



XXVI. On the tides

M. Laplace

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by a professional gentleman who had previously tried it in Walls End and Hebburn collieries; and its merits appear to be still greater than those of Mr. Stephenson's. The lamp being suspended in a vessel of glass open at the top, and the carburetted hydrogen admitted from below, the bright flame of the wick nearly disappeared, but the cylinder of wire-gauze was filled with a feeble but steady greenish light. On a greater volume of inflammable air being thrown in, the flame gradually died out. Results more satisfactory could not be expected nor wished for, particularly when we were assured that these accorded with numerous trials made in the most hazardous drifts of our coal-mines.

Notwithstanding all that has been lately said in some of the periodical publications, respecting the obstinacy of the viewers employed here, and the stupidity of their under agents and pit-men, you may depend upon it that these safe-lamps are hailed by this class of people as a most fortunate discovery, which will soon be adopted by them in every mine infected with fire-damp. And could a mode be struck out, of preventing inflammation taking place by means of the furnace placed at the bottom of the up-cast shaft to accelerate the circulation of air through the workings, little would be wanting to render the occupation of the collier as safe at least as that of the persons employed in lead and copper mines.

Your most obedient servant,

Newcastle-upon-Tyne, Feb. 16, 1816.

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XXVI. *On the Tides.* By M. LAPLACE*.

[Read to the first Class of the Institute the 10th of July 1815.†]

THIS phænomenon particularly merits the attention of observers, both because it is the nearest and most perceptible effect of the action of the heavenly bodies, and because the numerous varieties it presents are well calculated to verify the law of universal gravitation. At the request of the Academy of Sciences a course of observations were made at the beginning of the last century in the port of Brest, which were continued during six successive years, and of which the greater part have been published by Lalande in the fourth volume of his *Astronomie*. The situation of the port is very favourable for observations of this kind. It communicates with the sea by means of a canal,

* From the *Connaissance des Temps* for 1818.

† For this translation we are indebted to T. S. Evans, jun. of the College school, Gloucester.

terminating

terminating in a very large road, at the extremity of which the port has been built. Thus the irregularities of the motion of the sea are considerably weakened before they reach the port, very nearly in the same manner as the oscillations produced in the barometer by the irregular motion of a vessel, are diminished by a contraction in the tube of this instrument. In other respects the tides being considerable at Brest, the casual variations occasioned by winds form only a small part of them. It may also be remarked in the observations made of these tides, however few there may be of them, that a great regularity prevails which is not altered by the little river, which loses itself in the immense road of this port. Struck with this regularity, I solicited government to order a new course of observations to be made at Brest, during an entire period of the motion of the nodes of the lunar orbit. They had long been wished for. These new observations are dated from the 1st of June 1806, and since that period they have been continued uninterruptedly to this day. There is still, however, much wanting. They relate neither to the same part of the port, nor to the same scale. The observations of the first five years have been made at the place called *La Mûture*, the others were taken near the bason. I observe that this change has produced only slight differences; but it would have been better, undoubtedly, if all the observations had been made at the same place and upon the same scale. It is time, indeed, that phænomena of this nature should be observed with the same care as those of astronomy.

In these new observations I have considered those of the year 1807 and of the seven subsequent years. In each equinox and in each solstice I have chosen the three syzygies and the three quadratures nearest to this equinox and this solstice. In the syzygies I have taken the excess of the high water of the evening above the low water of the morning of the day which precedes the syzygy, of the day of the syzygy, and of the four following days, because the highest tide happens about the middle of this interval. I have made a sum of these excesses corresponding to each day, by doubling the excesses which relate to the intermediate syzygy, or that nearest to the equinox or the solstice. By this means the effects produced by the variation of the distances of the sun and of the moon from the earth are destroyed: for if the moon were, for example, towards its perigeum in the intermediate syzygy, it would be near its apogeum in the two extreme syzygies. The sums of the excesses thus obtained are, therefore, very nearly independent of the variations of the motion and of the distances of the heavenly bodies. There are still inequalities of the tides, different from that inequality, whose period is about half a day, and which in our ports is

much greater than the others. For by considering at the same time the observations at the two equinoxes and at the two solstices, the effect of the small inequality, whose period is nearly a day, is mutually destroyed. The sums in question are consequently entirely owing to the great inequality. The winds can have little influence on them; for, if they raise the high water, they must equally depress the low water. I have determined the law of these sums for each year, by observing, that their variation is very nearly proportional to the square of their distance in time from the maximum which has given me this maximum; its distance at the mean of the times of the syzygy tides, and the coefficient of the square of the times in the law of the variation. With regard to this coefficient, the little difference which the observations of each year present, proves the regularity of these observations: and according to the laws which I have elsewhere established, on the probability of results deduced from a great number of observations, some judgement may be formed of the accuracy of results determined from the whole of the observations of eight years.

In the same manner I have considered the quadrature tides, by taking the excess of the high water in the morning above the low water of the evening of the day of the quadrature, and of the three following days. The increase of the tides, beginning from the minimum, being much more rapid than their decrease, beginning from the maximum, I have thought it necessary to confine the law of the variation proportional to the square of the time within a much shorter interval.

In all these results the influence which the declinations of the heavenly bodies have on the tides, and upon the law of their variation in the syzygies and in the quadratures, is evidently shown. In considering, by the same method, eighteen equinoctial syzygy tides towards both the perigeum and the apogeum of the moon, the influence which the changes of the lunar distance have upon the height and upon the law of variation of the tides, is manifested with the same degree of evidence. It is thus that by combining observations in such a way as to bring out every element, which we are desirous of knowing, we are able to separate the laws of the phenomena when mixed and confounded together in the collections of observations.

After having obtained the results I have just mentioned, I compared them with the theory of the tides delivered in the fourth book of the *Mécanique Céleste*. This theory is founded on a principle of dynamics, which renders it very simple, and independent of the local circumstances of the port, which circumstances are too complicated for the possibility of submitting them to calculation. By means of this principle, they enter
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into the results of the analysis as arbitrary quantities, which ought thus to represent the observations, if the universal gravitation is in fact the true cause of the tides. The principle is this: *The state of a system of bodies in which the primitive conditions of motion have disappeared by the resistance it meets with, is periodical, as well as the forces which animate it.* By reuniting this principle to that of the coexistence of very small oscillations, I have obtained an expression for the height of the tides, of which the arbitrary quantities comprise the effect of the local circumstances of the port. To deduce this, I have reduced the generating expression of lunar and solar forces acting upon the ocean into a series containing the sines and cosines of angles increasing proportionally to the time. Each term of the series may be considered as representing the action of another object, which moves uniformly at a constant distance, in the plane of the equator. Thence arise several kinds of partial tides, the periods of which are about half a lunar day, a day, a month, a half-year, a year; and lastly, eighteen years and a half, which is the duration of the periodical motion of the nodes of the lunar orbit.

In the book which I have quoted of the *Mécanique Céleste*, I have compared this theory with the observations made at Brest at the commencement of the last century; and I have determined the constant arbitrary quantities relative to this port. I was curious to see whether these circumstances were found to be the same by the observations made a century afterwards, or whether they have experienced any alteration by the changes which the operations of nature and art have produced, either at the bottom of the sea or in the port, and on the adjacent coasts. The result of this inquiry is, that the actual heights of the tides, in the port of Brest, surpass the heights determined by the old observations by about $\frac{1}{45}$ th. One part of this difference may arise from the distance of the points where these observations were made; another part may be attributed to the errors of the observations: but these two causes do not seem to me sufficient to produce the whole difference which indicate with great probability a secular change in the action of the sun and moon on the tides of Brest, if we could be well assured of the exactness of the graduations of the old scale, and taking into account its inclination to the horizon. But the uncertainty we are in with respect to this point, does not permit us to pronounce upon this change, which ought in future to fix the attention of observers. In other respects the agreement is surprising between the old and the modern observations, as well as the theory; with regard to the variations of the heights of the tides depending on the declinations and distances of the heavenly

heavenly bodies from the earth, and the laws of their increase and decrease in proportion as they recede from their maximum and from their minimum. In the *Mécanique Céleste* I had not considered those laws relatively to the variations of the distances of the moon from the earth. Here I take them into consideration, and I find the same agreement between the observation and the theory.

The retardation of the greatest and least tides which follow the times of syzygies and quadratures, was observed by the ancients themselves, as we read in Pliny the naturalist. Daniel Bernouilli, in his paper on the Tides, that gained the prize proposed in 1740 by the Academy of Sciences, attributes this retardation to the inertia of the water; and perhaps also, adds he, to the time taken by the action of the moon to transmit itself to the earth. But I have proved in the fourth book of the *Mécanique Céleste*, that by allowing for the inertia of the water, the highest tides would coincide with the syzygies, if the sea covered uniformly the whole surface of the earth. As to the time of the transmission of the action of the moon, I have discovered by a comparative view of the whole of the celestial phenomena, that the attraction of matter is transmitted with a velocity incomparably greater than even the velocity of light itself. We must therefore seek some other cause for the retardation in question. I have proved in the book quoted above, that this cause is the rapidity of the motion of the celestial body in its orbit, combined with the local circumstances of the port. I have remarked, moreover, that the same cause may increase the ratio of the action of the moon on the sea to that of the sun; and I have given a method of determining this increase by means of the observations, the idea of which is this: Let us suppose the motion of the sun to be uniform:—if we consider only the great inequality of the tides whose period is about half a day, the solar tide is decomposed very nearly into two others, which are exactly those that would be produced by two celestial bodies moving uniformly, but with different velocities, in the plane of the equator, at the mean distance of the sun from the earth. The mass of the first body is that of the sun, multiplied by the cosine of the inclination of the ecliptic to the equator: its motion is that of the sun in its orbit. The second body constantly corresponds with the spring equinox, and its mass is that of the sun multiplied by the half of the square of the sine of the obliquity of the ecliptic. At the equinox these bodies are either in conjunction or in opposition, and the tide is the sum of the tides produced by each of them:—at the solstice the bodies are in quadrature, and the tide is the difference of these partial tides. The observations of the solar tide in these two points
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show, therefore, the relation of the partial tides, and consequently the ratio of the actions of the heavenly bodies on the ocean; and by comparing it to the ratio of their masses, the increase produced on it by the difference of their motion will be determined.

This increase is almost insensible for the sun, on account of the slowness of its motion; but it is very evident for the moon, whose motion is thirteen times more rapid, and whose action on the sea is nearly three times greater.

By comparing in the fourth book of the *Mécanique Céleste*, the observations of equinoctial and solstitial tides in the syzygies and the quadratures, I was led by this method to an increase of at least a tenth in the ratio of the action of the moon to that of the sun; but I remarked that an element so delicate ought to be determined by a greater number of observations. The collection of modern observations has procured me this advantage. These observations, employed in double number, confirm the increase indicated by the ancient observations, and they make it more than one-eighth. Another method founded on the comparison of the tides towards the apogee and perigee of the moon, and applied to the ancient as well as modern observations, leads us also to a similar result.—Thus the increase of the action of the heavenly bodies on the tides in the port of Brest ought not to leave any doubt.

The results of observations being always susceptible of errors, it is necessary to know the probability that those errors are contained within given limits. It is conceived, and with truth, that the probability remaining the same, those limits are the more diminished as the observations are more numerous, and agree better with each other. But this general view of the subject is not sufficient to warrant the exactness of the results of observations and the existence of regular causes which they seem to point out. Sometimes, indeed, it has induced us to seek for the cause of phenomena which were only the accidents of chance. The calculation of probabilities can alone enable us to appreciate these objects, which renders its use of the highest importance in physical and moral sciences. The preceding researches afforded me an opportunity too favourable to be neglected, of applying the new formulæ which I have obtained in my *Théorie analytique des Probabilités*, to one of the grandest phenomena of nature. I there explain at full length the application I have made of it to the laws of the tides. My object has been, not only to confirm the truth of those laws, but to trace the way which must be pursued in applications of this kind. Among these laws, the most delicate are those of the increase and decrease of the tides towards their maximum and their minimum, and the influence which the declinations of the heavenly bodies
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and the variation of their distances from the earth, exercise in this respect. It is evident that these laws are determined by the observations with extreme precision and probability, which explains the remarkable agreement between the results of modern observations with those which the old observations had given me, and with the theory of gravitation. According to this theory, the action of the moon on the sea follows the inverse ratio of the cube of its distance from the centre of the earth; and this law represents the observations of the tides with such exactness, that by these observations alone the law of attraction being reciprocally as the square of the distances might be determined.

My principal desire was to apply my formulæ of probability to the increase of the action of the moon on the tides, depending on local circumstances. To determine this, the preceding observations have furnished me with sixteen equations of condition, and from them I have determined this increase to be equal to the $\frac{1}{1000000}$ part of the action of the moon on the ocean. By applying my formulæ to this result, I find that there are 21,400 chances to one, that the local circumstances of the port of Brest increase the ratio of the action of the moon on the tides to that of the sun: this increase may therefore be considered as certain; but there are only fourteen chances to one that the preceding value is not in error one half. We must therefore wait for new observations to obtain it with great probability of being mistaken by only very small quantities.

The ratio of the actions of the moon and of the sun on the sea, corrected for the effect of local circumstances, is very important to be known, because it determines the coefficients; 1st, of the terrestrial nutation; 2dly, of the inequality of the precession of the equinoxes; and 3dly, of the lunar equation of the sun's motion. Newton and Daniel Bernouilli had deduced this ratio from the phenomena of the tides, but without having regard to the correction I have just spoken of, which they did not suspect. The ratio which I have determined, and corrected by the whole of the preceding observations, gives the mass of the moon equal to $\frac{1}{80000}$; that of the earth being unity. It therefore gives in sexagesimal seconds, 9.65" for the coefficient of the nutation; which surpasses the coefficient determined by the observations of Maskelyne only by $\frac{1}{10000}$ of a second. My formulæ of probability show that there are 21,400 chances to one, that the nutation is not below 9.31", and there are the same number of chances to one, that it is not above 9.94". According to this same ratio the coefficient of the inequality of the precession is 18.04, and that of the lunar equation of the solar tables is 7.56", which differs from the coefficient that M. Delambre found directly from the examination
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of a great number of solar observations only by $\frac{1}{1000}$ of a second. In this calculation, I have supposed the mean parallax of the sun equal to $8.59''$, corresponding with that which I have deduced from my theory of the moon, compared with the inequality of the lunar motion, known by the name of *parallactic inequality*, and which M. Burckhardt has determined by means of a very great number of observations. M. Ferère, a learned Spanish astronomer, has lately confirmed this parallax by a new investigation of the transit of Venus in 1769; in which, by his own observations, he has corrected the latitude and longitude of places where this transit has been observed in America. The agreement between all these computations determined by phenomena so vague, is an additional confirmation of the principle of universal gravitation.

XXVII. *A new Instrument for comparing Linear Measures.*
By M. DE PRONY*.

THE comparison of linear measures, when great accuracy is necessary, requires careful and delicate operations, as well as the use of machinery not generally employed in commercial concerns, and difficult to be procured. I have already published the description of an instrument of this kind, invented and made by M. Lenoir, member of the Board of Longitude, which is as perfect as can be wished; but its expense and size put it out of the power of common observers to procure, and render it useless to travellers, who wish to know the proportion between some given standard, and the linear measures of any country through which they may pass.

I have had made for my own use, a *comparer*, which joins the two advantages of œconomy and portability: all the pieces of which it is composed may be fitted into a box of the size of a quarto book. The dearest part is a microscope; but even this requires nothing different from those with which observers are commonly furnished. In general it is only necessary to be at the expense of making three additional pieces, which I shall describe presently.

The properties and use of my *comparer* are founded on the progress which the art of dividing a right line has made within the last half century. This instrument has therefore, independently of the above advantages, that of requiring no *vernier* nor *micrometer screw*, &c.

It is well known that M. Richer, one of the first artists of Paris for the construction of mathematical instruments, has

* Communicated by Dr. Evans.