to suffer from it, however, in following the footsteps of the indefatigable Anglada amongst the numerous and often almost inaccessible hot-springs of the Eastern Pyrenees (near Thuez, in Roussillon).

There is almost a romantic interest associated with these vast bodies of hot water ceaselessly pouring from the heart of the earth, and for centuries together, as the Roman remains in the Pyrenees, in Auvergne, at Baden, at Naples, and in very many other places attest. But there is the far greater scientific interest attaching to the cause of phenomena so strange, especially where wholly detached from apparent volcanic agency. Could we have known whether the temperatures of the waters have undergone any *general* change since remote times, the result would have been highly interesting and instructive; yet we are not, *even now*, preparing for such future investigations unless we commence a method of experiment commensurate with the accuracy of the present state of science.

The researches of Fourier lead us to believe that if the temperature of hot springs be due solely to that of the earth itself, at considerable depths, the changes during historic periods must be very minute. The chemical theory, first brought forward in relation to volcanos and since extensively adopted both in this country and on the

Continent, would lead us to infer that constancy of temperature is improbable, and that even supposing that there were no uniform diminution from age to age, occasional irregular fluctuations would be inevitable. The influence of earthquakes upon the temperature of hot springs is also admitted; and it would be very desirable to know from continued observations whether abrupt changes are not frequent, similar but on a smaller scale to that, for example, which raised the temperature of the Source de la Reine at Bagnères de Luchon by about 75° on the occasion of the earthquake of Lisbon in 1755. Most curious effects have likewise occurred on occasion of earthquakes to many springs *. Baron HUMBOLDT did me the honour to mention a circumstance perhaps connected with a similar cause. Two springs in South America, at a distance from any active volcano, have *increased* in temperature by 4° centigrade since the period of his observations, as determined by M. BOUSSINGAULT.

It is a singular fact that we are not only unacquainted with the progressive variations of temperature in springs during long periods of time, but even with the diurnal or monthly changes to which many thermal waters are probably subject. The usual statement of the constancy of the heat of such springs at all seasons is abundantly general, but perfectly vague. I have reason to believe that, except in the case of particularly abundant springs, it does not hold true; but I am unacquainted with any systematic experiments on the subject, although I have made many inquiries on the subject in almost every place I have visited where the medicinal importance of the springs had rendered them objects of attention to physicians or intelligent persons generally. Such information as I have been able to collect will be mentioned in alluding to the particular localities: I will only observe, that the *absolute* constancy of the temperature of springs is a circumstance in itself impossible, owing to the variation of temperature of the uppermost strata through which they flow at different seasons. When the flow of water is abundant, this, however, appears to be very small, or even quite insensible.

The observations here recorded must always possess a certain degree of value. I lament, however, that my want of information on the points just alluded to prevents me from assuming that they are directly applicable to the chief object I had in view, —the future determination of secular changes small in amount. We may, however, conclude, that before a sufficient time has elapsed to render such comparisons a matter of interest, more extended *local* observations shall have made us acquainted with the variations (if any) which the regular change of season produces, and those which may be due to the meteorological inequalities of various years.

I shall have the opportunity of pointing out by specific examples, in the sequel, the impossibility of *as yet* commencing any comparisons of the kind alluded to, owing to the unsatisfactory vagueness of published observations; not less from errors in the modes of observing than from imperfect instruments, inadequate descriptions of loca-

* See GAIRDNER on Mineral Springs, p. 211.

lities, and other causes, producing inconsistencies so enormous that occasionally we are unable even to guess at them. Nothing is more common than to see the temperature of springs set down at 212°, on the supposition that the escape of gas indicates actual ebullition.

When we refer to any table of the temperatures of springs, the difficulty would be, not to point out which are erroneous, but which are correct. If this be the case even amongst the later observations, how much more must it apply to those of the last century! M. Legrand of Toulouse has lately attempted to compare the temperatures of the Eastern Pyrenean waters observed by Carrere in 1754 with the recent results given by M. Anglada. The former were probably made with the imperfect alcohol thermometer of Reaumur; and though the latter were made by their indefatigable and estimable author, probably with all care and with good instruments, yet since (so far as I know) no examination of the scale of the instruments has been published, there may yet be errors of such magnitude as to diminish our confidence in future comparisons with them. Yet such results are amongst the very best we possess on this subject. It is remarkable, however, that in the memoir just alluded to, it has been shown, that if the observations were really made with the *original* instrument of Reaumur, in which the degree marked 80° was not the temperature of boiling water, but of the alcohol employed, the coincidence with Anglada's observations becomes as close as could possibly be expected, and does not decidedly indicate any variation *.

Springs.	Temperature. Carrere, 1754.	Reduced to the *modern* scale of Reaumur.	Anglada, 1819.
Nyer	19°·0	18°·0	18°·5
Vinça (Source de Nossa)	20·5	19·4	18·8
Molitg (Grande Source)	33·0	30·3	30·3
La Preste (Grande Source)	38·5	35·2	35·2
Escaldas (Source du Milieu)	38·5	35·2	34·0
Vernet (Source Extérieure)	48·0	43·0	42·8
Vernet (Source du Milieu)	51·0	45 5	44·5
Arles (Escaldadou Gros)	55·5	49·0	49·0
Thuez (Olette; Carrere)	70·5	60·0	60·0

Enough has perhaps already been said to point out the importance of the inquiry, and the necessity of fixing data for future observers with a degree of accuracy hitherto unattempted. The application to the actual cases I have investigated will afford a better illustration than a mere detail of the required precautions would do; whilst the remarks just made will explain the minuteness of local indication which I have entered into, and which might otherwise have appeared superfluous.

With reference to the observations connected with the physical geography of hot

* Comptes Rendus des Séances de l'Académie des Sciences, No. 7, 1835.

springs, little preface seems necessary. I have found much to confirm the views stated by Dr. DAUBENY and others as to the connexion of hot springs with fissures and lines of elevation. The remarks which I have to make respecting the Pyrenean waters chiefly occurred to myself from personal examination; though I believe several of them are contained in the little known and less read works connected with that country. It is a fact worthy of remark, that the literature connected with the hot springs of the Pyrenees is very extensive, to the amount even of forming a small library. An examination and analysis of these works, and of the manuscript collections formed by persons connected with that country, could scarcely fail to be really valuable. In a medical point of view the quantity of information is immense, and in many cases at least I have reason to believe that it is faithfully collected and impartially recorded. The objects of my inquiries brought me frequently in contact with the medical officers of the thermal establishments, from whom I received much kindness, as well as valuable information; and amongst the multitude of proprietors or of attendants at the baths from whom I had often minute inquiries to make, I can scarcely recall a case where I was not received with the utmost civility, and even zealously aided in my experiments. To M. DARRALDE, Inspector of the Eaux Bonnes, M. BALARD of Barèges, and MM. BARRAU and BOILEAU of Bagnères de Luchon, my thanks are especially due.

The abundance of hot springs in the Pyrenean range harmonizes with the very violent action which appears to have characterized the process of their elevation. Of this we have constant proofs; and the fissured character of the valleys seems to be a consequence of the same event.

The general connexion of the appearance of hot springs with granite is so remarkable in that country as to strike the observer at once; but there are several other peculiarities not less worthy of note. The abundance of hot springs increases in a very remarkable manner as we advance eastward in the range, nor can any one have a just idea of the prodigal abundance of these thermal waters who has not visited the departments of the Arriège and the Pyrénées Orientales. Their temperatures are also the highest. In this part of the chain granitic formations preponderate; yet in almost every case which I have examined, if springs rise in granite, *it is just at the boundary of that formation with a stratified rock*. In a great many cases it happens that part of the springs rise from granite, and part from the slate or limestone in contact with it; and a more striking instance of the immediate connexion between thermal waters and disturbed strata could not be desired.

The springs of the Eaux Chaudes, Cauteretz, Bagnères de Luchon, Lez (in Spain), Aulus, Ax, Las Escaldas, Dorres, and Arles are all seated exactly *at the boundary* of the granite of the principal chain; the six first on its northern*, the three last on its southern side.

* It may perhaps be doubted whether a distinctly exposed connexion exists between the granite to which the springs of Bagnères de Luchon and Lez owe their origin, and the principal granitic axis.

The springs of the Eaux Bonnes, Bagnères de Bigorre, St. Sauveur, Barèges, Caudiac, and Ussat appear in stratified rocks in close dependence upon granitic rocks, either in great masses or in small patches (as at Bagnères and Caudiac), which invariably appear within a very short distance, and for the most part give proofs of alteration or dislocation of the rocks.

Lastly, what is extremely interesting, even where we find springs in the heart of a granitic chain, as near Olette in the valley of the Tet, where thermal waters issue in incredible numbers and run to the nearest mountain torrent, a patch of the stratified formations also appears. There is such an insulated deposit between Olette and Villefranche; and whilst the springs first alluded to seem to be connected with its western extremity, we may probably refer to it the appearance of the waters of Vernet and Molitg* at the opposite one.

In short, amongst all the nuclei of hot springs which I have visited in the Pyrenees, there is not a single exception to the connexion which I have mentioned. That this is the result of accident no one can for a moment suppose. But it seems very inexplicable how we should have in many other countries a geological conformation almost identical, without the appearance of hot springs. On the occurrence of fissures and metamorphic rocks in the case of many of the Pyrenean thermal sites, I shall speak when I come to enumerate them individually.

I have only one more general remark at present to offer, and it seems important as to the theory of mineralization of these springs. A common opinion prevails that the quantity of the hydrosulphurets contained in these springs is in proportion to their temperature, and I have even heard the existence of cold sulphureous springs in the Pyrenees denied altogether. Yet not only are such to be found, but even within not many yards of others having *a high temperature, and almost an identical mineral composition.* Of this I have met with two examples in very different parts of the chain, one at the Eaux Bonnes (south of Pau), where a perfectly cold spring rises within two hundred yards of the principal hot spring of the place, has similar medicinal properties, and is even more strongly impregnated with sulphur, as I saw proved by direct experiment. The other example occurs at Las Escaldas, on the southern declivity of the Eastern Pyrenees, where a most efficacious cold sulphureous spring rises within about one hundred yards of a hot one. When to these facts we add others scarcely less curious, of springs of totally different mineral composition issuing from nearly the same spot, and with temperatures from 160° to 180° Fahr., as we see at Ax and at Thuez, we are forced to conclude that the source of mineralization must be independent to a great extent of that of high temperature, and that the arguments as to the origin of thermal springs founded upon their chemical composition must be to a certain degree fallacious†.

* The waters of Molitg I have not visited.

† A very singular circumstance was mentioned to me by M. Arago relative to the hot springs of Aix in Provence. A perforation having been made by an individual in their vicinity, though it yielded only *cold* water,

The first point we have to attend to is the true scale of the thermometers employed.

§ 1. *Verification of Thermometers.*

In by far the majority of the experiments to be detailed one thermometer was employed. It is a standard thermometer made for me by Troughton and Simms, purposely for these experiments, in 1832. In my experiments in the Pyrenees, when temperatures below 125° Fahr. occurred, another thermometer (a pocket one by Crichton, sen., of Glasgow,) was generally employed simultaneously, both as a check upon the other observations, and in event of Troughton's being unfortunately broken. Troughton's instrument is happily entire, and to it reference will always be made when no other is specified. Some observations taken by Crichton's thermometer alone, will be reduced to a true scale through the medium of Troughton's, with which, as will be seen, I have preserved abundant comparisons.

In all the experiments made in France with Troughton's thermometer, the following precautions were rigidly observed.

1. The scale was carefully immersed in the water up to the point indicated by the mercurial column.

2. The thermometer was held in a horizontal position, or nearly so, when practicable; or if not, the position was specified. This was owing to a suspicion, that in consequence of the pressure of the column of mercury on the bulb, the indications were lower in a vertical than in a horizontal position. Since my return I put this to a careful proof as follows: The bulb was placed in a tube of stiff paper, so as to protect it from external pressure. The thermometer so defended was placed in the axis of a tin cylinder filled up with sand, the upper part of the scale projecting. The whole was gradually heated till the thermometer indicated above 200°, and varied very slowly, owing to the difficultly-conducting envelope. Being then observed many times in succession in a horizontal and vertical position, I found the excess in the former extremely small, not exceeding 0°·15 even at that high temperature. I have consequently considered any error on this score as negligible.

3. The temperature of different parts of a stream or reservoir has been noticed, and if any difference occurred, the mode of observation described.

4. The observations have been made always to tenths of a degree of Fahrenheit by estimation; and all are in that scale unless otherwise expressly mentioned.

But by far the most important precaution was the ascertainment of the true scale of the instrument, an element which I did not consider myself entitled to assume as

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yet diminished extremely the discharge of the hot springs. When, in consequence of legal measures, the proprietor was forced to close the aperture he had made, the water returned to the original springs in the same quantity and having the same temperature as before.

P.S. January 1837. Since the above was written, experiments have been made by M. Freycinet on the springs at Aix, by direction of the Academy of Sciences. See the Comptes-Rendus des Séances de l'Académie, 1836, 1^er^ Semestre.

fixed, notwithstanding the celebrity of the makers, especially as I knew that the freezing point had undergone that permanent change which I have observed in *most* of the thermometers which I have examined.

I first fixed the standard points, and then determined the intermediate points of the scale by a simple and, I believe, a new method.

The *freezing point* was fixed by supporting the thermometer vertically in a tall glass vessel partially filled with pounded ice, freshly made by LESLIE's process. This vessel was placed in a saucer filled with ice-cold water, in order to keep it externally at the same temperature. By a mean of seven observations, none differing 0°·2 from the mean, and continued during an hour and a half, the freezing point was fixed at 32°·33 on the scale.

The *boiling point* was fixed with great care on two different days. A tall tin vessel was provided, with a steam orifice and in which eight ounces of water were placed, (spring water the first day, distilled water the second,) the ball of the thermometer being placed in the steam a few inches above the surface of the water. It is assumed that 212° ought to coincide with the boiling point of water under a pressure of thirty inches.

Date.	Barometer.	Boiling point by TROUGHTON.	Reduced to pressure 30 in.	Error.
1835.		°	°	°
Nov. 7.	29·582 Att. Th. 51°·0	210·9	211·61	— 0·39
Nov. 19.	29·636 Att. Th. 50°·4	210·98	211·59	— 0·41
			Error at 212°.... Mean	— 0·40
			And we had before, Error at 32° .... Mean	+ 0·33

The principle of the determination of errors at the intermediate points is the same as that of BESSEL *, viz. causing a detached column of mercury to traverse the tube, but is simpler in practice. Instead of employing columns of mercury quite arbitrary in point of length, and deducing by a complex and tentative process portions of the tube of equal capacity, I proceed at once in the following manner.

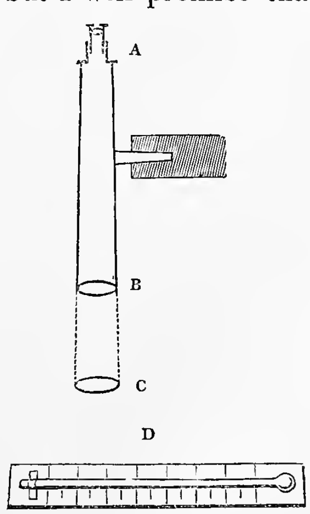
I detach a column of mercury from the rest, (by a known method †,) of such a length as to be nearly an aliquot part of 180°, which may be done with great accuracy. I then cause it to *step* along the tube, the lower extremity of the column being brought successively to the exact points which the upper extremity had occupied, noting carefully these points. At length (having started from 32°) the upper end of the column coincides nearly with 212° if its length has been properly chosen.

* See the Philosophical Magazine, vol. lxiii. p. 307.

† A column of any length being detached from the body of mercury in a tube of moderately wide bore, may have its length adjusted with great accuracy by bringing the divided part just into contact with the remainder of the mercury, and at the same moment heating or cooling the ball. If heated, the detached column will be enlarged; if cooled, some mercury will be abstracted from it.

If we assume that the small space by which it exceeds or falls short of 212° is correct, (and the error must necessarily be extremely trifling, and by a little pains in adjusting the length of the column may be reduced as low as we please,) since we also know by experiment the true thermometric interval between the points 32° and 212°, we know the true thermometric interval between the point marked 32° and the point which the summit of the moveable column last marked. But this thermometric interval was divided into a certain number of spaces of equal capacity and therefore corresponding to the same number of true thermometric degrees. Whence, by division, the true length of the moveable column employed is known, and therefore as many points of a true scale are fixed as the column was caused to *step* to between 32° and 212°.

Or the method may be more concisely stated thus: The errors of the points marked 32° and 212° being directly determined, let the number of *true* thermometric degrees between them be $180° + a$ (a being $+$ or $-$). Let the moveable column (starting from 32° on the scale), after measuring n intervals of equal capacity (whose coincidences with the scale are noted), have its summit but little differing in position (at the nth step) from 212° on the scale; let its indication be $212° + b$ (b being $+$ or $-$ as before). Then, admitting that the small space of the scale b does not sensibly differ from a *true* scale, the true interval which it has measured will be $180°+a+b$; the true interval, corresponding to the length of the column itself, will be $\frac{180° + a + b}{n} = \mathrm{I}$; and the true temperatures, corresponding to the divisions noted on the scale at the successive steps, will be I, 2 I, 3 I, &c. degrees above the true temperature, corresponding to 32° upon the scale. This mode of operating may be applied as often as desired; and it is better to reiterate it with columns of different lengths than to employ too short columns. An example will best illustrate the method; but I will premise that its accuracy depends on two essentials in practice: 1st, that the column may be brought with considerable accuracy to the desired length (how this is done has been mentioned in a preceding note); and 2ndly, that the lower extremity of the column can be brought with great accuracy to the spot marked before by its upper extremity. This may be accomplished with extreme accuracy by tapping one end or other of the tube or scale. By the same process of tapping, any sensible error which might arise from the unequal convexity of the extremities of the mercurial column under different circumstances may be overcome. To secure accuracy, and to avoid parallax in the readings, I have found a telescopic apparatus particularly adapted. A B is a telescope having cross wires in its focus, and having a lens, C, attached in front of the object-glass, so that an object placed at D, in the focus of the lens C, can be distinctly seen by the



eye at A. The thermometer is then placed so that the point of the scale to be viewed is exactly under the cross wire; all points so placed are therefore viewed in the same direction.

In TROUGHTON and SIMMS's thermometer in question (marked J. D. F.), we have seen that the error of the scale at the freezing point is $+0°·33$, at the boiling point of water it is $-0°·40$. Consequently,

212° on the scale corresponds to 212·40 nearly.
32° on the scale corresponds to 31·67 nearly.

Difference . . . 180·73

In the first experiment a column of mercury was detached, which having been made to fill *six* consecutive and adjoining spaces in the tube, commencing with 32° on the scale, terminated at 211°·25: hence, assuming the error at the last point to be the same as at 212°, (with which it nearly coincides,) we have

First reading	32°·00	
Correction	−0°·33	
True		31°·67
Last reading	211°·25	
Correction	+0°·40	
True		211°·65
True thermometric interval corresponding to six times the length of the column employed . .		179°·98
Length of the column, or I, . . .		30°·00

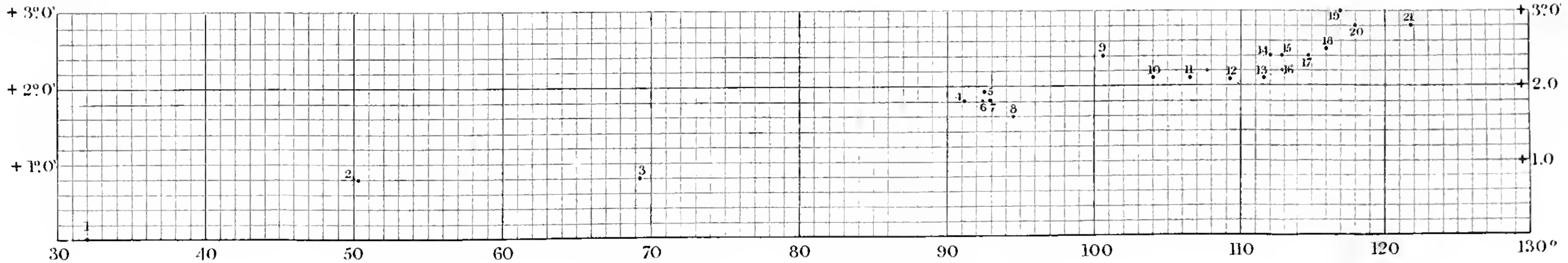
We may then immediately compare the readings taken at the successive *steps* with the true temperatures which the preceding value of I gives us, and which will be $31°·67 + I$; $31°·67 + 2\,I$, &c.

True Temperatures.	Readings of Scale.	Errors of Scale.
° 31·67	° 32·0	° + 0·33
61·67	62·2	+ 0·53
91·67	91·75	+ 0·08
121·66	121·75	+ 0·09
151·66	151·45	− 0·21
181·65	181·5	− 0·15
211·65	211·25	− 0·40

Such experiments may be repeated with columns of different lengths, which will serve not only to fix the errors at a greater number of points of the scale, but likewise to test one another by their mutual agreement. Experiments were made with columns containing $\frac{180}{10} = 18°$ nearly, and $\frac{180}{8} = 22°·5$ nearly. They were performed exactly as above described, and with the following results:

Projection of the Errors of Troughton's Thermometer.

The numerals 1. 2. 3. refer to the three Series of Experiments.



Projection of the Errors of Crichton's Thermometer.

The numerals refer to the individual Comparisons.

J. Basire sculp.

True Temperatures.	Readings of Scale.	Errors of Scale.
31°·67	32°·0	+ 0°·33
50·16	50·65	+ 0·49
68·65	69·1	+ 0·45
87·14	87·25	+ 0·11
105·63	105·8	+ 0·17
124·12	124·2	+ 0·08
142·61	142·6	— 0·01
161·10	160·95	— 0·15
179·57	179·3	— 0·27
198·06	197·7	— 0·36
216·55	216·15	— 0·40

The concluding series gives us,

True temperatures.	Readings of Scale.	Errors of Scale.
31°·67	32°·0	+ 0°·33
53·97	54·45	+ 0·48
76·27	76·75	+ 0·48
98·57	98·75	+ 0·18
120·86	120·95	+ 0·09
143·15	143·1	— 0·05
165·45	165·3	— 0·15
187·75	187·65	— 0·10
210·45	209·65	— 0·40

If we form an interpolating curve, of which the abscissæ represent the readings of the scale, and the ordinates the errors, we shall be able to deduce the correction accurately for any part of the scale. The projected errors are shown in Plate XXXIX. The agreement between the three series is very satisfactory. It is quite clear that the abrupt changes about 90° are owing to inequalities of the tube. The following Table exhibits the correction applicable, with the sign affixed, to the readings of the scale; and it is improbable that the uncertainty in these corrections should in any case much exceed 0°·1.

Table of Corrections applicable to Troughton's Thermometer.

Reading.	Correction.	Reading.	Correction.	Reading.	Correction.
30°	— 0°·3	100°	— 0°·2	170°	+ 0°·2
40	— 0·4	110	— 0·1	180	+ 0·2
50	— 0·5	120	— 0·1	190	+ 0·2
60	— 0·5	130	— 0·1	200	+ 0·3
70	— 0·5	140	0·0	210	+ 0·4
80	— 0·4	150	+ 0·2		
90	— 0·1	160	+ 0·2		

This process is incomparably simpler than that of Bessel; so simple indeed when once understood, that I would much rather determine the error of any thermometer *independently*, than attempt to compare it throughout the scale with a standard instrument. After the freezing and boiling points have been fixed, from two to three hours' labour will suffice for the rest, and the computations are of the simplest character. How far the method might be applicable to thermometers of extremely fine bore, I am not prepared to say.

In all cases in which this thermometer (which I designate Troughton, J. D. F.) has been used, which is in by far the majority of cases, we have simply to apply the above corrections*.

The next thermometer we have to do with is that of Crichton, sen., already mentioned, on which one or two determinations depend. Notwithstanding the character of the maker, it seems to have been constructed with more regard to neatness than accuracy, as the following comparison with Troughton J. D. F. will show, which were obtained by the most unexceptionable of *all* modes of comparing thermometers, by plunging them in natural hot and cold springs. The scale only goes as high as 125°. The following comparisons are arranged in the order of temperatures. I have inserted a verification of the freezing point, which had fortunately been made on the 27th of January 1835, the instrument having been broken by a fall at the moment of my return to London from the Pyrenees. These comparisons are important to be preserved for another reason, since they form the best possible check upon the care observed in reading the standard thermometer, an operation during which considerable errors (as of 5° or 10°) may by chance occur unchecked. As this Table, then, may be made to serve the purpose of verification, I shall omit the repetition of them in detailing the observations at large.

No.	Spring.	Crichton.	Troughton *reduced.*	Error of Crichton.
1.	Melting snow	32°·0	32°·0	0°·0
2.	Cold spring, St. Sauveur	50·3	49·5	+ 0·8
3.	St. Sauveur, Hontalade	69·3	68·5	+ 0·8
4.	Bagnères, Sources des Jeux	91·2	89·4	+ 1·8
5.	Cauteretz, Petit St. Sauveur	92·5	90·6	+ 1·9
6.	Las Escaldas, Source Merlat	92·5	90·7	+ 1·8
7.	Las Escaldas, Grande Source	92·7	90·9	+ 1·8
8.	Bagnères, Foulon	94·7	93·1	+ 1·6
9.	Barèges, Polard	100·3	97·9	+ 2·4
10.	Cauteretz, Raillière	104·0	101·9	+ 2·1
11.	Dorres	106·5	104·4	+ 2·1
12.	Las Escaldas, Source Colomer	109·2	107·1	+ 2·1
13.	Arles, Maujolet	111·4	109·3	+ 2·1
14.	Bagnères, St. Roch	111·8	109·4	+ 2·4
15.	Cauteretz, La Poze	112·7	110·3	+ 2·4
16.	Bagnères de Luchon, La Reine	112·8	110·6	+ 2·2
17.	Cauteretz, Bois	114·7	112·3	+ 2·4
18.	Cauteretz, La Nouvelle Poze	116·0	113·5	+ 2·5
19.	Bagnères, La Reine	117·0	114·0	+ 3·0
20.	Bagnères, Roc de Lanne	118·0	115·2	+ 2·8
21.	Bagnères, Dauphin	121·8	119·0	+ 2·8

* We have already seen that the correction for the inclination of the thermometer, owing to the pressure of the column of mercury, is almost insensible. It is to be understood, however, that in all the experiments which follow, made in France (1835), the scale was held horizontally, unless otherwise noted.

If we now project these errors as before, and form an interpolating curve, we shall obtain the following Table of Corrections applicable to the scale of CRICHTON's thermometer. The observations made with it are to be considered simply as checks upon the general accuracy of the other, and will not finally be employed, unless in the few cases where TROUGHTON's thermometer has not been used.

Table of Corrections applicable to CRICHTON's Thermometer.

Reading.	Correction.	Reading.	Correction.
32°	0°·0	80°	− 1°·2
40	− 0·4	90	− 1·7
50	− 0·7	100	− 2·0
60	− 0·8	110	− 2·1
70	− 0·9	120	− 2·9

There is but one other thermometer specifically referred to in the following paper, and that only in two instances. So long ago as the year 1826 I took the temperature of the hot spring in the cavern of Nero's baths near Naples, with a thermometer made by CARY, and not belonging to myself. I have recently had an opportunity of examining this identical thermometer. I restricted my experiments to determining the error at the particular temperature at which it had been employed, viz. 183°·5, by a comparison with my standard TROUGHTON. To make the comparison I plunged both instruments in diluted alcohol in a state of ebullition, just enough of water being added to bring it up to the required temperature. I satisfied myself by many experiments that the indications were sufficiently constant, and fixed the error of the CARY's thermometer at + 1°·3 (at temp. 183°). I had stated when I first published the experiment in Dr. BREWSTER's Journal*, that I conceived the indications of the thermometer to be about a degree too high, which was thus nearly confirmed. This thermometer was likewise employed (together with another now broken) to determine the temperature of the spring of La Pisciarella, also near Naples, which was 114° on its scale. By another special comparison with TROUGHTON's thermometer, by plunging them in hot water (intended only as an approximation), I found the error of CARY at 115° to be + 2°·1 by several concordant observations. To avoid an affectation of accuracy which the original observation would not warrant, I shall simply reduce the indication of CARY by 2°.

§ 2. *Springs of the Pyrenees.*

The following observations were made in the months of July and August 1835. I follow the natural order of the springs from west to east.

* New Series, vol. ii. p. 90.

I. *Eaux Chaudes.*

A. *Geological Position.*—The locality of these springs is one of the most interesting in the Pyrenees; and though there are few parts of the whole chain which excel it in romantic beauty, it is little known or visited. The Vallée d'Ossau, which conducts the traveller almost due south from the town of Pau, stops abruptly at the town of Laruns, or rather divides into two parts, of which the more conspicuous turns to the eastward, forming the valley of the Eaux Bonnes; whilst the other consists of a mere chasm, at its entrance narrow and tortuous, but increasing in sublimity as we ascend, which furnishes the chief tributary of the Gave* d'Oleron, and which descends directly from the Pic du Midi d'Ossau. The rock here is a limestone, probably transition, but which rests immediately upon the granite. The limestone for the most part rises *towards* the granite, and ultimately *rests* upon it, as if vertically elevated by it. The hot springs occur *exactly where the limestone meets the granite*, near the bed of the river; those whose source I was able accurately to trace, issue from granite, which is here a beautiful rock with greenish felspar. This valley is more obviously one of *disruption* than almost any other I could name; it resembles the pass of the Via Mala near Splugen in the Alps. It rather increases in breadth above the baths, but with mural precipices on both sides of very great height. The direction of the fissure or valley is nearly parallel to the line of dip of the limestone strata†.

B. *Specialties of the Springs.*—The springs of the Eaux Chaudes are numerous and of very various temperatures. Most of them, however, are conveyed in pipes from stone reservoirs at a considerable distance from the baths; and as I had no means of examining these but where they flowed into the bath, the determinations of these temperatures have but little interest (comparatively). Wherever we have either cisterns, by which the water is intercepted before it can be examined, or long conduits, it is clear that the temperature may be affected by that of the air or of the ground, and is therefore variable with the season and other circumstances. To this class belong the Esquirette, Clot, and Rey, which therefore are marked with an asterisk, having been taken at the bath cocks. The Arresecq and Baudot were taken where they issue from the granite, being merely conducted by short tubes through small vertical walls built so as to sustain the rock from which they issue. They are in the open air. The springs are all sulphureous.

* It is hardly necessary to observe that *Gave* is the provincial name for a mountain stream in the Pyrenees.

† See Mr. Hopkins on Physical Geology, Cambridge Transactions, vol. vi.

C. *Temperatures of the Springs.*—1835, July 11. Estimated height by barometrical measurements of my own†, (referred to the height of Argellez, determined by Reboul and Vidal,) 2208 feet. Temperature of a fine common spring in the town, 51°·7 (Cr.) = 50°·9 (reduced).

	Crichton.	Reduced.
*Esquirette	93°·2	91°·4
*Clot	96·5	94·6
*Rey	93·8	92·0
Arresecq { Robinet A	77·5	76·3
Arresecq { Robinet B	77·4	76·2
Baudot	81·5	80·2

II. *Eaux Bonnes.*

A. *Geological Position.*—The valley of the Eaux Bonnes forms, as we have stated, a lateral branch of that of Ossau. It is entirely formed in limestone, and does not present the same decided marks of disruption as that of the Eaux Chaudes. Its declivity is also much greater, the height of the baths being 784 feet above Laruus, which is placed at the union of the two valleys, and of which I estimate the height at about 1774 feet above the sea. The springs rise from limestone, in a very small lateral valley on the side of the principal valley next the axis of the chain. Although these springs are not immediately connected with granite (so far as we can see), yet that rock is to be found at no great distance, and in vast masses in two directions; the one in the valley of Ossau, as already specified, the other near the top of the Vallée d'Azun, stretching, I suspect, further westward than is indicated in the map of Charpentier.

B. *Specialties of the Springs.*—The springs of the Eaux Bonnes are sulphureous, like those of the Eaux Chaudes. The only hot spring which does not pass into closed reservoirs (afterwards to be artificially heated for the baths) is the Source Vieille, which issues directly from a marble pillar by a spout close to the spot from which it rises, and without the intervention of any reservoir or conduit‡. About 200 yards distant is the cold sulphureous spring of which I have spoken in the introduction to this paper, which issues directly from the limestone rock, and is merely protected by a small vertical wall and spout. It flows constantly, but is inconsiderable in amount, as are all the springs which I have had an opportunity of *directly* inspecting at the Eaux Bonnes and Eaux Chaudes, which are only about two leagues separate. The cold spring contains even more sulphur than the hot one; it is also

† In these experiments I used Bunten's barometer, (Quai Pelletier, Paris,) which amongst the many forms which I have tried appears to me by far the best adapted to the wants of the traveller.

‡ On the authority of M. Darralde, the Medical Inspector.

considered to be highly medicinal. Its temperature must exceed by a few degrees the mean temperature of the place.

C. *Temperature of the Springs.*—1835, July 11. Elevation estimated from barometrical observations, 2558 feet above the sea:

	Crichton.	Reduced.
Source Vieille (from repeated observations) . .	93°·2	91°·4
Source Froide, ou de la Montagne	55·2	54·4

The valley of Ossau and that of the Eaux Bonnes have a very damp climate, being situated near the first lofty summits which meet the warm westerly winds, and condense their moisture. Being almost completely insulated, these valleys are seldom visited except by invalids. The traveller on foot, however, need not return to Pau to resume his journey, but may cross the Col de Tortue, (clay slate,) at a height of 5970 feet, (by my observations,) and descend upon the charming valley of Azun, and the yet richer environs of Argellez. From thence by Pierrefitte he may arrive at Cauteretz, which is the next thermal establishment in the order we have adopted.

III. *Cauteretz.*

A. *Geological Position.*—The Valley of Cauteretz, like most of those of the Pyrenees, is transverse to the axis of the chain. From the opening into the valley of Barèges, or Lavedan, at Pierrefitte, (between Argellez and Luz,) it consists chiefly of clay slate, intersected by some veins of quartzose porphyry and beds of limestone. These strata are highly inclined, and at Cauteretz are nearly in a vertical position. They at the same spot undergo a very remarkable alteration, becoming much harder and heavier, and altogether assuming, for some distance, a character analogous to the very remarkable "Barèges formation," of which I shall presently have to speak, the rocks of which are generally referrible to the hornblende family. I impute this change to the neighbourhood of the granite, which rises in vast masses to the southward, forming part of the great central mass extending to the Vignemale, and in which lies the upper part of the Valley of Cauteretz and that of Lutour, which, for romantic interest, equal almost any in the Pyrenees, and strikingly resemble the pineclad ravines of the higher Alps. Nothing can be more striking than the reference which the positions of the hot springs bear to these geological features. The actual junction of the granite and slate is beautifully seen by the side of the road, just before crossing the bridge of La Raillière, above Cauteretz. This junction likewise separates two very distinct groups of hot springs; the one group rising in the slate formation in and behind the town of Cauteretz, the other issuing immediately from the granite further up the valley (commencing at about three quarters of a mile from the town, and occurring at intervals as we ascend towards the Pont d'Espagne).

Those which issue from the slate have to traverse a bed of alluvium before reaching the surface.

B. *Specialties of the Springs.*—The following springs rise behind, that is, to the eastward of the town of Cauteretz, from altered slate rock through alluvium: 1. La Poze, carefully preserved for baths; it is accessible only within the walls of the establishment, but the temperature was taken at a pipe stated to be only *one or two feet* from the true source. 2. La Nouvelle Poze (not immediately employed,) issues from a spout in a vertical wall sustaining the alluvial soil (clay with boulders) only six or seven feet (as stated to me) from the source, and without any reservoir interposed. 3. Le César: we can arrive almost at the origin of this spring. My observation was made within the bathing-house, where it passes through a wooden tube close to the source. These springs are all rather copious, particularly the César: they are sulphureous. Some other baths exist near these, but the springs cannot be directly arrived at.

The following springs occur further up the valley, beyond the Pont de la Raillière, originating in granite, in the following order: 1. Source de la Raillière, the most important spring of Cauteretz, and very abundant. A handsome bathing establishment has recently been built. With some difficulty I got access to the spring itself, and took the temperature in the very basin * in which it rises from the granite, and in which gas is copiously disengaged. This is in an apartment not open to the public, immediately behind the "Buvette," or cock for drinking from. The temperature I found 2°·1 higher than at the buvette, this quantity of heat being lost in the intermediate space, though not many yards in extent.—2. Le Petit St. Sauveur, so called from a supposed resemblance in the properties of the water to that of St. Sauveur in the Valley of Lavedan. On account of its low temperature it is artificially heated before being used. I took its temperature where it issued from a *very thick* wooden pipe, conveying it from the source, which was stated to me to be about five feet distant.—3. Bain du Pré, near the last. There being an inaccessible reservoir, I did not take the temperature.—4. Immediately above the last, ascending towards the Pont d'Espagne, is the Source du Mahourat (*mauvais trou*), which issues quite naturally from a great fissure in the granite by the side of the road. I first took the temperature at the wooden spout from which it is drunk; but perceiving that the wooden conduit entered further into the rock, I obtained a light, and squeezed myself into the remotest part of the fissure, where I again took the temperature, which was 0°·5 higher than at the other point. I was struck with the vast accumulation of gelatinous matter (*Barègine, Glairine, Matière animale,*) which had taken place in the conduit, and of which I collected a quantity. This spring, though eagerly drunk by the visitors of Cauteretz, is nearly insipid, and is stated to contain little solid matter.—5. The Source des Oeufs. This spring is by no means easily reached. It issues from a

* This basin is about $2\frac{1}{2}$ feet deep.

crevice in granite, very near the Mahourat, but quite in the bed of the torrent*, into which it immediately flows, no use whatever being made of it. I arrived at it by wading through a pool of hot water, and plunged my thermometer as far as possible into the fissure. There seems but one principal source, so that there is not likely to be any mistake about the spot. It is the hottest spring in this part of the Pyrenees: the flow of water is copious. This spring and the last, if not meddled with, would be excellent points of comparison for determining any change of temperature. There is probably little or no annual variation.—6. Bain du Bois. Here I was fortunate enough to arrive at the spring, or at least within a foot of it. The temperature was measured in a little stone conduit exterior to and immediately to the south of the bath-house. This is the last of this remarkable series of springs. All the above springs are abundant, but especially La Raillière.

C. *Temperature of the Springs.*—1835, August 4. Elevation of Cauteretz above the sea, by my observations, deduced from the height of Luz, according to REBOUL and VIDAL, 3096 feet. Copious cold spring near Cauteretz, temperature 52°·7 CRICHTON = 51°·9 reduced. The springs, however, are all at a higher level.

		TROUGHTON.	Reduced.
To the East of Cauteretz.	1. La Poze	110°·4	110°·3
	2. La Nouvelle Poze	113·6	113·5
	3. Le César	118·25	118·1

			TROUGHTON.	Reduced.
To the South of Cauteretz.	1. La Raillière	Source	102°·1	101°·9
		Buvette	100·0	99·8
	2. Le Petit St. Sauveur		90·7	90·6
	3. Bain du Pré		Reservoir.	
	4. Le Mahourat	Source	121·8	121·7
		Spout	121·3	121·2
	5. Source des Oeufs		130·2	130·1
	6. Bain du Bois		112·4†	112·3

From Cauteretz, which is in all respects one of the most interesting thermal establishments of the Pyrenees, we may go to Luz and St. Sauveur, either by re-descending the Valley of Cauteretz to Pierrefitte, or by crossing the Col d'Oleon, of which the height is 6660 feet by my observations.

* The Gave du Marcadan, which, with the Gave de Lutour, forms the Gave de Cauteretz.

† Scale inclined about 45°.

IV. *St. Sauveur.*

A. *Geological Position.*—The site of St. Sauveur is justly celebrated for its beauty. Situated upon a shelf in a steep acclivity, the little town commands views of the Valley of Barèges or Lavedan, the entrance of the Valley of Bastan, and the Pas des Echelles, leading to Gavarnie. The rock upon which it is built is a blue slaty limestone, but exhibiting some variety of structure: this limestone, I presume, belongs to the transition series, but it has some very interesting relations to other rocks. We are here upon the margin of those hornblende slates to which allusion has already been made, which Charpentier calls primitive trap, and to which he has also given the less exceptionable name of the System of Barèges. It seems to me that there is a remarkable connexion between the geological positions of Cauteretz, St. Sauveur, and Barèges. I cannot doubt that these slates are altered rocks; and it is natural to attribute the alteration to the near vicinity of granite in all these cases. Above St. Sauveur, these slates are separated by a very small interval from the granite forming the Valley of Lutour, and the bold summits which divide that valley from the Pas des Echelles. This is the same mass which extends to Cauteretz. Again, near Barèges, we see these slates in contact with sienites and granites near the Lac d'Escoubous. At that point, too, we find felspar beds or veins intermixed with the slates, and these even extend to form mountain masses, as, for instance, the barrier of the lake just named. I have little doubt that these veins communicate directly with the granitic chain; and we have near the same spot (in the Pic d'Escoubous) examples of granitic veins similar to those of Cornwall. I cannot doubt that the slates of the Pic d'Ereslids, near the above, owe their extreme hardness, their peculiarities of mineral composition, and vertical position, to the action of the granite and its tributary veins. Now in the magnificent section afforded by the ravine immediately above St. Sauveur, called the Pas des Echelles, we recognise a similar series of rocks, and these too have frequent veins or beds of felspar intermixed: the strata are almost vertical, and their direction is parallel to those of the Pic d'Ereslids, stretching from N.W. to S.E. by the compass. Now the limestone from which the springs of St. Sauveur issue, seems nearly to coincide with that which occurs on the opposite side of the valley, at a spot well known to mineralogists, named the Ravin de Rioumaou. The limestone bed which there appears is worked for useful purposes. It *coincides precisely* with the commencement of the trap slates of Charpentier; itself contains numerous minerals, such as prehnite (koupholite) and stilbite, usually associated with trap. Besides this it is distinctly rendered crystalline by the contact of felspar rock, and at the same time metalliferous, containing iron pyrites abundantly, and also, according to Charpentier, arsenical nickel and gray cobalt; then follow the very remarkable series of hornblende and other slates, which form the walls of the chasm of the Pas des Echelles, as far as the Pont de Sias, above which limestone reappears; and that this chasm owes its origin to convulsion, and not to erosion, there can hardly be a doubt, though its direction is not perpendicular to that of the strata. The same rocks are continued through the mass of the Pic de Bergons, on the same side of the ravine with Rioumaou; and I believe that the thinly slaty limestone which forms the summit of that hill (4501 feet above Luz by my observations, and therefore 6916 above the sea, differing only 14 feet from the measurement of RAMOND,) belongs to the same bed as the one above described. On the side of the Gave opposite to Rioumaou, or above St. Sauveur, we find a confirmation of these views. The alteration of the slates and limestones seems manifestly due to certain interfering veins of excessively hard porphyry, having a basis containing much quartz, in which crystals of garnet are imbedded. I conceive that these veins are connected with the neighbouring granitic masses. They are best seen by skirting the cliffs which bound the west side of the Gave,—a walk which presents both magnificent and savage scenery *. On the whole, I conclude that the hot springs of Barèges, St. Sauveur, and Cauteretz, which are placed nearly in one straight line, owe their origin in a great measure to circumstances connected with the presence of what modern geologists might justly term "metamorphic rocks †"; that these rocks are intimately connected with the granite in their vicinity; and that distinct convulsions have accompanied or succeeded their elevation.

B. *Specialties of the Springs.*—The springs of St. Sauveur present but little opportunity for increasing our precise knowledge of temperature. The four springs which take their rise in the town of St. Sauveur discharge themselves into one common reservoir beneath the street, whence the water is distributed to the baths. There is, however, one spring to which attention has recently been directed, which rises some hundred yards from the town, and a little higher. In order to arrive at its source, and to separate it from the cold water which accompanied it, a considerable cavern has been excavated. It is called La Hontalade. It is accessible within this cavern at a distance of ten or twelve feet from the point which is viewed as the source; but as this space is passed over under the floor of the excavation, and as the cavern must always have nearly the mean temperature of the soil, little error is to be apprehended from this circumstance, provided that matters are allowed to remain as they now stand.

C. *Temperatures of the Springs.*—Elevation of St. Sauveur 2526 feet (LA ROCHE). There are many fine cold springs near St. Sauveur. One in the town had a temperature of 54°·0 CRICHTON = 53°·3 reduced (this was near the hot springs); two others a little higher were 50°·2 CRICHTON and 50°·3 CRICHTON, which give 49°·5 and 49°·6 when reduced. We have stated that there are reservoirs belonging to the thermal establishment of St. Sauveur. The consequence is, that the temperature perpetually varies. I have repeatedly tried it at the "Buvette." Thus on the 20th of July 1835

* A series of specimens illustrative of the altered slate formation, and of most of the sites described in this paper, has been presented to the University of Edinburgh.

† See LYELL's Geology, vol. iii. p. 374.

I found it to be 90°·2 (Crichton), and on the 24th only 88°·8. The spring of La Hontalade is about sixty or eighty feet higher: it is rather copious.

	Troughton.	Reduced.
July 28.—La Hontalade . . .	69°·0	68°·5

V. *Barèges.*

A. *Geological Position.*—In order to treat of the springs with more connexion, we have anticipated, in a great measure, what we have to state on this subject. The town of Barèges is seated on the bank of the torrent which occupies the Valley of Bastan. It is entirely surrounded by clay slate formations, but these, near Barèges, are concealed by immense alluvial deposits, which are in a great measure derived from the neighbouring elevations, which annually devastate the town with mud avalanches. Consequently we can neither trace the proper rise of the springs, nor are they well situated for affording permanent results. They have frequently been lost, owing to subsidence or other changes in the alluvial soil; and in one remarkable instance a spring was recovered after many fruitless attempts by judicious boring: this spring is called the Source Polard, in honour of its re-discoverer. From what has been said in speaking of St. Sauveur, it will be seen that I think it reasonable to conclude that the springs of Barèges owe their origin to the altered slate rocks in the vicinity, and more remotely to the granite of the Néouvielle, which probably produced the alteration.

B. *Specialties of the Springs.*—The waters of Barèges are in such request, and the supply is so inadequate to the demand, that they are almost all husbanded in cisterns, in which the barégine, or fatty matter, may be collected. Hence in general these springs are ill adapted for our experiments, especially when we consider the liability to change, owing to the alluvial soil. There is one spring, however, of much interest, Le Tambour, or Grande Douche. This flows in a copious constant stream from a spout in a vertical wall; and M. Balard, physician at Barèges, assured me that this was the origin of the spring, and that no reservoir intervenes. The Source Polard I examined in the built cistern into which it rises: this, too, may be considered as a satisfactory experiment. The other two, the Bain de l'Entrée and Bain de la Chapelle, were taken at the cocks through which they flowed into the baths.

C. *Temperatures of the Springs.*—Height of Barèges 4163 feet (Reboul and Vidal).

On account of their celebrity we have more numerous records of the temperatures of these than perhaps of any other springs. There were formerly five Douches instead of one, so that the value of the comparison is in some degree lost. The Tambour, or principal spring, which was formerly called Le Grand Bain*, seems to have remained remarkably constant for almost a century. Two observers (Meighan in 1739, and Secondat in 1750,) seem to have observed it with care; and, what is very

* On the authority of M. Balard.

interesting, made their observations with FAHRENHEIT's thermometer, which therefore are not liable to the error with which most observations of that period were chargeable, arising from the use of the imperfect dilute alcohol thermometer of REAUMUR. We have, then, the following comparisons for Le Tambour:

1739. MEIGHAN . . . . $111\frac{1}{4}°$ FAHR.
1750. SECONDAT* mentions five Douches, but they are all between 111° and 112°
1826. ARAGO . . . . 44°·1 cent. = 111°·4 FAHR.
1835. My observations give . . . . 111°·9

a coincidence very remarkable, and which there is no reason to believe accidental.

I think it needless to refer to the observations which have been given of the other springs, as their temperature and relations seem to have materially changed. I am indebted to M. BALARD of Barèges for almost the only definite statement I have been able to obtain as to the constancy of the temperature of hot springs at different seasons. He informs me (I translate from a memorandum made in his presence) that he has made perhaps ten series of observations on all the springs of Barèges between the months of June and September, and that he has not observed the temperature to vary by *one tenth of a degree* of REAUMUR. This is a very interesting fact; and it will be seen below that two experiments made by myself on the Douche of Le Tambour at the interval of a fortnight, and with very great care, do not differ by one tenth of a degree of FAHRENHEIT.

		TROUGHTON.	Reduced.
1835, July 14.—	Grand Douche, or Tambour .	112°·0	111°·9
	Bain de l'Entrée	104·6	104·4
	Bain de la Chapelle . . .	88·8	88·7
July 27.—	Grand Douche	112·0	111·9
	Source Polard	98·1	97·9

VI. *Bagnères de Bigorre.*

A. *Geological Position.*—A superficial view of the environs of Bagnères would induce us to question the general application of the views we have given respecting the connexion of hot springs with intrusive rocks. But it is a very interesting fact, that, low as these springs occur, indeed at the very outskirts of the range, three distinct outbreaks of granite appear in the vicinity; and though Bagnères itself is situated on limestone, clay slate is immediately connected with it. This granite has a remarkable structure. It consists in great proportion of felspar, is devoid of mica, and is extremely friable. It resembles much some of the decomposed granites of Western Cornwall.

* Taken from BALARD, Essai sur les Eaux Thermales de Barèges, p. 71. I have not now the work to refer to, otherwise I might ascertain whether the name of Le Grand Bain (synonymous with Le Tambour) occurs.

B. *Specialties of the Springs.*—The number and copiousness of the springs of Bagnères astonish and almost confound us. They are, unlike most of the other Pyrenean springs, saline, and not sulphureous, and some are nearly pure. Relinquishing at once the idea of examining all the multiplied private thermal establishments, I confined myself to the examination of those contained in the great public baths (Bains Marie-Thérèse). These have been very recently constructed with great care, and at a vast expense. Though we can in no case arrive at the exact source, yet the solidity of the constructions leaves room to hope that they may be left in their present state for many years. Whilst the immense discharge of the principal springs probably renders any change of temperature during the passage from the source wholly inappreciable, it is important to add that in no case did any reservoir intervene between the origin and the place of observation. The springs belonging to the great establishment are,

1. *Source Dauphin.* This issues from the rising ground immediately behind the building, and is conveyed by a stone conduit for a considerable distance before it can be observed. The descent is rapid, and the rush impetuous, so much so as to render observation a little difficult.

2. *La Reine.* This issues under circumstances quite similar to the last. They are both *exceedingly copious,* and the temperatures were taken *in* the conduits.

3. *Roc de Lanne.* Observed in a stone conduit only a few feet from where it rises; also behind the building. Moderately copious.

4. *Source des Yeux.* Taken at the cock in the bath, at which it arrives by a long stone conduit. Quantity of water small.

5. *St. Roch.* Also small, observed in a stone conduit some yards long, behind the baths.

6. *Foulon.* Rises about 14 feet by a vertical wooden pipe from the spring to the bath, where its temperature was taken. Flow moderately large.

C. *Temperatures.*—Bagnères is 1823 feet above the sea (Ramond). Temperature of an enormous spring issuing from the limestone at Medous, at a mile from Bagnères, (so copious as immediately to turn a mill-wheel,) 51°·5 Crichton = 50·7 reduced (August 7). The temperatures of the hot springs were on the 6th of August 1835, the following:

	Troughton.	Reduced.
Le Dauphin	119°·1	119°·0
La Reine	114·1	114·0
Roc de Lanne	115·3	115·2
Source des Yeux	89·5	89·4
St. Roch	109·5	109·4
Foulon	93·2	93·0

VII. *Caudiac.*

A. *Geological Position.*—These trifling springs are merely noticed on account of their geological position, which is in a limestone country, but characterized by the intrusion of small patches of granite, as at Bagnères. They occur in the magnificent valley of the Aure (the upper part of which is one of the most peculiar and least frequented scenes in the Pyrenees), a little above Arreau.

B. *Specialties of the Springs.*—C. *Temperature.*—There are several springs; they are sulphureous. They have but little warmth, and are not abundant. A mean bathing establishment exists.

VIII. *Bagnères de Luchon.*

A. *Geological Position.*—The springs of Bagnères de Luchon issue from granite very near its junction with clay slate. This portion of granite is not marked in Charpentier's map, though it forms part of a regular band crossing the valley *at* and *above* Bagnères de Luchon, and is, I have every reason to believe, connected to the eastward with the granite of the valley d'Aran in Spain, and to the westward probably with that of Oo. Its mineralogical character is generally less crystalline than that of Oo; but I am confirmed in my opinion of its identity by having found near the Lac d'Espingo granite *in situ,* containing beautiful arborescent mica, similar to the *mica palmier* found in granite masses near Luchon, and which have every appearance of belonging to its immediate vicinity.

B. *Specialties of the Springs.*—It is a curious fact that all the chief springs of Bagnères de Luchon issue from the granite within a few feet of one another, although their properties are believed to differ considerably, and their temperatures certainly do. They are kept separate by partitions connected with a vertical wall, into which slabs of stone (which may be removed) are cemented. The springs are called La Reine, La Grotte Supérieure, La Source aux Yeux, La Blanche, and La Froide; but all of these, excepting the two first, are apt to mix with one another; and I even learned that such a mixture was practised in order to give a greater apparent supply to some of the more esteemed of the springs. It is quite certain too that rain water mixes with some of these, which with other facts immediately to be noticed, render observations of temperature here of little avail. My observations were made on the springs as they flowed from beneath the wall just mentioned. These springs are highly sulphureous, and the two whose temperature I measured were copious.

C. *Temperature.*—Height of Bagnères de Luchon, 2008 feet (Charpentier).

The springs of Luchon have undergone most surprising changes. Camperdon, a writer of credit, and himself physician for thirty years at this place, assures us that the Source de la Reine was *cold* until 1755, when (on occasion of the great earthquake of Lisbon) it assumed a temperature of 41° Reaumur. The hot springs of many parts of Europe were affected by the same event*. What shows that these springs are much connected with the sources of ordinary springs, is a curious fact mentioned to me by M. Barrau, one of the Inspecting Physicians of Luchon. In 1835, after great rains (in the month of May if my memory serves me rightly), the same spring, La Reine, delivered four times as much water as usual, and its temperature fell 16° Reaumur. This continued for twenty-five days, when it resumed its former state. The following Table contains: 1. The observations of Camperdon in 1761; 2. Those of M. Barrau in 1818; 3. Two sets of observations by M. Boisgiraud, Professor of Chemistry at Toulouse, made at two different periods. I have reduced the whole to Fahrenheit's scale.

	Camperdon, 1761.	Barrau, 1818.	Boisgiraud.	
			1832, Sept. 16.	1833, Aug. 19.
	Reaum. Fahr.	Reaum. Fahr.	Cent. Fahr.	Cent. Fahr.
La Reine	41° = 124°·2	39° = 119°·7	50°·45 = 122°·8	50°·45 = 122°·8
La Grotte Supérieure ..	51 = 146·7	50 = 144·5	58·75 = 137·8	61·25 = 142·3
Source aux Yeux	22 = 81·5	39 = 119·7	45·5 = 113·9	46·3 = 115·3
La Blanche............	18 = 72·5	23 = 83·7	34·75 = 94·6	22·65 = 72·8
La Froide	....	....	22·25 = 72·1	21·1 = 70·0

M. Boisgiraud's observations were made with great care; I had an opportunity of verifying the accuracy of the copy I obtained of them by the originals. M. Boisgiraud informed me that he had verified the zero of his thermometer by frequently plunging it in melting snow. The obvious conclusion is that the springs of Bagnères de Luchon are quite useless for the solution of the problem in which we are engaged. For the reasons already mentioned I confined my observations to the two first.

		Troughton.	Reduced.
1835, August 14.—	La Reine	110°·7	110°·6
	La Grotte Supérieure . .	139·1	139·1

Hence it would appear that the Source de la Reine has by no means recovered its former temperature, since the derangement of the year 1835, above noticed.

IX. *Lez. Vallée d'Aran, in Spain.*

A. *Geological Position.*—This trifling thermal site, of which, from being little known, I had heard exaggerated accounts, is chiefly interesting from the conformity of its geological position with that of more important springs. The springs of Lez rise near the boundary of a patch of granite on which the Spanish town of Bososte

* See Gairdner on Mineral Springs, p. 211. A most extraordinary effect of an earthquake which occurred in the Pyrenees since my visit, has been stated to the Académie des Sciences; namely, that on that occasion a *strong sulphureous smell* was perceptible in the air in the environs of Gavarnie. It is not in the least unlikely that some of the springs I have noticed have already changed their temperature. See L'Institut (Journal), Decembre 1835.

in the Vallée d'Aran is seated, and which is probably intimately connected with that of Luchon, which is not many miles distant.

B. C. *Specialties and Temperature.*—The springs are trifling and unstable. They are sulphureous, and contain much barégine. Of the two principal ones, the one (A.) rises in the bottom of a deep narrow cistern, and disengages much gas (probably azote); the other flows into the same cistern by the side. As nearly as I could determine them on the 16th of August 1835, the temperatures were

	Crichton.	Reduced.
(A.)	88°·0	86°·4
(B.)	84·3	82·9

Bagnères de Luchon limits, for the most part to the eastward, the excursions of travellers, whether in search of health or amusement. We should form, however, but an imperfect conception of the Pyrenean range by confining ourselves to the frequented little district bounded by Cauteretz and Luchon. Least of all should we appreciate the marvellous abundance of its mineralized springs by such a survey. To see these in their true character we must visit the departments of the Arriege and of the Pyrénées Orientales, districts little known even to Frenchmen, nay, almost overlooked even by some French writers on mineral waters, although perhaps they are the most abundant, and nearly the most powerful in their action and elevated in their temperature, of any in Europe. Whilst in the over-crowded establishment of Barèges invalids are compelled to economize the water by bathing at all hours of the night, waters containing the very same ingredients, far more abundant, and of far more varied temperatures, are running to waste in the Eastern Pyrenees.

X. *Aulus.*

A. *Geological Position, &c.*—At not a very great distance, in a right line to the eastward of the last-mentioned place, Lez, the traveller finds the village of Aulus. He will, however, probably reach it by a circuitous route, since even if he travel on foot or horseback his most natural course* from Bagnères de Luchon is by St. Beat, Castillon, St. Girons, and the Valley of Sallat, up its tributary valley, that of Erce, near the head of which, amidst grand scenery and in profound seclusion, lies the humble watering-place of Aulus. I mention the baths of Aulus, like those of Lez, merely on account of their geological position, which *is exactly at the junction* of granite with stratified rocks. I was given to understand that the springs are ferruginous, and of low temperature, which prevented me from examining them, though I am not now

* This route is also the most interesting, because it brings us in contact with several examples of that very singular formation, the ophite of Palassou. It is particularly exposed near the Col de Mende, at St. Lary in the Vallongue, and at Lacour in the Valley of Sallat; gypsum accompanies it in the latter site, and epidote most abundantly. I feel no doubt as to the general common character of this and our trap rocks. It is generally admitted that the hot springs of Dax near Bayonne (a point which I much regret not to have visited) owe their high temperature and mineralization to this intrusive rock.

confident as to the correctness of the statement. Near Aulus there issues from the limestone abruptly an entire rivulet of the clearest water; temp. 46°·1 CRICHTON = 45°·5 reduced.

XI. *Ussat, near Tarascon.*

A. *Geological Position, &c.*—Of this place, too, I have little to say. It is seated on the bank of the Arriège in the department of that name. The valley is here composed of limestone, precipitous, and full of caverns. We should be disposed to conjecture that this, as well as the neighbouring metalliferous valley of Vicdessos, owed its origin to a process of disruption. The granite here is also very near, though not exposed immediately at Ussat. It is a portion in immediate connexion with the vast granitic nucleus of the Eastern Pyrenees. Elevation above the sea according to PARROT, 1654 feet.

B. *Specialties, &c. of the Springs.*—The mode in which the springs of Ussat rise is worthy of notice, though they are not well adapted for determining fixed temperatures. The waters are ferruginous; there is no great spring, but each bath is filled by means of a hole bored in the sand in which it is excavated, through which the water rises. Each has therefore its own temperature, and the heat is not great. Under the circumstances I did not think it worth while to determine it, especially as the water flowing constantly through the bath is exposed to be cooled by the contact of air.

XII. *Ax**.

A. *Geological Position.*—In this most remarkable thermal site, reported to be the most prolific in Europe, we are not disappointed in finding a complete confirmation of the general principle *that hot springs take their rise at the boundary of granite.* Ax is seated on the river Arriège, several leagues above Ussat. We pass from limestone to slate, and from slate to granite, exactly at Ax. This granite immediately rises into lofty mountain masses, forming a sort of nucleus or centre of elevation, comprehending the sterile country between Ax and Mont Louis. It is here that in all the extent of the Pyrenees, reckoning from the Atlantic, the granite first constitutes the *ridge* or *geographical axis* of the chain, though it invariably forms (where visible) the geological axis. From this group of granitic mountains the country slopes in three directions, to the east, north, and south. It may, in fact, be viewed as the true termination of the ridge, which gradually descends by subdivided ramifications to the Mediterranean, the chain being split by the valleys of the Tet and Tech running east and west, which are not to be viewed as *longitudinal* valleys, but as valleys radiating from this the most easterly centre of elevation, just as the transverse valley of the Arriège radiates to the north, and that of the Segre to the south-west. It is important to keep this in view in examining the magnificent circuit of hot springs which surrounds this granitic nucleus. The system of the Canigou, though perhaps

* In the name Ax we trace the word *Aqua*, as we also do in the appellation of a multitude of points celebrated for hot springs in France and the Pyrenees; as Aix, Chaudesaigues, Caldegas, Escaldas.

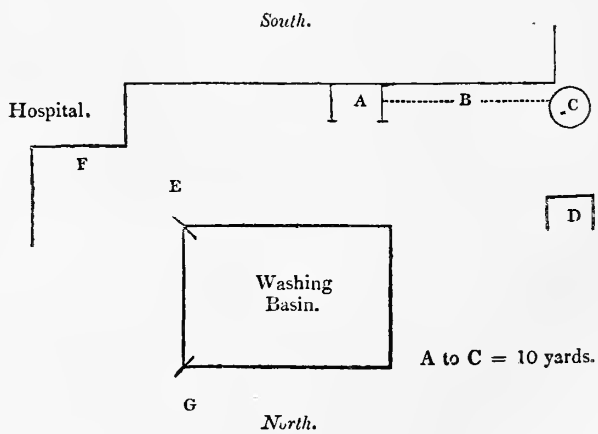
an independent one, is, geologically speaking, subordinate. I examined with some care the environs of Ax, but did not notice any other geological phenomena particularly requiring notice.

B. *Specialties of the Springs.*—I believe it has been stated that in the town of Ax alone seventy or eighty hot springs take their origin; scarcely a pit can be sunk to any depth without hot water making its appearance. The fountains in the market-place are supplied with it; manufactories are conducted by it; even culinary operations are performed by its aid in the open air, and all this in addition to the supply of innumerable baths, with such varieties of the water as are more highly mineralized. The chief groups of springs are,

I. In the market-place.

α. Of these the most remarkable is the *Source des Canons*, consisting of *nearly pure water**, issuing in immense abundance, and having a temperature of 168°. In the annexed sketch A represents the point where it issues from two spouts, each of which contains nearly an equal quantity of water, which if united might make a stream as thick as the human arm, and flowing with great velocity. Its point of rise is C, at a distance of ten yards, and a little higher. I was assured by a person who had seen the spring several times opened that it issued at that point (under the pavement of the street) with great velocity. It is conveyed by means of a solid brick conduit B at the base of the wall of a house. Considering the great volume and velocity of the water, there is little change of temperature to be feared on this account. Hence I conceive that this is one of the most eligible springs in the Pyrenees for determining the question of invariability of temperature.

Market-place of Ax.



β. The *Fontaine des Rossignols* is evidently only a ramification of the last, rising within a few yards of it into a broken sort of cistern at D. It is used for culinary

* As generally stated, and as nearly as I recollect after having tasted it. It is, however, occasionally used medicinally (Alibert, Eaux Minérales, p. 429), and it is said with marked effects. It can scarcely be denied, however, that there is something in the therapeutic action of mineral waters which baffles our chemistry, and that some springs apparently pure (as the Source de Mahourat at Cauteretz, and that of Pfeffers in the Alps) are by no means uninfluential on the human frame.

purposes. This is the real point of emergence; but here we find that difficulty which sometimes besets us, where a rare accident enables us to arrive at the actual source. It rises in so many points that it is impossible to say which is the principal source; and these vary in temperature about 2° according to my observation. The temperature given below was obtained at the hottest part I could arrive at; but this was a matter of some difficulty. Much gas was disengaged. It is copious.

γ. The *Source de l'Etuve* takes its rise at the corner of the Hospital at F: part of it is used as a vapour-bath, and the remainder (which is copious) flows directly to the point E, and is discharged into the basin. It was there that I took its temperature.

δ. A trifling spring flows into the same basin at G.

II. The *Bains de Breil* are attached to the Hotel Sicre, a little to the south-east of the market-place. The number of the springs is surprising. They are chiefly obtained by making excavations in the soil of the garden, and building reservoirs, into which the water flows; but as they can only be reached in these reservoirs, thermometric observations are of little avail.

III. *Bains de Couloubret*, at the promenade to the north of the preceding ones. The springs are numerous, but they are all cased in masonry.

IV. *Bains de Tech.*—These very copious and hot springs are most provokingly situated. They rise in caverns apparently natural, in concreted alluvial soil. Their level is maintained by artificial walls. The access is so inconvenient, that it was impossible, at the time that I was there, to obtain a good thermometric observation of these springs, the scalding temperature preventing the possibility of wading into the caverns, which otherwise might have been practicable. After all, it is nearly certain that the heat must vary in every part, since the springs actually rise from the alluvial soil at innumerable points, and with a copious disengagement of gas.

C. *Temperature of the Springs.*—Elevation of Ax, according to Parrot, 2454 feet. It is stated explicitly by M. Monicault, in his Statistique du Département de l'Arriège, that the temperature and the volume of those springs of Ax whose temperature is above 35° cent. are invariable in all seasons, whilst the others vary a little; the volume increasing about *one twelfth* in the month of May, and being again reduced in June*. This information is valuable. I have already stated that I conceive the Source des Canons to be one of the most valuable in the inquiry we are engaged in, to be found in the Pyrenees. The following observations were made on the 25th and 26th of August 1835:

Spring.	Troughton.	Reduced.
Source des Canons (both spouts exactly the same) .	167·8°	168·0°
Fontaine des Rossignols, hottest part (varies 2° from one part to another)	161·0	161·2
Source de l'Etuve	150·8	151·0

* See also Alibert, Sur les Eaux Minérales, p. 427, who quotes Dr. Boin as his authority.

XIII. *Las Escaldas.*

A. *Geological Position.*—The springs of Las Escaldas are most remarkably situated on the southern side of the Pyrenees, in the district called *La Cerdagne Française*, being in the French territory, though almost close to the Spanish frontier, looking down the valley of the Segre into Spain, and being almost entirely frequented by Spaniards. The character of the spot, too, is romantic : it lies at a distance from any road except that which leads to it, and is thus off the track even of the casual traveller who may pass through this remote and often disturbed district. To the north it is surrounded by granitic hills, bare and strewed with detached blocks; to the south, the town of Puycerda and its parched plain are visible. The watering-place itself, consisting of but a few houses, is situated however in a green and fertile hollow, which pleasingly contrasts with the scenery around. The springs all rise from granite, but as usual they are almost at the limit of that rock, the slates of the Spanish territory rising upon it at a very short distance.

B. *Specialties of the Springs.*—There are two bathing establishments, and one detached spring.

I. *Bains Colomer.* These baths, the principal ones of the place, and which have been known for many centuries, are supplied from one great spring, which discharges, according to the accurate Anglada *, no less than 795·5 cubic metres in twenty-four hours; a quantity which enables it to work a fulling-mill at a short distance. The principal issue of this spring has been inclosed in a sort of reservoir, but it is easy to empty this, and to obtain the flow of water direct from the source. It was in this way that I measured it, and I had the satisfaction to find it agree within *one tenth* of a degree with that of the "buvette," which is merely a ramification of the same spring, but so far distinct that it is raised to a higher level. Nothing, therefore, can be more satisfactory than the manner in which this spring may be observed.

II. The *Bains Merlat* are near the others, but a little lower. *α. Source de la Douche*: small, issues from a crack in the granite rock, and is carried through a baked earth conduit six feet long, at the extremity of which its temperature was taken. *β. Grande Source*: tolerably abundant. It rises *in* a reservoir, and its temperature was taken immediately at its issue from that reservoir into the nearest bath. *γ.* A small but highly mineralized *cold* spring, rising within not many yards of the above. It appears to be highly energetic on the animal economy, and is *impregnated with the same principles as the adjoining hot springs.* This is quite similar to the case of the cold sulphureous spring at the Eaux Bonnes already mentioned. Anglada does not mention this spring of Escaldas. Within a very short distance is a fine spring of pure cold

* Traité des Eaux Minérales du Département des Pyrénées Orientales, tom. i. p. 92. Paris, 1833. Anglada minutely describes the circumstances of the rise of the spring, and the precautions (similar to my own) which he employed in taking its temperature.

water; so that within a radius of a few hundred feet we have the curious spectacle of hot mineralized springs, a cold mineralized spring, and a cold spring of pure water, rising at once.

III. *La Source Margail* is an unemployed mineral spring, which issues from amongst blocks of granite behind the Bains Colomer. I took its temperature where it first appears.

All these springs are sulphureous. They deposit barègine; and Anglada states that the gas which they disengage is pure azote.

C. *Temperature.*—The elevation of Las Escaldas I do not exactly know*: it can differ, however, but very little from that of the neighbouring village of Dorres, which, according to Parrot, is 4764 feet above the sea. For the observations of Carrere in 1764, on the springs of the "Pyrénées Orientales," I refer to the table given in the introduction; nor shall I now quote the observations of Anglada, (who was provided with two thermometers by Fortin, the errors of which, however, I am not aware that he determined,) as I shall give them in a tabular view in the sequel.

			Troughton.	Reduced.
1835, August 27.—Bains Colomer	Spring	. . .	107°·1	107°·0
	Buvette	. . .	107·2	107·1
Bains Merlat	La Douche	. .	90·8	90·7
	Grande Source	.	91·0	90·9
La Source Margail		. . . .	92·0	91·9

XIV. *Dorres.*

A. *Geological Position.*—The spring of Dorres is between Las Escaldas and the village of Dorres, being about ten minutes' walk from the former. Its geological position is quite similar to that of Las Escaldas.

B. *Specialties, &c.*—This remarkable spring rises in a sort of by-path from a crack in the granitic mass. It is extremely copious, and its origin well marked, so that there is no difficulty in ascertaining its true temperature. There is no bathing establishment further than a rude sort of pool formed by the peasants. The waters are sulphureous, and contain barègine. They run neglected into the nearest brook.

C. *Temperature.*—The elevation of this spring must be nearly the same with that of the village, or about 4800 feet (Parrot).

	Troughton.	Reduced.
1835, August 27.—Dorres . . .	104°·6	104°·4

* My barometer having been broken in the Vallée d'Aran, I had no means subsequently of ascertaining heights.

XV. *Thuez.*

A. *Geological Position.*—The springs of Thuez are very remarkable, on account of their great number, high temperature, varied composition, and absolutely neglected condition. They rise near the torrent called La Tet, or Teta, which derives its name from the great valley which it traverses, almost entirely excavated in granite, and extending from Mont Louis to near Perpignan, in a direction from west to east. The occurrence of mineral waters in it is attended with some interesting peculiarities. There is a large patch of limestone insulated by granite, and upon which the town of Villefranche is built, between Olette and Prades; and what is most curious is, that we find springs *encircling* this insulated portion of stratified rock, *though they all take their rise in the granite.* Such are the springs of Molitg, Vernet, and Thuez. It is only with the latter that we have now to do. The *commune* of Thuez is situated a little above the junction of the granite and stratified rock; but there are two physical peculiarities that deserve notice: 1. That the immediate focus of thermal action is close to the *Graus d'Olette*, a winding part of the road, rendered necessary by the narrowness of the ravine which the torrent penetrates. There can hardly be a doubt that this is a line of fissure; and it is precisely at this fissure that the springs rise most abundantly. It may also be observed, that the hot springs here take their rise remarkably from the precipitous banks of tributary torrents whose beds have perhaps formed subordinate fissures. 2. Almost at the same point is the copper mine of Canavielles in granite. Hence, as at St. Sauveur, we have the co-ordinate (and I think connected) phenomena of intrusive rocks, dislocations or fissures, metalliferous impregnation, and hot springs.

B. *Specialties of the Springs.*—Nothing can give a fitter idea of the necessity of the precautions we have adopted for identifying the springs described in this paper, and also of the number and importance of those of Roussillon, than the fact, that with the minute and faithful work of Anglada before me, I have had great difficulty in ascertaining whether or not the springs which I observed occur amongst those mentioned by him at Thuez. Indeed, considering their wild and almost inaccessible position, the little interest taken by the people in them, and the impossibility of making myself understood where nothing is spoken but the Catalonian dialect, I had rather cause of congratulation that I should have discovered any of them. Even the "zêle hydrologique" of Anglada was almost exhausted on the banks of the Tet, where, he observes, "il m'est surtout arrivé d'executer des analyses, suspendu, pour ainsi dire, sur des abîmes." A complete examination of springs so abundant and so varied, (partaking of the medicinal characters of the waters of Barèges, Plombières, and Bagnères,) which are, besides, the hottest in the Pyrenees, could not fail to be of the highest interest in a general as well as a scientific point of view. I was instructed to cross the river Tet (from the *left* to the *right* bank) a little below Thuez. I found myself at the foot of a steep rocky bank covered with tangled brushwood. I scrambled along this parallel to the river for a distance of a mile or a mile and a half without any indication of hot springs ; at last, in a ravine to the right, at a very considerable height above the Tet, I discovered steam rising near a cascade. This spring, which I shall call (A), I at one time conceived to be identical with the *Source de la Cascade* of ANGLADA ; I am now, however, persuaded that it is a different one, and was perhaps never visited by him. From his description it clearly appears that his "Source de la Cascade" was in the Gorge of Carensac, through which flows the *Torrent Real*, near Thuez, and therefore considerably above the trifling ravine I have mentioned, and which is distinctly marked nearly opposite Canavielles in the Departmental Map of the *Atlas National*. This, in fact, is the distinctive mark of the spring (A) just mentioned. It occurs in a small ravine very near the Tet, but at a considerable height above it, *almost* opposite to the copper mine of Canavielles, and within sight of the *Graus d'Olette*. It is on the *right* bank of the small torrent traversing the ravine, and issues copiously from a cleft in a slaty granite rock. The principal point of issue is well marked, and at some height above the torrent, into which it flows. It is sulphureous, and contains barègine.

The other spring (B) I have distinctly identified with the "*Source du Bord de la Rivière*" of ANGLADA, whose position he has very accurately defined. It is a little further down the bank of the river, but instead of being at a great height above it, it is only separated from the Tet by a piece of flat meadow. It is almost exactly opposite to the mine of Canavielles, and its aspect is towards the chasm through which the Tet runs at the *Graus d'Olette*. It is extremely copious and sulphureous. ANGLADA has noticed justly its remarkable limpidity. It contains barègine. It issues from alluvium by a number of streamlets, which vary a little in temperature, so that the determination is not so good as the preceding one. It varies about 1° at different points: I have recorded the hottest which I noticed.

A little below this is a spring noticed by ANGLADA, as consisting of water as pure as that of ordinary springs, and having a temperature of 55° cent. = 131° FAHR.

C. *Temperature.*—The elevation of Thuez I am not acquainted with. PARROT makes the height of Fontpedrouse, a village a little higher up the river, = 3402 feet.

As the valley falls rapidly, we shall not perhaps be far from the truth if we estimate the elevation of these springs at about 2700 feet.

	Troughton.	Reduced.
1835, August 28.—Thuez, Spring (A)	171°·3	171°·5.
"Source Sulfureuse du Bord de la Tet," hottest part (B) . . .	164·8	165·0

The spring (A) is almost the hottest, not only in the Pyrenees, but even on the continent of Europe*. ANGLADA mentions one nameless spring in this same fertile

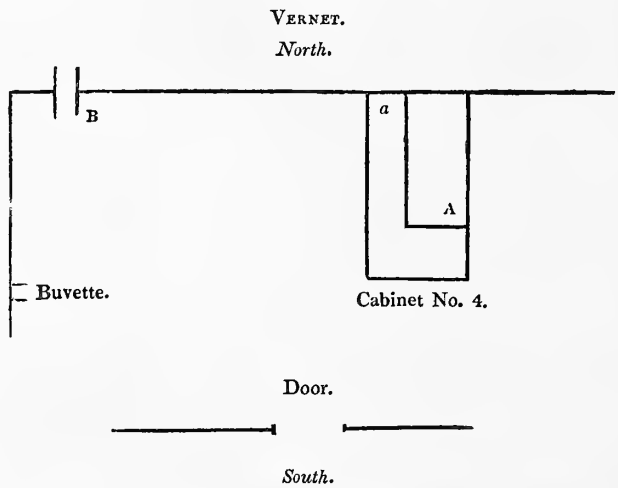
* According to M. ARAGO the hottest spring in Europe unconnected with modern volcanic action, is that of Chaudesaigues (Auvergne), whose temperature he quotes at 80° cent. = 176° FAHR.—Annuaire du Bureau des Longitudes, 1836.

neighbourhood, which is almost the same (78°·1 cent. = 172°·6 Fahr.), and which is the hottest he met with.

XVI. *Vernet.*

A. *Geological Position.*—The springs of Vernet, at the foot of the Mont Canigou, are the best known in the Eastern Pyrenees. Their position has already been adverted to, occupying the outposts of the patch of stratified rocks upon which Villefranche is situated, and which is surrounded by granite. Granite is the predominant rock near Vernet; limestone, however, occurs, and contains (in some instances in great quantities) compact sulphate of barytes *. The aspect of the Canigou from Vernet is most imposing, and the precipitous nature of the ravines in the neighbourhood is strongly indicative of convulsive action. The position of the ruined monastery of St. Martin du Canigon illustrates this statement.

B. *Specialties of the Springs.*—There is only one considerable thermal establishment at Vernet, though within a few years some adjacent springs have also been turned to use. The springs are particularly described by Anglada; but to prevent all ambiguity, I shall refer to a sketch of their present arrangement.



The spring A flows into the bath of the cabinet No. 4, and its temperature was measured there. Its point of rise was stated to me to be at *a*, about four feet distant. This is the hottest spring, and the "Source Intérieure" of Anglada. It is moderately copious. B represents the point of influx of a second spring, entering the building at a considerable height above the floor by a baked-earth conduit, which conveys it from the rock, which was stated to me to be only two or three feet distant. It is tolerably copious, and probably corresponds with "Source No. 1." of Anglada. The buvette appears near the middle of the western wall of the building. It is quite insignificant. A new spring has lately been opened a little to the south-east of the baths, which,

* The eastern part of the Pyrenees seems to have been less carefully examined by Charpentier than any other; and I suspect that his map is considerably imperfect.

however, I was not able to approach nearer than at one of the cocks in a small establishment (belonging to the same proprietor, and also on the left bank of the stream,) erected to receive it. I was informed that it was conveyed by thirty or forty yards of baked earthen tube, so that the observation is of little value. This may be called "Source Nouvelle de 1835." The springs of Vernet are sulphureous.

On the *right* bank of the river is a new establishment, that of the Bains Mercader. A very trifling spring was materially enlarged by judicious boring. As it is received by a large covered reservoir, I was unable to ascertain its temperature.

C. *Temperature.*—On the subject of the early observations of CARRERE, I refer to what has been said in the introduction to this paper. ANGLADA, speaking of the temperature of the spring A, or *Source Intérieure*, expresses regret at a discrepancy of 1°·4 cent. between his observations and those of M. ARAGO in 1826. I am enabled, however, by the kindness of that philosopher to correct this statement, and to show that the coincidence between their observations, and also with mine, is as satisfactory as could be expected;

Spring A.—ARAGO, 1826, 55°·8 c.; ANGLADA, 55°·62; J. D. F., 1835, 55°·7.

ANGLADA also informs us that he frequently took the temperature of these springs without finding any variation. The following are the particulars of my observations.

1835, August 29. Elevation of Vernet not known, but may be about 1700 or 1800 feet above the sea.

	TROUGHTON.	Reduced.
Spring A (Source Intérieure)	132°·3	132°·2
Spring B	124·2	124·1
"Source Nouvelle de 1835"	118·9	118·8

Some other thermal sites occur in the Valley of the Tet, particularly those of Vinça and Molitg; but being anxious to visit the Valley of the Tech, the extreme southern limit of France (in lat. 42° 10′), and the Roman thermal establishment of Arles, I crossed the western shoulder of the highly picturesque mountain of Canigou to Prats de Mollo, and thence proceeded to Arles, which closes our list of Pyrenean waters.

XVII. *Bains près d'Arles.*

A. *Geological Position.*—The situation of the springs near the town of Arles in the Valley of the Tech, is perfectly in unison with our general statement of the subject. They issue from granite almost close to its junction with limestone*. This is the

* I have some recollection of having noticed slate rocks between the granite and the limestone just mentioned; and this is confirmed by the circumstance of M. ARAGO, in the observations which will presently be adverted to, having characterized the principal spring as "sortant de la roche schisteuse." This may, however, be the slaty granite which so remarkably characterizes the southern side of the Canigou, and my notes expressly state that the bed of the torrent issuing from the defile is composed of granite.

termination of the granite in this line, for during the remainder of the course of the Tech it is not again met with, and after a few miles that river joins the Mediterranean. The limestone overlying the granite is of a variegated kind, having a marked resemblance to the coloured limestones of the valleys of Arreau (near Caudiac), Luchon (at Cierp), and Sallat (near St. Girons). This identity of mineral character is well marked throughout many of the Pyrenean rocks. It may be added that the springs of Arles rise in a granitic ravine, which, like that of the *Graus d'Olette*, is almost certainly a fissure. This narrow defile contains a wall of Roman construction, destined to separate a part of the river and convey it to the village of the baths; it is called in the country the "Mur d'Annibal." It is worthy of remark, that in the Valley of the Tech we have an insulated deposit of stratified rocks surrounded by granite, as in the Tet; and that the baths of La Preste (which I did not visit), above Prats de Mollo, must be situated close to the boundary.

B. *Specialties of the Springs.*—These are very numerous; I confined my attention to one or two of the most remarkable.—1. Gros Escaldadou. Extremely copious. I arrived almost close to its source in the vineyard behind the baths, and took its temperature in the stone conduit by which it is conveyed, a very unexceptionable observation. It furnished, according to Anglada, the enormous quantity of 715 litres per minute. The litre is $\frac{1}{1000}$dth part of a cubic metre.—2. Petit Escaldadou; at a little distance from the other in a vineyard. It is hotter but much less copious, and is not employed. I took its temperature as it rose from the soil just under a wall.—3. Fontaine de Manjolet. Small. Taken at the buvette. Anglada states that it is collected first in a reservoir.—4. Along the banks of the little tributary of the Tech, which passes the baths of Arles, are several smaller springs, which rise from granite in a perfectly natural state. I took the temperature of one of these, called the Gourg-Negre (gouffre noir). It rises from a crevice in the granite close to the rivulet, in a difficultly accessible spot a little above a slight expansion of the rivulet, used as a bathing pond, but considerably below the Mur d'Annibal. It is pretty copious. It appears to be the *Source Villesèque* of Anglada. I must add that the thermal establishment of Arles is as a relic of antiquity by far the most remarkable in the Pyrenees. The stately vaulted apartment in which the modern cabinets have been erected is entirely a Roman structure, and is still allowed to retain its ancient piscinæ. The springs are sulphureous, and contain barègine.

C. *Temperature.*—The elevation of the baths of Arles must be almost the same as that of the town (which is about a mile further up the Tech), and which, according to Rocheblave, is 909 feet above the sea. Anglada informs us that he frequently took the temperature of the Gros Escaldadou, and found it invariably the same; on one occasion after the interval of a year.

		Troughton.	Reduced.
1835, August 30.—	Gros Escaldadou	139°·0	139°·0
	Petit Escaldadou	145·2	145·3
	Fontaine de Manjolet	109·4	109·3
	Gourg-Negre (Source de Villesèque)	140·0	140·0

§ 3. *Hot Springs in some other parts of Europe.*

I. *Baths of Mont Dor.*

A. *Geological Position.*—We feel no surprise at the appearance of hot springs occurring amidst distinct traces of volcanic energy, after contemplating the much more unaccountable relations of those of the Pyrenees. On the subject of the baths of Mont Dor it is sufficient to say that they are situated almost at the geographical centre of that group of hills, and also at the position of greatest dislocation, two of the centres of elevation which MM. Elie de Beaumont and Dufrenoy have pointed out being found on one side and one on the other. The springs immediately issue from trachyte, which is most remarkably and beautifully columnar just at the baths. These columns have an extremely slaty cleavage perpendicular to their axes.

B. *Specialties of the Springs.*—These springs are all saline, and charged with carbonic acid in immense quantity, strikingly in contrast with the sulphureous azotic springs, which for the most part characterize the Pyrenees. The springs of Mont Dor were well known to the Romans, and several of their structures are still preserved. The present thermal establishment is extremely solid and commodious. It was finished in 1825, and offers all reasonable security for a permanent condition of the springs, which also very fortunately are all accessible at their sources. Of these I made a very careful examination; the volume of the springs is stated on the authority of the keeper of the baths*.—1. The Bain de César rises in the bottom of a cistern of Roman construction, and with an immense disengagement of carbonic acid gas. I was at pains to ascertain whether this gas had a peculiar temperature; but I did not find it to differ sensibly from that of the water. It discharges 84 litres per minute. The Bain de César is in a detached apartment behind the great building. A little above it are cold saline springs.—2. The Grand Bain consists of five distinct excavations, which are *directly* supplied by numberless small springs rising through the soil and disengaging carbonic acid. These baths are simply allowed to overflow; but since they are occasionally emptied, the cooling of their walls during that operation must render the temperature somewhat variable. But there is a more serious difficulty. From the number of springs flowing into each, the temperature varies from one point to another. This seems unavoidable. I have numbered these springs 1, 2, 3, 4, and 5, proceeding from north to south. The discharge of the whole together

* These are probably also stated in Dr. Bertrand's very interesting work on these springs. I am sorry not to be able at present to consult it in order to verify these measures.

is 60 litres per minute. The Grand Bain is at the extremity of the upper part of the great building.—3. The Bain Ramond rises in the ancient Roman basin, and was only discovered within a few years. Discharges 21 litres per minute, and much gas. It is on the ground floor.—4. Bain Rigny, near the last; it formerly supplied a Roman piscina, and rises now into a small deep square cistern, discharging much gas, and 18½ litres per minute.—5. Source de la Madelaine. This spring is stated to be nearly destitute of any foreign ingredient except carbonic acid. It issues in an exceedingly copious spring at the base of a stone pillar which marks its source, which is at a lower level than the preceding ones, and exterior to the building. Discharges 102 litres per minute.

C. *Temperature.*—Elevation above the sea, 3425 feet. These springs, excepting the Grand Bain (for reasons already stated), seem to be extremely well adapted for ascertaining the constancy of temperature of hot springs. We must recollect, however, that from the purely volcanic character of the district, changes in temperature may possibly depend upon causes merely local. Dr. BERTRAND, the Inspecting Physician, states expressly that their temperature is invariable throughout the year.

The following are my determinations.

835 , September 16, 5^h P.M.

			TROUGHTON.	Reduced.
César			108°·1	108°·0
Grand Bain.				
From north to south	No. 1.	Hottest part	105·4	105·2
		Coldest part	105·2	105·0
	No. 2. In the middle		106·7	106·5
	No. 3.*	Middle	108·0	107·9
		Front	108·0	107·9
	No. 4.	Middle	104·3	104·1
		Front	104·2	104·0
	No. 5. Hottest part		103·5	103·3
Bain Ramond			107·2	107·1
Bain Rigny			108·2	108·1
Source de la Madelaine			111·0	110·9

II. *Bourboule les Bains.*

A. *Geological Position.*—The little village of Bourboule is situated in the group of Monts Dor on the right bank of the Dordogne, three or four miles below the Bains du Mont-Dor. The neighbouring rock is entirely a volcanic tufa, similar to that of Naples, and like it excavated, and these excavations used as dwellings. It is from the tufa that the springs rise.

* No. 3 is the centre bath; the temperature, it will be observed, regularly decreases on either hand.

B. and C. *Specialties and Temperature.*—The principal spring rises into a very narrow circular vessel, with much disengagement of carbonic acid gas. This is in one corner of the bathing-house in the interior, which is mean and incommodious. The spring, however, is pretty copious, yielding twenty litres per minute*, and is hotter than any of those of the Bains du Mont Dor. Elevation above the sea, 2769 feet.

	Troughton.	Reduced.
1835, September 17.—La Grande Source . . .	121°·3	121°·2

III. *Baden-Baden.*

A. *Geological Position.*—These springs, on the border of the Schwartzwald, have a position almost identical with that which we have so invariably remarked in the Pyrenees. They occur just where the slate rocks have been violently upraised by a curious granitoid porphyry, which forms the picturesque elevations near the *Alte Schloss,* and which passes into a true granite. Upon the slate, red sandstone lies unconformably, and I believe horizontally. The elevation of this range is among the older of M. Elie de Beaumont's systems: he expressly states that the *Grès bigarré* is undisturbed.

B. *Specialties, &c.*—The only spring whose temperature I observed is the principal one, situated near the church. It rises into a large basin, in which I could perceive no evolution of gas. Nearly insipid: copious.

C. *Temperature.*—1832, August 9.

	Troughton.	Reduced.
Principal spring of Baden-Baden . . .	147°·3	147°·4

IV. *Loèsche or Leuk. Vallais.*

A. *Geological Position.*—These springs rise from limestone, but not at a great distance from the vast granitic chain which extends by the upper parts of the valley of Lauterbrunnen to the Jungfrau. The baths of Leuk are situated in a deep and precipitous valley (at this part, however, of considerable breadth), very near the foot of the Gemmi. The evidence of disruption on the great scale in the Valley of Leuk is almost as clear as such evidence can ever be. It is surrounded by mural precipices of singular boldness.

B and C. *Specialties, Temperature, &c.*—The baths of Leuk rise at a height of 4692 feet above the sea. A fine spring issuing at the end of the promenade I found to have a temperature of 43°·4 (by what thermometer not stated). The only hot spring of which I took the temperature was that which rises in the place just in front of the principal inn, called La Maison Blanche. I believe it to be the principal spring. The water is nearly insipid: copious.

	Troughton.	Reduced.
1832, September 21.—Spring at Leuk . .	123°·2	123°·1

* Lecoq, Le Mont Dore et ses Environs, p. 239.

IV. *Pfeffers. Canton of St. Gall.*

A. *Geological Position.*—Issues from limestone in a very remarkable fissure, often described by travellers. Dr. DAUBENY has dwelt upon the appearances of convulsion presented by this site*; and such no doubt there are, though I am of opinion that the chasm in which the spring itself occurs is one of erosion. The perpendicular precipice above it, however, seems to indicate fissure; nor have I the slightest doubt that the course of the Rhine in this neighbourhood has been determined by a very extensive local disruption of the strata.

B and C. *Specialties, Temperature, &c.*—There is, I believe, only one spring at Pfeffers. I took its temperature at its point of rise from the rock. It is insipid, and moderately copious. Its elevation is 2251 feet above the sea. I made particular inquiries of a resident priest as to its constancy of temperature and volume. He assured me that the former varied *but little* throughout the year, and estimated it at 29⅔° R. (= 98°·7 F.), but that its quantity was by no means so constant; its discharge in summer being 29·8 Paris cubic feet per minute, and *always* diminishing towards winter, when occasionally (as in winter 1831–2) he declared that it became *quite dry*. This is a singular and important fact, and would almost force us to suppose that this thermal water owes its origin to the neighbouring glaciers.

	Troughton.	Reduced.
1832, October 11.—Pfeffers . .	98°·1	97°·9

V. *Baths of Nero, near Naples.*

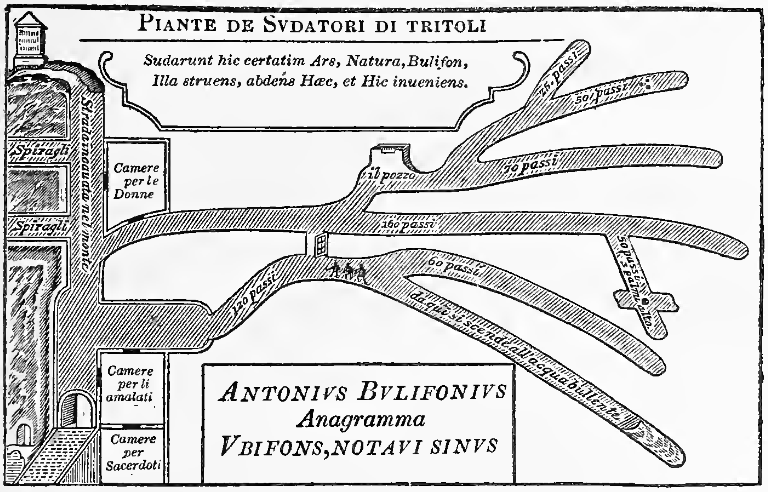
A. *Geological Position.*—The baths of Nero, commonly called Sudatorij di Tritoli, occur close to the shore of the bay of Baja near Pozzuoli. They consist of vapour baths cut from the solid tufaceous rock of volcanic formation. Perhaps the most remarkable fact connected with their geological position, is the proximity of the Monte Nuovo, elevated by volcanic explosion in 1538. It is singular that the spring immediately to be mentioned should not have been affected by that circumstance; for there is every reason to believe that it has flowed since the time of the Romans.

B. *Specialties, &c.*—I have very fully described the origin and circumstances of this remarkable spring in my series of papers on the Bay of Naples in the Edinburgh Journal of Science†. I there also quoted the curious mistakes into which travellers have been led from the supposed difficulty and danger of reaching the hot spring. I am led to conclude that the observation I then published is perhaps the only accurate one which has been made upon this, one of the most curious, and I believe the hottest spring on the continent of Europe. It is reached by a low excavated passage, the latter half of which dips rapidly, and having a sharp turn about the middle of its length, which is about 120 paces in all. At the extremity (which is difficultly reached from the heat and oppressive nature of the atmosphere) rises the spring. I took the

* Edinburgh New Philosophical Journal, January 1832.

† New Series, vol. ii. p. 88.

temperature near the edge of the pool. What is the amount of discharge, or where the exit of the water, I do not know. So far as I know it has not been analysed. Its complete insulation in the heart of the hill, and its high temperature, renders it a most interesting subject for the experiment on constancy of temperature; though we must not forget its near approximation to active volcanoes. There are various other passages in the rock which are used for vapour baths. I am glad to have this opportunity of reproducing a curious and apparently accurate plan of these by BULIFON, published in SARNELLI's *Guida dei Forestieri*, Napoli, 1688, and not, so far as I know, copied or superseded by later authors, whose works indeed are generally full of blunders on this subject.



C. *Temperature.*—The elevation above the Mediterranean is, I believe, about 30 feet. Almost all authors have stated that the water boils. My observation was made with much care, though the difficulty of observing was so great that it may be doubtful to half a degree. The thermometer employed was one by CARY. I have already stated in the introduction the satisfaction which I had in recovering it after nine years, and thus rendering this observation available to science, by comparing it with my standard thermometer.

	CARY.	Reduced.
1826, December 11.—Baths of NERO or Sudatorij di Tritoli	183°·5	182°·2

VI. *La Pisciarella, near Naples.*

A. *Geological Position.*—This spring rises from the exterior of the cone of the Solfatara amidst decomposed volcanic rocks. It is within a short distance of the lake Agnano.

B and C. *Specialties and Temperature.*—La Pisciarella rises from the ground under cover of a small hut. It contains sulphate of alumina, sulphuric acid, sulphur, and much sulphate of iron. To judge by the evidence of authors its temperature must be subject to extraordinary alternations. Sir WILLIAM HAMILTON declares that he saw the thermometer rise to the boiling-point, but admits that after rain he found it lower. Della Torre gives it a temperature of 68° R. = 185° F. The Abbé Soulavie states it at 101° F., Humboldt* at 93° cent. = 199°·4 F. Hence there can hardly be a doubt that this spring is liable to great variations of temperature.

1826, December 7. Exterior air, 41°·5. Interior of the hut, 70°·5.

	Cary.	Reduced.
Spring of La Pisciarella, hottest part	114°·0	112°·0

Note.—The temperatures of the following springs I also measured; and though I cannot speak so precisely as to the limits of error of the observations, they are undoubtedly very small.

1827, March 24. Hot spring behind the temple of Jupiter Serapis at Pozzuoli, 98°·5.
March 28. Gurgitello, Ischia (taken in a sort of well, into which it rises), 149°.

I shall conclude by recapitulating in a tabular form the final results obtained in this paper, and which it may be hoped are adapted for future reference as ascertained scientific facts†. I have much satisfaction in being able to subjoin, through the kindness of M. Arago, a series of unpublished observations which he made in 1826 on several of the hot springs of France, by a thermometer whose indications are reduced to the true scale‡. I have likewise placed for reference the observations of

* Relation Historique, 4to, tom. ii. p. 86. note. Whether from his own observations I do not know.

† The following tabular view of the concurrence of five classes of geological facts already alluded to in the Pyrenees may not be uninteresting:

Place.	Hot Springs.	Neighbourhood of Granite.	Fissures and Faults.	Metamorphic Rocks.	Metalliferous Veins.
Eaux Chaudes	*	*	*	*	*
Eaux Bonnes . .	*	?	*	. . . .	*
Cauteretz	*	*	*	*	*
St. Sauveur . .	*	*	*	*	*
Barèges	*	?	*	*	?
Bagnères	*	*	. . . .	*	
Caudiac	*	*	?	. . . .	*
B. de Luchon . .	*	*	. . . .	?	*
Lez	*	*	. . . .	. . . .	?
Aulus	*	*	*	. . . .	*
Ussat	*	?	*	. . . .	*
Ax	*	*			
Las Escaldas . .	*	*	. . . .	*	
Thuez	*	*	*	*	*
Vernet	*	*	*	. . . .	*
Arles	*	*	*		

The indications of metals are chiefly taken from Galabert, Carte Minéralogique des Pyrénées, 1831.

‡ These observations are not accompanied by minute indications of the *state* of the springs, or the particular points at which they were made.

Anglada, as being apparently most carefully made, and by good instruments of Fortin, though their scales do not seem to have been specially examined.

I have attempted to give a general (though I am aware exceedingly loose) estimate of the relative magnitudes of the springs by the numerals 1, 2, 3, 4, 5, the highest number being applied only to two springs, whose discharge is quite enormous. The prefix ⊙ is applied to those springs which seem best adapted to determine the general question of constancy of temperature, and * to those which from passing through long pipes, or for other reasons, are of little value.

Pyrenees.

No.	Place.	Spring.	Volume.	J. D. F. 1835. Fahr.	J. D. F. 1835. Cent.	Arago. 1826. Cent.	Anglada. Cent.
* 1.	Eaux Chaudes (2200 feet above sea)	Esquirette	?	91°·4	33°·0	°	
* 2.	———	Clot	?	94·6	34·7		
* 3.	———	Rey	?	92·0	33·3		
4.	———	Arresecq (A)	1	76·3	24·6		
5.	———	——— (B)	1	76·2	24·5		
6.	———	Baudot	1	80·2	26·7		
7.	Eaux Bonnes (2600 feet)........	Source Vieille	1	91·4	33·0		
8.	———	S. de la Montagne....	1	54·4	12·4		
9.	Cauteretz (3100 feet)	La Poze	2	110·3	43·5	45·0†	
10.	———	Nouvelle Poze	2	113·5	45·3		
11.	———	César	2	118·1	47·8	47·6	
⊙ 12.	———	Raillière............	3	101·9	38·8	38·4 ‡	
13.	———	Petit St. Sauveur	2	90·6	32·5		
⊙ 14.	———	Mahourat	2	121·7	49·8	49·6 §	
⊙ 15.	———	S. des Oeufs	2	130·1	54·5		
16.	———	Bois	2	112·3	44·6	45·9	
17.	St. Sauveur (2500 feet)	La Hontalade........	2	68·5	20·3		
⊙ 18.	Barèges (4200 feet)	Grande Douche......	3	111·9	44·4	44·1 ‖	
19.	———	L'Entrée	?	104·4	40·2	37·7	
20.	———	La Chapelle	?	88·7	31·5		
21.	———	Polard..............	2	98·3	36·8	37·1	
⊙ 22.	Bagnères (1800 feet)	Dauphin............	4	119·0	48·3		
⊙ 23.	———	La Reine	4	114·0	45·6	46·0¶	
24.	———	Roc de Lanne	2	115·2	46·3		
* 25.	———	S. des Yeux	1	89·4	31·9		
26.	———	St. Roch	1	109·4	43·0		
27.	———	Foulon	2	93·2	34·0		
28.	Bagnères de Luchon (2000 feet) ..	La Reine	2	110·6	43·6		
29.	———	Grotte Supérieure....	2	139·1	59·5		
* 30.	Lez..........................	A	very	86·4	30·2		
* 31.	———	B	small	82·9	28·3		

† Besides the springs of Cauteretz here given, M. Arago communicated to me the following, which I did not take account of, as on visiting them I ascertained that the sources themselves were inaccessible:

Source de la Reine, ou des Espagnols	47°·5
Source du Pré	45·4
Une autre à cote	25·6

‡ In the notes I had from M. Arago 48°·4; but I conclude that this is an error of transcription, as the temperature given by Alibert agrees exactly with mine.

§ M. Arago's observation would no doubt be taken *at the spout*, where I found it 121°·2 F. = 49°·6 cent. (See § 2. of this paper.) The agreement, therefore, is complete.

‖ At Barèges, besides these, M. Arago gives,

La Petite Douche	43°·1
La Buvette	40·7

¶ At Bagnères, besides these, M. Arago gives,

Source de Salise sur la place (Eau pure) . . .	50°·5
Casaux	50·8

No.	Place.	Spring.	Volume.	J. D. F. 1835. Fahr.	J. D. F. 1835. Cent.	Arago. 1826. Cent.	Anglada. Cent.
⊙ 32.	Ax (2500 feet)	S. des Canons	4	168°·0	75°·6	°	°
33.	———	S. des Rossignols	2	161·2	71·8		
34.	———	L'Etuve	2	151·0	66·1		
⊙ 35.	Las Escaldas (about 4700 feet) ..	Colomer. Source....	5	107·0	41·7	......	42·5
36.	———	Buvette ..	2	107·1	41·7		
37.	———	Merlat. Douche	1	90·7	32·6	}	33·1
38.	———	Grande S. ..	2	90·9	32·7		
39.	———	Margail	2	91·9	33·3	......	33·1
⊙ 40.	Dorres (4700 feet)	Dorres	3	104·4	40·2	......	40·6
41.	Thuez (about 2700 feet)	Source A	2	171·5	77·5		
42.	———	B. S. du Bord de la Tet	3	165·0	73·9	......	75·0
43.	Vernet (about 1700 feet)	A. Intérieure........	2	132·2	55·7	55·8	55·6
44.	———	B.	2	124·1	51·2	......	52·7†
* 45.	———	Source de 1835......	?	118·8	48·2		
⊙ 46.	Arles (900 feet)................	Gros Escaldadou	5	139·0	59·4	59·9‡	61·2
47.	———	Petit Escaldadou	2	145·3	62·9	62·7	62·9
48.	———	Manjolet	1	109·3	42·9	42·1	42·5
49.	———	Gourg-Negre........	2	140·0	60·0	......	60·4§
	Monts-Dor.						
⊙ 50.	Bains du Mont-Dor (3400 feet) ..	César	3	108·0	42·2	42·2	
51.	———	Grand Bain, hottest part	2	107·9	42·2		
52.	———	Ramond	2	107·1	41·7	40·9	
53.	———	Rigny..............	2	108·1	42·3		
⊙ 54.	———	Madelaine	4	110·9	43·8	42·5	
⊙ 55.	Bourboule (2800 feet)	Grand Source	2	121·2	49·6		
	Schwarz-Wald; Alps.			1832.			
56.	Baden-Baden..................	Principal Spring	?	147·4	64·1		
57.	Leuk (4700 feet)	....	3 ?	123·1	50·6		
58.	Pfeffers (2300 feet)	....	3	97·9	36·6		
	Italy.			1826.			
⊙ 59.	Baths of Nero (30 feet)	....	?	182·2	83·4		
60.	La Pisciarella (near Lago d'Agnano)	....	?	112·0	44·4		
				1827.			
61.	Spring at Temple of Serapis at Pozzuoli..................	....	?	98·5	36·9		
62.	Ischia......................	Gurgitella	?	149·	65·0		

† I have some doubt whether the spring I have denoted by B is No. 1. or No. 2. of Anglada; nor does it much matter, since No. 1, according to him, has a temperature of 52°·75 (as given in his Table, vol. i. p. 16, which differs a little from that in the detailed part of his work), and No. 2, of 52°·5; but the observations were made at the proper sources, while mine were not.

‡ At Arles, besides these, M. Arago gives,

Source d'eau pure sur la place . . . 54°·9

The same, according to Anglada,

At the fountain . . . . 55°

At its proper source . . . . 59°·4

§ This is the source Villesèque of Anglada, which appears to be the same with the Gourg-Negre.

I apprehend that the probable error, whether instrumental or of observation, in the above results, scarcely exceeds 0°·2 Fahr., with the following exceptions: Nos. 1. to 8. inclusive, 30, 31, 59, 60, may be under a doubt of perhaps 0°·4 Fahr., and Nos. 61, 62, of somewhat more.

Edinburgh, 11*th February,* 1836.

INDEX

TO THE

PHILOSOPHICAL TRANSACTIONS

FOR THE YEAR 1836.

M.

N.

P.

S.

T.

V.

W.

CORRIGENDA.

Page 351, line 8 from bottom, *for* analogy, *read* resemblance.
Page 365, line 5, *for* demanded, *read* obtained.

PRINTED BY RICHARD TAYLOR, RED LION COURT, FLEET STREET.

METEOROLOGICAL JOURNAL,

KEPT BY THE ASSISTANT SECRETARY,

AT THE APARTMENTS OF THE

ROYAL SOCIETY,

BY ORDER OF

THE PRESIDENT AND COUNCIL.

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge.............=83 feet $2\frac{1}{2}$ in.

——————————— above the mean level of the Sea (presumed about) ....=95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House...............=79 feet.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

1836.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	External Thermometer. Fahrenheit. 3 P.M.	External Thermometer. Self-registering. Lowest.	External Thermometer. Self-registering. Highest.	Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
JANUARY												
F 1	30.184	37.8	30.243	36.3	30	32.6	28.2	31.4	34.6		ESE	Overcast—light brisk wind. Evening, Fine and clear.
S 2	30.493	30.3	30.473	30.2	12	19.6	27.5	17.5	33.9		E	A.M. Fine and cloudless—light wind. P.M. Overcast—light snow and wind.
○ ⊙ 3	30.311	31.7	30.249	32.7	29	35.8	40.6	19.2	43.4		SSW	Overcast throughout the day—deposition—light fog and wind. Evening, Rain and wind.
M 4	30.045	36.6	30.018	38.6	35	45.2	48.3	34.2	47.2	.063	SW	Overcast—deposition—light brisk wind.
T 5	30.071	42.5	30.079	43.6	40	46.3	50.2	43.6	48.8		SW	Fine—light clouds and wind. Evening, Cloudy—high wind.
W 6	30.075	45.0	30.010	45.7	42	46.3	46.4	44.9	46.6		SW	A.M. Thick fog—deposition. P.M. Overcast—light rain and wind.
T 7	29.780	44.3	29.748	43.8	33	38.2	40.0	34.9	40.2	.030	SE	Overcast—high wind throughout the day.
F 8	29.823	44.6	29.825	44.5	37	41.4	42.4	37.3	41.3		ESE	Overcast—light wind. Evening, Fine and clear—sharp frost.
S 9	29.831	41.0	29.746	40.4	30	31.5	34.3	30.3	.		E	Overcast—high wind. Evening, Light snow, with very high wind.
⊙ 10	29.465	38.7	29.209	37.5	29	32.2	31.3	.	.		E	A.M. Overcast—light brisk wind. P.M. Overcast—light snow and wind.
M 11	29.126	37.2	29.116	37.8	30	33.3	35.5	.	43.7		SW	A.M. Overcast—heavy snow during the night. P.M. Overcast—rain and wind. Evening, Heavy rain—high wind.
T 12	29.277	37.7	29.303	38.0	26	30.7	35.7	28.6	35.2	.625	SW	Fine & cloudless—very light fog & wind. Evening, Fine & clear.
W 13	29.683	36.0	29.694	36.6	27	30.7	39.2	28.0	43.4		SW	A.M. Fine and cloudless—light fog. P.M. Fine and cloudless—light wind. Evening, Cloudy.
T 14	29.752	38.2	29.649	40.2	35	43.7	47.6	29.0	48.4		S	A.M. Overcast—very light rain and wind. P.M. Cloudy—light brisk wind. Evening, Rain—high wind.
F 15	29.314	45.2	29.346	45.4	41	43.2	42.7	42.6	44.2	.280	W	A.M. Overcast—light rain and wind. P.M. Fine—light clouds and wind. Evening, Cloudy—light brisk wind.
S 16	29.982	41.9	30.061	41.6	31	35.6	37.7	33.3	37.3	.041	N	Fine and cloudless—light fog and wind. Evening, Clondy.
⊙ 17	30.095	38.9	30.140	39.3	27	33.2	40.9	30.0	40.3		SW	Fine—light clouds, fog, and wind. Evening, Cloudy—light fog.
◐ M 18	30.075	38.4	29.909	39.4	29	36.7	40.8	31.0	44.2		SW	Overcast—light fog and wind. Evening, Fine and clear.
T 19	30.093	40.7	30.110	40.6	30	35.8	38.2	35.2	37.7		N	A.M. Fine and cloudless—light haze and wind. P.M. Fine—light clouds and wind. Evening, Cloudy.
W 20	30.115	39.3	30.023	40.0	32	38.2	41.7	31.3	41.5		SSW	Fine and cloudless—light wind. Evening, Fine and clear.
T 21	29.709	40.4	29.606	41.2	31	37.3	40.6	35.5	44.0		ESE	A.M. Hazy—lt. wind. P.M. Lightly overcast. Evening, Cloudy.
F 22	29.386	43.2	29.437	44.6	39	44.6	47.2	36.3	52.3		S var.	A.M. Overcast—high wind. P.M. Fine—light clouds and wind. Evening, Rain—high wind.
S 23	29.297	47.2	29.451	48.7	44	52.2	52.3	43.7	53.4		SW var.	A.M. Fine—light clouds and wind. P.M. Overcast—rain and high wind. Evening, Fine and clear.
⊙ 24	29.881	47.5	29.875	47.9	41	44.6	48.7	42.8	48.6		S	Fine—nearly cloudless—light wind. Evening, Overcast—lt. rain.
M 25	30.203	46.9	30.214	47.3	40	42.8	44.6	41.3	44.4	.033	S	A.M. Foggy. P.M. Fine—light clouds. Evening, Fine & clear.
T 26	30.085	45.9	29.984	46.4	39	42.4	45.0	38.6	45.3		S	Cloudy—light brisk wind.
W 27	29.909	44.8	29.806	45.5	38	44.3	44.3	39.0	46.3		SW	A.M. Overcast—deposition. P.M. Fine—nearly cloudless. Evening, Overct.—lt. wind.
T 28	29.623	45.4	29.449	46.6	39	44.3	47.4	41.2	48.8		SW	A.M. Cloudy—light wind. P.M. Fine—light clouds & wind. Evening, Overcast—hail, rain, and high wind.
F 29	29.346	45.6	29.190	46.0	35	38.0	42.6	35.2	42.5	.158	SW	A.M. Fine and cloudless—light haze and wind. P.M. Overcast—high wind. Evening, Overcast—rain and wind.
S 30	28.922	42.9	29.114	43.4	31	36.2	43.0	32.5	43.2	.319	SW	A.M. Overcast—light snow and wind. P.M. Fine—light clouds and wind. Evening, Cloudy.
⊙ 31	29.267	41.4	29.037	42.5	34	39.0	48.8	33.3	48.2	.050	S var.	Overcast—light steady rain—high wind. Evening, Fine & clear.
MEANS..	29.781	41.2	29.746	41.7	33.4	38.6	41.7	34.5	43.8	Sum. 1.599		Mean of Barometer, corrected for Capillarity and reduced to 32° Fahr. ...... 9 A.M. 29.757; 3 P.M. 29.721
FEBRUARY												
M 1	29.136	42.6	29.122	43.4	36	40.5	43.8	37.3	45.0	.175	SW	Fine—light clouds, and light brisk wind. Evening, Fine and clear.
○ T 2	28.768	42.3	28.512	42.6	33	38.2	39.9	35.5	40.7		ESE	Overcast—light rain and wind thoughout the day.
W 3	28.740	41.6	28.970	41.7	35	36.7	39.6	35.8	39.3	.481	N	A.M. Overcast—light rain with high wind. P.M. Overcast—light brisk wind. Evening, Overcast—rain and wind.
T 4	29.598	41.0	29.779	41.2	36	38.3	38.5	35.2	38.6	.300	NE var.	Overcast—light rain and wind throughout the day.
F 5	30.014	41.3	29.988	41.4	35	38.2	38.8	36.5	38.9	.038	N var.	Overcast—deposition—light brisk wind.
S 6	29.825	41.2	29.797	42.4	34	39.0	47.3	33.2	47.5		W	Overcast—light wind.
⊙ 7	29.614	43.2	29.676	44.2	37	43.2	45.8	37.8	46.7		WSW	A.M. Overcast—very light rain. P.M. Fine—light clouds and wind. Evening, Overcast—light rain.
M 8	29.885	43.3	29.770	44.0	35	39.2	45.5	37.2	47.8	.047	E	Overcast—light rain and wind.
T 9	29.754	46.3	29.774	47.2	39	47.8	51.0	38.2	50.6		SSW	Overcast—light wind.
W 10	29.739	48.4	29.566	49.5	45	47.7	50.7	45.8	50.3		S var.	A.M. Fine—light clouds and wind. P.M. Cloudy—light brisk wind. Evening, Overcast—heavy rain.
T 11	29.897	46.3	30.063	45.8	35	39.2	41.2	36.4	41.2	.202	SW	Fine—light clouds and wind. Evening, Fine and clear.
F 12	29.946	43.8	29.907	44.9	32	39.7	48.6	32.8	47.6		SSW	Cloudy—light brisk wind. Evening, Fine and clear.
S 13	30.213	43.2	30.208	43.6	33	35.7	43.0	33.3	42.7		W	A.M. Thick fog. P.M. Fine and cloudless—light wind. Evening, Fine and clear.
⊙ 14	30.255	44.6	30.261	45.7	37	42.6	49.2	34.2	48.6		SW	Fine—light clouds and wind. Evening, Fine and clear.
M 15	30.319	43.7	30.289	44.8	33	38.5	48.7	34.2	47.7		SW	Fine and cloudless—light haze. Evening, Fine and clear.
◐ T 16	30.178	43.7	29.994	44.6	35	38.9	47.2	34.5	47.0		SW	A.M. Fine—light clouds. P.M. Cloudy—light wind. Evening, Overcast—heavy rain, with high wind.
W 17	29.711	42.6	29.639	42.4	23	34.2	39.2	31.8	39.0	.222	S var.	A.M. Fine and cloudless—light brisk wind. P.M. Cloudy—snow and wind. Evening, Cloudy—high wind.
T 18	29.893	41.6	29.980	41.6	32	38.2	39.5	33.3	38.2		N var.	A.M. Cloudy—light brisk wind. P.M. Fine—light clouds and wind. Evening, Cloudy—high wind.
F 19	30.119	38.3	30.097	38.9	27	33.6	36.6	31.2	36.2		S	A.M. Ovt.—brisk wind. P.M. Fine & cldless. Evg. Fine & clear.
S 20	30.231	37.0	30.241	37.8	25	30.2	36.8	26.5	36.2		N	Fine—light clonds and wind. Evening, Fine and clear.
⊙ 21	30.180	36.2	30.049	37.2	25	29.1	38.2	25.3	38.3		NW	A.M. Hazy—white frost during the night. P.M. Fine and cloudless. Evening, Cloudy.
M 22	29.802	37.9	29.687	39.5	30	39.2	44.2	27.8	37.7		SSW	Overcast—deposition. Evening, Overcast—very light rain.
T 23	29.481	40.5	29.402	41.6	33	38.0	43.7	34.2	43.2		SSW	A.M. Cloudy—light wind. P.M. Fine—light clouds and wind. Evening, Cloudy.
W 24	29.219	40.6	29.063	41.7	33	38.6	44.3	32.9	44.3		NE var.	A.M. Cloudy—light brisk wind. P.M. Overcast—lt. brisk wind.
T 25	28.883	41.6	28.857	42.3	33	34.7	41.2	32.4	40.0		SSW	A.M. Overcast—light wind. P.M. Fine—light clouds. Evening, Fine and clear.
F 26	28.786	40.4	28.752	40.7	33	35.6	37.4	31.7	37.9	.061	E	A.M. Overcast—light rain & wind throughout the day.
S 27	28.730	40.8	28.827	41.0	35	36.6	38.3	34.2	37.7	.494	NW	Overcast—deposition—light brisk wind.
⊙ 28	29.103	40.5	29.122	41.2	32	34.2	39.6	33.2	38.5		SW	Overcast—light wind. Light rain during the night.
M 29	29.287	40.3	29.346	40.7	34	37.4	40.6	33.3	39.8	.036	SSW	Overcast—light wind throughout the day.
MEANS..	29.631	41.9	29.612	42.5	33.3	38.0	42.7	34.0	42.3	Sum. 2.056		Mean of Barometer, corrected for Capillarity and reduced to 32° Fahr. ...... 9 A.M. 29.605; 3 P.M. 29.585

1836.	9 o'clock, A.M.		3 o'clock, P.M.		Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer.				Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
	Barom.	Attach. Therm.	Barom.	Attach. Therm.		Fahrenheit. 9 A.M.	Fahrenheit. 3 P.M.	Self-registering. Lowest.	Self-registering. Highest.			
MARCH T 1	29.013	41.2	28.829	42.2	36	40.6	46.2	35.4	47.0		S var.	Overcast throughout the day, with light brisk wind. 8½ P.M. Heavy storm.
W 2	29.285	43.8	29.299	45.3	37	42.2	48.8	38.9	49.4	.194	SSW	A.M. Fine and cloudless—light wind. Evening, Overcast—very light rain.
○ T 3	29.477	45.2	29.530	46.5	41	45.5	48.5	40.7	48.4		S	Overcast—light wind.
F 4	29.532	46.6	29.497	47.4	40	43.3	49.0	40.4	48.7	.072	S	A.M. Overcast—light rain. P.M. Fine—light clouds and wind. Evening, Cloudy.
S 5	29.314	47.5	29.215	48.8	42	44.4	49.6	42.3	49.4	.038	S	A.M. Fine and cloudless. P.M. Cloudy—light rain and wind.
☉ 6	29.122	46.2	28.998	46.9	40	43.8	47.7	37.7	48.4	.186	ENE	Overcast—light drizzling rain and wind.
M 7	29.247	45.3	29.217	47.2	37	42.2	48.6	36.4	48.7		ESE	A.M. Overcast—light wind. P.M. Fine—light clouds and wind. Evening. Overcast.
T 8	29.202	46.0	29.223	40.9	39	40.3	42.4	39.2	42.2	.033	NE var.	Overcast—very light rain. Evening, Cloudy.
W 9	29.287	44.5	29.126	45.6	34	38.4	45.2	33.3	45.5	.088	S	A.M. Fine—light clouds and wind. P.M. Cloudy. Evening, Overcast—light rain.
T 10	29.208	45.0	29.182	46.2	38	43.8	46.5	37.2	47.2	.069	S.	Overcast—very light rain and wind.
F 11	28.916	47.8	28.964	48.7	42	47.7	51.2	42.5	51.2	.052	SW	A.M. Fine—light clouds and wind. P.M. Cloudy—light hail and rain. Evening, Fine and clear.
S 12	29.123	47.4	29.158	48.7	42	47.6	50.4	41.5	51.6		S var.	A.M. Cloudy—high wind. P.M. Showery—hail and wind. Evening, Cloudy—light rain—high wind.
☉ 13	29.412	47.3	29.489	48.6	40	43.0	50.8	39.2	50.4	.227	SW	Fine—light clouds and wind. Evening, High wind throughout the night, with light rain.
M 14	29.049	48.6	29.057	49.2	43	50.3	45.2	42.2	50.6	.166	SW var.	Overcast—light rain and wind throughout the day.
T 15	28.891	48.2	29.143	49.7	41	44.9	48.5	40.3	50.7	.444	SW var.	A.M. Overcast—light rain and wind. P.M. Fine—light clouds and wind. Evening, Cloudy—high wind.
W 16	29.584	46.8	29.681	47.8	37	41.4	46.0	35.3	47.8		SW	Fine—light clouds and wind. Evening, Fine and clear.
● T 17	29.792	47.0	29.808	48.6	40	48.3	51.8	38.7	52.2	.061	S var.	Cloudy—high wind throughout the day. Evening, Overcast—very light rain.
F 18	30.322	51.2	30.346	58.8	47	52.2	58.8	47.7	59.4		SW	A.M. Overcast—light wind. P.M. Fine—light clouds and wind. Evening, Cloudy.
S 19	30.245	52.6	30.148	57.2	47	47.8	61.5	45.5	61.6		ENE	Fine—nearly cloudless. Evening, Fine and clear.
☉ 20	30.182	55.3	30.184	58.4	49	53.3	61.2	47.2	64.4		SW	Fine and cloudless. Evening, Fine and clear.
M 21	30.113	53.7	30.017	56.6	48	48.2	53.7	46.7	54.2		SW	A.M. Overcast—light wind. P.M. Cloudy.
T 22	29.922	55.2	29.875	55.8	49	50.8	51.7	48.2	52.4		S	Overcast—very light rain.
W 23	29.720	51.7	29.588	54.5	45	48.2	49.0	42.3	50.7	.047	S	Overcast—very light rain and wind.
T 24	29.544	51.2	29.546	53.2	47	44.6	49.3	39.5	48.7	.050	S var.	A.M. Fine—nearly cloudless—light brisk wind. P.M. Cloudy—occasional showers of rain & hail. Ev. Cloudy—high wind.
F 25	28.938	49.6	29.027	53.2	45	47.7	49.2	41.3	51.3	.105	S var.	Overcast—lt. rain with h. wind. Ev. Fine and clear—h. wind.
S 26	29.239	47.3	29.344	50.2	37	42.6	42.3	37.7	47.6	.044	SW var.	A.M. Cloudy—light brisk wind. P.M. Overcast—light rain. Evening, Fine and clear.
☉ 27	29.556	46.4	29.506	49.9	38	39.6	47.9	31.8	48.2		S	A.M. Fine & cloudless. P.M. Cldy.—lt. wind. Ev. Cldy.—lt. rain.
M 28	28.662	44.0	28.675	46.5	40	40.2	45.6	36.2	45.5	.244	E	Overcast—lt. rain & wind. Ev. Overcast—light rain—very high wind. *Sudden depression of the Barometer.*
T 29	29.548	46.4	29.606	48.7	37	42.3	47.3	35.9	48.2	.130	WSW	A.M. Fine and cloudless—light brisk wind. P.M. Fine—light clouds and wind.
W 30	29.677	46.6	29.336	49.2	42	47.2	52.2	41.2	53.3		SW var.	Overcast—very light rain with brisk wind. Ev. Cldy.—high wind.
T 31	29.679	50.3	29.804	51.0	42	47.4	47.8	42.3	49.4	.180	SW var.	A.M. Fine—light clouds—light brisk wind—occasional showers of rain and snow. P.M. Cloudy—light brisk wind.
MEANS ..	29.445	47.9	29.433	49.7	41.4	45.2	49.5	40.2	50.5	Sum. 2.430	Mean of Barometer, corrected for Capillarity and reduced to 32° Fahr.	9 A.M. 29.401; 3 P.M. 29.384
APRIL ○ F 1	29.792	45.8	29.576	46.2	34	43.4	36.6	36.9	44.7		SW	Overcast—rain, snow, and wind. Evening, Overcast—continued rain and wind.
S 2	29.511	43.2	29.609	46.4	37	39.3	40.9	33.7	44.3	.591	SW	A.M. Fine—light clouds—light brisk wind. P.M. Overcast—light rain and wind.
☉ 3	29.913	42.9	30.072	45.3	36	41.6	43.5	33.8	45.0	.047	WNW v.	Overcast—light rain and snow, with light brisk wind.
M 4	30.366	43.6	30.336	46.4	35	39.7	46.2	32.3	46.6	.133	NW	A.M. Fine—nearly cloudless—light wind. P.M. Cloudy—light wind.
T 5	30.179	43.0	30.003	47.0	35	42.3	48.8	34.7	50.2		SW	A.M. Overcast—light wind. P.M. Overcast—light rain & wind.
W 6	29.893	44.8	29.857	48.2	41	44.2	47.7	40.8	48.2	.111	SE var.	Overcast—light rain and wind throughout the day.
T 7	29.348	45.0	29.169	48.6	41	43.6	50.3	40.3	50.3	.061	S var.	A.M. Overcast—light rain. P.M. Fine—light clouds & wind. Evening, Overcast—light rain and wind.
F 8	29.006	47.4	29.095	51.6	41	46.4	48.5	38.3	49.8	.355	SE var.	A.M. Fine—light clouds and wind. P.M. Overcast—light brisk wind. Evening, Overcast—light rain.
S 9	29.283	46.2	29.334	48.3	42	42.4	47.0	40.2	46.7	.036	E	A.M. Ovct.—lt. rain & wind. P.M. Fine—lt. clds. & wind. Ev. Cldy.
☉ 10	29.550	49.9	29.580	51.8	43	45.4	52.9	40.2	53.2	.194	N	A.M. Lightly overcast—light wind. P.M. Fine—light clouds and wind. Evening, Cloudy—light wind.
M 11	29.707	47.0	29.722	50.0	43	43.7	47.7	42.8	48.8	.019	N	A.M. Overcast—light brisk wind. P.M. Fine—light clouds. Evening, Cloudy.
T 12	29.775	49.2	29.776	51.6	42	48.2	51.2	38.5	53.7		SW	Lightly overcast—light wind. Evening, Cloudy.
W 13	29.816	51.3	29.756	54.5	46	51.4	55.7	45.2	56.4		SW	Overcast—light wind. Evening , Cloudy.
T 14	29.992	50.5	29.981	52.3	43	48.2	50.5	42.9	54.5		W	A.M. Overcast—light wind. P.M. Overcast—very light rain.
● F 15	30.152	53.4	30.180	56.6	49	50.3	53.5	47.5	54.2		NNW	A.M. Overcast—deposition—light wind. P.M. Cloudy. Evening, Fine and clear.
S 16	30.162	49.3	30.095	52.3	45	43.4	52.7	38.8	52.6	.036	E	A.M. Foggy. P.M. Lightly overcast.
☉ 17	30.115	50.0	30.162	50.5	43	46.6	47.5	42.3	47.8	.027	E	Overcast—light rain. Evening, Cloudy.
M 18	30.095	50.2	30.083	54.4	44	48.2	54.3	42.0	54.7	.061	S	A.M. Overcast—light wind. P.M. Cloudy—light wind.
T 19	30.117	53.4	30.093	56.2	45	51.2	54.0	44.3	56.2		S	A.M. Cloudy—lt. brisk wind. P.M. Overcast. Evening, Cloudy.
W 20	29.984	55.3	29.893	56.8	47	53.3	54.3	48.5	55.2		SW var.	A.M. Cloudy—light brisk wind. P.M. Overcast—light rain. Evening, Cloudy.
T 21	29.936	55.0	29.909	57.6	44	49.5	56.0	43.3	56.3	.063	SW	Cloudy—light wind. Evening, Overcast—light rain.
F 22	29.814	55.6	29.855	58.4	48	53.6	61.0	47.2	61.3	.061	SW	A.M. Cloudy—light wind. P.M. Fine—light clouds and wind. Evening, Overcast—light rain.
S 23	29.890	54.5	29.802	57.4	48	51.6	52.3	47.4	54.6	.094	SW	A.M. Overcast—light rain and wind. P.M. Fine—light clds. and wind. Evening, Cloudy.
☉ 24	29.699	52.3	29.740	52.7	46	46.9	47.6	45.3	47.9	.161	E	Overcast—light steady rain throughout the day.
M 25	30.117	51.7	30.117	54.9	43	45.8	54.4	39.7	54.7	.375	NW	Fine—light clouds and wind. Evening, Cloudy.
T 26	30.111	55.6	29.981	55.2	43	50.6	52.4	44.4	53.5		W	A.M. Fine—light clouds and wind. P.M. Lightly overcast—light wind. Evening, Cloudy—light brisk wind.
W 27	29.780	49.6	29.804	52.4	38	44.6	48.3	38.2	48.7		NW	A.M. Overcast—light brisk wind. P.M. Cloudy—rain and hail. Evening, Fine and clear.
T 28	29.938	53.0	29.800	53.3	38	46.3	51.7	37.5	52.3	.036	NNW	Cloudy—light wind throughout the day.
F 29	29.903	53.3	29.849	51.9	32	39.4	46.6	35.5	46.7	.027	NW var.	Fine—light clouds and wind. Evening, Fine and clear.
S 30	29.774	50.3	29.718	51.6	32	42.3	49.6	32.2	49.4		WSW	Fine—light clouds and wind. Evening, Cloudy.
MEANS ..	29.857	49.7	29.832	52.0	41.5	46.1	50.1	40.5	51.3	Sum. 2.488	Mean of Barometer, corrected for Capillarity and reduced to 32° Fahr.	9 A.M. 29.808; 3 P.M. 29.777

METEOROLOGICAL JOURNAL FOR MAY AND JUNE, 1836.

1836.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	External Thermometer. Fahrenheit. 3 P.M.	External Thermometer. Self-registering. Lowest.	External Thermometer. Self-registering. Highest.	Rain in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
MAY ☉ 1	29.794	48.7	29.800	52.3	39	43.6	51.2	35.3	49.0		NW	A.M. Cloudy—light wind. P.M. Fine—light clouds and wind. Evening, Cloudy—light showers.
M 2	29.855	48.2	29.909	52.6	40	49.5	56.6	39.3	52.3	.052	NE var.	Cloudy—light brisk wind.
T 3	29.940	48.8	29.853	53.8	40	46.3	50.2	40.4	53.3		NE var.	A.M. Cloudy—high wind. P.M. Cloudy—high wind—light rain. Evening, Overcast—continued rain.
W 4	29.679	49.8	29.683	53.7	44	49.2	50.2	43.7	54.6	.091	N	Overcast—light rain and wind throughout the day.
T 5	29.699	49.3	29.780	53.2	43	45.7	51.7	42.5	50.6	.133	N	A.M. Overcast—light rain and wind. P.M. Fine—light clouds and wind. Evening, High wind.
F 6	30.073	52.3	30.122	55.7	41	50.5	58.4	42.3	58.7	.355	E	A.M. Cloudy—light brisk wind. P.M. Fine—light clouds with brisk wind. Evening, Fine and clear.
S 7	30.295	57.2	30.267	56.7	42	53.3	57.6	40.8	56.6		N	Fine and cloudless—light wind. Evening, Cloudy.
☉ 8	30.275	51.3	30.219	55.2	42	47.7	57.8	43.0	58.3		N	A.M. Cloudy—light wind. P.M. Fine and cloudless—light wind. Evening, Fine and clear.
M 9	30.259	58.7	30.237	61.2	44	49.7	55.7	43.2	57.2		NE	Fine—nearly cloudless—light wind. Evening, Fine and clear.
T 10	30.212	53.5	30.190	57.4	41	50.2	58.6	42.3	58.6		NE	A.M. Cloudy—light brisk wind. P.M. Fine—light clouds and wind. Evening, Fine and clear.
W 11	30.204	57.2	30.150	59.3	43	53.6	65.2	40.5	64.8		WSW	Fine & cloudless—light haze and wind. Evening, Fine & clear.
T 12	30.216	59.5	30.214	61.4	45	57.4	68.3	46.5	68.6		SW	A.M. Fine—light clouds and wind. P.M. Fine and cloudless—light wind. Evening, Fine and clear.
F 13	30.356	61.9	30.348	63.4	47	58.5	67.6	46.8	67.4		W	Fine and cloudless—light haze. Evening, Fine and clear.
S 14	30.562	66.5	30.540	65.7	53	60.2	66.8	51.5	67.4		N	Fine and cloudless—light wind. Evening, Fine and clear.
● ☉ 15	30.608	64.5	30.523	65.8	50	57.7	64.0	49.9	66.7		E	A.M. Fine—thick haze. P.M. Fine and clear.
M 16	30.548	66.3	30.511	67.3	51	60.0	68.9	50.9	69.5		N	A.M. Hazy—light wind. P.M. Fine and cloudless.
T 17	30.564	66.0	30.507	66.9	52	59.3	67.3	49.6	68.5		SE	Fine and cloudless—light wind.
W 18	30.388	64.0	30.321	66.5	53	57.2	63.3	50.5	65.3		E	Fine and cloudless—light wind. Evening, Cloudy.
T 19	30.311	58.3	30.339	61.7	49	51.4	57.7	48.0	58.2		E	A.M. Overcast—light wind. P.M. Fine and cloudless.
F 20	30.089	57.4	30.002	62.6	47	53.5	66.3	44.3	66.5		E	Fine—light clouds—haze and wind. Evening, Cloudy—rain during the night.
S 21	30.016	61.5	30.033	64.8	52	56.7	59.8	49.6	61.7	.133	NE	Fine—nearly cloudless—light wind. Evening, Fine and clear.
☉ 22	30.010	63.3	29.917	61.0	43	52.6	54.8	42.6	55.3		E	Fine—light clouds and wind. Evening, Cloudy.
M 23	29.846	54.7	29.899	60.4	45	52.3	57.3	44.0	57.3	.044	S var.	A.M. Overcast—very light rain and wind. P.M. Fine—light clouds and wind. Evening, Cloudy.
T 24	30.109	55.7	30.148	60.0	43	51.8	57.4	46.3	59.3		NE var.	A.M. Cloudy—light brisk wind. P.M. Fine—light clouds & wind. Evening, Fine and clear.
W 25	30.265	57.3	30.253	60.9	45	51.2	57.6	44.2	58.2		NNE	A.M. Lightly ovct.—lt. wind. P.M. Fine—lt. clds. & wind. Ev. Fine [and clear.
T 26	30.338	60.9	30.346	59.4	44	53.2	56.2	45.7	56.2		E	A.M. Cloudy—light brisk wind. P.M. Fine and cloudless—light wind. Evening, Fine and clear.
F 27	30.425	60.0	30.395	59.0	42	54.6	58.3	41.8	58.9		E	A.M. Fine—light clouds and wind. P.M. Fine and cloudless—light wind. Evening, Fine and clear.
S 28	30.396	60.3	30.334	60.8	44	53.2	62.5	42.0	62.7		NE var.	A.M. Overcast—light brisk wind. P.M. Fine—light clouds and wind. Evening, Cloudy.
☉ 29	30.380	65.0	30.330	63.6	45	54.7	63.3	47.3	63.8		NE	Fine and cloudless—light wind. Evening, Fine and clear.
○ M 30	30.301	61.2	30.287	63.2	46	54.7	63.5	43.4	65.0		NNE	Fine—light clouds and wind. Evening, Fine and clear.
T 31	30.109	62.4	29.998	64.2	46	55.3	65.6	47.4	67.6		N	Fine—light clouds and wind. Evening, Cloudy—light rain and wind.
MEANS..	30.197	58.1	30.176	60.3	45.2	53.1	60.0	44.7	60.6	Sum. .808		Mean of Barometer, corrected for Capillarity and reduced to 32° Fahr. ...... 9 A.M. 30.123; 3 P.M. 30.094
JUNE W 1	29.927	60.0	29.901	63.6	50	56.0	59.2	49.2	61.6	.027	NE	Overcast—very light rain and wind.
T 2	29.736	59.8	29.637	65.4	52	57.2	61.0	51.0	63.7		E	Overcast—light rain, with light brisk wind.
F 3	29.614	64.0	29.596	67.3	54	62.3	64.6	53.8	66.7	.069	S	A.M. Cloudy—light rain and wind. P.M. Fine—light clouds and wind. Evening, Cloudy—light rain.
S 4	29.619	64.0	29.621	67.6	54	59.7	66.9	55.3	67.4	.036	SSW	A.M. Overcast—very light rain—light brisk wind. P.M. Fine—light clouds and wind. Evening, Overcast—light rain.
☉ 5	29.780	70.3	29.837	65.2	54	60.7	56.8	50.9	62.6	.111	SW	A.M. Fine—light clouds and wind. P.M. Overcast—light rain and wind. Evening, Cloudy.
M 6	30.058	65.3	30.027	67.5	50	55.9	65.0	47.7	66.8	.038	WSW	A.M. Fine—lt. clouds & wind. P.M. Overcast. Evening, Cldy.
T 7	29.940	61.7	29.855	64.9	51	58.0	58.5	53.9	62.6		S	Overcast—very light rain and wind.
W 8	29.606	63.5	29.623	67.4	53	62.6	67.2	54.6	68.3		SW	A.M. Cloudy—light wind. P.M. Fine—light clouds and wind. Evening, Cloudy.
T 9	29.759	65.4	29.835	68.3	55	61.8	64.7	54.7	66.3		S	A.M. Fine—light clouds and wind. P.M. Cloudy—light wind. High wind throughout the night.
F 10	29.837	64.9	29.825	67.2	57	61.9	66.8	58.0	67.6		S	A.M. Overcast—high wind. P.M. Cloudy. Evening, Overcast—light rain, with high wind.
S 11	29.756	67.9	29.756	70.9	58	64.9	64.6	59.2	68.6	.175	SSE	Cloudy—light brisk wind.
☉ 12	30.146	71.7	30.194	68.6	56	62.3	65.3	51.0	65.2		SW	Fine—light clouds and wind.
M 13	30.317	66.5	30.324	69.2	56	64.7	70.2	53.0	72.3		SW	Cloudy—light wind.
● T 14	30.269	70.4	30.188	72.2	60	67.5	74.4	59.2	70.7		SW	A.M. Overcast. P.M. Fine—light clouds. Evening, Cloudy.
W 15	29.958	74.4	29.865	73.6	60	69.0	76.6	55.3	77.6		E	Fine—nearly cloudless. Evening, Fine and clear.
T 16	29.934	74.5	29.948	75.3	61	67.4	72.5	63.5	73.6		SSE	Cloudy—light wind. Evening, Fine and clear.
F 17	29.907	70.2	29.840	73.5	59	64.8	68.4	58.2	72.3		S	A.M. Fine—thick haze—light wind. P.M. Overcast. Evening, Overcast—light rain and wind.
S 18	29.841	76.0	29.764	74.5	59	64.6	70.4	53.9	71.3	.075	SSW	Fine—light clouds & wind. Evening, Cloudy—very light rain.
☉ 19	29.748	70.6	29.790	72.6	58	63.6	68.7	55.2	70.3		SW	A.M. Fine—light clouds and wind. P.M. Overcast—light rain, with thunder and lightning.
M 20	30.035	74.0	30.033	72.2	59	61.4	66.5	55.2	68.6	.102	SW	Fine—light clouds and wind. Evening, Fine and clear.
T 21	30.033	66.2	30.005	70.4	57	60.6	66.4	53.3	70.3		W	Overcast—light steady rain and wind.
W 22	29.950	67.3	29.928	68.8	60	63.2	63.6	58.0	65.3	.094	S var.	Overcast—light rain and wind. Evening, Fine and clear.
T 23	29.825	66.0	29.820	70.2	60	63.0	67.2	59.2	68.2	.016	S var.	A.M. Overcast—light brisk wind. P.M. Fine—light clouds. Evening, Cloudy.
F 24	29.804	66.8	29.764	70.3	58	62.2	65.3	56.7	68.2		SE var.	A.M. Overcast—lt. brisk wind. P.M. Cloudy—lt. rain & wind.
S 25	30.073	72.2	30.115	70.0	55	62.6	64.2	52.4	67.3	.119	SW	A.M. Lightly overcast—light brisk wind. P.M. Cloudy—light showers.
☉ 26	30.284	69.2	30.313	69.5	58	64.9	66.4	54.6	68.7	.036	SW	A.M. Fine—light clouds and wind. P.M. Cloudy.
M 27	30.342	67.2	30.285	70.3	59	64.2	71.4	56.9	72.0		S	A.M. Overcast. P.M. Cloudy—light wind.
○ T 28	30.136	76.4	30.115	73.5	61	71.3	80.3	58.9	82.6		SSE	Fine—light clouds and wind. Evening, Cloudy.
W 29	30.342	75.6	30.321	73.6	58	64.5	71.5	57.2	72.6		NE	A.M. Fine—nearly cloudless—light wind. P.M. Fine—light clouds. Evening, Cloudy.
T 30	30.334	75.5	30.253	74.2	56	67.2	71.3	55.3	72.7		E	Fine—light clouds and wind. Evening, Cloudy, with occasional flashes of lightning.
MEANS..	29.964	68.6	29.946	69.9	56.6	63.0	67.2	55.2	69.1	Sum. .898		Mean of Barometer, corrected for Capillarity and reduced to 32° Fahr. ...... 9 A.M. 29.858; 3 P.M. 29.837

METEOROLOGICAL JOURNAL,

KEPT BY THE ASSISTANT SECRETARY,

AT THE APARTMENTS OF THE

ROYAL SOCIETY,

BY ORDER OF

THE PRESIDENT AND COUNCIL.

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge.............. =83 feet 2½ in.

———————————— above the mean level of the Sea (presumed about) ....=95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House................=79 feet.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR JULY AND AUGUST, 1836.

1836.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	External Thermometer. Fahrenheit. 3 P.M.	External Thermometer. Self-registering. Lowest.	External Thermometer. Self-registering. Highest.	Rain in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
JULY F 1	30.136	71.2	30.134	74.7	62	68.2	81.2	61.9	77.6		E	A.M. Thick haze. P.M. Fine & cloudless. Evening, Cloudy.
S 2	30.220	79.8	30.200	79.2	67	75.0	80.6	65.3	76.9		SE	Fine—light clouds and wind. Evening, Fine and clear.
⊙ 3	30.275	79.7	30.243	77.9	60	69.6	78.8	56.4	79.9		SW	A.M. Cloudy—light wind. P.M. Fine and cloudless. Evening, Fine and clear.
M 4	30.291	81.3	30.229	79.5	63	74.6	84.6	61.6	85.2		SW	Fine and cloudless—light haze. Evening, Fine and clear.
T 5	30.170	79.3	30.087	79.9	65	76.2	83.6	63.8	77.8		E	A.M. Fine—light clouds. P.M. Fine and cloudless. Evening, Cloudy, with lightning.
W 6	30.071	80.0	30.126	81.4	70	72.7	74.2	67.2	77.6	.116	SSW	A.M. Thunder and lightning, with heavy rain and wind. P.M. Fine—light clouds. Evening, Cloudy.
T 7	30.261	77.4	30.249	78.4	58	66.0	72.2	57.2	73.6		SSW	Fine—light clouds and wind. Evening, Cloudy.
F 8	30.305	74.4	30.301	75.6	58	64.5	71.2	58.3	73.5		WSW	Fine—light clouds, with light brisk wind. Evening, Cloudy.
S 9	30.257	75.6	30.220	76.6	59	69.6	75.4	58.9	76.7		SW	A.M. Cloudy—light wind. P.M. Fine—nearly cloudless. Evening, Cloudy.
⊙ 10	30.158	75.3	30.144	77.3	65	72.0	78.8	61.4	80.7		SW	A.M. Cloudy—light wind. P.M. Fine—light clouds and wind. Evening, Cloudy.
M 11	30.142	81.3	30.041	79.5	65	73.3	80.7	63.8	81.2		SSW	Fine and cloudless—light wind. Evening, Cloudy.
T 12	29.734	73.8	29.814	77.4	67	67.2	71.5	65.0	74.3		SSW	A.M. Overcast—very light rain. P.M. Cloudy—light shower. Evening, Cloudy.
● W 13	30.075	77.6	30.004	75.5	53	64.5	71.6	53.8	72.2		SW	A.M. Cloudy—light wind. P.M. Fine and cloudless—light wind. Evening, Overcast—light brisk wind.
T 14	30.002	75.5	30.006	75.4	59	65.4	70.6	58.6	71.7		SW	Fine—light clouds, with light brisk wind. Evening, Cloudy.
F 15	29.898	69.2	29.757	71.4	59	61.6	63.8	57.0	66.4		S	A.M. Overcast—light brisk wind. P.M. Overcast—heavy rain, with high wind. Evening, Fine and clear.
S 16	29.774	69.6	29.828	71.8	53	61.2	68.0	50.4	69.0	.316	SW	Fine—light clouds and wind. Evening, Overcast—light wind.
⊙ 17	29.911	68.2	29.998	71.2	54	61.8	68.6	58.5	69.8		W	A.M. Lightly overcast—light brisk wind. P.M. Fine—light clouds and wind. Evening, Cloudy.
M 18	30.140	72.4	30.093	71.6	54	62.6	69.6	54.8	70.8		W	A.M. Lightly overcast—light brisk wind. P.M. Fine—light clouds and wind. Evening, Fine and clear.
T 19	29.938	66.9	29.786	69.9	54	62.8	64.7	52.7	67.8		SSW	Cloudy—light rain, with light brisk wind. Evening, Overcast—light rain.
W 20	29.560	62.5	29.453	64.2	54	54.3	57.8	52.8	59.2	.311	E	A.M. Overcast—lt. steady rain & wind. P.M. Cloudy—lt. wind.
T 21	29.677	67.7	29.665	61.2	51	59.2	61.3	46.2	63.2	.269	SW	A.M. Fine—light clouds and wind. P.M. Overcast—thunder and lightning, with heavy rain. Evening, Fine and clear.
F 22	29.794	64.9	29.816	65.9	51	58.5	61.4	50.3	65.0	.233	WSW	A.M. Lightly overcast—light rain and wind. P.M. Cloudy—light wind.
S 23	30.070	65.8	30.069	66.3	52	60.4	64.6	51.7	65.8		SW	Fine—light clouds and wind. Evening, Cloudy.
⊙ 24	29.835	61.3	30.705	65.3	53	55.8	64.3	51.3	65.0	.047	S	A.M. Overcast—light steady rain & wind. P.M. Cloudy—light wind.
M 25	29.800	62.3	29.895	67.2	54	57.0	64.7	52.8	65.7	.186	WNW	Overcast—light brisk wind. Evening, very light rain.
T 26	30.138	67.7	30.134	69.5	55	62.6	68.2	54.3	71.3		S	A.M. Fine—light clouds and wind. P.M. Lightly overcast.
W 27	30.168	66.3	30.164	71.3	59	64.7	72.9	60.2	73.6		SW	A.M. Overcast. P.M. Cloudy—light wind. Evening, Fine & clear.
○ T 28	30.091	72.5	30.014	72.8	60	67.5	75.5	55.8	76.8		SSW	A.M. Fine—nearly cloudless. P.M. Fine—light clouds and wind. Evening, Cloudy.
F 29	29.689	68.0	29.594	72.5	62	63.3	68.2	61.4	69.7	.250	NE var.	Overcast—light rain and wind. Heavy rain, 6½ A.M. Evening, Cloudy—light wind.
S 30	29.990	67.4	30.152	69.2	54	61.6	61.4	54.9	65.5		SW	Fine—light clouds, with light brisk wind. Evening, Cloudy.
⊙ 31	30.453	70.3	30.433	69.8	54	59.2	66.2	50.5	66.8	.158	SW	A.M. Fine—light clouds & wind. Evening, Cloudy—light wind.
MEANS..	30.033	71.8	30.001	72.9	58.2	68.2	70.8	57.1	71.9	Sum. 1.886		Mean of Barometer, corrected for Capillarity and reduced to 32° Fahr. …… 9 A.M. 29.919 3 P.M. 29.883
AUGUST M 1	30.181	64.7	30.059	69.7	56	60.4	69.0	54.3	69.7		SSW	A.M. Overcast—light rain and wind. P.M. Fine—light clouds and wind. Evening, Cloudy.
T 2	30.080	70.0	30.097	68.9	51	68.9	67.6	51.6	69.0		SW	Fine—light clouds and wind. Evening, Fine and clear.
W 3	30.029	70.7	29.871	72.3	57	65.3	74.4	54.3	74.6		SE	Fine—light clouds and wind. Evening, Fine and clear.
T 4	29.859	69.3	29.899	72.4	60	66.3	68.8	57.0	72.2		SW	Lightly overcast—light wind. Evening, Cloudy.
F 5	30.000	66.7	30.016	71.3	60	61.6	66.8	58.7	70.5		N	A.M. Overcast—light wind. P.M. Overcast—light rain and wind. Evening, Fine and clear.
S 6	30.119	67.7	30.158	71.5	59	63.6	65.4	57.2	67.5		N	Overcast—light brisk wind. Evening, Fine and clear.
⊙ 7	30.245	68.2	30.204	70.2	57	59.2	67.5	51.9	68.9		N	A.M. Fine—light clouds and wind. P.M. Overcast.
M 8	30.202	70.2	30.186	71.0	57	62.0	70.0	54.5	70.5		N	Fine—light clouds and wind. Evening, Fine and clear.
T 9	30.241	67.0	30.188	68.9	58	60.8	66.4	54.9	69.2		N	A.M. Lightly overcast—light wind. P.M. Fine & cloudless—light wind.
W 10	30.204	66.4	30.166	69.0	57	59.6	69.5	50.9	69.0		N	Fine—light clouds, with light brisk wind. Evening, Cloudy.
T 11	30.307	64.4	30.303	68.9	54	62.0	69.0	53.7	68.9		NE	A.M. Cloudy—brisk wind. P.M. Fine—light clouds, with brisk wind. Evening, Cloudy.
● F 12	30.348	64.0	30.303	67.5	58	59.5	68.8	56.4	69.0		ENE	A.M. Overcast—light brisk wind. P.M. Fine—nearly cloudless.
S 13	30.225	69.2	30.122	69.9	62	65.4	72.0	54.5	72.0		NE	A.M. Fine—light clouds, with brisk wind. P.M. Fine—light clouds and wind.
⊙ 14	29.930	66.0	29.887	71.0	62	62.2	71.8	59.5	71.9	.272	N	A.M. Overcast—light rain—thunder and lightning, with heavy rain early. P.M. Cloudy—light wind.
M 15	29.960	67.0	29.988	71.2	62	62.5	71.8	59.0	72.5		WSW	Overcast—light wind.
T 16	30.194	65.0	30.158	69.0	59	62.7	68.7	54.5	71.6		NNE	A.M. Overcast—light wind. P.M. Cloudy.
W 17	30.111	69.5	30.113	72.9	63	66.4	73.2	57.2	73.4		WSW	Fine—light clouds and wind.
T 18	30.081	68.0	29.994	72.0	61	63.8	71.0	56.7	72.0		SW	Overcast—light wind.
F 19	30.196	69.0	30.188	69.8	52	59.2	66.5	54.5	67.0	.044	SSW var.	A.M. Cloudy—light brisk wind. P.M. Fine—light clouds & wind.
S 20	29.956	64.5	29.766	66.5	58	59.0	62.0	52.4	62.4		SSW	Overcast throughout the day.
⊙ 21	29.954	64.0	29.950	67.0	52	58.5	65.3	51.9	66.8	.075	NNE	A.M. Overcast. P.M. Fine—light clouds.
M 22	29.794	63.9	29.724	69.0	58	59.0	66.7	53.3	67.4	.013	SW var.	A.M. Overcast. P.M. Cloudy—light wind.
T 23	29.641	64.5	29.691	67.5	57	60.2	63.6	57.2	63.9	.027	N	Overcast throughout the day.
W 24	29.976	60.2	30.079	65.0	55	55.4	61.5	49.5	61.5	.125	NE	A.M. Overcast—brisk wind. P.M. Fine—light clouds and wind.
T 25	30.168	61.2	30.115	64.3	53	58.2	64.4	48.4	65.3		E	Overcast throughout the day.
○ F 26	29.984	61.5	29.976	64.5	58	59.0	65.2	55.6	65.5		SW	Overcast—light rain and wind.
S 27	29.936	66.0	29.913	67.6	57	62.0	69.5	52.7	70.2	.088	WSW	Fine—light clouds and wind.
⊙ 28	29.964	62.9	29.968	66.8	59	59.2	65.0	56.4	65.7	.161	E	A.M. Overcast—light rain. P.M. Cloudy.
M 29	30.148	64.9	30.130	67.8	54	58.5	67.0	51.0	67.7	.188	WNW	A.M. Fine—light clouds and wind. P.M. Cloudy. Evening, Fine and clear.
T 30	30.212	64.8	30.176	68.9	55	61.0	68.0	52.5	68.0		SSW	P.M. Overcast—light wind. P.M. Cloudy.
W 31	30.113	69.0	30.035	68.9	60	63.5	69.8	56.0	70.9		SSW	Fine—light clouds and wind. Evening, Fine and clear.
MEANS..	30.076	66.1	30.046	69.1	57.5	61.5	67.9	54.5	68.9	Sum. .993		Mean of Barometer, corrected for Capillarity and reduced to 32° Fahr. …….. 9 A.M. 29.978 3 P.M. 29.939

*** The observations for this month were not taken by the Assistant Secretary on account of absence.

METEOROLOGICAL JOURNAL FOR SEPTEMBER AND OCTOBER, 1836.

1836.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	External Thermometer. Fahrenheit. 3 P.M.	External Thermometer. Self-registering. Lowest.	External Thermometer. Self-registering. Highest.	Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
SEPTEMBER T 1	29.816	69.8	29.794	69.4	57	66.4	68.5	56.2	69.6		SW	Fine—nearly cloudless—light wind. Evening, Cloudy.
F 2	29.768	65.4	29.724	66.4	57	60.3	59.6	54.4	63.5		SW var.	A.M. Cloudy—very light rain and wind. P.M. Fine—light clouds. Evening, Fine and clear.
S 3	29.959	63.3	29.829	66.4	52	57.1	63.3	46.9	64.2	.169	S	A.M. Fine—lt. clds. & wind. P.M. Cloudy. Rain during the night.
⊙ 4	29.386	61.8	29.421	68.0	55	61.8	66.9	54.0	68.0	.463	SW	A.M. Overcast—heavy rain early. P.M. Fine—light clouds and wind. Evening, Overcast—light rain and wind.
M 5	29.560	62.7	29.633	66.2	54	56.4	62.4	53.5	62.5	.113	SW var.	A.M. Fine—light clouds—light brisk wind. Cloudy, with light wind, during the remainder of the day.
T 6	29.360	64.3	29.309	65.2	54	56.3	61.4	50.9	61.7	.091	SSE var.	A.M. Overcast—very lt. rain with high wind. 1 P.M. Thunder, with heavy rain and wind. Evening, Cloudy.
W 7	29.641	60.2	29.712	63.3	52	53.7	59.8	51.7	60.5	.158	NW	Fine—light clouds & wind throughout the day. Evening, Cloudy.
T 8	29.780	60.0	29.732	63.6	53	55.9	63.0	52.0	63.6		WNW	Fine—light clouds. Evening, Cloudy.
F 9	29.746	59.7	29.802	62.2	53	57.0	53.8	51.9	62.5		SSW	A.M. Lightly overcast—high wind. P.M. Overcast—heavy rain and wind. Evening, Fine and clear.
S 10	29.792	57.3	29.831	60.9	50	53.6	56.3	46.5	56.8	.272	WNW	A.M. Fine—light clouds and wind, with light shower. P.M. Fine—light clouds. Evening, Cloudy.
○⊙ 11	30.025	56.8	29.998	58.8	46	50.6	51.8	42.4	56.7	.050	WSW	A.M. Fine—very light clouds and wind. P.M. Overcast—light steady rain & wind. Ev., Ovct.—continued rain—h. wind.
M 12	30.002	56.2	30.033	59.0	50	54.5	55.7	49.5	57.2	.122	NE	Overcast—light rain, with high wind.
T 13	30.005	57.9	30.069	61.2	51	55.8	56.7	51.7	58.3		NNE	A.M. Fine—light clouds and wind. P.M. Overcast—light brisk wind. Evening, Fine and clear.
W 14	30.117	56.6	30.130	60.0	52	55.8	56.6	48.4	59.0	.022	NNE	Cloudy—light wind throughout the day.
T 15	30.170	57.6	30.182	60.0	52	55.2	59.4	47.4	60.2		NE	A.M. Fine—light clouds and wind. P.M. Overcast—light wind. Evening, Cloudy.
F 16	30.075	56.0	30.058	59.3	51	53.7	58.2	50.0	58.4		NE var.	Overcast—light rain and wind. Evening, Cloudy.
S 17	30.077	57.5	30.045	59.6	50	56.4	58.6	49.5	60.6	.088	NE	A.M. Fine—light clouds and wind. P.M. Overcast—light rain.
⊙ 18	30.002	57.2	29.998	60.3	52	54.8	56.7	50.5	58.5	.141	NNE	Overcast—light brisk wind. Evening, Cloudy—light wind.
M 19	29.974	55.3	29.942	58.6	48	51.9	56.8	48.3	58.2		NW	Cloudy—light wind throughout the day.
T 20	29.989	54.9	29.954	59.4	48	51.6	58.8	48.7	59.0		W	Overcast—very light rain and wind. Evening, Cloudy—light rain.
W 21	30.152	53.5	30.176	57.1	45	48.4	51.9	42.6	52.3	.091	W	A.M. Cloudy—light wind. P.M. Fine—light clouds and wind. Evening, Cloudy.
T 22	30.277	51.6	30.235	56.2	43	47.4	56.3	40.7	57.8		W	A.M. Light fog. P.M. Cloudy—light wind.
F 23	29.958	55.3	29.897	58.0	52	57.8	62.6	46.7	63.2	.044	SW var.	Overcast—very light rain with high wind throughout the day.
● S 24	30.152	59.6	30.107	61.6	54	60.0	66.4	53.9	66.4	.161	WSW	A.M. Fine—light clouds and wind. P.M. Fine—nearly cloudless. Evening, Fine and clear.
⊙ 25	30.178	60.6	30.180	63.8	55	59.9	66.3	53.9	66.7		SW	A.M. Cloudy—light wind. P.M. Fine—light clouds and wind. Evening, Fine and clear.
M 26	30.144	61.5	30.053	65.7	57	60.4	67.3	57.9	67.9		SSW	A.M. Overcast—light wind. P.M. Fine—light clouds and wind. Evening, Cloudy—light wind.
T 27	29.837	63.4	29.836	66.3	59	62.3	64.3	58.6	65.2		SSW	A.M. Ovct—lt. wind. P.M. Cloudy—lt. brisk wind. Ev. Cloudy.
W 28	29.683	62.4	29.592	63.9	57	58.6	57.4	52.2	62.6	.047	SSW	A.M. Fine—light clouds—rain during the night. P.M. Overcast—heavy rain, 1 P.M. Evening, Fine and clear.
T 29	29.272	59.3	29.316	61.8	54	54.4	59.2	51.9	58.9	.486	NNE	A.M. Overcast—lt. rain & wind. P.M. Overcast—continued rain.
S 30	29.426	58.3	29.477	59.6	49	52.5	52.3	46.6	54.6	.255	WSW	A.M. Cloudy—light wind. P.M. Fine—light clouds and wind. Evening, Overcast—light rain.
MEANS ..	29.877	59.2	29.869	62.1	52.1	56.0	59.6	50.3	61.2	Sum. 2.773		Mean of Barometer, corrected for Capillarity and reduced to 32° Fahr........ 9 A.M. 29.800 3 P.M. 29.783
OCTOBER S 1	29.356	53.7	29.085	55.9	48	52.3	51.4	41.5	55.3	.125	SE var.	A.M. Overcast—light brisk wind. P.M. Overcast—light rain, with high wind.
⊙ 2	29.247	53.0	29.346	55.5	44	47.9	51.2	42.9	52.5	.294	SW	A.M. Fine—light clouds and wind. P.M. Cloudy—light wind. Evening, Overcast—light steady rain.
M 3	29.008	51.6	29.322	54.2	44	43.8	50.3	43.4	52.2	.380	SSW var	A.M. Overcast—very light rain, with high wind. P.M. Fine—nearly cloudless. Evening, Fine and clear.
T 4	29.497	48.4	29.523	52.3	41	45.2	53.6	39.3	53.3		SSW	A.M. Foggy—lt. wind. P.M. Fine—lt. clouds & wind. Ev. Foggy.
W 5	29.829	49.0	29.886	53.0	40	44.6	56.3	40.4	56.2		SSE	A.M. Foggy. P.M. Fine—light clouds & wind. Ev. Overcast.
T 6	29.853	50.0	29.748	53.8	45	49.3	54.2	43.3	57.2	.036	ESE	Overcast—very light rain throughout the day.
F 7	29.511	54.8	29.423	56.7	51	56.6	58.8	48.6	59.0	.752	SE	Overcast—light rain and wind throughout the day.
S 8	29.386	56.0	29.390	58.6	52	54.8	56.6	52.7	57.7	.091	SSE	Overcast—light wind throughout the day.
⊙ 9	29.360	54.6	29.402	57.4	50	51.7	55.2	47.3	56.3	.283	S	A.M. Fine & cloudless—lt. wind. Heavy rain early, with h. wind. P.M. Fine—lt. clds. with brisk wind. Ev. Overcast—lt. rain.
○ M 10	29.219	54.4	29.322	58.9	50	54.5	58.7	48.4	60.6	.133	SE var.	Overcast—light rain and wind. Evening, Cloudy.
T 11	29.247	57.9	29.388	60.0	52	55.7	58.4	53.8	58.8	.080	SSW	A.M. Fine—lt. clouds with high wind (very h. during the night). P.M. Fine—lt. clouds & wind. Ev. Overcast—very lt. rain.
W 12	29.606	55.4	29.530	58.4	48	52.4	55.9	46.9	58.7		S	A.M. Fine—light clouds. P.M. Overcast—light rain & wind.
T 13	29.140	57.5	29.333	59.5	52	58.7	58.9	51.5	59.2	.097	SW var.	A.M. Ovct.—lt. rain with h. wind (very high wind during the night). P.M. Fine—lt. clds. & wind. Ev. Cldy.—lt. rain.
F 14	29.782	56.4	29.717	59.2	50	54.5	57.7	51.3	58.5	.172	SW	A.M. Fine—lt. clds. & wind. P.M. Overcast—very lt. rain & wind.
S 15	29.853	56.2	29.887	59.2	52	55.6	59.3	51.0	59.3		E	A.M. Overcast—light rain and wind. P.M. Fine—light clouds and wind. Evening, Fine and clear.
⊙ 16	30.229	54.7	30.212	57.8	48	48.8	57.5	46.4	57.0	.061	SSW	A.M. Thick fog. P.M. Fine—light clouds and wind. Evening, Thick fog.
M 17	30.210	55.7	30.176	57.7	51	53.4	56.2	48.2	56.5		E	Overcast—very light rain and wind. Evening, Foggy.
T 18	30.174	56.9	30.150	60.3	53	56.2	59.4	52.8	61.2		SSE	A.M. Overcast. P.M. Fine—light clouds and wind. Evening, Cloudy.
W 19	30.241	58.5	30.322	60.3	54	57.6	58.8	54.4	59.4	.158	SW	A.M. Overcast—rain during the night. P.M. Fine and cloudless. Evening, Fine and clear.
T 20	30.469	53.4	30.418	54.0	43	46.2	50.2	41.8	50.3		SW	Thick fog throughout the day. Evening, Fine and clear.
F 21	30.332	52.2	30.305	55.5	46	49.2	53.3	44.8	52.6		ESE	A.M. Cloudy—light wind. P.M. Fine—light clouds & wind. Evening, Fine and clear.
S 22	30.398	51.2	30.382	53.9	45	49.3	53.6	45.8	53.8		E	A.M. Fine—light clouds and wind. P.M. Cloudy—light wind.
⊙ 23	30.408	49.3	30.378	52.2	42	44.7	54.6	40.4	55.2		E	A.M. Foggy. P.M. Fine & cloudless. Evening, Fine & clear.
● M 24	30.394	51.5	30.360	53.6	45	49.5	53.2	43.9	53.2		SSW	A.M. Foggy. P.M. Overcast. Evening, Cloudy.
T 25	30.371	52.6	30.225	53.2	48	49.6	50.6	51.7	51.3		SW	A.M. Overcast—deposition—light wind. Evening, Light fog.
W 26	30.196	52.6	30.113	54.4	50	50.9	52.4	48.7	52.7		SSW	A.M. Light fog and wind. P.M. Overcast—light wind.
T 27	29.663	52.7	29.837	52.5	47	51.8	43.7	48.8	47.7		SW	A.M. Cloudy—light brisk wind. P.M. Fine and cloudless. Evening, Overcast.
F 28	29.913	46.3	29.857	47.6	39	40.2	40.8	37.2	40.6	.080	WSW	A.M. Overcast—light rain. P.M. Fine—light clouds and wind. Evening, Cloudy.
S 29	29.473	42.3	29.703	41.6	35	35.6	33.8	33.6	34.2		S	A.M. Overcast—heavy fall of snow during the night. P.M. Overcast—light wind. Evening, Snow.
⊙ 30	30.019	39.0	30.035	41.0	33	35.2	37.7	32.9	37.7	.288	W	Fine and cloudless—light wind. Evening, Fine and clear.
M 31	30.089	37.3	30.009	40.0	30	33.2	39.5	30.0	39.3	.033	W	Fine and cloudless—light haze and wind. Evening, Cloudy.
MEANS ..	29.822	52.1	29.832	54.5	46.1	49.3	52.6	45.3	53.5	Sum. 3.063		Mean of Barometer, corrected for Capillarity and reduced to 32° Fahr. ...... 9 A.M. 29.766 3 P.M. 29.769

METEOROLOGICAL JOURNAL FOR NOVEMBER AND DECEMBER, 1836.

1836.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	Fahrenheit. 3 P.M.	Self-registering. Lowest.	Self-registering. Highest.	Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
NOVEMBER T 1	30.099	37.6	30.008	40.4	32	35.2	41.6	31.7	37.8		SSW	A.M. Fine—lt. clouds. & wind. P.M. Overcast—lt. rain & wind.
W 2	29.942	42.5	29.895	44.9	39	47.7	52.5	34.8	53.2	.044	SSW	Overcast—very light rain and wind throughout the day.
T 3	29.840	44.8	29.679	48.2	39	44.8	51.7	42.3	51.7		SW	Overcast—deposition. 5½ P.M. Heavy storm of hail and rain.
F 4	29.570	45.4	29.451	47.0	40	44.5	45.2	40.9	46.4	.080	SW	A.M. Overcast—light wind. P.M. Overcast—light rain.
S 5	29.140	45.6	29.287	47.2	40	45.4	44.9	42.8	46.2	.122	SW	A.M. Overcast—light rain and wind. P.M. Fine—nearly cloudless. Evening, Fine and clear.
⊙ 6	29.459	42.1	29.439	44.6	36	36.4	43.8	34.0	43.8		WSW	A.M. Fine and cloudless—light wind. P.M. Cloudy—lt. wind.
M 7	29.631	40.0	29.722	42.8	35	35.8	42.7	32.3	43.2	.055	WSW	Fine and cloudless—light wind. (Sharp frost during the night.) Evening, Fine and clear.
T 8	29.978	38.7	30.049	40.7	33	33.6	40.6	31.3	45.8		SW	A.M. Light fog and wind. (White frost.) P.M. Fine and cloudless. Evening, Overcast—light rain.
○ W 9	30.010	41.2	29.909	45.2	36	45.8	48.6	33.5	50.3	.036	SSE	A.M. Lightly ovct. P.M. Cldy.—lt. wind. Ev. Ovct.—lt. rain.
T 10	29.614	46.3	29.621	49.0	43	50.4	52.0	45.8	53.8	.211	S	A.M. Lightly cloudy. (Heavy rain during the night.) P.M. Overcast—rain and hail.
F 11	29.390	47.4	29.429	49.3	44	50.5	49.4	42.6	50.6	.136	E	Overcast—light rain and wind throughout the day.
S 12	29.821	46.9	29.863	48.0	41	41.4	46.2	40.2	52.0	.052	SW	A.M. Thick fog—deposition. P.M. Overcast—light wind. Evening, Overcast—light rain and wind.
⊙ 13	29.687	49.3	29.627	51.5	45	52.2	54.5	40.7	54.9	.038	SE var.	A.M. Overcast—light rain with high wind. (Very high wind throughout the night.) P.M. Ovct.—lt. rain with h. wind.
M 14	29.656	49.4	29.699	50.7	44	42.7	46.4	41.0	46.0	.216	SW	Fine & cloudless—lt. wind throughout the day. Ev. Fine & clear.
T 15	30.011	45.2	30.035	47.6	41	38.4	46.3	35.9	46.7		SW	A.M. Fine—nearly cloudless—light haze. P.M. Thick haze. Evening, Cloudy.
W 16	29.964	47.3	29.800	49.4	42	46.7	50.2	37.9	50.2		SW	A.M. Cloudy—lt. wind. P.M. Ovct.—very lt. rain. Ev. Cloudy.
T 17	29.475	48.2	29.434	49.2	43	46.7	47.4	45.2	47.5		SW	A.M. Fine—lt. clouds & wind. (High wind during the night.) P.M. Fine—nearly cloudless. Ev. Overcast—light rain.
F 18	29.103	46.7	29.172	48.0	42	40.8	43.7	39.8	44.0	.069	WSW	Fine—nearly cloudless throughout the day. Ev. Fine & clear.
S 19	29.253	42.5	29.186	44.6	38	37.4	42.0	34.4	38.7		SSW	A.M. Fine—light clouds and haze. P.M. Cloudy—light rain and wind. Evening, Overcast—light rain.
⊙ 20	29.784	42.2	29.917	43.7	37	38.7	42.8	36.9	42.7	.180	ENE	A.M. Fine—light clouds and wind. P.M. Fine and cloudless—light wind. Evening, Fine and clear.
M 21	30.119	40.0	30.071	41.2	34	36.7	38.2	32.6	41.5		SW	Thick fog throughout the day. Evening, Overcast—light rain.
T 22	29.960	41.5	29.889	41.8	37	38.8	39.2	33.8	49.5	.069	NW	A.M. Light fog, with deposition. P.M. Overcast. Evening, Overcast—light rain with high wind.
● W 23	29.272	43.7	29.377	45.2	38	44.6	44.8	37.9	44.3	.163	SW	A.M. Overcast—light brisk wind. P.M. Cloudy—light brisk wind. Evening, Fine and clear.
T 24	29.479	41.4	29.528	44.8	36	38.5	43.8	36.6	43.6		SW	A.M. Fine & cloudless—lt. wind. P.M. Cldy.—lt. wind. Ev. Fine & clear.
F 25	29.732	40.2	29.558	41.3	35	35.7	39.8	33.5	39.3		WNW	Overcast—light wind throughout the day. Evening, Overcast—light fog and rain.
S 26	29.356	39.8	29.274	41.4	36	39.7	44.0	34.5	50.7	.136	E	Lt. fog & rain throughout the day. Ev. Ovct.—lt. rain & wind.
⊙ 27	29.530	44.9	29.534	46.8	40	44.5	51.4	39.0	55.3	.158	SW	Overcast—light fog and rain, with light brisk wind. Ev. Ovct.—very high wind, with light rain.
M 28	29.267	51.8	29.303	54.0	48	55.4	56.2	49.2	56.7	.033	S var.	A.M. Cloudy—high wind. (Very high wind, with light rain throughout the night.) P.M. Cloudy—high wind.
T 29	28.954	54.2	29.384	54.3	51	56.8	49.4	52.2	52.6	.183	S var.	A.M. Ovct.—high wind. (High wind, with light rain during the night.) P.M. Fine—lt. clouds & wind. Ev. Fine & clear.
W 30	29.516	51.6	29.536	51.8	46	48.4	49.3	47.5	49.3	.127	S	Overcast—heavy rain and wind throughout the day.
MEANS..	29.620	44.6	29.626	46.5	39.7	43.1	46.3	38.7	47.6	Sum. 2.108	Mean of Barometer, corrected for Capillarity and reduced to 32° Fahr.	9 A.M. 29.586 3 P.M. 29.587
DECEMBER T 1	29.980	48.0	30.037	48.7	42	41.3	44.9	39.0	48.7	.325	SW	A.M. Overcast—light wind. Heavy rain during the night. P.M. Fine—light clouds. Evening, Overcast.
F 2	30.012	47.5	29.938	49.9	42	49.0	51.9	40.4	51.7		S	Overcast—very light rain, with high wind. Evening, Overcast.
S 3	29.772	49.6	29.796	51.2	44	49.3	54.2	48.5	54.2	.036	SSW	A.M. Overcast—very light rain and wind. Very high wind during the night. P.M. Overcast—light wind.
⊙ 4	29.816	51.6	29.812	53.0	47	53.7	55.5	48.6	55.5	.161	SW	Overcast—light wind throughout the day, with occasional rain. Very high wind during the night.
M 5	29.867	52.8	29.833	54.2	48	54.2	55.6	51.3	55.7	.125	WSW	Overcast—lt. wind throughout the day. Evening, Fine—lt. clds.
T 6	29.998	52.6	29.964	54.6	48	51.9	52.6	47.2	53.7		S	A.M. Overcast—very lt. rain with high wind. P.M. Fine and cloudless—lt. wind. Ev. Overcast—lt. rain and wind.
W 7	29.535	52.3	29.445	53.6	47	50.6	51.7	49.3	52.7	.055	SSW	Fine—lt. clouds & wind throughout the day. Very high wind during the night. Ev. Fine—lt. clouds, with brisk wind.
○ T 8	29.229	44.5	29.166	50.2	43	46.2	45.3	42.5	46.5	.380	SW	7¼ A.M. Very heavy rain, with high wind. P.M. Fine—light clouds and wind. Evening, Fine and clear.
F 9	29.069	45.9	29.017	46.9	41	41.3	42.9	38.2	43.6	.036	SSW	Fine—light clouds throughout the day. Evening, Fine & clear.
S 10	29.213	42.0	29.235	43.6	37	36.5	42.4	34.6	42.0		SSW	A.M. Fine—nearly cloudless—light haze. P.M. Cloudy—light rain. Evening, Fine and clear.
⊙ 11	29.586	41.8	29.584	43.0	36	38.0	40.2	35.0	40.2		SW	Overcast—light wind throughout the day. Ev. Thick fog.
M 12	29.584	39.9	29.426	42.8	35	38.7	49.4	33.5	50.3	.119	ESE	Overcast—light rain and wind throughout the day. Evening, Overcast—light rain with high wind.
T 13	29.241	46.9	29.227	48.4	42	47.7	47.7	38.2	49.3	.066	SSW	Overcast—light brisk wind throughout the day. Very high wind during the night. Evening, Cloudy.
W 14	29.295	43.8	29.368	45.8	38	38.6	45.3	37.4	45.2		SSW	A.M. Fine—nearly cldless.—lt. wind. P.M. Fine—lt. clds. & wind.
T 15	29.967	42.3	30.006	42.8	37	38.0	40.6	37.0	44.6		SW	A.M. Hazy. P.M. Fine—light clouds and wind. Evening, Overcast—heavy rain with high wind.
F 16	29.728	42.2	29.853	43.7	36	39.3	43.8	37.0	44.0	.183	S	Fine—light clouds & wind throughout the day. Heavy rain with high wind during the night. Evening, Cloudy.
S 17	30.124	41.0	30.087	43.2	37	37.2	44.7	35.6	49.0		SW	Hazy—lt. wind throughout the day. Ev. Overcast—very lt. rain.
⊙ 18	30.095	45.0	30.099	46.7	39	49.2	50.9	36.4	51.2		SSW	Cloudy—light wind throughout the day. Evening, Overcast—very light rain.
M 19	30.168	46.9	30.136	58.6	41	47.6	49.8	45.8	49.9		SSW	Overcast—light wind throughout the day. Evening, Cloudy.
T 20	30.208	48.3	30.251	48.8	43	46.5	44.0	45.7	46.2	.058	SW	Foggy—deposition—light wind throughout the day. Evening, Overcast.
W 21	30.279	46.0	30.285	47.2	41	42.8	46.3	39.3	47.0		SSW	A.M. Foggy—deposition—light wind. P.M. Overcast.
T 22	30.378	46.4	30.273	47.3	40	42.8	46.4	41.9	48.0		SW	A.M. Light fog, P.M. Fine—lt. clds. & wind. Ev. Overcast.
● F 23	29.857	46.2	29.766	45.3	39	39.7	37.8	38.9	43.7	.102	N	A.M. Lightly overcast—lt. wind. Rain during the night. P.M. Cloudy—lt. wind. Evening, Fine and clear.
S 24	29.711	39.7	29.758	40.4	33	34.3	35.5	32.7	38.2		W	Cloudy—light wind throughout the day. Sharp frost during the night. Evening, Fine and clear.
⊙ 25	29.766	34.4	29.693	35.2	27	28.3	32.0	27.4	34.0		N var.	A.M. Ovct.—snow with lt. brisk wind. P.M. Fine—lt. clouds. Ev. Ovct.—snow—lt. wind.
M 26	29.580	32.9	29.548	33.8	27	29.7	31.6	29.2	32.5		NW	Snow, with high wind nearly the whole of the day. Evening, Ovct.—high wind.
T 27	29.578	33.7	29.641	34.3	28	32.5	31.2	31.9	33.0		NE var.	A.M. Snow, with high wind—freezing very sharply. P.M. Fine—light clouds & wind. Evening, Overcast—snow.
W 28	29.885	33.2	29.901	35.2	27	33.3	32.3	29.4	31.8		NW	A.M. Fine—lt. clouds & wind. P.M. Overcast—lt. brisk wind. Ev. Overcast—slight thaw.
T 29	29.998	32.6	30.018	34.7	28	31.9	33.4	29.7	33.2		NE var.	A.M. Overcast—lt. snow & wind. P.M. Ovct.—slight thaw. Ev. Overcast—air frosty.
F 30	30.053	32.7	30.085	34.6	27	30.2	31.7	29.2	29.8		N	A.M. Overcast—light wind. P.M. Overcast—light snow & wind. Evening, Fine and star-light.
S 31	30.212	31.8	30.326	33.7	27	30.0	32.3	28.2	32.3		NE	Lightly overcast—lt. wind throughout the day. Ev. Light snow.
MEANS..	29.799	43.0	29.793	44.9	37.6	41.0	43.4	38.0	44.4	Sum. 1.646	Mean of Barometer, corrected for Capillarity and reduced to 32° Fahr.	9 A.M. 29.770 3 P.M. 29.758