The Tides

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Anderson, James
Some observations on the peculiarity of the tides ...

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XV. *Some Observations on the peculiarity of the Tides between Fairleigh and the North Foreland; with an explanation of the supposed meeting of the Tides near Dungeness. By* James Anderson, *Captain in the Royal Navy. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. G. C. B. P. R. S.*

Read March 25, 1819.

HAVING observed that several Charts and Books of Navigation assert, that the tides from the North Sea and the Channel, or the Eastern and Western tides, meet in the vicinity of Dungeness and Rye harbour; and that, on such authority, this opinion has been too generally adopted by those, who have not had the opportunity or the inclination of making personal observations; as well as by the pilots on this part of the coast, who from being incapable, for the most part, of making observations or deducing inferences from facts before them, readily embrace the first theory they meet with *in print*, however erroneous or inconsistent; I have been induced to bestow all the attention in my power to the phænomena of the tides between Fairleigh and the North Foreland, and now venture to submit the result of my observations to the notice of the Royal Society. From having cruised constantly within these limits for nearly two years and a half, I have had many opportunities of making observations; but I must, nevertheless, profess my readiness to admit any alteration or improvement which may be pointed out by those more conversant with the subject; truth alone being the object of my enquiries.

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I. *Phænomena of the Tides between Fairleigh and the North Foreland on the English coast, and Cape D'Alprèe and Calais on the French coast.**

The tides rise between the easternmost point of Fairleigh and the North Foreland from seven to eight feet higher than on either side of these points; and during the last three hours and a quarter in which the tides run to the eastward, the water *falls by the shore*, making it half tide of ebb on the shore, or by the ground, when the current of the tide changes and begins to run from the eastward to the westward; and it still continues to fall by the shore for two hours and three quarters after the tide has so changed; at which time it is *low water* every where within these limits. The course of the tide continues to run to the westward two hours and three quarters longer, during which time the water gradually rises by the shore, making nearly *half flood* by the land, at the time the current of the tide ceases to run to the westward; and returns again to the eastward, and continues to rise for three hours and a quarter, when it is high water by the ground. It then begins to fall again during the last three hours and a quarter, whilst the current of the tide sets to the eastward, as above stated; and so on in continual rotation.†

These appearances have, no doubt, given rise to the

* In the detail I shall principally confine myself to the English coast, as the phænomena, and their causes and effects, on the French coast, within the same limits, are precisely similar.

† Every one who has attended to the tides, in general, knows that, where there are no local obstacles to prevent it, they flow regularly about six hours in one direction and then make high water, and ebb about the same time in a contrary direction, then making low water.

erroneous opinion that the tides meet between Dungeness Point and Rye Harbour; but the real cause of these states of the tides within the particular limits I have described, seems to me to be the very great and sudden contraction of the channel between Dungeness and Cape D'Alprèe, and the South Foreland and Calais Point. In that part it becomes, all at once, narrower by more than half of its width to the eastward or westward of these points. Dungeness is a long narrow point which projects from Winchelsea on the west side, and Hythe on the east side, to the extent of nine or ten miles, at least, directly into the sea across the channel; and forms two deep bays, one on each side. Opposite to Dungeness is Cape D'Alprèe on the French coast, jutting out also into the sea, so as to contract the distance between it and the opposite point to about twenty-four miles, and this cape has also a large bay on each side, of which Boulogne bay is to the eastward. (See the charts.)

The distance from the South Foreland to Calais is only eighteen or nineteen miles, and between these opposite shores lie the Ridge, the Varn, the Goodwin, and several other shoals on both sides of the channel, all of which serve to contract this strait still more. The western tide, therefore, coming up the English channel, meets with a resistance to its course at Dungeness and Cape D'Alprèe, by the very sudden contraction of the space between these points; where, from the passage being insufficient to discharge the quantity of water brought from the westward, it must necessarily accumulate, until it encreases the channel both by deepening and widening it, so as to become adequate to the discharge of the body of water supplied by the impulse of the tide.

This accumulation is of course the same every where within the straits of Dover (from Dungeness to a ridge of rocks to the eastward of the South Foreland), and also extends some distance without them, as far as the easternmost point of Fairleigh on the one side, and the North Foreland on the other; Dungeness west bay, and the Downs, forming as it were two large natural basons or reservoirs at each extremity of these straits, for the reception of the accumulated water, until it can find a passage. On this account the water must rise accordingly by the ground, or on the shore, during the time of this accumulation, wherever it takes place; and it is indeed found to be at its greatest height or making *high water by the ground*, at about three hours and a quarter after the tide of flood has run from the westward. At this time all the sands without the North Foreland are covered, and afford a greater vent for the discharge of the accumulated water. The extensive flats also on both sides of the channel, on which the sea now flows in like a torrent, demand a greater supply than is received through the Dungeness passage. From this period then (viz. three hours and a quarter after the regular flood tide has run to the eastward), the water is drawn off from the places where it had accumulated, and begins to fall gradually by the shore during the remaining three hours and a quarter, in which the current of the tide runs to the eastward; making *half tide of ebb* by the ground, within the straits of Dover and the two reservoirs or basons, when the current of the tide ceases to run to the eastward; at which time it is *high water* every where without these limits, allowing for the inequalities of the coast, the water having now generally acquired a level.

When it is high water without the North Foreland, as at Margate, the Kentish Knock, &c., and the tide, which is the true or regular ebb tide, returns to the westward through the Downs, the water still continues to fall within the Foreland, and on to the easternmost point of Fairleigh, for two hours and three quarters of the first of the true or regular ebb tide; because the tide is falling generally, and the passage by Dungeness discharges the quantity brought by the ebb tide during that time. But when the true or regular ebb tide has run two hours and three quarters, it is *low water* by the shore, between the North Foreland and Fairleigh; because the channel through the straits of Dover, (becoming again too contracted to give vent to the great body of water which now presses forward from the Medway and the North Sea, augmented by the currents and tides discharged from the great continental rivers and inlets,) now again accumulates in the narrow passage, and in the Downs, from the North Foreland, and thus begins, from the above stated period, to rise by the shore.

It thus continues to rise for the remaining two hours and three quarters, at which time the true or regular ebb tide has ceased to run to the westward, and it is *low water* every where without the North Foreland, and to the westward of Fairleigh. But within these limits (viz. between the North Foreland and Fairleigh), it is *half flood,* in consequence of the accumulation of the water during the latter part of the ebb tide. The true or regular ebb tide, or tide *to* the westward, now ceases to run, and the true or regular flood tide *from* the westward returns, bringing with it a greater quantity or body of water than the Dungeness passage can yet admit,

consequently it must accumulate in Dungeness west bay, and rises proportionally from thence along the shore to the North Foreland for three hours and a quarter of the first of the regular flood tide, because the tide is rising generally every where; and about this time the channel, becoming broader and deeper by the accumulation of the water and rising of the tide, is again sufficiently large to discharge the supply. The accumulated water being thus drawn off, as before mentioned, and with an accelerated current, to cover the flats and fill up the Medway, and the continental rivers, again begins to subside by the shore, at which precise period it is *high water* by the ground within the limits of accumulation, both on the English and French coasts; but without these limits it is only *half tide of flood;* and therefore the true or regular flood tide must run three hours and a quarter longer to the eastward, during which time the water falls by the shore, within the limits of accumulation, until it finds its level every where; and so rises and falls in perpetual rotation.

The tide, within the limits where the water accumulates, is found to rise from 28 feet to 30 feet perpendicularly, which is from 7 to 8 feet higher than it generally rises in the Channel. The following seem to be causes of this extraordinary rise. At half tide by the shore, within these limits, the water has found its level every where, and half the rise of the tide here, at high water (viz. 14 or 15 feet), being drained off to make high water without the North Foreland, and produce the level, is now contained in a space twice the breadth it formerly occupied; of course it follows, the same quantity of water will only be half the depth (or from 7 to 8 feet) that

it was when confined in half the space it now occupies. It may hence be inferred that the rise of the tide here, more than elsewhere, is nearly equal to about one quarter the rise of tide, whatever it may be; but as this must always depend upon local circumstances, as the same effects could not be produced if the situation was different, no general reasoning can apply. It has also been ascertained, that the true or regular flood tide runs six hours and an half to the eastward, while the true or regular ebb runs only five hours and an half to the westward; which makes the current of the tide run an hour longer to the eastward than the westward; but I have always found, from actual observation, that these tides are very much influenced by the winds.

Upon the whole, however, from the easternmost point of Fairleigh to the North Foreland, on both sides of the channel, it is always *high water by the ground*, when the true or regular flood tide has run three hours and a quarter from the westward; always *half ebb by the ground*, when the true or regular flood tide ceases to run from the westward; always *low water*, when the true or regular ebb tide has run two hours and three quarters from the eastward; always *half flood tide by the ground*, when the true or regular ebb tide ceases to run from the eastward; and always *high water by the ground again*, when the true or regular flood tide has run three hours and a quarter from the westward, or nearly so, and so on continually.

II. *Meeting of the Tides near Dungeness.*

Although the foregoing observations may not decidedly prove that *the meeting of the tides* cannot take place at or near

Dungeness, yet I trust, that they rationally and intelligibly account for the peculiar phænomena of the tides which occur there, without attributing them to the *meeting* of the tides, which could never produce these appearances. But if the tides do meet at Dungeness, they must meet in a line directly across the channel; for it is a fact so well established, that no one I believe has ever ventured to contradict it, that the western or true flood tide makes high water at Beachy Head, Fairleigh, Dungeness, and Deal, at nearly the same time as at Dieppe, the Soame, Boulogne, and Calais, the opposite points on the French coast, each to each; and that the eastern or regular ebb tide makes low water, at the same time, at the same places.

But if the tides met in a line across the channel, it must be evident to every one who has been at sea, that such a meeting would occasion so tumultuous a war of elements, between two large bodies of water impelled against one another, by the current of the tide and force of the winds, at a velocity of from four to six miles an hour, according to the age of the moon and strength of the wind, as would produce, from their furious and violent concussion, so great a sea, that no ship could venture to encounter it without the most imminent danger.

That this is not the case, experience daily proves; and therefore the absurdity and fallacy of the doctrine which asserts it, are obvious. But every master of a collier or coasting vessel, trading from the northward to any western port, as Portsmouth, Plymouth, &c., knows that the flood tide sets from the northward and eastward, along the English coast until he gets as far as the sand called the Kentish Knock:

and if he can reach it by high water, he calculates rightly, that he will have an ebb tide thence, which will carry him to the westward for nearly six hours longer. From this it is evident that the tides from the northward and eastward, and southward and westward, both meet at the Kentish Knock,* as they both make high water about the same time at the same point; and then the ebb tide recedes from this point in the opposite directions to which the flood had advanced. The formation of the coast too, by gradually altering the course of the flood tide between the South Foreland and Buoy of the Nore, from E. N. E. to W. N. W. within the stream of the Goodwin Sands (while without this sand it continues to run E. N. E. and easterly), in a great measure prepares for their meeting, without that wild commotion and furious contention which their coming together in a directly opposite line across the channel, would inevitably occasion. It also admits of their gently blending their waters together, and smoothly taking the same course, along both sides of the Long Sand, &c. the one, viz. the flood tide from the eastward up the King's Channel into the Thames, and the other (the flood tide from the westward through the Downs) up the Queen's Channel into the Medway, making only a strong eddy or whirlpool about the Knock, and a foamy rippling where they meet, as they proceed onwards together.

But, although the tide from the northward and eastward makes flood tide along the N. E. coast of England to the Kentish Knock, yet it is equally well ascertained, that the tide from the southward and westward makes flood tide along the opposite coasts of Flanders, Holland and Jutland,

* This sand is of a circular shape, so formed by the continually whirling eddy of the tides.

as far as the entrance of the Sleeve. From this last mentioned fact it evidently appears, that the flood tide from the westward forms two distinct branches at the Kentish Knock, taking different directions; the smaller of which, consisting of the stream of the tide *within* the Goodwin Sands, takes its course W. N. W. up the Queen's Channel, as before stated; whilst the larger, consisting of the stream of tide *without* the Goodwin, continues its course E. N. E. and easterly along the Flemish and Dutch coasts, until it is lost near the entrance of the Sleeve, in the great body of tide from the northward and eastward.

The opposite tides which meet in the North Sea do not meet in a line directly across any part of it, but in a *diagonal line*, extending from the Kentish Knock to the entrance of the Sleeve; where there is no tide, but a strong current, which almost always sets from the Jutland to the Norway side in the Sleeve; and which most probably proceeds from the eddy, produced by the great body of water coming round the Naze of Norway, meeting the remains of the western tide, aided by the reaction of the Jutland shore. In fact, there is hardly any tide observable between the Horne reef and the entrance of the Sleeve.

The tides thus meeting in a diagonal line in the North Sea, gently and gradually blend their waters together, without causing the least tumultuous appearance, exhibiting merely a little foamy rippling, which can be discerned in fine weather only, when the general mass of water is perfectly smooth.

To prove farther that this is the nature of the tides in this part of the North Sea, let a ship, for instance, sail from North Yarmouth, or Harwich, for the Texel or Flushing on

the opposite coast, with the wind from the north-eastward, so that she can lay her course on the *larboard tack*; the pilot will prefer getting under weigh at *high water*, on the English coast, to take the ebb tide under his lee; and if he can get half channel over during the ebb tide, or by the time of low water on the English side, he will find a flood tide from thence, setting along the opposite coast, which will also set under his lee for six hours longer, running in the same direction as the ebb tide did on the English side of the channel; and thus he will carry twelve hours tide with him; whereas had he continued on either side, he would have had a regular six hours tide each way; with this difference, that he would always have the tide setting in opposite directions on the one side, to what it would be on the other. That is; if the flood tide was setting to the *westward* on the English side, the flood tide would, at the same time set to the *eastward* on the Dutch side. Hence were a ship to sail, as above stated, with the wind so that she could lay her course on the *starboard tack,* she ought to get under weigh at *low water* on the English side, by which she would be able to carry twelve hours tide again under her lee, suppposing her to reach the meeting of the tides at high water.

Every person who has been at Spithead may have observed, that the water rises there, and every where *within* the Isle of Wight, as far as Hurst Castle, for more than three hours after it is high water at the Owers, Dunnose, and every where *without* the Wight, and when the ebb tide has, of course, made to the westward; and that it is not high water at Spithead, Portsmouth Harbour, Southampton River, or any where within the Wight, until the ebb tide has run that time.

This is evidently occasioned by the narrow passage between Hurst Castle and the Island not having sufficient capacity to discharge the quantity of water brought by the ebb tide from the eastward through St. Helens; which therefore meeting with a resistance at Hurst Castle, accumulates and *rises* within the Wight, at the same time filling up Portsmouth Harbour, Southampton River, &c. &c. when the tide is falling every where in the English channel.

This circumstance arises from the same cause which occasions the tides to rise and fall in the Straits of Dover; with this difference, that it is high water by the ground, at the last mentioned place when the *flood tide* has run three hours and a quarter from the westward; but it is high water by the ground at the former, when the *ebb tide* has run about the same time from the eastward. It might therefore as well be asserted that the tides meet at St. Helens, Portsmouth Harbour, or Hurst Castle, as at Dungeness; but the fact is, that the phænomena which appear at these different places, are produced by the same cause producing similar effects, with only the difference occasioned by local circumstances in the time and manner; and this cause is the accumulation of the water brought forward by the tide; an accumulation which is occasioned by there not being a sufficient space for its discharge, in consequence of the contraction of the channel at the particular places where these phænomena are exhibited.

There is in fact a meeting of the tides, on a small scale, within the Wight; for the tide of ebb from Southampton River meets the tide of flood from the Needles, at the sand called Bramble (which has probably been originally formed by their meeting); from this they flow to Spithead, and meet

the tide of ebb from Portsmouth Harbour, at the sand called the Spit (perhaps also formed originally by their meeting there), and causing an eddy tide, which would deposit such sand, mud, &c. &c. as the current of the tide brings along with it: nor do I think it at all improbable that the Long Sand, at which I have stated the meeting of the tides through the Downs and from the North Sea to take place, has been likewise formed by the deposit of such things as the opposing tides brought with them, to the place where they met.

Being employed on the expedition against Walcheren, the laborious and difficult duty of passing the transports through the Slough passage into the West Scheldt devolved upon me, and afforded me an opportunity of observing another peculiarity of the tides in that place.

The Slough passage lies between Walcheren and South Beveland, communicating with the West Scheldt and the Veer Gat. From its junction with the last channel, the tide flows through several different channels between the islands, to the northward of South Beveland. On each side of the channels in the Veer Gat and Slough passage are extensive flats or mud banks, which begin to be covered about half tide of flood, and again begin to be dry about half tide of ebb. The flood flows regularly up the West Scheldt, carrying with it a vast body of water, which takes its course by Rammekins through the Slough passage, and meets the flood tide which flows up the Veer Gat at the north end of South Beveland; whence they flow together through the different channels formed by the adjacent islands. At high water the ebb sets again regularly down the West Scheldt and Veer Gat, but the ebb tide in the Slough continues to run to the northward, the same course as the flood tide, and passes down

the Veer Gat until the flats and mud banks become dry; at which time the current of tide in the Slough changes, and runs to the S. Westward into the West Scheldt by Rammekins; thus making the current of tide run nine hours one way, and only three hours the other. This may be accounted for in the following manner: when it is high water in the Scheldt, and the tide of ebb sets down the river, it sets over the extensive flat between the north-west point of South Beveland and Rammekins into the Slough, until the flat becomes dry, which occasions the tide to continue the same course as before, although the water is falling. But when all the flats become dry, and the water is confined within the proper limits of the respective channels of the West Scheldt, the Veer Gat, and the Slough; and the Veer Gat being then only about 30 yards or less in width, three or four times narrower than the Slough, the water through the Slough cannot any longer find a vent through the Veer Gat, and therefore seeks one by the West Scheldt, where there is sufficient space for it; and hence the tide in the Slough changes and runs out by Rammekins into the Scheldt.

It would scarcely have been important to mention this peculiarity, as it is confined to a very small space, and where vessels of any considerable burthen never perhaps passed before the above mentioned expedition, and never may again; had it not on this occasion presented one of the greatest obstacles,* next to the continual adverse gales, which the transports had to contend with, in getting into the West Scheldt; and which could not have been overcome, but by dint of the most laborious and persevering exertions; and also as it furnishes a

* This obstacle, I confidently believe, was never known to the Commander in Chief, down to this moment.

proof of what I have before advanced, "that local circumstances will always have an effect upon the tides, to which no general reasoning can apply, in all straits and insular situations." These circumstances, however, may readily be acertained by observation and by observation *only*.

J. ANDERSON. CAPT. R.N.

36 Hans Place, 6th February, 1819.

I have annexed a table showing the gradual rising and falling of the tides in Boulogne Bay, from soundings* taken every half hour whilst laying at anchor there, and which I think will greatly tend to confirm what I have advanced, with respect both to the rise and setting of the tides in the Straits of Dover, with the times of high and low water, and of the change of the current of tide there; circumstances which, I have reason to fear, have not been hitherto sufficiently attended to; but which would prove of the utmost importance, especially on expeditions where much boat service must be had recourse to; and in disembarking troops at a particular point, or in making an attack upon vessels at anchor during the darkness of the night; when a want of the necessary knowledge of the tides, or as it has often been called "a mistake in reckoning them," might be productive of the most fatal consequences.†

* As these soundings were taken with a common lead and line, and by different hands, I cannot venture to say that they were taken very accurately; and there might also be some irregularities in the ground, which would occasion a difference; and besides, they were taken 6 or 7 miles from the shore, where the tides do not rise quite so high as on the shore, owing to the re-action of the ground.

† A mistake in calculating the tide at this very place is mentioned by Lord NELSON, as a reason why the boats sent in by him to attack the French flotilla in Boulogne Bay, in 1801, did not get up with the enemy till long after the appointed time.

REFERENCES TO THE TABLE.

The first column contains the month, and day of the month.

The second column contains the wind and weather.

The third column contains the time the soundings were taken:

And the fourth contains the soundings, and the time the ship *tended,* or turned round with the tide. T. E. signifies tended to the eastward or to the flood. T. W. signifies tended to the westward or to the ebb tide.

Soundings taken in Boulogne Bay, at anchor, on 31st July, 1st, 2d, 3d and 4th August, 1811, on Board His Majesty's Sloop Rinaldo, Capt. ANDERSON.

Month and Days.	Wind and Weather.	Time. H.	Time. M.	Soundings in fathoms.
July 31, 1811. P.M.	Tended to E^d.	3	P.M.	13¼
		3	30	T. 13¾
	Fresh Breezes. NE.bN.	4	—	14½
		4	30	14¾
		5	—	15¼
		5	30	16¼
		6	—	16¾
		6	30	17
		7	—	H.W. 17½
		7	30	17
		8	—	17
		8	30	16¾
		9	—	16½
		9	30	Wd.T. 16
		10	—	15¾
		10	30	15½
		11	—	15¼
		11	30	15
		Midnight		14¾
August 1, 1811. A.M.	Fresh Breezes. NE.bN.	A.M.	30	14¼
		1	—	14
		1	30	L.W. 13¾
		2	—	14
		2	30	14¼
		3	—	14½
		3	30	14¾
		4	—	15
		4	30	Ed.T. 15¼
		5	—	15¾
		5	30	16¼
		6	—	16¾
		6	30	17¾
		7	—	H.W. 17½
		7	30	17½
		8	—	17¼
		8	30	17¼
		9	—	17¼
		9	30	17¼
		10	—	17
		10	30	Wd.T. 16½
		11	—	15¾
		11	30	15¼
		Noon		14¾
		P.M.	30	14½
		1	—	14¾
		1	30	14½
		2	—	L.W. 14½
		2	30	14½
		3	—	14½
		3	30	14½
		4	—	14¾
		4	30	14¾
		5	—	T.Ed. 15¼
		5	30	15½
		6	—	15¾
		6	30	16¼
		7	—	17
		7	30	17¼

Month and Days.	Wind and Weather.	Time. H.	Time. M.	Soundings in fathoms.
August 2, 1811.	Calm.	8	—	17¼
		8	30	17¼
		9	—	17¼
		9	30	H.W. 17½
		10	—	17¼
		10	30	17
		11	—	16½
		11	30	16
		Midnight		15½
		A.M.	30	T.Wd. 14¾
		1	—	14½
		1	30	14
		2	—	14
		2	30	14
		3	—	L.W. 14
		3	30	14
		4	—	14
		4	30	14¼
		5	—	14¾
		5	30	15
		6	—	15½
		6	30	T.Ed. 16
		7	—	16½
		7	30	17
		8	—	17½
		8	30	17¾
		9	—	17¾
		9	30	18
		10	—	H.W. 18
		10	30	18
		11	—	17½
		11	30	17¼
		Noon		16¾
		P.M.	30	16
		1	—	T.Wd. 15½
		1	30	14¾
		2	—	4 ¼
		2	30	14
		3	—	14
		3	30	L.W. 14
		4	—	14
		4	30	14
		5	—	14¼
		5	30	14¾
		6	—	15
		6	30	T.Ed. 15¾
		7	—	16½
		7	30	17
		8	—	17¼
		8	30	17½
		9	—	17½
		9	30	17¾
		10	—	18
		10	30	H.W. 18
		11	—	18
		11	30	17¼
		Midnight		16¾

Month and Days.	Wind and Weather.	Time. H.	Time. M.	Soundings in fathoms.
August 3, 1811.	Light variable airs inclining to calm.	A.M.	30	16¼
		1	—	T.Wd. 16
		1	30	15¾
		2	—	15½
		2	30	15
		3	—	14½
		3	30	14½
		4	—	L.W. 14½
		4	30	14½
		5	—	14½
		5	30	14¾
		6	—	15
		6	30	15½
		7	—	16¼
		7	30	T.Ed. 17
		8	—	17½
		8	30	17¾
		9	—	18
		9	30	18
		10	—	H.W. 18
		10	30	18
		11	—	18
		11	30	17¾
		Noon		17½
		P.M.	30	17
		1	—	16¾
		1	30	15¼
		2	—	T.Wd. 15
		2	30	14¾
		3	—	14½
		3	30	14¼
		4	—	14
		4	30	14
		5	—	L.W. 14
		5	30	14
		6	—	14¼
		6	30	15
		7	—	15½
		7	30	15¾
		8	—	T.Ed. 16
		8	30	16¼
		9	—	16¾
		9	30	17
		10	—	17¼
		10	30	H.W. 17¾
		11	—	17¼
		11	30	17
		Midnight		17
August 4, 1811.	Moderate Breezes, W.	A.M.	30	17
		1	—	17¼
		1	30	17
		2	—	16½
		2	30	15½
		3	—	T.Wd. 15
		3	30	14¾
		4	—	14½
		Weighed.		

Full moon, and the tides not yet at their highest.

XVI. *On the Ova of the different tribes of Opossum and Ornithorhynchus. By Sir* Everard Home, *Bart. V. P. R. S.*

Read March 25, 1819.

Now it is determined that the ova of quadrupeds in general are formed in corpora lutea, and that in all such animals the ova become attached to the uterus, and by this means the fœtus receives its support and increase; we are enabled to ascertain the modes of formation of the ova of the opossum tribes, which from the want of this previous knowledge have not been investigated with the smallest degree of success. This becomes the best apology that can be made for the failure of every former attempt.

In this enquiry it will be found, that the ova of all the animals of these tribes are not formed in the same manner, and that the differences met with, make two distinct links between quadrupeds in general and the ornithorhynchi; these again approach so nearly to the bird, as to complete the links of gradation between the human species and the feathered race, so far at least, as concerns their mode of generation.

The mode of formation of the ova in the kangaroo, constitutes the first link in this beautiful series.

In the kangaroo, Mr. Bauer has found the corpus luteum, similar to that in quadrupeds; it is represented in the annexed drawing. Indeed it is to Mr. Bauer's talents and microscopical observations that we are indebted for all our information upon this subject.

In the kangaroo, the ovum when expelled from the corpus luteum, passes along the Fallopian tube, and as there is a thickening and glandular structure surrounding the portion of the tube next to its termination in the uterus, it is there that the yelk, or something analogous to it, is probably secreted; the ovum with the newly acquired yelk, drops from a pendulous opening into the uterus, in which it receives the albumen. In one specimen the uterus in a pregnant state, came under my observation; but as it was sent from New South Wales, in spirit which had not been timely renewed, the contents of the uterus were reduced to a confused mass, in which only a part of the bones could be made out; enough was however seen, to determine that the ovum of the kangaroo in the uterus has an abundant supply of albumen. There was no attachment whatever between the albumen and the uterus. There are two lateral canals that communicate between the uterus and vagina: these answer the purpose of aerating the fœtus by means of atmospheric air.

As the penis of the male has only one orifice at the point adapted to the os tincæ, the ovum must of necessity be impregnated in the uterus, the structure of the Fallopian tubes, and their mode of terminating in the uterus, rendering it impossible for the semen to pass into them.

The fœtus, as soon as it arrives at a certain size (at which time it in general weighs about 12 grains,) is expelled from the uterus, and is received into the marsupium, where it becomes attached to the point of one of the nipples, at first by simple contact; but as the fœtus grows, the nipple is found farther in the mouth upon the surface of the tongue. In the 85th volume of the Philosophical Transactions, Plate XVIII. XIX. XX. and XXI. and in the 100th volume, Plate XIII. most of

these circumstances are represented, but the uses of the different parts, I readily confess, I was at that time unable accurately to comprehend.

The mode of formation of the ova in the Koli and the Wombat of New South Wales, and in the great and small Opossum of North America, constitutes the second link in this chain of gradation.

In none of these different genera are there corpora lutea in the ovaria, but in their place a certain number of yelk-bags of different sizes; and these are so completely imbedded in the substance of the ovarium, that to common observation they appear to be so many corpora lutea. There is no thickened glandular structure surrounding the Fallopian tubes near their termination in the uterus. Instead of one uterus having two Fallopian tubes, there are two uteri and one tube to each; and in proof that the ovum in each uterus is impregnated separately in its own cavity, the point of the penis in the male is so formed as to throw the semen into both. The lateral tubes, by which the fœtus is aerated in the kangaroo, are formed in these genera in a different manner; there is only one to each uterus, and this, instead of communicating with the uterus at the fundus, opens into it at the cervix. The yelk-bags are shown in the annexed drawing. I have not had an opportunity of examining the ovum of any of these animals in utero, but Mr. BELL, a very intelligent surgeon, transmitted an account to Sir JOSEPH BANKS, of the dissection of a female koli, in which he met with an imperfectly formed embryo in each of the uteri, surrounded by a mass of albumen. The young of all these genera are expelled from the uterus into the marsupium, and become attached to the prominent points of the nipples.

The mode of formation of the ova in the ornithorhynchi, constitutes the intermediate link between that of the American opossum and the bird.

The yelk-bags in the ovaria of the ornithorhynchi are more distinct, and less deeply imbedded than in the opossum: there is no regular uterus, nor Fallopian tubes; the yelk-bags pass along an oviduct, the lower part of which performs the office of a uterus. In this situation the ova are impregnated; the penis of the male, which is bifid, throwing the semen into both oviducts at the same time, through several points like a watering pot, so as to scatter it all over the cavity. The ova are aerated by the vagina. The ova in a magnified state are represented in the annexed drawing. The organs of generation are figured in the 92nd volume, Plate IV. of the Philosophical Transactions. To show that the yelk-bags in the ornithorhynchus resemble those of the pullet, a magnified drawing of them made by Mr. BAUER in that bird, is annexed.

To those members not conversant in comparative anatomy, the following summary may be acceptable.

In the human species, and quadrupeds in general, the ova are formed in corpora lutea, and pass into the uterus, to the sides of which they become attached; when the fœtus is completely formed it is expelled by the vagina, and afterwards sucks the mother.

In the kangaroo the ova are formed in corpora lutea, receive their yelks in the Fallopian tube, and their albumen in the uterus. The ovum thus completed, is impregnated in the uterus, aerated by means of lateral tubes, and when the young is expelled from the uterus, it is received into the marsupium, and attached to the nipple of the mother.

In the American opossum, the yelk bags are formed in the ovaria; pass into the uteri, there receive the albumen, and are then impregnated; the fœtus in each uterus is aerated by one lateral tube. When expelled from these uteri, the young are received into the marsupium, and become attached to the nipples of the mother.

In the ornithorhynchi the yelk-bags are formed in the ovaria; received into the oviducts, in which they acquire the albumen, and are impregnated afterwards; the fœtus is aerated by the vagina, and hatched in the oviduct, after which the young provides for itself, the mother not giving suck.

In the pullet, the yelk-bags are formed in one ovarium, impregnated in one oviduct, and hatched out of the body.

EXPLANATION OF THE PLATES.

PLATE XVI.

Ovarium of the Kangaroo.

Fig. 1. The ovarium of a young kangaroo laid open: natural size; showing the corpus luteum.

Fig. 2. The same section magnified four diameters, to show the corpus luteum more distinctly.

Fig. 3. A section of the corresponding ovarium of the same kangaroo, magnified four diameters; to show an incipient corpus luteum.

Fig. 4. A similar section of the ovarium of an old kangaroo: natural size; in which there are two corpora lutea.

Fig. 5. The same section, magnified four diameters.

Plate XVII.

Ovarium of the American Opossum.

Fig. 1. Front view of the ovarium of the large American opossum: natural size.

Fig. 2. The same view, magnified five diameters.

Fig. 3. Back view, magnified five diameters.

Fig. 4. A perpendicular section, magnified five diameters.

Fig. 5. The same section, magnified ten diameters.

Fig. 6. A young yelk-bag, magnified twenty diameters.

Fig. 7. A full grown yelk-bag, magnified twenty diameters.

Fig. 8. A full grown yelk-bag opened, to show its contents; magnified twenty diameters.

Plate XVIII.

Ovarium of the Ornithorhynchus Paradoxus.

Fig. 1. Front view of the ovarium of the ornithorhynchus paradoxus: natural size.

Fig. 2. The same view, magnified five diameters.

Fig. 3. Back view, magnified five diameters.

Fig. 4. A small portion cut off from the upper end of the left side of Fig. 3.; magnified ten diameters.

Fig. 5. An internal view of the same portion, magnified ten diameters, to show the yelk-bags.

Fig. 6. A full grown yelk-bag, magnified twenty diameters.

Fig. 7. A young yelk-bag opened, to show its contents: magnified twenty diameters.

Fig. 8. A full grown yelk-bag opened, to show its contents: magnified twenty diameters.

Fig. 9. The globules of the yelk diluted with water, magnified four hundred diameters.

PLATE XIX.

Ovarium of the Hen.

Fig. 1. Front view of the ovarium of a hen: natural size.

Fig. 2. A small portion of the same, with some very young yelks: natural size.

Fig. 3. The same small portion, magnified five diameters.

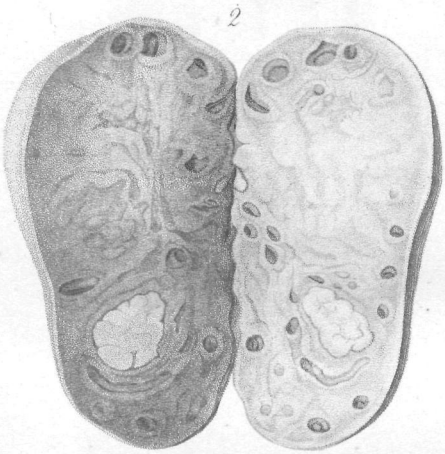
Fig. 4. Back view of the ovarium of the hen: natural size.

Since this Paper was sent to the press, the author has received, through the kindness of Governor MAC QUARRIE and Sir JOHN JEMISON, four specimens of female Ornithorhynchi Paradoxi from New South Wales, and finds in all of them, as well as in every other specimen that has come under his observation, that there are yelk-bags only in the left ovarium, showing that both ovaria are not generally in use at the same time. This is an approach to the bird, in which there is only one, lying on the left side.

In the chick of the common fowl before it is hatched, there is a small portion of an oviduct on the right side, but his disappears before the chick is completely formed.

Fig 1

2



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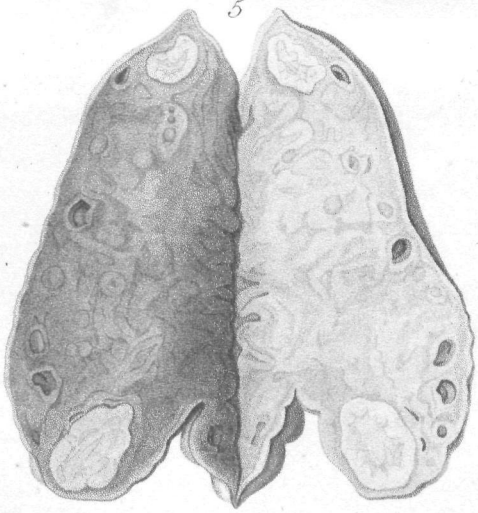
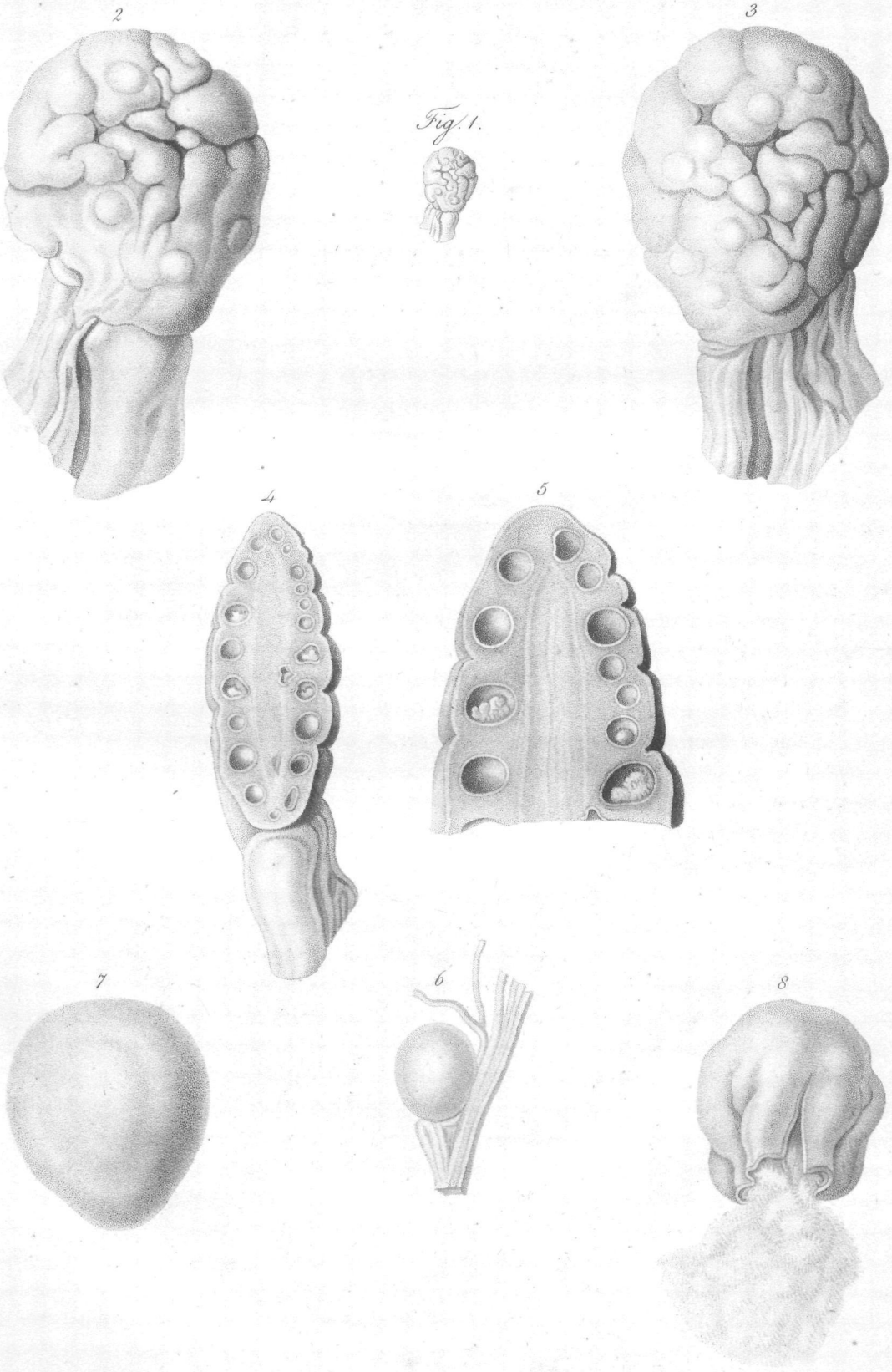


Franz Bauer, del.

Js. Basire, sc.



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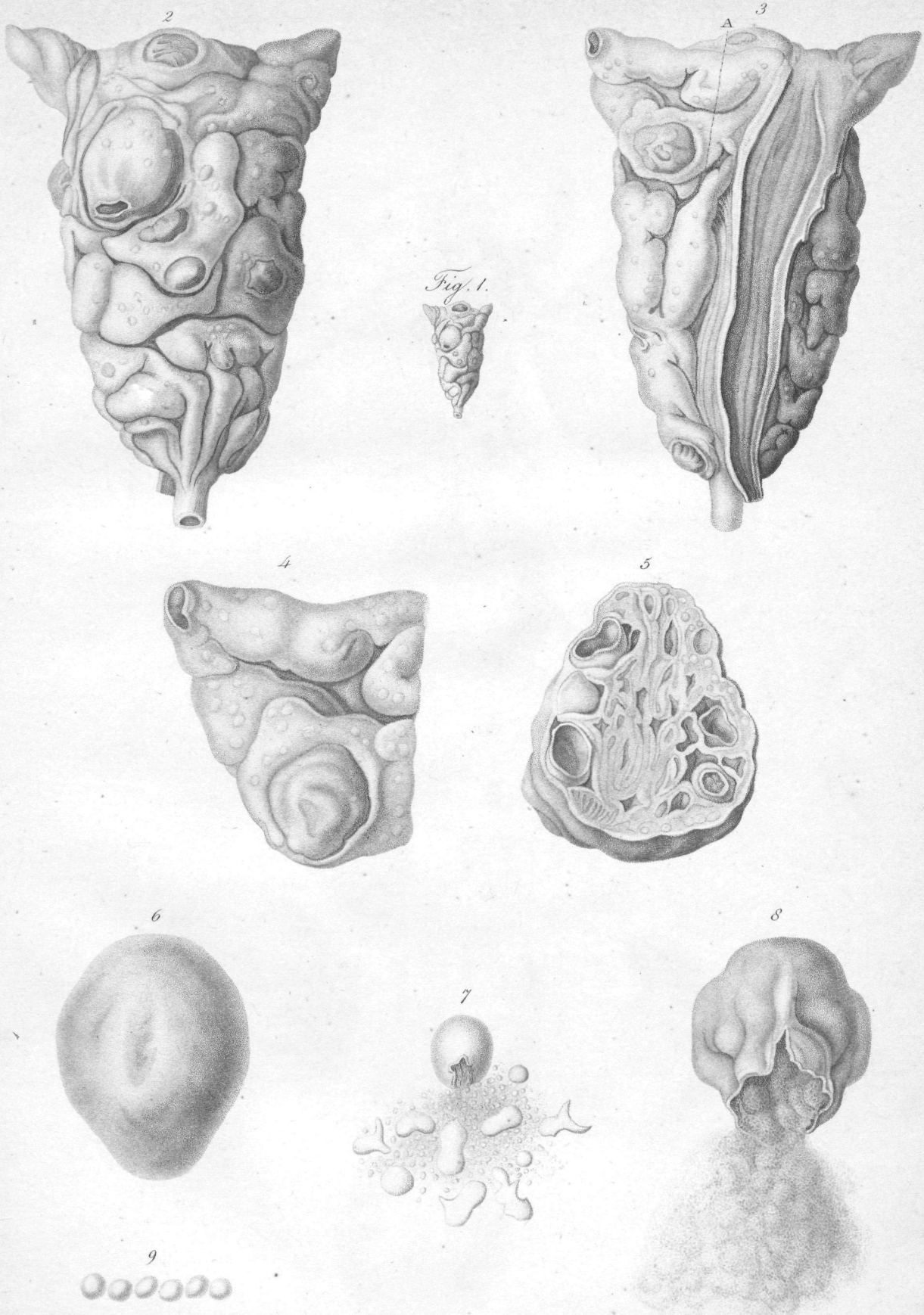

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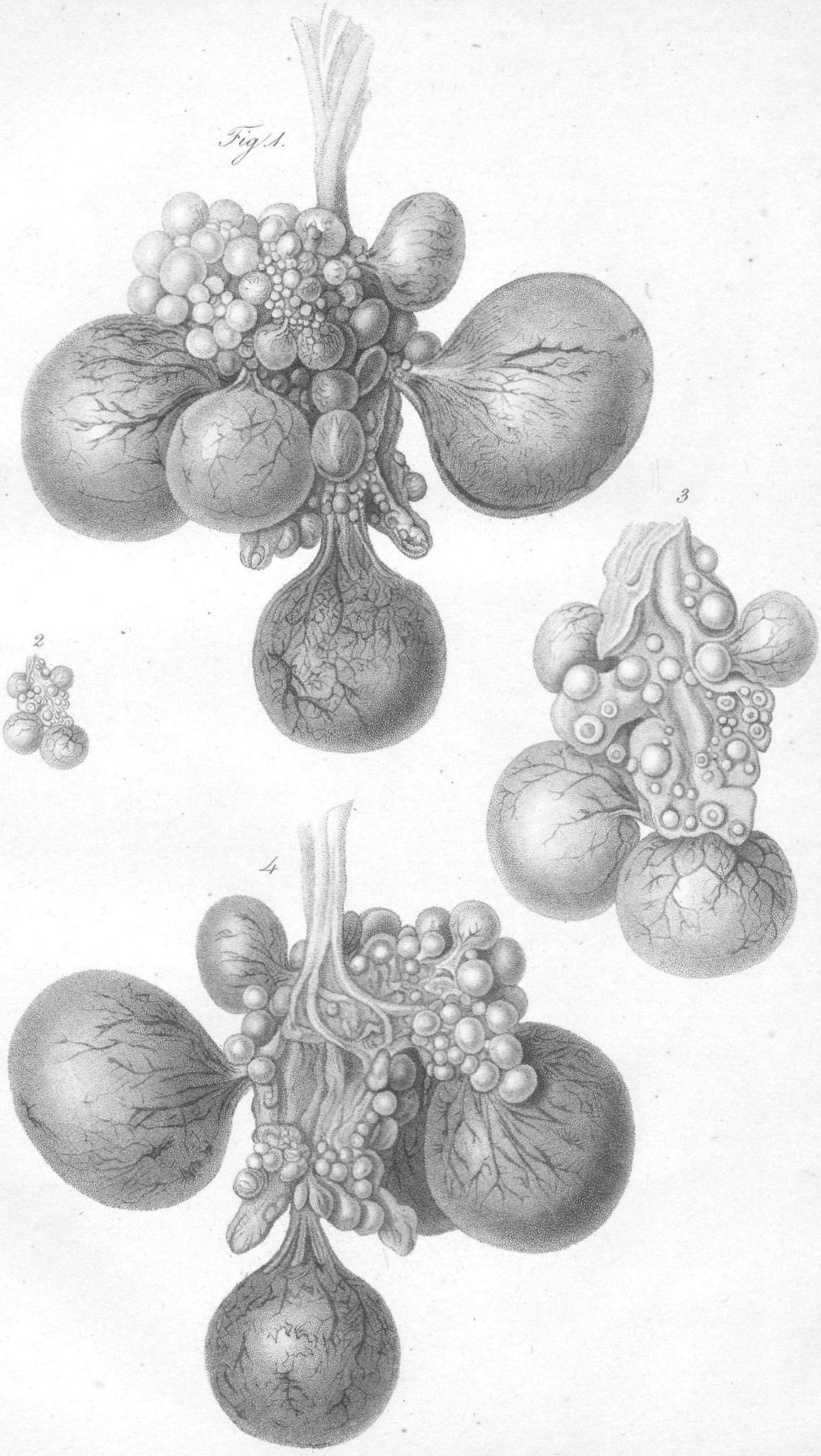

Franz Bauer, del.

J. Basire, sc.

II. *On the Tides.* *By* John William Lubbock, *Esq., V.P. and Treas. R.S.*

Read November 17, 1831.

When I was lately at Paris, M. Bouvard kindly allowed me to copy some of the Observations made at Brest. Since my return to this country, the observations I obtained have been discussed by M. Dessiou, with regard to the principal inequality, or that which is independent of the parallaxes and declinations of the luminaries and depends solely on the moon's age, that is, on the time of her passage through the plane of the meridian.

The result is exhibited in the following Table.

Table showing the interval between the Moon's Transit and the time of High Water at Brest, from the Observations made there in the year 1816.

Time of Moon's Transit.	Interval Observed.	Moon's Transit.	Interval Observed.	Moon's Transit.	Interval Observed.	Moon's Transit.	Interval Observed.
h m	h m	h m	h m	h m	h m	h m	h m
0 0	3 47·8	3 0	3 7·8	6 0	2 52·8	9 0	4 9
0 30	3 43·5	3 30	3 4·9	6 30	3 2·2	9 30	4 9·9
1 0	3 36·6	4 0	2 58·4	7 0	3 18·2	10 0	4 9·5
1 30	3 23	4 30	2 50	7 30	3 33·5	10 30	4 6·9
2 0	3 15·5	5 0	2 48·5	8 0	3 46·4	11 0	4 2·5
2 30	3 10·9	5 30	2 49·5	8 30	4 0	11 30	3 54·6

It appears from this Table, that the establishment of that part of the port of Brest where these observations were made is $3^h\ 48^m$; the Annuaire for 1831 gives $3^h\ 33^m$ for that quantity. The constant $\lambda - \lambda_{,}$ may be taken about $1^h\ 20^m$, or 20° in space, being the value assigned to it by Bernoulli, but differing considerably from its value in the port of London, which is 2^h, or 30° in space. This result is important, as showing, unfortunately, that Tables of the Tides for London are not applicable to Brest by merely changing the establishment, that is, by adding a constant quantity, as has been supposed hitherto. The same remark applies of course generally to any distant ports.

The preceding Table gives also $\frac{m\,\Pi^3}{m_{,}\,\Pi_{,}^3} = \tan 18°\ 45'$ about, the logarithm of

which quantity is 9·5307813. The determination of LAPLACE, by other means, of the same quantity is 9·5385031 = log tan 19° 4′. BERNOULLI took this constant = 9·60286 = log. $\frac{2}{5}$, without explaining how he obtained it. NEWTON determined $\frac{m\,\Pi^3}{m_{,}\,\Pi_{,}^3} = \frac{1}{4{\cdot}4815}$, making the mass of the moon much too great.

19° 4′ — 18° 45′ = 19′ or $1^m\ 16^s$ in time. A difference therefore of about one minute in the sum of the *intervals* when the moon passes the meridian at $4^h\ 20^m$ and at $10^h\ 20^m$ would remove altogether the discrepancy between my determination and that of LAPLACE.

The following TABLE shows the differences between the times of high water calculated by M. DESSIOU with the constants $3^h\ 47^m{\cdot}8$, $1^h\ 20^m$ and 9·5307813, according to the formula Phil. Trans. 1831, p. 387, line 17, and the times given by observation, and also the differences between the times calculated (with the correct establishment) by means of the Table of BERNOULLI given in the Annuaire for 1829, p. 40, and the same times given by observation.

Moon's Transit.	Observed.	Calculated with the constants above.	Error of Calculation.	Calculated from the Table in the Annuaire.	Error of Calculation.
h m	h m	h m	h	h m	m
12 0	3 47·8	3 47·8	0	3 47·8	0
0 30	3 43·5	3 40·7	— 2·8		
1 0	3 36·6	3 33·2	— 3·4		
1 30	3 23	3 25·6	+ 2·6		
2 0	3 15·5	3 18·1	+ 2·6	3 14·3	— 1·2
2 30	3 10·9	3 10·9	0		
3 0	3 7·8	3 4	— 3·8		
3 30	3 4·9	2 58	— 6·9		
4 0	2 58·4	2 53·1	— 5·3	2 45·8	—12·6
4 30	2 50	2 49·7	— 0·3		
5 0	2 48·5	2 48·5	0		
5 30	2 49·5	2 50·1	+ 0·6		
6 0	2 52·8	2 55·3	+ 2·5	2 45·3	— 7·3
6 30	3 2·2	3 4·7	+ 2·5		
7 0	3 18·2	3 18	— 0·2		
7 30	3 33·5	3 33·3	— 0·2		
8 0	3 46·4	3 47·1	+ 0·7	3 50·8	+ 4·4
8 30	4 0	3 58·3	— 1·7		
9 0	4 9	4 4·9	— 4·1		
9 30	4 9·9	4 7·7	— 2·2		
10 0	4 9·5	4 7·3	— 2·2	4 10·8	+ 0·7
10 30	4 6·9	4 4·5	— 2·4		
11 0	4 2·5	4 0·1	— 2·4		
11 30	3 54·6	3 54·4	— 0·2		

The agreement so far between theory and observation is not less remarkable than that at the London Docks which I have before noticed, (see Phil. Trans. 1831, p. 388). The irregularities in the errors given in the fourth column arise from the paucity of the observations employed.

As it would be of great importance to predict, if possible, any remarkably high tides which might take place, in order that precautions might be taken to avoid any disastrous consequences, I requested M. Dessiou to calculate, from the Tables in the Companion to the British Almanac * for 1831, the times and heights of high water at the London Docks corresponding to some remarkably high tides which have been observed, in order to see how nearly those Tables can be depended upon in extreme cases.

The following Table exhibits the results he obtained.

	Time of High Water.		Height of High Water.		Direction of the Wind.
	Observed.	Calculated.	Observed.	Calculated.	
	h m	h m	ft. in.	ft. in.	
1812. Oct. 21.	2 0 A.M.	2 10 A.M.	25 1	23 7½	NW
	2 10 P.M.	2 30 P.M.	25 8	23 10	NW
1821. Dec. 28.	3 45 A.M.	4 10 A.M.	23 10	22 9¾	SE
	4 15 P.M.	4 29 P.M.	25 10	22 8½	ESE
1824. Dec. 23.	3 10 A.M.	3 28 A.M.	25 11	22 5½	NW
	3 40 P.M.	3 46 P.M.	23 6	22 5¼	S
1827. Oct. 23.	11 45 P.M.	0 5 A.M.†	26 0	21 6½	NW
Nov. 1.	0 10 P.M.	0 25 P.M.	22 3	21 8½	NW

These results are extremely unsatisfactory; and I fear that it will happen sometimes, although but rarely, that a considerable error will occur in the calculated times and heights of high water, owing no doubt to gales of wind in the Channel or North Sea, or even perhaps in the Atlantic. The average error in using the Tables of the Companion, as M. Dessiou found by his calculations for the year 1826, (see Phil. Trans. 1831, p. 381,) in the time of high water is about 12^m, and in the height about 8 inches; this error however is more I be-

* In using these Tables the moon's transit should be equated or reduced to mean time before the corrections are applied. The example given in the Companion is therefore incorrect.

† November 1.

lieve to be attributed to the imperfection of the observations than to the inaccuracy of the Tables. The time is only recorded in the Dock books to the nearest five minutes.

The Committee of the Astronomical Society, to whom the improvement of the Nautical Almanac was referred, having recommended the insertion in that work of a "Table of the mean time of high water at London Bridge for every day in the year, and also at the principal ports at the time of new and full moon," (see Report of the Committee of the Astronomical Society relative to the improvement of the Nautical Almanac, p. 14,)—without doubt that accuracy will be introduced into these calculations which has long been applied to all other astronomical phænomena.

In the open ocean the rise of the tide is so small that it is difficult to fix the time of high water, and the effect of the wind is so capricious, that it seems difficult to do more than to determine the establishment of the port; to which the mean of all the times of high water observed at any point of the lunation, will *in this case* afford a sufficient approximation. When this constant has been obtained at many places on the surface of the globe, the march of the great tide-wave will be ascertained, the numbers given on the map drawn by Mr. WALKER, and which accompanies my former paper on this subject, may be rectified, and many anomalies which it now presents will no doubt disappear.

In narrow channels and archipelagoes the case is widely different: here the moon's age and even her parallax and declination have a perceptible influence; and if accuracy be required, all these circumstances, together with the period of the year, must be taken into account.

The observations which already exist would, if carefully discussed, furnish the means of determining the establishment of the port (λ_i), the fundamental hour of the port (λ), and the constant $\left(\frac{m\,\Pi^3}{m_i\,\Pi_i^3}\right)$, which contains implicitly the mass of the moon throughout the British Isles, and probably in many other places, as along the coast of France, at Madras, &c. * Having obtained these constants, Tables might be constructed, which by merely adding a given quantity would be sufficiently correct practically for a considerable extent of coast. These constants have been determined for the London Docks, and for the

* As is done in this paper for Brest.

present time, with great precision, but the *establishment* is subject to change, and the determination of this quantity will probably require to be repeated after several years.

With respect to the determination of the influence of the parallax and declination of the moon, it is desirable to employ more observations than I have done; I contented myself with about 5000, in order to spare M. DESSIOU's time. A similar discussion of observations of the tides at Brest or some other favourable situation is greatly to be wished for, in order to ascertain how far these effects are the same in different ports.

The discussion of the observations of the times and heights of low water at the London Docks also remains, which I have been obliged to postpone.

The height of the water at any given time and place may be calculated when the requisite constants have been determined. At the London Docks the height of the water expressed in feet is

$$16{\cdot}68 + 4{\cdot}448 \left\{ \cos 2\,(\theta_{,} - \lambda_{,}) + {\cdot}3788 \cos 2\,(\theta - \lambda) \right\}$$

which formula affords results agreeing nearly with observation, and which may be compared with the curves given by Mr. PALMER, (see Phil. Trans. 1831, Part I.). According to this expression the mean rise of the tide is 12 ft. 3 in.

When $\lambda - \lambda_{,} = 0$, (that is, at the London Docks when the moon passes the meridian at 2 o'clock,) the curve in question is the *curve of sines*.

According to M. DAUSSY, (Mémoire sur les Marées des Côtes de France, Connaissance des Temps 1834,) the height of high water varies with the atmospheric pressure, being highest when the barometer is lowest. This paper did not come to my knowledge until after these pages were in the press; but the determination by M. DAUSSY of the establishment of the port of Brest coincides with that which I have given, namely $3^{h}\ 48^{m}$.

RESEARCHES ON THE TIDES.

Fifth Series.

ON

THE SOLAR INEQUALITY

AND ON

THE DIURNAL INEQUALITY

OF

THE TIDES AT LIVERPOOL.

BY

THE REV. WILLIAM WHEWELL, M.A., F.R.S.
Fellow of Trinity College, Cambridge.

From the PHILOSOPHICAL TRANSACTIONS.—PART I. FOR 1836.

LONDON:

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1836.

XII. *Researches on the Tides.—Fifth Series. On the Solar Inequality and on the Diurnal Inequality of the Tides at Liverpool.* By the Rev. WILLIAM WHEWELL, *F.R.S., Fellow of Trinity College, Cambridge.*

Received February 23,—Read March 3, 1836.

SECT. 1. *Present State of the subject.*

1. THE great success with which recent researches on the Tides have been attended, has encouraged me to attempt some further advances in this subject. The laws of the semimenstrual inequality of the times were shown by Mr. LUBBOCK, from the London observations, to agree very closely with the equilibrium-theory: and this result has been confirmed by the examination of observations made at many other places. I have shown, from the Liverpool observations, that the semimenstrual inequality of the heights presents a still more complete agreement with the equilibrium-theory*; and by the help of Mr. LUBBOCK's discussions of the London and of the Liverpool observations, I have shown, in the Second and Fourth Series of these Researches, that the inequalities depending on the changes of lunar parallax and declination may be very well represented by the equilibrium-theory, with certain modifications, which are far from inconsistent with the best mechanical views we can at present form of the laws of the motion of fluids.

The most obvious points which now remain requiring still to be made out and explained, are the diurnal inequality, and the solar inequalities of the time and height of high water.

2. The *Diurnal Inequality* of the tides is that which makes the tide of the morning and evening of the same day at the same place, differ both in height and time of high water, according to a law depending on the time of the year. This is called the diurnal inequality, because its cycle is a day.

The existence of such an inequality in the heights of high water has often been noticed by seamen and other observers, as I have stated in the First Series of these Researches†. But its reality has only recently been confirmed by regular and measured observations, and its laws have never been correctly laid down. Its existence appeared very palpably in the curves which I constructed in order to examine the results of the tide observations made by the coast-guard in June 1834, and also in the curves drawn by the self-registering tide-gauge erected near Bristol; although the Sheerness machine did not exhibit it, in consequence of the tide at that place

* Fourth Series: Philosophical Transactions, 1836, page 1.

† Philosophical Transactions, 1833, page 221.

being a compound of two others. But this inequality had never been obtained in numbers till the recent discussion of the Liverpool tides. Under Mr. LUBBOCK's direction, Mr. DESSIOU has obtained it from the observations of Mr. HUTCHINSON. Mr. BYWATER, who has calculated Tide-Tables for Liverpool for the present year, has also obtained this inequality from his own calculations, (suggested, as he states, by the remarks made in the First Series of these Researches,) and has introduced it, for the first time, into published tables; and I have just learnt from Mr. BUNT, who is at present employed in forming Tide-Tables for Bristol, that he has also obtained the general form of this inequality, agreeing, on the whole, with the other results, although with some discrepancies. These arise, I conceive, principally from the shortness of the series of observations which he had at his command (less than two years). The tide observations now going on at these ports, when hereafter discussed as the preceding ones have been, will, I have not any doubt, lead to the production of tide-tables possessing a degree of accuracy which, a little while ago, would have been considered unattainable.

3. It is natural for us to wish to refer the effects of the diurnal inequality, so far as they have yet been obtained, to the equilibrium-theory. I will say a few words on this subject.

The general relation of these results to the equilibrium-theory it is not difficult to see. When the moon is south of the equator, the equilibrium-tide corresponding to her upper transit at any place having southern latitude, would be greater than the tide corresponding to her lower transit: when she is north of the equator the contrary would be the case. When she is in the equator the two tides are equal. Now in one lunation she moves in an orbit inclined to the equator. Hence, while she moves from the sun to the sun again, (that is, while the time of her upper transit passes through the whole twenty-four hours from noon to noon again,) the tide which corresponds to her upper transit will, during one lunation, be greater during half the period, and less during the other half, than the tide of the next half-day; and at two particular times in the lunation the difference will vanish. The times of moon's transit for which the diurnal difference vanishes, correspond to the times when the moon is in the equator, and are therefore different at different seasons of the year. Now from the general correspondence of the phenomena of the tides with the equilibrium-theory, we may expect that the circumstances of the diurnal inequality will be the same as those which have been described; but that the time when the diurnal inequality vanishes will not be the time when the moon is in the equator, but some time afterwards.

4. By the statements above referred to on this subject it appears that the time at which the diurnal difference vanishes is when the moon's transit takes place about $9^h\ 30^m$ in January, and two hours earlier in each succeeding month, taking the general average of the facts. Now in the middle of January the sun is $4^h\ 30^m$ from the vernal equinox, and hence the moon is 5^h beyond the equinox when the diurnal inequality

vanishes. The same result would be obtained from the other months, since the sun's right ascension increases on an average two hours in each month*. Hence the *evanescence of the diurnal inequality which, in the equilibrium-spheroid, would take* place when the moon is in the equator, does not take place till she has described 5^h of right ascension after that time, and this requires six days and a quarter.

5. In the inequalities hitherto considered, which were the effects of the sun, and of changes of the moon's parallax and declination, the circumstances of the tide agree with those of the equilibrium-spheroid one day and a half or two days previous. It appears that such an interval suffices for the forces, when near their maximum, to accumulate and transmit their effects to Liverpool; and after this interval the diminution of the actual forces overbalances the increase arising from their continued action. But in this case of the diurnal inequality we find that above six days are required for this accumulation and transfer. The inequality vanishes six days after its cause vanishes, and in the same way it reaches its maximum six days after the producing force is greatest. On a little consideration we shall not be surprised at the great time required to bring this inequality to its full magnitude. The semi-diurnal tides, alternately greater and less, which are transmitted from the Southern Ocean to Liverpool, may be compared to oscillations of the ocean, and these are augmented by the action of the forces occurring at intervals equal to those of the oscillations. Hence the oscillations go on increasing for a considerable period after the forces have gone on diminishing, and reach their maximum almost a week after the forces have passed theirs.

6. In January the tide at Liverpool, which follows the moon's upper transit by about eleven hours, is *less* than the following tide, when the moon is near the sun, and consequently when the moon is south of the equator. This agrees with the equilibrium-tide depending upon the position of the moon at the time of transit. But it is clear that the tide which reaches Liverpool does in fact come from the Southern Ocean; and hence the equilibrium-tide for the time of the moon's upper transit at Liverpool will be *greater* than the following one, when the moon is near the sun in January. Hence the tide at Liverpool is not that which corresponds to the equilibrium-tide *at* the time of transit, but to the equilibrium-tide about either twelve or thirty-six hours *earlier*. The latter is probably the right conclusion, and agrees with the inference already obtained from the effects of the solar forces, that the tide agrees with an equilibrium-tide thirty-seven hours and a half previous to the moon's transit.

8. The succeeding sections of this paper will be devoted to the investigation of the *Solar Inequalities* at Liverpool. By carefully eliminating the lunar effects, which the preceding researches enable us to do, I have determined, as I conceive beyond dis-

* In order to detect the diurnal inequality, the observations have been classed by calendar months; but as this inequality depends mainly on the moon's declination, it would probably have been obtained more distinctly if the observations had been classed according to the moon's declination, distinguishing north and south declination, and also increasing and decreasing declination.

pute, the approximate circumstances of the solar correction for the *height*. I have also obtained, though by no means with the same certainty, evidence of the existence and laws of the solar inequality of the *times*. These inequalities, as thus discovered, exhibit the same general agreement with the equilibrium-theory which has been disclosed in all the inequalities hitherto detected*.

9. But though the equilibrium-theory thus seems to suggest and express the laws of the various inequalities of the tides, I would by no means be understood to rate this theory above its true value. It is not the true theory, but a very inaccurate and insufficient substitute for it, which we are compelled to adopt in consequence of the extremely imperfect state of the mathematical science of hydrodynamics. The tides are a problem of the motion, not of the *equilibrium* of fluids; and we can never fully explain the circumstances of the phenomena till the problem has been solved in its genuine form. This solution is perhaps not beyond the powers of modern mathematics, but it has certainly never yet been given. LAPLACE's solution, besides being obtained by means of a precarious assumption, rests upon several arbitrary hypotheses, fatal to it even as a first approximation; and, I believe it will be found, leaves out of consideration an essential portion of the forces. To obtain any useful result, the question must be taken up afresh and treated in another manner.

10. I hope some mathematician will be found able and willing to execute this task. But in the mean time I may be permitted to observe, that what has been already done in the discussion of tide observations, and in bringing to light the empirical laws of the phenomena, has entirely altered the position of this branch of science with respect to the mathematical theory. A little while ago the theory was in advance of observation; at present observation is in advance of theory. A very few years since, the equilibrium-theory and the Laplacian theory were in a condition to assign laws regulating the changes of the times and heights under given astronomical circumstances, and it had not been shown from observation whether these laws were obeyed. We can now state what the agreement and disagreement is between such theoretical laws and the facts; and we call upon the mathematician to substitute for these two theories, both confessedly false, some other, which shall come nearer to the true state of the case, and, by that means, nearer to the laws of the phenomena. The performance of this task is requisite for the completion of the Newtonian theory of the universe.

§ 2. *On the Effect of the Moon's Declination on the Tides at Liverpool*†.

11. In order to obtain the Solar Inequality, it was necessary to eliminate the effect of the variations of the lunar tidal forces; the results of the two sets of forces being combined in the tides when tabulated according to months, as is done in Mr. LUB-

* While I write this, I am informed by Mr. BUNT that he has obtained the law of the solar inequality of the times at Bristol. The agreement with my results is very remarkable.

† In the calculations of this Memoir, I have been assisted by Mr. NAYLER of Queen's College in this University.

BOCK's Tables I., II., III.* In these tables the parallax may be already considered as reduced to its mean value; for each number is the result of about ninety observations, distributed through nineteen years, in which time the moon's perigee moves twice round the ecliptic; and hence the deviations above and below the mean will balance each other. But it is otherwise with the declination; for since the mean moon may be considered as moving in the mean elliptic, a certain time of the year, with a certain hour of moon's transit, implies a certain declination; for the moon's time of transit added to the sun's right ascension is the moon's right ascension, which of course determines the declination. Hence the variations of the numbers in Tables II. and III. are those which arise from the varying forces of the mean moon and of the sun; and the greater part of the variation arises from the change of the declination of the moon. This part must be eliminated, in order to bring into view the solar effect; and we are able to perform this elimination by means of Table I., since that Table contains also the mean declination of the moon for each set of observations. In each set, the number of observations being large, and the limits of declination small, the mean correction for the lunar declination may be taken to be identical with the correction for the mean lunar declination, although the correction varies nearly as the square of the declination. But in order to apply this correction, we must have a table of the effect of every degree of declination; whereas Mr. LUBBOCK's Tables XII. and XVI. only contain the effect for every 3° of declination. It is obvious also, by inspection of this table, that it is affected by many casual irregularities which must be got rid of by interpolation. Among these irregularities, however, I do not include the apparent difference of the effect of north and south declination, which is shown in Tables XI. and XV.; since it will be seen by comparison with Table X. that the greater part of this difference arises from the effect of parallax; for the changes of parallax and declination so nearly recur in the same cycle, that their effects are not insulated in a period of nineteen years. The remainder of the difference arises from the solar inequality of which we are in search.

12. I have therefore laid down Mr. LUBBOCK's Tables XII. and XVI., and interpolated them by means of curves, and have thus obtained two new declination tables for the *Times* and the *Heights*. These tables I shall designate as Declination Tables (W. T.) and (W. H.) respectively; meaning by the former letter to distinguish them from Mr. LUBBOCK's Tables XII. and XVI. They may be used in calculating the time and height of high water at Liverpool (the table for the height being altered by a constant if the measures are not taken from Mr. HUTCHINSON's zero). When so applied, they take the place of Mr. LUBBOCK's Tables XXV. and XXVI., from which they differ—in including the semimenstrual inequality, in being interpolated independently, in being given for the middle of each hour instead of the beginning, and for every degree of declination instead of every three degrees. These Tables will be found at the end of this Memoir, namely,

* Philosophical Transactions, 1835, p. 282.

Declination Table (W. T.). *To be used in predicting the Time of high water at Liverpool, for the mean parallax.*

Declination Table (W. H.). *To be used in predicting the Height of high water at Liverpool, for the mean parallax.*

When these tables are used in predicting the tides, the times and heights found by these must be corrected for parallax by Mr. LUBBOCK's Tables XXIII. and XXIV., or by the formulæ given in my Memoir on the Empirical Laws of the Tides in the Port of Liverpool. I will insert, at the end of this paper, Tables of the correction for parallax given by these formulæ.

§ 3. *On the Solar Inequality of the Heights of High Water at Liverpool.*

13. By means of the Declination Tables (W. T.) and (W. H.) we can reject from Mr. LUBBOCK's Tables II. and III. the part which depends on the moon's declination, and we thus have the remainder, which is the solar correction as far as it can be obtained from the observations of Mr. HUTCHINSON. This is what I have done in the following calculations (Table A.). The third column of each table, marked (W. H.), contains the height due by the declination table; the fourth column, marked (L. III.), contains the observed height as given by Mr. LUBBOCK's Table III.; and the column (Diff. H.) is the excess of the latter, and is therefore the residual height due to the solar effect. In like manner the columns marked (W. T.), (L. II.) and (Diff. T.) contain the *Intervals* due to the moon's declination, the observed intervals, and the residual solar effect.

I shall consider the solar effect on the *Heights* in the first place, since its law is more manifest. It may be expected, like the other tidal inequalities, to be resolvable into a non-periodic and a periodic part. In order to effect this, I take the mean of each monthly column of differences, and subtract it from each number in the column. In this manner I obtain Table (B. H.).

In order to trace the law of the periodical part, which exists in the remainders, if at all, I lay down these remainders by coordinates, as in Plate XVII.

14. If we draw lines through the dots belonging to each hour of the moon's transit, it becomes manifest that these remainders really result from a solar inequality. For the curves (the dotted curves in Plate XVII.) have all the same general form, having a reference to an annual cycle. Thus there are, for all of them, a maximum or minimum in March, another in July, another in October, and another in December. Hence the effect of lunar declination has been nearly or altogether eliminated; for this effect is a maximum for different hours at different times of the year, as appears by the reason of the case, and as is shown in Mr. LUBBOCK's Tables IV. and V.

It appears also by the curves, that the effects of the changes of the solar force are the greatest for the hours of moon's transit, $1^h\ 30^m$ and $7^h\ 30^m$, and are in these two cases the opposite of one another, which agrees with the nature of the case; for the

height of the solar tide is added to that of the lunar in the former case (at spring-tides), and subtracted in the latter (at neap-tides).

15. Thus the general course of the phenomena shows the existence of the solar inequality; but we shall trace its law more distinctly by taking the suggestions of the equilibrium-theory.

If h, h', be the height of the solar and lunar tides, ϕ the angular distance of the sun and moon, y the compound tide, we have

$$y = \sqrt{\{h^2 + h'^2 + 2\,h\,h' \cos 2\,(\phi - \alpha)\}},$$

as shown on former occasions; α being a quantity which is to be determined so as to accommodate the equilibrium-theory to the actual case.

Let h undergo any change so as to become $h + \Delta h$, Δh being small. Then y becomes approximately

$$y + \Delta y = y + \frac{dy}{dh}\,\Delta h = y + \frac{h + h' \cos 2\,(\phi - \alpha)}{\sqrt{\{h^2 + h'^2 + 2\,h\,h' \cos 2\,(\phi - \alpha)\}}}\,\Delta h.$$

The quantity Δh varies according to the different season of the year, and depends principally on the sun's declination, the changes of solar parallax being too small to affect much the amount of solar tidal force. The curve which expresses the changes of Δh has, as appears in the figures, a maximum at the end of March, soon after the time when the sun is in the equator, and when, consequently, his tidal force is the greatest. It cuts the axis in May, soon after the sun has his mean effective declination: it has a minimum in July, soon after his greatest declination, at which time his force is least. The mean effect recurs in the end of August: there is another maximum in the end of September. About November, December, and January, there is another mean and another minimum, arising from the mean declination in November and the greatest declination in December. Thus the general course of the value of the correction for a given value of ϕ agrees with the equilibrium-theory. The want of perfect regularity in the form of the curves is due partly to the combination of the effects of solar parallax and solar declination. According to the theory, the greatest amount of the correction for solar declination is about one tenth, and the greatest amount of the correction for solar parallax about one twentieth, of the whole solar tide.

16. For a given season of the year, if we follow the changes of ϕ through twelve hours, we easily see from the formula that Δy has a continuous series of values, among which are a maximum and a minimum value. Hence the curves in Plate XVII. ought to be such that the ordinates for each month, taken in the successive hour-lines, form a continuous series. The subtraction of the non-periodical part, which we have performed, will not affect this continuity, since this part is constant for the month.

Hence in correcting the original curves of Plate XVII., so as to get rid of irregularities, we must endeavour to make them conform to these two conditions:—that the ordinates for the same month shall form a continuous series, with a maximum and minimum; and, that the curves for the different hours shall be similar to each other,

and have their maxima aud minima at the same seasons. This latter condition may perhaps be slightly modified, so that α may be somewhat different for different values of Δh.

The curves drawn with full lines in Plate XVII., are drawn under these conditions, the ordinates being those contained in Table (C. H.). Their agreement with the original curves is such as to entitle us to consider them as correct interpolations, for the coincidence is almost complete in the cases where the corrections are the largest; as the hours $0^h\ 30^m$, $1^h\ 30^m$, $2^h\ 30^m$, $6^h\ 30^m$, $7^h\ 30^m$, $8^h\ 30^m$; and there are no material discrepancies except in the lines $4^h\ 30^m$ and $10^h\ 30^m$; and even in these the difference is only a displacement of a maximum of four inches by about a month. The observations, therefore, prove that there is a solar correction of the heights which follows, nearly, the law suggested by the equilibrium-theory. The greatest amount of the periodical part of this correction is about half a foot *plus* and *minus*; and to this must be added the non-periodical part, which at some seasons amounts to one fifth of a foot.

17. We may expand the formula above given for Δy into a non-periodical and a periodical part. We have

$$\Delta y = \frac{h + h' \cos 2(\varphi - \alpha)}{\sqrt{\{h^2 + h'^2 + 2hh' \cos 2(\varphi - \alpha)\}}} \cdot \Delta h,$$

$$= \frac{h'}{\sqrt{(h^2 + h'^2)}} \frac{c + \cos 2(\varphi - \alpha)}{\sqrt{\{1 + \frac{2c}{1 + c^2} \cos 2(\varphi - \alpha)\}}} \cdot \Delta h,$$

Making $c = \frac{h}{h'}$. Expanding and omitting c^2, &c.

$$\Delta y = \frac{h'}{\sqrt{(h^2 + h'^2)}} \Big(c + \cos 2(\varphi - \alpha)\Big) \Big(1 - c \cos 2(\varphi - \alpha)\Big) \Delta h$$

$$= \frac{h'}{\sqrt{(h^2 + h'^2)}} \Big\{c + \cos 2(\varphi - \alpha) - c \cos^2 2(\varphi - \alpha)\Big\} \Delta h$$

$$= \frac{h'}{\sqrt{(h^2 + h'^2)}} \Big\{\frac{c}{2} + \cos 2(\varphi - \alpha) - \frac{c}{2} \cos 4(\varphi - \alpha)\Big\} \Delta h$$

Hence the periodical part is

$$\frac{h' \Delta h}{\sqrt{(h^2 + h'^2)}} \Big\{\cos 2(\varphi - \alpha) - \frac{c}{2} \cos 4(\varphi - \alpha)\Big\}.$$

This vanishes for two values of φ, which are a little less than $90°$ or 6^h from each other when c is small. In Table (B. H.) it appears that the periodical solar correction vanishes for two values of the hour-angle which are nearly 6^h from each other in each month. Hence we may suppose that the periodical part involving $\cos 2(\varphi - \alpha)$ is alone sensible. This vanishes when $\cos 2(\varphi - \alpha) = 6^h$ or 18^h, $\varphi = 3^h + \alpha$ or $9^h + \alpha$. Hence we find that in January and September α is $2^h\ 30^m$; in March, April, June, July, October α is about 2^h; in August and November it is $1^h\ 30^m$; in December it is 3^h.

These differences in the value of α may arise from the lunar declination not being completely eliminated in our previous calculation, the interpolation of the Table being slightly inexact. But it is by no means improbable that they are the discrepancies, arising from mechanical principles, which exist between the tides of water in motion and the results of the equilibrium-hypothesis. The general agreement of the equilibrium-theory with the facts (assuming $\alpha = 2^h$) is near enough to be very remarkable. It may be possible, by additional care and labour, to bring the solar interpolated curves nearer to the observations, preserving the requisite conditions: but I conceive that enough has been done to establish the general law.

§ 4. *On the Solar Inequality of the Time of High Water at Liverpool.*

18. The solar inequality of the time must be found in exactly the same manner as the solar inequality of the heights. By interpolation of Mr. LUBBOCK's Table XVI., I have obtained Declination Table (W. T.); and by comparing the time of high water due to lunar declination by this Table with the observed times as given in column (L. II.) of the calculations, I obtain the residual quantities in the columns (Diff. T.), which should exhibit the solar correction of the times.

I then find the mean of the column for each month, and subtract it from every number in the column, in order to separate the non-periodical from the periodical part of the residual quantities. The means and the remainders are exhibited in Table (B. T.). The remainders were laid down by coordinates, but I have not thought it necessary to give these curves.

The points thus found being joined by continuous lines, I had a series of curves which ought to exhibit the solar inequality. These lines were less obviously regular than those which we obtained by a similar treatment of the heights; but they were still apparently free from any lunar effect, which would have given a maximum passing successively from one month to another; and I could trace a solar cycle in them; namely, the curves for $1^h\ 30^m$, $2^h\ 30^m$, and $3^h\ 30^m$ had a maximum about April, and a minimum about August, while the curves for $5^h\ 30^m$, $6^h\ 30^m$, $7^h\ 30^m$ had these features inverted, and the rest of the curves had small ordinates only.

19. Let us compare the laws of the phenomena of which we thus catch a glimpse with the laws according to the equilibrium-theory. We have, on that hypothesis,

$$\tan(\theta' - \lambda') = -\frac{h \sin 2(\varphi - \alpha)}{h' + h \cos 2(\varphi - \alpha)} = t \text{ suppose,}$$

where θ' is the interval of the tide and moon's transit.

Now let h become $h + \Delta h$, and θ' become $\theta' + \Delta\theta'$, we have, approximately,

$$\tan(\theta' - \lambda') + \frac{d.\tan(\theta' - \lambda')}{d\theta'}\Delta\theta' = t + \frac{dt}{dh}\Delta h$$

$$\sec^2(\theta' - \lambda') . \Delta\theta' = \frac{dt}{dh}\Delta h$$

$$\Delta \theta' = \frac{1}{1+t^2} \cdot \frac{d\,t}{d\,h} \Delta h$$

$$= - \frac{h' \sin 2 (\varphi - \alpha)}{h^2 + h'^2 + 2\,h\,h' \cos 2 (\varphi - \alpha)} \cdot \Delta h.$$

Hence for a given value of Δh, that is, for a given month, $\Delta \theta'$ goes through a cycle which has a minimum and a maximum, when $\cos 2 (\varphi - \alpha) = - \frac{2\,h\,h'}{h^2 + h'^2}$. If $\frac{h'}{h} = 3$, which has been found to be nearly the value by other phenomena, the two values of $2 (\varphi - \alpha)$ are 144° and 216°, or 10^h and 17^h nearly; and if in this case, α be $- 1^h\ 30^m$, the maxima and minima will agree with $\varphi = 3^h\ 30^m$, $\varphi = 7^m\ 0^m$, which appears best to represent the phenomena.

20. After drawing the lines described in Art. 18, I drew interpolated curves for the hours $1^h\ 30^m$, $2^h\ 30^m$, $3^h\ 30^m$, and $5^h\ 30^m$, $6^h\ 30^m$, $7^h\ 30^m$; the general agreement of the course of the interpolated and the original curves appeared to me to be such as to show that the errors arose from a solar inequality, following in its general changes the law given by the equilibrium-theory. I have given the corrections upon this supposition in Table (C. T.). The displacement of the zero points and maxima, and the want of proportionality in the maximum values, which appear in some of the lines, I have admitted, because such modifications may arise from my not having got rid of the whole of the non-periodical effect. Such irregularities may also arise from there being still some vestige of the lunar declination correction not got rid of by the processes which I have employed: but this, if it exists, must be very small, and I do not think there can be any doubt as to the general form of the solar correction of the time. I have, however, abstained from filling up the Table, as not thinking my present materials sufficient to enable me to do so with any confidence.

DECLINATION TABLE (W. T.). To be used in predicting the *Time* of high water at Liverpool for the mean parallax.

Moon's Transit.	0° Decl.	1° Decl.	2° Decl.	3° Decl.	4° Decl.	5° Decl.	6° Decl.	7° Decl.	8° Decl.	9° Decl.	10° Decl.	11° Decl.	12° Decl.	13° Decl.
h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m
0 30	11 20	11 20	11 20	11 20	11 20	11 20	11 20·5	11 20	11 20	11 19·5	11 19	11 19	11 18	11 18·5
1 30	11 5	11 5	11 5·5	11 5	11 5	11 5	11 5	11 5	11 4	11 4	11 4	11 3·5	11 3	11 2·5
2 30	10 51	10 51	10 51	10 51	10 51	10 51	10 50	10 50	10 50	10 49·5	10 49	10 49	10 47·5	10 47·5
3 30	10 39	10 39·5	10 39·5	10 39·5	10 39·5	10 39·5	10 39	10 39	10 38·5	10 38	10 38	10 37	10 36·5	10 36
4 30	10 31	10 31·5	10 31·5	10 31·5	10 31	10 31·5	10 31·5	10 31	10 30	10 29	10 28·5	10 28	10 27·5	10 27
5 30	10 35	10 35	10 35	10 34·5	10 34·5	10 34·5	10 34	10 34	10 33·5	10 32·5	10 32	10 31	10 30	10 28·5
6 30	10 54	10 54	10 54	10 53·5	10 53	10 52·5	10 52	10 51·5	10 51	10 50	10 49	10 48	10 47	10 46
7 30	11 24	11 24	11 24	11 24	11 24	11 24	11 23·5	11 23	11 23	11 22	11 22	11 20·5	11 20	11 19
8 30	11 47	11 47	11 47	11 47	11 47	11 47	11 47	11 47	11 47	11 47	11 46	11 46	11 45	11 44·5
9 30	11 53·5	11 53·5	11 53·5	11 53·5	11 53·5	11 53·5	11 53	11 53	11 53	11 52·5	11 52	11 52	11 52	11 51·5
10 30	11 46	11 46	11 46	11 46	11 46	11 46	11 46	11 46	11 46	11 46	11 46	11 46	11 46	11 46
11 30	11 35	11 35	11 35	11 35	11 35	11 35	11 35	11 35	11 35	11 34·5	11 34·5	11 34·5	11 34	11 34

	14° Decl.	15° Decl.	16° Decl.	17° Decl.	18° Decl.	19° Decl.	20° Decl.	21° Decl.	22° Decl.	23° Decl.	24° Decl.	25° Decl.	26° Decl.	27° Decl.
0 30	11 17·5	11 17	11 16·5	11 16	11 15	11 14	11 14	11 13	11 12·5	11 12	11 11·5	11 11	11 10	11 9
1 30	11 2	11 1·5	11 1·5	11 0·5	10 59·5	10 59	10 58·5	10 58	10 57·5	10 57	10 56	10 54	10 53	10 52
2 30	10 47	10 46·5	10 43·5	10 44	10 44	10 43	10 42	10 41	10 40	10 39	10 37·5	10 37	10 36	10 34
3 30	10 35	10 34	10 33	10 32·5	10 32	10 31	10 29·5	10 28	10 27	10 26	10 25	10 24	10 22	10 21
4 30	10 26·5	10 25	10 24	10 23	10 22	10 21·5	10 20	10 18·5	10 17·5	10 16	10 14	10 12·5	10 11	10 9
5 30	10 27	10 26	10 25	10 23·5	10 22·5	10 21·5	10 19	10 18	10 17	10 15	10 13·5	10 12·5	10 11·5	10 10
6 30	10 44·5	10 43·5	10 42·5	10 41	10 39	10 37·5	10 36	10 35	10 33	10 32	10 31	10 29	10 27	10 25
7 30	11 18	11 18	11 17	11 15	11 13·5	11 12·5	11 11	11 10	11 8·5	11 7	11 6	11 4	11 2·5	11 0·5
8 30	11 44·5	11 44	11 43	11 42	11 42	11 41	11 40·5	11 40	11 39	11 38	11 37	11 36	11 34	11 33·5
9 30	11 51	11 51	11 50	11 50	11 49	11 49	11 48·5	11 48	11 47·5	11 47	11 46·5	11 46	11 45	11 44
10 30	11 45·5	11 44·5	11 44·5	11 44	11 43	11 43	11 42·5	11 42	11 41·5	11 40	11 39·5	11 39	11 38	11 37·5
11 30	11 33·5	11 32·5	11 32·5	11 32	11 32	11 31·5	11 30	11 29	11 28·5	11 28	11 27·5	11 27	11 26	11 25

DECLINATION TABLE (W. H.). To be used in predicting the *Height* of high water at Liverpool for the mean parallax.

The heights in feet from Mr. HUTCHINSON's zero.

Moon's Transit.	0° Decl.	1° Decl.	2° Decl.	3° Decl.	4° Decl.	5° Decl.	6° Decl.	7° Decl.	8° Decl.	9° Decl.	10° Decl.	11° Decl.	12° Decl.	13° Decl.
h m	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.
0 30	18·3	18·3	18·3	18·3	18·3	18·3	18·3	18·3	18·3	18·25	18·25	18·2	18·15	18·1
1 30	18·3	18·3	18·3	18·3	18·3	18·3	18·3	18·3	18·3	18·25	18·25	18·2	18·15	18·1
2 30	17·65	17·65	17·65	17·65	17·65	17·65	17·65	17·65	17·65	17·6	17·6	17·55	17·5	17·5
3 30	16·6	16·6	16·6	16·6	16·6	16·55	16·55	16·55	16·55	16·5	16·5	16·5	16·45	16·4
4 30	15·2	15·2	15·2	15·2	15·2	15·2	15·15	15·15	15·1	15·1	15·05	15·05	15·0	14·95
5 30	13·95	13·95	13·9	13·9	13·9	13·9	13·85	13·8	13·75	13·75	13·75	13·7	13·65	13·65
6 30	13·0	13·0	13·0	13·0	12·95	12·95	12·9	12·9	12·85	12·8	12·75	12·7	12·65	12·6
7 30	12·95	12·95	12·95	12·95	12·95	12·9	12·	12·9	12·85	12·8	12·75	12·7	12·65	12·6
8 30	13·95	13·95	13·9	13·9	13·85	13·85	13·85	13·8	13·75	13·75	13·7	13·65	13·65	13·55
9 30	15·35	15·35	15·35	15·35	15·35	15·35	15·35	15·35	15·3	15·3	15·25	15·25	15·2	15·15
10 30	16·8	16·8	16·8	16·8	16·75	16·75	16·75	16·75	16·7	16·7	16·65	16·6	16·55	16·5
11 30	17·7	17·7	17·7	17·7	17·65	17·65	17·65	17·65	17·65	17·65	17·6	17·55	17·55	17·45

	14° Decl.	15° Decl.	16° Decl.	17° Decl.	18° Decl.	19° Decl.	20° Decl.	21° Decl.	22° Decl.	23° Decl.	24° Decl.	25° Decl.	26° Decl.	27° Decl.
0 30	18·05	17·95	17·9	17·8	17·7	17·65	17·5	17·4	17·3	17·2	17·1	17·0	16·8	16·7
1 30	18·05	17·95	17·9	17·8	17·7	17·65	17·5	17·4	17·3	17·2	17·1	17·0	16·8	16·7
2 30	17·45	17·4	17·35	17·25	17·2	17·1	17·0	16·9	16·85	16·75	16·65	16·55	16·4	16·3
3 30	16·35	16·35	16·3	16·25	16·25	16·2	16·15	16·1	16·0	15·95	15·9	15·8	15·7	15·6
4 30	14·95	14·9	14·9	14·8	14·75	14·75	14·65	14·6	14·55	14·5	14·4	14·35	14·25	14·15
5 30	13·6	13·55	13·5	13·45	13·4	13·35	13·25	13·2	13·1	13·05	12·95	12·9	12·8	12·75
6 30	12·55	12·5	12·45	12·35	12·25	12·2	12·1	12·0	11·9	11·8	11·7	11·5	11·35	11·2
7 30	12·5	12·5	12·4	12·35	12·25	12·2	12·1	12·0	11·9	11·75	11·65	11·5	11·35	11·2
8 30	13·5	13·45	13·4	13·3	13·25	13·2	13·15	13·05	12·95	12·9	12·8	12·75	12·65	12·55
9 30	15·1	15·05	15·0	14·95	14·9	14·85	14·75	14·7	14·6	14·5	14·4	14·3	14·2	14·1
10 30	16·45	16·4	16·35	16·3	16·25	16·2	16·1	16·0	15·85	15·75	15·6	15·45	15·3	15·1
11 30	17·4	17·35	17·25	17·2	17·1	17·0	16·9	16·8	16·7	16·6	16·45	16·3	16·15	16·0

PARALLAX TABLE (W. T.). To be used in correcting the *Time* of high water at Liverpool, predicted for mean parallax 57′.

Moon's Transit.	54′.	55′.	56′.	57′.	58′.	59′.	60′.	61′.
h m	m	m	m	m	m	m	m	m
0 30	9	6	3	0	− 3	− 6	− 9	−12
1 30	7	5	2	0	− 2	− 5	− 7	− 9
2 30	5	3	2	0	− 2	− 3	− 5	− 6
3 30	3	2	1	0	− 1	− 2	− 3	− 3
4 30	1	1	0	0	0	− 1	− 1	− 1
5 30	1	1	0	0	0	− 1	− 1	− 2
6 30	4	3	1	0	− 1	− 3	− 4	− 5
7 30	9	6	3	0	− 3	− 6	− 9	−12
8 30	13	8	4	0	− 4	− 8	−13	−17
9 30	14	9	5	0	− 5	− 9	−14	−19
10 30	13	9	4	0	− 4	− 9	−13	−18
11 30	11	7	4	0	− 4	− 7	−11	−15

PARALLAX TABLE (W. H.). To be used in correcting the *Height* of high water at Liverpool, predicted for mean parallax 57′.

H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
ft.	ft.	ft.	ft.	ft.	ft.	ft.	ft.
−1·2	−0·8	−0·4	0	+0·4	+0·8	+1·2	+1·6

TABLE (A).

Calculation of the Differences between the Time and Height of High Water at Liverpool, as due to the Moon's Declination, and as shown by Mr. HUTCHINSON's Observations, for every month of the year.

January.							
Moon's Transit.	Declination.	(W. H.) Height due to Declination.	(L. III.) Observed Height.	Diff. H.	(W. T.) Time due to Declination.	(L. II.) Observed Time.	Diff. T.
h m	°	feet.	feet.		h m	h m	m
0 30	18	17·70	17·47	−·23	11 15	11 13·7	−1·3
1 30	15	17·95	17·74	−·21	11 1	10 59·5	−1·5
2 30	10	17·60	17·52	−·08	10 48	10 47·0	−1·0
3 30	5	16·55	16·08	−·47	10 40	10 37·1	−2·9
4 30	5	15·15	15·14	−·01	10 31	10 31·1	+ ·1
5 30	8	13·75	13·19	−·06	10 33	10 33·8	+ ·8
6 30	14	12·55	12·61	+·06	10 44	10 49·3	+5·3
7 30	19	12·20	12·38	+·18	11 12	11 16·1	+4·1
8 30	19	13·20	13·56	+·36	11 41	11 37·8	−3·2
9 30	23	14·50	14·95	+·45	11 47	11 45·0	−2·0
10 30	22	15·85	15·98	+·13	11 41	11 38·5	−2·5
11 30	22	16·70	16·73	+·03	11 28	11 34·2	+6·2

TABLE (A). Continued.

Moon's Transit.	Declination.	(W. H.) Height due to Declination.	(L. III.) Observed Height.	Diff. H.	(W. T.) Time due to Declination.	(L. II.) Observed Time.	Diff. T.
		February.					
h m	°	feet.	feet.		h m	h m	m
0 30	10	18·25	18·05	−·20	11 19	11 18·3	− ·7
1 30	5	18·30	18·04	−·26	11 5	11 8·2	+3·2
2 30	5	17·65	17·69	+·04	10 50	10 49·7	− ·3
3 30	8	16·55	16·73	+·18	10 39	10 37·3	−1·7
4 30	14	14·95	15·20	+·25	10 26	10 27·3	+1·3
5 30	18	13·40	13·39	−·01	10 22	10 26·3	+4·3
6 30	21	12·00	11·96	−·04	10 35	10 37·9	+2·9
7 30	21	11·95	11·78	−·17	11 10	11 8·0	−2·0
8 30	23	12·90	12·79	−·11	11 38	11 37·4	− ·6
9 30	22	14·60	14·47	−·13	11 47	11 51·0	+4·0
10 30	19	16·20	15·96	−·24	11 43	11 44·0	+1·0
11 30	15	17·35	17·39	+·04	11 32	11 31·5	− ·5
		March.					
0 30	5	18·3	18·38	+·08	11 20	11 19·1	− ·9
1 30	8	18·25	18·49	+·24	11 4	11 3·9	− ·1
2 30	13	17·5	17·63	+·13	10 47·5	10 49·4	+1·9
3 30	16	16·3	16·31	+·01	10 33	10 33·3	+ ·3
4 30	21	14·6	14·47	−·13	10 19	10 20·3	+1·3
5 30	22	13·1	13·01	−·09	10 17	10 16·2	− ·8
6 30	22	11·9	11·42	−·48	10 33	10 30·2	−2·8
7 30	22	11·85	11·31	−·54	11 8·5	11 5·8	−2·7
8 30	20	13·1	12·68	−·42	11 40	11 40·7	+ ·7
9 30	15	15·05	14·62	−·43	11 50·5	11 50·3	− ·2
10 30	10	16·65	16·54	−·11	11 46	11 44·9	−1·1
11 30	6	17·65	17·67	+·02	11 35	11 33·4	−1·6
		April.					
0 30	12	18·15	18·01	−·14	11 19	11 19·4	+ ·4
1 30	17	17·8	17·88	+·08	11	11 1·1	+1·1
2 30	20	17	17·01	+·01	10 42	10 43·4	+1·4
3 30	22	16·05	15·98	−·07	10 27	10 30·4	+3·4
4 30	23	14·45	14·44	−·01	10 15	10 14·2	− ·8
5 30	22	13·1	12·89	−·21	10 17	10 12·8	−4·2
6 30	20	12·1	11·44	−·66	10 36	10 31·8	−4·2
7 30	16	12·4	11·71	−·69	11 16·5	11 8·5	−8·0
8 30	11	13·65	13·28	−·37	11 45	11 44·0	−1·0
9 30	6	15·35	15·07	−·28	11 53	11 54·3	+1·3
10 30	5	16·75	16·61	−·14	11 46	11 48·2	+2·2
11 30	7	17·65	17·60	−·05	11 35	11 36·0	+1·0
		May.					
0 30	20	17·5	17·33	−·17	11 14	11 16·3	+2·3
1 30	22	17·3	17·15	−·15	10 57	10 57·9	+ ·9
2 30	23	16·75	16·56	−·19	10 39	10 38·6	− ·4
3 30	22	16	15·75	−·25	10 27	10 25·1	−1·9
4 30	20	14·65	14·48	−·17	10 20	10 16·0	−4·0
5 30	16	13·5	13·33	−·17	10 25	10 18·8	−6·2
6 30	12	12·65	12·35	−·30	10 47	10 41·9	−5·1
7 30	7	12·85	12·58	−·27	11 23	11 20·6	−2·4
8 30	5	13·85	13·75	−·10	11 46·5	11 46·9	+ ·4
9 30	7	15·35	15·01	−·34	11 53	11 52·5	− ·5
10 30	12	16·55	16·28	−·27	11 46	11 46·9	+ ·9
11 30	17	17·2	17·01	−·19	11 32	11 31·2	− ·8

Table (A). Continued.

Moon's Transit.	Declination.	(W. H.) Height due to Declination.	(L. III.) Observed Height.	Diff. H.	(W. T.) Time due to Declination.	(L. II.) Observed Time.	Diff. T.
			June.				
h m	°	feet.	feet.		h m	h m	m
0 30	23	17·2	16·73	−·47	11 12	11 13·4	+1·4
1 30	22	17·3	16·84	−·46	10 57	10 55·5	−1·5
2 30	20	17	16·74	−·26	10 42	10 39·8	−2·2
3 30	16	16·3	15·81	−·49	10 33	10 29·6	−3·4
4 30	11	15·05	14·69	−·36	10 28	10 24·9	−3·1
5 30	6	13·85	13·75	−·10	10 34	10 31·6	−2·4
6 30	5	12·95	13·07	+·12	10 52·5	10 52·9	+ ·4
7 30	8	12·8	13·09	+·29	11 22·5	11 24·6	+2·1
8 30	12	13·6	13·86	+·26	11 45	11 44·0	−1·0
9 30	17	14·95	14·93	−·02	11 50	11 49·2	− ·8
10 30	20	16·1	15·91	−·19	11 42	11 41·8	− ·2
11 30	22	16·7	16·57	−·13	11 28	11 28·9	+ ·9
			July.				
0 30	19	17·6	17·08	−·52	11 14	11 13·5	− ·5
1 30	16	17·9	17·14	−·76	11 1	10 57·8	−3·2
2 30	11	17·55	16·84	−·61	10 49	10 45·2	−3·8
3 30	6	16·55	16·29	−·26	10 39	10 37·6	−1·4
4 30	5	15·15	14·96	−·19	10 31	10 33·1	+2·1
5 30	8	13·75	13·83	+·08	10 33	10 34·2	+1·2
6 30	13	12·6	12·87	+·27	10 45	10 51·6	+6·6
7 30	18	12·25	12·72	+·47	11 13	11 12·2	−0·8
8 30	21	13·05	13·39	+·34	11 40	11 38·9	−1·1
9 30	22	14·6	14·64	+·04	11 45·5	11 48·5	+3·0
10 30	23	15·75	15·61	−·14	11 40	11 39·6	− ·4
11 30	22	16·7	16·52	−·18	11 28	11 27·2	− ·8
			August.				
0 30	11	18·2	17·68	−·52	11 19	11 17·5	−1·5
1 30	6	18·3	17·96	−·34	11 5	11 4·5	− ·5
2 30	5	17·65	17·37	−·28	10 51	10 51·6	+ ·6
3 30	7	16·55	16·46	−·09	10 39	10 35·4	−3·6
4 30	13	14·95	15·03	+·08	10 27	10 30·2	+3·2
5 30	17	13·45	13·46	+·01	10 23	10 28·7	+5·7
6 30	21	12	12·00	·00	10 35	10 38·4	+3·4
7 30	22	11·85	11·80	−·05	11 8	11 8·6	+ ·6
8 30	25	12·7	12·82	+·12	11 36	11 37·4	+1·4
9 30	22	14·6	14·34	−·26	11 47·5	11 46·9	− ·6
10 30	19	16·2	15·84	−·36	11 43	11 42·4	− ·6
11 30	16	17·25	16·99	−·26	11 32	11 30·1	−1·9
			September.				
0 30	4	18·3	18·30	·00	11 20	11 20·4	+ ·4
1 30	7	18·3	18·39	+·09	11 10	11 3·0	−7·0
2 30	12	17·5	17·89	+·39	10 48	10 48·8	+ ·8
3 30	17	16·25	16·50	+·25	10 32·5	10 34·0	+1·5
4 30	20	14·65	14·94	+·29	10 20	10 21·3	+1·3
5 30	22	13·1	13·19	+·09	10 17	10 16·9	− ·1
6 30	23	11·8	11·63	−·17	10 32	10 32·1	+ ·1
7 30	22	11·85	11·78	−·07	11 8	11 4·8	−3·2
8 30	20	13·1	12·86	−·24	11 40	11 39·3	− ·7
9 30	16	15	14·78	−·22	11 50	11 50·3	+ ·3
10 30	11	16·6	16·38	−·22	11 46	11 46·4	+ ·4
11 30	6	17·65	17·69	+·04	11 35	11 35·2	+ ·2

TABLE (A). Continued.

Moon's Transit.	Declination.	(W. H.) Height due to Declination.	(L. III.) Observed Height.	Diff. H.	(W. T.) Time due to Declination.	(L. II.) Observed Time.	Diff. T.
October.							
h m	°	feet.	feet.		h m	h m	h m
0 30	11	18·2	18·52	+·32	11 19	11 19·1	+ ·1
1 30	16	17·85	18·28	+·43	11 1	11 1·7	+ ·7
2 30	20	17	17·62	+·62	10 42	10 42·3	+ ·3
3 30	22	16	16·48	+·46	10 27	10 26·7	− ·3
4 30	23	14·45	14·76	+·31	10 15	10 14·7	− ·3
5 30	22	13·1	13·15	+·05	10 16	10 12·2	−3·8
6 30	20	12·1	11·70	−·40	10 36	10 29·1	−6·9
7 30	17	12·3	11·95	−·35	11 15	11 11·0	−4·0
8 30	13	13·55	13·37	−·18	11 45	11 45·1	+ ·1
9 30	7	15·35	15·13	−·22	11 53	11 54·0	+1·0
10 30	4	16·75	16·83	+·08	11 46	11 48·7	+2·7
11 30	7	17·65	17·92	+·27	11 35	11 35·3	+ ·3
November.							
0 30	20	17·5	17·95	+·45	11 14	11 18·4	+4·4
1 30	22	17·3	17·73	+·43	10 57·5	10 58·7	+1·2
2 30	23	16·75	16·99	+·24	10 38	10 39·1	+1·1
3 30	22	16	16·01	+·01	10 27	10 24·1	−2·9
4 30	20	14·65	14·50	−·15	10 20	10 14·7	−5·3
5 30	16	13·5	13·33	−·17	10 24·5	10 17·1	−7·4
6 30	13	12·6	12·28	−·32	10 45	10 40·4	−4·6
7 30	7	12·85	12·62	−·23	11 23	11 21·3	−1·7
8 30	5	13·85	13·95	+·10	11 47	11 47·9	+ ·9
9 30	6	15·35	15·61	+·26	11 53	11 53·8	+ ·8
10 30	12	16·55	16·73	+·18	11 46	11 46·7	+ ·7
11 30	17	17·2	17·50	+·30	11 32	11 32·9	+ ·9
December.							
0 30	23	17·2	17·25	+·05	11 12·5	11 12·9	+ ·4
1 30	21	17·4	17·28	−·12	10 58	10 54·8	−3·2
2 30	19	17·1	16·80	−·30	10 43	10 40·8	−2·2
3 30	16	16·3	15·89	−·41	10 33	10 28·7	−4·3
4 30	11	15·05	14·95	−·10	10 28	10 23·9	−4·1
5 30	8	13·75	13·82	+·07	10 33	10 30·6	−2·4
6 30	5	12·95	13·06	+·11	10 52·5	10 52·5	− ·0
7 30	7	12·85	13·21	+·36	11 23	11 23·4	+ ·4
8 30	12	13·6	14·07	+·47	11 45	11 45·7	+ ·7
9 30	17	14·95	15·35	+·40	11 50	11 48·2	−1·8
10 30	20	16·05	16·38	+·33	11 42·5	11 42·2	− ·3
11 30	21	16·8	17·14	+·34	11 29	11 27·7	−1·3

TABLE (B. H.). The Lunar Effect rejected.

Residual quantities. *Heights.*

☽'s Transit.	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.
Means	+·01	−·05	−·14	−·21	−·20	−·15	−·12	−·16	+·02	+·12	+·09	+·10
	Remainders.											
h m												
0 30	−·24	−·15	+·22	+·07	+·03	−·32	−·40	−·36	−·02	+·20	+·36	−·05
1 30	−·22	−·21	+·38	+·29	+·05	−·31	−·64	−·18	+·07	+·31	+·34	−·22
2 30	−·09	+·09	+·27	+·22	+·01	−·11	−·49	−·12	+·37	+·50	+·15	−·40
3 30	−·48	+·23	+·15	+·14	−·05	−·34	−·14	+·07	+·23	+·36	−·08	−·51
4 30	−·02	+·30	+·01	+·20	+·03	−·21	−·07	+·24	+·27	+·19	−·24	−·20
5 30	−·07	+·04	+·05	−·00	+·03	+·05	+·20	+·17	+·07	−·07	−·26	−·03
6 30	+·05	+·01	−·34	−·45	−·10	+·27	+·39	+·16	−·19	−·52	−·41	+·01
7 30	+·17	−·12	−·40	−·48	−·07	+·44	+·59	+·11	−·09	−·47	−·32	+·26
8 30	+·35	−·06	−·28	−·16	+·10	+·41	+·46	+·28	−·26	−·30	+·01	+·37
9 30	+·44	−·08	−·29	−·07	−·14	+·13	+·16	−·10	−·24	−·34	+·17	+·30
10 30	+·12	−·19	+·03	+·07	−·07	−·04	−·02	−·20	−·24	−·04	+·09	+·23
11 30	+·02	+·09	+·16	+·16	+·01	+·02	−·06	−·10	+·02	+·15	+·21	+·24

TABLE (C. H.). The Remainders in Table (B. H.) corrected by Interpolation.

Periodical and non-periodical part of the Solar Effect, in *tenths of feet.*

☽'s Transit.	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.
h m												
0 30	− 1	0	+ 2	+ 2	0	− 3	− 5	− 3	0	+ 3	+ 4	0
1 30	− 3	0	+ 3	+ 3	0	− 4	− 6	− 3	+ 1	+ 4	+ 3	− 2
2 30	− 3	0	+ 3	+ 3	0	− 4	− 5	− 2	+ 3	+ 5	+ 2	− 4
3 30	− 3	0	+ 2	+ 2	0	− 3	− 3	− 1	+ 3	+ 4	+ 1	− 5
4 30	− 2	0	+ 1	+ 1	0	− 1	− 1	0	+ 2	+ 2	0	− 3
5 30	0	0	− 1	− 1	0	+ 2	+ 2	+ 1	− 1	− 2	− 2	− 1
6 30	+ 1	0	− 4	− 4	0	+ 4	+ 4	+ 2	− 2	− 5	− 4	+ 1
7 30	+ 2	0	− 5	− 5	0	+ 5	+ 6	+ 3	− 3	− 5	− 3	+ 2
8 30	+ 3	0	− 4	− 4	0	+ 4	+ 5	+ 3	− 3	− 3	− 2	+ 3
9 30	+ 4	0	− 2	− 2	0	+ 3	+ 3	+ 2	− 3	− 3	− 1	+ 3
10 30	+ 2	0	− 1	− 1	0	+ 1	0	0	− 2	− 2	+ 0	+ 2
11 30	0	0	+ 2	+ 2	0	− 1	− 2	− 2	0	+ 2	+ 2	+ 1
Non-periodical	0	−1	− 2	− 2	−2	− 2	− 1	− 1	0	+ 1	+ 1	+ 1

SOLAR TABLE (W. H.). Correction of the Heights for the Sun's Effect, in *tenths of feet.*

☽'s Transit.	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.
h m												
0 30	− 1	− 1	0	0	− 2	− 5	− 6	− 4	0	+ 4	+ 5	+ 1
1 30	− 3	− 1	+ 1	+ 1	− 2	− 6	− 7	− 4	+ 1	+ 5	+ 4	− 1
2 30	− 3	− 1	+ 1	+ 1	− 2	− 6	− 6	− 3	+ 3	+ 6	+ 3	− 3
3 30	− 3	− 1	+ 0	0	− 2	− 5	− 4	− 2	+ 3	+ 5	+ 2	− 4
4 30	− 2	− 1	− 1	− 1	− 2	− 3	− 2	− 1	+ 2	+ 3	+ 1	− 2
5 30	0	− 1	− 3	− 3	− 2	0	+ 1	0	− 1	− 1	− 1	0
6 30	+ 1	− 1	− 6	− 6	− 2	+ 2	+ 3	+ 1	− 2	− 4	− 3	+ 2
7 30	+ 2	− 1	− 7	− 7	− 2	+ 3	+ 5	+ 2	− 3	− 4	− 2	+ 3
8 30	+ 3	− 1	− 6	− 6	− 2	+ 2	+ 4	+ 2	− 3	− 2	− 1	+ 4
9 30	+ 4	− 1	− 4	− 4	− 2	+ 1	+ 2	+ 1	− 3	− 2	0	+ 4
10 30	+ 2	− 1	− 3	− 3	− 2	− 1	− 1	− 1	− 2	− 1	+ 1	+ 3
11 30	+ 0	− 1	0	0	− 2	− 3	− 3	− 3	0	+ 1	+ 3	+ 2

TABLE (B. T.). The Lunar Effects rejected.

Residual Quantities.—*Times.*

☽'s Transit.	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.
Means.....	+·2	+·9	−·5	−·6	−1·4	−·8	+·1	+·5	−·5	−·8	−1·0	−1·5
	Remainders.											
h m	m	m	m	m	m	m	m	m	m	m	m	m
0 30	−1·5	−1·6	− ·4	+1·0	+3·7	+2·2	− ·6	−2·0	+ ·9	+ ·9	+5·4	+1·9
1 30	−1·7	+2·3	+ ·4	+1·7	+2·3	− ·7	−3·3	−1·0	−6·5	+1·5	+2·2	−1·7
2 30	−1·2	−1·2	+2·4	+2·0	+1·0	−1·4	−3·9	+ ·1	+1·3	+1·1	+2·1	− ·7
3 30	−3·1	−2·6	+ ·8	+4·0	− ·5	−2·6	−1·5	−4·1	+2·0	+ ·5	−1·9	−2·8
4 30	− ·1	+ ·4	+1·8	− ·2	−2·6	−2·3	+2·0	+2·7	+1·8	+ ·5	−4·3	−2·6
5 30	+ ·6	+3·4	− ·3	−3·6	−4·8	−1·6	+1·1	+5·2	+ ·4	−3·0	−6·4	− ·9
6 30	+5·1	+2·0	−2·3	−3·6	−3·7	+1·2	+6·5	+2·9	+ ·6	−6·1	−3·6	+1·5
7 30	+3·9	−2·9	−2·2	−7·4	−1·0	+2·9	− ·9	+ ·1	−2·7	−3·2	− ·7	+1·9
8 30	−3·4	−1·5	+1·2	− ·4	+1·8	− ·2	−1·2	+ ·9	− ·2	+ ·9	+1·9	+2·2
9 30	−2·2	+3·1	+ ·3	+1·9	+ ·9	− ·0	−2·9	−1·1	+ ·8	+1·8	+1·8	·3
10 30	−2·7	+ ·1	− ·6	+2·8	+2·3	+ ·6	− ·5	−1·1	+ ·9	+3·5	+1·7	+1·2
11 30	+6·0	−1·4	−1·1	+1·6	+ ·6	+1·7	− ·9	−2·4	+ ·7	+1·1	+1·9	+ ·2

TABLE (C. T.). Periodical part of the Solar Effect.

The remainders in Table (B. T.) corrected by interpolation.

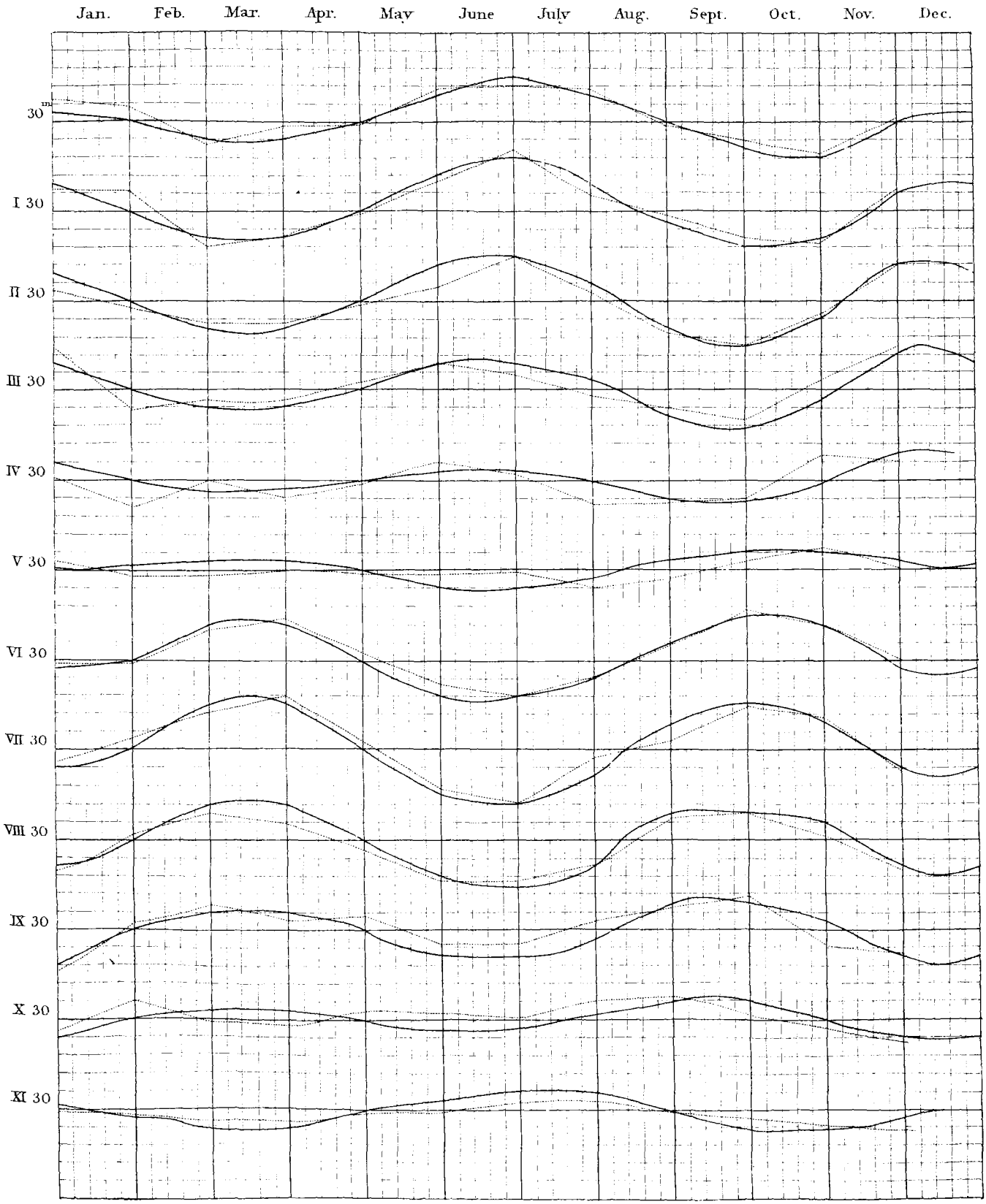
☽'s Transit.	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.
h m												
0 30												
1 30	−2	0	+2	+3	+2	0	−3	−4	−2	0	+2	0
2 30	−2	0	+3	+3	+2	0	−3	−2	0	+2	+2	0
3 30	−3	0	+3	+4	+3	0	−4	−4	0	+1	+1	0
4 30												
5 30	+3	0	−3	−4	−3	0	+4	+4	0	−4	−4	0
6 30	+4	0	−4	−5	−4	0	+5	+5	0	−5	−5	0
7 30	+4	0	−4	−5	−4	0	+2	+2	0	−3	−3	0
8 30												
9 30												
10 30												
11 30												

SOLAR TABLE (W. T.). Correction of the Times for the Sun's Effect.

For this correction we may use Table (C. T.), omitting the correction for the numbers there omitted till further investigations have made the Table more correct: and introducing also the interpolated nonperiodical correction, viz.

Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.
	m		m	m	m		m		m	m	m
0	+1	0	−1	−1	−1	0	+1	0	−1	−1	−1

SOLAR CORRECTION OF HEIGHTS. PERIODIC PARTS. LIVERPOOL.



The ordinates are here six times too large. The positive ordinates are measured downwards.

J. Basire sculp.

Tides

At the close of the last session, the Council received a communication from Capt. *Basil Hall,* expressing his readiness to attend to any instructions on subjects wherein he might be of service to the science of Astronomy, in his intended voyage to the South Seas. They availed themselves of the offer of this intelligent and enterprizing officer, and requested his attention to the following principal points.

To observe, as frequently as possible, the conjunctions of the moon and planets with the fixed stars; measuring with a micrometer their differences of right ascension and declination, or taking the measure of their distance in a straight line: the time and place of observation being correctly noted.

To look out for occultations of the fixed stars by the moon; and particularly for those which may be presumed to be of short duration, with a view to the illustration of the theory of Cagnoli respecting this mode of determining the figure of the earth. And it was remarked to him that, as the moon was now, and would be for some few years, in such a position with respect to her nodes as to pass over the *Pleiades* every lunation, it would be particularly desirable to look out for the occultations of those stars.

To make frequent sweeps of the heavens, with a telescope having a large field of view and small magnifying power, for the purpose of discovering any comets; and to note the progress and circumstances of the same; making sketches of their appearance. The Council at that time were not in possession of the calculated place of the comet which is expected to return in 1822. But, having since received an ephemeris of its apparent positions, computed by M. Encke, they will endeavour to forward it to Capt. Hall, and request him to look out for the same: a circumstance the more to be desired, since it is expected to assume a different appearance in the southern hemisphere to that which it will present in Europe.

It appeared unnecessary to remind Capt. Hall of the several eclipses of the sun and moon, together with the eclipses of Jupiter's satellites, the transit of Mercury over the sun's disc on Nov. 4, 1822, and the several phænomena noted in the various ephemerides; and which would of course be the object of his attention without any particular suggestions from this Society. But the Council took the liberty of directing his attention to certain points, should he

Part

be favourably situated for observing any of the solar eclipses, or the transit of Mercury.

They also requested him to make observations on the position of Mars, at the time of his opposition in February 1822, with respect to the three following fixed stars, which are situated near the path of his orbit, and whose mean places for the day of opposition are here stated: viz.

Star.	AR. in time.	N. Dec.
	h ′ ″	° ′ ″
42 Leonis	10 . 12 . 15,9	15 . 52 . 11,9
446 Mayer	19 . 17,6	15 . 1,0
i 46 Leonis	22 . 41,6	2 . 53,3

When Mars approaches either of these stars, the differences in right ascension and declination, between the planet and the star, must be taken as accurately as possible for several successive days, with a micrometer; or their distances measured, in a straight line: the time and place being correctly noted down. Such observations, compared with corresponding ones made in England, will serve to determine the parallax of Mars.

With the same view it was proposed to Capt. Hall, to make observations on Venus, at the time of her inferior conjunction in March 1822, by comparing her with α Ceti, on the parallel of which she will be a day or two before and after the conjunction: a simple and easy method being at the same time suggested, whereby Venus might be readily found in the day time, notwithstanding her proximity to the sun.

The attention of Capt. Hall was also directed to the subject of refraction, in order to determine whether the quantity of refraction varies (cæteris paribus) in different parts of the globe; or whether any new light can be thrown on this uncertain phænomenon, in the various places he might visit.

Upon most of these subjects, it must be evident to the members of the society, that there is a necessity of having corresponding and simultaneous observations

in this country: otherwise the labours of Capt. Hall (should he be favourably situated for observation) will be in a great measure lost to the public. Those members, therefore, who possess the requisite instruments, are called upon to co-operate on these points, and to register their observations, in order that they may be compared at a future opportunity. Without this assistance the efforts of the Council will have been exerted in vain, and the time of an active observer employed to little or no advantage. And here the Council cannot avoid suggesting, to those astronomers who possess the requisite instruments, the propriety of observing and recording the position (in right ascension and *declination*) of those stars which are situated near to, and on the same parallel with, any of the planets near the time of their opposition; since such observations would serve as a mode of comparison for those observers who, with less powerful instruments, might be more favourably situated for making observations at this important point of the orbit of the planets.

The Council further represented to Capt. Hall that it was scarcely to be expected that a traveller, who is frequently changing his situation, can make many *fundamental* observations in astronomy; such as may serve as a basis for future researches. But, that he might do much in *comparative* astronomy; by taking those elements as correct which have been determined by observations made in fixed observatories, and comparing the objects of research therewith: some examples of which have been already alluded to; and others were suggested for his consideration.

But there was still another, and a very important point, to which the attention of Capt. Hall was requested (and the same cannot be too strongly pressed on any future voyager, or settler in distant climates, favourably situated for such inquiries): namely, to make regular observations on the tides, in favourable situations for determining their theory. It is well known that the tides, adjoining large continents and their contiguous islands, are so affected with the various sources of error arising from the situation of the harbour and the nature of the bottom of the ocean for a considerable distance around it, that not only a very long series of observations is required to destroy or compensate those errors, and allow the true co-efficients of the formulæ, for determining their value, to appear with any tolerable exactness; but also the co-efficients themselves, so determined, are essentially affected by such local peculiarities; and consequently incapable of affording any thing beyond relative results. In

order to obtain results unaffected with these inappreciable causes of error, the places of observation should, if possible, be chosen on small islands shooting up abruptly from an unfathomable depth in the midst of a wide ocean, extending 30 or 40 degrees, at least, in all directions; or, at all events, a very great distance from any large continent. The islands in the Pacific and South Atlantic oceans, which are bedded on coral banks or the effect of volcanic eruptions, are precisely of this nature. If we may trust the accounts of voyagers, many of these are mere vertical shafts, or insulated columns, shooting at once from the very bottom of the ocean, without shoals, or any gradual declivity. Round these, the tides must rise and fall with perfect uniformity: and it is exceedingly probable that, in these cases, a much shorter series of observations would be requisite for framing accurate results; and that even those of a single month, in moderately calm weather, might have considerable value in the present improved state of the theory. The situation of the Gallapagos islands, on which Capt. Hall will probably spend some time (it being one of the stations at which he proposes to swing the invariable pendulum), possesses peculiarities which entitle it to notice, although it does not satisfy all the conditions. It is immediately under the equator: and should he be there about the time of the equinox, the very vertex of the aqueous spheroid, which will then pass over the spot, may be made the subject of his observations. These islands likewise present another remarkable peculiarity of situation. For, according to the results obtained from a theorem given by M. Biot, they stand within a very few degrees of the point where the magnetic intersects the terrestrial equator. It is therefore desirable that observations should be made with a view to ascertain the accuracy of this conclusion. It may also be remarked, that it is near this spot that the magnetic equator is supposed to deviate into the serpentine form, as mentioned by the same eminent writer.

The formation of an Astronomical Library being one of the objects of the Society, the attention of the Council has been directed thereto. At present, however, they have not thought it advisable to expend any money on this department. Nevertheless they are happy in announcing that the foundation has been laid, by the donation of many valuable works on the science by several members of the society.

It having been represented to the Council, that the observations made at

RAPPORT

Sur la mesure de la méridienne de France, et les résultats qui en ont été déduits pour déterminer les bases du nouveau système métrique (1).

CITOYENS,

EMPLOYER pour unité fondamentale de toutes les mesures un type pris dans la nature même, un type aussi inaltérable que le globe que nous habitons; proposer un système métrique dont toutes les parties sont intimement liées entre elles, toutes dépendantes de ce type primitif, et dont les multiples et les subdivisions suivent une progression naturelle, simple, facile à saisir, et toujours uniforme : c'est assurément une idée belle, grande, sublime, digne du siècle éclairé dans lequel nous vivons. Aussi l'Académie des sciences, qui se rappeloit que, dès

(1) Il avoit été lu à la classe des sciences physiques et mathématiques, au nom de la commission des poids et mesures, deux rapports particuliers: l'un, le 6 prairial an 7, par le citoyen Van-Swinden, sur la mesure de la méridienne et la détermination du mètre; l'autre, le 11 du même mois, par le citoyen Tralles, sur l'unité des poids. La classe a décidé que ces deux rapports seroient réunis et refondus en un seul, pour être lu à une séance générale de l'Institut; et elle a chargé la commission de nommer un de ses membres pour en faire la rédaction. Cette rédaction a été faite par le citoyen Van-Swinden.

sa naissance, la théorie et les expériences de Huygens sur le pendule simple avoient fixé les yeux du monde savant sur l'invariabilité et l'universalité des mesures, qui en sentoit toute l'importance, qui connoissoit les vœux des mathématiciens sur ce sujet, qui avoit vu l'un de ses membres, le célèbre La Condamine, s'employer avec un grand zèle pour en faire goûter l'idée, et pour détruire les objections que l'ignorance et la cupidité ne cessoient alors, comme elles ne cessent encore aujourd'hui, d'y opposer (1), ne manqua-t-elle pas de saisir le moment même auquel le peuple français commençoit à s'occuper de sa régénération politique et sociale, pour reprendre cette matière intéressante, dont l'exécution n'attendoit, peut-être, que l'instant où l'impulsion donnée aux esprits feroit saisir avidement tout ce qui peut tendre au bien public, et où les circonstances permettroient de s'en occuper sans entraves et avec succès. Consultée bientôt par l'Assemblée constituante, dont l'attention venoit d'être fixée sur cet objet par la proposition qu'en fit le citoyen Talleyrand (2), et chargée par elle de déterminer l'unité des mesures et celle des poids, elle employa, par des raisons sages qu'elle a développées dans le temps (3), pour base de tout le système métrique, le quart du méridien terrestre compris entre l'équateur et le pole boréal; elle adopta la dix-millionième partie de cet arc pour l'unité des mesures, et

(1) Mémoires de l'Académie pour 1748.

(2) Décret du 8 mai 1790.

(3) Mémoires de l'Acad. pour 1789.

nomma *mètre* cette unité, qu'elle appliqua également aux mesures de surface et de contenance, en prenant pour l'unité des premières le carré du décuple, et pour celle de contenance le cube de la dixième partie du mètre; elle choisit pour *unité de poids* la quantité d'eau distillée que contient ce même cube lorsqu'elle est réduite à un état constant que la nature elle-même présente; enfin elle décida que les multiples et les sous-multiples de chaque sorte de mesure, soit de poids, soit de contenance, soit de surface, soit de longueur, seroient toujours pris en progression décimale, comme la plus simple, la plus naturelle et la plus facile pour le calcul dans le système de numération que l'Europe entière emploie depuis des siècles. Tels sont les points fondamentaux et essentiels du nouveau système métrique que l'Académie a proposé et qui a été adopté par l'Assemblée constituante, lesquels, sous des noms différens, à la vérité, de ceux dont l'Académie avoit fait choix, ont été consacrés par la loi du 18 germinal de l'an 3 de la République.

Mais, puisque la base du nouveau système métrique dépend du quart du méridien terrestre, il faut connoître la grandeur de cet arc, sinon avec une précision extrême, au moins avec une précision suffisante pour la pratique. On avoit déja fait en France, depuis la fin du dernier siècle, différentes opérations pour déterminer la grandeur de plusieurs arcs de la méridienne qui traverse ce vaste empire; et quoiqu'il restât des doutes sur l'entière exactitude de ces opérations, malgré les vérifications qu'on

en avoit faites à différentes reprises, on étoit autorisé à croire, d'après les recherches du célèbre La Caille, que le degré moyen ne s'écarteroit pas beaucoup de 57027 toises; conséquemment que le quart du méridien en contiendroit 5132430, et que la dix-millionième partie de cet arc répondroit à 443 lignes $\frac{443}{1000}$. Dans la juste impatience où l'on étoit de jouir du grand bienfait de mesures exactes, uniformes, universelles, on attribua *provisoirement* au mètre la longueur de 443 lignes $\frac{44}{100}$, persuadé, comme on croyoit pouvoir l'être, que les déterminations plus précises qu'on attendoit n'apporteroient à cette grandeur que de légers changemens.

Cependant l'Académie, qui considéroit cette matière sous son vrai point de vue, dans son ensemble, et sous tous ses rapports, sous le rapport de l'utilité publique, sous celui de sa liaison intime avec les points les plus importans de la physique céleste, sous le rapport même de la gloire nationale, à laquelle il importe que les bases d'un nouveau système métrique qu'on propose à une grande nation, qu'on voudroit voir adopter par toutes, soient déterminées avec la plus grande précision, conçut le beau projet de faire faire une nouvelle mesure de la méridienne qui traverse la France, de l'étendre au-delà des frontières, d'aller jusqu'à Barcelone, et de faire servir ce grand arc à déterminer le quart du méridien de la Terre. L'Assemblée constituante adopta ce vaste projet, elle en confia l'exécution à l'Académie : celle-ci nomma, sans délai, plusieurs de ses membres pour s'occuper des différentes parties qui font l'ensemble du

système métrique; et définitivement elle chargea de la mesure du méridien les citoyens Méchain et Delambre, si dignes à tous égards de la mission glorieuse, mais pénible, dont on les a honorés. L'Institut nomma, par la suite, le citoyen Lefévre-Gineau pour faire les expériences relatives à la détermination de l'unité de poids: il a prouvé, par la beauté et l'exactitude de son travail, combien il étoit digne d'être associé à ses illustres confrères.

Cette grande et importante opération projetée par l'Académie des sciences pour l'établissement d'un nouveau système métrique, commencée par ses ordres, et heureusement terminée sous les auspices de l'Institut après sept années de peines et de travaux, est remarquable à plusieurs égards. Elle l'est d'abord par l'étendue de l'arc terrestre qu'on a employé, et qui, étant de plus de neuf degrés et deux tiers, surpasse tous ceux qui avoient été mesurés jusqu'ici. Elle l'est ensuite par l'extrême exactitude avec laquelle toutes les parties en ont été exécutées: mesure géodésique de l'arc terrestre, observations astronomiques, travail pour la fixation de l'unité de poids, expériences sur la longueur du pendule, tout a marché de pair; chaque genre a été traité avec la même précision. Elle est enfin remarquable, et peut-être unique, par le degré d'authenticité dont elle est revêtue. En effet, l'Institut a desiré, non seulement que des commissaires choisis dans son sein examinassent tout ce qui avoit été fait, mais encore que des savans étrangers pussent se joindre à eux pour faire un travail commun.

Le gouvernement a accueilli ce vœu ; il a invité les puissances alliées ou neutres à envoyer des députés pour cet objet. Plusieurs se sont rendues à cette invitation ; et ces députés, réunis aux commissaires français, composent la commission des poids et mesures (1), qui s'est assemblée depuis quelques mois dans ce palais, et sous vos auspices, pour fixer définitivement la grandeur des bases du nouveau système métrique. Cette commission a pris une connoissance intime de tous les détails de chaque observation, de chaque expérience ; elle en a pesé les circonstances : conjointement avec les observateurs eux-mêmes, elle a déduit des observations les résultats qui devoient servir au calcul, et a arrêté les unités de

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(1) Voici, par ordre alphabétique, les noms des membres de la commission des poids et mesures :

Aeneae, député de la république batave ;
Balbo, député du roi de Sardaigne, remplacé ensuite par le citoyen Vasalli ;
Borda, mort en ventose dernier ;
Brisson ;
Bugge, député du roi de Danemarck ;
Ciscar, député du roi d'Espagne ;
Coulomb ;
Darcet ;
Delambre ;
Fabroni, député de la Toscane ;
Lagrange ;
Laplace ;
Lefévre-Gineau ;
Legendre ;
Franchini, député de la république romaine ;
Mascheroni, député de la république cisalpine ;
Méchain ;
Multedo, député de la république ligurienne ;
Pederayes, député du roi d'Espagne ;
Prony ;
Tralles, député de la république helvétique ;
Wan-Swinden, député de la république batave ;
Vasalli, député du gouvernement provisoire du Piémont.

mesures et de poids, résultats définitifs de tout le travail. Jamais pareille opération n'avoit été soumise à pareille épreuve ; et la commission se fait un devoir et un plaisir de faire connoître à l'Institut que les citoyens Méchain, Delambre et Lefévre-Gineau se sont empressés à faire passer sous ses yeux jusqu'aux moindres détails de leurs registres originaux ; qu'ils lui ont donné sur chaque objet tous les éclaircissemens possibles ; qu'ils lui ont expliqué avec précision tous les instrumens dont ils se sont servis ; qu'ils ont rendu compte des méthodes qu'ils ont employées ; qu'ils ont prévenu les desirs des commissaires sur tous les points, avec toute la complaisance qu'on pouvoit attendre de confrères et d'amis, et avec cette noble franchise qui caractérise des observateurs exacts, lesquels, loin de redouter un examen sévère, desirent, au contraire, qu'on le fasse rouler minutieusement sur tous les détails, et qu'on le pousse même jusqu'au scrupule, bien sûrs que c'est le meilleur moyen de faire paroître la vérité dans tout son éclat.

Chargé de vous rendre compte du travail de ces excellens observateurs, et de ce qui a été fait par la commission des poids et mesures pour la fixation des *unités* qui servent de base au nouveau système métrique, qu'il me soit permis, pour mettre de l'ordre dans la multitude des matières que je dois soumettre à votre jugement, de vous entretenir d'abord de ce qui concerne la mesure de l'arc du méridien, et la détermination du mètre, ou de l'*unité* des mesures linéaires, qui en est le résultat ;

de vous exposer ensuite les expériences qu'il a fallu faire pour parvenir à fixer l'*unité* du poids; enfin, en vous présentant les étalons de ces *deux unités*, de vous proposer quelques réflexions sur leur nature, leur usage, et la manière de les rétablir avec la plus grande exactitude, quand même tous les étalons viendroient à être anéantis, et qu'il n'en restât que le nom : avantage précieux de ces nouvelles mesures, et qui leur assure le titre de *mesures invariables*.

Commençons par ce qui concerne la mesure de la méridienne. Les citoyens Méchain et Delambre se sont partagé cet immense travail. La partie boréale, depuis Dunkerque jusqu'à Rodès, est échue à celui-ci, et le citoyen Méchain a fait tout le reste depuis Rodès jusqu'à Barcelone; il a vivement regretté que les circonstances ne lui aient pas permis de prolonger ses opérations jusqu'à l'île de Cabrera, comme il l'avoit desiré. Il avoit même fait tous les préparatifs pour ce travail; il avoit entrepris les courses nécessaires pour examiner le local et constater les stations qu'il conviendroit d'employer; il a tracé sur le papier les triangles qu'il faudra mesurer : de sorte que toute cette partie est ébauchée, et que, grace à son activité et aux soins qu'il s'est donnés sur cet objet, il sera facile d'ajouter cet arc à celui qui vient d'être mesuré, et de prolonger encore la méridienne de deux degrés. Espérons que des circonstances favorables permettront d'exécuter un jour ce qui n'a pu l'être jusqu'ici.

Vous savez qu'il faut, pour la détermination de la

méridienne, quatre genres d'observations : d'abord des observations géodésiques, qui consistent à mesurer tant les angles que font entre elles les stations qu'on a choisies, que ceux d'élévation ou de dépression de chacune des stations par rapport à celle à laquelle on pointe l'instrument, afin de pouvoir réduire à l'horizon les angles primitivement observés, et de former une chaîne non interrompue de triangles, qui se termine aux deux extrémités de la méridienne : il s'agit ensuite de mesurer des bases, qu'on lie à la chaîne des triangles ; l'une d'elles sert à déterminer par le calcul les côtés de chaque triangle, et l'autre est employée à vérifier l'opération et à la rectifier, s'il est nécessaire : il faut, en troisième lieu, connoître la direction des côtés des triangles par rapport à la méridienne ; ce qui exige des observations d'azimuth : enfin il est nécessaire de faire des observations astronomiques pour connoître l'arc céleste auquel répond l'arc terrestre de la méridienne qu'on a mesuré géodésiquement. Nous allons reprendre ces quatre genres d'observations, pour faire connoître ce que les observateurs ont fait, quel est le degré d'exactitude auquel ils sont parvenus, quelle est la manière dont la commission a discuté leur travail et s'est convaincue de la précision rare avec laquelle cette opération a été exécutée.

La partie géodésique forme un travail long et pénible par sa nature, mais qui a été singulièrement augmenté par les différens obstacles que les observateurs ont eus à surmonter. Les circonstances des temps pendant lesquels

ils ont fait leurs opérations, et dont nous ne vous rappellerons pas le souvenir, en ont fait naître un grand nombre; mais les observateurs ont trouvé des ressources contre ce genre d'obstacles, dans leur fermeté, dans leur courage, dans leur prudence, et dans ce zèle actif qui les a engagés à supporter les peines les plus cuisantes, les privations les plus dures, les fatigues les plus rudes, plutôt que de négliger le travail qui leur avoit été confié, ou même de passer légèrement sur ce qui pouvoit contribuer à sa perfection. A ces obstacles s'en joignoient d'autres produits par des circonstances locales: souvent, et sur-tout dans la partie boréale et jusqu'à Bourges, au lieu d'employer des signaux faits exprès et placés à volonté, on a été obligé de se servir de clochers. Les circonstances et la nature du terrain empêchoient d'en agir autrement: on avoit d'ailleurs l'intention de tirer de cette nouvelle mesure de la méridienne tout le parti possible pour vérifier l'ancienne opération; ce qui a exigé beaucoup de recherches, quelquefois infructueuses, pour constater l'identité des stations. L'intérieur des clochers rendoit l'observation très-pénible, et celle au centre de la station ordinairement impossible: il falloit donc imaginer des moyens pour déterminer ce centre avec exactitude, et y réduire l'observation faite d'un autre point. La figure des clochers exigeoit beaucoup d'attention pour être sûr qu'on pointoit constamment sur la même arête, et que le rayon visuel passoit par le centre; ce qui n'étoit pas toujours facile. Les différentes manières dont les objets ronds sont éclairés

à différentes heures du jour, produiroient encore des erreurs si on n'y avoit égard. Les signaux même exigent de l'attention, selon qu'ils se projettent différemment. Il s'agissoit d'étudier la nature des erreurs qui pouvoient résulter de ces différentes causes, et de trouver des formules pour en calculer l'effet. Ce sont autant de recherches que les observateurs ont faites. L'un d'eux, le citoyen Delambre, vient de publier les siennes, et toutes les méthodes de réduction qu'il a employées, dans un mémoire singulièrement intéressant (1); et si le citoyen Méchain faisoit également part au public de ses profondes méditations sur ces objets, la classe des livres de sciences se trouveroit derechef enrichie d'un ouvrage du premier mérite. En un mot, c'est en employant tout ce qu'une longue habitude d'observer leur donnoit de dextérité, ce que leur sagacité leur fournissoit de moyens pour discerner et pressentir même les différentes causes d'erreurs qui pouvoient avoir lieu, et leurs connoissances mathématiques de ressources pour les calculer, que les citoyens Méchain et Delambre sont parvenus à vaincre tous les obstacles, et à élever un monument éternel à la gloire de l'Académie, de l'Institut, des Sciences, de la Nation française même; gloire à laquelle, grace à leurs travaux, la leur propre est à jamais intimement liée.

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(1) *Méthodes analytiques pour la détermination d'un arc du méridien :* à Paris, chez Duprat, *in*-4o. Cet ouvrage est précédé d'un mémoire du citoyen Legendre sur le même sujet.

Les observateurs se sont servis pour la mesure des angles, dans quelque genre d'observations que ce soit, du cercle entier de Borda, qu'on pourroit nommer à juste titre *cercle répétiteur*, par le précieux avantage qu'il procure de répéter, pour ainsi dire, l'angle à observer, en permettant d'en prendre tel multiple qu'on desire, et conséquemment de diminuer en même raison les erreurs, inévitables d'ailleurs, soit à cause des limites de nos sens, soit à cause de celles de la perfection des instrumens, et de les rendre à la fin insensibles. L'utilité de ce cercle, construit avec un grand soin, sous les yeux de Borda même, par le célèbre artiste Lenoir, avoit déja été pleinement prouvée par les observations que les citoyens Cassini, Méchain et Legendre, avoient faites en 1787 pour la jonction des observatoires de Paris et de Greenwich, et dans lesquelles ils sont parvenus à un degré de précision inconnu jusqu'alors; et s'il pouvoit rester encore quelques doutes sur l'extrême exactitude qu'on peut obtenir au moyen de ce cercle, quand on s'en sert d'ailleurs avec les précautions qu'il exige, les observations des citoyens Méchain et Delambre suffiroient pour les dissiper entièrement.

Ordinairement il a été fait à chaque station plus d'une série d'observations, et les observateurs ont formé chaque série du nombre d'observations qu'ils ont crues nécessaires pour parvenir à un résultat constant et suffisamment exact; ils ont noté dans leurs registres les nombres indiqués par chaque observation, ainsi que les circonstances particulières qui avoient eu lieu, soit

pour la manière dont les objets étoient éclairés, soit pour celle dont ils se projetoient, soit pour la partie à laquelle on pointoit, soit pour l'état de l'atmosphère; en un mot, ils y ont marqué tout ce qui peut servir à constater la valeur intrinsèque d'une observation. Aussi les membres de la commission qui ont été nommés pour le dépouillement de ces registres, ont-ils pu juger de cette valeur, et par les notes dont nous venons de parler, et par les renseignemens que les observateurs ont eu la complaisance d'ajouter de vive voix, et par la marche de chaque série d'observations, et par l'accord des différentes séries entre elles.

Cet examen a mis les commissaires en état de fixer la valeur de chaque angle d'une manière abstraite, et sans faire attention, ni aux autres, ni à ce que la somme des trois angles d'un même triangle, fixés de cette manière, pourroit fournir; ils ont cru devoir prendre les observations telles qu'elles sont, sans y faire la moindre correction, sans rien arranger après coup. Pour cet effet, ils ont pris pour chaque angle le milieu entre les résultats des différentes séries d'observations faites pour le déterminer, résultats qui d'ailleurs différoient très-peu entre eux; et ils l'ont déterminé ce milieu, soit en ayant simplement égard aux résultats de chaque série, soit en faisant entrer en ligne de compte le nombre des observations, soit en accordant plus de poids à celles qui paroissoient préférables et en rejetant celles que les observateurs eux-mêmes avoient notées comme peu dignes de confiance, enfin en employant toutes les

ressources que l'art de discuter des observations et une saine critique en ce genre peuvent fournir, et en donnant autant d'attention et de soin à la détermination de dixièmes de seconde (car c'est ordinairement sur des quantités de ce genre que rouloient les discussions, rarement sur des secondes entières) que s'il s'agissoit de quantités considérables. Les commissaires ont formé de cette manière des tableaux de tous les triangles qui ont servi à la détermination de la méridienne; ils les ont présentés à la commission générale, ensemble avec le détail de la méthode qu'ils ont employée, et des raisons de leurs déterminations. La commission a arrêté ces tableaux et les a déposés dans les archives de l'Institut comme des pièces authentiques, lesquelles renferment tous les principes qui doivent servir au calcul des triangles et des parties de la méridienne; comme c'est effectivement sur eux que les calculs ont été faits par la suite.

Pour vous faire juger de la précision que les observateurs ont obtenue dans cette partie de leur travail, nous vous dirons que sur quatre-vingt-dix triangles qui joignent les extrémités de la méridienne, il y en a trente-six dans lesquels la somme des trois angles diffère de moins d'une seconde de ce qu'elle auroit dû être, c'est-à-dire, dans lesquels l'erreur des trois angles pris ensemble est de moins d'une seconde; qu'il y en a de plus vingt-sept où cette erreur est au-dessous de deux secondes; que dans dix-huit autres elle ne monte pas à trois secondes; et qu'il n'y en a que quatre dans lesquels

elle est entre trois et quatre secondes, et trois seulement où elle est au-dessus de quatre, mais au-dessous de cinq. Nous doutons qu'on puisse parvenir à une plus grande exactitude, sur-tout dans les pays qu'il a fallu traverser. Aussi ceux qui considéreroient ces tableaux sans être instruits de la manière dont ils ont été formés, pourroient être tentés de croire, à la vue de cette précision, qu'on a arrangé les choses après coup, pour donner à l'ensemble cet air d'exactitude : mais les registres originaux des observateurs, les résultats qu'eux-mêmes avoient envoyés à Paris long-temps avant la mesure des bases et dans le temps qu'ils étoient encore occupés à leurs opérations, et le travail des commissaires, prouvent le contraire de la manière la plus authentique ; on ne s'est permis aucune correction arbitraire ou conjecturale, quelque légère qu'elle pût être ; et tous les angles ont été déterminés d'après des considérations puisées dans les observations mêmes.

De la mesure des angles passons à ce qui concerne les bases. Le citoyen Delambre en a mesuré deux : l'une entre Melun et Lieursaint ; l'autre près de Perpignan, entre Vernet et Salces.

Ce n'est pas un travail aussi facile qu'on pourroit le croire au premier abord, que cette mesure d'une base : il faut une infinité d'attentions scrupuleuses sur tous les élémens qui constituent cette mesure, et de précautions sur les causes multipliées qui pourroient produire des erreurs ; il faut des méthodes exactes pour réduire la somme de toutes les parties contenues entre les deux

extrémités de la base, à cette longueur, qui doit être considérée comme la vraie base, comme l'arc terrestre compris entre ces deux extrémités. On peut assurer que rien n'a été négligé, ni dans la mesure, ni dans les calculs de réductions. Le citoyen Delambre a détaillé, dans le mémoire que nous avons déja cité, les méthodes qu'il a adoptées et les moyens dont il s'est servi dans des cas qui présentoient des difficultés.

Il faut, disons-nous, des attentions sur les différens élémens qui constituent cette opération. Il en faut d'abord sur la longueur exacte des instrumens qu'on emploie; elles ont été prises. Ces instrumens ont été construits avec beaucoup de soin, par le citoyen Lenoir, d'après les idées du citoyen Borda et sous ses yeux. Ce sont quatre règles de platine : chacune d'elles est recouverte, jusqu'à quelques pouces de son extrémité antérieure, d'une pareille lame de laiton, mobile selon la longueur de la règle de platine, et fixée à celle-ci par l'autre extrémité. Cette lame forme, par les différentes dilatations que la même variation de température fait éprouver au laiton et au platine, un thermomètre métallique très-sensible, dont les divisions sont gravées sur l'extrémité antérieure, laquelle porte un vernier et un microscope pour voir et évaluer les sous-divisions. On sent qu'il a été fait, avant qu'on se soit servi de ces règles, nombre d'expériences pour constater la dilatation de ces métaux, l'état des thermomètres métalliques, leur marche, et leur comparaison aux thermomètres ordinaires. On a également comparé les longueurs des

règles n° 2, n° 3, n° 4, à la règle n° 1, à laquelle on a tout réduit, et que, par cette raison, nous nommerons désormais *le module;* comparaison qui a été faite par des moyens si exacts, qu'ils ne laissent pas de doute sur des deux-cent-millièmes parties. Le citoyen Borda a remis à la commission le mémoire qui contient le détail de toutes ces expériences. Cette pièce fera une partie intéressante et essentielle du recueil qu'on publiera sur cette grande opération.

Il faut ensuite des précautions pour que ces règles ne subissent aucune altération, soit pendant le transport, soit pendant qu'on les emploie à la mesure : pour cet effet, elles sont posées chacune, avec les précautions convenables pour ne pas nuire au mouvement de dilatation et de contraction qu'elles doivent éprouver par les changemens de température, sur des pièces de bois assez fortes pour ne pas fléchir ni se travailler; elles sont recouvertes, à quelques pouces de distance, d'un toit qui les met à l'abri de l'action directe des rayons du soleil.

Il faut encore, avons-nous dit, des précautions dans l'opération même. D'abord, des précautions pour l'alignement des règles : des pointes placées avec l'exactitude convenable sur le toit dont nous venons de parler, servoient de mire, et ont été substituées à l'alignement au cordeau dont on se servoit anciennement. Ensuite, des précautions pour que les règles qui sont encore posées à terre, ne soient pas déplacées de la plus petite quantité et par le choc le plus léger, lorsqu'on veut en

placer une bout à bout avec la dernière de celles-ci. Pour en être sûr, on ne plaçoit jamais les règles de cette manière ; mais on laissoit entre chaque règle et celle qui la précédoit et la suivoit immédiatement, un intervalle qu'on mesuroit ensuite en poussant légèrement, jusqu'au contact parfait, la languette de platine qui est à l'extrémité antérieure des règles et s'y meut dans une coulisse ; languette qui, d'ailleurs, porte un vernier et un microscope, pour connoître le nombre des divisions contenues dans l'intervalle qu'on a laissé entre les deux règles, et qui se trouve rempli par la languette. Précautions encore pour recommencer chaque jour l'opération au même point où elle avoit été terminée la veille : elles ont été prises par des moyens aussi exacts que simples. Précautions enfin, pour être sûr de ne pas se tromper dans le compte du nombre des règles qu'on a posées sur le terrain, ni dans celui des parties de languette, ou des thermomètres métalliques, qu'on a observés et qu'on note dans le registre, ni dans aucun des plus petits détails : elles ont toutes été employées jusqu'au scrupule ; et l'on peut être sûr qu'il n'y a aucune erreur sensible dans la mesure actuelle des deux bases. On en trouve d'ailleurs la preuve dans l'opération même, puisque la différence entre la partie qu'on avoit mesurée pendant un jour entier, et qui s'élevoit à soixante-dix modules, mais sur laquelle on croyoit pouvoir former quelque doute, à cause qu'il avoit soufflé ce jour-là un vent très-violent, et la même partie mesurée une seconde fois le lendemain dans des circonstances favorables, n'a guère monté qu'à

la quatre-millième partie du module, ou environ à la deux-cent-soixante-dix-millième partie de tout l'intervalle mesuré ce jour-là.

Mais la somme de toutes les parties comprises entre les extrémités de la base, et mesurées avec l'exactitude dont nous venons de parler, ne forme pas la base vraie. D'abord ces règles ont eu à différens jours des températures différentes, indiquées par les thermomètres métalliques, et, par conséquent, des longueurs qui n'ont pas toujours été les mêmes; il s'agit de les réduire à une température donnée, et par-là à une longueur constante: première réduction. Ensuite ces règles, quoique portées sur des trépieds montés sur des vis, afin que les languettes puissent être en contact immédiat précisément au point qu'il faut, ne sauroient être de niveau, à cause des inégalités du terrain : leur ensemble forme une somme de lignes droites différemment inclinées. Il a donc fallu connoître l'inclinaison des règles par rapport à l'horizon : aussi a-t-elle toujours été mesurée pour chaque règle, au moyen d'un niveau aussi simple qu'ingénieux, inventé par le citoyen Borda, et exécuté par le citoyen Lenoir; on le posoit sur le toit de chaque règle à des points fixes, uniquement destinés à cet objet. On a donc pu connoître, par le calcul, l'erreur que produit l'inclinaison de chaque règle, et avoir la longueur de la ligne unique qu'il s'agit de connoître : seconde réduction.

Mais cette ligne unique n'est pas posée, pour ainsi dire, sur la surface de la mer, niveau constant auquel

il faut réduire tous les autres. Le cercle de Borda, dont on s'est servi pour la mesure des angles, a fourni le moyen de faire cette réduction avec beaucoup d'exactitude, parce qu'il a servi à déterminer avec une très-grande précision l'élévation de chaque station au-dessus de celles qui forment avec elle un même triangle, ou sa dépression au-dessous de ces mêmes stations, ou de quelqu'une d'entre elles; de sorte que, connoissant, comme on les connoissoit, la hauteur de la tour de Dunkerque au-dessus du niveau de l'Océan, et celle de Montjouy au-dessus du niveau de la mer Méditerranée, cette même opération a servi à faire un nivellement exact de toute cette partie de la France et de l'Espagne que les observateurs ont traversée sur une longueur de près de dix degrés de latitude; avantage vraiment précieux à beaucoup d'égards. On a donc pu faire le calcul nécessaire pour réduire les bases mesurées aux bases vraies, à l'arc qu'elles forment sur la surface de la Terre, au niveau même de la mer : c'est la troisième réduction qu'il s'agissoit de faire. Et voilà ce qu'il en coûte de peines, de soins, d'attentions, de précautions, de calculs, pour parvenir à ce degré de perfection auquel l'état actuel des sciences permet d'atteindre, et qu'il exige conséquemment qu'on emploie. Aussi la commission des poids et mesures a-t-elle été intimement convaincue que cette base a été mesurée avec une exactitude rare, supérieure à celle qu'on a pu obtenir dans les opérations du même genre faites précédemment en France, au Pérou ou au Nord; et il

suffit, d'une part, de cette conviction, puisée dans la nature même des moyens et des précautions qu'on a employés, et de se rappeler, de l'autre, que sur des bases de pareille longueur mesurées au Pérou par des méthodes moins dignes d'une entière confiance, il n'y a pas eu deux pouces, ou un deux-cent-vingt-millième de la base entière, d'incertitude, pour être persuadé qu'il eût été inutile de faire une seconde fois des opérations aussi pénibles.

La longueur des bases se trouve donc exprimée en nombres dont l'unité est la règle n° 1, ou *le module;* et conséquemment celle de la méridienne, celle du quart du méridien terrestre, seront exprimées en *unités* du même genre. Mais, pour se faire entendre dans la société, et donner une idée exacte de cette *unité*, il faut nécessairement la comparer aux anciennes mesures connues, comme d'autre part, pour ne pas perdre le fruit de tout ce qui a été mesuré dans des temps précédens, il faut réduire les anciennes mesures aux nouvelles. On sent aisément qu'un point aussi important n'a pas été négligé. Avant qu'on eût entrepris la mesure des bases, la règle n° 1, ou *le module*, a été comparée exactement à la toise de l'Académie, dite *toise du Pérou*, et l'on a employé des moyens qui permettent de s'assurer de cent millièmes de toise. Les détails de ces expériences sont consignés dans le mémoire du citoyen Borda, que nous avons déja cité plus d'une fois. Après son retour, le citoyen Delambre n'a pas manqué de faire la comparaison des règles qui avoient servi à la

mesure des bases, et il a trouvé qu'elles n'avoient pas subi le plus léger changement dans leur longueur, et qu'elles avoient conservé avec la double toise le même rapport qu'elles avoient avant d'être employées, sans qu'il y ait aucune différence que nous puissions assigner. Enfin la commission elle-même a chargé quelques-uns de ses membres de faire encore une fois la même comparaison, et de tirer de leur travail tout le parti possible, en comparant, à cette occasion, entre elles, la toise du Pérou, celle du Nord, et celle de Mairan, toutes trois devenues célèbres ou importantes, les premières par les grandes opérations auxquelles elles ont servi, et la troisième parce que c'est en parties de cette toise que Mairan a exprimé les résultats de ses belles expériences sur la longueur du pendule, et que c'est sur elle qu'ont été étalonnées les toises qui ont servi à la mesure de deux degrés terrestres faite près de Rome par les célèbres Boscovich et Lemaire. Cette nouvelle comparaison du module à la toise du Pérou a encore donné le même résultat; savoir, que les règles n'ont subi aucun changement; et elle a prouvé de plus que le module est exactement le double de la toise du Pérou, et a conséquemment douze pieds de longueur, lorsque le thermomètre centigrade est à $12^{\circ}\frac{1}{2}$: d'où l'on déduit, soit par le calcul de la dilatation des métaux, soit par les expériences directes de Borda, qu'à la température de $16^{\circ}\frac{1}{4}$ (ce qui revient à 13° du thermomètre de Réaumur) le module est plus court que la double toise de $\frac{2}{100}$ de ligne, c'est-à-dire, d'environ un quatre-vingt-cinq-millième du total.

Les observations d'azimut, si délicates et si difficiles, ont été faites avec toute l'exactitude dont elles sont susceptibles, et calculées avec la plus grande précision. On auroit pu se contenter d'observer un seul azimut pour déterminer la direction que forme avec la méridienne un des côtés d'un seul triangle, puisque cela suffit pour faire le calcul de la méridienne entière : mais il étoit extrêmement important d'en observer plusieurs, parce que la théorie fait entrevoir que si les azimuts calculés diffèrent de ceux qu'on observe réellement, ces différences et leur marche peuvent servir à perfectionner nos connoissances sur la figure de la Terre, sur les irrégularités qui peuvent se trouver dans son intérieur, sur l'action des causes locales ; et il étoit de la plus haute importance de faire servir cette belle opération à tout ce qui peut contribuer au perfectionnement de nos connoissances sur ces intéressans objets. Les observateurs l'avoient trop à cœur ce perfectionnement, auquel d'ailleurs ils contribuent tant eux-mêmes par leurs travaux, pour ne pas saisir avec empressement une occasion aussi favorable de faire des observations d'azimut utiles, et plus parfaites que celles qu'on faisoit anciennement en de pareilles occasions. D'ailleurs, pour déterminer les azimuts, ils ont non seulement employé le Soleil, mais encore l'étoile polaire ; et ils n'ont rien négligé dans les réductions et dans les calculs de ce qui pouvoit contribuer à l'exactitude du résultat. Ces observations ont été faites à Watten, à Bourges, à Carcassonne et à Montjouy, c'est-à-dire, aux deux

extrémités de la méridienne et dans deux endroits intermédiaires.

Les observations de latitude, les dernières dont nous avons à vous rendre compte, ont un degré d'exactitude proportionné à l'importance dont elles sont pour fixer les résultats d'une opération du genre de celle-ci. C'est encore le cercle de Borda que les observateurs ont employé; et si, après les épreuves faites précédemment, et les observations faites en 1790 à l'observatoire national par les citoyens Cassini, Borda et Méchain, et imprimées dans le dernier volume des Mémoires de l'Académie, il pouvoit rester encore quelque doute sur la grande précision que donne cet instrument pour les observations des distances au zénith, et par conséquent des latitudes, il suffiroit de consulter les registres des citoyens Méchain et Delambre pour se convaincre qu'il n'y en a aucun. On verra dans ces registres la multitude vraiment étonnante des observations; la marche régulière des séries; l'accord des différentes séries entre elles; les précautions qu'on a prises, tant dans les observations que dans les réductions; les étoiles dont on a fait choix; leurs passages, tant supérieurs qu'inférieurs, qui ont été observés; et l'on finira par être aussi sûr que le sont les membres de la commission qui ont été spécialement chargés de cet examen, que l'est la commission entière, qu'il n'y a dans aucune des latitudes observées par les citoyens Méchain et Delambre une seconde d'incertitude, et que celle qui pourroit y rester encore ne monte pas, ni à beaucoup près, à une demi-seconde.

Ces observations ont été faites à Dunkerque et à Évaux par le citoyen Delambre, à Carcassonne et à Montjouy par le citoyen Méchain, et à Paris, par le citoyen Méchain à l'observatoire national, et par le citoyen Delambre dans son observatoire particulier, rue de Paradis, au Marais : mais aucun de ces deux observatoires n'entre dans la chaîne des triangles; c'est le Panthéon français, dont la distance à chacun des observatoires dont nous venons de parler est suffisamment connue pour déterminer sa latitude. Or on trouve pour le Panthéon, à une quantité insensible près, la même latitude, soit qu'on la déduise des observations du citoyen Méchain, soit qu'on emploie celles du citoyen Delambre; preuve de l'extrême exactitude des unes et des autres.

Telles sont les différentes parties de l'opération que les citoyens Méchain et Delambre ont si heureusement terminée; opération qui surpasse par son étendue, et égale par sa précision, ce qui a été fait de plus accompli en ce genre : elle fournit toutes les données nécessaires pour parvenir à des résultats propres non seulement à fixer les bases du nouveau système métrique, mais encore à faire naître, sur la question si importante de la figure de la Terre, des recherches fort intéressantes et dignes des mathématiciens les plus célèbres, qui, sans doute, vont reprendre cette question avec une nouvelle ardeur.

Il ne s'agit plus que de vous indiquer quel a été le travail de la commission pour déduire des résultats de

cette opération, l'unité des mesures de longueur, ou le mètre.

Quatre commissaires se sont spécialement chargés du calcul des triangles; ils ont fait leurs calculs séparément et par des méthodes différentes, afin de ne rien laisser à desirer sur la certitude des résultats. Ils ont aussi calculé, et toujours par différentes méthodes, les quatre parties de la méridienne qui se trouvent comprises entre les endroits dont la latitude a été observée, c'est-à-dire, les arcs terrestres compris entre Dunkerque et le Panthéon, le Panthéon et Évaux, Évaux et Carcassonne, Carcassonne et Montjouy. Les détails de pareils calculs, et des principes sur lesquels ils sont fondés, ne sauroient se trouver dans un rapport tel que celui-ci; ils ont été exposés à la commission, dans un mémoire qui est déposé dans les archives de l'Institut. Nous dirons seulement,

1°. Que la distance entre les parallèles de Dunkerque et du Panthéon, qui sous-tend un arc de 2°,18909722, et dont le milieu passe par la latitude de 49° 56′ 30″, est de	62472,59 modules.
2°. Que la distance entre les parallèles du Panthéon et d'Évaux, qui sous-tend un arc de 2°,66868055, et dont le milieu passe par la latitude de 47° 30′ 46″, est de	76145,74

3°. Que la distance entre les parallèles d'Évaux et de Carcassonne, qui sous-tend un arc de 2°,96336111, et dont le milieu passe par la latitude de 44° 41′ 48″, est de . . . . . 84424,55 modules.

4°. Enfin, que la distance entre les parallèles de Carcassonne et de Montjouy, qui sous-tend un arc de 1°,85265833, et dont le milieu passe par la latitude de 42° 17′ 20″, est de 52749,48

D'où il résulte que la méridienne entière comprise entre Dunkerque et Montjouy, qui sous-tend un arc céleste de 9°,67379722, et dont le milieu passe par la latitude de 46° 11′ 5″, est de 275792 modules et 36 centièmes.

S'il s'agissoit de vous présenter les différentes idées que les résultats du calcul des parties de la méridienne ont fait naître, nous fixerions principalement vos regards sur ces deux conclusions : la première, que les degrés moyens qu'on conclut pour les quatre intervalles dont nous venons de faire mention, décroissent tous à mesure qu'on s'approche de l'équateur, et qu'ainsi cette opération pourroit elle seule prouver l'aplatissement de la Terre, s'il étoit encore besoin de preuve sur cet article ; la seconde, qu'on étoit bien loin de soupçonner, et qui présente un phénomène très-remarquable, digne des recherches des plus profonds mathématiciens, c'est que ces mêmes degrés ne suivent pas dans leur diminution

une marche graduelle. En effet, si l'on déduit des quatre intervalles énoncés ci-dessus le degré moyen qu'on en peut conclure, en employant simplement l'hypothèse sphérique, qui suffit pour un premier apperçu, on trouvera en nombres ronds pour le degré moyen,

	Modules.	Différence.	Différence pour un degré de latitude.
Entre Dunkerque et le Panthéon, à la latitude moyenne de 49° 56′ 30″.	28538		
		5	2
Entre le Panthéon et Évaux, à la latitude moyenne de 47° 30′ 46″ . . .	28533		
		44	16
Entre Évaux et Carcassonne, à la latitude moyenne de 44° 41′ et 48″ . .	28489		
		12	7
Entre Carcassonne et Montjouy, à la latitude moyenne de 42° 17′ 20″ . .	28472		

C'est-à-dire, en deux mots, que les degrés moyens décroissent d'abord très-peu et très-lentement entre Paris et Évaux, seulement de deux modules pour un degré de latitude; ensuite très-rapidement et très-fortement, de seize modules par degré de latitude, entre Évaux et Carcassonne; et que cette diminution rapide se ralentit entre cette ville et Montjouy, n'étant plus que de sept modules.

Nous ajouterions à cet exposé succinct, que ce fait si remarquable est intimement lié à un autre, à celui que

présentent, tant les différences qu'il y a entre les azimuts calculés pour Bourges, pour Carcassonne, pour Montjouy, d'après celui de Dunkerque pris pour base, et les azimuts observés dans ces trois stations, que la marche de ces mêmes différences; de sorte que ces deux faits se servent mutuellement de confirmation et d'appui, et que, réunis, ils indiquent, soit une irrégularité dans les méridiens terrestres, soit une ellipticité dans l'équateur et ses parallèles, soit une irrégularité dans l'intérieur de la Terre, soit un effet de l'attraction des montagnes, soit une action puissante de ces différentes causes réunies, ou de quelques-unes d'entre elles; action qui n'avoit pas été démontrée d'une manière aussi frappante qu'elle l'est par les résultats que nous venons d'indiquer. Ce sera aux mathématiciens les plus célèbres à fixer leur attention sur ces faits, pour tâcher d'en démêler les élémens, et de parvenir, sur la figure de la Terre, à une théorie plus parfaite que celle que nous possédons jusqu'ici.

Nous ne pouvons vous indiquer ces objets qu'en passant : ils ne sont pas du ressort de la commission des poids et mesures; mais ils l'avoient trop frappée, et ils sont trop importans, pour qu'elle pût les passer sous silence. Bornée, comme elle l'a été, à ce qui concerne la détermination du quart du méridien, puisque c'est de celle-ci que dépend l'unité des mesures, elle a tourné toute son attention vers cet objet; elle l'a considéré sous toutes ses faces, et s'est déterminée à s'en tenir uniquement aux faits, sans y mêler aucune idée théo-

rique sur tel ou tel point susceptible de discussion : elle a donc employé dans ses calculs l'arc total compris entre Dunkerque et Montjouy, et qui est, comme nous l'avons dit, de 275792 modules et 36 centièmes. Cet arc est le plus grand de tous ceux qui ont été déterminés jusqu'ici; et par là il rend plus petite l'influence, soit des irrégularités qui peuvent se trouver dans la figure et dans l'intérieur de la Terre, soit de celles que de légères erreurs, toujours inséparables des observations les mieux faites, pourroient produire.

En prenant cet arc pour base, on en a déduit le quart du méridien par un calcul rigoureux dans l'hypothèse elliptique. Il falloit, pour faire ce calcul, connoître l'aplatissement de la Terre : c'est encore l'expérience que la commission a consultée pour cette détermination. Pour cet effet, elle a employé, d'une part, le grand arc que les citoyens Méchain et Delambre viennent de mesurer en France; et de l'autre, celui que d'excellens observateurs ont mesuré au Pérou, il y a soixante ans, à peu près sous l'équateur même : c'est un de ceux qui ont été déterminés avec le plus de soin, et discutés avec le plus d'attention et d'exactitude. Il est d'ailleurs le plus grand de tous ceux qui ont été mesurés hors de France, soit par les ordres de différens gouvernemens, soit, comme celui-ci, par les ordres du gouvernement français. Enfin sa distance même de l'arc auquel on le compare diminuera l'influence des erreurs qui pourroient s'être glissées dans sa détermination, puisqu'elles se trouveront distribuées sur un plus grand intervalle.

La comparaison de ces deux arcs, faite avec soin et par différentes formules, a donné un trois-cent-trente-quatrième pour l'aplatissement de la Terre; et il est très-remarquable que cet aplatissement, calculé d'après les données que nous venons d'indiquer, est le même que celui qui résulte de la combinaison d'un grand nombre d'expériences faites dans différens endroits sur la longueur du pendule simple, et qu'il est encore conforme à celui qu'exige la théorie de la nutation et de la précession. L'accord de ces trois résultats, tirés de trois genres d'observations très-différens, mérite la plus grande attention, et il est bien propre à inspirer beaucoup de confiance sur chacun d'eux. D'ailleurs une légère erreur sur ce point auroit d'autant moins d'influence sur le résultat définitif, que le milieu de l'arc entier, terminé par Dunkerque et Montjouy, passe près du quarante-cinquième degré de latitude, ou du degré moyen.

Cet élément du calcul une fois arrêté, le calcul même du quart du méridien ne pouvoit plus offrir de difficulté; et l'on a trouvé par différentes méthodes, en employant l'arc intercepté entre Dunkerque et Montjouy et un trois cent trente-quatrième pour l'aplatissement de la Terre, que le quart du méridien terrestre est de 2565370 modules : d'où il suit, et c'est là le résultat définitif de tout le travail, que sa dix-millionième partie, ou *le mètre*, *unité de mesure*, est de 0,256537, ou, en s'arrêtant pour la pratique à quatre décimales, de 0,2565 parties du *module*.

Pour réduire cette longueur aux anciennes mesures,

nous dirons d'abord que si le module et la toise du Pérou étoient supposés l'un et l'autre à la température qu'avoit celle-ci lorsqu'elle a été employée par les académiciens, qui se rapporte au treizième degré du thermomètre à mercure, divisé en quatre-vingts parties, ou au seizième et un quart du thermomètre centigrade, le mètre seroit égal à 443,291 lignes de cette toise; ensuite, qu'en réduisant, comme il le faut, le module à la température à laquelle il a été réduit dans l'expression de la longueur des bases, laquelle a servi à calculer les triangles et la méridienne, *le mètre vrai et définitif* est de 443,296 lignes de la toise du Pérou, celle-ci toujours supposée à la température de $16^{\circ}\frac{1}{4}$, puisque c'est à cette seule température que cette toise peut être considérée comme étant celle dont les académiciens se sont servis. Les variations de longueur que les métaux éprouvent par différentes températures exigent ces attentions.

Nous vous avons entretenus assez en détail du travail de la commission pour fixer la vraie longueur du *mètre*, base de tout le système métrique, unité des mesures de longueur. Les mesures de surface et de capacité s'en déduisent trop facilement, pour qu'il soit nécessaire de s'y arrêter. Il n'en est pas de même de l'unité de poids : sa détermination dépend d'une foule d'expériences, de considérations, de réductions, plus délicates les unes que les autres; et ce n'est qu'à force de patience, de soins, d'attention, de dextérité, que le citoyen Lefévre-Gineau, auquel l'Institut a confié ce travail, est parvenu à un degré de précision rare. Sachant combien les

opérations qu'il avoit à faire sont difficiles, il a desiré (car le vrai mérite, lors même qu'il est universellement reconnu, est toujours modeste, et se défie de ses propres forces) que la commission lui adjoignît un de ses membres pour vérifier les expériences qu'il avoit déja faites, et pour assister à celles qu'il se proposoit de faire encore. Il suffira de dire que le citoyen Fabroni de Florence a été nommé, pour que tout le monde soit convaincu que ces expériences ne pouvoient tomber en de meilleures mains, ni être faites et vérifiées avec plus d'exactitude, ou revêtues d'une plus grande authenticité, ni inspirer plus de confiance. Enfin une commission spéciale s'est occupée de l'examen de tous les registres d'observations et d'expériences, des réductions et des calculs. Nous pourrions nous étendre sur toutes les particularités de ce beau travail, si la nature d'un rapport tel que celui-ci pouvoit nous permettre de vous présenter un grand nombre de résultats purement numériques; mais, obligés comme nous le sommes, d'une part, de nous restreindre, et, de l'autre, de vous présenter néanmoins des données qui puissent vous faire connoître ce qui a été fait, ce qui devoit se faire, et vous mettre en état de juger du degré de confiance que méritent les résultats définitifs, permettez-nous de vous proposer simplement quelques considérations sur l'esprit général de ces expériences, sur les différens points qu'il s'agit de déterminer, et sur la méthode qu'il a fallu employer pour fixer avec exactitude la véritable unité de poids.

Le poids d'un corps exprime la quantité de matière

qu'il contient : mais comme tous les corps ne sont pas également denses, que les uns contiennent sous le même volume beaucoup plus de matière que d'autres, on n'auroit qu'une expression vague et indéterminée, si à l'idée de quantité de matière on ne joignoit celle du volume sous lequel elle est contenue; conséquemment déterminer l'unité de poids, c'est déterminer la quantité de matière qu'un certain corps qu'on emploie de préférence, contient sous un volume dont on est préalablement convenu, afin de rappeler à cette quantité, et de mesurer par elle, celle que contiennent tous les corps quelconques. Or, comme la détermination de ce volume dépend des mesures linéaires, il en résulte que cette question, *quelle est l'unité de poids?* tient intimement à celle de la fixation des mesures linéaires, c'est-à-dire, du *mètre;* et ensuite, que, pour la résoudre entièrement, il faut, 1°. fixer le volume qu'on emploiera pour terme de comparaison, 2°. faire choix d'un corps propre à le remplir, 3°. enfin déterminer le poids ou la quantité de matière que ce corps contient sous ce volume.

Il peut y avoir de l'arbitraire dans le volume qu'on emploie : mais les usages de la société demandent qu'on ne prenne pas d'unité trop grande ou trop petite; et la nature du système métrique décimal exige qu'elle soit exprimée par un nombre cubique dont la racine est un sous-multiple décimal du *mètre*. L'Académie des sciences a sagement adopté la millième partie du *cube du mètre*, ou, ce qui revient au même, le cube du décimètre.

Le corps dont on fait choix pour remplir ce volume n'est nullement indifférent : personne ne doute qu'il ne doive être fluide; qu'il ne doive être en état de conserver sa fluidité à une température qu'il soit facile d'obtenir par-tout; qu'il ne faut pas qu'il ait un degré de densité qui rendroit les expériences trop difficiles, ou leurs résultats peu exacts : enfin, et sur-tout, il doit être de nature à pouvoir être retrouvé par-tout dans le même degré de pureté, à se dépouiller facilement de toutes les matières hétérogènes qui pourroient se combiner chimiquement avec lui, ou s'y mêler mécaniquement, et propre à rendre la comparaison immédiate avec tous les autres corps très-facile. L'eau paroît posséder ces qualités dans un degré éminent, ou du moins plus qu'aucun autre corps que nous connoissions; et distillée elle est toujours également pure. Aussi l'Académie des sciences a-t-elle choisi cette eau pour le corps dont la quantité de matière contenue sous le cube du décimètre seroit l'unité de poids.

Il n'est point de physicien qui ne sache qu'il faut renoncer à l'idée qui se présente la première et le plus naturellement à l'esprit, celle de remplir d'eau distillée un cube dont le côté seroit un décimètre, et de la peser. Le peu d'exactitude d'un pareil procédé est trop évident pour qu'il soit nécessaire de le développer; tout le monde sent qu'il faut en revenir à ce principe d'hydrostatique si connu, que le poids d'un fluide contenu sous un certain volume est égal au poids que ce volume, pesé d'abord dans l'air, vient à perdre si on le pèse ensuite dans ce

fluide. Mais l'expérience par laquelle on confirme ce principe, et qui paroît si simple, si facile, quand on la voit faire dans des cours de physique, devient singulièrement délicate et difficile quand il s'agit de déterminer des quantités absolues. En effet, il faut d'abord connoître avec une précision rigoureuse le volume du corps qu'on emploie; opération très-compliquée : il faut ensuite peser ce corps dans l'air et dans l'eau; deux opérations qui exigent des attentions que la plupart des personnes, même instruites, sont bien loin de connoître, et qu'il est rare de savoir apprécier : il faut enfin faire aux résultats de ces expériences les réductions qu'exigent différentes considérations, comme, par exemple, celle du poids et de la température de l'air ; considérations qui demandent des expériences, des soins et des calculs. Le résumé général de ce qui a été fait sur chacun de ces articles donnera des notions exactes et précises de toute l'opération.

Il s'agit d'abord de construire un corps qui soit propre à être pesé et dans l'air et dans l'eau avec exactitude, et d'en connoître le volume avec la plus grande précision. Comme ce dernier point est d'une extrême importance, la figure du corps, qui seroit par elle-même assez indifférente, au moins jusqu'à un certain point, ne l'est plus : elle doit être celle du corps auquel il sera le plus facile de donner exactement une figure régulière; et on a, comme de raison, choisi le cylindre. Le citoyen Fortin, qui a donné, dans l'exécution des machines dont nous vous parlerons successivement, de

nouvelles preuves de ses talens, a construit en laiton un cylindre *creux* (n'oublions pas cette circonstance, car ici rien de ce qui est même minutieux ne doit être omis) dont le diamètre égale à peu près la hauteur, dont le volume est de plus de onze décimètres cubes (ou d'environ cinq cent soixante pouces); c'est-à-dire qu'il vaut onze fois celui qu'il s'agit de déterminer; circonstance qui mérite d'être remarquée, parce que les conclusions qu'on tire d'expériences faites en grand méritent, dans leur application, plus de confiance que celles qui se trouveroient dans un cas contraire. Les parois du cylindre sont soutenues intérieurement par une carcasse qui empêche que ce corps ne change de volume par la pression de l'eau, lorsqu'il s'y trouve plongé; et il a été fait des expériences pour constater qu'il n'en change pas.

Mais ce cylindre, avec quelque soin qu'il ait été construit, nous dirons même quel que soit le degré de perfection auquel le citoyen Fortin l'a amené, n'est point un cylindre parfait, et il ne sauroit l'être dans la rigueur mathématique; car tel est le sort de l'homme, que sa main ne peut jamais exécuter ce que son génie crée, avec cette précision rigoureuse que son imagination attribue à l'objet idéal : mais aussi telles sont ses ressources, que la sagacité de son esprit lui fait saisir des moyens propres à connoître combien ce qu'il a exécuté diffère de la perfection idéale, et conséquemment à ramener à celle-ci ce qui ne peut, physiquement parlant, qu'en différer. Ce sont ces moyens que le citoyen Lefévre-

Gineau a su mettre habilement en usage, à l'aide d'une machine très-ingénieuse du citoyen Fortin, par laquelle il a pu mesurer de légères différences de longueur avec la précision d'un quatre-millième de ligne des anciennes mesures, ou d'un dix-sept-centième de millimètre. En effet, si le corps dont il s'agit est un cylindre parfait, il faut d'abord, au moins dans la pratique, qu'il soit un cylindre droit, et toutes les expériences démontrent qu'il l'est, sans qu'il y ait aucune différence que nous soyons en état d'assigner; il faut que toutes les perpendiculaires abaissées d'une des bases sur l'autre, prise pour un plan, soient égales; il faut que ces bases, et les coupes qui leur sont parallèles, soient des cercles parfaits; il faut enfin que les diamètres de ces cercles soient exactement égaux. Il ne s'agit donc que de mesurer ces perpendiculaires et ces diamètres, pour savoir s'ils le sont réellement, ou pour connoître leur inégalité.

Imaginons donc qu'on ait tracé sur les deux bases en partant du centre, et sur chacune d'elles aux mêmes distances de celui-ci, trois cercles; que les circonférences soient chacune divisées en douze parties par six diamètres : on aura sur chaque base trente-six points d'intersection. Supposons qu'on tire une ligne droite de chacun de ces points, pris sur une des bases, à son point correspondant sur l'autre base, et l'on aura trente-six lignes, lesquelles font avec la ligne des centres, ou l'axe, trente-sept hauteurs qui doivent être rigoureusement égales si le cylindre est parfait. Le citoyen

Lefévre-Gineau a mesuré chacune de ces hauteurs plusieurs fois, et à chaque fois il les a comparées à une lame de laiton bien déterminée, que nous nommerons *règle des hauteurs*. Figurons-nous encore qu'on ait tracé sur la surface convexe du cylindre, à des distances déterminées, huit cercles, et qu'on ait tiré des droites qui joignent les extrémités des six diamètres correspondans tirés précédemment sur les bases, et on aura quatre-vingt-seize intersections qui formeront quarante-huit diamètres, six pour chaque cercle. Ces diamètres ont été mesurés avec les mêmes soins que les hauteurs, et comparés successivement à une règle de laiton bien déterminée, que nous nommerons *règle des diamètres*. Il seroit superflu d'ajouter qu'on a eu égard à la température, qu'on a pris toutes les précautions pour qu'elle ne variât point pendant le cours de l'expérience, enfin qu'on a porté l'attention la plus scrupuleuse sur tous les détails.

Ces comparaisons ont prouvé que le corps dont il est question n'est pas un cylindre parfait, puisque les deux bases ne sont pas exactement parallèles entre elles, et que même elles ont une légère courbure; que les sections parallèles aux bases ne sont pas, rigoureusement parlant, des cercles, quoiqu'elles en diffèrent d'une quantité extrêmement petite; enfin que les diamètres de ces sections ne sont pas parfaitement égaux, mais augmentent progressivement, quoique très-peu, d'une base à l'autre, et qu'ainsi le corps approche un peu d'être un cône tronqué. Toutes ces différences, quelque

petites qu'elles soient réellement, sont donc exactement connues, déterminées avec une grande précision; et conséquemment il n'a pas été difficile à des géomètres de calculer quel doit être le diamètre moyen, quelle doit être la hauteur moyenne d'un cylindre idéal égal au volume du corps employé, sans qu'il en résulte aucune erreur sensible; et c'est ainsi que la légère imperfection que la main la plus habile ne sauroit éviter dans ce qu'elle entreprend de faire, disparoît, et n'a plus d'influence, dès que des physiciens et des mathématiciens se réunissent pour en faire l'examen et l'évaluation.

Mais cette hauteur et ce diamètre moyens ne sont encore que des quantités relatives, puisque l'une est rapportée à la *règle des hauteurs*, l'autre à celle *des diamètres*. Il a donc fallu déterminer la longueur de ces règles en mesures connues; ce qui a été fait par des moyens analogues à ceux que les citoyens Borda et Brisson ont employés pour vérifier la longueur du mètre provisoire, et qu'ils ont décrits dans leur rapport (1) sur ce sujet. La nature de celui-ci nous interdit tout détail numérique qui ne présenteroit par lui-même aucun intérêt. Il suffira de dire qu'à la température de $17^{\circ}\frac{6}{10}$ du thermomètre centigrade, le volume du cylindre employé est, à très-peu près, 11 fois le cube du décimètre, plus 29 centièmes (2).

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(1) *Rapport sur la vérification du mètre.* A Paris, de l'imprimerie de la République. Thermidor an 3.

(2) Exactement 0,0112900054 du mètre cube.

Ce volume étant déterminé, il s'agit de le peser d'abord dans l'air, ensuite dans l'eau distillée, pour connoître le poids d'un pareil volume de cette eau. Il est à ce sujet plus d'une précaution à prendre. Il faut d'abord des balances extrêmement exactes. Celles que le citoyen Fortin a faites pour ces expériences sont d'une construction particulière. L'une d'elles, chargée d'un peu plus de deux livres, poids de marc, dans chaque bassin, est encore sensible à la millionième partie de ce poids, c'est-à-dire, d'un cinquantième de grain ; et elle trébuche à un dixième de grain lorsque chaque bassin porte environ vingt-trois livres.

Il ne suffit pas d'avoir des balances exactes, il faut que les poids qu'on emploie le soient aussi. Le citoyen Lefévre-Gineau en a fait faire onze, tous en laiton, tous parfaitement égaux, et vérifiés avec l'attention la plus scrupuleuse : comme ce sont des poids arbitraires, nous les nommerons *unités*. Les subdivisions, faites également avec la plus grande exactitude, étoient des dixièmes, centièmes, millièmes, et ainsi de suite jusqu'à des millionièmes. Les subdivisions de même nom ont été comparées entre elles pour juger de leur parfaite égalité, et ensuite réunies à leur décuple, pour être certain de leur valeur réelle et absolue. Le citoyen Lefévre-Gineau a mis beaucoup d'attention et de patience à tous ces préparatifs, persuadé que ce n'est qu'à ce prix qu'on achète la précision dans ce genre d'expériences.

Il y a plus; la construction du corps qu'il s'agit de peser n'est pas indifférente. Pour l'exactitude des pesées

il faut qu'il soit aussi léger qu'il sera possible, afin qu'il ne fatigue pas trop la balance, et néanmoins il doit être assez pesant pour qu'il plonge dans l'eau par son propre poids : c'est la raison pour laquelle le cylindre dont on s'est servi est creux, comme nous l'avons dit ci-dessus; et l'excès du poids de sa partie solide sur le poids d'un volume d'eau égal à tout le corps est très-petit. Mais, puisque ce cylindre est creux, il s'ensuit qu'il contient de l'air : on a sagement laissé, au moyen d'un tube de laiton qu'on y applique, une communication libre entre l'air intérieur et celui de l'atmosphère, lors même que le cylindre est plongé dans l'eau. Vous sentirez, dans un moment, quelle a été la principale raison de ce procédé.

Il faut enfin des précautions dans les pesées mêmes, pour être sûr de l'équilibre vrai. Il faut avoir soin que le centre de gravité des masses qui font équilibre, corresponde avec les centres des bassins; et comme il se pourroit qu'il y eût quelque inégalité dans les deux bras de la balance, il faut se servir du même bras, et pour le corps qu'on veut peser, et pour le contre-poids qu'on emploie. On cherche donc d'abord l'équilibre entre le corps à peser et une masse quelconque; on ôte le corps à peser du bassin qui le contenoit, et on lui substitue le contre-poids, qu'on rend égal à la masse équilibrante : l'égalité de ce contre-poids et du corps à peser est conséquemment déterminée d'une manière sûre, et absolument indépendante de la parfaite égalité des bras de la balance, qu'il est si rare de pouvoir obtenir.

Les pesées dans l'air forment la partie la moins difficile de l'opération. Le milieu de cinquante-trois expériences, dont les extrêmes ne diffèrent pas de quarante-cinq millionièmes parties, a donné pour ce poids 11,4660055 unités. Quoique ce cylindre ait été pesé dans l'air, ce poids est exactement celui qu'il auroit étant pesé dans le vide, parce que, d'une part, le contre-poids employé est de la même matière que le cylindre, et par conséquent est, à poids égal, de même volume que la partie solide de ce corps; et que, de l'autre, l'action de l'air qui soutiendroit le reste du volume apparent de ce cylindre creux, est détruite par la communication qu'on a laissée entre l'air intérieur du cylindre et l'atmosphère : de sorte que, si l'on transportoit dans le vide tout l'appareil d'une balance à laquelle seroient suspendus, d'un côté le cylindre, de l'autre le contre-poids, l'équilibre qui auroit lieu dans l'air n'y seroit pas détruit.

Il est bien plus difficile (et tous les physiciens en conviendront aisément) de peser le cylindre dans l'eau que dans l'air; et cependant les extrêmes de trente-six pesées n'ont varié que de quarante-cinq millièmes parties, tant on a employé de soins et de dextérité; et leur terme moyen a donné, pour le poids *apparent* du cylindre dans l'eau, 0,209419 parties de l'unité. Je dis le poids *apparent;* car le poids vrai diffère, par plusieurs raisons, de celui que nous venons d'énoncer : en voici les preuves.

Premièrement, l'air soutient le contre-poids, et ne

soutient pas le corps plongé dans l'eau : si donc on transportoit l'appareil dans le vide, ce contre-poids, perdant son support, se trouveroit trop fort de toute la quantité dont il a été soutenu, c'est-à-dire, du poids de l'air sous un volume égal : première réduction.

Secondement, ce poids apparent n'exprime pas seulement le poids que le cylindre a dans l'eau, mais en outre le poids de l'air contenu dans le creux du cylindre. Il faut donc retrancher celui-ci pour obtenir le poids du cylindre seul : seconde réduction.

Troisièmement, ce poids n'est encore que relatif, tant qu'on ne fait pas attention à l'état dans lequel l'eau se trouve, et qu'on ne détermine pas pour celle-ci un état constant. L'eau, comme tous les corps, se dilate par la chaleur, se condense par le froid ; et un même volume d'eau se trouve par-là avoir différens poids à différentes températures. C'est pourquoi l'Académie des sciences a choisi une température constante, celle de la glace fondante : c'est aussi à peu près à cette température qu'ont été faites les expériences dont nous venons de rendre compte. Mais, quelques soins que se soient donnés les citoyens Lefévre-Gineau et Fabroni, en entourant le vase qui contenoit l'eau, d'une grande quantité de glace pilée, et renouvelant fréquemment celle-ci, ils n'ont jamais pu parvenir à faire descendre le thermomètre centigrade au-dessous de deux dixièmes de degré ; et la température moyenne de l'eau, pendant le cours de leurs expériences, a été de $\frac{3}{10}$.

Mais cette règle générale, que les corps se condensent

à mesure que leur température s'abaisse, n'est vraie qu'autant que ces corps ne changent pas de nature : au moment où ils en changent, toute loi de continuité cesse; et l'on sait que l'eau est bien près d'en changer lorsque le thermomètre est à la glace fondante, ou un peu au-dessous de ce point, puisqu'il suffit d'une légère augmentation de froid pour la faire passer de l'état de corps fluide à celui de solide. Mais elle se dilate au moment de sa congélation; et si rien ne se fait par saut, cette dilatation ne commence-t-elle pas avant la congélation même? Les expériences de Deluc paroissoient annoncer qu'elle a lieu dès le cinquième degré, c'est-à-dire que là seroient la limite de la condensation, le point qui sépare la condensation de la dilatation, celui où l'eau est à son *maximum* de densité. Cet objet étoit trop important pour qu'on ne fît pas les recherches nécessaires pour le déterminer; et c'est sur-tout sur ce point que l'on doit beaucoup au zèle et aux lumières du citoyen Tralles, qui a profondément discuté tout ce qui y a rapport. En effet, les expériences du citoyen Lefévre-Gineau ont fourni les moyens de parvenir à un résultat précis. Ce physicien, desirant lui-même de connoître ce qui pouvoit avoir lieu sur cette matière, avoit eu l'attention de faire des pesées très-exactes, non seulement aux environs du point de la glace fondante, mais encore à des températures plus élevées : on les a examinées, combinées entre elles; on en a calculé les résultats, et il a été prouvé que le corps plongé dans l'eau est d'autant plus soutenu par ce fluide que celui-ci se refroidit davantage,

et cela jusque vers le quatrième degré ; mais que, passé ce terme, il l'est graduellement moins à mesure que la température approche du terme de la glace : d'où il suit que l'eau se condense jusqu'à un certain degré, et se dilate ensuite passé ce terme ; point de physique important, qui ne peut plus être sujet au doute. Et c'est ainsi que des expériences bien faites présentent toujours des résultats intéressans, souvent même nouveaux ; mais ce n'est que l'homme de génie qui les entrevoit, que le mathématicien qui peut les saisir avec précision et en calculer la valeur. Il y a plus : cette vérité, directement constatée par les pesées, c'est-à-dire, par les poids successivement plus grands jusqu'à un certain terme, et puis graduellement plus petits, que perd le corps plongé dans l'eau, méritoit d'être confirmée par l'évaluation immédiate des condensations ou des dilatations mêmes. Le citoyen Lefévre-Gineau a encore fait, sur ce sujet, des expériences qui seront publiées en détail. Elles sont infiniment précieuses pour notre objet, puisqu'elles nous prouvent que la nature nous présente un état de l'eau non seulement constant, mais même *unique*, celui où elle a un *maximum* de densité : d'où il suit que cet état unique seul doit servir de mesure aux autres, qui sont variables. Aussi la commission n'a-t-elle pas hésité à l'employer, et à retrancher encore du poids apparent primitivement fixé, $\frac{144}{10000}$ parties de l'unité, que le corps perd de plus lorsque l'eau est à son *maximum* de densité, que lorsqu'elle est à $\frac{3}{10}$ au-dessus de la glace ; et c'est-là une troisième réduction : réduction

nouvelle, importante, et absolument indépendante de la connoissance de la température. Toutes ces réductions donnent pour le vrai poids du cylindre dans l'eau distillée, prise au *maximum* de sa densité, 0,1953268 parties de l'unité.

Tel est le résultat des pesées; il ne s'agit plus que d'en déduire les conclusions.

Si l'on retranche le poids du cylindre pesé dans l'eau, du poids qu'il a étant pesé dans l'air, et qui, comme nous l'avons dit, est le même que celui qu'il auroit eu pesé dans le vide, on trouvera que ce poids est de 11,2706787 unités; et c'est-là le poids de l'eau distillée, prise à son *maximum* de densité, et contenue sous un volume égal à celui du cylindre. Mais quel est ce volume? Nous vous avons dit ci-dessus qu'il étoit de 11 décimètres cubes et $\frac{29}{100}$ (1); mais, dans la pesée, le volume a changé, il n'est plus celui que nous venons d'énoncer. En effet, le cylindre avoit ce volume à la température de 17° $\frac{1}{4}$; mais il étoit à la température de $\frac{3}{10}$ quand il a été pesé dans l'eau : il a donc éprouvé une contraction, une diminution de volume, à laquelle il faut faire attention, et que le résultat de l'expérience sur la dilatation du laiton nous met en état de calculer. D'un autre côté, le volume a acquis une petite augmentation, parce qu'une partie du tube auquel on le suspendoit plongeoit dans l'eau; augmentation à laquelle on a eu égard. Ces deux considérations ont réduit le

(1) Exactement 11,2900055.

volume primitif à 11,2796203 décimètres cubes, et c'est-là le volume d'eau qui pèse 11,2706787 unités : d'où il est aisé de conclure qu'un seul *décimètre cube d'eau* réduite à son *maximum* de densité, pèse 0,9992072 parties de l'unité; poids qui constitue ce qu'on nomme, dans le nouveau système métrique, le *kilogramme; kilogramme* vrai, et qui se trouve déterminé par une suite d'expériences, de calculs et de réductions, auxquels on ne se seroit peut-être pas attendu au premier abord.

Mais quel est le rapport de ce poids arbitraire que nous avons nommé *unité*, aux anciens poids? C'est une dernière question qu'il s'agit de résoudre. On s'est servi de ce corps précieux, et respectable même par son antiquité, qu'on nomme *la pile de Charlemagne*, et dont le poids est de cinquante marcs. Le citoyen Lefévre-Gineau a pesé itérativement, et avec le plus grand soin, ces cinquante marcs, c'est-à-dire cette pile entière, et il a trouvé qu'elle est égale à 12,2279475 unités : d'où il résulte que chaque unité est égale au poids de 18842,088 grains poids de marc; et que le vrai *kilogramme*, le poids d'un décimètre cube d'eau distillée, prise à son *maximum* de densité, et pesée dans le vide, ou *l'unité de poids*, est de 18827,15 grains, ou de 2 livres 5 gros 35,15 grains (1).

(1) Comme les physiciens se sont beaucoup occupés de fixer le poids d'un pied cube d'eau distillée, nous ajouterons que, d'après ces expériences, le pied cube d'eau distillée, prise à son *maximum* de densité, est de 70 livres 223 grains; qu'il pèse 70 livres 141 grains, si on prend l'eau à la température de $\frac{1}{10}$

Si la pile dite *de Charlemagne* avoit été faite avec une précision rigoureuse, le marc unique creux et le marc plein, qui en font partie, seroient égaux entre eux, et chacun d'eux seroit égal à la cinquantième partie de la pile entière. Mais quoique cette pile ait été faite avec soin, et avec une exactitude à laquelle on ne s'attendroit peut-être pas dans un monument de ce genre du quatorzième siècle, où l'on prétend que ce poids a été fait ou renouvelé, le marc creux et le marc plein diffèrent, et entre eux, et de la cinquantième partie du total, d'une quantité petite à la vérité, mais néanmoins réelle et sensible (1). Le *marc* que le célèbre Tillet a employé en 1767, dans le grand travail qu'il fit alors pour la comparaison des poids employés dans plusieurs parties de la France et dans d'autres pays (*marc* que la commission a eu occasion de vérifier, puisque l'un de ses membres, le citoyen Brisson, en possède un qui lui a été fourni par Tillet même), est encore différent de ceux dont nous venons de parler. Les marcs employés dans le commerce se trouveront donc différer entre eux, selon les étalons d'après lesquels ils auront été faits;

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de degré, et qu'il seroit de 70 livres 130 grains, si on prenoit l'eau à la glace fondante.

(1) Le marc, supposé la cinquantième partie de la pile entière, a été trouvé de . . . . . 0,2445589 unité.

Le marc creux 0,2445127

Le marc plein 0,2444675

Ainsi les différences sont, entre le marc pris de la pile entière et le marc creux, de 0,87 grain; entre le même et le marc plein, de 1,72 grain; entre le marc creux et le marc plein, de 0,85 grain.

différence qui, en prouvant, d'un côté, que jusqu'à ce jour on n'a pas eu de poids uniformes, et qu'il est temps de remédier à un inconvénient aussi grave, fait voir, de l'autre, que dans l'évaluation qu'elle fait du kilogramme en poids anciens, la commission doit s'en tenir au marc moyen de la pile de Charlemagne. C'est aussi à ce marc moyen qu'on a comparé le kilogramme provisoire, qui avoit été fixé, d'après les expériences des citoyens Lavoisier et Haüy, à 18841 grains.

Tel est le précis des expériences qui ont été faites pour les déterminations de l'unité de poids, seconde base essentielle du système métrique. Dignes émules des citoyens Méchain et Delambre, les citoyens Lefévre-Gineau et Fabroni ont contribué avec eux, comme à l'envi, chacun dans la partie qui lui a été confiée, à la perfection d'un système métrique, attendu depuis long-temps avec impatience par tous ceux qui attachent de l'importance au bien-être de la société, à la facilité des opérations de commerce, à leur intégrité, et à tout ce qui peut contribuer à en bannir les fraudes, les voies obliques, et ces manœuvres si fréquentes, mais non moins condamnables, fondées uniquement sur les différences réelles qu'il y a entre des mesures qui portent le même nom, et que néanmoins on fait tacitement passer pour égales; différences sur lesquelles la plupart des hommes ne sont ni ne peuvent être instruits.

Il nous reste à vous présenter les étalons que la commission des poids a fait faire, et à vous proposer quelques réflexions intéressantes sur ce sujet.

Commençons par l'étalon du mètre.

Nous avons dit que le mètre, la dix-millionième partie du quart du méridien, est de 443 l. $\frac{296}{1000}$ de la toise du Pérou. Une ligne mathématique qui auroit cette longueur, seroit donc le mètre, un mètre mathématique, idéal, et à l'abri de toute variation. Mais il s'agit d'un étalon, c'est-à-dire, d'un mètre, si je puis m'exprimer ainsi, *matériel*, *physique*, qui représente le mètre idéal dont nous venons de parler. La loi du 18 germinal an 3 fixe la matière dont ce mètre étalon doit être fait. « Ce » sera, dit l'article II, une règle de platine sur laquelle » sera tracé le mètre : cet étalon sera exécuté avec la » plus grande précision, d'après les expériences et les » observations des commissaires chargés de sa déter- » mination, et il sera déposé près du Corps législatif, » ainsi que le procès-verbal des opérations qui auront » servi à le déterminer ». Et l'article III nomme cet étalon, *l'étalon prototype*. La commission a donc employé le platine, conformément à la loi. Mais ce métal, comme tous les autres corps, éprouve des variations de longueur par celles de température : ainsi un mètre fait de platine ne sauroit avoir dans tous les temps la longueur du mètre idéal, comme aussi des mètres faits de différens métaux ne sauroient être égaux entre eux à toutes les températures ; il n'en est qu'une à laquelle ils le sont et peuvent l'être. Ces différences tiennent à la nature même des choses, et sont hors de la puissance de l'homme ; ce qui lui reste, c'est la faculté de tout réduire à un terme constant et invariable. Ce terme

dépend ici du degré de température qu'on choisira, pour donner exactement au mètre de platine la longueur de la dix-millionième du quart du méridien terrestre déterminée ci-dessus, et au degré de température auquel tous les mètres, de quelque matière qu'ils soient faits, seront exactement égaux entre eux et à celui-ci. La commission, en suivant l'esprit du système métrique proposé par l'Académie et adopté par la loi, a choisi la température de la glace fondante, ou ce que nous nommons le *zéro* de nos thermomètres; température constante. C'est donc à cette température que l'étalon de platine a été rendu égal à 443 l. $\frac{296}{1000}$ de la toise du Pérou, cette toise étant supposée à 16° $\frac{1}{4}$, comme il a été dit ci-dessus.

Nous présentons à l'Institut, au nom de la classe des sciences mathématiques et physiques, le mètre en platine destiné à être offert au Corps législatif, et à y rester en dépôt. Il a été fait, comme tous les autres, par l'excellent artiste Lenoir, sous la direction des membres de la commission qui ont été nommés pour suivre cet objet; et il a été vérifié avec le plus grand soin, et avec des précautions qui seront constatées par un procès-verbal. Cet étalon sera, sans doute, conservé avec le même soin, je dirois volontiers, avec ce même respect religieux avec lequel on a conservé *la pile de Charlemagne* pendant cinq siècles, au bout desquels ce précieux monument se trouve n'avoir pas subi de changement. Mais, par sa nature même, cet étalon de platine ne doit servir que dans les cas, extrêmement

rares, où il s'agiroit de faire des vérifications très-importantes; il ne sauroit servir aux étalonnages ordinaires, et ne doit absolument pas être employé. Aussi la commission a-t-elle fait faire, avec le même soin et avec les mêmes précautions, des mètres de fer exactement égaux entre eux, et, à la température de la glace fondante, à celui de platine dont nous venons de parler. Nous en présentons quelques-uns à l'Institut : ils devront servir à étalonner les mètres destinés aux usages de la société, et ils portent aux deux extrémités des saillies en laiton pour les préserver de toute usure. Mais puisqu'aucun métal ne conserve constamment la même longueur, et que différens métaux éprouvent des changemens différens par les mêmes variations de température, il conviendroit de faire ces étalonnages au dixième ou au quinzième degré du thermomètre centigrade, puisqu'alors une variation de dix degrés dans la température, variation qui produit, ou le froid à peu près glacial, ou un assez grand degré de chaleur, ne feroit différer entre eux des mètres faits de différens métaux, que de $\frac{3}{100}$ de millimètre, s'ils sont, l'un de fer, et l'autre de platine; et de $\frac{6}{100}$ de millimètre, s'ils sont de laiton et de fer : à quoi nous croyons devoir ajouter que le mètre provisoire, qui a été fait en laiton, a été déterminé pour la température de 10 du thermomètre centigrade.

Nous présentons aussi les étalons des poids : d'abord, un kilogramme de platine, destiné pour le Corps législatif, et pour y être conservé avec les attentions les plus

scrupuleuses, sans qu'on en fasse jamais d'usage que pour les cas rares, d'une grande importance ; ensuite plusieurs kilogrammes de laiton faits avec la même exactitude, égaux entre eux, et qui sont destinés aux usages civils et aux étalonnages ordinaires. Tous ces kilogrammes ont été faits par le citoyen Fortin.

Quoique ces deux kilogrammes, celui de platine et celui de laiton, soient l'un et l'autre des *kilogrammes vrais*, ils n'ont pas le même poids étant pesés à l'air, et ne doivent pas l'avoir : le kilogramme de laiton est le seul qu'il faille employer pour les pesées dans l'air. C'est un paradoxe que nous devons nécessairement vous expliquer : il tient uniquement à la différence des métaux, et l'explication sera aussi courte que simple.

Qu'est-ce qu'une masse de métal qu'on nomme *kilogramme?* C'est le représentatif d'une masse d'eau prise à son *maximum* de condensation, contenue dans le cube du décimètre, et pesée dans le vide. Nos deux kilogrammes de platine et de laiton, ces deux représentatifs d'une même masse d'eau, doivent donc avoir le même poids dans le vide : mais par-là même ils ne peuvent être égaux en poids que là, et doivent être inégaux dans l'air. Figurons-nous, en effet, qu'ils soient suspendus dans un récipient, mais dans l'air, à la balance la plus exacte et la plus mobile, et qu'ils soient dans un équilibre parfait : nous aurons, d'un côté, un volume, celui de laiton, d'un peu plus de six pouces cubiques ; et de l'autre, un volume, celui de platine, de deux pouces $\frac{4}{10}$ seulement : c'est l'image

d'une expérience de physique que tout le monde connoît. Supposons qu'on fasse le vide dans ce récipient, c'est-à-dire, qu'on en fasse sortir l'air qui soutenoit les corps à raison de leur volume : qu'arrivera-t-il? Le kilogramme de laiton, perdant deux fois et demie plus de support que celui de platine, prévaudra; il se trouvera avoir plus de poids; et cet excès sera le poids de trois pouces et $\frac{6}{10}$ d'air qui formoient l'excès du support pour le laiton au-dessus de celui pour le platine, et conséquemment il sera de 1 gr. $\frac{2}{5}$. Au contraire, si le kilogramme de platine avoit été à l'air plus pesant de 1 gr. $\frac{2}{3}$, ou de 88 milligrammes et $\frac{4}{10}$, le kilogramme de laiton devenant dans le vide plus pesant de cette quantité, l'équilibre auroit été rétabli; et les deux masses auroient dans le vide le même poids, celui de la masse d'eau dont ils sont les représentatifs, et qui, comme nous l'avons dit ci-dessus, est exprimé dans le vide, comme dans l'air, par le contre-poids de laiton qu'on a employé dans le cours des expériences. Nous avons cru devoir faire cette observation, simple à la vérité, mais d'un genre assez délicat, pour expliquer par quelles raisons deux corps de différente densité, représentatifs l'un et l'autre d'une même masse d'eau, ou du *kilogramme vrai*, doivent nécessairement être inégaux en poids quand on les pèse à l'air, et pourquoi, puisque c'est dans ce fluide que nous faisons toutes nos pesées, la masse de laiton est la seule qu'on doit employer pour les étalonnages et pour représenter le *kilogramme primitif*.

Tels sont donc les étalons vrais des deux unités dans

le nouveau système métrique, celui de l'unité de longueur, et celui de l'unité de poids; ils seront sans doute conservés avec le plus grand soin. Mais tel est encore l'avantage du nouveau système métrique, avantage non accidentel, mais qui lui est vraiment essentiel, parce que son essence est d'employer des types de mesures pris dans la nature: c'est que, quand même tous les étalons viendroient à être détruits, anéantis, de sorte qu'il ne restât de tout le système d'autre trace que le seul souvenir que l'une des deux unités est la dix-millionième partie du quart du méridien terrestre, et l'autre la masse d'eau prise à son *maximum* de densité et contenue dans le cube de la dixième partie de la première unité, on pourroit encore retrouver parfaitement leur valeur primitive. Il est aisé de sentir que, pour recouvrer celle des poids, il n'y auroit qu'à répéter les expériences du citoyen Lefévre-Gineau, et qu'à y mettre les mêmes soins et la même dextérité qu'il a employés; expériences pénibles, il est vrai, mais qu'on peut faire dans tous les temps, et par-tout, sans se déplacer. Il ne s'agiroit donc que de rétablir *le mètre;* et il ne seroit pas nécessaire pour cela de répéter une opération aussi difficile, aussi délicate, que celle que les citoyens Méchain et Delambre viennent de terminer. Il suffiroit d'exprimer dès à présent en parties du mètre la longueur du pendule simple, qui bat les secondes dans un lieu déterminé, et de donner aux expériences qui serviroient à fixer cette longueur un degré d'exactitude qui ne laissât rien à desirer. La longueur du pendule deviendroit par

là une *unité secondaire*, infiniment précieuse à tous égards; unité encore puisée dans la nature, et dont aucune cause destructive quelconque ne sauroit altérer la longueur. Aussi l'Académie des sciences avoit-elle parfaitement saisi cette idée; et un de ses premiers soins, en méditant sur le système métrique, a été de nommer des commissaires pour faire des expériences sur la longueur du pendule : elles ont été faites à l'observatoire national par les citoyens Borda, Méchain et Cassini, avec un appareil digne du génie de ceux qui l'ont imaginé, et à l'exactitude duquel il seroit difficile, pour ne pas dire impossible, de rien ajouter. C'est encore le citoyen Lenoir qui l'a exécuté. Borda a décrit ces expériences dans un mémoire dont il a présenté une copie à la commission, et qui sera imprimé. Nous nous contenterons de dire que par un milieu de vingt expériences, toutes faites avec une précision singulière, puisque ce milieu ne s'écarte pas d'un cent-millième des extrêmes, et discutées avec cette sagacité rare qui caractérisoit d'une manière si distinguée le citoyen Borda, dont nous pleurons encore amèrement la perte, cette longueur du pendule simple qui bat les secondes à Paris a été trouvée de $\frac{2549919}{10000000}$ du module, supposé à la glace fondante : d'où il est aisé de conclure que cette longueur est de $\frac{99827}{10000000}$ du mètre. Il sera donc toujours facile de retrouver le mètre en déterminant à Paris la longueur du pendule simple; il seroit même très-avantageux, pour le perfectionnement des sciences physiques, que la longueur fût déterminée avec la plus grande exactitude

pour plusieurs endroits, et principalement au bord de la mer, sous la latitude du quarante-cinquième degré. L'Académie des sciences, qui sentoit toute l'importance dont cette expérience pouvoit être, l'avoit proposée comme devant couronner cette grande opération, et lui servir de complément : espérons qu'elle pourra être exécutée sous peu, comme elle mérite de l'être.

Tel est, citoyens, le résumé général de ce qui a été fait pour la détermination des bases du systême métrique, et des conclusions les plus générales déduites d'une opération qui fera époque dans l'histoire des sciences. La commission des poids et mesures a fait tous ses efforts pour remplir la tâche qui lui avoit été prescrite, d'une manière qui pût mériter votre approbation, comme elle a obtenu celle de la classe des sciences physiques et mathématiques. Il ne nous reste qu'à former des vœux pour que ce beau systême métrique s'établisse dans la République française entière avec toute la célérité que son bien-être, la nature des choses et la prudence pourront permettre; qu'il soit adopté par tous les peuples de la terre, et qu'il serve à faciliter leurs liaisons commerciales, à en assurer l'intégrité, et à resserrer entre eux les nœuds fraternels qui devroient les unir. Puisse une paix aussi glorieuse qu'elle est ardemment desirée, hâter le moment de cette union, et assurer à l'Europe entière un état heureux et tranquille!

XVII. *Observations on the Measurement of three Degrees of the Meridian conducted in England by Lieut. Col.* William Mudge. *By Don* Joseph Rodriguez. *Communicated by* Joseph de Mendoza Rios, *Esq. F. R. S.*

Read June 4, **1812.**

THE determination of the figure and magnitude of the earth has at all times excited the curiosity of mankind, and the history of the several attempts made by astronomers to solve this problem might be traced to the most remote antiquity. But the details of the methods pursued by the ancients on this subject being extremely vague, and their results expressed in measures of which we do not know the relation to our own, in fact give us very little assistance in learning either the figure or dimensions of our globe.

It was not till the revival of science in Europe that the two great philosophers, HUYGHENS and NEWTON, first engaged in the consideration of this question, and reduced to the known laws of mechanics, the principles on which the figure of the earth should be determined.

They demonstrated that the rotatory motion should occasion differences in the force of gravity in different latitudes, and consequently that parts of the earth in the neghbourhood of the equator should be more elevated than those near the poles.

The most simple hypothesis, which first presented itself to

their imagination, was that which supposed the earth to be throughout composed of the same kind of matter, and its surface that of a spheroid generated by revolution round its axis. This hypothesis, adopted by NEWTON only as an approximation to the truth, is, in fact, perfectly consistent with the equilibrium to which particles in a state of paste, or of tardy fluidity, would arrive in a short time after their present motion was impressed; and the eccentricity derived from this hypothesis is at least not very remote from that which actually obtains in the present state of consistence and stability which the earth has since acquired.

But the homogeneity of the matter, of which the earth consists, is at variance with all geological observations, which prove evidently that at least 5000 toises of the exterior crust is formed of an immense mass of heterogeneous matters varying in density from each other; and upon the supposition of a state of fluidity of the whole, it should follow that the strata should successively increase in density from the surface towards the centre, that the more dense would accordingly be subjected to less of centrifugal force, and consequently that the spheroidical form resulting from this cause would be less eccentric than would arise from a state of perfect homogeneity.

The most simple, as well as the most effectual means of verifying the hypothesis respecting the figure of the earth, is to measure in the two hemispheres several arcs of its meridians in different latitudes, at some distance from each other. On this subject it must be allowed, that the Academy of Sciences at Paris set the example, in giving the original impulse to the undertaking, and not only commenced, but put

in execution those parts of the plan which were most difficult and most decisive.

The results of the first measurements made of different arcs on the meridian of different parts of the world, were found to be perfectly conformable to the expectations of HUYGHENS and of NEWTON, and also with experiments made on the vibration of the pendulum in different latitudes; and they left no doubt that the earth was in fact flattened at the poles; establishing thereby one point extremely interesting in natural philosophy.

These results, however, did not correspond with sufficient accuracy for ascertaining with precision the degree of eccentricity, or even the general dimensions of the earth, as might naturally be expected when we consider the necessary imperfection of the means then employed in these operations, and the great difficulties that are to be encountered.

For the purpose of making a nearer approximation to the true dimensions of the earth, and of verifying former measurements, it is necessary in some instances to repeat them, and also to make others in different situations, which may be expected to be improved in proportion to the progress that is made in the means of perfecting the several departments of science.

At the commencement of the French revolution, men of science took advantage of the general impulse which the human mind received in favour of every species of innovation, or change, and they proposed making a new measurement of an arc of the meridian in France, for the purpose of establishing a new system of weights and measures, which should be permanent, as being founded on the nature of things.

A commission, composed of some of the most distinguished members of the Academy of Sciences, was charged to form the plan of these operations, which were to serve as the basis of the new system. They invented new instruments, new methods, new formulæ, and in short almost the whole of this important undertaking consisted of something new in science.

Two celebrated astronomers, DELAMBRE and MECHAIN, were engaged to perform the astronomical and geodetical observations, and these they continued as far as Barcelona in Spain. The details of their operations, observations, and calculations, were subsequently examined by a committee of men of science, many of whom were foreigners collected at Paris, who confirmed their results, and by the sanction of such an union of talents, gave such a degree of credit and authenticity to their conclusions as could scarcely be acquired by other means.

Since that time, in the year 1806, Messrs. BIOT and ARAGO, members of the National Institute, were sent into Spain for the express purpose of carrying on the same course of operations still further southward, from Barcelona as far as Formentera, the southernmost of the Balearic islands. Fortunately this last undertaking, which forms a most satisfactory supplement to the former, was completed by the month of May, 1808, at a period when political circumstances would not admit of any further operations being pursued, as a means of verifying the results, by measuring a base which should be independent of those formerly obtained in France.

In the year 1801, the Swedish Academy of Sciences, encouraged by the success of the operations conducted in France, sent also three of its members into Lapland, to verify their former measurement taken in 1736, by new methods, and by

the use of new instruments, similar to those which had recently been used in France, and of which the National Institute made a handsome present to the Swedish Academy. The results of this new undertaking, which terminated in 1803, were drawn up by M. SVANBERG, and are highly interesting, by their exactness, by the perspicuity of the details, and even a certain degree of novelty given to the subject by the arrangement adopted by the learned author M. SVANBERG.

These new measures were found to confirm, in a remarkable manner, the general results of those which had preceded, and gave very nearly the same proportion for the eccentricity and other dimensions of the globe, so that there would not have remained the smallest doubt respecting the figure of the earth being flattened at the poles, had there not been a fourth measurement performed in England at the same time as that undertaken in Lapland, the results of which were entirely the reverse. This measurement, which comprised an arc of 2° 50′, was undertaken by Lieut. Col. MUDGE, Fellow of the Royal Society, with instruments of the most perfect construction that had ever yet been finished by any artist, contrived and executed for that express purpose, by the celebrated RAMSDEN. The details of the observations and other operations of Lieut. Col. MUDGE, may be seen in the volume of the Philosophical Transactions for the year 1803; and one cannot but admire the beauty and perfection of the instruments employed by that skilful observer, as well as the scrupulous care bestowed on every part of the service in which he was engaged. Bengal lights were employed on this occasion, as objects at the several stations, and their position appears to have been determined with the utmost precision by the theodolite of RAMSDEN, which

reduces all angles to the plane of the horizon, and with such a degree of correctness, that the error in the sum of the three angles of any triangle is scarcely, in any instance, found to exceed three seconds of a degree, and in general not more than a small fraction of a second.

Accordingly the geodetical observations were conducted with a degree of exactness, which hardly can be exceeded; and even if we suppose for a moment, that the chains made use of in the measurement of the bases may not admit of equal precision with the rods of platina employed in France, nevertheless, the degree of care employed in their construction, in the mode of using them, and the pains taken to verify their measures was such, that no error that can have occurred in the length of the base, could make any perceptible difference in the sides of the series of triangles, of which the whole extent does not amount to so much as three degrees.

Nevertheless, the results deduced by the author, from this measure alone, would lead to the supposition that the earth, instead of being flattened at the poles, is in fact more elevated at that part than at the equator, or at least, that its surface is not that of a regular solid. For the measures of different degrees on the meridian, as reduced by Lieut. Col. MUDGE, increase progressively toward the equator.

The following table of the different measures of a degree in fathoms is given by the author in his Memoir.

Latitude.	
52° 50′ 30″	60766
52 38 56	60769
52 28 6	60794
52 2 20	60820

Latitude.	
51° 51′ 4″	60849
51 25 18	60864
51 13 18	60890
51 2 54	60884

The singularity of these results excites a suspicion of some incorrectness in the observations themselves, or in the method of calculating from them. The author has not informed us in his Memoir, what were the formulæ which he employed in the computations of the meridian; but one sees, by the arrangement of his materials, that he made use of the method of the perpendiculars without regard to the convergence of the meridians; and although this method is not rigorously exact, it can make but a very few fathoms more in the total arc, and will have very little effect on the magnitude of each degree. It is therefore a more probable supposition, that, if any errors exist, they have occurred in the astronomical observations. But it is scarcely possible to determine the amount of the errors, or in what part of the arc they may have occurred, excepting by direct and rigorous computation of the geodetical measurement. I have therefore been obliged to have recourse to calculations, which I have conducted according to the method and formulæ invented and published by M. DELAMBRE.

The means generally employed for finding the extent of a degree of the meridian, consists in dividing the length of the total arc in fathoms, by the number of degrees and parts of a degree deduced from observations of the stars; but if these observations are affected by any error, arising from unsteadiness of the instrument, from partial attractions, or from any other accidental causes, then the degrees of the meridian will

be affected, without a possibility of discovering such an error in this mode of operating. It is consequently necessary, in such a case, to employ some other method, which may serve as a means of verifying the observations themselves, of detecting their errors, if there be any, or at least of shewing their probable limits.

My object therefore is to communicate the result of calculations that I have made, from the data published by Lieut. Col. MUDGE in the Philosophical Transactions; and I hope to make it appear, that the magnitude of a degree of the meridian, corresponding to the mean latitude of the arc measured by this skilful observer, corresponds very exactly with the results of those other measurements that have been above noticed.

In M. DELAMBRE's method nothing is wanting but the spherical angles, that is to say, the horizontal angles observed, corrected for spherical error. Moreover, for our purpose, we have no occasion for the numerical value of the sides of the series of triangles, but only for their logarithms. Thus the logarithm of the base measured at Clifton, as an arc gives us that of its sine in feet or in fathoms, so that by means of this latter logarithm, and the spherical angles of the series of triangles, we obtain at once, and as easily as in plane trigonometry, the logarithms of the sines of all their sides in fathoms.

After this, it is extremely easy to convert them into logarithms of chords or of arcs, for the purpose of applying them to the computation of the arcs on the meridian or azimuths. I give the preference to taking the logarithms of the sides as arcs, because the computations become in that case much more simple and expeditious.

Near to Clifton, which is the northern extremity of the arc,

in a situation elevated 35 feet above the level of the sea, a base was measured of 26342,7 feet in length, the chains being supposed at the temperature of 62° FAHRENHEIT, or $13\frac{1}{3}°$ REAUMUR.

For reducing this base to toises, we have the proportion of the English foot to that of France, as 4 : 4,263, so that if p be taken to express the fractional part of the French foot, corresponding to English measure, then log. $p = 9,97234,46587$,

and then log. of $26,342,7 = 4,42066,02860$,

and hence the log. of the base in toises will be found equal to 3,61485,36943, and the number of toises corresponding is 4119,5 taken at the same temperature, which corresponds to $16\frac{2}{3}°$ of the centigrade thermometer.

This base we must consider as an arc of a circle, and it is easy to reduce it to the sine of the same arc, according to the method given in a note at the end of this memoir. The logarithm of the *sine* of the base in toises is found to be 3,61485,35800.

With this quantity as base, and by means of the spherical triangles given by Lieut. Col. MUDGE in his paper, I have found the logarithmic sines in toises of all the sides of his series of triangles, and have subsequently reduced them to logarithmic arcs of the same, which enable me to complete the rest of the calculation. With these we may compute any portions of the meridian, or successive intervals of different stations expressed in toises, and in parts of the circle, or their respective azimuths, having regard always to the relative convergence of different meridians.

The author has made observations for determining the latitude of the two extremities of his arc, and has also determined

the azimuths of the exterior sides in his series of triangles by means of the greatest elongation of the pole star.

In the calculations that I have made, I began at Clifton in Yorkshire, the northern extremity of the arc, and for this purpose the following are the data furnished by Lieut. Col. MUDGE.

Latitude of Clifton reduced to the centre of the station 53° 27′ 36,″62.

Azimuth of Gringley, seen from Clifton, and reckoned from the north toward the west 256° 17′ 25″.

Azimuth of Heathersedge, seen from Clifton, and reckoned in the same direction 118° 8′ 8″,81.

With these data, and the two tables of spherical triangles, and the logarithms of their sides expressed in arcs, the intervals between Clifton and the two stations Gringley and Heathersedge were found in toises and in seconds of a degree, as well as all the corrections to be made on the first azimuths increased by 180°, as azimuths of Clifton seen on the horizon at these latter places.

The same process was continued for the following stations in succession, all the way to Dunnose in the Isle of Wight, which is the southernmost extremity of the series.

In this manner we have the latitudes and azimuths of each station, by means of two or three preceding stations, and consequently we have a verification of all the calculations that have been before made by Lieut. Col. MUDGE.

The results of my calculations are contained in the two following tables.

First Table of Distances in Toises and in Seconds of a Degree on the Meridian, comprised between the westerly Stations in the Series of Triangles.

Names of the Stations.	Arcs in Toises.	Arcs in Seconds.
Clifton -	0,0	0,0
Heathersedge	6834,324	430,9928
Orpit -	15818,489	997,5928
Castlering -	19801,1934	1248,8226
Corley -	14295,384	901,6207
Epwell -	22327,008	1408,2543
Stow -	9555,479	602,7284
Whitehorse	18799,645	1185,8656
Highclere -	14990,567	945,6354
Dean Hill -	16105,614	1016,0180
Dunnose -	23529,886	1484,4531
Sum total -	162057,5437	10221,9837

Second Table of successive Intervals between the Eastern Stations.

Names of the Stations.	Arcs in Toises.	Arcs in Seconds.
Clifton -	0,0	0,0
Gringley -	2809,105	177,149
Sutton -	10838,816	1061,931
Holland Hill	4681,190	295,2251
Bardon Hill -	18092,261	1141,0462
Arbury Hill	27956,417	1763,2683
Brill -	22374,106	1411,2769
Nuffield -	14350,3834	905,2155
Bagshot -	12137,933	765,6822
Hindhead -	14449,2027	911,5140
Butser Hill -	7853,644	495,4551
Dunnose -	20514,036	1294,1974
Sum total -	162057,0941	10221,9607

Now if we take the arithmetic mean of the sums contained in the two tables, we have for measures of the entire arc, comprised between the stations of Clifton and Dunnose, the following quantities 162057,32 toises, and 10221,972 seconds of a degree, or 2° 50′ 21″,972. By dividing the former of these by the second, we get the measure of a degree, corresponding to the mean latitude of the whole arc, equal to 57073,74 toises, or 60826,34 fathoms, at the temperature of $16\frac{2}{3}$° of the centigrade thermometer, the latitude being 52° 2′ 20″.

The station at Arbury Hill happens to be very nearly in the meridian of Clifton and Dunnose, and divides the interval between them into nearly equal parts. The measures of that part of the arc, which lies between Arbury and Dunnose, is by the tables 91679,47 toises, and 9783″,34 seconds, or 1° 36′ 23″,34 of the common division of the circle. The mean latitude of the arc is 51° 25′ 21″. And the measure of 1 degree corresponding to it is 57068,41 toises.

In the same manner the measure of the arc comprised between Arbury Hill and the northern extremity at Clifton, is 70377,85 toises, and 4438,63 seconds, or 1° 13′ 58″,63. Its mean latitude is 52° 50′ 32″. And we have for one degree of the meridian, corresponding to this latitude, 57080,70 toises.

Hence, if we divide the entire arc into two equal parts, we deduce the following values of a degree corresponding to the middle of the whole and of its parts.

Latitudes.	
51° 25′ 20″	57068
52 2 20	57074
52 50 30	57081

These values are, as appears, perfectly in conformity with the theory, and with the results of other measures that have been taken in different parts of the northern hemisphere; but, in order to place that agreement in a more distinct point of view, I shall show how nearly these estimates agree with the elliptic hypothesis, by comparing them with those measures of a degree, on which we can place the greatest reliance for exactness.

Now, if we compare the results of these calculations with those deduced by Lieut. Col. MUDGE from his observations, we shall see the probable source of those *errors*, which it appears to me have led him to false conclusions. It has already been observed, that the station at Arbury Hill divides the whole arc into two parts nearly equal, and that it is also nearly in the meridian of the two extremities at Dunnose and Clifton. It was, in all probability, this circumstance which determined the author to observe the latitude of Arbury Hill, as he would then have two partial arcs independent of the whole and of each other.

For determining the angular extent of these arcs, Lieut. Col. MUDGE observed the zenith distances of several stars on the meridian above the pole, by means of a large zenith sector constructed by RAMSDEN, with the same pains that he had bestowed upon the theodolite. Lieut. Col. MUDGE paid all possible attention, and took all such precautions as might naturally be expected from an observer of his experience and address. Nevertheless the results of his observations made on different stars, differ no less than 4 seconds from each other. But, by taking a mean of all, the dimensions of the three arcs reduced to the centre at each station are as follows.

Between Clifton and Dunnose	2° 50′ 23″,35
Clifton and Arbury	1 14 3 ,40
Arbury and Dunnose	1 36 19 ,95

The extent of the first arc, in linear measure, is 1036339½ feet English, and when this is reduced to toises, we have for the lengths of the three arcs from Lieut. Col. MUDGE's measures,

From Clifton to Dunnose	162067,3
Clifton to Arbury	70380,2
Arbury to Dunnose	91687,1

These last values exceed those resulting from my computations, the first by 10 toises, the second by 2, the third by 8 toises; and these differences arise from the convergence of the meridians, which the author thought might safely be neglected, and in fact it does not make a difference that is perceptible in the value of a degree upon the meridian. For the difference of 8 toises, in the distance between Dunnose and Arbury, makes but 5 toises difference in the value of a degree upon that arc, and the difference of 10 in the whole distance from Dunnose to Clifton, makes 3½ in the measure of each degree on that arc. So that, as far as this source of disagreement is concerned, the author's results and mine would not be found to differ materially from each other.

But, if we attend to the angular dimensions of the several arcs, as deduced from observation and from calculation, these will not be found to agree so nearly.

The following table will shew the differences in each instance.

Clifton and Dunnose	2° 50′ 23″,35	observed
	2 50 21 ,97	calculated
Difference	+ 1 ,38	
Clifton and Arbury	1° 14′ 3″,40	observed
	1 13 58 ,63	calculated
Difference	+ 4 ,77	
Arbury and Dunnose	1° 36′ 19″,95	observed
	1 36 23 ,34	calculated
Difference	− 3 ,39	

These differences are really considerable, and are capable of producing important errors in the results dependent on them.

In the first place we see, that the southernmost arc between Dunnose and Arbury is smaller than it would appear by computation, by as much as 3″,4, and when this deficiency is combined with an excess of 8 toises in the linear dimensions of the same arc, it makes as much as 40 toises difference in the estimated length of a degree. The reverse of this occurs in the northern portion of the arc comprised between Clifton and Arbury Hill. This is larger than it ought to be by 4″,77, and hence the value of a degree on the meridian turns out too small by about 62 toises in its linear dimensions. Fortunately, however, the excess of the total arc is extremely small, as it does not exceed 1″,38, so as to make but 5 or 6 toises difference in the length of a degree observed on the meridian, and corresponding to the mean latitude of the arc examined.

From what has been above stated, it seems almost beyond a doubt that it is to errors in the observations of latitude, that the appearance of progressive augmentation of degrees towards the equator, as represented by Lieut. Col. MUDGE in his paper, are to be ascribed, and that it is especially at the intermediate station at Arbury Hill, that the observations of the stars are erroneous nearly 5 seconds, notwithstanding the goodness of the instruments, and the skill and care of the observer. But, before I insist farther on this head, I will answer one objection that may be made to the principles of the method that I have pursued in this Memoir.

Those astronomers, who have hitherto undertaken the measurement of degrees of the meridian, have deduced their measures by simply dividing the linear extent by the number of degrees and minutes found by observation of the fixed stars taken at the two extremities of the arc. This is indeed the most simple that can be adopted; and it has the advantage of being independent of the elliptic figure of the earth, especially in arcs of small extent. The elements dependent on this figure, are too uncertain to be employed in calculating the angular intervals in the short distances between successive stations, even as a means of verification, without risk of committing greater errors than those to which astronomical observations can be liable. Accordingly one cannot safely make any use of it in cases where great accuracy is required.

I must admit the justness of this objection, and must therefore shew the extent to which it really applies to the present subject.

In the first place, I may suppose, that in consequence of some fault in the instrument, with respect to vertical position,

construction, or some accidental derangement, there is an error of some seconds in the observations of the fixed stars. How is this to be discovered? This is not to be done by comparing the value of a degree on the meridian, as deduced from these observations, with the results of other measurements in distant parts of the globe. For if we find that these degrees so taken do not agree in giving the same ellipsoid, we are not to attribute all the differences to irregularities of the earth, without supposing any error on the part of the observer, of his instrument, or of other means employed in his survey.

But this, in fact, is what has generally been done. It must, however, be acknowledged, that the majority of observers have not been in fault, as they could do nothing better; but too much reliance has been placed on the goodness of their instruments, their means, and other circumstances. It is true that irregularities of the earth and local attractions may occasion considerable discrepancies which are even inevitable; but before we decide that these are the real source of disagreement, we ought carefully to ascertain that there are no others.

But to return to our subject, of the English measurement. If the uncertainty which yet subsists, with respect to the exact figure of the earth and its dimensions, occasions some small errors in the calculation of the series of triangles, the sum of these errors will be found in the estimate of the entire arc, and will increase in proportion to the extent of the arc measured. Now, in the English measurement, we find exactly the reverse of this. For the difference between the results of calculation and observation is only $1'',38$ on the whole arc; but is even as high as $4'',77$ on one of the smaller arcs. So that, whatever error we may suppose to have been introduced

into the calculation by assuming a false estimate of the spheroidity of the earth, or of other elements employed in the calculation, it is very evident that the zenith distances of stars taken at Arbury Hill are affected by some considerable error, wholly independent of these elements.

It was not till the date of the measurement of the meridian in France, that M. DELAMBRE published and explained, with admirable perspicuity and elegance, all the formulæ and methods relative to the calculation of spheroids, and put it in the power of astronomers in general to make use of the elliptic elements in verifying the results of their observations. In the present state of science these elements are well known, and the errors that can arise from any uncertainty in them, are not so considerable as is generally supposed. The oblateness and the diameter at the equator are the only elements wanting in the calculation; for the purpose of seeing what effect our present uncertainty respecting them can have on the subject in question, I have employed three different estimates of the oblateness $\frac{1}{330}$, $\frac{1}{320}$, and $\frac{1}{310}$. With respect to the radius of the equator, that is ascertained with sufficient precision by the mean of the arc extending from Greenwich to Formentera, corresponding to latitude 45° 4′ 18″. The value of the degree in toises is 57010,5, and it is highly probable that in this estimate the error does not amount to so much as half a toise, as it is deduced from an entire arc of 12° 48′ between the two extremities, the latitudes of which have been determined with extreme care, and by a great number of observations.

The following are the logarithms of radius at the equator, which I have employed as adapted to each degree of oblateness,

and opposite to them are placed the corresponding computed estimate of the entire arc between Clifton and Dunnose.

$\frac{1}{330}$ .... 6,5147,400 ..... 2° 50′ 21,972
$\frac{1}{320}$ .... 6,5147,485 ..... 2° 50′ 21,974
$\frac{1}{310}$ .... 6,5147,670 ..... 2° 50′ 21,592

so that the greatest difference is but 0″,38. Let us suppose it 0″,4, or even 0″,5, for the second calculation was made only by means of the western series of triangles, and the third only with the eastern; but even then the error arising from uncertainty in the elements is not half the difference we find between the results of computation and of observations of the fixed stars. It appears therefore, that these elements are by no means to be neglected as a method of verification; and in fact the quantity of 1″,38 is so small, that it is extremely difficult to ascertain this quantity with the very best instruments. Of this we shall find further proof hereafter; but as this discussion is not without its use, I shall enter into some details on this subject.

The measurement in Lapland was performed by means of a double metre, and with a repeating circle of BORDA, sent by the National Institute of France. In order to see to what degree of accuracy the arc computed would agree with that obtained by observations of the pole star above and below the the pole, I assumed an oblateness of $\frac{1}{320}$, and as logarithm of radius I had 6,5147500 expressed in toises and in round numbers. With these elements, and with the data to be found in the work of M. SVANBERG, we have by the western series of triangles 5840″,196 and 5840″,138 by the eastern. So that the mean calculated arc is 1° 37′ 20″,167, while the arc observed was 1° 37′ 19″,566. The difference then is 0″,6 for

the total arc, and 0″,37 for the mean degree, or 5,86 toises excess in the linear extent. One can never depend upon quantities so small as this, so that the agreement between the results of computation and actual observation, proves not only the skill of the observers and the accuracy of which their instruments admit; but also that the elliptic elements employed in the calculation are a sufficiently near approximation to the truth to be deserving of confidence.

In the 8th volume of the Asiatic Researches, published by the Society at Calcutta, are contained the details of another measurement performed in 1802, by Major WILLIAM LAMBTON in Bengal, on the Coromandel coast. In this undertaking, which was executed with great skill and attention, Major LAMBTON employed Bengal lights as signals, chains for the linear measures, and a theodolite, and a zenith-sector made by RAMSDEN. The base measured was 6667,740 fathoms reduced to the level of the sea, and to the temperature of 62° FAHRENHEIT; and the stations were so chosen, that four of the sides of the triangles were almost in the same line, and nearly parallel to the meridian at the southern extremity of the arc, so that their sum but little exceeds its whole extent. The lengths of these arcs in fathoms reduced to the meridian are thus given in the Memoir of Major LAMBTON.

AB 20758,13 north latitude of A 11° 44′ 52″,59
BC 17481,245
CD 22237,04 north latitude of E 13° 19′ 49″,018
DE 35246,43

From these data Major LAMBTON deduces the degree of the meridian to be 60435 fathoms, or 56762,3 toises. By applying to this the same elements as we did to the measurement

by SVANBERG, we have the entire arc measured equal to 1° 34′ 55″,896; so that the difference between the results of calculation and of the observations, is only 0″,532 for the whole arc, or 0″,337 for the mean degree. The elliptic hypothesis and observation agree more correctly in this instance, for the difference is rather less than in that of Lapland, although the two arcs are very nearly of the same extent. Thus the degree on the meridian measured in Bengal, in the latitude of 12° 32′ 21″ north, cannot be supposed to exceed Major LAMBTON's estimate by more than 5,22 toises; and it is extremely difficult to speak with certainty to quantities so small as this.

The same observer also measured one degree perpendicular to the meridian, by means of a large side of one of his triangles cutting the meridian nearly at right angles, and of which he observed the azimuth at the two extremities. The data from which his results may be verified are these:

Length of the chord of the long side in English feet AB = 291197,20.

Azimuth of the eastern extremity A equal to 87° 0′ 7″,54 NW.

Azimuth of the western extremity B equal to 267° 10′ 44″,07 NW.

North latitude of A 12° 32′ 12″,27
North latitude of B 12° 34′ 38″,86.

With these data in the triangle formed by the long side, the meridian at B, and the perpendicular from B on the meridian at A, we have the chord of this last arc equal to 290845,8 feet, and the arc itself 290848,03 feet. By applying the method of M. DELAMBRE, we find the azimuth of the extremity B less by 2″ than it was observed to be; so that we have no

reason to suppose a greater error than one second in the observation of each azimuth, and it seems next to impossible to arrive at greater exactness.

The difference of longitude between the points A and B is 48′ 57″,36. With this angle and the co-latitude at A, we have in the spherical triangle right angled at the point A, the extent of the normal arc equal to 2867,330 seconds, and dividing its length in feet by this number, we have for the degree perpendicular to the meridian, at the extremity A, 60861,20 fathoms, or 57106,5 toises. Now these values are precisely what we find on the elliptic hypothesis, with an oblateness of $\frac{1}{320}$ or $\frac{1}{310}$; and in short, the correspondence between the hypothesis and the measures of Major LAMBTON, is as complete as can be wished. Major LAMBTON, indeed, finds the degree on the perpendicular too great by 200 fathoms, but this arises from a mistake in his calculation.

Lastly, I shall apply the same method, and see how nearly the elliptic hypothesis agrees with the last measures taken in France, which merit the highest degree of confidence both with respect to the observers who have executed it, and the means which they had it in their power to employ. I have taken only the arc between Dunkirk and the Pantheon at Paris, from the data published by the Chevalier DELAMBRE in the 3d Vol. of the Measurement of the Meridian. I employed the same elements and similar calculations to those made on the English arc. The oblateness of $\frac{1}{330}$ gives the difference between the parallels equal to 7883,615 seconds by the eastern series of triangles, and 7883,617 by the western series. The mean of these 7883,616 may be taken as the true extent of the total arc.

The two other elements give for this quantity 7883″,621

and 7883″,493, or 2° 11′ 23″,6 and 23″,49, as the calculated extent of the arc. But the arc observed was 2° 11′ 19″,83, according to M. DELAMBRE, and 2° 11′ 20″,85 according to M. MECHAIN; so that the least difference between the calculation and the observations will be 2″,64. M. DELAMBRE is of opinion, that the latitude of Dunkirk, which is supposed to be 51° 2′ 9″,20, should be diminished; and in fact the distance between the parallels of Dunkirk and Greenwich, which is 25241,9 toises, gives by the mean of the three assumed ellipticities 26′ 32″,3 for the difference of latitude. After deducting this quantity from 51° 28′ 40″, the supposed latitude of Greenwich, there remains 51° 2′ 7″,7 or 8″, for that of the tower at Dunkirk. If from this again we deduct the calculated arc 2° 11′ 23″,5, we have 48° 50′ 44″,5 for the latitude of the Pantheon, while, according to the observations of M. DELAMBRE, it is 49″,37, or 48″,35 by those of M. MECHAIN. If various circumstances, with regard to unfavourable weather, and also others of a different kind connected with the revolution, and of which M. DELAMBRE complains with much reason, have occasioned some uncertainty with respect to the observations at Dunkirk, still the numerous observations made at Paris, both by him and by M. MECHAIN at a more favourable season, and in times of perfect tranquillity, render the supposition of an error of 4 seconds in the latitude of the Pantheon wholly inadmissible. It is, however, too true, that such errors are possible, and it is only by careful perseverance, and by repeated verification, that they are to be discovered and removed, as we have seen to be highly probable with respect to the station at Arbury Hill.

But the same celebrated observer, M. MECHAIN, who handled

instruments with great delicacy, and was possessed of peculiar talents for this species of observation, has given us an instance of singular irregularity in the observations made at Montjui and at Barcelona.

The latitude of Montjui, determined by a very long and regular series of zenith distances, is full 3″,24 less than that deduced from a similar series of observations made at Barcelona, with the very same instruments, and with equal care. Moreover, there is reason to think, from other observations, that the latitude of Barcelona (which is supposed to be 45″) ought to be diminished still one second, so that the difference between the observations at Montjui and at Barcelona will probably amount to as much as 4″. Local attractions are supposed to have been the cause of this irregularity; but then the latitude, as deduced from observations made at Barcelona, should have been less than it appeared by those made at Montjui itself; for the deviation of the plumb-line (or of the spirit contained in a level) *could only* be occasioned by the little chain of land elevated to 120 or 130 toises, which passes to the north of Barcelona in a north-easterly direction. Now since the deviations arising from this source would be northward, the zenith distance of circumpolar stars would be augmented by that deviation, and consequently the latitude deduced therefrom would be diminished by just so much. But here the contrary occurs; for the latitude of Montjui deduced from the observations at Barcelona is 48″,23, whilst that obtained by direct observations at Montjui is only 45″. Hence it seems probable, that the cause of this irregularity must be sought elsewhere, and that it is not likely to be discovered without repeating over again the same observations.

Moreover it does not follow that the latitudes of two places are correct, because the declinations of the stars deduced from them correspond; for the deviations caused by local attractions, or from any other source, are made to disappear in correcting the declination, but remain uncorrected in the latitude of each.

Lieut.Col. MUDGE is also of opinion, that the irregularity in the value of his degree may be ascribed to deviation of the plumb-line, occasioned by local attractions. This is certainly very possible, and may be decided by an examination of all circumstances on the spot. But if there be really an error of 1″ in the extent of the whole arc, this should rather be ascribed to some defect in the observations themselves, than to any extraneous source; for the observations of different stars give results that differ more than 4 seconds from each other.

I shall now conclude this Memoir, by expressing a wish, which men of science in England have it more in their power than any others to gratify; I mean by making new measurements in the southern hemisphere. Those which have been made hitherto in the northern hemisphere are extremely satisfactory by their agreement, and give us great reason to presume that the general level of the earth's surface is elliptical, and very regularly so; and hence we might expect the opposite hemisphere to be equally so, and to be a portion of the same curve. Nevertheless the degree measured at the Cape of Good Hope by LACAILLE, in latitude 33° 18′ appears to indicate an ellipse of less eccentricity, or of greater axis; for the linear extent of 57037 toises, corresponds to the measure of a degree in latitude 47° 47′ in the northern hemisphere. If now we calculate the arc as before, with an oblateness of

$\frac{1}{320}$, and with the sides of LACAILLE's triangles reduced to the meridian, we find it greater by 10″ than it was found to be by observations of the stars. An error of 10 seconds, by an astronomer so skilful and scrupulous as LACAILLE, is too extraordinary to be admitted as probable. It is true, that there was a greater error well ascertained to have occurred in the measurement in Lapland, amounting to 13 seconds; but the academicians engaged in this undertaking were by no means equally conversant with observations as LACAILLE.

There remains therefore but one method of removing all doubt on this subject, and this is to repeat and verify the measurement at the Cape, and, if possible, to extend it still farther to the north. The same Major LAMBTON, who has succeeded so well in Asia, and is in possession of such perfect instruments for the purpose, would be singularly qualified for a similar undertaking in Africa, and would furnish us with a measurement in the other hemisphere, as much to be relied upon as the former. He would have the glory of deciding two important questions by his own observations; first, the similarity and magnitude of the two hemispheres; and, secondly, the degree of reliance to be placed on the elliptic hypothesis.

It might be still further desirable, if other measurements could also be undertaken, either in New Holland, or in Brazil; for though neither of these countries differs much in latitude from the Cape of Good Hope, they are so remote in longitude, that a correspondence of measures so taken would nearly establish the similarity of all meridians.

Note.

I shall now explain the formulæ employed in deducing the results to which I have come in the foregoing Memoir. The demonstration of them is to be found in the work of M. DELAMBRE, on the Meridian.

In the first place, let a be the radius of the equator, e the eccentricity, ψ the latitude of one extremity of a side, or arc, in any series of triangles, and θ the azimuth of that side. The radius of curvature of this arc will be expressed by

$$\frac{1}{R1} = \frac{\left(1 + \frac{e^2}{1-e^2} \cdot \cos.^2\psi \cdot \cos.^2\theta\right)}{R} \text{ and } \frac{1}{R} = \frac{(1-e^2 \cdot \sin.^2\psi)^{\frac{3}{2}}}{a}.$$

Hence we see that R is the radius of the arc at right angles to the meridian. One may in general neglect the azimuth, and take the last radius for the radius R1. Now, in computing the arc between Clifton and Dunnose, I have supposed the oblateness to be $\frac{1}{330}$ or $e^2 = \frac{669}{330^2}$, and log. $a = 6{,}5147200$ expressed in toises.

The latitude of the southern extremity of the base is the same as that of Clifton, and its azimuth, if we choose to attend to it, is nearly 335° 23′. This base, considered as an arc of a circle, is reduced to its sine by the formula $\varepsilon = \log. \varepsilon - \frac{K \cdot \varepsilon^2}{6R^2}$, (K being the modules of the table of logarithms, so that log. K = 9,6377843.)

By means of the logarithmic sine of the base, and the angles

of the triangles, considered as spherical, the logarithmic sines of the sides in the series were next computed and then reduced to logarithms of the arcs themselves by the formula $\log. \varepsilon = \log. \sin. \varepsilon + \frac{K . \sin.^2 \varepsilon}{6R^2}$.

For the purpose of making this last reduction, it is sufficient to take a single value of R, corresponding to the mean latitude of the entire arc 52° 2′ 20″. It was thus that the table was formed of logarithmic sides considered as arcs.

Let m be one of these arcs, and let us represent by $\delta\psi$ and $\delta\psi''$ its value reduced to the meridian, the one in toises, the other in seconds of a degree, and we shall have the following formulæ;

$$\delta\psi = m . \cos. \theta - \left(\frac{m^2 . \sin.^2\theta}{2R}\right) . \tan. \psi - \left(\frac{m^2 . \sin.^2\theta}{2R}\right) . \left(\frac{m . \cos. \theta}{3R}\right) . (1 + 3 . \tan.^2\psi)$$

$$\delta\psi'' = \left(\frac{\delta\psi}{R . \sin. 1''}\right) + \left(\frac{\delta\psi}{R . \sin. 1''}\right) . e^2 . (1 + e^2) . \cos.^2\psi . \left\{1 \mp \left(\frac{3 \tan. \psi}{2}\right) . \left(\frac{\delta\psi}{R}\right)\right\}$$: the superior sign being taken when the latitude ψ'' is greater than ψ, and the inferior when it is less.

The correction dependent on the convergence of the meridian for the azimuths is $\delta\theta = \left(\frac{m . \sin. \theta}{R 1 . \sin. 1''}\right) . \left(\frac{\sin.^2(\psi + \psi')}{\cos. \psi' . \cos. \frac{1}{2} \delta\psi''}\right)$.

Hence the azimuth of the first station seen from the second and reckoned westward from the north, is $\theta' = 180° + \theta + \delta\theta$.

If P″ be put for the difference of longitude between two points distant by an arc which measures m, we have $\sin. P'' = \frac{\sin. m . \sin. \theta}{\cos. \psi}$, $\log. \sin. m = \log. \left(\frac{m}{R1}\right) - \frac{K}{6} . \left(\frac{m}{R1}\right)^2$, and $\log. P'' = \log. \left(\frac{\sin. P''}{\sin. 1''}\right) + \frac{6}{K} . (\sin. P'')$.

The arc of the meridian, between Greenwich and Formentera, is so fortunately situated, that its middle point is in latitude 45°. Its whole extent measures 12° 48′ 44″, and the distance between the parallels, in linear measure, was found to be 730430,7 toises. Hence the mean degree, corresponding to the latitude of 45° 4′ 18″, is 57010,5 toises; and if we multiply this number by 90°, we get one-fourth part of the meridian of the earth.

The correction to be deduced for oblateness is 58, 59, or 61 toises, according as it is assumed to be $\frac{1}{330}$, $\frac{1}{320}$, or $\frac{1}{310}$, and if we take the mean of these, we have the fourth part of the meridian $Q = 5130886$ toises; and hence the metre = 44330867 lines; so that the value of the metre turns out to be almost entirely independent of the elliptical form of the earth.

The radius of the equator is derived from the expression $\log. a = \log. \left(\frac{2Q}{\pi}\right) + K . \left(\frac{1}{2} . \varepsilon + \frac{1}{16} . \varepsilon^2 - \frac{1}{48} . \varepsilon^3\right)$, ε being the oblateness, and π the periphery of a circle = 3,1416.

In order to compare any degrees measured with those obtained on the elliptic hypothesis, we have a very simple formula. Let m and m' be the values of two degrees on the meridian, of which the mean latitudes are $\psi 1$ and $\psi 2$; in comparing the analytic expressions for these two degrees, developing them, and then making $\psi = 45°$, we have $m' = m . \left(1 - \frac{1}{2} . p . \cos. 2\psi 2 + g . \cos. 2\psi 2\right)$, $m = 57010,5$ toises, $p = \frac{3}{2} e^2 . \left(1 + \frac{1}{2} e^2\right) . \frac{\sin. 1°}{1° . \sin. 1'}$, and $g = \frac{15}{64} . e^4 . \left(\frac{\sin. 2°}{1° . \sin. 1''}\right)$.
And then we shall find that the oblateness $\frac{1}{320}$ gives 57075,66 and 57192,38 toises for the degrees in England and Lapland.

I shall here subjoin one reflection more, which appears of

importance. The oblateness of the earth is a quantity which varies considerably, by the least difference in the elements on which it depends. Accordingly it is not surprising, that its value fluctuates between two proportions which differ sensibly from each other. To illustrate this, let p be the function which serves to determine the oblateness of the earth, so that $\frac{1}{\epsilon} = p$. When this equation varies $-\delta\epsilon = \epsilon^2 . \delta p$.

Now the coefficient ϵ^2 being very great, we see why the least variation in the elements of the function p, occasions so considerable a variation in the denominator of the oblateness. This is precisely what happens in the lunar equations dependent on the figure of the earth, and which M. LAPLACE has deduced from his beautiful theory. Thus, for example, in the inequality that depends on the longitude of the moon's node, which he has determined analytically with so much precision, the numerical coefficient found by BURG gives $\frac{1}{305}$ for the oblateness; but if this coefficient be diminished by $0'',665$, then the oblateness becomes $\frac{1}{320}$, so that a variation even to this small amount in the coefficient augments the denominator of the oblateness nearly $\frac{1}{20}$ part.

The same happens with regard to the pendulum vibrating seconds; for, supposing its length at 45° to have been correctly ascertained by M M. BIOT and MATHIEU, if we wish to know the length of a second's pendulum at the equator, corresponding to an oblateness of $\frac{1}{320}$, we find it to be 439,1810 lines. Now this length differs from that determined by BOUGUER only by 0,029 of a line, and M. LAPLACE even thinks that the result of BOUGUER should be diminished by about double this quantity. We see from hence how much these little differences, whether produced by errors of observation,

or irregularities in the earth itself, are liable to affect the denominator of the fraction expressing the oblateness.

Fortunately, it seems probable, that the utmost latitude of our present uncertainty is between the limits of 330 and 310, and the mean of these may be considered as a very near approximation to the truth.

XVIII. *An Account of some Experiments on different Combinations of Fluoric Acid. By* John Davy, *Esq. Communicated by Sir* Humphry Davy, *Knt. LL.D. Sec. R.S.*

Read June 11, 1812.

Introduction.

Two years ago, I engaged, at the request of my brother, Sir H. Davy, in an inquiry respecting the nature of common fluoric acid gas. My principal object was to ascertain whether silex is essential to its constitution, and whether the proportion is constantly the same. This subject, and experiments on the fluoric and fluoboracic acids, occupied me for about six months. Since that time, the work of M. M. Gay Lussac and Thenard has appeared, entitled "Recherches Physico-Chemiques," in the second volume of which is an elaborate dissertation on fluoric acid. These philosophers, I find, have anticipated many of my results, and consequently very much abridged my labour of detail in the following pages. To repeat what is already known would be useless, I shall therefore confine myself to describe what I have observed, which appears to me yet novel, or different from the observations of the French chemists. The order which I shall pursue, will be that which I observed in my experiments. I shall divide what I have to advance into four parts. The first part will relate to the silicated fluoric acid gas, and to the subsilicated fluoric acid; the second to the

combinations of these acids, and of pure fluoric acid with ammonia; the third to fluoboracic acid; and the fourth to its ammoniacal salts.

SECT. 1. *On silicated fluoric acid Gas, and subsilicated fluoric Acid.*

The facts which have already been published by M. M. GAY LUSSAC and THENARD and others, appear to me to be sufficient to prove that pure fluoric acid has not yet been obtained in the gaseous state, and that silex, or boracic acid, is requisite that it may assume this form. Were more evidences necessary, I could advance many in point. One circumstance only I shall mention, proving that common fluoric acid gas is perfectly saturated with silex. I have preserved this gas, made by heating, in a glass retort, a mixture of fluor spar and sulphuric acid, for several weeks over mercury in a glass receiver uncoated with wax, without observing the slightest erosion to be produced.*

This gas, with great propriety, has lately been called silicated fluoric. Before I proceed to its analysis, I shall notice what method I have found the best for obtaining it. I have, for a considerable time, long before M. M. GAY LUSSAC and THENARD's work was published, added to the mixture of fluor spar and sulphuric acid, a quantity of finely pounded glass, and have thus procured the gas with the greatest facility. The advantages of this addition are considerable. The retort is saved, which otherwise, in less than one operation, would be

* The sides of the receiver indeed became obscure; but this was not from erosion, but from deposition, as appeared from the transparency and polish of the glass being readily restored by slight friction. What the deposition was, I am ignorant of. After several weeks it was so trifling, as to give only a slight degree of opacity to the receiver.

destroyed; and a much larger quantity of gas is procured from the same materials, and with less trouble and less heat; the action indeed at first is so powerful, that gas begins to come over before the application of heat is made, and a very gentle one only is required to continue its production.

Previous to its analysis, it was necessary to ascertain the specific gravity of the gas. This I have endeavoured to do. The gas, the subject of experiment, was quite pure, being totally condensed by water. A Florence flask was exhausted; in this state, weighed by a very delicate balance, it was

	= 1452.2 grains.
Filled with common air -	= 1452.2 + 10.2
Again exhausted - -	= 1452.2
Filled with silicated fluoric gas	= 1452.2 + 36.45

Hence as 10.2 : 31 : : 36.45 : : 110.78.

Thus it appears, that 100 cubic inches of silicated fluoric acid gas, at ordinary temperature and pressure, are equal to 110.78 grains.

When silicated fluoric acid gas is condensed by water, it is well known that part only of the silex is deposited. To obtain the whole, in order to ascertain the proportion in the gas, I have employed ammonia in excess. 40 cubic inches of the gas (barom. 30, therm. 60) were transferred in portions of 10 cubic inches, at a time to a solution of ammonia. The silex precipitated was carefully collected on a filter, and washed till the water that passed through it, ceased to be affected by nitrat of lime. It was next dried, and strongly heated in a platina crucible. It weighed 27.2 grains, and was pure silex. Supposing fluoric acid to be the remaining 17.1 grains, which added to 27.2 grains are equivalent to the weight of 40 cubic

inches of the gas, it appears that 100 parts by weight of this gas consist of

61.4	silex
38.6	fluoric acid
100.0	

That this estimate may be correct, it is evident, that ammonia should have the property of precipitating the whole of the silex of silicated fluoric gas; which I shall not now endeavour to prove, but leave it to be considered in another part of the paper.

There is no improbability attached to the idea, that silicated fluoric acid gas may, from the manner in which it is prepared, contain a proportion of alkali. To discover whether this was the case, a solution of nitrat of lime was added to the ammoniacal solution neutralized by nitric acid, from which the silex in the preceding experiment had been removed. The precipitate of fluat of lime was separated by filtration. The filtered liquid was evaporated to dryness; and the ammoniacal salt heated in a platina crucible till it was entirely dissipated. The residue had the appearance and taste of quick lime. It was dissolved in acetic acid, and the solution yielded sulphat of lime on the addition of sulphat of ammonia. The liquid was evaporated to dryness, and when the residuum had been heated to dull redness, nothing remained but a little white powder, weighing about a grain, and having all the properties of gypsum. Thus it appears that silicated fluoric acid gas contains no alkali.

My next object was to ascertain the composition of common liquid fluoric acid—that acid obtained by the decomposition of

silicated fluoric acid gas by water, and which, on account of the separation that occurs of part of the silex, may, with greater propriety, be called subsilicated fluoric acid. For this purpose, 43.21 cubic inches, barom. 30.4, therm. 50, or 44 cubic inches at common temperature and pressure, were successively added, two cubic inches at a time, to one cubic inch of distilled water in a small jar over mercury. The whole of this, the gas being pure, was readily condensed. The temperature was somewhat raised. The silex precipitated, formed a gelatinous mass of a blueish colour, which had absorbed all the water like a sponge, so that none appeared fluid. This gelatinous mass was carefully transferred to a filter, and washed with distilled water till it was rendered insipid and incapable of reddening litmus paper. It retained its blueish hue only whilst moist. When dried and ignited, it was in thin lamellæ, and of a snow-white colour, and surprisingly bulky. It weighed 7.33 grains, and was found to be pure silex. Thus it appears that the subsilicated fluoric acid formed by the decomposition of 44 cubic inches of silicated fluoric acid gas contains 7.33 grains of silex less than the gas itself. Consequently independent of water, which no doubt is essential to this acid, 100 parts of it seem to consist of

54.56	silex
45.44	acid
100.00	

I have endeavoured to ascertain what quantity of silicated fluoric acid gas a given quantity of water will condense. In one instance $\frac{19}{100}$ of a cubic inch of distilled water absorbed 51 cubic inches, barom. 30.5, therm. 60. The gas was added to

the water in a jar over mercury, as fast as it was absorbed. The experiment was stopped, when the gas, after having remained in contact with the water a whole night, ceased to be diminished. According to this result, the proper correction being made for the additional pressure, water decomposes about 263 times its bulk of silicated fluoric acid gas.

Dr. PRIESTLEY observed, that muriatic acid gas reproduced silicated fluoric gas from the crust of silex formed, when the latter is condensed by water.* This experiment I have repeated, and as it appears to show more correctly the quantity of gas water can condense, I shall describe the result. 2.4 cubic inches of muriatic gas were added to a drop of water, that had previously absorbed one cubic inch of silicated fluoric gas, in a jar over mercury. There was an immediate absorption equal to $\frac{2}{10}$ of a cubic inch. The mixture of silex and subsilicated fluoric acid effervesced, and from an apparent solid became fluid, the whole of the silex gradually disappearing. After the first mentioned absorption, there was no farther. The gas produced was silicated, as appeared from the crust it deposited when removed to water, and the liquid formed was pure muriatic acid, for decomposed by concentrated sulphuric, it afforded merely muriatic acid gas, without any silicated fluoric. The evident conclusion from the preceding result is, that water condenses equal quantities of the muriatic and silicated fluoric acid gasses, and consequently that the first estimate is too low, and instead of 263 times its bulk, it is probably more correct to say that water to be saturated requires at least 365 times its volume. Neither will this estimate appear inconsistent with the former result, when the

* Vide PRIESTLEY on Air, Vol. II. p. 202.

deposition of silex is considered as an obstacle to the free exposure of the surface of the water to the gas.

Subsilicated fluoric acid is decomposed by ammonia and the fixed alkalies, and by all the earths that I have made trial of. It is also decomposed by the sulphuric acid and the boracic, as well as by the muriatic acid gas.

Of the particular changes which occur when it is acted upon by the alkalies, I defer giving any account at present, as it is my intention to do it in the next section.

To learn the effect of heat on it, a small quantity of strong acid, pure and transparent was introduced into a retort connected with mercury. A spirit lamp being applied about three cubic inches of silicated fluoric acid gas were produced. The neck of the retort was lined with silex in a gelatinous state, and much liquid subsilicated fluoric acid, that had distilled over, was condensed in the colder part of the neck, and was absorbed by bibulous paper previously introduced, to prevent the distilled fluid from entering the jar for the reception of the gas. When the whole of the acid in the bulb of the retort had been evaporated, little or no silex remained.

The general result of this experiment is very different from that which Dr. PRIESTLEY, who first made it, obtained. Instead of silicated fluoric acid gas, he procured "vitriolic acid air," sulphureous acid gas.

I have tried also the effect of heat on the silicious crust, formed by the decomposition of silicated fluoric acid gas, by water; but could obtain no sulphureous acid gas, as Dr. PRIESTLEY did only a small quantity of silicated fluoric.

The correctness of Dr. PRIESTLEY's observations cannot be doubted. I can only account for his results, by supposing that

some sulphuric acid in consequence of the high temperature employed in making the gas was volatilized, and mixed with the subsilicated fluoric acid, and that mercury also was present from the acid being prepared over this metal.

These experiments too oppose another statement relative to a method prescribed for making fluoric acid gas free from silex, by merely heating strong subsilicated fluoric acid in a retort, and collecting the gas over mercury. It is asserted, in chemical works of some reputation, that this process is successful. I have never found it so, having always obtained results similar to those above stated. This, I suppose, is one of the many errors that have secretly crept into repute, and has been believed, because never subjected to the test of experiment.

The action of concentrated sulphuric acid on subsilicated fluoric acid, is similar to that of muriatic acid gas, occasioning a disengagement of silicated fluoric acid gas. Facts which appear to prove, that water is absolutely essential to the existence of this acid.

Boracic acid decomposes it, in a very different way, not from any predominant affinity for the water, but in consequence of a stronger attraction for the fluoric acid itself. Silicated fluoric acid of course is not produced; but liquid fluoboracic acid and the silex is precipitated in a gelatinous state, as when ammonia is employed.

These are the principal facts I have to notice respecting this acid. Before I conclude, I shall briefly mention a few other circumstances. Applied to the tongue, in its concentrated state, it produces a very painful sensation, like that which strong muriatic acid does, and it has a very similar effect on the cuticle. It does not appear to erode glass, for I have kept it in

bottles of this substance more than a month without any action being perceptible. Exposed to the air, it slowly and almost completely evaporates, there being only a very trifling silicious residue; and when gently heated in an open vessel, it is rapidly dissipated in white fumes.

SECT. II. *On the Combinations of silicated fluoric acid Gas, and the subsilicated Fluoric, and the fluoric Acids with Ammonia.*

M. GAY LUSSAC has shewn that silicated fluoric acid gas, like carbonic acid gas, condenses twice its volume of the volatile alkali.* The experiment I have several times repeated, and constantly with the same result, no difference appearing when the acid gas was added in great excess to the alkaline, or the alkaline to the acid. This being the case, and knowing the specific gravities of the two gasses,† 100 parts by weight of silicated fluat of ammonia seem to consist of

24.5	ammonia
75.5	acid
100.0	

Silicated fluat of ammonia volatilizes unaltered, if heated by a spirit-lamp in the vessel in which it is formed, and provided moisture be entirely excluded.

Like silicated fluoric acid gas itself, this salt is decomposed by water, and a similar precipitation of silex occurs, and in the same proportion. Thus the salt formed by the union of 30

* Vide Mém. d'Arcueil, Tom. II.

† According to Sir H. DAVY, 100 cubic inches of ammonia, barom. 30, therm. 60, weigh 18 grains. It is this estimate which I have taken.

cubic inches of silicated fluoric gas, and 60 of volatile alkali (barom. 30, therm. 60) in a small glass jar over mercury, being carefully collected and introduced into water, afforded five grains of pure silex, weighed after being well washed and heated to redness.

The saline solution, since part of the silex of the silicated fluoric acid gas is separated during its production, appears to be a subsilicated fluat, or a combination of subsilicated fluoric acid and ammonia. Another mode of making it, more directly proves that this is its composition. When ammonia is added to the subsilicated fluoric acid in excess, this salt is formed without any precipitation. From these facts, it may be concluded, that independent of water, which appears to be essential to its existence, 100 parts of it consist of

28.34	ammonia
71.66	acid
100.00	

Subsilicated fluat of ammonia has a pungent saline taste. It just perceptibly reddens litmus paper. Slowly evaporated, it forms small transparent and brilliant crystals. The largest I could obtain, appeared to be tetrahedral prisms. The solid salt is very soluble in water; but is not deliquescent. When heated it appears to sublime unaltered. It is curious that the solution of this salt, when evaporated by a heat near its boiling point, powerfully erodes the glass or porcelain vessel, and a residuum of silex appears, on the addition of water, to redissolve the salt. This erosion and residue of silex I have seen produced three times following, with the same quantity of salt. I mention the fact, which, I believe, was before observed by SCHEELE, without

attempting an explanation of it. It may perhaps be said, that as the water evaporates, the affinity of the subsilicated fluat for silex increases.

Subsilicated fluat of ammonia is decomposed by the sulphuric acid, and by muriatic acid gas, and also by the fixed alkalies and by ammonia.

Sulphuric acid expels from it, silicated fluoric gas and hydrated fluoric acid fumes.

Muriatic acid gas acts slowly on it, and effects its decomposition apparently through the medium of its water. A little of the crystalline salt was introduced into muriatic acid gas in a jar over mercury. In a short time some silicated gas was produced, as the silicious deposition, on the addition of water, indicated. Strong muriatic acid was substituted for the acid gas. Now no apparent change took place, for on evaporating the acid, the residue, decomposed by sulphuric acid, afforded only silicated fluoric acid gas.

The alkalies form by the decomposition of this salt, the same compounds that they do by their action on subsilicated fluoric acid.

Potash expels the ammonia, and produces the silicated fluat and fluat of potash, as M. M. GAY LUSSAC and THENARD have described.

The changes occasioned by soda appeared to me similar; but the gentlemen just mentioned, assert that this alkali precipitates the whole of the silex, and does not form a triple salt with it and part of the acid.

Ammonia seems to me to separate completely the silex, and by uniting with the pure acid to constitute a true fluat. MM. GAY LUSSAC and THENARD are of a different opinion. They

say that the whole of the silex cannot by this method be removed, but only the principal part. Their reason for this belief, is, that on repeatedly evaporating the salt after the addition of ammonia and redissolving it, they have each time observed a residue of silex. If they employed metallic evaporating vessels, the results of my experiments do not agree with theirs; for making use of platina for this purpose, and adding an excess of ammonia, I never detected traces of silex on evaporating the filtered fluat. But our results agree, if they employed glass or porcelain vessels, which fluat of ammonia has the property of corroding.

I now proceed to the consideration of fluat of ammonia; but before I describe some of the properties of this fluat which I have observed, I shall briefly mention the means pursued for ascertaining the proportions of its constituent parts.

The composition of subsilicated fluat of ammonia being known, that of the fluat (granting what is already advanced respecting its formation to be correct) may be inferred from the proportion of silex, that a given quantity of ammonia will precipitate. 18 cubic inches of ammoniacal gas were condensed by $\frac{1}{4}$ of a cubic inch of distilled water in a small glass tube over clean mercury. This ammoniacal solution was added to a clear filtered solution of subsilicated fluat of ammonia. A precipitate of silex was immediately produced. After several hours standing, this precipitate was collected on a filter, well washed and dried and heated to redness. It was pure silex, and weighed 1.6 grains. This experiment, like all the preceding, was repeated, and the result confirmed. In both instances there was an excess of subsilicated fluat. The precipitations were made in a platina vessel, and the solutions were neither heated before or after

the separation of the silex. Calculating from this result, 100 parts of fluat of ammonia seem to consist of

76.4	ammonia
23.6	fluoric acid
100.0	

Water appears to be a constituent part of this salt.

It may be rendered neutral by means of a gentle heat, which expels the excess of ammonia employed in its formation. In its neutral state, it has a strong saline taste, and it readily deliquesces when exposed to the atmosphere. Like the neutral carbonats, it is decomposed by heat; but there is this difference between them, part of the pure alkali is expelled instead of the acid, and an acid fluat of ammonia is formed. A gentle heat only is required for the purpose, that of boiling water is nearly sufficient. When the heat is much stronger, the salt fuses and passes off in dense fumes of a most peculiar suffocating odour. The effects of these fumes, when inhaled, are very powerful and disagreeable, and even dangerous, I might venture to say, were I to speak from my own experience. In one instance, when I inhaled only a small quantity, they produced in a few minutes a violent cough and catarrh, and apparent accumulation of blood in the neck and head, and symptoms altogether not unlike those the attendants of apoplexy, which continued for about a quarter of an hour, and then slowly diminished, and gradually disappeared without leaving any permanent bad effect. The fluat of ammonia, when heated in a metallic vessel, appears to sublime unaltered. But the result is different when the experiment is made in a glass one. Ammonia is expelled, the glass is corroded, and subsilicated fluat

of ammonia is formed and sublimed. Its action on glass is so powerful, that I have successfully employed it instead of fluoric acid itself, for etching on this substance. It has one advantage, that it is more manageable. The solution may be applied by means of a hair pencil or a common pen to the glass, and the erosion will be produced by exposure to a moderate temperature.

The fixed alkalies, and all the earths that I have tried, decompose this salt; they expel the ammonia, and form true fluats with the acid itself. I have examined all the fluats thus formed, and have endeavoured to ascertain the proportions of their constituent parts; but I am not sufficiently satisfied of the accuracy of the results, to venture to give an account of them.

SECT. III. *On Fluoboracic Acid Gas.*

MM. GAY LUSSAC and THENARD, who first discovered this gas, obtained it by heating strongly, in an iron tube, a mixture of fluor spar and fused boracic acid. I have found that it may be more easily procured, in greater abundance, and at less expence, by gently heating, in a common glass retort, a mixture of finely pounded boracic acid * and fluor spar with concentrated sulphuric acid. 1 part by weight of fused boracic acid, 2 parts of fluor spar, and about 12 of sulphuric acid appear to be the proportions best adapted for the purpose. This method will require no explanation when it is considered that boracic acid, as has already been observed, precipitates silex from liquid subsilicated fluoric acid. If the heat is gentle, not

* Common calcined borax answers the same end, but no so well. Its only recommendation to preference is cheapness.

nearly sufficient to occasion the ebullition of the sulphuric acid, and the proportions just recommended are used, the retort will not be injured, and pure fluoboracic acid gas will be produced in abundance. When the gas ceases to come over, if the heat is raised, more will be evolved, and there will be distilled over at the same time, a viscid fluid, which is a compound of sulphuric acid and fluoboracic acid gas. Now the operation should be stopped, if the object is to obtain merely pure fluoboracic gas, a long continuation of the heat producing some silicated fluoric. Before quitting the subject, it should be observed that the quantity of sulphuric acid employed is of considerabl consequence to the success of the experiment. If too much is used, there is a great loss of gas from the property which sulphuric acid has of absorbing fluoboracic acid gas; and if too little is employed, it soon becomes diluted, and loses the power of generating the gas, though it may still decompose the fluor spar. Both extremes, therefore, are to be avoided, and the proportion of acid mentioned above, as far as my experience goes, appears to be the best.

I have endeavoured to ascertain the specific gravity of fluoboracic gas.

The flask exhausted weighed	1400.5 grains.
Filled with common air -	1400.5 + 6.2
Again exhausted - -	1400.5
Filled with pure acid gas -	1400.5 + 14.7.

Thus it appears that 100 cubic inches of fluoboracic gas are equal to 73.5 grains.

M M. GAY LUSSAC and THENARD have described the compound of this gas and water, a fuming fluid, in many respects similar to concentrated sulphuric acid. Like this acid, I have

observed that it possesses a slight degree of tenacity, so that it has an oily appearance when poured from one vessel to another; and similar in another respect, it possesses the property of charring animal and vegetable substances, and which the French chemists observed belonging to the gas itself. I have found that water condenses more of this, than it does of any other known gas, no less than 700 times its volume. The experiment was then made, barom. 30.5, therm. 50, $\frac{14}{100}$ of a cubic inch of water were introduced into a tube over mercury, and the gas, in portions of 5 cubic inches at a time, was added until 100 cubic inches had been absorbed, when the water was apparently saturated. This acid was of the specific gravity 1.77.

The property which sulphuric acid has of absorbing fluoboracic acid gas has already been noticed. I found that $\frac{1}{2}$ cubic inch of sulphuric acid, of the specific gravity 1.85, condensed 25 cubic inches of the gas, or 50 times its volume. The compound acid was strongly fuming, and appeared more tenacious than pure sulphuric acid, yet not nearly so much so as that compound of the two which distills over during the latter part of the operation of making fluoboracic gas.

This latter compound has some peculiarities. It is so tenacious, that it flows very slowly. It appears to be far more volatile than pure sulphuric acid. When poured into water, a dense white precipitate is formed, the exact nature of which I have not yet satisfactorily ascertained; but which is not produced by the direct compound of sulphuric acid, and the fluoboracic.

SECT. IV. *On the Combinations of fluoboracic acid Gas, and ammoniacal Gas.*

M. GAY LUSSAC has combined fluoboracic acid gas with ammonia. He states, that equal volumes of the two gasses condense each other.* This I have found to be the case, and I have also found that fluoboracic acid gas condenses twice, and even three times its volume of the volatile alkali. The compound observed by M. GAY LUSSAC is solid, white, and opaque, like the ammoniacal salts. The combinations I have obtained are liquid, transparent, and colourless, like water, though they are entirely free from this fluid. They were made by the direct union of the two gasses. 5 cubic inches of ammoniacal gas were added to the same volume of the fluoboracic gas contained in a small jar over dry mercury. There was a complete condensation of both, and the solid salt was the result. 5 cubic inches more of ammonia were introduced. The whole was quickly absorbed, and the solid salt was converted into the transparent fluid. 5 cubic inches more were added, which too were slowly absorbed, but without any change of form.

The solid salt volatilizes in close vessels unaltered, on the application of a gentle heat.

Both fluid compounds, when heated, are rendered solid, from the expulsion of part of the ammonia. Exposure to the air is attended with the same change, and the same effect is produced by the muriatic and carbonic acid gasses.

Knowing the volumes of the acid, and alkaline gasses which

* Vide Mém. d'Arcueil, Tom. II.

combine, it is easy to calculate the proportions of each by weight in the respective salts.

100 Parts consist of	Ammonia.	Acid.
The solid compound	19.64	80.32
The first fluid -	32.9	67.1
The second fluid	42.4	57.6

These combinations are curious in many points of view. They are the first salts that have been observed liquid, at the common temperature of the atmosphere, without containing water. And they are additional facts in support of the doctrine of definite proportions, and of the relation of volumes.

XIX. *On a Periscopic Camera Obscura and Microscope.* By William Hyde Wollaston, *M. D. Sec. R. S.*

Read June 11, 1812.

ALTHOUGH the views, which I originally had of the advantage to be derived from the periscopic construction of spectacles,* naturally suggested to me a corresponding improvement in the *camera obscura*, by substituting a meniscus for the double convex lens, I have hitherto deferred making it known to others, except as a subject of occasional conversation.

Since in vision with spectacles, as in common vision, the pencil of rays received by the eye in each direction is small, the superiority of that form of glass, which disposes all parts of it most nearly at right angles with the visual ray, admits of distinct demonstration; but with respect to the camera obscura, where the portion of lens requisite for sufficient illumination, is of considerable magnitude, although it is evident that some improvement may be made in the distinctness of oblique images on the same principles, yet as the focus of oblique rays is far from being a definite point, the degree in which it may be improved is not a fit subject of mathematical investigation.

I have therefore had recourse to experiments, in order to determine by what construction the field of distinct representation may be most extended; and, I trust, the result will be acceptable to this Society. I shall take the same opportunity

* Phil. Magaz. Vol. XVII. Nicholson's Journal, VII. 143.

to describe an improvement in the construction of the simple microscope, which may also be termed periscopic, as the object of it is to gain an extension of the field of view, upon the same principles as in the preceding instances, namely, by occasioning all pencils to pass as nearly as may be at right angles to the surfaces of the lens. The mode, however, in which this is effected is apparently somewhat different in the practical execution.

In the common *camera obscura*, where the images of distant objects are formed on a plane surface to which the lens is parallel, if the surfaces of the lens be both convex, and equally curved (as in fig. 1); and if the distance of the lens be such, that the images formed in the direction of its axis CF be most distinct, then the images of lateral objects are indistinct in a greater or less degree, accordingly as they are more or less remote from the axis. The causes of this indistinctness may be considered as twofold; for in the first place, all parts of the plane, excepting the central point, are at a greater distance from the centre of the lens than its principal focus; and secondly, the point *f*, to which any pencil of parallel rays passing obliquely through the lens are made to converge, is less distant than the principal focus. On this account, it is in general best to place the lens at a distance somewhat less than that which would give most distinctness to the central images, because in that case a certain moderate extension is given to the field of view, from an adjustment better adapted to lateral objects, without materially impairing the brightness of those in the centre. The want of distinctness, however, is even then only diminished in degree, but is not remedied.

The construction, by which I propose to obviate this defect,

is represented in the second figure, in which are seen the essential parts of a periscopic camera in their due proportion to each other. The lens is a meniscus, with the curvatures of its surfaces about in the proportion of two to one, so placed that its concavity is presented to the objects, and its convexity toward the plane on which the images are formed. The aperture of the lens is four inches, its focus about twenty-two. There is also a circular opening, two inches in diameter, placed at about one-eighth of the focal length of the lens from its concave side, as the means of determining the quantity and direction of rays that are to be transmitted.

The advantage of this construction over the common camera obscura is such, that no one who makes the comparison, can doubt of its superiority; but the causes of this may require some explanation. It has been already observed, that by the common lens, any oblique pencil of rays is brought to a focus at a distance less than that of the principal focus. But in the construction above described, the focal distance of oblique pencils is not merely as great, but is greater than that of a direct pencil. For since the effect of the first surface is to occasion divergence of parallel rays, and thereby to elongate the focus ultimately produced by the second surface, and since the degree of that divergence is increased by obliquity of incidence, the focal length resulting from the combined action of both surfaces will be greater than in the centre, if the incidence on the second surface be not so oblique as to increase the convergence. On this account, the opening E is placed so much nearer to the lens than the centre of its second surface, that oblique rays E*f*, after being refracted at the first surface, are transmitted through the lens nearly in the direction of its

shorter radius; and hence are made to converge to a point so distant that the image (at *f*) falls very nearly in the same plane with that of an object centrally placed.

In the use of spectacles by long-sighted persons, the course of the rays in the opposite direction is so precisely similar, that the same figure might serve to illustrate the advantages of the periscopic construction. For the purpose of seeing the extended page of a book (as at AB) with least fatigue to the eye, that form of lens will be most beneficial, which renders the rays received from each part of its surface parallel; and this is effected by the exact counterpart to the preceding arrangement; for in this case the opening E represents the place of the eye receiving parallel rays from the lens in each direction, instead of transmitting them from a distance towards it.

There is, however, this difference between the two cases, that in the camera obscura a much larger portion of the lens is required to conspire in giving a distinct image of any one object; so that the conformation best adapted for lateral objects, would not be consistent with distinctness at the centre; and hence arises a limit to the application of the principle. On the common construction, the whole lens is so formed as to give brilliancy and distinctness at the centre alone, without regard to lateral objects. In adopting such a deviation from the customary form, as I propose, in favour of a more extended view, some diminution of the aperture is required in order to preserve the desired distinctness at the centre. In my endeavours to ascertain the most eligible form of meniscus for this purpose, I have assumed sixty degrees to be the field of view required. But when so large a field is not wanted,

then a lens that is less curved will be preferable; and the proportion of the radii must be varied according to the angular extent intended to be included.

For the purpose of estimating by what combination of radii any required focal length may be given to a meniscus, I have contrived a diagram by which very much labour of computation may be saved, as a very near result may be obtained by mere inspection. This contrivance is founded on the well known formula for the focal length of any lens $F = \frac{mrR}{R \pm r}$: m being a certain multiple obtained by dividing the sine of refraction by the difference of the sines of incidence and refraction. Hence, in applying this formula to the meniscus, $F : R :: mr : R - r$. In fig. 3, lines expressive of these quantities are so arranged, that by assuming any point F corresponding to the focal length desired, and drawing a line FR through a point R indicating any supposed length of the greater radius, the corresponding length of the other radius will be found where the line drawn intersects the middle line in the diagram.

In laying down these lines, the length and position of AF and AR were assumed at pleasure; and they were divided into any number of equal parts. But the position and length of the middle line Ax was adapted with care to the refractive power of plate glass in the following manner. Since $m = \frac{1}{1,505 - 1} = 1,98$, a line BC was drawn from the point 10 in the line AR, parallel to AF, and equal to 19,8 divisions of the primary lines; so that if r be $= 10$, then the line BC $= mr$. The distance AC being then divided into ten equal parts, with their subdivisions, afforded the means of continuing the

same scale to any desired length. Since the first line BC was laid down parallel to AF, and equal to *mr*, any other lines drawn through corresponding numbers 7 and 7, 8 and 8, &c. will be also parallel, and by preserving due proportion, will correctly represent *mr*. Hence in all positions of the line FR, the same similarity of triangles obtains, and the same proportion of F : R : : mr : R $-$ r; and consequently the focal length, corresponding to any assumed radii, is truly ascertained.

For the purpose of duly proportioning the curvatures of flint-glass, a second line A*y* might be laid down in a mode similar to the preceding, by adapting the multiple $m = \frac{1}{1,58-1} = \frac{19}{11}$ to the different density of this glass.

With respect to the construction of a microscope on periscopic principles, I believe the contrivance to be equally new with the former, and equally advantageous. The great desideratum in employing high magnifiers is sufficiency of light; and it is accordingly expedient to make the aperture of the little lens, as large as is consistent with distinct vision. But if the object to be viewed, is of such magnitude as to appear under an angle of several degrees on each side of the centre, the requisite distinctness cannot be given to the whole surface by a common lens, in consequence of the confusion occasioned by oblique incidence of the lateral rays, excepting by means of a very small aperture, and proportionable diminution of light.

In order to remedy this inconvenience, I conceived that the perforated metal, which limits the aperture of the lens, might be placed with advantage in its centre; and accordingly I procured two plano-convex lenses ground to the same radius,

and applying their plane surfaces on opposite sides of the same aperture in a thin piece of metal (as is represented by a section, fig. 4), I produced the desired effect; having virtually a double convex lens so contrived, that the passage of oblique pencils was at right angles with its surfaces, as well as the central pencil. With a lens so constructed, the perforation that appeared to give the most perfect distinctness was about one-fifth part of the focal length in diameter; and when such an aperture is well centered, the visible field is at least as much as twenty degrees in diameter. It is true, that a portion of light is lost by doubling the number of surfaces; but this is more than compensated by the greater aperture, which, under these circumstances, is compatible with distinct vision.

Beside the foregoing instances of the adaptation of periscopic principles, I should not omit to notice their application to the camera lucida; as there is one variety in its form, that was not noticed in the description which I originally gave of that instrument.*

In drawing, by means of the camera lucida, distant objects are seen by rays twice reflected (*d*, fig. 5), at the same time and in the same direction that rays (*e*) are received from the paper and pencil by the naked eye. The two reflections are effected in the interior of a four-sided glass prism, at two posterior surfaces inclined to each other at an angle of 135 degrees. In the construction formerly described, the two other surfaces of the prism are both plane, through which the rays are simply transmitted at their entrance and exit. But since an eye that is adjusted for seeing the paper and pencil, which are at a short distance, cannot see more distant objects dis-

* Nicholson's Journal, XVII. p. 1. Phil. Magaz. XXVII. p. 343.

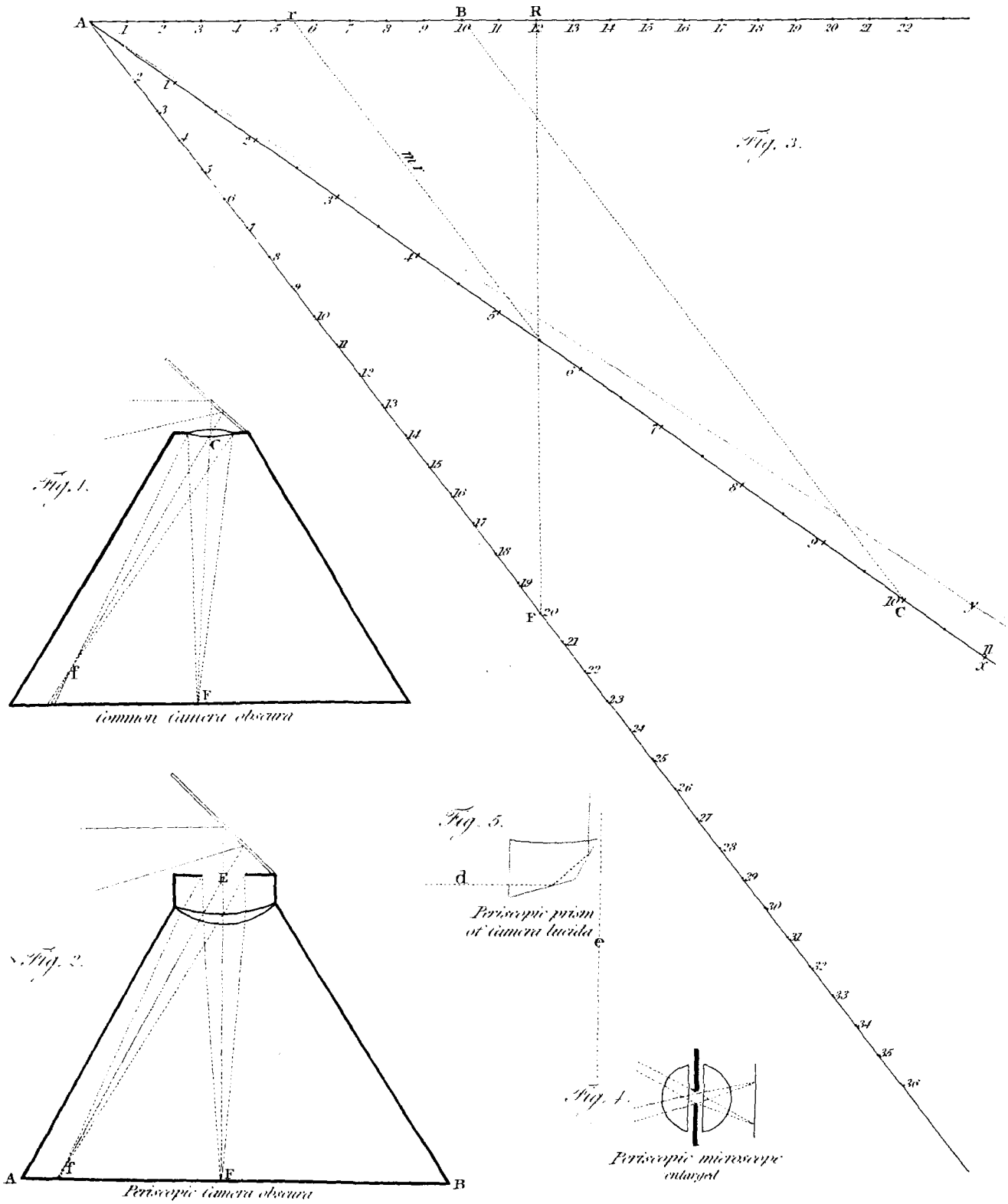
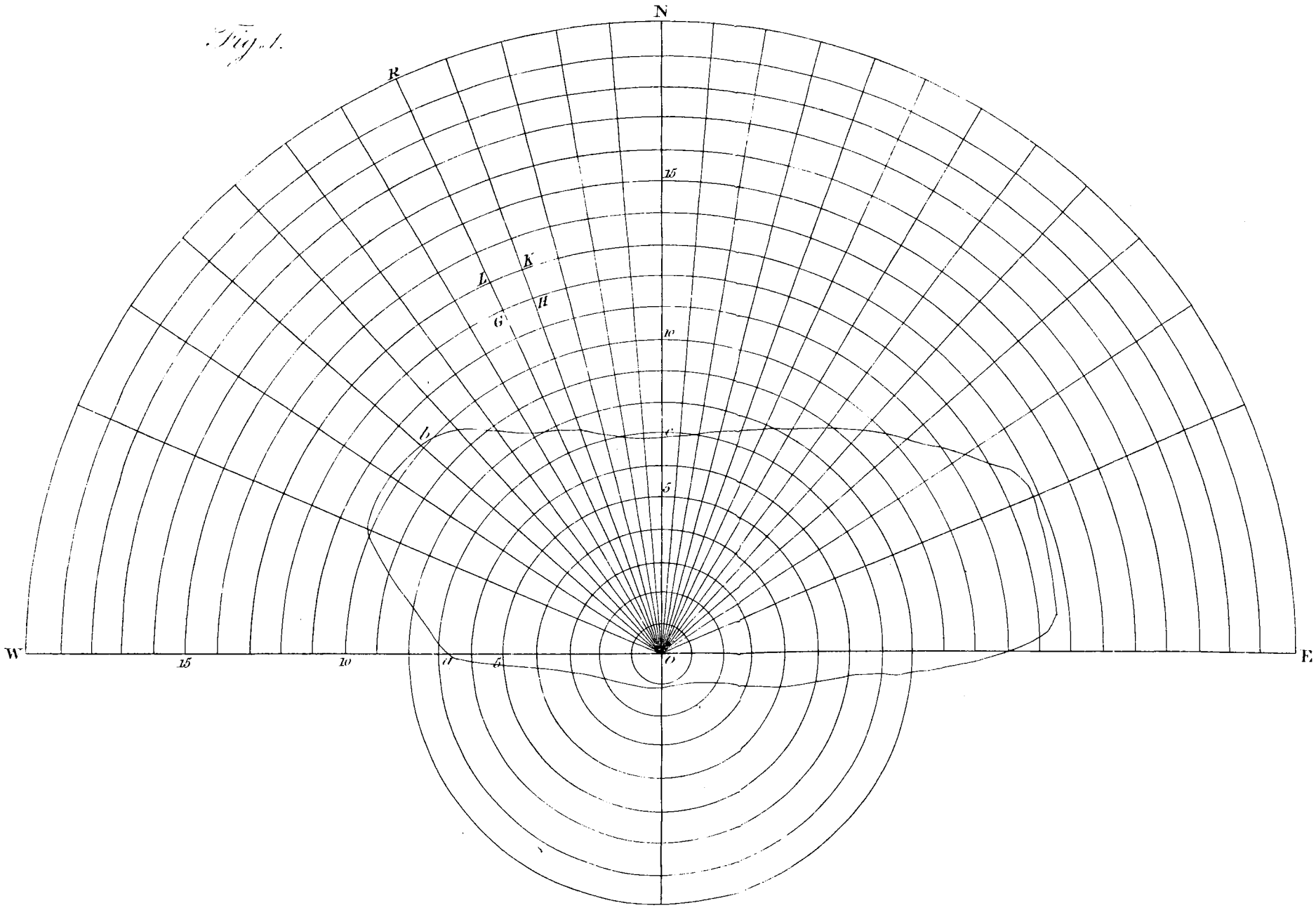


Fig. 3.
Fig. 1.
common camera obscura
Fig. 2.
Periscopic camera obscura
Fig. 5.
Periscopic prism or camera lucida
Fig. 4.
Periscopic microscope enlarged

J. Basire sc.

Fig. 1.



J. Basire sc.

tinctly without the use of a concave glass, it may be assisted in that respect by a due degree of concavity given to either, or to both the transmitting surfaces of the prism. It is, however, to the upper surface alone that this concavity is given; for since the eye is then situated on the side toward the centre of curvature, it receives all the benefit that is proposed from the periscopic principles.

XIX. *Further Experiments and Observations on the influence of the Brain on the generation of Animal Heat. By* B. C. Brodie, *Esq. F. R. S. Communicated to the Society for promoting the knowledge of Animal Chemistry, and by them to the Royal Society.*

X. *An Account of Operations carried on for ascertaining the Difference of Level between the River Thames at London Bridge and the Sea; and also for determining the Height above the Level of the Sea, &c. of intermediate Points passed over between Sheerness and London Bridge.* By John Augustus Lloyd, *Esq. F.R.S. F.R.G.S. & F.S.A.*

Read March 3, 1831.

In February 1830, at the suggestion of the Royal Society, I had the honour to receive directions from the Lords Commissioners of the Admiralty to make such observations as I might consider necessary, to ascertain the difference, if any, between the level of the waters at certain points on the river Thames, and the mean level of the sea near Sheerness, as well as the height of different intermediate points above the sea, such as Gravesend, Greenwich Observatory, &c.

Having found, while employed in the Isthmus of Darien, how inadequate the present levelling instruments were to obtain very accurate results, and being desirous of conducting the interesting observations, I now had orders to make, with the most scrupulous exactness, I thought it necessary, in the first instance, to bestow some attention to the improvement of the instruments required to be used, endeavouring to combine superior steadiness and motion in azimuth, more delicacy in the level itself, more permanency in its position, and greater power in the telescope.

After several trials, and by the assistance of Mr. Cary, (to whom I am indebted for many valuable suggestions,) I determined upon having an instrument made exactly after the accompanying Plate.

Fig. 1. is a perspective view of the instrument. It is supported by three foot-screws, similar to those used in the best altitude and azimuth circles, with thirty threads to an inch, and serving to place the instrument horizontal.

The stand is formed by two plates of brass, which are firmly connected together by three pillars. To the limb that carries the telescope, in very sub-

stantial Ys, is fixed the inner conical centre, three inches in diameter and eight in length, ground to move perfectly smooth in the hollow conical centre fixed to the stand.

This limb projects over the upper part of the frame about one inch, and is bevelled at the edge, in order to slide in a groove attached in two pieces to a clamp, screwed underneath the upper part of the frame, and which, by means of a tangent screw, gives a slow motion in azimuth to the limb. This limb, having two solid projections, allows the Ys being at some distance over the periphery of the circle, by which means the supports to the telescope are nearer its extremities. One of these Ys has a short vertical motion, with a pushing screw at the side, which fixes it at any height required. The telescope itself is of thirty inches focal length, and magnifies about twenty-two times, having two thick gun-metal collars, by which the telescope rests on and is turned in the Ys. These were quickly worn by the continued friction from the supporting part of the Ys, and would have caused an error in using the instrument; I therefore had small thick steel plates, highly polished and hardened, dovetailed into each supporting part of the Ys. The friction on the collars was now transferred to the pieces of steel; and although they likewise continually wear, it does not affect the correctness of the instrument in any other way, than in altering the adjustment of one of the Ys. (See page 20.)

At the eye end of the telescope is an adjustment with a rack and pinion, for distinct vision, and another by the same means to regulate the distance of the eye from the wires: this I found to be indispensable, as the eye occasionally becomes fatigued, and requires a different focus to view the wires distinctly.

The wires themselves are adjusted as usual, by a sliding piece for azimuth, and two pushing screws for vertical motion.

In the centre of the telescope there are two orifices opposite one another; the one to receive a small lamp, and the other admitting a spindle and speculum at the end, ground to an angle of 45°, which, by reflection, illuminates the wires for night observation. The speculum being made to turn, the quantity of reflected light may be regulated at pleasure.

As the glass would not admit of distinct vision for objects at a less distance than one hundred feet, and it being necessary to use the instrument, at times,

not more than three feet distance from the station-staff, several lenses were made of different focal distances, as sixty, thirty, fifteen, eight feet, and thirty inches; which, being applied to the object end of the telescope, converts it, in fact, to a microscope.

To the lower part of the telescope, within the collars, are affixed the cocks, into which fits the tube protecting the level: one of these cocks gives a vertical, and the other a horizontal motion to the tube, in order to place the level parallel to the axis of the telescope.

When the instrument was first made, these motions were effected by means of endless screws; but I found it so difficult (almost impossible) to keep a delicate level in adjustment by this mode, that I substituted the old fashion of capstan-headed pushing screws.

To the upper part of the telescope are attached (outside either points of support) cocks* or braces, carrying a swinging level, having, as well as the cocks, separate adjustments.

This additional level was intended as a check to the lower level, and to detect any occasional variation in the figure of the tube itself.

The glass bubbles themselves were placed in the tubes at first with paper, and wedged at either end with small pieces of wood; but the wedges are liable to distort the bubble itself, and after some time get loose in the tube; and the level alters in its position, and is never to be depended on. I found it better to push the level into its proper place in the tube, not tightly, and with paper underneath, taking care that the paper touches the middle part, and then filling up the two ends with plaster of Paris.

To the upper part of the telescope, near the eye-piece, is fixed a small level, adjusted to the horizontal wire of the telescope, and by the assistance of which the same surface of the large level is used at each observation. There is also another small level at the other end of the telescope, to adjust the vertical wire by, but of less use than the former.

On the upper limb, near one of the Ys, is fixed a small thermometer, with the bubble inclining downwards, at an angle of about 10°, the use of which will be explained hereafter.

The large stand of this instrument is made with a solid top of African oak.

* These cocks are not shown in the Plate.

The legs, which are very strong, are iron-shod, and braced at the bottom, about six inches from the shoeing, with three thick iron rods, rendering the whole steady, and affording the men a purchase to press down with their feet, and rough level the stand ready for the reception of the instrument.

Adjustments.

The above instrument requires several adjustments, which I shall endeavour to describe in the order they are made.

First, To make the lower level parallel to the axis of the telescope :

Place the telescope directly over one of the foot-screws, and clamp it; then bring the bubble of the level, by means of the foot-screw, to the same division on either side the graduated scale affixed to and over the level, taking care that the bubble of the little level at the eye end of the tube (and at right angles to the large level) is in the centre; then reverse the telescope in its collars, observing if the bubble reaches to the same division, and correct one half of that number by the pushing screws on the level itself, and the other half by the foot-screw: this must be repeated until the bubble remains in the same spot.

Second, To place the large level in the same vertical as the axis of the telescope:

Move the telescope in its collars until the level is brought considerably to one side, and observe if the bubble still remains at the same division; if not, move the side pushing screws on the level, until the bubble has returned to its proper place; move the telescope again as much to the other side, and observe if the bubble comes to the same division; if not, it must be re-adjusted, until it is as near as the accuracy of the grinding of the level will allow.

These two adjustments are naturally dependent on one another, therefore they must be both examined, until no alteration in the bubble can be perceived.

Note.—As the collars may wear a little hollow in time, care must be taken that one particular shoulder of the two collars rests against the Y when reversed, in order to use the same point of support.

Third, To place the axis of the telescope parallel to the plane of the instrument :

Loosen now the clamp which confines the telescope over the foot-screw, and adjust the bubble exactly to the centre; bring the telescope and limb half round on its conical centre, and observe the bubble; half the number of divisions, in error on the scale, must be corrected by the foot-screw, and the other half by the vertical motion to the Y, securing it, when it is sufficiently adjusted, by the little side screw.

Fourth, To place the vertical and horizontal wires at right angles, and to connect them with the little levels on the telescope:

Place one of the station-staves about five feet from the ground in a horizontal position (by means of a small hand level and two nails); from the centre thereof, suspend a white plumb-line; adjust the vertical wire to the horizontal one, by loosening the two screws, which admit of its moving diagonally, and making them coincide with the surface of the staff and the plumb-line; then by loosening the screws of the small level at the eye end, move it until the bubble rests in the centre, reverse the position of the wires, making the vertical horizontal, and adjust in the same manner the other little level.

Fifth, To make the hanging level parallel to the plane of the instrument:

Adjust by the foot-screw, until the bubble of the lower level is in its position; then observe the variation of the riding level, and alter it one half the error by the vertical screw on the cock, and the other half by the pushing screws on the riding level itself.

Or, By the foot-screw, bring the bubble of the hanging level in the centre; then reverse it on the cocks; one half the difference is to be altered by means of the adjustment on the level itself, and one half by the foot-screw: now place the lower level perfectly horizontal, and by the vertical screw on the cock, bring the bubble of the hanging level to correspond.

The usual adjustment for collimation is here purposely omitted. The eye tube of the telescope altering continually in its position, renders it most difficult to make this adjustment correctly; for although it may be found in perfect adjustment for a distant object, when directed to a near one there will be a considerable error, which would affect the results in levelling, if the station-staves were not equidistant from the instrument.

To avoid this inconvenience therefore, as well as to avert the difficulty of placing the extra lenses for short distances, so as not to alter the line of colli-

mation, I permitted the wires to be some distance from the axis of the telescope; and, in levelling, a mean of four or six observations were taken, with the telescope turned half round in its collars at each observation.

Note.—I take this opportunity of mentioning a substitute I have occasionally used for wire or cobweb with success, viz. asbestus; very fine fibres of which can be obtained by being thrown into hot water, when it easily divides. These fibres are tough enough to be placed with ease on the diaphragm, and have the advantage of being opaque.

Being now in possession of an instrument equal to perform the most delicate observations, my next object was to make some improvement in the station-staves, so that they might point out as minute a quantity as the instrument could detect a difference of. This was something difficult, without encroaching on the portability and quick application of the staff to its use.—The following is a description of the staves I used, a Plate of which is given.

The staff itself is a rod of six feet six inches in length, of solid seasoned mahogany on the face of which is let-in a slip of brass, riveted at intervals from end to end; by the side of this is also a slip of holly, fixed in the same manner. The divisions are laid down in feet and tenths, on the holly; and in feet, tenths and hundredths, on the brass. The lower part of the staff is fitted into a square tube of brass about eight inches long, four inches of which are occupied by the staff, and the remainder filled up with lead.

The vane is a plane of seasoned holly, with two semicircles of stained ivory let into the face of it (see Plate). It is fixed by screws to a brass box with tightening springs on the staff itself: there is another small slide on the staff, having two clamping screws, and a long tangent screw, which is attached to the box of the vane by a female screw; the last gives a slow vertical motion to the vane.

On the top of this vane, and at right angles to one another, are two small spirit-levels, mounted in brass; affixed to the vane, in a square hole in the centre, and levelled on one side, is a small brass vernier, the edge of which slides on the divisions of the staff reading to the 1000dth, and, by practice, to the 10,000dth of a foot.

The station-staff thus described is in itself complete; but for accurate observation it requires to be immoveable on the picket: a three-legged support

is therefore added, having a box and ring with double compass gimbles, and a horizontal motion; into this the staff slides.

There is also a small brass tripod with iron legs, having a hole in the centre plate, over which another small plate slides, fixing to the tripod by two clamping screws; this is used to confine the bottom of the station-staff over the picket.

Adjustment.

The only adjustment required to this instrument, is to enable it to be placed vertically over the picket.

Slide the staff into the piece carrying the gimbles; suspend it with the gimbles as nearly as possible at right angles, and as distant as conveniently may be from the ground. When the staff does not oscillate, observe if the two bubbles on the vane are correct; if not, by means of the small screws at either end raise or depress them, until the bubble remains in the centre.

Having now the means of placing the staff immoveably and vertically over a spot, it is to be accomplished as follows: Fix the support that carries the staff, as nearly as possible over the required spot; pushing the legs into the ground, place the small tripod as shown in the Plate, unclamp the milled heads, and pass the staff through the gimbles of the support into the plate of the tripod. As this plate will move in any direction horizontally, the staff is to be adjusted, until perfectly upright, by means of the levels on the vane; then clamp the plate firmly, and push the staff down to the head of the picket, and turn it until it is directly in front of the level.

In March 1830, having every thing prepared, I departed for Sheerness, determining to commence my observations at that point.

As part of the main object of my commission was to ascertain the height of different places above the level of the sea, it was necessary to endeavour to determine that point with accuracy. From the observations made from time to time, at the caisson at Sheerness Dock Yard, of high and low water, I could only obtain this point within a certain degree of accuracy. I therefore determined to take advantage of the permission granted me by the Admiralty, to commence the erection of a tide-gauge at the Dock Yard. Accordingly, after having a model made on a principle that I hoped would be the most simple

and yet the most accurate, I examined the whole of the front of the Dock Yard, and selected the corner of the boat basin adjoining the ordnance basin, as the only spot indeed at all eligible for the erection of a tide-gauge. (The spot is shown on the plan of the Yard.)

By the friendly and prompt assistance of His Majesty's Commissioner at the Dock Yard (J. Lewis, Esq.), I was enabled to have my tide-gauge quickly and substantially erected, under the excellent superintendence of Mr. Mitchell, the master millwright at the Yard.

Description of the Tide-Gauge.

The model is on a scale of three quarters of an inch to a foot.* The flat board on which the house stands, represents the Wharf at Sheerness; it is made long at the back, and balanced so as to be placed on a table, to show the trunk in the actual position of the gauge. The top or cap to the covering of the staff is left unfixed, in order to be taken off, and allow the house to be lifted over it, and show the gauge alone.

The slide rods in the model are of iron wire, and out of proportion; but no smaller was at that time to be found in the Dock Yard; in the original they are $\frac{7}{8}$ copper bolts.

The slides or pointers have springs to tighten them to the rods, but they are too minute to act in the model. The lower end of the tube in the model is nearly filled up with wood, in order to secure it to the trunk, leaving only a part for the gauge-rod to pass; but in the original the tube is left open below, there being sufficient strength in the timber to allow of its being bolted to the trunk and platform. There are three small friction-wheels at the upper end of the rod, to steady it.

There is a distance of fifteen feet between the top catch or point on the rod which brings the slide down to indicate low water, and the point on the middle of the rod which takes the other slide up to indicate high water. Therefore allowing an eighteen-feet tide, the top catch will bring the slide down to four on the index, and the middle catch on the rod will raise the slide to seven on the other index, the difference of which, three added to fifteen (the length between the points), gives eighteen. Therefore it will be observed that the dif-

* The model is deposited at the Royal Society's Apartments in Somerset House.

ference between the slides is always to be added or subtracted, to or from the length between the points on the rod. The divisions on either index are continued from 0 feet to 11 feet, to allow for any extraordinary tides. The little brass standard in front of the tide-gauge, and about six or eight feet from it, has been connected with it; therefore the height of any particular tide above the standard is easily found, knowing the exact distance from the middle catch to the line of immersion (which is registered). For example: Let the distance from the centre catch or point, to the line of immersion be 12.444, and the face of the brass standard on a level with 0.747 of the indices; and that high water carries the pointer to 6.546 on the index, then $12.444-(6.546-0.747) = 5.151$, the height of that particular tide below the standard. Again, assuming 4.328, as pointed on the index by the upper catch for low water, then $15+12.444-(4.328-0.747) = 23.863$, the height of standard above that of low water. The difference between the two, gives 18.712 for the rise and fall of the tide.

Many interesting observations may also be made by the same instrument on the irregular rise and fall of the tides, as there is an excellent clock within the house, and I believe an index connected with a weathercock on the outside.

The trunk of the gauge is substantially fixed between large piles, driven in for the purpose, and the whole partitioned off to low-water mark, to render the gauge secure from boats or vessels*.

My next object was to select a standard mark, from whence to commence my levelling, and to form a zero point.

I made examinations of the Dock Yard in different parts, particularly that part facing the Medway. I at first fixed on the northern part of the Dock Yard wall adjoining the garrison; but upon due inquiry I found that part was not considered so good a foundation, being built on piles driven in at some distance from one another, and to no great depth. Wishing, however, to shorten the distance between my zero point and the long level I should have to take across the Medway to the Isle of Grain, and desirous of having the

* This tide-gauge is on the same principle as that mentioned by Mr. Lubbock in the Companion to the British Almanac.

standard in the vicinity of the tide-gauge, so that future observations might be easily connected with the standard, I selected a large block of granite in the southern pier of the entrance to the boat basin, the position of which is shown in the plan of the Yard. I caused a block of gun-metal (cast for the purpose), two inches and a half square and eight inches long, to be sunk in the centre of the granite, about an inch below the surface, thereby allowing a brass box and cover to be placed over the standard, to protect it from injury.

In order that there should be a sufficient number of checks to the stability of this standard mark, I caused three more to be placed in the Yard; viz. one near the southern extremity on the wall of the Dock Yard, one at the eastern side of the great basin, and one in a large block of stone resting on the brick-work of the navy well 330 feet deep.

But however satisfied I might be that these standards were sufficiently firm, I thought it advisable to seek some spot more unquestionable in its foundation than Sheerness Dock Yard. I found a place that possessed this advantage; it was a slight eminence about two miles and a half to the southward of the Dock Yard, and surrounded by a moat.

On this eminence formerly stood the old castle of Queenborough, within a short distance of the present town of Queenborough. The castle, which was in the form of a pentagon, was some years ago pulled down, but a very small part of the foundation was left. On this foundation, which is rubble and chalk, some feet under the surface a very large block of granite was placed for me, by order of the commanding officer of engineers, and into which was let one of the brass standards. The place is now covered over, but marked by a small mound of earth near it, and reference can be easily made to it if required.

Having now standard marks enough to ensure, by comparison, the knowledge of any alteration (if any should occur) in the zero point, I commenced levelling.

My first business was to ascertain the difference of the several standards in the Dock Yard above the level of the sea.

From a series of observations made at the caisson at the entrance to the great basin, in the years 1827, 1828, 1829, the mean of the tides was as follows:

Mean high water spring tide for	1827,	26.475	Low water	8.700	Mean	17.587
Do. . . . do. . . .	1828,	26.50	. do. .	8.61	do.	17.55
Do. . . . do. . . .	1829,	26.09	. do. .	8.93	do.	17.51
Means . .		26.355	. do. .	8.74	do.	17.549
High water neap tides . . .	1827,	22.56	. do. .	11.12	do.	16.84
Do. . . . do. . . .	1828,	22.69	. do. .	11.44	do.	17.06
Do. . . . do. . . .	1829,	22.72	. do. .	11.45	do.	17.08
Means . .		22.656	. do. .	11.336	do.	16.993

Mean for the three years.

Spring tide high water,	26.355	Spring low .	8.74	Mean level	17.549
Neap tide do. . .	22.656	Neap low .	11.336	. do. .	16.993
Means . .	24.50		10.03		17.27

As there are many blanks in the three years' observations that these were taken from, I have also selected the most perfect year (1827), and taken the mean of all the tides for each month in the year.

The following is the

Summary.

January,	Mean high water	24.78	Mean low	10.20	Mean level	17.49
February . .	do. . .	24.08	. do. .	10.00	. do. .	17.04
March . . .	do. . .	24.70	. do. .	9.60	. do. .	17.15
April	do. . .	24.25	. do. .	9.90	. do. .	17.07
May	do. . .	24.35	. do. .	10.35	. do. .	17.35
June	do. . .	24.31	. do. .	10.29	. do. .	17.20
July	do. . .	24.39	. do. .	10.16	. do. .	17.27
August . . .	do. . .	24.39	. do. .	10.26	. do. .	17.32
September . .	do. . .	24.29	. do. .	10.00	. do. .	17.14
October . .	do. . .	24.79	. do. .	9.66	. do. .	17.22
November . .	do. . .	24.65	. do. .	9.90	. do. .	17.27
December . .	do. . .	24.53	. do. .	9.62	. do. .	17.27
	Means . .	24.46		9.995		17.23

These results agree so very nearly, that they may be safely taken as correct. I have given them at length, to afford the data for finding the true level of the sea. It will be seen that the mean level taken from spring tides differs 0.556 from the mean level deduced from neap tides. I shall, however, assume the mean level as 17.27, differing only 0.04 from the same of the whole year's observations of 1827.

At the north-western end of the caisson I caused a small cross (+) to be cut in the granite, which corresponded exactly with the XXXI feet of the indices cut in the masonry at the side of the caisson. This cross is situated as follows, with regard to the standard marks in the yard. (See page 3 of Levelling Book.)

The northern standard, which is my zero point, is 0.5789 below the cross. The southern standard at the other end of the Dock Yard wall is 0.0259 above the cross; therefore 0.0259 + 0.5789 = 0.6048 gives the height of the south standard above the northern one, although they are set equally beneath the surface of the granite.

The standard at the eastern edge of the great basin is 0.6735 above the northern, and 0.6735 − 0.6048 = 0.0687 above the southern standard, and 0.6735 − 0.5789 = 0.0946 above the +.

The standard at the navy well is 6.0656 below eastern standard.
5.9968 below south standard.
5.3920 below north standard.
and *5.9710 below the cross.

Now, the mean level of the sea being 17.27 feet, the + or XXXI feet of the indices is 13.73 above the mean level of the sea, and 4.645 above the mean spring tide high-water mark. And

North standard or zero is .	13.73−0.5789=13.1511	above the level of the sea, and	4.0661	above spring tide high-water mark.
South do .	13.73+0.0259=13.7559	. . . do.	4.6709	. . do.
East do .	13.73+0.0946=13.8246	. . . do.	4.7396	. . do.
Navy well .	13.73−5.9710= 7.7590	. . . do.	4.3260	below do.
Tide-gauge standard .	13.73−0.0946=13.6354	. . . do.	4.5504	above do.

* In page 4 of Levellings there will be observed a difference of 0.0001 in the two sets of levels, the first set taken in March, the last in June.

Having now completed this part of my observations, I levelled from the Dock Yard to Queenborough:—the result of the operation will be seen in pages 1 and 2 of the Levelling Book. And it will be there observed, that between the levels and proofs there is a difference (in a distance of nearly three miles) of 0.0269 about $\frac{3}{8}$ths of an inch. In the commencement of my commission I had a new instrument, and many little difficulties to overcome. In part of the Queenborough observations, I made use of another level which I had from TROUGHTON, on a different principle to the one by CAREY; and in page 3 of the rough Levelling Book, an observation is there made on the difficulty I had in getting the level to adjust, from the fault I have mentioned in my remarks, of the bubble having been placed in its tube with only a little paper, and becoming loose.

I used also my large instrument with a level adapted to it, made by RIECHENBACH, which was lent me by Captain SABINE. It was of a large diameter, and ground most accurately, but not hermetically sealed, having two plates of glass ground in to the ends. It was exceedingly delicate, but at any considerable motion, the bubble would break into many globules, requiring a long time to collect again. The second day I used it in the marshes the day was hot, and I had not at that time the means of shading it from the sun; the bubble, while observing, suddenly contracted, and almost immediately disappeared. I threw some water on the instrument, and the bubble gradually appeared again, but much lengthened. The next day I placed the level in the sun with a thermometer; when at 68° the end was forced out, and I found it contained ether, which appears to be quite unfit for a level in a high temperature. For future observations I replaced it by one of CARY's make.

The mean of the observations give as follows:—Queenborough standard is higher than southern standard 9.9981, and 10.6029 higher than northern standard; and 14.6690 above spring tide high-water mark, and 23.7540 above the mean level of the sea.

Having driven a large picket into the embankment at the Isle of Grain, at a point the shortest distance I could obtain from the north standard (viz. about 5000 feet), across the Medway, I now commenced the observations necessary to ascertain the difference of level between the zero or north standard, and the picket at the Isle of Grain. One instrument was placed near

the standard mark, and the other on the Isle of Grain, about fifty feet from the picket.

Between the high-water mark at the Isle of Grain and the standard at Sheerness, there is a long ledge or bank of mud, extending nearly half a mile, and quite dry at low water. At these times I found it very difficult to make observations, and together with the wind occasionally interfering, and the interruption from the sun's rays, which caused a great vibration in the atmosphere, I spent day after day uselessly waiting at the instrument. I was, however, more successful afterwards, by taking advantage of the first hour or two of day-light, which was more favourable for observation, as the wind had generally lulled, and the air was in a quiescent state.

After the instruments were in perfect adjustment, an observation was made at each side of the river alternately; first by myself at Sheerness, and then by my assistant at Grain. A mean of a considerable number was taken, the difference of which, after the curvature was subtracted from each, would give the refraction, provided the instruments were in good adjustment, and the observations correctly taken. (See page 5 of Levellings.)

Not being satisfied with the observations made at my first visit to Sheerness, I returned at a subsequent period, and made a great number most satisfactorily, from which the best were selected, and a mean taken.

As there was not a fit place for a standard mark near the margin of the river Medway, I caused a block of granite, about 900lbs. weight, to be placed on the foundation of the old church wall (the church was once much larger) of St. James, near the porch-door, into which block was sunk one of the brass standards. (See page 6 and 8.)

The face of this standard is 28.7454 feet above the north standard mark; 32.8115 above spring tide high-water mark, and 41.8965 above the mean level of the sea.

From St. James I almost immediately came into the marshes, levelling in a direct line towards Yantlet Creek. In consulting the Ordnance map of Kent, there appear but two or three ditches in the marshes; but in a distance of less than three miles I passed thirty-three dykes and ditches, from fifteen to twenty-five feet broad, and four or five deep, over which there is no mode of passing, but a bridge perhaps a mile distant.

It was desirable therefore to find some means of conveying my instruments, men and apparatus over, without losing so much time by going round. The men first used leaping-poles; and a case for the instrument was constructed water-tight, so as to swim over the ditches. But from the sides of the ditches being steep, the instrument was constantly subject to heavy blows in being taken out of the water; I therefore adopted another mode. On our arrival at a broad ditch, two leaping-poles, about eighteen feet in length, with cross pieces at the bottom to prevent their sliding too far in the mud, were connected together at the top by passing the eye of a rope just over the ends of them, where it was confined by two thumb-cleets; to this rope was attached, at any height that the depth of the ditch might require, a hook having four tails, and likewise hooks to them; these fixed into the four arms of the box, through which slid the poles for carrying it. The ends of the two leaping-poles were merely placed in the centre of the ditch, forming a pair of sheers, the apex of which was inclined on one side or the other by guys. The instrument was in this manner taken from one bank and landed on the other with the greatest gentleness; it was quickly unhooked, and men and apparatus passed over in the same manner. In this mode 263 ditches were passed between Sheerness and Greenwich.

On my arrival at the marshes, I found, from the nature of the soil, the greatest difficulty in adjusting the instruments; the movement of any of the men at a considerable distance caused a motion in the bubble, and the least alteration in my position whilst standing at the instrument shifted the bubble. In order to avoid moving, which heretofore was necessary to examine the level, I caused a square mirror, about eight inches by five, to be mounted on a mahogany frame, which permitted it to stand on the upper limb with the face placed at an angle of about 60° from the telescope, by which I could, without moving my head from the eye end of the telescope, read off the length of the bubble. This precaution was not however sufficient in some parts of the marshes, where the ground was of hardly more consistence than a bog, but invariably most unsubstantial where the most dry.

At these places, large pickets, from four to six feet in length, were driven in, having a groove on the iron head of each. On three of these, the iron-shod points of the stand rested; and after the observations were made, the pickets were drawn.

The pickets used for the stations were also of a larger class than those used in other parts; but the ground was occasionally so spongy, that it was with much difficulty that a picket could be driven, and frequently the iron heads of several would break off, before we could succeed in getting one down.

It will be seen by the Observation-book, that from a mean of pickets (page 5 to 11), the Queenborough and Sheerness marshes are in some parts 6.3652 below the northern standard; but these marshes are unlike the rest passed over, being particularly rugged and undulating.

In the commencement of the marshes between St. James and Yantlet Creek, it appears by a mean from picket 11 to 17, that the surface of the marshes is 5.8524 below the standard, or 1.7863 below mean spring tide high-water mark; and opposite Allhallows, by a mean from picket 35 to 44, the marshes are 3.7247 below the north standard, rising there 5.8524−3.7247=2.1277.

About half a mile past the Decoy in St. Mary's marshes, nine miles distant from Sheerness, the marshes again fall: the mean from picket 75 to 86 gives 5.9916 below the north standard at Sheerness, and 5.9916−4.0661=1.9255 below mean spring tide high-water mark.

Some distance past Cliff Canal, and between that and the Gravesend Canal, in Higham marshes, by a mean from 120 to 124, the surface is 6.6356 below the north standard, which −4.0661 gives 2.5695 below spring tide high-water mark, having fallen in a distance of seven miles and a half 2.9109, and in a distance of five miles 0.6440; the marshes between Northfleet and Greenhithe, by a mean from picket 208 to 211, are 7.4889 below the north standard, and −4.0661=3.4228 below spring tide high-water mark.

On the eastern side of Dartford Creek, the marshes, by a mean from picket 247 to 252, are 8.8676 below the north standard, therefore 8.8676−4.0661 =4.8015 below spring tide high-water mark.

On the western side of the marshes, as far as the mean from picket 256 to 259 will show, the marshes are 9.7207 below the north standard, and 5.6546 below spring tide high-water mark, and lower by 0.8531 than the marshes on the eastern side of Dartford Creek.

The marshes to the eastward, and in the immediate vicinity of the arsenal at Woolwich, are (from a mean of pickets 305 to 312) 10.1404 below north standard, and 6.0743 below spring tide high-water mark, and only 3.0107 above the mean level of the sea.

The only remaining pickets that were directly in the marshes, are from 347 to 352, the mean of which gives 9.6321 below north standard, and 5.5660 below spring tide high-water mark.

The following therefore is a statement of the depression of the marshes, from Sheerness towards the source of the Thames.

The Yantlet Creek marshes are 0.5128 higher than the Sheerness marshes.

The Allhallows marshes, in a distance of three miles, are 2.1277 higher than the Yantlet Creek, and 2.6405 higher than the Sheerness marshes.

The St. Mary's marshes are 0.1392 lower than the Yantlet Creek marshes, and 2.2669 below the Allhallows marshes, having fallen that quantity in a distance of three miles.

The Higham marshes are lower than those of Yantlet Creek by 0.7832, than those of Allhallows by 2.9109, and than those of St. Mary's by 0.6440, having fallen the last quantity in five miles.

The marshes between Northfleet and Greenhithe are lower than Yantlet Creek 1.6365, than Allhallows 3.7642, than St. Mary's 1.4973, than Higham 0.8533, being a fall of this last quantity in 6½ miles.

On the eastern side of Dartford Creek, the marshes are 3.0152 below those of Yantlet Creek, 5.1429 of Allhallows, 2.8760 of St. Mary's, 2.2320 of Higham, and 1.3787 below the marshes between Northfleet and Gravesend; being a fall of the last quantity in 4½ miles.

The marshes near Woolwich Arsenal to the eastward of the practising ground are 4.2880 below those of Yantlet Creek, 6.4157 of Allhallows, 4.1488 of St. Mary's, 3.5048 of Higham, 2.6515 of the marshes between Northfleet and Gravesend, and 1.2728 below the eastern Dartford Creek marshes, being a fall of 1.2728 in six miles.

The marshes at Greenwich, as far as the few observations I had the opportunity of making, are 0.5083 higher than those of Woolwich, therefore less that sum than the comparison of the Woolwich marshes.

At picket 131 (page 14), I intersected the Thames and Medway Canal, three miles from its mouth at the Thames.

The above picket was driven into the water's edge, another was at the same minute driven to the water's edge in the basin close to Gravesend. I then levelled along the banks, imagining that from a mean of simultaneous observa-

tions made at the two pickets at the water's edge, I should obtain the best proof to my levellings; the results (page 6 of Proof Levels) will show how little confidence is to be placed on water, as a true level, under such circumstances.

At the Lock Gate of the canal, close to the Thames (see page 15), I made some observations of the tides, and found, June 7th, that high-water mark at Gravesend was 1.1018 higher than at Sheerness, and June 8th, was 0.8367 higher; but these two observations are not to be depended on as giving a mean difference between the two places, as the height of the tides at Gravesend are much affected by any winds.

On the new pier at Gravesend, I caused one of the brass standards to be sunk in the granite on the eastern side, the face of which is 0.1828 below the north standard mark, and 3.8833 above mean spring tide high-water mark at Sheerness.

On my arrival at Greenwich Hospital, I commenced a set of branch levels, from thence to the Royal Observatory at Greenwich, in order to determine the height of that place above the level of the sea. From the abruptness of the ascent, the operation was very tedious; and I here found the advantage of the extra lens to the telescope, as there was seldom a distance of more than twelve or twenty feet between the pickets.

I levelled up to a small brass standard already placed for me by the direction of the Astronomer Royal in the block of stone immediately under the eye-end of the transit instrument pointing southward.

This standard (page 31 of Levels) is

140.6806 above the north standard at Sheerness.
153.8317 above the mean level of the sea.
144.7467 above the mean spring tide high-water mark.
162.3611 above the mean spring tide low-water mark.

These observations being completed, it occurred to the Astronomer Royal, after minutely examining my instrument, that it might be used as a proof in ascertaining the correctness of the horizontal point of the two mural circles.

By his directions I placed my level upon the high window-frame in front of and about eight feet from the object-end of the mural circle, pointed towards the north, and at such a height that (from a known principle in optics that all rays are parallel *,) I could intercept some of the rays from the object-glass.

* If any object be placed at the focus of a lens (viz. the wires), the emergent rays are parallel.

Having adjusted my level most exactly, I directed my telescope into the tube of the great telescope of the mural circle; and adjusting as for infinite distance, by placing a disc of white paper about an inch from the eye-end of the great telescope, I observed all the wires most distinctly. I then adjusted my horizontal wire for collimation.

The mean horizontal point was then taken, and the circle adjusted to that by the micrometer; and after again observing my instrument to be in perfect adjustment, I sought for the horizontal wire of the circle, and I was astonished and delighted to see so perfect a coincidence of the horizontal wires of the two instruments, that, until I slightly depressed the eye-end of my telescope, I could not see the horizontal wire of the circle separate from my own. The circle was then altered, and the wires were again made to coincide; the quantity was then read off, and found to agree within a very few hundredths of a second to the horizontal point.

The Astronomer Royal was present at these experiments, and expressed himself much pleased at the proof given of the coincidence of the two instruments.

From the stairs of Greenwich Hospital I crossed the river in order to level up to the different places where tide-registers had been kept.

After crossing the Isle of Dogs, I arrived at a spot on the south side of the lock of the City Canal at Limehouse Reach (see No. 381, page 26), which was 1.9008 above the north standard at Sheerness; and this spot was 3.8446 above the $\frac{\text{TRINITY HW}}{\text{1800}}$ marked on the face of the masonry. Therefore 3.8446 — 1.9008 = 1.9438 is the height of Trinity mark at the canal below northern standard at Sheerness, and 4.0661 (height of north standard above mean spring tide high-water mark) — 1.9438 = 2.1223, the height of Trinity mark above mean spring tide high-water mark at Sheerness.

This spot (picket No. 381) is also 0.5202 above a particularly high tide 21/ft. 11 in., 1827, marked on the masonry; but upon referring to the tide-register at Sheerness, of the 20th and 22nd of November 1827, no particular rise in the tide is to be remarked. It must therefore have been caused by land floods, which are the occasion of most of the extraordinary tides near London.

I next levelled to a standard placed at the West India Docks on the S.S.E. side, close to the entrance. This standard is 1.3018 above the northern standard, and 2.3367 above the index mark XXIII. Therefore 2.3367 − 1.3018 = 1.0349, the index mark below the north standard at Sheerness, and 4.0661 − 1.0349 = 3.0312 is the height of XXIII above mean spring tide high-water mark at Sheerness.

Not having succeeded in procuring a copy of the observations on the tides at the West India Docks, I cannot make any other comparison.

At the Regent's Canal Dock, in a very large stone near the office of the canal works (see page 26), I placed another standard mark. This standard is 2.4418 above the northern standard at Sheerness, and 2.2308 above the index mark XXI; therefore the index is 0.2110 above the north standard at Sheerness.

From two years' observations on the tides (1828—1829), the following are the results.

Mean Spring Tide High-Water Mark for				
	1828.		1829.	
	Spring Tide, High Water.	High Water, Neap.	Spring Tide, High Water.	High Water, Neap.
	Ft. In.	Ft. In.	Ft. In.	Ft. In.
January	19.8[3]	15.	19.1	14.8
February	18.11	14.3	19.1[5]	13.11
March	19.9	14.3	19.5	14.8
April	19.9[8]	14.3	19.6	13.7
May	18.9	15.5	19.	14.9
June	18.10	15.7	18.9[5]	15.2
July	19.2[6]	15.9	18.9[6]	15.7
August	19.3	15.4[5]	19.3	14.7[6]
September	19.9	14.6	19.7	14.7
October	19.7	14.5		
November	19.1[5]	14.3	19.3	14.3
December	19.2[5]	13.9	18.7	14.9
Means	19.325	14.733	19.141	14.592

Spring 1829	19.141	Neap 1829	14.592
—— 1828	19.325	—— 1828	14.733
Mean Spring Tide for two years	19.233	Mean Neap Tide for two years	14.662

Mean Spring Tide	19.233
Mean Neap Tide	14.662
Mean High Water	16.947

The mean high-water mark, taking every high water through the months, is as follows:

	1828.	1829.
January	17.342	16.904
February	16.904	16.804
March	17.070	17.208
April	16.925	17.270
May	17.352	17.175
June	17.258	17.091
July	17.595	17.350
August	17.466	17.100
September	17.345	17.591
October	17.000	,,
November	17.001	17.412
December	16.691	16.787
Means	17.1624	17.1538
	17.1581	

The difference therefore between the mean of spring and neap tide and the means of the months, as above, is (17.1581 — 16.947) 0.2111.

The index mark XXI at the canal is 0.2110 + 4.0661 = 4.2771 above spring tide high-water mark at Sheerness; and the spring tide high-water mark at the Regent's Canal being 19.233, XXI — 19.233 = 1.767, and 4.2771 — 1.767 = 2.5101, the height of mean spring tide high-water mark at the Regent's Canal above the same at Sheerness.

The index mark XXI is also 6.1321 above mean high-water mark at Sheerness. And the mean high-water mark at Regent's Canal being

16.947, XXI — 16.947 = 4.053 and 6.1321 — 4.053 = 2.0791

or 17.1581, XXI — 17.1581 = 3.8419 and 6.1321 — 3.8419 = 2.2902 the height of mean high-water mark at the canal above the same at Sheerness.

And the mark XXI is 7.9761 above mean neap tide high-water mark at Sheerness. The mean neap tide mark at the canal being 14.662, XXI — 14.662 = 6.338 and 7.9761 — 6.338 = 1.6381, the height of mean neap tide high water at the canal above the same at Sheerness. Therefore the water of spring tides at the canal above spring tides at Sheerness is higher by 0.8720 than it is at neap tides at both places.

Having no observations of low-water mark at the canal, I have not the means of ascertaining the difference at the two places.

The next place where a comparison of the tides was made was at the London Docks, at No. 418 of the Levels (see page 27), being on an iron on the south-west pier of the main entrance close to the first lock. This iron was 3.7570 above the northern standard, and 5.7682 above the index mark by the side of the gates XXIIII, which mark answers to the 18-feet Trinity $\frac{\text{HW}}{\underset{\wedge}{1800}}$

Therefore $3.7570 + 4.0661 - 5.7682 = 2.0549$ the height of XXIII above spring tide high-water mark at Sheerness. And $2.0549 + 1.855$ (the difference between spring tide and mean tide high-water mark at Sheerness) $= 3.9099$, the height of XXIII above mean high-water mark at Sheerness.

And $2.0549 + 3.699$ (difference between spring and neap tides at Sheerness), $= 5.7539$, the height of XXIII above neap tide at Sheerness.

And $2.0549 + 9.085$ (the difference between mean level and spring tide at Sheerness), $= 11.1399$, the height of XXIII above the mean level of the sea at Sheerness.

And $2.0549 + 17.615$ (difference between spring high and low water at Sheerness), $= 19.6699$, the height of XXIII above spring tide low-water mark at Sheerness.

By the kindness of Mr. LUBBOCK I have received the following results of twenty-six years' observations on the tides at the London Docks:

	Mean High Water. ft. in.	Spring High Water. ft. in.	Neap High Water. ft. in.
January	21.275	22.729	19.750
February	21.275	23.002	19.145
March	21.291	23.541	19.125
April	21.395	22.854	19.375
May	21.475	22.708	20.125
June	21.395	22.50	20.250
July	21.291	22.604	19.687
August	21.250	22.708	19.562
September	21.291	23.979	19.292
October	21.291	22.937	19.229
November	21.395	22.687	19.833
December	21.312	22.458	19.870
Mean	21.333	22.812	19.604

Therefore XXIII − 21.333 − 1.667.

And 3.9099 − 1.667 = 2.2429, the height of mean high-water mark at the London Docks above the same at Sheerness.

And XXIII − 22.812 = 0.188, therefore 2.0549 − 0.188 = 2.0361, the height of spring tide high-water mark at London Docks, above spring tide high-water mark at Sheerness.

And XXIII − 19.604 = 3.396, and 5.7539 − 3.396 = 2.3579, the height of neap tide high water at London Docks above the same at Sheerness.

No observations have been made on low-water mark; but from the Trinity mark it appears the spring tide low-water mark is considered to be 17.833 below Trinity mark, or rather below the high-water mark.

Therefore 22.812 − 17.833 = 4.979, and XXIII − 4.979 = 18.021, the height of XXIII above spring tide low-water mark.

And 19.6699 − 18.021 = 1.6679, the height of spring tide low water at London Docks, above spring tide low water at Sheerness.

Taking 22.812 and 4.979, the mean level of the sea is 13.896.

Therefore XXIII − 13.896 = 9.104, the height of XXIII above the mean level.

Then 11.1399 − 9.104 = 2.0359 gives the mean level at London Docks above the mean level of the sea.

The following is a Summary of the different heights.

Spring tide H. W. at London Docks, above the same at Sheerness				2.0361	
Mean H. W. mark	ditto	ditto	ditto	2.2429	0.2068
Neap tide Ditto	ditto	ditto	ditto	2.3579	0.1150
Spring tide low water	ditto	ditto	ditto	1.6679	0.6900
Mean level of the tides	ditto	ditto	ditto	2.0359	0.3680
Or taking more correctly the ½ difference between spring high and low water at Sheerness, the mean spring level is 10.8289 below XXXIII, therefore 10.8289 − 9.104 =				1.7249	

Note.—It seems by the above summary that as the water decreases in height, so the height of the water's surface at London Docks, above the same at Sheerness also decreases, with the exception of spring tide at London Docks and the neap tide; these are means, not of the highest tides, but of tides at a particular time of the moon's southing.

The next spot levelled to where any tidal observations were made, was St. Catherine's Docks, where a brass standard was placed close to the south-west side of the Dock-gates at the entrance.

This standard is 4.3143 above the north standard at Sheerness, and 6.2563 above the index mark XXVIII, upon a level with which is a —+— denoting Trinity $\frac{\text{HW}}{\underset{\wedge}{1800}}$

Therefore 4.3143 + 4.0661 − 6.2563 = 2.1241, the height of this index or Trinity mark above mean spring tide high-water mark at Sheerness.

After passing along the Tower Wharf, and placing a standard mark in one of the large blocks of granite lately put down near the Traitor's Arch (see Levellings, page 28), I arrived (not without much vexatious interruption and annoyance in passing along Thames-street and Billingsgate) at the starlings of Old London Bridge, where I had some difficulty in making observations, owing to the tremor caused by the vehicles above. I levelled up to the Trinity mark on the western side of the bridge, and found as follows (see page 29):

No. 443 was 4.1047 below the north standard mark at Sheerness, and the Trinity $\frac{\text{HW}}{\underset{\wedge}{1800}}$ was 1.9426 above No. 443.

Therefore 4.1047 − 1.9426 = 2.1621, the height of Trinity mark below north standard mark at Sheerness.

And 4.0661 − 2.1621 = 1.9040 gives the height of Trinity mark above the mean spring tide high-water mark at Sheerness.

Having now made all the observations that time and means afforded me, I concluded my levellings at a standard mark sunk in the large plinth of the landing-place (near the wall) of the stairs on the north-east side of the New London Bridge.

This standard was (page 29) 2.3967 below the north standard mark at Sheerness.

I have subjoined a list of the different Trinity marks I have observed, and their respective heights, and also of the brass standards placed by me, as well as various substantial points passed over in the levellings.

	Below North Stand. Sheerness.
Trinity high-water mark near the dock-gates of the City Canal .	1.9438
Ditto, answering to XXIII of the indices marked on the south-west side of the entrance to London Docks	2.0112
Ditto, at the west side of the lock-gate at the entrance to St. Catherine's Docks, answering to the index mark XXVIII . .	1.9420
Ditto, on the west side of London Bridge	2.1621

Finding so great a difference between the marks at the City Canal, London Docks, &c. and that at London Bridge, I levelled again in October last from St. Catherine's Docks to London Bridge, but found the same results. (See page 33.)

List of Standard Marks and other Points of Reference between Sheerness and London Bridge.

		Feet.
North standard at Sheerness		0.0000
South ditto ditto	higher	0.6048
Eastern ditto ditto	ditto	0.6735
Navy Well ditto ditto	lower	5.3921
Little ditto near tide-gauge at Sheerness	higher	0.4843
Queenborough standard	ditto	10.6029
St. James ditto	ditto	28.7454
The boundary post of Hoo, on a little eminence in the marshes, &c. (See page 10.)	below	2.6317
The 3 mile stone at the bank of canal. (See page 14)	above	2.4204
2 mile ditto	ditto	2.0038
1 mile ditto	ditto	1.1448
An iron clamp at second gate of Gravesend Canal. (See page 15)	ditto	0.5564
Brass standard mark on the pier at Gravesend . .	below	0.1828
The boundary stone of Swanscomb. (Page 18) . .	ditto	4.7578
Erith Church. (Page 20)	ditto	1.5669
Standard in Woolwich Arsenal	above	1.2019
A ⊓ in a corner-stone near the officers' guard-room	ditto	1.5576
The top of the $4\frac{1}{2}$ mile post in the river (□) . . .	ditto	1.7182
□ on a stone at the west end of the dock-yard . .	ditto	1.9975

		Feet.
Brass standard in the dock-yard on the eastern point of mast slip	above	1.6545
A ⊕ in small north-east gateway of Greenwich College, from the main road	ditto	9.9975
Small brass standard underneath the transit at Greenwich Observatory	ditto	140.6897
Little brass standard on the plinth of the statue of George II. in Greenwich Hospital	ditto	5.1157
□ on one of the iron plates near the south side of the lock of the City Canal	ditto	1.9008
Brass standard at the West India Docks	ditto	1.3018
Brass standard at Regent's Canal	ditto	2.4418
A □ on the top of a granite post close outside the entrance from Ratcliffe Highway to the London Docks	ditto	16.0915
On the top of a gun in the Docks near the bridge of the eastern basin	ditto	13.1646
Brass standard St. Catherine's Docks	ditto	4.3143
Brass standard near the Traitor's Arch in the Tower	ditto	1.2854
Brass standard at landing-place of New London Bridge	below	2.3967

As it may be expected that I should state the manner in which I made the preceding observations, in order that a judgement may be formed of the confidence to be placed in them, I shall give a concise description of the field operations, and of the calculations necessary to complete the observations, and place them as they are in the book*.

After examining the ground, the particular line for carrying on the levellings was selected and marked out, when pickets were driven in at proper distances, according to the range of the instrument over the ground.

In the first part of the work the levelling was carried on as follows, the instrument being perfectly adjusted, and the station staves placed on their respective pickets. I made four observations, the telescope being in a different

* The field-book and all the other papers connected with the preceding operations are preserved at the Royal Society's Apartments.

position at each observation, and making a complete revolution in its collars: my assistant read off each, and noted it down in a book. I then went to the staff and examined its position on the picket, and read off the last observation myself; my assistant then read from his book the last observation; the two, of course, when correct, would correspond.

The telescope of the instrument was then reversed in its collars (which is a good check to the adjustment at each observation), and by the motion in azimuth directed to the next station staff, when the same mode was used in observing: the spot was marked, and the instrument moved to the next station, and the station staff turned half round in its collars, and gimbles ready for the next level. At the end of the day's work an additional picket was driven in about twelve or fifteen feet from that just used, and compared with it; this was to ensure the detection of any alteration in the pickets during the intervening time, either from mischief or accident, being compared before the commencement of the day's work. After a few days the ground was gone over again, generally from the opposite end, and two observations taken at each picket, which were sometimes more and sometimes less in number than in the former levellings; these were the proof-levels. The distances were then measured, and the necessary angles taken to lay down the work. In this manner proof levels were taken up to picket No. 111; but finding in this method that I was liable to great inconvenience and loss of time from many circumstances, and amongst others by the pickets being mischievously drawn or moved; and if a trivial mistake occurred in the levellings (of several days back perhaps), it was only detected at the summary of the levels. I determined to endeavour to adopt some method of proof, not liable to the inconveniences of the former.

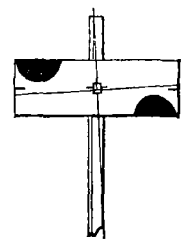
I accordingly made use of the following mode, which I have found, after repeated trial, to be a most correct proof.

After having finished the four observations at one picket, I threw the instrument out of adjustment by the foot-screws; and after adjusting it again most correctly, I took a pair of observations; but instead of reading off the staff from the picket, it was now read from above, the staff being imaginarily continued to the length of ten feet. The mean of these observations, therefore, gave the complement to 10,000 of the true observation: by this mode much time was saved, and an error, if any, was immediately detected of a tenth of a

foot, or a foot (which are the errors most likely to occur), without the trouble of bringing the instrument and adjusting it two distinct times at the same spot. The mean of the four and the mean of the two observations ought of course to make 10,000 *. In the detail of the proof-levels, the correction for curvature is additive instead of subtractive; and in fact the whole operation is reversed, + standing for —.

In the course of my levellings, having instruments not generally used, I made some few notes, which I take leave here to transcribe.

In adjusting the station-staff, it is difficult to know when the zero on the vane is made to coincide with the horizontal wire of the telescope. I have found the most convenient and correct mode to be, to observe with the wire forming a diagonal to the lines on the vane, by which, when the staff is near, the two small black lines at each end of the vane could be seen; and when the vane was adjusted to the proper height, one of these lines was above and the other below the horizontal wire at equal distances, thus:



—At a greater distance, the two little white right-angled triangles, formed by the edges of the vane and bounded by the black semicircle, are very distinctly seen, the one above and the other below the (now-placed) horizontal wire, and can be compared in size with great nicety.

But in observing with the wire diagonally, great care must be taken that, by the vertical wire (the error of which, if any, will by practice be accurately known), the axis of the telescope shall bisect the centre of the staff.

In an instrument with a very sensitive level, there is usually some difficulty in adjusting the level. It arises from no fault in the level itself, but from a

* In the proof-levels it will be found, that generally, upon adding them to the mean of the four observations, there will be a quantity of from .0010 to .0060 to make up the 10,000. I could not discover the cause for some time.

The wires being at right angles to each other, of course have been at different distances from the eye-glass; but the difference not being much more than the thickness of a hair, I did not alter the eye-tube: however, upon examination, I found that at the usual distance I took levels, when I altered from extreme distinct vision of the horizontal wire to extreme distinct vision of the vertical wire (which was the one I used for proofs), it made a difference of from .0020 to .0050, the distinct vision of the vertical wire being that quantity lower than when observed with the same adjustment as for the horizontal wire.

difference in weight of those parts of the telescope outside the collar. I found this difficulty in my instrument: to remedy it, I measured off from one of the resting points half the distance between the two Ys on the collars, and suspended the telescope by a fine wire from this point, which was the proper centre of the telescope. I found the eye-end with the tube close in to be more than four ounces heavier than the object-end. To remedy this, I caused a thick ring of lead of the above weight to be placed inside the tube near the object-end, by which the telescope was balanced; and I found it, when adjusted, to reverse without differing a quarter of a degree.

Distinct vision is certainly desirable, but not so absolutely requisite as that there shall be no parallax of the wires. The best way to avoid this, is, after adjusting for distinct vision, to move the eye as far as the hole in the eye-piece will admit of, and observe if the wires have any motion over the object or vane: if so, it must be remedied by sliding the eye-tube in or out, until the objects appear motionless.

Mirage.

I have found that the tremulous motion or jumping in the air, termed as above, appears not to be caused so much by evaporation as probably by some oscillation in the particles of light: for I have remarked, when the sun shines brightly and is occasionally obscured by clouds, that while the sun is out, the tremor is so great as to prevent the possibility of making a correct observation; yet the moment the sun is obscured, the intermediate space between the instrument and object (provided the sun is obscured so as to cast a shadow the whole distance) will be immediately perfectly tranquil; and again, at the instant of the sun's appearance, the same tremor will be observed.

I have found this motion to be exactly equal above and below any object; for upon placing the wire of the telescope one half the distance between the extreme oscillations, whenever the sun is obscured, the wire will be found to bisect the object.

Description of the Observation-Book.

Each page of the book will be found to contain sixteen columns: the first and ninth contain the numbers of each staff or picket; the second and eleventh the mean from the rough book of the four observations at each staff; the third

and eleventh the distance in feet from staff to instrument and instrument to staff; the fourth and twelfth the correction for the curvature of the earth; the fifth and thirteenth the mean observations minus the curvature; the sixth and eighth are the length of the bubble and the thermometer attached to the instrument. This last is useful as a check to the bubble, which, when the instrument is moved suddenly, shakes into several small globules, that sometimes do not immediately unite again: this is detected by the length of the bubble, which ought to correspond to a certain degree of the thermometer. Column fourteen is the difference between columns five and thirteen; column fifteen is that difference + or − ; and column sixteen is the amount of that difference added or subtracted, according to the sign, from the former quantity. These quantities are a continuation of heights above or below the first picket, or the northern standard in the dock-yard at Sheerness.

In order to prove the correctness of the different columns, they are summed up at the bottom, when the gross sum for curvature, being deducted from the gross sum of columns two and eleven, show the correctness of columns five and thirteen; and the difference between the sums of columns five and thirteen + or −, added to or subtracted from the little figures above the top line in column fifteen, ought to give the last true level in column sixteen at the bottom of the page.

The whole is further proved by taking the sums of columns five and thirteen of each page, and the difference of the whole amount of each gives a proof of the correctness of the whole work, by giving the difference at once between picket 1 and 445.

The same method is pursued in the proof-levels up to No. 112: after that, all the corrections become reversed, the curvature being additive (as the complement to 10,000 is read off on the staff). The difference between the proof-levels continue from standard at Sheerness to New London Bridge, and will be found to be 0.0110, and varying at different distances from Sheerness, but never more than 0.0300.

As there is not, in any work that I have seen, a correct table of curvatures of the earth for small distances, I have added one for every five feet from 60 to 1000.

A Table of the Curvature of the Earth, the mean diameter being 41,807,803 feet.

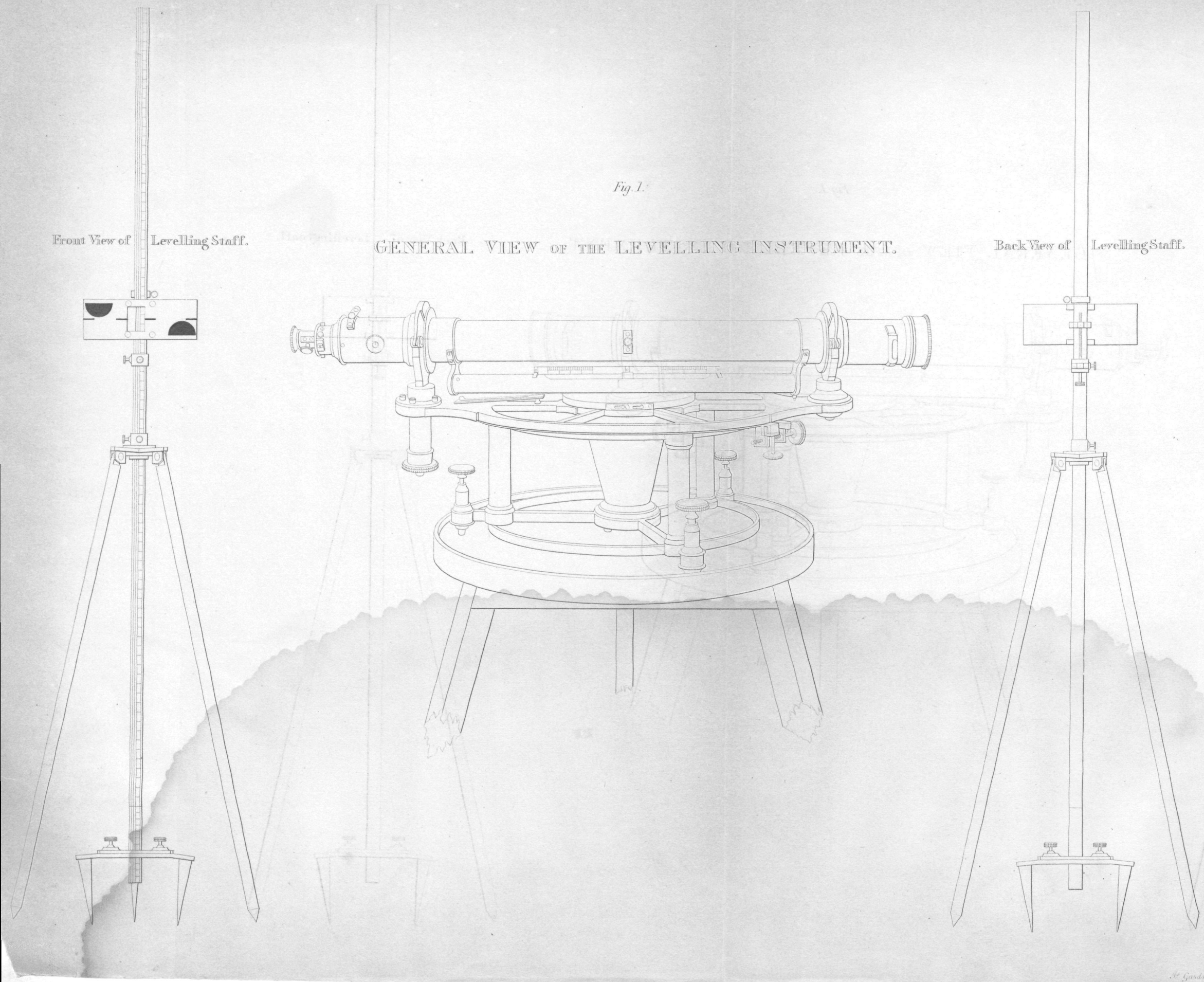
Dist.	Curvature.	Dist.	Curvature.	Dist.	Curvature.	Dist.	Curvature.	Dist.	Curvature.	Dist.	Curvature.
Feet.		Feet.		Feet.		Feet.		Feet.		Feet.	
60	000086	220	001157	380	003453	540	006975	700	011721	860	017691
65	000101	225	001216	385	003544	545	007104	705	011888	865	017897
70	000117	230	001265	390	003637	550	007235	710	012057	870	018104
75	000134	235	001321	395	003730	555	007368	715	012228	875	018313
80	000153	240	001378	400	003827	560	007501	720	012399	880	018523
85	000173	245	001435	405	003923	565	007636	725	012572	885	018734
90	000194	250	001495	410	004021	570	007771	730	012746	890	018946
95	000216	255	001555	415	004119	575	007908	735	012921	895	019159
100	000239	260	001617	420	004219	580	008046	740	013098	900	019379
105	000264	265	001680	425	004321	585	008186	745	013275	905	019591
110	000290	270	001744	430	004422	590	008326	750	013454	910	019807
115	000316	275	001809	435	004526	595	008468	755	013634	915	020025
120	000344	280	001875	440	004621	600	008611	760	013816	920	020245
125	000374	285	001943	445	004735	605	008755	765	013998	925	020466
130	000404	290	002016	450	004843	610	008901	770	014181	930	020687
135	000436	295	002082	455	004952	615	009047	775	014366	935	020911
140	000469	300	002152	460	005060	620	009195	780	014552	940	021135
145	000503	305	002224	465	005171	625	009344	785	014740	945	021360
150	000538	310	002298	470	005284	630	009494	790	014928	950	021586
155	000575	315	002373	475	005397	635	009645	795	015118	955	021824
160	000612	320	002449	480	005511	640	009798	800	015308	960	022043
165	000651	325	002526	485	005626	645	009951	805	015500	965	022274
170	000691	330	002605	490	005743	650	010105	810	015693	970	022505
175	000732	335	002684	495	005861	655	010262	815	015888	975	022738
180	000775	340	002764	500	005978	660	010419	820	016083	980	022972
185	000818	345	002847	505	006100	665	010577	825	016280	985	023207
190	000863	350	002929	510	006221	670	010737	830	016478	990	023443
195	000909	355	003014	515	006324	675	010898	835	016677	995	023681
200	000957	360	003100	520	006467	680	011061	840	016877	1000	023919
205	001005	365	003187	525	006593	685	011223	845	017079		
210	001054	370	003275	530	006719	690	011383	850	017281		
215	001105	375	003363	535	006846	695	011554	855	017485		

NOTE.—I received the following rule for curvature and refraction together, from DAVIES GILBERT, Esq., and it will be found useful in ascertaining heights approximately.

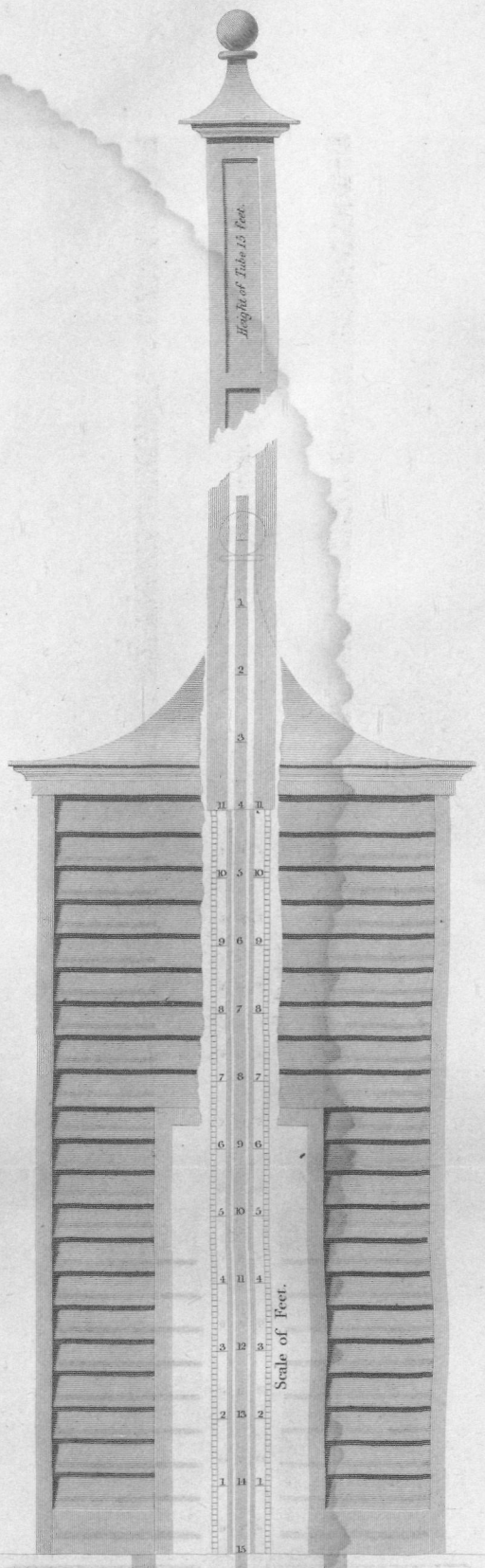
Rule.—Assume the diameter of the earth as 10,000 instead of 7918 miles, and which will give the refraction about one-tenth the intercepted arc.

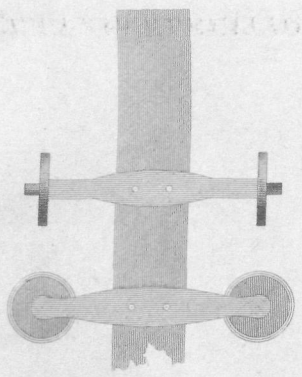
Fig. 1.

GENERAL VIEW OF THE LEVELLING INSTRUMENT.

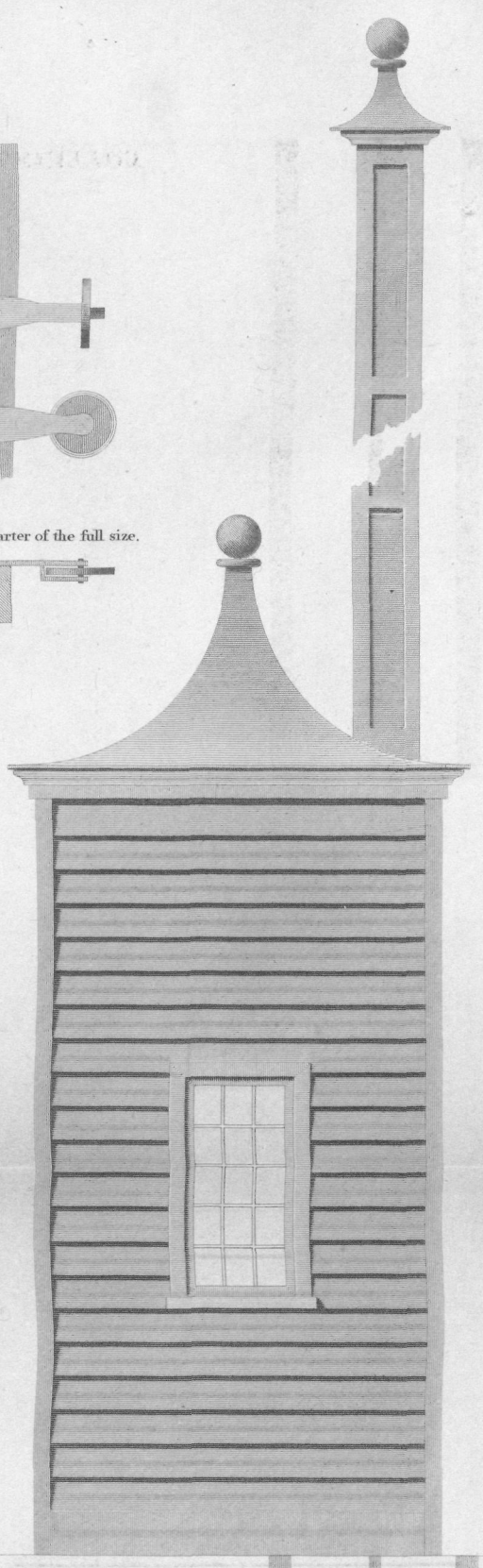


J. Gardner, sc.

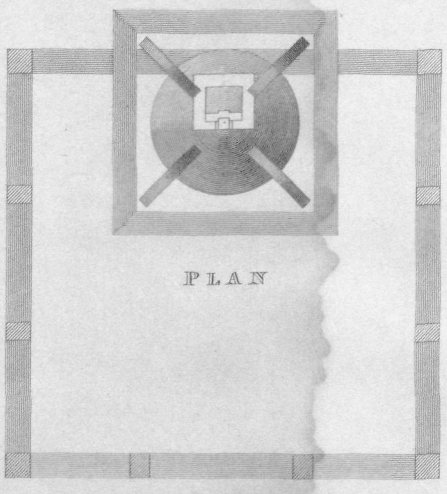




Plan of the Rollers. a quarter of the full size.

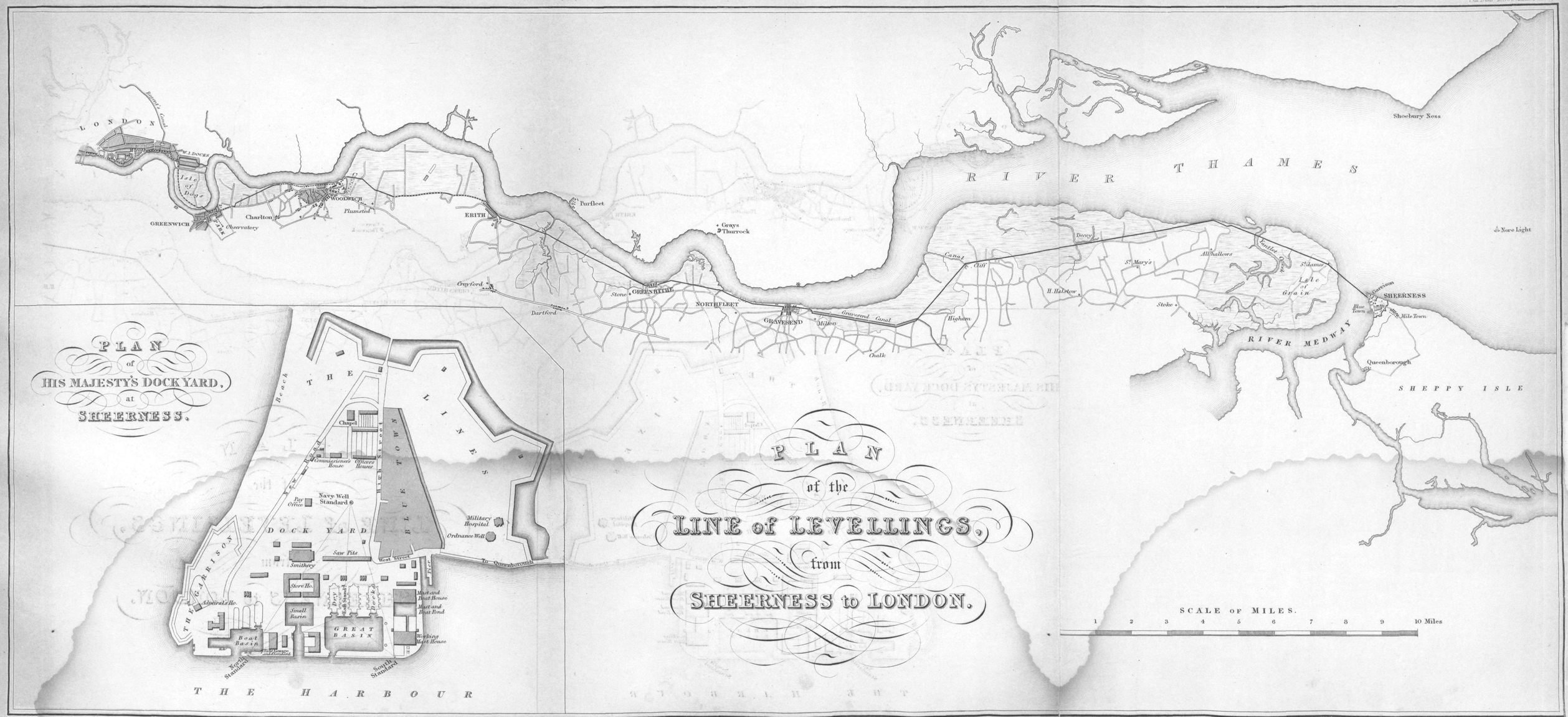
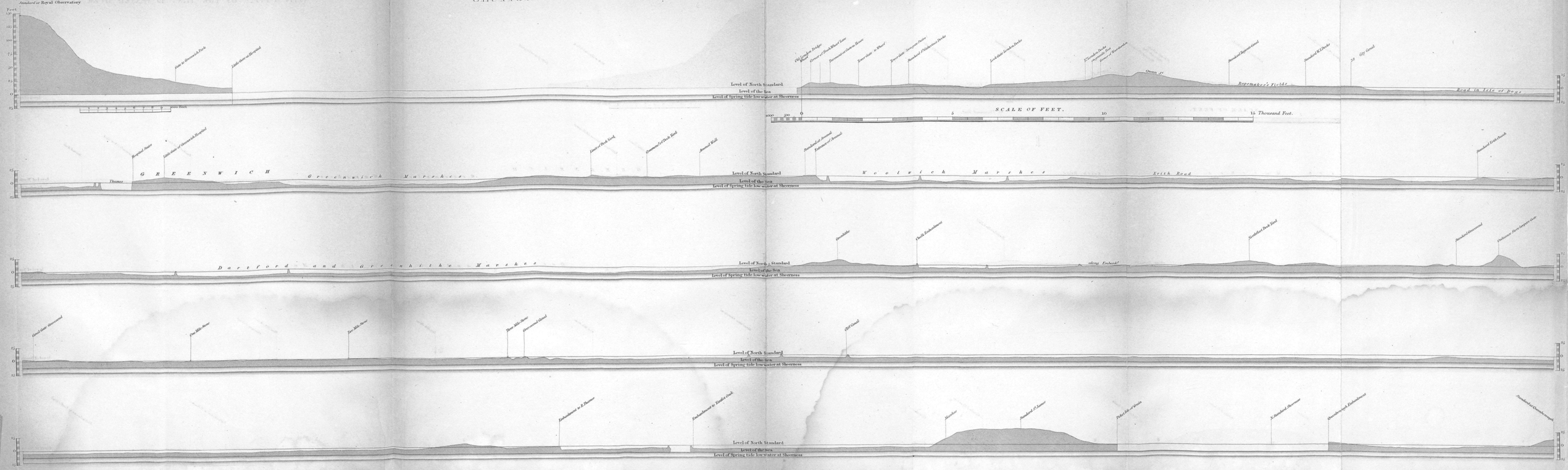


PLAN AND ELEVATION of the TIDE GAUGE at H. M. DOCKYARD SHEERNESS, and at PORTSMOUTH.



Scale $\frac{3}{4}$ of an Inch to 2 Feet.

J. Gardner sc.


PLAN of the LINE of LEVELLINGS, from SHEERNESS to LONDON.
PLAN of HIS MAJESTY'S DOCKYARD, at SHEERNESS.
LONDON
Regent's Canal
W.I. DOCKS
Isle of Dogs
GREENWICH
Observatory
Charlton
WOOLWICH
Plumstead
ERITH
Purfleet
Grays Thurrock
Cray
Cray Road
Gravford
Dartford
Stone
GREENHITHE
NORTHFLEET
GRAVESEND
Milton
Gravesend Canal
Chalk
Higham
Canal
Cliff
Decoy
H. Halstow
St. Mary's
Stoke
All Hallows
Yantlet Creek
St. James
Isle of Grain
Port Victoria
SHEERNESS
Blue Town
Mile Town
Queenborough
RIVER MEDWAY
SHEPPY ISLE
RIVER THAMES
Shoebury Ness
Nore Light
SCALE OF MILES.
1 2 3 4 5 6 7 8 9 10 Miles
THE LINE
Beach
Chapel
Commissioners House
Officers Houses
New Road
High Street
BLUE TOWN
Navy Well Standard
Pay Office
DOCK YARD
Military Hospital
Ordnance Well
Saw Pits
West Street
To Queenborough
Smithery
Pier
THE GARRISON
Store Hs.
Admiralty Hs.
Dry Dock
Slip Dock
Mast and Boat House
Mast and Boat Pond
Small Basin
GREAT BASIN
Working Mast House
Boat Basin
To Gillingham and Chatham
North Standard
South Standard
THE HARBOUR

SECTION OF THE LAND PASSED OVER IN THE LEVELLINGS FROM SHEERNESS TO LONDON BRIDGE.