X. *Researches on the Tides.—Ninth Series. On the Determination of the Laws of the Tides from short Series of Observations.* By the Rev. W. WHEWELL, *M.A., F.R.S., Fellow of Trinity College, Cambridge.*

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1. THE discussion of tide observations, for the purpose of obtaining from them the laws of the phenomena, has hitherto been usually conducted upon the supposition that a series of several years in succession was requisite, in order that the accidental irregularities might disappear in the *means* of the observations, and the effects of the lunar inequalities thus come clearly into view. But in the present memoir I shall endeavour to show with what degree of exactness the laws of the phenomena, and the effects of the lunar inequalities, may be deduced from shorter series of observations; for example, from series of one year.

2. I conceive that such an investigation will be of value in several ways. If the principal elements of tide tables for each particular place can be obtained with moderate accuracy from short series of observations properly discussed, the formation of such independent tide tables for different places and times will become far less laborious than it has hitherto been deemed, and may be expected to be far more commonly practised. This will be a great advantage, not only because the tide tables will thus become better, but also because several important questions may thus be settled; for instance, whether, and how far, the laws of tide phenomena change from place to place, and from time to time. These questions we are as yet unable to answer with confidence or with accuracy, although they affect the very foundations of all tide theory, as well as the permanent value of tide tables.

3. But there is another consideration which makes it desirable to compare the results of short series with those of long ones. Without such a comparison we cannot appreciate the *practical* accuracy of our tide tables. If, for instance, the mean of nineteen years of tide observations gave a very exact rule for the effect of lunar parallax, while each single year deviated widely from this rule, it would be clear that the individual observations must be commonly affected by casual irregularities considerably greater than the parallax correction; and therefore the practical accuracy of the tables would be very little improved by introducing into them the parallax correction. I hope to establish, on the contrary, in the following pages, that the general law, and the approximate amount, of the parallax correction, may be traced in the observations of a single year; and thus, that the tables are rendered practically

as well as theoretically better, by such a correction being employed. The same is true, though in a less marked manner, of the correction for lunar declination.

4. It is hardly necessary to remark, as an additional recommendation of the mode of discussing tide observations which this memoir contains, that it leads us, quite as well as any other method, to the results of longer series. When we have obtained the table or the curve of the parallax correction for several single years in succession, the more accurate table or curve which the whole group of years would give is the mean of the single instances. We thus obtain all the accuracy which a longer series of years can supply, while we have, at the same time, the advantage of seeing how much each year contributes to this accuracy.

6. In obtaining the laws of tide phenomena from the observations, different modes of procedure may be adopted, and it still remains to be decided which of these modes is the best. For instance, instead of referring each tide to the moon's transit immediately preceding, we may refer it to the transit one day, or a day and a half, or two days anterior; and it is important to determine which of these modes of reference represents most closely the laws of the phenomena. It is also desirable to ascertain what is the nature and amount of the variations, which these alterations of the epoch of the lunitidal interval introduce into the correction tables for declination and parallax. To settle this point has been one object of the present Researches; and for that purpose I have calculated the correction tables for Bristol, referring the tide to the lunar transit one day, one day and a half, and two days anterior to the transit immediately preceding the tide. Of the results of these different hypotheses I shall hereafter speak.

7. I will add that the present memoir, by clearing up such points as I have noticed, appears to be suited to wind up the series of general researches respecting the tides, which I have, during some years, laid before the Royal Society from time to time. For if methods of discussing tide observations at any place be given, so complete, that the general laws of the corrections may be easily obtained from the observations of a single year, and good tables from a few years, there remains nothing to be done except to apply these methods to good observations, and thus to construct and improve our tide tables for all the most important places; an employment which can easily be carried on by those persons who have performed the calculations on which the present and preceding memoirs are founded. And thus the study of the tides might be pursued, and, to do the subject justice, ought to be pursued, in the same manner as the study of the other provinces of astronomy: that is, constant and careful observations should be made of the phenomena; and, as fast as they are made, should be reduced and discussed at the public expense; so as to test the accuracy of the tables already obtained, and to supply the means of making them still more accurate. In this manner also, any new corrections, and any changes in the elements of the old corrections, would be brought into view as soon as there was evidence of

their existence. Till the subject of the tides is thus treated in a manner worthy of its scientific importance, and of the promise which it now holds forth, it must be considered as offering a blot in that system of the national cultivation of astronomy, of which our public observatories are, in other departments of science, such effective and magnificent examples.

8. The reference of the phenomena of the tides to mechanical principles by rigorous reasoning has not yet been executed; and we can see enough of the difficulties of the subject to believe that it will probably be some time before this problem can be fully solved. In the meanwhile, we have an intermediate type of comparison for the facts, in the equilibrium theory of BERNOULLI: for that theory, by modifying the epochs and other elements, may be made to represent in an approximate manner the laws of the phenomena. Nor does it appear to be too much to expect, that it may hereafter be rigorously shown from mechanical principles, that the form of an irregular moving fluid mass, constantly dragged along by certain forces, shall at every instant resemble the form of equilibrium which the forces would produce at some anterior epoch, the anterior epoch being somewhat different for the different features of the fluid form. If such a hydrodynamical proposition could be established, almost all the facts hitherto discovered respecting the tides would be fully explained.

9. The materials of the following researches are the tide observations made at Plymouth in the years 1834, 1835, 1836, and 1837, under the direction of the Dock Master and Assistant Dock Master; and the observations made at Bristol in 1834, 1835, 1836, 1837, by direction of the Dock Committee.

The Plymouth observations were discussed by Mr. DESSIOU and Mr. D. ROSS of the Hydrographer's Office, whose valuable services were placed at my disposal by the Hydrographer, Captain BEAUFORT. The Bristol observations were discussed by Mr. BUNT, whom I was enabled to engage in this employment by means of grants voted for that purpose at the Meetings of the British Association for the Advancement of Science which took place in 1836 and 1837.

10. The methods of discussing the observations which were employed were somewhat different for the two places. As that practised at my suggestion by Mr. BUNT for the Bristol tides appears to be the better of the two, I will subjoin his description of the process.

I may previously observe that in all cases we employ the method already described in previous papers: namely, upon a series of parallel ordinates* corresponding to the times of moon's transit, we lay down the successive tides, that is, the heights or the lunitidal intervals, as the one or the other are the subject of examination. This curve is more or less irregular, but for most places the leading feature is the zigzag form which arises from the diurnal inequality†. A curve is drawn by the eye so as

* For the purpose of all these operations it is most convenient to have paper ruled into small squares of inches and twelfths of inches.

† Researches, Seventh Series, Philosophical Transactions, 1837.

to cut off this inequality, leaving equal differences above and below. We may then proceed as follows to find the other inequalities.

"Having laid down the observed intervals and heights, referring both to the *apparent* time of the moon's transit, and drawn through each series of points the dotted line which cuts off the diurnal inequality only, but retains every other, I trace off on a piece of transparent paper, having an axis drawn on it extending from $0^h\ 0^m$ to $12^h\ 0^m$ transit, the successive portions of the dotted line of observation just mentioned, which are included between those hours during the first three months of the year; fitting the tracing paper in its place every time by means of the two extreme points of the axis. I thus obtain six irregular curves, the mean of which is found by drawing across them, at equal distances, twenty-four vertical lines, and finding by my scale, a point in each which is the exact mean of the six intersections. In this manner I get four mean curves on separate pieces of paper, which by repeated combinations are reduced into one, being the mean semimenstrual curve for the year.

"The next step is to reduce this curve to a mean parallax (57'·2) at each hour of transit. For this purpose an arrangement must be made, showing the mean parallax for that year at each of the twelve hours, which will be found to vary from about 56'·9 to 57'·5. Thus the mean parallax for 1836, at the several hours of transit, is

Hour of Transit...	h m 0 30	h m 1 30	h m 2 30	h m 3 30	h m 4 30	h m 5 30	h m 6 30	h m 7 30	h m 8 30	h m 9 30	h m 10 30	h m 11 30
Parallax	57'·5	57'·4	57'·3	57'·1	57'·0	56'·9	56'·9	57'·0	57'·1	57'·2	57'·4	57'·5
Mean of all 57'·2.												

"The parallax table of the preceding year, if already discussed, will be sufficiently near for making the requisite small alteration of the curve to the mean parallax: otherwise an approximate parallax table for the current year must be first made. I then calculate the mean declination, which varies, not only as the parallax, slightly from hour to hour, but also considerably from year to year. The hourly differences (being only about half a degree from the mean) I have disregarded, and I prefer marking on each annual curve the mean declination of that year, to any attempt to reduce the different annual curves to one common declination. Having thus very carefully obtained the mean semimenstrual curve, I cut it out nicely on a piece of thick drawing-paper, and laying the intersections of the vertical hour lines of 0^h and 12^h with the axis, on the corresponding point, on my sheets, I pencil in the mean curves, both of time and of height, and then ink them. The residue, or space between the mean curve and that of observation is next transferred to a straight line below.

"In examining this residue, the first step is to lay on an *approximate* line of parallax. This I take from the parallax correction-curves of the preceding year, if it has been discussed. If not, I make an arrangement for every hour of transit, and for 54', 55', 56', and 59', 60', 61' of parallax (omitting 57' and 58'), of all the vertical distances, at the successive hours of transit, of the curve from the straight line, adding to each a constant, to avoid negative quantities. I thus obtain twenty-four parcels, the means of which give an approximate correction for 55′ .. 60′ (or rather 55′ .. 59½′) at every hour of transit. From this a first line of parallax is laid down, preparatory to the obtaining of the declination corrections.

"This is done by collecting the measured distances from the parallax line to the curve, into parcels of 0° decl. to 13° decl., and 21° decl. to 28° decl. for every hour of transit, omitting those of 14° to 20° decl., and taking the axis, with the mean declination of the year, as a better representative of the mean declinations. The means of these twenty-four parcels give me a declination correction very near the truth. From this I lay down, on the parallax line, the effect of declination, and thus get an approximate curve of declination combined with parallax. Both this and the former curve of parallax are drawn in with pencil only, being merely used as approximations, whence *corrections* of the first parallax and declination *corrections* are obtained. This is done by an arrangement (as before) of the spaces still remaining between the pencil curve of declination combined with parallax, for hours of transit, and 54′ .. 56′, 59′ .. 61′ parallax, which gives a small additional correction of the parallax corrections first obtained. From this corrected parallax correction I draw in, with ink, the true parallax line, making the requisite alteration in the line of declination combined with parallax, which is then also inked in. Finally, a *correction* of the declination *corrections* being made from this latter line, is considered as giving the true effect of the declinations.

"A further improvement has been introduced into the discussions of 1836, which ought not to be neglected. In former arrangements, e. g. of the twelve parcels of observations with the parallaxes 54′, 55′, 56′, the mean of the *measured distances only* was taken, the mean of the *corresponding parallaxes* having been all assumed = 55′, whereas they differ sometimes half a minute. I have now taken, in all cases, both the mean of the observed quantities and the mean of the parallaxes, or declinations corresponding to that particular parcel; these are afterwards reduced to a common value.

"The above description applies equally to the two processes, for time and for height, which are conducted both in the same manner."

11. By the methods thus described, we obtain curves which represent the semimenstrual inequality and the corrections for lunar parallax and declination, both of the times and of the heights, for any assumed parallaxes and declinations both below and above the mean. By operating in this manner upon the observations of each year separately, we obtain curves for each year; and by combining these for a sufficient number of years, we obtain a mean result, which will possess greater accuracy. From these curves, and especially from the mean of them, we can construct tables of the corrections, which may be used in calculating predictions of the tides, such as are commonly called Tide Tables.

12. But though this is undoubtedly one main use of a discussion of tide observations,

our labours were conducted, as I have already stated, with a peculiar view to several questions respecting the connection of tide phenomena at different times and places; and I will now state the answers to these questions, which our results supply.

I. *To which transit of the moon ought we to refer the tide?*

It has been well shown by Mr. LUBBOCK that the agreement between the empirical laws of the tides and the equilibrium theory is much improved, by referring the tides to a transit anterior to their occurrence by two or three days. He denotes the successive transits of the moon (at intervals of about twelve hours) by the letters A, B, C, D, E, F; F being the transit which immediately precedes (by about two hours) the high water at London: and he finds that the laws of the tides at London and Liverpool agree best with theory when they are referred to the transit B. The tide which reaches London at two hours after the transit F, was at Plymouth about six hours after the transit E; and as from transit B to transit E is about thirty-eight hours, if we refer the Plymouth tides to transit B, we take a transit about forty-four hours before the tide. Transits A and C are about fifty-six and thirty-two hours anterior to the tide. Nearly the same may be said of Bristol, for the tide there is nearly contemporaneous with that at Plymouth. I have, therefore, referred the Bristol tides of 1834 and 1835 to each of the three transits A, B, C, and have compared the results.

The general result is, that the transit B gives the best tables. This is shown both by the unaccounted-for residue of the observed quantities, which is smallest when transit B is used; and by the form of the curves for the parallax and declination corrections, which is most regular for this transit.

We may therefore, it would seem, assume, at least for our own coasts, that the tides are to be referred to a transit of the moon, which takes place a day and a half before the tide reaches the coasts of Europe.

We may, however, observe that we do not in this way obtain an exact agreement of observation and theory, even with regard to the semimenstrual inequality. It has appeared from Mr. LUBBOCK's researches respecting the Liverpool tides*, that while the transit A gives a very exact agreement of the theoretical and observed times, we must take a still earlier transit if we would obtain this agreement with respect to the heights. Nor does that selection of a transit which best represents the semimenstrual inequality, bring about an agreement with theory in the parallax and declination corrections, as we shall see. We must allow, therefore, that though there appear to be, in the actual laws of the tides, inequalities *corresponding* to all these which arise from the supposition of the equilibrium-tide of an anterior epoch transmitted along the ocean to our shores, we cannot so assume the epochs to produce all the inequalities at once. The epoch is of one value for the times, of another for the heights; different again for the parallax correction, and again different for the effect of declination.

* Philosophical Transactions, 1836, Part II.

II. *How does a change of the epoch affect the semimenstrual inequalities?*

The moon's transit takes place every half-day about twenty-four minutes later by solar time. The semimenstrual inequality, both of interval and of height, is referred to the solar time of the moon's transit. The height is the same whether it be referred to the transit E immediately preceding, or to the transit B, which is a day and a half sooner. Also if the moon moved uniformly in her orbit, the *inequality* of the interval of the tide and the transit would be the same, whether the tide were referred to the transit E, or to any anterior one, as B; for the interval is increased by a constant quantity (twelve hours twenty-four minutes) for every transit that we go back. But though the inequality of the interval for any given tide would be the same, it would not occupy the same place in the table or curve, since it would be referred to a different hour of transit. For example, if the mean interval of transit and tide at Bristol, referred to transit E, be 7^h, and if when the moon's transit is 2^h, the tide be at 8^h, the interval is 6^h, and therefore in this case the inequality is *minus* one hour. If now we refer this tide to the transit B, a day and a half sooner, the interval of the transit and tide will be $6^h + 36^h + 72^m$, and the mean interval will be $7^h + 36^h + 72^m$; and therefore, as before, the inequality is *minus* one hour. But the moon's transit E taking place at 2^h, solar time, the transits D, C, B will take place at $1^h\ 36^m$, $1^h\ 12^m$, $0^h\ 48^m$ respectively, and therefore the inequality of minus one hour, which was referred to the transit happening at 2^h when transit E was used, is referred to the transit at $0^h\ 48^m$, when we employ the anterior epoch B.

We thus see, that by referring to an anterior epoch, the whole semimenstrual inequality is moved backwards through twenty-four minutes of lunar transit, for every step of one transit backwards. This is the mean result; supposing the moon's motion to be uniform, and neglecting all other inequalities; on which suppositions nothing would be gained or lost in accuracy by the change of epoch. In the actual case we shall find this mean result modified by the influence of the other inequalities, which make one transit a better epoch than another.

The transfer of the semimenstrual inequality curve which we are thus led to expect on changing the epoch, shows itself in the results of the Bristol observations. For example, the time of transit for which the semimenstrual inequality of the interval is 0, being taken for the three transits A, B, C, we find

	For A.	For B.	For C.
In 1834,	$6^h\ 4^m$;	$6^h\ 28^m$;	$6^h\ 52^m$.
In 1835,	$5^h\ 58^m$;	$6^h\ 22^m$;	$7^h\ 4^m$.

Again, for the semimenstrual inequality of heights we find that the minimum height corresponds to the following hours of moon's transit:

	For A.	For B.	For C.
In 1834,	$5^h\ 30^m$;	$5^h\ 54^m$;	$6^h\ 24^m$.
In 1835,	$5^h\ 29^m$;	$5^h\ 46^m$;	$6^h\ 18^m$.

The maximum height corresponds to the following hours of moon's transit:

	For A.	For B.	For C.
In 1834,	$11^h\ 27^m$;	$11^h\ 57^m$;	$12^h\ 28^m$.
In 1835,	$11^h\ 25^m$;	$11^h\ 47^m$;	$12^h\ 7^m$.

We may observe, that if we could find an epoch which should make the results exactly correspond with the theory, the semimenstrual inequality of the time would be 0^h for the time of transit 6^h; and the height would have its maximum and minimum values for the times of transit 6^h and 12^h. From the above results it follows, by an easy calculation, that the epoch which makes the inequality in time agree with the theory is about the transit A; and the epoch which makes the inequality of heights agree with the theory is about four hours after the transit B. We have therefore here still clearer evidence, of that which the discussion of the Liverpool tides by Mr. LUBBOCK sufficiently showed, that there is no one anterior epoch which will reduce the observed tides to an agreement with the equilibrium theory.

III. *How does a change of the epoch affect the (lunar) parallax correction of the time?*

The parallax corrections of the times, calculated with different epochs, have a general agreement with each other, and with the theory. Taking the correction for parallax 60′ as our example, it has a maximum and a minimum which correspond to transits differing only about three hours from each other; so that the inequality increases from its minimum to its maximum through about nine hours of transit, and then diminishes more rapidly from the maximum to the minimum through three hours. In this all the empirical curves and the theoretical curve agree. But in general the empirical curves differ in two respects from the theoretical one; first, according to theory, the transit which gives the maximum and that which gives the minimum are equally distant from 6^h; one being as much before as the other after that period; whereas in the observation, this mean point corresponds to $6\frac{1}{2}^h$ or 7^h transit; and secondly, the empirical correction contains, besides the variable part, a constant part which depends on the parallax, but does not vary with the hour of transit. This I had already noticed in the second series of these Researches*.

It follows from what is there stated, that while theory gives the formula†

$$(P - p)\ (b \sin 2\ \varphi),$$

observation gives such a formula as

$$(P - p)\ (a + b \sin 2\ (\varphi - \beta)).$$

But the quantity a as well as β, changes with the epoch, as Mr. LUBBOCK has justly observed; and the question occurs, whether we can, by a proper assumption of the epoch, cause the quantity a to disappear, and thus render the curve which represents the observed law symmetrical with regard to the axis, as the theoretical curve is.

* Philosophical Transactions, 1834, p. 40.

† Here P is the parallax, p the mean parallax, ϕ the hour of moon's transit, β a time depending on the epoch.

This question our discussion of the Bristol tides enables us to answer. For if we take the parallax correction for 60′, for the year 1834, and for the transit A, we find that it has a maximum 16^m at $5\frac{1}{4}^h$ transit; a minimum $= 1^m$ at $7\frac{3}{4}^h$ transit. Hence it may be represented by $8\frac{1}{2}^m - 7\frac{1}{2}^m . s, 2\varphi - 13^h$, where $s, 2\varphi$ represents a function of 2φ, which differs from $\sin 2\varphi$ in having its maximum and minimum at a smaller distance than six hours ($2\frac{1}{2}$ hours in this case), the distance depending on some other quantity*; but which, like $\sin 2\varphi$, is 0 when φ is 0, + when φ is less than 6^h, — when φ is greater than 6^h; and has for its maximum and minimum values + 1 and — 1, at equal distances from the value $2\varphi = 0$.

In the same manner we may reduce to formulæ of the same kind the parallax corrections for the other transits. And putting them together we have the following results.

		m	m	h
Year 1834.	Transit A.	$8\frac{1}{2}$	$- 7\frac{1}{2} . s, 2\varphi$	$- 13$
	Transit B.	$10\frac{3}{4}$	$- 12\frac{1}{4} . s, 2\varphi$	$- 14$
	Transit C.	$3\frac{1}{2}$	$- 7\frac{1}{2} . s, 2\varphi$	$- 14$
Year 1835.	Transit A.	$9\frac{1}{2}$	$- 7\frac{1}{2} . s, 2\varphi$	$- 13\frac{1}{2}$
	Transit B.	$8\frac{1}{2}$	$- 10\frac{1}{2} . s, 2\varphi$	$- 14$
	Transit C.	$2\frac{1}{2}$	$- 9\frac{1}{2} . s, 2\varphi$	$- 14$

These results show, that by taking a later transit the quantity a is diminished, although irregularly; and therefore the epoch which would reduce the correction to the symmetry of theory is later than C; but we cannot pretend to say with precision how much, without further calculation. We may observe that the circumstance of the coefficients of the variable part being largest for the transit B, appears to indicate that the discussion relative to that transit is the best-conditioned for bringing into view the parallax inequality.

The results of the Bristol observations for 1836 and 1837, discussed for transit B, give, for the parallax correction of the times,

$$\text{for 1836, } 6^m + 9^m . s, 2\varphi - 14^h;$$
$$\text{for 1837, } 11^m + 12^m . s, 2\varphi - 13^h.$$

The Plymouth observations for 1834, 1835, 1836, 1837, referred to the transit B, give a parallax correction, which follows nearly the same laws as those just stated for Bristol. It has a maximum of 22^m at 5^h, and a minimum of $- 3^m$ at 9^h. Hence its formula is (for 61′ parallax)

$$9\tfrac{1}{2}^m - 12\tfrac{1}{2}^m . s, 2\varphi - 14^h,$$

which agrees very nearly with the Bristol formula for transit B.

IV. *How does a change of the epoch affect the (lunar) declination correction of the times?*

* The expression $\frac{\sin 2(\phi - \beta)}{1 + c \cos 2(\phi - \beta)}$ is an instance of such an expression, the maximum and minimum being equally distant from the value $\phi = \beta$, and each of these distances depending upon the value of c.

Observations similar to those which have been made concerning the correction of the times for parallax, may be repeated with respect to the declination. The theoretical correction is of the form*

$$(\sin^2 \delta - \sin^2 \Delta)\, d \,.\, s,\, 2\,\phi\,;$$

but observation gives

$$(\sin^2 \delta - \sin^2 \Delta)\, (c + d \,.\, s,\, 2\,\phi - 2\,\beta).$$

And the quantity c varies with a change of the epoch.

The following are the results of the Bristol observations.

For the year 1834, transit A, we find for the declination 7°, a maximum 7^{m} at $4\frac{1}{4}^{h}$; a minimum -7^{m} at $7\frac{3}{4}^{h}$. We find also for the declination 24°, a minimum -3^{m} at 5^{h}; a maximum 9^{m} at $7\frac{1}{4}^{h}$. Hence the corrections for declination of this series may be expressed by these formulæ:

$$\text{for } 7°,\ 0^{m} - 7^{m} \,.\, s,\, 2\,\phi - 12^{h};\ \text{for } 24°,\ 3^{m} + 6^{m} \,.\, s,\, 2\,\phi - 12\tfrac{1}{4}^{h}.$$

Collecting in the same manner the corrections for the other series, we have

		Decl. 7°.	Decl. 24°.
		m m h	m m h
Year 1834.	Transit A,	$0 - 7 \,.\, s, 2\,\phi - 12$,	$3 + 6 \,.\, s, 2\,\phi - 12\frac{1}{4}$
	B,	$-\frac{1}{2} - 5\frac{1}{2} \,.\, s, 2\,\phi - 12\frac{1}{2}$,	$\frac{1}{2} + 7\frac{1}{2} \,.\, s, 2\,\phi - 13$
	C,	$5 - 5 \,.\, s, 2\,\phi - 14$,	$-2\frac{3}{4} + 5\frac{1}{4} \,.\, s, 2\,\phi - 13\frac{3}{4}$
Year 1835.	Transit A,	$1\frac{3}{4} - 8\frac{1}{4} \,.\, s, 2\,\phi - 12$,	$2\frac{1}{4} + 5\frac{3}{4} \,.\, s, 2\,\phi - 12$
	B,	$1\frac{1}{2} - 7\frac{1}{2} \,.\, s, 2\,\phi - 13\frac{1}{4}$,	$-1\frac{1}{2} + 7\frac{1}{2} \,.\, s, 2\,\phi - 13$
	C,	$7 - 11 \,.\, s, 2\,\phi - 14$,	$-1 + 7 \,.\, s, 2\,\phi - 14$

We here see that the transits A and B make c very small, so that when they are used, the declination correction curve approaches very near to the symmetry of theory. Also these epochs make the curve cut the axis very nearly at 6^{h}, as the theory gives.

The observations at Bristol for 1836 and 1837 give for the declination corrections, transit B being used,

	Decl. 7°.	Decl. 24°.
	m m h	m m h
1836,	$1\frac{1}{2} - 7\frac{1}{2} \,.\, s, 2\,\phi - 13\frac{3}{4}$;	$\frac{1}{2} + 5\frac{1}{2} \,.\, s, 2\,\phi - 12$;
1837,	$3\frac{3}{4} - 7\frac{3}{4} \,.\, s, 2\,\phi - 12\frac{3}{4}$;	$-1\frac{3}{4} + 7\frac{1}{2} \,.\, s, 2\,\phi - 12$.

The Plymouth observations for 1834, 1835, 1836, 1837, referred to transit B, give in like manner for the low declination 6°, a curve which has a maximum 10^{m} at 5^{h}, and a minimum -3^{m} at $8\frac{1}{2}^{h}$. Hence its formula would be

$$3\tfrac{1}{2}^{m} - 6\tfrac{1}{2}^{m} \,.\, s,\, 2\,\phi - 13\tfrac{1}{2}^{h}.$$

The correction for 24° has a minimum of -9^{m} at 4^{h}, a maximum of 4^{m} at 8^{h}. Hence its formula is

$$-2\tfrac{1}{2}^{m} + 6\tfrac{1}{2}^{m} \,.\, s,\, 2\,\phi - 12^{h}.$$

V. *How does a change of the epoch affect the parallax correction of heights?*

* Here δ is the declination, and Δ the mean declination.

The parallax correction of heights, according to theory, is of the form

$$(\mathrm{P} - p)\,(a + b\,.\,s,\ 2\,\phi).$$

This agrees with the Bristol observations, which give

$$(\mathrm{P} - p)\,(a + b\,.\,s,\ 2\,\phi - 2\,\beta)\,;$$

for the amount of this correction for a given parallax (as 60′, for example,) is nearly constant for all hours of transit, having, however, a maximum about $6\frac{1}{2}^{h}$ and a minimum about $8\frac{1}{2}^{h}$. Hence β is about $7\frac{1}{2}^{h}$ in this case; the ratio of b to a, which is that of the solar to the lunar tide in theory, appears to be smaller in fact. Indeed for transit A the quantity b vanishes or becomes very small, and the parallax correction is nearly constant for all hours. This appears to show that the transit B is under better conditions than A for obtaining this correction.

The Plymouth parallax correction of heights follows nearly the same law. It has a maximum about $5\frac{1}{2}^{h}$ and a minimum about $8\frac{1}{2}^{h}$.

VI. *How does a change of the epoch affect the declination correction of the heights?*

The declination correction of heights as collected from the Bristol observations is somewhat irregular, the curve for the high and the low declinations being in some cases different in form. It does not appear that the change of epoch very much modifies these forms, at least in any steady manner. For all the epochs (transit A, B, and C) the correction for low declinations (as 7°) has a maximum about $6\frac{1}{2}^{h}$, which diminishes both ways, so as to make a protuberance in the curve from about 4^{h} to 8^{h}, the rest of the correction being nearly constant. For the high declinations (as 24°) the curve, in the best discussions (those of 1836 and 1837), appears to have nearly the same form as for the low declinations.

The Plymouth observations of this correction offer a minimum about 4^{h}, and a maximum about 7^{h}.

VII. *Does the parallax correction of height vary as the parallax?*

In the course of the discussions of the Plymouth tides, the parallax correction of height was taken for each of the twelve hours of transit, and laid down by means of ordinates corresponding to the minutes of transit from 54′ to 61′. The result was, for each year separately (1834–7), very nearly the same straight line, inclined to the axis, and cutting the axis about 57′, which is the mean value of the parallax. The mean of all these years does not differ sensibly from a straight line for each hour; thus showing that the parallax correction of the height does, in reality, vary in proportion to the parallax itself, as according to the theory it ought to do.

We may remark, however, as appears from the same discussion, that from 5^{h} to 9^{h} transit the highest parallaxes are wanting; and that for these hours the mean parallax is smaller than 57′, and the correction line cuts the axis before 57′, whereas for the other hours the point of intersection is after 57′. This result arises in a great measure from the moon's variation, which, increasing her distance from the earth at quadrature, diminishes the parallaxes in that part of her orbit, and makes their greatest values less than they are in other situations.

The Bristol observations give the same result, which was established by our examination in a manner somewhat different. In 1836 the sum of the parallax corrections of height for all the twelve hours, from the parallax 60′ to the mean 57′·2, was 315 parts; from 57′·2 to 55′ it was 246 parts; the simple ratio of the parallaxes would give 315 to 247·5.

VIII. *Does the parallax correction of time vary as the parallax?*

The observations just referred to enable us to answer this question in the affirmative. In 1836 the corrections of time from 60′ to 57′·2 were 125, and from 57′·2 to 55′ they were 96 parts; the simple ratio of the parallaxes gives 125 to 98·2.

IX. *Does the declination correction of heights vary as the square of the declination?*

According to the theory, the correction for declination, both in the time and the height, will vary as the square of the declination, or as the square of its sine nearly, the correction being supposed to be applied to the declination 0°. And it was shown in the examination of the London tides*, that this is the law which prevails with tolerable regularity in the mean of nineteen years' observations, both for the times and heights. We may perhaps consider this rule therefore as established by much better evidence than two or three years' observations can supply. Still it is not unimportant to examine how far this rule manifests itself in the result of short series of observations. The Plymouth tides were examined with reference to this point. The diurnal, semimenstrual, and parallax corrections being determined and subtracted, the residues of the heights were for each hour expressed by ordinates corresponding to the degrees of declination (taken in groups 0°..3°, 3°..9°, 9°..15°, 15°..21°, 21°..28°). It appeared that in each year (from 1834 to 1837) the curve thus obtained was nearly a straight line, and it was still more nearly so in the mean of the years. This result appeared to show that the declination correction of heights is proportional to the declination itself, and not to its square. But the Bristol observations give a different result. Thus in 1835 the correction in height (for all the twelve hours together) from decl. 7°·3 to mean decl. 18°·6 was 192 parts; and from 18°·6 to 24°·4 it was 173 parts. This proportion of 192 to 173 is very nearly agreeable to the proportion of the squares of the declinations; indeed it is a still higher ratio; for the proportion of the squares gives 192 to 164, while the simple ratio of the declination would give 192 to 98. In 1836 the correction in height from decl. 8° to the mean 19°·7 was 227 parts; and from 19°·7 to 25° it was 138. The law of the squares would require the proportion 227 to 166, the law of the simple ratio, 227 to 103. In this case the result is nearly intermediate between the square and the simple ratio, but the mean of the two years gives the law of the square of the declination very nearly. And this result, agreeing with the London mean of nineteen years, may be considered as sufficiently established;

* Researches, Second Series, Philosophical Transactions, 1834, pp. 24. 32.

the occasional deviations in short series arising from the irregularity of the phenomena, and from the imperfection of the other corrections.

X. *Does the declination correction of times vary as the square of the declination?*

The Bristol observations give

1835.	Total time correction from decl.	$7^\circ{\cdot}3$ to $18^\circ{\cdot}6$;	81 parts
	from —	$18^\circ{\cdot}6$ to $24^\circ{\cdot}4$;	75 —

The ratio of the squares is 81 to 69; the simple ratio is 81 to 41.

1836.	Total time correction from decl.	8° to $19^\circ{\cdot}7$;	76 parts
	from —	$19^\circ{\cdot}7$ to 25;	46 —

The ratio of the squares is 76 to 56; the simple ratio is 76 to 34. In both these cases the actual result is in a ratio considerably higher than the simple ratio; and the mean approaches near to the ratio of the square.

The Plymouth tides of 1835, 1836, 1837, were discussed in a different manner, the declination corrections for each hour being kept separate. The effects due to declination being thrown into curves according to the degrees of declination, the form of these curves from 2^h to 6^h transit appeared to show very clearly, that at those hours the correction is as the square of the declination; while for the other hours of transit the correction is small and irregular. It has already been shown that the declination correction of times is of the form $(\sin^2 \delta - \sin^2 \Delta)\ (c + d\,.\,s,\ 2\,\varphi - 2\,\beta)$, and the form of the irregularity appears to indicate that β is smaller for the high than for the low declinations. It may perhaps be possible hereafter to put this correction in a form which shall escape this irregularity.

XI. *Can the laws of the corrections be deduced from a single year?*

Whether the observations of a single year, or of a few years, suffice for determining the tide corrections, is a question which the discussions just described enable us to answer. For if the curves representing the laws of these corrections be regular for each year and similar in successive years, we cannot doubt that the law is given with some approximation by each, although the mean may be still more accurate. On examining our curves with this view we find that most of the corrections are given with tolerable precision, even by a single year's observations. The semimenstrual inequality may be determined from that period, and gives a very regular curve if the observations are good. It may be still improved by reducing each hour to the parallax and declination which is the mean of the whole, as I shall further explain. The parallax correction from a single year is less regular, as might be expected, the observations for each hour of transit being further distributed to each minute of parallax, so that each resulting number is the mean of a few observations only. Still the general form of the parallax correction curve for one year exhibits the features which belong to the mean; namely, a maximum and minimum about 4^h and 8^h both for time and height. And when the process is conducted with care, and the curves which represent the corrections adjusted nearer and nearer to the observations, in the manner which the discussion itself suggests, the results may be rendered far more exact than would have been conceived possible. This was done by Mr. Bunt in the discussions of the Bristol tides for 1836 and 1837, and the result is that the form of the parallax correction deduced from the observations above, and from those below the mean, is almost identical. Without using any arbitrary improvement of the curves, they are absolutely symmetrical above and below the axis. The same is the case with the curves which express the corrections for high and for low declinations. The above methods applied to a single year appear to give as good forms of the correction curves as were at first obtained from the whole mass of nineteen years' observations. It is clearly shown by our researches, that a series of five years will give, with great regularity, the laws and amounts of all the corrections, especially if we attend to what we now know of their general form, and of the cycles of longer period by which they are affected.

XII. *Are there any regular differences between the corrections of successive years?*

The mean parallax of one year is very nearly the same as the mean parallax of another year*. But the case is different with the declination. The inclination of the moon's orbit continues nearly the same during one year, but varies from year to year in consequence of the revolution of the nodes. When the ascending node of the moon's orbit is in the first point of Aries, the inclination of the orbit to the equator is the sum of its inclination to the ecliptic, and of the obliquity of the ecliptic. When the descending node is at the first point of Aries, the inclination to the equator is the difference of the other two. In this manner the inclination of the moon's orbit to the equator may vary from about $18\frac{1}{2}°$, which value it had in 1829 and 1830, to $28\frac{1}{2}°$, which is its value in the present year 1838. Consequently the mean declination of the moon for different years will be different, and the semimenstrual curve obtained by taking the mean of the year will correspond to different declinations in different years. The mean declination for 1834, obtained by taking the correction to be proportional to the square, is, for 1834, 17°·2; for 1835, it is 18°·6; for 1836 it is 19°·7; for 1837 it is 20°·2. Hence on this account alone the semimenstrual curve of 1837 and 1834 would differ. For instance, for 8^h transit, the difference of effect of declination 17°·2 and 20°·2 is 6^m or 8^m; and hence the semimenstrual curves will deviate from each other by such a quantity. Accordingly this feature appears in the results of the Bristol observations; and we have in this fact a very remarkable evidence that the effects of declination are discoverable in the tides of each year.

The Bristol observations for 1834, 1835, 1836, and 1837 being reduced, according to this view, to a common parallax and declination, give the following results, from which the agreement of different years may be judged of.

* The mean parallax of the different hours of transit is not the same: the greatest parallax occurring near the syzygies, in consequence of the moon's variation, as I have already observed. Hence the semimenstrual curve obtained by taking the mean of each hour of transit, requires a correction for parallax to reduce it to the general mean.

Hour of Transit	h m 0 30	h m 1 30	h m 2 30	h m 3 30	h m 4 30	h m 5 30	h m 6 30	h m 7 30	h m 8 30	h m 9 30	h m 10 30	h m 11 30
Times. Semimenstrual Inequality. Mean Parallax 57′·2. Mean Declination 19°.												
Interval.	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m
1834. (Mean Interval $43^h\ 56^m$)	44 3	43 47	43 30	43 15	43 8	43 21	43 57	44 34	44 44	44 42	44 34	44 19
1835. (Mean Interval $43^h\ 36^m$)	44 8	43 48	43 29	43 13	43 7	43 21	44 2	44 34	44 44	44 42	44 33	44 21
1836. (Mean Interval $43^h\ 56^m$)	44 4	43 45	43 28	43 15	43 6	43 19	44 4	44 36	44 47	44 44	44 35	44 22
1837. (Mean Interval $43^h\ 56^m$)	44 7	43 47	43 29	43 13	43 5	43 20	44 2	44 35	44 47	44 45	44 37	44 24
Mean of 1834–7	44 5·5	43 46·7	43 29	43 14	43 6·5	43 20·2	44 1·2	44 34·7	44 45·5	44 43·2	44 34·7	44 21·5
Greatest Difference	2·5	1·7	1·0	1·0	1·5	1·2	4·2	1·3	1·5	1·8	2·3	2·5
Heights. Semimenstrual Inequality. Mean Parallax 57′·2. Mean Declination 19°.												
	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.
1834.	31·6	31·0	29·3	26·9	24·3	22·1	22·3	25·0	27·6	29·6	31·0	31·7
1835.	31·7	30·7	28·8	26·5	24·0	22·2	22·8	25·3	27·8	29·7	31·1	31·7
1836.	31·5	30·7	29·1	26·8	24·1	22·0	22·5	25·0	27·6	29·6	31·0	31·6
1837.	31·7	30·7	29·0	26·8	24·2	22·0	22·3	24·8	27·4	29·8	31·2	31·8
Mean of 1834–7	31·6	30·8	29·0	26·8	24·1	22·1	22·4	25·0	27·6	29·7	31·1	31·7
Greatest Difference	0·14	0·25	0·22	0·21	0·14	0·09	0·32	0·03	0·20	0·11	0·10	0·13

It appears from these Tables that the greatest difference of the semimenstrual inequality of time in any one year from the mean of the four years is $4\frac{1}{5}$ minutes; and the greatest difference of height in the same manner is about 4 inches. It is to be recollected that this is the error in a mean tide of 33 feet.

XIII. *Do the corrections at different places agree in laws and amount?*

The amount of agreement in this respect will be seen by the following statement.

The semimenstrual inequality in time, although according to theory it is determined by the proportion of the force of the moon to that of the sun, is by the observations different at different places. The total amount of this inequality, that is, the difference of the greatest and least lunitidal intervals, is 95 minutes at Plymouth, and 93 minutes at Bristol. But these are greater than the values which this inequality assumes at any other places. It is 90^m at London and at Sheerness; 86^m at Liverpool and Howth; 84^m at Leith; 83^m at Portsmouth and at Pembroke; 82^m at Ramsgate; and only 80^m at Brest. In each of these cases the value is determined from observations so numerous as to be certain within a minute or two. We see, therefore, how different the mass of the moon would be found to be by calculations from the tide observations of different places.

The comparison of the semimenstrual inequality of height at different places gives a similar result. In this case, however, we must not take the actual amount of the inequality, but its proportion to the mean tide. At Portsmouth the mean range of the tide is $12\frac{1}{2}$ feet; the total semimenstrual inequality, or difference of the height of high water at neap and spring tides, is $2\frac{1}{2}$ feet; that is, only $\frac{1}{5}$ of the mean tide. But at Plymouth, where the mean tide is also $12\frac{1}{2}$ feet, the total semimenstrual inequality is 3·4 feet, and thus the fraction is $\frac{1}{3·4}$. And at Bristol, where the mean range of tide is 33 feet, the semimenstrual inequality is 10 feet, or $\frac{1}{3·3}$ of the mean tide.

The parallax corrections of time are as follows (for 60′), in minutes and decimals:

London	$-\overset{m}{4 \cdot 6} - \overset{m}{8 \cdot 8}$	$. s, 2 \phi - \overset{h}{14}$;	
Liverpool...	$2 \cdot 5 - 7 \cdot 3$	$. s, 2 \phi - 13$;	
Plymouth...	$7 \cdot 2 - 9 \cdot 1$	$. s, 2 \phi - 14$;	
Bristol,	1834,	$10 \cdot 7 - 12 \cdot 3 \,. s, 2 \phi - 14$;	
	1835,	$8 \cdot 5 - 10 \cdot 5 \,. s, 2 \phi - 14$;	
	1836,	$6 - 9 \cdot \,. s, 2 \phi - 13$;	
	1837,	$11 - 12 \cdot \,. s, 2 \phi - 13$.	

The parallax corrections of height are as follows, in decimals of feet (for 60′):

London		$\cdot 75 + \cdot 23 \,. s, 2 \phi - \overset{h}{18}$;
Liverpool...		$1 \cdot 35 + \cdot 09 \,. s, 2 \phi - 18$;
Plymouth...		$\cdot 57 + \cdot 09 \,. s, 2 \phi - 14$;
Bristol,	1834,	$2 \cdot 35 + \cdot 65 \,. s, 2 \phi - 15$;
	1835,	$2 \cdot 1 + \cdot 60 \,. s, 2 \phi - 16$;
	1836,	$1 \cdot 85 + \cdot 65 \,. s, 2 \phi - 14$;
	1837,	$2 \cdot 1 + \cdot 50 \,. s, 2 \phi - 13\frac{1}{2}$.

The constant part, which is here much the most considerable, is nearly in the proportion of the mean tide.

The declination corrections of time are (for 7°),

Plymouth...		$\overset{m}{3 \cdot 5} - \overset{m}{6 \cdot 5} \,. s, 2 \phi - \overset{h}{12\frac{1}{2}}$;
Bristol,	1834,	$-0 \cdot 5 - 5 \cdot 5 \,. s, 2 \phi - 12\frac{1}{2}$;
	1835,	$1 \cdot 5 - 7 \cdot 5 \,. s, 2 \phi - 13\frac{1}{4}$;
	1836,	$1 \cdot 5 - 7 \cdot 5 \,. s, 2 \phi - 13\frac{3}{4}$;
	1837,	$3 \cdot 3 - 7 \cdot 7 \,. s, 2 \phi - 12\frac{3}{4}$.

The declination corrections of height are (for 7°),

Plymouth...		$\cdot 28 + \cdot 15 \,. s, 2 \phi - \overset{h}{9}$;
Bristol,	1834,	$\cdot 9 + \cdot 6 \,. s, 2 \phi - 14$;
	1835,	$1 \cdot 1 + \cdot 3 \,. s, 2 \phi - 15\frac{1}{4}$;
	1836,	$1 \cdot 5 + \cdot 7 \,. s, 2 \phi - 15\frac{3}{4}$;
	1837,	$1 \cdot 3 + \cdot 5 \,. s, 2 \phi - 15\frac{1}{2}$.

I do not compare with these the declination corrections for London and Liverpool, because they are calculated from a different mean declination (15°), and therefore a difference might not prove a real discrepancy. For the same reason the Bristol results for different years are not to be expected to agree exactly.

The mean declination of the moon, about which the effects balance each other in an entire revolution of her nodes, appears by calculation to be about $16\frac{1}{2}°$. It is this declination which should, in the Tables, have the correction 0; and the semimenstrual inequality of all years must be taken for this mean declination, in order to make the correction tables general. I have not yet reduced the Bristol corrections to this basis, having a hope of still further improving the declination correction; and also of obtaining a solar correction from the observations. These tasks, however, must be reserved for a future occasion, if I am able to execute them at all. I hope the present attempt to solve some of the most obvious and important questions of Tidology will not be without its value.

Trinity College, Cambridge,
June 5, 1838.