

Fig. 9.

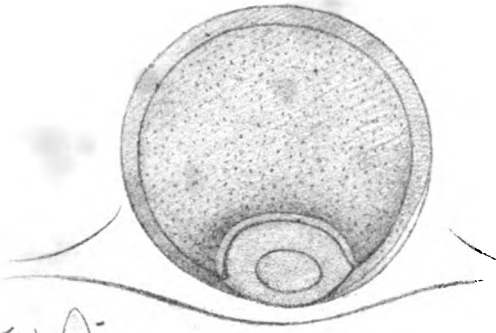


Fig. 10.

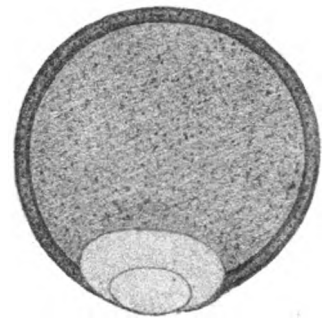


Fig. 8.

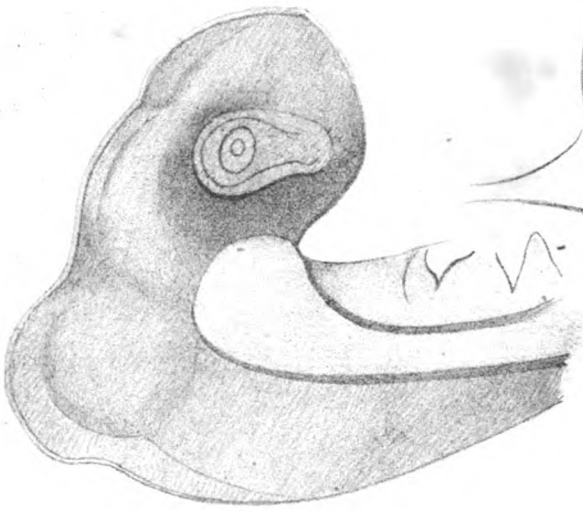


Fig. 11.

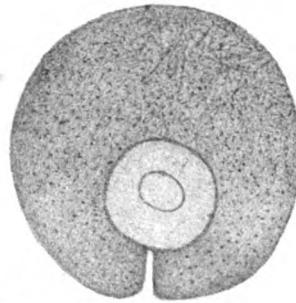


Fig. 12.

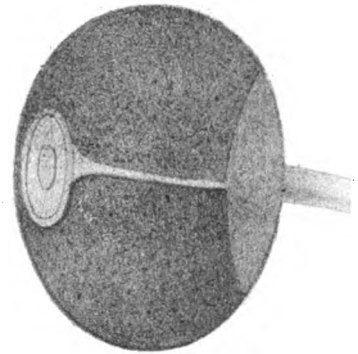


Fig. 13.

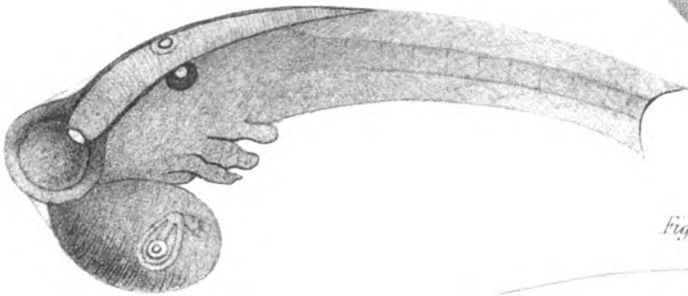


Fig. 15.

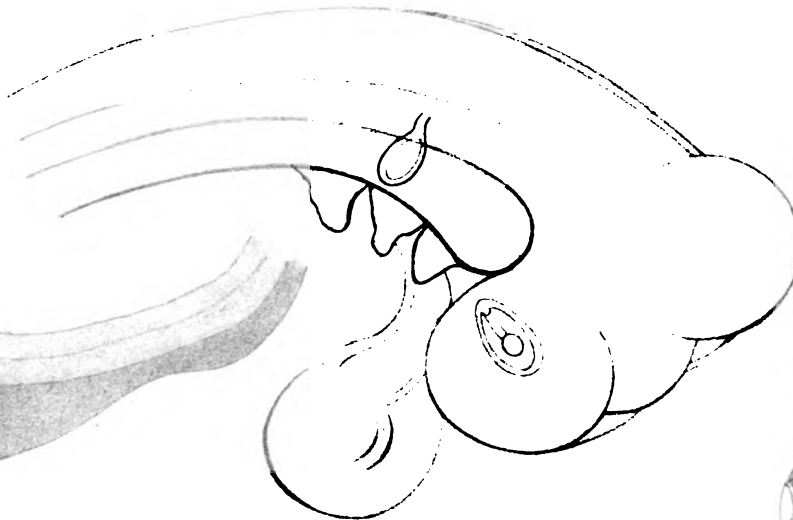


Fig. 17.

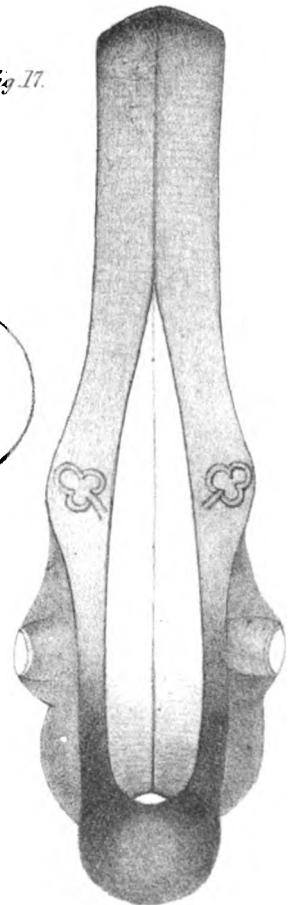


Fig. 14.

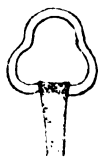
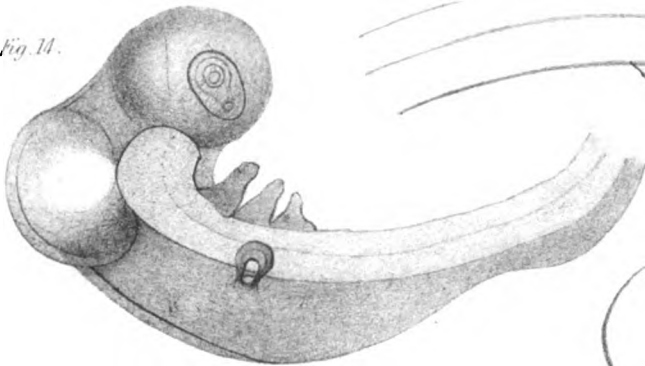


Fig. 16.



Fig. 18.

EXPLANATION OF THE PLATES.

PLATE VIII.

- Fig. 1. An embryo chick at the thirty-first hour of incubation.
- Fig. 2. An embryo chick at the thirty-third hour.
- Fig. 3. An embryo chick at the thirty-sixth hour. The first rudiment of the eye is here seen to be visible in the form of a protrusion from the anterior cerebral cell.
- Fig. 4. An embryo chick at the forty-sixth hour. The optic vesicle is seen now very distinct, presenting a slight contraction at its connection with the cerebral cell from which it arises.
- Fig. 5. The cephalic extremity of a chick at the same period more highly magnified. This Plate is intended to represent the cavity which exists both in the cerebral cell and optic vesicle, and the communication between them.
- Fig. 6. The cephalic extremity of an embryo at the fifty-fifth hour viewed from the dorsal aspect, principally intended to represent the mode of formation of the crystalline lens.
- Fig. 7. The under surface of the eye of a chick at the seventieth hour. This figure shows the spherical form of the eyeball, its contracted tubular end, the optic nerve, the mode of formation of the lens, and the fissure which exists on the under surface of the eye.

PLATE IX.

- Fig. 8. The cephalic extremity of an embryo chick at the sixty-second hour (lateral view).
- Fig. 9. In this Plate the retina is seen overlapping the margin of the lens at the latter half of the third day.
- Figs. 10 and 11. The eye of an embryo chick on the fourth day.
- Fig. 12. The eye of an embryo chick on the fifth day.
- Fig. 13. In this Plate the first rudiment of the ear is visible in the form of a small vesicular protrusion from the central part of the medulla oblongata (from an embryo chick at the fiftieth hour of incubation).
- Fig. 14. An embryo chick at the fifty-sixth hour. The ear-bulb is now seen to be of a pyriform shape, and communicating with the cavity from which it arises.
- Fig. 15. The cephalic extremity of an embryo chick at the sixty-fifth hour of incubation. The rudimentary vestibule and auditory nerve are now seen fully formed.
- Fig. 16. The ear of an embryo chick at the seventy-second hour of incubation. The

auditory nerve is now seen quite solid, and the vestibule presents a slight contraction near its centre.

- Fig. 17. An embryo chick at the eighty-second hour, viewed from the dorsal aspect. The cavity of the fourth ventricle is distinctly seen, and on either side the rudimentary ear is shown *in situ*; the contraction of the wall of the vestibule is more marked.
- Fig. 18. The ear of an embryo chick at the ninety-sixth hour. The vestibular contraction is more marked, and a depression is observed at the extremity of the terminal vesicle, which subsequently forms one of the semicircular canals.

8 *Wilton Street*,
October 14th, 1849.

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

Received February 6,—Read April 25, 1850.

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

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

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

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

It is true that the instruments proposed by the Royal Society, as well as the in-

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

been by no means found to be the case with the magnets employed in the different Bifilars of the Colonial Observatories, whose loss of magnetism appears to be subject to no general or systematic law, and even occasionally to intermit and to recommence without any apparent or discernible cause. To eliminate the effect of the loss, the magnetic moment of the bar would require therefore to be examined from time to time, and at short intervals; but the removal of the magnet for this purpose would break the connexion and thus interrupt the continuity of the series.

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

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

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

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

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

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

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

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

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

The constant, depending upon the value of the moment of inertia of the deflecting

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

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

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

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

TABLE I.

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

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

This is on the most simple hypothesis, in which neither secular change nor annual variation is supposed to exist. If we call X the arithmetical mean as above derived, and $X'_1, X'_2, X'_3 \dots X'_{12}$ the several observed monthly results, we shall have the several errors remaining over, $X'_1 - X, X'_2 - X, \dots, X'_{12} - X$, as follows:—

TABLE II.

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

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

x the interval in months between the date of X' and March the 1st, 1847, and y the monthly variation occasioned by secular change. We have fifty-two such equations furnished by the series, which, treated by the method of least squares, give $X=3\cdot53043$, and $y=-\cdot000347$ the monthly secular change, the latter number being equivalent to an annual decrease of $\cdot0042$ in the horizontal force during the period comprehended by the observations. For the purpose of obtaining the errors remaining over on this hypothesis of secular change, we must apply to each of the results in Table I. a correction, equivalent to the effect of secular change in the interval elapsed between the dates of the particular observation and the mean epoch of the 1st of March, 1847; and having done so, we now find, on the hypothesis of the existence of a uniform secular decrease of the horizontal force annually of $\cdot0042$, $3\cdot53043$ as the value of the horizontal force on the 1st of March, 1847, with a probable error of $\pm\cdot00025$; whilst the probable error of a single monthly determination is reduced to $\pm\cdot0018$ instead of $\pm\cdot0040$ as before; and as the weights of different hypotheses are measured by the inverse squares of the probable errors, the hypothesis which supposes a secular decrease of force amounting to $\cdot0042$ annually is more probable than the hypothesis which supposes no secular change, in the proportion of 4·7 (nearly) to 1.

Having thus obtained the value of the horizontal component of the magnetic force corresponding to the 1st of March, 1847, and the mean value of the secular change of this element during the period of the observations, we require, for the purpose of deriving the values of the *total* magnetic force and its secular change, at the same epoch, to know the magnetic inclination corresponding to the epoch, and the secular change of that element also. For the first we have to seek the mean result of the observations of inclination, which were also made monthly during the same fifty-two months. In the three first years, 1845 to 1847 inclusive, the observations of inclination were made on every Tuesday and Friday, three hours before noon on the Tuesdays, and three hours after noon on the Fridays; thus furnishing eight or nine partial determinations in each month according to the number of Tuesdays and Fridays contained in it; each determination being complete in respect to the several positions of the circle and needle required for that purpose. In 1848 and 1849 the same number, or occasionally a greater number, of partial determinations was made monthly; but instead of the Tuesdays and Fridays, the days of observation were the same as those on which the horizontal force was observed. The circle employed, from January 1845 to March 1846 inclusive, was one of GAMBEY's well-known 9-inch circles, and from April 1846 to April 1849 one of ROBINSON's, of the same dimension and of the same pattern. In GAMBEY's circle two needles were used, one from January 1845 to December of the same year, and the second from January 1846 to March of the same year; in ROBINSON's circle also two needles were used, one from April 1846 to August 1847, when an accident befell it, and a second from September 1847 to April 1849. The needles used in GAMBEY's and ROBINSON's circles were made by those artists respectively, and both circles and needles were probably as perfect of their kind as any

that have ever been made. The intercomparability of the results might possibly have been more perfect if one and the same instrument had been used throughout (an important consideration in respect to the secular change); but, on the other hand, the probable accuracy of the general result as regards the mean inclination must be viewed as in some degree strengthened by the employment of different instruments. The following Table exhibits the fifty-two monthly results:—

TABLE III.

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

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

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

TABLE IV.

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

The prevalence of — signs in the earlier months, and of + signs in the later

months, indicates distinctly the existence of secular change. Pursuing therefore the same method of obtaining its mean value during the period of observation as was adopted in the case of the horizontal force, we obtain from the fifty-two conditional equations $y = +0.0741$ as the monthly value of the secular change, equivalent to a mean annual increase of the inclination during the period of observation of 0.89 ; and $75^\circ 16.09$ as the mean inclination on the 1st of March, 1847, with a probable error reduced to ± 0.17 , the probable error of a single monthly determination being ± 1.23 ; whence we may infer that the hypothesis of a secular increase in the inclination of 0.89 annually during the period of observation is more probable than the hypothesis of no secular change, in the proportion of 1.4 to 1.

Total Force; its mean value and secular change.—Having thus derived from the series of fifty-two months of observation the mean value of the horizontal force $= 3.53043$, and of the inclination $75^\circ 16.09$, each for the epoch of the 1st of March, 1847, we have for the value of the total force in absolute measure at the same epoch $3.53043 \times \sec. 75^\circ 16.09 = 13.8832$. With reference to the inclination element of the result, we might safely regard this value as a final determination; but we cannot quite do as much in respect to the element of the horizontal force, as it may yet have to receive the corrections already noticed (though they are likely to be extremely small), when the values of the constants of inertia and induction shall be finally ascertained on the return of the instruments to Woolwich. When these constants and their probable errors are known, the probable error of the finally corrected value of the total force will also be assignable.

The elements from which we have to infer the *secular change* of the total force are the secular changes of the horizontal force and of the inclination derived from the observations; these are an annual decrease of $.0042$ in absolute measure of the horizontal force, and an annual increase of 0.89 of inclination. A secular change of the horizontal force may be produced, either by a secular change of the inclination affecting the horizontal component of the total force according to the known principles of the resolution of forces, or by a secular change in the total force itself; or, finally, it may be the joint production of both. An increase of the inclination causes a decrease of horizontal force, and *vice versa*; so far therefore we may regard the annual decrease of the horizontal force at Toronto as attributable in part at least to the annual increase of the inclination. But an annual increase of 0.89 in the latter element is equivalent to an annual decrease in the horizontal component of the force of not more than $.0035$. There remains, therefore, an excess of $.0007$ in the secular decrease of the horizontal force, which is unaccounted for by the secular change of the inclination, and is indicative of the existence of a small annual decrease in the total force during the period of observation. The uncompensated portion of the horizontal force on which this inference is founded is indeed small in absolute amount, but its magnitude must be judged of in relation to the probable errors

of the determinations of secular change of the inclination and horizontal force. Viewed in this light, the probabilities are in favour of the existence of a small annual decrease in the total force, as the legitimate conclusion from the portion of the series of absolute determinations in progress at Toronto which has been received in this country, and is here discussed: whilst it is obvious that the groundwork is laid of a positive conclusion, admitting of no uncertainty, attainable by steady perseverance in the prolongation of the series; avoiding as far as possible, upon all occasions, all changes in the instruments employed or in the methods of observation. It will be shown in the sequel that certainty in respect to the question, whether the total force at Toronto is at the present epoch increasing or decreasing, may have a very considerable theoretical importance.

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

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

TABLE V.

	Residual quantities.	
	Inclination.	Horizontal force.
January, mean of 5 years	+ 1.40	— .0004
February, mean of 5 years	+ 1.06	— .0012
March, mean of 5 years	— 0.44	.0000
April, mean of 5 years	— 0.58	— .0010
May, mean of 4 years	— 0.38	+ .0002
June, mean of 4 years	— 1.03	+ .0028
July, mean of 4 years	— 1.93	+ .0033
August, mean of 4 years	— 0.90	+ .0022
September, mean of 4 years	+ 0.13	— .0009
October, mean of 4 years	+ 0.35	— .0017
November, mean of 4 years	+ 0.97	— .0023
December, mean of 4 years	+ 0.60	— .0002

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

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

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

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

The question of an annual variation of the horizontal force appeared to me so important either to verify or disprove, that, at my request, Captain LEFROY employed, during the years 1847 and 1848, a third method of experimenting, which, although it may not be quite so satisfactory in respect to the individual monthly results as the method of absolute determinations, in consequence of the magnetic moment of the bar not being subject to monthly examination, has yet the advantage of affording a third conclusion perfectly independent of the others, and but little inferior to the absolute method in proportion to the time of their respective continuance. One of the cylindrical magnets of 3.67 inches in length, which had been employed in the North American survey, and appeared to have attained a state of steady magnetism (which however did not prove so thoroughly steady as was expected), was suspended in the usual manner in a light stirrup, with an attached mirror and a detached telescope. The horizontal force of the earth was measured at stated hours, twice in every day, at 10 A.M. and 5 P.M., by the times of vibration of the bar derived from four hundred vibrations observed in the usual manner, and reduced to a standard temperature and to infinitely small arcs. The magnetic moment of the bar was carefully examined before the commencement and after the conclusion of the series, viz. on the 31st of December, 1846, and on the 3rd of January, 1849, and also intermediately on January 5th, 1848. The magnetic moment at these periods was as follows:—

		Loss of magnetism in nearly equal intervals.
1846. December 31st	=0.6104	} .0104
1848. January 5th	=0.6000	
1849. January 3rd	=0.5913	} .0087

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

TABLE VI.

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

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

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

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

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

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

In the case of the horizontal force, a regular and consecutive series of monthly determinations, similar to that at Toronto, was commenced at Hobarton in January 1846, and the results have been received in England to December 1848 inclusive. The series treated in a similar manner to that at Toronto shows an annual variation of the same character as respects the seasons, and almost identical in amount with that at Toronto. In the months from October to February inclusive (or in the summer months at Hobarton), the horizontal force is $\cdot0017$ *greater* on the average than its mean amount, and from April to August inclusive (or in the winter months at Hobarton), it is on the average $\cdot0013$ *less* than its mean amount.

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

It may assist the recollection of the facts regarding the annual variation of the two

magnetic elements at Toronto and Hobarton to state;—that in the months from October to February the magnetic needle more nearly approaches the *vertical* direction at both stations, and from April to August the *horizontal* direction;—and that the total force is greatest at both stations from October to February, and least from April to August.

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

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

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

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

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

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

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

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

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

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

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

† In Silliman's Journal.

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

**X. *Observations on the Freezing of the Albumen of Eggs.* By JAMES PAGET, Esq.,
Professor of Anatomy and Surgery to the Royal College of Surgeons of England.
Communicated by THOMAS BELL, Sec. R.S.**

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IN 1777, JOHN HUNTER communicated to the Royal Society a series of experiments on the heat of animals and vegetables, among which were some showing that an egg, after having been frozen and thawed, will, on a second exposure to cold, freeze more quickly than it did before, and more quickly than a fresh egg does when exposed to the same temperature. From these and other experiments, he concluded “that a fresh egg has the power of resisting heat, cold and putrefaction in a degree equal to many of the more imperfect animals;” and he adds, “it is more than probable this power arises from the same principle [*i. e.* a living principle] in both*.” Mr. HUNTER’s pupils generally adopted this conclusion: and the facts on which it was based have formed a chief part of the evidence for the existence of a special vital principle capable of resisting, by a kind of passive opposition, the changes that physical forces produce in dead organic matter.

In the course of some inquiries into the nature of the life of the blood, I repeated and extended Mr. HUNTER’s experiments, and obtained results which, I venture to hope, may be deemed worthy of the consideration of the Society.

My experiments consisted chiefly in submitting to temperatures near to zero of FAHRENHEIT, fowls’ eggs, into which thermometers with slender bulbs were introduced. The decrements of heat were registered every minute, and the time was noted at which each egg began to freeze, the general indication of that event being the swelling of the albumen, and its protrusion from the aperture through which the thermometer was passed.

I found, as Mr. HUNTER did, that a fresh egg generally resists freezing longer than one does which, having been previously frozen and thawed, is exposed, in all similar circumstances, to the same temperature. The result of twenty experiments, in which such eggs were placed in temperatures ranging from zero to 10° FAHR., was, that the fresh eggs were frozen, on an average, in 26 minutes, and the eggs that had been previously frozen and thawed, were frozen for the second or third time in 15½ minutes.

I next determined, by similar experiments, the respective times of freezing of fresh eggs, and of such as were variously changed in structure and chemical composition. Similar results appeared. If the yolks of the eggs were broken, so as to be mingled

* HUNTER’s Works, by PALMER, vol. iv. p. 150.

with the albumen; if their whole substance were decomposed, so as to emit a more or less putrid odour; if a powerful electric shock had been passed through them,—in all these conditions, they froze more quickly than fresh and uninjured eggs did, that were exposed with them to the same low temperature. The average difference in the respective times of freezing was nearly the same as that already stated.

All these experiments tended to confirm Mr. HUNTER's explanation; for all seemed to show that, by the influence of such forces as commonly destroy animal life, eggs lose some capacity of resisting the abstraction of heat. But when I examined the registers of the decrements of heat sustained by the several eggs during each minute of their exposure to the cold, it appeared that the fresh eggs almost always lost heat more rapidly than those did which had been frozen, broken, decomposed, or electrified. An average result of experiments upon thirty-three eggs of each class was, that, between the fifth and fifteenth minutes after first exposure to a temperature of from 5° FAHR. to 10° FAHR., the fresh eggs lost 17° of heat, and the others only $13\frac{1}{2}^{\circ}$. The reason why the fresh eggs, though they lost heat more rapidly than the others, yet were longer in freezing, was, that in most cases their temperature was reduced some degrees below 32° before freezing took place; while the eggs that had been frozen before, or that were in any other way spoiled, always began to freeze as soon as they reached the temperature of 32° .

Mr. HUNTER had remarked this difference. In one of his experiments, he says, "a fresh egg sank to $29\frac{1}{2}^{\circ}$, and in twenty-five minutes later than the dead one, it rose to 32° , and began to swell and freeze." But I found that fresh eggs could be reduced to a much lower temperature without freezing. In several experiments they fell to 20° ; in some to 16° , before, with a rapid rise to 32° , freezing took place; and from some observations, which will be hereafter mentioned, I believe that, under favourable circumstances*, the temperature of a fresh and unbroken egg may be reduced to within 5° of zero of FAHRENHEIT without freezing, although its proper freezing-point, and that to which its temperature rises when it begins to freeze, is 32° or between 31° and 32° †.

It thus appeared that a fresh egg does not resist freezing as a living animal does, which either parts with its heat slowly, or else produces heat, compensating in some measure for that which is lost. It is as much the peculiarity of the fresh egg to lose its heat more quickly than another does, as it is to be longer in freezing; and, indeed, this quicker loss of heat seems essentially connected with the ability to be reduced far below 32° without freezing; for, among thirty-two fresh eggs, there were eleven which began to freeze at 32° , and all these had lost heat slowly; the average decrement between the fifth and fifteenth minutes of their exposure to cold being only $13\frac{1}{2}^{\circ}$.

* The chief of these circumstances are, that the egg should be unmoved, and that its albumen should be not even so much disturbed as it is by the introduction of the thermometer.

† With so slender thermometers as I was obliged to use, it was not possible to determine within half a degree the precise freezing-point.

Many things observed in the course of these experiments seemed to indicate that the freezing of the fresh egg was retarded by some peculiarity of its mechanical construction, which was destroyed by the several means supposed to destroy its life. It was, therefore, desirable to ascertain whether the capability of being reduced far below 32° without freezing, and the consequent apparent resistance to freezing, could be destroyed by any means that would not at the same time prevent the egg from manifesting, by development in incubation, the surest evidences of vitality. In experiments to determine this point, I found that one might, with a bent probe, gently detach the whole of the albumen of an egg from its connection with the membrane of the shell, and that after this the chick would be developed, although, perhaps, not to perfection. But when eggs with the albumen thus disturbed were exposed to cold, they did not descend below 32° without freezing; they lost heat less quickly, but froze sooner, than uninjured eggs did, when exposed with them to the same temperature; they froze like the eggs which Mr. HUNTER considered dead.

Again, cracking the shell of an egg does not prevent the development of the young bird, although, in consequence of the excessive evaporation through the fissures, the development may be imperfect. But when eggs with their shells cracked in many places, and with slight injuries of the membrane of the shell, were exposed to cold, they lost heat and froze just as those did which might be reputed dead. Thus, then, mechanical injuries, such as could not have affected the chemical composition of the fresh egg, and such as did not prevent its development in incubation, were found sufficient to deprive it of its power of resisting freezing; and thus its power of resistance appeared to be due, not to any vital principle, but to some peculiarity of mechanical construction.

It may be very difficult to prove what this peculiarity of construction is. That it is a property of the albumen was proved by some experiments, in which albumen, gently removed from fresh eggs, exhibited the same mode of freezing as the entire fresh eggs did: and the following facts, as well as those already mentioned, are favourable to the opinion, that the property which enables fresh albumen to descend far below 32° without freezing, is its peculiar tenacity or viscosity, by means of which the water combined with it is held so steadily, that the agitation favourable, or even necessary, to the freezing at or near 32° cannot take place.

1. The decay and putrefaction of an egg, the freezing and again thawing of one, and (as egg-preservers well know) the stirring and frequent concussion all tend to diminish the viscosity of the albumen to such a degree, that, instead of forming a consistent substance, the greater part of it will flow like a thin liquid from an aperture in the egg-shell.

2. The albumen of eggs does not freeze like a dense solution of albumen, or of saline substances, in water, but like water of which the ordinary freezing is prevented. The freezing-points of aqueous solutions are, according to their densities, more or less below 32° ; and if, in exposure to intense cold, the temperature of any

such solution be reduced below its freezing-point, it will, at the instant of freezing, rise to that point, whatever it may be, and not to 32° . Thus, I placed in a freezing-mixture of which the average temperature was 10° FAHR., solutions of common salt in the proportions of 3, 5, 6 and 9 parts to 100 of distilled water; they fell to from one to six degrees below their several freezing-points, and then, in the act of freezing, rose respectively to 28° , 25° , $24\frac{1}{2}^{\circ}$, and 23° . At these temperatures they remained till they were thoroughly frozen, and then they all descended to the temperature of the medium in which they were placed. The same is observable in the freezing of serum and blood, which, in their relations to heat, may be regarded as mere solutions of albumen and saline matters: they freeze at from 29° to 31° , and retain these temperatures till they are thoroughly frozen. I found the same also in the freezing of milk and of thick mucilage of gum; the former froze at 30° , the latter at 28° , but neither of them in freezing rose to 32° . Unlike all these substances, the albumen of fresh eggs, however far below its freezing-point it may have descended, always in freezing rises to 32° ; it freezes therefore like water, or like a very weak saline solution, which, by some mechanical disposition of its particles, is prevented from freezing as soon as it is reduced to 32° , or a few degrees lower.

3. The sudden strong agitation of fresh albumen, when its temperature is reduced several degrees below 32° , will often cause it to freeze at once*, as water under the same circumstances freezes.

4. It is well known, that when once a portion of any given quantity of water is frozen, the portion in contact with the ice cannot be reduced below 32° without freezing. I thought, therefore, that if I could bring ice just formed into contact with the albumen of an egg, the water in the albumen would freeze as soon as it fell to 32° , and so it proved; for in three experiments, in which the air-cavities of fresh eggs were filled with water before exposing them to cold, the albumen did not descend below 32° , but froze at that temperature.

Whether the explanation here offered of the peculiar property of the albumen of fresh eggs be right or not, the property will, I think, merit consideration in reference to both the nature and the purpose of the substance to which it belongs.

For, in regard to the nature of this form of albumen, its mode of freezing proves it to be essentially different both from all solutions of albumen and from organic tissues holding albuminous matter in suspension. As I have already stated, the freezing-

* In the course of the experiments, I observed that the effect of agitation, on either albumen or a saline solution, when its temperature is reduced below its freezing-point, depends in some measure on the temperature of the medium in which it is placed. Thus a saline solution, whose freezing-point was 28° , might be reduced to 25° in a medium of 24° , and, on being now agitated, it would not freeze; but a similar solution, reduced to 25° in a medium of 10° , would freeze at the instant of agitation. In a medium of which the temperature averaged 21° , no length of exposure and no agitation would, in one of my experiments, make albumen freeze, though it fell to the temperature of the medium; but in others, when albumen, in a medium averaging 10° , fell to 28° , it froze as soon as it was agitated.

points of albuminous solutions are lower than 32° in direct proportion to their densities; they do not, in freezing, rise to 32° , but freeze in all respects like saline solutions. And the same mode of freezing may be observed in organic tissues in which albuminous fluids are suspended or infiltrated. An eye, or its vitreous humour, a piece of muscle, gland, or brain, exposed to intense cold, loses temperature to 32° or $31\frac{1}{2}^{\circ}$, and remains at this, its freezing-point, till it is frozen hard throughout, and then descends to the temperature of the medium in which it is placed.

The purpose or utility of this peculiar property of the albumen of eggs is manifest in the defence which it provides for eggs exposed to a temperature below 32° . If an egg be frozen, the damage sustained by its structure is such that the germ cannot be fully developed; but mere cold, however intense, if freezing does not take place, does not prevent the complete development of the young bird. I placed three eggs in a freezing mixture, varying from zero to 5° FAHR.: one of them froze, and its shell was cracked from end to end; another froze, and when it thawed, its yelk was burst and mixed with the albumen. In incubation, two spots of blood were developed in the former, and an enlargement of the cicatrix ensued in the latter of these two eggs,—sufficient indications that the intense cold and freezing had not killed them, though it had spoiled their structure. But in the third egg, which had been exposed for nearly an hour to a temperature below 5° FAHR., perfect development took place in incubation. Even this degree of cold therefore had neither killed nor frozen the egg, though, according to the average rate at which eggs part with heat, its whole substance must have been for half an hour at a temperature between 5° and 10° FAHR.

This security of eggs from the injurious influence of cold has also been proved, on a large scale, by the ingenious inventor of the hydro-incubator, Mr. CANTELO. He has told me, that, to test the truth of the popular belief that eggs are always destroyed by exposure to a frosty air, he sometimes exposed baskets of eggs, through the whole of a Canadian winter's night, to a temperature ranging, he believes, between 5° and 10° FAHR. Some of them were cracked, and these he threw away; they were doubtless frozen and spoiled; but the rest were placed in his incubator, and the usual proportion were hatched.

The need of such a provision against the influence of cold must exist in the case of many, or perhaps of all, birds that breed in cold climates; for accident must occasionally drive from their nests even those parent-birds that have not, like the common fowl, the custom of leaving their eggs for a certain time exposed to the open air.

In conclusion, I may repeat that the experiments I have related show that it is not by the power of a vital principle that eggs resist the influence of cold. They show that certain things will destroy the power of resisting cold without affecting the capability of being developed, and of therein manifesting the best evidence of life; and that when eggs yield to the influence of intense cold, they are not damaged unless

they are frozen, and are not killed even when frozen. The experiments thus remove almost the only remaining support of the hypothesis that such a vital principle may exist in organized bodies, as may enable them, even while inactive and displaying no other signs of life, to resist passively the influence of physical forces.

XI. *Researches on the Tides.—Fourteenth Series.*

On the Results of continued Tide Observations at several places on the British Coasts.

By the Rev. W. WHEWELL, D.D., F.R.S.

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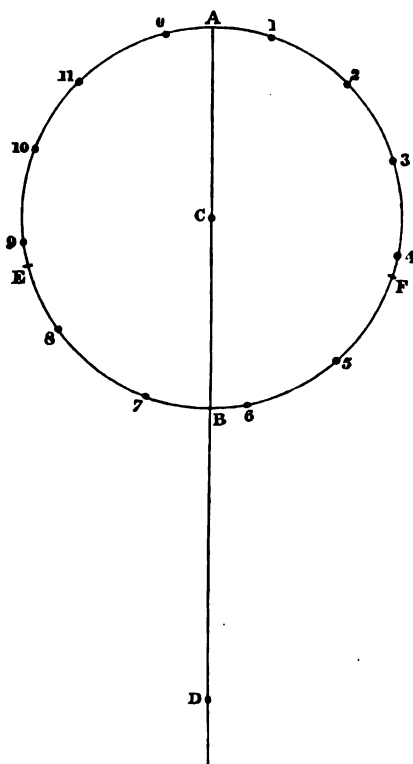
TIDE observations made at several different parts of the British and the neighbouring shores, and in some instances continued for a considerable period, have been discussed by Mr. D. Ross of the Hydrographer's Office, with great labour and perseverance; and as the results which his labours afford may be of use to mariners, I offer to the Royal Society a brief statement of these results.

The discussions at present referred to relate to the height of high water, and the variations which this height undergoes in proceeding from springs to neaps and from neaps to springs. It is found, by examining the observations at 120 places and throwing the heights into curves, that the curve is very nearly of the same form at all these places. Hence the semimensual series of heights at any place affords a rule for the series of heights at all other places where the difference of spring height and neap height is the same. For instance, Portsmouth, where the difference of spring height and neap height is 2 feet 8 inches, is a rule for Cork, Waterford, Inverness, Bantry, Boucout on the French coast, and other places.

And the Tables of the height of high water at one of these places suffice for all the others, a constant being of course added or subtracted according to the position of the zero-point from which the heights at each place are measured.

The series of heights of high water for a semi-lunation also agrees very exactly, as to the form of the curve, with the equilibrium theory. The following construction gives this curve.

With centre C and radius CA (half the difference of the height at spring and neaps), describe a circle; and in AC produced take CD to CA as 12 to 5. Divide the circumference of the circle into twelve hours, representing the twelve hours of moon's transit; and join D with each of these divisions. The lines thus drawn to the hours will give the heights of high water for each hour of the moon's



transit; a constant quantity being, as before stated, added or subtracted in order to refer the height to the proper zero.

According to the theory, the 0^h or 12^h hour-points would be at A; the ratio of DC to AC would be that of the lunar to the solar tide; and the distances of the hour-points from D would be the heights of high water above mean water. But all these properties are, in the actual cases, modified in a manner which must be noticed.

The tides in these discussions are not referred to the transit of the moon *immediately* preceding, but to some earlier transit, namely, the second, third, fourth or fifth preceding transit; it being found that in this way the accordance with the theory becomes more exact. Thus in the British Channel the tides are referred to the *third* preceding transit; and this extends also to Ireland and to the west coast of England and Scotland. On the east coast of England, in the northern parts, as at Shields, Sunderland, Scarborough and Hull, the *fourth* preceding transit is used; at Harwich, Sheerness and London, the *fifth* (see Table B). But this reference to an earlier transit does not make the highest tide correspond exactly with the hour of transit 0^h or 12^h: and it is found, in the cases which have been included in the present examination, that a displacement of the 0^h point about fifteen minutes from A will best make the theoretical and the observed curves agree with each other.

The ratio of DC to AC is, as has been said, 12 to 5; and this, according to the theory, would be the ratio of the lunar to the solar tide. If this were the case, the spring tide measured above mean water would be 17, and the total spring tide above spring tide low water would be 34. The neap tide in this case would be 7 above mean water, and therefore 24 above spring tide low water. Hence the difference of springs and neaps would be to the height of neaps above low water springs as 10 to 24, a ratio constant for all places.

But in fact, this ratio of the excess of springs to the total height of neaps above low water springs is different at different places: and the observations now under consideration show in some measure the law of this difference. The ratio is smaller when the tide is smaller. This appears from the observations at different places, as arranged by Mr. Ross in the annexed Table A. We have there the following results, taking the means of groups of places according to the amount of tide.

Number of places.	Mean neap tide above spring low water.	Mean excess of spring high water above neap.	Ratio.
37	ft. 9 in. 3	ft. 2 in. 5	38 : 10
40	12 0	3 8	33 : 10
39	17 10	5 9	31 : 10
4	27 0	9 8	28 : 10

Where it appears that the actual ratio approaches to the theoretical ratio in proportion as the amount of tide increases.

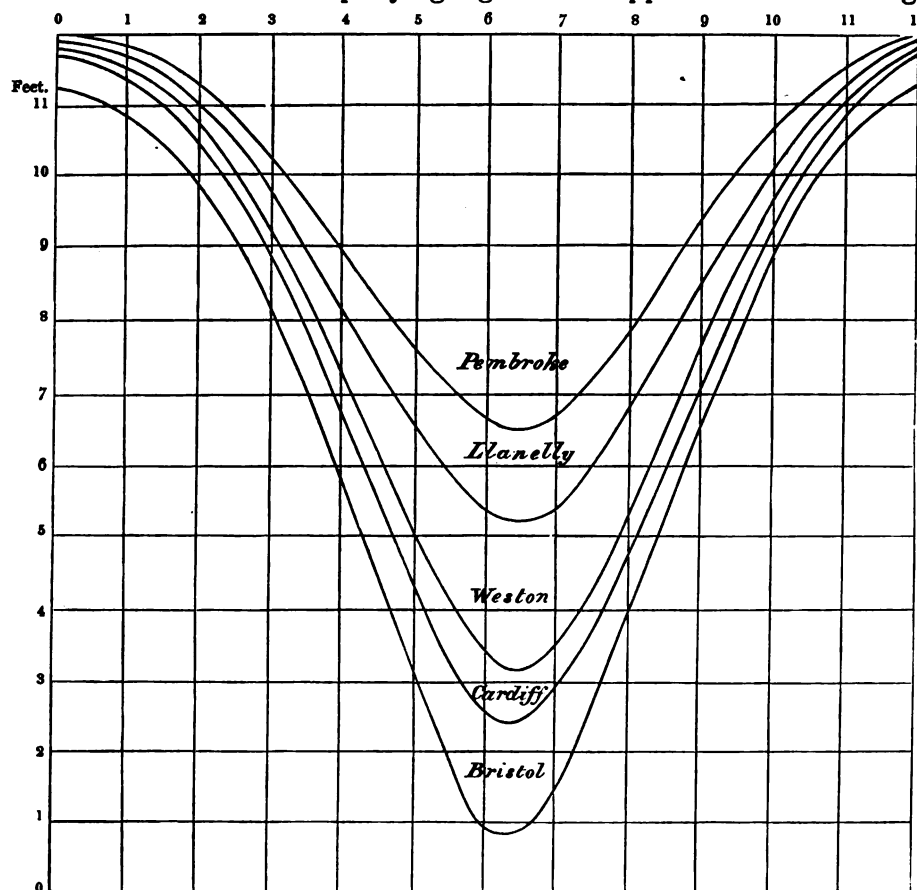
If the ratio just spoken of were constant, we should be able to find the height of

mean water by knowing the excess of springs above neaps: the excess being 10, the mean water would be 7 below the neap high water. But it appears that in general the mean water is lower than this: and the excess of springs being 10, the mean water is from 14 to 19 below neap high water at various points on the coast of Great Britain and France.

In consequence of the law of the high waters, given alike by the theory and by the observations, the spring high waters are above the mean high water for a longer period than the neaps are below it. For it is evident that if DE and DF be each equal to DC , the heights are greater than the mean DC through the arc EAF , which is greater than a semicircle. And it is evident that the excess of AE above a quadrant will be an arc of which the sine is $\frac{\frac{1}{2}EC}{DC}$ or $\frac{5}{24}$; or 12° nearly. Hence the two portions of the semicircle will be, in time, $3^h 24^m$ and $2^h 38^m$; and the tides will be above the mean during $6^h 48^m$ of lunar transit, and below the mean during $5^h 12^m$; and this is found to be very nearly the case at all the places examined; thus confirming the identity of the rule of different places one with another, and with the construction given above.

Additional Note on the Tides of the Bristol Channel.

Mr. Ross has traced the modification which the semimensual inequality of heights undergoes in ascending the Bristol Channel from Pembroke to Bristol. This modification is shown in the accompanying figure. It appears from the diagram which



Mr. Ross has drawn from the observations, that the difference of springs and neaps increases gradually from Pembroke to Llanelly, Weston, Cardiff, and finally Bristol, the difference being 5 ft. 6 in. at the first place, and 10 ft. 6 in. at the last; and the curve which represents the change from day to day being at all the places of the same form, namely, of the form described in the preceding paper.

W. W.

Trinity College, Cambridge,
Nov. 3, 1849.

TABLE A.—Results of Tide Observations arranged according to the amount of excess of Springs above Neaps.

TABLE B.—Places along the same coasts arranged in the order of their “Establishment.”

TABLE A.

Number of observations from which curves were formed.		"Establishment" of the port.	Mean spring rise above mean low water spring.	Mean neap rise above mean low water spring.	Excess of spring over neap.	
		h m	ft. in.	ft. in.	ft. in.	
128	Ardrihaig	12 0	9 2	8 7	0 7	
704	Lowestoft.....	9 57	6 6	5 4	1 2	
673	Belfast	10 43	9 5	8 1	1 4	Third preceding transit.
375	Greenock.....	12 8	9 9	8 2	1 7	Third preceding transit.
107	Ayr Harbour	12 10	8 9	7 2	1 7	
418	Harwich	0 6	11 6	9 9	1 9	Fifth preceding transit.
595	Donaghadee	11 13	11 3	9 5	1 10	
218	Crookhaven.....	4 9	9 10	8 0	1 10	
190	Baltimore.....	4 23	10 2	8 3	1 11	
144	Courtmacsherry	4 36	10 8	8 7	2 1	
233	Skull	4 2	9 8	7 7	2 1	
1,059	Kingstown	11 10	11 0	8 10	2 2	Third preceding transit.
209	Castletown	4 14	9 10	7 7	2 3	
351	Peterhead	0 34	10 9	8 6	2 3	
130	Bantry Harbour	3 47	10 2	7 8	2 6	
90	Arcachon, France	4 37	11 8	9 2	2 6	
77	Boucout, France	3 39	8 8	6 1	2 7	
123	Dunmore.....	5 27	12 3	9 8	2 7	
4,230	Portsmouth	11 41	12 8	10 0	2 8	Third preceding transit.
373	Cork.....	5 1	11 9	9 1	2 8	Third preceding transit.
51	Castletownsend	4 21	10 9	8 1	2 8	
299	Waterford	6 6	13 5	10 9	2 8	
193	Inverness	12 18	12 2	9 6	2 8	
171	Kinsale	4 43	11 7	8 9	2 10	
522	Sligo	6 0	8 8	5 9	2 11	Third preceding transit.
377	Sheephaven.....	5 25	11 11	9 0	2 11	
1,383	Ramsgate.....	11 41	15 6	12 7	2 11	
124	Bordeaux, France	6 50	14 1	11 2	2 11	
4,233	Sheerness.....	0 37	16 1	13 1	3 0	Fifth preceding transit.
105	Loch Inver	6 41	13 11	10 11	3 0	
69	East Looe	5 26	16 2	13 2	3 0	
446	Inishbofin	5 5	12 10	9 9	3 1	
76	Omonville, France	7 29	15 6	12 5	3 1	
692	Westport	4 57	12 8	9 6	3 2	
92	Peel, Isle of Man.....	11 8	16 3	13 1	3 2	
103	Caernarvon	9 33	13 9	10 7	3 2	
156	Port Navallo, France	3 42	12 11	9 9	3 2	
183	Tobermorey.....	5 36	12 10	9 7	3 3	
79	Ramsay, Isle of Man	11 12	19 3	16 0	3 3	
125	Royan, France.....	3 38	13 3	10 0	3 3	
86	St. Surin, France	4 11	14 3	11 0	3 3	
277	Roundstone	4 28	13 6	10 2	3 4	
117	Goodick Pier	6 56	11 7	8 3	3 4	
937	Dundee	2 32	14 7	11 3	3 4	
125	Patiras, France	5 10	15 6	12 2	3 4	
13,400	London	1 59	19 6	16 1	3 5	Fifth preceding transit.
705	Sunderland	3 22	14 5	11 0	3 5	Fourth preceding transit.
541	Holyhead.....	10 11	16 0	12 7	3 5	Third preceding transit.
236	Foynes Island, Shannon	5 35	15 5	12 0	3 5	
356	Scarborough	4 11	15 10	12 5	3 5	Fourth preceding transit.
821	Hartlepool	3 28	15 0	11 7	3 5	Fourth preceding transit.
688	Granton Pier	2 20	16 0	12 7	3 5	
	North Shields	3 30	13 8	10 3	3 5	Fourth preceding transit.
148	Socoa, France	3 19	12 3	8 10	3 5	
159	Dunkirk, France.....	12 8	16 10	13 5	3 5	
2,820	Devonport	5 43	15 5	11 11	3 6	Fourth preceding transit.
2,823	Leith	2 17	16 4	12 9	3 7	Fourth preceding transit.
299	Barfleur, France	8 51	17 0	13 5	3 7	
155	Concarneau, France	3 12	13 1	9 6	3 7	
154	Port Louis, France.....	3 11	13 1	9 5	3 8	

TABLE A. (Continued.)

Number of observa- tions from which curves were formed.		"Establish- ment" of the port.	Mean spring rise above mean low water spring.	Mean neap rise above mean low water spring.	Excess of spring over neap.	
		h m	ft. in.	ft. in.	ft. in.	
104	Cordouan Lighthouse, France	3 37	13 10	10 2	3 8	Third preceding transit.
437	Thurso.....	8 27	13 3	9 6	3 9	
134	Melville, France.....	9 36	21 1	17 4	3 9	
107	La Hougue, France.....	8 42	18 5	14 7	3 10	
66	Aberystwyth.....	7 31	13 5	9 7	3 10	Third preceding transit.
684	Galway.....	4 35	14 10	10 11	3 11	
164	Belleisle, France.....	3 18	14 3	10 4	3 11	
147	Calais, France.....	11 49	19 6	15 7	3 11	
121	Ile d'Yeu.....	3 6	14 2	10 2	4 0	Third preceding transit.
125	Pwllheli.....	7 46	13 8	9 8	4 0	
1,207	Great Grimsby.....	5 36	19 2	15 1	4 1	
153	Limerick, Shannon.....	6 20	16 10	12 8	4 2	
194	Tarbert Island, Shannon.....	4 57	14 6	10 4	4 2	Third preceding transit.
259	Cherbourg, France.....	7 49	16 11	12 9	4 2	
134	Havre, France.....	9 51	22 1	17 11	4 2	
2,116	Dover.....	11 12	18 8	14 5	4 3	
170	Aix Island, France.....	3 20	16 11	12 8	4 3	Fourth preceding transit.
174	St. Nazaire, France.....	3 40	15 2	10 11	4 3	
154	Beagh Castle, Shannon.....	5 49	17 6	13 2	4 4	
157	Mellon, Shannon.....	6 1	18 3	13 10	4 5	
115	Port en Bessin, France.....	8 57	19 11	15 6	4 5	Fourth preceding transit.
703	Hull.....	6 29	20 10	16 4	4 6	
118	Alderney.....	6 46	17 4	12 10	4 6	
258	Douglas, Isle of Man.....	11 12	20 8	16 1	4 7	
156	Noirmoutier Island, France...	3 2	15 11	11 4	4 7	Fourth preceding transit.
129	Goury, France.....	7 6	21 11	17 3	4 8	
153	Cape Grisnez, France.....	11 27	21 6	16 9	4 9	
223	Tarn Point.....	11 22	23 1	18 1	5 0	
213	Whitehaven.....	11 14	23 3	18 2	5 1	Third preceding transit.
138	Beaumaris.....	10 32	21 5	16 4	5 1	
132	Fécamp, France.....	10 44	23 3	18 1	5 2	
513	Brest, France.....	3 47	19 1	13 10	5 3	
62	Honfleur, France.....	9 29	23 4	18 1	5 3	Third preceding transit.
13,400	Liverpool.....	11 16	25 7	20 3	5 4	
135	Boulogne, France.....	11 25	24 10	19 5	5 5	
74	Sein Island, France.....	3 21	17 6	12 1	5 5	
4,236	Pembroke.....	6 12	21 0	15 6	5 6	Third preceding transit.
74	Ushant.....	3 32	19 4	13 9	5 7	
143	Pontasval, France.....	4 26	22 4	16 8	5 8	
305	Roscoff, France.....	4 46	23 1	17 4	5 9	
175	Poulton-le-Sands.....	11 26	27 3	21 6	5 9	Third preceding transit.
149	Ploumarach, France.....	5 15	24 3	18 5	5 10	
176	Abervrach, France.....	4 14	21 9	15 10	5 11	
184	Morlaix Roads, France.....	4 53	23 9	17 10	5 11	
122	St. Ives.....	4 44	21 0	15 1	5 11	Third preceding transit.
72	Ilfracombe.....	5 42	27 4	21 4	6 0	
221	Dieppe.....	11 6	27 0	20 8	6 4	
303	Fleetwood.....	11 12	26 3	19 8	6 7	
292	Port des Enfants, France.....	5 17	25 5	18 10	6 7	Third preceding transit.
117	Cayeux, France.....	11 5	27 6	20 11	6 7	
391	Brehat, France.....	5 51	31 2	23 7	7 7	
81	Lezardrieux, France.....	5 54	30 3	22 2	8 1	
119	Jersey.....	6 15	29 9	21 5	8 4	Third preceding transit.
370	Weston-super-Mare.....	6 54	37 3	28 9	8 6	
129	Ecrehou.....	6 32	30 11	22 5	8 6	
139	Erqui.....	5 59	33 3	24 5	8 10	
153	St. Malo, France.....	6 5	35 0	25 10	9 2	Third preceding transit.
277	Chausey, France.....	6 9	35 2	25 10	9 4	
294	Granville, France.....	6 13	37 1	27 2	9 11	
138	Portishead.....	7 11	41 4	31 1	10 3	

TABLE B.

	Establishment.		Establishment.		Establishment.
	h m		h m		h m
<i>From the Land's End to Ramsgate.</i>		Sunderland (4).....	3 22	<i>From Arcachon in the Bay of Biscay to Dunkirk.</i>	
East Looe.....	5 26	Hartlepool.....	3 28	Arcachon	4 37
Devonport (4th transit?)	5 43	Scarborough (4)	4 11	Bordeaux	6 50
Portsmouth (3rd transit)	11 41	Hull (4)	6 29	St. Surin	4 11
Dover (3).....	11 12	Great Grimsby	5 36	Royan	3 38
Ramsgate	11 41	Lowestoft	9 57	Cordouan	3 27
<i>From the Land's End up St. George's Channel round the North of Scotland to London.</i>		Harwich (5).....	0 6	Ile d'Aix	3 20
St. Ives.....	4 44	Sheerness (5)	0 37	Ile d'Yeu	3 6
Ilfracombe	5 42	London (5)	1 59	Noirmoutier Island	3 2
Weston-super-Mare (3)	6 54	<i>From Bantry Bay up St. George's Channel round the North of Ireland to the Shannon.</i>		St. Nazaire.....	3 40
Portishead	7 11	Bantry Harbour	3 47	Belleisle.....	3 18
Pembroke (3)	6 12	Castletown (Berehaven)	4 14	Port Louis.....	3 11
Goodick Pier	6 56	Skull	4 2	Concarneau	3 12
Aberystwyth.....	7 31	Crookhaven	4 9	Brest (3)	3 47
Pwllheli	7 46	Baltimore	4 23	Ushant	3 32
Caernarvon	9 33	Castletownsend	4 21	Morlaix Roads	4 53
Holyhead	10 11	Courtmacsherry	4 36	Brehat	5 51
Beaumaris.....	10 32	Kinsale	4 43	Erqui.....	5 59
Liverpool (3)	11 16	Cork (3)	5 1	St. Malo	6 5
Fleetwood.....	11 12	Dunmore	5 27	Granville	6 13
Tarn Point	11 22	Waterford	6 6	Chausey.....	6 9
Poulton-le-Sands	11 26	Kingstown (3)	11 10	Jersey	6 15
Peel, Isle of Man	11 8	Donaghadee	11 13	Ecrehou.....	6 32
Douglas, Isle of Man ...	11 12	Belfast (3).....	10 43	Alderney	6 46
Ramsay, Isle of Man ...	11 12	Sheephaven	5 25	Cherbourg.....	7 49
Whitehaven	11 14	Sligo (3)	6 0	Barfleur	8 51
Ayr	12 10	Westport	4 57	La Hougue	8 42
Greenock (3)	12 8	Inishbofin	5 5	Honfleur	9 29
Thurso (3)	8 27	Roundstone	4 28	Havre	9 51
Inverness	12 18	Galway (3)	4 35	Fécamp.....	10 44
Peterhead.....	0 34	Tarbert, Shannon	4 57	Dieppe	11 6
Dundee.....	2 32	Foynes Island, Shannon	5 35	Cayeux	11 5
Granton (4).....	2 20	Beagh Castle, Shannon...	5 49	Boulogne	11 25
Leith.....	2 17	Mallon, Shannon	6 1	Cape Grisnez.....	11 27
North Shields (4)	3 30	Limerick, Shannon	6 20	Calais.....	11 49
				Dunkirk.....	12 8

For the places not otherwise marked in these Tables, the tides were referred to the transit immediately preceding, as giving sufficient exactness for general maritime purposes: but observations received at the Admiralty since the above laws were discovered, have been referred to the third, fourth, or fifth preceding transit, according to their place in Table B.

The Devonport tides discussed some years ago, apart from those of neighbouring places, appeared to give the greatest exactness with the *fourth* preceding transit, which has accordingly been used in the Admiralty Tables. There can be no doubt at present that the *third* preceding transit is more correct for this port; but the labour of recalculating new Tables would be great, and the difference of the result would never be more than one minute in the time and one inch in the height.

XII. *Experiments and Observations upon the Properties of Light.*

By Lord BROUGHAM, F.R.S.,

Member of the National Institute, and of the Royal Academy of Naples.

Received November 6, 1849,—Read January 10, 1850.

THE optical inquiries of which I am about to give an account, were conducted at this place in the months of November and December 1848, and continued in autumn 1849 at Brougham, where the sun proved of course much less favourable than in Provence: they were further prosecuted in October. I had thus an opportunity of carefully reconsidering the conclusions at which I had originally arrived; of subjecting them first to analytical investigation, and afterwards to repetition and variation of the experiments; and of conferring with my brethren of the Royal Society and of the National Institute. The climate of Provence is singularly adapted to such studies. I find, by my journal of 1848, that during forty-six days which I spent in those experiments, from 8 A.M. to 3 P.M., I scarcely ever was interrupted by a cloud, although it was November and December*. I have since had the great benefit of a most excellent set of instruments made by M. SOLEIL of Paris, whose great ingenuity and profound knowledge of optical subjects can only be exceeded by his admirable workmanship. I ought however to observe, that although his heliostate is of great convenience in some experiments, it yet is subject (as all heliostates must be) to the imperfection of losing light by reflexion, and consequently I have generally been obliged to encounter the inconvenience of the motion of the sun's image, especially when I had to work with small pencils of light. This inconvenience is materially lessened by using horizontal prisms and plates.

Although I have made mention of the apparatus of great delicacy which I employed, it must be observed that this is only required for experiments of a kind to depend upon nice measurements. All the principles which I have to state as the result of my experiments in this paper, can be made with the most simple apparatus, and without any difficulty or expense, as will presently appear.

It is perhaps unnecessary to make an apology for the form of definitions and propositions into which my statement is thrown. This is adopted for the purpose of making the narrative shorter and more distinct, and of subjecting my doctrines to a fuller scrutiny. I must further premise that I purposely avoid all arguments and

* Of seventy-eight days of winter in 1849, I had here only five of cloudy weather. Of sixty-one days of summer at Brougham, I had but three or four of clear weather; one of these fortunately happened whilst Sir D. BREWSTER was with me, and he saw the more important experiments.

suggestions upon the two rival theories—the Newtonian or Atomic, and the Undulatory. The conclusions at which I have arrived are wholly independent, as it appears to me, of that controversy. I cautiously avoid giving any opinion upon it; and instead of belonging to the sect of undulationists or anti-undulationists, I incline to agree with my learned and eminent colleague M. BIOT, who considers himself as a “*Rieniste*,” and neither “*ondulationiste*” nor “*anti-ondulationiste*.”

Chateau Eleanor-Louise (Provence),*
1st November 1849.

DEFINITIONS.

1. *Flexion* is the bending of the rays of light out of their course in passing near bodies. This has been sometimes termed *diffraction*, but *flexion* is the better word.

2. Flexion is of two kinds—*inflexion*, or the bending towards the body; *deflexion*, or the bending from the body.

3. *Flexibility*, *deflexibility*, *inflexibility*, express the disposition of the homogeneous or colour-making rays to be bent, deflected, inflected by bodies near which they pass.

Although there is always presumed to be a flexion and a separation of the most flexible rays from the least flexible (the red from the violet for example) when they pass by bodies, yet the compound rays are not so presumed to be decomposed when reflected by bodies. This is probably owing to the successive inflexions and deflexions before and after reflexion, correcting each other and making the whole beam continue parallel and undecomposed instead of becoming divergent and being decomposed.

PROPOSITION I.

The flexion of any pencil or beam, whether of white or of homogeneous light, is in some constant proportion to the breadth of the coloured fringes formed by the rays after passing by the bending body. Those fringes are not three, but a very great number, continually decreasing as they recede from the bending body, in deflexion, where only one body is acting; and they are real images of the luminous body by whose light they are formed.

Exp. 1. If an edge be placed in a beam or in a pencil of white light, fringes are formed outside the shadow of the edge and parallel to it, by deflexion. They are seen distinctly to be coloured, the red being furthest from the shadow, the violet nearest, the green in the middle between the red and the violet. The best way to observe this is to receive the light on an instrument composed of two vertical and two horizontal plates, each moving by a screw so as to increase or lessen the distance

* In experiments at this place, in winter, I found one great advantage, namely, the more horizontal direction of the rays. In summer they are so nearly vertical, that a mirror must be used to obtain a long beam or pencil, which is often required in these experiments, and so the loss of light countervails the greater strength of the summer sun's light.

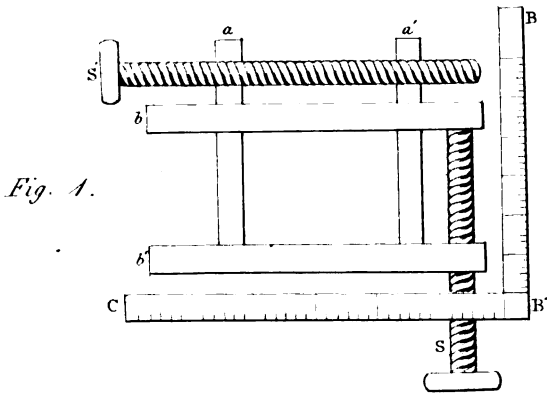


Fig. 2.

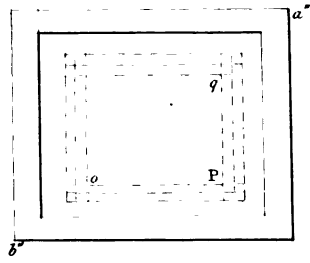


Fig. 3.

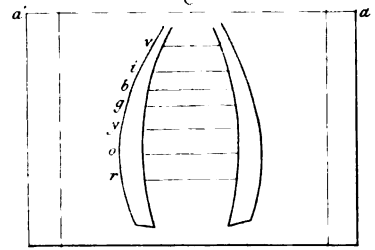


Fig. 4.

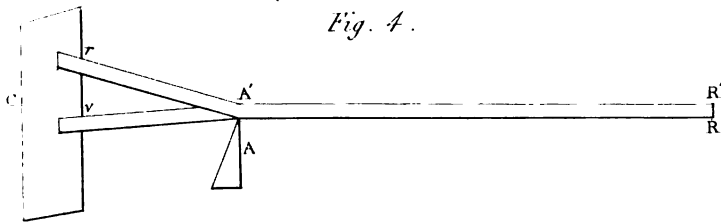


Fig. 6.

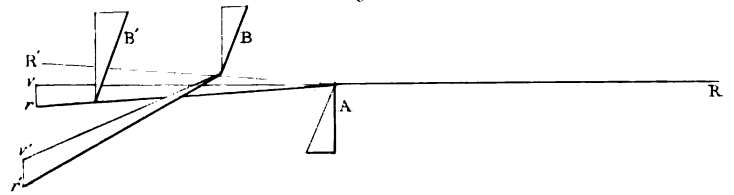


Fig. 5.

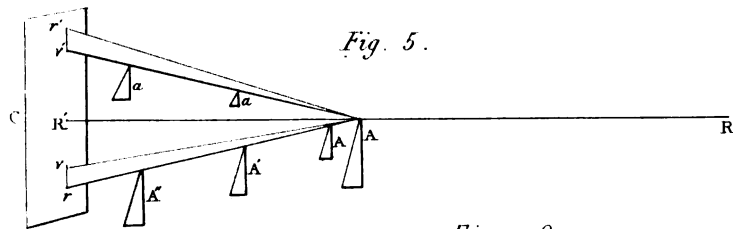


Fig. 7.

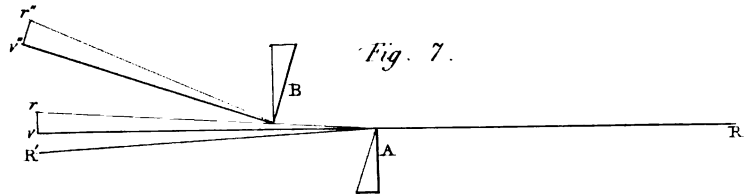


Fig. 8.

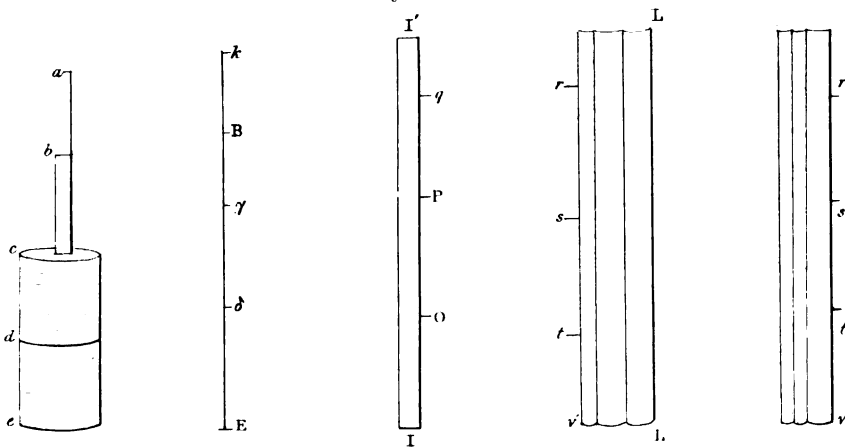


Fig. 9.

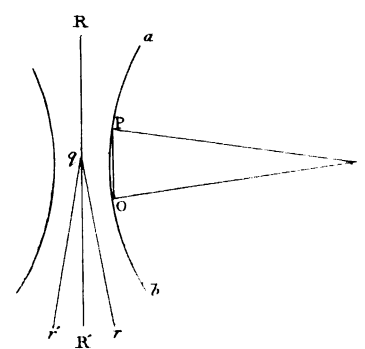


Fig. 10.

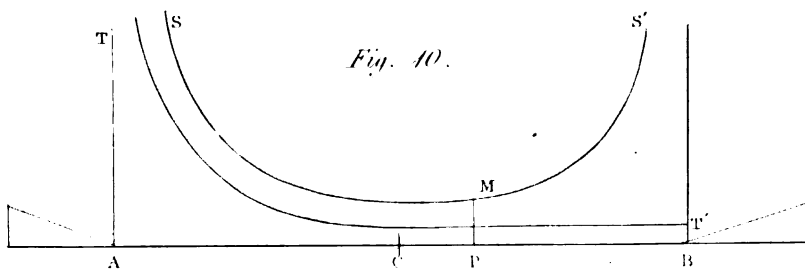
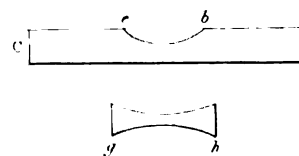


Fig. 12.



between the opposite edges. a, a' are (Plate X. fig. 1) the vertical, b, b' the horizontal edges, s, s' are the screws; and these may be fitted with micrometers, so as to measure very minute distances of the edges by graduated scales $BB', B'C$. For the purpose of the present proposition the aperture only needs be considered, of about a quarter of an inch square. The light passing through this aperture is received on a chart placed first one foot, and then several feet from the instrument. The fringes are increased in breadth by inclining the chart till it is horizontal, or nearly so, when the fringes parallel to b, b' are to be examined, and holding it inclined laterally when the fringes parallel to a, a' are to be examined. It is also convenient to let the white light beyond the fringes pass through; and for this purpose, a'', b'' being the figure of the instrument (fig. 2), and the light received on the chart, a hole may be made in its centre opq , through which the greater portion of the white light may be suffered to pass. The fringes are plainly seen to run parallel to the edges forming them; as op parallel to b'' and pq parallel to a'' . The reddish is furthest from the shadow, the bluish nearest that shadow; also the fringe nearest the shadow is the broadest, the rest decrease as they recede from the shadow into the white light of the disc. Sometimes it is convenient to receive the fringes on a ground glass plate, and to place the eye behind it. They are thus rendered more perceptible.

When the edges are placed in homogeneous light, they are all of the colour which passes by any edge; and two diversities are here to be noted carefully. *First*, the fringes made by the red light are broader than those made by any of the other rays, and the violet are the narrowest, the intermediate fringes being of intermediate breadths. *Second*, the fringes made by the red are furthest from the direct rays, the violet nearest those rays, the intermediate at intermediate distances. This is plainly shown in the following experiment.

Exp. 2. In fig. 3, C represents the image of the aperture when the rays of the prismatic spectrum are made to pass through it. But instead of making the fringes by a single edge deflecting, and so casting them in the spectrum, I approach the opposite edges, so that both acting together on the light, the fringes are seen in the shadow and surrounding the spectrum. These fringes are no longer parallel to the shadows of the edges as they were in the white light, but incline towards the most refrangible and least flexible rays, and away from the least refrangible and most flexible. Thus the red part r of the fringes is nearest the shadow of the edge a' ; the orange, o , next; then yellow, y ; green, g ; blue, b ; indigo, i ; and violet, v . Moreover, the fringe rv is both inclined in this manner, so that its axis is inclined, and also its breadth increases gradually from v to r . This is a complete refutation of the notion entertained by some that Sir I. NEWTON's experiment of measuring the breadths in different coloured lights and finding the red broadest, the violet narrowest, explains the colours of the fringes made in white light as if these were only owing to the different breadths of the fringes formed by the different rays. The present experiment clearly proves, that not only the fringes are broadest in the least refrangible rays, but

those rays are bent most out of their course, because both the axis of the fringes is inclined, and also their breadths are various.

Exp. 3. Though called by GRIMALDI, the discoverer, the three fringes, as well as by NEWTON and others who followed him, they are seen to be almost innumerable, if viewed through a prism to refract away the scattered light that obscures them. I stated this fact many years ago*.

Exp. 4. That the fringes are images may be at once perceived, not when formed in the light disc as in some of the foregoing experiments, but when formed in the shadow. Thus when the opposite edges are moved so near one another as to form fringes bordering the luminous body's image, they are formed like the disc they surround. When you view a candle through the interval of the opposite edges, you perceive that the fringes are images of its flame, with the wick, and that they move as the flame moves to and fro. When you observe the half-moon in like manner, you perceive that the side of the fringes answering to the rectilinear side of the moon, are rectilinear, and the other side circular; and when the full moon is thus viewed, the fringes on both sides are circular. The circular disc of the moon is, indeed, drawn or elongated as well as coloured. It is, that is to say, the fringe or image is exactly a spectrum by flexion. Like the prismatic spectrum, it is oblong, not circular, and it is coloured; only that its colours are much less vivid than those of the prismatic spectrum.

PROPOSITION II.

The rays of light, when inflected by bodies near which they pass, are thrown into a condition or state which disposes them to be on one of their sides more easily deflected than they were before the first flexion; and disposes them on the other side to be less easily deflected: and when deflected by bodies, they are thrown into a condition or state which disposes them on one side to be more easily inflected, and on the other side to be less easily inflected than they were before the first flexion.

Let RA (fig. 4) be a ray of light whose opposite sides are RA, R'A', and let A be a bending edge near which the ray passes, the side R'A' acquires by A's inflexion, a disposition to be more easily deflected by another body placed between A and the chart C, and the side RA acquires a disposition to be less easily deflected than before its first flexion; and in like manner R'A' acquires a disposition to be more easily inflected, and RA a disposition to be less easily inflected by a body placed between A and C.

Exp. 1. Place A' (fig. 5) in any position between A and vr , the image made on C by A's influence, as at A' or A'', or close to A at A'''. If it is placed on the same side of the ray with A, no difference whatever can be perceived to be made on the breadth of rv , or on its distance vR' from the direct ray RR' . In like manner the image by deflexion $r'v'$ is not affected at all, either in its breadth, or in its removal from RR' by any object, a , a' , placed on the same side with A of the deflected ray $A v'$.

* Philosophical Transactions, 1797, Part II.

But (fig. 6) place B anywhere between A and vr on the side of the ray opposite to A, and the breadth of rv is increased, and also its distance from the direct ray RR' , as $v'r'$; and in like manner (fig. 7) the deflected rays Av, Ar are both more separated, making a broader image at $r''v''$, and are further removed from RR' by B's inflexion.

Exp. 2. If you bend the rays either by a single edge, or by the joint action of two edges, it makes not the least difference either in the breadth or in the distance from the direct rays of the images, or in the distension or elongation of the luminous body's disc, whether the bending body is a perfectly sharp edge (which in regard to the rays of light is a surface, though a narrow one), or is a plane (that is, a broader surface), or is a curve surface of a very small, or of a very large radius of curvature.

In fig. 8, ae is an instrument composed of four pieces of different forms, but all in a perfectly straight line; ab is an extremely sharp edge; bc a flat surface; cd a cylindrical or circular surface of a great radius of curvature; de one of a small radius of curvature. But all these pieces are so placed that $E\delta\gamma$ is a tangent to ed, dc , and is a continuation of $\gamma\beta K$, that is, of cb, ba . So the light passing by the whole $abcde$, passes by one straight line EK , uniting or joining the four surfaces. It is found that the image or fringe II' , made by $abcde$ (or $E\delta\gamma\beta K$), is of the same breadth and in the same position throughout its whole length. So if directly opposite to this edge another straight edge is placed, and acts together with $abcde$ on the light passing, the breadth of the fringe I is increased, and its distance is increased from the direct rays, but it has the exact same breadth from I to I' ; its portion $I'q$ answering to ab , qP answering to bc , PO answering to cd , and OI answering to de , are of the same breadth, provided care be taken that the second edge is exactly parallel to the edge EK . And this experiment may be made with the second edge behind $abcde$, as in Exp. 1 of this proposition; also it may be usefully varied by having the second edge composed of four surfaces like the first, only it becomes the more necessary to see that this compound edge is accurately made and kept quite parallel to the first, any deviation, however minute, greatly affecting the result. When care is thus used the fringes are as in $rv, v'r'$, quite the same in breadth and in position through their whole length; and not the least difference is to be discerned in them, whether made by a second edge, which is one sharp edge, or by a compound second edge, similar to $abcde$.

Hence I conclude that the beam passing by the compound edge, or compound edges, is exactly as much distended by the different flexibility of the rays, and is exactly as much bent from its direct course when the flexion is performed by a sharp edge, by a plane surface, by a very flat cylinder, or by a very convex cylinder; and therefore that all the action of the body on the rays is exercised by one line, or one particle, and not first by one and then by others in succession; and this clearly proves that after a first flexion takes place, no other flexion is made by the body on the same side of the rays. This is easily shown.

For a plane surface is a series or succession of edges infinitely near each other;

and a curve surface in like manner is a succession of infinitely small and near plane surfaces or edges. Let ab (fig. 9) be the section of such a curve surface. The particle P coming first near enough the ray RR' to bend it, then the next particle O is only further distant from RR' , the unbent ray, than the particle P by the versed sine of the infinitely small arch OP . But O is not at all further distant than P from the ray bent by P into qr , and yet we see that O produces no effect whatever on the ray after P has once bent it. No more do any of the other particles within whose spheres of flexion the ray bent by P passes. The deflected ray qr' no doubt is somewhat more distant from O than the incident ray was from P , but not so far as to be beyond O 's sphere of deflexion; for O acts so as to make the other fringes at greater distances than the first. Consequently O could act on the first fringe made by P as much as P can in making the second, third, and other fringes; and if this be true of a curve surface, it is still more so of a plane surface; all whose particles are clearly equidistant from the ray's original path, and the particles after the first are in consequence of that first particle's flexion nearer the bent ray, at least in the case of inflexion. But it is to be observed, moreover, that in the experiment with two opposite edges, inflexion enters as well as deflexion, and consequently this demonstration, founded on the exact equality of the fringes made by compound double edges, appears to be conclusive. For it must be observed that this experiment of the different edges and surfaces, plane and curve, having precisely the same action, is identical with the former experiment of two edges being placed one behind the other, and the second producing no effect if placed on the same side of the ray with the first edge. These two edges are exactly like two successive particles of the same surface near to which the rays pass. Consequently the two experiments are not similar but identical; and thus the known fact of the edge and the back of a razor making the same fringes, proves the polarization of the rays on one side. Thus the proposition is proved as to polarization.

Exp. 3. The proposition is further demonstrated, as regards disposition, in the clearest manner by observing the effect of two bodies, as edges, whether placed directly opposite to each other while the rays pass between them so near as to be bent, or placed one behind the other but on opposite sides of the rays. Suppose the edges directly opposite one to the other, and suppose there is no disposition of the rays to be more easily bent by the one edge in consequence of the other edge's action. Then the breadth and distension and removal of the fringes caused by the two edges acting jointly, would be in proportion to the sum of the two separate actions. Suppose that one edge deflects and the other inflects, and suppose that inflexion and deflexion are equal at equal distances, following the same law; then the force exerted by each edge being equal to d , that exerted by both must be equal to $2d$. But instead of this we find it equal to $5d$, or $6d$, which must be owing to the action of the two introducing a new power, or inducing a new disposition on the rays beyond what the action of one did.

If, however, we would take the forces more correctly (fig. 10), let A and B be the two edges, and let their spheres of flexion be equal, AC(=a) being A's sphere of inflexion and B's sphere of deflexion; BC(=a) being A's sphere of deflexion and B's sphere of inflexion; and let the flexion in each case be inversely as the m th power of the distance. Let CP= x , PM= y , the force acting on a ray at the distance $a+x$ from A and $a-x$ from B. Then if B is removed and only A acts, $y = \frac{1}{(a+x)^m}$. If B also acts,

$$y' = \frac{1}{(a+x)^m} + \frac{1}{(a-x)^m}.$$

Now the loci of y and y' are different curves, one similar to a conic hyperbola, the other similar to a cubic; but of some such form when $m=1$, as SS' and TT'. It is evident that the proportion of $y:y'$ can never be the same at any two points, and consequently that the breadths of the fringes made by the action of one can never bear the same proportion to the breadths of those made by the action of both, unless we introduce some other power as an element in the equation, some power whereby from both values, y and y' , x may disappear, else any given proportion of $y:y'$ can only exist at some one value of x . Thus suppose (which the fact is) $y:y'::1:5$ or $1:6$, say $::1:6$, this proportion could only hold when

$$x = \frac{\left(5^{\frac{1}{m}} - 1\right)a}{5^{\frac{1}{m}} + 1} \text{ or } = \frac{\left(4^{\frac{1}{m}} - 1\right)a}{4^{\frac{1}{m}} + 1}, \text{ if } y:y'::1:5.$$

When $m=2$, the force being inversely as the square of the distance, then $x = \frac{a}{3}$ and $x = \frac{(\sqrt{5}-1)}{\sqrt{5}+1} a$, are the values at which alone $y:y'::1:5$ and $1:6$ respectively.

But this is wholly inconsistent with all the experiments; for all of these give nearly the same proportion of $y:y'$ without regard to the distance, consequently the new element must be introduced to reconcile this fact. Thus we can easily suppose the conditions, *disposition* and *polarization* (I use the latter term merely because the effect of the first edge resembles polarization, and I use it without giving any opinion as to its identity), to satisfy the equation by introducing into the value of y some function of $a-x$. But that the joint action of the two edges never can account for the difference produced on the fringes, is manifest from hence, that whatever value we give to m , we find the proportion of $y':y$ when $x=0$ only that of double, whereas 5 or 6 times is the fact. The same reasoning holds in the case of the spheres of flexion being of different extent; and there are other arguments arising from the analysis on this head, which it would be superfluous to go through, because what is delivered above enables any one to pursue the subject. The demonstration also holds if we suppose the deflective force to act as $\frac{1}{n}$ of the distance, while that of inflexion acts as $\frac{1}{m}$.

But I have taken $m=n$ as simpler, and also as more probably the fact.

I have said that the rays become less easily inflected and deflected; but it is plain

that on the polarized side they are not inflected or deflected at all. Their disposition on the opposite side is a matter of degree; their polarization is absolute and their flexion null.

PROPOSITION III.

The rays disposed on one side by the first flexion are polarized on that side by the second flexion, and the rays polarized on the other side by the first flexion are depolarized and disposed on that side by the second flexion.

This proposition is proved by carefully applying the first experiment of Prop. II.; but great care is required in this experiment, because when three edges are used consecutively, the third edge often appears to act on rays previously acted on by both the other two, when it is only acting on those previously acted on by one or other of those two. Thus when edge A has inflected and edge B afterwards deflects the rays disposed by A, a third edge C may, when applied on the side opposite to B, seem to increase the flexion, and yet on removing A altogether we may find the same effect continue, which proves that the only action exercised had been by B and C, and that C had not acted on rays previously bent by both A and B, which the experiment of course requires to prove the proposition. I was for a long while kept in great uncertainty by this circumstance, whether the third edge ever acted at all. That it never acted on the side of the ray on which the second edge acted, I plainly saw; but I frequently changed my opinion whether or not it acted on the opposite side, that is, on the same side with the first edge. Nor could I confidently determine this important point until I had the benefit of an instrument which I contrived for the purpose, and which, executed by M. SOLEIL, enabled me satisfactorily to perform the *experimentum crucis* as follows:—

In fig. X. AB is a beam, on a groove (of which the sides are graduated) three uprights are placed, the one, B, fixed, the other two, C and D, moving in the groove

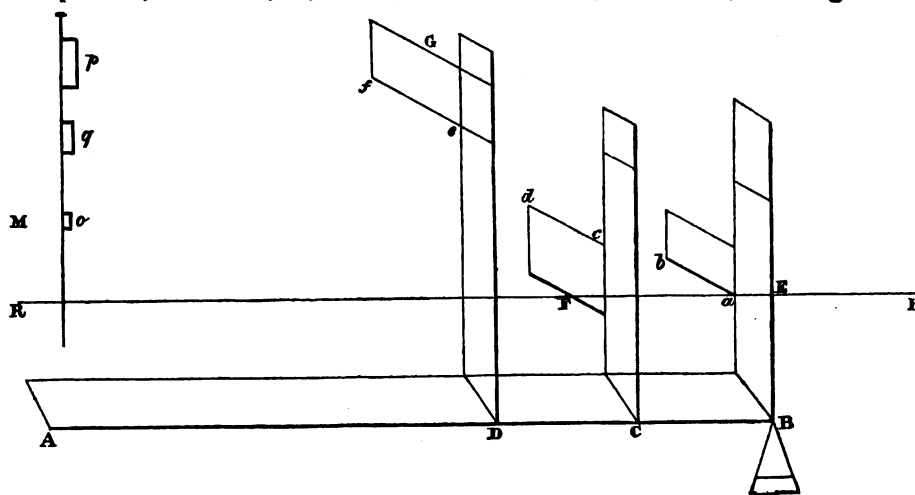


fig. X.

of AB. On each of the uprights is a broad sharp-edged plate, moving up and down the upright by a rack and pinion, so that both the plates F, G could be approached as near as possible to each other, and so could F be approached to the plate E on the

fixed upright B; while also each of the three plates could be brought as near the rays that passed as was required; and so could each be brought as near the opposite edge of the neighbouring plate. It is quite necessary that this instrument should be heavy in order to give it solidity; it is equally necessary that the rack and pinion movement should be just and also easy; for the object is to fix the plates at will, so that their position in respect of the rays may be easily changed, and when once adjusted may be immovable until the observer desires to change their position.

The light was passed under the plate E and acted upon by ab , its lower edge. The second plate E was then raised on C so as to act on the side of the rays opposite to ab , by its upper edge cd . The fringes inflected by ab were thus deflected by cd , in virtue of the disposition given to the side next cd . Then the third plate G, on its stand D, was moved so that it could be brought to act by its lower edge ef , which was approached to the rays deflected by cd , and placed on their opposite side. The action was observed by examining the fringes on the chart M. Those which had been as o , made by the joint action of the two first edges E, F, were seen to move upwards to p as the third edge G came near the rays; and p was both broader than o , and further removed from the direct rays RR' . In order to make quite sure that this change in the size and position of o had not been occasioned by the mere action of two plates, as E and G or F and G, it was quite necessary to remove first E, by drawing it up the stand B. If the fringe p then vanished, complete proof was afforded that E had acted as well as G. Then F was removed, and if p vanished, proof was afforded that F acted as well as E and G. A very convenient variation of the experiment was also continued and was found satisfactory. When the joint action of F and G gave a fringe, as at q , E being removed up the stand B, then E was gently moved down that stand, and as it approached the pencil, which was on its way to F and G, you plainly perceived the fringe enlarged and removed from q to p . These experiments were therefore quite crucial, and demonstrated that all the edges had concurred to form the fringe at p , the first and third inflecting, the second deflecting.

The same experiments were made on the fringes formed by the deflexion of the first edge and the inflexion of the second, and the deflexion of the third.

It is thus perfectly clear that the rays bent by the first edge and disposed on their side opposite to that edge, are bent in the other direction by the second edge acting on that opposite side, and are afterwards again bent in the direction of the first bending by the action of the third edge upon the side which was opposite the second edge and nearest the first edge. But this side is the one polarized by the first edge, and therefore that side is depolarized by the action of the second edge. Hence it is proved that the rays polarized by one flexion are depolarized by a second; and as it is proved by repeated experiments that no body placed on the same side of the rays with any of the bending bodies, whether the first or the second or the third, exercises any action on those rays, it is thus manifest that any one flexion having

disposed, a second polarizes the disposed side; and that any one flexion having polarized, a second flexion depolarizes and disposes the polarized side.

Exp. 3. Another test may be applied to this subject. Instead of a rectilinear edge, I made use of edges formed into a curve, as in fig. 12, where C is such an edge, and then the figure made is $g h$, corresponding to the curve $e b$. The first edge in the last experiment being formed like C, instead of a straight-lined edge, we can at once perceive that it has acted on the rays as well as the second and third edges, because these being straight-lined, never could give the comb-like shape $g h$ to the fringes. This completely confirmed the other observations, and made the inference irresistible.

PROPOSITION IV.

The disposition communicated by the flexion to the rays is alternative; and after inflexion they cannot be again inflected on either side; nor after deflexion can they be deflected. But they may be deflected after inflexion and inflected after deflexion, by another body acting upon the sides disposed, and not by its acting upon the sides polarized.

This is gathered from the experiments in proof of the second and third propositions.

PROPOSITION V.

The disposition impressed upon the rays, whether to be easily deflected or easily inflected by a second bending body, is strongest nearest the first bending body, and decreases as the distance between the two bodies increases.

Plate XI. fig. 11. Let $AB = a$ be the distance between the first bending body and a given point, more or less arbitrarily assumed; P the second body; $AP = x$; $PM = y$, the force exerted by the second body at P ; $C =$ the chart; $PM = y$ is in some inverse proportion to AP , but not as $\frac{1}{AP^m}$ or $\frac{1}{x^m}$, because it is not infinite at A , but of an assignable value there; therefore $y = \frac{1}{(a+x)^m}$; and the curve which is the locus of P has an asymptote at B , when $x = -a$. The fringes being received on the chart at C , it might be supposed that the difference in their breadth, by which I measure the force, or y , is owing to P approaching the chart C , in proportion as it recedes from A , and thus making the divergence less in the same proportion; but the experiments are wholly at variance with this supposition.

Exp. 1. The following table is the result of one such experiment. The first column contains the distances horizontally of P from A , being the sines of the angle made by the rays with the vertical edges; the second column contains the real distance of the second from the first edge, the secant of that angle; the third column gives the breadths of the fringes at the distances given in the preceding columns; the fourth gives the values of y , supposing MN were a conic hyperbola.

Fig. 11.

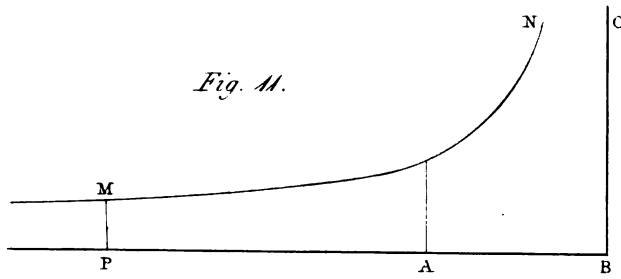


Fig. 13.



Fig. 14.

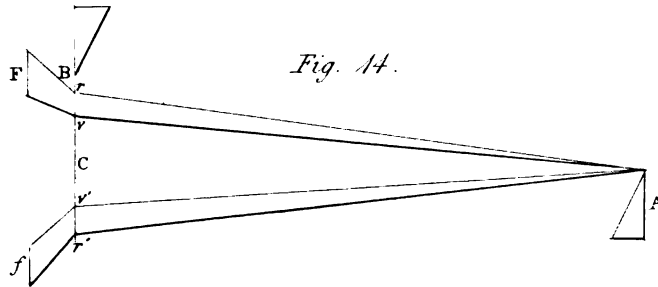


Fig. 15.

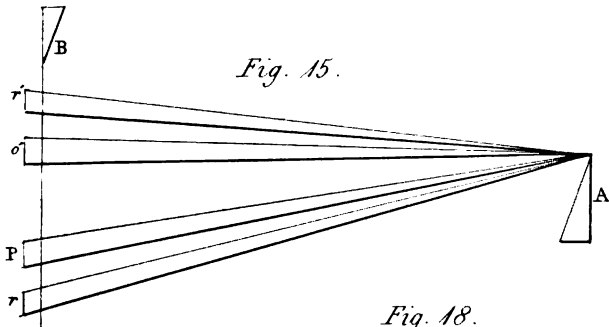


Fig. 18.

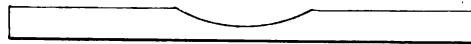


Fig. 20.

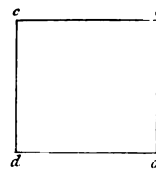
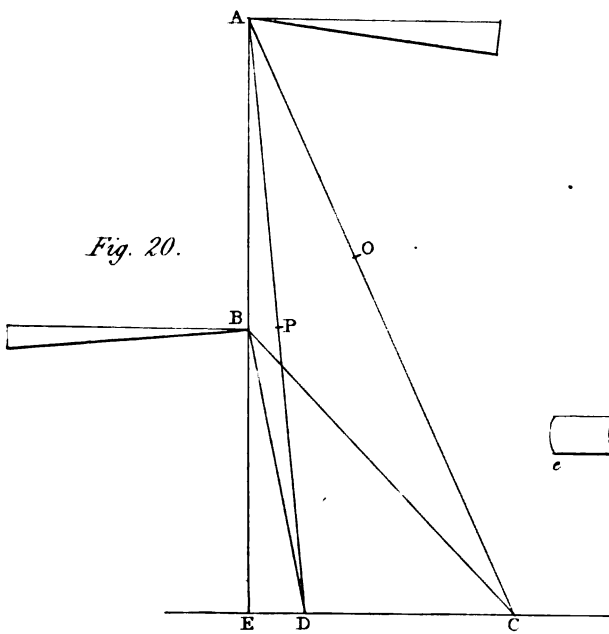


Fig. 19.

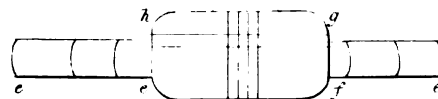


Fig. 19.

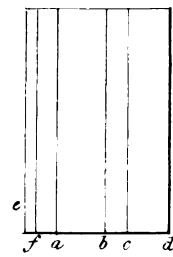


Fig. 16.

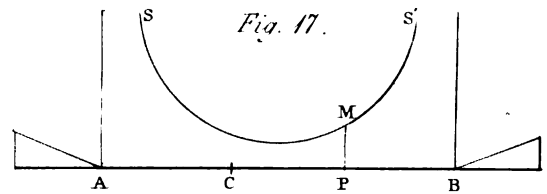


Fig. 17.

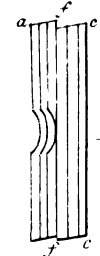
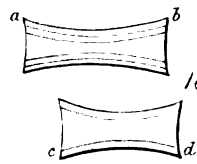


Fig. 21.

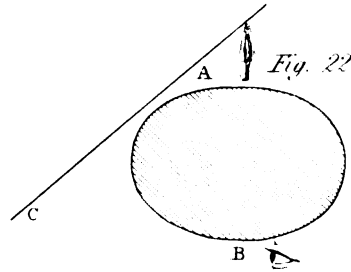


Fig. 22.

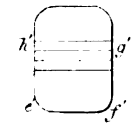


Fig. 19.

Sines.	Secants.	Real value of y .	Hyperbolic value.
20	35	$3\frac{1}{2}$	$3\frac{1}{2}$
65	85	$1\frac{1}{2}$	$1\frac{1}{2}$
85	$107\frac{1}{2}$	$1\frac{1}{2}$	$1\frac{1}{2}$
195	240	$0\frac{1}{2}$	$0\frac{1}{2}$

The unit here is $\frac{1}{40}$ th of an inch.

It is plain that this agrees nearly with the conic hyperbola, but in no respect with a straight line; and upon calculating what effect the approach of P to C would have had, nothing could be more at variance with these numbers. But

Exp. 2. All doubt on this head is removed by making P the fixed point, and moving the first edge A nearer or further from it. In this experiment, the disturbing cause, arising from the varying distance from the chart, is entirely removed; and it is uniformly found that the decrease in the force varies notwithstanding with the increase of the distance. I have here only given the measures by way of illustration, and not in order to prove what the locus of y (or P) is, or, in other words, what the value of m is.

Exp. 3. When one plate with a rectilinear edge is placed in the rays, and a second such plate is placed at any distance between it and the chart, the fringes are of equal breadth throughout their length, and all equally removed from the direct rays, each point of the second edge being at the same distance from the corresponding point of the first. But let the second plate be placed at an angle with the first, and the fringes are very different. It is better to let the second be parallel to the chart, and to incline the first; for thus the different points of the fringes are at the same distance from the edge which bends the disposed rays. In fig. 13, B is the second plate, parallel to the chart C; A is the first plate; all the points of B, from D to E, are equidistant from C; therefore nothing can be ascribed to the divergence of the bent rays. B bends the rays disposed by A at different distances DD' and EE' from the point of disposition. The fringe is now of various breadths from dd' to e , the broadest part being that answering to the smallest distance of D, the point of flexion, from D' the point of disposition; the narrowest part, e , answering to EE', or the greatest distance of the point of flexion from the point of disposition. Moreover, the whole fringe is now inclined; it is in the form of a curve from dd' to e , and the broad part dd' , formed by the flexion nearest the disposition, is furthest removed from the direct rays; the narrowest part, e , is nearest these direct rays. It is thus quite clear that the flexion by B is in some inverse proportion to the distance at which the rays are bent by B from the point where they were disposed by A. I repeatedly examined the curve de , and found it certainly to be the conic hyperbola. Therefore $m=1$, and the equation to the force of disposition is $y=\frac{1}{x}$.

In order to ascertain the value of m , I was not satisfied with ordinary admeasurements, but had an instrument made of great accuracy and even delicacy. It con-

sisted of two plates, A and B (Plate VI.), with sharp rectilinear edges, one, A, horizontal, the other, B, moving vertically on a pivot, and both nicely graduated. The angle at which the second plate was vertically inclined to the first, was likewise ascertained by a vertical graduated quadrant E. Moreover the edges moved also horizontally, and their angle with each other was measured by a horizontal graduated quadrant K. There was a fine micrometer F to ascertain the distances of the two edges from each other, and another to measure the breadth of the fringes on the chart. The observations made with this instrument gave me undoubted assurance that the equation to the curve M N in fig. 11 is $yx=a$, a conic hyperbola, and that the disposing force is inversely as the distance at which the flexion of the rays bent and disposed takes place.

Scholium.—It is clear that the extraordinary property we have now been examining, has no connexion with the different breadths of the pencils at different distances from the point of the first flexion, owing to the divergence caused by that flexion.

By the same kind of analysis, which we shall use in demonstrating the 6th Proposition, it may be shown,—*first*, that the divergence of the rays alone would give a different result, the fringes made by an inflexion following a deflexion and those made by a deflexion following an inflexion; *secondly*, that in no case would the equation to the disposing force be the conic hyperbola, even where that fringe decreased with the increase of the distance; *thirdly*, even where the effect of increasing the distance is such as the dispersion would lead to expect, the rate of decrease of the fringes is very much greater in fact than that calculation would lead to, five or six times as great in many cases; and *lastly*, that instead of the law of decrease being uniform, it would, if caused by the dispersion, vary at different distances from the two edges*. Nothing therefore can be more manifest than that the phenomena in question depend upon a peculiar property of the rays, which makes them change in their disposition with the length of the space through which they have travelled.

It should seem that light may be compared, when bent and thereby disposed, to a body in its nascent state, which, as we find by constant experience, has properties different from those which it has afterwards; and I have therefore contrived some experiments for the purpose of ascertaining whether or not light at the moment of its production (by artificial means) has properties other than those which it possesses after it has been some time produced. This will form the subject of a future inquiry. I would suggest, however, at present that the electric fluid ought to be examined with a view to find whether or not it has any property analogous to disposition, that is, whether it becomes more difficultly attracted at some distance from its evolution, as light is more difficultly bent at a distance from the point of its being disposed. On heat a like experiment may be made. The thermometer would no doubt stand at a different height at different distances from the source of the heat; but the question

* I have given demonstrations of these propositions in a memoir presented to the National Institute, but I am reluctant to load the present paper with them.

is if it will not reach its full height, whatever that may be, more quickly near its source than far from it. This experiment ought above all to be made on radiant heat, in which I confidently expect a property will be found similar to the disposition of light. It is also plain that we may expect strong analogies in magnetism and electro-magnetism.—I throw out these things because my time for such investigations may not be sufficiently extended to let me undertake them with success.

PROPOSITION VI.

The figures made by the inflexion of the second body acting upon the rays deflected by the first, must, according to the calculus applied to the case, be broader than those made by the second body deflecting those rays inflected by the first.

In fig. 14, let $A v'$ be the violet rays and $A r'$ the red, inflected by A and deflected by B. Let $A r$ be the red and $A v$ the violet deflected by A and inflected by B. The action of B must inflect $A r$, $A v$ into a broader fringe F , than the action of B deflects $A v'$, $A r'$ into the fringe f .

Let $B r = a$ be the distance at which B acts on $A r$; $r v = d$ be the divergence of the red and violet; c be the distance of the two bent pencils, and $v' r'$ the divergence of the inflected pencil, equal also to d , because we may take the different inflexibility to be as the different deflexibility. B acts on the red of $A r v$ as $\frac{r}{a^m}$; on the violet as $\frac{v}{(a+d)^m}$; and so on $A v'$ as $\frac{v}{(a+d+c)^m}$; on $A r'$ as $\frac{r}{(a+2d+c)^m}$. It is evident that the action in bending $A r$, $A v$, or the fringe made by that action, is to the fringe made by the action on $A r'$, $A v'$, as $\frac{r}{a^m} - \frac{v}{(a+d)^m} : \frac{r}{(a+2d+c)^m} - \frac{v}{(a+d+c)^m}$; and ultimately the two actions (or sets of fringes) are (supposing $a=1$ and d also $=1$, for simplifying the expression) as

$$2^m \times r (3+c)^m (2+c)^m - v (3+c)^m (2+c)^m \text{ to } 2^m r (2+c)^m - 2^m v (3+c)^m.$$

Now the former of these expressions must always be greater than the latter, because $(3+c)^m > 1$, and also $(3+c)^m - 1 > (2+c)^m - 1$; and this whatever be the value of m and of c , and whatever proportion we allow of r to v , the flexibilities. But it is also manifest that the excess of the first expression above the second will be greater if the flexibility of the red exceed that of the violet, or if r is greater than v , as $2v$. Hence we conclude; *first*, that in mixed or white light the fringes inflected by B after deflexion by A are greater than those deflected by B after inflexion by A; *secondly*, that they are also greater in homogeneous light; *thirdly*, that the excess of the inflected fringes over the deflected is greater in mixed than in homogeneous light.

The action of flexion after disposition is so much greater than that of simple flexion, that I have only taken into the calculation the compound flexion. But the most accurate analysis is that which makes the two fringes as

$$D + \frac{r}{a^m} - \frac{v}{(a+d)^m} \text{ to } D + \frac{r}{(a+2d+c)^m} - \frac{v}{(a+d+c)^m},$$

D being the breadth of the fringes on the chart by simple flexion in case the rays had passed on without disposition and without a second flexion. If it be carefully kept in mind that D is much less than $\frac{r}{a^m}$, or even $\frac{r}{(a+2d+c)^m}$, and that d is still less than D, then it will always be certain that the first quantity is larger than the second.

Cor.—It is a corollary to this proposition that the difference of the two sets of fringes is increased by the disposition communicated by the rays in passing by the first body. For the excess of the value of r over that of v being increased, the difference between the two expressions is increased.

PROPOSITION VII.

When one body only acts upon the rays, it must, by deflexion, form them into fringes or images decreasing as the distance from the bending body increases. But when the rays deflected and disposed by one body are afterwards inflected by a second body, the fringes will increase as they recede from the direct rays. Also when the fringes made by the inflexion of one body, and which increase with the distance from the direct rays, are deflected by a second body, the effect of the disposition and of the distances is such as to correct the effect of the first flexion, and the fringes by deflexion of the second body are made to decrease as they recede from the direct rays.

In fig. 15, A P is the pencil inflected by A and forming the first and narrower fringe p ; A r is the pencil inflected nearer to A and forming the broader fringe r . Such are the relative breadths, because they are inversely as some power of the distance at which A acts on them. But if B afterwards acts, it is shown by the same reasoning which was applied to the last proposition that r will be less than p ; and so in like manner will r' be made less than o' , though o' was greater than r' , until B's action, and the effects of disposition with the greater proximity of the smaller fringe, altered the proportions.

PROPOSITION VIII.

It is proved by experiment that the inflexion of the second body makes broader fringes or images than its deflexion after the inflexion of the first body; and also that the inflecto-deflexion fringes decrease, and the deflecto-inflexion fringes increase with the distance from the direct rays.

Exp. 1. It must be observed that when we examine the fringes (or images) made by the second edge deflecting the rays which the first had inflected, we can see the effects of the disposition communicated to the rays at a much greater distance of the second edge from the first, than we can perceive the effects of that disposition upon the inflexion by the second edge of the rays deflected by the first. Indeed we only lose the fringes thus made by deflexion, in consequence of their becoming so minute as to be imperceptible to our senses. But it is otherwise with the fringes or images

made by the second edge inflecting the rays which the first had deflected. These can only be seen when the second edge is near the first, because the rays cannot pass on so as to form the images on the chart, if the second is distant from the first. The pencils diverge both by the deflexion and by the inflexion of the first edge. But we can always, when the inflected rays pass too far from the second edge, bring this so near them as to act on them, whereas we in so doing intercept the deflected rays. However, after this is explained, we find no difficulty in examining the effects of the inflexion by the second edge, only we must place it near the first, and thus we have two sets of fringes, one extending into the shadow of the first edge at an inch distance between the two edges; but at three-fourths of an inch, nay, at two inches, or even more, this experiment can well be made.

Exp. 2. At these distances I examined repeatedly the comparative breadths of the two sets. In fig. 16, ab is the white disc, on each side of which are fringes; those on the one side, bc , cd , are by the inflexion of the second edge; those on the opposite side, af , fe , are by the deflexion of that second edge. I repeatedly measured these sets of fringes, and at various distances from the second edge; and I always found them much broader on the side of the second edge than on the opposite side. Thus ab being the breadth of 5, bc was 3, and cd $4\frac{1}{2}$, while, on the opposite side, af was $=1$ and fe only $\frac{3}{4}$ or $\frac{1}{2}$. The fringes by inflexion of the second edge also uniformly increased as they receded from ab , the direct rays, whereas the opposite fringes as constantly decreased.

Exp. 3. If however the distance between the two edges be reduced, it is observed that the disparity between the two sets of fringes decreases, and they become gradually nearly equal; and when the edges are quite opposite each other there is no difference observable in the two sets. Each ray is disposed and polarized alike and affected alike by the two edges, and no difference can be perceived between the two sets.

Exp. 4. The experiments also agree entirely with the calculus in respect of the relative values of r and v affecting the result. It appears that the fringes by the second edge's inflexion are broader than those by that edge's deflexion, whether we use white or homogeneous light. In the latter, however, the difference is not so considerable. This I have repeatedly tried and made others try, whose sight was better than my own. I may take the liberty of mentioning my friend Lord Douro, who has, I believe, hereditarily, great acuteness of vision.

PROPOSITION IX.

The joint action of two bodies situated similarly with respect to the rays which pass between them so near as to be affected by both bodies, must, whatever be the law of their action, provided it be inversely as some power of the distance, produce fringes or images which increase with the distance from the direct rays.

Let (fig. 17) A and B be the two bodies, and $AC=CB=a$ be their spheres of
MDCCCL.

flexion, so that A inflects and B deflects through A C, and A deflects and B inflects through C B. Let $CP=x$, $PM=y$. The force y , exerted by the joint action of A and B on any ray passing between them at P, is equal to $\frac{1}{(a+x)^m} + \frac{1}{(a-x)^n}$, supposing deflexion and inflexion to follow different laws. To find the minimum value of y , take its differential $dy=0$; therefore we have

$$-m(a+x)^{-m-1}dx + n(a-x)^{-n-1}dx = 0, \text{ or } m(a-x)^{n+1} = n(a+x)^{m+1}.$$

If $m=n$ (as there is every reason for supposing), then $a-x=a+x$, or $x=0$; and therefore, whatever be the value of m (that is whatever be the law of the force), the minimum value of y is at the point C where A's deflexion begins. The curve SS', which is the locus of M, comes nearest the axis at C, and recedes from that axis constantly between C and B. Hence it is plain that the fringes must increase (they being in proportion to the united action of A and B) from C to B; and in like manner must those made by B's deflexion and A's inflexion increase constantly from C to A; and this is true whatever be the law of the bending force, provided it is in some inverse ratio to the distance.

PROPOSITION X.

It is proved by experiment that the fringes or images increase as the distance increases from the direct rays.

Exp. 1. Repeated observations and measurements satisfy us of this fact. We may either receive the images on a chart at various distances from the double edge instrument, approaching the edges until the fringes appear, or we may receive them on a plate of ground glass held between the sun and the eye. We may thus measure them with a micrometer; but no such nicety is required, because their increase in breadth is manifest. The only doubt is with respect to their relative breadth when the edges are not very near and just when they begin to form fringes. Sometimes it should seem that these very narrow fringes decrease instead of increasing. However, it is not probable that this should be found true, at least when care is taken to place the two edges exactly opposite each other; because if it were true that at this greater distance of A from B (fig. 17) they decreased, then there must be a minimum value of PM between C and B, and between C and A; and consequently the law of flexion must vary in the different distances of A and B from the rays P, a supposition at variance it should seem with the law of continuity.

Exp. 2. The truth of this proposition is rendered more apparent by exposing the two edges to the rays forming the prismatic spectrum. The increase is thus rendered manifest. If the fringes are received on a ground glass plate, you can perceive twelve or thirteen on each side of the image by the direct rays. It is also worth while to make similar observations on artificial lights, and on the moon's light. The proposition receives additional support from these. But care must always be taken in such observations, which require the eye to be placed near the edges, that we are

not misled by the effect of the small aperture in reversing the action of the edges. Thus when viewing the moon or a candle through the interval of two edges, one being in advance of the other, we have the coloured images (or fringes) cast on the wrong side. But if we are only making the experiment required to illustrate this proposition, the edges being to be kept directly opposite, no confusion can arise.

It is to be noted that the increase of breadth in the fringes is not very rapid in any of these experiments; nor are we led by the calculus to expect it. Thus suppose $m=1$, we find (because $y = \frac{2a}{a^2 - x^2}$) at the point C, when $x=0$, the breadth should be proportional to $\frac{2}{a}$. Take $x = \frac{a}{10}$, and the breadth is as $\frac{200}{99}$, or the breadth of the one fringe is to the other only as 200 to 198 or 100:99. We need not wonder therefore if there is only a gradual increase of breadth from C to B and from C to A. The increase is more rapid between $x = \frac{a}{2}$ and B than between C and $\frac{a}{2}$. Thus between the value of $x = \frac{a}{4}$ and $\frac{a}{2}$ the increase is as 4:5. But from $\frac{a}{2}$ to $\frac{3a}{4}$ the increase is as 7:12; and this too agrees exactly with the experiments; for as the edges are approached the increase of the fringes becomes more apparent.

PROPOSITION XI.

The phenomena described in the foregoing propositions are wholly unconnected with interference, and incapable of being referred to it.

1. When the fringes in the shadow are formed by what is supposed to be interference, there are also formed other fringes outside the shadow and in the white light. If the rays passing on one side the bending body (as a pin or needle) are stopped, the internal fringes on the opposite side of the shadow are no longer seen. But no effect whatever is produced on the external fringes. These continue as long as the rays passing on the same side of the body on which they are formed, continue to pass. The external fringes have many other properties which wholly distinguish them from the internal or interference fringes.

2. Interference is said to be in proportion to the different lengths of the interfering rays, and not to operate unless those lengths are somewhat near an equality. In my experiments the second body may be placed a foot and a half away from the first, and the fringes by disposition are still found, though much narrower than when the bending bodies are more near to one another.

3. The breadth of the interference fringes is said to be in some inverse proportion to the difference in length of the interfering rays. It is commonly said to be inversely as that difference.

In fig. 20, A is the first and B the second edge. By interference the fringe at C should be broadest and at D narrowest, because $AC - BC = AO$ is less than $AD - BD = AP$; and so as you recede from D, the fringes should become broader and broader, because the two rays become more nearly equal. But the very reverse is

notoriously the case, the breadth of the fringes decreasing with their distance from the direct rays.

4. In the case of the fringes formed by the second body inflecting and the first deflecting, there can be no interference at all; for the whole action is on one and the same pencil or beam. A deflects and then B inflects the same ray; and when a third edge is placed on the opposite side to B, it only deflects the same ray, which is thus twice bent further from the direct rays, the last bending increasing that distance.

5. Let A be the first and B the second edge as before (fig. 20). Suppose B to be moveable, and find the equation to the disposing force at different distances of the two edges, we shall find this to be $y = \frac{1}{\sqrt{a^2 + b^2} - \sqrt{(a-x)^2 + b^2}}$, a being = A E, b = E D, and AB = x . But all the experiments show it to be $y = \frac{a}{x}$, a wholly different curve.

Again, let B be fixed, or the distance of the two edges be constant, we shall get the equation (a being = A E, b = B E, b = D E and E C = x) $y = \frac{1}{\sqrt{a^2 + (x-b)^2} - \sqrt{c^2 + x^2}}$, also a wholly different curve from the conic hyperbola, which all experiments give. Therefore the conclusion from the whole is that the phenomena have no reference to interference.

Having delivered the doctrines resulting from these experiments, I have some few particulars to add, both as illustrating and confirming the foregoing propositions, as removing one or two difficulties which have occurred to others until they were met by facts, and also as showing the tendency of the results at which we have arrived.

1. It may have been observed that in all these propositions I have taken for granted the inflexion of the rays by the body first acting upon them as well as their deflexion by that body, and have reasoned on that supposition. It is, however, not to be denied that we cannot easily perceive the fringes made by the single inflexion, as we can without any difficulty perceive those made by the single deflexion, and fully described in Proposition I. Sir I. NEWTON even assumes that no fringes are made within the shadow. I here purposely keep out of view the fringes made in the shadow of a hair or other small body, because the principle of interference there comes into play. However, I will now state the grounds of my assuming inflexion and separation of the rays by their different flexibility, when only a single body acts on them. In the *first* place, the first body does act in some way; for the second only acts after the first, and if the first be removed the fringes made in its shadow by the second at once vanish. *Secondly*, these fringes made by the second depend upon its proximity to the first. *Thirdly*, the following experiment seems decisive. Place instead of a straight edge one of the form in fig. 18, and then apply at some distance from it, the second edge, as in the former experiments. You find that the fringes assume the form, somewhat like a small tooth-comb, of $a b$. If the second edge is

furnished with a similar curve surface the form is more complete, as in *cd*. But the straight edge being used after the first flexion of the curved one, clearly shows that the first edge bends as well as the second, indeed more than the second, for the side of the figure answering to that curved edge is most curved. *Fourthly*, the whole experiments with two edges directly opposite each other negative the idea of there being no inflexion; indeed they seem to prove the inflexion equal to the deflexion. The phenomena under Proposition X. can in no way be reconciled to the supposition of the first edge not inflecting the rays*.

2. We must ever keep in view the difference between the fringes or images described by Sir I. NEWTON and measured by him, as made by the rays passing on each side of a hair, and the fringes or images which are made without the interference of rays passing on both sides. It is clear that the rays which form those fringes with their dark intervals do not proceed after passing the hair in straight lines. Sir I. NEWTON'S measures† prove this; for at half a foot from the hair he found the first fringe $\frac{1}{10}$ th of an inch broad, and the second fringe $\frac{1}{9}$ th; and at nine feet distance the former were $\frac{1}{3}$, the latter $\frac{1}{3}$, instead of being $\frac{1}{3}$ and $\frac{1}{10}$, and the latter less than $\frac{1}{10}$, and so of all the other measures in the table, each being invariably about one-third what it ought to be if the rays moved in straight lines; and this also explains why the fringes do not run into one another, or encroach on the dark intervals in the case of the hair, as they must do if the rays moved in straight lines.

But the case of the fringes or images which we have been examining and reasoning upon is wholly different. I have measured the breadths of those formed by disposition and polarization, and found that they are broad in proportion to the distance from the bending edge of the chart on which they are received; and vary from the results given by similar triangles in so trifling a degree, that it can arise only from error in measurement. Thus in an average of five trials, at the relative distances of 41 and 73 inches, the disc was $6\frac{2}{3}$ at the shorter, and $10\frac{1}{3}$ at the longer distance; the fringe next it $3\frac{7}{10}$ at the shorter, and $5\frac{7}{10}$ at the longer distance, whereas the proportions by similar triangles would have been $9\frac{1}{3}$ and $5\frac{1}{3}$, so that the difference is small, and is by excess, and not, as in the hair experiment, by defect. Had the difference been as in Sir I. NEWTON'S experiment, instead of $10\frac{1}{3}$ and $5\frac{7}{10}$, it would have been $3\frac{1}{4}$ and $1\frac{7}{4}$. In another measurement at 101 and 158 inches respectively, the disc was $15\frac{1}{3}$, the fringe $8\frac{2}{3}$ instead of $14\frac{2}{3}$ and $9\frac{1}{3}$ respectively. But by Sir I. NEWTON'S proportions these should have been $4\frac{2}{3}$ and $3\frac{1}{4}$. It is plain that if the measures had been taken with the micrometer instruments, which had not been then furnished, there would have been no deviation. I have since tried the experiment, not as above, on the fringes formed by the double-edged instrument, but on those formed by one edge at a distance behind the other, and have found no reason to doubt that the rays follow a rectilinear course.

* If you hold a body between the eye and a light, as that of a candle, and approach it to the rays, you see the flame drawn towards the body; and a beginning of images or fringes is perceived on that side.

† Optics, B. iii. obs. 3.

It may further be observed, that in the fringes or images by disposition and polarization, the dark intervals disappear at short distances from the point of flexion, and that the fringes run into one another, so that we find the red mixed with the blue and violet. This is one reason why I often experimented with the prismatic rays.

3. It follows from the property of light, which I have termed disposition, on one side the ray, and polarization on the opposite side, superinduced by flexion, that those two sides only being affected, the other two at right angles to these are not at all affected by the flexion which has disposed and polarized the two former. Consequently, although an edge placed parallel to the disposing edge and opposite to it acts powerfully on the disposed light, yet an edge placed at right angles to the former edge or across the rays, does not affect them any more than it would rays which had not been subjected to the previous action of a first edge. Thus (fig. 19) if $abcd$ be the section of the ray, an edge parallel to ab , after the ray has been disposed, will affect the ray greatly, provided it had been disposed by an edge also parallel to ab . The sides ab and cd , however, are alone affected; and therefore the second edge, if placed parallel to ad or bc , will not at all bend the ray more or make images (or fringes) more powerfully than it would do if no previous flexion and disposition had taken place. Let us see how this is in fact: $efgh$ is the distended disc after flexion, by passing through the aperture of the two-edged instrument (Plate XI.). It is slightly tinged with red at the two ends fg and eh , beyond which, and in the shadow of the edges, are the usual fringes or coloured images by flexion and disposition, e, c , the edges being parallel to eh, fg . Place another edge at some distance from the two, as 3 or 4 inches, and parallel to these two, but in the light, and you will see in the disc a succession of narrow fringes parallel to the edges, and in front of the third edge's shadow. These fringes are on the white disc, and their colours are very bright, much more so than the colours of those fringes described in Proposition I., and which are fringes made by deflexion without any disposition. But whether this superior brightness is owing to the glare of the disc's light being diminished by the flexion of the first two edges, or not, for the present I stop not to inquire. This is certain, that if the third edge be placed across the beam, and at right angles to the two first edges, you no longer have the small fringes. They are not formed in the direction hg , parallel to the edges as now placed. If the double edges are changed, and are placed in the direction $h'g'$, you again have the bright fringes; but then, if the third edge is now placed parallel to $h'e'$, you cease to have them. Care must, however, be taken in this experiment not to mistake for these bright fringes the ordinary deflexion fringes made by one flexion without disposition, as described in Proposition I. For these may be perceived, and even somewhat more distinctly in the disc than in the full light of the white pencil or beam.

Now are these bright fringes only the flexion fringes, that is fringes by simple flexion without disposition? To ascertain this I made these experiments.

Exp. 1. If they are the common fringes, and only enlarged by the greater divergence of the rays after flexion, and more bright by the dimness of the distended disc,

then it will follow that the greater the distension, and the greater the divergence of the rays, the broader will be the bright fringes in question. I repeatedly have tried the thing by this test, and I uniformly find that increasing the divergence, by approaching the edges of the instrument, has no effect whatever in increasing the breadth of the fringes in question.

Exp. 2. If these fringes are not connected with disposition, it will follow that the distance of the edge which forms them from the double-edged instrument cannot affect them. But I have distinctly ascertained that their breadth does depend on that distance, and in order to remove all doubt as to the distance between the chart and the third edge which forms them, I allowed that edge to remain fixed, and varied its distance from the other two by bringing the double-edge instrument nearer the third edge. The breadths of the bright fringes varied most remarkably, being in some inverse power of that distance. Thus, to take one measurement as an example of the rest, at 4 feet from the third edge the chart was fixed and the third edge kept constantly at that distance from it. Then the double-edge instrument was placed successively at $14\frac{1}{2}$, at 9 and at $4\frac{1}{2}$ eighths of an inch from the third edge. The breadths were respectively 2, $3\frac{3}{4}$ and $4\frac{1}{4}$ twentieths of an inch. In some experiments these measures approached more nearly the hyperbolic values of y , but I give the experiment now only for the important and indeed decisive evidence which it affords, that these fringes are caused by disposition, and are wholly different from those formed without previous flexion.

Exp. 3. If the greater breadth of these fringes is owing to dispersion, then they should be formed more in the rays of the prismatic spectrum than in white light, or even in light bent by flexion. Yet we find it more difficult to trace fringes across the prismatic spectrum than in white light, and more difficult across the spectrum when there is divergence, than when formed parallel to its sides when there is no divergence. There are fringes formed, but of the narrow kind, which are described in Prop. I.

Exp. 4. I have tried the effect on the fringes in question of the curvilinear edge described in the first article of these observations, and the effect of which is represented in fig. 18. It is certain that at a distance from the double-edge instrument the third edge seems only to form fringes rectilinear, or of its own form. But when placed very near, as half an inch from the instrument, plainly there is a curvilinear form given to the fringes in question; and this is most easily perceived, when, by moving the third edge towards the side of the pencil, you form the smaller fringes so as to be drawn across or along the greater ones made by the two first edges.

I think, without pursuing this subject further, it must be admitted that these fringes in light, which is bent and disposed, lend an important confirmation to the doctrine of disposition. It is clear that the rays are affected only on two of their four sides, or ab and cd , if these are parallel to the bending body's edge, and not at all on the sides cb and da ; that, on the other hand, cb and da are affected when the

edges are placed parallel to these two sides of the rays ; and thus the connection of the fringes in question, with the preceding action of which disposed and polarized, is clearly proved.

4. It is an obvious extension and variation of this experiment both to apply edges parallel to the first and disposing edges, and also to apply edges at right angles to their direction; and important results follow from this experiment. But until a more minute examination of the phenomena with accurate admeasurements can be had, I prefer not entering on this subject further than to say, that the extreme difficulty of obtaining fringes or images at once from the edges parallel to the first two, and from edges at right angles to these, indicates an action not always at right angles to the bending body, but whether conical or not I have not hitherto been able to ascertain. That the first body only disposes and polarizes in one direction is certain. But it seems difficult to explain the effect of the first two edges in preventing the fringes or images from being made by the second at right angles to those formed by the first two edges, if no lateral action exists. One can suppose the approaching of those two first edges to make the fringes narrower and narrower than those which the second two edges form when placed at right angles to the first. But this is by no means all that happens. There is hardly any set of fringes at all formed at right angles to the first set (parallel to the first two edges) when the first two are approached so near each other as greatly to distend the disc.

5. I reserve for future inquiry also the opinion held by Sir I. NEWTON, that the different homogeneous rays are acted upon by bodies at different distances, this action extending furthest over the least refrangible rays. He inferred this from the greater breadth of the fringes in those rays.

It is in my apprehension, though I once held a different opinion*, not impossible to account for the difference of the breadth of the fringes by the different flexibility of the rays ; and the reasoning in one of the foregoing propositions shows how this inquiry may be conducted. But one thing is certain, and probably Sir I. NEWTON had made the experiment and grounded his opinion upon the result. If you place a screen, with a narrow slit in the prismatic spectrum's rays, parallel to the rectilinear sides, and then place a second prism at right angles to the first and between the screen and the chart, you will see the image of the slit drawn on one side, the violet being furthest drawn, the red least drawn ; but you will find no difference in the breadth of the image cast by the slit. Flexion, however, operates in a different manner, because it acts on rays, which, though of the same flexibility, are at different distances from the body.

6. The internal fringes in the shadow (said by interference) deserve to be examined much more minutely than they ever have been ; and I have made many experiments on these, by which an action of the rays on one another is, I think, sufficiently proved. I shall here content myself with only stating such results as bear on

* Philosophical Transactions, 1797.

the question of interference affecting my own other experiments. *First.* I observe that when one side of a needle or pin is grooved so as to be partly curvilinear, the other side remaining straight, we have internal fringes of the form in fig. 21. *Secondly.* It is not at all necessary the pin or other body forming them should be of very small diameter, although it is certain that the breadth of the fringes is inversely as the diameter. I have obtained them easily from a body one-quarter or one-third of an inch in diameter, but they must be received at a considerable distance from the body. *Thirdly,* and this is very material as to interference at all affecting my experiments, although certainly the internal fringes vanish when the rays are stopped coming from the opposite side of the object, the external fringes are not in the smallest degree affected, unless you stop the light coming on their own side; stopping the opposite rays has no effect whatever. Thus, stopping the light on the side *a* (fig. 19), the fringes *ff* vanish, but not the external fringes *c*. This at once proves there is no interference in forming the external ones. *Lastly.* I may observe, that the law of disposition and polarization in some sort, though with modification, affects the internal fringes as well as the external.

It is a curious fact connected with polarization by inflexion, and which indeed is only to be accounted for by that affection of light, that nothing else prevents the rays from circulating round bodies exposed to them, at least bodies of moderate diameter. If the successive particles of the surface inflected, one particle acting after the other, the rays must necessarily come round to the very point of the first flexion. We should thus see a candle placed at *A* (fig. 22) when the eye was placed at *B*, because the rays would be inflected all round; and even in parts of the earth where the sea is smooth, nothing but the small curvature of the surface could prevent us from seeing the sun many hours after light had begun by placing the eye close to the ground. This, however, in bodies of a small diameter, must inevitably happen. The polarization of the rays alone prevents it, by making it impossible they should be more than once inflected on their side which was next the bending body, therefore they go on straight to *C*. But for polarity they must move round the body.

7. It must not be lightly supposed, that because such inquiries as we have been engaged in are on phenomena of a minute description and relate to very small distances, therefore they are unimportant. Their results lead to the constitution of light, and its motion, and its action, and the relations between light and all bodies. I purposely abstain from pursuing the principles which I have ventured to explain into their consequences, and reserve for another occasion some more general inquiries founded upon what goes before. This course is dictated by the manifest expediency of first expounding the fundamental principles, and I therefore begin by respectfully submitting these to the consideration of the learned in such matters.

In the meantime, however, I will mention one inference to be drawn from the foregoing propositions of some interest.

As it is clear that the disposition varies with the distance, and is inversely as that

distance, and as this forms an inherent and essential property of the light itself, what is the result? Plainly this, that the motion of light is quite uniform after flexion, and apparently before also. The flexion produces acceleration but only for an instant. If ss is the space through which the ray moves after entering the sphere of flexion, and v the velocity before it enters that sphere; it moves after entering with a velocity $=\sqrt{v^2+Zdz}$, Z being the law of the bending force. Then this is greater than v ; consequently there is an acceleration, though not very great; but because $y=\frac{a}{x}$, if s is the space, t the time, the force of acceleration is $\frac{s}{tds} \times \frac{tds-sdt}{t^2}$; but $y=\frac{a}{x}$ shows that s is as t , else $y=\frac{a}{x}$ would be impossible; therefore the accelerating force $\frac{s}{ds} \times \frac{tds-sdt}{t^2}=0$, and so it is shown there is no acceleration after the ray leaves the sphere of flexion.

DESCRIPTION OF THE INSTRUMENTS.

PLATE XII.

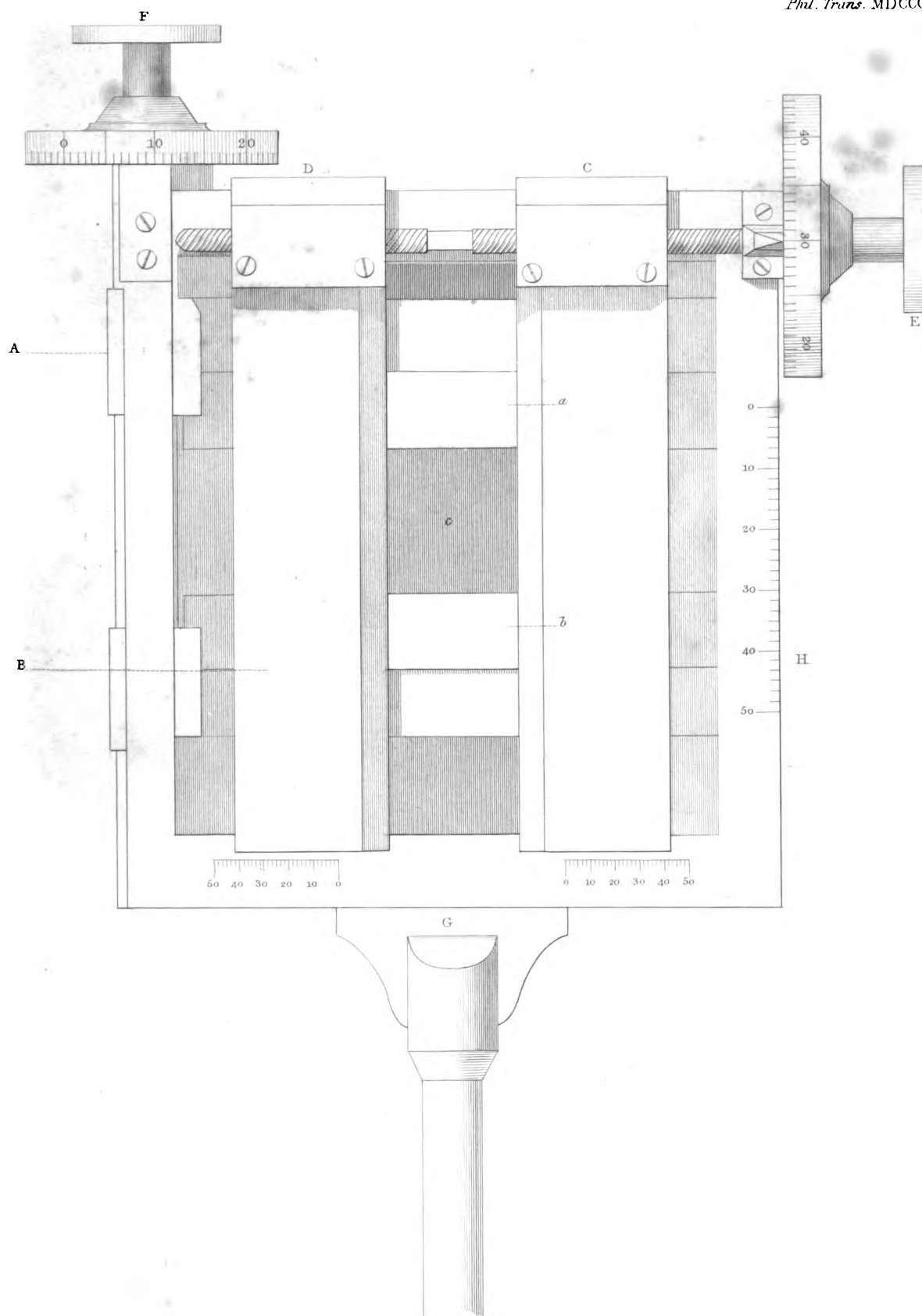
Is the instrument with two plates or edges. A, B, horizontal, D, C vertical; the former moved by the screw E, which has also a micrometer for the distances on the scale G; the latter, in like manner, moved by F, connected with micrometer and scale H.

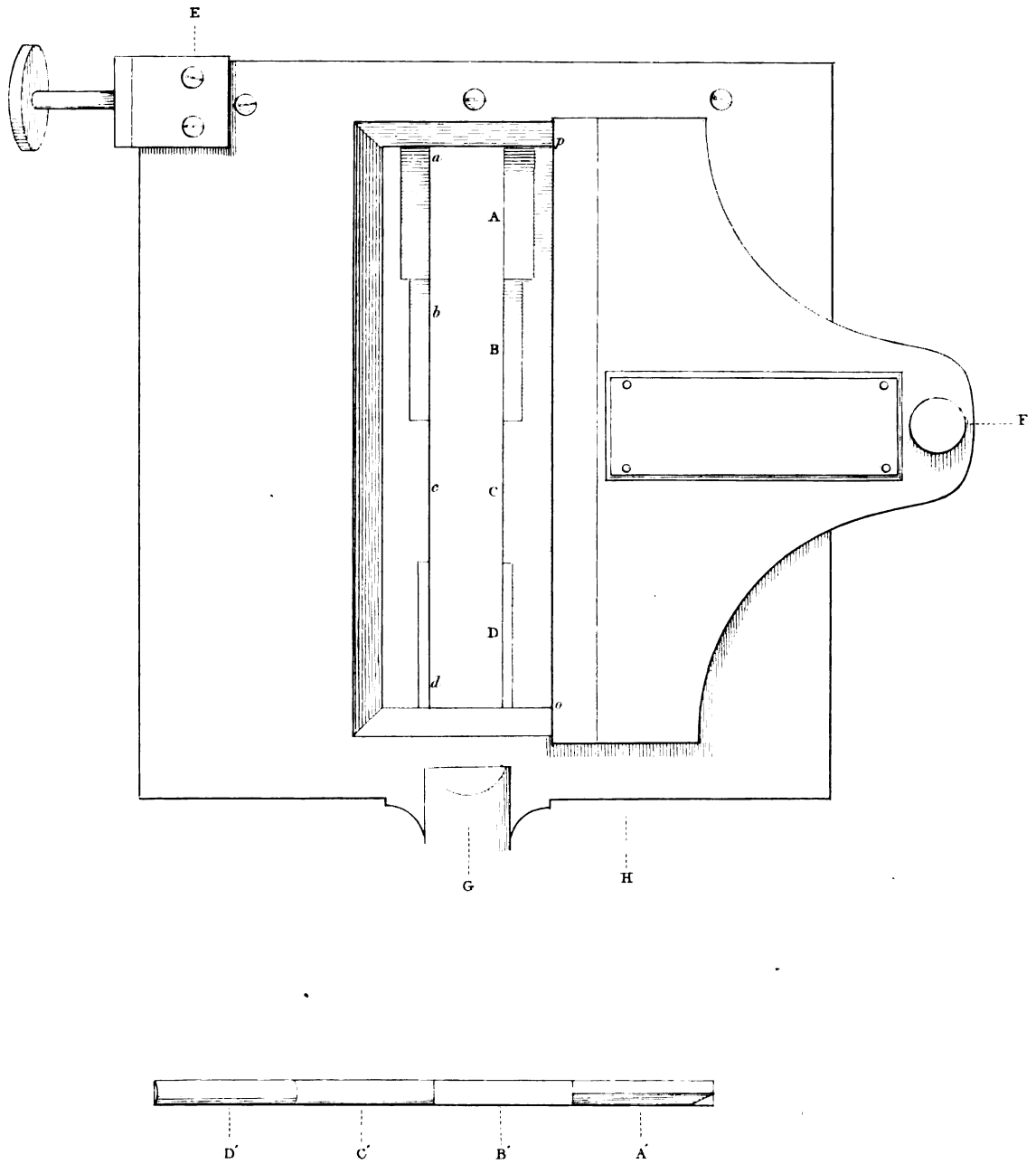
PLATE XIII.

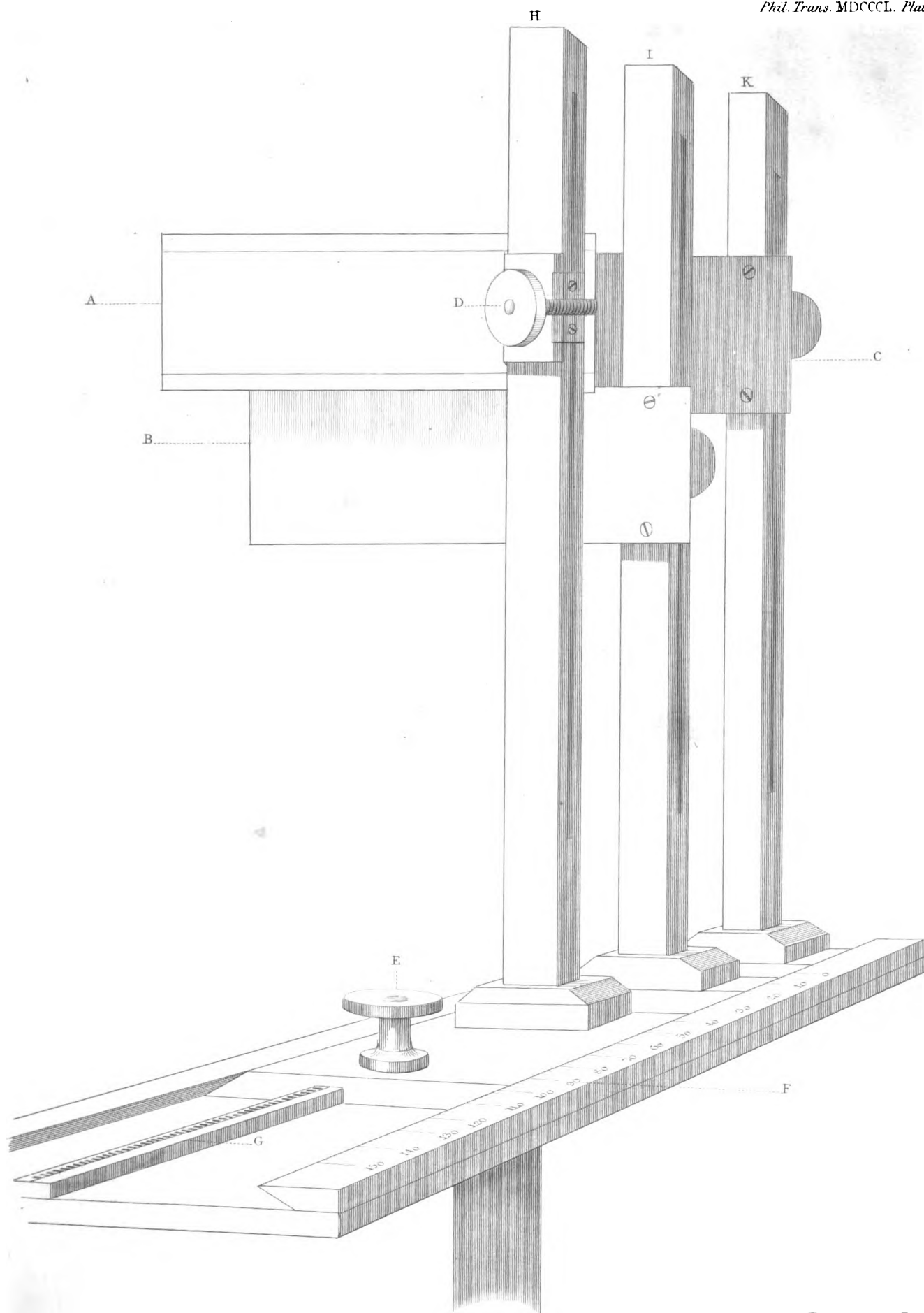
Is the instrument with four surfaces. A D, $a d$ are two parallel plates, moving horizontally by a rack and pinion E. Each plate has an edge composed of four surfaces; A, a , a sharp edge or very narrow surface; B, b , a flat surface; C, c , a cylindrical surface of large radius of curvature, and so flat; D, d , one of small radius, and so very convex: this is represented on the figure by A' B' C' D' beside the other. Care is to be taken that A B C D and $a b c d$ be a perfectly straight line, made up of the sharp edge, the plane surface and the tangents to the two cylinders. H is a plate with a sharp and straight edge, op , which can be brought by its handle F to come opposite to the compound edge $a b c d$, when it is desired to try the flexion by the latter, without another flexion by an opposite compound edge, but only with a flexion by a rectilinear simple edge.

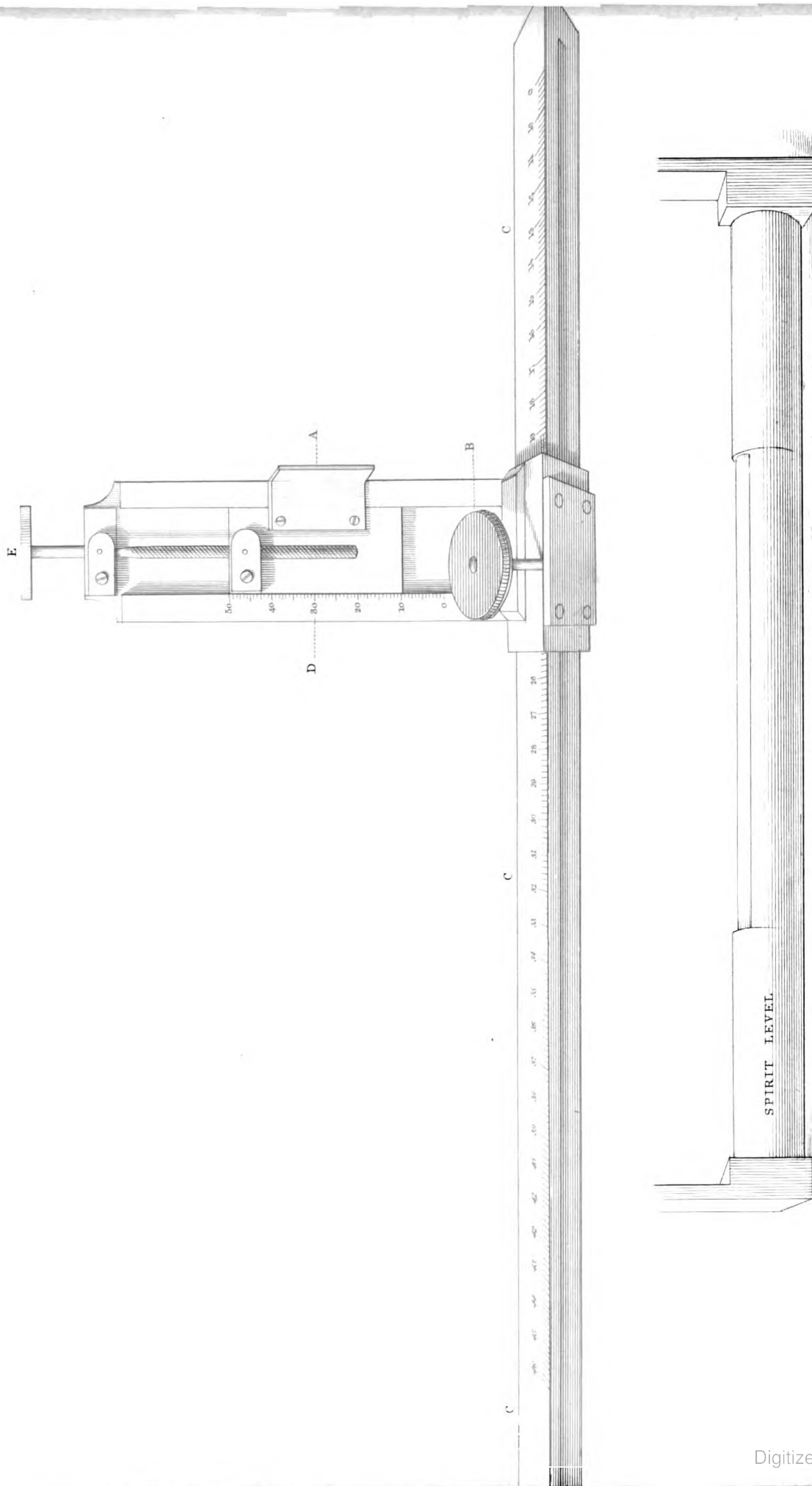
PLATE XIV.

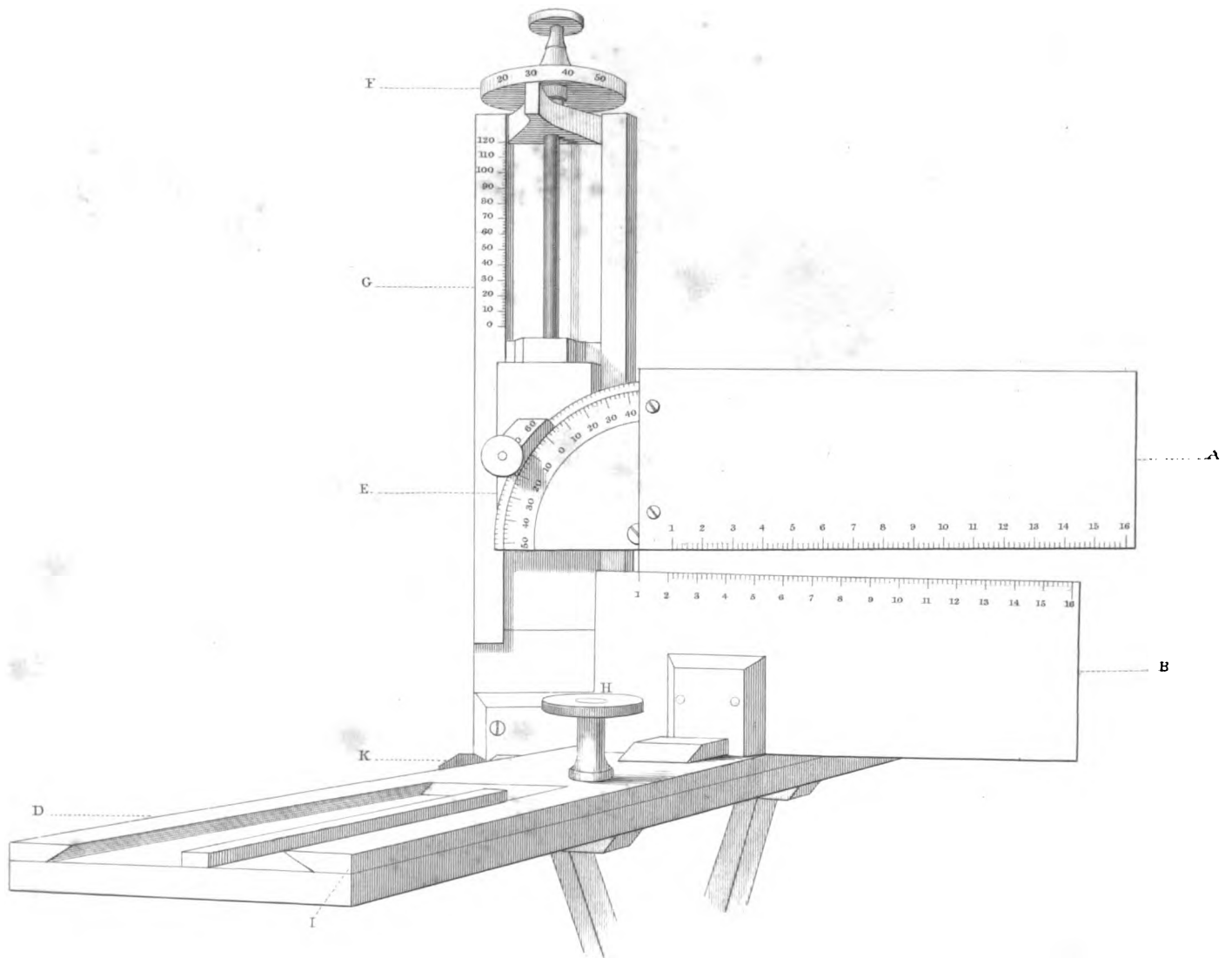
Is the instrument by which is tried the *experimentum crucis* on the action of the third edge, and also the experiments on the distances of the edges as affecting the disposing force. G is the groove in which the uprights H, I, K move. There is a scale











J. Barrow sc.

graduated, F, by which the relative distances can always be determined of the plates A, C and B. A moves up and down upon H, B upon I, and C upon K; each plate is moved up and down by rack and pinion D. The uprights also move along the groove G by rack and pinion E.

PLATE XV.

Is the instrument for ascertaining more nicely the effects of distance on disposition. A is a plate with graduated edge; it moves vertically on a pivot, and its angle with the horizontal line is measured by the quadrant E. A also moves horizontally, and its horizontal angle is measured by the quadrant K. B is another plate with graduated edge, moving in a groove D, by rack and pinion H, and along a graduated beam I. F is a fine micrometer, by which the distance of A above B, when A is horizontal, can always be measured to the greatest nicety by the circle F and the scale G.

PLATE XVI.

Is an instrument also for measuring the effect of the distance of the edges upon the disposing forces. C C C is a graduated beam, adjusted by the spirit-level, and on it moves the upright on which a plate A moves by micrometer screw E, so that the distance of A from the rays that pass along C C C after flexion by a plate fixed at one end of the beam, can be ascertained by the scale D. I have experimented with this, but I did not find it so easy to work by as the other apparatus. C C C is brought to an exact level by screws not noted in the drawing.

XIII. *General Methods in Analysis for the resolution of Linear Equations in Finite Differences and Linear Differential Equations.* By CHARLES JAMES HARGREAVE, Esq., L.L.B., F.R.S., Professor of Jurisprudence in University College, London.

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Preliminary Remarks.

1. THE investigations presented in this paper consist of two parts; the first offers a solution, in a certain qualified sense, of the general linear equation in finite differences; and the second will be found to give an almost complete analysis of the resolution in series of the general linear differential equation with rational factors. The second part is deduced directly from the results of the first, although the subjects of which they respectively treat appear to be wholly independent of each other.

With the exception of a few cases capable of solution by partial and artificial methods, there does not at present exist any mode of solving linear equations in finite differences of an order higher than the first; and with reference to such equations of the first order, we are obliged to be content with those insufficient forms of functions which are intelligible only when the independent variable is an integer, and which may be obtained directly from the equation itself by merely giving to the independent variable its successive integer values. It is in this insufficient and qualified sense that the solutions here given are to be taken; and the first part of the following investigations may be considered as an extension of this form of solution from the general equation of the first order to the general equation of the n th order.

Linear Equations in Finite Differences.

2. A complete analytical theory of the general equation of the n th order,

$$u_x = P_x u_{x-1} + Q_x u_{x-2} + R_x u_{x-3} + S_x u_{x-4} + \dots + W_x u_{x-n+1} + Z_x u_{x-n} + G_x, \dots \quad (1.)$$

would involve its resolution into a series of equations of the first order of the form

$$\begin{aligned} u_x - P_x u_{x-1} &= G'_x, \\ u_x - P''_x u_{x-1} &= G''_x, \\ &\cdot \quad \cdot \quad \cdot \\ &\cdot \quad \cdot \quad \cdot \\ u_x - P^{(n)}_x u_{x-1} &= G^{(n)}_x; \end{aligned}$$

from which the complete solution would result, so far as known methods extend, in the form

$$u_x = P_1 P_2 \dots P_x \Sigma \left(\frac{G_x'}{P_1 P_2 \dots P_{x+1}} \right) + \dots + P_1^{(n)} P_2^{(n)} \dots P_x^{(n)} \Sigma \left(\frac{G_x^{(n)}}{P_1^{(n)} P_2^{(n)} \dots P_{x+1}^{(n)}} \right),$$

or in an expanded form,

$$\begin{aligned} u_x = & G_x' + P_x G_{x-1}' + P_{x-1} P_x G_{x-2}' + P_{x-2} P_{x-1} P_x G_{x-3}' + \dots + c' P_m' P_{m+1}' \dots P_x' \\ & + G_x'' + P_x G_{x-1}'' + P_{x-1} P_x G_{x-2}'' + P_{x-2} P_{x-1} P_x G_{x-3}'' + \dots + c'' P_m'' P_{m+1}'' \dots P_x'' \\ & + \dots \\ & + G_x^{(n)} + P_x^{(n)} G_{x-1}^{(n)} + P_{x-1}^{(n)} P_x^{(n)} G_{x-2}^{(n)} + P_{x-2}^{(n)} P_{x-1}^{(n)} P_x^{(n)} G_{x-3}^{(n)} + \dots + c^{(n)} P_m^{(n)} P_{m+1}^{(n)} \dots P_x^{(n)}. \end{aligned}$$

The last column contains, as is well known, the general solution of the original equation deprived of the term G_x ; and the remaining columns contain the particular solution of the original equation.

3. The investigation the results of which are given in this part, although it actually succeeds in solving the original equation, will be found to contribute little or nothing to the analytical theory as above explained; and this arises from the circumstance, that the particular solution, instead of being produced in the separated form above written, is produced in an aggregated form,

$$G_x + AG_{x-1} + BG_{x-2} + \dots;$$

and the complementary part of the general solution, instead of being produced as above, appears in the form of a *sum* of the complementary functions above written, the constants c' , c'' , &c. being the same in all. But as we shall thus obtain a solution with an arbitrary constant, capable of solving the original equation deprived of G_x , we have it in our power to solve the equation completely by reducing the order of the equation in successive steps.

If we take the equation of the first order,

$$u_x = P_x u_{x-1} + G_x,$$

and arrive at its solution, not properly by any analytical method, but by giving to x the successive values

$$x, x-1, x-2 \dots m+1, m, m-1,$$

and eliminating all values of u_x between the first and the last, we have

$$\begin{aligned} u_x &= P_x u_{x-1} + G_x \\ &= P_x (P_{x-1} u_{x-2} + G_{x-1}) + G_x \text{ or } P_x P_{x-1} u_{x-2} + P_x G_{x-1} + G_x \\ &= P_x P_{x-1} (P_{x-2} u_{x-3} + G_{x-2}) + P_x G_{x-1} + G_x \text{ or } P_x P_{x-1} P_{x-2} u_{x-3} + P_x P_{x-1} G_{x-2} + P_x G_{x-1} + G_x \\ &= \dots \\ &= (P_x P_{x-1} \dots P_m) u_{m-1} + (P_x \dots P_{m+1}) G_m + (P_x \dots P_{m+2}) G_{m+1} + \dots + P_x P_{x-1} G_{x-2} + P_x G_{x-1} + G_x \end{aligned}$$

$$\begin{aligned}
&= (P_x \dots P_m) \left\{ u_{m-1} + \frac{G_m}{P_m} + \frac{G_{m+1}}{P_m P_{m+1}} + \dots + \frac{G_{x-2}}{P_m \dots P_{x-2}} + \frac{G_{x-1}}{P_m \dots P_{x-1}} + \frac{G_x}{P_m \dots P_x} \right\} \\
&= P_x \dots P_m \left(\Sigma \frac{G_{x+1}}{P_m \dots P_{x+1}} + c \right);
\end{aligned}$$

which is the solution ordinarily given, though it is arrived at by a process somewhat less coarse than the above.

By applying the same process of successive elimination to the general linear equation (1.), and carefully observing the law by which the entrance of the factors $P_x, Q_x, R_x, \&c.$ is governed, it will be found that an exactly similar solution may be found in the form

$$u_x = \epsilon^v \left\{ P_x \dots P_m \left(\Sigma \left(\frac{G_{x+1}}{P_m \dots P_{x+1}} \right) + c \right) \right\},$$

or

$$u_x = G_x + P_x G_{x-1} + \epsilon^v (P_x P_{x-1}) \cdot G_{x-2} + \epsilon^v (P_x \dots P_{x-2}) \cdot G_{x-3} + \dots + \epsilon^v (P_x \dots P_{m+1}) \cdot G_m + c \epsilon^v (P_x \dots P_m);$$

where v denotes the sum of a set of distributive operations $V_1, V_2, V_3, \dots V_{n-1}$ not of a strictly algebraical character, which are capable of being performed only upon factorial expressions containing consecutive values of P_x , and which have the following significations. V_1 denotes one operation of this character, signifying that the factor $P_{m-1} P_m$ is changed into Q_m as often as it occurs, any term in which it does not occur disappearing, and the sum of the terms thus obtained being the result of the operation; so that, for example,

$$V_1(P_{x-1} P_x) = Q_x, \quad V_1^2(P_{x-1} P_x) = 0,$$

$$V_1(P_{x-2} P_{x-1} P_x) = Q_{x-1} P_x + P_{x-2} Q_x, \quad V_1^2(P_{x-2} P_{x-1} P_x) = 0,$$

$$V_1(P_{x-3} P_{x-2} P_{x-1} P_x) = Q_{x-2} P_{x-1} P_x + P_{x-3} Q_{x-1} P_x + P_{x-3} P_{x-2} Q_x, \quad V_1^2(P_{x-3} P_{x-2} P_{x-1} P_x) = 2 Q_{x-2} Q_x, \\ \&c. \&c.$$

Again, V_2 denotes another operation of a similar character, signifying that the factor $P_{m-2} P_{m-1} P_m$ is changed into R_m as often as it occurs, the result of the operation being as before the sum of the terms; so that, for example, we have

$$V_2(P_{x-2} P_{x-1} P_x) = R_x,$$

$$V_2(P_{x-3} P_{x-2} P_{x-1} P_x) = R_{x-1} P_x + P_{x-3} R_x,$$

$$V_2(P_{x-4} P_{x-3} P_{x-2} P_{x-1} P_x) = R_{x-2} P_{x-1} P_x + P_{x-4} R_{x-1} P_x + P_{x-4} P_{x-3} R_x,$$

$$V_2(P_{x-5} \dots P_x) = R_{x-3} P_{x-2} P_{x-1} P_x + P_{x-5} R_{x-2} P_{x-1} P_x + P_{x-5} P_{x-4} R_{x-1} P_x + P_{x-5} P_{x-4} P_{x-3} R_x,$$

$$V_2^2(P_{x-5} \dots P_x) = 2 R_{x-3} R_x, \&c. \&c.;$$

$$V_3 \text{ denotes the change in a similar manner of } P_{m-3} P_{m-2} P_{m-1} P_m \text{ into } S_m;$$

.

.

$$V_{n-2} \text{ denotes the change in a similar manner of } P_{m-n+2} \dots P_m \text{ into } W_m;$$

and

$$V_{n-1} \text{ denotes the change in a similar manner of } P_{m-n+1} \dots P_m \text{ into } Z_m.$$

It might at first sight be supposed that this process, if successful at all, would give the complete complementary solution in the form

$$u_x = c_1 \epsilon^r (P_1 \dots P_x) + c_2 \epsilon^r (P_2 \dots P_x) + \dots;$$

but it will be found that in reality this introduces only one arbitrary constant.

It now remains to place these expressions under a properly algebraic form, and to verify the result; and in order to do this we must express the general term $\epsilon^r (P_x \dots P_{p+1})$ in terms of the factors of the original equation; and afterwards give to p , which in the first instance is regarded as a constant, the successive values required for forming the several terms of the solution.

This may be done as follows:—

$$\text{Let } \frac{Q_x}{P_{x-1}P_x} = q_x, \frac{R_x}{P_{x-2}P_{x-1}P_x} = r_x, \frac{S_x}{P_{x-3}P_{x-2}P_{x-1}P_x} = s_x, \dots, \frac{W_x}{P_{x-n+2} \dots P_x} = w_x, \text{ and } \frac{Z_x}{P_{x-n+1} \dots P_x} = z_x.$$

Then it is easily seen that

$$V_1(P_x \dots P_{p+1}) = P_x \dots P_{p+1} \{q_{p+2} + q_{p+3} + \dots + q_x\} = P_x \dots P_{p+1} \Sigma_{p+1}^x q_{x+1},$$

$$V_2(P_x \dots P_{p+1}) = P_x \dots P_{p+1} \{r_{p+3} + r_{p+4} + \dots + r_x\} = P_x \dots P_{p+1} \Sigma_{p+2}^x r_{x+1},$$

and generally

$$v(P_x \dots P_{p+1}) = P_x \dots P_{p+1} \{ \Sigma_{p+1}^x q_{x+1} + \Sigma_{p+2}^x r_{x+1} + \Sigma_{p+3}^x s_{x+1} + \dots + \Sigma_{p+n-2}^x w_{x+1} + \Sigma_{p+n-1}^x z_{x+1} \}.$$

To find $V_1^2(P_x \dots P_{p+1})$, it will be convenient to proceed by steps, beginning with a small number of terms.

Thus

$$V_1(P_x \dots P_{x-3}) = (P_x \dots P_{x-3})(q_x + q_{x-1} + q_{x-2}), \quad \frac{1}{2} V_1^2(P_x \dots P_{x-3}) = P_x \dots P_{x-3}(q_x q_{x-2})$$

$$V_1(P_x \dots P_{x-4}) = (P_x \dots P_{x-4})(q_x + q_{x-1} + q_{x-2} + q_{x-3}), \quad \frac{1}{2} V_1^2(P_x \dots P_{x-4}) = P_x \dots P_{x-4}(q_x(q_{x-2} + q_{x-3}) + q_{x-1}q_{x-3})$$

$$V_1(P_x \dots P_{x-5}) = (P_x \dots P_{x-5})(q_x + q_{x-1} + q_{x-2} + q_{x-3} + q_{x-4}),$$

$$\frac{1}{2} V_1^2(P_x \dots P_{x-5}) = (P_x \dots P_{x-5})(q_x(q_{x-2} + q_{x-3} + q_{x-4}) + q_{x-1}(q_{x-3} + q_{x-4}) + q_{x-2}q_{x-4}) \text{ \&c. ;}$$

and generally, $V_1(P_x \dots P_{p+1})$ being $P_x \dots P_{p+1}(\Sigma_{p+1}^x q_{x+1})$, we have

$$\begin{aligned} \frac{1}{2} V_1^2(P_x \dots P_{p+1}) &= P_x \dots P_{p+1} (q_x \Sigma_{p+1}^{x-2} q_{x+1} + q_{x-1} \Sigma_{p+1}^{x-3} q_{x+1} + \dots + q_{p+4} \Sigma_{p+1}^{p+2} q_{x+1}) \\ &= P_x \dots P_{p+1} \Sigma_{p+3}^x (q_{x+1} \Sigma_{p+1}^{x-1} q_{x+1}). \end{aligned}$$

Similarly, it will be seen that

$$\frac{1}{2.3} V_1^3(P_x \dots P_{p+1}) = P_x \dots P_{p+1} \Sigma_{p+5}^x (q_{x+1} \Sigma_{p+3}^{x-1} (q_{x+1} \Sigma_{p+1}^{x-1} q_{x+1})), \text{ and so on.}$$

In like manner we shall find

$$\frac{1}{2} V_2^2(P_x \dots P_{p+1}) = P_x \dots P_{p+1} \Sigma_{p+3}^x (r_{x+1} \Sigma_{p+2}^{x-2} r_{x+1})$$

$$\frac{1}{2.3} V_2(P_s \dots P_{p+1}) = P_s \dots P_{p+1} \sum_{p+8}^x (r_{s+1} \sum_{p+6}^{x-2} (r_{s+1} \sum_{p+2}^{x-2} r_{s+1})), \text{ and so on,}$$

$$\frac{1}{2} V_m(P_s \dots P_{p+1}) = (P_s \dots P_{p+1}) \sum_{p+2m+1}^x (v_{s+1} \sum_{p+m}^{x-m} v_{s+1}),$$

$$\frac{1}{2.3} V_m(P_s \dots P_{p+1}) = (P_s \dots P_{p+1}) \sum_{p+3m+2}^x (v_{s+1} \sum_{p+2m+1}^{x-m} (v_{s+1} \sum_{p+m}^{x-m} v_{s+1})), \text{ and so on,}$$

v_s being that term of the series $q_s r_s s_s$, &c. which corresponds to the operation V_m , as q_s corresponds to V_1 , r_s to V_2 , &c.

$$V_1 V_2(P_s \dots P_{p+1}) = P_s \dots P_{p+1} \sum_{p+4}^x (r_{s+1} \sum_{p+1}^{x-2} q_{s+1}),$$

$$V_l V_m(P_s \dots P_{p+1}) = P_s \dots P_{p+1} \sum_{p+m+l+1}^x (t_{s+1} \sum_{p+m}^{x-l} v_{s+1}), \text{ } (t_s \text{ corresponding to } V_l),$$

$$V_l^2 V_m(P_s \dots P_{p+1}) = P_s \dots P_{p+1} \sum_{p+m+2l+2}^x (t_{s+1} \sum_{p+m+l+1}^{x-l} (t_{s+1} \sum_{p+m}^{x-l} v_{s+1})), \text{ \&c. \&c.}$$

Finally, if $N'_{s,p} + N''_{s,p} + N'''_{s,p} + \dots$ represent a series in which

$$N'_{s,p} = 1, N''_{s,p} = \sum_{p+1}^x q_{s+1} + \sum_{p+2}^x r_{s+1} + \dots + \sum_{p+s-1}^x z_{s+1},$$

and generally

$$N_{s,p}^{(a+1)} = \sum_{p+2a-1}^x (q_{s+1} N_{s-1,p}^{(a)}) + \sum_{p+3a-1}^x (r_{s+1} N_{s-2,p}^{(a)}) + \sum_{p+4a-1}^x (s_{s+1} N_{s-3,p}^{(a)}) + \dots \\ + \sum_{p+(n-1)a-1}^x (w_{s+1} N_{s-n+2,p}^{(a)}) + \sum_{p+na-1}^x (z_{s+1} N_{s-n+1,p}^{(a)});$$

then, if this series be called $N_{s,p}$, we shall find that $\epsilon^a(P_s \dots P_{p+1}) = (P_s \dots P_{p+1}) N_{s,p}$.

It may be here observed that the number of the terms of the series $N'_{s,p} + N''_{s,p} + \dots$ cannot exceed $\frac{1}{2}(x-p)+1$ when $x-p$ is even, and $\frac{1}{2}(x-p+1)$ when $x-p$ is odd; and since p has the successive values $x-2$, $x-3$, &c., it is always known whether $x-p$ be odd or even. The number of terms may be less; for if Q_s be zero, the number of terms would be the next whole number above $\frac{1}{3}(x-p)$, &c.

That the equation

$$u_s = c(P_s \dots P_{p+1}) N_{s,p}$$

is a complementary solution of the original equation, or in other words, a solution of the original equation wanting the term G_s , may be directly verified; for we have

$$u_s - P_s u_{s-1} = c(P_s \dots P_{p+1}) (N_{s,p} - N_{s-1,p}) \\ = c(P_s \dots P_{p+1}) \Delta N_{s-1,p}.$$

Now $\Delta N'_{s-1,p} = 0$,

$$\Delta N''_{s-1,p} = q_s + r_s + s_s + \dots + z_s,$$

$$\Delta N'''_{s-1,p} = q_s N''_{s-2,p} + r_s N''_{s-3,p} + s_s N''_{s-4,p} + \dots + w_s N''_{s-n+1,p} + z_s N''_{s-n,p},$$

and generally

$$\Delta N_{s-1,p}^{(a+1)} = q_s N_{s-2,p}^{(a)} + r_s N_{s-3,p}^{(a)} + s_s N_{s-4,p}^{(a)} + \dots + w_s N_{s-n+1,p}^{(a)} + z_s N_{s-n,p}^{(a)},$$

whence

$$\Delta N_{s-1,p} = q_s N_{s-2,p} + r_s N_{s-3,p} + s_s N_{s-4,p} + \dots + w_s N_{s-n+1,p} + z_s N_{s-n,p},$$

and

$$u_s - P_s u_{s-1} = Q_s u_{s-2} + R_s u_{s-3} + S_s u_{s-4} + \dots + W_s u_{s-n+1} + Z_s u_{s-n}.$$

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In this part of the solution, I apprehend that it will not generally be necessary to have regard to the lower limits, since p may have any constant value, and that value may be taken which is most convenient in each case; and if we make $p = -\infty$, all the lower limits will have this value.

This part of the solution would then be

$$u_x = cP_x \dots P_1 \{1 + A_x + B_x + C_x + \dots\},$$

where

$$A_x = \Sigma q_{x+1} + \Sigma r_{x+1} + \Sigma s_{x+1} + \dots + \Sigma w_{x+1} + \Sigma z_{x+1};$$

and generally each term of the part within the brackets is formed from the preceding term thus: first change x into $x-1$, multiply by q_{x+1} , and effect a summation; then change (in such preceding term) x into $x-2$, multiply by r_{x+1} , and effect a summation; and so on until lastly we change x into $x-n+1$, multiply by z_{x+1} , and effect a summation; and the sum of the parts thus obtained is the next term.

The verification of the particular solution is easily derived from the above; but it rests on the assumption that the algebraic value above given for $\epsilon^v(P_x \dots P_{p+1})$ is correct; to which therefore particular attention is directed.

The equation

$$u_x = c\epsilon^v(P_x \dots P_{p+1}),$$

considered as a solution of the original equation wanting the term G_x , implies that

$$\epsilon^v(P_x \dots P_{p+1}) = P_x \epsilon^v(P_{x-1} \dots P_{p+1}) + Q_x \epsilon^v(P_{x-2} \dots P_{p+1}) + R_x \epsilon^v(P_{x-3} \dots P_{p+1}) + \dots + Z_x \epsilon^v(P_{x-n} \dots P_{p+1}).$$

Now in order to verify the particular solution,

$$u_x = G_x + P_x G_{x-1} + \epsilon^v(P_x P_{x-1}) G_{x-2} + \epsilon^v(P_x P_{x-1} P_{x-2}) G_{x-3} + \dots + \epsilon^v(P_x \dots P_{x-p+1}) G_{x-p} + \dots,$$

it is only requisite that

$$\epsilon^v(P_x \dots P_{x-p+1}) = P_x \epsilon^v(P_{x-1} \dots P_{x-p+1}) + Q_x \epsilon^v(P_{x-2} \dots P_{x-p+1}) + R_x \epsilon^v(P_{x-3} \dots P_{x-p+1}) + \dots + Z_x \epsilon^v(P_{x-n} \dots P_{x-p+1});$$

and this is true, for it is identical in form with the equation last above given, notwithstanding the occurrence of x in the lowest value of P , for this lowest value remains the same throughout the expression.

7. If $P_x = 0$, the expressions $\epsilon^v(P_x \dots P_{x-m})$ reduce themselves to those terms which do not contain any value of P . It would not be difficult to determine generally what terms these are; but probably the most convenient general method of arriving at the solution in an algebraical form would be to make P_x equal to a constant b , and to make b equal to zero in the result finally obtained.

8. The above particular solution of the original equation is in such a form that the general term of the indefinite series representing the solution is given in explicit terms; but that general term may be represented in an implicit form, which perhaps is more convenient for practical use.

By attending to the formation of the successive expressions $\epsilon^v(P_x \dots P_m)$, it will readily be seen that the series

$$M_0 G_x + M_1 G_{x-1} + M_2 G_{x-2} + \dots + M_p G_{x-p} + \dots$$

is a solution of the original equation, if

$$M_0 = 1,$$

$$M_1 = P_x M_0,$$

$$M_2 = P_{x-1} M_1 + Q_x M_0,$$

$$M_3 = P_{x-2} M_2 + Q_{x-1} M_1 + R_x M_0,$$

$$\begin{array}{cccc} \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot \end{array}$$

$$M_p = P_{x-p+1} M_{p-1} + Q_{x-p+2} M_{p-2} + R_{x-p+3} M_{p-3} + \dots + W_{x-p+n-1} M_{p-n+1} + Z_{x-p+n} M_{p-n};$$

which last is a general relation determining the coefficient of any term from the coefficients of the n preceding terms; or from the coefficients of all the preceding terms when the number of these preceding terms is less than n . This relation is the equation of formation, and may be regarded as universal, bearing in mind that when p is less than n some of the terms of this relation vanish.

The solution of the original equation is therefore reduced to that of a similar equation without second member: and, by what has preceded, this solution is

$$M_p = \varepsilon^v (P_{x-p+1} \dots P_x),$$

it being understood that $M_0 = 1$; and this is the value of M_p before given.

But the value of M_p may be found otherwise, thus:

Make $P_{x-p+1} = P'_p$, $Q_{x-p+2} = Q'_p$, &c. &c.

Then the solution is evidently

$$M_p = \varepsilon^v (P'_p \dots P'_1),$$

where the operation v has the same meaning as before, except that it is applied to the accented letters.

Consequently

$$M_p = (P'_p \dots P'_1) \{1 + A_p + B_p + C_p + \dots\},$$

where

$$A_p = \Sigma_1 q'_{p+1} + \Sigma_2 r'_{p+1} + \dots + \Sigma_{x-1} z'_{p+1},$$

$$B_p = \Sigma_3 (q'_{p+1} A_{p-1}) + \Sigma_5 (r'_{p+1} A_{p-2}) + \dots + \Sigma_{2x-1} (z'_{p+1} A_{p-x+1}),$$

and generally the terms are formed as stated in section 5, using p for x .

9. I shall conclude this part of the subject with a few simple examples for the purpose of illustrating the processes here given.

Ex. 1. Let the equation be

$$u_x = au_{x-1} + b^2 u_{x-2} + G_x.$$

That part of the solution which is independent of G_x , is

$$u_x = c \{ a^x + (x-1)a^{x-2}b^2 + \frac{1}{2}(x-2)(x-3)a^{x-4}b^4 + \frac{1}{2.3}(x-3)(x-4)(x-5)a^{x-6}b^6 + \dots \};$$

and since in the present case P_r and Q_r are constants, we have

$$M_p = a^p + (p-1)a^{p-2}b^2 + \frac{1}{2}(p-2)(p-3)a^{p-4}b^4 + \frac{1}{2.3}(p-3)(p-4)(p-5)a^{p-6}b^6 + \dots$$

whence the particular value of u_r is a series of terms of the form

$$G_r + aG_{r-1} + (a^2 + b^2)G_{r-2} + (a^3 + 2ab^2)G_{r-3} + (a^4 + 3a^2b^2 + b^4)G_{r-4} + (a^5 + 4a^3b^2 + 3ab^4)G_{r-5} + \dots$$

The difference in character between the solution proposed in this paper, and that which would result from a perfect analysis of the general equation, may be exemplified by the present instance.

The perfect solution of this equation is known to be

$$u_r = \frac{1}{\alpha - \beta} \left(\alpha^r \Sigma \frac{G_{r+1}}{\alpha^{r+1}} - \beta^r \Sigma \frac{G_{r+1}}{\beta^{r+1}} \right),$$

where α and β are the roots of $t^2 - at - b^2 = 0$, and each Σ introduces a constant. Now if the constants be made equal, and the expression be written at length, we shall obtain the form derived above.

The expansion of the two terms within the parenthesis gives

$$G_{r+1} + \alpha G_r + \alpha^2 G_{r-1} + \dots + \alpha^{p+1} G_p + \dots + c\alpha^r$$

and

$$G_{r+1} + \beta G_r + \beta^2 G_{r-1} + \dots + \beta^{p+1} G_p + \dots + c\beta^r;$$

and if the difference of these be divided by $\alpha - \beta$, we get

$$G_r + \frac{\alpha^2 - \beta^2}{\alpha - \beta} G_{r-1} + \dots + \frac{\alpha^{p+1} - \beta^{p+1}}{\alpha - \beta} G_p + \dots + c \frac{\alpha^r - \beta^r}{\alpha - \beta},$$

which is the same in effect as the result which we have obtained.

Ex. 2. Let the equation be

$$u_r = xu_{r-1} + au_{r-2}.$$

Then

$$\begin{aligned} u_r &= c\Gamma(x+1) \left\{ 1 + a\Sigma^r \frac{1}{x(x+1)} + a^2\Sigma^r \left(\frac{1}{x(x+1)} \Sigma^{r-1} \frac{1}{x(x+1)} \right) + \dots \right\} \\ &= c\Gamma(x+1) \left\{ 1 - a\frac{1}{x} + a^2 \frac{1}{(x-1)x} - \frac{a^3}{2.3} \frac{1}{(x-2)(x-1)x} + \dots \right\} \\ &= c \left\{ \Gamma(x+1) - a\Gamma x + \frac{a^2}{2} \Gamma(x-1) - \frac{a^3}{2.3} \Gamma(x-2) + \dots \right\}; \end{aligned}$$

which may be put under the form of the definite integral

$$u_r = c \int_0^\alpha e^{-v} e^{-\frac{a}{v}} v^x dv.$$

If we add a second member G_r , the equation for determining M_p is

$$M_p = (x-p+1)M_{p-1} + aM_{p-2};$$

whence

$$\begin{aligned} M_p &= x \dots (x-p+1) \left\{ 1 + a \sum_1^p \frac{1}{(x-p+1)(x-p)} + a^2 \sum_2^p \left(\frac{1}{(x-p+1)(x-p)} \sum_1^{p-1} \frac{1}{(x-p+1)(x-p)} \right) + \dots \right\} \\ &= x \dots (x-p+1) \left\{ 1 + a \left(\frac{1}{x-p+1} - \frac{1}{x} \right) \right. \\ &\quad \left. + a^2 \left(\frac{1}{2} \left(\frac{1}{(x-p+2)(x-p+1)} - \frac{1}{(x-1)(x-2)} \right) - \frac{1}{x} \left(\frac{1}{x-p+1} - \frac{1}{x-2} \right) \right) + \dots \right\} \end{aligned}$$

from which the series for u_x can be expressed.

Ex. 3. Let the equation be

$$u_x = au_{x-1} + xu_{x-2}.$$

Then

$$\begin{aligned} u_x &= c a^x \left\{ 1 + \frac{1}{a^2} \sum^x (x+1) + \frac{1}{a^4} \sum^x ((x+1) \sum^{x-1} (x+1)) + \dots \right\} \\ &= c \left\{ a^x + \frac{1}{2} x(x+1) a^{x-2} + \frac{1}{2.4} (x+1)x(x-1)(x-2) a^{x-4} + \frac{1}{2.4.6} (x+1)x \dots (x-4) a^{x-6} + \dots \right\}. \end{aligned}$$

Ex. 4. Let the equation be

$$u_x = \frac{n}{x} u_{x-1} + c^2 u_{x-2}.$$

Then

$$\begin{aligned} u_x &= \frac{n^x}{\Gamma(x+1)} \left\{ 1 + \frac{c^2}{x^2} \sum^x x(x+1) + \frac{c^4}{x^4} \sum^x (x(x+1) \sum^{x-1} x(x+1)) + \dots \right\} \\ &= \frac{n^x}{\Gamma(x+1)} \left\{ 1 + \frac{c^2}{x^2} \frac{1}{3} (x-1)x(x+1) \right. \\ &\quad \left. + \frac{c^4}{x^4} \left(\frac{1}{6} (x+1)x(x-1)(x-2)(x-3)(x-4) + \frac{3}{5} (x+1)x(x-1)(x-2)(x-3) \right) + \dots \right\}. \end{aligned}$$

Ex. 5. Let the equation be

$$u_x = P_x u_{x-1} + x P_x P_{x-1} u_{x-2} + G_x.$$

Here

$$\begin{aligned} q_x &= x, \quad N'_{x,p} = 1, \quad N''_{x,p} = \frac{x(x+1)}{2} - \frac{(p+1)(p+2)}{2}, \\ N'''_{x,p} &= \frac{(x+1)x(x-1)(x-2)}{2.4} - \frac{(p+2)(p+1)}{2} \cdot \frac{(x+1)x}{2} + \frac{(p+4)(p+3)(p+2)(p+1)}{2.4}, \\ N^{(iv)}_{x,p} &= \frac{(x+1)x(x-1)(x-2)(x-3)(x-4)}{2.4.6} - \frac{(p+2)(p+1)}{2} \cdot \frac{(x+1)x(x-1)(x-2)}{2.4} \\ &\quad + \frac{(p+4)(p+3)(p+2)(p+1)}{2.4} \cdot \frac{(x+1)x}{2} - \frac{(p+6)(p+5)(p+4)(p+3)(p+2)(p+1)}{2.4.6}, \\ N^{(m+3)}_{x,p} &= \frac{(x+1) \dots (x-2m)}{2.4 \dots (2m+2)} - \frac{(p+2)(p+1)}{2} \cdot \frac{(x+1) \dots (x-2m+2)}{2.4 \dots 2m} + \frac{(p+4) \dots (p+1)}{2.4} \cdot \frac{(x+1) \dots (x-2m+4)}{2.4 \dots (2m-2)} \\ &\quad + \dots \pm \frac{(p+2m) \dots (p+1)}{2.4 \dots 2m} \cdot \frac{(x+1)x}{2} \mp \frac{(p+2m+2) \dots (p+1)}{2.4 \dots (2m+2)}. \end{aligned}$$

The particular solution, therefore, is

$$u_x = G_x + P_x G_{x-1} + (1+x)P_x P_{x-1} G_{x-2} + 2xP_x P_{x-1} P_{x-2} G_{x-3} + (x^2+x-2)(P_x \dots P_{x-3})G_{x-4} \\ + (3x^2-5x-2)(P_x \dots P_{x-4})G_{x-5} + (x^3-11x+6)(P_x \dots P_{x-5})G_{x-6} + \dots;$$

and the part of the general solution found by this method is

$$u_x = c(P_x \dots P_0) \left(1 + \frac{(x+1)x}{2} + \frac{(x+1) \dots (x-2)}{2.4} + \frac{(x+1) \dots (x-4)}{2.4.6} + \dots \right).$$

Ex. 6. Let the equation be

$$u_x + a_1 P_x u_{x-1} + a_2 P_x P_{x-1} u_{x-2} + \dots + a_n (P_x \dots P_{x-n+1}) u_{x-n} = G_x;$$

then the equation for determining M_p is

$$M_p + a_1 P_{x-p+1} M_p + a_2 P_{x-p+1} P_{x-p+2} M_{p-2} + \dots + a_n (P_{x-p+1} \dots P_{x-p+n}) M_{p-n} = 0;$$

or making

$$P_{x-p+1} = P'_p,$$

$$M_p + a_1 P'_p M_{p-1} + a_2 P'_p P'_{p-1} M_{p-2} + \dots + a_n (P'_p \dots P'_{p-n+1}) M_{p-n} = 0;$$

the solution of which is evidently the solution of

$$M_p + a_1 M_{p-1} + a_2 M_{p-2} + \dots + a_n M_{p-n} = 0,$$

multiplied by $(P'_p \dots P'_1)$.

$$\text{Let} \quad (t^n + a_1 t^{n-1} + a_2 t^{n-2} + \dots + a_n)^{-1} = \frac{A}{t-\alpha} + \frac{B}{t-\beta} + \dots,$$

then

$$M_p = (P'_p \dots P'_1) \{ c_1 A \alpha^{p+n} + c_2 B \beta^{p+n} + \dots \};$$

and taking the parts affected by each constant separately, it will be seen that the original equation reduces itself to a set of equations of the first order,

$$v_x - \alpha P_x v_{x-1} = \alpha^{n-1} A G_x,$$

$$v_x - \beta P_x v_{x-1} = \beta^{n-1} B G_x,$$

$$\begin{array}{ccc} \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot \end{array}$$

so that its complete solution can be adequately represented.

Solution of Differential Equations in Series.

10. I now proceed to point out a method by which the processes above indicated may be made to give solutions of certain general forms of linear differential equations.

In a paper on Linear Differential Equations presented by me to the Royal Society, and which the Society has done me the honour to publish in the Philosophical Transactions (Part I. for 1848, p. 31), I have enunciated, and so far as is material to the present purpose, demonstrated the following theorem:—

That if, in a linear differential expression $\phi(x, D)u = X$ and its solution $u = \psi(x, D)X$, the letter x be changed into the operative symbol D and D into $-x$, we shall thus

obtain another linear differential expression $\phi(D, -x)u = X$, the solution of which will be $u = \psi(D, -x)X$.

In the application of this theorem, care must be taken that the first-mentioned solution is so written that the operations included under the function ψ are not suppressed; and it must also be borne in mind that the expressions obtained by this interchange of symbols will not in all cases be obviously interpretable.

For applications of this singular analytical process I beg leave to refer to the memoir above cited, where it is employed for the solution, in finite terms, of extensive classes of linear differential equations, and equations in finite differences. So far as the process is legitimate, it is to be observed that it is founded on reasoning of a purely analytical character. It does not in any manner whatever flow from the calculus of operations, or depend for its validity upon the soundness of the logical basis on which this calculus rests.

Now it is a remarkable property of this mechanical interchange of symbols, that it instantaneously converts a linear equation in finite differences into a linear differential equation; so that wherever the former is soluble, the latter is soluble also, provided the result be intelligible, a condition always satisfied when the functions employed are rational algebraical functions.

As an instance worthy of notice, let us take the example last above given (*Ex. 6.*). Bearing in mind that $u_{x-n} = \epsilon^{-nD}u_x$, the proposed interchange gives the equation (writing ϕx for P_x),

$$u + a_1\phi(D)(\epsilon^x u) + a_2\phi(D)\phi(D-1)(\epsilon^{2x} u) + \dots + a_n\phi(D)\dots\phi(D-n+1)(\epsilon^{nx} u) = G;$$

whose solution, therefore, depends upon that of

$$v - \alpha\phi(D)(\epsilon^x v) = \alpha^{n-1}AG,$$

a proposition established by Mr. BOOLE by the methods of the Calculus of Operations.

I propose, therefore, now to employ this theorem of the interchange of symbols for the purpose of converting the forms of solution, above given, of equations in finite differences into the particular solutions of some general forms of differential equations; viz. those equations whose factors do not contain any irrational or transcendental functions of x , or contain them only in the form of series of ascending powers of x .

11. Mr. BOGLE, in his General Method in Analysis, has shown that expressions of this character may be placed in the form

$$f_0(D)u + f_1(D)(\epsilon^x u) + f_2(D)(\epsilon^{2x} u) + \dots = U,$$

by changing the independent variable from x to its logarithm θ , and making use of the relation, (D being $\frac{d}{d\theta}$),

$$D(D-1)\dots(D-n+1)u = x^n \left(\frac{d}{dx}\right)^n u.$$

Making use of this relation, we immediately convert our equation, which we assume to be in the form

$$(a_n + b_n x + \dots + k_n x^{p-1} + l_n x^p + \dots) \frac{d^n u}{dx^n} + (a_{n-1} + b_{n-1} x + \dots + k_{n-1} x^{p-1} + l_{n-1} x^p + \dots) \frac{d^{n-1} u}{dx^{n-1}} \\ + \dots + (a_0 + b_0 x + \dots + k_0 x^{p-1} + l_0 x^p + \dots) u = G,$$

into

$$\left. \begin{aligned} a_n(D \dots (D-n+1))u + & b_n((D-1) \dots (D-n))(\epsilon^\theta u) + c_n((D-2) \dots (D-n-1))(\epsilon^{2\theta} u) + \dots + l_n((D-p) \dots (D-n-p+1))(\epsilon^{p\theta} u) + \dots \\ & + a_{n-1}((D-1) \dots (D-n+1))(\epsilon^\theta u) + b_{n-1}((D-2) \dots (D-n))(\epsilon^{2\theta} u) + \dots + k_{n-1}((D-p) \dots (D-n-p+2))(\epsilon^{p\theta} u) + \dots \\ & + a_{n-2}((D-2) \dots (D-n+1))(\epsilon^{2\theta} u) + \dots + k_{n-2}((D-p) \dots (D-n-p+3))(\epsilon^{p\theta} u) + \dots \\ & + \dots \end{aligned} \right\} = \epsilon^{n\theta} G \dots (2.)$$

or

$$a_n u + \left(\frac{b_n(D-n) + a_{n-1}}{D} \right) (\epsilon^\theta u) + \left(\frac{c_n(D-n)(D-n-1) + b_{n-1}(D-n) + a_{n-2}}{D(D-1)} \right) (\epsilon^{2\theta} u) \\ + \left(\frac{d_n(D-n)(D-n-1)(D-n-2) + c_{n-1}(D-n)(D-n-1) + b_{n-2}(D-n) + a_{n-3}}{D(D-1)(D-2)} \right) (\epsilon^{3\theta} u) + \dots = (D \dots (D-n+1))^{-1} (\epsilon^{n\theta} G).$$

Now if in this equation we change D into θ and θ into $-D$, we obtain the equation in finite differences (which suppose to be of the n th order),

$$a_n u_\theta + \frac{b_n(\theta-n) + a_{n-1}}{\theta} u_{\theta-1} + \frac{c_n(\theta-n)(\theta-n-1) + b_{n-1}(\theta-n) + a_{n-2}}{\theta(\theta-1)} u_{\theta-2} + \dots = (\theta(\theta-1) \dots (\theta-n+1))^{-1} G_{\theta-n},$$

or

$$u_\theta + \frac{f_1 \theta}{f_0 \theta} u_{\theta-1} + \frac{f_2 \theta}{f_0 \theta} u_{\theta-2} + \frac{f_3 \theta}{f_0 \theta} u_{\theta-3} + \dots = (f_0 \theta)^{-1} G_{\theta-n} = H_{\theta-n} \text{ (suppose) ;}$$

the solution of which, by section 8, is of the form

$$u_\theta = M_0 H_{\theta-n} + M_1 H_{\theta-n-1} + M_2 H_{\theta-n-2} + \dots + M_m H_{\theta-n-m} + \dots,$$

where $M_0 = 1$,

$$M_1 + \frac{f_1 \theta}{f_0 \theta} M_0 = 0,$$

$$M_2 + \frac{f_1(\theta-1)}{f_0(\theta-1)} M_1 + \frac{f_2 \theta}{f_0 \theta} M_0 = 0,$$

$$\vdots$$

$$M_m + \frac{f_1(\theta-m+1)}{f_0(\theta-m+1)} M_{m-1} + \frac{f_2(\theta-m+2)}{f_0(\theta-m+2)} M_{m-2} + \dots + \frac{f_r(\theta-m+r)}{f_0(\theta-m+r)} M_{m-r} = 0.$$

Restoring the symbols, and thereby converting $H_{\theta-n}$ into $(f_0(D))^{-1}(\epsilon^{n\theta} G)$, which call H , we have

$$u = M_0 H + M_1(\epsilon^\theta H) + M_2(\epsilon^{2\theta} H) + \dots + M_m(\epsilon^{m\theta} H) + \dots,$$

where $M_0, M_1 \dots$ denote a series of *operations* having the following significations and relations;—

$$M_0 = 1,$$

$$M_1 + \frac{f_1(D)}{f_0(D)} M_0 = 0,$$

$$M_2 + \frac{f_1(D-1)}{f_0(D-1)} M_1 + \frac{f_2(D)}{f_0(D)} M_0 = 0,$$

$$\vdots \quad \quad \quad \vdots \quad \quad \quad \vdots$$

$$M_m + \frac{f_1(D-m+1)}{f_0(D-m+1)} M_{m-1} + \frac{f_2(D-m+2)}{f_0(D-m+2)} M_{m-2} + \dots + \frac{f_r(D-m+r)}{f_0(D-m+r)} M_{m-r} = 0;$$

or, by passing ϵ^θ , $\epsilon^{2\theta}$, &c. outside the operations by the equation

$$\varphi(D)(\epsilon^{m\theta}H) = \epsilon^{m\theta}\varphi(D+m)H,$$

$$u = M_0H + \epsilon^\theta M_1H + \epsilon^{2\theta} M_2H + \dots + \epsilon^{m\theta} M_mH + \dots, \quad (3.)$$

where the general law of relation is

$$M_m + \frac{f_1(D+1)}{f_0(D+1)} M_{m-1} + \frac{f_2(D+2)}{f_0(D+2)} M_{m-2} + \dots + \frac{f_r(D+r)}{f_0(D+r)} M_{m-r} = 0.$$

Now the expression H , which is the subject of all the operations, contains n arbitrary constants, since $f_0(D)$ is of the n th order; that is, provided a_n is not zero.

Let $\beta_1 \beta_2 \dots \beta_n$ be the roots of $f_0t=0$; then the complete value of H is

$$(f_0(D))^{-1}(\epsilon^{n\theta}G) + c_1\epsilon^{\beta_1\theta} + c_2\epsilon^{\beta_2\theta} + \dots + c_n\epsilon^{\beta_n\theta};$$

of which the first term will give us the particular solution of the original equation, and each of the other terms a complementary solution. Bearing in mind the equation $\varphi(D)(\epsilon^{p\theta}) = \varphi p \cdot \epsilon^{p\theta}$, we see at once that the first complementary solution is

$$c_1\epsilon^{\beta_1\theta}(A_0 + A_1\epsilon^\theta + A_2\epsilon^{2\theta} + \dots + A_m\epsilon^{m\theta} + \dots),$$

or

$$c_1x^{\beta_1}(A_0 + A_1x + A_2x^2 + \dots + A_mx^m + \dots),$$

where $A_0=1$, and the law of formation of the coefficients is

$$f_0(\beta_1+m)A_m + f_1(\beta_1+m)A_{m-1} + f_2(\beta_1+m)A_{m-2} + \dots + f_r(\beta_1+m)A_{m-r} = 0;$$

and the remaining complementary solutions merely require the substitution of $\beta_2 \dots \beta_n$ successively for β_1 , with new constants.

The reader will not fail to perceive the peculiarity of the series when a_n is unity, (which value it can always have when it is not zero); for in that case the roots of $f_0t=0$ are the natural numbers from 0 to $n-1$ inclusive, so that the series begin respectively with the terms 1, x , x^2 , $\dots x^{n-1}$.

If a_n be zero, that is, if the factor of the highest differential coefficient of u do not contain an absolute term, then in order that the transformed equation may take the form (2.), it will be necessary to pass ϵ^θ outside the operative functions in each term, and divide by ϵ^θ . The initial function f_0t will then be of the form

$$b_n(t \dots (t-n+1)) + a_{n-1}(t \dots (t-n+2)),$$

and H will be

$$(f_0(D))^{-1}(\epsilon^{(n-1)\theta}G).$$

Similarly, if in addition to a_n being zero, we have also b_n and a_{n-1} respectively equal to zero, it will be necessary to pass $\epsilon^{2\theta}$ outside the operative functions, and divide by

ϵ^0 ; and so on if other terms of the factors are wanting. Thus we can in all cases obtain the transformed equation in the required shape; but in all the cases where coefficients thus vanish, there is the important qualification that $f_0 t$ is no longer necessarily of the n th order; so that H does not necessarily contain the proper number of arbitrary constants. The consequences of this consideration will be afterwards developed; in the meantime we proceed to consider what modifications the series undergo where $f_0 t$ has two or more equal roots.

12. If there should be two roots of $f_0 t = 0$ equal to β_1 , one series will be deficient; and it will be supplied as follows. The expression H , or $(f_0(D))^{-1}(\epsilon^0 G)$, contains in that case a term of the form $k_1 \theta \epsilon^{\beta_1}$; and it is easily seen that

$$\phi(D)(\theta \epsilon^{\beta_1}) = \theta \phi(D)(\epsilon^{\beta_1}) + \phi'(D)(\epsilon^{\beta_1}).$$

The wanting series is, therefore,

$$k_1 x^{\beta_1} \{ \log x (1 + A_1 x + A_2 x^2 + \dots) + (A'_1 x + A'_2 x^2 + \dots) \},$$

where

$$A'_1 = \frac{dA_1}{d\beta_1} \text{ \&c.}$$

If there should be three roots of $f_0 t = 0$ equal to β_1 , two series will be deficient, one of which will be supplied as last mentioned; and the other by the introduction of the series

$$k_2 x^{\beta_1} \{ (\log x)^2 (1 + A_1 x + A_2 x^2 + \dots) + 2 \log x (A'_1 x + A'_2 x^2 + \dots) + (A''_1 x + A''_2 x^2 + \dots) \},$$

where $A''_1 = \frac{d^2 A_1}{d\beta_1^2}$ &c.; for we have a term $k_2 \theta^2 \epsilon^{\beta_1}$; and it is easily seen that

$$\phi(D)(\theta^2 \epsilon^{\beta_1}) = \theta^2 \phi(D)(\epsilon^{\beta_1}) + 2\theta \phi'(D)(\epsilon^{\beta_1}) + \phi''(D)(\epsilon^{\beta_1}).$$

And generally since, where there are $p+1$ equal roots β_1 , we have terms

$$c_1 \epsilon^{\beta_1} + k_1 \theta \epsilon^{\beta_1} + k_2 \theta^2 \epsilon^{\beta_1} + \dots + k_p \theta^p \epsilon^{\beta_1};$$

and since

$$\phi(D)(\theta^p \epsilon^{\beta_1}) = \theta^p \phi(D)(\epsilon^{\beta_1}) + p \theta^{p-1} \phi'(D)(\epsilon^{\beta_1}) + p \frac{p-1}{2} \theta^{p-2} \phi''(D)(\epsilon^{\beta_1}) + \dots;$$

the deficient series will be supplied by the following, taken with a different constant for every value of p from unity upwards:

$$k_p x^{\beta_1} \left\{ (\log x)^p (1 + A_1 x + A_2 x^2 + \dots) + p (\log x)^{p-1} (A'_1 x + A'_2 x^2 + \dots) \right. \\ \left. + p \frac{p-1}{2} (\log x)^{p-2} (A''_1 x + A''_2 x^2 + \dots) + \dots + p \log x (A_1^{(p-1)} x + A_2^{(p-1)} x^2 + \dots) + (A_1^{(p)} x + A_2^{(p)} x^2 + \dots) \right\}.$$

As a matter of convenience, when the equal roots are zero, a temporary nominal value should be given to them for the purpose of differentiating A_1 , &c.

13. Hitherto we have attended especially to the complementary solutions, or in other words, regarded G as zero. The operations, however, indicated in (3.) may be readily performed when G is a rational function of x ; which we will suppose to be

cleared of fractions, and so cleared to consist of a set of terms $\alpha_p x^p$. Then for this term $(f_0(D))^{-1}(\epsilon^n G) = \alpha_p (f_0(D))^{-1} \epsilon^{(n+p)} = \alpha_p (f_0(n+p))^{-1} \epsilon^{(n+p)}$; and we have, as before,

$$u = \alpha_p x^{(n+p)} (B_0 + B_1 x + B_2 x^2 + \dots + B_m x^m + \dots),$$

where $B_0 = 1$, and the law of formation is

$$f_0(n+p+m)B_m + f_1(n+p+m)B_{m-1} + f_2(n+p+m)B_{m-2} + \dots = 0;$$

and the whole solution is found by taking all the values of p . This will undergo a slight alteration, as the incipient term will be of the form $f_0(D)(\epsilon^{(n-r)} G)$, where r factors of (2.) vanish by reason of some of the constant coefficients a_n , &c. being zero.

In like manner it would be easy to represent the series if G contained $\log x$ and its powers; but for most other forms it would be necessary to expand G in order to represent the series explicitly. The solution however is theoretically complete, since it consists solely in the performance of operations which are known explicit functions of D .

14. Before proceeding further with the main subject, I shall illustrate this process by a few examples.

Ex. 1. Let the equation be

$$(1 + b_2 x + c_2 x^2) \frac{d^2 u}{dx^2} + (a_1 + b_1 x) \frac{du}{dx} + a_0 u = G.$$

Referring to (2.), we have

$$f_0 D = D(D-1), \quad f_1 D = b_2(D-1)(D-2) + a_1(D-1),$$

$$f_2(D) = c_2(D-2)(D-3) + b_1(D-2) + a_0;$$

roots of $f_0 t = 0$ are 0 and 1.

First complementary series,

$$c_1(A_0 + A_1 x + A_2 x^2 + \dots + A_m x^m + \dots),$$

where

$A_0 = 1$, and $m(m-1)A_m + (b_2(m-1)(m-2) + a_1(m-1))A_{m-1} + (c_2(m-2)(m-3) + b_1(m-2) + a_0)A_{m-2} = 0$ is the law of formation.

Second complementary series,

$$c_2 x(A_0 + A_1 x + A_2 x^2 + \dots + A_m x^m + \dots),$$

where

$A_0 = 1$, and $(m+1)mA_m + (b_2(m)(m-1) + a_1 m)A_{m-1} + (c_2(m-1)(m-2) + b_1(m-1) + a_0)A_{m-2} = 0$ is the law of formation.

Particular solution,

$$u = \psi_0(D)H + \epsilon^1 \psi_1(D)H + \epsilon^2 \psi_2(D)H + \dots + \epsilon^m \psi_m(D)H + \dots,$$

where

$$H = \left(\frac{d}{dx}\right)^{-2} G, \quad \psi_0(D) = 1, \quad \text{and} \quad f_0(D+m)\psi_m(D) + f_1(D+m)\psi_{m-1}(D) + f_2(D+m)\psi_{m-2}(D) = 0$$

is the law of the formation of the operative functions.

The three laws of formation, which are here written down at length, are in substance the same; for from the last the others are made by merely writing the roots successively in lieu of D in the factors.

Ex. 2. Let the equation be

$$(x^2 + qx^4) \frac{d^2u}{dx^2} + (x + px^2 + 5qx^3) \frac{du}{dx} + (px - n^2 + (4q + r)x^2)u = G.$$

Referring to (2.), we have in the first instance,

$$((D-2)^2 - n^2)(\epsilon^{20}u) + p(D-2)(\epsilon^{20}u) + (q(D-4)D + 4q + r)\epsilon^{40}u = G,$$

whence

$$(D^2 - n^2)u + pD(\epsilon^2u) + (qD^2 + r)(\epsilon^{20}u) = \epsilon^{-20}G;$$

roots of $f_0t=0$ are n and $-n$.

First complementary series,

$$c_1x^n(A_0 + A_1x + A_2x^2 + \dots + A_mx^m + \dots),$$

where

$$A_0 = 1, \text{ and } ((n+m)^2 - n^2)A_m + p(n+m)A_{m-1} + (q(n+m)^2 + r)A_{m-2} = 0$$

is the law of formation.

Second complementary series,

$$c_2x^{-n}(A_0 + A_1x + A_2x^2 + \dots + A_mx^m + \dots),$$

where the law is, as before, changing the sign of n .

Particular solution,

$$u = \psi_0(D)H + \epsilon^2\psi_1(D)H + \dots + \epsilon^{2m}\psi_m(D)H + \dots,$$

where

$$H = (D^2 - n^2)^{-1}(\epsilon^{-20}G), \psi_0(D) = 1,$$

and

$$((D+m)^2 - n^2)\psi_m(D) + p(D+m)\psi_{m-1}(D) + (q(D+m)^2 + r)\psi_{m-2}(D) = 0$$

is the law of the formation of the operative functions.

If $p=0$, $q=0$, and $r=1$, so that the equation becomes

$$x^2 \frac{d^2u}{dx^2} + x \frac{du}{dx} + (x^2 - n^2)u = 0,$$

the law is

$$A_m = -\frac{A_{m-2}}{m(2n+m)},$$

whence

$$u = c_1x^n \left\{ 1 - \frac{1}{4}(1+n)^{-1}x^2 + \frac{1}{4.8}(1+n)^{-1}(2+n)^{-1}x^4 - \frac{1}{4.8.12}(1+n)^{-1}(2+n)^{-1}(3+n)^{-1}x^6 + \dots \right\},$$

$$+ c_2x^{-n} \left\{ 1 - \frac{1}{4}(1-n)^{-1}x^2 + \frac{1}{4.8}(1-n)^{-1}(2-n)^{-1}x^4 - \frac{1}{4.8.12}(1-n)^{-1}(2-n)^{-1}(3-n)^{-1}x^6 + \dots \right\}.$$

Ex. 3. Let the equation be

$$x^3 \frac{d^3u}{dx^3} + 3x^2 \frac{d^2u}{dx^2} + x \frac{du}{dx} + qx^nu = G.$$

Referring to (2.), we have

$$\begin{aligned} D^3 u + q \epsilon^3 u &= G; \\ f_0(D) &= D^3; \text{ three roots equal to zero;} \\ f_n(D) &= q; \\ m^3 A_m + q A_{m-n} &= 0, \text{ law of formation.} \end{aligned}$$

Hence the complementary series are,

$$\begin{aligned} u &= c_1 \left\{ 1 - \frac{q}{n^3} x^n + \frac{q}{n^3} \frac{q}{(2n)^3} x^{2n} - \frac{q}{n^3} \frac{q}{(2n)^3} \frac{q}{(3n)^3} x^{3n} + \dots \right\} \\ &+ c_2 \left\{ \log x \left(1 - \frac{q}{n^3} x^n + \frac{q}{n^3} \frac{q}{(2n)^3} x^{2n} - \frac{q}{n^3} \frac{q}{(2n)^3} \frac{q}{(3n)^3} x^{3n} + \dots \right) \right. \\ &\quad \left. + 3 \left(\frac{1}{n} \frac{q}{n^3} x^n - \left(\frac{1}{n} + \frac{1}{2n} \right) \frac{q}{n^3} \frac{q}{(2n)^3} x^{2n} + \left(\frac{1}{n} + \frac{1}{2n} + \frac{1}{3n} \right) \frac{q}{n^3} \frac{q}{(2n)^3} \frac{q}{(3n)^3} x^{3n} + \dots \right) \right\} \\ &+ c_3 \left\{ (\log x)^2 \left(1 - \frac{q}{n^3} x^n + \frac{q}{n^3} \frac{q}{(2n)^3} x^{2n} - \frac{q}{n^3} \frac{q}{(2n)^3} \frac{q}{(3n)^3} x^{3n} + \dots \right) \right. \\ &\quad + 6 \log x \left(\frac{1}{n} \frac{q}{n^3} x^n - \left(\frac{1}{n} + \frac{1}{2n} \right) \frac{q}{n^3} \frac{q}{(2n)^3} x^{2n} + \left(\frac{1}{n} + \frac{1}{2n} + \frac{1}{3n} \right) \frac{q}{n^3} \frac{q}{(2n)^3} \frac{q}{(3n)^3} x^{3n} + \dots \right) \\ &\quad + 6 \left(-\frac{2}{n^2} \frac{q}{n^3} x^n + \left(\frac{2}{n^2} + \frac{2}{(2n)^2} + \frac{3}{n(2n)} \right) \frac{q}{n^3} \frac{q}{(2n)^3} x^{2n} \right. \\ &\quad \left. - \left(\frac{2}{n^2} + \frac{2}{(2n)^2} + \frac{2}{(3n)^2} + \frac{3}{n(2n)} + \frac{3}{n(3n)} + \frac{3}{(2n)(3n)} \right) \frac{q}{n^3} \frac{q}{(2n)^3} \frac{q}{(3n)^3} x^{3n} + \dots \right) \left. \right\}; \end{aligned}$$

and the particular solution is

$$u = H - q x^n (D+n)^{-3} H + q^2 x^{2n} (D+n)^{-3} (D+2n)^{-3} H - \dots,$$

H being $D^{-3}(\epsilon^3 G)$.

15. The completeness of the preceding forms of solution depends, as above intimated, upon the circumstance that the function $f_0(D)$ is of an order not lower than the order of the original equation. It may however be of a lower order, as would take place in the first of the examples above given, if a_2 instead of being 1 were zero and b_2 were also zero.

Let the equation then be (*Ex. 4.*),

$$c_2 x^2 \frac{d^2 u}{dx^2} + (1 + b_1 x) \frac{du}{dx} + a_0 u = G.$$

Referring to (2.), we have, in the first instance,

$$(D-1)(\epsilon^0 u) + (c_2(D-2)(D-3) + b_1(D-2) + a_0)(\epsilon^2 u) = G,$$

or

$$Du + (c_2(D-1)(D-2) + b_1(D-1) + a_0)(\epsilon^0 u) = \epsilon^{-2} G;$$

so that $f_0(D)$ is of the first order, and the root is zero.

The only complementary function to be obtained is therefore

$$u = c_1 \left\{ 1 - a_0 x + \frac{1}{2} a_0 (b_1 + a_0) x^2 - \frac{1}{2.3} a_0 (b_1 + a_0) (1.2c_2 + 2b_1 + a_0) x^3 \right. \\ \left. + \frac{1}{2.3.4} a_0 (b_1 + a_0) (1.2c_2 + 2b_1 + a_0) (2.3c_2 + 3b_1 + a_0) x^4 - \dots \right\},$$

a series which is divergent, but which, as will also be seen afterwards, is finite when the constant coefficients are connected by the formula $(p-1)pc_2 + pb_1 + a_0 = 0$, p being any positive integer.

Further, it may happen that the series obtained by the process, even when they afford a complete solution in respect of the number of constants, are divergent for all or for some values of x . This may evidently be the case in the solution of the second of the examples above given; for the law of the coefficients, as we advance in the series, approximates to $A_m = -qA_{m-2}$.

In these cases, other solutions in series may be obtained by resolving the equation in finite differences in a series of terms of the form $H_{\theta+p}$ instead of $H_{\theta-p}$. In order to effect this, all that is necessary is to write $\theta+r$ for θ , (r being the order of the equation in finite differences), and to divide by the factor of the last term instead of the factor of the first term; or in other words, we must pass ϵ^r outside the functions in (2.), and multiply by $\epsilon^{-r\theta}$; so that this equation now assumes the form

$$f_r(D)u + f_{r-1}(D)(\epsilon^{-\theta}u) + f_{r-2}(D)(\epsilon^{-2\theta}u) + \dots = \epsilon^{(n-r)\theta}G;$$

and the equation in finite differences is

$$f_r(\theta)u_\theta + f_{r-1}(\theta)u_{\theta+1} + f_{r-2}(\theta)u_{\theta+2} + \dots = G_{\theta+r-n}.$$

We have now to inquire for the roots of $f_r t = 0$; the incipient term is $(f_r(D))^{-1}(\epsilon^{(n-r)\theta}G)$, which call H_1 ; and the particular solution will be found to be

$$u = \psi_0(D)H_1 + \epsilon^{-\theta}\psi_1(D)H_1 + \epsilon^{-2\theta}\psi_2(D)H_1 + \dots + \epsilon^{-m\theta}\psi_m(D)H_1 + \dots,$$

where

$$\psi_0(D) = 1, \text{ and } f_r(D-m)\psi_m(D) + f_{r-1}(D-m)\psi_{m-1}(D) + \dots = 0$$

is the law of the formation of the operative functions.

The substitution of the roots of $f_r t$ successively for D gives the law of formation of the complementary series.

Taking the last example, the transformed equation now becomes

$$(c_2 D(D-1) + b_1 D + a_0)u + (D+1)(\epsilon^{-\theta}u) = \epsilon^{-2\theta}G.$$

Let the roots of $f_r t$ or $c_2 t(t-1) + b_1 t + a_0 = 0$, be β_1 and β_2 .

The first complementary series is

$$c_1 x^{\beta_1} (A_0 + A_1 x^{-1} + \dots + A_m x^{-m} + \dots),$$

where $A_0=1$, and $f_r(\beta_1-m)A_m+(\beta_1-m+1)A_{m-1}=0$ is the law of formation ; the series then are

$$c_1 \left\{ x^{\beta_1} - \frac{\beta_1}{f_r(\beta_1-1)} x^{\beta_1-1} + \frac{\beta_1}{f_r(\beta_1-1)} \frac{\beta_1-1}{f_r(\beta_1-2)} x^{\beta_1-2} - \frac{\beta_1}{f_r(\beta_1-1)} \frac{\beta_1-1}{f_r(\beta_1-2)} \frac{\beta_1-2}{f_r(\beta_1-3)} x^{\beta_1-3} + \dots \right\} \\ + c_2 \{ \text{similar function of } \beta_2 \dots \},$$

which is, (since $f_r(\beta_1-n)=n(n+1)c_2-2n\beta_1-nb_1$),

$$c_1 \left\{ x^{\beta_1} - \frac{\beta_1}{2c_2-2\beta_1-b_1} x^{\beta_1-1} + \frac{\beta_1}{2c_2-2\beta_1-b_1} \frac{\beta_1-1}{2.3c_2-4\beta_1-2b_1} x^{\beta_1-2} \right. \\ \left. - \frac{\beta_1}{2c_2-2\beta_1-b_1} \frac{\beta_1-1}{2.3c_2-4\beta_1-2b_1} \frac{\beta_1-2}{3.4c_2-6\beta_1-3b_1} x^{\beta_1-3} + \dots \right\} \\ + c_2 \{ \text{similar function of } \beta_2 \dots \}.$$

When β_1 or β_2 is a positive integer, one of these series is terminable ; and if both are positive integers, the series derived from the smaller root is terminable, and the other gives no result, the coefficients becoming infinite. The first of the series will then be found to give the same result as that produced from the divergent series (which is evidently terminable in form in the case indicated), except that it begins at the other end. In this case the other complementary solution can be found in finite terms by reducing the order of the equation.

The particular solution is

$$u = H_1 - x^{-1} \frac{D}{f_r(D-1)} H_1 + x^{-2} \frac{D}{f_r(D-1)} \frac{D-1}{f_r(D-2)} H_1 - \dots,$$

H_1 being $(f_r(D))^{-1}(\epsilon^{-2\theta}G)$.

Let us now return to example 2, the solution of which, as above found, is in some cases divergent.

The transformed equation now becomes

$$(q(D+2)^2+r)u+p(D+2)(\epsilon^{-\theta}u)+((D+2)^2-r^2)(\epsilon^{-2\theta}u)=\epsilon^{-2\theta}G.$$

Roots of $f.t=0$ are $-2 \pm \sqrt{\left(\frac{r}{q}\right)}$, which call β_1 and β_2 .

First complementary series,

$$c_1 x^{\beta_1} \left(A_0 + \frac{A_1}{x} + \frac{A_2}{x^2} + \dots + \frac{A_m}{x^m} + \dots \right),$$

where

$A_0=1$, and $(q(\beta_1-m+2)^2+r)A_m+p(\beta_1-m+2)A_{m-1}+((\beta_1-m+2)^2-r^2)A_{m-2}=0$, is the law of formation ; and the second complementary series is the same, using the other root with a different constant.

In like manner the particular solution is easily represented.

16. We are now in a position to discuss the character of the various series obtained by this process with reference to their convergency or divergency, a subject of the highest importance to the value of the process. The investigations which follow, will, it is apprehended, be found to afford a complete test of the nature of the series.

In general, it will of course be understood that these researches, as to convergency and divergency, relate only to the complementary series, or in other words, to equations deprived of their second member; nevertheless they will to a great extent, if not throughout, apply to cases in which the second member is in the form of integer powers of x or of $\log x$.

1st. When in the set of functions

$$f_0(D), f_1(D), f_2(D), \dots, f_{r-1}(D), f_r(D),$$

the function $f_0(D)$ is of a higher dimension with regard to D than any other of the set, or is what we shall here call the dominant function, the solution of the equation can always be found in a *convergent* series of ascending powers of x ; and if in such a case we solve the equation in a series of descending powers of x , which we can do if we please, that series is certainly divergent.

This is immediately apparent from the consideration of the law of the coefficients

$$f_0(\beta_1 + m)A_m + f_1(\beta_1 + m)A_{m-1} + \dots + f_r(\beta_1 + m)A_{m-r} = 0;$$

which, as m increases without limit, approaches to the form

$$A_m = -\frac{f_1 m}{f_0 m} A_{m-1} - \frac{f_2 m}{f_0 m} A_{m-2} - \dots - \frac{f_r m}{f_0 m} A_{m-r}, \text{ or } mA_m = -a_{n-1}A_{m-1} - b_{n-1}A_{m-2} - \dots,$$

assuming that all the functions are only one degree lower than A_m , which is the least favourable case for convergency. Therefore, if the largest of the terms of the right-hand side of this equation be $l_{n-1}A_{m-p}$, we have

$$A_m < \frac{l_{n-1}}{m} A_{m-p};$$

and we can therefore arrive at a point in the series at which the ratio of the coefficient of x^m to that of x^{m-p} diminishes without limit.

It will also be observed that the series introduced by two or more equal roots are of the same character as the original series from which they are derived; for, A_m being of the form $\phi(\beta_1 + m)$, we have $\frac{dA_m}{d\beta_1} = \frac{dA_m}{dm}$; and when A_{m-p} is of a higher order with reference to m than A_m , $\frac{dA_{m-p}}{dm}$ is also of a higher order than $\frac{dA_m}{dm}$, and so for the other differential coefficients.

We have now merely to inquire what must be the form of the original equation that $f_0(D)$ may be the dominant function. Referring to (2.), we see that it is necessary that the factor of the highest differential coefficient of u should contain one term only. If this factor be 1 or x , no restriction need be imposed on the succeeding factors. If it be x^2 , the factor of the next lower differential coefficient must not contain an absolute term; and generally, if x^p be the factor of $\frac{d^n u}{dx^n}$, the factor of $\frac{d^{n-1} u}{dx^{n-1}}$ must begin with a term not lower than x^{p-1} , that of $\frac{d^{n-2} u}{dx^{n-2}}$ with a term not lower than x^{p-2} , and so on.

In short, all equations where x does not enter into the factor of $\frac{d^n u}{dx^n}$, and all equations of the form

$$x^p \frac{d^n u}{dx^n} + (h_n x^{p-1} + l_n x^p + \dots) \frac{d^{n-1} u}{dx^{n-1}} + (h_{n-2} x^{p-3} + \dots) \frac{d^{n-2} u}{dx^{n-2}} + \dots = 0,$$

where p is a positive integer, are soluble in *convergent* series of *ascending* powers of x . The third example above given is an instance of this form.

2ndly. When in the set of functions

$$f_r(D), f_{r-1}(D), \dots f_1(D), f_0(D),$$

the function $f_r(D)$ is the dominant function, the solution of the equation can always be found in a *convergent* series of descending powers of x ; and if in such a case we solve the equation in a series of ascending powers of x , that series is certainly divergent.

This is apparent, as before, from the consideration of the law of the coefficients,

$$f_r(\beta_1 - m)A_m + f_{r-1}(\beta_1 - m)A_{m-1} + \dots + f_0(\beta_1 - m)A_{m-r} = 0,$$

which, as m increases without limit, approaches to

$$mA_m = h_{n-1}A_{m-1} - k_{n-1}A_{m-2} + \dots$$

in the least favourable case for convergency.

On proceeding to inquire what must be the form of the original equation, we see again that it is necessary that the factor of the highest differential coefficient of u should contain one term only. If this be x^p , then the other restrictions are, that the factor of the next differential coefficient must stop at x^{p-1} , that of the next at x^{p-2} , and so on.

In short, for all integer values of p the equation

$$x^p \frac{d^n u}{dx^n} + (a_{n-1} + b_{n-1}x + \dots + k_n x^{p-1}) \frac{d^{n-1} u}{dx^{n-1}} + (a_{n-2} + b_{n-2}x + \dots + h_{n-2} x^{p-2}) \frac{d^{n-2} u}{dx^{n-2}} + \dots = 0$$

is soluble in a *convergent* series of *descending* powers of x . The 4th example above given is an instance of this form.

3rdly. When in the set of functions

$$f_0(D), f_1(D), \dots f_{r-1}(D), f_r(D),$$

the functions $f_0(D)$ and $f_r(D)$ are of the same dimensions, and are both dominant over all the other functions, the solution of the equation can be found in a series of ascending powers of x , which for some values of x is convergent, and for other values of x is divergent; and the solution can also be found in a series of descending powers of x which is divergent for all values of x for which the other series is convergent, and convergent for all values of x for which the other series is divergent.

For in the ascending series the law of the coefficients approaches the form, (the

coefficient of the highest power of m in $f_0 m$ being 1, and that in $f_r m$ being l_n .)

$$A_m = -l_n A_{m-r},$$

and in the descending series the law approaches to

$$l_n A_m = \pm A_{m-r}.$$

The former series is therefore convergent for all values of x numerically less than $(l_n)^{-\frac{1}{r}}$, and divergent for all values of x numerically greater than this limit; and the latter series is divergent for all values of x numerically greater than this quantity, and divergent for all values of x numerically less.

The equations to which this rule is applicable are of the forms, (p' being less than p .)

$$(x^{p'} + l_n x^p) \frac{d^n u}{dx^n} + (h'_{n-1} x^{p'-1} + \dots + k_{n-1} x^{p-1}) \frac{d^{n-1} u}{dx^{n-1}} + (g_{n-2} x^{p'-2} + \dots + h_{n-2} x^{p-2}) \frac{d^{n-2} u}{dx^{n-2}} + \dots = 0,$$

and

$$(a_n + l_n x^p) \frac{d^n u}{dx^n} + (a_{n-1} + b_{n-1} x + \dots + k_{n-1} x^{p-1}) \frac{d^{n-1} u}{dx^{n-1}} + (a_{n-2} + \dots + h_{n-2} x^{p-2}) \frac{d^{n-2} u}{dx^{n-2}} + \dots = 0.$$

The second example above given is an instance of the first of these forms.

4thly. When in the set of functions

$$f_0(D), f_1(D), \dots, f_{r-1}(D), f_r(D),$$

one or more of the intermediate functions is or are of the same order as the extreme functions $f_0(D)$ and $f_r(D)$, or as the highest of these two when they differ in dimensions, the series obtained by the above processes will be divergent for some values of x , and we have not as yet any method of deriving a convergent series corresponding to these values; and if one or more of the intermediate functions be of a higher dimension than the extreme functions, the series obtained by the above processes will certainly be divergent.

These remaining cases therefore sever into two species; first, where some of the intermediate functions are of the same order as the highest of the extreme functions; secondly, where one or more of the intermediate functions are dominant.

The first of these species includes equations of the two following forms:—

$$(a_n x^{p'} + b_n x^{p'+1} + \dots + l_n x^p) \frac{d^n u}{dx^n} + (a_{n-1} x^{p'-1} + b_{n-1} x^{p'} + \dots) \frac{d^{n-1} u}{dx^{n-1}} + \dots + (a_0 x^{p'-n} + \dots) u = 0,$$

in which there can be no function higher than $f_0(D)$; and

$$(a_n + b_n x + \dots + l_n x^p) \frac{d^n u}{dx^n} + (a_{n-1} + b_{n-1} x + \dots + k_{n-1} x^{p-1}) \frac{d^{n-1} u}{dx^{n-1}} + \dots = 0,$$

in which there can be no function higher than $f_r(D)$.

In these cases it will easily be seen that the law of the coefficients of the ascending series, as m increases without limit, approximates to

$$a_n A_m + b_n A_{m-1} + \dots + l_n A_{m-p} = 0,$$

and the series therefore approaches without limit to a recurring series, in which the constants of relation are

$$-\frac{b_n}{a_n}x, -\frac{c_n}{a_n}x^2, \dots -\frac{l_n}{a_n}x^p;$$

and the denominator of the rational fraction, to which the residue of the series approaches, is

$$\frac{1}{a_n}(a_n + b_n x + \dots + l_n x^p).$$

In the descending series, the law of the coefficients approximates to

$$l_n A_n + k_n A_{n-1} + \dots + a_n A_{n-p} = 0,$$

and the series approaches without limit to a recurring series, in which the constants of relation are

$$-\frac{k_n}{l_n} \frac{1}{x}, \dots -\frac{b_n}{l_n} \frac{1}{x^{p-1}}, -\frac{a_n}{l_n} \frac{1}{x^p};$$

and the denominator of the rational fraction, to which the residue of the series approaches, is

$$\frac{1}{l_n x^p} (a_n + b_n x + \dots + l_n x^p).$$

When $f_0(D)$ is higher than $f_r(D)$, the ascending series alone can be used; when $f_r(D)$ is higher than $f_0(D)$, the descending series alone can be used; and when $f_0(D)$ and $f_r(D)$ are of the same dimension, either may be used; and the approximations above referred to render it probable that these series, notwithstanding that they may be divergent, are the developments of continuous algebraical expressions.

The second of the species above referred to includes all equations which are excluded from the preceding forms; that is, all forms which transgress *both* the restrictions to which the equation in the third case is subjected.

Of these forms, the solutions, whether obtained in ascending or in descending series, are always divergent; and the divergency appears to be of an extreme and unmanageable character. In this case we have an intermediate dominant function; and the convergent solutions might, from considerations of analogy, be presumed to be series infinite in both directions, the roots of the dominant function determining the incipient terms.

The treatment of these forms requires the solution of the equation in finite differences

$$\dots + R_n u_{n+3} + Q_n u_{n+2} + P_n u_{n+1} + u_n + P_n u_{n-1} + Q_n u_{n-2} + R_n u_{n-3} + \dots = G_n,$$

not starting from either of the two extreme terms, as is done above, but from the term u_n , so as to get a result in the form

$$u_n = \dots + M_{-2} G_{n+2} + M_{-1} G_{n+1} + M_0 G_n + M_1 G_{n-1} + M_2 G_{n-2} + \dots$$

It would probably not be difficult to show that such a solution exists; but I have not found one in a form available for the purpose to which it is desired to be applied.

17. Throughout the preceding investigations, the series obtained by the processes here displayed undergo a modification of form in the event of the expression $f_0 t$ or $f_1 t$, as the case may be, having one or more sets of imaginary roots.

Let there be a couple of such roots of the form $\alpha \pm \beta \sqrt{-1}$. In the ascending series these roots give

$$c_1 x^{\alpha + \beta \sqrt{-1}} (A_0 + A_1 x + A_2 x^2 + \dots + A_m x^m + \dots) \\ c_2 x^{\alpha - \beta \sqrt{-1}} (B_0 + B_1 x + B_2 x^2 + \dots + B_m x^m + \dots),$$

where

$$f_0(\alpha + m + \beta \sqrt{-1}) A_m + f_1(\alpha + m + \beta \sqrt{-1}) A_{m-1} + \dots = 0$$

and

$$f_0(\alpha + m - \beta \sqrt{-1}) B_m + f_1(\alpha + m - \beta \sqrt{-1}) B_{m-1} + \dots = 0$$

are the respective laws of formation.

It is apparent, therefore, that if A_m be of the form $\phi(\alpha + \beta \sqrt{-1})$, B_m is of the form $\phi(\alpha - \beta \sqrt{-1})$.

Making $c_2 = c_1$, and remembering that

$$x^{\beta \sqrt{-1}} + x^{-\beta \sqrt{-1}} = 2 \cos(\beta \log x) \\ x^{\beta \sqrt{-1}} - x^{-\beta \sqrt{-1}} = 2 \sqrt{-1} \sin(\beta \log x),$$

the sum of the two series gives the double series,

$$2c_1 x^\alpha \{ \cos(\beta \log x) \{ A_0 + B_0 + (A_1 + B_1)x + \dots + (A_m + B_m)x^m + \dots \} \\ + \sqrt{-1} \sin(\beta \log x) \{ A_0 - B_0 + (A_1 - B_1)x + \dots + (A_m - B_m)x^m + \dots \} \};$$

which is necessarily real, since $A_m + B_m$ is purely real, and $A_m - B_m$ is purely imaginary.

Making now $c_1 = -c_2$, the sum of the two series gives another double series, (making $c_2 = -k \sqrt{-1}$),

$$2kx^\alpha \{ \sqrt{-1} \cos(\beta \log x) \{ A_0 - B_0 + (A_1 - B_1)x + \dots + (A_m - B_m)x^m + \dots \} \\ + \sin(\beta \log x) \{ A_0 + B_0 + (A_1 + B_1)x + \dots + (A_m + B_m)x^m + \dots \} \},$$

which is likewise real.

The descending series may be treated in a similar manner.

18. Most of the examples to which the preceding processes are applied have been taken from the paper in the Philosophical Transactions for 1844, in which Mr. BOOLE developed his new General Method in Analysis, with which the subject matter of the present paper is closely connected, though the methods exhibited are distinct; unless indeed it should prove, that the interchange of the symbol of operation and the independent variable, and the general relation exhibited by Mr. BOOLE's fundamental theorem of development connecting any system of linear differential equations with a corresponding system of equations in finite differences, are merely different representations of a part of some more general method or process.

The principal difference in results, so far as concerns the solution in series of linear differential equations, appears to be, that in this paper the law of relation of the

coefficients of each series is distinct, and substantially the same in form for all; and that it is not necessary to have recourse to the method of parameters in the cases of equal roots, or in the case of there being a term G on the right-hand side of the equation. In the case of imaginary roots, the laws of the series are in the first instance distinct from each other, and afterwards combined in couples.

19. The investigations contained in the latter part of this paper reduce the problem of the integration in finite terms of the general linear differential equation with rational coefficients, to the finding of an algebraical expression representing the development

$$A_0 + A_1x + A_2x^2 + \dots + A_mx^m + \dots,$$

where $A_0=1$, and the law of relation is

$$f_0mA_m + f_1mA_{m-1} + f_2mA_{m-2} + \dots + f_rmA_{m-r} = 0;$$

the functions $f_0, f_1, f_2, \dots, f_r$ being known specific functions. The series to be summed closely resembles a recurring series; it differs from it in this particular, that the law of relation, instead of being constant, has a uniform and simple variation as it progresses along the series. If the rational coefficients should be themselves infinite series, the process still applies, the only difference being that each term would be formed from all the preceding terms, instead of being formed from the r immediately preceding terms, or from all the preceding terms when the number of them is less than r .

In those cases in which the equation is soluble in finite terms by known methods, we are enabled to assign the algebraical expression for the series; a result which may be used for the discovery of generating functions.

Thus, taking the general equation of the first order,

$$(a_1 + b_1x + c_1x^2 + \dots + l_1x^n) \frac{du}{dx} + (a_0 + b_0x + c_0x^2 + \dots + k_0x^{n-1})u = 0,$$

we see that

$$\int_0^u \frac{a_0 + b_0x + \dots + k_0x^{n-1}}{a_1 + b_1x + \dots + l_1x^n} dx = A_0 + A_1x + A_2x^2 + \dots + A_mx^m + \dots,$$

where $A_0=1$, and the general law of the series is

$$a_1mA_m + (b_1(m-1) + a_0)A_{m-1} + (c_1(m-2) + b_0)A_{m-2} + \dots + (l_1(m-n) + k_0)A_{m-n} = 0.$$

If we take the general equation of the second order,

$$(a_2 + b_2x + \dots + l_2x^n) \frac{d^2u}{dx^2} + (a_1 + b_1x + \dots + k_1x^{n-1}) \frac{du}{dx} + (a_0 + b_0x + \dots + h_0x^{n-2})u = 0,$$

the law of the series will be

$$a_2m(m-1)A_m + (b_2(m-1)(m-2) + a_1(m-1))A_{m-1} + (c_2(m-2)(m-3) + b_1(m-2) + a_0)A_{m-2} + \dots + (l_2(m-n)(m-n-1) + k_1(m-n) + h_0)A_{m-n} = 0;$$

and generally, in equations of the n th order, the law of relation involves the n th power of m , the number of the term sought for. Thus the determination of soluble

cases of linear equations resolves itself into the inquiry, in what cases can a series whose scale is of the n th order be resolved into a number of series whose scale is of the first order.

20. I have not thought it necessary here to extend these solutions in series to linear partial differential equations. The process by which the extension can be made is well known, and has no peculiar relation to the methods here developed.

XIV. *Electro-Physiological Researches.—Eighth Series.* By Signor CARLO MATTEUCCI. Communicated by W. R. GROVE, Esq., F.R.S.

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I PROPOSED to myself in my former memoirs* to embrace under some general views the phenomena of muscular contraction, of the production of electricity in fishes, and of the relation between the electric current and nervous force. I shall now endeavour to redeem my pledge, thankful to Providence in being permitted to resume my studies, and to seek in them some alleviation of the profound grief occasioned by the recent disasters of my country.

Previously to giving an account of my recent researches, it will be advisable to recall, in a few words, the leading results which may be said to form the summary of my former studies on electro-physiology, and which have been the starting-point of my later investigations.

1. A constant development of electricity takes place in all animal tissues, and principally in the muscles. All the laws of the muscular current, which have been established by a great number of experiments, lead to the conclusion that this development of electricity is owing to the chemical actions of nutrition, and particularly to that of muscular fibre on arterial blood. This electro-physiological phenomenon is therefore simply dependent on a general physical law: we may compare a muscle in which arterial blood circulates, conveying thither the elements of nutrition, to a collection of particles of zinc surrounded by acidulated water. The two electricities separate, and are reunited instantaneously between the molecules of metal and of liquid. If we establish a circuit between muscular tissues and the parts which communicate with them, which do not suffer the same chemical action on the part of the blood, we obtain signs of an electric current the direction of which is already determined by the physical conditions of this source of electricity.

2. In each prism of the electrical organ of fishes, the two electricities are separated under the influence of nervous action propagated from the brain towards the extremities of the nerves: a relation exists between the direction and the intensity of the nervous current, and also between the position and the quantity of the two electricities developed in the prism: according to this relation, verified by experiment, if we represent (as AMPÈRE did for electro-magnetic action) the nervous current by the figure of a man extended on the nerve and looking towards the tail of the torpedo, or the dorsal surface of the gymnotus, the positive electricity of the prism

* Phil. Trans. 1847.

is found invariably to the left of the man. We may account for the position of the poles at the extremities of each prism, and the intensity of the discharge being proportionate with the length of the prisms, by the fact that each prism of the organ is a temporary electrical apparatus, as has been proved by numerous experiments.

3. It is proved by experiments that the strictest analogy exists between the discharge of electrical fishes and muscular contraction; there is not a circumstance which modifies one of these phenomena without acting equally on the other.

4. The contraction of a muscle developes in the nerve with which it is in contact, the cause by means of which the nerve excites contractions in the muscle through which it is ramified. Although it has not been possible as yet to decide by experiment whether this phenomenon ought to be considered as a case of nervous induction, or as the proof of an electric discharge produced by muscular contraction, we are compelled by analogy to adopt the latter hypothesis.

5. The electric current modifies the excitability of the nerve which it traverses, in a manner which differs greatly according to its direction: the electric current which is propagated in the same direction as the ramification of the nerve, destroys its excitability: the current which is propagated in a direction contrary to that of the ramification of the nerve, increases its sensibility: the phenomena excited by the cessation of the electric current which traverses the nerves of the animal, are produced by the modification which the excitability of the nerve has undergone by the passage of the current according to its direction: the voltaic alternatives are explained by the same cause, which is as follows:—the muscular contractions are excited by a current which is made to pass in a contrary direction to that in which its action is nullified.

After having thus briefly recapitulated the fundamental conclusions which I have scrupulously drawn from my lengthened researches in electro-physiology, I shall begin the exposition of my recent studies on this subject by describing some experiments the application of which I shall give in the sequel. I wished to satisfy myself whether the nervous filaments which conduct an electric current into a liquid, are capable, like metallic wires, of acting as electrodes and giving rise to the production of electro-chemical decomposition. In order to ascertain this point, I plunged in a solution of iodide of potassium two large nervous filaments taken from living animals, each of which was separately attached to the metallic extremities of a pile of fifteen couples. I did not obtain the slightest trace of electro-chemical decomposition. I concluded from this experiment that terminals formed of nervous filaments cannot serve to obtain electro-chemical decomposition, which appears to me to demonstrate that the conductivity of nervous matter is due to the liquid part of the matter itself.

I studied again the relative conductivity of the muscles to that of the nerves: I had already discovered that one might estimate the conductivity of the muscle as four times greater than that of the nervous substance. I had also found that a

hempen thread soaked in water, and a very fresh nervous filament, both being as nearly as possible of equal dimensions, presented resistances to the electric current in the proportion of 12 to 15. But the main object which I had in view in studying the relative conducting power of muscles and nerves, was to ascertain whether, when a current was impelled through a mass of muscle, any part of the current might have passed through the nervous filaments spread throughout that muscle. For this purpose I took a piece of muscle from the thigh of a rabbit, a dog, or a fowl which had been dead for sufficient time for muscular irritability to have ceased; I cut this mass of muscle with a knife, and introduced into the opening thus made the nerve of a highly sensitive galvanoscopic frog; I covered over the nerve and a part of the leg, endeavouring to envelope them perfectly in the piece of muscle. When the contractions, which often occur in the galvanoscopic frog itself immediately after it has been prepared, had ceased, I passed an electric current of from 25 to 30 elements of FARADAY through the mass of muscle, applying the poles to different parts of its surface. In whatever way I varied this experiment, provided I did not touch very close to the nerves of the galvanoscopic frog, this frog never entered into contraction, although a very powerful electrical current traverses in all directions the mass of muscle in which the nerve of the frog was contained. I have remarked already that in this experiment a mass of muscle must be employed in which all irritability is extinct, because if muscles are made use of which contract on the passage of the electric current, the galvanoscopic frog exhibits phenomena of *induced* contraction; we are convinced of this by observing that they cease when the muscular irritability has disappeared. We may therefore conclude from this experiment, that when the poles of a pile of 25 or 30 elements are applied to the surface of the muscles of a living animal, the phenomena produced by the passage of the current must depend either on the *direct* action of the current on the muscular fibre, or on the *indirect* action or *influence* of the electric current transmitted by the muscular fibre on its own nervous filaments, or, to express it more clearly, on the nervous force existing in these filaments.

Convinced of the great importance of this conclusion, I have varied these experiments in order to confirm them in different ways. I have already, in the commencement of this memoir, spoken of the difference of excitability produced in a nerve by an electric current according to its direction. This electro-physiological law is easily demonstrated by an experiment which I have detailed at some length in my memoirs*, and which is made by preparing the frog in the ordinary manner,—cutting it at the junction of the two thigh-bones, and then placing it astride between two glasses of water with one foot in one glass and one in another. When the two poles of the pile are plunged into the liquid (pure water) contained in the two glasses, it is obvious that one of the limbs is traversed by the electric current in the same direction as the ramification of the nerve, and the other in a contrary direction.

* Philosophical Transactions, 1846.

The experiment consists in this: that after fifteen or twenty minutes of the passage of the current, that limb only which is traversed by the inverse current, that is to say, in a direction contrary to the ramification of the nerve, contracts at the opening of the circuit.

When the frog is reduced to this state, if the nerve traversed by the inverse current is touched by a small portion of muscle, we see the limb instantly contract as if the circuit had been interrupted; and in fact the current ceases to pass through the nerve and enters the muscle on account of the greater conductivity of the latter substance.

I shall cite another experiment of the same kind. Having succeeded in modifying the excitability of the nerve by the passage of the current, as in the experiment just described, we can easily convince ourselves that this modification is confined to the exposed or isolated portion of the nerves. For if those portions of the nerves in both thighs, which had been previously buried in the muscles, be now laid bare, no alteration in their excitability is found to have occurred; but the altered excitability is limited to the pelvic portions of the nerves previously exposed and traversed by the current. It is evident that if the nerve, buried among the muscles of the thigh, were traversed by the current as its exposed portion above is, the modification of the excitability would extend through the whole length of the nerve. I repeat, therefore, once again, that when a muscular mass is traversed by an electric current, we are compelled to admit that no sensible quantity of that current is conducted by the nervous filament belonging to such muscle.

Another subject of research which has greatly interested me, and the exposition of which will precede that of the experiments which form the principal subject of this memoir, relates to the influence which the integrity of the nervous system exercises on the excitability of the nerves. In other words, supposing that a certain contraction is produced in the limb of a frog by the passage of a constant electric current through its lumbar nerves, would that contraction remain the same, or would it be increased or diminished were the spinal marrow to be cut? I had read in the 'Comptes Rendus' of the Academy of Paris an experiment by M. BOIS SEGUARD, from which one would be led to conclude that the section of the spinal marrow increased the excitability of the lumbar nerve, at least during a certain period of time.

In order completely to satisfy myself on this point, which I consider as very important with regard to the theory of nervous functions, I was obliged to operate with the greatest possible exactitude, and to measure in every instance the contractions excited. In order to this I employed an apparatus invented by M. BREGUET, the description of which I have given in my fourth memoir*. The results to be obtained from this apparatus are as exact as can be desired, provided the operation is carried on with sufficient patience, and that the necessary degree of practice has been acquired in the use of the machine. As it is necessary, in the first instance, that the current

* Philosophical Transactions, 1846.

should be constant, a WHEATSTONE's pile is the best adapted for the experiment. As the nervous filament by which the current is transmitted must be perfectly free from all trace of blood or other animal substance, it must be carefully cleaned, using a pair of forceps and lightly wiped with a piece of blotting-paper. It is also requisite that the gilded steel needle, which is introduced into the muscle of the thigh as nearly as possible to the insertion of the nerve, and which serves to complete the circuit, should, so far as is practicable, be always inserted in the same position in all the experiments. Lastly, care must be taken in choosing suitably the weight attached to the limb of the frog: if the weight be too small, the limb is not brought back to its position after contraction; on the other hand, if the weight be too great, the nerve is stretched, and the indications given by the apparatus are too small. I found that a weight of 0.600 gr. is the best adapted for the purpose.

I here give one of a series of experiments, undertaken with the view of resolving the question which I had proposed to myself.

I fasten a living frog to the clamp of the apparatus by passing a thread of silk round the thorax under the front claws: I then remove all the viscera of the abdomen, one of the limbs, and the muscles, as well as the bones of the pelvis: the frog, thus prepared, retains its natural liveliness for at least twenty or thirty minutes. I proceed to pass the *direct* current through the lumbar nerve, and I obtain contractions measured by the apparatus at 14° , 12° , 10° , 9° , 8° . I continue the passage of the current as short a time as possible, and close the circuits instantly after having opened them. The contractions first of all diminish rapidly, and then for a certain time they remain the same, if we are careful to leave the circuits closed as short a time as possible. When the deviation of the needle points constantly at 8° , I cut the spinal marrow of the frog, and pass the current again immediately: I have again the contraction 8° as before. I have frequently repeated this same experiment, and invariably with the same result. I also found that the duration of the contractions which persist the longest does not vary, in consequence of the section of the spinal marrow. It is therefore certain that the excitability of the nerve undergoes no immediate change after its separation from the nervous centre.

I then continued my investigations of this subject, conducting my experiments in a different manner from that already described, that is, by comparing the contraction of the muscular fibre excited by the passage of the electric current in a nerve which had been separated for several hours from the nervous centre, to that of another similar muscle the nerve of which had not undergone this operation.

It is hardly necessary to repeat, that, in order to the success of these experiments, BREGUET's apparatus must be used, employing the precautions already mentioned; the most indispensable of which is the careful removal of all traces of blood, &c. from the lumbar nerves of frogs submitted to these comparative experiments; they should also, as far as possible, be prepared in the same manner, so that their conditions should be similar.

I took the frogs as they came to hand, from among a considerable number, and divided the spinal marrow at about the middle. Twelve hours after this operation I began an experiment, preparing at the same time the frogs whose spinal marrow had been divided, and others which were intact.

In order that the comparison should be as perfect as possible, I employed successively in BREGUET'S apparatus for measuring contractions, two frogs, one of which had the spine divided, the other entire. In each experiment I operated on four frogs in the former, and four in the latter state. I made my experiments with a direct current, keeping the circuit closed as short a time as possible, and marking the variations of the needle of the dynamometer after ten or twelve passages of the current, because it is then only that the variations become constant; if the passage of the current in the same frog is prolonged, the variations gradually diminish; operating with a certain degree of dexterity, experiments on eight frogs may be completed in less than a quarter of an hour.

The following are the results of two experiments :—

Exp. 1.

Contraction of a frog the spinal marrow of which had been divided 12 hours.	Contraction of a frog with the spinal marrow entire.
16° to 14°	14°
18 20	18
18 16	12
20	12
20	12

Exp. 2.

After 18 hours.

24° to 22°	8°
22 24	10
22 16	12
17 16	12

According to these results, and others of the same kind, which it is unnecessary to give here, it is clearly proved by experiment that the contraction excited in the muscles of a frog, of which the spinal marrow has been divided from twelve to eighteen hours, is stronger than that which is obtained under the same circumstances from the muscle of a frog immediately after it is killed, without having been previously subjected to any alteration in its nervous system.

An observation which I had occasion to make in all my experiments, gave me some insight into the cause of this singular phenomenon. If a vigorous frog is rapidly prepared and subjected to experiment in the dynamometer, the first con-

tractions which are obtained are very feeble, particularly if the weight attached to the limb, to bring it back to its position after the contractions have ceased, be extremely small. If we continue to operate on this same frog, we see the contractions become stronger and increase during a certain time. In all cases the muscles of frogs killed and prepared rapidly, are stiff, and seized with a kind of tetanic contraction; while the muscles are in this state, the contractions which are obtained by the passage of the electric current are necessarily less strong than those which are developed when these same muscles have ceased to be contracted naturally.

After having repeated some experiments of this kind with the dynamometer, I soon came to the conviction that this was the real cause of the phenomenon under observation. I shall only give one of these experiments, which proves the truth of this explanation.

I operated on very vigorous frogs, four of which had had the spinal marrow divided twenty-four hours before experiment, and four other similar frogs which were intact.

The following are the numbers found immediately after the preparation :—

Contraction of the frog of which the spinal marrow had been divided for 24 hours.	Contraction of the frog in the natural state.
30°	26°
28	24
26	24
24	20

I left the frogs untouched for forty minutes, and then repeated the experiment: the following are the results :—

10°	22°
8	20
8	16
8	16

The difference of contraction obtained in the two experiments is therefore evidently due to the state of the muscles differing in the two cases, according to whether the frog was submitted to experiment immediately after death, or whether it had had the spinal marrow divided for a long time, and consequently the muscle relaxed for a considerable period. The contractions excited by the electric current in a frog immediately after death, are feeble at first, on account of the nearly tetanic condition of the muscles; whereas, if left in repose, so as to give time for the muscles to become relaxed, the contractions then excited by the current are stronger. The contrary to this occurs if the experiment be made on a frog the muscles of which have been for some time without contraction, in consequence of the spinal marrow having been

divided ; the contractions are very strong at first, but they soon diminish in strength, and that rapidly.

It would therefore be a mistake to conclude that muscular contraction increases because the nerve traversed by the electric current has been separated from the nervous centre : we have seen what is the true explanation of the phenomenon, and that this conclusion is only apparent.

On the other hand, these experiments prove, as did also those of the preceding section, that the excitability of a nerve is not altered by its separation from the nervous centres, or, rather, that the only alteration which it undergoes consists in the increased rapidity with which the excitability diminishes under the action of stimulants. Thus, the contraction produced by the passage of an electric current through a nerve is sensibly the same, whether this nerve be in communication with the nervous centres, or separated from them for several hours, or an instant after the separation has been effected : when the action of the current is repeated, its effects diminish the more rapidly exactly in proportion to the length of time which has elapsed since the separation of the nerve from the nervous centre.

I think it is scarcely necessary for me to remark, that these conclusions are not contrary to those of MÜLLER and LONGET ; according to which it was shown that when the nerves had been separated from the central mass, for weeks and months, the sensibility of these nerves and the irritability of the muscular fibre were diminished. M. LONGET has given the interpretation of this phenomenon, grounded on experiment.

In order to complete the first part of this memoir, I now proceed to relate the experiments and considerations by which the strict analogy existing between electricity and nervous force, together with the nature of that analogy, are demonstrated in a most satisfactory manner.

At the beginning of this memoir, referring to my preceding researches in electrophysiology, I mentioned the law according to which, by the influence of the nervous current, a development of electricity takes place in the organ of electrical fishes. We shall now consider what may be called the converse of this phenomenon, that is to say, the development of nervous force under the influence of the electric current, which development is produced exactly according to the same law as that of electricity by nervous force. This influence of electricity on nervous force is manifested in the muscular fibre : we have already observed at the beginning of this memoir that the phenomena presented by the organ of electrical fishes, as well as those of the muscular fibre, together with the modifications of these phenomena under various circumstances, invariably followed the same course, and preserved the strictest analogy with each other, so that what was true with regard to the electrical discharge of the torpedo, was equally so with regard to muscular contraction.

This fact admitted, we proceed to the experiments.

Expose in a living dog, rabbit or frog, the muscles of the thighs, removing entirely the skin and membranes, then transmit through the muscles the electric current

from an ordinary pile of from 30 to 40 elements, applying one of the poles to the upper, and the other to the lower part of the thigh. If the positive pole is placed above and the negative below, the current traverses the muscle in the same direction as the ramification of the nerves; the contrary is the case if the position of the poles be reversed.

Experiments of this kind date from the earliest times of galvanism; but the difficulty in these researches of separating truth from error, that which is constant and invariable from that which is merely accidental, and, above all, the still imperfect state of electro-physiological science, have hitherto prevented the attainment of any positive conclusions.

In experiments on this subject, great precaution is necessary in order to be certain that the electric current does not traverse the nervous filaments of the muscle; this certainty is obtained, as we have already seen, by keeping the poles of the electrical pile in contact with the surface of the muscle, at the greatest possible distance from the nervous filaments which spread through its interior. We must carefully remove from the surface of the muscle all traces of blood or other liquids; and I have generally been in the habit of applying one pole to the upper and the other to the under surface of the muscle.

When the electric current traverses the muscular mass of the thigh of a living animal in the same direction as the ramification of the nerves, a very strong contraction always takes place, which contraction is excited not only in the muscle directly traversed by the current, but also in the inferior muscles of the leg and in the foot.

When the electric current traverses the muscular mass in the contrary direction to that of the ramification of the nerves, the animal utters loud cries, and gives other indications of suffering severe pain, accompanied by contractions much less violent than in the preceding case, and which never extend beyond the muscles traversed by the current.

If we were to satisfy ourselves with seeing these experiments once only, or were to confine ourselves to a few trials only, we might easily be led to form an erroneous conclusion; in fact, at the beginning of the experiment, particularly if a somewhat powerful current be employed, there are both cries of pain and contractions at the same time; but this soon ceases, particularly if we know how to regulate duly the strength of the current. The constant effects which distinguish the action of the electric current, according to its direction, in the muscles of living animals, are those which I have indicated, namely, pain, when the electric current is what is commonly called inverse,—contraction, when the current is direct.

Now, setting aside all hypothesis, there can be but one way of explaining these phenomena: when there is a contraction, there must necessarily be a current of nervous force propagated from the brain towards the extremities of the nerves: when there is a sensation of pain, this current must be impelled in a contrary direction, that is,

from the nervous extremities towards the brain. We must bear in mind that it has been demonstrated by direct experiments, that when an electric current is propagated in a muscle, this current never quits the muscular fibre in order to pass into the nervous filaments. It is therefore perfectly evident that the phenomena of which we have spoken, excited by the passage of the electrical current through a mass of living muscle, are exclusively owing to the *influence* of electrical states propagated in the muscles upon the nervous force contained in the nerves. To speak with more precision, we should say thus: it is demonstrated by experiment that an electric current transmitted through a living mass of muscle in the same direction as its nervous ramification, develops a nervous current which is propagated in the direction of the ramification of the nerve, and which reaches to its extremities; if the electric current pass in a contrary direction to that of the ramification of the nerve, the nervous current which it develops follows the same direction, that is, from the extremity of the nerve towards the brain.

The great importance of the conclusions drawn from these experiments consists in this, that they lead to the same law which establishes the analogy between nervous force and the electrical discharge of fishes.

We obtain an electrical discharge in fishes, when we produce a nervous current by stimulating the nerve which communicates with the organ. In experimenting on living animals, we produce a nervous current by the electric discharge which we transmit through the muscles. When this discharge is directed through the muscle in such a way as that the positive and negative states of electricity are disposed relatively to the ramification of the nerves, as in the discharge of electrical fishes, a nervous current is produced by the influence of the electric current having the same direction in both cases; in the torpedo, the nervous current produces the electrical states; in the muscular fibre, the nervous current is produced by the influence of the electric current.

Following up the analogy, we are led to expect that when the electric current traverses a muscular mass in a contrary direction to that of the ramification of the nerve, it would produce a nervous current in an opposite direction to that which is developed by the electric current passing along the muscle in the same direction as the ramification of the nerves. This is a conclusion the truth of which is clearly demonstrated by the phenomena of sensation or of pain, which are produced by the passage of the electric current in a contrary direction to that of the ramification of the nerves.

Satisfied to go forward with a slow but sure step in the vast and very obscure field of electro-physiological science, I cannot but regard as highly important the discovery set forth at the close of this memoir, of the strict correlation existing between the electric current and nervous force.

Pisa, June 1849.

PHILOSOPHICAL TRANSACTIONS.

XV. *Discussion of Meteorological Observations taken in India, at various heights, embracing those at Dodabetta on the Neelgherry Mountains, at 8640 feet above the level of the sea. By Lieutenant-Colonel W. H. SYKES, F.R.S.*

Received January 12,—Read March 21, 1850.

IN the year 1835 the Royal Society did me the honour to publish the results of six years' meteorological observations, taken by me on the elevated plateau of the Dukhun (Deccan) from 1825 to 1830, both years inclusive; my chief object being to illustrate the diurnal oscillations of the barometer as indicative of the periodic tides (if they may be so called) of the atmosphere. I showed that in many thousand observations, taken personally, there was not a solitary instance in which the barometer was not higher at 9—10 A.M. than at sunrise; and lower at 4—5 P.M. than at 9—10 A.M., *whatever the state of the weather or the indications of the thermometer or hygrometer might be*; nor was there a solitary instance in 1830, during which year only I took systematic night observations, and observed at the turning-points every five minutes, in which the maximum nocturnal tide was not higher at 9—10 P.M. than at 4—5 P.M. The 4—5 A.M. minimum tide was less regularly noticed than the other three tides; but nevertheless sufficiently often to render it perfectly clear, that in the twenty-four hours there were two minima as well as two maxima of pressure in twenty-four hours. I had the advantage at times of carrying on my observations in the six years, at a mean elevation of 1800 feet, simultaneously with observations made in Bombay at the level of the sea, and at Mahabuleshwur at 4500 feet above the sea. These simultaneous observations for limited periods led me to remark, that the amount or range of the diurnal oscillation between the maximum and minimum hours did not correspond at the different elevations. At the level of the sea in Bombay the range between 9—10 A.M. and 4—5 P.M., appeared generally to be less than the range between the same hours in Dukhun (Deccan) at 1800 feet above the sea; but at Mahabuleshwur, at 4500 feet, the range was *constantly* less than in Bombay or on the plateau of the Deccan. Not having had further means of prosecuting inquiry into the fact, it was with much interest I remarked that the orders of the Court of Directors of the East India Company had been carried out by the astronomer at Madras, and meteorological observations taken for a whole year at a

greater elevation above the sea-level, than had ever been attempted before in India. Observations so made were peculiarly acceptable to me, as they would subject the accuracy of my own observations to an additional test, and would supply the means of investigating the supposed diminished diurnal oscillation of the barometer in relation to elevation above the sea. At the present time I have the further advantage of being enabled to subject my Deccan observations to the test of comparison with recent observations at the Bombay and Madras Observatories, taken chiefly with a view to determine the amount of the hourly and diurnal changes of the barometer. Those for Madras are from the years 1841 to 1845, both inclusive*: for Bombay, I am sorry to say, the records which have reached my hands are for more limited periods. Dr. BUIST, LL.D., while in temporary charge of the observatory, made observations for 1842, 1843 and 1844, but those for 1843 and 1844 only are available to me, and Professor ORLEBAR has published observations from April to December 1845. In addition to the above, a meteorological register is kept in the office of the Deputy Surveyor-General in Calcutta, the instruments since 1844 being new, and of NEWMAN's construction; but hourly observations do not appear to have been taken, and three out of the five periods of observation, namely, sunrise, noon and sunset, are useless for determining the diurnal oscillation of the barometer; and the barometrical observations taken before the receipt of the new instruments, which came to hand respectively in 1844 and 1847, were recorded from defective instruments.

TROUGHTON's barometer reduced to 32° stood at	29·493
Colonel EVEREST's reduced to 32° stood at	29·637
While NEWMAN's barometer of Sept. 1844 stood at	29·654
And that of April 1847 at	29·667

The height of the cistern above the level of the sea being 18·21 feet. Observations are also taken at the Royal Observatory at Oude, and at the Rajah of Travancore's Observatory at Trevandrum; but the records have not reached my hands, nor are those of the magnetic observatory at Simla available to me.

Independently of the diurnal atmospheric tides, the Dodabetta observations afford the means of further investigating the debatable question of the influence of elevation upon the quantity of rain precipitated; for these observations, contrasted with the records from Mahabuleshwur, Mercara in Coorg, and from the numerous stations in Travancore furnished by General CULLEN (all the localities being under nearly the same meridian, although differing greatly in latitude), will supply data, which, if they do not set the question at rest, will assist to guide the judgement to deductions approximating to the truth.

Diurnal Tides.—I shall first notice the observations taken at the greatest elevation. The Dodabetta observations commenced in February 1847. The locality of the observatory, which is in latitude 11° 23' 22" N., and longitude approximately 76° 47' E. of Greenwich, is understood to be the highest point in the range of Ghauts in Western

* At Madras, from the years 1822 to 1837, the barometric records are useless for determining the daily atmospheric tides, the hours of observation being sunrise,—10 A.M., noon,—2 P.M., and sunset,—10 P.M. was added in some of the latter years. Hourly observations only commenced in 1841.

India, that run parallel to the sea-coast from latitude 24° — 25° to Cape Comorin. This range of mountains rise abruptly above the western coast of India, and the watershed from them is almost entirely to the Coromandel or eastern coast of the Peninsula of India. The aqueous vapours from the ocean are condensed on these mountains, and as Dodabetta is within the influence of both the south-west and north-east monsoons, it would appear to be favoured with rain in every month in the year. The observations were taken by an assistant of the Madras Observatory, JOHN DE CRUZ; and as the instruments had been selected by the astronomer, Mr. TAYLOR, and every precaution taken to ensure accuracy in the observations, there is every reason to suppose they are worthy of confidence. To determine the pressure of the atmosphere, observations were taken only twice in the day at the supposed hours of maxima and minima; namely, $9^h 40^m$ A.M. and $3^h 40^m$ P.M., excepting on the 21st and 22nd of each month, when the observations were taken hourly for twenty-four consecutive hours.

With respect to the daily oscillation of the barometer the following Tables are given, and the records show, that, as in my observations in the Deccan, there is not at this great elevation *a single day throughout the whole year* in which the pressure at $3^h 40^m$ P.M. is higher than at $9^h 40^m$ A.M., and there is but a solitary instance in which the pressure at the two hours is identical. On the 15th of August the barometer is recorded as standing at $21^{\circ}952$ at $9^h 40^m$ A.M., and at $21^{\circ}952$ at $3^h 40^m$ P.M.; but as there is not an approximation to anything similar in the preceding or following days, it may be questioned whether this supposed suspension of the atmospheric tide may not be a typographical error, that might well be excused in a multitude of figures. As these observations taken at such a height are the first of the kind recorded in India, I have thought it right to append them *in extenso*; at least for pressure and temperature, and wet bulb depression*.

* While this paper is passing through the press, Lieut. R. STRACHEY of the Bengal Engineers is arrived in England from his travels in the Himalayas and Thibet. He made some hourly observations of the barometer at an elevation of 18,400, at 16,000, and at 11,500 feet, and found that the horary oscillations or atmospheric tides were as regular as on the plains of Hindoostan, and the hours of maxima and minima were the same.

Meteorological observations made by Lieut. R. STRACHEY on Lunjar Mountain in Thibet (lat. $31^{\circ} 2'$ north) at an elevation of 18,400 feet above the sea, August 22nd and 23rd, 1849.

Date.	Hour.	Bar. at 32° .	Air.	Wet bulb.		Date.	Hour.	Bar. at 32° .	Air.	Wet bulb.	
	P.M. h m	in.					P.M. h m	in.			
Aug. 22.	2 0	15.374	45.0	37.7	Moderate wind and a few light clouds.	Aug. 23.	4 0	15.381	29.0	24.3	Calm, clear.
	4 30	.355	48.7	37.8	Moderate wind and a few light clouds.		5 0	.389	29.0	24.5	Calm, clear.
	5 0	.355	42.7	36.2	Moderate wind and a few light clouds.		6 0	.393	28.0	26.5	Sunrise.
	6 0	.362	37.7	35.5	Moderate wind and a few light clouds.		7 0	.397	33.0	32.8	Calm and clear.
	7 0	.369	31.5	29.9	Wind light.		8 0	.405	36.0	35.8	Calm and clear.
	8 0	.396	34.0	29.5	Wind light.		9 0	.420	39.8	31.75	Calm and clear.
	9 0	.410	33.6	28.9	Calm, clear.		10 0	.424	44.9	35.5	Light winds, S.E., few clouds.
	10 0	.412	33.0	28.5	Calm, clear.		11 0	.421	56.2	46.5	Wind increasing, also clouds.
	11 0	.414	32.0	26.7	Calm, clear.		noon.	.415	56.0	40.2	
	midnight.	.394	31.7	27.7	Calm, clear.		1 0	.407	56.9	40.9	
Aug. 23.	1 15	.399	31.0	25.25	Calm, clear.		1 30	.404			
	2 0	.395	30.7	27.4	Calm, clear.		2 0	.397	49.1	37.15	
	3 0	.383	29.7	27.6	Calm, clear.		3 0	.387	46.5	36.5	Strong wind and more cloudy.

Daily Oscillations of the Barometer at Dodabetta, at 8640 feet above the level of the sea, &c.

FEBRUARY.										MARCH.										APRIL.									
Date.	h m 9 40 A.M.		h m 3 40 P. M.		Rain.	Date.	h m 9 40 A.M.		h m 3 40 P.M.		Rain.	Date.	h m 9 40 A.M.		h m 3 40 P.M.		Rain.	Depression of the wet bulb.											
	Baro- meter.	Att.	Baro- meter.	Att.			Baro- meter.	Att.	Baro- meter.	Att.			Baro- meter.	Att.	Baro- meter.	Att.		Baro- meter.	Att.	Baro- meter.	Att.	Baro- meter.	Att.	Baro- meter.	Att.				
1847.	in.		in.		in.	1847.	in.		in.		in.	1847.	in.		in.		in.	h m 9 40 A.M.	h m 3 40 P.M.	h m 9 40 A.M.	h m 3 40 P.M.	h m 9 40 A.M.	h m 3 40 P.M.						
1	22-105 52.5	54.0	22-046 51.0	52.0	—	1	22-166 52.0	53.0	22-072 51.0	52.0	—	1	22-180 58.0	59.0	22-106 58.0	58.5	—	10.5	1.5	6.0	6.0	—	2.0						
2	115 53.0	54.0	062 50.0	52.0	—	2	182 54.0	54.0	130 52.0	53.0	—	2	177 59.5	61.5	080 58.5	60.0	—	2.0	5.0	3.0	3.0	—	7.5						
3	114 53.0	53.8	068 53.0	52.9	0.02	3	186 53.0	53.4	122 52.8	53.0	2.29	3	172 59.0	60.5	115 58.0	59.0	—	1.8	1.5	0.9	1.0	—	4.5						
4	124 51.8	52.5	060 53.0	52.8	0.31	4	168 50.0	50.5	116 50.0	51.0	0.03	4	186 58.0	59.0	—	—	—	0.5	0.8	4.5	2.0	—	7.0						
5	128 50.0	51.0	072 52.9	52.5	0.03	5	180 50.4	50.8	124 50.5	51.5	—	5	199 57.5	58.5	164 58.0	58.0	—	2.0	1.1	4.3	2.1	—	4.0						
6	140 48.8	50.0	086 52.2	50.0	0.02	6	174 50.0	53.0	126 50.8	51.8	—	6	188 56.0	57.0	150 60.0	59.0	—	3.5	2.2	6.0	3.3	—	7.0						
7	136 47.8	48.0	085 51.0	51.0	—	7	166 50.0	52.0	130 50.0	51.0	—	7	172 56.0	57.5	119 57.0	57.4	—	6.5	3.5	4.0	2.0	—	6.5						
8	138 51.4	53.0	082 52.0	52.5	—	8	182 52.2	53.0	130 54.0	54.0	—	8	160 57.0	58.5	110 58.5	58.5	—	1.8	1.5	6.0	3.5	—	4.5						
9	144 51.5	52.5	064 52.5	52.5	0.38	9	159 50.0	51.8	098 53.0	53.0	—	9	182 54.8	56.0	106 57.0	58.0	—	0.5	1.5	3.8	1.0	—	6.5						
10	082 51.4	48.0	022 50.0	50.5	0.03	10	172 54.0	55.0	100 56.8	57.0	—	10	176 56.0	57.0	110 56.5	57.5	—	1.5	0.5	7.0	5.0	—	2.5						
11	082 50.0	51.0	014 51.0	51.4	0.05	11	153 55.0	56.0	078 53.0	54.0	—	11	136 56.5	57.0	114 56.0	57.0	—	0.2	0.4	9.0	2.0	—	6.2						
12	120 52.2	53.0	056 50.5	52.2	0.05	12	148 55.0	56.0	106 53.0	55.0	—	12	122 56.5	57.5	058 57.0	58.0	—	0.5	0.2	8.8	2.0	—	2.5						
13	154 52.4	53.0	081 52.4	53.0	1.39	13	158 55.0	56.0	105 55.0	55.0	—	13	098 55.0	56.0	038 56.0	56.0	—	0.0	0.4	7.2	6.0	—	1.0						
14	134 51.8	52.0	068 52.0	53.0	0.39	14	152 52.0	53.0	118 58.8	59.0	—	14	100 53.8	54.0	026 55.0	55.0	—	0.0	0.5	4.5	4.5	—	0.5						
15	142 53.0	54.0	073 53.0	53.4	0.69	15	150 51.8	53.0	088 53.0	53.5	0.77	15	036 52.5	53.5	21.983 51.8	51.5	—	0.4	0.4	1.0	1.5	—	1.0						
16	138 51.4	55.0	072 54.5	54.5	3.24	16	176 51.0	52.0	147 53.0	53.0	0.16	16	21.998 51.0	51.8	912 51.0	51.5	—	0.5	0.5	0.8	0.5	—	0.5						
17	140 53.5	55.0	089 54.5	54.5	—	17	218 21.0	51.5	128 52.0	52.0	0.21	17	946 53.5	54.0	886 53.0	53.0	—	2.0	1.5	0.5	1.0	—	1.5						
18	165 52.0	54.0	099 53.0	53.5	—	18	186 54.2	55.2	084 53.0	53.0	0.25	18	22-016 53.0	53.0	952 53.5	53.5	—	4.0	0.9	2.2	2.0	—	0.3						
19	132 50.5	52.5	066 52.5	53.0	—	19	160 53.0	54.0	123 55.0	55.0	—	19	056 53.0	53.0	22-012 53.0	53.0	—	0.7	0.5	2.2	4.2	—	0.0						
20	123 51.8	51.8	068 53.8	53.0	0.03	20	186 53.0	54.8	123 55.0	55.0	—	20	088 52.0	53.0	056 54.0	54.0	—	0.6	0.5	6.2	4.0	—	0.5						
21	139 49.8	50.5	079 51.8	52.0	0.77	21	168 51.0	57.0	104 55.0	56.0	—	21	126 52.0	52.0	078 57.0	57.0	—	1.3	0.8	6.0	7.0	—	1.0						
22	132 50.5	51.0	080 51.3	52.0	—	22	140 56.0	57.0	116 56.0	56.5	—	22	128 52.8	53.0	072 54.0	55.0	—	0.5	0.5	10.0	10.0	—	7.0						
23	128 51.0	52.0	066 52.5	53.0	—	23	131 58.0	58.0	076 55.0	56.8	—	23	128 54.8	55.0	070 55.0	56.5	—	0.0	0.0	8.0	9.5	—	5.0						
24	134 51.8	54.0	076 52.0	53.0	—	24	146 57.0	55.0	086 58.5	58.0	—	24	128 54.8	55.5	070 55.0	56.5	—	0.5	0.6	7.0	4.8	—	1.5						
25	116 51.0	52.0	080 52.0	53.0	—	25	132 57.0	58.0	089 57.0	57.5	—	25	142 58.0	58.0	100 57.0	58.0	—	4.0	1.0	5.0	6.0	—	2.0						
26	134 51.0	52.0	080 52.0	52.0	—	26	132 57.0	58.0	089 57.0	57.5	—	26	170 56.5	57.0	100 57.0	58.0	—	3.0	1.5	6.0	5.0	—	3.0						
27	110 50.0	51.0	062 51.0	52.4	—	27	138 56.0	57.0	061 56.0	57.5	—	27	164 57.0	58.0	090 58.0	59.0	—	2.0	2.4	7.2	7.7	—	3.0						
28	128 48.5	49.5	084 50.0	51.0	—	28	140 56.0	57.0	080 56.0	57.0	—	28	172 58.0	59.0	093 57.0	58.5	—	2.0	1.2	4.0	2.0	—	4.0						
29					—	29	136 56.0	57.0	080 56.0	57.0	—	29	160 55.4	56.5	078 58.5	57.0	—					—	1.5						
30					—	30	184 57.0	58.0	120 57.5	58.8	—	30	160 58.0	58.0	088 58.0	58.5	—					—	3.0						
31					—	31	190 57.0	59.0	117 57.5	58.0	—	31	160 58.0	58.0	088 58.0	58.5	—					—	2.0						
Mean	22-129 51.3	52.1	22-068 52.1	52.5	7.43	Mean	22-160 53.5	54.4	22-105 54.2	54.6	3.61	Mean	22-128 55.5	56.3	22-066 56.1	56.6	19.80	1.9	1.1	5.0	3.5	3.6	2.5						

TABLE (Continued).

MAY.										JUNE.										JULY.									
Date.		h m 9 40 A.M.		h m 3 40 P.M.		Date.		h m 9 40 A.M.		h m 3 40 P.M.		Date.		h m 9 40 A.M.		h m 3 40 P.M.		Date.		h m 9 40 A.M.		h m 3 40 P.M.		Date.		h m 9 40 A.M.		h m 3 40 P.M.	
1847.		Baro- meter.	Att.	Det.	Rain.	1847.		Baro- meter.	Att.	Det.	Rain.	1847.		Baro- meter.	Att.	Det.	Rain.	1847.		Baro- meter.	Att.	Det.	Rain.	1847.		Baro- meter.	Att.	Det.	Rain.
1	22-150 58.5 59.0	22-072 57.0 56.5	in.	—	—	1	22-016 52.8 53.0	21-964 53.5 52.8	in.	—	—	1	22-026 51.2 51.2	21-942 53.0 52.0	in.	—	—	1	22-026 51.2 51.2	21-942 53.0 52.0	in.	—	—	1	22-026 51.2 51.2	21-942 53.0 52.0	in.	—	—
2	134 56.5 57.0	062 57.4 58.0	—	—	—	2	038 51.2 51.5	22-002 52.8 52.0	0-10	—	—	2	005 51.2 51.6	946 52.8 53.1	0-13	—	—	2	005 51.2 51.6	946 52.8 53.1	0-13	—	—	2	005 51.2 51.6	946 52.8 53.1	0-13	—	—
3	140 58.0 59.0	084 59.0 59.0	—	—	—	3	042 51.4 52.0	016 52.5 52.0	—	—	—	3	21-976 51.6 51.9	930 52.6 53.0	0-15	—	—	3	21-976 51.6 51.9	930 52.6 53.0	0-15	—	—	3	21-976 51.6 51.9	930 52.6 53.0	0-15	—	—
4	135 57.5 58.8	080 57.5 58.0	0-01	—	—	4	069 52.8 53.0	008 53.8 54.0	0-02	—	—	4	22-010 51.9 52.2	965 53.0 53.2	0-35	—	—	4	22-010 51.9 52.2	965 53.0 53.2	0-35	—	—	4	22-010 51.9 52.2	965 53.0 53.2	0-35	—	—
5	138 57.2 58.0	052 56.0 55.4	—	—	—	5	062 54.8 55.0	21-978 52.0 52.2	0-40	—	—	5	016 51.8 51.9	946 51.0 51.0	0-29	—	—	5	016 51.8 51.9	946 51.0 51.0	0-29	—	—	5	016 51.8 51.9	946 51.0 51.0	0-29	—	—
6	154 57.0 58.0	078 57.0 57.0	1-26	—	—	6	026 52.5 52.5	999 55.5 55.5	0-19	—	—	6	029 51.8 52.2	984 52.9 52.9	0-21	—	—	6	029 51.8 52.2	984 52.9 52.9	0-21	—	—	6	029 51.8 52.2	984 52.9 52.9	0-21	—	—
7	154 57.5 58.0	093 57.2 58.0	—	—	—	7	068 52.4 52.6	22-008 53.2 53.8	—	—	—	7	024 52.4 52.6	990 51.0 50.8	0-12	—	—	7	024 52.4 52.6	990 51.0 50.8	0-12	—	—	7	024 52.4 52.6	990 51.0 50.8	0-12	—	—
8	161 56.5 57.5	096 58.5 59.0	0-06	—	—	8	067 52.0 52.2	008 53.6 53.9	0-04	—	—	8	030 52.8 52.9	968 53.0 53.0	0-16	—	—	8	030 52.8 52.9	968 53.0 53.0	0-16	—	—	8	030 52.8 52.9	968 53.0 53.0	0-16	—	—
9	140 57.5 58.5	082 58.2 58.0	—	—	—	9	056 53.2 53.6	21-974 52.4 52.8	0-03	—	—	9	022 51.0 51.2	960 52.0 52.4	0-02	—	—	9	022 51.0 51.2	960 52.0 52.4	0-02	—	—	9	022 51.0 51.2	960 52.0 52.4	0-02	—	—
10	142 58.0 58.0	048 58.0 58.0	—	—	—	10	21-984 50.5 50.8	968 52.5 53.0	—	—	—	10	21-990 51.0 51.4	946 53.2 53.5	0-02	—	—	10	21-990 51.0 51.4	946 53.2 53.5	0-02	—	—	10	21-990 51.0 51.4	946 53.2 53.5	0-02	—	—
11	118 58.5 59.4	074 57.4 58.0	—	—	—	11	962 51.6 51.9	918 51.5 51.8	0-13	—	—	11	032 52.0 52.2	958 52.8 53.2	0-24	—	—	11	032 52.0 52.2	958 52.8 53.2	0-24	—	—	11	032 52.0 52.2	958 52.8 53.2	0-24	—	—
12	114 58.0 59.0	053 58.1 59.0	—	—	—	12	940 49.8 50.0	884 50.2 50.3	0-08	—	—	12	22-020 51.5 51.8	980 54.2 54.5	0-04	—	—	12	22-020 51.5 51.8	980 54.2 54.5	0-04	—	—	12	22-020 51.5 51.8	980 54.2 54.5	0-04	—	—
13	138 57.4 58.4	082 57.0 58.4	0-64	—	—	13	963 50.8 51.4	930 53.8 54.2	0-56	—	—	13	032 52.0 52.2	942 52.4 52.5	0-53	—	—	13	032 52.0 52.2	942 52.4 52.5	0-53	—	—	13	032 52.0 52.2	942 52.4 52.5	0-53	—	—
14	112 57.5 58.0	062 57.0 57.4	—	—	—	14	22-004 51.8 52.2	956 53.4 53.8	—	—	—	14	21-992 51.8 52.0	934 53.6 53.8	0-17	—	—	14	21-992 51.8 52.0	934 53.6 53.8	0-17	—	—	14	21-992 51.8 52.0	934 53.6 53.8	0-17	—	—
15	126 57.8 58.2	052 57.5 57.9	—	—	—	15	063 52.8 53.2	961 51.2 51.6	0-05	—	—	15	090 52.0 52.2	957 52.5 53.0	—	—	—	15	090 52.0 52.2	957 52.5 53.0	—	—	—	15	090 52.0 52.2	957 52.5 53.0	—	—	—
16	102 58.0 58.4	030 58.8 59.2	0-05	—	—	16	068 50.5 50.9	924 51.3 51.5	0-04	—	—	16	092 51.8 52.0	970 53.2 53.4	0-08	—	—	16	092 51.8 52.0	970 53.2 53.4	0-08	—	—	16	092 51.8 52.0	970 53.2 53.4	0-08	—	—
17	090 58.0 58.5	040 57.2 58.0	0-30	—	—	17	21-964 49.5 50.0	924 51.8 51.8	—	—	—	17	012 52.0 52.2	956 52.4 52.8	0-05	—	—	17	012 52.0 52.2	956 52.4 52.8	0-05	—	—	17	012 52.0 52.2	956 52.4 52.8	0-05	—	—
18	096 57.0 57.0	044 57.5 58.0	0-02	—	—	18	986 52.0 52.8	940 52.3 52.8	0-43	—	—	18	016 51.0 51.0	972 53.4 53.9	0-03	—	—	18	016 51.0 51.0	972 53.4 53.9	0-03	—	—	18	016 51.0 51.0	972 53.4 53.9	0-03	—	—
19	102 59.4 60.0	024 58.8 58.5	0-12	—	—	19	996 52.2 52.8	940 52.6 56.0	—	—	—	19	004 53.8 53.9	955 52.4 52.8	0-02	—	—	19	004 53.8 53.9	955 52.4 52.8	0-02	—	—	19	004 53.8 53.9	955 52.4 52.8	0-02	—	—
20	046 55.0 55.0	21-972 56.2 56.8	1-05	—	—	20	966 52.4 52.9	958 52.8 53.0	0-25	—	—	20	024 53.9 53.9	962 54.4 54.5	0-21	—	—	20	024 53.9 53.9	962 54.4 54.5	0-21	—	—	20	024 53.9 53.9	962 54.4 54.5	0-21	—	—
21	012 57.8 58.0	948 58.0 57.5	0-03	—	—	21	916 52.2 52.4	800 50.0 50.0	0-09	—	—	21	092 53.2 53.5	924 54.3 54.6	0-03	—	—	21	092 53.2 53.5	924 54.3 54.6	0-03	—	—	21	092 53.2 53.5	924 54.3 54.6	0-03	—	—
22	018 58.9 58.5	982 58.0 57.6	0-02	—	—	22	916 52.2 52.4	800 50.0 50.0	0-09	—	—	22	092 53.2 53.5	924 54.3 54.6	0-03	—	—	22	092 53.2 53.5	924 54.3 54.6	0-03	—	—	22	092 53.2 53.5	924 54.3 54.6	0-03	—	—
23	040 56.5 56.9	942 58.2 57.8	0-07	—	—	23	848 52.0 52.0	880 51.8 51.8	1-00	—	—	23	092 53.2 53.5	924 54.3 54.6	0-03	—	—	23	092 53.2 53.5	924 54.3 54.6	0-03	—	—	23	092 53.2 53.5	924 54.3 54.6	0-03	—	—
24	020 55.2 55.8	952 56.2 56.0	0-31	—	—	24	924 51.4 51.5	880 51.8 51.8	1-00	—	—	24	092 53.2 53.5	924 54.3 54.6	0-03	—	—	24	092 53.2 53.5	924 54.3 54.6	0-03	—	—	24	092 53.2 53.5	924 54.3 54.6	0-03	—	—
25	006 54.0 54.5	971 55.8 56.2	0-09	—	—	25	956 51.7 51.7	912 52.0 52.2	0-07	—	—	25	092 53.2 53.5	924 54.3 54.6	0-03	—	—	25	092 53.2 53.5	924 54.3 54.6	0-03	—	—	25	092 53.2 53.5	924 54.3 54.6	0-03	—	—
26	026 55.4 55.0	971 55.8 56.2	0-09	—	—	26	22-006 51.6 51.8	954 52.5 52.2	0-21	—	—	26	092 53.2 53.5	924 54.3 54.6	0-03	—	—	26	092 53.2 53.5	924 54.3 54.6	0-03	—	—	26	092 53.2 53.5	924 54.3 54.6	0-03	—	—
27	036 55.4 55.6	962 57.4 57.0	0-12	—	—	27	21-952 49.2 49.2	926 50.8 51.0	0-07	—	—	27	092 53.2 53.5	924 54.3 54.6	0-03	—	—	27	092 53.2 53.5	924 54.3 54.6	0-03	—	—	27	092 53.2 53.5	924 54.3 54.6	0-03	—	—
28	21-986 53.8 54.0	943 53.5 53.8	0-04	—	—	28	946 51.0 51.0	880 51.8 52.0	0-33	—	—	28	092 53.2 53.5	924 54.3 54.6	0-03	—	—	28	092 53.2 53.5	924 54.3 54.6	0-03	—	—	28	092 53.2 53.5	924 54.3 54.6	0-03	—	—
29	090 59.0 59.5	946 53.6 53.8	0-25	—	—	29	912 51.0 51.2	879 50.0 50.4	0-08	—	—	29	092 53.2 53.5	924 54.3 54.6	0-03	—	—	29	092 53.2 53.5	924 54.3 54.6	0-03	—	—	29	092 53.2 53.5	924 54.3 54.6	0-03	—	—
30	22-030 52.5 52.0	—	—	—	—	30	964 51.0 51.0	920 52.0 52.4	0-35	—	—	30	092 53.2 53.5	924 54.3 54.6	0-03	—	—	30	092 53.2 53.5	924 54.3 54.6	0-03	—	—	30	092 53.2 53.5	924 54.3 54.6	0-03	—	—
31	054 54.5 54.8	960 53.5 53.2	0-18	—	—	31	21-992 51.8 52.1	21-940 52.4 52.9	4-55	—	—	31	092 53.2 53.5	924 54.3 54.6	0-03	—	—	31	092 53.2 53.5	924 54.3 54.6	0-03	—	—	31	092 53.2 53.5	924 54.3 54.6	0-03	—	—
Mean	22-087 56.6 57.1	22-024 57.1 57.3	4-86	—	—	Mean	21-992 51.8 52.1	21-940 52.4 52.9	4-55	—	—	Mean	22-000 52.0 52.3	949 52.8 53.1	7-41	—	—	Mean	22-000 52.0 52.3	949 52.8 53.1	7-41	—	—	Mean	22-000 52.0 52.3	949 52.8 53.1	7-41	—	—

TABLE (Continued).

AUGUST.										SEPTEMBER.										OCTOBER.												
Date.	h m 9 40 A.M.			h m 3 40 A.M.			Rain.	Date.	h m 9 40 A.M.			h m 3 40 P.M.			Rain.	Date.	h m 9 40 A.M.			h m 3 40 P.M.			Rain.	Date.	h m 9 40 A.M.			h m 3 40 P.M.			Rain.	
	Baro- meter.	Att.	Det.	Baro- meter.	Att.	Det.			Baro- meter.	Att.	Det.	Baro- meter.	Att.	Det.			Baro- meter.	Att.	Det.	Baro- meter.	Att.	Det.			Baro- meter.	Att.	Det.	Baro- meter.	Att.	Det.		Baro- meter.
1847.	in.			in.			in.	1847.	in.			in.			in.	1847.	in.			in.			in.			in.			in.			
1	22-012	52.1	52.2	21-934	52.4	52.9	0.46	1	22-032	53.5	53.8	21-976	53.0	53.0	0.36	1	22-000	51.0	51.5	21-960	53.1	52.5	0.02	1	0.1	0.5	0.3	0.1	0.1	0.1	0.7	0.3
2	21-972	52.8	53.0	912	52.2	52.2	0.25	2	050	53.1	53.2	22-010	53.5	53.5	0.55	2	046	52.5	53.0	22-013	52.1	52.2	0.02	2	0.1	0.1	0.0	0.1	0.0	0.1	0.7	0.3
3	979	52.0	52.2	937	52.0	52.1	0.21	3	052	53.5	53.4	21-990	53.0	53.0	0.17	3	086	52.8	53.1	032	53.5	53.0	0.02	3	0.2	0.2	0.0	0.0	0.0	0.1	1.0	1.0
4	971	51.0	51.2	920	52.5	52.8	0.12	4	030	51.8	52.0	980	53.0	53.0	0.35	4	122	53.5	53.8	058	55.0	55.2	0.02	4	0.2	0.8	0.2	0.1	0.2	0.3	0.3	0.3
5	979	51.9	52.0	920	52.1	52.2	0.59	5	030	50.1	50.2	967	51.8	51.8	0.36	5	100	53.4	54.0	028	53.4	53.6	0.36	5	0.0	0.1	1.2	1.0	1.2	1.0	0.3	0.3
6	978	51.8	51.9	910	52.4	52.5	0.79	6	21-928	48.5	48.6	913	50.1	50.2	0.15	6	084	51.6	52.0	012	52.3	52.5	0.44	6	0.1	0.5	1.6	0.1	0.8	0.5	0.8	0.5
7	974	52.4	52.5	892	51.4	51.5	0.74	7	948	51.5	51.8	900	50.2	50.4	0.48	7	104	52.0	52.5	026	53.0	53.0	0.02	7	0.1	0.0	0.3	0.2	0.4	0.5	0.3	0.3
8	972	52.0	52.2	922	53.0	53.2	0.03	8	932	50.4	50.5	876	51.5	51.6	0.50	8	124	52.5	53.1	040	53.2	53.5	0.14	8	0.2	0.3	0.2	0.4	0.4	0.4	0.4	0.4
9	22-034	53.4	53.5	974	53.1	53.1	0.50	9	880	50.8	50.8	879	51.9	52.0	0.32	9	116	52.6	53.0	028	52.8	53.1	0.05	9	0.5	0.1	0.3	0.2	0.5	0.3	0.3	0.3
10	072	53.2	53.4	22-026	54.0	54.2	0.37	10	976	51.8	51.9	934	52.2	52.5	0.34	10	090	53.4	53.5	022	53.2	53.4	0.05	10	0.4	0.2	0.4	0.4	0.4	0.4	0.4	0.4
11	090	53.2	53.5	032	53.0	53.0	0.18	11	967	51.2	51.5	900	52.0	52.2	0.40	11	072	53.4	53.5	21-994	55.1	55.2	1.21	11	0.3	0.0	0.3	0.2	0.1	0.1	0.1	0.1
12	070	52.0	52.2	009	53.0	53.2	0.73	12	984	51.3	51.5	—	—	—	0.28	12	098	53.8	54.1	22-030	54.2	54.4	0.67	12	0.9	0.2	0.2	—	—	0.1	0.3	0.3
13	052	52.2	52.5	21-988	52.1	52.1	0.08	13	22-006	51.6	52.2	978	51.9	52.0	0.20	13	076	53.0	53.2	21-989	53.0	53.0	0.11	13	0.3	0.3	0.4	0.5	0.1	0.0	0.0	0.0
14	21-973	50.2	50.3	932	50.6	0.9	0.18	14	024	52.2	52.4	22-010	51.9	52.1	0.15	14	21-999	51.9	52.1	900	51.4	51.6	1.82	14	0.2	0.3	0.2	0.2	0.2	0.2	0.2	0.2
15	932	52.0	52.2	952	51.1	51.2	—	15	050	53.1	53.3	21-990	52.0	52.2	0.12	15	22-016	53.1	53.4	958	53.8	54.0	0.92	15	0.2	0.8	0.2	0.2	0.2	0.2	0.1	0.1
16	22-022	51.4	51.8	984	51.5	51.8	0.25	16	066	52.3	52.5	22-012	54.4	54.5	0.07	16	052	53.5	53.8	990	53.9	54.1	0.17	16	0.4	2.6	0.3	0.2	0.2	0.2	0.4	0.4
17	046	53.9	53.4	022	53.8	54.2	0.02	17	070	52.6	53.0	002	53.8	54.1	0.08	17	088	53.4	53.5	22-022	54.4	54.8	0.09	17	0.1	0.5	0.6	0.3	0.0	0.4	0.4	0.4
18	057	53.9	53.2	22-017	55.1	55.5	0.20	18	078	49.5	49.8	019	53.8	54.1	—	18	110	53.5	53.5	032	55.1	55.3	0.11	18	0.2	0.2	0.3	2.6	0.8	0.1	0.3	0.3
19	066	52.5	52.8	018	54.0	54.2	—	19	074	52.6	53.1	012	52.8	53.2	—	19	089	54.2	54.4	010	54.3	54.5	0.30	19	0.2	0.4	0.6	0.6	0.5	0.2	0.3	0.3
20	071	53.0	53.4	022	53.8	54.2	0.02	20	088	52.0	52.4	020	53.2	53.5	0.37	20	122	53.4	54.0	031	53.5	53.8	1.24	20	0.9	0.5	0.4	0.6	0.5	0.2	0.3	0.3
21	090	53.5	54.0	022	55.0	55.2	—	21	066	53.6	54.0	21-988	54.5	54.7	0.78	21	129	54.4	54.9	035	53.0	53.3	0.54	21	0.5	0.2	0.5	0.2	0.4	0.3	0.3	0.3
22	084	54.9	55.0	022	55.1	55.3	0.18	22	071	53.2	53.5	22-006	54.0	54.3	0.66	22	116	54.0	54.5	034	54.8	55.2	0.29	22	0.4	0.3	1.0	0.4	0.5	0.8	0.8	0.8
23	055	53.2	53.3	006	54.3	54.5	1.63	23	100	53.4	54.0	034	53.8	54.0	—	23	104	52.2	52.5	048	53.8	53.8	2.29	23	0.8	0.4	0.9	0.2	0.2	0.3	0.3	0.3
24	048	54.5	54.8	014	53.0	53.5	—	24	088	47.2	48.0	022	52.5	54.0	—	24	111	52.2	52.5	047	52.4	52.5	0.07	24	1.8	0.5	3.5	7.0	0.3	0.1	0.3	0.1
25	078	53.1	53.4	010	53.6	54.0	0.50	25	079	49.8	51.0	020	52.5	53.0	—	25	092	52.4	52.8	026	53.2	53.5	0.19	25	0.9	0.4	0.5	0.8	1.1	0.3	0.1	0.1
26	060	52.2	52.5	21-986	55.0	55.2	0.30	26	082	51.5	52.2	020	53.5	54.4	—	26	116	53.9	54.2	034	53.9	54.0	0.01	26	0.0	0.6	0.8	0.6	0.3	0.1	0.3	0.3
27	032	52.0	52.0	998	53.5	53.5	0.23	27	064	52.5	53.1	21-984	52.9	53.1	0.03	27	092	52.5	52.8	032	54.2	54.5	0.04	27	0.0	0.3	0.7	0.1	0.1	0.3	0.3	0.3
28	059	54.8	55.1	990	53.1	53.1	0.06	28	047	52.3	52.5	986	52.5	53.0	0.55	28	102	51.6	52.2	020	51.5	51.5	2.1	28	0.1	0.6	0.6	0.6	1.4	0.6	0.6	0.6
29	040	52.2	52.5	980	51.9	52.0	0.29	29	029	49.6	50.1	963	52.6	53.1	0.18	29	077	49.4	50.2	014	50.5	50.5	0.06	29	0.3	0.1	0.6	0.8	1.4	1.3	0.3	0.3
30	019	53.0	53.1	960	53.5	53.8	0.03	30	028	51.4	52.0	946	52.5	52.6	0.07	30	056	51.4	51.6	21-991	53.0	53.0	0.10	30	0.1	1.4	1.0	1.1	0.3	0.0	0.0	0.0
31	026	53.0	53.0	955	53.0	53.0	0.15	31	—	—	—	—	—	—	—	31	064	52.3	52.5	998	53.0	53.2	0.66	31	0.0	0.0	...	...	...	0.3	0.4	0.4
Mean.	22-080	52.8	52.9	21-976	53.1	53.3	9.32	Mean.	22-031	51.6	52.0	21-977	52.7	52.9	7.52	Mean.	22-086	52.8	53.2	22-014	53.2	53.4	12.49	Mean.	0.8	0.4	0.8	0.6	0.6	0.6	0.6	0.4

TABLE (Continued).

NOVEMBER.										DECEMBER.										JANUARY.										Depression of the wet bulb.											
Date.		h m 9 40 A.M.		h m 3 40 A.M.		Baro- meter.		Rain.		Date.		h m 9 40 A.M.		h m 3 40 P.M.		Baro- meter.		Rain.		Date.		h m 9 40 A.M.		h m 3 40 P.M.		Baro- meter.		Rain.		Date.		h m 9 40 A.M.		h m 3 40 P.M.		Baro- meter.		Rain.			
1847.		22-093	53-2	53-3	22-030	52-6	52-8	1-03	in.	1		22-040	53-0	54-2	21-990	47-8	47-8	0-24	in.	1848.		1		22-128	49-5	53-5	22-080	51-9	52-0	—	—	1		22-128	49-5	53-5	22-080	51-9	52-0	—	—
2		-083	51-8	52-1	-022	52-2	52-4	1-64	—	2		-036	51-5	52-1	-962	50-0	50-1	—	—	2		2		-138	47-0	49-4	-064	50-1	50-8	—	—	2		-138	47-0	49-4	-064	50-1	50-8	—	—
3		-014	52-2	52-5	21-982	52-6	52-9	1-10	0-52	3		-032	51-5	51-8	-972	51-3	51-5	0-52	0-52	3		3		-125	49-0	49-4	-066	51-2	53-0	—	—	3		-125	49-0	49-4	-066	51-2	53-0	—	—
4		-114	52-6	52-9	22-046	53-5	53-5	2-32	0-53	4		-056	53-2	54-0	22-006	51-0	52-0	0-53	0-53	4		4		-132	51-6	52-5	-070	54-2	54-4	0-03	0-03	4		-132	51-6	52-5	-070	54-2	54-4	0-03	0-03
5		-140	53-0	53-3	-060	52-6	52-8	0-75	5-15	5		-108	52-2	52-8	-056	52-3	52-5	5-15	5-15	5		5		-128	51-4	52-5	-050	52-9	53-9	—	—	5		-128	51-4	52-5	-050	52-9	53-9	—	—
6		-113	52-0	52-8	-050	53-0	53-4	1-35	0-40	6		-099	52-2	52-6	-038	52-8	52-8	0-40	0-40	6		6		-112	52-0	53-2	-035	52-4	52-9	0-02	0-02	6		-112	52-0	53-2	-035	52-4	52-9	0-02	0-02
7		-060	52-2	52-6	21-966	51-0	51-2	0-24	0-15	7		-100	48-6	50-2	-026	50-5	51-2	0-15	0-15	7		7		-092	51-1	52-4	-021	53-0	53-2	0-04	0-04	7		-092	51-1	52-4	-021	53-0	53-2	0-04	0-04
8		-064	52-6	53-0	22-010	52-2	52-6	0-12	0-02	8		-114	49-4	50-2	-010	49-2	50-4	0-02	0-02	8		8		-080	49-6	50-4	-026	52-0	52-0	0-03	0-03	8		-080	49-6	50-4	-026	52-0	52-0	0-03	0-03
9		-080	49-0	49-5	-064	50-0	50-2	—	—	9		-081	48-8	51-0	-010	49-2	49-5	—	—	9		9		-102	47-3	50-0	-040	52-3	52-3	—	—	9		-102	47-3	50-0	-040	52-3	52-3	—	—
10		-074	48-0	49-0	-035	50-2	50-5	0-03	—	10		-016	46-2	49-4	21-960	46-5	47-8	—	—	10		10		-092	49-2	50-6	-037	52-8	52-8	—	—	10		-092	49-2	50-6	-037	52-8	52-8	—	—
11		-120	48-8	50-0	-056	51-5	51-5	0-06	—	11		-004	46-8	48-5	-942	49-0	49-4	—	—	11		11		-120	52-5	54-2	-072	52-0	52-5	—	—	11		-120	52-5	54-2	-072	52-0	52-5	—	—
12		-150	51-5	51-5	-080	51-8	52-2	—	—	12		21-990	49-0	50-9	-926	50-0	50-2	—	—	12		12		-130	52-8	55-3	-066	55-0	54-2	—	—	12		-130	52-8	55-3	-066	55-0	54-2	—	—
13		-152	51-2	51-5	—	—	—	0-04	—	13		-979	50-0	50-5	-921	50-0	50-4	0-89	0-89	13		13		-099	51-5	57-0	-028	52-8	53-5	—	—	13		-099	51-5	57-0	-028	52-8	53-5	—	—
14		-136	51-4	51-8	—	—	—	0-17	—	14		-973	50-1	50-3	-928	50-2	50-3	0-96	0-96	14		14		-090	52-0	54-5	-021	56-4	56-4	—	—	14		-090	52-0	54-5	-021	56-4	56-4	—	—
15		-108	46-8	50-4	-052	48-9	49-2	0-04	—	15		-056	47-2	47-8	22-004	51-2	51-8	—	—	15		15		-074	48-6	50-0	-016	52-3	52-0	—	—	15		-074	48-6	50-0	-016	52-3	52-0	—	—
16		-068	45-2	48-5	-006	47-0	47-2	—	—	16		-065	49-9	50-6	-014	50-4	51-0	—	—	16		16		-132	53-5	56-5	-056	56-0	55-2	—	—	16		-132	53-5	56-5	-056	56-0	55-2	—	—
17		-058	47-5	49-5	-002	48-0	48-6	—	—	17		-089	51-8	52-0	-022	52-0	52-4	0-30	0-30	17		17		-119	50-8	51-0	-040	52-0	52-4	—	—	17		-119	50-8	51-0	-040	52-0	52-4	—	—
18		-104	53-0	55-8	-034	51-8	52-5	—	—	18		-112	53-0	53-5	-033	51-9	52-4	1-29	1-29	18		18		-100	50-3	51-1	-026	51-8	51-6	—	—	18		-100	50-3	51-1	-026	51-8	51-6	—	—
19		-130	52-2	53-1	-052	51-4	51-8	0-24	—	19		-120	51-5	52-0	-050	52-4	52-5	0-60	0-60	19		19		-108	49-0	49-6	-032	51-8	51-6	—	—	19		-108	49-0	49-6	-032	51-8	51-6	—	—
20		-117	51-9	52-5	-046	51-5	51-5	0-18	—	20		-120	51-8	52-1	-046	52-0	52-1	—	—	20		20		-101	49-2	50-0	-038	51-5	51-1	—	—	20		-101	49-2	50-0	-038	51-5	51-1	—	—
21		-108	49-0	51-4	-039	51-6	51-5	—	—	21		-120	51-8	52-1	-044	52-5	52-5	0-05	0-05	21		21		-120	52-8	54-0	-056	51-8	51-8	—	—	21		-120	52-8	54-0	-056	51-8	51-8	—	—
22		-136	54-0	56-1	-064	53-0	53-9	—	—	22		-110	51-6	52-1	-040	49-4	49-0	—	—	22		22		-112	49-2	51-0	-049	52-5	52-6	—	—	22		-112	49-2	51-0	-049	52-5	52-6	—	—
23		-116	52-2	53-0	-048	53-0	53-3	—	—	23		-099	48-0	51-9	-040	49-4	49-0	—	—	23		23		-090	49-0	52-1	-032	51-0	51-3	—	—	23		-090	49-0	52-1	-032	51-0	51-3	—	—
24		-106	52-0	52-9	-035	53-1	53-5	0-19	—	24		-081	50-0	51-2	-046	48-5	48-2	—	—	24		24		-102	49-0	51-8	-039	53-3	52-3	—	—	24		-102	49-0	51-8	-039	53-3	52-3	—	—
25		-102	53-8	54-0	-030	53-4	53-5	0-85	—	25		-090	47-4	51-5	-032	51-0	50-2	—	—	25		25		-102	49-0	51-8	-039	53-3	52-3	—	—	25		-102	49-0	51-8	-039	53-3	52-3	—	—
26		-109	52-2	52-5	-032	53-9	54-0	0-11	—	26		-096	48-9	51-6	-044	51-1	51-0	—	—	26		26		-100	48-8	51-0	-039	52-0	51-2	—	—	26		-100	48-8	51-0	-039	52-0	51-2	—	—
27		-098	53-4	54-0	-034	53-1	53-4	0-20	—	27		-100	49-8	50-4	-042	50-0	50-5	—	—	27		27		-120	49-0	50-8	-040	50-0	51-0	—	—	27		-120	49-0	50-8	-040	50-0	51-0	—	—
28		-094	53-5	54-0	-048	52-8	52-0	0-73	—	28		-100	50-8	51-0	-050	49-1	49-4	0-36	0-36	28		28		-134	49-8	51-5	-088	53-2	52-7	—	—	28		-134	49-8	51-5	-088	53-2	52-7	—	—
29		-100	52-0	52-5	-052	52-5	52-5	0-34	—	29		-150	48-4	50-0	-081	49-0	49-0	0-21	0-21	29		29		-163	47-5	50-0	-100	52-5	52-3	—	—	29		-163	47-5	50-0	-100	52-5	52-3	—	—
30		-106	52-0	52-4	-034	52-2	52-4	0-12	—	30		-105	48-0	49-4	-039	49-0	49-1	—	—	30		30		-132	49-5	50-2	-072	49-2	50-0	—	—	30		-132	49-5	50-2	-072	49-2	50-0	—	—
31		—	—	—	—	—	—	—	—	31		-112	47-0	49-5	-064	48-8	49-0	—	—	31		31		-124	49-5	51-2	-085	51-2	50-5	—	—	31		-124	49-5	51-2	-085	51-2	50-5	—	—
Mean.		22-117	51-3	52-3	22-038	51-8	52-0	11-85	12-28	Mean.		22-073	49-8	51-1	22-013	50-3	50-5	12-28	12-28	Mean.		Mean.		22-113	50-1	51-9	22-050	52-4	52-5	0-12	0-12	Mean.		22-113	50-1	51-9	22-050	52-4	52-5	0-12	0-12

On the 15th, 16th and 17th of November ice was found around the office at sunrise.

Range of Barometer at Dodabetta.

An inspection of the above Tables shows that the range of the daily pressure, on the whole, had been very regular. The minimum oscillation was 0·08 on the 25th of July, although on the preceding and following days it was respectively 0·059 and 0·069; on the 25th of July 0·18 of rain fell, but the oscillation was only 0·16 on the 10th of June, on which day no rain fell, and the wet bulb was depressed 3°·6 in the morning and 1° in the afternoon. There are two other instances in which the diurnal oscillation was less than three hundredths,—on the 11th of April ·022, and on the 3rd of June ·026. The maximum oscillation occurred on the 16th of June, and was ·144. The preceding day it was ·102, but the following day it was only ·040. On the 18th of March it was ·102, on the 1st of March ·094, and on the 19th of February ·092; with these few exceptions of maxima and minima in diurnal oscillation, the daily range of the barometer was from ·040 to ·060, showing no violent atmospheric changes. The maximum pressure was on the 17th of March, and was 22·218 in., and the minimum on the 23rd of June, when the barometer stood at 21·800, although only ·09 of an inch of rain fell on that day, and little for several days before. The extreme annual range of the barometer, therefore, was only ·418, or less than half an inch. I have shown, in my former paper in the *Philosophical Transactions*, that the annual range in the Deccan, at 1800 feet above the sea, was only ·672 in 1830, from 28·242 in. in January to 27·570 in July, the difference of the thermometer at the extreme periods being only 1°·4; and in no instance, on comparing the maximum pressure of one year with the minimum pressure of any other year, did the difference amount to eight-tenths of an inch. In GOLDINGHAM'S tables at Madras, for twenty-one years, the greatest range in any year (with the single exception of a terrific storm in May 1820) appears to have been ·960 in 1818. In the more careful observations recorded by Capt. LUDLOW at the Madras Magnetic Observatory, from 1841 to 1845 both inclusive, *taken hourly*, the maximum range in 1842 (the observations for 1841 being imperfect) was from 30·156 at forty-one minutes past 9 A.M. on the 5th of December, to 29·454 on the 1st of June at 3^h 41^m P.M., and 29·455 on the 3rd of June at 4^h 41^m P.M., the range being 0·702 of an inch, the standard thermometer at the maximum pressure being 80°·6, and at the minimum 97°·3. In 1843 the maximum pressure was 30·208 on the 24th of January at 9^h 41^m A.M., and the minimum on the 21st of May being 29·256 at 3^h 41^m A.M., the range therefore 0·952; the thermometer at the maximum 81°·2, and at the minimum period 77° 8^m. In 1844 the maximum pressure was on the 16th of January, *i. e.* 30°·170 at 9^h 41^m A.M., and the minimum on the 1st of June, at 4^h 41^m P.M., *i. e.* 29·537, the range therefore 0·633, the thermometer at the first period being 79°·5, and at the second 89°·8. In 1845 the maximum pressure was on the 10th of January, 30°·196 at 9^h 41^m A.M., and the minimum on the 31st of May, 29°·531 at 4^h 41^m P.M., the range therefore 0·665: the thermometer at the maximum period was 79°·9, and at the minimum it was 101°·5. The following Table shows the extreme range of pressure at Madras, Bombay, Calcutta, &c.

Dates and Hours of the Maximum and Minimum Pressure at Madras, and extreme range of Barometer at Madras, Bombay, Calcutta, &c.

Years.	Max. Pres.	Date.	Hour.	Min. Pres.	Date.	Hour.	Annual range at Madras.	Annual range at Bombay.	Annual range at Calcutta.	Annual range at Poona.	Annual range at Mahabuleshwar.	Annual range at Dodabetta.
1842.	30·156	Dec. 5.	^h 9 ^m 41 A.M.	29·454	June 1.	^h 3 ^m 41 P.M.	0·702			*0·672		
1843.	30·208	Jan. 24.	^h 9 ^m 41 A.M.	29·256	May 21.	^h 3 ^m 41 P.M.	0·952	0·705				
1844.	30·170	Jan. 16.	^h 9 ^m 41 A.M.	29·537	June 1.	^h 4 ^m 41 P.M.	0·633	0·661	0·957			
1845.	30·196	Jan. 10.	^h 9 ^m 41 A.M.	29·531	May 31.	^h 4 ^m 41 P.M.	0·665					
1846.							0·723		0·869			
1847.									0·859			0·418
1848.									0·961		0·443†	

The maximum pressure, therefore, at Madras occurred at the *same hour* annually and within a range from the 5th of December to the 24th of January; and the minimum pressure occurred between 3^h 41^m and 4^h 41^m P.M., and within the range of ten days between the 21st of May and 1st of June. The maximum pressure at Dodabetta, in 1847, was on the 17th of March, instead of December or January, as at Madras, but the minimum occurred in June, as at Madras. The Poona maximum was in January and the minimum in July; at Bombay, in 1843, the maximum pressure occurred on the same day and the same hour as at Madras, and the minimum pressure occurred equally in May, but on the 16th instead of the 21st of May. These facts Dr. Buist recorded in a series of meteorological observations, taken hourly in Bombay for the year 1843, on the opposite side of the peninsula from Madras; and in his tables it is stated that the maximum pressure of the atmosphere, 30·136, occurred on the 24th of January at 10 A.M., and the minimum pressure, 29·431, on the 16th of May, before the monsoon set in, at 4 P.M.; the extreme range therefore being 0·705, while the extreme range at Madras in that year was 0·952, but the mean of the five years at Madras was 0·735. In Bombay, in 1844, the range was ·661 from 30·119 on the 13th of January to 29·458 on the 4th of June. At Calcutta unfortunately the observations were not taken hourly, and exact data are not attainable; but the record in 1846 states that the maximum pressure occurred on the 12th of January at 9^h 50^m A.M., viz. 30·225, and the minimum on the 25th of July at 4 P.M. 29·356, the annual range therefore 0·869. In 1847 the barometer stood highest on the 5th of February at 9^h 50^m, being 30·169, and the least pressure was on the 25th of May, at *sunset*, 29·310, the annual range therefore 0·859, singularly approximating to that of the preceding year, but in both years being greater than at Madras or Bombay. In 1848, at Calcutta, the maximum pressure was on the 24th of December 30·231 (9th of February 30·200, and 16th of February 30·191), and the minimum pressure on the 20th of July 29·270 (on 18th of June 29·353, and on 20th of May 29·459), the extreme range in the year

* For 1830.

† For ten months of 1828–29.

therefore was 0·961. At no one of the places in the Table does there appear to have been a range of an inch ; not only within the year, but taking the maximum pressure of one year with the minimum pressure of another year, the range of the barometer never amounted to 1 inch ; and but for a record left by the late Mr. GOLDINGHAM, astronomer at Madras, it might be supposed the peninsula of India was not subject to any great changes of pressure. In a furious hurricane however which occurred at Madras on the 30th of October 1836, at 6 A.M., the barometer stood at 29·940, and at 7 P.M. it stood at 28·285, and stood at that during an awful lull until 7^h 45^m P.M., when the hurricane recommenced and the barometer rose to 28·725 ; the removal of pressure in thirteen hours amounting to 1·665 inch. But the area of any such removal of pressure would appear to be comparatively limited, and we are indebted to the elaborate researches of Mr. PIDDINGTON of Calcutta, and Colonel REID of the Royal Engineers, in their investigation of the Rotatory Storms or Cyclones within the tropics, for enabling us to give an approximative area to the great depressions of the barometer at sea, of which the observations on shore scarcely record a trace. For instance, in a storm at Madras on the 16th of May 1841, the hourly observations at the observatory gave the barometer 29·650 at 10½ A.M., and the lowest depression was 29·513 at 4^h 41^m P.M., while the barque Tenasserim, which had been compelled to slip her cable and run to sea at 1 P.M., at 8 P.M., when not many miles distant from Madras, had the barometer at 28·60, the mercury at Madras at the same hour standing at 29·641, there being a difference of pressure of more than an inch of mercury *within a few miles*. The only effects of this storm upon the diurnal tide at Madras were, that the maximum nocturnal tide turned at 10^h 41^m P.M. instead of 9^h 41^m P.M., and the minimum ebb tide stopped at 2^h 41^m A.M. instead of 3^h 41^m A.M., but the minimum day-tide turned at the usual hour, 3^h 41 P.M.

Again, in a storm at Madras which took place on the 21st of May 1843, the simultaneous record of the barometer at the observatory and on board the General Kyd, which had been compelled to slip her cables and put to sea from Madras roads, was as follows :—

	Hours.											
	h m 12 41	h m 3 41	h m 5 41	h m 7 41	h m 9 41	h m 11 41	h m 12 41	h m 2 41 A.M.	h m 12 41	h m 2 41 P.M.	h m 5 41	23rd noon.
Madras	29·323	29·256	29·284	29·381	29·420	29·412	29·391	29·333	29·409	29·382	29·411	29·522
General Kyd	29·45	29·38	29·28	29·26	29·19	28·17	29·11	29·11	29·18	29·19	29·27	29·42

From this Table it is seen that at 11^h 41^m on the night of the 21st of May, the barometer stood respectively at Madras and on board the General Kyd at 29·412 and 28·17, a difference of 1·242 ; and as the General Kyd was only sixty-eight miles east of Madras at noon on the 22nd, this prodigious difference of pressure occurred within fifty or sixty miles. The daily atmospheric tides continued at Madras despite the storm. The barometer reached its maximum at 9^h 41^m A.M., 29·421 ; and at 3^h 41^m P.M. it had fallen to its minimum, 29·360 ; then as usual it rose to 29·498 at 9^h 41^m P.M., and

as usual fell gradually to 29·373 at 3^h 41^m A.M. Although therefore the general pressure of the atmosphere had been lowered three or four-tenths of an inch from the 20th and 21st of May, the daily tides had not been interrupted, the maxima and minima occurring at their usual hours. In a storm at Madras on the 24th of October 1842, the P.M. tide reached its maximum at 10^h 41^m P.M., but the ebb stopped at 1^h 41^m A.M. instead of 3^h 41^m A.M., and then rose until 9^h 41^m A.M., the duration of the ebb being three hours only, and the flow from 1^h 41^m A.M. to 9^h 41^m A.M., no less than eight hours; the next ebb six hours, and the subsequent flow seven hours. One other illustration will show how little the pressure of the atmosphere is affected at a great height by a proximate storm. On the 17th and 18th of April 1847 a great storm occurred on the Malabar coast. The following is the record of the barometer on the 17th, at different distances from the *centre of the storm*, as recorded by Colonel REID; the Dodabetta observations being interpolated by myself.

	in.
Madras, 300 miles distant from centre, barometer . . .	29·97
Dodabetta, 166 miles distant from centre, barometer . . .	21·917
Cannanore, 100 miles distant from centre, barometer . . .	29·64
Ship Mermaid, 60 miles distant from centre, barometer . . .	29·35

On the 18th of April.

	in.
Madras, 400 miles distant from centre of storm, barometer	29·94
Bombay, 240 miles distant from centre of storm, barometer	29·70
Dodabetta, 196 miles distant from centre of storm, barometer	21·984
Cannanore, 130 miles distant from centre of storm, barometer	29·78
Ship Buckinghamshire, 20 miles distant from centre of storm, barometer	28·35
Ship in centre of the storm, barometer	28·00

At the centre of the storm, distant from Cannanore 130 miles, the difference of pressure therefore was 1·78; at Dodabetta, at 8642 feet above the sea, and 196 miles from the Buckinghamshire, the greatest difference on the 17th, 18th, or 19th of April from the mean pressure of the month, was less than two-tenths of an inch, as the following records at Dodabetta show :—

1847. April 17.		April 18.		April 19.		Mean pressure of month of April.	
Barometer.	Oscillation.	Barometer.	Oscillation.	Barometer.	Oscillation.	Barometer.	Oscillation.
21·917	·058	21·984	·064	22·034	·044	22·097	·062

And although the wind at Dodabetta on the 17th and 18th blew from the east round to the west with a maximum pressure of 35 and 22 lbs., and with a mean pressure for the two days of 21 and 14 lbs.; and though ten inches of rain fell on the 18th, the daily atmospheric tides were neither suppressed, inverted, nor interrupted, and scarcely differed from the daily mean oscillation of the month. Sufficient instances have thus been

adduced of the comparatively narrow area of very great barometrical depressions, and the annual ranges given in the preceding Table may for the present be considered normal conditions, at least at Madras.

The following Table gives the monthly means of the diurnal oscillation of the barometer at Dodabetta, together with the monthly and annual range of the barometer, and the monthly fall of rain :—

Barometer at Dodabetta, at 8640 feet above the level of the sea.

	Barometer. Monthly means.		Difference.	Maximum.	Minimum.	Extreme monthly range.	Rain.
	9 ^h 40' A.M. in.	3 ^h 40' P.M. in.					
1847.				in.	in.		
February	22·129	22·068	·061	22·165	22·014	·151	7·43
March	22·160	22·105	·055	22·218	22·064	·154	3·61
April	22·128	22·066	·062	22·199	21·888	·311	19·80
May	22·087	22·024	·063	22·169	21·930	·239	4·86
June	21·992	21·940	·052	22·069	21·800	·269	4·55
July	22·000	21·949	·051	22·032	21·889	·143	7·41
August	22·030	21·976	·054	22·090	21·898	·198	9·32
September	22·031	21·977	·054	22·100	21·876	·224	7·52
October	22·086	22·014	·072	22·129	21·958	·171	12·49
November	22·117	22·038	·079	22·152	21·966	·186	11·85
December	22·073	22·013	·060	22·150	21·921	·229	12·28
1848.							
January	22·113	22·050	·063	22·163	22·016	·147	0·12
Year	22·079	22·019	·060	22·218	21·800	·418	101·24

Horary Oscillations.

It is thus shown that the mean daily range for the year is 0·060, and in no month of the year did it exceed 0·079 in. This is different from Mahabuleshwur, Poona, Madras and Bombay. At Mahabuleshwur, at 4500 feet, the mean diurnal oscillation for ten months, by simultaneous observations, was 0·0694, and in no month did the monthly mean exceed 0·0835. At Poona, at 1823 feet, for 1827, it was 0·1009, for 1828 it was 0·1075, and for 1829 it was 0·0991; but this last diminished oscillation is partly to be attributed to three months' observations having been taken at an elevation of 4000 feet above the sea; but in 1830 the barometers were stationed for the whole year at Poona, at 1823 feet, and the mean diurnal fall of the barometer from 9—10 A.M. to 4—5 P.M. was 0·1166; the mean of the four years was 0·1060. The greatest *mean* diurnal range for any month in those four years was 0·1616 in the month of December 1827. At Madras hourly observations were recorded from the years 1842 to 1845, both inclusive, and for ten months in 1841, of which I shall not take any account, as the year is incomplete. The mean monthly fall of the barometer from 9^h 41^m A.M. to 3^h 41^m P.M. daily, was respectively 0·124, 0·120, 0·122 and 0·121, exhibiting a singular uniformity, the differences being only in the thousandths of an inch of pressure. This uniformity is equally marked in the successive months of the several years, and I have gone through the labour of working out the details to show it, the Madras printed observations being a record of facts only without deductions or comment.

Mean Diurnal Horary Oscillations.

	Madras.			Bombay, 35 feet.		Calcutta, 18 feet.		Deccan, about 1800 feet.			Poona, 1823 feet.	Mahabuleshwur, 4500 feet.		Dodabetta, 8640 feet.
	1842.	1843.	1844.	1845.	1843.	1844.	Means, 1829-31.	1827.	1828.	1829.	1830.	1828-29.	Means, 15 years.	1847-48.
January	0.120	0.105	0.114	0.111	0.112	0.120	0.123	0.113	0.148	0.125	0.136	0.073	0.158	0.063
February ...	0.119	0.121	0.119	0.120	0.114	0.112	0.117	0.125	0.150	0.108	0.140	0.066	0.087	0.061
March	0.126	0.127	0.128	0.125	0.113	0.123	0.125	0.124	0.112	0.102	0.133	0.082	0.094	0.055
April	0.134	0.133	0.134	0.138	0.119	0.118	0.124	0.083	0.133	0.098	0.143	0.083	0.092	0.062
May	0.126	0.111	0.121	0.120	0.110	0.098	0.115	0.062	0.083	0.093	0.132	0.075	0.085	0.063
June	0.126	0.119	0.118	0.120	0.074	0.071	0.095	0.090	0.100	0.073	0.106	0.052	0.062	0.052
July	0.135	0.129	0.126	0.118	0.034	0.063	0.090	0.048	0.047	0.065	0.075	0.055	0.017	0.051
August	0.131	0.127	0.129	0.139	0.087	0.070	0.099	0.060	0.070	0.086	0.085	0.053	0.043	0.054
September ...	0.124	0.134	0.131	0.128	0.086	0.097	0.101	0.081	0.091	0.077	0.090		0.073	0.054
October	0.123	0.115	0.123	0.122	0.108	0.122	0.110	0.114	0.110	0.111	0.125		0.130	0.072
November ...	0.108	0.110	0.111	0.103	0.119	0.127	0.107	0.144	0.127	0.106	0.125	0.080	0.152	0.079
December ...	0.116	0.110	0.108	0.107	0.116	0.122	0.114	0.161	0.114	0.133	0.110	0.073	0.082	0.060
Year	0.124	0.120	0.122	0.121	0.099	0.095	0.110	0.102	0.109	0.099	0.116	0.069	0.089	0.060

Note.—The above means are the daily fall of the barometer from 9 to 10 A.M. to 4 to 5 P.M.

At Madras the mean daily oscillation would appear to have been least when the barometer was highest in November, December, or January; and the reverse of this was the case when the barometer was lowest in May, June and July. The *mean* of the four years, at Madras, in the months of highest pressure, December and January, is 0·111, and the mean of the four years in the months of least pressure 0·122. The mean daily oscillation therefore was greatest when the pressure of the atmosphere was least. The same was not observed to be the case in the observations made in the Dukhun in the years 1827, 1828, 1829 and 1830; the means for the two periods being 0·131 and 0·081; nor in Calcutta in the years 1829, 1830 and 1831, the oscillations being respectively 0·118 and 0·100; and in 1848 the difference was more marked, the means of the daily oscillation for the periods of greatest and least annual pressure being 0·135 and 0·106: the same was the case at Bombay and at Dodabetta. This peculiar feature exhibited at Madras is not satisfactorily explained by its not being subjected, like Calcutta and Bombay, to the south-west monsoon, because the general curve of pressure at Madras follows the order of pressure at Calcutta and Bombay, with the exception of the month of November in the years 1844 and 1845, when the mean monthly pressure was greater in that month than in December. The hourly observations at Madras were too systematically and apparently accurately taken to suppose there could have been inaccuracy in the records, and they are quite as trustworthy, if not more so, than any other meteorological records in India. There appears a great discrepancy in the mean daily range at Madras and Bombay in the months from March to November, particularly in the month of April, which does not belong to the monsoon of either place. In the other months, the small range at Bombay may be accounted for by the prevalence of the monsoon at Bombay and the absence of the monsoon at Madras, but the means of the daily range for the year indicate a greater oscillation at Madras than might have been expected.

Times of ebb and flow of the Atmospheric Tides, or Turning-points.

I have given separately the chief of the four daily tides, namely, the fall between 9—10 A.M. to 4—5 P.M., to facilitate comparison of the movements of the same tide at different places by simple inspection. The following Table gives the movements of the three other daily tides; but I have confined it to the Madras records, as three daily movements for various places could not have been put conveniently into juxtaposition.

Monthly means of the Daily Atmospheric Tides between the hours 3^h 41^m—4^h 41^m P.M. to 9^h 41^m—10^h 41^m A.M.

	Madras, 1842.				Madras, 1843.				Madras, 1844.				Madras, 1845.			
	3 ^h 41 ^m —4 ^h 41 ^m P.M. to 9 ^h 41 ^m —10 ^h 41 ^m A.M.	3 ^h 41 ^m —4 ^h 41 ^m A.M. to 9 ^h 41 ^m —10 ^h 41 ^m P.M.	3 ^h 41 ^m —4 ^h 41 ^m P.M. to 9 ^h 41 ^m —10 ^h 41 ^m A.M.	3 ^h 41 ^m —4 ^h 41 ^m A.M. to 9 ^h 41 ^m —10 ^h 41 ^m P.M.	3 ^h 41 ^m —4 ^h 41 ^m P.M. to 9 ^h 41 ^m —10 ^h 41 ^m A.M.	3 ^h 41 ^m —4 ^h 41 ^m A.M. to 9 ^h 41 ^m —10 ^h 41 ^m P.M.	3 ^h 41 ^m —4 ^h 41 ^m P.M. to 9 ^h 41 ^m —10 ^h 41 ^m A.M.	3 ^h 41 ^m —4 ^h 41 ^m A.M. to 9 ^h 41 ^m —10 ^h 41 ^m P.M.	3 ^h 41 ^m —4 ^h 41 ^m P.M. to 9 ^h 41 ^m —10 ^h 41 ^m A.M.	3 ^h 41 ^m —4 ^h 41 ^m P.M. to 9 ^h 41 ^m —10 ^h 41 ^m A.M.	3 ^h 41 ^m —4 ^h 41 ^m P.M. to 9 ^h 41 ^m —10 ^h 41 ^m A.M.	3 ^h 41 ^m —4 ^h 41 ^m P.M. to 9 ^h 41 ^m —10 ^h 41 ^m A.M.	3 ^h 41 ^m —4 ^h 41 ^m P.M. to 9 ^h 41 ^m —10 ^h 41 ^m A.M.			
Jan.	+0.078	—0.063	+0.105	+0.080	—0.069	+0.094	+0.078	—0.060	+0.096	+0.079	—0.062	+0.094				
Feb.	+0.091	—0.068	+0.096	+0.084	—0.058	+0.095	+0.072	—0.049	+0.095	+0.083	—0.063	+0.100				
March	+0.100	—0.064	+0.090	+0.094	—0.058	+0.091	+0.092	—0.055	+0.091	+0.097	—0.071	+0.099				
April	+0.113	—0.056	+0.077	+0.103	—0.059	+0.089	+0.103	—0.048	+0.072	+0.116	—0.046	+0.075				
May	+0.114	—0.064	+0.076	+0.096	—0.065	+0.080	+0.110	—0.059	+0.070	+0.100	—0.053	+0.073				
June	+0.106	—0.046	+0.064	+0.098	—0.052	+0.073	+0.090	—0.038	+0.066	+0.105	—0.043	+0.058				
July	+0.113	—0.046	+0.068	+0.103	—0.043	+0.076	+0.106	—0.052	+0.072	+0.102	—0.039	+0.057				
Aug.	+0.110	—0.054	+0.075	+0.095	—0.046	+0.078	+0.107	—0.048	+0.070	+0.106	—0.036	+0.059				
Sept.	+0.108	—0.059	+0.075	+0.101	—0.039	+0.072	+0.104	—0.048	+0.075	+0.105	—0.044	+0.067				
Oct.	+0.101	—0.058	+0.080	+0.087	—0.058	+0.087	+0.097	—0.058	+0.084	+0.093	—0.050	+0.079				
Nov.	+0.085	—0.063	+0.086	+0.091	—0.070	+0.089	+0.086	—0.059	+0.087	+0.096	—0.075	+0.083				
Dec.	+0.087	—0.063	+0.092	+0.086	—0.067	+0.091	+0.091	—0.070	+0.087	+0.086	—0.048	+0.089				
Means	+0.1005	—0.0587	+0.082	+0.0932	—0.0577	+0.0846	+0.0947	—0.0537	+0.0804	+0.0973	—0.0525	+0.0786				

A close inspection of the hourly observations upon which the above Table of means is founded, shows that the tides turn occasionally on consecutive days at different hours, and the intervals between the maxima and minima in the several diurnal tides frequently vary from five hours to seven hours in the duration of each tide; and not less frequently it is found that at the turning-point or turn of the tide, there is no movement of the atmosphere at all

for an hour to an hour and a half; nevertheless the *monthly means* of the same tide for successive years are almost identical; take for instance the *rising* afternoon tide from 3—4 P.M. to 10 or 11 P.M. in the month of January, and it is seen that the means in the successive four years were ·078, ·080, ·078 and ·079, so that the daily irregularities were merged in the monthly means. Similarly, the rising tide from 3^h 41^m—4^h 41^m A.M. to 9^h 41^m—10^h 41^m A.M. in the month of February was ·096, ·095, ·095 and ·100 in successive years. The falling night-tide between P.M. and A.M. does not exhibit quite the same regular features; for instance, in the month of March we have ·064, ·058, ·055 and ·071. November also has ·063, ·070, ·059 and ·075; but the annual means of each tide, as in the great diurnal tide, remarkably approximate to each other in successive years, as is shown by the Table, which is worked out from the Madras hourly observations. Another feature exhibited by the Table is the alteration in the range of the monthly means of the diurnal movement of the same tide in different months of the year, the increment and decrement alternating at different seasons of the year with the increment and decrement of another of the tides; for instance, in the month of January throughout the four years the rising tide from 3^h 41^m—4^h 41^m P.M. to 9^h 41^m—10^h 41^m P.M. is LESS than the rising tide from 3^h 41^m—4^h 41^m A.M. to 9^h 41^m—10^h 41^m A.M.; but the reverse of this takes place as the year advances, and in April the P.M. *rising* tide is considerably greater than the A.M. rising tide, and this continues until November, when the previous relations return. The same remarks do not apply to the two *falling* tides, as the falling tide by day is invariably greater (more than double) than that of the falling tide by night.

In my paper on the Atmospheric Tides of the Deccan, I gave a Table (No. 3) of the anomalies in the period of the ebb and flow of the different tides in 1830, and to enable me to do this I had observed the barometer every five minutes about the period of the expected turn of the tide. The anomalies were numerous. In the Madras hourly observations the same anomalies are observable, as the intervals of maxima and minima vary from five hours to eight hours, although the absolute time of the turn of the tide within the hour is not shown. Dr. Buist's observations in Bombay for 1843 and 1844 are also hourly, and exhibit the same occasional irregularities in the time of the tides turning as at Poona and Madras. The recent observations at Calcutta do not afford the means of detecting this no doubt existing fact; for observations at Calcutta are not recorded by night at all, and only every two hours during the day. A bare inspection of and reliance upon the Calcutta monthly tables therefore would justify the assertion that the daily maxima and minima in the oscillations of the barometer occurred at fixed hours, which, although generally true, is not absolutely so. The observations at Dodabetta were unfortunately not made hourly, with the exception of those for twenty-four hours between the twenty-first and twenty-second days of each month; the means of comparison therefore of the diurnal hourly oscillations with those of Madras and Bombay, are limited to those twenty-four hours in each month; but although so limited, features of interest are exhibited. It has hitherto I believe been supposed that the two ascending and two descending diurnal

tides of the atmosphere within the tropics, *although turning after a longer or shorter interval in each tide, were never interrupted or retrograde in the onward or backward movement of each tide.* The Dodabetta hourly observations indicate that such is not the case, and I have thrown the facts into a table for a comprehensive view of them.

Movements of the Atmosphere in twenty-four hours in each month (21st to 22nd) of the year 1847-48 at Dodabetta, at 8640 feet above the sea.

	Hours.																							
	P.M.												A.M.											
	10	11	Noon.	1	2	3	4	5	6	7	8	9	10	11	Midnight.	1	2	3	4	5	6	7	8	9
Feb.		...	...	...	...	-.060	-.052	-.052	-.050	...	...	+.086	+.086	...	...	-.050	-.030	±.032	±.032					+.129
March	+310	...	...	...	...	-.250	±.269	-.260	...	...	...	...	...	+.320			-.238	±.244	-.228					
April	+130	...	...	...	...		-.078			...	...	+.118	+.118						-.048					
May		...	...	...	...		-.953	±.970	-.964	+.974	...	...	+.018					-.960						+.012
June	+962	...	...	...	...			-.882			...	+.944	+.944						-.880	±.804	-.880			
July	+026	...	...	...	...		-.960				...	+.015	+.015				±.970		-.950					
Aug.		...	...	...	...		-.022				...	+.073	+.073				-.030	-.030	-.030					+.084
Sept.		...	...	...	...		-.988				...			+.042				-.998						+.072
Oct.	+128	...	...	...	...	-.028					...	+.040						-.040						
Nov.		...	...	...	...			-.056			...		+.100					-.058						+.133
Dec.	+120	...	...	...	...		-.036				...	+.090						-.034	-.034					
Jan.		...	...	...	...			-.035			...	+.078	+.078			-.058	±.062	-.042	-.036	-.028				+.114

+ Indicates the maximum tide. — Indicates the minimum tide.
The barometer ranged between 21.800 and 22.218 inches.

± Indicates an interruption to usual rise or fall.

Observations on Horary Oscillations.

It is hence seen that in the month of March the diurnal tide, which usually falls from 10 A.M. until 4 P.M., turned at 3 P.M., and should then have gradually risen until 9—10 P.M., instead of which it rose only until 4 P.M. and then fell until 5 P.M., when it turned again and ran its usual course. In the same month the A.M. tide, which usually falls from 10 P.M. until 4 A.M., had its fall interrupted at 2 A.M. and rose until 3 A.M., when it turned and fell to its ordinary minimum, and then gradually rose until 10 A.M. In the month of May the daily minimum occurred at the proper hour, 4 P.M., and its ordinary regular rise continued until 5 P.M.; but at this hour it changed and fell until 6 P.M., but at that hour resumed its usual course. The other tides were free from these aberrations, but two of them turned at 3 and 9 A.M. instead of 4 and 10 A.M. In the month of June the flow is irregular in the 4 A.M. tide. Its minimum occurs properly at 4 A.M., and it rises until 5 A.M., but at this hour it has turned, instead of continuing to rise, and falls until 6 A.M., when the barometer is as low as at 4 A.M.,—a solitary instance within the year. In the month of January 1848, the falling tide from 10 P.M. goes on regularly until midnight, when it turns and rises until 1 A.M.; it then turns again in the proper direction, but instead of stopping at 4 A.M., the minimum hour, it continues until 5 A.M. and then turns. An inspection of the Table shows that the barometer never appeared to remain stationary at the turning hour of the maximum A.M. (9—10) tide, but that in the months of January, February and April, no movement of the tide took place for a full hour at the 9—10 P.M. maximum tide. In August, although the other tides flowed as usual, that of the A.M. period remained stationary from 2—4 A.M., both inclusive. The maximum A.M. tide occurred six times in six months of the year at 9 A.M., and six times in the other six months at 10 A.M. The maximum nocturnal tide took place only twice at 11 P.M., thrice at 9 P.M., five times at 10 P.M., and thrice was stationary between 9 and 10 P.M. The minimum diurnal tide turned once only at 6 P.M. The chief irregularities appear to have been in the A.M. minimum nocturnal tide, embracing the hours from 1—6 A.M. On looking over the Madras hourly observations for four years for the twenty-four hours between the 21st and 22nd of each month in the year 1842, there are *only two instances of similar irregularities*. On the 22nd of March in the falling A.M. tide at 2^h 41^m A.M., the barometer stood at 29·859, and at 3^h 41^m A.M., instead of continuing to fall, it had risen to 29·869, but at 4^h 41^m it resumed its regular course and fell to 29·865. On the 22nd of November an irregularity occurred in the nocturnal P.M. tide. At 10^h 41^m the barometer stood at 30·004; at 11^h 41^m it had fallen in due course to 29·991, but at 12^h 41^m it had risen to 29·992, and only at 1^h 41^m resumed its usual ebb, and fell to 29·981. In the year 1843 there are also *only two instances* of irregularity in the specified days; namely, on the 21st of August in the A.M. falling tide. At 1^h 41^m A.M. the barometer stood at 29·722; at 2^h 41^m, instead of falling, it rose to 29·725, then fell, at 3^h 41^m, to 29·724; then rose, at 4^h 41^m, to 29·726, and continued regular to the maximum period at 9 A.M. On the 21st of December, at 11^h 41^m

P.M., the barometer was 30·121 ; at 12^h 41^m, instead of continuing to fall, it had risen to 30·125, but then changed ; and at 2^h 41^m it was 30·104, and fell regularly till 3^h 41^m A.M., when it was at its minimum, 30·089. On the 21st of September, in 1843, the minimum A.M. nocturnal tide occurred at 1^h 41^m A.M. instead of 3^h 41^m, at which hour it took place on the morning of the 22nd ; the interval, therefore, between the minimum of this A.M. nocturnal tide and the maximum of the 9—10 morning tide, was seven hours, the maximum occurring at 8^h 41^m, a very rare instance of the continued flow of this A.M. tide, which usually runs its course within four or five hours, and on rare occasions in three hours ; the change of pressure, however, was only equal to 0·114, from 29·763 to 29·877. In 1844 there is *but one instance* of irregularity within the prescribed hours. On the 22nd of September, at 12^h 41^m A.M., the barometer stood at 29·820 ; at 1^h 41^m it had fallen as usual to 29·808, but at 2^h 41^m it had risen to 29·819 ; at 3^h 41^m to 29·833 ; but at 4^h 41^m it had fallen to 29·832, and then continued regular. In 1845 there is also *only a solitary instance* on the 21st of March. At 7^h 41^m A.M. the barometer stood at 29·933 ; at 8^h 41^m it had fallen, instead of rising, to 29·932, but at 9^h 41^m resumed its usual course. It will be remarked that at Madras there is *not a single instance* of irregularity in four years in the fall of the great diurnal tide from 9—10 A.M. to 4—5 P.M., and the instances of irregularities in the other tides only serve to prove that the laws are not without exceptions. From the above comparisons it is seen that the irregular movements in the tides were not common to Dodabetta and Madras at simultaneous periods of time, nor at proximate periods of time. The irregularities in the intervals of the different tides are numerous, as are also the instances of stationary periods in which the atmosphere appears to be quiescent from one to two hours. It now remains to notice the deviations from the usual laws at Bombay, as recorded by Dr. Buist in his Bombay hourly observations for 1843 and 1844. On the 21st of January, at 3 A.M., the barometer stood at 29·864, at 4 A.M. at 29·865 ; but at 5 A.M., instead of continuing to rise, it had fallen to 29·863, and continued to fall until 6 A.M., when it stood at 29·856 ; at 7 A.M. it resumed its usual course, and continued to rise until the maximum hour. At Madras, on the same day, making allowance for the difference of longitude, *nothing of the kind occurred*. At Bombay, on the 21st of March, at 11 P.M., the barometer stood at 29·688 ; but at midnight, instead of continuing to fall, it had risen to 29·699, and it was only at 1 A.M. it resumed its ebb. *There was nothing of the kind at Madras*, where the maximum, 29·861, was attained at 10^h 41^m with the ordinary regularity. On the 22nd of May, at Bombay, there were irregularities in all the tides, a thing so unusual, and not occurring at all on the 20th or 23rd of May, as to render the table apocryphal, either from error in the record or in the lithography ; the A.M. falling tide stops at 2 A.M., then rises to 8 A.M., falls to 9 A.M., and then rises to 10 A.M., its maximum, however, being at 8 A.M. From 4 P.M. the tide as usual rises to 6 P.M., then falls to 8 P.M., and afterwards proceeds to attain its maximum at 11 P.M. Something similar occurs between 1 and 5 A.M. on the 21st of June, but not on the 22nd. *Nothing of the kind occurs at Madras, either on the*

20th, 21st, 22nd or 23rd of May. The last irregularity to be noticed at Bombay, within the limit hours, was on the 21st of October. At midnight the barometer stood at 29·803; at 1 A.M. it had risen in the usual course to 29·887, but at 2 o'clock it had fallen to 29·881, and only resumed its ordinary rise at 3 A.M.; as far as the record goes, there does not appear to have been any irregularity at Madras*. Similar instances occur within the limit hours in 1844,—on 21st of January at 3 A.M., 21st of May at 1 A.M., when the tide remained stationary at 3^h 41^m, and 5 A.M., and at 1 A.M. on the 22nd of December. In September the maximum horary pressure occurred at 11^h 30^m A.M., and in other months several times at 11 A.M.

If we extend our comparisons to Aden, on the coast of Arabia, nearly in the latitude of Madras, but 35° 6' of longitude to the west of Madras, we find the very same exceptions to normal conditions. Dr. Buist has been kind enough to transmit to me proof sheets, now going through the press, of part of a meteorological journal kept at Aden for 1847 and 1848. Hourly observations were taken on certain term-days monthly; instead of taking up the whole of these term-days, it will suffice to select those of January and June of each year, these being months in which the maximum and minimum pressure of the atmosphere usually occur in India. Barometer 187 feet above the sea at Aden. All corrections made.

January, 1847.	Hours.																								Means.
	P.M.												A.M.												
	Noon.	1	2	3	4	5	6	7	8	9	10	11	12	1	2	3	4	5	6	7	8	9	10	11	
15	29-760	...	...	743	...	...	...	...	821	...	...	...	778	780	...	770	...	...	...	...	...	853	...	...	29-790
25	29-684	...	...	661	...	...	...	...	717	...	...	...	...	...	...	646	...	...	...	...	...	743	...	...	29-691
30	29-763	...	...	716	...	...	...	...	...	...	786	...	...	...	...	722	...	...	...	...	...	808	...	...	29-760
June, 1847.																									
8	29-648	...	...	557	...	...	...	...	...	...	637	...	...	...	...	583	...	...	...	...	...	698	...	...	29-614
16	29-582	...	...	488	488	...	...	...	...	542	...	...	...	...	...	507	...	...	...	...	...	613	...	...	29-533
20	29-631	...	...	502	...	...	...	...	...	574	...	...	...	...	...	506	...	...	...	...	...	620	...	...	29-552
29	29-490	...	...	...	385	...	...	...	...	...	484	...	...	...	...	431	...	...	...	...	...	548	...	...	29-468
January, 1848.																									
7	29-856	...	810	...	...	...	...	...	...	921	...	...	858	864	...	844	...	...	...	...	...	914	...	...	29-876
14	29-868	...	...	835	...	...	...	...	...	...	902	...	...	...	...	860	...	...	...	...	...	915	...	...	29-874
24	29-791	...	...	732	...	...	...	...	...	...	807	...	...	...	...	733	...	...	...	...	...	853	...	...	29-781
28	29-774	...	720	720	...	...	...	...	...	...	814	...	...	...	...	761	...	...	...	...	...	853	...	...	29-781
June, 1848.																									
8	29-497	...	...	416	...	...	...	...	...	...	507	...	...	...	395	...	...	...	572	563	...	532	...	609	29-460
15	29-546	...	...	481	...	...	...	...	...	549	...	...	...	...	...	479	...	...	...	...	...	...	...	...	29-533
21	29-522	...	...	382	382	...	...	...	...	469	446	466	462	...	...	377	...	...	...	...	...	...	567	...	29-456
29	29-651	...	...	542	...	...	...	...	...	...	599	...	...	...	...	544	...	...	...	...	...	673	...	...	29-589

A glance of the eye over the above Table shows the turning-points of the several tides and their irregularities. The first marked feature is the almost constant turning of the descending P.M. tide and ascending A.M. tide at 3 P.M. and 3 A.M., instead of at 4 or 5 P.M. and A.M., as on the continent of India; to this there are only five

* In a diagram of the comparative readings of seven barometers at Bombay, between the 20th and 21st of June 1843, these anomalies appear to have occurred between 2 and 3 P.M., 7 and 8 P.M., 10 and 11 P.M. and 1 to 4 A.M., but the barometers did not exactly harmonize in their movements.

exceptions to the former and only one to the latter ; but two of the exceptions are remarkable, the tide having turned on the 7th and 28th of January 1848, at 2 P.M. I do not recollect a single case of the kind in the records in India. In the first of these cases the tide flowed from 2 until 9 P.M., seven hours ; and in the second it remained stationary from 2 until 3 P.M., and then flowed until 10 P.M. ; this portion of the diurnal movement of the atmosphere occupying eight hours instead of six. As a contrast to these lengthened periods, we find that the 3 P.M. ascending tide, on the 25th of January and 20th of June 1847, flowed only from 3 P.M. until 8 P.M., five hours instead of eight. On the 15th of January 1848, the ascending A.M. tide did not attain its maximum until 11 A.M., but this retardation was probably owing to an inversion in the flow at 7 A.M. ; the pressure, instead of increasing at this hour, having diminished from 29.572 at 6 A.M. to 29.563, but after this interruption the usual increase took place up to 11 A.M. It will be seen that there are two instances of a stationary state of the barometer on the 28th of January 1848, from 2 to 3 P.M., and on the 21st of June 1848, from 3 to 4 P.M. Meteorologists know that this circumstance, which would excite no attention without the tropics, is of rare occurrence within the tropics. The next great feature in this Table is the absence of any retrograde movement in the P.M. descending and ascending tides ; but this is not the case with the other two tides ; as at Dodabetta, several interruptions in the flow or ebb are recorded. In the descending nocturnal A.M. tide there are three instances. On the 15th of January 1847, at 1 A.M., the tide has risen from midnight instead of continuing to fall. Precisely the same thing occurs on the 7th of January 1848, at the same hours. On the 21st of June the interruption occurs at an earlier hour. The pressure is at its maximum at 9 P.M. ; it then diminishes until 10 P.M., but instead of continuing to diminish it increases until 11 P.M., but at midnight has resumed its usual course. In the A.M. ascending tide there is only one instance of inversion ; on the 15th of June 1848, the usual rise stops at 6 A.M. and retrogrades until 7 A.M., it then resumes its usual flow and continues until 11 A.M., a very unusual hour for the period of the maximum of the A.M. tide. Had the term-days of the other months been noticed, numerous instances of deviations from normal conditions could have been given. *It would be desirable to ascertain whether these aberrations have any relation with changes in the electrical tension in the atmosphere.*

If these anomalies be tested by a comparison with hourly readings of the barometer, at Aden CORRECTED FOR THE TENSION OF VAPOUR*, for four days in each month of the year 1847, not only is their occurrence rendered unquestionable, but new phases in abnormal conditions appear. Annexed is a Table of the "Means" of hourly readings of the barometer in four days in each month at Aden in 1847, CORRECTED FOR MOISTURE.

* Corrected by the observer.

Means of the Hourly Readings of the Barometer at Aden for 1847, corrected for Moisture.

	Noon.	P.M.												A.M.												Means.
		1	2	3	4	5	6	7	8	9	10	11	12	1	2	3	4	5	6	7	8	9	10	11	12	
Jan.	28-969	...	·954		...	...	...	...	...	29-018				28-973	...			...		...	...	...	29-063	...	...	28-996
Feb.	29-118	·121	...		...	...	...	...	...		29-171				...	29-110		...		...	...	...	29-188	...	...	29-132
March	29-051	...	...	·005	...	...	...	...	...		·081				...		30-020	...		...	...	·103		...	...	29-052
April	28-914	...	...		·867	...	...	·896	·878		·904				...	·870		...		...	...	...	·931	...	...	28-893
May	28-729	...	...	·643	...	...	...	·691	·686	...	·717				...		·667	...	·739	·722	...	...	·738	...	...	28-702
June	28-679	...	...		·550	...	·634	·684	...	·654	·634	·664			...			·554		...	...	...	·667	...	...	28-624
July	28-655	...	...		...	·715	...	...	...					·590	·599	·591		...	·667	·613	...	...	·656	...	...	28-639
Aug.	28-709	·707	...		...	...	...	·777	·747	·758		·695	·744	·727	·714	·717	·704	·676		...	...	...	·836	...	...	28-735
Sept.	28-739	·784	...	·733	...	·762	·746	...	...	·765				·715	...			...	·775	·751	...	...		...	...	28-748
Oct.	29-011	...	...	28-933	...	...	...	...	...		29-051	29-032	29-068		...	29-031		...	29-166	...	...	...		...	...	29-032
Nov.	29-195	...	...	·135	...	...	...	...	...		·309			·252	·252			...	·311	...	...	·262	·269	...	...	29-239
Dec.	29-259	...	...	·174	...	...	...	...	...		·292				...	·240		...		·280	·275	...	·307	...	...	29-253

It has been asserted that when the barometer is corrected for moisture its diurnal movements in the tropics are resolvable into a simple ascending and descending curve in the twenty-four hours, nevertheless the first feature of the Table is the existence of two ascending and two descending tides, although the correction for moisture had reduced the air in which the mercurial column had moved to a supposed dry state. The usual hours of the ebb and flow are generally sufficiently manifest, particularly in the hours of greatest pressure, yet there is the unusual circumstance of the ascending A.M. tide turning in five months at 6 A.M. instead of 9—10 A.M., and in this tide also there are three instances of a check to its usual movements. The ascending P.M. tide has also its anomalies. In eight months it attained its maximum at the usual time between 9 and 10 P.M., but in July there is the rare circumstance of its turning at 5 P.M. instead of 9—10 P.M., and in June and August the equally rare circumstance of its turning at 7 P.M.; the next anomaly in this tide is in October, when it did not turn until midnight. The chief anomalies, as at Madras, Bombay and Dodabetta, occur in the A.M. and P.M. ebb or descending tide. The Table shows that the turning-points of the P.M. diurnal ebb, ranged from noon in February, and July, to 4 P.M. in April and June. In February, when the minimum occurred at noon, the maximum took place at the usual hour, 10 P.M.; there was therefore the almost unprecedented circumstance of *a tide continuing to flow for nine hours*. In the A.M. descending or ebb tide, the anomalies are even more marked than in the corresponding P.M. tide, for the turning-points range from 1 A.M. to 5 A.M. In January it turned at 1 A.M., and then the ascending tide continued to flow uninterruptedly until 10 A.M., a second instance of a *nine hours' flow*. In July, September and November, it also turned at 1 A.M., but the maximum occurred at 6 A.M. instead of 10 A.M., the flow in each of these months being five hours! a glance of the eye over the Table will show; although the different tides ultimately attained their respective maxima and minima, yet in many instances they were subject to checks or interruptions, which appeared to give way after a short resistance to the periodic movements of the atmosphere. In the month of August, however, the atmosphere appeared in so vacillating and disturbed a state, that the only tide which turned at the normal hour was the 10 A.M. maximum tide. The means of the hourly readings on four days in each month, corrected for moisture, give a mean curve of pressure in each month which nearly corresponds with the curve of pressure of the daily readings of the barometer corrected for temperature only. The maximum pressure with both corrections occurs in December, but the minimum occurs in June corrected for temperature and in July with both corrections. The whole of the facts connected with the meteorology of Aden of which I have made use, are from observations taken by Sergeant MOYES, with excellent instruments, and Dr. BUIST is now passing the observations through the press.

These facts, which could be very greatly multiplied, have been somewhat dwelt upon, with a view to a right understanding of HUMBOLDT's observations in his *Cosmos**,

* BOHN'S Edition, vol. i. p. 320.

where, speaking of the horary oscillations of the barometer, he says, "their regularity is so great, that in the day-time especially, the hour may be ascertained from the height of the mercurial column without error, on the average of fifteen or seventeen minutes. In the torrid zones of the new continent, on the coasts, as well as at elevations of nearly 13,000 feet above the level of the sea, where the mean temperature falls to $44^{\circ}6$, I have found the regularity of the ebb and flow of the aërial ocean undisturbed by storms, hurricanes, rain and earthquakes." It is now shown that the horary oscillations have a different range in different months of the year; their range is influenced by height above the sea, and the tides do not always flow and ebb in equal periods of time; but the existence of two ascending and two descending but unequal tides within twenty-four hours within the tropics, is established beyond all question; and if not altogether *undisturbed*, as HUMBOLDT says, by storms, hurricanes or rain, yet the meteorological records in India prove that the periodic daily movements of the aërial ocean are never suppressed*.

Pressure of the Atmosphere.

With respect to the mean pressure of the atmosphere in India near the sea-level, I shall limit myself to the insertion of a comparative *table* showing the means of four years' hourly observations at Madras, the means of two years' hourly observations at Bombay, and the observations taken every two hours (but during the day only) at Calcutta, for 1843-44 and 1848. I append however curves of pressure at these places projected for longer periods, and for which I am indebted to Dr. BUIST, LL.D. The barometers at these respective localities, being each only a few feet above the sea-level, should have differed from each other in their mean annual results only in the third place of decimals, or at most only slightly in the second place of decimals, nevertheless the pressure at Bombay differs from that at Madras a twentieth of an inch; and at Calcutta, where the barometer is 17 feet nearer the mean sea-level than at Bombay, the mean annual pressure is even less than at Bombay, while on the other hand the pressure at Aden reduced to the sea-level is greater than anywhere else. These discrepancies originate probably in the neglect of using previously compared instruments. At Madras, the annual means in successive years differ from each other only in hundredths of an inch. For the mean pressure at different elevations tables are annexed.

* The observations of Lieut. R. STRACHEY in Thibet, at 18,400 feet above the sea, show that on the 22nd and 23rd of August 1849, the following were the oscillations of the four tides:—

	B. uncorrected for temperature.	B. corrected for temperature.
5 P.M. to 11 P.M.	+·059	+·075
11 P.M. to 4 A.M.	—·033	—·029
4 A.M. to 10 A.M.	+·043	+·016
10 A.M. to 3 P.M.	—·037	—·032

Monthly Mean Pressure of the Atmosphere, reduced to 32°.

Madras.					Bombay, 35 feet.		Calcutta, 18 feet.		Aden, 187 feet.		Poona, 1823 feet.	Mahabuleshwur, 4500 feet.		Dodabetta, 8640 feet.	Royal Observatory, Greenwich, 159 feet.
Years . . .	1842.	1843.	1844.	1845.	1843.	1844.	1848.	1843, 1844.	1847.	1848.	1830.	1828, 1829.	Means of 1 year.	1847, 1848.	Means of 8 years, 1841 to 1848.
January.	29.995	29.985	29.998	30.015	29.923	29.947	30.000	29.937	29.774	29.872	28.087	25.465	25.737	22.081†	29.766
February.	29.972	29.966	29.980	29.965	29.889	29.928	29.980	29.915	29.832	29.853	28.002	25.452	25.765	22.098	29.737
March...	29.860	29.886	29.907	29.924	29.839	29.869	29.838	29.793	29.770	29.783	27.952	25.576	25.688	22.132	29.750
April ...	29.805	29.864	29.809	29.818	29.813	29.804	29.716	29.656	29.690	29.711	27.907	25.538	25.667	22.097	29.708
May ...	29.713	29.692	29.700	29.712	29.662	29.750	29.636	29.563	29.635	29.584	27.846	25.467	25.643	22.055	29.785
June ...	29.664	29.709	29.668	29.705	29.654	29.628	29.516	29.184	29.545	29.511	27.768	25.377	25.664	21.966	29.797
July ...	29.697	29.709	29.723	29.726	29.661	29.648	29.528	29.517	29.491	29.475	27.766	25.319	25.600	21.974	29.799
August..	29.729	29.759	29.731	29.742	29.730	29.701	29.571	29.516	29.540	29.483	27.840	25.387	25.647	22.003	29.787
Sept. ...	29.768	29.788	29.793	29.830	29.779	29.795	29.713	29.662	29.639	29.626	27.925		25.524	22.004	29.809
October	29.873	29.863	29.868	29.850	29.845	29.825	*29.881	29.633	29.811	29.745	27.923		25.810	22.050	29.858
Nov. ...	29.941	29.926	29.963	29.984	29.887	29.924	30.005	29.867	29.897	29.855	28.018	+25.500	25.733	22.077	29.714
Dec. ...	30.201	30.001	29.926	29.965	29.961	29.893	30.021	29.951	29.902	29.876	28.068	+25.498	25.739	22.043	29.857
Means of year }	29.8515	29.8457	29.839	29.853	29.805	29.809	29.783	29.707	29.711	29.698	27.925		25.684	22.046	29.781

The mean monthly pressure at Calcutta for 1843–44 is from reductions of Dr. M'CLELLAND, and the record commences in November 1843 and ends in October 1844. An inspection of the above Table shows that the maximum mean monthly pressure does not occur in the same month in successive years. At Madras, in 1842 and 1843, the maximum pressure was in the month of December, in the two following years it was in the month of January. At Bombay, in 1843, the maximum was in December, in 1844 in January, at Calcutta, in 1843, in December, and in 1848 it was in December. At Madras the minimum monthly mean pressure, in three years out of the four, occurred in the month of June. In 1843 it was in the month of May, but at Bombay in the same year in June: at Bombay and Calcutta in 1844, and in 1848, at Calcutta, it took place also in June. At Poona, at 1823 feet, the maximum was in January and the minimum in July. At Dodabetta, at 8640 feet, unlike any of the stations at the sea-level, the maximum mean monthly pressure took place in March: the same thing occurred at Mahabuleshwur § in 1828–29, at 4500 feet above the sea; but like the other stations the minimum pressure at Dodabetta occurred in June, but at Mahabuleshwur in July. Dr. BUIST of Bombay mentions that for three years at Aden (1846–48) the minimum occurred in July, and the maximum in February, but by the preceding Table the maximum in 1847 and 1848 occurred in December, and the minimum in both years in July. At Greenwich the means of thirty years' observations, recorded in BELVILLE's Manual, give results inverse to those in India, as the maximum pressure is in June and the minimum in November; but there is a second maximum in January and a second minimum in March; but the greatest *daily* mean pressure occurs about the 9th of January, and

* Thirteen days' observations wanting.

† For 1828, remaining months for 1829.

‡ For 1848, remaining months for 1847.

§ The means of one year's pressure at Mahabuleshwur is inserted from the Bombay Medical Journal, but the observations do not appear to have been corrected for temperature, and as the maximum pressure is represented to occur in October and the minimum in September, there evidently must be some mistake, and I therefore only notice my own simultaneous observations with Dr. WALKER for 1828–29.

the minimum daily mean depression at the end of November. From the means of eight years' observations, obligingly furnished to me by Mr. GLAISHER, from the Royal Observatory, Greenwich, the maximum was in October; with a second maximum in December, the minimum was in April; corrected however for the tension of vapour, the maximum was in December and the minimum in August; but consecutive years differ greatly when the vapour correction is applied.

I attempt no explanation of these anomalies, as much more lengthened observations are required than those at my disposal afford for philosophical deductions. From the month of the maximum pressure, there is a gradual mean monthly decline of pressure until the minimum pressure; then there is a gradual monthly increase of pressure, even when the monsoon sets in at Madras in October until the maximum is attained again; but there is no rule without an exception, and the curve was interrupted in the month of December in the years 1844 and 1845 at Madras, and in 1844 at Bombay; but in Calcutta, in October 1844, and at Mahabuleshwur, and at Doda-betta in December 1828 and 1847 respectively. At Aden, in 1847, it was interrupted in February. In 1848 the curve was regular.

Appended to this paper are annual pressure curves at Madras, Bombay and Calcutta, protracted from the records of the barometer at the several places. The three localities are under very different conditions of temperature and moisture in the same months, and they differ considerably in latitude and longitude; Madras and Bombay, situated upon opposite sides of the peninsula of India, are subject to different monsoons; Madras to the N.E. monsoon, which commences in October and ends in February, and Bombay is subject to the S.W. monsoon, which commences in June and ends in October. Calcutta is under the full influence of the S.W. monsoon, but has occasional showers from the Madras rains. When Bombay is deluged with rain Madras is comparatively dry, and the hot weather prevails; and when Madras is under the influence of the Coromandel rains, Bombay is cold and dry. Very different atmospheric conditions therefore exist at the two places in the same months. Nevertheless the annual curves of pressure protracted from monthly means, may be said to be identical, not only for Madras and Bombay, but also for Calcutta. The few exceptional cases are lost or disappear in the means, and may originate in local causes. It would thus appear that the periodic movements of the *mass* of the atmosphere within the northern tropic are independent of, or only very slightly affected by, the hygrometric and thermometric conditions of the *lower strata of the atmosphere*. It occurs to me that this phenomenon may be owing to the sun's place in the ecliptic. When the sun is at the southern tropic, the air above the northern tropic is comparatively cold and therefore dense, and at its greatest pressure; this would be from December to January inclusive. As the sun returns to the north the air gets gradually warmer and dilates, and the pressure being inversely as the volume, the barometer gradually sinks, until the sun is returning from the northern tropic again, when the mass of the atmosphere gradually cools, gets denser, and the pressure gradually increases again. Supposing this explanation to have any foundation in truth, the curve of pressure within the tropic south of the equator would be the reverse of that in the tropic north of the equator. The maximum pressure would be from June to July inclusive,

and the minimum pressure from December to January inclusive. On referring to the St. Helena observations for 1843 this is very nearly the case, the maximum pressure and minimum temperature being in August instead of July, and the minimum pressure and maximum temperature in February to March instead of January.

Meteorological Observations at St. Helena, 1843.

	Barom. in.	Therm. °
January . . .	28·238	64·76
February . . .	28·214	66·25
March . . .	28·214	65·35
April . . .	28·251	64·93
May . . .	28·292	61·53
June . . .	28·335	58·88
July . . .	28·341	57·43
August . . .	28·358	56·71
September . . .	28·328	56·92
October . . .	28·279	57·23
November . . .	28·250	59·54
December . . .	28·251	62·44*

From the occurrence of the maxima and minima of the horary oscillations of the barometer at the same *local hours* in different meridians, these phenomena also would seem to be connected with the sun's action upon the atmosphere.

In the fitful movements of the atmosphere beyond the northern tropic the sun's influence would not appear to be similarly felt, as the maximum pressure at Greenwich is in October, with a second maximum in December, and the minimum pressure, instead of being when the sun is at the northern tropic, is in November.

Temperature.

The hourly observations at Madras for four years afford the most complete and trustworthy data for determining at that place the fluctuations of temperature, the exact diurnal and annual range, and the exact periods of the fluctuations in the occurrence of the maxima and minima, which cannot be satisfactorily shown by any observations short of hourly record. For Bombay I have only such records made by Dr. Buist for the years 1843 and 1844. Those at Calcutta were two-hourly only during the daytime, and those at Dodabetta were made but twice a day, with the exception of twenty-four hours once a month between the 21st and 22nd, when the observations were taken hourly. For exact determinations I am constrained to abandon many proposed comparisons, and in many instances to use less frequently recorded observations to express general features. The following Table shows the monthly means and annual temperatures at Madras and Bombay from hourly observations. For the other localities the means are derived from less frequent observations, and are therefore only approximations to the truth.

* The above table is confirmed by the Mauritius and Cape observations; the minimum pressure being from January to February inclusive, and the maximum July to September inclusive.

Monthly Mean Temperatures at Madras and Bombay from Hourly Observations, and at Calcutta and elsewhere
from less frequent periods of record.

	Madras.			Bombay, 35 feet.		Calcutta, 18 feet.			Phul- ton, about 1700 feet.	Bija- pur, about 1700 feet.	Poona, 1823 feet.	Sattarah, 2320 feet.	Mahabuleshwur, 4500 feet.			Mercara, Coorg, 4500 feet.	Ultra Mullay, 4500 feet.		Dodabetta, 8640 feet.
	1842.	1843.	1844.	1845.	1843.	1844.	1845.	1846.	1847.	1848.	1830.	Means of 1844-47.	1828-29.	1834.	1829 to 1843.	Means of 1838-40.	1845.	1846.	
Jan.	76.98	77.59	75.71	76.81	76.4	74.9	76.52	75.72	75.95	73.70	75.4	70.0	65.38	65.70	64.0	66.6	62.08	63.42	52.20
Feb.	77.92	77.90	77.74	79.41	78.1	76.0	78.55	76.78	76.08	80.20	78.85	72.6	68.30	67.50	66.3	70.80	64.25	65.75	52.30
March	82.36	81.52	82.20	83.13	79.9	79.3	89.05	87.85	87.90	88.08	85.9	77.8	74.68	74.00	71.5	73.7	69.58	65.58	54.55
April	85.67	85.19	86.34	86.58	84.3	83.9	87.97	91.85	88.60	88.08	87.95	80.6	75.71	74.40	74.5	73.1	68.42	67.83	56.20
May	88.09	85.02	86.98	88.63	85.9	85.7	90.67	88.67	89.93	91.80	86.70	80.1	74.79	73.90	72.4	71.9	67.83	65.33	57.20
June	88.05	85.52	88.58	86.64	85.4	85.0	88.12	87.00	87.50	89.85	83.65	77.0	68.08	66.30	66.3	69.1	65.08	62.33	52.50
July	86.68	85.79	85.76	86.12	83.1	81.8	86.25	86.88	85.88	86.03	76.95	73.8	65.43	64.90	63.2	67.1	66.00	63.83	52.65
Aug.	84.45	84.51	85.25	85.88	81.1	81.4	85.20	86.28	85.82	85.18	76.85	73.0	64.60	65.30	63.2	66.0	66.66	64.42	53.10
Sept.	82.36	84.26	82.97	83.91	81.2	80.5	87.70	85.35	85.66	86.10	78.35	74.0	64.34	65.00	63.9	65.6	67.50	64.92	52.45
Oct.	81.86	80.72	80.53	82.95	82.3	83.4	85.30	83.70	84.58	83.55	80.65	76.1	65.68	65.50	66.6	66.8	65.58	63.42	53.30
Nov.	78.27	77.83	79.17	79.11	80.2	80.4	80.42	80.65	77.88	78.50	77.15	72.0	64.76	63.50	64.4	66.5	65.58	63.92	52.15
Dec.	76.57	75.89	76.94	77.70	76.6	79.3	73.55	73.08	73.28	75.30	75.05	71.8	63.56	62.30	63.2	64.4	63.50	63.17	50.80
Year	82.44	81.81	82.35	83.07	81.2	80.9	84.11	83.65	83.26	83.86	80.28	75.0	67.52	67.30	66.60	68.6	66.00	64.49	53.20

* Means of two-hourly observations. The mean temperature at Madras, in 1848, was 83°.15, and the curve the same as that of the preceding years.

Sattarah is in Lat. 17° 40'. Long. 74° 2'. Mercara is sixty-five miles E. of Cannanore. Ultra Mullay, twenty-two miles N.E. by E. from Trevandrum.

The mean temperature at Mahabuleshwur, from 1829 to 1843, was deduced from the mean of a daily maximum and minimum thermometer in the shade.

The first feature is the close approximation of the annual means at Madras for the four years; the difference between the greatest and least being only $1^{\circ}26'$ FAHR., although the difference between the means of the same month in successive years may have exceeded three degrees. Nevertheless the means of most of the months in successive years show a great amount of uniformity. The coldest months are December or January, and the hottest May or June. In 1842 and 1843 December was the coldest month. In 1844 and 1845 January was the coldest. In 1842 and 1845 May was the hottest month. In 1843 the maximum mean heat was not reached until July, and in 1844 it occurred in June. Although these discrepancies occur, the monthly increment or decrement to or from the maximum period is gradual, and rarely checked or inverted. In Bombay the annual mean temperature for 1843 is almost identical with that of Madras for 1843, although the two places differ $5^{\circ}41'$ in latitude. The monthly means of the two places correspond tolerably well from January until July; for instance, February corresponds within a tenth of a degree, and June within two-tenths. But in July there is a difference of $3^{\circ}79'$, August $3^{\circ}31'$, September $3^{\circ}16'$, Madras being plus: then the sign changes and Bombay becomes plus for the remaining months of the year, and also in January. The coldest month in Bombay was January, and the hottest May. In Bombay, in 1843 and 1844, unlike Madras, there is an inversion in the decrement of heat on the sun's going south; for October is represented as hotter than September in both years: a very remarkable fact is exhibited by these hourly means, *namely, that neither the south-west monsoon at Bombay, nor the north-east monsoon at Madras, at all affect the monthly mean regular increment or decrement of heat*, with the exception of October in Bombay. It will be observed that the maximum heat, both at Madras and Bombay, was not when the sun was vertical at either place passing to the northern tropic; but when the sun was near to the northern tropic in June, the mean temperature in 1843 at Madras, in lat. $13^{\circ}4'$, was at its maximum in July, while at Bombay, in lat. $18^{\circ}55'$, the greatest heat was in May of that year; and though the sun passes over both places again to the south the mean temperature gradually declines in each year, with the exceptions noticed. I was desirous of inserting in the Table of Mean Temperatures those for 1843 and 1844 at Calcutta, for comparison with the Madras and Bombay observations for the same year; but on referring to the volumes of the Bengal Asiatic Researches for 1843 and 1844, I found that the tables had not been inserted. They were met with however in the Journal of the Horticultural Society of Calcutta, but proved to be records of the temperature during the day only at $9\frac{1}{2}$ A.M., noon, 4 P.M. and sunset*. A mean temperature from such data would neces-

* In the fifth volume of the Calcutta Journal of Natural History, Dr. M'CLELLAND states that the mean temperature of 1844, from daily observations, was $82^{\circ}35'$, the coldest period at sunrise, and the hottest at $2^h 40^m$ P.M. daily; the maximum heat 104° on the 10th of April, the minimum $51^{\circ}7'$ on the 19th of January, and the annual range $52^{\circ}3'$. He speaks also of the minimum daily pressure of the barometer occurring twice at 6 P.M. in January, February and May.

sarily be unsatisfactory. In 1847 meteorological tables appear in the Journal of the Asiatic Society of Bengal from the same source as those used by the Horticultural Society in the preceding years, namely, from the office of the Deputy Surveyor-General in Calcutta, but they were even of less use than the preceding tables, for they had but two daily records, namely, at $9\frac{1}{2}$ A.M. and 4 P.M., and the records were otherwise useless for determining the absolute range of the thermometer, as there was only a record of the maximum temperature and no minimum. In 1848 more elaborate tables make their appearance, containing two-hourly records from sunrise to sunset, at sunrise, $9\frac{1}{2}$ A.M., noon, $2^h 40^m$ P.M., 4 P.M., and sunset, together with the indications of a maximum and minimum thermometer, but without any observations after sunset. This is an improvement upon the former records, but falls short of the requisites for scientific purposes. The maximum and minimum thermometer certainly gives the range of temperature, but does not give the hours of the occurrence of the maxima and minima. Mean temperatures deduced from a maximum and minimum thermometer may possibly be true; but an arithmetical mean from two extreme observations daily would be incorrect, unless the increment and decrement of heat from a mean point were regular, which is known to be rarely the case. Annual mean temperatures deduced from formulæ, of which the latitude is the element, are often fallacious; for independently of the great discrepancies in mean temperatures between America and Europe on the same parallels of latitude, and as indicated also by isothermal lines in Europe, there are places differing little in longitude where the annual mean temperature is higher than at places nearer to the equator, both within and without the tropics: taking an instance from Dove's temperature tables, we have the following:—

1 Aberdeen .	Latitude $57^{\circ} 9'$	Mean temperature $49^{\circ} 18'$
2 Dundee . .	Latitude $56^{\circ} 27'$	Mean temperature $51^{\circ} 94'$
3 Edinburgh .	Latitude $55^{\circ} 58'$	Mean temperature $47^{\circ} 13'$
4 Liverpool .	Latitude $53^{\circ} 25'$	Mean temperature $50^{\circ} 80'$
5 London . .	Latitude $51^{\circ} 30'$	Mean temperature $50^{\circ} 83'$, or $49^{\circ} 7'$ by GLAISHER.

Aberdeen, therefore, nearly six degrees north of London, has almost the same mean temperature as London: and Edinburgh, intermediate between both, has a lower mean temperature than either. Dundee, five degrees north of London, has absolutely a higher mean temperature; and Liverpool, two degrees north of London, has the same mean temperature. In the tropics similar instances are found.

Calcutta	Latitude $22^{\circ} 34' 40''$	Mean temperature $83^{\circ} 72'$
Bombay	Latitude $18^{\circ} 55' 42''$	Mean temperature $81^{\circ} 1'$
Madras	Latitude $13^{\circ} 4' 10''$	Mean temperature $82^{\circ} 42'$
Aden	Latitude $12^{\circ} 46' 26''$	Mean temperature $80^{\circ} 2'$

In this case Calcutta, 9 degrees north of Madras and $3\frac{1}{2}$ of Bombay, has a higher mean temperature than either; and Aden, in a lower latitude than any of the places,

instead of a higher has a lower mean temperature than Calcutta, Bombay or Madras. This mean temperature at Madras and Bombay is deduced from hourly observations; and had DOVE's previously noticed mean temperature been derived from similar records, the anomalies might have been modified or have disappeared altogether. With the reservations contingent upon the explanations given, I shall comprise my observations upon the temperature tables of places other than those for Madras and Bombay, within very narrow limits. And first, with respect to Calcutta, the means are derived from observations taken at the Surveyor-General's office, with excellent instruments, but the observations are day observations, and it is only for the last six months of 1848 that records were made every two hours. The means, however, of those six months correspond sufficiently near with those of the same six months in the three preceding years to show that the conversion of three-hourly observations into two-hourly observations, had very little effect upon the mean results. What was wanting were observations at night, and of these there are not any. The mean temperature of the years, from 1845 to 1848, both inclusive, never falls below $83^{\circ}26$ (in 1847), and was as high as $84^{\circ}11$ in 1845, and the mean temperature of the four years is $83^{\circ}72$. The coldest month was December, excepting in 1848, when January was the coldest. The hottest month was May, excepting in 1846, when April was the hottest; the mean temperature of April and May, for the four years, being respectively $87^{\circ}12$ and $90^{\circ}27$. For the years 1846 and 1847 the mean monthly increment to the maximum heat, and mean monthly decrement to the minimum heat, is gradual and regular, but in 1845 and 1847 there are instances of inversion; in 1845 March is hotter than April; September is hotter than August; in 1848 September is also hotter than August, instead of being cooler. This very high mean temperature of Calcutta is not of ready explanation, even after making an allowance for the want of night observations: situated on a broad river, in the midst of cultivated, well-wooded and moist plains, and within the influence of the sea; on the verge of the northern tropic, it might have been supposed that its position, in respect to latitude, would have affected its mean temperature. The sun is vertical at Calcutta in the first week in June and July, but Calcutta attains its maximum heat in May, while the sun is yet approaching, and the mean temperature is actually diminishing, while the sun is passing and repassing from the tropic, and while the heat ought to be accumulating from the lengthened days, as is shown in the following tabular statement:—

Latitude N.		Longitude E.	Sun vertical.	Longest day.
Calcutta	$22^{\circ} 34' 40''$	$88^{\circ} 28' 15''$	June 5—July 7.	h m 13 23
Bombay	$18^{\circ} 55' 42''$	$72^{\circ} 54' 24''$	May 15—July 28.	13 08
Madras	$13^{\circ} 4' 10''$	$80^{\circ} 21' 35''$	April 25—August 18.	12 46
Aden.....	$12^{\circ} 46' 26''$	$45^{\circ} 15' 0''$		

Admitting that the lengthened time for which the sun is nearly vertical over Calcutta might raise the mean temperature of May, June and July, there would be a corresponding diminution in the months of December, January and February, in the long nights, and the mean temperature of the year should not be raised by the

great heats of April and May; but such, nevertheless, would not appear to be the case, at least in the absence of night observations. It will be remarked also, that the return of the sun over Bombay in July does not prevent a reduction of three degrees in mean temperature; this however might be attributed to the rains commencing; but in its passage over Madras in August the continued mean monthly fall of the thermometer is not interrupted, although it will be recollected there is not any monsoon at Madras in August to correct the effect of the sun's passage. The slanting rays of the sun, within certain angles of incidence without the tropics, produce a heat of great intensity, even where the physical characters of the country do not lead us to expect such a result; for instance, on the banks of the broad Indus in Scinde the thermometer is known to rise in the shade to 110° — 120° FAHR. Recently, at Peshawur, the following is the record of the thermometer in a house from the 7th to the 14th of May 1849:—

	Sunrise.	8 A.M.	Noon.	4 P.M.	9 P.M.	Latitude N.
Highest	75	89	102	104	86	33 59
Lowest	67	84	96	98	78	

On the 16th of June, at 4 P.M., thermometer 109° ; on the 25th of July, 109° ; August 3rd, 4th and 13th, 104° ; the extremes being 67° and 104° , the mean 87° , and the range 37° . A register for the whole month, by Dr. J. MALCOLMSON, gives the following facts; the maximum being 109° on the 6th in a tent, and 106° on the 24th and 31st in a house.

Register of the Thermometer at Peshawur for May 1849.

FAHRENHEIT Thermometer. In the shade in a house in the city.							
Date.	Sunrise.	Noon.	4 P.M.	Date.	Sunrise.	Noon.	4 P.M.
1	63	79	66	17	70	98	104
2	65	73	66	18	73	99	100
3	64	87	91	19	81	97	103
4	63	75	85	20	80	102	105
5	61	96	91	21	83	99	105
6	64	102	109	22	91	102	105
7	90	100	104	23	85	100	104
8	67	108	99	24	88	98	106
9	70	107	101	25	82	99	105
10	70	104	100	26	85	99	104
11	65	102	95	27	84	99	104
12	70	106	106	28	84	97	103
13	70	107	104	29	85	99	104
14	71	100	101	30	84	99	105
15	90	106	98	31	88	102	106
16	70	104	105				

Note by Dr. J. MALCOLMSON.—The register was kept in a house, and that may account for the maximum being no higher than 109° .

Heavy rain, with thunder, lightning and hail, for the first four days in the month. Winds generally westerly; and south-west. Severe dust-storms occasionally.

On the 22nd of May 1849, at Ferozepore, lat. $30^{\circ} 53'$, on the Sutlege river, the thermometer stood at 104° in a good house, the usual precautions being taken against the hot winds. Even in August, at Peshawur, with thunder-storms and heavy rain on the 7th, 9th, 15th, 16th, 17th, 20th and 29th, and with several light showers besides, the maximum was 104° at 4 P.M., the minimum at sunrise 81° , the midday maximum 101° , and with a *midnight* maximum of 100° , and yet the Report says, "The month had not however been characterized, as would be supposed by the indications of the thermometer, by any unusual degree of heat *over those which had just preceded it*. It even did not range so high as in May, June and July*."

Even at Alten, in Finmark in Norway, in latitude $69^{\circ} 58'$, where the mean annual temperature is between 35° and 36° FAHR., and where in 1846 the thermometer sunk to $14^{\circ} 8$ below zero; on the 27th of July 1847, at noon, the thermometer in the shade rose to $84^{\circ} 7$ FAHR. Capt. SCORESBY, in his Arctic Voyages, somewhere mentions, I think, that the rays of the sun were sufficiently powerful to melt the pitch on the sunny side of his vessel, while the air was at a freezing temperature on the shady side. But these intense heats of summer are compensated for by a depression of temperature when the sun is at the southern tropic and the mean annual temperature is not raised. The high mean temperature of Calcutta, therefore, would seem to be influenced by local causes, independently of the vertical or oblique action of the sun, or the length of time the sun is above the horizon. But the anomalies of mean temperature are not limited to Calcutta. Aden, situated in latitude $12^{\circ} 46' 26''$ N., longitude $45^{\circ} 15'$ E., on the shores of Arabia,—shores dreaded for their supposed intolerable heat, has a mean temperature ($80^{\circ} 2$) lower than that of Bombay, Madras or Calcutta, with a less range of the thermometer, with a maximum heat only of 89° in May and October, and a minimum of $68^{\circ} 5$ in January! and most singularly the mean temperature for *every month* in 1848 is lower than the temperature of the corresponding month at Calcutta, and, with the exception of November, December and January, lower than at Madras or Bombay. From the maximum mean monthly heat in April 1830, at Calcutta, the temperature gradually declined until the mean monthly minimum in December, excepting in September and October, when the curve was interrupted by a rise of $1^{\circ} 5$ in the former month, and of $2^{\circ} 3$ in the latter; after December the temperature gradually rose to its mean maximum.

* Dr. WALLIN of Helsingfors, the traveller in Arabia, has furnished me with a copy of his register of the thermometer, from which I learn that at Bagdad, lat. $33^{\circ} 20'$ N., long. $44^{\circ} 24'$ E., the thermometer in the shade of a house on the second story, with a N.W. aspect upon the banks of the Tigris, on the 19th of July 1848, at 2 P.M. stood at $122^{\circ} 9$ FAHR., wind W.N.W., on the 13th and 18th at $120^{\circ} 2$, and on seven other days in the month of July at $118^{\circ} 4$; and the *lowest* heat in the month at 2 P.M. was $101^{\circ} 3$ on the 2nd of July, wind N.W., clear.

Curves of Temperature at High Levels.

Proceeding from the sea-level to considerable altitudes, we find that the mean temperature at Poona, in 1830, at 1823 feet above the sea-level, was $80^{\circ}28$, differing only six-tenths from the mean temperature of Bombay for 1844, and less than one degree of FAHR. from the mean temperature of 1843 at Bombay. A difference of level of 1823 feet therefore gave a difference of less than one degree of temperature.

The comparison of the temperatures of the three stations of Mahabuleshwur, Mercara in Coorg, and Uttray Mullay in Travancore, all at the height of 4500 feet above the sea-level, and differing little in longitude, but several degrees in latitude, affords some interesting facts. The latitude of Uttray Mallay is *about* $8^{\circ}00'$ N., long. $76^{\circ}00'$ E., Mercara, lat. $12^{\circ}30'$, long. $73^{\circ}30'$, Mahabuleshwur, lat. $17^{\circ}58'$, long. $73^{\circ}29'$. The mean temperature of Uttray Mullay, situated nearest to the equator, is *lower* by $3^{\circ}35$ than the mean temperature of Mercara, and $2^{\circ}66$ lower than that of Mahabuleshwur, while the mean temperatures of Mercara and Mahabuleshwur are almost identical. The maximum heat of Mahabuleshwur, in both years recorded, is in April, while the mean of three years at Mercara fixes it in March. At Uttray Mullay the maximum heat, in 1845, was in April, while in 1846 it was in March. At 4140 feet above these levels, namely, at Dodabetta, the maximum heat was, in May, as in Bombay and Calcutta. The minimum heat, at the height of 4500 feet, was in December, at Mahabuleshwur and Mercara in all the years. At Uttray Mullay, in 1845, the minimum mean monthly heat was in June, the only instance of the kind in all the observations discussed in this paper; and in this same year January and October had the same mean temperature. In the next year the minimum heat was in January. At Dodabetta, at 8640 feet, in 1847, the minimum temperature was in December, but, with the exceptions of April and May, the mean monthly range was so small that most of the months were nearly the same in their mean heat. Mahabuleshwur and Mercara being within the S.W. monsoon, while Uttray Mullay and Dodabetta are subject to both S.W. and N.E. monsoons, the annual curves of temperature might be expected to vary considerably. At Mahabuleshwur and Mercara there is a gradual decrement of heat from the maximum point until the month of October, when the mean monthly temperature rises at both places, but falls after that month to the minimum of the year. The same thing occurs in Bombay and Poona, but there is no rise in October at Madras nor at Calcutta in 1846 or 1847; and in 1845 and 1848, when the gradual decrement of heat was interrupted, it occurred in September instead of October, as at the other places within the influence of the same monsoon. At Uttray Mullay and Dodabetta, within the influence of both monsoons, we find the annual curve of temperature interrupted in the first place in March, then in July, October and November in 1845, and in July, August and September in 1846. At Dodabetta the increase is quite gradual from December to May; but the natural decrement is interrupted in July, and the temperature rises in August, sinks in September, rises in October, and then falls to the minimum of the year. From these facts, it appears that the two monsoons derange the

curves of temperature at the different places, which might be expected otherwise to coincide with the sun's path in the ecliptic; but further observations from the hill stations may modify these conclusions.

Range of Temperature and Hours of Maxima and Minima.

Unless where hourly observations are taken, the exact periods of the occurrence of the maxima and minima cannot be determined; and though a self-registering thermometer will determine the range of temperature, yet the hours of the turning-points cannot be learnt from it. For the hours of the occurrence of the maxima and minima, therefore, I am limited to the hourly observations taken at Madras and Bombay; but for the range of temperature, without reference to specific times, I can avail myself of a maximum thermometer registered at Aden for 1848, and one at Calcutta registered since June 1848.

Madras.—Hours of Maximum and Minimum Temperature.

The usual impression in India is, that the maximum heat occurs about 2 p.m., and the minimum a little after sunrise. The Madras hourly observations for four years, from which the following Table is prepared, exhibit great anomalies in the hours of the occurrence of the maxima and minima. In the forty-eight months of monthly range there are seventeen records of the greatest heat occurring about noon (between 11^h 41^m and 12^h 41^m); eighteen records between 1 and 2 p.m., and thirteen records between 2 and 3 p.m. In 1842 the maximum daily heat occurred only *once* about noon (12^h 41^m) on the 20th of April (96°·5). In 1843 it occurred at the same hour, or before it, in January, February, March, May and November. In 1844, at the same hour, in January, April, November and December. In 1845, in March, April, May, September, October, November and December; so that in fact the maximum daily heat, although it took place only at noon in the month of April in 1842, in the other three years, either in one year or the other, occurred at noon in all the months. There is not a single instance of the maximum daily heat occurring after 2^h 41^m p.m. The minimum heat of the month, in 1842, occurred in May at 2^h 41^m a.m. (81°·1), the only record of the kind in the year. In 1843 there is no similar record, but in 1844 there are two instances of the least heat taking place in July and August at 41^m past midnight, and one instance at 2^h 41^m a.m. in September. In 1845 there is a solitary case of the lowest temperature at 2^h 41^m a.m. In the whole four years there are only three instances of the minimum heat being at 3^h 41^m a.m., two at 4^h 41^m a.m., twenty-five at 5^h 41^m a.m., and twelve at 6^h 41^m a.m.; and one singular instance of the greatest cold in the month being as late as 7^h 41^m a.m. on the 14th of May 1844. It may be affirmed, therefore, that the greatest cold occurs at Madras most often at 5^h 41^m a.m. The monthly range of the thermometer does not differ very much in the different months of the year, nor does the range of the

thermometer in the same months in successive years exhibit very marked discrepancies, although there are some differences; for instance, in February 1842 the range was $20^{\circ}8$, and in February 1845 only $14^{\circ}2$. In April 1842 the range was $22^{\circ}0$, and in April 1843 only $14^{\circ}8$. It might have been expected that the greatest range, as at Bombay and Calcutta, would have occurred in the coldest months, and that the greatest discrepancies would have occurred in the comparisons of the same months in succeeding years, but such was not the case. The greatest monthly range in any month at Madras, twice occurred in the month of May in 1844 and 1845, namely, $25^{\circ}9$ and $25^{\circ}6$ respectively. The least range, $12^{\circ}5$, took place in January 1843. The annual ranges varied only from $27^{\circ}0$ in 1843 to $37^{\circ}1$ in 1844*. For Bombay I have only hourly readings of the thermometer for 1843 and 1844. The maximum heat twice occurred at noon, on the 11th of August and 6th of December; twice at 1 P.M., on 31st of March and 29th of April; five times at 2 P.M., and three times at 5 P.M. There is not any conformity in the times or dates of these maxima with those of the same year at Madras. A remarkable feature in the minimum daily heat at Bombay is the comparatively late period of the morning at which it took place. On the 4th of January the minimum heat was at 8 A.M., in five other months it occurred at 7 A.M., in two months at 6 A.M., and once only at 5 A.M.; and entirely unlike the numerous instances at Madras, there is not a single case of the minimum heat falling before 5 A.M. in 1843. The greatest range of the thermometer was in the cold months and least in the monsoon; the maximum, $19^{\circ}3$, was in January, and the minimum, $8^{\circ}4$, in August. The annual range for 1843 was $24^{\circ}4$. In 1844 the chief feature is, that the minimum heat in the month occurred at midnight on the 28th of August, and the minimum temperature occurred at 1 and 2 A.M. on the 22nd of July, as well as at 5 A.M.; twice only the least heat occurred as late as 7 A.M.; the maximum heat never occurred after 3 or before 1 P.M. The maximum heat was $91^{\circ}9$ and the minimum $64^{\circ}7$, the annual range therefore $27^{\circ}2$. The greatest monthly range was in February, $20^{\circ}7$, and the least in August, $8^{\circ}5$. The monthly ranges of the two years had a close correspondence. The maximum heat observed in the sun in Bombay was $142^{\circ}6$ at 1 P.M., 10th of November 1846.

Calcutta Range of Temperature.

Hourly observations not having been taken at Calcutta, and the records of a maximum and minimum thermometer having only appeared in the Asiatic Journal since June 1848, I can only give the range of temperature since that date; but I know nothing of the days or hours of record, excepting what is derived from a foot-note at page 550 of the Number of the Journal for June 1848. As at Bombay, the greatest monthly range was in the cold months and least in the monsoon months. Two

* A memorandum just received gives the mean temperature of Madras for 1848 at $83^{\circ}15$; the maximum on the 20th of June 106° , at 3^h 41^m P.M., and the minimum $63^{\circ}5$ on January 26th, at 6^h 41^m A.M.; the annual range therefore was $42^{\circ}5$.

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Range of Thermom

	Madras, 1842.							Madras, 1843.							Madras, 1844.						
	Minimum.			Maximum.				Minimum.			Maximum.				Minimum.			Maximum.			
	Therm.	Hour. A.M.	Date.	Therm.	Hour. P.M.	Date.	Range.	Therm.	Hour. A.M.	Date.	Therm.	Hour. P.M.	Date.	Range.	Therm.	Hour. A.M.	Date.	Therm.	Hour. P.M.	Date.	
January	69°·5	^h 6 ^m 41	6	85°·1	^h 1 ^m 41	27	15°·6	70°·5	^h 5 ^m 41	5	83°·0	^h 0 ^m 41	4	12°·5	66°·2	^h 6 ^m 41	24	82°·8	^h 0 ^m 41	27	
February	67°·2	5 41	17	88°·0	2 41	28	20°·8	69°·2	6 41	9	85°·4	0 41	19	16°·2	67°·7	5 41	19	87°·2	1 41	26	
March	72°·4	6 41	10	92°·0	1 41	20	19°·6	74°·1	3 41	16	89°·8	0 41	26	15°·7	72°·0	6 41	10	90°·7	1 41	25	
April	74°·5	5 41	1	96°·5	0 41	20	22°·0	78°·5	4 41	9	93°·3	1 41	30	14°·8	77°·2	5 41	8	99°·0	0 41	3	
May	81°·1	2 41	1	100°·8	2 41	31	19°·7	76°·2	5 41	23	93°·8	^{A.M.} 11 41	1	17°·6	77°·4	7 41	14	103°·3	1 41	5	
June	78°·8	5 41	12	100°·2	2 41	21	21°·4	78°·7	3 41	14	95°·4	1 41	9	16°·7	80°·3	3 41	16	103°·2	1 41	11	
July	79°·1	5 41	5	98°·6	1 41	10	19°·5	78°·7	5 41	31	95°·5	2 41	21	16°·8	77°·8	0 41	21	96°·2	2 41	8	
August	75°·6	6 41	19	94°·8	2 41	1	19°·2	78°·2	5 41	28	95°·3	1 41	7	17°·1	78°·1	0 41	6	95°·3	1 41	23	
September ...	75°·7	6 41	12	92°·5	1 41	6	16°·8	76°·9	6 41	28	95°·3	2 41	1	18°·4	75°·4	2 41	18	94°·7	2 41	9	
October	74°·2	5 41	25	90°·5	2 41	3	16°·3	72°·9	5 41	23	87°·5	2 41	23	14°·7	73°·5	5 41	24	88°·5	2 41	25	
November ...	71°·0	5 41	21	84°·6	1 41	2	13°·6	69°·4	5 41	29	85°·2	0 41	1	15°·8	70°·9	5 41	19	86°·0	^{A.M.} 11 41	8	
December ...	68°·5	6 41	12	82°·8	1 41	25	14°·3	68°·0	5 41	31	82°·5	1 41	8	14°·5	69°·6	6 41	3	83°·5	0 41	2	
Year	67°·2	5 41	...	100°·8	2 41	...	33°·6	68°·0	5 41	...	95°·5	2 41	...	27°·0	66°·2	6 41	...	103°·3	1 41	...	

[Lieut.-Colonel SYKES's Discussion of the Meteorological Observations taken in India.]

maxima ranges of $21^{\circ}5$ and 21° occur in March and January, and the minimum, $9^{\circ}4$, was in August. The annual range was nearly double that of Bombay, namely, $41^{\circ}9$. The foot-note above alluded to has the following records:—

1846. Maximum heat in May	105 $\cdot$ 0
1846. Minimum heat in January	55 $\cdot$ 0
1847. Maximum heat in May	109 $\cdot$ 6
1847. Minimum heat in February	50 $\cdot$ 0

The range in these two years, therefore, being respectively 50° and $59^{\circ}6$ FAHR.; the thermometer rising higher and sinking lower than either at Madras, Bombay or Aden near the sea-level.

Range of Temperature at Aden.

Extending the comparison of thermometric range near the sea-level to Aden, in the latitude of Madras, but $35^{\circ}6$ to the westward of Madras, it will be seen that, unlike Bombay or Calcutta, the greatest range is not in the cold months, nor in the months in which the maximum heat occurs, but in those months in which the mean temperature is comparatively moderate, viz. March, April and October; the smallest range however occurring, as at Calcutta and Bombay, in what are called the monsoon months at those places, namely, May, June, July, August and September, while in those months at Madras the greatest range of the thermometer takes place. The greatest monthly range at Aden, $11^{\circ}2$, occurs in October, and the least monthly range, $6^{\circ}0$, is almost identical in the consecutive months of June, July, August and September. The annual range is $20^{\circ}5$. If the records of 1848 exhibit normal conditions, then the climate of the dreaded Aden is more equable than that of places on the sea-level on the coasts of India.

The annexed Table contains the elements of the preceding notices:—

Range of Temperature at High Levels.

With respect to the majority of the stations at different elevations above the sea, as hourly observations were not kept, and a maximum and minimum thermometer only used, at Mahabuleshwur and Dodabetta the *hours* of the occurrence of maxima and minima cannot be stated.

Sattarah.—Range of Thermometer.

At Sattarah, the results of four years' observations by Dr. MURRAY, from 1844 to 1847 inclusive, are as follows :—

	Minimum temperature.	Maximum temperature.	Maximum daily range.	Maximum monthly range.
January	53·0	86·0	23·0	31·0
February	50·5	90·0	26·0	33·0
March.....	62·0	100·0	32·0	38·5
April	64·0	102·0	31·5	36·5
May	70·0	103·0	26·0	29·5
June	69·0	93·0	22·0	22·0
July	60·0	91·0	7·5	11·0
August	60·0	80·0	10·0	10·0
September	64·0	83·0	16·0	19·0
October	63·0	93·0	21·0	30·0
November	58·0	85·0	21·0	25·0
December	57·0	86·0	25·0	27·0
Year	50·5	103·0	32·0	38·5

The minima and maxima in temperature represent the highest and lowest state of the thermometer ever indicated in January in four years, and the same for the other months; and the maxima and minima are not necessarily in the same year. The ranges represent the greatest and least in any particular day or month.

The maximum temperature, at 2320 feet, exceeded that recorded in Bombay, and the daily and monthly range were greater, but considerably less than at Mahabuleshwur at 4500 feet. In four years at Sattarah the extreme range of the thermometer was 52°·5, from 50°·5 to 103°·0, while at Mahabuleshwur in fifteen years the range was only 47°·0. The minimum temperature does not appear to have been below 50°·5, while I have observed it about the latitude of Sattarah, and at 400 feet lower level, at 40°·5 in January; and at 3123 feet in May I had it rise, at 3½ P.M., to 105°.

The following is extracted from a synopsis of fifteen years' observations at Mahabuleshwur, from January 1829 to December 1843, from observations successively taken by Doctors WALKER, MOREHEAD and MURRAY :—

Extreme daily range.	Extreme monthly range.	Extreme depression at night, thermometer exposed.	Months.	Maximum temperature.	Minimum temperature.	Range in fifteen years.
21·0	30·5	30·0	January	79·5	45·0	34·5
23·8	32·1	31·5	February	85·5	46·0	39·5
23·5	38·0	33·0	March	89·0	49·5	40·5
22·0	34·5	36·5	April	92·0	56·0	36·0
22·0	32·0	30·2	May	90·0	57·3	32·7
18·5	25·5		June.....	84·0	53·0	31·0
12·7	18·5		July	73·8	51·5	22·3
11·1	13·0		August.....	70·8	53·0	17·8
14·0	17·0		September	77·0	56·0	21·0
17·5	23·0	34·1	October	78·5	54·0	24·5
16·5	23·0	29·5	November	75·0	51·5	23·5
19·0	27·5	27·5	December	76·0	48·5	28·5
23·8	38·0	27·5	Year.....	92·0	45·0	47·0

Mahabuleshwur.—Range of Thermometer.

The range of the thermometer in fifteen years was 47° FAHR., from 92° in April to 45° in January, but it will be seen that the thermometer exposed to radiation at night had been as low as $27^{\circ}\cdot5$, and in every month of the year, excepting the four monsoon months, verged upon the freezing-point; the maximum heat in the sun was 110° in April and May, at 9 A.M., but at midday it had risen to 127° . The extreme daily range never exceeded $23^{\circ}\cdot8$, and the maximum monthly range was 38° FAHR. The record of the night thermometer at Mahabuleshwur giving instances of the temperature in five months of the year sinking below the freezing-point, although it is not described as being placed on the grass or ground, affords an explanation of an otherwise puzzling record in the Dodabetta observations, namely, "on the 15th, 16th, 17th and 22nd of November 1847, found ice around the office" (at sunrise?), although there is not any record of the thermometer below $45^{\circ}\cdot2$ on the 16th at Dodabetta, and in the *hourly* observations of the 22nd the thermometer only once sank to $48^{\circ}\cdot6$ at 11 o'clock at night; the extreme cold, therefore, must have been caused by radiation and evaporation*. An old officer, who had often resided at Mahabuleshwur, tells me that he has frequently witnessed hoar-frost on the ground at Mahabuleshwur, though he had never *remarked* the thermometer sinking below 45° . The highest temperature in the sun at Mahabuleshwur was 127° in March and May.

Dodabetta.—Range of Thermometer.

At Dodabetta the following are the maxima, minima and ranges:—

	Minimum temperature.	Maximum temperature.	Maximum daily range.	Maximum monthly range.
1847.				
February.....	$41^{\circ}\cdot2$	$61^{\circ}\cdot0$	$12^{\circ}\cdot0$	$19^{\circ}\cdot8$
March.....	$43^{\circ}\cdot5$	$67^{\circ}\cdot0$	$15^{\circ}\cdot5$	$23^{\circ}\cdot5$
April	$47^{\circ}\cdot0$	$66^{\circ}\cdot5$	$16^{\circ}\cdot7$	$19^{\circ}\cdot5$
May	$46^{\circ}\cdot0$	$65^{\circ}\cdot8$	$15^{\circ}\cdot3$	$19^{\circ}\cdot8$
June	$44^{\circ}\cdot0$	$60^{\circ}\cdot0$	$11^{\circ}\cdot8$	$16^{\circ}\cdot0$
July.....	$44^{\circ}\cdot3$	$59^{\circ}\cdot0$	$13^{\circ}\cdot9$	$14^{\circ}\cdot7$
August	$44^{\circ}\cdot5$	$59^{\circ}\cdot1$	$14^{\circ}\cdot1$	$14^{\circ}\cdot6$
September	$41^{\circ}\cdot8$	$59^{\circ}\cdot0$	$10^{\circ}\cdot6$	$17^{\circ}\cdot2$
October	$42^{\circ}\cdot8$	$58^{\circ}\cdot8$	$10^{\circ}\cdot3$	$16^{\circ}\cdot0$
November	$42^{\circ}\cdot9$	$61^{\circ}\cdot1$	$13^{\circ}\cdot1$	$18^{\circ}\cdot2$
December	$41^{\circ}\cdot0$	$60^{\circ}\cdot4$	$15^{\circ}\cdot5$	$19^{\circ}\cdot4$
1848.				
January	$38^{\circ}\cdot5$	$62^{\circ}\cdot8$	$19^{\circ}\cdot5$	$24^{\circ}\cdot3$
Year	$38^{\circ}\cdot5$	67	$19^{\circ}\cdot5$	$28^{\circ}\cdot5^{\dagger}$

Hence it is seen that the Dodabetta temperature, at 8642 feet, compared with that of Mahabuleshwur at 4500 feet, has a decidedly diminished daily, monthly and annual range. It so happens that the lowest but not the highest state of the thermo-

* There is not any record of the radiation thermometer in November at Dodabetta, but in February it appears to have been twice below the freezing-point.

$\dagger$ Annual range.

meter, and the greatest daily and monthly range, occur in the same month at Dodabetta, namely, January 1848. The lowest temperature in the year was $38^{\circ}5$, the highest 67° , and the annual range $28^{\circ}5$. The greatest monthly range was $24^{\circ}3$.

Aqueous Vapour.

Preliminary to the discussion of the question of aqueous vapour, a few words are necessary on the caution requisite in generalizing on a limited number of facts, or on observations not extending over lengthened periods of time. The hourly observations of the wet bulb at Madras and Bombay, for 1843 and 1844, are apparently trustworthy and satisfactory, supposing the wet bulb theorem to be correct. Those at Calcutta were taken only twice daily, at $9^h 40^m$ A.M. and 4 P.M., and how far observations, taken twice only during the daytime, can be relied upon for the expression of normal conditions, will best be shown by the following comparison of observations of the wet bulb at Dodabetta at 8640 feet above the sea-level. The regular meteorological observations were recorded twice a day only, at $9^h 40^m$ A.M. and $3^h 40^m$ P.M., but on one day in each month independent observations were taken for twenty-four consecutive hours. In a table I have compared the means of the $9^h 40^m$ A.M. and $3^h 40^m$ P.M. observations with the means of the twenty-four consecutive hours for the same day. The discrepancies are considerable, and too numerous to admit of the supposition of their resulting from accident or carelessness. On the 21st and 22nd of January the hourly observations give a depression of the wet bulb of $9^{\circ}2$. The twice a day observations give a depression of $9^{\circ}4$; on the 21st and 22nd of March a depression respectively of $5^{\circ}7$ and $7^{\circ}7$; on the 21st and 22nd of April of $5^{\circ}2$ and $4^{\circ}2$; on the 21st and 22nd of May of $1^{\circ}8$ and $1^{\circ}33$; and on the 21st and 22nd of November of $7^{\circ}8$ and $6^{\circ}1$. A bare inspection of the several hygrometric records for Dodabetta will show the anomalous results in working out the dew-points, elastic force of vapour, and per-centage of vapour or fraction of saturation in the atmosphere by the tables founded on Dr. APJOHN's formula. The hourly observations also demonstrate, that on the same day, in a transient fog without rain, there shall not be any depression whatever of the wet bulb; while in the course of the twenty-four hours there may be a great depression. For instance, at noon, on the 21st of December 1847, there was not any depression, but at 9 the next morning there was a depression of $3^{\circ}2$, the mean being $1^{\circ}6$, while the mean of the twenty-four hours was $0^{\circ}7$. At the former hour there was a fog, at the latter, partly a blue sky. Again, at 5 P.M., on the 21st of November, the depression was $1^{\circ}5$, but at 8 P.M. the depression was $10^{\circ}9$, the mean being $6^{\circ}2$, the mean of the twenty-four hours $7^{\circ}8$; in both cases there was nearly a blue sky. At noon, on the 21st of July, there was not any depression in a fog; at 5 A.M. of the 22nd a depression of $2^{\circ}0$, and *yet it was raining*. On the 21st of April, at 8 P.M., the depression was nil, an hour after it was $5^{\circ}5$; at both hours with nearly a blue sky, while at 7 A.M. of the 22nd the depression was $9^{\circ}5$, with a cloudy sky. Supposing these to have been the only observations available for the respective days,

could any of them or their means have been safely taken to express normal conditions? Professor ORLEBAR, in his Meteorological Observations at Bombay for 1846, gives proofs of the necessity for caution in the use of the wet bulb: in the first place, he was obliged to abandon the records of the air-thermometer attached to the wet bulb from the irregular depression of the former, by the cold of the evaporating surface of the latter; and in the next place, on the comparison of the action of two wet bulbs, one inside the observatory and the other outside, he records discrepancies in March ranging from $3^{\circ}2$ plus to $2^{\circ}6$ minus; the comparisons in April and May exhibited minor discrepancies, excepting on the 27th of April at the 18th hour, when the discrepancy was 3° minus. Professor ORLEBAR having used DANIELL's hygrometer simultaneously with the wet bulb for eight months in 1846, the means of comparison are afforded, and the following Table exhibits the results. I have taken the two daily observations at $9^h 12^m$ A.M. and $3^h 12^m$ P.M. on the first day of each month for the comparison.

Dates.			Barometer.	Thermometers.		Dew-point by		Tension of vapour by		Fraction of saturation or per-centage of moisture in the air by	
Months.	Day.	Local hours.		Dry.	Wet.	Wet bulb.	DANIELL's hygrometer.	Wet bulb.	DANIELL's hygrometer.	Wet bulb.	DANIELL's hygrometer.
January	1st	h m	in.								
		9 12	30.092	79.0	72.8	70	66.8	0.726	0.655	75	67½
February ...	1st	3 12	29.942	83.5	73.3	68.6	69.9	0.695	0.724	62	64½
		3 12	29.987	76.3	71.6	69.4	73.0	0.713	0.801	80	90
March.....	1st	3 12	29.879	80.4	72.0	68.05	71.0	0.682	0.751	67	74
		9 12	30.003	79.6	73.5	70.8	75.1	0.746	0.856	75½	86½
April	1st	3 12	29.878	83.3	75.6	72.35	75.5	0.785	0.867	70	78
		9 12	29.899	84.4	75.4	71.6	73.5	0.765	0.814	66½	70½
May	1st	3 12	29.804	87.6	74.2	68.15	73.1	0.684	0.803	54	63½
		9 12	29.834	92.5	80.8	76.55	80.8	0.897	1.027	60½	69
June	1st	3 12	29.731	96.8	80.5	74.4	76.9	0.838	0.907	50	54
		9 12	29.748	83.2	80.8	80.0	81.5	1.000	1.049	90	94½
July	1st	3 12	29.668	90.7	84.4	82.45	81.5	1.081	1.049	78	75
		9 12	29.615	81.4	80.0	79.2	78.5	0.976	0.955	90	88½
August	2nd	3 12	29.585	81.8	79.0	78.0	78.8	0.939	0.964	88½	91½
		9 12	29.728	81.2	79.9	79.45	78.6	0.984	0.958	94½	92
September ...	1st	3 12	29.678	84.4	81.0	79.85	77.5	0.996	0.925	86½	80½
		9 12	29.735	84.0	80.0	78.6	77.5	0.957	0.925	84½	81½
		3 12	29.666	83.8	80.0	78.7	76.3	0.959	0.890	85	79

These observations having been made by the instructed and practiced manipulators of the Bombay Observatory, claim to be worthy of confidence; but we find ourselves in doubt which of the two sets to use for the correction of the barometer and to determine the real amount of moisture in the atmosphere. It would appear, that when the depression of the wet bulb and of DANIELL's hygrometer is small in the monsoon months, their results do not differ very widely, the wet bulb however giving a higher tension of vapour and greater per-centage of moisture or fraction of saturation in the atmosphere than DANIELL's hygrometer; but in the cold and hot months of the year

this is reversed; considerable discrepancies occur in the results, and the tension of vapour is much higher (with one exception) and the per-centage of moisture much greater by DANIELL's hygrometer than by the wet bulb, the dew-point by the two instruments differing from 3° to 5° . On the 1st of February, in the morning, the two instruments differ ten per cent. in the amount of moisture in the atmosphere. In the afternoon they differ seven per cent., in both instances DANIELL's hygrometer giving the greatest amount. On August the 2nd and September the 1st, in the afternoon, the instruments differ above six per cent., but at this period of the year the wet bulb gives the greatest amount of moisture in the atmosphere and DANIELL's hygrometer the least. The greatest depression of the dew-point in the above observations was $22^{\circ}\cdot4$ on the 1st of May at 3^h 12^m P.M. In a former paper in the Philosophical Transactions I mentioned a depression of 40° , from 67° to 27° , on the 13th of March 1828 at sunrise in the Hill Fort of Loghur, and of 61° on the 16th of February 1828 at 4 P.M. at Downde near Pairgaum on the Beema river.

I have put into juxtaposition with this table a comparison of the results of hourly observations at Dodabetta of the wet bulb, with the two regular observations made on the same day. In November the simultaneous dew-points by the two processes are $36^{\circ}\cdot1$ and $42^{\circ}\cdot15$; in January $30^{\circ}\cdot7$ and $33^{\circ}\cdot2$; in March $43^{\circ}\cdot3$ and $39^{\circ}\cdot6$, and in April $42^{\circ}\cdot2$ and $40^{\circ}\cdot95$. Whether therefore there be $59\frac{1}{2}$ or 69 per cent. of moisture in the atmosphere in November; 71 or $61\frac{1}{2}$ per cent. in March; or 73° or $78\frac{1}{2}$ per cent. in April, nothing short of continued hourly observations, like those at Madras and Bombay, will enable the meteorologist to determine.

Dodabetta.—Simultaneous observations, 8640 feet, on the 21st and 22nd of each month.

Depression of the wet bulb by				
	Hourly observations.	Twice a day, ditto.	Dew-point by hourly.	Dew-point by twice a day.
January	9·2	9·4	30·7	33·2
February	1·4	0·9	49·7	50·3
March	5·7	7·7	43·3	39·6
April	5·2	4·2	42·2	40·95
May	1·8	1·33	54·2	55·8
June	0·5	0·2	51·2	51·75
July	0·8	0·23	53·15	53·75
August	0·5	0·37	54·35	55·43
September	0·4	0·6	52·95	53·1
October	0·7	0·4	53·1	52·9
November	7·8	6·1	36·1	42·15
December	0·7	0·95	50·65	50·65

Any such high per-centage of moisture however was not the case on the plains of the Deccan, where the dew-point was determined by DANIELL's hygrometer.

The natives of the Deccan divide the year into three seasons—the Hewalla (cold), Oonalla (hot), and Pawsalla (wet) seasons ; the per-centage of vapour was in the

Cold months, Nov., Decem., Jan., Feb. 46½ per cent. of moisture.

Hot months, March, April and May 42½ per cent. of moisture.

Wet or monsoon months, June, July, Aug., Sept., Oct. . . 77·4 per cent. of moisture.

But another uncertainty is the numerical value to be given to the wet bulb readings, whether by APJOHN'S formula or by the means of factors, which Mr. GLAISHER of the Royal Observatory has adopted from a comparison of observations between the wet bulb and DANIELL'S hygrometer. Dr. MURRAY, in charge of the Sanatorium at Mahabuleshwur, at 4500 feet, observed with the wet bulb for nine years, and has given the mean monthly depression of the wet bulb for that period in the Journal of the Physical Society of Bombay. He does not give any details of the character of his instruments, or with what precautions he used them, and as from 250 to 300 inches of rain fall at Mahabuleshwur in the S.W. monsoon months, it might have been expected that the air would have been much nearer to a state of saturation in those months than appears to be the case at Dodabetta in the same months, in which only 29 to 30 inches of rain fall. Subjecting these observations to the two methods, it is seen that the results go very well together with small depressions of the wet bulb and at temperatures ranging from 65° to 75°, but with depressions above 10° they immediately diverge, and the divergence would increase with greater depressions and at lower temperatures of the air.

Mahabuleshwur.—Nine years' Means of the Wet Bulb.

	Dry.	Depression.	Dew-point by GLAISHER.	Dew-point by APJOHN.	Tension by GLAISHER.	Tension by APJOHN.	Per-centage moisture by GLAISHER.	Per-centage moisture by APJOHN.
January	69·2	9·8	54·5	53·35	·435	·418	61·4	59·0
February	69·6	13·0	50·1	47·4	·376	·341	32·3	47·5
March	75·3	15·7	51·75	49·35	·396	·365	45·9	42·3
April	78·1	15·4	55·0	53·8	·442	·424	46·9	44·9
May	76·4	9·5	62·15	62·4	·562	·566	62·9	63·3
June	70·7	2·6	66·8	67·0	·655	·658	88·1	88·5
July	68·8	1·8	66·1	66·2	·640	·641	91·5	91·6
August	67·4	1·7	64·68	64·95	·611	·615	91·4	92·0
September	67·9	2·7	63·58	63·95	·589	·596	86·7	87·7
October	68·2	6·4	57·96	58·25	·489	·493	71·4	71·9
November	67·3	7·7	54·98	54·95	·442	·442	66·4	66·4
December	66·9	7·8	54·42	54·35	·434	·432	66·0	65·7
Fifteen years ...	70·4	7·84	58·64	58·25	·500	·493	67·9	66·9

Bearing therefore these discrepancies in mind, it may be justifiable to generalize so far only as to assert that two or three (or even four) observations with any meteorological instrument within the twenty-four hours, even under favourable circumstances, can do little more than give approximations to the truth ; and that hourly observations, extending through four or five years at least, can alone satisfy the scientific

meteorologist, to enable him to determine the normal or abnormal atmospheric phenomena of the *locality* in which the observations are taken ; and he will distrust the applicability of these *local* determinations to any other places situated beyond a certain circumscribed area.

With these remarks I proceed to consider the hygrometric features of certain widely separated stations in India, both at the sea-level and elevated at 1800 and 4500 and 8640 feet above the sea. The most marked feature is the singularly small monthly mean depression of the wet bulb, the high figures of the elastic force of vapour, and the great per-centage of moisture in the atmosphere ; not only at the sea-levels of Madras, Bombay, Aden and Calcutta, but at Dodabetta, at 8640 feet above the sea, whether during the monsoons or during the cold and hot months, which are generally supposed to be the dry portion of the year, and the annexed Table exhibits the results.

At neither of the Presidencies is there a mean monthly per-centage of vapour at Madras below 67, at Bombay below 66, at Calcutta below 63, and at Dodabetta below 51, while the maximum at Dodabetta goes up to 98, and at Calcutta to 94 ; at Bombay it did not exceed 88, and at Madras, in two years, it only twice attained a mean maximum of 83 per cent. of moisture in the atmosphere. The mean annual per-centage of moisture in the air, it will be seen for the years 1843 and 1844, was at Madras respectively 75 and $74\frac{1}{2}$, at Bombay 76 and 76, at Calcutta 80 and 84, at Aden 71, at Mahabuleshwur $67^{\circ}9$, and at Dodabetta, for 1847-48, it was 90° . In the Deccan, in 1827, only 55° , as determined by DANIELL's hygrometer. The difference between the annual mean temperature of the air and the annual mean temperature of the dew-point, was—

Madras.		Bombay.		Calcutta.		Dodabetta.	Deccan.	Aden.	Mahabuleshwur.
1843.	1844.	1843.	1844.	1843.	1844.	1847-48.	1827.	1848.	Means of 9 years.
9	9.24	8.65	8.65	6.95	5.3	3.1	17.9	7.5	12.2

The air at Madras, Bombay and Calcutta, is unquestionably more moist than that of the interior ; but the feeling and experience so little lead one to expect the high dew-points indicated, particularly in the cold and hot months, that some inaccuracy of observation or fallacy in deduction might be feared, were not the ability and zeal of the observers at Madras and Bombay, combined with the observations being hourly, a sufficient guarantee against error. At Calcutta, however, situated sixty miles from the sea, the hygrometric observations make the air much more moist even than at Madras and Bombay. This is contrary to probability, and may be owing to the means of the observations, made only twice a day, not giving the real mean of the twenty-four hours. At the peak or ridges of Dodabetta the air would appear to be nearly in a constant state of saturation ; and there is a possibility in the circumstance, considering that it is the highest eminence in the peninsula of India and might be expected

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Months.			Mahabuleshwur, 4500 feet.				Dodabetta, 8640 feet.				Dodabetta, 8640 feet.				Month.
	and		Means of 9 years, 1835 to 1843 inclusive.				1847 to 1848. Monthly means.				Hourly observations, 21st to 22nd. Monthly, 1847-48.				
	Thermometer, dry.	Thermometer, of vapour.	Thermometer, dry.	Thermometer, wet.	Elastic force.	Per-centage of vapour.	Thermometer, dry.	Thermometer, wet.	Elastic force.	Per-centage of vapour.	Thermometer, dry.	Thermometer, wet.	Elastic force.	Per-centage of vapour.	
January	77°59	72°47	69°2	59°4	·435	61·4	52°1	46°15	·277	69	49°5	40°3	·190	51½	January.
February ...	77°90	71°36	69°6	56°6	·375	52·3	52°3	50°8	·379	93	52°0	50°6	·369	91	February.
March	81°52	75°42	75°3	59°6	·396	45·9	53°0	48°8	·323	78	53°3	47°6	·296	71	March.
April	85°19	78°36	78°1	62°7	·442	46·9	56°4	53°35	·393	84	52°4	47°2	·296	78	April.
May	85°02	79°52	76°4	66°9	·561	62·9	57°2	54°95	·423	88	57°9	56°1	·444	91¼	May.
June	85°52	78°34	70°7	68°1	·655	88·1	52°5	51°8	·391	96	52°0	51°5	·389	97	June.
July	85°79	76°34	68°8	67°0	·640	91·5	52°60	52°15	·398	97	54°4	53°6	·415	96	July.
August	84°51	76°34	67°4	65°7	·611	91·4	53°10	52°5	·401	96	55°1	54°6	·432	97	August.
September ...	84°26	77°32	67°9	65°2	·589	86·7	52°45	51°75	·390	96	53°6	53°2	·413	98	September.
October	80°72	76°53	68°2	65°8	·489	71·4	53°30	52°8	·406	97	54°2	53°5	·415	96	October.
November ...	77°83	71°54	67°3	59°6	·442	66·0	52°15	50°95	·378	94	51°0	43°2	·230	59½	November.
December ...	75°89	70°49	66°9	59°1	·434	66·0	50°80	48°75	·341	86	51°8	51°1	·382	96	December.
	81·81	75·55	70·4	62·56	·500	66·9	53·16	51·2	·373	90	53·02	50·13	·352	87	Year.

[Lieut.-Colonel SYKES]

to attract much vapour, nevertheless the greatest quantity of rain does not fall at Dodabetta. The mean of the hourly observations once a month, and the mean of the two observations daily throughout the year, giving respectively 87 per cent. and 90 per cent. of moisture in the air at Dodabetta, observation and expectation thus go pretty well together, and the records might be satisfactory, were there not doubts about the manipulations with the wet bulb. The hygrometric observations in the Deccan with DANIELL's hygrometer, which were taken by myself thrice daily, are quite in accord with the feelings and with the expectations of the observer, excepting for the month of March, in which the feelings indicate the air to be quite as dry as in the months of February or April, but which the hygrometer indicates to be 6 per cent. nearer to saturation than in either February or April. At Aden, in the latitude of Madras, on the arid coast of Arabia, where little rain falls, the mean monthly and annual per-centage of moisture appears unexpectedly high; the lowest per-centage ($60\frac{3}{4}$) is in November, and the next lowest $68\frac{1}{2}$ and $65\frac{1}{2}$ respectively in August and September. The maximum per-centage was $77\frac{1}{2}$ in March, and the mean for the year 71. These results have a certain relation to the phenomena at Madras, which is destitute of a S.W. monsoon. The mean maximum tension of vapour was .902 in May at Aden.

The mean monthly and annual results at the several stations have no doubt a certain relation to truth, as it is seen that the per-centages of moisture or fractions of saturation in the atmosphere in the different months of the year have an increasing or diminishing amount as the several months approximate to, or recede from, the monsoon months of the year; this is sufficiently shown at Madras, where the monsoon months are the dry months at Bombay and in the Deccan. Nevertheless the amount of moisture in the atmosphere, deduced from the observations of the wet bulb, is so very great compared with the amount determined by the direct method by DANIELL's hygrometer (itself an imperfect instrument) in the Deccan, and at some of the stations is so little in accord with personal recollections and experience, that I cannot refrain from suspecting some error of observation, some mismanagement in the manipulations, or a fallacy in the formula by which the dew-point is deduced from the temperature of the wet bulb. The first cause of error that struck me was that arising from the proximity of the dry to the wet bulb, as noticed by Professor ORLEBAR in his Report of Meteorological Observations taken at the Bombay Observatory in 1846. He had a stand erected out of doors, 6 feet high, and with a thatched roof, and every precaution was taken to guard off radiation by layers of cotton and tow upon a board under the roof upon which the meteorologic instruments were placed; there was lateral access for the air all round. He soon found that the dry bulb, in the neighbourhood of the wet bulb, was almost always depressed below the neighbouring *standard* thermometer, and that the depression of the *dry* bulb was greater as the depression of the wet bulb below the standard was greater. Professor ORLEBAR explains this in the following words:—"This seems accountable only on the

supposition that heat is extracted from the air to form the shell of moisture round the wet bulb at a distance as far off as the dry bulb." These discrepancies amounted on the 3rd and 14th of November, at the 19th hour, Göt. mean time:—

Standard . . . 82°·4	Dry . . . 76°·5	Wet bulb . . . 71°·8	Diff. . . . 5°·9
Standard . . . 82°·4	Dry . . . 75°·4	Wet bulb . . . 69°·3	Diff. . . . 7°·0

Professor ORLEBAR therefore abandoned observing with the attached dry bulb. But supposing this cause of error to have been overlooked by other observers, the tension of vapour and the per-centage humidity would have been recorded by them greatly higher than the truth; and if we apply this source of error to the mean monthly and annual results in the comparative table I have given;—for instance, to the annual mean for Dodabetta, the 90 per cent. is reduced, for the first difference, to 64 per cent., and if the correction be made for 7°, by depression of the dry bulb below a standard owing to its proximity to the wet bulb, the 90 per cent. of moisture at Dodabetta is reduced to 60 per cent. Professor ORLEBAR says the dry bulb *always* stood below the standard (and he had determined that it was not owing to error in graduation of the thermometers), often to the extent of 2°, and even in the monsoon month of September I observe that on the 2nd it was 3°·4 minus. Any amount of error in depression would necessarily affect the numerical determinations of the tension of vapour and degree of humidity; but supposing it not to exceed 2°, even this small depression would reduce the 90 per cent. of moisture in the air at Dodabetta to 80 per cent. Supposing therefore that the same error was not discovered at the other places of observation as was discovered in Bombay, there is necessarily some ground for the expression of my doubts, whether the air really did hold at the different stations the quantity of moisture represented by the figures I have elaborated. But Professor ORLEBAR observed another source of error, contingent upon the *locality* of the wet bulb apparatus, whether placed within doors or out of doors. To determine the amount of error he placed a wet bulb *within* the observatory, observing simultaneously with the wet bulb *out* of doors upon the meteorologic stand. This was done hourly for March, April, and to the 10th of May. The reading was almost always plus with the wet bulb inside; on the 22nd of March, at 19th hour, to the extent of 3°·2, while at 18th hour it had been only 0°·2 plus; but there were great irregularities in the readings, being plus or minus dependent apparently upon drafts of air within the observatory, which would depress the wet bulb or raise it. Also the "atmosphere within the room would tend to keep up a reading at any time to whatever it had been at a time preceding," and the latter, Professor ORLEBAR says, was the principal cause of the plus readings in-doors. Supposing this error of 3° to be applied as a correction to the reading of the annual means of the wet bulb at Bombay for 1843, the per-centage of moisture in the atmosphere would only be 65 instead of 76. The distinguished experimental philosopher REGNAULT has pointed out the same sources of error. He placed the dry and wet bulb in the open air in the court of the College of

France, in a closed room in the College, and in the theatre of the College, opening the windows. In the open air, with a temperature ranging from $7^{\circ}16$ to $17^{\circ}88$ Centigrade, and a depression ranging from $1^{\circ}86$ to $9^{\circ}60$, the results, by observation and by M. REGNAULT's tentative formula*, were sufficiently satisfactory; but in the closed chamber he says, "Les fractions de saturation calculées avec la formule†, sont ici beaucoup plus fortes que celles que l'on deduit des pesées directes de l'eau renfermée dans l'air; en d'autres termes la température t' marquée par le thermomètre mouillé n'est pas assez abaissée par la vaporisation de l'eau que se fait à sa surface pour donner dans la formule la véritable force élastique x de la vapeur. Cette circonstance tient évidemment à ce que l'air se trouve beaucoup moins agité qu'à l'extérieur."—Page 219. At page 220 M. REGNAULT adds, "Ces expériences démontrent de la manière la plus évidente que la formule ne peut pas rester la même pour les divers états d'agitation de l'air."

Here then is a second source of error; and it is somewhat curious that Professor ORLEBAR, in guarding against another grave source of error, which will be adverted to, himself contributes to an error of observation. He had observed the effect of wind blowing upon the wet bulb in unduly depressing the temperature, and to guard against this he says, "As it was equally essential that the bulb of this thermometer should not be exposed to the wind, and that it should be in the same body of air as the air-thermometer when the latter was exposed to the wind, a small mirror, about an inch square, was put on a little stand, and this being placed upon the tin board could be moved about by the observer into such a position that it might always *cut off the wind from the bulb of the wet thermometer only*" (page lxiii.). Now the wet bulb being thus screened, would be buried in its own vapour and the reading would necessarily be too high. When the air is perfectly calm the same would be the result without the screen, for there would be a shell or coat of saturated air round the bulb. I had occasion to notice this local character of aqueous vapour in my Meteorology of the Deccan, where I constantly witnessed it, as regulated in its distribution by nature. Speaking of dew, I said in the year 1828, "At Marheh in the Pergunnah of Mohol, garden produce (which is usually irrigated during the day-time) was covered with a copious dew every morning; the lands bordering the gardens for forty or fifty yards around were slightly sprinkled with it, *but there was not a vestige of it* in the fields constituting the rising ground north and south of the tract of garden land." Hence I inferred that "aqueous vapour had been taken up by the action of the sun during the day, *suspended over the spot*, and deposited by the lower temperature at night as dew upon the land in proportion to the supply obtained by day." My tents were within 200 yards of the fields where I observed these phenomena, but from the 11th to the 30th of January 1828, there was not any deposition of dew about them, excepting on the 13th of January. In consequence of these observations I was induced

* Annales de Chimie, tom. xv. p. 218.

† $x = f' - \frac{0.429(t-t')}{610-t'}H.$

to remark particularly the localities of dew at Poona and in its neighbourhood. In September and October I found that when there was not a trace of dew in the cantonment, there would be a deposition on the fields of standing grain half a mile distant, and when there was not any dew either in the cantonment or in the fields, it would yet be found on the banks of running rivulets and on the banks of the Mota Mola River; but with respect to the rivulets, "*fifteen or twenty feet from the water were the limits of the deposition.*" I gave numerous other instances of the local deposition of dew proximate to irrigated lands, or in the neighbourhood of water, indicating the suspension of vapours over the localities, in complete analogy with what occurs to the wet bulb thermometer when the air is calm. That agitation of the air is necessary to disperse the vapour surrounding a wet bulb, has been noticed by British chemists. BRAND says (page 111, last edition), "It is now established that the pressure of air is really an obstacle to evaporation, and that *a current* is useful, not by supplying new quantities of air, but *by removing the vapour* according as it is formed and leaving fresh spaces into which the vapours may expand." He elsewhere says (page 82), "Evaporation is proportional to the surface exposed; it is also accelerated by agitating the superincumbent air, as in the case of a brisk wind, or by artificial means. When the *air is tranquil the vapour rests upon the surface of the water, and it is the pressure of its own vapour on the surface of a liquid, and not that of the gaseous atmosphere which stops the process.*" M. REGNAULT has demonstrated the truth of this in an elaborate manner. Accounting for the different results of observations in a closed and open chamber, he says the wet bulb was not sufficiently depressed in the closed chamber. "Cette circonstance tient évidemment à ce que l'air se trouve beaucoup moins agité qu'à l'extérieur*." After experimenting in a room with two windows open, he adds, as before stated, "Ces expériences démontrent de la manière la plus évidente que la formule ne peut pas rester la même pour divers états d'agitation de l'air." (P. 220.) The vapour therefore resting upon the wet bulb is a source of error, but the removal of it leads to one much more grave. M. REGNAULT, in reference to M. AUGUST's formula, says (p. 207), "La formule ne tient aucun compte de la vitesse du courant d'air; d'après cette formule, la différence de température devrait être la même, quelle que soit cette vitesse. Ce résultat paraît impossible *à priori*. J'ai cherché à déterminer *par des expériences directes*, l'influence de cette vitesse et à reconnaître si, à partir d'une certaine valeur de la vitesse, les différences de température des thermomètres sec et mouillé deviendraient indépendantes de la vitesse absolue du courant d'air, conséquence à laquelle on se trouve naturellement conduit par le raisonnement que M. AUGUST applique au calcul de la formule du psychromètre." M. REGNAULT then describes his apparatus and mode of making his experiments. He gives two series of experiments; in the second experiment the air being made to blow upon the wet bulb with a greater velocity than in the first. It will be sufficient to give the first and sixth figures of each series.

* Annales de Chimie, tom. xv. p. 219.

	t^* .	t' .	$t-t'$.
1st series. 1st experiment	14°66	7°28	7°38 Centigrade thermometer.
2nd experiment	14°96	4°33	10°63 Centigrade thermometer.
2nd series. 1st experiment	21°48	10°78	10°70 Centigrade thermometer.
2nd experiment	21°70	8°56	13°14 Centigrade thermometer.

In FAHRENHEIT's scale.

58°37	45°10	13°27
58°93	39°79	19°14
70°66	51°40	19°26
71°06	47°41	23°65

In the first series, in the second experiment, the wind blows faster than in the first, and the wet bulb is reduced nearly 3°, and the difference between the wet and dry bulbs is increased from 7°38 to 10°63, while the temperature of the dry bulb is only raised 0°30. In the second series the temperature of the wet bulb is reduced from 10°78 to 8°56, and the difference between the wet and dry is increased from 10°70 to 13°14, while the temperature of the dry bulb is only raised 0°22. Upon these experiments M. REGNAULT says, "On voit que, pour une même température t les températures t' dependent beaucoup de la vitesse du courant d'air" (p. 209); and he further says (p. 210) that these depressions are less than he has found on other occasions with an increased velocity of the air; and on using dry air he found the depression $t-t'=13°52$ instead of 5°91, and another depression of 15°60 instead of 8°60. M. REGNAULT gives other experiments, and finishes by saying, "Il résulte de tout ce qui vient d'être dit, que l'agitation de l'air doit exercer une influence très-sensible sur les indications du psychromètre" (p. 211). The truth of M. REGNAULT's experiments are borne out by the experience of families in the Deccan (and no doubt elsewhere) in India in the fair season, who cool their wine and beer by the following simple process down to a temperature the cold of which makes the teeth ache in drinking. At any time of the day a thick layer of straw is put down on the ground in the shade of a building, but not in the lee of the wind. The bottles of wine or beer to be cooled are put upon the straw, some more straw is thinly and lightly shaken over the prostrate bottles, and the mass is sprinkled at intervals with water through the nozzle of a watering-pot with very fine apertures, thus dewing as it were the straw; the force of evaporation and the consequent cold is proportioned to the velocity and dryness of the wind; but even with a moderate wind the temperature of the liquors is soon greatly lowered; and in certain hot and therefore parching winds, even at a temperature of the air ranging from 85° to 90° FAHR., I have often, at Ahmednugger, had the temperature of the wine or beer (judging from my sensations at the moment) approaching to the freezing-point†. In this cooling process we have

* t . Temperature of dry thermometer. t' . Temperature of wet bulb.

† My sensations deceived me. While this paper is going through the press, I have received from a friend, commanding the Artillery at Ahmednugger, the following results of experiments he made at my request, with

the bottle of wine or beer corresponding to the bulb of the thermometer ; the straw lying thinly over it represents the muslin, and the operations of the watering-pot complete the wet bulb apparatus, evaporation does the rest ; but as the velocity and dryness of the wind regulate this, it is plain that such an instrument can only give uncertain and fallacious results when used to determine, with any pretension to accuracy, the fractional saturation of the atmosphere. I come now to a source of error in my reductions of the wet bulb observations which I have collected in this paper from various parts in India, a source of error that may operate with a greater or less power as the depression of the wet bulb is greater or less. I allude to the formula used for the reductions of the readings of the wet bulb. M. REGNAULT says that M. GAY-LUSSAC was the first to propose the determination of the dew-point by the observations of a dry and wet bulb apparatus*, but that to effect the object satisfactorily it would require extensive observations upon which to found tables. Subsequently to the period of M. GAY-LUSSAC's proposition, AUGUST, Professor at Berlin, occupied himself with the subject and published some papers, in which he sought to determine, upon theoretical considerations, the formula by which the elastic force of aqueous vapour, really existing in the air, could be found by the difference of temperature of a dry and wet bulb thermometer. The dry and wet bulb apparatus he called a Psychromètre. His chief memoir is published in the *Annalen der Physik und Chemie*, V. Band. Leipzig, 1825. It will suffice to say that he considered the wet bulb surrounded at all times with a coat of vapour of the same temperature as the bulb, which was, he stated, necessarily lower than that of the surrounding air,—that the successive supplies of air coming into contact with the wet bulb, parted with a portion of their heat and took the temperature of the wet bulb ; but on the other hand, the air so supplied in vaporizing the water upon the surface of the wet bulb took from it a portion of its heat ; and a stationary state of the temperature of the wet bulb was established by these two quantities of heat balancing each other.

$$\text{AUGUST's formula was } x = \frac{1 + \frac{\gamma}{\delta\lambda}(t-t')}{1 + \frac{x}{\lambda}(t-t')} f' - \frac{\frac{\gamma}{\delta\lambda}(t-t')}{1 + \frac{x}{\lambda}(t-t')} h, \text{ where } t \text{ denotes the tempe-}$$

perature of the dry bulb (in Centigrade degrees), t' the temperature of the wet bulb, γ the specific heat of dry air, δ the density of aqueous vapour, λ the latent heat of aqueous vapour between the temperatures t and t' , x the specific heat of vapour, h the height of the barometer, f' the elastic force of vapour in saturated air at the temperature t' , and x the elastic force of the vapour actually existing in the atmosphere ; x, f' and h being expressed in inches, or in terms of any common unit.

the wet straw process, on the 21st of May 1850, and preceding days. Temperature of air in shade, free from radiation, 98° FAHR. Temperature of water in bottles under wet straw exposed to wind 65° , difference 33° . The dew-point by APJOHN's formula would be about $41^{\circ} \cdot 7$, and by GLAISHER's factors about $48^{\circ} \cdot 5$. My friend says, " When the wind does not blow, the temperature of the water in the bottles, under the straw, cannot be got lower than 71° FAHR. When the wind blows, the bottles cool to 65° FAHR."

* *Annales de Chimie et de Physique*, 2nd series, t. xxi. p. 91.

With assumed values for γ , δ and λ , as the real values are not accurately known, and neglecting small quantities, and supposing $\kappa=\gamma$, AUGUST's formula became

$$x=f'-\frac{0.428(t-t')}{640-t}h,$$

for the determination of the elastic force and consequently the dew-point, and after certain comparisons of the dew-point from his formula with the direct dew-point from DANIELL's hygrometer, he found what he considered a sufficient agreement between them. In a comparative table* of the results by his formula and the results by DANIELL's hygrometer, they appear to go pretty well together, while the variations of temperature and of the depression of the wet bulb are small; but the moment the temperature exceeds 20° Centigrade (68° FAHR.), and the depression exceeds 5° (9° FAHR.), the discrepancy is very considerable; for instance, at bar. 755.3 millims. (29.736 inches), dry 28.5 (83.3 FAHR.), wet 21.1 (69.98 FAHR.), depression 7.4 (13.32 FAHR.), the tension of vapour by the formula is 14.181 millims. (.558 in. =dew-point 62°), and by DANIELL's hygrometer 12.087 millims. (.475 in.=dew-point 57.15), the difference 2.094 millims (.083 in.). Even with a depression of only 0.6 (1.08 FAHR.) the tension of vapour by the formula and by DANIELL is respectively 8.612 millims. (.339 in.), and 8.534 millims. (.335 in.), difference 0.078. These discrepancies induced M. REGNAULT to modify, in 1845, nearly twenty years afterwards, AUGUST's formula, and he in common with AUGUST assumed $\gamma=0.2669$, $\delta=0.622$ and $\kappa=\gamma$, but $\lambda=610-t$. Substituting these numbers and neglecting small quantities, the formula became $x=f'-\frac{0.429(t-t')}{610-t}h$, and it is this formula that

M. REGNAULT tests by his various experiments; and at the close of his able and elaborate paper he says, "Je ne pense pas que l'on puisse admettre comme base du calcul du psychromètre l'hypothèse fondamentale adoptée par AUGUST: à savoir, que tout l'air qui fournit de la chaleur au thermomètre mouillé descend jusqu'à la température t' indiquée par celui-ci, et se sature complètement d'humidité. Il me paraît probable que la portion de l'air que se refroidit ne descend pas jusqu'à t' , et qu'elle ne se sature pas d'humidité. Le rapport de la quantité de chaleur que l'air enlève à la boule par vaporisation de l'eau, à la quantité de chaleur qu'il perd en se refroidissant, est probablement d'autant plus grand que cet air est plus sec, parceque dans cet état, il est beaucoup plus avide d'humidité que quand il approche de son état de saturation.

"Enfin, la température de la boule mouillée est influencée encore autrement que par l'air immédiatement ambient, elle est soumise au rayonnement de l'enceinte dont l'influence sera variable suivant l'état d'agitation de l'air.

"Il me paraît impossible de faire entrer toutes ces circonstances dans le calcul théorique de l'instrument; est je crois qu'il est plus sage de ne faire servir les considérations théorétiques qu'à la recherche de la forme de la fonction, et à déterminer ensuite les constantes par des expériences faites dans les conditions déterminées. Cette

* Annalen der Physik, B. v. p. 87.

manière d'opérer me parait d'autant plus nécessaire, *qu'il reste beaucoup d'incertitude sur plusieurs des éléments numériques que entrent dans le calcul, notamment sur la chaleur spécifique de l'air, sur celle de la vapeur, et sur la chaleur absorbée par l'eau lorsqu'elle se vaporise dans l'air.*"—(P. 212.)

These are the opinions of M. REGNAULT expressed twenty years after AUGUST had invented his formula, and after his own elaborate experiments; and I have preferred giving them in his own language to free myself from any possible misconstruction in translation. M. REGNAULT thought that AUGUST's formula modified by him, would meet some of the difficulties expressed by him in the above quoted opinions, and he gives tables of results, which, with a limited range of the thermometer and small depressions, are sufficiently satisfactory; but he candidly admits "*l'accord a été beaucoup moins parfait dans les bas températures et dans l'air très humide;*" in these cases M. REGNAULT says the fractions of saturation calculated are always above the fractions of saturation obtained by direct means, often to a very notable extent. M. REGNAULT got one of his pupils to try the psychrometer, under considerably diminished pressure, on the mountains in Switzerland, but the results were so little satisfactory that he does not give a detail of them. He induced also his friend M. IZARN, to compare the readings of the wet bulb in the Pyrenees, at a pressure of 700 millims. (27·559 in.), with the readings of an hygrometer invented by himself, which he calls "*hygromètre condenseur,*" and which he considers to be free from the objections to which DANIEL's hygrometer is subject; and in a table given, the fractions of saturation are almost always much higher by the wet bulb than by the condenser, the depression in no case exceeding 3°·91. In a depression of 1°·97, the differences of the fraction of saturation are respectively 0·7542 and 0·7937. Finally, M. REGNAULT says that his modified formula may give the elastic force of x a little too high, and that the coefficient 0·480 might be used instead when the fraction of saturation exceeds 0·40 Centigrade, but that the coefficient 0·429 gives results nearer to the truth where the fraction of saturation is below 0·40. When the wet bulb descends below zero, he considers that the value of λ should be increased from 610 to 689, but in the present imperfect knowledge of the true numerical value of several of the elements he will not venture to put forth a new formula for the psychrometer (p. 227). Several other philosophers have also given formulæ: BURG from observation, $f'' = f' - \cdot 0004528(t - t')p$; BOHNENBERGER also from observation, $f'' = f' - \cdot 0003962(t - t')p$; also KUPFFER, ERMAN and KAMTZ.

On the 24th of November 1834 and 27th of April 1835, Dr. APJOHN of Dublin read to the Royal Irish Academy a very elaborate and able paper upon the theory of the moist bulb hygrometer. The results of his observations, experiments and theoretical considerations, induced him to adopt the formula

$$f'' = f' - \frac{d}{87} \times \frac{p}{30} :$$

at p. 436 of the volume of the Proceedings of the Royal Irish Academy for 1840, the formula is

$$f'' = f' - \frac{48a(t - t')}{e} \times \frac{p - f'}{30} ;$$

and finally, with the coefficient

$$f'' = f' - \cdot 01147(t-t') \times \frac{p-f'}{30}^*;$$

and with this formula† Colonel BOILEAU of the Bengal Engineers, in charge of the Magnetic Observatory at Simla, to aid meteorologists, has calculated a series of tables of the tension of vapour from minus 10° FAHR. to 170° FAHR. for 30° of depression of the wet bulb by tenths, and for variations of pressure from 19 inches to 31 inches. It was these tables I used in the reduction of the preceding wet bulb observations; and it was in the progress of their use, during several months' labour, that at certain temperatures doubts were raised in my mind respecting the accuracy of the formula, from supposing the fraction of saturation unreasonably high when the depression of the wet bulb was inconsiderable, and unreasonably low when the depression of the wet bulb was considerable. The tension of vapour at considerable depressions startled me, until at last I found in testing the tables that with a depression of the wet bulb of 19° at a temperature of 52° FAHR., and at a pressure of 29 or 30 inches, the results became impossible; that is to say, the tension of vapour of the dew-point arrived at by the formula had a greater numerical value than the tension of vapour at the temperature of the wet bulb, *e. g.*

	FAHR.	Bar.
	°	in.
Dry bulb	52	30
Wet bulb	33	 20640
Difference	19	= (a) = ·01147 (t-t') = 21794

It is objected that a depression of the *wet* bulb of 19°, at a temperature of 52°, never can occur in nature‡: but very considerable depressions, both of the wet

* APJOHN'S formula is expressed in English measures: f'' is the force of aqueous vapour at the dew-point; f' the tension of vapour at the temperature of evaporation; a specific heat of air; e the latent heat of aqueous vapour; d the depression or difference between the temperature of the air and wet bulb ($t-t'$); p the pressure of the air in inches.

† Differing little from AUGUST'S formula, and converting it into the measures and scale used by AUGUST; for all practical purposes, the two formulæ are the same.

‡ In a synopsis of nine years' observations of the wet bulb at Mahabuleshwur, the following maximum depressions of the *wet* bulb occur in the respective months of the year:—

Months.	Depression of the wet bulb.	Months.	Depression of the wet bulb.
January	17·2	July	5·2
February.....	24·0	August	7·7
March.....	24·6	September	8·5
April	26·3	October	16·0
May	26·0	November	15·5
June	7·0	December	16·7

In a preceding page it was shown that water in bottles under wet straw, exposed to the wind at Ahmednugger, cooled down to 65° FAHR., the temperature of the air being 98°, difference 33°, barometer 28 inches, dew-point by APJOHN 41°·7, by GLAISHER 48°·5.

bulb and of the dew-point, have been observed even in our damp climate at the Royal Observatory at Greenwich. For instance, in the records on the 6th of April 1845, at 6 P.M., the dry bulb was $52^{\circ}6$, wet bulb $39^{\circ}2$, and *dew-point* by DANIELL's hygrometer 22° . The depression of the wet bulb being $13^{\circ}4$, the *dew-point* by APJOHN's formula would be 14° , by GLAISHER's factors $25^{\circ}8$, and by DANIELL's hygrometer it is found to be 22° . Which of these dew-points is to be taken to give the real numerical value of the tension of vapour? At Greenwich, on the 3rd of June 1846, at 4 P.M., the dry bulb was $79^{\circ}1$, and the dew-point by DANIELL's hygrometer 44° , the depression therefore $35^{\circ}1$. But I have recorded in the Meteorology of the Deccan, a depression of 61° of DANIELL's hygrometer taking place before the deposition of dew; the temperature of the air being 90° , the dew-point 29° , at 4 P.M., on the 16th of February 1828, at Downd, near Pairgaon, on the Beema River*. Mr. GLAISHER, of the Royal Observatory, a most persevering and able observer, finding from experience that the formulæ in use with a constant coefficient did not give satisfactorily the dew-points at varying temperatures and varying depressions of the wet bulb, adopted a series of factors quite independent of theory, from the results of very extensive comparisons of simultaneous observations of the wet bulb with DANIELL's hygrometer. These comparisons extended over several years, and through several thousand observations. He found that "the dew-point temperature was so related to the temperatures of air and evaporation, that at the same temperature of the air, the difference of temperatures of air and of the dew-point, divided by the difference of the temperature of the air and evaporation, was constant, but that it was different at every different temperature." Arranging several thousand observations made during five years at Greenwich, and during three years at Toronto, and taking the mean value at every degree of temperature, he obtained a series of factors ranging from $8\cdot5$ at temperatures below 24° FAHR., up to $1\cdot5$ above 70° FAHR.; these were published in the volume of the Greenwich Magnetical and Meteorological Observations for 1842, 1843 and 1844. In 1847 Mr. GLAISHER published hygrometrical tables in which some slight alterations of the original factors were made, and he has since made some further slight modifications. For the limited range in temperature, depression of the wet bulb and pressure, in which the comparisons were made and the factors deduced, they are probably the very best agents (at least above the freezing-point) for giving a close approximation to the true value of the readings of the wet bulb; supposing always the wet bulb not to be subjected to the anomalous indications noticed by Professor ORLEBAR and M. REGNAULT. For considerable differences of barometrical pressure, and very considerable depressions of the wet bulb, the factors would require further correction. When the dew-point was obtained with difficulty by DANIELL's hygrometer, or in other words, when the depression of the temperature of evaporation was great, Mr. GLAISHER found that his method of determining his factors would not hold good, which he attributed to certain objec-

* Philosophical Transactions, Part I. for 1835, p. 184.

tions to DANIELL's hygrometer, and to which it is no doubt subject, but which are applicable to all temperatures and all depressions, objections which were urged by M. REGNAULT, and which induced him to invent his "hygromètre condenseur," an instrument which he pronounces to be free from the errors of DANIELL's instrument, but the use of which I have not heard of in England.

I have thus reviewed, *in extenso*, the possible, indeed the probable sources of error in the very high degree of humidity constantly in the air, as represented by the wet bulb observations made in India, and I have no hesitation in expressing my belief that the results, which I have obtained with the labour of some months, do not represent the real fractions of saturation of the air at the several places where the wet bulb was observed*; and I am the more confirmed in this opinion, with respect to the Neelgherries, by an officer now in London, who resided some time at Ootacamund and kept a meteorological register, who says that if the mean annual moisture in the air had amounted to anything like 90 per cent. it must have been most inconveniently felt in the clothes, hats, bedding and furniture of the residents; but so far from such being the case, that, with the exception of occasional fogs, the hills were looked upon as rather dry than otherwise. From my doubts respecting the correctness of the deduced fraction of saturation of the air at places in India, with the formulæ I have passed in review, I may possibly except the observations made in the Deccan with DANIELL's hygrometer, which observations have a semblance of truth, although the instrument may be imperfect. Even with a comparatively low mean annual percentage of 55 of moisture in the air, I would say such fraction of saturation was rather higher than the truth, for during some months of the year in the Deccan the air is so dry that it is difficult to prevent the disposition of the leaves of tables to curl up into hollow cylinders, and the nib of a quill pen is always most provokingly straddling out into the form of a pair of open compasses.

By reason of the above-noticed sources of error in the wet bulb itself, and of the inadequacy of the formulæ to give a satisfactory value to its readings, supposing the indications to be correct, I have deliberately *not* applied any corrections to the readings of the barometer on account of moisture. But even had the instrument been free from error and the formulæ exact, I still should have deemed it ineffectual to attempt to measure the moisture in a whole column of the atmosphere *from a local observation*† with a view to apply to the barometer, which represents the pressure of an entire column of the atmosphere, a correction for the tension of vapour‡. Further, I have doubts of the propriety of applying to the barometer a correction for

* Dr. M'CLELLAND says, "The wet bulb thermometer would add greatly to the value of these results, but there are discrepancies in the register of the instrument in use (at Calcutta) which *prove it to be imperfect*."—Calcutta Journal of Nat. Hist., vol. v. p. 554.

† "This instrument [wet bulb] simply indicates the conditions of the air of the place where it is situated: at 100 feet above it the conditions may be very different."—GLAISHER's Hygrometrical Tables, p. 16, edition of 1847.

‡ The capacity of air to hold water in solution, or in a state of vapour, diminishes with the temperature.

moisture *subtractively*, even were the tension of vapour satisfactorily determined for the *whole pressure*. The barometer *falls* with increasing saturation of the atmosphere with moisture, indicating the displacement of comparatively dry air by *dilated* vapour, the density of which is less than that of drier air, and a certain amount of pressure is taken off the barometer and the mercury falls on account of the diminished density and elasticity of humid air; but as soon as this vapour is condensed into rain the drier air resumes its ordinary density and elasticity, and the mercury mounts again. If the vapour came simply as an *addition* to the drier air, the density and pressure of the compound should increase, and the barometer should rise; but this is contrary to fact. I would therefore not apply a subtractive correction for that vapour, which has already acted directly upon the barometer in diminishing pressure by displacing denser air. The object of applying any corrections at all to the barometer, on account of moisture in the atmosphere, is stated to be to "obtain from them the pressure of the atmosphere of dry air*;" but as such a state of the air is a physical impossibility as long as there is a drop of water upon the earth to be vaporized, as evaporation goes on at all temperatures, it may be asked, what practical advantage can be obtained from any such determination? and the more so, may the question be asked, when experimenters are not in accord with respect to the numerical values of the tension of vapour at different temperatures to be applied as corrections†.

On the whole it appears very desirable that to DIRECT MEANS recourse should always be had, if possible, for the determination of the dew-point, to DANIELL's hygrometer, or to REGNAULT's condensing hygrometer; and short of this, that persevering comparisons should be made for years, in *extended* ranges of *temperature*, *depression* and *pressure*, and at different elevations, to obtain a more trustworthy wet bulb formula, or an unquestionable series of factors.

Rain.

If it were necessary to suggest caution in generalizations from a limited number of local observations for the determination of the dew-point, caution is equally, if not more necessary in attempting to fix the normal rain-fall even in a narrow area, much less in a district or province. For instance, within the limits of part of the small island of Bombay, seven miles by two miles, the following is the result of observations with nine rain-gauges. The Fort and Esplanade are necessarily proximate; the Observatory at Colabah two miles distant, and the most remote gauge at the Government House at Parell, only five miles from the Fort, the other stations being intermediate between the Fort and Parell.

* GLAISHER's Hygrometrical Tables, p. 12, edition of 1847.

† At 32° FAHR. DALTON's tension of vapour, in inches of mercury, is 0·200, at 95° = 1·59297, at 122° = 3·500 in. The Physical Committee of the Royal Society adopted for 32° FAHR. 0·186, and for 95° = 1·610, and 122° FAHR. 3·542; REGNAULT, at 32° FAHR., has 0·18124, and at 95° FAHR. 1·64380, and at 122° = 3·619. KAEMTZ, at 32° FAHR., has 0·17999, and at 95° FAHR. 1·57789; of course different results are come at by the use of these different values.

Bombay, 1849. Rain-fall.

	Colabah Observatory.	Fort.	Esplanade.	Byculla.	Chinchpoogly.	Airy Cottage, Mazagon.	Agricultural garden, Parell.	Parell Flagstaff.	Parell Hill.
	in.	in.	in.	in.	in.	in.	in.	in.	in.
1	0·86	0·36	0·28	0·40	0·00	0·35	0·00	0·00	0·00
2	0·00	0·00	0·00	0·00	0·00	0·04	0·35	0·00	0·30
3	0·39	0·14	0·00	0·06	0·00	0·03	0·05	0·06	0·05
4	0·39	0·55	0·76	0·58	0·00	0·70	0·66	1·23	0·55
5	0·31	0·21	0·12	0·08	0·00	1·50	0·55	0·80	0·55
6	0·00	0·00	0·00	0·02	0·00		0·30	0·46	0·25
7	0·25	0·10	0·06	0·00	0·00		0·00	0·00	0·00
8	0·00	0·00	0·00	0·04	0·00	0·00	0·10	0·00	0·10
9	0·00	0·03	0·00	0·03	0·00	0·00	0·00	0·00	0·00
10	0·00	0·01	0·00	0·00	0·00	0·00	0·15	0·10	0·20
11	0·16	0·32	0·27	0·88	0·00	0·09	0·30	0·70	0·30
12	3·14	1·78	1·35	1·06	0·00	1·26	0·82	1·92	0·95
13	0·34	0·00	0·00	0·08	4·00†	0·00	1·20	2·16	1·30
14	0·00	0·00	0·00	0·02	1·86	1·70	0·00	0·00	0·00
15	0·00	0·83	1·57	1·43	2·25	0·00	1·25	2·18	1·30
16	4·20	4·65	2·10	3·76	4·75	5·00	3·50	6·79	3·70
17	4·61	2·34	2·60	2·22	2·54	2·25	3·30	7·58	3·35
18	0·65	0·86	1·01	1·09	1·10	0·06	1·35	3·56	1·50
19	0·16	2·25	1·81	1·34	0·75	2·58	1·65	3·38	1·50
20	1·77	5·22	2·20	4·55	4·67	4·70	0·90	1·60	0·70
21	3·34	0·40	0·50	0·43	0·55	0·74	3·95	11·10	4·10
22	4·46	4·28	4·38	3·93	5·11	4·70	3·50	7·66	3·40
23	0·21	0·11	0·10	0·13	0·24	0·30	3·80	5·88	3·70
24	3·62	4·53	5·20	5·22	6·00	6·43	0·43	0·90	0·45
25	3·93	10·51	6·73	7·10*	7·50‡	11·75§	5·40	13·72	5·50
26	5·72	1·26	1·50	0·85	0·85	1·05	4·95	9·00	4·90
27	1·25	1·27	1·86	3·35	2·55	2·70	1·25	1·90	1·25
28	2·22	2·54	3·00	2·28	2·80	3·25	6·25	11·50	6·10
29	2·78	1·90	2·50	2·13	2·20	2·90	2·10	2·85	2·15
30	1·41	1·55	2·15	1·42	2·55	2·70	2·15	3·40	2·40
31	2·47	3·68	4·50	4·57	3·75	4·40	2·35	3·15	2·45
July	48·67	51·68	46·55	49·05	56·02	61·23	52·50	102·14	53·00
June	23·37	22·82	23·34	19·40		25·56	28·54	46·60	26·71
Totals	72·04	74·50	69·89	68·45		86·79	81·04	148·74	79·71

It is seen that in June the fall of rain varied from 19·46 in. at Byculla to 46·60 in. at the flagstaff at Parell, and in July from 46·55 in. on the Esplanade to 102·14 in. at the flagstaff at Parell; or omitting this as a doubtful return, to 61·23 in. at Airy Cottage, Mazagon; and the total of the two months varies from 68·15 in. to 86·79 (omitting the gauge at the Parell flagstaff) at Airy Cottage. I do not know whether the investigations were carried out for a whole monsoon, but in case the discrepancies continued, it would be difficult to determine the normal fall of rain in Bombay. But the anomaly is not confined to Bombay; Dr. MURRAY, in a paper published in the Journal of the Physical Society of Bombay, No. 9, p. 172, has the following

* Gauge overflowed.

† No register kept for first thirteen days—4 inches set down at guess.

‡ Gauge overflowed.

§ The gauge was running over, but this seems pretty nearly all that fell. It poured in torrents from 5 P.M. on the 25th till 6 A.M. 26th.

|| The difference here is the same as last month (twice as much of fall being set down here as fell everywhere else); and so enormous and unaccountable as to lead to the inference of some tremendous blunder.

observations respecting the fall of rain in the town of Sattarah and military cantonment adjoining: "The quantity of rain in the town of Sattarah usually exceeds that in the cantonment, situated a mile N.E., by 6 or 8 inches." Annexed are the results, to which he has added the fall at his own bungalow or house, equidistant between the town and cantonment, so that the three localities are within the range of a mile.

Years.	1844.	1845.	1846.	1847.	1848.
Town Hospital		44·39	42·92	43·17	33·41
Dr. MURRAY's bungalow	36·06	40·24	39·52	40·98	
Cantonment Hospital		38·34	31·65	39·00	27·81
Hill Fort, above the town of Sattarah...					39·36

The increased fall of rain in the town of Sattarah, which is 2320 feet above the sea-level, is no doubt chiefly caused by the hill, 3200 feet above the sea-level, upon which the fort is situated, rising from the back of the town; but this will not account for the difference in the fall at the Bungalow and Cantonment Hospital. The facts show how necessary is a local knowledge of physical features before generalizations can be ventured upon safely.

In my former paper in the Philosophical Transactions I pointed out the remarkable differences in the fall of rain, twenty to twenty-five years ago on the sea coast, at the top of the Ghâts, and at Poona, thirty-five miles to the eastward of the Ghâts. Dr. MURRAY confirms my views in a much more detailed manner than I was enabled to attempt, in the following Table for the year 1848:—

	Bombay.	Ghâts.		Western Districts.							Eastern districts.				
	Means of 33 years.	Mahabuleshwur.		Meera.	Entesh-war.	Sattarah.			Bhore.	Wye.	Phul-tun.	Jhutt.	Bija-poor.	Punder-poor.	Akal-kote.
		Sanatarium, means 14 years.	Sindola, 1848.			Hill Fort.	Town.	Cantonment.							
January		0·05													
February.....		0·20													
March.....		0·15					0·80	0·04							
April		1·31					0·74	0·76							
May		3·31	3·60	2·75	2·60	2·50	3·04	2·79	2·10	2·09	6·24			0·75	1·83
June	22·26	46·53	41·70	10·93	5·98	4·10	3·71	3·26	2·37	2·20	4·30	1·59	0·80	2·02	3·68
July	25·04	92·10	74·28	22·90	19·83	18·77	14·64	12·02	13·20	9·05	2·70	3·07	3·67	4·38	4·65
August	17·08	72·33	47·81	6·16	6·02	5·00	2·60	2·55	5·19	1·61	1·24	4·82	8·52	6·21	4·83
September ...	11·25	31·32	6·17	2·30	1·80	1·46	2·14	1·40	0·91	0·96	3·66	2·64	4·01	4·35	2·84
October	1·19	4·58	7·23	2·75	2·27	4·64	4·89	3·28	1·50	2·75	2·45	3·88	4·64	8·35	5·32
November		2·07	4·37	1·85	0·19	2·91	1·57	1·71	4·15	2·25	3·59	2·17	3·78	2·49	2·30
December ...		0·05													
Year	76·82	254·05	185·16	49·64	38·69	39·38	33·41	27·81	29·42	20·73	24·18	18·17	25·42	28·55	25·45

With the exception of Bombay and the Sanatarium at Mahabuleshwur, the records are for the year 1848. The following are the respective elevations of the places above the sea-level:—

Mahabuleshwur.	Sindola.	Meera.	Entesh-war.	Sattarah Fort.	Sattarah Town.	Sattarah Cantonment.	Bhore.	Wye.	Phultun.	Jhutt.	Bija-poor.	Punder-poor.	Akal-kote.
4500	4600	2340	3700	3200	2320	2320	2350 ?	2320 ?	2000 ?	2000 ?	2000 ?	2000 ?	2000 ?

Dr. MURRAY adds, "The prodigious influence of the Ghâts in modifying the amount of the S.W. monsoon rain, is perhaps nowhere more strikingly shown than in the N.W. parts of the Sattarah territory. If we draw a line nearly straight from west to east, from Mahabuleshwur, on the summit of the Ghâts, to Phultun, a distance of little more than forty miles, we shall find at the commencement of the line a rain-fall of 240 inches, at an altitude of 4500 feet; 180 inches at Sindola, *a mile distant*, and elevated 4600 feet; 50 inches at Paunchgunnee, at a further distance of eleven miles and an elevation of 4000 feet; 25 inches at Wye, four miles further east and 2300 feet in height above the sea, while at the extremity of the line at Phultun, thirty miles from Sattarah, and about the same level as Wye, the quantity is reduced to 7 or 8 inches." But Dr. MURRAY must have meant during the S.W. monsoon, as he had previously represented the fall of rain at Phultun at 24·18 in. in 1848, in 1847 at 24·04 in., and in 1846 at 18·09 in. Some inches of those amounts however are attributable to the Madras monsoon, which commences in October, when the Malabar coast monsoon terminates, and Phultun, from its easterly position, gets an uncertain sprinkling from the Madras side.

Dr. MURRAY, with a view to show not only the discrepancies in the total annual fall of rain at places within comparatively limited distances situated on the plateau of the Deccan, but the remarkable contrasts in the monthly fall at two proximate places, has given the following Table of the fall of rain at Sattarah and Phultun for 1846 and 1847, Phultun being thirty miles east of Sattarah :—

	Sattarah.		Phultun.	
	1846.	1847.	1846.	1847.
January	0·18			
February	0·18			0·02
March		0·18		0·52
April		10·88	1·44	4·19
May	3·49	0·39	1·06	2·88
June	10·49	3·77	3·80	1·53
July	16·04	6·28	1·95	0·84
August	2·13	2·68		0·50
September	0·77	3·55	1·05	2·00
October	2·98	5·25	2·50	3·06
November	0·98	8·00	5·04	4·60
December	2·46	0·05	1·53	0·89
Year	39·52	40·98	18·09	24·04

Dr. MURRAY, in a paper published in the autumn Number of the Journal of the Physical Society of Bombay for 1849, adds to the proofs of the extraordinary discrepancies in the fall of rain in proximate localities. He states (page 18), "At the Sanatorium at Mahabuleshwur, at 4500 feet, the rain-fall during 1848 was 245 inches, being within 3 inches of the average fall during the preceding twenty years. At Sindola, the residence of Mr. FRERE (the British minister), situated *a mile* east from

the Sanatorium, about 100 feet higher, with the *intervening* eminence of Mount Charlotte rising to a further height of 100 feet, the quantity of rain did not exceed 185 inches during the same period; hence a difference of 60 inches or 24 per cent. at two houses *situated at the same station*. The position of the ground explains the difference, but probably few people, on examining the localities, would have anticipated its amount. It shows the supreme importance of instituting multiplied observations, before deciding on the site of a sanitary hill station."

To add to the means of comparison, some other rain-fall localities from the sea-coast, and Konkun, or country between the Ghâts and the sea,—from the Ghâts and from the plateau of the Deccan, are annexed, being the mean fall for the years 1844–47.

Sea-coast of Konkun.		Konkun, somewhat inland.		Western Ghâts.		Western Ghâts, East branch.	Deccan.							
Bombay, sea-level.	Rutnagherry, 150 feet.	Tanna, sea-level.	Dapoollee, 900 feet.	Kundalla, 1740 feet, 1833 and 1835.	Mahabuleshwur, 4500 feet.	Paunchgunnee, 4000 feet, 1835, 1842 and 1843.	Sattarah, 2320 feet.	Kolapur, 1847.	Poona, 1842 feet.	Nassick.	Belgaum, 2000 feet.	Dharwar.	Ahmednugger, 1900 feet.	Sholapur, 1847.
68·73	114·55	106·16	134·96	141·59	254·84	50·69	39·20	20·74	19·02	26·72	40·90	38·81	21·83	32·16

Tanna and Dapoollee are situated between the sea-coast and the foot of the Ghâts. Kundalla is on the crest of the Ghâts on the high road from Bombay to Poona. The places in the Deccan lie eastward of the Ghâts. Arranging the above places according to their supply of rain, it appears the greatest fall is on the crest of the Ghâts, increasing from 141·59 inches at Kundalla at 1740 feet to a mean fall of 254·84 inches at Mahabuleshwur, at 4500 feet. From the crest of the Ghâts the supply of rain decreases towards the sea-coast westward, but decreases in an infinitely greater ratio eastward on the plateau of the Deccan. Along the coast the supply of rain diminishes with the increase in latitude.

In further illustration of the unequal fall of rain in the Bombay Presidency, the returns for May, June and July 1849 are annexed. They are the latest I have received except from Bombay.

	Bombay.	Poona.	Surat.	Nassick.	Asseerghur.	Ahmedabad.	Phoonda Ghât.	Mahabuleshwur.	Paunchgunnee.	Calcutta.
May	0·00	0·405	0·00	0·00	0·23	2·03	0·00	0·00		6·00
June	22·82	9·055	11·16	8·63	5·45	4·10	50·00	59·90		13·0
July	51·68	6·425	19·00	7·03	16·31	7·62	83·00	89·24		8·25
Total	74·50	15·885	30·16	15·66	21·99	13·75	133·00	149·14	11·95	27·25

To General CULLEN of the Madras Artillery, and British minister, with the Rajah of Travancore, I am indebted for the following comparison of rain-fall at stations on the coast of Malabar and Coromandel:—

	Years.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Total.
20 feet. Cochin.	1842.	...	...	...	3-05	25-07	25-05	13-70	21-95	10-15	3-55	3-65		105-27
	1843.	5-15	...	2-00	4-50	27-15	37-32	21-05	4-27	7-75	9-45	0-10	5-75	124-49
	1844.	...	0-45	1-70	1-70	19-35	22-42	19-10	11-75	2-37	17-55	4-50	1-07	101-97
	1845.	3-42	...	5-80	2-20	3-57	31-37	16-10	11-22	1-67	11-85	0-92	4-45	92-60
	1846.	0-02	...	0-70	4-80	19-70	37-32	16-72	16-27	2-15	5-95	2-25	0-10	106-00
Means		1-72	0-09	2-04	3-25	18-97	30-69	17-33	13-09	4-82	9-67	2-28	2-27	106-06
30 feet. Quilon.	1842.	...	1-17	1-32	3-30	22-24	16-00	8-65	8-60	7-42	4-87	7-47		81-06
	1843.	1-42	0-47	0-50	9-85	24-62	26-52	20-72	7-45	5-15	5-85	2-15	1-00	105-72
	1844.	...	...	0-55	0-70	8-15	15-55	5-75	6-70	2-37	13-95	3-25	3-60	60-57
	1845.	3-30	...	4-65	0-25	4-85	13-80	9-55	3-95	0-45	15-35	1-85	3-70	61-70
	1846.	...	...	2-90	1-60	22-70	17-65	10-55	3-80	1-30	9-40	4-75	0-10	74-75
Means		0-94	0-33	1-98	3-14	16-51	17-90	11-04	6-10	3-34	9-88	3-89	1-68	76-76
30 feet. Allepy.	1842.	1-15	0-25	2-92	2-50	27-67	20-00	10-60	12-30	7-57	12-95	6-50	0-10	104-52
	1843.	3-20	0-75	6-90	5-75	30-12	32-80	21-57	5-22	9-72	9-12	0-90	5-77	131-85
	1844.	0-37	2-87	2-55	3-00	24-33	23-12	16-60	10-42	4-92	15-37	9-90	5-40	118-70
	1845.	4-92	0-50	8-80	2-65	17-80	25-20	11-92	6-47	1-00	9-67	4-35	4-50	97-80
	1846.	0-00	1-70	0-20	3-15	31-45	29-00	12-45	12-27	3-42	13-00	5-77	1-00	113-43
Means		1-93	1-21	4-27	3-41	26-27	26-02	14-63	9-33	5-32	12-02	5-48	3-35	113-26
50 feet. Cape Comorin.	1843.	0-90	...	...	...	6-50	0-80	0-95		0-65	4-80		4-60	19-20
	1844.	...	...	...	...	1-20	3-00			1-00	7-50	4-60	2-20	19-50
	1845.	0-20	...	0-90	...	1-30	2-20	0-70			10-70	1-40	Heavy shower	18-00
	1846.	...	...	2-80	3-30	9-40	12-25	1-20	1-45		12-10	9-82	4-40	56-72
Means		0-27	...	0-92	0-75	4-55	4-56	0-71	0-36	0-41	8-77	3-95	2-80	28-35
60 feet. Vaurioor, close to Cape Comorin.	1842.	0-35	0-65	...	...	2-25	1-20		0-40	1-82	4-35	9-25		20-27
	1843.	1-40	1-00	0-75	0-40	9-90	2-05	0-80	0-10	0-35	4-15	0-45	4-40	25-75
	1844.	...	0-50	0-20	...	0-90	3-00	0-80		0-15	7-45	3-70	1-35	18-05
	1845.	2-75	...	3-65	...	1-70	1-45	2-70		0-35	12-92	2-40	8-90	36-82
	1846.	...	...	0-90	1-65	5-20	2-90	0-55			4-00	5-50	1-75	22-45
Means		0-90	0-43	1-10	0-41	3-99	2-12	0-97	0-10	0-53	6-57	4-26	3-28	24-67
130 feet. Trevandrum.	1842.	3-70	0-35	0-85	3-60	13-70	9-50	4-25	3-75	6-30	3-05	8-30	0-35	57-70
	1843.	0-80	0-02	2-52	8-80	17-12	16-62	12-90	2-55	3-10	7-52	2-05	11-42	85-45
	1844.	...	...	1-10	0-30	4-55	6-15	3-80	5-67	3-75	15-17	4-40	2-15	47-05
	1845.	4-40	0-77	3-40	0-85	4-55	15-80	4-30	0-90	0-15	18-52	3-92	5-00	62-57
	1846.	0-10	...	1-07	4-02	11-42	17-75	6-92	3-67	0-75	17-50	4-40	2-30	69-92
Means		1-80	0-23	1-79	3-51	10-27	13-16	6-53	3-31	2-81	12-35	4-61	4-24	64-54
200 feet. Palemcottah.	1842.	1-20	0-19	1-05	0-45	0-55	0-02	0-01	0-03	2-50	6-60	9-62	0-95	23-12
	1843.	4-02	2-50	0-77	1-20	5-65					7-15	1-65	3-95	26-90
	1844.	0-20	0-55	...	0-75	0-10	1-17	0-05		1-75	2-65	1-70	2-76	11-68
	1845.	0-43	...	5-27	0-55	1-62	0-15			0-65	6-17	2-02	8-75	25-63
	1846.	0-80	1-35	0-57	2-30	4-05	0-07	0-07		0-30	1-80	5-97	0-65	17-75
Means		1-33	0-92	1-53	1-05	2-39	0-28	0-03	0-006	1-05	4-87	4-19	3-41	21-06
600 or 700 feet. Shenkottah.	1842.	1-20	1-30	...	2-05	3-90	5-65	2-80	1-40	3-10	3-20	14-85		39-45
	1843.	4-15	0-80	2-45	4-15	11-20	4-80	6-20	1-05	1-15	7-15	1-50	3-50	48-10
	1844.	...	...	2-40	...	0-50	3-60		1-80	1-60	6-25	4-25	3-70	24-10
	1845.	2-00	...	4-40	2-20		3-80	4-65	0-35		15-40	4-05	5-85	42-70
	1846.	0-57	...	0-70	4-65	6-20	8-60	3-40	1-10	2-50	5-95	4-47	3-45	41-60
Means		1-58	0-42	1-99	2-61	4-36	5-29	3-41	1-14	1-67	7-59	5-82	3-30	39-19

The stations extend along the Travancore or western coast from Cape Comorin, lat. $8^{\circ} 4'$, to Cochin, lat. $10^{\circ} 30'$. Of the three stations on the Tinnevely or Coromandel coast, Shenkottah is near Courtallum, immediately at the east base of the Ghâts, and about sixty miles from the sea-coast of Travancore; Palamcottah is about thirty miles from the east base of the Ghâts, is sixty miles from the western coast, and in the latitude of Quilon; Vaurioor is only three miles north and a little east of Cape Comorin. The western and eastern stations are separated by the Ghâts, which in some places rise to a height of 6000 feet, but within ten miles of Cape Comorin they break off into separate groups and peaks of greatly diminished height.

Bearing in mind the preceding extraordinary discrepancies, not only in the annual and monthly fall of rain in the same locality and in proximate localities, I proceed to place in juxtaposition the fall of rain at various places in India, from such returns as are within my reach, from places at the sea-level and at different elevations above the sea-level; and it may well be a question how far even the means of many years' observations in any one locality, exhibit normal conditions for the fall of rain in circumscribed areas, much less for districts or provinces.

The relative positions of the stations on the Malabar and Coromandel coasts having been previously stated, it only remains to notice the places somewhat inland. Poona, at 1823 feet, is about forty miles eastward of the western scarp of the Ghâts, and about sixty miles as the crow flies from the sea-shore. Kotergherry, at 6100 feet, is part of the Neelgherry mountains, and lies a little to the eastward of the ridges of Dodabetta at a lower level of 2340 feet. Dodabetta is part of the Neelgherries, and the highest point of the peninsula of India. Mahabuleshwur, Mercara and Uttra Mullay, at the common level of 4500 feet, are nearly in the same meridian, but lie between 9° and 18° north latitude, and are situated near to the western scarp of the Ghâts. (See annexed Table.)

The first feature of this Table is the almost total want of rain at every station in the month of February, with the exception of Kotergherry and Dodabetta, and it may be accounted for by this month being intermediate between the times of the S.W. and N.E. monsoons; the former, on the Malabar coast, commencing in May and ending in October, and the latter commencing on the Coromandel coast in October and ending in January. The other two intermediate months, March and April, have very limited falls of rain, excepting at Kotergherry, Uttray Mullay and Dodabetta, at which places the supply is very liberal; indeed at Kotergherry and Dodabetta the maximum monthly fall of the year appears to occur in April, a month that belongs to neither the Malabar nor Coromandel monsoons, but as the wind at Dodabetta was almost entirely from northerly and easterly points, the supply of rain may be more reasonably considered to appertain to the tail of the N.E., rather than to the commencement of the S.W. monsoon. Poona, at 1823 feet, and Mahabuleshwur in the Deccan and Mercara in Coorg, at 4500 feet, have scarcely a sprinkling from the N.E. monsoon, and the months of December, January, February, March and April, may be

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	Poona, 1823 feet.					Mahabuleshwur, 4500 feet.			Mercara in Coorg, 4500 feet.	Uttray Mullay. 4500 feet.		Doda- betta. 8640 feet.	
	1826.	1827.	1828.	1829.	1830.	Means of 15 years.	1834.	1842.	Means, 1838 to 1840.	1845.	1846.	1847, 1848.	
January	10	2·29	0·0	0·0	0·0	0·05	0·0	0·66	0·00	8·40	4·65	0·12	January.
February ...	60	0·0	0·0	0·0	0·0	0·25	0·25	0·0	0·45	0·15	0·45	7·43	February.
March.....	60	0·4	0·0	0·0	0·0	0·15	0·0	0·02	1·51	14·20	0·73	3·61	March.
April	60	0·0	0·0	0·0	1·04	1·31	{ Two showers. }	0·0	2·60	1·37	9·60	19·80	April.
May	641	0·04	1·95	2·74	0·79	3·31	0·16	3·74	7·37	22·68	36·50	4·86	May.
June	130	13·47	1·63	4·86	5·57	46·53	32·03	37·86	30·40	52·15	51·05	4·55	June.
July	343	1·79	7·58	4·38	5·35	92·10	118·60	117·76	55·88	50·15	33·32	7·45	July.
August	303	2·01	3·35	3·21	1·72	72·33	75·91	77·75	27·00	25·57	21·12	9·32	August.
September ...	554	4·51	6·92	0·33	0·29	31·32	65·97	56·00	11·91	8·08	7·33	7·52	September.
October	790	4·33	6·34	1·81	3·07	4·58	9·29	7·48	4·60	70·70	38·25	12·49	October.
November ...	1233	0·15	2·04	0·0	0·0	2·07	0·0	4·00	1·38	16·10	21·67	11·85	November.
December ...	640	0·0	0·0	1·20	0·0	0·05	{ One light shower. }	0·0	0·25	21·00	10·20	12·28	December.
Year	3634	28·63	29·81	18·53	17·83	254·05	302·21	305·27	143·35	298·55	235·87	101·24	Year.

[Lieut.-Colonel]

considered their dry months ; but Mercara, lying much further south than Mahabuleshwur, would appear to benefit slightly from the terminal falls of the N.E. monsoon, or from the preliminary squalls of the S.W. monsoon. On the eastern or Coromandel coast, the months that are comparatively dry (or rather destitute of rain, for the wet bulb won't admit them to be dry) on the western coast, are necessarily the reverse of the eastern, and we thus see October, November, December and January, at Madras, and the other stations on the east coast, with a monthly fall of rain ranging from 0·90 to 15·13 inches. The mean annual fall at Madras, for twenty-two years, from 1822 to 1843, ranges from 18·45 inches in 1832 to 88·68 inches in 1827. Calcutta, although within the regular S.W. monsoon, appears to benefit more from the Coromandel monsoon in January than any other of the stations noticed in the Table. The Tables indicate that as the stations approximate towards each other, and lie nearer to the apex of the triangular-formed peninsula of India, excepting at Cape Comorin, so do they respectively benefit in an increasing ratio from the monsoons of either coast. For instance, Madras derives considerable advantage from the Malabar monsoon in the months of July, August and September, while Cochin, Quilon, Allepy and Trevandrum, on the Malabar coast, and the mountain stations of Uttray Mullay, Kotergherry and Dodabetta, lying between the Malabar and Coromandel coasts, are liberally supplied from the N.E. monsoon in the months of October, November and December, while Mercara and Mahabuleshwur, similarly situated as Uttray Mullay and Dodabetta, but in a higher latitude, and nearly in the same meridian, derive scarcely any benefit from the N.E. monsoon ; indeed, north of the eighteenth parallel of latitude, that is to say of Poona, the rain-fall of the N.E. monsoon does not extend, at least as far as the returns I have been able to collect afford me the means of judging. Another feature of the Table indicates that the S.W. monsoon does not burst simultaneously along the whole line of the Malabar coast, but would appear to creep up slowly from Allepy towards Bombay, commencing in fact on the coast of Travancore partly in April, but absolutely in May, and rarely, if ever, commencing in Bombay before the first week in June ; the S.W. monsoon therefore on the coast of Travancore and Malabar, precedes the time of its occurrence at Bombay by a month at least. I had thought that along the coast the quantity of rain diminished as the latitude increased, and the large annual fall at Allepy and Cochin, compared with the fall at Bombay, seemed to justify the opinion ; but the fall at Cape Comorin, Trevandrum and Quilon, does not justify the opinion ; and supposing that any such law held good on the Malabar coast, it plainly has not any existence on the Coromandel coast. It is certain, however, that as the tropic is approached on the western side of India, the annual fall of rain sensibly diminishes along the coasts of Kattywar and Cutch ; and at the mouths of the Indus, not only is there no trace of the S.W. monsoon, but Lower Scinde would appear to resemble those singular tracts on the earth, the Deserts of Sahara and Gobi, and the coast of Chili, denominated "rainless." I regret not having been enabled to get more than one re-

gular meteorological register from the military cantonment at Kurrachee, situated near one of the mouths of the Indus ; but the scanty information supplied to me affords sufficient proof that if Lower Scinde does not belong absolutely to the rainless districts, it approaches very nearly to them. Unfortunately, neither in the large cantonment at Kurrachee, nor at other stations in Scinde, up to 1849 were regular meteorological registers kept, excepting perhaps for mere temperature, or if kept they were not made available to the public. Casual observations have been made and sufficient attention paid to the rain-fall, or rather the absence of it, to enable us to say definitely, that to Lower Scinde the S.W. monsoon, in its usual sense, does not extend, and that the country does not appear to have other sources of supply. The following Table is arranged from the details in a letter dated Kurrachee, July 6th, 1848, noticing the occasional showers that fell from April 1847, a pluviometer having been set up, but apparently for very little purpose :—

Register of Rain at Kurrachee, from April 1847 to July 1848.

1847.								1848.						
May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.	Jan.	Feb.	March.	April.	May.	June.	July.
None.	None.	A shower.	None.	None.	None.	None.	None.	None.	None.	None.	None.	None.	None.	A few drops.

It thus appears, if the register be correct, that in fifteen months only two showers occurred, and those so light as not to be appreciable by the pluviometer.

The following extract from a subsequent letter, dated 1st of September 1849, to a certain extent is in harmony with the previous record :—

“ We have had a few smart showers lately. Ever since June it has been constantly threatening rain ; the sky has been always overcast, and both in July and August showers fell, but the whole fall for the season has hardly amounted to 3 inches ; and yet in Upper Scinde, at Mooltan, in the Punjab, and generally in India, where the S.W. monsoon prevails, the usual averages have in some places been doubled.”

In this year, at Bombay, the unusual quantity of 118·88 inches fell. The only other part of Scinde from which I have any record is from Kotri near to Hyderabad on the Indus, and the return transmitted to me is annexed. The public duties of the observer called him occasionally away from his station and prevented a continuous record, and it is only for 1846 that a year's observations are complete, and in 1847 four months were omitted.

Extract from a journal kept at Kotri near Hyderabad on the Indus, by Mr. STRATH, engineer; for the months entered the blanks indicate the months of no observation.

	1845.	1846.	1847.	1848.	1849.
January		None.	None.		None.
February ...		None.			None.
March		*	None.		0·35 in.
April		None.	†		
May		1·75 in.	None.		
June		6·33 in.			
July		0·27 in.			
August		None.	‡		
September ...		None.	None.	None.	
October		None.	None.	§	
November ...		None.	None.	None.	
December ...	None.	None.	None.	None.	
		8·35 in.			0·35 in.

I am enabled, however, since the preceding notices were written, by the arrival in England of the medical returns from the Bombay army for 1847, to give an official and authentic record of the rain-fall and temperature at Kurrachee for 1847. It will be seen that for *eleven* months not a single shower fell, and that in one month only did rain fall, the record being in accord so far with the letter dated 6th of July 1848, differing, however, inasmuch as the rain in the official report is represented to have fallen in June and the latter in July.

1847.	Thermometer.			Rain.	Rain.
	Max.	Min.	Mean.		
January	83°	53°	67°	in. 3·0	On 24th, 25th and 31st.
February ...	74	59	67		
March	84	70	77		
April	84	75	79		Heavy showers, 23rd and 24th.
May	90	77	83		On 24th and 25th.
June	94	80	87		
July	90	78	84		Slight showers.
August	95	82	88		
September ...	90	80	86		
October	93	72	80		
November ...	91	59	72		
December ...	83	44	63		
Mean	88·42	69·08	77·75		

In a review of the above rain-records we find some singular features. Along the western coast of India, from Cape Comorin to the mouth of the Indus, there is a paucity of rain at the former place, and an almost total absence of it at the latter,

* On the 4th of this month a few drops of rain fell at sunset.

† Rain during the night and light showers during the day.

‡ At sunset drops of rain.

§ At P.M. of the 7th heavy showers of rain with wind, thunder and lightning.

|| On the 30th light showers.

while a few miles from Cape Comorin the annual supply amounts to 113 inches ; and in the latitude of Bombay it does not fall below a mean of 76·82 inches for the monsoon alone, but proceeding along the coast northwards the supply further diminishes, or becomes irregular and uncertain in quantity, and at the mouths of the Indus almost disappears. Along the Coromandel coast the N.E. monsoon does not appear to supply much more than one-half of the fall on the Malabar coast, the mean of twenty-two years at Madras being 44·57 inches, the quantity slightly increasing with the latitude from Cape Comorin. Another marked feature is the greatly increased fall of rain with the increased elevation above the sea-level of the place of observation up to a certain height, beyond which the quantity of rain gradually diminishes as the place of observation is more elevated ; at least such are the indications in the tract of land which lies between the sea-shore and the base of the Ghâts between Cape Comorin and Goozerat, and along the western face of the Ghâts themselves. The observation does not hold good however on the elevated lands to the eastward of the crest of the Bombay and Malabar Ghâts. Along the sea-coast the falls vary from 28·35 inches at Cape Comorin to 113·26 inches at Allepy, but increase at stations nearing the Ghâts at different elevations to more than 300 inches at 4500 feet, and above that height the falls gradually diminish in quantity. For instance, the

	inches.
Mean of seven stations at sea-level, western coast, is	81·70
At 150 feet, Rutnagherry, in the Konkun	114·55
At 900 feet, Dapoollee, Southern Konkun	134·96
At 1740 feet, Kundalla, the pass from Bombay to Poona	141·59
At 4500 feet, Mahabuleshwur, means of fifteen years	254·05
At 4500 feet, Mercara in Coorg, means of three years	143·35
At 4500 feet, Uttray Mullay*, in Travancore, means of two years	263·21
At 6100 feet, Kotergherry, on the Neelgherries, one year	81·71
At 8640 feet, Dodabetta, highest point of Western India, one year	101·24

Hence the elevation of the line of maximum fall would appear to be about 4500 feet, and above this level the supply of rain is diminished ; Mahabuleshwur, Mercara and Uttray Mullay, although differing greatly in latitude, lie nearly in the same meridian, and are all at the same elevation. The comparative small fall at Mercara is accounted for by the fact of its not being so near the western scarp of the Ghâts as Mahabuleshwur and Uttray Mullay, and the effect of a station being placed a few

* Since the above was written I have received a letter from General CULLEN, dated Trevandrum, 6th of January 1850, giving an account of the fall of rain for 1849 at the Uttray Mullay range.

	inches.
At 500 feet, base of range.....	99
At 2200 feet, Attagherry.....	170
At 4500 feet, Uttray Mullay	240
At 6200 feet, Agusta Peak	194

Showing at 6200 feet a fall of 46 inches less of rain than at 4500 feet.

miles east of the Ghâts upon the fall of rain has already been shown at Mahabuleshwur and Paunchgunny, the latter place being eleven miles eastward of the former, and at a lower level of only 500 feet, yet the mean fall of 15 years at Mahabuleshwur was 254·05 inches, and of the latter 50·69 inches. In 1849 the contrast was still greater, the fall at Mahabuleshwur amounting to the enormous quantity of 338·38 inches, while at Paunchgunny only 58 inches fell.

Rain at different Elevations.

Mr. MILLER, in his Meteorology of the Lake Districts of Cumberland and Westmoreland, has adduced sufficient evidence to prove that the same law, if it be a law, obtains in England in mountainous districts, but Mr. MILLER's elevation of maximum fall is about 2000 feet instead of 4500, as in India. This difference no doubt results from the differences of latitude and consequent mean temperature, and would indicate that the stratum of vapour supplying the maximum quantity of rain floats at a less height beyond the tropics than within them.

In a communication from Mr. MILLER, dated June 7, 1849, he gives as follows the fall of rain at different heights in the Lake Districts of Cumberland and Westmoreland.

“The Mountain Gauges.

No.		In 13 months. 31st of December 1847 to 31st of December 1848.	Summer months. 1st of May to 31st of October.	Winter months. December 1847 to April 1848, and November and December 1848.
XXI.	Sca Fell Pike, 3166 feet above the sea	in. 64·73	in. 49·46	in.
XXII.	Great Gable, 2928 feet above the sea	From 1st of May. 91·32	46·81	44·51
XXIII.	Sprinkling Tarn, 1900 feet above the sea	148·59	70·95	77·64
XXIV.	Stye Head, 1290 feet above the sea	138·72	60·35	78·37
XXV.	Brunt Rigg, 500 feet above the sea	109·19	43·18	66·01
XIV.	Valley to the west, Wastdale	127·47	50·16	77·81
XIII.	Valley to the S.E., Eskdale	95·71	37·69	58·02
XXVI.	Seatollar Common, } 1334 feet above the sea ...	139·48	57·97	81·51
XIX.	Valley, Seathwaite, } Borrowdale	177·55	68·96	108·59

“From the table for the summer months, it appears that between the 1st of May and the 31st of October, the gauge at 1290 feet has received $20\frac{1}{2}$ per cent. more rain than the valley; at 1334 feet, $15\frac{1}{2}$ per cent. more; at 1900 feet, $41\frac{1}{2}$ per cent. more; at 2928 feet, 6 per cent. less; and at 3166 feet, 1 per cent. less than the valley.

“In the winter months, the gauge at 1290 feet has collected 0·5 per cent. more; at 1344 feet, $5\frac{1}{2}$ per cent. more; at 1900 feet, 1 per cent. more; and at 2928 feet, $42\frac{1}{2}$ per cent. less than the adjacent valley.”

These results are in accordance with the observations I have given from India. In addition to the preceding from Mr. MILLER, I annex his rain-records at lower levels

for the years 1845, 1846, 1847 and 1848, as they confirm, from European localities, the facts I have stated respecting the great discrepancies in the fall of rain at proximate stations.

Fall of Rain in the Lake Districts of Cumberland and Westmoreland at various heights up to 3166 feet. Wind almost uniformly from a westerly quarter.

Years.	Whitehaven.				Keswick, 258 feet.	Cockermouth.	Bassenthwaite Hills, 210 feet.	Vale of Gillerthwaite, Ennerdale, 286 feet.	Loweswater, 336 feet.	Foot of Crummock Lake, 283 feet.	Gatesgarth, Buttermere, 326 feet.	Eskdale.			Westmoreland.				Borrowdale.		
	High Street, 90 feet.	Round Close, 480 feet.	Saint James's Church Steeple, 78 feet.	The Flish, 3 miles south of Whitehaven.								Centre of Vale, 166 feet.	Head of Vale.	Wastdale Head, 166 feet.	The How, Troutbeck, 300 feet.	Amble-side, 190 feet.	Langdale Head, 250 feet.	Seathwaite, in Garden, at 6 inches, 1334 feet.	Seathwaite, in the Field, at 18 inches, 1334 feet.	Stone-thwaite.	Seatoller Common, 1334 feet.
1848.	47.342	46.700	36.344	60.82	66.407	52.37	47.06	97.73	76.668	98.07	133.55	86.78	70.38	115.32	91.347	76.82	130.38	160.89	157.22	130.24	139.48
1847.	42.921	42.023	30.713	47.80	58.286	42.55	44.45	80.13	66.209	82.32	106.25	58.66	74.93	96.34	78.004		112.95	129.24	126.80	106.21	
1846.	49.134		35.422	55.16	67.678	52.41		83.87	70.249	96.47	121.90			106.93	77.719		127.40	143.51			
1845.	49.207		33.480	53.00	62.212	46.93		76.88	69.542	87.48	124.13			108.55	76.305		136.00	151.87			

These records attest that in the town of Whitehaven the difference in the fall of rain in the High Street and on the steeple of St. James's Church in 1845, was 15.727 inches, and in 1848 the difference was 10.998 inches, while at the Flish, only three miles south of Whitehaven, the fall was in excess in those years respectively over the fall in the High Street, 2.873 inches and 13.478 inches, while the excess over the fall on the steeple was 19.600 inches and 24.476 inches. In Borrowdale also, in a field adjoining a garden, in the years 1847 and 1848, the fall was in excess in the garden of 2.44 inches and 3.67 inches respectively. These facts suggest caution to all observers in other parts of England and elsewhere.

Professor PHILLIPS for some years made observations at York on the top of the Minster, the Museum, and on the ground, to determine the effect of elevation upon the rain-fall, but objections being taken to the results in respect to eddies of wind and other causes of error, he lifted rain-gauges into the air, independently of buildings, and the results of the observations for the years 1843 and 1844 he communicated to the British Association at York in 1844.

The sums of the rain-fall in the two years at different heights were,—

	inches.
24 feet.	24.158
12 feet.	26.039
6 feet.	26.109
3 feet.	26.298
1½ foot.	26.559

The nearer the earth, therefore, the greater the amount of rain collected. Mr. MILLER's records at Whitehaven, giving for four years the amount of rain collected in the High Street and on the Church Steeple, confirm Professor PHILLIPS's observations, supposing the steeple to be 78 feet above the High Street. At Calcutta, in the

Surveyor-General's Office, similar observations were made at the respective heights of 4 feet and 40 feet, and I have inserted the results in the general rain-fall Table for four years. The sums were respectively 261·97 inches and 247·41 inches, and the annual means 65·49 inches and 61·85 inches.

Dr. BUIST states that at the observatory in Bombay, in two rain-gauges, one at 3 feet from the ground, and the other above the observatory at 32 feet, the

	inches.
Lower gauge for 1843 . . .	=56·24
Upper gauge for 1843 . . .	=49·07
Lower gauge for 1844 . . .	=66·51
Upper gauge for 1844 . . .	=66·08

These results therefore are in accordance with Professor PHILLIPS and Mr. MILLER's observations, taken at limited heights, but entirely antagonist to Mr. MILLER's own observations and those I have supplied in this paper from India for heights exceeding a few hundred feet. The supposed law may hold good for small differences in elevation on the plains, but the law is reversed in mountainous districts.

Two great features of my Rain-table demand express notice, the paucity of rain at Cape Comorin and at Kurrachee, at the extremities of nearly the same meridional line, and the prodigious fall of rain at the elevation of 4500 feet at three stations differing greatly in latitude, but situated nearly in the same meridian. I have not any satisfactory explanation to offer of the want of rain either at Cape Comorin or at Kurrachee. I am not at all acquainted with the physical character of the country about Cape Comorin, and cannot therefore express an opinion how far local circumstances occasion the phenomenon. I had thought that a very high mean temperature, with a limited range of the thermometer, might account for the want of rain at Kurrachee on the Indus, the vapour from the ocean not coming into an air cold enough to condense it; but the register of the thermometer for 1847 at Kurrachee shows that I could not find a satisfactory argument upon its records, for the maxima are not so great as at Calcutta, Madras and Bombay; the minima are lower, and the mean temperature is lower than at either of the places; some other cause therefore must be looked for than a high mean temperature. The clouds which constantly pass over Kurrachee, would appear not to be condensed until they impinge upon the Sulimane Mountains, which run parallel to the right bank of the Indus. The explanation of the prodigious fall of rain at the level of 4500 feet is simple and satisfactory. The chief stratum of aqueous vapour brought from the equator by the S.W. monsoon is of a high temperature, and floats at a *lower* level than 4500 feet; indeed I have looked over, or upon the upper surface of the stratum at 2000 feet. It is dashed with considerable violence against the western mural faces of the Ghâts, and is thrown up by these barriers in accumulated masses into a colder region than that in which it naturally floats; it is consequently rapidly condensed and rain falls in floods. The uncondensed vapour which escapes up the chasms and over the crest of the Ghâts,

affords the precarious and scanty supply to the lands to the eastward, which the Tables show.

Maximum Daily Rain-Fall.

As might be looked for, the greatest fall of rain in any one day is met with in the records of those stations where there is the greatest annual fall, Mahabuleshwur and Uttray Mullay. At the former station the greatest fall in any one day in fifteen years was 13·06 inches on the 2nd of September 1833, but the months of June, July and August, have numerous instances of a daily fall of 11·32 inches, 12·76 inches, and 12·69 inches in those months respectively. The greatest monthly fall at Mahabuleshwur was 134·42 inches in July 1840. At Uttray Mullay the greatest daily fall in three years was 15·1 inches on the 14th of October 1845. On the 11th of December of the same year there was a fall in one day of 11·4 inches, and on the 9th of October 1844 the fall in one day was 9·0 inches. In 1846 there was not a daily fall approaching these figures. In Bombay, in 1845, the greatest daily fall was 4·71 inches on the 24th of July, and the next year, on the 16th of July, a daily fall occurred of 5·16 inches; but Dr. Buist mentions that on the 1st of July 1844 there was a fall of rain in twenty-four hours of 7·44 inches, two inches having fallen in seventy minutes; but this was on the first burst of the monsoon, which set in later than usual by three weeks. On the 10th of July, however, 9·43 inches fell, the greatest of the year. In the Deccan there is rarely a greater daily fall than two inches, but in the Sattarah records a maximum daily fall of 4·40 inches in four years is stated to have occurred in April; but this was in one of the squalls which are the precursors of the monsoon. Annexed is a comparison of the fall of rain in Bombay and at Mahabuleshwur for many years.

Register of the Pluviometer at Bombay, from the year 1817 to 1849.

Years.	June.	July.	August.	September.	October.	Total fall in June, July, August, September, and October of each year.	Mahabuleshwur, 4500 feet.
	in.	in.	in.	in.	in.	in.	
1817.	45.72	23.67	9.34	24.87		103.60	
1818.	22.54	17.69	28.45	10.39	2.07	81.14	
1819.	15.95	31.66	20.24	10.11		77.96	
1820.	18.82	28.37	29.49	10.66		77.34	
1821.	15.18	20.60	28.52	18.29		82.59	
1822.	29.64	26.59	33.83	22.16		112.22	
1823.	21.76	15.96	19.70	4.28		61.70	
1824.	3.89	8.07	17.86	1.78	2.37	33.97	
1825.	24.45	25.17	12.94	9.68		72.24	
1826.	17.75	26.97	8.40	23.50	1.87	78.49	
1827.	49.15	10.29	10.51	10.16	0.92	81.03	
1828.	23.53	52.75	17.22	22.08	6.40	121.98	
1829.	27.86	19.78	12.40	4.95	0.66	65.65	257.06
1830.	20.96	32.46	10.66	7.78		71.86	232.93
1831.	22.46	27.31	27.64	22.34	2.08	101.83	185.82
1832.	13.63	48.05	4.65	7.11	0.65	74.09	226.87
1833.	12.50	21.80	13.35	23.54	0.20	71.39	203.74
1834.	14.16	21.83	18.05	12.55	3.88	70.47	297.31
1835.	9.99	4.27	35.76	12.17	0.42	62.61	226.71
1836.	21.36	24.53	37.41	4.69		87.99	243.56
1837.	12.61	24.39	22.43	5.15		64.58	267.76
1838.	29.70	8.70	7.34	5.04		50.78	180.17
1839.	18.28	32.19	18.45	4.70		73.62	233.23
1840.	25.04	24.24	4.20	7.55	2.12	63.15	284.43
1841.	25.27	21.21	20.53	1.27	3.21	71.49	281.04
1842.	16.84	26.45	37.10	10.41	4.36	95.16	304.90
1843.	9.33	22.49	18.20	9.00	0.25	59.27	285.67
1844.	14.17	35.52	6.55	9.16		65.40	262.32
1845.	19.70	20.44	6.56	8.03		54.73	249.93
1846.	31.71	40.56	5.60	8.45	1.16	87.48	288.34
1847.	35.47	16.80	8.92	5.80	0.32	67.31	218.83
1848.	42.37	13.83	7.87	4.01	5.34	73.42	245.01
1849.	22.82	51.68	13.66	29.65	1.07	118.88	338.38
Average for } 33 years.	22.26	25.04	17.08	11.25	1.19	76.82	

Note.—The Bombay register is only for the months of the monsoon, and it does not appear that any record has been kept of the fall in the other months annually. The record at Mahabuleshwur is for the whole year.

Winds at Madras.

Direction of the Wind at Madras, blowing from the different *quarters* of the compass, from hourly observations.

	1841.				1842.				1843.				1844.				1845.			
	N.W.	S.W.	S.E.	N.E.	N.W.	S.W.	S.E.	N.E.	N.W.	S.W.	S.E.	N.E.	N.W.	S.W.	S.E.	N.E.	N.W.	S.W.	S.E.	N.E.
Jan....	...	...	...	...	105	51	80	508	74	29	92	549	64	47	193	440	87	27	196	434
Feb....	...	...	...	...	95	124	227	229	23	82	434	133	45	92	309	249	11	71	393	197
March	2	170	372	0	26	201	276	1	15	179	527	23	12	266	449	17	13	221	477	33
April	120	471	115	14	0	307	269	0	0	230	488	2	15	236	451	19	3	380	335	2
May	57	407	219	37	56	468	211	9	73	376	260	35	67	426	215	36	73	437	204	30
June	86	539	86	9	86	447	133	30	88	553	76	3	196	397	84	43	228	396	51	45
July...	144	511	76	13	96	578	62	13	121	562	54	7	127	498	111	8	128	498	108	10
Aug.	85	352	288	19	37	487	144	26	120	494	117	3	200	411	95	38	135	472	129	8
Sept.	115	356	182	43	91	377	174	76	153	355	168	44	114	359	187	60	93	371	228	28
Oct....	265	93	145	241	101	203	221	219	194	183	119	248	171	251	125	197	83	274	229	158
Nov.	232	16	58	414	139	16	34	531	194	2	11	513	236	0	51	433	96	0	66	558
Dec.	219	0	10	515	157	16	70	501	207	19	144	374	246	8	64	426	149	14	104	477
Total	...	...	...	...	1039	3275	1901	2145	1262	3064	2490	1934	1493	2991	2340	1966	1099	3161	2520	1980

Note.—N.W. includes the winds blowing between N. and W. points, and the same applies to the other quarters.

The prevailing wind at Madras is the S.W., which wind brings to Bombay and the Malabar coast their monsoon rains, but which to Madras becomes mostly a *hot* wind after traversing the peninsula, and carries with it to Madras only a few showers. The S.W. wind begins to prevail at Madras over every other wind in the month of April, and mostly remains the dominant wind until the end of September: the S.E. wind then prevails until October, about the middle of which month the approaching N.E. or rainy monsoon of the Coromandel coast almost displaces the S.E. wind. Its full development takes place in November, and it remains the prevailing wind until February, when the S.E. regains its influence, and is greatly the dominant wind until April, when it is gradually displaced by the S.W. wind. The N.W. wind makes its appearance in every month of the year, chiefly in the months of the S.W. or Malabar monsoon, but least so in the months of March and April, in the years 1841 not in March, and 1842 not at all in April; the N.E. wind failed in the same months in the respective years. The N.W. is the quarter from whence Madras has the smallest amount of wind. The largely prevailing wind is the S.W., which, coming up from the equator, is the chief source of rain supply to India generally, but of which the Coromandel coast gets but a scanty proportion in consequence of the high western Ghâts intercepting and condensing the chief part of the vapour brought by that wind. The next prevailing wind is the S.E., and this equally blows from the equator upon Madras and the Coromandel coast; but it is nevertheless not the rain-bearing wind of the Madras monsoon, which is from the N.E., sweeping over the bay of Bengal from Burmah. The S.W. wind from the Indian Ocean is a rain-bearing wind, but

the S.E. wind, also from the Indian Ocean, is not a rain-bearing wind in the sense of a monsoon. By a comparison of the Wind-table with the Rain-table of Madras, it will be seen that in the months in which the S.E. winds prevail, February, March, April, September, and early part of October, the first three months are almost entirely rainless, and the little that falls in September is owing to occasional S.W. winds. This may be owing to the air over the peninsula in those months, from the position of the sun in the ecliptic being too hot to condense the S.E. vapour. On the contrary, the N.E. wind from the bay of Bengal comes loaded with vapour in November, December and January, and the sun being far south, the peninsula is rapidly cooling, and the vapour from the bay of Bengal being of a higher temperature than the air upon which it is thrown, is condensed. The valuable table from the Madras Observatory shows by inspection, the gradual changes of the wind from one quarter to another, and the total annual numbers under each quarter show considerable uniformity in successive years.

Winds at Bombay.

The following Table of the winds at Bombay, for the year 1843, is from the *hourly* observations taken by Dr. BUIST, LL.D. Although the numbers of hours at which the winds blew are placed under the points N.W., S.W., S.E., N.E., the numbers embrace the winds within each quarter of the compass; for instance, N.W. embraces all the winds blowing between N. and W., and so on for the other quarters.

	N.W.	S.W.	S.E.	N.E.
January	382	56	11	157
February	397	15	32	132
March.....	451	30	28	138
April	442	94	17	47
May	380	273	12	1
June	85	438	73	7
July	133	491	0	0
August	184	460	2	3
September	98	390	43	53
October	280	41	50	253
November	224	1	51	348
December	130	0	64	382
Year	3184	2289	383	1521

The Table shows that the prevailing wind at Bombay, in 1843, was from the points between N. and W., having blown at 3184 hours. The next prevailing wind is from the points between S. and W., having blown for 2289 hours, being the rain-bearing wind of the so-called S.W. monsoon, from May to September inclusive; but in truth the wind most generally blows from the W.S.W., with a leaning to a point more westward, and a S.W. and by S. wind during the monsoon is of rare occurrence. The wind from the points between N. and E. is the next most prevalent. It begins to prevail in October, and continues prevalent as long as the N.E. monsoon blows at

Madras, on the opposite coast of India ; it may be said to cease entirely when the Malabar monsoon approaches in May. The S.E. wind, unlike Madras, is little felt at Bombay, having blown at 383 hours only during 1843. Few as the occasions are on which it blew at Bombay, it would appear to have resulted from the veering of the N.E. wind through E. to a point southward of east ; a wind strictly from the S. is scarcely known at Bombay. The N.W. wind, which is the dominant wind at Bombay and the least prevailing wind at Madras, blows at Bombay most in those months when it is least felt at Madras, namely, the first five months of the year, and in the months of March and April it almost disappears at Madras ; but at Bombay in those months it blows at more hours than any other wind blows in any other two months of the year, excepting probably the W.S.W. in July and August. In January, February, March, April, and up to the third week in May, the wind from the N. to W. points blows from eleven to twenty-two hours daily, the remaining hours having the wind from the N. to E. points, constituting the land-wind, the N.W. being considered the sea-breeze. In part of October, November and December, the reverse is the case, the N.E. or land-wind prevailing from thirteen to nineteen hours daily, and the sea-breezes appearing between noon and 10 P.M. The suddenness with which the prevailing winds commence and terminate is a marked feature in the meteorology of Bombay. The wind from the S. to W. points begins suddenly the third or fourth week in May, and as suddenly terminates in the first or second week of October. The wind from the N. to E. points takes the place of the S.W. wind and terminates suddenly in May. The N.W. wind, owing to its being a daily alternating wind with the land-breeze, from the heating action of the sun upon the land daily, has a more continuous character throughout the year, appearing even in the monsoon months, when a few days' sunshine heats the land ; it is however much less frequent in those months than when the sun bears with continued force upon the land.

Winds at Calcutta.

With regard to the winds at Calcutta, Dr. J. M'CLELLAND, in the fifth volume of the Calcutta Journal of Natural History, has given a reduction of the meteorological register kept at the Surveyor-General's Office, Calcutta, from the 31st of October 1843 to the 31st of October 1844 ; from his paper the following extracts are made in a condensed form :—

	Wind at noon.			
	N.W.	S.W.	S.E.	N.E.
1843.				
November	26	1	0	3
December	24	0	0	5
1844.				
January	25	0	3	3
February	23	2	0	4
March	1	20	8	0
April	0	26	3	1
May	0	24	4	3
June	5	21	3	2
July	1	17	11	1
August	1	17	13	0
September	4	19	5	2
October	4	7	6	3
Year	114	154	56	27

As these observations are only for noon in each day of the year, they are of little value; supposing them however to be typical of the variations of the wind, there is a certain accordance with the winds at Madras and Bombay in the respective months of the year, with respect to the winds blowing between the points S. and W., and with Bombay in the winds blowing between the points N. and W., excepting that the latter cease to prevail in March in Calcutta, but do not relax in Bombay until June. The winds of the S.W. monsoon would appear to commence in March in Calcutta, but not until May in Bombay. The tendency of the N.W. and S.W. winds in Calcutta, however, is rather to blow, the former from the N. and the latter from the S., than from intermediate points; out of 353 observations, the wind blew sixty-eight times from the N. and ninety-one times from the S., while the N.W. blew only thirty-four times and the S.W. wind only forty-two times. Judging from the Table, Calcutta is little affected by the winds of the N.E. monsoon. There is only a single instance of "no wind" at noon during the whole twelve months.

The following Table shows the direction of the wind at six different hours, and the relative duration of each wind throughout the year:—

	N.	N.W.	W.	S.W.	S.	S.E.	E.	N.E.	Calm.	No. of observations.
Sunrise	32	16	7	19	68	29	25	20	137	353
9 ^h 50 ^m A.M.	50	29	42	49	75	34	23	37	3	351
Noon	58	34	53	42	90	19	29	26	1	352
2 ^h 40 ^m P.M.	52	40	58	27	105	20	20	24	6	352
4 ^h P.M.	53	42	45	36	93	34	31	12	6	352
Sunset	56	18	11	23	116	23	17	9	78	341
Total	310	179	216	190	536	150	145	118	231	2101
Proportion of each.....	51.6	29.8	36	32.6	89.3	26.5	24.1	19.6	38.5	350

From this Table the S.W. monsoon, which at Bombay has a disposition chiefly to

blow half a point to the westward of W.S.W., at Calcutta blows chiefly from the S., and the prevailing N.W. wind of Bombay becomes a northerly wind in Calcutta. The Table shows that almost the only hours of calm are at sunrise and at sunset.

Winds at Mahabuleshwur.

At Mahabuleshwur, at 4500 feet above the sea-level, the following is the mean monthly direction of the wind, as given by Dr. MURRAY in a synoptical view of fifteen years' observations :—

	Direction.	Force.
January	Easterly.	Moderate.
February	E.N.E., N.N.W.	Moderate.
March	N.E., N.N.W.	Moderate.
April	N.E., N.N.W.	Moderate.
May	N.E., N.N.W.	Moderate.
June	W.S.W.	Moderate.
July	W.S.W.	High.
August	W.S.W.	High.
September	W.S.W.	Moderate.
October	Variable.	Light.
November	Easterly.	High.
December	Easterly.	High.

From the generalities in the Table, it would not be safe to deduce any normal conditions from comparisons with the wind tables of Madras, Calcutta or Bombay. In the months of January, November and December, Mahabuleshwur would appear to share more largely in the easterly winds of Madras than in the N.W. winds of Bombay in those months; but in February, March, April and May, and the rest of the year, the station would appear to have much the same winds as prevail at Bombay. Its elevated position, therefore, does not remove it from the periodic currents of air of a lower level. Even Dodabetta, at its great elevation of 8640 feet, is partly subject to the periodic winds of Madras and Bombay, at least as far as one year's record of observations recorded *hourly* from OSLER's anemometer enables us to judge.

Winds at Dodabetta.

	N.W.	S.W.	S.E.	N.E.
1847.				
February	5	0	17	6
March	4	0	2	25
April	7	3	2	18
May	3	8	2	18
June	4	26	0	0
July	18	13	0	0
August	27	0	1	3
September	21	5	0	4
October	9	0	12	10
November	5	7	3	15
December	6	0	8	17
1848.				
January	2	0	16	13
Year	111	62	63	129

The winds of the S.W. monsoon, however, terminate in July instead of October. This is the more remarkable, as Dodabetta lies between Madras, where these winds are the prevailing winds of May, June, July, August and September, and Bombay, where the same winds prevail in the same months. It is probable, therefore, that Dodabetta is situated just above the upper surface of the stratum of wind and aqueous vapour which supplies the S.W. monsoon to Western India, and therefore has comparatively a small supply of rain from this source. But it is not situated (although on the western coast) above the stratum of wind and aqueous vapour which supplies the Coromandel coast during the N.E. monsoon, as it has the same prevailing winds between the N. and E. points in the same months as at Madras, from October to February, when the N.E. ceases at Madras, but continues at Dodabetta until late in May. The *prevalence* of winds from points between N. and W. in the months of July, August and September, is peculiar to Dodabetta: neither Mahabuleshwur, at 4500 feet, nor Madras, Bombay nor Calcutta has similar indications. However, as this so-denominated N.W. wind very frequently blows from only one or two points to the northward of west, the wind may belong to the monsoon of Western India, local physical circumstances having given it a slant. On the whole it may be said, that the peninsula of India, in its length and breadth and the atmosphere over it to the height of nearly 9000 feet, is subject to periodic winds; but at widely separated places, varying a point or two from the apparent normal winds, and commencing or terminating a week or two earlier or later. May, June, July, August and September have prevailing winds from points between W. and S.; October, November, December, January and February have prevailing winds from points between N. and E.; February, March and April, at Madras, have a prevailing wind from points between S. and E., as opposed to Bombay, where the wind in those months prevails from points between N. and W., with rare instances of a S.E. wind; while at Calcutta the N.W. wind terminates in February; but a series for years of hourly observations, like those at Madras, are necessary to give that confidence which cannot be given to deductions from the meteorological data at present available from India.

It is usually understood that very high winds materially depress the barometer, but the records at Dodabetta do not support this view. On the 17th and 18th of April 1847, the wind blew with a mean pressure of 21 lbs., and 14 lbs. respectively upon the square foot; but the barometer only fell from 21·955 on the 16th to 21·917 on the 17th, and rose to 21·984 on the 18th; and there was a maximum pressure from the wind on the 17th at one time of 35 lbs. 26th of May, maximum wind 28·5 lbs., barometer not affected more than 0·010 inch; 12th of June 30 lbs., 26th of July 32 lbs., 10th of September 35 lbs., and 14th of October 22 lbs.; but these pressures of the wind had little or no effect upon the barometer.

Fogs.

In the Madras observations there is not any mention made of fogs; and Captain LUDLOW of the Madras Engineers, who recorded the observations, tells me that fogs are almost unknown at Madras, the nearest approach to a fog being an occasional mist on the ground, not visible more than 3 feet above the surface.

	Madras.	Bombay. 1843.	Calcutta, 1843-44.	Mahabuleshwur, 15 years.	Dodabetta, 1847-48.	
	Fogs.	Fogs.	Fogs.	Fogs.	Fogs.	Dense fog.
January	0	0	5	0	0	5
February	0	0	3	0	11	9
March	0	0	4	0	4	0
April	0	0	4	0	1	4
May	0	0	0	Foggy last half. Foggy. Continued dense fogs. Continued dense fogs. Frequent light fogs. First half light fogs.	6	1
June	0	0	0		16	2
July	0	0	1		2	9
August	0	0	0		5	20
September	0	0	0		2	15
October	0	0	1		2	29
November	0	0	0	0	3	26
December	0	0	2	0	1	16
Year	0	0	20	0	53	136

At Bombay there are not any records of fogs in the hourly observations for 1843 nor 1844, but Dr. BUIST informs me that mists are of regular occurrence in March; they begin early in February and continue for five or six weeks. They disappear after sunset, and are slightest from sunrise to noon*. At Calcutta, from Dr. M'CLELLAND's Report, there were only twenty instances in 1843-44, almost entirely at sunrise. At Mahabuleshwur, from the latter end of May until the middle of October, there is almost a continued fog with deluges of rain. At Dodabetta the prevalence of fogs is very remarkable, there being no less than 136 records of dense fogs and 53 of light fogs during the year. Of the dense fogs 52 occurred at 9^h 40^m A.M. and 85 at 3^h 40^m P.M.; 27 light fogs at 9^h 40^m A.M. and 25 at 3^h 40^m P.M.

Electricity.

I have not any data of a character to enable me to speak satisfactorily on the subject of the electric state of the air at the several stations.

I conclude with a rapid analysis of the preceding Tables.

In my paper published in the Philosophical Transactions in 1835 upon the Meteorology of the Deccan, I arrived at the following conclusions:—That HUMBOLDT was mistaken in supposing, upon the authority of HORSBURGH, that the diurnal atmo-

* I presume Dr. BUIST means misty and comparatively semi-transparent *heated* air, as distinct from fogs.

spheric tides were suspended during the monsoon in Western India. The fact was also unquestionably established of the existence of four atmospheric tides within twenty-four hours,—two diurnal and two nocturnal, each consisting of a maximum and minimum tide, and the occurrence of these tides within the same limit hours as in America and Europe; the greatest mean diurnal oscillations taking place in the coldest months, and the smallest tides in the damp months of the monsoon in the Deccan, whilst at Madras the smallest oscillations were in the hottest months: it was shown also that these tides took place regularly without a single instance of interruption, whatever the thermometric or hygrometric indications might be, or whatever the state of the weather, even storms and hurricanes only modifying and not interrupting them. The anomalous fact was also shown of the mean diurnal oscillations being greater at Poona, at an elevation of 1823 feet, than at the level of the sea, in a lower latitude at Madras or Bombay, while at an elevation greater than Poona, the mean diurnal oscillations were *less* than at Poona. It was shown also that the seasons did not affect the limit hours of the tides; but it was shown at the same time that the turning-points of the tides were sometimes irregular; that a tide flowed for a longer or shorter period, and that there were numerous cases of a stationary state of the atmosphere at the hours when the tides should turn. It was shown that the maximum mean pressure of the atmosphere was greatest in December or January, gradually diminished to June, July or August, and subsequently increased to the coldest months. The trifling daily and annual range of the barometer, compared with the ranges in extra-tropical climates, was shown; also the more limited annual range of the thermometer in the Deccan than in Europe, but the existence of a greater daily range; the maximum mean temperature was in April or May, and then gradually declined to the coldest months; very considerable differences in the dewing-points within very narrow areas were shown; dew being frequently local and occurring under anomalous circumstances, and the great contrast between the dewing-points, on the sea-coast at Bombay and in the Deccan, was pointed out; the rain in the Deccan was not more than 28 per cent. of the quantity that fell in Bombay; the winds principally westerly and easterly, rarely from due north or south; and finally, the rarity of fogs was stated.

It is very satisfactory to find in the discussion of the meteorological observations in the present paper, from a very extended area, from different heights and from observations, some of which are hourly, and which run through several years at the same station, that after an interval of twenty years the accuracy of the former deductions should be established almost to the letter; but as some of the observations now discussed were from hourly records, continued through considerable periods of time, an opportunity has been afforded of investigating abnormal conditions, which the former limited number of diurnal observations did not permit; and from these sources a rapid review of what appear to be normal and abnormal conditions is now appended.—

1st. The annual and daily range of the barometer diminishes from the sea-level up to

the greatest height observed, 8640 feet at Dodabetta, from a mean annual and mean daily range at Madras of 0·735 and 0·122 to 0·410 and 0·060 at Dodabetta;—the annual range would appear to increase, about and beyond the northern tropic, as the annual range at Calcutta (not by hourly observations) is 0·911 : but the diurnal range is somewhat less (0·115) than at Madras. At no one of the places of observation, even taking the maximum pressure of one year with the minimum pressure of another year, does there appear to have been a range of pressure equivalent to an inch of mercury; nevertheless in the Cyclones, or rotatory storms, there occurs at times a range of pressure of nearly 2 inches of mercury within forty-eight hours; but it is shown from a comparison of the simultaneous records on board ship, where these great depressions were noted, with the records at the observatories on shore, that the great depressions occurred within very limited areas.

I had formerly shown that the times or turning-points of ebb and flow (if the terms be permitted) of the ærial ocean were occasionally retarded or accelerated, although the means fixed the turning-points within certain limit hours; but I was not aware that in the ebb or flow of the four daily tides, they ever retrograded or halted in their onward or retiring course. The hourly observations now satisfy us that abnormal conditions are of no infrequent occurrence,—that the tides at times flow or ebb for four, five, six or even seven and eight hours*,—that frequent instances occur of retrograde movements for short periods of time, as if the tide had met with a check and been turned back; and at the turning-points there are numerous instances of the atmosphere being stationary for a couple of hours.

The maximum pressure of the atmosphere is in the coldest months, December or January, but the minimum pressure is not in the hottest months, but in June or July. The barometric readings, when protracted †, show a gradual curve from December or January descending to June or July, and then ascending again to December or January, there being an occasional interruption in October or November; and as the curves at Madras, Bombay and Calcutta correspond, and as Madras has no S.W. monsoon, while Bombay has a S.W. monsoon, and as Bombay is destitute of the N.E. monsoon of Madras, it would appear that the general movements of the mass of the atmosphere are little influenced by any conditions of its lower strata; but the curve of pressure would seem to have some relation to the sun's place in the ecliptic.

The normal conditions of daily temperature are, that it is coldest in India at sunrise, and hottest between the hours of 1 and 3 P.M.; but the preceding tables show many aberrations from this rule. The regular increment or decrement of mean monthly heat from the maximum or minimum period is somewhat remarkable, as the curve is independent of the S.W. monsoon at Bombay and the N.E. monsoon at Ma-

* One instance at Aden of nine hours!

† To that very able and zealous meteorologist, Dr. BUIST, LL.D. of Bombay, I am indebted for the protracted curves of pressure of the barometer appended to this paper. Plates XVII. XVIII. XIX. XX.

dras ; and the passage of the sun twice over both places does not derange the curve. The anomalies of the annual mean temperatures of Madras, Bombay, Calcutta and Aden, not diminishing with the increase in the latitude of the respective places, are pointed out, and numerous instances are given of the very great power of the slanting rays of the sun beyond the tropic. As is the case with the barometric, so do the heat tables indicate that the annual and daily ranges of the thermometer diminish with the elevation of the place of observation above the sea-level, the elevated tableland of the Deccan however being an exception to this rule. At Mahabuleshwur, at 4500 feet, the temperature of *the air* was never below 45° with a maximum and minimum thermometer; and at Dodabetta the temperature of *the air* was never below $38^{\circ}5$; nevertheless at both places ice and hoar-frost were frequently found on the ground at sunrise, resulting from the separate or conjoined effects of radiation and evaporation. I have already stated that I do not attach much value to the readings of the wet bulb, owing to the various sources of error in the instrument itself, and to the manifest sources of error in the existing theoretical formulæ for giving a numerical value to its readings to fix the tension of vapour in the atmosphere for the determination of the dew-point. No doubt the dew-points obtained by means of the wet bulb have a certain relation to the truth; and in some favourably concurring conditions may be as proximate to the truth as dew-points would be, obtained by direct means; but the elaborate experiments of REGNAULT show that in a calm in the open air or in a chamber, the wet bulb surrounds itself with its own humid atmosphere, and with a breeze blowing upon it the temperature of the wet bulb falls or alternates as the wind blows more or less rapidly upon it. Moreover, Professor ORLEBAR points out a source of error to which no doubt all the observations in India were more or less subject, namely, the proximity of the dry bulb to the wet bulb, and the cold from the latter in consequence depressing the temperature of the dry bulb, two, three, or more degrees below the temperature of the air, necessarily producing fallacious results. Making allowance for the defects of DANIELL's hygrometer, the dew-points obtained by its means are infinitely more worthy of confidence than those obtained by means of the wet bulb. On the whole, I would not venture to say more with respect to normal conditions of moisture in India, than that the air of the sea-coast has always a much greater fraction of saturation than the lands of the interior, and that the elevated plateau of the Deccan is periodically subject to very great degrees of dryness.

The rain-tables are so extensive that it will only be necessary to point out some unexpected phenomena connected with the distribution of rain. It is found both on the sea-coasts and on the table-lands of the Deccan, that within very limited areas, the differences in the fall of rain may be very great. With nine rain-gauges employed in the small island of Bombay in the months of June and July, in the monsoon of 1849, the quantity collected in the different gauges ranged in July from 46 inches to 102 inches, and in June from 19 inches to 46 inches. At Sattarah, with three rain-

gauges within the distance of a mile, they differed in their contents several inches from each other ; and at Mahabuleshwur and Paunchgunny, nearly on the same level, the latter place being only eleven miles to the eastward of the former, the difference in the annual fall of rain was respectively 254 inches and 50 inches ! The normal conditions are, that there is a much greater fall of rain on the sea-coasts than on the table-lands of the Deccan, but that the Ghâts intervening between the coasts and the table-lands have three times the amount of the fall on the coasts, and from ten to fifteen times the amount of the fall on the table-lands of the interior ; the paucity of the fall of rain at Cape Comorin and in the mouths of the Indus would also appear to be normal conditions.

The Tables must be referred to for the winds ; the normal states are those of the S.W. and N.E. monsoons, and the influence of the latter is periodically felt at the height of 8640 feet, which height would appear just at the upper surface of the stratum of air constituting the S.W. monsoon ; but hourly observations for lengthened periods of time are necessary from Dodabetta, to determine what really are the periodical winds at that height. From the points other than those between S. and W., and N. and E., there is also at the several stations a certain amount of periodicity in the winds ; the winds that are common to different stations having only a slant more or less at the different stations ; for instance, the S.W. and N.W. winds of Bombay blowing in the summer months in Calcutta incline rather to be S. and N. winds, than S.W. and N.W. winds ; but to be enabled to speak with any precision upon this branch of the meteorology of India, and indeed upon most other branches with a comprehensive and philosophical object, hourly observations are necessary,—simultaneously taken with previously compared instruments by zealous observers ; and having the records in a form common to all the observers, so as to admit of rigid comparisons :—when this is done, not only in India but in Europe, meteorologists will be in a better condition to generalize and propound normal conditions, than the state of our knowledge at present would justify, for it must be borne in mind that “Error latet in generalibus.”

The Plates referred to in the preceding paper are numbered XVII. XVIII. XIX. XX.

XVI. *On the PELOROSAURUS ; an undescribed gigantic terrestrial reptile whose remains are associated with those of the Iguanodon and other Saurians in the Strata of Tilgate Forest, in Sussex.*

By GIDEON ALGERNON MANTELL, *Esq.*, LL.D., F.R.S., F.L.S.,
Vice-President of the Geological Society, &c.

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I HAD for a long while entertained the idea that among the fossil remains collected from the Wealden deposits of the South-East of England, there were indications of an enormous Lizard entirely distinct from the Iguanodon, Megalosaurus, Cetiosaurus, and other genera which have been named and more or less accurately determined ; and I have at length obtained such evidence in support of my opinion, as induces me to submit to the Royal Society the data which appear to establish the existence of a terrestrial reptile contemporary with the Iguanodon, and which equalled, if not surpassed in magnitude, that colossal herbivorous Saurian.

I shall not on the present occasion enter upon those minute anatomical details which are indispensable for the solution of many of the difficult problems which but too often perplex and bewilder the palæontologist, but content myself with faithful descriptions and figures of such facts as will suffice to establish my proposition ; in the hope that these guesses at truth, which recent investigations have suggested to my mind, may serve to direct future labourers in the right path of inquiry, and tend to enlarge our knowledge of that remarkable fauna which prevailed in the islands and continents of the Cretaceous, Wealden, and Oolitic ages.

The occurrence of very large isolated vertebræ, and portions of femora with medullary cavities, indicating animals of terrestrial habits, and of great size, and which, though assigned to the Cetiosaurus, could not properly be included in a genus of aquatic marine reptiles with solid bones like the Cetaceans, first suggested to me the probability of there having existed another genus of Saurians contemporary with those previously mentioned, and to which some of the supposed Cetiosaurian remains might belong. This idea, though vague, seemed to offer an explanation of certain discrepancies between some of my statements and those of other cultivators of this branch of comparative anatomy.

The stupendous humerus or arm-bone of a terrestrial reptile from the strata of Tilgate Forest, in Sussex, which I have now the honour to place before the Royal Society, will, I believe, establish the correctness of that opinion.

This splendid fossil was obtained from the locality in which was situated the

quarry represented in the frontispiece of my work on the Geology of the South-East of England, and that yielded to my early researches the teeth of the *Iguanodon* and numerous other highly interesting remains. The bone was imbedded in the fawn-coloured sandstone that prevails in the Wealden of that part of Sussex, at the depth of 25 feet beneath the surface of the soil. The distal part of the bone, to the extent of 2 feet, was discovered in 1847, by Mr. PETER FULLER of Lewes, and some months afterwards the middle portion was found at a higher level; a line of fault having traversed the rock and imbedded bone, and occasioned the subsidence of the portion previously met with. At length other fragments were discovered and extricated from the rock, and the whole replaced and cemented together in the admirable state in which the fossil now appears, by the intelligent collector in whose possession I had, a few weeks since, first the gratification of seeing this unique and stupendous relic from the Weald of my native county*.

This bone is the right humerus, and bears a closer resemblance in its general character to the analogous element in the Crocodile, than to that of the Lacertians. It is $4\frac{1}{2}$ feet long, and 32 inches in circumference at the distal extremity. It is in the ordinary state of mineralization of the bones from the Wealden sandstones, being of a rich umber colour, and very heavy from an impregnation of oxide of iron. The surface presents a smooth appearance, but upon close inspection is found to be finely striated: it evidently belonged to an animal arrived at maturity, but not aged†.

The inferior or distal end, and nearly three-fourths of the shaft, are perfect; but a considerable part is wanting on each side the proximal extremity. Fortunately the base of the salient deltoid process, so characteristic of the humerus in the Crocodiles, is well-defined (Plate XXI. fig. 1^b c), and affords grounds for supposing that the upper part, which is deficient, did not materially differ from the recent type.

The transverse fracture through the middle of the shaft (Plate XXI. fig. 1^a f) has been left disunited, to show the large medullary canal which is filled up with concretioned sand; as is usually the case in the long bones of the *Iguanodon* from the same locality.

The thickness of the wall of the shaft at this section (Plate XXI. fig. 1^c) is 1 inch; the transverse diameter of the medullary cavity 3 inches. Mr. FULLER informs me that the canal extended to within one-third of the head of the bone, as in the femur of the *Iguanodon*.

The fossil, in its general form, is straighter than the humerus of the Crocodile, and the depressions between the condyles, both anteriorly and posteriorly, are relatively shallower. The surface for articulation with the fore-arm (Plate XXI. fig. 1^b) is

* I cannot refrain from expressing my warmest thanks to Mr. FULLER, for the gratifying compliment paid me (though a personal stranger) as the original explorer of the Geology of Sussex, in allowing me to possess this interesting fossil, although he had previously refused several liberal offers.

† Thin sections of the bone exhibit under the microscope the intimate structure beautifully preserved; the bone-cells, and Haversian canals, are as distinct as in recent bones.

smoother and more uniform, but as the epiphyses are wanting, and both extremities of the bone somewhat abraded by attrition, I am led to conclude that in the recent state, the radio-ulnar articulation must have closely resembled the crocodilian type.

In the posterior aspect (Plate XXI. fig. 1^b), the double bend or curve so strikingly conspicuous in the shaft of the crocodilian arm-bone, is wanting; the deltoid ridge (Plate XXI. fig. 1^b c) commences much lower down, and above the fractured end of this process (fig. 1^b c) the bone expands into the wide flattened head, for articulation with the glenoid socket, formed by the union of the coracoid and scapula. Although the general character of the fossil corresponds in so many respects with that of the Crocodile, yet there are such marked discrepancies, that I have not ventured to introduce an outline indicative of the probable form of the parts that are deficient; for there is ample space for the commission of important errors in such a substitution, which might form a stumbling-block to future observers.

To facilitate comparison I have subjoined figures of the right humerus of the recent Gavial, and of the Iguanodon, and Hylæosaurus, reduced to an uniform scale: the arm-bone of the Gavial belongs to a skeleton 18 feet long, in the museum of that eminent physiologist, my friend Dr. ROBERT GRANT.

It will be seen at a glance that the enormous bone under consideration differs essentially from the humeri of all the Saurians with whose remains it was found associated. The large medullary cavity at once separates it from the bones referred to the Polyptychodon and Cetiosaurus; for in these animals the long bones are composed of "coarse cancellous tissue without any trace of medullary cavity*."

These data appear to me sufficient to warrant the establishment of a new genus for the colossal air-breathing reptile to which this remarkable humerus belonged, and I propose the name of *Pelorosaurus*† to indicate the enormous magnitude of the original.

I now pass to the consideration of other parts of the skeleton, or, to express myself more correctly, of certain detached and isolated bones found in the same quarry with the gigantic humerus, and which, for reasons to be stated in the sequel, may with greater probability be assigned to the *Pelorosaurus*, than to any other of the colossal reptiles obtained from the Wealden deposits.

Anterior Caudal Vertebrae of the Pelorosaurus, Plate XXII. and Plate XXIV. and XXV.

The vertebrae which I would assign to the *Pelorosaurus* with but little hesitation, are four anterior caudals of a very remarkable character, which I found many years since in the same stratum as the humerus above described, and at the distance of but a few yards. They were firmly imbedded and lying in various positions in a

* Reports on British Fossil Reptiles, 1841, p. 102. I have a series of bones from Brook in the Isle of Wight, through the kindness of my distinguished friend Sir R. I. MURCHISON, proving the existence of Cetiosaurs in the Wealden; all the long bones are destitute of a medullary cavity.

† Πέλωρ *pelor*, monster, unusually gigantic.

large block of sandstone, and I succeeded, after much labour, in clearing them from the stone with their processes nearly entire; in the progress of my task a chevron bone of the crocodilian type, 10 inches long (Plate XXII. fig. 8), was laid bare and extricated.

When these splendid fossils were first discovered, I referred them to the Iguanodon; subsequently they were named by Professor OWEN *Cetiosaurus brevis**; and lastly, Dr. MELVILLE and myself, in my Memoir on the Iguanodon†, suggested the necessity of adopting a different specific appellation, and proposed that of "*Conybeari*;" we were unwilling to remove them from the genus *Cetiosaurus*, till corroborative evidence was obtained to justify the change.

The description of these vertebræ in detail will be found in the British Association Reports for 1841 (p. 97), and by Dr. MELVILLE in the Philosophical Transactions, 1849 (p. 296); but without figures no adequate idea can be given of the originals‡. I have therefore annexed delineations on a reduced scale, $\frac{1}{4}$ linear, Plate XXII., and two views of the largest vertebra $\frac{5}{8}$ ths the natural size, Plate XXIV. and XXV. I have been induced to add the two last drawings in order that the subject may be fully comprehended.

These vertebræ are distinguished by the subquadrangular form of the articulating facets of the centrum or body, and the relative shortness of the antero-posterior diameter (Plate XXII. fig. 7). The largest is $7\frac{1}{2}$ inches in the transverse, and $6\frac{3}{4}$ inches in the vertical diameter of the anterior face, and but 6 inches in the posterior: the length or antero-posterior dimension is but $3\frac{1}{2}$ inches. The height to the top of the spinous process is 13 inches. The diameter of the neural canal, for the spinal marrow, is 2 inches. The other three bones are somewhat smaller; the most distal being only 6 inches transversely.

These vertebræ are slightly concave in front, and almost flat behind; the upper part of the anterior face being the deepest, as shown in Plate XXII. fig. 5^a. The sides of the body are concave, both lengthwise and vertically, with a transverse median convexity, as seen in Plate XXII. fig. 5^a.

The inferior surface of the centrum (Plate XXII. fig. 6) is slightly concave in its antero-posterior diameter, and divided by a longitudinal sulcus into two ridges, whose terminations obscurely indicate the position of the hæmapophysial articulations. It is noticeable, that in vertebræ of such magnitude, well-defined articulating spaces for the chevron bone are not present.

The neural arch presents the most peculiar characters; it is large, and anchylosed to the anterior half of the upper surface of the centrum; the posterior part of which is left free, as shown in fig. 5^c and 7, Plate XXII. The anterior oblique processes

* These vertebræ and the chevron bone, are placed in the same case as the bones of the Iguanodon, in the Gallery of Organic Remains of the British Museum.

† Philosophical Transactions, 1849, p. 297.

‡ Sir J. G. DALYELL very properly remarks, that "delineation should be the inseparable accompaniment of description in natural history."

(Plate XXII. fig. 5^a, *g*, *g*) project directly forwards, and advance over the exposed part of the body of the contiguous vertebra (Plate XXII. fig. 7); as there are no posterior oblique processes, the anterior are received in depressions on each side the spinous process (see Plate XXII. fig. 5 and 7 *h*, *h*). The transverse processes (*f*, *f*) are very strong and short, and project at nearly right angles from the body; the spinous process (*e*) is short and thick.

In Plate XXII. fig. 7, the four vertebræ are represented in a consecutive line, for the purpose of explaining the mode of articulation above described; but it is doubtful whether the two posterior bones are in their natural position; it seems probable that there was an intermediate vertebra between the second and third, and between the third and fourth, so that two more would be required to complete the series. The hæmapophysis or chevron bone, Plate XXII. fig. 8, is obviously too small for articulation with either of the above vertebræ; it is however important, as showing the crocodilian modification of this caudal element of the gigantic original.

Median caudal vertebræ, Plate XXIII. fig. 1 and 3, and Plate XXVI.—From the same quarry I obtained two vertebræ belonging in all probability to the middle part of the tail; and which, though scarcely large enough to appertain to the same individual as the above anterior caudal, present such characters as might be expected in the more distal part of the same region. The centrum of the largest specimen (Plate XXIII. fig. 10 and Plate XXVI.) is $7\frac{1}{4}$ inches long; the transverse diameter of the anterior face is 5 inches, the vertical $4\frac{1}{4}$ inches; height to the top of the spine $5\frac{1}{4}$ inches. Both the facets of the centrum are slightly concave, and are most deeply excavated in the upper part, as in the large anterior caudals. The neural arch is ankylosed to the anterior half of the body, the posterior part being uncovered. The inferior surface (Plate XXIII. fig. 10^c *c*) is concave antero-posteriorly, and two slightly elevated ridges terminate behind in distinct hæmapophysial surfaces (*k*, *k*), which are $1\frac{3}{4}$ inch apart, and are evidently fitted for articulation with a chevron bone of the type already described (Plate XXII. fig. 8).

Distal caudal vertebra, Plate XXIII. fig. 11.—In referring the unique caudal here figured to the same category as the preceding, I offer the suggestion as only probable. The centrum is of a subcylindrical form, $4\frac{1}{2}$ inches long, and slightly concave on both facets. The most remarkable feature in this bone is the ankylosis or rather confluence of the heads of the chevron bone with the body (Plate XXIII. fig. 11 *j*, *j*), a character common in fishes, but which, I believe, is unknown in reptiles, save in one genus, the fossil animal of Mæstricht, the *Mosasaurus*, whose occurrence in the English chalk was first ascertained in 1820, by my discovery of two concavo-convex caudals with confluent chevron bones*. The remarks of the illustrious CUVIER on the caudal vertebræ of the *Mosasaurus*, are in every respect applicable to the specimen under consideration. After mentioning the median caudals as having “à leur face inférieure deux petites facettes pour porter l’os en chevron,” he describes the more

* Geology of the South Downs, Plates XXXIII. and XLI.

distal series which form a great part of the tail; in these "*l'os en chevron n'y est plus articulé, mais sondé, et fait corps avec elles**."

The structure of the spinal column of the *Mosasaurus* therefore proves that vertebræ having the chevron bone articulated by two distinct facets (as Plate XXII. fig. 8), may be followed, in a more distal part of the caudal region, by a series with the hæmapophysis anchylosed to the centrum (as in Plate XXIII. fig. 11).

Femora and Tibiæ.—From the Wealden strata of Tilgate Forest, Hastings, and the Isle of Wight, I have seen fragments of the distal extremity of femora with medullary cavities, which, though too imperfect to admit of accurate determination, were obviously those of a gigantic terrestrial reptile, distinct from the *Iguanodon* and *Megalosaurus*.

From Sandown Bay, in the Isle of Wight, I have the proximal end of a tibia of enormous size, the circumference of the head of the bone being 34 inches, a magnitude surpassing that required for a tibia to articulate with the largest known femur, and presenting such deviations in form from the tibiæ of the *Iguanodon*, as to render it highly probable that this bone belonged to the *Pelorosaurus*.

Indications of the Pelorosaurus in the Oolitic strata.—The general accordance of the terrestrial fauna and flora of the Oolitic period, (as proved by the remains of land animals and plants imbedded in the fluvio-marine deposits of that formation,) with those of the Cretaceous and Wealden, renders it probable that vestiges of most, if not all, of the genera and species of land reptiles that occur in the latter will be found in the former strata. Thus as the *Iguanodon*, *Pterodactyles*, with *Clathrariæ* and *Dracænæ*, are found in the Chalk, and the *Megalosaurus*, with *Cycadææ* and *Coniferæ*, in the Wealden, traces of the *Pelorosaurus* may be expected to occur in the Oolite. To ascertain this fact, I availed myself of the liberal permission of my friend the DEAN OF WESTMINSTER to examine his splendid collection, and I repaired to Oxford and diligently inspected the numerous specimens of Saurian remains which it contains, especially those from the Wealden and Oolite.

To avoid prolixity I will but remark, that among immense quantities of huge vertebræ and bones of the extremities of unequivocally marine Saurians, as proved by the cancellated structure of their centres, and which had been properly referred to *Pliosaurus*, *Cetiosaurus*, &c., there are portions of femora, vertebræ, and tarsal phalangeal and ungual bones, which appear to be distinct from those of any established genus. I refer especially to several caudal vertebræ resembling that from Tilgate Forest (Plate XXIII. figs. 9 and 10), which are probably of the *Pelorosaurus*, and large curved claw-bones, from 4 to 6 inches long. These were found at Chipping Norton, associated with vertebræ, &c. of *Cetiosaurus*, and were accordingly assumed to belong to the same genus of reptiles.

But the specimens which come more immediately within the scope of this inquiry are portions of femora from Enstone, which appear to differ from those of the *Mega-*

* *Ossements Fossiles*, tome v. p. 327. Edit. 1824.

losaurus and Iguanodon. They belong to a huge terrestrial reptile, in which the *patellar* space is smooth, and not traversed by a deep furrow as in the femur of the latter. The structure of these fossils led me to examine with great care the enormous femur obtained by Mr. STRICKLAND from the Bradford clay at Enslow Bridge, on the Charwell, eight miles from Oxford. This specimen is now affixed to the wall in Dr. BUCKLAND's museum, at a considerable height from the floor, and therefore cannot be examined with facility. Unfortunately too the bone was found in so shattered a condition that it was necessary to cement its anterior face to a board. The posterior surface pressed flat, the condyles and popliteal space, and the outline of the sides of the shaft, are therefore the only characters now displayed; the proximal extremity or head is wanting. This bone exactly corresponds in length and width with the humerus of the *Pelorosaurus*. The condyloid extremity is the only portion in a normal state. The condyles (so far as I could ascertain from an elevated and inconvenient position on a ladder) are more equal, and wider apart, than in the Iguanodon and Megalosaurus; but the general appearance of the bone is so similar to that of the femur of the Iguanodon when shattered and pressed flat, that until I ascertained there were no indications of a median trochanter on the mesial border of the shaft, I could not convince myself it did not belong to that reptile. There were no visible traces of a medullary cavity, yet it seemed improbable that the shaft of this enormous and strong bone could have admitted of the degree of compression it had sustained, (for the entire thickness did not appear to exceed 3 or 4 inches,) if it was solid as in the *Cetiosauri*: Dr. BUCKLAND entirely concurred in this opinion.

To ascertain this important point I wrote to Mr. STRICKLAND, who very obligingly favoured me with an immediate reply. In answer to my inquiries, Mr. STRICKLAND stated, that upon comparing the femur with that of the Megalosaurus, it was evident that it belonged to a different genus: and he had labelled it "*Cetiosaurus*," from its resemblance to portions of femora and other bones found in the same locality, and so named in the Oxford Museum; and likewise, because in the crushed mass of bony fibre which filled the interior, he did not perceive any traces of a medullary cavity; but it is quite possible, Mr. STRICKLAND adds, "that such a cavity may have existed, though now so much obliterated by compression as to have escaped my observation."

The character of the condyloid extremity, and the general form of this bone, appear to me to separate it from the femur of the marine reptiles, to which it has been referred, provisionally, by this distinguished naturalist; if upon a more accurate examination a medullary cavity should be detected, there will be strong grounds for assigning this gigantic thigh-bone to the *Pelorosaurus*; on the contrary, if the shaft should prove to be solid throughout, the supposed relation of this femur to the humerus previously described, will of course be negatived.

Summary.—From the facts described the following inferences result:—

1st. Upon the evidence of the humerus alone, the existence during the Wealden

æra, of a stupendous terrestrial Saurian, generically distinct from any previously described; this reptile I propose to name *Pelorosaurus Conybeari*.

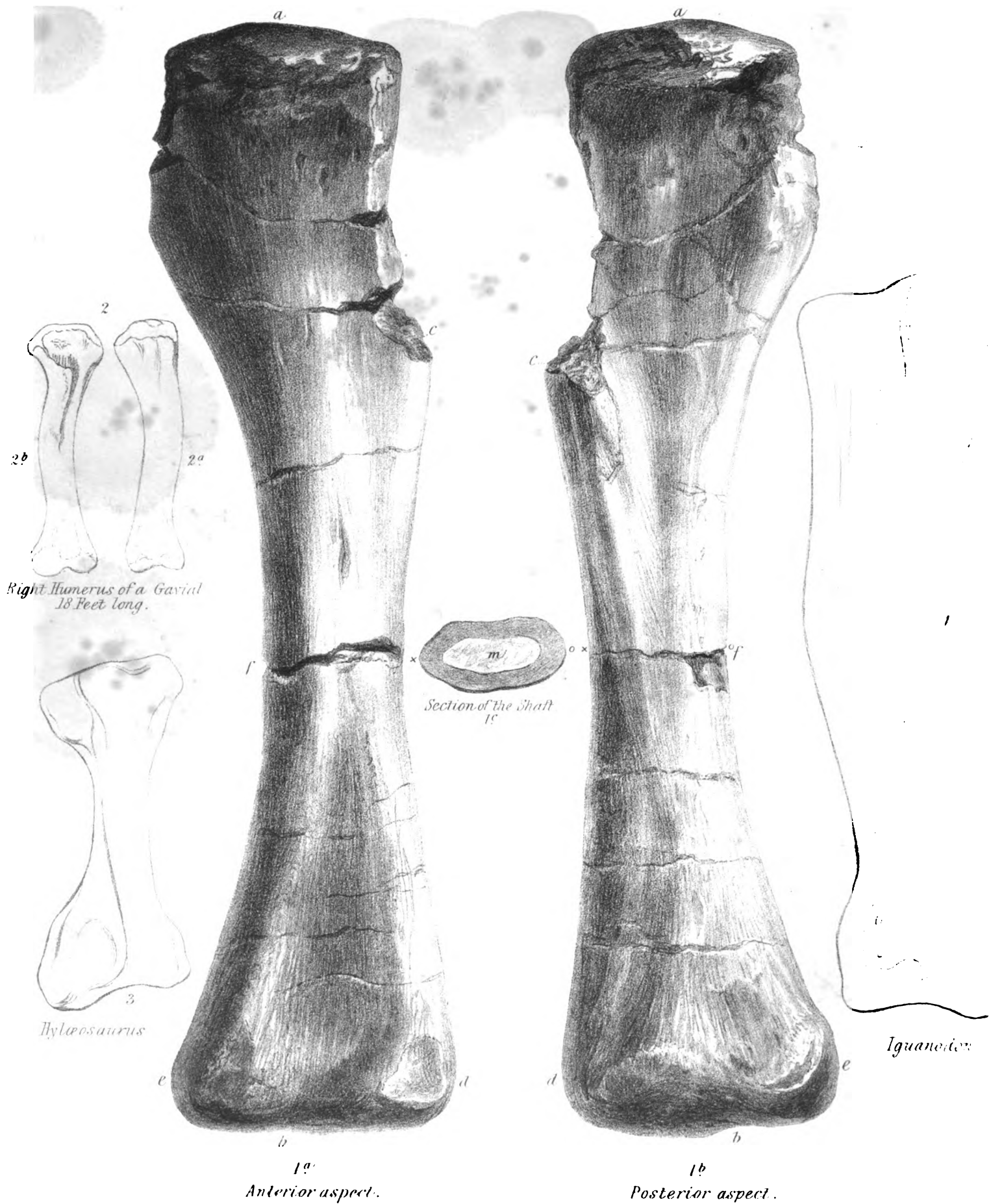
2ndly. The great probability that the four large anterior caudal vertebræ with the chevron bone, termed *Cetiosaurus brevis* by Professor OWEN, and the two median caudals found in the same stratum as the humerus, and at no great distance from it, belong to the same species; and

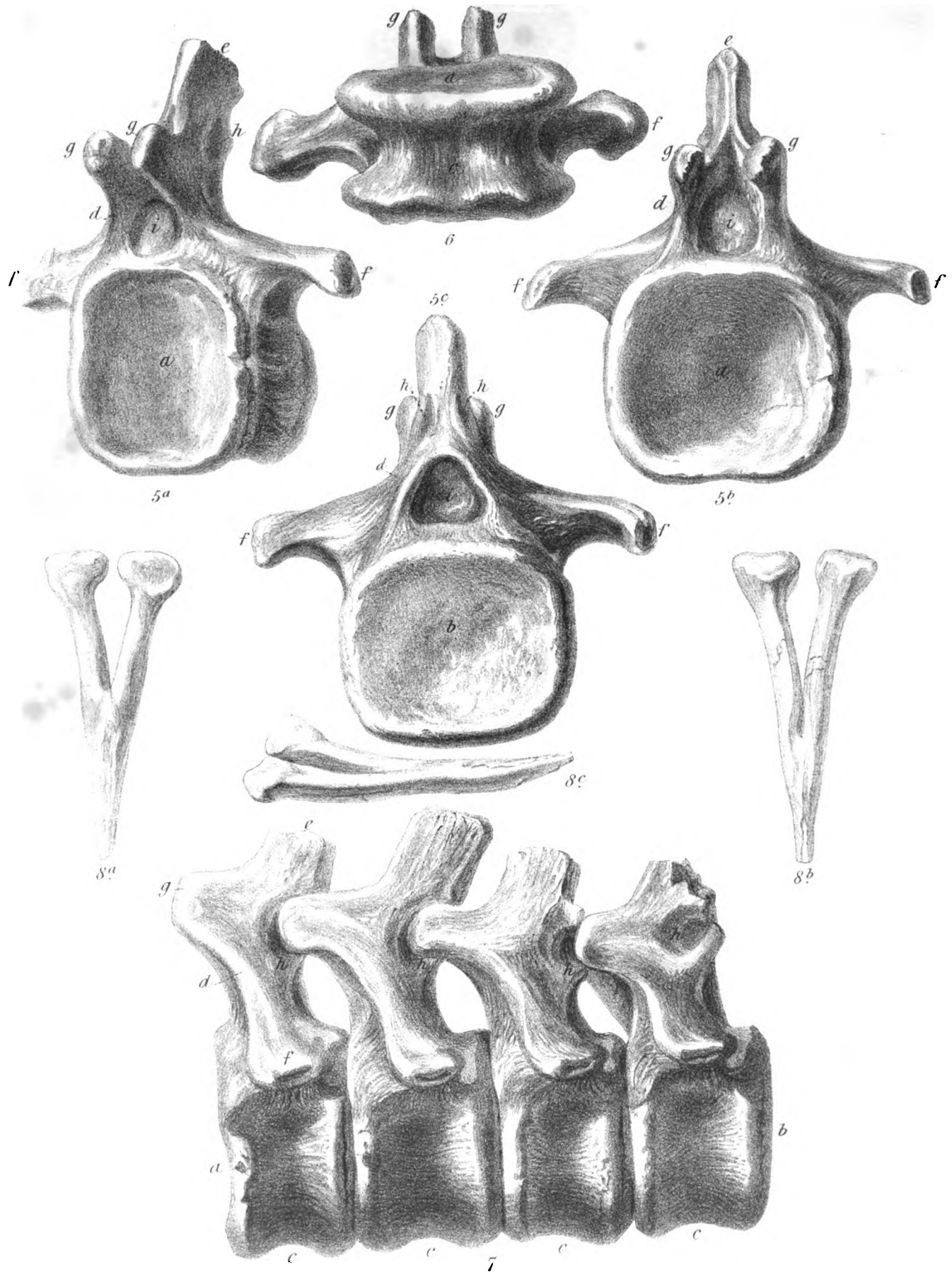
3rdly. That certain large bones of Saurians from the oolitic deposits of Oxfordshire, at Enstone and at Enslow Bridge, and hitherto considered as Cetiosaurian, may appertain to the Pelorosaurus.

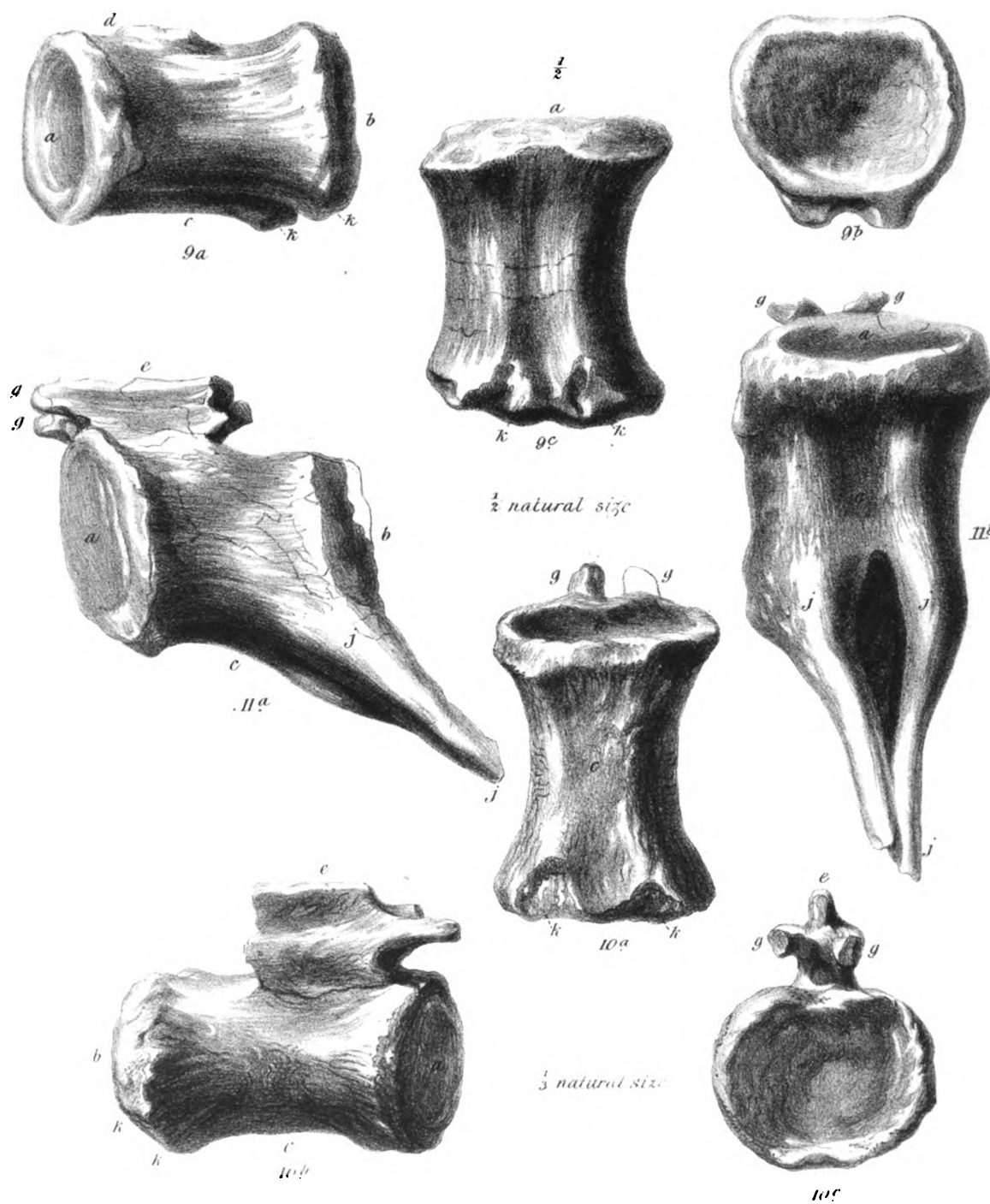
It may perhaps be expected that some estimate should be given of the probable magnitude of the reptile to which the humerus belonged; but calculations of the length and proportions of the original animal taken from a single bone, or from a few detached bones, can afford but vague and unsatisfactory results. With the view, however, of conveying some idea of the almost incredible bulk of the *Pelorosaurus*, it may be stated, that in the Gavial or Gangetic Crocodile, the length of the humerus is equal to one-eighteenth of the entire length of the animal from the snout to the end of the tail; thus in Dr. GRANT'S specimen the humerus is 1 foot long; the entire skeleton 18 feet. Computed by this standard the length of the Pelorosaurus would be 81 feet, and the girth of its body about 20 feet. But if we assume the length and number of the vertebræ as the scale, we should have a reptile of relatively very abbreviated proportions; but in either case, a Saurian far exceeding in size all living types, and equalling if not surpassing in magnitude the most colossal of the extinct forms.

From what has been advanced, we perceive that every addition to the zoology of the countries that flourished during the secondary geological ages, affords proof of the high development of the terrestrial reptiles, which appear to have enjoyed the same predominance in those ancient faunas, as the large Mammalia in those of the tertiary and human epochs. The trees and plants associated with the remains of the extinct Saurians, manifest by their affinity to existing forms, that the countries in which they grew possessed as pure an atmosphere, as high a temperature, and as unclouded skies, as those of our tropical climes. There are, therefore, no legitimate grounds to support the hypothesis that during the "Age of Reptiles"—the period when the reptilian class most prevailed—the earth was "in the state of a half-finished planet," and its atmosphere too heavy from an excess of carbon, for the respiration of warm-blooded animals! Such an opinion can only have originated from an imperfect view of the phenomena which these problems embrace. There is as great a discrepancy between certain existing faunas and those of modern Europe, as that presented by the Wealden: for example, those of Australia, Tasmania, New Zealand, and the Galapagos Islands.

By a singular coincidence, on the same day that I obtained the humerus of the *Pelorosaurus* from Tilgate Forest, I received from my eldest son in New Zealand, the most interesting collection of the remains of the extinct gigantic birds of those







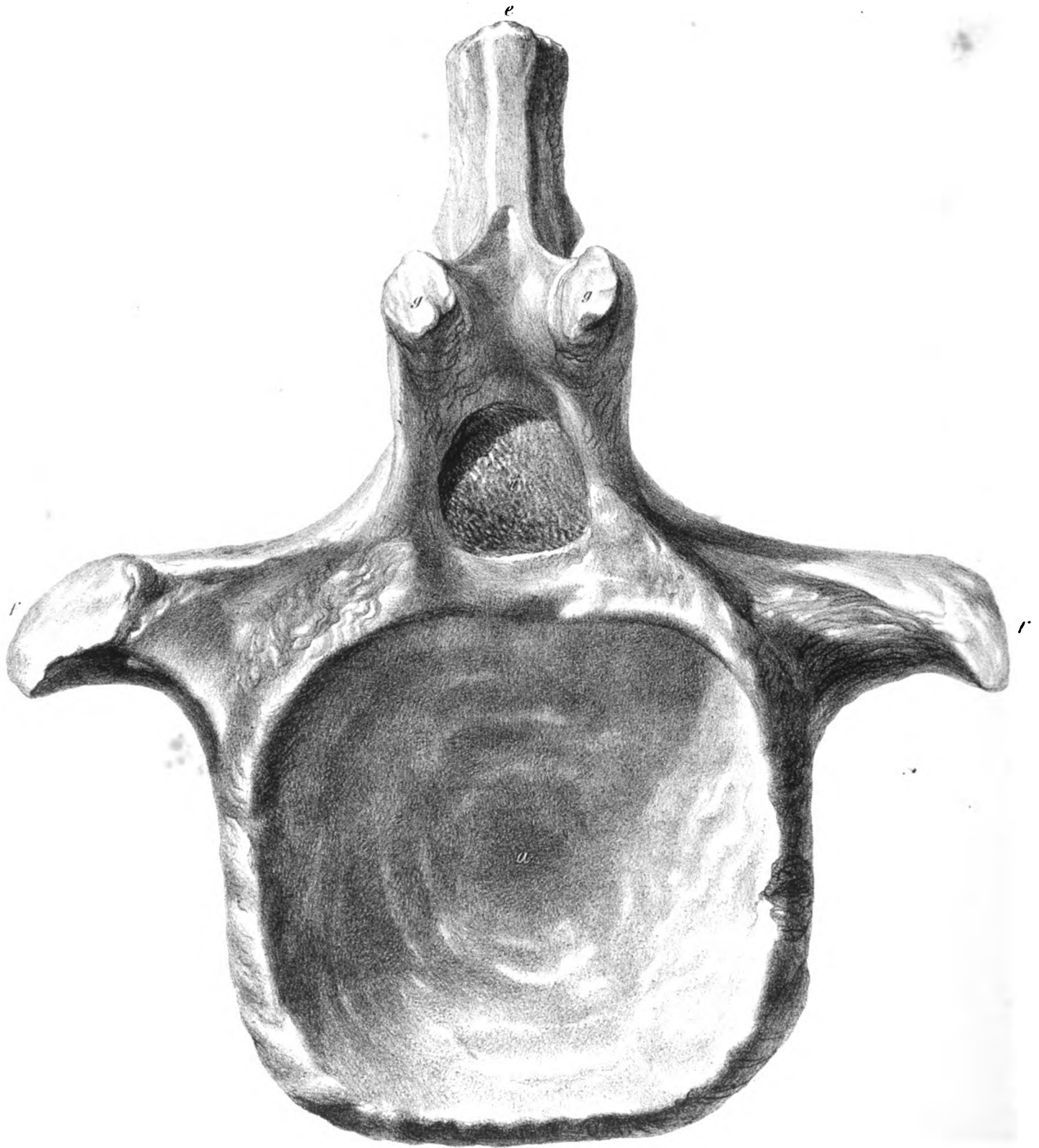
Saurian caudal Vertebrae, from the strata of Tilgate Forest.



Caudal Vertebra of Peloresaurus Conybearei.

($\frac{2}{3}$ Natural Size.)

Engraved by J. Smith.



*Anterior view of a caudal vertebra of Pelorosaurus Conybearci.
($\frac{1}{2}$ Natural Size)*



Median caudal Vertebra of *Pterosaurius* (2)
from the strata of *Trilgate Forest*, by Capⁿ Lambert Brückenden.

(Natural Size.)

Printed by J. Gougeon.

1 inch = 10

islands that has reached this country: and I could but think, that had the respective localities and periods of these birds and reptiles,—both of a size far surpassing all other known types of their respective classes,—been interchanged, and the bones of the *Dinornis* of New Zealand referred to the Wealden epoch, what speculations would in all probability be hazarded to account for physical conditions assumed to be required by so marvellous a development in numbers and magnitude of the class Aves, to the almost entire exclusion of the Mammalia!

In attempting to explain the natural records of the ancient physical history of the globe and its inhabitants, by our acquaintance with the physiology, anatomy, and economy, of existing organic beings, we cannot be too often reminded of the caution of the sagacious QUETELET, that “our knowledge and our judgments are in general only founded on probabilities more or less great, which it is very important, but very difficult, to estimate at their just value.”

In conclusion I would beg to remark, that in adding another genus of terrestrial reptiles to those previously established as belonging to the Fauna of the Wealden, I am fully aware of the imperfect manner in which, from various unavoidable causes—especially the pressure of professional duties—my investigations have been carried out. But encouraged in my earliest researches by the illustrious founder of Palæontology, Baron CUVIER, and honoured by the highest award of the Geological Society, I felt reluctant to discontinue researches which no other naturalist seemed disposed to undertake, lest some important additions to our knowledge of the ancient physical condition of the earth and its inhabitants should be unrecorded and forgotten.

Chester Square, Pimlico,
November 1849.

DESCRIPTION OF THE PLATES.

PLATE XXI.

. The figures of this Plate are all on the same scale, which is $\frac{1}{8}$ linear the natural size.

Fig. 1. The right humerus of the *Pelorosaurus Conybeari*, obtained from the strata of Tilgate Forest in Sussex, by Mr. PETER FULLER of Lewes, and now in the possession of the author.

1^a. Anterior view.

1^b. Posterior view.

1^c. Transverse section of the shaft; *m*, the medullary canal filled with fawn-coloured sandstone.

The dimensions of this bone are as follow :—

Length 54 inches.

Circumference of the distal end 32 inches.

Transverse diameter of the same 13 inches.

Circumference of the shaft at the transverse section 17 inches.

———— the fractured end of the deltoid ridge 23 inches.

a. The head or proximal extremity.

b. Distal or radio-ulnar articulation.

c. Deltoid ridge.

d. Inner condyle.

e. Outer condyle.

f. Transverse fracture exposing the section seen in fig. 1^c.

Fig. 2. Outline of the right humerus of a Gavial 18 feet long.

2^a. Anterior view.

2^b. Posterior view.

Fig. 3. Humerus of the Hylæosaurus.

Fig. 4. Humerus of the Iguanodon ; from Philosophical Transactions, Part II. 1849, Plate XXXI. fig. 19*.

PLATE XXII.

* * All the specimens here delineated are in the British Museum : the figures are reduced $\frac{1}{4}$ linear the natural size.

Anterior caudal vertebræ and chevron bone of the *Pelorosaurus*?

Fig. 5. The largest and most anterior bone of the series.

5^a. Perspective view showing the slightly concave anterior face of the body of the vertebra (*a*), the excavated form of the sides, the anterior oblique processes (*g, g,*) and the spinous process (*e*), with the pits or depressions for the reception of the heads of the oblique processes of the contiguous posterior vertebra (*h*).

5^b. Front view of the same fossil.

5^c. Posterior view, to show the remarkable character occasioned by the absence of oblique processes. The several parts in this and the vertebræ figured in the subsequent plates are thus indicated :—

a. Anterior articulating surface of the body or centrum.

b. Posterior face.

c. Inferior or visceral aspect of the body.

d. Neural arch, or neurapophysis.

* 'There is a typographical error both in the text and lithograph of this bone in the Philosophical Transactions, *loc. cit.*, as to the scale of the figure ; it is marked reduced to $\frac{1}{4}$, it should be $\frac{1}{2}$ linear the natural size.

- e.* Spinous process.
- f.* Transverse process.
- g.* Anterior oblique process.
- h.* Depression on the spinous process, occupying the usual situation of the origin of the posterior oblique.
- i.* Spinal canal.
- j.* Chevron bone or hæmapophysis.
- k.* Hæmapophysial surfaces for articulation with the chevron bone.

Fig. 6. A vertebra seen on the inferior aspect.

Fig. 7. Four anterior caudal vertebræ placed in a consecutive series.

Fig. 8. A chevron bone 10 inches long, found imbedded in the same block of sandstone as the vertebræ.

8^a. Outer aspect.

8^b. Inner aspect.

8^c. Lateral view.

PLATE XXIII.

. The figures are $\frac{1}{2}$ linear the natural size.

Median and distal caudal vertebræ of the Pelorosaurus ?

Fig. 9. A median or distal caudal vertebra with two eminences on the posterior end of the inferior aspect of the body, to articulate with a double-headed chevron of the crocodilian type.

9^a. Lateral view.

9^b. Posterior face.

9^c. View of the inferior surface.

k, k. The articulating facets to unite with the chevron bone.

Fig. 10. A larger vertebra of a similar type, obtained from the same locality in Tilgate Forest as the other specimens figured in this memoir, by my friend Capt. LAMBART BRICKENDEN, F.G.S.

10^a. Inferior surface.

10^b. Lateral view.

10^c. Anterior view, showing the neural arch.

Fig. 11. A caudal vertebra with the chevron bone anchylosed to the body, as in the distal caudals of the Mosasaurus.

11^a. Lateral view.

11^b. View of the inferior surface.

PLATE XXIV.

Perspective view of the largest anterior caudal, $\frac{5}{8}$ th the natural size.

PLATE XXV.

Anterior view of the same.

PLATE XXVI.

Perspective view of a median caudal vertebra of the Pelorosaurus, of the natural size; found in the same quarry and stratum as the specimens figured in the preceding Plates; by Capt. LAMBART BRICKENDEN.

XVII. *On a Dorsal dermal Spine of the Hylæosaurus, recently discovered in the Strata of Tilgate Forest.*

By GIDEON ALGERNON MANTELL, *Esq., LL.D., F.R.S., F.L.S.,*
Vice-President of the Geological Society, &c.

Received June 13,—Read June 13, 1850.

IN the highly interesting and unique specimen of part of the skeleton of the Hylæosaurus discovered by me in 1832, and now preserved in the Gallery of Organic Remains of the British Museum, the most striking peculiarity is a series of large thin angular processes extending nearly parallel with the left side of the vertebral column. These bones vary in length from 4 to 17 inches, and in form from a somewhat obtuse to an isosceles triangle: there are six or seven in a connected line, and several others detached and dispersed in the block. They terminate distally in a blunt apex, and are expanded at the base in the antero-posterior diameter, but unfortunately in every instance the proximal end is so imperfect that their mode of attachment to the other parts of the skeleton is not clearly demonstrable.

From the general resemblance of these processes to the dermal bones imbedded in the surrounding stone, I was induced to consider them as the remains of a dorsal crest formed by a series of erect dermal plates or spines, which extended along the back of the Hylæosaurus, in the same manner as the cartilaginous scaly dorsal fringe in the Iguana; an opinion which appeared to be corroborated by some detached specimens of a similar character that were subsequently discovered*.

Professor OWEN, however, whilst admitting the probability of that suggestion, considered it more likely that the bones in question were abdominal ribs, remarking that “the want of symmetry, and the difference in size and form, in the four succeeding spine-shaped plates, agreed better with the costal than with the dermal hypothesis†.” Mr. BRODERIP and other naturalists have regarded this opinion of Professor OWEN as conclusive‡.

For reasons fully stated in my former memoirs on the Wealden Reptiles, it appeared to me highly improbable that these spines could have belonged to the costal system. In 1848 I had an opportunity, for the first time, of making sections of a specimen for microscopical examination, and I then found the internal structure to

* A comparison of these spines with the dermal crest of the *Cyclura*, is given in my original memoir on the Hylæosaurus; *Geology of the South-East of England*, 1832.

† *British Association Report on Fossil Reptiles*, by Professor OWEN, 1841, p. 116.

‡ *Zoological Researches*, Chapter on Reptiles.

be identical with that of the dermal scutes of the Hylæosaurus, presenting "long, straight, spicular fibres, decussating each other in all directions, and representing, as it seems, the ossified ligamentous fibres of the original corium*."

At length, after the lapse of eighteen years, I have obtained one of these spines in which the proximal end or base is sufficiently entire to show the nature of its connection with the body of the reptile. This specimen (for which I am indebted to the liberality of Mr. PETER FULLER of Lewes) was found a few weeks since in the same quarry in Tilgate Forest, whence the first discovered skeleton of the Hylæosaurus was obtained; and there is reason to conclude that it may have belonged to the same individual, for several detached dermal scutes and spines of corresponding size and character, have from time to time been found in the sandstone near the spot.

This spine, if perfect, would be 15 inches in length; its base or proximal end is of an elliptical form, with a longitudinal median depression, which is bordered by a gentle rounded eminence; the articulating surface has the corrugated aspect that characterizes the ossified dermal scutes of the Hylæosaurus; and a comparison of the base of this spine with that of the unquestionable dermal bones, confirms the correctness of my original interpretation of the nature of these remarkable processes.

In the accompanying drawings the characters of the original are sufficiently defined, to render further description unnecessary.

This fact is of considerable interest, since it demonstrates in the dermal system of the extinct colossal Saurians of the secondary geological æras, a similar exaggerated development to that which prevails in the living diminutive representatives; but while in the existing Lizards the dermal appendages are flexible and cartilaginous, in the fossil reptiles they are rigid osseous spines; the difference arising from the ossification of the ligamentous fibres of the corium in the dermal bones and spines of the Hylæosaurus.

DESCRIPTION OF THE PLATE.

PLATE XXVII.

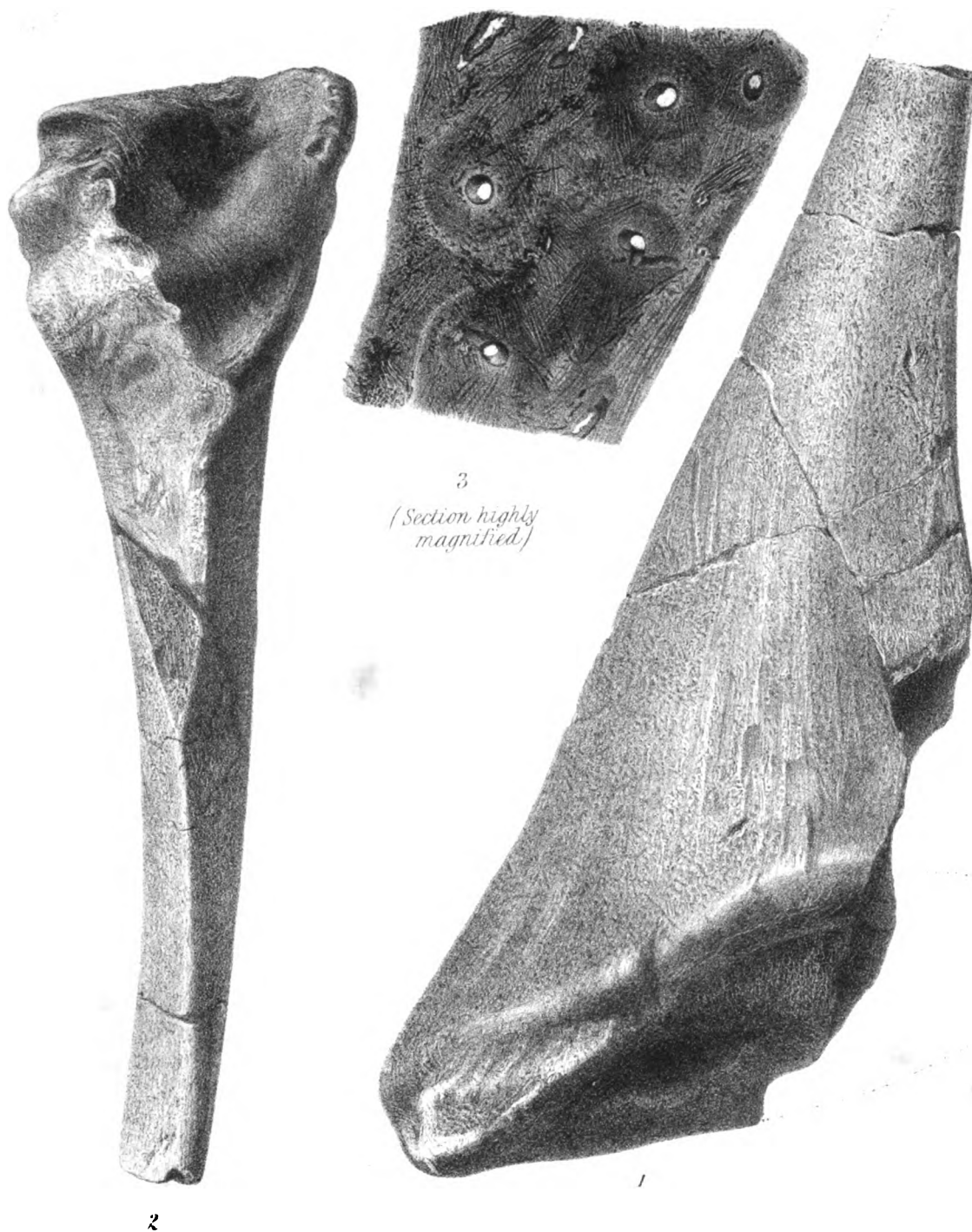
Fig. 1. Lateral view of the dorsal dermal spine of the Hylæosaurus; reduced to one-half linear.

Fig. 2. View of the articulating surface of the base.

Fig. 3. Magnified view of a portion of the internal structure, showing the decussating ossified fibres of the corium, the bone-cells, and the Haversian canals, as exposed in a section seen by transmitted light.

Chester Square, Pimlico,
June 1850.

* See Wonders of Geology, 6th edition, p. 437.



Dorsal dermal Spine of the Hylaeosaurus

($\frac{1}{2}$ Nat. Size. linear)

XVIII. *Supplementary Observations on the Structure of the BELEMNITE and BELEMNOTEUTHIS.*

By GIDEON ALGERNON MANTELL, *Esq., LL.D., F.R.S., F.L.S.,*
Vice-President of the Geological Society, &c.

Received November 5, 1849,—Read February 14, 1850.

AS several eminent naturalists have expressed doubts of the correctness of my interpretation of some of the facts described in the Memoir on the Belemnite and Belemnoteuthis, published in the Philosophical Transactions, Part II. 1848, I am induced to lay before the Royal Society the following additional observations in confirmation of the opinions advanced in my previous communication on this subject.

That distinguished naturalist, Mr. J. E. GRAY, has especially controverted my statement that the phragmocone of the Belemnites of the Oxford Clay* possessed a pair of elongated shelly processes, which extended beyond the peristome or upper border of the conical chambered shell; the aperture resembling in this respect that of certain species of Ammonites†. In the recently published "*Catalogue of the Mollusca in the Collection of the British Museum*‡," Mr. GRAY remarks, "Dr. MANTELL has figured a specimen which appears to have an elongated process on each side, like the processes on the sides of the mouth of certain Ammonites; but on examining his specimen I am very doubtful if this appearance does not arise from an accidental fracture of the upper part of the conical shell."

Since the publication of my former Memoir, three Belemnites with phragmocones, in which the parts in question are unequivocally manifest, have come under my observation. Two of these interesting fossils are in the Gallery of Organic Remains in the British Museum: the third is in my possession. In addition to these examples, evidence in proof that this structure is normal, and not the result of accident, has been afforded by numerous detached portions of belemnital phragmocones from the Oxford Clay and Lias, which display well-defined vestiges of the expanded bases of these processes: and that eminent palæontologist, Mr. JOHN MORRIS of Kensington, informs me that he has recognized their presence in specimens from various strata and localities.

In the three examples above mentioned, that part of the process which extends

* The species of Belemnite to which my observations refer, Mr. MORRIS informs me is *Belemnites Puzosianus* of D'ORBIGNY, *B. Owenii* of Mr. PRATT.

† Philosophical Transactions, 1848, Plate XIII. fig. 3. p. 181.

‡ Part 1, *Cephalopoda Antepedia*, p. 124.

beyond the upper or basilar margin of the phragmocone is well defined; and in each of these specimens the extension of the longitudinal plates or bands may be distinctly traced downwards, almost to the distal end or apex, as a thin nacreous shelly plate, striated longitudinally, and having obliquely divergent lines on the ventral margin.

The finest specimen from Wiltshire in the British Museum (Plate XXVIII. fig. 1), like that discovered by my son*, consists of the osselet or guard (*b*) partially invested by its capsule, with the phragmocone in natural apposition: the latter is shattered and pressed almost flat, but preserves a conical outline, and has on the upper part the *right process* (*e*), which extends 5 inches above the peristome or border of the chambered cone: a portion of the left process is seen at *f*; *c*, denotes the lower termination of the process.

Fig. 2 represents the upper portion of this fossil of the natural size, and so clearly shows the parts described that further detail is unnecessary.

I would particularly direct attention to the evident extension of the base of the process (*x, x, x*) down the phragmocone as a nacreous band or plate finely striated. Under a slightly magnifying power this expansion appears like a thin shelly integument deposited on the external surface of the chambered cone, and is marked with curved diverging lines on the lateral border.

In the former Memoir the number and position of these processes could not be determined with precision; the specimens recently obtained show that there were but two,—one on each side the aperture or peristome,—and these were situated nearer to the dorsal than to the ventral aspect. The siphunculus always occupies the median and ventral side of the phragmocone; and its excentric apex is directed towards the same region, in which is also situated the sulcus of the guard (see Plate XXIX. fig. 5).

In the fragment of a large uncompressed belemnital phragmocone from Lyme Regis, for the loan of which I am indebted to Mr. MORRIS, the relative position of the parts above described is distinctly exhibited, Plate XXIX.: fig. 3, *f, f*, the remains of the lateral processes; *g*, the siphunculus, occupying the ventral aspect; *h*, the dorsal line.

The outline, Plate XXIX. fig. 4, is intended to express concisely the facts described.

With regard to the osselet (the distal solid part of which is generally termed the *guard* or *rostrum*), I would remark, that since my former communication I have inspected many hundred specimens from various localities, and have ascertained that although in detached examples (the ordinary condition in which these fossils occur) the rostrum appears to terminate by a well-defined line at the upper part, yet this is not really the case; for the same radiated structure originally extended upwards, and surrounded and protected the phragmocone as a sheath, and gradually became confluent with the investing capsule or periostricum; probably terminating in a horny flexible integument. A fragment of the receptacle taken from the upper part of a specimen, and which is not thicker than stout paper, is delineated under a mag-

* Philosophical Transactions, 1848, Plate XV. fig. 3.

nifying power of eight diameters in Plate XXIX. fig. 6. In this section are clearly seen the outer integument or periostricum (*a*), the radiated structure of the osselet (*b*), and the shell of the phragmocone (*c*). Among the numerous Belemnites I have examined, *not the slightest trace of an ink-bag or its contents, the sepia*, could be detected.

BELEMNOTEUTHIS.—Some uncompressed examples of the distal end of the phragmocones of this Cephalopod have lately been discovered, which must dispel any remaining doubts as to the generic distinction, first established by the late Mr. CHANNING PEARCE in 1842, being based on natural characters.

The specimen figured, Plate XXIX. fig. 7, appears to me to afford conclusive evidence on the points in dispute. By a reference to my former paper, it will be seen that the longitudinal ridges, which are always present on the distal end of the phragmocone of the Belemnoteuthis, are regarded by those observers who contend that the latter belong to Belemnites, as plaits, or folds, originating from fracture and lateral compression. In the fossil before us these ridges are entire and well-defined; two are situated on the ventral and one on the dorsal region. Fig. 7^a is a transverse section, enlarged four diameters, in which are shown the form of the ridges and the *radiated structure of the solid part of the phragmocone*: I scarcely need observe, that in the true Belemnite the cone is chambered to the extreme apex. These characters will remind the palæontologist of the very analogous organization of the *Beloptera* of the tertiary strata, alluded to in my former memoir*. In fact it is now proved that the Belemnoteuthis (as was originally suggested by Mr. CHANNING PEARCE and Mr. CUNNINGTON) possessed an osselet of a radiated structure containing a chambered siphunculated cone, but without an extended rostrum or guard; thus forming an interesting transition to the Belemnite, from which it is generically distinct†. From the facts before us, our knowledge of the organization of the genus *Belemnites* comprises the following structures:—

1. The investing periostricum or *capsule*, which enveloped the osselet, and extending upwards, constituted the external parietes of the receptacle.

2. The *Osselet*, characterized by its radiated structure, composed of trihedral prismatic fibres, which terminated distally in a solid rostrum having an alveolus or conical hollow to receive the lower portion of the chambered phragmocone, and proximally in a thin cup-like expansion, which became confluent with the capsule, and formed the receptacle for the viscera.

3. The *Phragmocone*, or conical siphunculated chambered internal shell, the apex of which occupied the alveolus of the rostrum, and the upper part expanded into a capacious chamber, from the basilar margin of which proceeded two elongated testaceous processes.

* Philosophical Transactions, 1848, p. 176.

† The *Conoteuthis Dupinianus* of M. D'ORBIGNY, from the Upper Neocomian strata of the Department d'Aube, appears, so far as can be determined by mere description, to approach very closely to the Belemnoteuthis. It is stated to have a long slender osselet, terminated by a chambered cone, but without a guard or rostrum.

Some of the specimens in the British Museum and in my own cabinet exhibit on the space around and between the processes, delicate striæ, apparently produced by the imprint of the muscular fibres of the mantle or other tissues; and these I believe are the only indications hitherto observed of the soft parts of the animal to which the Belemnite belonged.

In fine, the *Belemnite* is characterized by its rostrum and the investing capsule, and its phragmocone chambered to the extreme apex and having a pair of testaceous processes at the basilar margin of the peristome*; while the *Belemnoteuthis* has simply an osselet of a radiated fibrous structure, inclosing a conical chambered shell that terminates distally in a solid obtuse point†. Whether the Belemnite resembled the Belemnoteuthis in possessing an ink-bag, and having a body with eight uncinated arms and a pair of long tentacula, future discoveries can alone determine.

Although this communication may be devoid of interest to those who do not especially cultivate the department of natural science to which this inquiry belongs, it will not I trust be deemed unimportant by the palæontologist, since the new facts herein described tend to remove in some degree the obscurity which veils the original structure of a race of highly organized beings, that swarmed in the seas of every part of the globe‡ during the secondary geological periods, from the Lias to the Chalk inclusive, and which appears to have become extinct with the contemporary Cephalopoda, the Ammonites, at the close of the cretaceous epoch§.

Chester Square, Oct. 1849.

Additional Note.

February 14, 1850.

Since the previous observations were communicated to the Secretary of the Royal Society, I have been so fortunate as to obtain from the Oxford Clay of Wiltshire, a Belemnite in which the two dorsal processes of the phragmocone are more perfectly displayed than in any specimen hitherto discovered (see Plate XXX.).

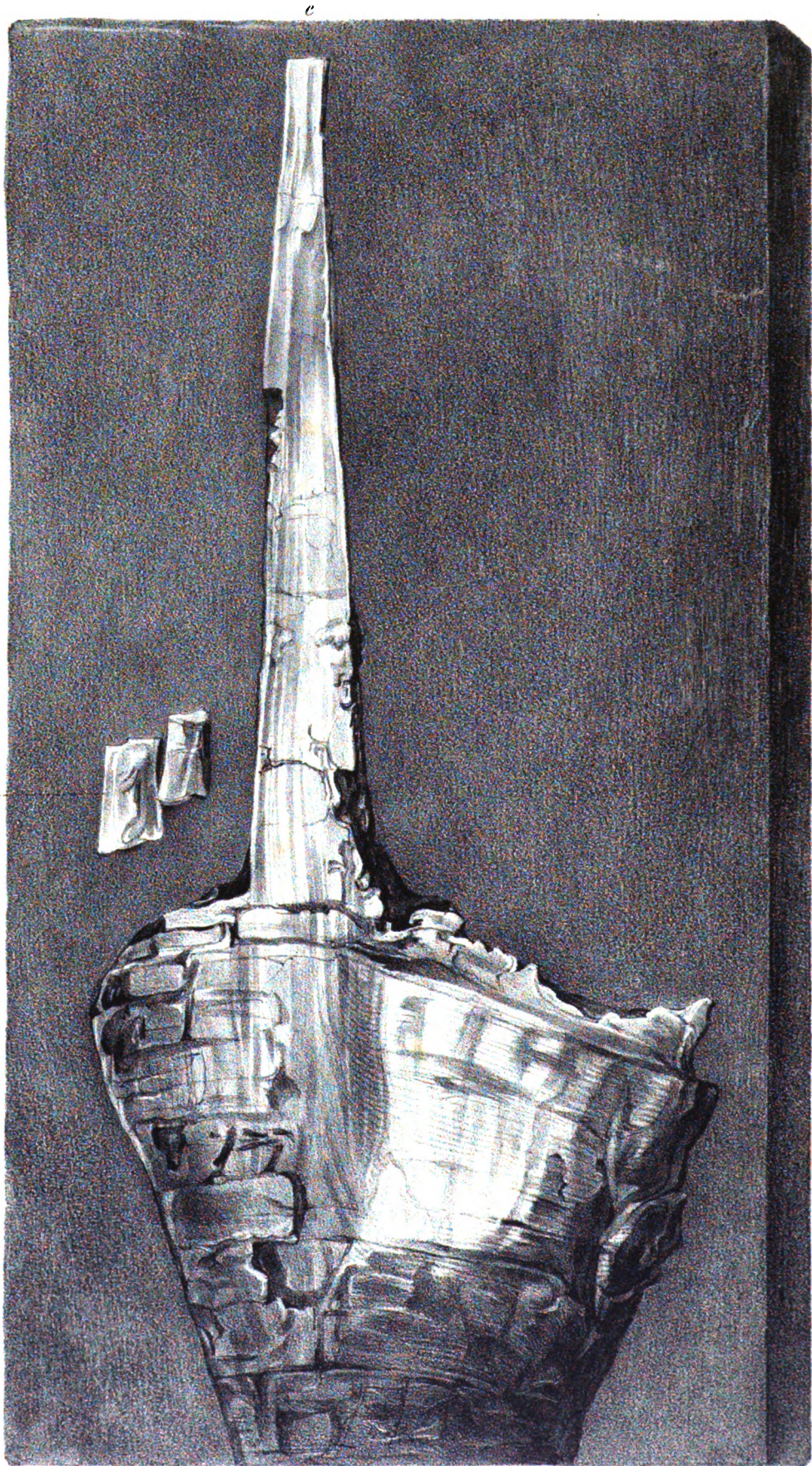
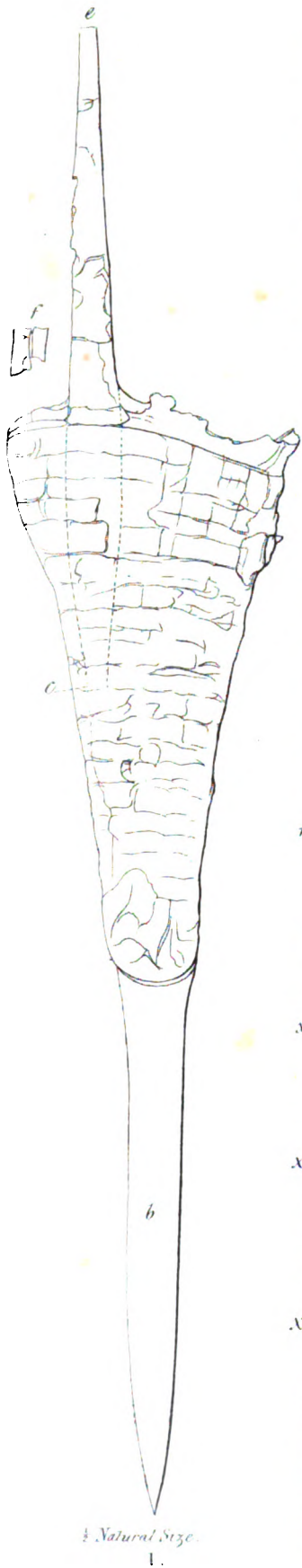
From the rarity of such fossils, and their extreme fragility and perishable nature,

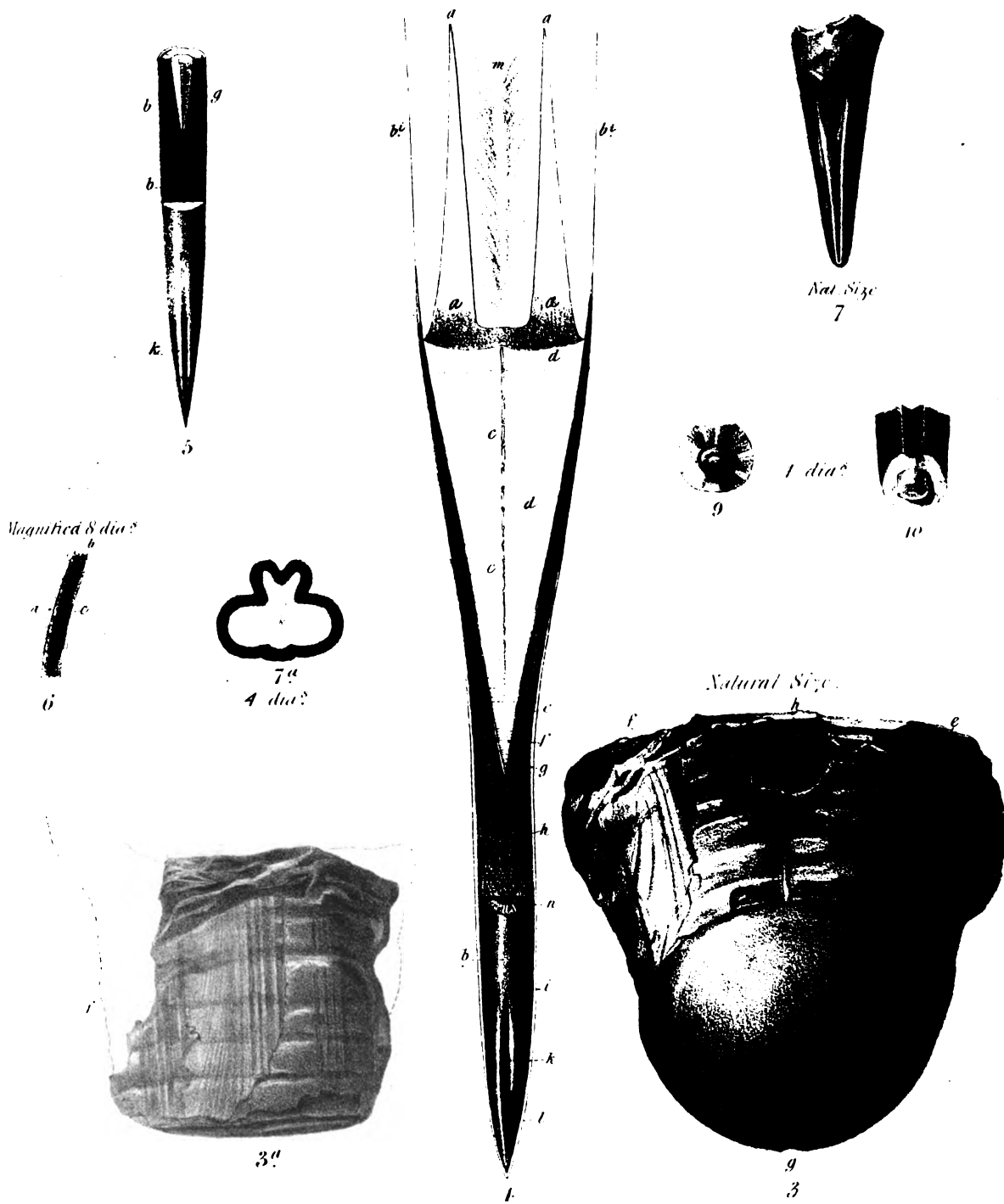
* I do not mean to aver that similar processes exist in *every species* of Belemnite, for it is probable that, as in Ammonites, there may have been considerable diversity in the form and size of these appendages; and in some species the basilar margin of the peristome may have been destitute of them: my remarks exclusively refer to the species of Belemnites described in the text.

† The facts described in the text are of course directly opposed to the views expressed in the following extract from the Philosophical Transactions for 1844, p. 74: "The association of the spathose guards with crushed phragmocones *identical in structure* with those in connection with the fossil ink-bags and muscular parts."

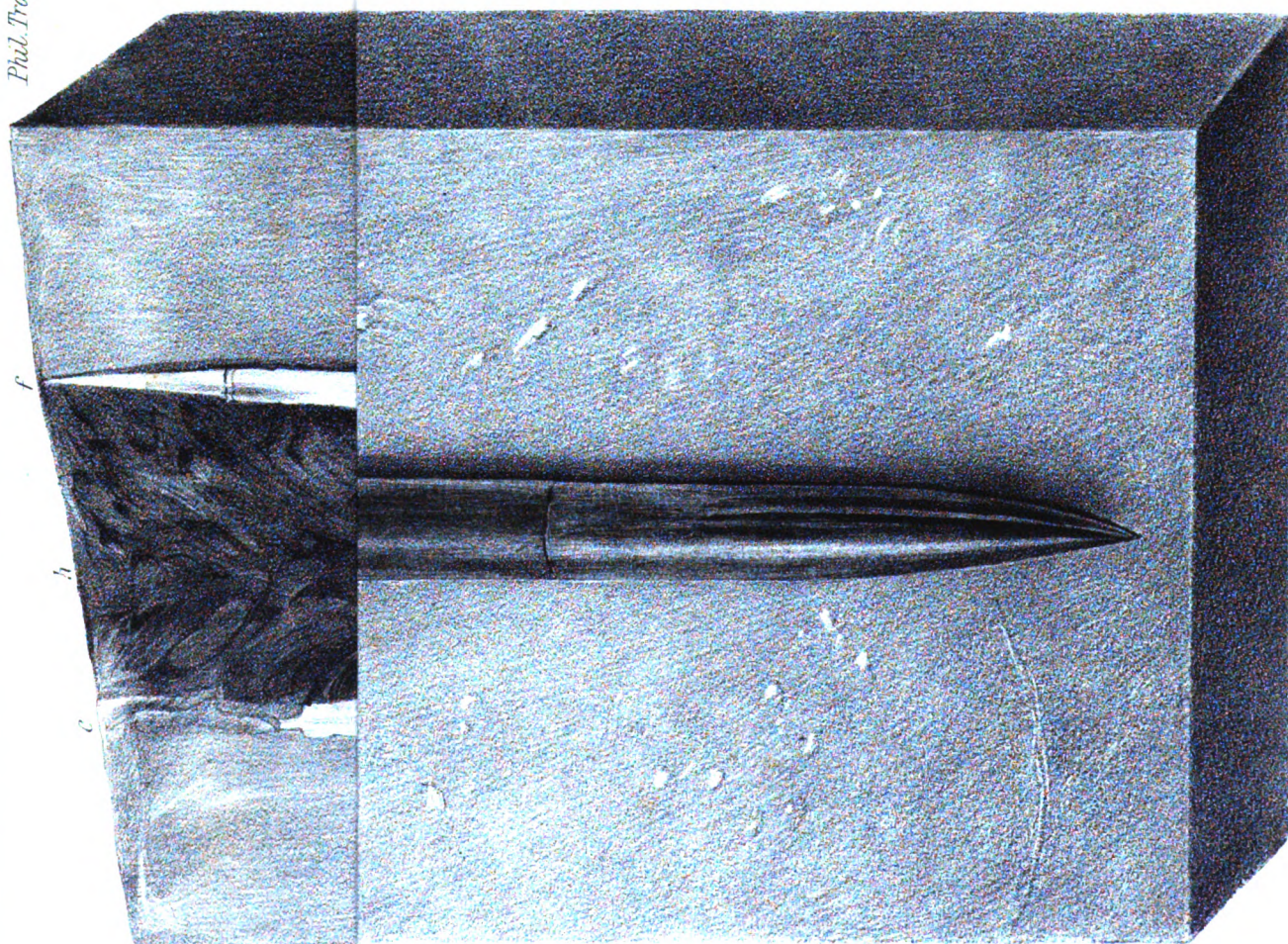
‡ Belemnites have recently been discovered in limestone in the Middle Island of New Zealand, by my eldest son, Mr. WALTER MANTELL.—See Geological Journal for August, 1850.

§ Belemnites first appear in the Lias, where they suddenly attain their maximum development. Many species abound in the Oolitic or Jurassic formation: in the *Greensand* they are likewise abundant; in the *Galt* there are but two or three small species. In the Chalk strata a modification of the type, termed *Belemnitella*, appears, and with these the race seems to have become extinct; at least no traces of its existence have been detected in any newer deposits, save the *Beloptera* of the tertiary previously mentioned. I am not aware that any examples of Belemnoteuthis have been found in the Lias: hitherto I have obtained specimens only from the Oxford Clay and contiguous strata.





Belemnites and Belemnoluthis.



Belemnites lutescens, showing the upper part of the phragmocone with the pair of elongated processes; from the Oxford Clay, Wiltshire.

(In the possession of Mr. Mantell.)

it is important to preserve faithful representations taken whilst the specimens are fresh from the stratum, for when the clay contracts by drying, the delicate shelly structure of the phragmocone but too frequently shrivels and flakes off; I know of no means by which this decomposition can be prevented, and am therefore desirous of adding to the illustrations of this communication the accompanying drawing, by Mr. MOUNSEY, of the beautiful fossil above mentioned, in which are shown the elongated processes of the phragmocone in their natural position on each side the dorsal line; the interval between them is occupied by a thin pellicle of a dark integument marked with very fine diverging parallel striæ; this substance is probably the inner lining of the capsule of the sepiostaire in a carbonized state, a condition in which animal tissues so often occur in argillaceous deposits.

I likewise annex a drawing of another specimen (Plate XXIX. figs. 9, 10) of the distal termination of the osselet of the *Belemnoteuthis*, in which the alveolus or hollow occupied by the chambered shelly cone is exposed; the cavity is filled with calcareous spar, and is surrounded by a dense fibrous radiated structure, analogous to that of the osselet of the true Belemnite; an additional proof that in the *Belemnoteuthis* this investment is the osselet or guard of the phragmocone. After this evidence, the presumed generic identity of the Belemnite and *Belemnoteuthis* must, I conceive, be abandoned by every accurate observer; consequently the form and structure of the body and arms, and other soft parts of the Cephalopoda to which the Belemnites belonged, have yet to be discovered.

DESCRIPTION OF THE PLATES.

PLATE XXVIII.

Fig. 1. Outline of a remarkably fine specimen of a Belemnite with the phragmocone and its elongated processes, from the Oxford Clay, Wilts. In the British Museum. The length of the original is 22 inches.

Fig. 2. Represents the basilar or upper portion of the above fossil of the natural size.

e. The right, and *f.* the left process.

x, x, x. The base of the process spread over the conical shell of the phragmocone.

PLATE XXIX.

Fig. 3. Part of the phragmocone of a Belemnite from the Lias, in the collection of JOHN MORRIS, Esq.

3^a. Lateral view of a portion of the same, showing the remains of one of the longitudinal processes on the shell of the phragmocone.

Fig. 4. Outline exhibiting all the known parts of the Belemnite in their relative position, the osselet being split asunder longitudinally, and one side removed to show the situation of the alveolus, &c.

Fig. 5. The rostrum of the osselet of *Belemnites Puzosianus* from the Oxford Clay, Trowbridge, Wilts. In this specimen the upper part of the guard is removed and exposes the distal extremity of the phragmocone lying in its natural position in the alveolus. The ventral aspect is delineated to show the form and situation of the sulcus or groove (the characteristic of this species) in that region: the siphunculus is on the front of the phragmocone in this view.

The several parts represented in the above figures are indicated as follow:—

- a, a.* The pair of elongated processes of the phragmocone.
- b.* Osselet, rostrum, or guard.
- b'*. The upward extension of the expanded osselet.
- c, c.* Siphuncle.
- d, d.* The phragmocone.
- e.* Section of the capsule or outer investment of the osselet.
- f.* The apical or distal termination of the phragmocone.
- g.* The alveolus or hollow in the rostrum in which the end of the phragmocone is situated.
- h.* Vertical section of the guard, showing its fibrous and radiated structure.
- i.* Ventral aspect.
- k.* Ventral sulcus or groove.
- l.* Section of the capsule investing the guard.

Fig. 6. Transverse section of a fragment from the upper part of the receptacle of the Belemnite near the base of the process; magnified eight diameters.

In this thin pellicle are seen, *a.* the Capsule; *b.* the radiated fibrous structure of the osselet distinctly visible, though taken several inches from above the line which is usually regarded as the termination of the spathose osselet; *c.* the shell of the phragmocone.

Fig. 7. The apical or distal end of the phragmocone of the Belemnoteuthis, showing the ventral aspect, and the pair of ridges: natural size.

7^a. A transverse section of the same, displaying the internal radiated structure of the phragmocone and its solid apex: magnified four diameters.

** The two ventral ridges. This specimen is in my collection.

Figs. 9, 10. The apical portion of the osselet of the Belemnoteuthis, exposing the alveolus, or cavity occupied by the apex of the chambered cone, filled with spar, and surrounded by the dense, fibrous, radiated investment, analogous to that of the guard of the true Belemnite: the figures are magnified four diameters.

PLATE XXX.

A Belemnite from the Oxford Clay of Wiltshire, showing the pair of dorsal processes in their natural position: size of the original.

(3.) Furthermore, if we observe that

$$1 - (1 + \Delta)^{p+q} = \{1 - (1 + \Delta)^p\} + (1 + \Delta)^p \{1 - (1 + \Delta)^q\},$$

we shall have, by applying each of these operative symbols to $F(\Delta)f(0)$,

$$\{1 - (1 + \Delta)^{p+q}\} F(\Delta)f(0) = \{1 - (1 + \Delta)^p\} F(\Delta)f(0) + \{1 - (1 + \Delta)^q\} F(\Delta)f(p+0); \quad (7.)$$

and therefore

$$\{1 - (1 + \Delta)^p\} F(\Delta)f(q+0) - \{1 - (1 + \Delta)^q\} F(\Delta)f(p+0) = \{(1 + \Delta)^p - (1 + \Delta)^q\} F(\Delta)f(0); \quad (8.)$$

(4.) In the particular case where $F(\Delta) = \frac{1}{\Delta}$, these become

$$\frac{1 - (1 + \Delta)^{p+q}}{\Delta} f(0) = \frac{1 - (1 + \Delta)^p}{\Delta} f(0) + \frac{1 - (1 + \Delta)^q}{\Delta} f(p+0); \quad \quad (9.)$$

and

$$\frac{1 - (1 + \Delta)^p}{\Delta} f(q+0) - \frac{1 - (1 + \Delta)^q}{\Delta} f(p+0) = \frac{(1 - \Delta)^p - (1 + \Delta)^q}{\Delta} f(0). \quad . . . \quad (10.)$$

(5.) Designating by $S(x^n)$ the sum of the n th powers of the natural numbers from 1 to x inclusive, or putting

$$S(x^n) = 1^n + 2^n + \dots + x^n,$$

it is demonstrated (Examples, § 6. Ex. 23.) that

$$S(x^n) = (-1)^n \cdot \frac{1 - (1 + \Delta)^{-x}}{\Delta} 0^n = (-1)^n \cdot \left\{ \frac{x}{1} \cdot 0^n - \frac{x(x+1)}{1 \cdot 2} \Delta 0^n + \frac{x(x+1)(x+2)}{1 \cdot 2 \cdot 3} \Delta^2 0^n - \&c. \right\}; \quad . . \quad (11.)$$

and again, in § 8, Exp. 11 of the same work, that

$$S(x^n) = (1 + \Delta) \cdot \frac{(1 + \Delta)^x - 1}{\Delta} 0^n. \quad \quad (12.)$$

(6.) Furthermore, it will be necessary to recall in what follows, the notation and conventions of 'circulating functions,' as explained in my paper on that subject, published in the Philosophical Transactions for 1818, vol. cviii. p. 144. Denoting by s_x the sum of the x th powers of the s th roots of unity divided by s , or the function

$$\frac{1}{s}(\alpha^x + \beta^x + \gamma^x + \&c.),$$

where $\alpha, \beta, \gamma, \&c.$ are those roots, any function of the form

$$A_x \cdot s_x + B_x \cdot s_{x-1} + \dots + N_x \cdot s_{x-s+1}$$

will circulate in its successive values as x increases by units from 0: being expressed by A_x when x is a multiple of s ; by B_x , when $x-1$ is such a multiple, and so on. If $A_x, B_x, \&c.$ be simply constant, the function may be termed a *periodic* one, since it assumes in periodic and constantly recurring succession the values $A, B, C, \dots, N, A, B, \&c. ad infinitum$. If s be a specified number, as 2, 3, &c., we shall not the less use the notations $2_x, 3_x, \&c.$ to express the respective quantities $\frac{1}{2}(\alpha^x + \beta^x), \frac{1}{3}(\alpha^x + \beta^x + \gamma^x), \&c.$, where $\alpha, \beta, \&c.$ are the corresponding roots of unity. And we shall accordingly

mon measure, and the functions to be combined be

$$P_x = a_x.s_x + b_x.s_{x-1} + \&c.$$

$$Q_x = A_x.t_x + B_x.t_{x-1} + \&c.,$$

we have by equation (22.),

$$P_x = a_x.m_x + b_x.m_{x-1} + \dots + a_x.m_{x-t} + b_x.m_{x-t-1} + \dots \&c.$$

$$Q_x = A_x.m_x + B_x.m_{x-1} + \dots + A_x.m_{x-t} + B_x.m_{x-t-1} + \dots \&c.,$$

and consequently

$$f\{P_x, Q_x\} = f\{a_x, A_x\}.m_x + f\{b_x, B_x\}.m_{x-1} + \&c.\dots$$

For example,

$$(a.2_x + b.2_{x-1})(A.3_x + B.3_{x-1} + C.3_{x-2}) = aA.6_x + bB.6_{x-1} + aC.6_{x-2} + bA.6_{x-3} + aB.6_{x-4} + bC.6_{x-5}.$$

(11.) The readiest way in practice and the surest way to avoid mistakes, which in complex cases are very likely to occur, is to proceed by a much easier process, as in the following example. Suppose we would express the product of the three periodic functions

$$P_x = 2_x + 2.2_{x-1}, \quad Q_x = 3_x + 2.3_{x-1} + 3.3_{x-2}, \quad R_x = 4_x + 2.4_{x-1} + 3.4_{x-2} + 4.4_{x-3},$$

the product of 2, 3, 4 divided by the greatest common measure of 2 and 4 is 12, which will therefore be the period of the product. Write then the several coefficients in order as follows: for

$$P_x \quad 1, 2; 1, 2; 1, 2; 1, 2; 1, 2; 1, 2; \&c.$$

$$Q_x \quad 1, 2, 3; 1, 2, 3; 1, 2, 3; 1, 2, 3; \&c.$$

$$R_x \quad 1, 2, 3, 4; 1, 2, 3, 4; 1, 2, 3, 4; \&c.$$

$$\hline 1, 8, 9, 8, 2, 12, 3, 16, 3, 4, 6, 24,$$

and we shall have for the product

$$12_x + 8.12_{x-1} + 9.12_{x-2} + 8.12_{x-3} + 2.12_{x-4} + \dots 24.12_{x-11}.$$

If the signs of the coefficients, or any of them, differ, they must be of course annexed to each, and the proper sign affixed to each product.

(12.) If we denote by $\frac{x}{s}$ the integer part of the quotient of any number x divided by another s , then, universally,

$$\frac{x}{s} = \frac{x}{s}.s_x + \frac{x-1}{s}.s_{x-1} + \dots + \frac{x-s+1}{s}.s_{x-s+1}; \dots \dots \dots (23.)$$

$$= \frac{x}{s} - \frac{1}{s} \left\{ 0.s_x + 1.s_{x-1} + 2.s_{x-2} + \dots \overline{s-1}.s_{x-s+1} \right\}$$

and therefore the remainder, in the same division, is expressed by

$$x - s.\frac{x}{s} = 0.s_x + 1.s_{x-1} + 2.s_{x-2} + \dots \overline{s-1}.s_{x-s+1}. \dots \dots \dots (24.)$$

(13.) If, again, we would express the integer part of the quotient in the division of

$\frac{x}{s}$ by a second integer division t , we have, putting y for $\frac{x}{s}$,

$$\begin{aligned} \frac{y}{t} &= \frac{y}{t} - \frac{1}{t} \left\{ 0.t_y + 1.t_{y-1} + \dots (t-1).t_{y-t+1} \right\} \\ &= \frac{x}{st} - \frac{1}{st} \left\{ 0.s_x + 1.s_{x-1} + \dots (s-1).s_{x-s+1} \right\} \\ &\quad - \frac{1}{t} \left\{ 0.t_y + 1.t_{y-1} + \dots (t-1).t_{y-t+1} \right\}; \dots \dots \dots (25.) \end{aligned}$$

and so on as far as we please.

(14.) The periodic function $0.t_y + 1.t_{y-1} + \&c.$ depends implicitly on x , because y is dependent on x . Its value however (as well as that of any other implicit periodical function) is very easily obtained by following out the process explained in (art. 11). Suppose, for example, we had the more general periodic function of y ,

$$q_y = a.t_y + b.t_{y-1} + \dots h.t_{y-t+1}.$$

Then we may write down the successive values of x, y, q_y in order thus:

$$\begin{array}{llll} x & 0, 1, 2, \dots (s-1); s, s+1, \dots (2s-1); 2s, \dots; ts, \dots \\ y & 0, 0, 0, \dots 0; 1, 1, \dots 1; 2, \dots; t, \dots \\ q_y & a, a, a, \dots a; b, b, \dots b; c, \dots; a, \dots \end{array}$$

Thus we see that q_y is a periodical function of x , having for its period st instead of either s or t separately, the first s coefficients being all alike and each $=a$, the next s all alike and each $=b$, and so on; or

$$q_y = a\{(st)_x + \dots (st)_{x-s+1}\} + b\{(st)_{x-s} + \dots (st)_{x-2s+1}\} + \dots + h\{(st)_{x-ts} + \dots (st)_{x-ts+1}\}. \quad (26.)$$

(15.) Hence we are enabled to express the value of $\frac{y}{t}$ explicitly as a periodic function of x ; for by equation (25.), if we put $st=n$,

$$\begin{aligned} \frac{y}{t} &= \frac{x}{n} - \frac{1}{n} \left\{ 0.s_x + 1.s_{x-1} + \dots \overline{s-1}.s_{x-s+1} \right\} \\ &\quad - \frac{1}{n} \left\{ 0.t_y + 1.t_{y-1} + \dots \overline{s-1}.t_{y-t+1} \right\}. \end{aligned}$$

But by equation (21.) we have

$$\begin{aligned} s_x &= n_x + n_{x-s} + n_{x-2s} + \dots n_{x-n+s} \\ s_{x-1} &= n_{x-1} + n_{x-1-s} + \dots n_{x-n+s-1}, \&c.; \end{aligned}$$

and by the equation (26.) of the foregoing article,

$$\begin{aligned} t_y &= n_x + n_{x-1} + \dots n_{x-t+1} \\ t_{y-1} &= n_{x-1} + n_{x-1-1} + \dots n_{x-2t+1} \\ t_{y-2} &= n_{x-2} + n_{x-2-1} + \dots n_{x-3t+1}, \&c.; \end{aligned}$$

and consequently by substitution,

$$\frac{y}{t} = \frac{x}{n} - \frac{1}{n} \left\{ 0.n_x + 1.n_{x-1} + 2.n_{x-2} + \dots \overline{n-1}.n_{x-n+1} \right\}. \dots \dots \dots (27.)$$

(16.) These relations premised, let it be required to express the sum of x terms of the series

$$\phi(a+b) + \phi(a+2b) + \dots \phi(a+xb) = S_x.$$

Developing the several terms, we find

$$\begin{aligned} S_x &= \phi(a) \{1 + 1 + \dots x \text{ terms}\} \\ &\quad + \frac{b}{1} \cdot \phi'(a) \{1 + 2 + 3 + \dots x\} \\ &\quad + \frac{b^2}{1.2} \phi''(a) \{1^2 + 2^2 + 3^2 + \dots x^2\} + \&c. \end{aligned}$$

Substituting then for the series of powers of 1, 2, 3, &c. their values as given by equation (11.), and separating the symbols of operation from that of quantity, we get

$$S_x = \frac{1 - (1 + \Delta)^{-x}}{\Delta} \left\{ \phi(a) \cdot 0^0 - \phi'(a) \cdot \frac{b}{1} \cdot 0^1 + \phi''(a) \cdot \frac{b^2}{1.2} \cdot 0^2 - \&c. \right\} = \frac{1 - (1 + \Delta)^{-x}}{\Delta} \phi(a - b \cdot 0). \quad (28.)$$

(17.) If we use in like manner equation (12.), it gives

$$S_x = (1 + \Delta) \frac{(1 + \Delta)^x - 1}{\Delta} \phi(a + b \cdot 0) = \frac{(1 + \Delta)^x - 1}{\Delta} \phi(\overline{a + b} + b \cdot 0), \quad \dots \quad (29.)$$

by employing the transformation of equation (3.), in which $x=1$, $f(0) = \phi(a + b \cdot 0)$. Hence also, if

$$S_x = \phi(a) + \phi(a+b) + \dots \phi(a + \overline{x-1} \cdot b),$$

we find in like manner

$$S_x = \frac{(1 + \Delta)^x - 1}{\Delta} \phi(a + b \cdot 0). \quad \dots \quad (30.)$$

(18.) Let it next be required to find the sum of the series

$$S_y = \phi(a+b) + \phi(a+2b) + \dots \phi(a+yb)$$

to y terms, where $y = \frac{x}{s}$ the integer part of the quotient of an independent index number x , divided by any given number s . By equation (28.) we have

$$S_y = \frac{1 - (1 + \Delta)^{-y}}{\Delta} \phi(a - b \cdot 0).$$

Now since

$$\begin{aligned} y &= \frac{x}{s} s_{x-1} + \frac{x-1}{s} s_{x-1} + \dots \frac{x-s+1}{s} s_{x-s+1} \\ &= \frac{x}{s} - \frac{1}{s} \left\{ 0 \cdot s_x + 1 \cdot s_{x-1} + \dots (s-1) \cdot s_{x-s+1} \right\}. \end{aligned}$$

If we put

$$p = -\frac{x}{s}; \quad q = \frac{1}{s} \left\{ 0 \cdot s_x + 1 \cdot s_{x-1} + \&c. \right\},$$

we get by equation (9.),

$$S_y = \frac{1 - (1 + \Delta)^p}{\Delta} \phi(a - b \cdot 0) + \frac{1 - (1 + \Delta)^q}{\Delta} \phi(\overline{a - pb} - b \cdot 0).$$

Now the first portion of this, since $p = -\frac{x}{s}$, is explicit in terms of x , but the other requires further development, for which we must have recourse to equation (16.), putting $Q_x = (1 + \Delta)$ and $P_x = q = \frac{1}{s}(0.s_x + 1.s_{x-1} + \&c.)$, where we find

$$(1 + \Delta)^q = (1 + \Delta)^0.s_x + (1 + \Delta)^{\frac{1}{s}}.s_{x-1} + (1 + \Delta)^{\frac{2}{s}}.s_{x-2} + \&c.$$

But we also have by equation (18.),

$$1 = s_x + s_{x-1} + s_{x-2} + \&c.$$

Therefore, subtracting and dividing by Δ , and applying each term of operation to the term $\phi(\overline{a-pb-b.0})$ of quantity,

$$\begin{aligned} \frac{1 - (1 + \Delta)^q}{\Delta} \phi(\overline{a-pb-b.0}) &= \left\{ 0.s_x + \frac{1 - (1 + \Delta)^{\frac{1}{s}}}{\Delta} s_{x-1} + \frac{1 - (1 + \Delta)^{\frac{2}{s}}}{\Delta} s_{x-2} + \&c. \right\} \phi(\overline{a-pb-b.0}) \\ &= 0.s_x + \frac{1 - (1 + \Delta)^{\frac{1}{s}}}{\Delta} \phi(\overline{a-pb-b.0}).s_{x-1} + \frac{1 - (1 + \Delta)^{\frac{2}{s}}}{\Delta} \phi(\overline{a-pb-b.0}).s_{x-2} + \&c. \quad (31.) \end{aligned}$$

(19.) The expression for S_y in the last article is general and entirely independent of any particular values assigned to x, a, b, s , the only relation established being that expressed by the equation $y = \frac{x}{s}$. Suppose therefore that in a certain proposed case we should have

$$a = x + s - 1; \quad b = -s;$$

and therefore

$$pb = x; \quad a - pb = s - 1,$$

and the expression for the sum of the series becomes

$$\begin{aligned} S_y &= \frac{1 - (1 + \Delta)^{-\frac{1}{s}}}{\Delta} \phi(x + s - 1 + s.0) \\ &\quad + \frac{1 - (1 + \Delta)^{\frac{1}{s}}}{\Delta} \phi(\overline{s-1+s.0}).s_{x-1} \\ &\quad + \frac{1 - (1 + \Delta)^{\frac{2}{s}}}{\Delta} \phi(\overline{s-1+s.0}).s_{x-2} + \&c.; \quad . \quad . \quad . \quad . \quad . \quad . \quad (32.) \end{aligned}$$

in which expression the first member or non-periodical part is an explicit function of x and s , and the periodical part has all its coefficients independent on x and functions of s alone.

(20.) The periodical part of S_y is however susceptible of another form, better adapted for numerical calculation, into which it may be thrown by making $p = q = \frac{1}{s}$ in equation (9.), when it becomes

$$\frac{1 - (1 + \Delta)^{\frac{2}{s}}}{\Delta} f(0) = \frac{1 - (1 + \Delta)^{\frac{1}{s}}}{\Delta} f(0) + \frac{1 - (1 + \Delta)^{\frac{1}{s}}}{\Delta} f\left(\frac{1}{s} + 0\right),$$

