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THE TIDES
AND KINDRED PHENOMENA IN THE SOLAR SYSTEM

THE SUBSTANCE OF LECTURES DELIVERED
IN 1897 AT THE LOWELL INSTITUTE,
BOSTON, MASSACHUSETTS

BY
GEORGE HOWARD DARWIN
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PREFACE

In 1897 I delivered a course of lectures on the Tides at the Lowell Institute in Boston, Massachusetts, and this book contains the substance of what I then said. The personal form of address appropriate to a lecture is, I think, apt to be rather tiresome in a book, and I have therefore taken pains to eliminate all traces of the lecture from what I have written.

A mathematical argument is, after all, only organized common sense, and it is well that men of science should not always expound their work to the few behind a veil of technical language, but should from time to time explain to a larger public the reasoning which lies behind their mathematical notation. To a man unversed in popular exposition it needs a great effort to shell away the apparatus of investigation and the technical mode of speech from the thing behind it, and I owe a debt of gratitude to Mr. Lowell, trustee of the Institute, for having afforded me the occasion for making that effort.

It is not unlikely that the first remark of many who see my title will be that so small a subject as the Tides cannot demand a whole volume; but, in fact, the subject branches out in so many directions that the difficulty has been to attain to the requisite compression of my matter. Many popular works on astronomy devote a few pages to the Tides, but, as far as I know, none of these books contain explanations of the practical methods of observing and predicting the Tides, or give any details as to the degree of success attained by tidal predictions. If these matters are of interest, I invite my readers not to confine their reading to this preface. The later chapters of this book are devoted to the consideration of several branches of speculative Astronomy, with which the theory of the Tides has an intimate relationship. The problems involved in the origin and history of the solar and of other celestial systems have little bearing upon our life on the earth, yet these questions can hardly fail to be of interest to all those whose minds are in any degree permeated by the scientific spirit.

I think that there are many who would like to understand the Tides, and will make the attempt to do so provided the exposition be sufficiently simple and clear; it is to such readers I address this volume. It is for them to say how far I have succeeded in rendering these intricate subjects interesting and intelligible, but if I have failed it has not been for lack of pains.

The figures and diagrams have, for the most part, been made by Mr. Edwin Wilson of Cambridge, but I have to acknowledge the courtesy of the proprietors of *Harper's*, the *Century*, and the *Atlantic Monthly* magazines, in supplying me with some important illustrations.

A considerable portion of Chapter III. on the "Bore" is to appear as an article in the *Century Magazine* for October, 1898, and the reproductions of Captain Moore's photographs of the "Bore" in the Tsien-Tang-Kiang have been prepared for that article. The *Century* has also kindly furnished the block of Dr. Isaac Roberts's remarkable photograph of the great nebula in the constellation of Andromeda; it originally appeared in an article on Meteorites in the number for October, 1890. The greater portion of the text and the whole of the illustrations of Chapter XX. were originally published in *Harper's Magazine* for June, 1889. Lastly, portions of Chapters XV. and XVI. appeared in the *Atlantic Monthly* for April, 1898, published by Messrs. Houghton, Mifflin & Co., who also make themselves responsible for the publication of the American edition of this book.

In conclusion, I wish to take this opportunity of thanking my American audience for the cordiality of their reception, and my many friends across the Atlantic for their abundant hospitality and kindness.

G. H. DARWIN.

CAMBRIDGE, *August*, 1898.

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THE TIDES

CHAPTER I

TIDES AND METHODS OF OBSERVATION

THE great wave caused by an earthquake is often described in the newspapers as a tidal wave, and the same name is not unfrequently applied to such a short series of enormous waves as is occasionally encountered by a ship in the open sea. We must of course use our language in the manner which is most convenient, but as in this connection the adjective "tidal" implies simply greatness and uncommonness, the use of the term in such a sense cannot be regarded as appropriate.

The word "tidal" should, I think, only be used when we are referring to regular and persistent alternations of rise and fall of sea-level. Even in this case the term may perhaps be used in too wide a sense, for in many places there is a regular alternation of the wind, which blows in-shore during the day and out during the night with approximate regularity, and such breezes alternately raise and depress the sea-level, and thus produce a sort of tide. Then in the Tropics there is a regularly alternating, though small, periodicity in the pressure of the atmosphere, which is betrayed by an oscillation in the height of the barometer. Now the ocean will respond to the atmospheric pressure, so that the sea-level will fall with a rising barometer, and rise with a falling barometer. Thus a regularly periodic rise and fall of the sea-level must result from this cause also. Again, the melting of the snows in great mountain ranges, and the annual variability in rainfall and evaporation, produce approximately periodic changes of level in the estuaries of rivers, and although the period of these changes is very long, extending as they do over the whole year, yet from their periodicity they partake of the tidal character.

These changes of water level are not, however, tides in the proper sense of the term, and a true tide can only be adequately defined by reference to the causes which produce it. A tide, in fact, means a rising and falling of the water of the ocean caused by the attractions of the sun and moon.

Although true tides are due to astronomical causes, yet the effects of regularly periodic winds, variation of atmospheric pressure, and rainfall are so closely interlaced with the true tide that in actual observation of the sea it is necessary to consider them both together. It is accordingly practically convenient to speak of any regular alternation of sea-level, due to the wind and to the other influences to which I have referred, as a Meteorological Tide. The addition of the adjective "meteorological" justifies the use of the term "tide" in this connection.

We live at the bottom of an immense sea of air, and if the attractions of the sun and moon affect the ocean, they must also affect the air. This effect will be shown by a regular rise and fall in the height of the barometer. Although such an effect is undoubtedly very small, yet it is measurable. The daily heating of the air by the sun, and its cooling at night, produce marked alternations in the atmospheric pressure, and this effect may by analogy be called an atmospheric meteorological tide.

The attractions of the moon and sun must certainly act not only on the sea, but also on the solid earth; and, since the earth is not perfectly rigid or stiff, they must produce an alternating change in its shape. Even if the earth is now so stiff that the changes in its shape escape detection through their minuteness, yet such changes of shape must exist. There is much evidence to show that in the early stages of their histories the planets consisted largely or entirely of molten rock, which must have yielded to tidal influences. I shall, then, extend the term "tide" so as to include such alternating deformations of a solid and elastic, or of a molten and plastic, globe. These corporeal tides will be found to lead us on to some far-reaching astronomical speculations. The tide, in the sense which I have attributed to the term, covers a wide field of inquiry, and forms the subject of the present volume.

I now turn to the simplest and best known form of tidal phenomena. When we are at the seashore, or on an estuary, we see that the water rises and falls nearly twice a day. To be more exact, the average interval from one high water to the next is twelve hours twenty-five minutes, and so high water falls later, according to the clock, by twice twenty-five minutes, or by fifty minutes, on each successive day. Thus if high water falls to-day at noon, it will occur to-morrow at ten minutes to one. Before proceeding, it may be well to remark that I use high water and low water as technical terms. In common parlance the level of water may be called high or low, according as whether it is higher or lower than usual. But when the level varies periodically, there are certain moments when it is highest and lowest, and these will be referred to as the times of high and low water, or of high and of low tide. In the same way I shall speak of the heights at high and low water, as denoting the water-level at the moments in question.

The most elementary observations would show that the time of high water has an intimate relationship to the moon's position. The moon, in fact, passes the meridian on the average fifty minutes later on each succeeding day, so that if high water occurs so many hours after the moon is due south on any day, it will occur on any other day about the same number of hours after the moon was south. This rule is far from being exact, for it would be found that the interval from the moon's passage to high water differs considerably according to the age of the moon. I shall not, however, attempt to explain at present how this rough rule as to the time of high water must be qualified, so as to convert it into an accurate statement.

But it is not only the hour of high water which changes from day to day, for the height to which the water rises varies so conspicuously that the fact could not escape the notice of even the most casual observer. It would have been necessary to consult a clock to discover the law by which the hour of high water changes from day to day; but at the seashore it would be impossible to avoid noticing that some rocks or shoals which are continuously covered by the sea at one part of a fortnight are laid bare at others. It is, in fact, about full and new moon that the range from low to high water is greatest, and at the moon's first or third quarter that the range is least. The greater tides are called "springs," and the smaller "neaps."

The currents produced in the sea by tides are often very complicated where the open sea is broken by islands and headlands, and the knowledge of tidal currents at each place is only to be gained by the practical experience of the pilot. Indeed, in the language of sailors, the word "tide" is not unfrequently used as meaning tidal current, without reference to rise and fall. These currents are often of great violence, and vary from hour to hour as the water rises and falls, so that the pilot requires to know how the water stands in-shore in order to avail himself of his practical knowledge of how the currents will make in each place. A tide table is then of much use, even at places where the access to a harbor is not obstructed by a bar or shoal. It is, of course, still more important for ships to have a correct forecast of the tides where the entrance to the harbor is shallow.

I have now sketched in rough outline some of the peculiarities of the tides, and it will have become clear that the subject is a complicated one, not to be unraveled without regular observation. I shall, therefore, explain how tides are observed scientifically, and how the facts are collected upon which the scientific treatment of the tides is based.

The rise and fall of the sea may, of course, be roughly estimated by observing the height of the water on posts or at jetties, which jut out into moderately deep water. But as the sea is continually disturbed by waves, observations of this kind are not susceptible of accuracy, and for scientific purposes more elaborate apparatus is required. The exact height of the water can only be observed in a place to which the sea has a moderately free access, but where the channel is so narrow as to prevent the waves from sensibly disturbing

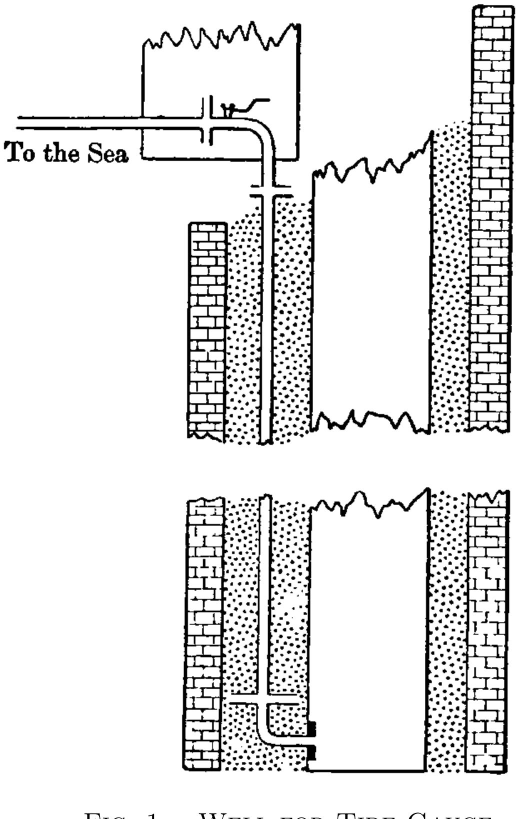

Fig. 1.—Well for Tide-Gauge

the level of the water. This result is obtained in a considerable variety of ways, but one of them may be described as typical of all.

A well (fig. 1) about two feet in diameter is dug to a depth of several feet below the lowest tide and in the neighborhood of deep water. The well is lined with iron, and a two-inch iron pipe runs into the well very near its bottom, and passes down the shore to the low-water line. Here it is joined to a flexible pipe running out into deep water, and ending with a large rose pierced with many holes, like that of a watering can. The rose (fig. 2) is anchored to the bottom of the sea, and is suspended by means of a buoy, so as to be clear of the bottom. The tidal water can thus enter pretty freely into the well, but the passage is so narrow that the wave motion is not transmitted into the well. Inside the well there floats a water-tight copper cylinder, weighted at the bottom so that it floats upright, and counter-poised so that it only just keeps its top clear of the water. To the top of the float there is fastened a copper tape or wire, which runs up to the top of the well and there passes round a wheel. Thus as the water rises and falls this wheel turns backwards and forwards.

It is hardly necessary to describe in detail the simple mechanism by which the turning of this wheel causes a pencil to move backwards and forwards in a straight line. The mechanism is, however, such that the pencil moves horizontally backwards and forwards by exactly the same amount as the water rises or falls in the well; or, if the rise and fall

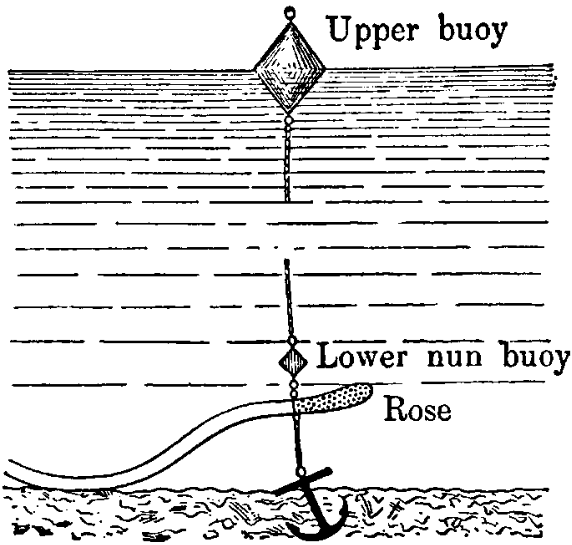

Fig. 2.—Pipe of Tide-Gauge

of the tide is considerable, the pencil only moves by half as much, or one third, or even one tenth as much as the water. At each place a scale of reduction is so chosen as to bring the range of motion of the pencil within convenient limits.

We thus have a pencil which will draw the rise and fall of the tide on the desired scale.

It remains to show how the times of the rise and fall are indicated. The end of the pencil touches a sheet of paper which is wrapped round a drum about five feet long and twenty-four inches in circumference. If the drum were kept still the pencil would simply draw a straight line to and fro along the length of the drum as the water rises and falls. But the drum is kept turning by clockwork, so that it makes exactly one revolution in twenty-four hours. Since the drum is twenty-four inches round, each inch of circumference corresponds to one hour. If the water were at rest the pencil would simply draw a circle round the paper, and the beginning and ending of the line would join, whilst if the drum remained still and the water moved, the pencil would draw a straight line along the length of the cylinder; but when both drum and water are in motion, the pencil draws a curve on the cylinder from which the height of water may be read off at any time in each day and night. At the end of twenty-four hours the pencil has returned to the same part of the paper from which it started, and it might be thought that there would be risk of confusion between the tides of to-day and those of yesterday. But since to-day the tides happen about three quarters of an hour later than yesterday, it is found that the lines keep clear of one another, and, in fact, it is

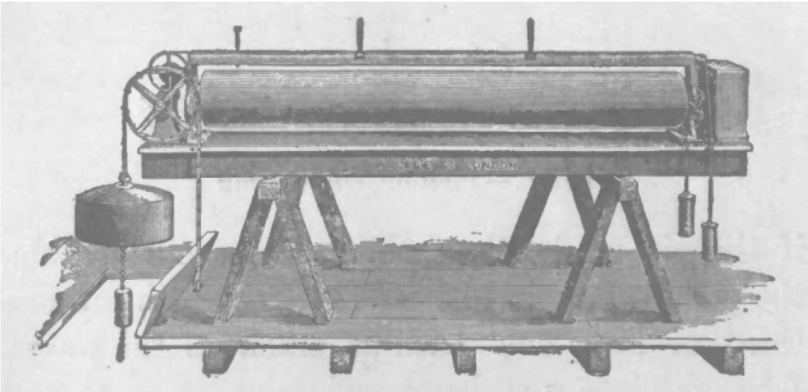

Fig. 3.—Indian Tide-Gauge

usual to allow the drum to run for a fortnight before changing the paper, and when the old sheet is unwrapped from the drum, there is written on it a tidal record for a fortnight.

The instrument which I have described is called a "tide-gauge," and the paper a "tide-curve." As I have already said, tide-gauges may differ in many details, but this description will serve as typical of all. Another form of tide-gauge is shown in fig. 4; here a continuous sheet of paper is placed over the drum, so that there is no crossing of the curves, as in the first example. Yet another form, designed by Lord Kelvin, is shown on p. 170 of vol. iii. of his "Popular Lectures."

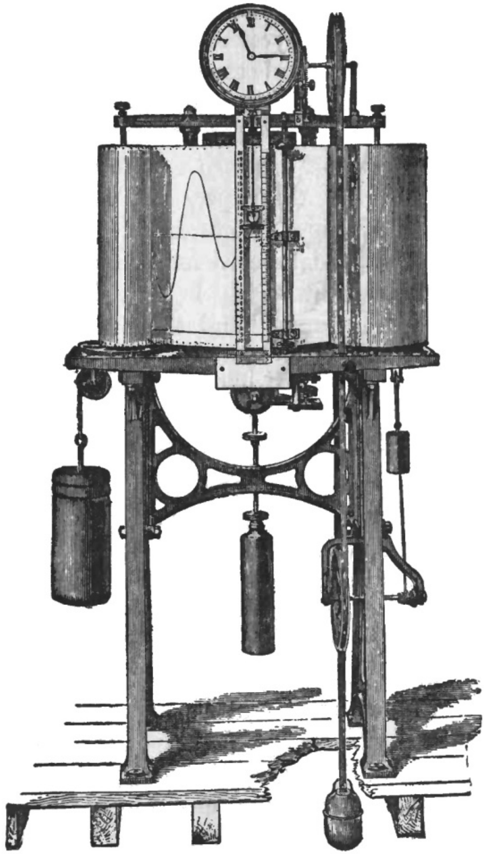

FIG. 4.—LÉGÉ'S TIDE-GAUGE

The actual record for a week is exhibited in fig. 5, on a reduced scale. This tide-curve was drawn at Bombay by a tide-gauge of the pattern first described. When the paper was wrapped on the drum, the right edge was joined to the left, and now that it is unwrapped the curve must be followed out of the paper on the left and into it again on the right. The figure shows that spring tide occurred on April 26, 1884; the preceding neap tide was on the 18th, and is not shown. It may be noticed that the law of the tide is conspicuously different from that which holds good on the coast of England, for the two successive high or low waters which occur on any day have very different heights. Thus, for example, on April 26 low water occurred at 5.50 P.M., and the water fell to 5 ft. 2 in., whereas the next low water, occurring at 5.45 A.M. of the 27th, fell to 1 ft. 3 in., the heights being in both cases measured from a certain datum. When we come to consider the theory of the tides the nature of this irregularity will be examined.

The position near the seashore to be chosen for the erection of the tide-gauge is a matter of much importance. The choice of a site is generally limited by nature, for it should be near the open sea, should be sheltered from heavy weather, and deep water must be close at hand even at low tide.

In the sketch map shown in fig. 6 a site such as A is a good one when the prevailing wind blows in the direction of the arrow. A position such as B, although well sheltered from heavy seas, is not so good, because it is found that tide-curves drawn at B would be much zigzagged. These zigzags appear in the Bombay curves, although at Bombay they are usually very smooth ones.

These irregularities in the tide-curve are not due to tides, and as the object of the observation is to determine the nature of the tides it is desirable to choose a site for the gauge where the zigzags shall not be troublesome; but it is not always easy to foresee the places which will furnish smooth tide-curves.

Most of us have probably at some time or other made a scratch on the sand by the seashore, and watched the water rise over it. We generally make our mark on the sand at the furthest point, where the wash of a rather large wave has brought up the water. For perhaps five or ten minutes no wave brings the water up as far as the mark, and one begins to think that it was really an extraordinarily large wave which was marked, although it did not seem so at the time. Then a wave brings up the water far over the mark, and immediately all the waves submerge it. This little observation simply points to the fact that the tide is apt to rise by jerks, and it is this irregularity of rise and fall which marks the notches in the tide-curves to which I have drawn attention.

Now in scientific matters it is well to follow up the clues

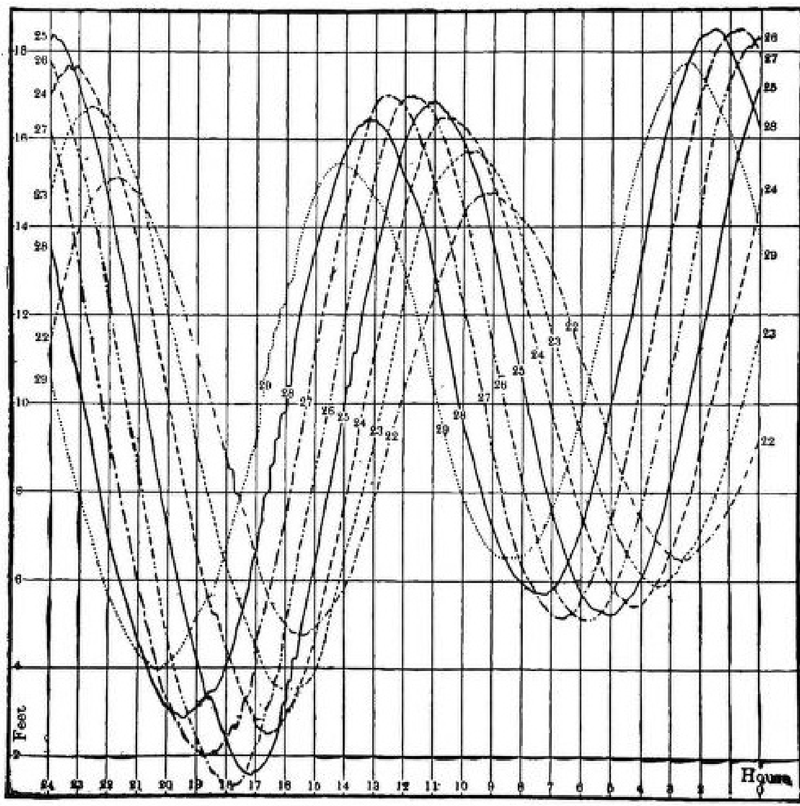

Fig. 5.—Bombay Tide-Curve from Noon, April 22, to Noon, April 30, 1884

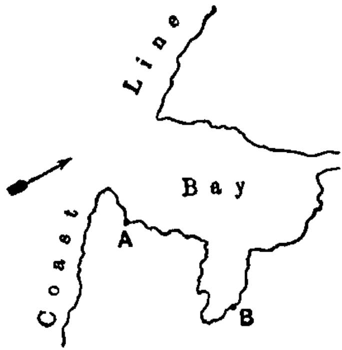

FIG. 6.—SITES FOR A TIDE-GAUGE

afforded by such apparently insignificant facts as this. An interesting light is indeed thrown on the origin of these notches on tide-curves by an investigation, not very directly connected with tidal observation, on which I shall make a digression in the following chapter.

AUTHORITIES.

Baird's *Manual for Tidal Observations* (Taylor & Francis, 1886). Price 7*s.* 6*d.* Figs. 1, 2, 3, 6 are reproduced from this work.

The second form of tide-gauge shown in fig. 4 is made by

Messrs. Légé, and is reproduced from a woodcut kindly provided by them.

Sir William Thomson's (Lord Kelvin's) *Popular Lectures and Addresses*, vol. iii. (Macmillan, 1891).

CHAPTER II

SEICHES IN LAKES

It has been known for nearly three centuries that the water of the Lake of Geneva is apt to rise and fall by a few inches, sometimes irregularly and sometimes with more or less regularity; and the same sort of oscillation has been observed in other Swiss lakes. These quasi-tides, called seiches, were until recently supposed only to occur in stormy weather, but it is now known that small seiches are of almost daily occurrence.[1]

Observations were made by Vaucher in the last century on the oscillations of the Lake of Geneva, and he gave an account of a celebrated seiche in the year 1600, when the water oscillated through three or four feet; but hardly any systematic observation had been undertaken when Professor Forel, of Lausanne, attacked the subject, and it is his very interesting observations which I propose to describe.

Doctor Forel is not a mathematician, but is rather a naturalist of the old school, who notes any interesting fact and then proceeds carefully to investigate its origin. His papers

[1] The word "seiche" is a purely local one. It has been alleged to be derived from "sèche," but I can see no reason for associating dryness with the phenomenon.

have a special charm in that he allows one to see all the workings of his mind, and tells of each difficulty as it arose and how he met it. To those who like to read of such work, almost in the form of a narrative, I can strongly recommend these papers, which afford an admirable example of research thoroughly carried out with simple appliances.

People are nowadays too apt to think that science can only be carried to perfection with elaborate appliances, and yet it is the fact that many of the finest experiments have been made with cardboard, cork, and sealing-wax. The principal reason for elaborate appliances in the laboratories of universities is that a teacher could not deal with a large number of students if he had to show each of them how to make and set up his apparatus, and a student would not be able to go through a large field of study if he had to spend days in preparation. Great laboratories have, indeed, a rather serious defect, in that they tend to make all but the very best students helpless, and thus to dwarf their powers of resource and inventiveness. The mass of scientific work is undoubtedly enormously increased by these institutions, but the number of really great investigators seems to remain almost unaffected by them. But I must not convey the impression that, in my opinion, great laboratories are not useful. It is obvious, indeed, that without them science could not be taught to large numbers of students, and, besides, there are many investigations in which every possible refinement of apparatus is necessary. But I do say that the number of great investigators is but little increased by laboratories, and that those who are interested in science, but yet have not access to laboratories, should not give up their study in despair.

Doctor Forel's object was, in the first instance, to note the variations of the level of the lake, after obliterating the small ripple of the waves on the surface. The instrument used in his earlier investigations was both simple and delicate. Its principle was founded on casual observation at the port of Morges, where there happens to be a breakwater, pierced by a large ingress for ships and a small one for rowing boats. He accidentally noticed that at the small passage there was always a current setting either inwards or outwards, and it occurred to him that such a current would form a very sensitive index of the rise and fall of the water in the lake. He therefore devised an instrument, illustrated in fig. 7, and called by him a plemyrameter, for noting currents of even the most sluggish character. Near the shore he made a small tank, and he connected it with the lake by means of an india-rubber siphon pipe of small bore. Where the pipe crossed the edge of the tank he inserted a horizontal glass tube of seven millimetres diameter, and in that tube he put a float of cork, weighted with lead so that it should be of the same density as water. At the ends of the glass tube there were stops, so that the float could not pass out of it. When the lake was higher than the tank, the water ran through

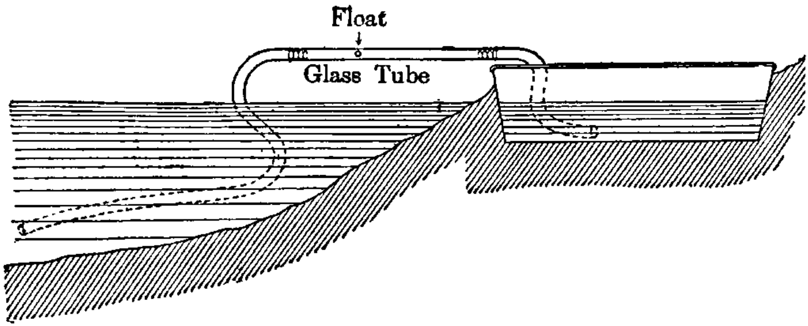

Fig. 7.—Plemyrameter

the siphon pipe from lake to tank, and the float remained jammed in the glass tube against the stop on the side towards the tank; and when the lake fell lower than the tank, the float traveled slowly to the other end and remained there. The siphon pipe being small, the only sign of the waves in the lake was that the float moved with slight jerks, instead of uniformly. Another consequence of the smallness of the tube was that the amount of water which could be delivered into the tank or drawn out of it in one or two hours was so small that it might practically be neglected, so that the water level in the tank might be considered as invariable.

This apparatus enabled Forel to note the rise and fall of the water, and he did not at first attempt to measure the height of rise and fall, as it was the periodicity in which he was principally interested.

In order to understand the record of observations, it must be remembered that when the float is towards the lake, the water in the tank stands at the higher level, and when the float is towards the tank the lake is the higher. In the diagrams, of which fig. 8 is an example, the straight line is divided into a scale of hours and minutes. The zigzag line gives the record, and the lower portions represent that the water of the lake was below the tank, and the upper line that it was above the tank. The fact that the float only moved slowly across from end to end of the glass tube, is indicated by the slope of the lines, which join the lower and upper portions of the zigzags. Then on reading fig. 8 we see that from 2 hrs. 1 min. to 2 hrs. 4 mins. the water was high and the float was jammed against the tank end of the tube, because there was a current from the lake to the tank. The float then slowly left the tank end and traveled across, so that at 2 hrs. 5 mins. the water was low in the lake. It continued, save for transient changes of level, to be low until 2 hrs. 30 mins., when it rose again. Further explanation seems unnecessary, as it should now be easy to read this diagram, and that shown in fig. 9.

The sharp pinnacles indicate alternations of level so transient that the float had not time to travel across from one end of the glass tube to the other, before the current was reversed. These pinnacles may be disregarded for the present, since we are only considering seiches of considerable period.

These two diagrams are samples of hundreds which were obtained at various points on the shores of Geneva, and of other lakes in Switzerland. In order to render intelligible the method by which Forel analyzed and interpreted these records, I must consider fig. 8 more closely. In this case it will be noticed that the record shows a long high water separated from a long low water by two pinnacles with flat tops. These pieces at the ends have an interesting significance. When the water of the lake is simply oscillating with a period of about an hour we have a trace of the form shown in fig. 9. But when there exists concurrently with this another oscillation, of much smaller range and of short period, the form of the trace will be changed. When the water is high in consequence of the large and slow oscillation, the level of the lake cannot be reduced below that of the tank by the small short oscillation, and the water merely stands a little higher or a little lower, but always remains above the level of the tank, so that the trace continues on the higher level. But when, in course of the changes of the large oscillation, the water has sunk to near the mean level of the lake, the short

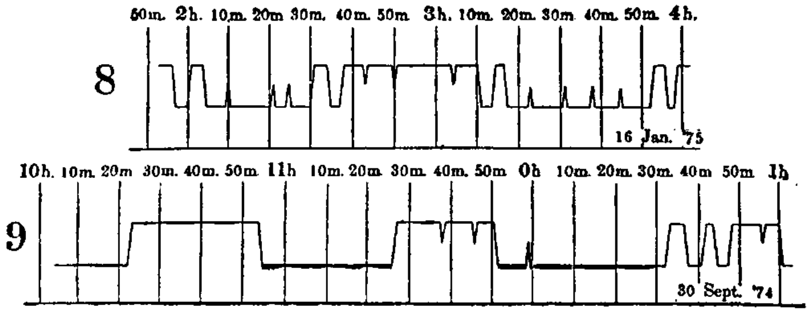

Fig. 8, 9.—Records of Seiches at Évian

oscillation will become manifest, and so it is only at the ends of the long flat pieces that we shall find evidence of the quick oscillation.

Thus, in these two figures there was in one case only one sort of wave, and in the other there were two simultaneous waves. These records are amongst the simplest of those obtained by Forel, and yet even here the oscillations of the water were sufficiently complicated. It needed, indeed, the careful analysis of many records to disentangle the several waves and to determine their periods.

After having studied seiches with a plemyrameter for some time, Forel used another form of apparatus, by which he could observe the amplitude of the waves as well as their period. His apparatus was, in fact, a very delicate tide-gauge, which he called a limnimeter. The only difference between this instrument and the one already described as a tide-gauge is that the drum turned much more rapidly, so that five feet of paper passed over the drum in twenty-four hours, and that the paper was comparatively narrow, the range of the oscillation being small. The curve was usually drawn on the full scale, but it could be quickly reduced to half scale when large seiches were under observation.

It would be impossible in a book of this kind to follow Forel in the long analysis by which he interpreted his curves. He speaks thus of the complication of simultaneous waves: "All these oscillations are embroidered one on the other and interlace their changes of level. There is here matter to disturb the calmest mind. I must have a very stout faith in the truth of my hypothesis to persist in maintaining that, in the midst of all these waves which cross and mingle, there is, nevertheless, a recognizable rhythm. This is, however, what I shall try to prove."[1] The hypothesis to which he here refers, and triumphantly proves, is that seiches consist of a

[1] *Deuxième Étude*, p. 544.

rocking of the whole water of the lake about fixed lines, just as by tilting a trough the water in it may be set swinging, so that the level at the middle remains unchanged, while at the two ends the water rises and falls alternately.

In another paper he remarks: "If you will follow and study with me these movements you will find a great charm in the investigation. When I see the water rising and falling on the shore at the end of my garden I have not before me a simple wave which disturbs the water of the bay of Morges, but I am observing the manifestation of a far more important phenomenon. It is the whole water of the lake which is rocking. It is a gigantic impulse which moves the whole liquid mass of Leman throughout its length, breadth, and depth.... It is probable that the same thing would be observed in far larger basins of water, and I feel bound to recognize in the phenomenon of seiches the grandest oscillatory movement which man can study on the face of our globe."[1]

It will now be well to consider the map of Geneva in fig. 10. Although the lake somewhat resembles the arc of a circle, the curvature of its shores will make so little difference in the nature of the swinging of the water that we may, in the first instance, consider it as practically straight.

Forel's analysis of seiches led him to conclude that the

[1] *Les Seiches, Vagues d'Oscillation*, p. 11.

oscillations were of two kinds, the longitudinal and the transverse. In the longitudinal seiche the water rocks about a line drawn across the lake nearly through Morges, and the water at the east end of the lake rises when that at the west falls, and vice versa. The line about which the water rocks is called a node, so that in this case there is one node at the middle of the lake. This sort of seiche is therefore called a uninodal longitudinal seiche. The period of the oscillation

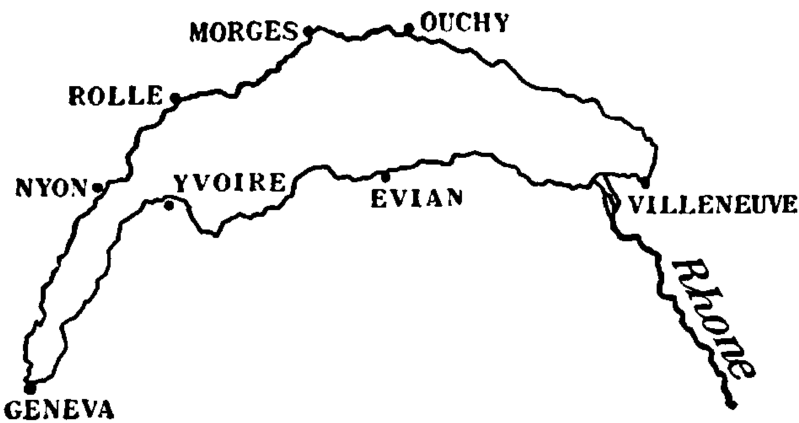

Fig. 10.—Map of Lake of Geneva

is the time between two successive high waters at any place, and it was found to be seventy-three minutes, but the range of rise and fall was very variable. There are also longitudinal seiches in which there are two nodes, dividing the lake into three parts, of which the central one is twice as long as the extreme parts; such an oscillation is called a binodal longitudinal seiche. In this mode the water at the middle of the lake is high when that at the two ends is low, and vice versa; the period is thirty-five minutes.

Other seiches of various periods were observed, some of which were no doubt multinodal. Thus in a trinodal seiche, the nodes divide the lake into four parts, of which the two central ones are each twice as long as the extreme parts. If there are any number of nodes, their positions are such that the central portion of the lake is divided into equal lengths, and the terminal parts are each of half the length of the central part or parts. This condition is necessary in order that the ends of the lake may fall at places where there is no horizontal current. In all such modes of oscillation the places where the horizontal current is evanescent are called loops, and these are always halfway between the nodes, where there is no rise and fall.

A trinodal seiche should have a period of about twenty-four minutes, and a quadrinodal seiche should oscillate in about eighteen minutes. The periods of these quicker seiches would, no doubt, be affected by the irregularity in the form and depth of the lake, and it is worthy of notice that Forel observed at Morges seiches with periods of about twenty minutes and thirty minutes, which he conjectured to be multinodal.

The second group of seiches were transverse, being observable at Morges and Évian. It was clear that these oscillations, of which the period was about ten minutes, were transversal, because at the moment when the water was highest at Morges it was lowest at Évian, and vice versa. As in the case of the longitudinal seiches, the principal oscillation of this class was uninodal, but the node was, of course, now longitudinal to the lake. The irregularity in the width and depth of the lake must lead to great diversity of period in the transverse seiches appropriate to the various parts of the lake. The transverse seiches at one part of the lake must also be transmitted elsewhere, and must confuse the seiches appropriate to other parts. Accordingly there is abundant reason to expect oscillations of such complexity as to elude complete explanation.

The great difficulty of applying deductive reasoning to the oscillations of a sheet of water of irregular outline and depth led Forel to construct a model of the lake. By studying the waves in his model he was able to recognize many of the oscillations occurring in the real lake, and so obtained an experimental confirmation of his theories, although the periods of oscillation in the model of course differed enormously from those observed in actuality.

The theory of seiches cannot be considered as demonstrated, unless we can show that the water of such a basin as that of Geneva is capable of swinging at the rates observed. I must, therefore, now explain how it may be proved that the periods of the actual oscillations agree with the facts of the case.

As a preliminary let us consider the nature of wave motion. There are two very distinct cases of the undulatory motion of water, which nevertheless graduate into one another. The distinction lies in the depth of the water compared with the length of the wave, measured from crest to crest, in the direction of wave propagation. The wave-length may be used as a measuring rod, and if the depth of the water is a small fraction of the wave-length, it must be considered shallow, but if its depth is a multiple of the wave-length, it will be deep. The two extremes of course graduate into one another.

In a wave in deep water the motion dies out pretty rapidly as we go below the surface, so that when we have gone down half a wave-length below the surface, the motion is very small. In shallow water, on the other hand, the motion extends quite to the bottom, and in water which is neither deep nor shallow, the condition of affairs is intermediate. The two figures, 11 and 12, show the nature of the movement in the two classes of waves. In both cases the dotted lines show the position of the water when at rest, and the full lines show the shapes assumed by the rectangular blocks marked out by the dotted lines, when wave motion is disturbing the water. It will be observed that in the deep water, as shown in fig. 11, the rectangular blocks change their shape, rise and fall, and move to and fro. Taking the topmost row of rectangles, each block of water passes successively in time through all the forms and positions shown by the top row of quasi-parallelograms. So also the successive changes of the second row of blocks are indicated by the second strip, and the third and the fourth indicate the same. The changes in the bottom row are relatively very small both as to shape

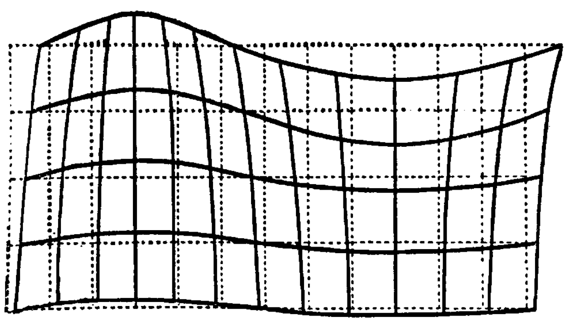

Fig. 11.—Wave in Deep Water

and as to displacement, so that it did not seem worth while to extend the figure to a greater depth.

Turning now to the wave in shallow water in fig. 12, we see that each of the blocks is simply displaced sideways and gets thinner or more squat as the wave passes along. Now, I say that we may roughly classify the water as being deep with respect to wave motion when its depth is more than half a wave-length, and as being shallow when it is less. Thus the same water may be shallow for long waves and deep for short ones. For example, the sea is very shallow for the great wave

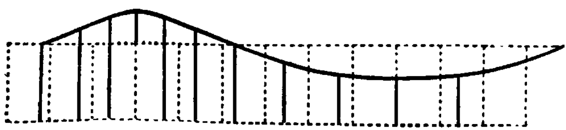

Fig. 12.—Wave in Shallow Water

of the oceanic tide, but it is very deep even for the largest waves of other kinds. Deepness and shallowness are thus merely relative to wave-length.

The rate at which a wave moves can be exactly calculated from mathematical formulæ, from which it appears that in the deep sea a wave 63 metres in length travels at 36 kilometres per hour, or, in British measure, a wave of 68 yards in length travels $22\frac{1}{2}$ miles an hour. Then, the rule for other waves is that the speed varies as the square root of the wave-length, so that a wave 16 metres long—that is, one quarter of 63 metres—travels at 18 kilometres an hour, which is half of 36 kilometres an hour. Or if its length were 7 metres, or one ninth as long, it would travel at 12 kilometres an hour, or one third as quick.

Although the speed of waves in deep water depends on wave-length, yet in shallow water the speed is identical for waves of all lengths, and depends only on the depth of the water. In water 10 metres deep, the calculated velocity of a wave is 36 kilometres an hour; or if the water were $2\frac{1}{2}$ metres deep (quarter of 10 metres), it would travel 18 kilometres (half of 36 kilometres) an hour; the law of variation being that the speed of the wave varies as the square root of the depth. For water that is neither deep nor shallow, the rate of wave propagation depends both on depth and on wave-length, according to a law which is somewhat complicated.

In the case of seiches, the waves are very long compared with the depth, so that the water is to be considered as shallow; and here we know that the speed of propagation of the wave depends only on depth. The average depth of the Lake of Geneva may be taken as about 150 metres, and it follows that the speed of a long wave in the lake is about 120 kilometres an hour.

In order to apply this conclusion to the study of seiches, we have to consider what is meant by the composition of two waves. If I take the series of numbers

&c. 100 71 0 -71 -100 -71 0 71 100 &c.

and plot out, at equal distances, a figure of heights proportional to these numbers, setting off the positive numbers above and the negative numbers below a horizontal line, I get the simple wave line shown in fig. 13. Now, if this wave is traveling to the right, the same series of numbers will represent the wave at a later time, when they are all displaced towards the right, as in the dotted line.

Now turn to the following schedule of numbers, and consider those which are written in the top row of each successive group of three rows. The columns represent equidistant spaces, and the rows equidistant times. The first set of numbers, -100, -71, 0, &c., are those which were plotted out as a wave in fig. 13; in the top row of the second group they

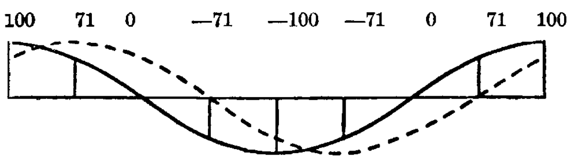

FIG. 13.—SIMPLE WAVE

are the same, but moved one space to the right, so that they represent the movement of the wave to the right in one interval of time. In the top row of each successive group the numbers are the same, but always displaced one more space to the right; they thus represent the successive positions of a wave moving to the right. The table ends in the same way as it begins, so that in eight of these intervals of time the wave has advanced through a space equal to its own length.

If we were to invert these upper figures, so that the numbers on the right are exchanged with those on the left, we should have a series of numbers representing a wave traveling to the left. Such numbers are shown in the second row in each group.

When these two waves coëxist, the numbers must be compounded together by addition, and then the result is the series of numbers written in the third rows. These numbers represent the resultant of a wave traveling to the right, and of an equal wave traveling simultaneously to the left.

It may be well to repeat that the first row of each group represents a wave moving to the right, the second row represents a wave moving to the left, and the third represents the resultant of the two. Now let us consider the nature of this resultant motion; the third and the seventh columns of figures are always zero, and therefore at these two places the water neither rises nor falls,—they are, in fact, nodes. If

−100	−71	0	71	100	71	0	−71	−100
−100	−71	0	71	100	71	0	−71	−100
−200	*−142*	*0*	*142*	*200*	*142*	*0*	*−142*	*−200*
−71	−100	−71	0	71	100	71	0	−71
−71	0	71	100	71	0	−71	−100	−71
−142	*−100*	*0*	*100*	*142*	*100*	*0*	*−100*	*−142*
0	−71	−100	−71	0	71	100	71	0
0	71	100	71	0	−71	−100	−71	0
0	*0*	*0*	*0*	*0*	*0*	*0*	*0*	*0*
71	0	−71	−100	−71	0	71	100	71
71	100	71	0	−71	−100	−71	0	71
142	*100*	*0*	*−100*	*−142*	*−100*	*0*	*100*	*142*
100	71	0	−71	−100	−71	0	71	100
100	71	0	−71	−100	−71	0	71	100
200	*142*	*0*	*−142*	*−200*	*−142*	*0*	*142*	*200*
71	100	71	0	−71	−100	−71	0	71
71	0	−71	−100	−71	0	71	100	71
142	*100*	*0*	*−100*	*−142*	*−100*	*0*	*100*	*142*
0	71	100	71	0	−71	−100	−71	0
0	−71	−100	−71	0	71	100	71	0
0	*0*	*0*	*0*	*0*	*0*	*0*	*0*	*0*
−71	0	71	100	71	0	−71	−100	−71
−71	−100	−71	0	71	100	71	0	−71
−142	*−100*	*0*	*100*	*142*	*100*	*0*	*−100*	*−142*
−100	−71	0	71	100	71	0	−71	−100
−100	−71	0	71	100	71	0	−71	−100
−200	*−142*	*0*	*142*	*200*	*142*	*0*	*−142*	*−200*

Composition of Two Equal and Opposite Waves the schedule were extended indefinitely both ways, exactly halfway between any pairs of nodes there would be a loop, or line across which there is no horizontal motion. In the schedule, as it stands, the first, fifth, and ninth columns are loops.

At the extreme right and at the extreme left the resultant numbers are the same, and represent a rise of the water from −200 to +200, and a subsequent fall to −200 again. If these nine columns represent the length of the lake, the motion is that which was described as binodal, for there are two nodes dividing the lake into three parts, there is a loop at each end, and when the water is high in the middle it is low at the ends, and vice versa. It follows that two equal waves, each as long as the lake, traveling in opposite directions, when compounded together give the motion which is described as the binodal longitudinal seiche.

Now let us suppose that only five columns of the table represent the length of the lake. The resultant numbers, which again terminate at each end with a loop, are:—

−200	−142	0	142	200
−142	−100	0	100	142
0	0	0	0	0
142	100	0	−100	−142

200	142	0	−142	−200
142	100	0	−100	−142
0	0	0	0	0
−142	−100	0	100	142
−200	−142	0	142	200

Since the middle column consists of zero throughout, the water neither rises nor falls there, and there is a node at the middle. Again, since the numbers at one end are just the same as those at the other, but reversed as to positive and negative, when the water is high at one end it is low at the other. The motion is, in fact, a simple rocking about the central line, and is that described as the uninodal longitudinal seiche.

The motion is here again the resultant of two equal waves moving in opposite directions, and the period of the oscillation is equal to the time which either simple wave takes to travel through its own length. But the length of the wave is now twice that of the lake. Hence it follows that the period of the rocking motion is the time occupied by a wave in traveling twice the length of the lake. We have already seen that in shallow water the rate at which a wave moves is independent of its length and depends only on the depth of the water, and that in water of the same depth as the Lake of Geneva the wave travels 120 kilometres an hour. The Lake of Geneva is 70 kilometres long, so that the two waves, whose composition produces a simple rocking of the water, must each of them have a length of 140 kilometres. Hence it follows that the period of a simple rocking motion, with one node in the middle of the Lake of Geneva, will be almost exactly $\frac{140}{120}$ of an hour, or 70 minutes. Forel, in fact, found the period to be 73 minutes. He expresses this result by saying that a uninodal longitudinal seiche in the Lake of Geneva has a period of 73 minutes. His observations also showed him that the period of a binodal seiche was 35 minutes. It follows from the previous discussion that when there are two nodes the period of the oscillation should be half as long as when there is one node. Hence, we should expect that the period would be about 36 or 37 minutes, and the discrepancy between these two results may be due to the fact that the formula by which we calculate the period of a binodal seiche would require some correction, because the depth of the lake is not so very small compared with the length of these shorter waves.

It is proper to remark that the agreement between the theoretical and observed periods is suspiciously exact. The lake differs much in depth in different parts, and it is not quite certain what is the proper method of computing the average depth for the determination of the period of a seiche. It is pretty clear, in fact, that the extreme closeness of the agreement is accidentally due to the assumption of a round number of metres as the average depth of the lake. The concordance between theory and observation must not, however, be depreciated too much, for it is certain that the facts of the case agree well with what is known of the depth of the lake.

The height of the waves called "seiches" is very various. I have mentioned an historical seiche which had a range of as much as four feet, and Forel was able by his delicate instruments still to detect them when they were only a millimetre or a twenty-fifth of an inch in height. It is obvious, therefore, that whatever be the cause of seiches, that cause must vary widely in intensity. According to Forel, seiches arise from several causes. It is clear that anything which heaps up the water at one end of the lake, and then ceases to act, must tend to produce an oscillation of the whole. Now, a rise of water level at one end or at one side of the lake may be produced in various ways. Some, and perhaps many, seiches are due to the tilting of the whole lake bed by minute earthquakes. Modern investigations seem to show that this is a more fertile cause than Forel was disposed to allow, and it would therefore be interesting to see the investigation of seiches repeated with the aid of delicate instruments for the study of earthquakes, some of which will be described in

Chapter VI. I suspect that seiches would be observed at times when the surface of the earth is much disturbed.

The wind is doubtless another cause of seiches. When it blows along the lake for many hours in one direction, it produces a superficial current, and heaps up the water at the end towards which it is blowing. If such a wind ceases somewhat suddenly, a seiche will certainly be started, and will continue for hours until it dies out from the effects of the friction of the water on the lake bottom. Again, the height of the barometer will often differ slightly at different parts of the lake, and the water will respond, just as does the mercury, to variations of atmospheric pressure. About a foot of rise of water should correspond to an inch of difference in the height of barometer. The barometric pressure cannot be quite uniform all over the Lake of Geneva, and although the differences must always be exceedingly small, yet it is impossible to doubt that this cause, combined probably with wind, will produce many seiches. I shall return later to the consideration of an interesting speculation as to the effects of barometric pressure on the oscillation of lakes and of the sea. Lastly, Forel was of opinion that sudden squalls or local storms were the most frequent causes of seiches. I think that he much overestimated the efficiency of this cause, because his theory of the path of the wind in sudden and local storms is one that would hardly be acceptable to most meteorologists.

Although, then, it is possible to indicate causes competent to produce seiches, yet we cannot as yet point out the particular cause for any individual seiche. The complication of causes is so great that this degree of uncertainty will probably never be entirely removed.

But I have not yet referred to the point which justifies this long digression on seiches in a book on the tides. The subject was introduced by the irregularities in the line traced by the tide-gauge at Bombay, which indicated that there are oscillations of the water with periods ranging from two minutes to a quarter of an hour or somewhat longer. Now these zigzags are not found in the sea alone, for Forel observed on the lake oscillations of short period, which resembled seiches in all but the fact of their more rapid alternations. Some of these waves are perhaps multinodal seiches, but it seems that they are usually too local to be true seiches affecting the whole body of the lake at one time. Forel calls these shorter oscillations "vibrations," thus distinguishing them from proper seiches. A complete theory of the so-called vibrations has not yet been formulated, although, as I shall show below, a theory is now under trial which serves to explain, at least in part, the origin of vibrations.

Forel observed with his limnimeter or tide-gauge that when there is much wind, especially from certain quarters, vibrations arise which are quite distinct from the ordinary visible wave motion. The period of the visible waves on the Lake of Geneva is from 4 to 5 seconds,[1] whereas vibrations have periods ranging from 45 seconds to 4 minutes. Thus there is a clear line separating waves from vibrations. Forel was unable to determine what proportion of the area of the lake is disturbed by vibrations at any one time, and although their velocity was not directly observed, there can be no doubt that these waves are propagated at a rate which corresponds to their length and to the depth of the water. I have little doubt but that the inequalities which produce notches in a tide-curve have the same origin as vibrations on lakes.

It is difficult to understand how a wind, whose only visible effect is short waves, can be responsible for raising waves of a length as great as a thousand yards or a mile, and yet we are driven to believe that this is the case. But Forel also found that steamers produce vibrations exactly like those due to wind. The resemblance was indeed so exact that vibrations due to wind could only be studied at night, when it was known that no steamers were traveling on the lake, and, further, the vibrations due to steamers could only be

[1]I observed when it was blowing half a gale on Ullswater, in Cumberland, that the waves had a period of about a second.

studied when there was no wind.

His observations on the steamer vibrations are amongst the most curious of all his results. When a boat arrives at the pier at Morges, the water rises slowly by about 5 to 8 millimetres, and then falls in about 20 to 30 seconds. The amount and the rapidity of the rise and fall vary with the tonnage of the boat and with the rate of her approach. After the boat has passed, the trace of the limnimeter shows irregularities with sharp points, the variations of height ranging from about two to five millimetres, with a period of about two minutes. These vibrations continue to be visible during two to three hours after the boat has passed. As these boats travel at a speed of 20 kilometres an hour, the vibrations persist for a long time after any renewal of them by the boat has ceased. These vibrations are called by Forel "the subsequent steamer vibrations."

That the agitation of the water should continue for more than two hours is very remarkable, and shows the delicacy of the method of observation. But it seems yet more strange that, when a boat is approaching Morges, the vibrations should be visible during 25 minutes before she reaches the pier. These he calls "antecedent steamer vibrations." They are more rapid than the subsequent ones, having a period of a minute to a minute and a quarter. Their height is sometimes two millimetres (a twelfth of an inch), but they are easily detected when less than one millimetre in height. It appears that these antecedent vibrations are first noticeable when the steamer rounds the mole of Ouchy, when she is still at a distance of 10 kilometres. As far as one can judge from the speed at which waves are transmitted in the Lake of Geneva, the antecedent vibrations, which are noticed 25 minutes before the arrival of the boat, must have been generated when she was at a distance of 12 kilometres from Morges. Fig. 14 gives an admirable tracing of these steamer vibrations.[1]

In this figure the line $a\ a'$ was traced between two and three o'clock in the morning, and shows scarcely any sign of perturbation. Between three and eight o'clock in the morning no observations were taken, but the record begins again at eight o'clock. The portion marked $b\ b'$ shows weak vibrations, probably due to steamers passing along the coast of Savoy. The antecedent vibrations, produced by a steamer approaching Morges, began about the time of its departure from Ouchy, and are shown at $c\ c'$. The point d shows the arrival of this boat at Morges, and d' shows the effect of another boat coming from Geneva. The portion marked *e e e* shows the subsequent steamer vibrations, which were very clear during more than two hours after the boats had passed.

Dr. Forel was aware that similar vibrations occur in the

[1] From *Les Seiches, Vagues d'Oscillation fixe des Lacs*, 1876.

sea, for he says: "What are these oscillations with periods of 5, 10, 20, or 100 minutes, which are sometimes irregular? Are they analogous to our seiches? Not if we define seiches as uninodal oscillations, for it is clear that if, in a closed basin of 70 kilometres in length, uninodal seiches have a period of 73 minutes, in the far greater basin of the Mediterranean, or of the ocean, a uninodal wave of oscillation must have a much longer period. They resemble much more closely what I have called vibrations, and, provisionally, I shall call them by the name of 'vibrations of the sea.' I venture to invite men of science who live on the seacoast to follow this study. It presents a fine subject for research, either in the interpretation of the phenomenon or in the establishment of the relations between these movements and meteorological conditions."[1]

These vibrations are obviously due to the wind or to steamers, but it is a matter of no little surprise that such insignificant causes should produce even very small waves of half a mile to a mile in length.

The manner in which this is brought about is undoubtedly obscure, yet it is possible to obtain some sort of insight into the way in which these long waves arise. When a stone falls into calm water waves of all sorts of lengths are instanta-

[1] *Seiches et Vibrations des Lacs et de la Mer*, 1879, p. 5.

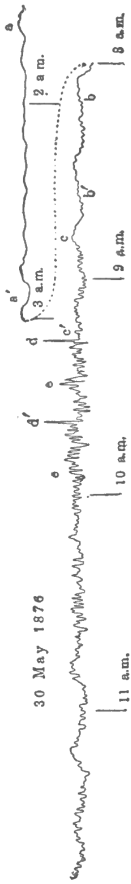

Fig. 14.—Vibrations due to Steamers

neously generated, and the same is true of any other isolated disturbance. Out of all these waves the very long ones and the very short ones are very small in height. Theoretically, waves of infinitely great and of infinitely small lengths, yet in both cases of infinitely small heights, are generated at the instant of the impulse, but the waves of enormous length and those of very small length are of no practical importance, and we need only consider the moderate waves. For the shorter of these the water is virtually deep, and so they will each travel outwards at a pace dependent on length, the longer ones outstripping the shorter ones. But for the longer waves the water will be shallow, and they will all travel together. Thus the general effect at a distance is the arrival of a long wave first, followed by an agitated rippling. The point which we have to note is that an isolated disturbance will generate long waves and that they will run ahead of the small ones. It is important also to observe that the friction of the water annuls the oscillation in the shorter waves more rapidly than it does that of the longer ones, and therefore the long waves are more persistent. Now we may look at the disturbance due to a steamer or to the wind as consisting of a succession of isolated disturbances, each of which will create long waves outstripping the shorter ones. These considerations afford a sort of explanation of what is observed, but I do not understand how it is that the separation of the long from the short waves is so complete, nor what governs the length of the waves, nor have I made any attempt to evaluate the greater rapidity of decrease of short waves than long ones.[1] It must then be left to future investigators to elucidate these points.

The subject of seiches and vibrations clearly affords an interesting field for further research. The seiches of Lake George in New South Wales have been observed by Mr. Russell, the government astronomer at Sydney; but until last year they do not seem to have been much studied on any lakes outside of Switzerland. The great lakes of North America are no doubt agitated by seiches on a much larger scale than those on the comparatively small basin of Geneva. This idea appears to have struck Mr. Napier Denison of Toronto, and he has been so fortunate as to enlist the interest of Mr. Bell Dawson, the chief of the Canadian Tidal Survey, and of Mr. Stupart, the director of the Meteorological Department. Mr. Denison's attention has been, in the first instance, principally directed towards those notches in tide-curves which have afforded the occasion for the present discussion of this subject. He has made an interesting suggestion as to the origin of these oscillations, which I will now explain.

[1] See, however, S. S. Hough, *Proc. Lond. Math. Soc.*, xxviii. p. 276.

The wind generally consists of a rather shallow current, so that when it is calm at the earth's surface there is often a strong wind at the top of a neighboring mountain; or the wind aloft may blow from a different quarter from that below. If we ascend a mountain or go up in a balloon, the temperature of the air falls on the average by a certain definite number of degrees per thousand feet. But the normal rate of fall of temperature is generally interrupted on passing into an upper current, which blows from a different direction. This abrupt change of temperature corresponds with a sudden change of density, so that the upper layer of air must be regarded as a fluid of different density from that of the lower air, over which it slides.

Now Helmholtz has pointed out that one layer of fluid cannot slide over another, without generating waves at the surface of separation. We are familiar with this fact in the case of sea-waves generated by wind. A mackerel sky proves also the applicability to currents of air of Helmholtz's observation. In this case the moisture of the air is condensed into clouds at the crests of the air waves, and reabsorbed in the hollows, so that the clouds are arranged in a visible ripple-mark. A mackerel sky is not seen in stormy weather, for it affords proof of the existence of an upper layer of air sliding with only moderate velocity over a lower layer. The distance from crest to crest must be considerable as measured in yards, yet we must regard the mackerel sky as a mere ripple formed by a slow relative velocity of the two layers. If this is so, it becomes of interest to consider what wave-lengths may be expected to arise when the upper current is moving over the lower with a speed of perhaps a hundred miles an hour. The problem is not directly soluble, for even in the case of sea-waves it is impossible to predict the wave-lengths. We do know, however, that the duration of the wind and the size of the basin are material circumstances, and that in gales in the open ocean the waves attain a very definite magnitude.

Although the problem involved is not a soluble one, yet Helmholtz has used the analogy of oceanic waves for an approximate determination of the sizes of the atmospheric ones. His method is a very fertile one in many complex physical investigations, where an exact solution is not attainable. The method may be best illustrated by one or two simple cases.

It is easy for the mathematician to prove that the period of a swing of a simple pendulum must vary as the square root of its length. The proof does not depend on the complete solution of the problem, so that even if it were insoluble he would still be sure of the correctness of his conclusion. If, then, a given pendulum is observed to swing in a certain period, it is certain that a similar pendulum of four times the length will take twice as long to perform its oscillation. In the same way, the engine power required for a ship is determinable from experiments on the resistance suffered by a small model when towed through the water. The correct conclusion is discovered in this case, although it is altogether impossible to discover the resistance of a ship by *à priori* reasoning.

The wave motion at the surface separating two fluids of different densities presents another problem of the same kind, and if the result is known in one case, it can be confidently predicted in another. Now oceanic waves generated by wind afford the known case, and Helmholtz has thence determined by analogy the lengths of the atmospheric waves which must exist aloft. By making plausible suppositions as to the densities of the two layers of air and as to their relative velocity, he has shown that sea-waves of ten yards in length will correspond with air-waves of as much as twenty miles. A wave of this length would cover the whole sky, and might have a period of half an hour. It is clear then that mackerel sky will disappear in stormy weather, because we are too near to the crests and furrows to observe the orderly arrangement of the clouds.

Although the waves are too long to be seen as such, yet the unsteadiness of the barometer in a gale of wind affords evidence of the correctness of this theory. In fact, when the crest of denser air is over the place of observation the barometer rises, and it falls as the hollow passes. The waves in the continuous trace of the barometer have some tendency to regularity, and have periods of from ten minutes to half an hour. The analogy seems to be pretty close with the confused and turbulent sea often seen in a gale of wind in the open ocean.[1]

Mr. Denison's application of this theory consists in supposing that the vibrations of the sea and of lakes are the response of the water to variations in the atmospheric pressure. The sea, being squeezed down by the greater pressure, should fall as the barometer rises, and conversely should rise as the barometer falls. He is engaged in a systematic comparison of the simultaneous excursions of the water and of the barometer on Lake Huron. Thus far the evidence seems decidedly favorable to the theory. He concludes that when the water is least disturbed, so also is the barometric trace; and that when the undulations of the lake become large and rapid, the atmospheric waves recorded by the barometer have the same character. There is also a considerable degree of correspondence between the periods of the two oscillations. The

[1] A gust of wind will cause the barometer to vary, without a corresponding change in the density of the air. It is not therefore safe to interpret the oscillations of the barometer as being due entirely to true changes of pressure. If, however, the intermittent squalls in a gale are connected with the waves aloft, the waviness of the barometric trace would still afford signals of the passage of crests and hollows above.

smaller undulations of the water correspond with the shorter air-waves, and are magnified as they run into narrower and shallower places, so as to make conspicuous "vibrations."

It is interesting to note that the vibrations of the water have a tendency to appear before those in the barometer, so that they seem to give a warning of approaching change of weather. It is thus not impossible that we here have the foreshadowing of a new form of meteorological instrument, which may be of service in the forecasting of the weather.

I must, however, emphasize that these conclusions are preliminary and tentative, and that much observation will be needed before they can be established as definite truths. Whatever may be the outcome, the investigation appears promising, and it is certainly already interesting.

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CHAPTER III[1]

TIDES IN RIVERS—TIDE MILLS

Since most important towns are situated on rivers or on estuaries, a large proportion of our tidal observations relates to such sites. I shall therefore now consider the curious, and at times very striking phenomena which attend the rise and fall of the tide in rivers.

The sea resembles a large pond in which the water rises and falls with the oceanic tide, and a river is a canal which leads into it. The rhythmical rise and fall of the sea generate waves which would travel up the river, whatever were the cause of the oscillation of the sea. Accordingly, a tide wave in a river owes its origin directly to the tide in the sea, which is itself produced by the tidal attractions of the sun and moon.

We have seen in Chapter II. that long waves progress in shallow water at a speed which depends only on the depth of the water, and that waves are to be considered as long when their length is at least twice the depth of the water. Now the tide wave in a river is many hundreds of times as long as the

[1]The account of the bore in this chapter appeared as an article in the *Century Magazine* for August, 1898. The illustrations then used are now reproduced, through the courtesy of the proprietors.

depth, and it must therefore progress at a speed dependent only on the depth. That speed is very slow compared with the motion of the great tide wave in the open ocean.

The terms "ebb" and "flow" are applied to tidal currents. The current ebbs when the water is receding from the land seaward, and flows when it is approaching the shore. On the open seacoast the water ebbs as the water-level falls, and it flows as the water rises. Thus at high and low tide the water is neither flowing landward nor ebbing seaward, and we say that it is slack or dead. In this case ebb and flow are simultaneous with rise and fall, and it is not uncommon to hear the two terms used synonymously; but we shall see that this usage is incorrect.

I begin by considering the tidal currents in a river of uniform depth, so sluggish in its own proper current that it may be considered as a stagnant canal, and the only currents to be considered are tidal currents. At any point on the river bank there is a certain mean height of water, such that the water rises as much above that level at high water as it falls below it at low water. The law of tidal current is, then, very simple. Whenever the water stands above the mean level the current is up-stream and progresses along with the tide wave; and whenever it stands below mean level the current is down-stream and progresses in the direction contrary to the tide wave. Since the current is up-stream when the water is higher than the mean, and down-stream when it is lower, it is obvious that when it stands exactly at mean level the current is neither up nor down, and the water is slack or dead. Also, at the moment of high water the current is most rapid up-stream, and at low water it is most rapid down-stream. Hence the tidal current "flows" for a long time after high water has passed and when the water-level is falling, and "ebbs" for a long time after low water and when the water-level is rising.

The law of tidal currents in a uniform canal communicating with the sea is thus very different from that which holds on an open seacoast, where slack water occurs at high and at low water, instead of at mean water. But rivers gradually broaden and become deeper as they approach the coast, and therefore the tidal currents in actual estuaries must be intermediate between the two cases of the open seacoast and the uniform canal.

A river has also to deliver a large quantity of water into the sea in the course of a single tidal oscillation, and its own proper current is superposed on the tidal currents. Hence in actual rivers the resultant current continues to flow up stream after high water is reached, with falling water-level, but ceases flowing before mean water-level is reached, and the resultant current ebbs down-stream after low water, and continues to ebb with the rising tide until mean water is reached, and usually for some time afterward. The downward stream, in fact, lasts longer than the upward one. The moments at which the currents change will differ in each river according to the depth, the rise and fall of the tide at the mouth, and the amount of water delivered by the river. An obvious consequence of this is that in rivers the tide rises quicker than it falls, so that a shorter time elapses between low water and high water than between high water and low water.

The tide wave in a river has another peculiarity of which I have not yet spoken. The complete theory of waves would be too technical for a book of this sort, and I must ask the reader to accept as a fact that a wave cannot progress along a river without changing its shape. The change is such that the front slope of the wave gradually gets steeper, and the rear slope becomes more gradual. This is illustrated in fig. 15, which shows the progress of a train of waves in shallow water as calculated theoretically. If the steepening of the advancing slope of a wave were carried to an extreme, the wave would present the form of a wall of water; but the mere advance of a wave into shallow water would by itself never suffice to produce so great a change of form without the concurrence of the natural stream of the river. The downward current in the river has, in fact, a very important influence in heading the sea-water back, and this coöperates with the natural change

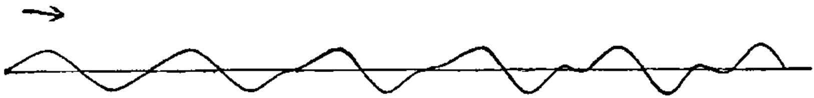

FIG. 15.—PROGRESSIVE CHANGE OF A TRAIN OF WAVES IN SHALLOW WATER

in the shape of a wave as it runs into shallow water, so as to exaggerate the steepness of the advancing slope of the wave.

There are in the estuaries of many rivers broad flats of mud or sand which are nearly dry at low water, and in such situations the tide not unfrequently rises with such great rapidity that the wave assumes the form of a wall of water. This sort of tide wave is called a "bore," and in French *mascaret*. Notwithstanding the striking nature of the phenomenon, very little has been published on the subject, and I know of only one series of systematic observations of the bore. As the account to which I refer is contained in the official publications of the English Admiralty, it has probably come under the notice of only a small circle of readers. But the experiences of the men engaged in making these observations were so striking that an account of them should prove of interest to the general public. I have, moreover, through the kindness of Admiral Sir William Wharton and of Captain Moore, the advantage of supplementing verbal description by photographs.

The estuary on which the observations were made is that of the Tsien-Tang-Kiang, a considerable river which flows into the China Sea about sixty miles south of the great Yang-Tse-Kiang. At most places the bore occurs only intermittently, but in this case it travels up the river at every tide. The bore may be observed within seventy miles of Shanghai, and within an easy walk of the great city of Hangchow; and yet nothing more than a mere mention of it is to be found in any previous publication.

In 1888 Captain Moore, R. N., in command of Her Majesty's surveying ship Rambler, thought that it was desirable to make a thorough survey of the river and estuary. He returned to the same station in 1892; and the account which I give of his survey is derived from reports drawn up after his two visits. The annexed sketch-map shows the estuary of the Tsien-Tang, and the few places to which I shall have occasion to refer are marked thereon.

On the morning of September 19, 1888, the Rambler was moored near an island, named after the ship, to the south-west of Chapu Bay; and on the 20th the two steam cutters Pandora and Gulnare, towing the sailing cutter Brunswick, left the ship with instruments for observing and a week's provisions.

Captain Moore had no reason to suspect that the tidal

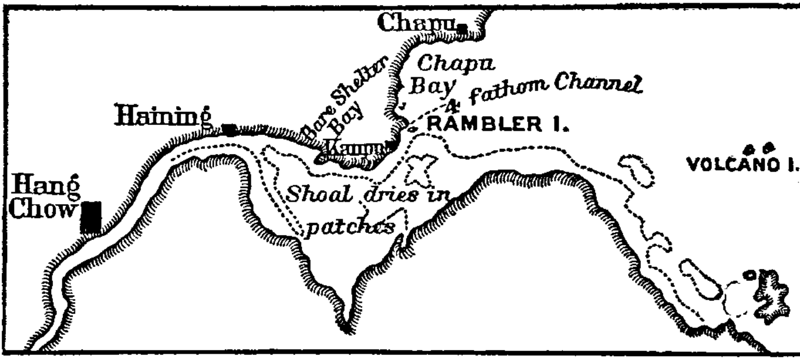

Fig. 16.—Chart of the Estuary of the Tsien-Tang-Kiang

currents would prove dangerous out in the estuary, and he proposed to go up the estuary about thirty miles to Haining, and then follow the next succeeding bore up-stream to Hangchow. Running up-stream with the flood, all went well until about 11.30, when they were about fifteen miles south-west by west of Kanpu. The leading boat, the Pandora, here grounded, and anchored quickly, but swung round violently as far as the keel would let her. The other boats, being unable to stop, came up rapidly; and the Gulnare, casting off the Brunswick, struck the Pandora, and then drove on to and over the bank, and anchored. The boats soon floated in the rising flood, and although the engines of the steam cutters were kept going full speed, all three boats dragged their anchors in an eleven-knot stream. When the flood slackened, the three boats pursued their course to the mouth of the river, where they arrived about 4 P.M. The ebb was, however, so violent that they were unable to anchor near one another. Their positions were chosen by the advice of some junkmen, who told Captain Moore, very erroneously as it turned out, that they would be safe from the night bore.

The night was calm, and at 11.29 the murmur of the bore was heard to the eastward; it could be seen at 11.55, and passed with a roar at 12.20, well over toward the opposite bank, as predicted by the Chinese. The danger was now supposed to be past; but at 1 A.M. a current of extreme violence caught the Pandora, and she had much difficulty to avoid shipwreck. In the morning it was found that her rudder-post and propeller-guard were broken, and the Brunswick and Gulnare were nowhere to be seen. They had, in fact, been in considerable danger, and had dragged their anchors three miles up the river. At 12.20 A.M. they had been struck by a violent rush of water in a succession of big ripples. In a few moments they were afloat in an eight-knot current; in ten minutes the water rose nine feet, and the boats began to drag their anchors, although the engines of the Gulnare were kept going full speed. After the boats had dragged for three miles, the rush subsided, and when the anchor was hove up the pea and the greater part of the chain were as bright as polished silver.

This account shows that all the boats were in imminent danger, and that great skill was needed to save them. After this experience and warning, the survey was continued almost entirely from the shore.

The junks which navigate the river are well aware of the dangers to which the English boats were exposed, and they have an ingenious method of avoiding them. At various places on the bank of the river there are shelter platforms, of which I show an illustration in fig. 17. Immediately after the passing of the bore the junks run up-stream with the after-rush and make for one of these shelters, where they allow themselves to be left stranded on the raised platform shown in the picture. At the end of this platform there is a sort of round tower jutting out into the stream. The object of this is to deflect the main wave of the bore so as to protect the junks from danger. After the passage of the bore, the water rises on the platform very rapidly, but the junks are just able to float in safety. Captain Moore gives a graphic account of the spectacle afforded by the junks as they go up-stream, and describes how on one occasion he saw no less than thirty junks swept up in the after-rush, at a rate of ten knots, past the town of Haining toward Hangchow, with all sail set but with their bows in every direction.

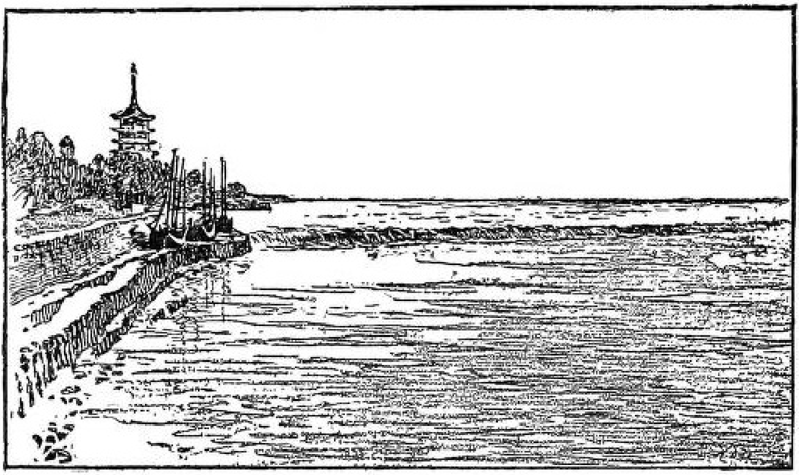

Fig. 17.—Bore-Shelter on the Tsien-Tang-Kiang

Measurements of the water-level were made in the course of the survey, and the results, in the form of a diagram, fig. 18, exhibit the nature of the bore with admirable clearness. The observations of water-level were taken simultaneously at three places, viz., Volcano Island in the estuary, Rambler Island near the mouth of the river, and Haining, twenty-six miles up the river. In the figure, the distance between the lines marked Rambler and Volcano represents fifty-one miles, and that between Rambler and Haining twenty-six miles. The vertical scales show the height of water, measured in feet, above and below the mean level of the water at these three points. The lines joining these vertical scales, marked with the hours of the clock, show the height of the water simultaneously. The hour of 8.30 is indicated by the lowest line it shows that the water was one foot below mean level at Volcano Island, twelve feet below at Rambler Island, and eight feet below at Haining. Thus the water sloped down from Haining to Rambler, and from Volcano to Rambler; the water was running up the estuary toward Rambler Island, and down the estuary to the same point. At 9 and at 9.30 there was no great change, but the water had risen two or three feet at Volcano Island and at Rambler Island. By ten o'clock the water was rising rapidly at Rambler Island, so that there was a nearly uniform slope up the river from Volcano Island to Haining. The rise at Rambler Island then continued to be very rapid, while the water at Haining remained almost stationary. This state of affairs went on until midnight, by which time the water had risen twenty-one feet at Rambler Island, and about six feet at Volcano Island, but had not yet risen at all at Haining. No doubt through the whole of this time the water was running down the river from Haining towards its mouth. It is clear that this was a state of strain which could not continue long, for there was over twenty feet of difference of level between Rambler Island, outside, and Haining, in the river. Almost exactly at midnight the strain broke down and the bore started somewhere between Rambler Island and Kanpu, and rushed up the river in a wall of water twelve feet high. This result is indicated in the figure by the presence of two lines marked "midnight." After the bore had passed there was an after-

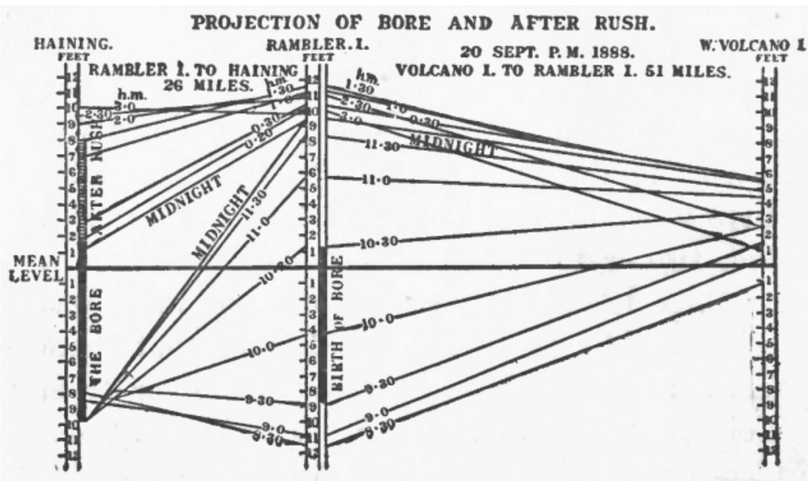

Fig. 18.—Diagram of the Flow of the Tide on the Tsien-Tang-Kiang

rush that carried the water up eight feet more. It was on this that the junks were swept up the stream, as already described. At 1.30 the after-rush was over, but the water was still somewhat higher at Rambler Island than at Haining, and a gentle current continued to set up-stream. The water then began to fall at Rambler Island, while it continued to rise at Haining up to three o'clock. At this point the ebb of the tide sets in. I do not reproduce the figure which exhibits the fall of the water in the ebbing tide, for it may suffice to say that there is no bore down-stream, although there is at one time a very violent current.

In 1892 Captain Moore succeeded, with considerable difficulty, in obtaining photographs of the bore as it passed Haining. They tell more of the violence of the wave than could be conveyed by any amount of description. The photographs, reproduced in fig. 19, do not, however, show that the broken water in the rear of the crest is often disturbed by a secondary roller, or miniature wave, which leaps up, from time to time, as if struck by some unseen force, and disappears in a cloud of spray. These breakers were sometimes twenty to thirty feet above the level of the river in front of the bore.

The upper of these pictures is from a photograph, taken at a height of twenty-seven feet above the river, as the bore passed Haining on October 10, 1892. The height of this bore was eleven feet. The lower pictures, also taken at Haining, represent the passage of the bore on October 9, 1892. The first of these photographs was taken at 1.29 P.M., and the second represents the view only one minute later.

The Chinese regard the bore with superstitious reverence, and their explanation, which I quote from Captain Moore's report, is as follows: "Many hundred years ago there was a certain general who had obtained many victories over the enemies of the Emperor, and who, being constantly successful and deservedly popular among his countrymen, excited the jealousy of his sovereign, who had for some time observed with secret wrath his growing influence. The Emperor accordingly caused him to be assassinated and thrown into the Tsien-Tang-Kiang, where his spirit conceived the idea of revenging itself by bringing the tide in from the ocean in such force as to overwhelm the city of Hangchow, then the magnificent capital of the empire. As my interpreter, who has been for some years in America, put it, 'his sowl felt a sort of ugly-like arter the many battles he had got for the Emperor.' The spirit so far succeeded as to flood a large portion of the country, when the Emperor, becoming alarmed at the distress and loss of property occasioned, endeavored to enter into a sort of compact with it by burning paper and offering food upon the sea-wall. This, however, did not have the desired effect, as the high tide came in as before; and it was at last determined to erect a pagoda at the spot where the worst breach in the embankment had been made. Hence

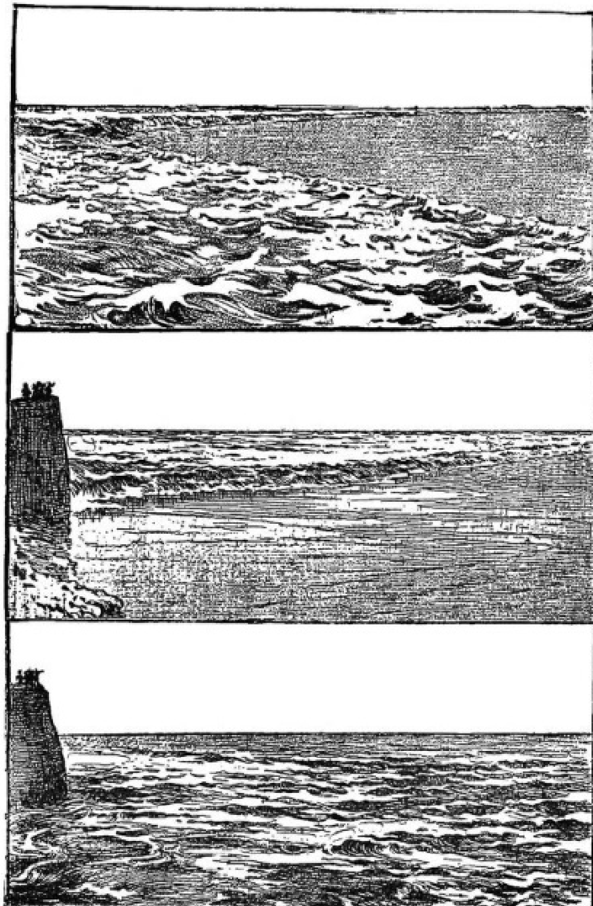

Fig. 19.—Pictures of the Bore on the Tsien-Tang-Kiang

the origin of the Bhota Pagoda. A pagoda induces the good *fungshui*, or spirit. After it was built the flood tide, though it still continued to come in the shape of a bore, did not flood the country as before."

We "foreign devils" may take the liberty of suspecting that the repairs to the embankment had also some share in this beneficial result.

This story is remarkable in that it refers to the reign of an Emperor whose historical existence is undoubted. It thus differs from many of the mythical stories which have been invented by primitive peoples to explain great natural phenomena. There is good reason to suppose, in fact, that this bore had no existence some centuries ago; for Marco Polo, in the thirteenth century, stayed about a year and a half at Hangchow, and gives so faithful and minute an account of that great town that it is almost impossible to believe that he would have omitted to notice a fact so striking. But the Emperor referred to in the Chinese legend reigned some centuries before the days of Marco Polo, so that we have reason to believe that the bore is intermittent. I have also learned from Captain Moore himself that at the time of the great Taiping rebellion, the suppression of which was principally due to "Chinese" Gordon, the intensity of the bore was far less than it is to-day. This shows that the bore is liable to great variability, according as the silting of the estuary changes.

The people at Haining still continue to pay religious reverence to the bore, and on one of the days when Captain Moore was making observations some five or six thousand people assembled on the river-wall to propitiate the god of the waters by throwing in offerings. This was the occasion of one of the highest bores at spring tide, and the rebound of the bore from the sea-wall, and the sudden heaping up of the waters as the flood conformed to the narrow mouth of the river, here barely a mile in width at low water, was a magnificent spectacle. A series of breakers were formed on the back of the advancing flood, which for over five minutes were not less than twenty-five feet above the level of the river in front of the bore. On this occasion Captain Moore made a rough estimate that a million and three quarters of tons of water passed the point of observation in one minute.

The bore of which I have given an account is perhaps the largest known; but relatively small ones are to be observed on the Severn and Wye in England, on the Seine in France, on the Petitcodiac in Canada, on the Hugli in India, and doubtless in many other places. In general, however, it is only at spring tides and with certain winds that the phenomenon is at all striking. In September, 1897, I was on the banks of the Severn at spring tide; but there was no proper bore, and only a succession of waves up-stream, and a rapid rise of water-level.

I have shown, at the beginning of this chapter, that the heading back of the sea water by the natural current of a river, and the progressive change of shape of a wave in shallow water combine to produce a rapid rise of the tide in rivers. But the explanation of the bore, as resulting from these causes, is incomplete, because it leaves their relative importance indeterminate, and serves rather to explain a rapid rise than an absolutely sudden one. I think that it would be impossible, from the mere inspection of an estuary, to say whether there would be a bore there; we could only say that the situation looked promising or the reverse.

The capriciousness of the appearance of the bore proves in fact that it depends on a very nice balance between conflicting forces, and the irregularity in the depth and form of an estuary renders the exact calculation of the form of the rising tide an impossibility. It would be easy to imitate the bore experimentally on a small scale; but, as in many other physical problems, we must rest satisfied with a general comprehension of the causes which produce the observed result.

The manner in which the Chinese avail themselves of the after-rush for ascending the river affords an illustration of the utilization by mankind of tidal energy. In going up-stream, a barge, say of one hundred tons, may rise some twenty or thirty feet. There has, then, been done upon that barge a work of from two to three thousand foot-tons. Whence does this energy come? Now, I say that it comes from the rotation of the earth; for we are making the tide do the work for us, and thus resisting the tidal movement. But resistance to the tide has the effect of diminishing the rate at which the earth is spinning round. Hence it is the earth's rotation which carries the barge up the river, and we are retarding the earth's rotation and making the day infinitesimally longer by using the tide in this way. This resistance is of an analogous character to that due to tidal friction, the consideration of which I must defer to a future chapter, as my present object is to consider the uses which may be made of tidal energy.

It has been supposed by many that when the coal supply of the world has been exhausted we shall fall back on the tides to do our work. But a little consideration will show that although this source of energy is boundless, there are other far more accessible funds on which to draw.

I saw some years ago a suggestion that the rise and fall of old hulks on the tide would afford serviceable power. If we picture to ourselves the immense weight of a large ship, we may be deluded for a moment into agreement with this project, but numerical calculation soon shows its futility. The tide takes about six hours to rise from low water to high water, and the same period to fall again. Let us suppose that the water rises ten feet, and that a hulk of 10,000 tons displacement is floating on it; then it is easy to show that only twenty horse-power will be developed by its rise and fall. We should then require ten such hulks to develop as much work as would be given by a steam engine of very moderate size, and the expense of the installation would be far better bestowed on water-wheels in rivers or on windmills. I am glad to say that the projector of this scheme gave it up when its relative insignificance was pointed out to him. It is the only instance of which I ever heard where an inventor was deterred by the impracticability of his plan.

We may, then, fairly conclude that, with existing mechanical appliances, the attempt to utilize the tide on an open coast is futile. But where a large area of tidal water can be easily trapped at high water, its fall may be made to work mill-wheels or turbines with advantage. The expense of building long jetties to catch the water is prohibitive, and therefore tide mills are only practicable where there exists an easily adaptable configuration of shoals in an estuary. There are, no doubt, many such mills in the world, but the only one which I happen to have seen is at Bembridge, in the Isle of Wight. At this place embankments formed on the natural shoals are furnished with lock-gates, and inclose many acres of tidal water. The gates open automatically with the rising tide, and the incipient outward current at the turn of the tide closes the gates again, so that the water is trapped. The water then works a mill wheel of moderate size. When we reflect on the intermittence of work from low water to high water and the great inequality of work with springs and neaps, it may be doubted whether this mill is worth the expense of retaining the embankments and lock-gates.

We see then that, notwithstanding the boundless energy of the tide, rivers and wind and fuel are likely for all time to be incomparably more important for the use of mankind.

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CHAPTER IV

HISTORICAL SKETCH

I Cannot claim to have made extensive investigations as to the ideas of mankind at different periods on the subject of the tides, but I propose in the present chapter to tell what I have been able to discover.

No doubt many mythologies contain stories explanatory of the obvious connection between the moon and the tide. But explanations, professing at least to be scientific, would have been brought forward at periods much later than those when the mythological stories originated, and I shall only speak of the former.

I have to thank my colleagues at Cambridge for the translations from the Chinese, Arabic, Icelandic, and classical literatures of such passages as they were able to discover.

I learn from Professor Giles that Chinese writers have suggested two causes for the tides: first, that water is the blood of the earth, and that the tides are the beating of its pulse; and secondly, that the tides are caused by the earth breathing. Ko Hung, a writer of the fourth century of our era, gives a somewhat obscure explanation of spring and neap tides. He says that every month the sky moves eastward and then westward, and hence the tides are greater and smaller alternately. Summer tides are said to be higher than winter tides, because in summer the sun is in the south and the sky is 15,000 li (5,000 miles) further off, and therefore in summer the female or negative principle in nature is weak, and the male or positive principle strong.

In China the diurnal inequality is such that in summer the tide rises higher in the daytime than in the night, whilst the converse is true in winter. I suggest that this fact affords the justification for the statement that the summer tides are great.

Mr. E. G. Browne has translated for me the following passage from the "Wonders of Creation" of Zakariyyā ibn Muhammad ibn Mahmūd al Qazvīnī, who died in A.D. 1283.[1]

"Section treating of certain wonderful conditions of the sea.

"Know that at different periods of the four seasons, and on the first and last days of the months, and at certain hours of the night and day, the seas have certain conditions as to the rising of their waters and the flow and agitation thereof.

"As to the rising of the waters, it is supposed that when the sun acts on them it rarefies them, and they expand and seek a space ampler than that wherein they were before, and

[1] Wüstenfeld's edition, pp. 103, 104.

the one part repels the other in the five directions eastwards, westwards, southwards, northwards, and upwards, and there arise at the same time various winds on the shores of the sea. This is what is said as to the cause of the rising of the waters.

"As for the flow of certain seas at the time of the rising of the moon, it is supposed that at the bottom of such seas there are solid rocks and hard stones, and that when the moon rises over the surface of such a sea, its penetrating rays reach these rocks and stones which are at the bottom, and are then reflected back thence; and the waters are heated and rarefied and seek an ampler space and roll in waves towards the seashore ... and so it continues as long as the moon shines in mid-heaven. But when she begins to decline, the boiling of the waters ceases, and the particles cool and become dense and return to their state of rest, and the currents run according to their wont. This goes on until the moon reaches the western horizon, when the flow begins again, as it did when the moon was in the eastern horizon. And this flow continues until the moon is at the middle of the sky below the horizon, when it ceases. Then when the moon comes upward, the flow begins again until she reaches the eastern horizon. This is the account of the flow and ebb of the sea.

"The agitation of the sea resembles the agitation of the humours in men's bodies, for verily as thou seest in the case of a sanguine or bilious man, &c., the humours stirring in his body, and then subsiding little by little; so likewise the sea has matters which rise from time to time as they gain strength, whereby it is thrown into violent commotion which subsides little by little. And this the Prophet (on whom be the blessings of God and his peace) hath expressed in a poetical manner, when he says: 'Verily the Angel, who is set over the seas, places his foot in the sea and thence comes the flow; then he raises it and thence comes the ebb.'"

Mr. Magnússon has kindly searched the old Icelandic literature for references to the tides. In the Rimbegla he finds this passage:—

"Beda the priest says that the tides follow the moon, and that they ebb through her blowing on them, but wax in consequence of her movement."

And again:—

"(At new moon) the moon stands in the way of the sun and prevents him from drying up the sea; she also drops down her own moisture. For both these reasons, at every new moon, the ocean swells and makes those tides which we call spring tides. But when the moon gets past the sun, he throws down some of his heat upon the sea, and diminishes thereby the fluidity of the water. In this way the tides of the sea are diminished."

In another passage the author writes:—

"But when the moon is opposite to the sun, the sun heats the ocean greatly, and as nothing impedes that warmth, the ocean boils and the sea flood is more impetuous than before—just as one may see water rise in a kettle when it boils violently. This we call spring tide."

There seems to be a considerable inconsistency in explaining one spring tide by the interception of the sun's heat by the moon, and the next one by the excess of that heat.

But it is not necessary to search ancient literature for grotesque theories of the tides. In 1722 E. Barlow, gentleman, in "An Exact Survey of the Tide,"[1] attributes it to the pressure of the moon on the atmosphere. And theories not less absurd have been promulgated during the last twenty years.

The Greeks and Romans, living on the shores of the Mediterranean, had not much occasion to learn about the tide, and the passages in classical literature which treat of this matter are but few. But where the subject is touched on we see clearly their great intellectual superiority over those other peoples, whose ideas have just been quoted.

[1] "The Second Edition, with Curious Maps." (London: John Hooke, 1722.)

The only author who treats of the tide in any detail is Posidonius, and we have to rely for our knowledge of his work entirely on quotations from him by Strabo.[1]

Posidonius says that Aristotle attributed the flow and ebb of the sea at Cadiz to the mountainous formation of the coast, but he very justly pronounces this to be nonsense, particularly as the coast of Spain is flat and sandy. He himself attributes the tides to the moon's influence, and the accuracy of his observations is proved by the following interesting passage from Strabo:[2]—

"Posidonius says that the movement of the ocean observes a regular series like a heavenly body, there being a daily, monthly, and yearly movement according to the influence of the moon. For when the moon is above the (eastern) horizon by the distance of one sign of the zodiac (i. e. 30°) the sea begins to flow, and encroaches visibly on the land until the moon reaches the meridian. When she has passed the meridian, the sea in turn ebbs gradually, until the moon is

[1] My attention was drawn to Strabo by a passage in Sir W. Thomson's (Lord Kelvin's) Popular Lectures, *The Tides*, vol. ii. I have to thank Mr. Duff for the translations which follow from Strabo and Posidonius. The work consulted was Bake's *Posidonius* (Leiden, 1810), but Mr. Duff tells me that the text is very corrupt in some places, and he has therefore also consulted a more recent text.

[2] Teubner's *Strabo*, i. p. 236.

above the western horizon by the distance of one sign of the zodiac. The sea then remains motionless while the moon is actually setting, and still more so (*sic*) so long as the moon is moving beneath the earth as far as a sign of the zodiac beneath the horizon. Then the sea again advances until the moon has reached the meridian below the earth; and retreats while the moon is moving towards the east, until she is the distance of a sign of the zodiac below the horizon; it remains at rest until the moon is the same distance above the horizon, and then begins to flow again. Such is the daily movement of the tides, according to Posidonius.

"As to their monthly movement, he says that the ebbs are greatest at the conjunctions [of the sun and moon], and then grow less until the time of half moon, and increase again until the time of full moon, and grow less again until the moon has waned to half. Then the increase of the tide follows until the conjunction. But the increases last longer and come quicker [this phrase is very obscure].

"The yearly movements of the tides he says he learned from the people of Cadiz. They told him that the ebb and flow alike were greatest at the summer solstice. He guesses for himself that the tides grow less from the solstice to the equinox, and then increase between the equinox and the winter solstice, and then grow less until the spring equinox, and greater until the summer solstice."

This is an excellent account of the tides at Cadiz, but I doubt whether there is any foundation for that part which was derived from hearsay. Lord Kelvin remarks, however, that it is interesting to note that inequalities extending over the year should have been recognized.

Strabo also says that there was a spring near Cadiz in which the water rose and fell, and that this was believed by the inhabitants, and by Polybius, to be due to the influence of the ocean tide, but Posidonius was not of this opinion. Strabo says:—

"Posidonius denies this explanation. He says there are two wells in the precinct of Hercules at Cadiz, and a third in the city. Of the two former the smaller runs dry while people are drawing water from it, and when they stop drawing water it fills again; the larger continues to supply water all day, but, like all other wells, it falls during the day but is replenished at night, when the drawing of water has ceased. But since the ebb tide often coincides with the replenishing of the well, therefore, says Posidonius, the idle story of the tidal influence has been believed by the inhabitants."

Since the wells follow the sun, whilst the tide follows the moon, the criticism of Posidonius is a very just one. But Strabo blames him for distrusting the Cadizians in a simple matter of everyday experience, whilst accepting their evidence as to an annual inequality in the tides.

There is another very interesting passage in Strabo, the meaning of which was obviously unknown to the Dutch commentator Bake—and indeed must necessarily have been unintelligible to him at the time when he wrote, on account of the then prevailing ignorance of tidal phenomena in remoter parts of the world. Strabo writes:—

"Anyhow Posidonius says that Seleucus of the Red Sea [also called the Babylonian] declares that there is a certain irregularity and regularity in these phenomena [the tides], according to the different positions [of the moon] in the zodiac. While the moon is in the equinoctial signs, the phenomena are regular; but while she is in the signs of the solstices, there is irregularity both in the height and speed of the tides, and in the other signs there is regularity or the reverse in proportion to their nearness to the solstices or to the equinoxes."

Now let us consider the meaning of this. When the moon is in the equinoxes she is on the equator, and when she is in the solstices she is at her maximum distances to the north or south of the equator—or, as astronomers say, in her greatest north or south declination. Hence Seleucus means that, when the moon is on the equator, the tides follow one another, with two equal high and low waters a day; but when she is distant from the equator, the regular sequence is interrupted. In other words, the diurnal inequality (which I shall explain in a later chapter) vanishes when the moon is on the equator, and is at its maximum when the declination is greatest. This is quite correct, and since the diurnal inequality is almost evanescent in the Atlantic, whilst it is very great in the Indian Ocean, especially about Aden, it is clear that Seleucus had watched the sea there, just as we should expect him to do from his place of origin.

Many centuries elapsed after the classical period before any scientific thought was bestowed on the tides. Kepler recognized the tendency of the water on the earth to move towards the sun and the moon, but he was unable to submit his theory to calculation. Galileo expresses his regret that so acute a man as Kepler should have produced a theory, which appeared to him to reintroduce the occult qualities of the ancient philosophers. His own explanation referred the phenomenon to the rotation of the earth, and he considered that it afforded a principal proof of the Copernican system.

The theory of tide-generating force which will be set forth in Chapter V. is due to Newton, who expounded it in his "Principia" in 1687. His theory affords the firm basis on which all subsequent work has been laid.

In 1738 the Academy of Sciences of Paris offered the theory of the tides as the subject for a prize. The authors of four essays received prizes, viz., Daniel Bernoulli, Euler, Maclaurin, and Cavalleri. The first three adopted, not only the theory of gravitation, but also Newton's theory to its fullest extent. A considerable portion of Bernoulli's work is incorporated in the account of the theory of the tides which I shall give later. The essays of Euler and Maclaurin contained remarkable advances in mathematical knowledge, but did not add greatly to the theory of the tides. The Jesuit priest Cavalleri adopted the theory of vortices to explain the tides, and it is not worth while to follow him in his erroneous and obsolete speculations.

Nothing of importance was added to our knowledge until the great French mathematician Laplace took up the subject in 1774. It was he who for the first time fully recognized the difficulty of the problem, and showed that the earth's rotation is an essential feature in the conditions. The actual treatment of the tidal problem is in effect due to Laplace, although the mode of presentment of the theory has come to differ considerably from his.

Subsequently to Laplace, the most important workers in this field have been Sir John Lubbock senior, Whewell, Airy, and Lord Kelvin. The work of Lubbock and Whewell is chiefly remarkable for the coördination and analysis of enormous masses of data at various ports, and the construction of trustworthy tide tables. Airy contributed an important review of the whole tidal theory. He also studied profoundly the theory of waves in canals, and considered the effects of frictional resistances on the progress of tidal and other waves.

Lord Kelvin initiated a new and powerful method of considering tidal oscillations. His method possesses a close analogy with that already used in discussing the irregularities in the motions of the moon and planets. His merit consists in the clear conception that the plan of procedure which has been so successful in the one case would be applicable to the other. The difference between the laws of the moon's motion and those of tidal oscillations is, however, so great that there is scarcely any superficial resemblance between the two methods. This so-called "harmonic analysis" of the tides is daily growing in favor in the eyes of men of science, and is likely to supersede all the older methods. I shall explain it in a future chapter.

Amongst all the grand work which has been bestowed on this difficult subject, Newton stands out first, and next to him we must rank Laplace. However original any future contribution to the science of tides may be, it would seem as though it must perforce be based on the work of these two. The exposition which I shall give hereafter of the theory of oceanic tides is based on the work of Newton, Bernoulli, Laplace, and Kelvin, in proportions of which it would be difficult to assign the relative importance.

The connection between the moon and the tide is so obvious that long before the formulation of a satisfactory theory fairly accurate predictions of the tides were made and published. On this head Whewell[1] has the following interesting passage:—

"The course which analogy would have recommended for the cultivation of our knowledge of tides would have been to ascertain by an analysis of long series of observations, the effects of changes in the time of transit, parallax, and declination of the moon, and thus to obtain the laws of phenomena; and then to proceed to investigate the laws of causation.

"Though this was not the course followed by mathematical theorists, it was really pursued by those who practically calculated tide tables; and the application of knowledge to the useful purposes of life, being thus separated from the promotion of the theory, was naturally treated as a gainful property, and preserved by secrecy. . . . Liverpool, London, and other places, had their tide tables, constructed by undivulged methods, which methods, in some instances at least, were handed down from father to son for several generations as a family possession; and the publication of new tables accompanied by a statement of the mode of calculation was resented as an infringement of the rights of property.

[1] *History of the Inductive Sciences*, 1837, vol. ii. p. 248 *et seq.*

"The mode in which these secret methods were invented was that which we have pointed out,—the analysis of a considerable series of observations. Probably the best example of this was afforded by the Liverpool tide tables. These were deduced by a clergyman named Holden, from observations made at that port by a harbor master of the name of Hutchinson, who was led, by a love of such pursuits, to observe the tides for above twenty years, day and night. Holden's tables, founded on four years of these observations, were remarkably accurate.

"At length men of science began to perceive that such calculations were part of their business; and that they were called upon, as the guardians of the established theory of the universe, to compare it in the greatest possible detail with the facts. Mr. Lubbock was the first mathematician who undertook the extensive labors which such a conviction suggested. Finding that regular tide observations had been made at the London docks from 1795, he took nineteen years of these (purposely selecting the length of the cycle of the motions of the lunar orbit), and caused them (in 1831) to be analyzed by Mr. Dessiou, an expert calculator. He thus obtained tables for the effect of the moon's declination, parallax, and hour of transit, on the tides; and was enabled to produce tide tables founded upon the data thus obtained. Some mistakes in these as first published (mistakes unimportant as to the theoretical value of the work) served to show the jealousy of the practical tide table calculators, by the acrimony with which the oversights were dwelt upon; but in a very few years the tables thus produced by an open and scientific process were more exact than those which resulted from any of the secrets; and thus practice was brought into its proper subordination to theory."

Authorities.

The history from Galileo to Laplace is to be found in the *Mécanique Céleste* of Laplace, book xiii, chapter i.

The other authorities are quoted in the text or in footnotes.

CHAPTER V

TIDE-GENERATING FORCE

It would need mathematical reasoning to fully explain how the attractions of the sun and moon give rise to tide-generating forces. But as this book is not intended for the mathematician, I must endeavor to dispense with technical language.

A body in motion will move in a straight line, unless it is deflected from its straight path by some external force, and the resistance to the deflection is said to be due to inertia. The motion of the body then is equivalent in its effect to a force which opposes the deflection due to the external force, and in many cases it is permissible to abstract our attention from the motion of the system and to regard it as at rest, if at the same time we introduce the proper ideal forces, due to inertia, so that they shall balance the action of the real external forces.

If I tie a string to a stone and whirl it round, the string is thrown into a state of tension. The natural tendency of the stone, at each instant, is to move onward in a straight line, but it is continuously deflected from its straight path by the tension of the string. In this case the ideal force, due to inertia, whereby the stone resists its continuous deflection, is called centrifugal force. This force is in reality only a substitute for the motion, but if we withdraw our attention from the motion, it may be regarded as a reality.

The centrifugal force is transmitted to my hand through the string, and I thus experience an outward or centrifugal tendency. But the stone itself is continually pulled inward by the string, and the force is called centripetal. When a string is under tension, as in this experiment, it is subject to equal and opposite forces, so that the tension implies the existence of a pair of forces, one towards and the other away from the centre of rotation. The force is to be regarded as away from the centre when we consider the sensation of the whirler, and as towards the centre when we consider the thing whirled. A similar double view occurs in commerce, where a transaction which stands on the credit side in the books of one merchant appears on the debit side in the books of the other.

This simple experiment exemplifies the mechanism by which the moon is kept revolving round the earth. There is not of course any visible connection between the two bodies, but an invisible bond is provided by the attraction of gravity, which replaces the string which unites the stone to the hand. The moon, then, whirls round the earth at just such a rate and at just such a distance, that her resistance to circular motion, called centrifugal force, is counterbalanced by the centripetal tendency of gravity. If she were nearer to us the attraction of gravity would be greater, and she would have to go round the earth faster, so as to make enough centrifugal force to counterbalance the greater gravity. The converse

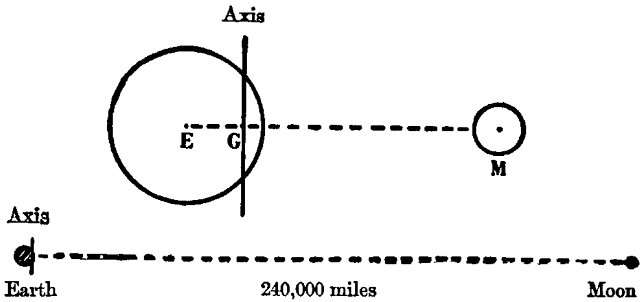

FIG. 20.—EARTH AND MOON

would be true, and the moon would go round slower, if she were further from us.

The moon and the earth go round the sun in companionship once in a year, but this annual motion does not affect the interaction between them, and we may put aside the orbital motion of the earth, and suppose the moon and earth to be the only pair of bodies in existence. When the principle involved in a purely lunar tide is grasped, the action of the sun in producing a solar tide will become obvious. But the analogy of the string and stone is imperfect in one respect where the distinction is important; the moon, in fact, does not revolve exactly about the earth, but about the centre of gravity of the earth and moon. The earth is eighty times as heavy as the moon, and so this centre of gravity is not very far from the earth's centre. The upper part of fig. 20 is intended to represent a planet and its satellite; the lower part shows the earth and the moon in their true proportions. The upper figure is more convenient for our present argument, and the planet and satellite may be described as the earth and the moon, notwithstanding the exaggeration of their relative proportions. The point G is the centre of gravity of the two, and the axis about which they revolve passes through G. This point is sufficiently near to the centre of the earth to permit us, for many purposes, to speak of the moon as revolving round the earth. But in the present case we must be more accurate and must regard the moon and earth as revolving round G, their centre of gravity. The moon and earth are on opposite sides of this point, and describe circles round it. The distance of the moon's centre from G is 237,000 miles, whilst that of the earth's centre is only 3,000 miles in the opposite direction. The 3,000 and 237,000 miles together make up the 240,000 miles which separate the centres of the two bodies.

A system may now be devised so as to resemble the earth and moon more closely than that of the string and stone with which I began. If a large stone and a small one are attached to one another by a light and stiff rod, the system can be balanced horizontally about a point in the rod called the centre of gravity G. The two weights may then be set whirling about a pivot at G, so that the rod shall always be horizontal. In consequence of the rotation the rod is brought into a state of stress, just as was the string in the first example, and the centripetal stress in the rod exactly counterbalances the centrifugal force. The big and the little stones now correspond to the earth and the moon, and the stress in the rod plays the same part as the invisible bond of gravity between the earth and the moon. Fixing our attention on the smaller stone or moon at the end of the longer arm of the rod, we see that the total centrifugal force acting on the moon, as it revolves round the centre of gravity, is equal and opposite to the attraction of the earth on the moon. On considering the short arm of the rod between the pivot and the big stone, we see also that the centrifugal force acting on the earth is equal and opposite to the attraction of the moon on it. In this experiment as well as in the former one, we consider the total of centrifugal force and of attraction, but every particle of both the celestial bodies is acted on by these forces, and accordingly a closer analysis is necessary.

It will now simplify matters if we make a supposition which departs from actuality, introducing the true conditions at a later stage in the argument.

The earth's centre describes a circle about the centre of gravity G, with a radius of 3,000 miles, and the period of the revolution is of course one month. Now whilst this motion of revolution of the earth's centre continues, let it be supposed that the diurnal rotation is annulled. As this is a mode of revolution which differs from that of a wheel, it is well to explain exactly what is meant by the annulment of the diurnal rotation. This is illustrated in fig. 21, which shows the successive positions assumed by an arrow in revolution without rotation. The shaft of the arrow always remains parallel to the same direction in space, and therefore it does not rotate, although the whole arrow revolves. It is obvious that every particle of the arrow describes a circle of the same radius, but that the circles described by them are not concentric. The circles described by the point and by the base of the arrow are shown in the figure, and their centres are separated by a distance equal to the length of the arrow. Now the centrifugal force on a revolving particle acts along the radius of the circle described, and in this case the radii of the circles described by any two particles in the arrow are always parallel. The parallelism of the centrifugal forces at the two ends of the arrow is indicated in the figure. Then again, the centrifugal force must everywhere be equal as well as parallel, because its intensity depends both on the radius and on the speed of revolution, and these are the same for every part. It follows that if a body revolves without rotation, every part of it is subject to equal and parallel centrifugal forces. The same must therefore be true of the earth when deprived of diurnal rotation. Accordingly every particle of the idealized non-rotating earth is continuously subject to equal and parallel centrifugal forces, in consequence of the revolution of the earth's centre in its monthly orbit with a radius of 3,000 miles.[1]

We have seen that the total of centrifugal force acting on the whole earth must be just such as to balance the total of the centripetal forces due to the moon's attraction. If, then, the attractional forces, acting on every particle of the earth, were also equal and parallel, there would be a perfect balance throughout. We shall see, however, that although there is a perfect balance on the whole, there is not that uniformity which would render the balance perfect at every particle.

As far as concerns the totality of the attraction the analogy is complete between the larger stone, revolving at the end of the shorter arm of the rod, and the earth revolving in its small orbit round G. But a difference arises when we compare the distribution of the tension of the rod with that of

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[1] I owe the suggestion of this method of presenting the origin of tide-generating force to Professor Davis of Harvard University.

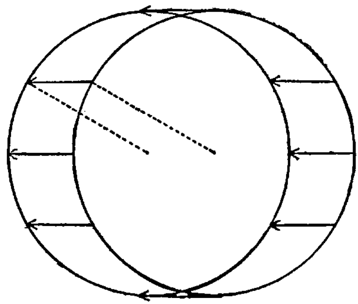

Fig. 21.—Revolution of a Body without Rotation

the lunar attraction; for the rod only pulls at the stone at the point where it is attached to it, whereas the moon attracts every particle of the earth. She does not, however, attract every particle with equal force, for she pulls the nearer parts more strongly than the further, as is obvious from the nature of the law of gravitation. The earth's centre is distant sixty times its radius from the moon, so that the nearest and furthest parts are distant fifty-nine and sixty-one radii respectively. Hence the attractions at the nearest and furthest parts differ only a little from the average, namely, that at the centre; but it is just these small differences which are important in this matter.

Since on the whole the attractions and the centrifugal forces are equal and opposite, and since the centrifugal forces acting on the non-rotating earth are equal and parallel at every part, and since the attraction at the earth's centre is the average attraction, it follows that where the attraction is stronger than the average it overbalances the centrifugal force, and where it is weaker it is overbalanced thereby.

The result of the contest between the two sets of forces is illustrated in fig. 22. The circle represents a section of the earth, and the moon is a long way off in the direction M.

Since the moon revolves round the earth, whilst the earth is still deprived of rotation, the figure only shows the state of affairs at a definite instant of time. The face which the earth exhibits to the moon is always changing, and the moon returns to the same side of the earth only at the end of the month. Hence the section of the earth shown in this figure always passes through the moon, while it is continually shifting with respect to the solid earth. The arrows in the figure show by their directions and lengths the magnitudes and directions of the overbalance in the contest between centrifugal and centripetal tendencies. The point V in the figure is the middle of the hemisphere, which at the moment portrayed faces full towards the moon. It is the middle of the round disk which the man in the moon looks at. The middle of the face invisible to the man in the moon is at I. The point of the earth which is only fifty-nine earth's radii from the moon is at V. Here attraction overbalances centrifugal force,

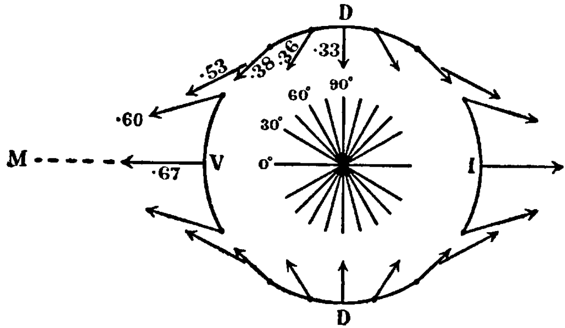

Fig. 22.—Tide-Generating Force

and this is indicated by an arrow pointing towards the moon. The point distant sixty-one earth's radii from the moon is at I, and attraction is here overbalanced, as indicated by the arrow pointing away from the moon.

I shall have to refer hereafter to the intensities of these forces, and will therefore here pause to make some numerical calculations.

The moon is distant from the earth's centre sixty times the earth's radius, and the attraction of gravity varies inversely as the square of the distance. Hence we may take $\frac{1}{60^2}$ or $\frac{1}{3,600}$ as a measure of the intensity of the moon's attraction at the earth's centre. The particle which occupies the centre of the earth is also that particle which is at the average distance of all the particles constituting the earth's mass. Hence $\frac{1}{60^2}$ or $\frac{1}{3{,}600}$ may be taken as a measure of the average attraction of the moon on every particle of the earth.

Now the point V is only distant fifty-nine earth's radii from the moon, and therefore, on the same scale, the moon attraction is measured by $\frac{1}{59^2}$ or $\frac{1}{3{,}481}$.

The attraction therefore at V exceeds the average by $\frac{1}{59^2} - \frac{1}{60^2}$, or $\frac{1}{3{,}481} - \frac{1}{3{,}600}$. It will be well to express these results in decimals; now $\frac{1}{3{,}481}$ is .000,287,27, and $\frac{1}{3{,}600}$ is .000,277,78, so that the difference is .000,009,49. It is important to notice that $\frac{2}{60^3}$ or $\frac{2}{216{,}000}$ is equal to .000,009,26; so that the difference is nearly equal to $\frac{2}{60^3}$.

Again, the point I is distant sixty-one earth's radii from the moon, and the moon's attraction there is to be measured by $\frac{1}{61^2}$ or $\frac{1}{3{,}721}$. The attraction at I therefore falls below the average by $\frac{1}{60^2} - \frac{1}{61^2}$, or $\frac{1}{3{,}600} - \frac{1}{3{,}721}$; that is, by .000,277,78 − .000,268,75, which is equal to .000,009,03. This again does not differ much from $\frac{2}{60^3}$.

These calculations show that the excess of the actual attraction at V above the average attraction is nearly equal to the excess of the average above the actual attraction at I. These two excesses only differ from one another by 5 per cent. of either, and they are both approximately equal to $\frac{2}{60^3}$ on the adopted scale of measurement.

The use of any particular scale of measurement is not material to this argument, and we should always find that the two excesses are nearly equal to one another. And further, if the moon were distant from the earth by any other number of earth's radii, we should find that the two excesses are each nearly equal to 2 divided by the cube of that number.[1]

We conclude then that the two overbalances at V and I, which will be called tide-generating forces, are nearly equal to one another, and vary inversely as the cube of the distance of the moon from the earth.

The fact of the approximate equality of the overbalance or excess on the two sides of the earth is noted in the figure by two arrows at V and I of equal lengths. The argument would be a little more complicated, if I were to attempt to follow the mathematician in his examination of the whole surface of the earth, and to trace from point to point how the balance between the opposing forces turns. The reader

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[1]This argument is very easily stated in algebraic notation. If x be the number of earth's radii at which the moon is placed, the points V and I are respectively distant $x-1$ and $x+1$ radii. Now $(x-1)^2$ is nearly equal to x^2-2x or to $x^2(1-\frac{2}{x})$, and therefore $\frac{1}{(x-1)^2}$ is nearly equal to $\frac{1}{x^2(1-\frac{2}{x})}$, which is nearly equal to $\frac{1}{x^2}(1+\frac{2}{x})$. Hence $\frac{1}{(x-1)^2}-\frac{1}{x^2}$ is nearly equal to $\frac{2}{x^3}$. By a similar argument $(x+1)^2$ is nearly equal to $x^2(1+\frac{2}{x})$, and $\frac{1}{(x+1)^2}$ is nearly equal to $\frac{1}{x^2}(1-\frac{2}{x})$; so that $\frac{1}{x^2}-\frac{1}{(x+1)^2}$ is nearly equal to $\frac{2}{x^3}$.

must accept the results of such an analysis as shown in fig. 22 by the directions and lengths of the arrows.

We have already seen that the forces at V and I, the middles of the faces of the earth which are visible and invisible to the man in the moon, are directed away from the earth's centre. The edges of the earth's disk as seen from the moon are at D and D, and here the arrows point inwards to the earth's centre and are half as long as those at V and I. At intermediate points, they are intermediate both in size and direction.

The only point in which the system considered differs from actuality is that the earth has been deprived of rotation. But this restriction may be removed, for, when the earth rotates once in 24 hours, no difference is made in the forces which I have been trying to explain, although of course the force of gravity and the shape of the planet are affected by the rotation. This figure is called a diagram of tide-generating forces, because the tides of the ocean are due to the action of this system of forces.

The explanation of tide-generating force is the very kernel of our subject, and, at the risk of being tedious, I shall look at it from a slightly different point of view. If every particle of the earth and of the ocean were acted on by equal and parallel forces, the whole system would move together and the ocean would not be displaced relatively to the earth; we should say that the ocean was at rest. If the forces were not quite equal and not quite parallel, there would be a slight residual effect tending to make the ocean move relatively to the solid earth. In other words, any defect from equality and parallelism in the forces would cause the ocean to move on the earth's surface.

The forces which constitute the departure from equality and parallelism are called "tide-generating forces," and it is this system which is indicated by the arrows in fig. 22. Tide-generating force is, in fact, that force which, superposed on the average force, makes the actual force. The average direction of the forces which act on the earth, as due to the moon's attraction, is along the line joining the earth's centre to the moon's centre, and its average intensity is equal to the force at the earth's centre.

Now at V the actual force is straight towards M, in the same direction as the average, but of greater intensity. Hence we find an arrow directed towards M, the moon. At I, the actual force is again in the same direction as, but of less intensity than, the average, and the arrow is directed away from M, the moon. At D, the actual force is almost exactly of the same intensity as the average, but it is not parallel thereto, and we must insert an inward force as shown by the arrow, so that when this is compounded with the average force we may get a total force in the right direction.

Now let us consider how these forces tend to affect an ocean lying on the surface of the earth. The moon is directly over the head of an inhabitant of the earth, that is to say in his zenith, when he is at V; she is right under his feet in the nadir when he is at I; and she is in the observer's horizon, either rising or setting, when he is anywhere on the circle D. When the inhabitant is at V or at I he finds that the tide-generating force is towards the zenith; when he is anywhere on the circle D he finds it towards the nadir. At other places he finds it directed towards or away from some point in the sky, except along two circles halfway between V and D, or between I and D, where the tide-generating force is level along the earth's surface, and may be called horizontal.

A vertical force cannot make things move sideways, and so the sea will not be moved horizontally by it. The vertical part of the tide-generating force is not sufficiently great to overcome gravity, but will have the effect of making the water appear lighter or heavier. It will not, however, be effective in moving the water, since the water must remain in contact with the earth. We want, then, to omit the vertical part of the force and leave behind only the horizontal part, by which I mean a force which, to an observer on the earth's surface, is not directed either upwards or downwards, but along the level to any point of the compass.

If there be a force acting at any point of the earth's surface, and directed upwards or downwards away from or towards some point in the sky other than the zenith, it may be decomposed into two forces, one vertically upwards or downwards, and another along the horizontal surface. Now as concerns the making of the tides, no attention need be paid to that part which is directed straight up or down, and the only important part is that along the surface,—the horizontal portion.

Taking then the diagram of tide-generating forces in fig. 22, and obliterating the upward and downward portions of the force, we are left with a system of forces which may be represented by the arrows in the perspective picture of horizontal tide-generating force shown in fig. 23.

If we imagine an observer to wander over the earth, V is the place at which the moon is vertically over his head, and the circle D, shown by the boundary of the shadow, passes through all the places at which the moon is in the horizon, just rising or setting. Then there is no horizontal force where the moon is over his head or under his feet, or where the moon is in his horizon either rising or setting, but everywhere else there is a force directed along the surface of the earth in the direction of the point at which the moon is straight overhead or underfoot.

Now suppose P to be the north pole of the earth, and that the circle A_1, A_2, A_3, A_4, A_5 is a parallel of latitude—say the

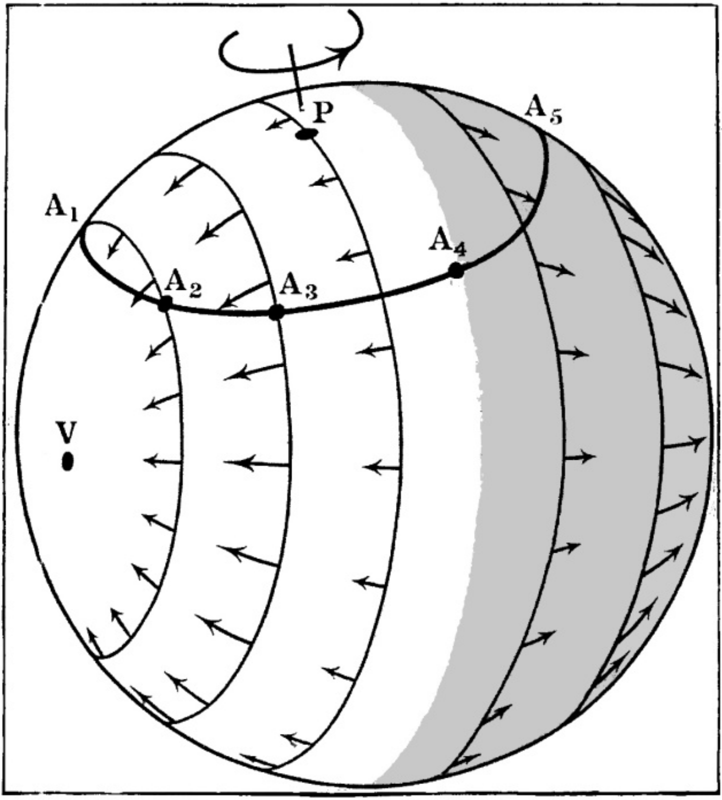

FIG. 23.—HORIZONTAL TIDE-GENERATING FORCE

latitude of London. Then if we watch our observer from external space, he first puts in an appearance on the picture at A_1, and is gradually carried along to A_2 by the earth's rotation, and so onwards. Just before he comes to A_2, the moon is due south of him, and the tide-generating force is also south, but not very large. It then increases, so that nearly three hours later, when he has arrived at A_3, it is considerably greater. It then wanes, and when he is at A_4 the moon is setting and the force is nil. After the moon has set, the force is directed towards the moon's antipodes, and it is greatest about three hours after moonset, and vanishes when the moon, still being invisible, is on the meridian.

It must be obvious from this discussion that the lunar horizontal tide-generating force will differ, both as to direction and magnitude, according to the position of the observer on the earth and of the moon in the heavens, and that it can only be adequately stated by means of mathematical formulæ. I shall in the following chapter consider the general nature of the changes which the forces undergo at any point on the earth's surface.

But before passing on to that matter it should be remarked that if the earth and sun had been the only pair of bodies in existence the whole of the argument would have applied equally well. Hence it follows that there is also a solar tide-generating force, which in actuality coëxists with the lunar force. I shall hereafter show how the relative importance of these two influences is to be determined.

Authorities.

Any mathematical work on the theory of the tides; for example, Thomson and Tait's *Natural Philosophy*, Lamb's *Hydrodynamics*, Bassett's *Hydrodynamics*, article *Tides*, "Encycl. Britan.," Laplace's *Mécanique Céleste*, &c.

CHAPTER VI

DEFLECTION OF THE VERTICAL

The intensity of tide-generating force is to be estimated by comparison with some standard, and it is natural to take as that standard the force of gravity at the earth's surface. Gravity acts in a vertical direction, whilst that portion of the tidal force which is actually efficient in disturbing the ocean is horizontal. Now the comparison between a small horizontal force and gravity is easily effected by means of a pendulum. For if the horizontal force acts on a suspended weight, the pendulum so formed will be deflected from the vertical, and the amount of deflection will measure the force in comparison with gravity. A sufficiently sensitive spirit level would similarly show the effect of a horizontal force by the displacement of the bubble. When dealing with tidal forces the displacements of either the pendulum or the level must be exceedingly minute, but, if measurable, they will show themselves as a change in the apparent direction of gravity. Accordingly a disturbance of this kind is often described as a deflection of the vertical.

The maximum horizontal force due to the moon may be shown by a calculation, which involves the mass and distance of the moon, to have an intensity of $\frac{1}{11,660,000}$ of gravity.[1] Such a force must deflect the bob of a pendulum by the same fraction of the length of the cord by which it is suspended.

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[1] It does not occur to me that there is any very elementary method of computing the maximum horizontal tidal force, but it is easy to calculate the vertical force at the points V or I in fig. 22.

The moon weighs $\frac{1}{80}$ of the earth, and has a radius $\frac{1}{4}$ as large. Hence lunar gravity on the moon's surface is $\frac{1}{80} \times 4^2$, or $\frac{1}{5}$ of terrestrial gravity at the earth's surface. The earth's radius is 4,000 miles and the moon's distance from the earth's centre 240,000 miles. Hence her distance from the nearer side of the earth is 236,000 miles. Therefore lunar gravity at the earth's centre is $\frac{1}{5} \times \frac{1}{240^2}$ of terrestrial gravity, and lunar gravity at the point V is $\frac{1}{5} \times \frac{1}{236^2}$ of the same. Therefore the tidal force at V is $\frac{1}{5} \times \frac{1}{236^2} - \frac{1}{5} \times \frac{1}{240^2}$ of terrestrial gravity. On multiplying the squares of 236 and of 240 by 5, we find that this difference is $\frac{1}{278,480} - \frac{1}{288,000}$. If these fractions are reduced to decimals and the subtraction is performed, we find that the force at V is .000,000,118,44 of terrestrial gravity. When this decimal is written as a fraction, we find the result to be $\frac{1}{8,450,000}$ of gravity.

Now it is the fact, although I do not see how to prove it in an equally elementary manner, that the maximum horizontal tide-generating force has an intensity equal to $\frac{3}{4}$ of the vertical force at V or I. To find $\frac{3}{4}$ of the above fraction we must augment the denominator by one third part. Hence the maximum horizontal force is $\frac{1}{11,260,000}$ of gravity. This number does not agree exactly with that given in the text; the discrepancy is due to the fact that round numbers have been used to express the sizes and distance apart of the earth and the moon, and their relative masses.

If therefore the string were 10 metres or 33 feet in length, the maximum deflection of the weight would be $\frac{1}{11,660,000}$ of 10 metres, or $\frac{1}{1,166}$ of a millimetre. In English measure this is $\frac{1}{29,000}$ of an inch. But the tidal force is reversed in direction about every six hours, so that the pendulum will depart from its mean direction by as much in the opposite direction. Hence the excursion to and fro of the pendulum under the

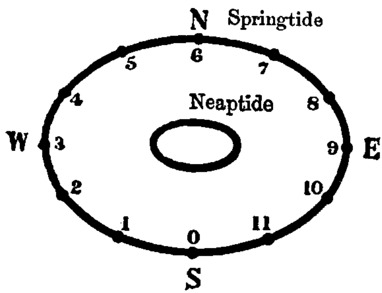

Fig. 24.—Deflection of a Pendulum; the Moon and Observer on the Equator

lunar influence will be $\frac{1}{14,500}$ of an inch. With a pendulum one metre, or 3 ft. 3 in. in length, the range of motion of the pendulum bob is $\frac{1}{145,000}$ of an inch. For any pendulum of manageable length this displacement is so small, that it seems hopeless to attempt to measure it by direct observation. Nevertheless the mass and distance of the moon and the intensity of gravity being known with a considerable degree of accuracy, it is easy to calculate the deflection of the vertical at any time.

The curves which are traced out by a pendulum present an infinite variety of forms, corresponding to various positions of the observer on the earth and of the moon in the heavens. Two illustrations of these curves must suffice. Fig. 24 shows the case when the moon is on the celestial equator and the observer on the terrestrial equator. The path is here a simple ellipse, which is traversed twice over in the lunar day by the pendulum. The hours of the lunar day at which the bob occupies successive positions are marked on the curve.

If the larger ellipse be taken to show the displacement of a pendulum when the sun and moon coöperate at spring tide, the smaller one will show its path at the time of neap tide.

In fig. 25 the observer is supposed to be in latitude 30°, whilst the moon stands 15° N. of the equator; in this figure no account is taken of the sun's force. Here also the hours are marked at the successive positions of the pendulum, which traverses this more complex curve only once in the lunar day. These curves are somewhat idealized, for they are drawn on the hypothesis that the moon does not shift her position in the heavens. If this fact were taken into account, we should

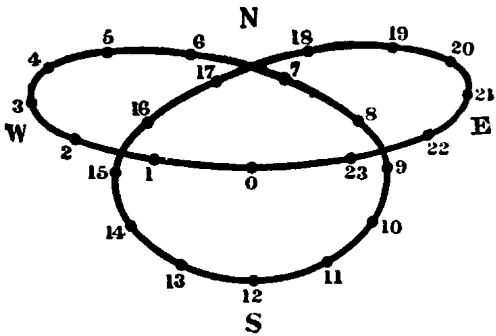

FIG. 25.—DEFLECTION OF A PENDULUM; THE MOON IN N. DECLINATION 15°, THE OBSERVER IN N. LATITUDE 30°

find that the curve would not end exactly where it began, and that the character of the curve would change slowly from day to day.

But even after the application of a correction for the gradual shift of the moon in the heavens, the curves would still be far simpler than in actuality, because the sun's influence has been left out of account. It has been remarked in the last chapter that the sun produces a tide-generating force, and it must therefore produce a deflection of the vertical. Although the solar deflection is considerably less than the lunar, yet it would serve to complicate the curve to a great degree, and it must be obvious then that when the full conditions of actuality are introduced the path of the pendulum will be so complicated, that mathematical formulæ are necessary for complete representation.

Although the direct observation of the tidal deflection of the vertical would be impossible even by aid of a powerful microscope, yet several attempts have been made by more or less indirect methods. I have just pointed out that the path of a pendulum, although drawn on an ultra-microscopic scale, can be computed with a high degree of accuracy. It may then occur to the reader that it is foolish to take a great deal of trouble to measure a displacement which is scarcely measurable, and which is already known with fair accuracy. To this it might be answered that it would be interesting to watch the direct gravitational effects of the moon on the earth's surface. But such an interest does not afford the principal grounds for thinking that this attempted measurement is worth making. If the solid earth were to yield to the lunar attraction with the freedom of a perfect fluid, its surface would always be perpendicular to the direction of gravity at each instant of time. Accordingly a pendulum would then always hang perpendicularly to the average surface of the earth, and so there would be no displacement of the pendulum with reference to the earth's surface. If, then, the solid earth yields partially to the lunar attraction, the displacements of a pendulum must be of smaller extent relatively to the earth than if the solid earth were absolutely rigid. I must therefore correct my statement as to our knowledge of the path pursued by a pendulum, and say that it is known if the earth is perfectly unyielding. The accurate observation of the movement of a pendulum under the influence of the moon, and the comparison of the observed oscillation, with that computed on the supposition that the earth is perfectly stiff, would afford the means of determining to what extent the solid earth is yielding to tidal forces. Such a result would be very interesting as giving a measure of the stiffness of the earth as a whole.

I must pass over the various earlier attempts to measure the lunar attraction, and will only explain the plan, although it was abortive, used in 1879 by my brother Horace and myself.

Our object was to measure the ultra-microscopic displacements of a pendulum with reference to the ground on which it stood. The principle of the apparatus used for this purpose is due to Lord Kelvin; it is very simple, although the practical application of it was not easy.

Fig. 26 shows diagrammatically, and not drawn to scale, a pendulum A B hanging by two wires. At the foot of the pendulum there is a support C attached to the stand of the pendulum; D is a small mirror suspended by two silk fibres, one being attached to the bottom of the pendulum B and the

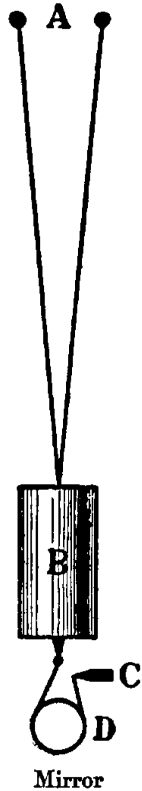

Fig. 26.—Bifilar Pendulum

other to the support C. When the two fibres are brought very close together, any movement of the pendulum perpendicular to the plane of the mirror causes the mirror to turn through a considerable angle. The two silk fibres diverge from one another, but if two vertical lines passing through the two points of suspension are $\frac{1}{1,000}$ of an inch apart, then when the pendulum moves one of these points through a millionth of an inch, whilst the other attached to C remains at rest, the mirror will turn through an angle of more than three minutes of arc. A lamp is placed opposite to the mirror, and the image of the lamp formed by reflection in the mirror is observed. A slight rotation of the mirror corresponds to an almost infinitesimal motion of the pendulum, and even excessively small movements of the mirror are easily detected by means of the reflected image of the light.

In our earlier experiments the pendulum was hung on a solid stone gallows; and yet, when the apparatus was made fairly sensitive, the image of the light danced and wandered incessantly. Indeed, the instability was so great that the reflected image wandered all across the room. We found subsequently that this instability was due both to changes of temperature in the stone gallows, and to currents in the air surrounding the pendulum.

To tell of all the difficulties encountered might be as tedious as the difficulties themselves, so I shall merely describe the apparatus in its ultimate form. The pendulum was suspended, as shown in fig. 26, by two wires; the two wires being in an east and west plane, the pendulum could only swing north and south. It was hung inside a copper tube, just so wide that the solid copper cylinder, forming the pendulum bob, did not touch the sides of the tube. A spike projected from the base of the pendulum bob through a hole in the bottom of the tube. The mirror was hung in a little box, with a plate-glass front, which was fastened to the bottom of the copper tube. The only communication between the tube and the mirror-box was by the hole through which the spike of the pendulum projected, but the tube and mirror-box together formed a water-tight vessel, which was filled with a mixture of spirits of wine and boiled water. The object of the fluid was to steady the mirror and the pendulum, while allowing its slower movements to take place. The water was boiled to get rid of air in it, and the spirits of wine was added to increase the resistance of the fluid, for it is a remarkable fact that a mixture of spirits and water has considerably more viscosity or stickiness than either pure spirits or pure water.

The copper tube, with the pendulum and mirror-box, was supported on three legs resting on a block of stone weighing a ton, and this stood on the native gravel in a north room in the laboratory at Cambridge. The whole instrument was immersed in a water-jacket, which was furnished with a window near the bottom, so that the little mirror could be seen from outside. A water ditch also surrounded the stone pedestal, and the water jacketing of the whole instrument made the changes of temperature very slow.

A gas jet, only turned up at the moment of observation, furnished the light to be observed by reflection in the little mirror. The gas burner could be made to travel to and fro along a scale in front of the instrument. In the preliminary description I have spoken of the motion of the image of a fixed light, but it clearly amounts to the same thing if we measure the motion of the light, keeping the point of observation fixed. In our instrument the image of the movable gas jet was observed by a fixed telescope placed outside of the room. A bright light was unfortunately necessary, because there was a very great loss of light in the passages to and fro through two pieces of plate glass and a considerable thickness of water.

Arrangements were made by which, without entering the room, the gas jet could be turned up and down, and could be made to move to and fro in the room in an east and west direction, until its image was observed in the telescope. There were also adjustments by which the two silk fibres from which the mirror hung could be brought closer together or further apart, thus making the instrument more or less sensitive. There was also an arrangement by which the image of the light could be brought into the field of view, when it had wandered away beyond the limits allowed for by the traverse of the gas jet.

When the instrument was in adjustment, an observation consisted of moving the gas jet until its image was in the centre of the field of view of the telescope; a reading of the scale, by another telescope, determined the position of the gas jet to within about a twentieth of an inch.

The whole of these arrangements were arrived at only after laborious trials, but all the precautions were shown by experience to be necessary, and were possibly even insufficient to guard the instrument from the effects of changes of temperature. I shall not explain the manner in which we were able to translate the displacements of the gas jet into displacements of the pendulum. It was not very satisfactory, and only gave approximate results. A subsequent form of an instrument of this kind, designed by my brother, has been much improved in this respect. It was he also who designed all the mechanical appliances in the experiment of which I am speaking.

It may be well to reiterate that the pendulum was only free to move north and south, and that our object was to find how much it swung. The east and west motion of a pendulum is equally interesting, but as we could not observe both displacements at the same time, we confined our attention in the first instance to the northerly and southerly movements.

When properly adjusted the apparatus was so sensitive that, if the bob of the pendulum moved through $\frac{1}{40,000}$ of a millimetre, that is, a millionth part of an inch, we could certainly detect the movement, for it corresponded to a twentieth of an inch in our scale of position of the gas jet. When the pendulum bob moved through this amount, the wires of the pendulum turned through one two-hundredth of a second of arc; this is the angle subtended by one inch at 770 miles distance. I do not say that we could actually measure with this degree of refinement, but we could detect a change of that amount. In view of the instability of the pendulum, which still continued to some extent, it may be hard to gain credence for the statement that such a small deflection was a reality, so I will explain how we were sure of our correctness.

In setting up the apparatus, work had to be conducted inside the room, and some preliminary observations of the reflected image of a stationary gas jet were made without the use of the telescope. The scale on which the reflected spot of light fell was laid on the ground at about seven feet from the instrument; in order to watch it I knelt on the pavement behind the scale, and leant over it. I was one day watching on the scale the spot of light which revealed the motion of the pendulum, and, being tired with kneeling, supported part of my weight on my hands a few inches in front of the scale. The place where my hands rested was on the bare earth, from which a paving stone had been removed. I was surprised to find quite a large change in the reading. It seemed at first incredible that my change of position was the cause, but after several trials I found that light pressure with one hand was quite sufficient to produce an effect. It must be remembered that this was not simply a small pressure delivered on the bare earth at, say, seven feet distance, but it was the difference of effect produced by the same pressure at seven feet and six feet; for, of course, the change only consisted in the distribution of the weight of a small portion of my body.

It is not very easy to catch the telescopic image of a spot of light reflected from a mirror of the size of a shilling. Accordingly, in setting up our apparatus, we availed ourselves of this result, for we found that the readiest way of bringing the reflected image into the telescopic field of view was for one of us to move slowly about the room, until the image of the light was brought, by the warping of the soil due to his weight, into the field of view of the telescope. He then placed a heavy weight on the floor where he had been standing; this of course drove the image out of the field of view, but after he had left the room the image of the flame was found to be in the field.

We ultimately found, even when no special pains had been taken to render the instrument sensitive, that if one of us was in the room, and stood at about sixteen feet south of the instrument with his feet about a foot apart, and slowly shifted his weight from one foot to the other, a distinct change was produced in the image of the gas flame, and of course in the position of the little mirror, from which the image was derived by reflection. It may be well to consider for a moment the meaning of this result. If one presses with a finger on a flat slab of jelly, a sort of dimple is produced, and if a pin were sticking upright in the jelly near the dimple, it would tilt slightly towards the finger. Now this is like what we were observing, for the jelly represents the soil, and the tilt of the pin corresponds to that of the pendulum. But the scale of the displacement is very different, for our pendulum stood on a block of stone weighing nearly a ton, which rested on the native gravel at two feet below the level of the floor, and the slabs of the floor were removed from all round the pendulum. The dimple produced by a weight of 140 lbs. on the stone paved floor must have been pretty small, and the slope of the sides of that dimple at sixteen feet must have been excessively slight; but we were here virtually observing the change of slope at the instrument, when the centre of the dimple was moved from a distance of fifteen feet to sixteen feet.

It might perhaps be thought that all observation would be rendered impossible by the street traffic and by the ordinary work of the laboratory. But such disturbances only make tremors of very short period, and the spirits and water damped out quick oscillations so thoroughly, that no difference could be detected in the behavior of the pendulum during the day and during the night. Indeed, we found that a man could stand close to the instrument and hit the tub and pedestal smart blows with a stick, without producing any sensible effect. But it was not quite easy to try this experiment, because there was a considerable disturbance on our first entering the room; and when this had subsided small movements of the body produced a sensible deflection, by slight changes in the distribution of the experimenter's weight.

It is clear that we had here an instrument of amply sufficient delicacy to observe the lunar tide-generating force, and yet we completely failed to do so. The pendulum was, in fact, always vacillating and changing its position by many times the amount of the lunar effect which we sought to measure.

An example will explain how this was: A series of frequent readings were taken from July 21st to 25th, 1881, with the pendulum arranged to swing north and south. We found that there was a distinct diurnal period, with a maximum at noon, when the pendulum bob stood furthest northward.

The path of the pendulum was interrupted by many minor zigzags, and it would sometimes reverse its motion for an hour together. But the diurnal oscillation was superposed on a gradual drift of the pendulum, for the mean diurnal position traveled slowly southward. Indeed, in these four days the image disappeared from the scale three times over, and was brought back into the field of view three times by the appliance for that purpose. On the night between the 24th and 25th the pendulum took an abrupt turn northward, and the scale reading was found, on the morning of the 25th, nearly at the opposite end of the scale from that towards which it had been creeping for four days previously.

Notwithstanding all our precautions the pendulum was never at rest, and the image of the flame was always trembling and dancing, or waving slowly to and fro. In fact, every reading of our scale had to be taken as the mean of the excursions to right and left. Sometimes for two or three days together the dance of the image would be very pronounced, and during other days it would be remarkably quiescent.

The origin of these tremors and slower movements is still to some extent uncertain. Quite recent investigations by Professor Milne seem to show that part of them are produced by currents in the fluid surrounding the pendulum, that others are due to changes in the soil of a very local character, and others again to changes affecting a considerable tract of soil.

But when all possible allowance is made for these perturbations, it remains certain that a large proportion of these mysterious movements are due to minute earthquakes.

Some part of the displacements of our pendulum was undoubtedly due to the action of the moon, but it was so small a fraction of the whole, that we were completely foiled in our endeavor to measure it.[1]

The minute earthquakes of which I have spoken are called by Italian observers microsisms, and this name has been very generally adopted. The literature on the subject of seismology is now very extensive, and it would be out of place to attempt to summarize here the conclusions which have been drawn from observation. I may, however, permit myself to add a few words to indicate the general lines of the research, which is being carried on in many parts of the world.

Italy is a volcanic country, and the Italians have been the pioneers in seismology. Their observations have been made by means of pendulums of various lengths, and with instruments of other forms, adapted for detecting vertical move-

[1]Since the date of our experiment the bifilar pendulum has been perfected by my brother, and it is now giving continuous photographic records at several observatories. It is now made to be far less sensitive than in our original experiment, and no attempt is made to detect the direct effect of the moon.

ments of the soil. The conclusions at which Father Bertelli arrived twenty years ago may be summarized as follows:—

The oscillation of the pendulum is generally parallel to valleys or chains of mountains in the neighborhood. The oscillations are independent of local tremors, velocity and direction of wind, rain, change of temperature, and atmospheric electricity.

Pendulums of different lengths betray the movements of the soil in different manners, according to the agreement or disagreement of their natural periods of oscillation with the period of the terrestrial vibrations.

The disturbances are not strictly simultaneous in the different towns of Italy, but succeed one another at short intervals.

After earthquakes the "tromometric" or microseismic movements are especially apt to be in a vertical direction. They are always so when the earthquake is local, but the vertical movements are sometimes absent when the shock occurs elsewhere. Sometimes there is no movement at all, even when the shock occurs quite close at hand.

The positions of the sun and moon appear to have some influence on the movements of the pendulum, but the disturbances are especially frequent when the barometer is low.

The curves of "the monthly means of the tromometric movement" exhibit the same forms in the various towns of Italy, even those which are distant from one another.

The maximum of disturbance occurs near the winter solstice and the minimum near the summer solstice.

At Florence a period of earthquakes is presaged by the magnitude and frequency of oscillatory movements in a vertical direction. These movements are observable at intervals and during several hours after each shock.

Some very curious observations on microsisms have also been made in Italy with the microphone, by which very slight movements of the soil are rendered audible.

Cavaliere de Rossi, of Rome, has established a "geodynamic" observatory in a cave 700 metres above the sea at Rocca di Papa, on the external slope of an extinct volcano.

At this place, remote from all carriages and roads, he placed his microphone at a depth of 20 metres below the ground. It was protected against insects by woolen wrappings. Carpet was spread on the floor of the cave to deaden the noise from particles of stone which might possibly fall. Having established his microphone, he waited till night, and then heard noises which he says revealed "natural telluric phenomena." The sounds which he heard he describes as "roarings, explosions occurring isolated or in volleys, and metallic or bell-like sounds" (*fremiti, scopii isolati o di moschetteria, e suoni-metallici o di campana*). They all occurred mixed indiscriminately, and rose to maxima at irregular intervals. By artificial means he was able to cause noises which he calls "rumbling (?) or crackling" (*rullo o crepito*). The roaring (*fremito*) was the only noise which he could reproduce artificially, and then only for a moment. It was done by rubbing together the conducting wires, "in the same manner as the rocks must rub against one another when there is an earthquake."

A mine having been exploded in a quarry at some distance, the tremors in the earth were audible in the microphone for some seconds subsequently.

There was some degree of coincidence between the agitation of the pendulum-seismograph and the noises heard with the microphone.

At a time when Vesuvius became active, Rocca di Papa was agitated by microsisms, and the shocks were found to be accompanied by the very same microphonic noises as before. The noises sometimes became "intolerably loud;" especially on one occasion in the middle of the night, half an hour before a sensible earthquake. The agitation of the microphone corresponded exactly with the activity of Vesuvius.

Rossi then transported his microphone to Palmieri's Vesuvian observatory, and worked in conjunction with him. He there found that each class of shock had its corresponding noise. The sussultorial shocks, in which I conceive the movement of the ground is vertically up and down, gave the volleys of musketry (*i colpi di moschetteria*), and the undulatory shocks gave the roarings (*i fremiti*). The two classes of noises were sometimes mixed up together.

Rossi makes the following remarks: "On Vesuvius I was put in the way of discovering that the simple fall and rise in the ticking which occurs with the microphone [*battito del orologio unito al microfono*] (a phenomenon observed by all, and remaining inexplicable to all) is a consequence of the vibration of the ground." This passage alone might perhaps lead one to suppose that clockwork was included in the circuit; but that this was not the case, and that "ticking" is merely a mode of representing a natural noise is proved by the fact that he subsequently says that he considers the ticking to be "a telluric phenomenon."

Rossi then took the microphone to the Solfatara of Pozzuoli, and here, although no sensible tremors were felt, the noises were so loud as to be heard simultaneously by all the people in the room. The ticking was quite masked by other natural noises. The noises at the Solfatara were imitated by placing the microphone on the lid of a vessel of boiling water. Other seismic noises were then imitated by placing the microphone on a marble slab, and scratching and tapping the under surface of it.

The observations on Vesuvius led him to the conclusion that the earthquake oscillations have sometimes fixed "nodes," for there were places on the mountain where no effects were observed. There were also places where the movement was intensified, and hence it may be concluded that the centre of disturbance may sometimes be very distant, even when the observed agitation is considerable.

At the present time perhaps the most distinguished investigator in seismology is Professor Milne, formerly of the Imperial College of Engineering at Tokyo. His residence in Japan gave him peculiar opportunities of studying earthquakes, for there is, in that country, at least one earthquake per diem of sufficient intensity to affect a seismometer. The instrument of which he now makes most use is called a horizontal pendulum. The principle involved in it is old, but it was first rendered practicable by von Rebeur-Paschwitz, whose early death deprived the world of a skillful and enthusiastic investigator.

The work of Paschwitz touches more closely on our present subject than that of Milne, because he made a gallant attempt to measure the moon's tide-generating force, and almost persuaded himself that he had done so.

The horizontal pendulum is like a door in its mode of suspension. If a doorpost be absolutely vertical, the door will clearly rest in any position, but if the post be even infinitesimally tilted the door naturally rests in one definite position.

A very small shift of the doorpost is betrayed by a considerable change in the position of the door. In the pendulum the door is replaced by a horizontal boom, and the hinges by steel points resting in agate cups, but the principle is the same.

The movement of the boom is detected and registered photographically by the image of a light reflected from certain mirrors. Paschwitz made systematic observations with his pendulum at Wilhelmshaven, Potsdam, Strassburg, and Orotava. He almost convinced himself at one time that he could detect, amidst the wanderings of the curves of record, a periodicity corresponding to the direct effect of the moon's action. But a more searching analysis of his results left the matter in doubt. Since his death the observations at Strassburg have been continued by M. Ehlert. His results show an excellent consistency with those of Paschwitz, and are therefore confirmatory of the earlier opinion of the latter. I am myself disposed to think that the detection of the lunar attraction is a reality, but the effect is so minute that it cannot yet be relied on to furnish a trustworthy measurement of the amount of the yielding of the solid earth to tidal forces.

It might be supposed that doubt could hardly arise as to whether or not the direct effect of the moon's attraction had been detected. But I shall show in the next chapter that at many places the tidal forces must exercise in an indirect manner an effect on the motion of a pendulum much greater than the direct effect.

It was the consideration of this indirect effect, and of other concomitants, which led us to abandon our attempted measurement, and to conclude that all endeavors in that direction were doomed to remain for ever fruitless. I can but hope that a falsification of our forecast by M. Ehlert and by others may be confirmed.

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CHAPTER VII

THE ELASTIC DISTORTION OF THE EARTH'S SURFACE BY VARYING LOADS

When the tide rises and falls on the seacoast, many millions of tons of water are brought alternately nearer and further from the land. Accordingly a pendulum suspended within a hundred miles or so of a seacoast should respond to the attraction of the sea water, swinging towards the sea at high water, and away from it at low water. Since the rise and fall has a lunar periodicity the pendulum should swing in the same period, even if the direct attraction of the moon did not affect it. But, as I shall now show, the problem is further confused by another effect of the varying tidal load.

We saw in Chapter VI. how a weight resting on the floor in the neighborhood of our pendulum produced a dimple by which the massive stone pedestal of our instrument was tilted over. Now as low tide changes to high tide the position of an enormous mass of water is varied with respect to the land. Accordingly the whole coast line must rock to and fro with the varying tide. We must now consider the nature of the distortion of the soil produced in this way. The mathematical investigation of the form of the dimple in a horizontal slab of jelly or other elastic material, due to pressure at a single point, shows that the slope at any place varies inversely as the square of the distance from the centre. That is to say, if starting from any point we proceed to half our original distance, we shall find four times as great a slope, and at one third of the original distance the slope will be augmented

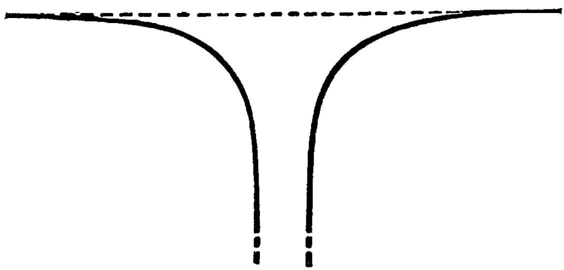

FIG. 27.—FORM OF DIMPLE IN AN ELASTIC SURFACE

ninefold.

The theoretical form of dimple produced by pressure at a single mathematical point is shown in fig. 27. The slope is exaggerated so as to render it visible, and since the figure is drawn on the supposition that the pressure is delivered at a mathematical point, the centre of the dimple is infinitely deep. If the pressure be delivered by a blunt point, the slope at a little distance will be as shown, but the centre will not be infinitely deep. If therefore we pay no attention to the very centre, this figure serves to illustrate the state of the case.

When the dimple is produced by the pressure of a weight, that weight, being endowed with gravitation, attracts any other body with a force varying inversely as the square of the distance. It follows, therefore, that the slope of the dimple is everywhere exactly proportional to the gravitational attraction of the weight. Since this is true of a single weight, it is true of a group of weights, each producing its own dimple by pressure and its own attraction, strictly proportional to one another. Thus the whole surface is deformed by the superposition of dimples, and the total attraction is the sum of all the partial attractions.

Let us then imagine a very thick horizontal slab of glass supporting any weights at any parts of its surface. The originally flat surface of the slab will be distorted into shallow valleys and low hills, and it is clear that the direct attraction of the weights will everywhere be exactly proportional to the slopes of the hillsides; also the direction of the greatest slope at each place must agree with the direction of the attraction. The direct attraction of the weights will deflect a pendulum from the vertical, and the deflection must be exactly proportional to the slope produced by the pressure of the weights. It may be proved that if the slab is made of a very stiff glass the angular deflection of the pendulum under the influence of attraction will be one fifth of the slope of the hillside; if the glass were of the most yielding kind, the fraction would be one eighth. The fraction depends on the degree of elasticity of the material, and the stiffer it is the larger the fraction.

The observation of a pendulum consists in noting its change of position with reference to the surface of the soil; hence the slope of the soil, and the direct attraction of the weight which causes that slope, will be absolutely fused together, and will be indistinguishable from one another.

Now, this conclusion may be applied to the tidal load, and we learn that, if rocks are of the same degree of stiffness as glass of medium quality, the direct attraction of the tidal load produces one sixth of the apparent deflection of a pendulum produced by the tilting of the soil.

If any one shall observe a pendulum, within say a hundred miles of the seacoast, and shall detect a lunar periodicity in its motion, he can only conclude that what he observes is partly due to the depression and tilting of the soil, partly to attraction of the sea water, and partly to the direct attraction of the moon. Calculation indicates that, with the known average elasticity of rock, the tilting of the soil is likely to be far greater than the other two effects combined. Hence, if the direct attraction of the moon is ever to be measured, it will first be necessary to estimate and to allow for other important oscillations with lunar periodicity. The difficulty thus introduced into this problem is so serious that it has not yet been successfully met. It may perhaps some day be possible to distinguish the direct effects of the moon's tidal attraction from the indirect effects, but I am not very hopeful of success in this respect. It was pointed out in Chapter VI. that there is some reason to think that a lunar periodicity in the swing of a pendulum has been already detected, and if this opinion is correct, the larger part of the deflection was probably due to these indirect effects.

The calculation of the actual tilting of the coast line by the rising tide would be excessively complex even if accurate estimates were obtainable of the elasticity of the rock and of the tidal load. It is, however, possible to formulate a soluble problem of ideal simplicity, which will afford us some idea of the magnitude of the results occurring in nature.

In the first place, we may safely suppose the earth to be flat, because the effect of the tidal load is quite superficial, and the curvature of the earth is not likely to make much difference in the result. In the second place, it greatly simplifies the calculation to suppose the ocean to consist of an indefinite number of broad canals, separated from one another by broad strips of land of equal breadth. Lastly, we shall suppose that each strip of sea rocks about its middle line, so that the water oscillates as in a seiche of the Lake of Geneva; thus, when it is high water on the right-hand coast of a strip of sea, it is low water on the left-hand coast, and vice versa. We have then to determine the change of shape of the ocean-bed and of the land, as the tide rises and falls. The problem as thus stated is vastly simpler than in actual-

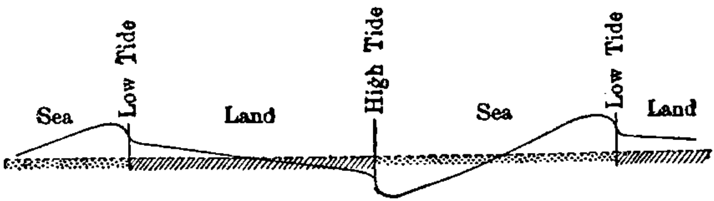

Fig. 28.—Distortion of Land and Sea-bed by Tidal Load

ity, yet it will suffice to give interesting indications of what must occur in nature.

The figure 28 shows the calculated result, the slopes being of course enormously exaggerated. The straight line represents the level surface of land and sea before the tidal oscillation begins, the shaded part being the land and the dotted part the sea. Then the curved line shows the form of the land and of the sea-bed, when it is low water at the right of the strip of land and high water at the left. The figure would be reversed when the high water interchanges position with the low water. Thus both land and sea rock about their middle lines, but the figure shows that the strip of land remains nearly flat although not horizontal, whilst the sea-bed becomes somewhat curved.

It will be noticed that there is a sharp nick at the coast line. This arises from the fact that deep water was assumed to extend quite up to the shore line; if, however, the sea were given a shelving shore, as in nature, the sharp nick would disappear, although the form of the distorted rocks would remain practically unchanged elsewhere.

Thus far the results have been of a general character, and we have made no assumptions as to the degree of stiffness of the rock, or as to the breadths of the oceans and continents. Let us make hypotheses which are more or less plausible. At many places on the seashore the tide ranges through twenty or thirty feet, but these great tides only represent the augmentation of the tide-wave as it runs into shallow water, and it would not be fair to suppose our tide to be nearly so great. In order to be moderate, I will suppose the tide to have a range of 160 centimetres, or, in round numbers, about 5 feet. Then, at the high-water side of the sea, the water is raised by 80 centimetres, and at the low-water side it is depressed by the same amount. The breadth of the Atlantic is about 4,000 or 5,000 miles. I take then, the breadth of the oceans and of the continents as 3,900 miles, or 6,280 kilometres. Lastly, as rocks are usually stiffer than glass, I take the rock bed to be twice as stiff as the most yielding glass, and quarter as stiff again as the stiffest glass; this assumption as to the elasticity of rock makes the attraction at any place one quarter of the slope. For a medium glass we found the fraction to be about one sixth. These are all the data required for determining the slope.

It is of course necessary to have a unit of measurement for the slope of the surface. Now a second of arc is the name for the angular magnitude of an inch seen at $3\frac{1}{4}$ miles, and accordingly a hundredth of a second of arc, usually written $0''.01$, is the angular magnitude of an inch seen at 325 miles; the angles will then be measured in hundredths of seconds.

Before the tide rises, the land and sea-bed are supposed to be perfectly flat and horizontal. Then at high tides the slopes on the land are as follows:—

Distance from high-water mark	Slope of the land measured in hundredths of seconds of arc
10 metres	10
100 metres	8
1 kilometre	6
10 kilometres	4
20 kilometres	$3\frac{1}{2}$
100 kilometres	2

The slope is here expressed in hundredths of a second of arc, so that at 100 kilometres from the coast, where the slope is 2, the change of plane amounts to the angle subtended by one inch at 162 miles.

When high water changes to low water, the slopes are just reversed, hence the range of change of slope is represented by the doubles of these angles. If the change of slope is observed by some form of pendulum, allowance must be made for the direct attraction of the sea, and it appears that with the supposed degree of stiffness of rock these angles of slope must be augmented in the proportion of 5 to 4. Thus, we double the angles to allow of change from high to low water, and augment the numbers as 5 is to 4, to allow for the direct attraction of the sea. Finally we find results which may be arranged in the following tabular form:—

Distance from high-water mark	Apparent range of deflection of the vertical
10 metres	$0''.25$
100 metres	$0''.20$
1 kilometre	$0''.15$
10 kilometres	$0''.10$
20 kilometres	$0''.084$
100 kilometres	$0''.050$

At the centre of the continent, 1,950 miles from the coast, the range will be $0''.012$.

If all the assumed data be varied, the ranges of the slopes are easily calculable, but these results may be taken as fairly representative, although perhaps somewhat underestimated. Lord Kelvin has made an entirely independent estimate of the probable deflection of a pendulum by the direct attraction of the sea at high tide. He supposes the tide to have a range of 10 feet from low water to high water, and he then estimates the attraction of a slab of water 10 feet thick, 50 miles broad perpendicular to the coast, and 100 miles long parallel to the coast, on a plummet 100 yards from low-water mark and opposite the middle of the 100 miles. This would, he thinks, very roughly represent the state of things at St. Alban's Head, in England. He finds the attraction such as to deflect the plumb-line, as high water changes to low water, by a twentieth of a second of arc. The general law as to the proportionality of slope to attraction shows that, with our supposed degree of stiffness of rock, the apparent deflection of a plumb-line, due to the depression of the coast and the attraction of the sea as high water changes to low water, will then be a quarter of a second of arc. Postulating a smaller tide, but spread over a wider area, I found the result would be a fifth of a second; thus the two results present a satisfactory agreement.

This speculative investigation receives confirmation from observation. The late M. d'Abbadie established an observatory at his château of Abbadia, close to the Spanish frontier and within a quarter of a mile of the Bay of Biscay. Here he constructed a special form of instrument for detecting small changes in the direction of gravity. Without going into details, it may suffice to state that he compared a fixed mark with its image formed by reflection from a pool of mercury. He took 359 special observations at the times of high and low tide in order to see, as he says, whether the water exercised an attraction on the pool of mercury, for it had not occurred to him that the larger effect would probably arise from the bending of the rock. He found that in 243 cases the pool of mercury was tilted towards the sea at high water or away from it at low water; in 59 cases there was no apparent effect, and in the remaining 57 cases the action was inverted. The observations were repeated later by his assistant in the case of 71 successive high waters[1] and 73 low waters, and he also found that in about two thirds of the observations the sea seemed to exercise its expected influence. We may, I think, feel confident that on the occasions where no effect or a reversal was perceived, it was annulled or reversed by a warping of the soil, such as is observed with seismometers.

Dr. von Rebeur-Paschwitz also noted deflections due to the tide at Wilhelmshaven in Germany. The deflection was

[1] Presumably the observation at one high water was defective.

indeed of unexpected magnitude at this place, and this may probably be due to the peaty nature of the soil, which renders it far more yielding than if the observatory were built on rock.

This investigation has another interesting application, for the solid earth has to bear another varying load besides that of the tide. The atmosphere rests on the earth and exercises a variable pressure, as shown by the varying height of the barometer. The variation of pressure is much more considerable than one would be inclined to suspect off-hand. The height of the barometer ranges through nearly two inches, or say five centimetres; this means that each square yard of soil supports a weight greater by 1,260 lbs. when the barometer is very high, than when it is very low. If we picture to ourselves a field loaded with half a ton to each square yard, we may realize how enormous is the difference of pressure in the two cases.

In order to obtain some estimate of the effects of the changing pressure, I will assume, as before, that the rocks are a quarter as stiff again as the stiffest glass. On a thick slab of this material let us imagine a train of parallel waves of air, such that at the crests of the waves the barometer is 5 centimetres higher than at the hollow. Our knowledge of the march of barometric gradients on the earth's surface makes it plausible to assume that it is 1,500 miles from the line of highest to that of lowest pressure. Calculation then shows that the slab is distorted into parallel ridges and valleys, and that the tops of the ridges are 9 centimetres, or $3\frac{1}{2}$ inches, higher than the hollows. Although the actual distribution of barometric pressures is not of this simple character, yet this calculation shows, with a high degree of probability, that when the barometer is very high we are at least 3 inches nearer the earth's centre than when it is very low.

The consideration of the effects of atmospheric pressure leads also to other curious conclusions. I have remarked before that the sea must respond to barometric pressure, being depressed by high and elevated by low pressure. Since a column of water 68 centimetres (2 ft. 3 in.) in height weighs the same as a column, with the same cross section, of mercury, and 5 centimetres in height, the sea should be depressed by 68 centimetres under the very high barometer as compared with the very low barometer. But the height of the water can only be determined with reference to the land, and we have seen that the land must be depressed by 9 centimetres. Hence the sea would be apparently depressed by only 59 centimetres.

It is probable that, in reality, the larger barometric inequalities do not linger quite long enough over particular areas to permit the sea to attain everywhere its due slope, and therefore the full difference of water level can only be attained occasionally. On the other hand the elastic compression of the ground must take place without sensible delay. Thus it seems probable that this compression must exercise a very sensible effect in modifying the apparent depression or elevation of the sea under high and low barometer.

If delicate observations are made with some form of pendulum, the air waves and the consequent distortions of the soil should have a sensible effect on the instrument. In the ideal case which I have described above, it appears that the maximum apparent deflection of the plumb-line would be $\frac{1}{90}$ of a second of arc; this would be augmented to $\frac{1}{70}$ of a second by the addition of the true deflection, produced by the attraction of the air. Lastly, since the slope and attraction would be absolutely reversed when the air wave assumed a different position with respect to the observer, it is clear that the range of apparent oscillation of the pendulum might amount to $\frac{1}{35}$ of a second of arc.

This oscillation is actually greater than that due to the direct tidal force of the moon acting on a pendulum suspended on an ideally unyielding earth. Accordingly we have yet another reason why the direct measurement of the tidal force presents a problem of the extremest difficulty.

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CHAPTER VIII

EQUILIBRIUM THEORY OF TIDES

It is clearly necessary to proceed step by step towards the actual conditions of the tidal problem, and I shall begin by supposing that the oceans cover the whole earth, leaving no dry land. It has been shown in Chapter V. that the tidal force is the resultant of opposing centrifugal and centripetal forces. The motion of the system is therefore one of its most essential features. We may however imagine a supernatural being, who carries the moon round the earth and makes the earth rotate at the actual relative speeds, but with indefinite slowness as regards absolute time. This supernatural being is further to have the power of maintaining the tidal forces at exactly their present intensities, and with their actual relationship as regards the positions of the moon and earth. Everything, in fact, is to remain as in reality, except time, which is to be indefinitely protracted. The question to be considered is as to the manner in which the tidal forces will cause the ocean to move on the slowly revolving earth.

It appears from fig. 23 that the horizontal tidal force acts at right angles to the circle, where the moon is in the horizon, just rising or just setting, towards those two points, V and I, where the moon is overhead in the zenith, or underfoot in the nadir. The force will clearly generate currents in the water away from the circle of moonrise and moonset, and towards V and I. The currents will continue to flow until the water level is just so much raised above the primitive surface at V and I, and depressed along the circle, that the tendency to flow downhill towards the circle is equal to the tendency to flow uphill under the action of the tide-generating force. When the currents have ceased to flow, the figure of the ocean has become elongated, or egg-shaped with the two ends alike, and the longer axis of the egg is pointed at the moon. When this condition is attained the system is at rest or in equilibrium, and the technical name for the egg-like form is a "prolate ellipsoid of revolution"—"prolate" because it is elongated, and "of revolution" because it is symmetrical with respect to the line pointing at the moon. Accordingly the mathematician says that the figure of equilibrium under tide-generating force is a prolate ellipsoid of revolution, with the major axis directed to the moon.

It has been supposed that the earth rotates and that the moon revolves, but with such extreme slowness that the ocean currents have time enough to bring the surface to its form of equilibrium, at each moment of time. If the time be sufficiently protracted, this is a possible condition of affairs. It is true that with the earth spinning at its actual rate, and with the moon revolving as in nature, the form of equilibrium can never be attained by the ocean; nevertheless it is very important to master the equilibrium theory.

Fig. 29 represents the world in two hemispheres, as in an ordinary atlas, with parallels of latitude drawn at 15° apart. At the moment represented, the moon is supposed to be in the zenith at 15° of north latitude, in the middle of the right-hand hemisphere. The diametrically opposite point is of course at 15° of south latitude, in the middle of the other hemisphere. These are the two points V and I of figs. 22 and 23, towards which the water is drawn, so that the vertices of the ellipsoid are at these two spots. A scale of measurement must be adopted for estimating the elevation of the water above, and its depression below the original undisturbed surface of the globe. It will be convenient to measure the elevation at these two spots by the number 2. A series of circles are drawn round these points, but one of them is, of necessity, presented as partly in one hemisphere and partly in the other. In the map they are not quite concentric with the two spots, but on the actual globe they would be so. These circles show where, on the adopted scale of measurement, the elevation of height is successively $1\frac{1}{2}$, 1, $\frac{1}{2}$. The fourth circle, marked in chain dot, shows where there is no elevation or depression above the original surface. The next succeeding and dotted circle shows where there is a depression of $\frac{1}{2}$, and the last dotted line is the circle of lowest water

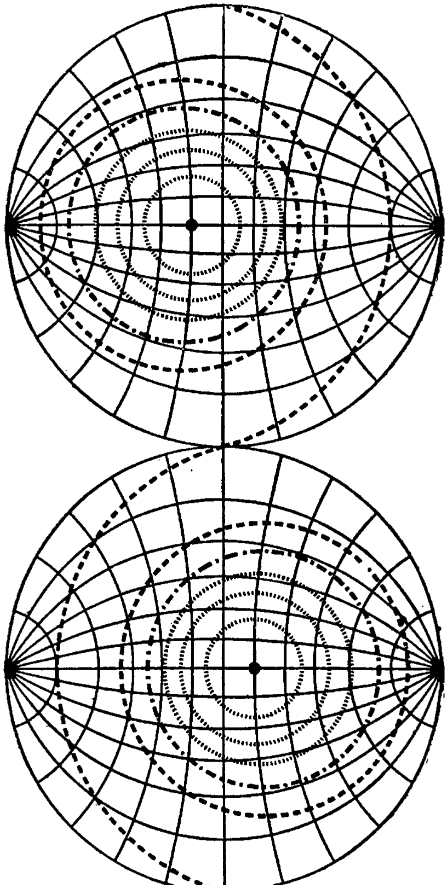

FIG. 29.—CHART OF EQUILIBRIUM TIDE

where the depression is 1; it is the circle D D of fig. 22, and the circle of the shadow in fig. 23.

The elevation above the original spherical surface at the vertices or highest points is just twice as great as the greatest depression. But the greatest elevation only occurs at two points, whereas the greatest depression is found all along a circle round the globe. The horizontal tide-generating force is everywhere at right angles to these circles, and the present figure is in effect a reproduction, in the form of a map, of the perspective picture in fig. 23.

Now as the earth turns from west to east, let us imagine a man standing on an island in the otherwise boundless sea, and let us consider what he will observe. Although the earth is supposed to be revolving very slowly, we may still call the twenty-fourth part of the time of its rotation an hour. The man will be carried by the earth's rotation along some one of the parallels of latitude. If, for example, his post of observation is in latitude 30° N., he will pass along the second parallel to the north of the equator. This parallel cuts several of the circles which indicate the elevation and depression of the water, and therefore he will during his progress pass places where the water is shallower and deeper alternately, and he would say that the water was rising and falling rhythmically. Let us watch his progress across the two hemispheres, starting from the extreme left. Shortly after coming into view he is on the dotted circle of lowest water, and he says it is low tide. As he proceeds the water rises, slowly at first and more rapidly later, until he is in the middle of the hemisphere; he arrives there six hours later than when we first began to watch him. It will have taken him about $5\frac{1}{2}$ hours to pass from low water to high water. At low water he was depressed by 1 below the original level, and at high water he is raised by $\frac{1}{2}$ above that level, so that the range from low water to high water is represented by $1\frac{1}{2}$. After the passage across the middle of the hemisphere, the water level falls, and after about $5\frac{1}{2}$ hours more the water is again lowest, and the depression is measured by 1 on the adopted scale. Soon after this he passes out of this hemisphere into the other one, and the water rises again until he is in the middle of that hemisphere. But this time he passes much nearer to the vertex of highest water than was the case in the other hemisphere, so that the water now rises to a height represented by about $1\frac{4}{5}$. In this half of his daily course the range of tide is from 1 below to $1\frac{4}{5}$ above, and is therefore $2\frac{4}{5}$, whereas before it was only $1\frac{1}{2}$. The fact that the range of two successive tides is not the same is of great importance in tidal theory; it is called the diurnal inequality of the tide.

It will have been noticed that in the left hemisphere the range of fall below the original spherical surface is greater than the range of rise above it; whereas in the right hemisphere the rise is greater than the fall. Mean water mark is such that the tide falls on the average as much below it as it rises above it, but in this case the rise and fall have been measured from the originally undisturbed surface. In fact the mean level of the water, in the course of a day, is not identical with the originally undisturbed surface, although the two levels do not differ much from one another.

The reader may trace an imaginary observer in his daily progress along any other parallel of latitude, and will find a similar series of oscillations in the ocean; each latitude will, however, present its own peculiarities. Then again the moon moves in the heavens. In fig. 29 she has been supposed to be 15° north of the equator, but she might have been yet further northward, or on the equator, or to the south of it. Her extreme range is in fact 28° north or south of the equator. To represent each such case a new map would be required, which would, however, only differ from this one by the amount of displacement of the central spots from the equator.

It is obvious that the two hemispheres in fig. 29 are exactly alike, save that they are inverted with respect to north and south; the right hemisphere is in fact the same as the left upside down. It is this inversion which causes the two successive tides to be unlike one another, or, in other words, gives rise to the diurnal inequality. But there is one case where inversion makes no difference; this is when the central spot is on the equator in the left hemisphere, for its inversion then makes the right hemisphere an exact reproduction of the left one. In this case therefore the two successive tides are exactly alike, and there is no diurnal inequality. Hence the diurnal inequality vanishes when the moon is on the equator.

Our figure exhibits another important point, for it shows that the tide has the greater range in that hemisphere where the observer passes nearest to one of the two central spots. That is to say, the higher tide occurs in that half of the daily circuit in which the moon passes nearest to the zenith or to the nadir of the observer.

Thus far I have supposed the moon to exist alone, but the sun also acts on the ocean according to similar laws, although with less intensity. We must now consider how the relative strengths of the actions of the two bodies are to be determined. It was indicated in Chapter V. that tide-generating force varies inversely as the cube of the distance from the earth of the tide-generating body. The force of gravity varies inversely as the square of the distance, so that, as we change the distance of the attracting body, tidal force varies with much greater rapidity than does the direct gravitational attraction. Thus if the moon stood at half her present distance from the earth, her tide-generating force would be 8 times as great, whereas her direct attraction would only be multiplied 4 times. It is also obvious that if the moon were twice as heavy as in reality, her tide-generating force would be doubled; and if she were half as heavy it would be halved. Hence we conclude that tide-generating force varies directly as the mass of the tide-generating body, and inversely as the cube of the distance.

The application of this law enables us to compare the sun's tidal force with that of the moon. The sun is 25,500,000 times as heavy as the moon, so that, on the score of mass, the solar tidal force should be $25\frac{1}{2}$ million times greater than that of the moon. But the sun is 389 times as distant as the moon. And since the cube of 389 is about 59 millions, the solar tidal force should be 59 million times weaker than that of the moon, on the score of distance.

We have, then, a force which is $25\frac{1}{2}$ million times stronger on account of the sun's greater weight, and 59 million times weaker on account of his greater distance; it follows that the sun's tide-generating force is $25\frac{1}{2}$-59ths, or a little less than half of that of the moon.

We conclude then that if the sun acted alone on the water, the degree of elongation or distortion of the ocean, when in equilibrium, would be a little less than half of that due to the moon alone. When both bodies act together, the distortion of the surface due to the sun is superposed on that due to the moon, and a terrestrial observer perceives only the total or sum of the two effects.

When the sun and moon are together on the same side of the earth, or when they are diametrically opposite, the two distortions conspire together, and the total tide will be half as great again as that due to the moon alone, because the solar tide is added to the lunar tide. And when the sun and moon are at right angles to one another, the two distortions are at right angles, and the low water of the solar tide conspires with the high water of the lunar tide. The composite tide has then a range only half as great as that due to the moon alone, because the solar tide, which has a range of about half that of the lunar tide, is deducted from the lunar tide. Since one and a half is three times a half, it follows that when the moon and sun act together the range of tide is three times as great as when they act adversely. The two bodies are together at change of moon and opposite at full moon. In both of these positions their actions conspire; hence at the change and the full of moon the tides are at their largest, and are called spring tides. When the two bodies are at right angles to one another, it is half moon, either waxing or waning, the tides have their smallest range, and are called neap tides.

The observed facts agree pretty closely with this theory in several respects, for spring tide occurs about the full and change of moon, neap tide occurs at the half moon, and the range at springs is usually about three times as great as that at neaps. Moreover, the diurnal inequality conforms to the theory in vanishing when the moon is on the equator, and rising to a maximum when the moon is furthest north or south. The amount of the diurnal inequality does not, however, agree with theory, and in many places the tide which should be the greater is actually the less.

The theory which I have sketched is called the Equilibrium Theory of the Tides, because it supposes that at each moment the ocean is in that position of rest or equilibrium which it would attain if indefinite time were allowed. The general agreement with the real phenomena proves the theory to have much truth about it, but a detailed comparison with actuality shows that it is terribly at fault. The lunar and solar tidal ellipsoids were found to have their long axes pointing straight towards the tide-generating bodies, and, therefore, at the time when the moon and sun pull together, it ought to be high water just when they are due south. In other words, at full and change of moon, it ought to be high water exactly at noon and at midnight. Now observation at spring tides shows that at most places this is utterly contradictory to fact.

It is a matter of rough observation that the tides follow the moon's course, so that high water always occurs about the same number of hours after the moon is due south. This rule has no pretension to accuracy, but it is better than no rule at all. Now at change and full of the moon, the moon crosses the meridian at the same hour of the clock as the sun, for at change of moon they are together, and at full moon they are twelve hours apart. Hence the hour of the clock at which high water occurs at change and full of moon is in effect a statement of the number of hours which elapse after the moon's passage of the meridian up to high water. This clock time affords a rough rule for the time of high water at any other phase of the moon; if, for example, it is high water at eight o'clock at full and change, approximately eight hours will always elapse after the moon's passage until high water occurs. Mariners call the clock time of high water at change and full of moon "the establishment of the port," because it establishes a rough rule of the tide at all other times.

According to the equilibrium theory, high water falls at noon and midnight at full and change of moon, or in the language of the mariner the establishment of all ports should be zero. But observation shows that the establishment at actual ports has all sorts of values, and that in the Pacific Ocean (where the tidal forces have free scope) it is at least much nearer to six hours than to zero. High water cannot be more than six hours before or after noon or midnight on the day of full or change of moon, because if it occurs more than six hours after one noon, it is less than six hours before the following midnight; hence the establishment of any port cannot possibly be more than six hours before or after. Accordingly, the equilibrium theory is nearly as much wrong as possible, in respect to the time of high water. In fact, in many places it is nearly low water at the time that the equilibrium theory predicts high water.

It would seem then as if the tidal action of the moon was actually to repel the water instead of attracting it, and we are driven to ask whether this result can possibly be consistent with the theory of universal gravitation.

The existence of continental barriers across the oceans must obviously exercise great influence on the tides, but this fact can hardly be responsible for a reversal of the previsions of the equilibrium theory. It was Newton who showed that a depression of the ocean under the moon is entirely consistent with the theory of gravitation. In the following chapter I shall explain Newton's theory, and show how it explains the discrepancy which we have found between the equilibrium theory and actuality.

Authorities.

An exposition of the equilibrium theory will be found in any mathematical work on the subject, or in the article *Tides* in the "Encyclopædia Britannica."

CHAPTER IX

DYNAMICAL THEORY OF THE TIDE WAVE

The most serious difficulties in the complete tidal problem do not arise in a certain special case which was considered by Newton. His supposition was that the sea is confined to a canal circling the equator, and that the moon and sun move exactly in the equator.

An earthquake or any other gigantic impulse may be supposed to generate a great wave in this equatorial canal. The rate of progress of such a wave is dependent on the depth of the canal only, according to the laws sketched in Chapter II., and the earth's rotation and the moon's attraction make no sensible difference in its speed of transmission. If, for example, the canal were 5 kilometres (3 miles) in depth, such a great wave would travel 796 kilometres (500 miles) per hour. If the canal were shallower the speed would be less than this; if deeper, greater. Now there is one special depth which will be found to have a peculiar importance in the theory of the tide, namely, where the canal is $13\frac{3}{4}$ miles deep. In this case the wave travels 1,042 miles an hour, so that it would complete the 25,000 miles round the earth in exactly 24 hours. It is important to note that if the depth of the equatorial canal be less than $13\frac{3}{4}$ miles, a wave takes more than a day to complete the circuit of the earth, and if the depth be greater the circuit is performed in less than a day.

The great wave, produced by an earthquake or other impulse, is called a "free wave," because when once produced it travels free from the action of external forces, and would persist forever, were it not for the friction to which water is necessarily subject. But the leading characteristic of the tide wave is that it is generated and kept in action by continuous forces, which act on the fluid throughout all time. Such a wave is called a "forced wave," because it is due to the continuous action of external forces. The rate at which the tide wave moves is moreover dependent only on the rate at which the tidal forces travel over the earth, and not in any degree on the depth of the canal. It is true that the depth of the canal exercises an influence on the height of the wave generated by the tidal forces, but the wave itself must always complete the circuit of the earth in a day, because the earth turns round in that period.

We must now contrast the progress of any long "free wave" in the equatorial canal with that of the "forced" tide wave. I may premise that it will here be slightly more convenient to consider the solar instead of the lunar tide. The lunar wave is due to a stronger tide-generating force, and since the earth takes 24 hours 50 minutes to turn round with respect to the moon, that is the time which the lunar tide wave takes to complete the circuit of the earth; but these differences are not material to the present argument. The earth turns with respect to the sun in exactly one day, or as we may more conveniently say, the sun completes the circuit of the earth in that time. Therefore the solar tidal influence travels over the surface of the earth at the rate of 1,042 miles an hour. Now this is exactly the pace at which a "free wave" travels in a canal of a depth of $13\frac{3}{4}$ miles; accordingly in such a canal any long free wave just keeps pace with the sun.

We have seen in Chapter V. that the solar tide-generating force *tends* to make a wave crest, at those points of the earth's circumference where it is noon and midnight. At each moment of time the sun is generating a new wave, and after it is generated that wave travels onwards as a free wave. If therefore the canal has a depth of $13\frac{3}{4}$ miles, each new wave, generated at each moment of time, keeps pace with the sun, and the summation of them all must build up two enormous wave crests at opposite sides of the earth.

If the velocity of a free wave were absolutely the same whatever were its height, the crests of the two tide waves would become infinite in height. As a fact the rate of progress of a wave is somewhat influenced by its height, and therefore, when the waves get very big, they will cease to keep pace exactly with the sun, and the cause for continuous exaggeration of their heights will cease to exist. We may, however, express this conclusion by saying that, when the canal is $13\frac{3}{4}$ miles deep, the height of the tide wave becomes mathematically infinite. This does not mean that mathematicians assert that the wave would really become infinite, but only that the simple method of treatment which supposes the wave velocity to depend only on the depth of water becomes inadequate. If the ocean was really confined to an equatorial canal, of this exact depth, the tides would be of very great height, and the theory would be even more complex than it is. It is, however, hardly necessary to consider this special case in further detail.

We conclude then that for the depth of $13\frac{3}{4}$ miles, the wave becomes infinite in height, in the qualified sense of infinity which I have described. We may feel sure that the existence of the quasi-infinite tide betokens that the behavior of the water in a canal shallower than $13\frac{3}{4}$ miles differs widely from that in a deeper one. It is therefore necessary to examine into the essential point in which the two cases differ from one another. In the shallower canal a free wave covers less than 25,000 miles a day, and thus any wave generated by the sun would tend to be left behind by him. On the other hand, in the deeper canal a free wave would outstrip the sun, and a wave generated by the sun tends to run on in advance of him. But these are only tendencies, for in both the shallower and the deeper canal the actual tide wave exactly keeps pace with the sun.

It would be troublesome to find out what would happen if we had the water in the canal at rest, and were suddenly to start the sun to work at it; and it is fortunately not necessary to attempt to do so. It is, however, certain that for a long time the motion would be confused, but that the friction of the water would finally produce order out of chaos, and that ultimately there would be a pair of antipodal tide crests traveling at the same pace as the sun. Our task, then, is to discover what that final state of motion may be, without endeavoring to unravel the preliminary chaos.

Let us take a concrete case, and suppose our canal to be 3 miles deep, in which we have seen that a free wave will travel 500 miles an hour. Suppose, then, we start a long free wave in the equatorial canal of 3 miles deep, with two crests 12,500 miles apart, and therefore antipodal to one another. The period of a wave is the time between the passage of two successive crests past any fixed point. In this case the crests are antipodal to one another, and therefore the wave length is 12,500 miles, and the wave travels 500 miles an hour, so that the period of a free wave is 25 hours. But the tide wave keeps pace with the sun, traveling 1,042 miles an hour, and there are two antipodal crests, 12,500 miles apart;

hence, the time between the passage of successive tide crests is 12 hours.

In this case a free wave would have a period of 25 hours, and the tide wave, resulting from the action of solar tide-generating force, has a period of 12 hours. The contrast then lies between the free wave, with a period of 25 hours, and the forced wave, with a period of 12 hours.

For any other depth of ocean the free wave will have another period depending on the depth, but the period of the forced wave is always 12 hours, because it depends on the sun. If the ocean be shallower than 3 miles, the free period will be greater than 25 hours, and, if deeper, less than 25 hours. But if the ocean be deepened to $13\frac{3}{4}$ miles, the free wave travels at the same pace as the forced wave, and therefore the two periods are coincident. For depths greater than $13\frac{3}{4}$ miles, the period of the free wave is less than that of the forced wave; and the converse is true for canals less than $13\frac{3}{4}$ miles in depth.

Now let us generalize this conception; we have a system which if disturbed and left to itself will oscillate in a certain period, called the free period. Periodic disturbing forces act on this system and the period of the disturbance is independent of the oscillating system itself. The period of the disturbing forces is called the forced period. How will such a system swing, when disturbed with this forced periodicity?

A weight tied to the end of a string affords an example of a very simple system capable of oscillation, and the period of its free swing depends on the length of the string only. I will suppose the string to be 3 feet, 3 inches, or one metre in length, so that the period of the swing from right to left, or from left to right is one second.[1] If, holding the string, I move my hand horizontally to and fro through a small distance with a regular periodicity, I set the pendulum a-swinging. The period of the movement of my hand is the forced period, and the free period is two seconds, being the time occupied by a metre-long pendulum in moving from right to left and back again to right. If I time the to and fro motion of my hand so that its period from right to left, or from left to right, is exactly one second, the excursions of the pendulum bob grow greater and greater without limit, because the successive impulses are stored up in the pendulum, which swings further and further with each successive impulse. This case is exactly analogous with the quasi-infinite tides which would arise in a canal $13\frac{3}{4}$ miles deep, and here also this case is critical, separating two modes of oscillation of the pendulum of different characters.

Now when the hand occupies more than one second in

[1] A pendulum of one metre in length is commonly called a seconds-pendulum, although its complete period is two seconds.

moving from right to left, the forced period is greater than the free period of the pendulum; and when the system is swinging steadily, it will be observed that the excursion of the hand agrees in direction with the excursion of the pendulum, so that when the hand is furthest to the right so is also the pendulum, and vice versa. When the period of the force is greater than the free period of the system, at the time when the force tends to make the pendulum move to the right, it is furthest to the right. The excursions of the pendulum agree in direction with that of the hand.

Next, when the hand occupies less than one second to move from right to left or from left to right, the forced period is less than the free period, and it will be found that when the hand is furthest to the right the pendulum is furthest to the left. The excursions of the pendulum are opposite in direction from those of the hand. These two cases are illustrated by fig. 30, which will, perhaps, render my meaning more obvious. We may sum up this discussion by saying that in the case of a slowly varying disturbing force, the oscillation and the force are consentaneous, but that with a quickly oscillating force, the oscillation is exactly inverted with respect to the force.

Now, this simple case illustrates a general dynamical principle, namely, that if a system capable of oscillating with a certain period is acted on by a periodic force, when

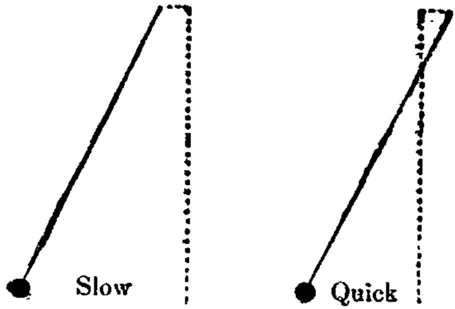

FIG. 30.—FORCED OSCILLATIONS OF A PENDULUM

the period of the force is greater than the natural free period of the system, the oscillations of the system agree with the oscillations of the force; but if the period of the force is less than the natural free period of the system the oscillations are inverted with reference to the force.

This principle may be applied to the case of the tides in the canal. When the canal is more than $13\frac{3}{4}$ miles deep, the period of the sun's disturbing force is 12 hours and is greater than the natural free period of the oscillation, because a free wave would go more than half round the earth in 12 hours. We conclude, then, that when the tide-generating forces are trying to make it high water, it will be high water. It has been shown that these forces are tending to make high water immediately under the sun and at its antipodes, and there accordingly will the high water be. In this case the tide is said to be direct.

But when the canal is less than $13\frac{3}{4}$ miles deep, the sun's disturbing force has, as before, a period of 12 hours, but the period of the free wave is more than 12 hours, because a free wave would take more than 12 hours to get half round the earth. Thus the general principle shows that where the forces are trying to make high water, there will be low water, and vice versa. Here, then, there will be low water under the sun and at its antipodes, and such a tide is said to be inverted, because the oscillation is the exact inversion of what would be naturally expected.

All the oceans on the earth are very much shallower than fourteen miles, and so, at least near the equator, the tides ought to be inverted. The conclusion of the equilibrium theory will therefore be the exact opposite of the truth, near the equator.

This argument as to the solar tide requires but little alteration to make it applicable to the lunar tide. In fact the only material difference in the conditions is that the period of the lunar tide is 12 hours 25 minutes, instead of 12 hours, and so the critical depth of an equatorial canal, which would allow the lunar tide to become quasi-infinite, is a little less than that for the solar tide. This depth for the lunar tide is in fact nearly 13 miles.[1]

This discussion should have made it clear that any tidal theory, worthy of the name, must take account of motion, and it explains why the prediction of the equilibrium theory is so wide from the truth. Notwithstanding, however, this condemnation of the equilibrium theory, it is of the utmost service in the discussion of the tides, because by far the most convenient and complete way of specifying the forces which act on the ocean at each instant is to determine the figure which the ocean would assume, if the forces had abundant time to act.

When the sea is confined to an equatorial canal, the tidal problem is much simpler than when the ocean covers the whole planet, and this is much simpler than when the sea is interrupted by continents. Then again, we have thus far supposed the sun and moon to be always exactly over the

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[1]It is worthy of remark that if the canal had a depth of between $13\frac{3}{4}$ and 13 miles, the solar tides would be inverted, and the lunar tides would be direct. We should then, at the equator, have spring tide at half moon, when our actual neaps occur; and neap tide at full and change, when our actual springs occur. The tides would also be of enormous height, because the depth is nearly such as to make both tides quasi-infinite. If the depth of the canal were very nearly $13\frac{3}{4}$ miles the solar tide might be greater than the lunar. But these exceptional cases have only a theoretical interest.

equator, whereas they actually range a long way both to the north and to the south of the equator; and so here also the true problem is more complicated than the one under consideration. Let us next consider a case, still far simpler than actuality, and suppose that whilst the moon or sun still always move over the equator, the ocean is confined to several canals which run round the globe, following parallels of latitude.

The circumference of a canal in latitude 60° is only 12,500 miles, instead of 25,000. If a free wave were generated in such a canal with two crests at opposite sides of the globe, the distance from crest to crest would be 6,250 miles. Now if an equatorial canal and one in latitude 60° have equal depths, a free wave will travel at the same rate along each; and if in each canal there be a wave with two antipodal crests, the time occupied by the wave in latitude 60° in traveling through a space equal to its length will be only half of the similar period for the equatorial waves. The period of a free wave in latitude 60° is therefore half what it is at the equator, for a pair of canals of equal depths. But there is only one sun, and it takes 12 hours to go half round the planet, and therefore for both canals the forced tide wave has a period of 12 hours. If, for example, both canals were 8 miles deep, in the equatorial canal the period of the free wave would be greater than 12 hours, whilst in the canal at 60° of latitude it would be less than 12 hours. It follows then from the general principle as to forced and free oscillations, that whilst the tide in the equatorial canal would be inverted, that in latitude 60° would be direct. Therefore, whilst it would be low water under the moon at the equator, it would be high water under the moon in latitude 60°. Somewhere, between latitude 60° and the equator, there must be a place at which the free period in a canal 8 miles deep is the same as the forced period, and in a canal at this latitude the tide would be infinite in height, in the modified sense explained earlier. It follows therefore that there is for any given depth of canal, less than 14 miles, a critical latitude, at which the tide tends to become infinite in height.

We conclude, that if the whole planet were divided up into canals each partitioned off from its neighbor, and if the canals were shallower than 14 miles, we should have inverted tides in the equatorial region, and direct tides in the polar regions, and, in one of the canals in some middle latitude, very great tides the nature of which cannot be specified exactly.

The supposed partitions between neighboring canals have introduced a limitation which must be removed, if we are to approach actuality, but I am unable by general reasoning to do more than indicate what will be the effect of the removal of the partitions. It is clear that when the sea swells up to form the high water, the water comes not only from the east and the west of the place of high water, but also from the north and south. The earth, as it rotates, carries with it the ocean; the equatorial water is carried over a space of 25,000 miles in 24 hours, whereas the water in latitude 60° is carried over only 12,500 miles in the same time. When, in the northern hemisphere, water moves from north to south it passes from a place where the surface of the earth is moving slower, to where it is moving quicker. Then, as the water goes to the south, it carries with it only the velocity adapted to the northern latitude, and so it gets left behind by the earth. Since the earth spins from west to east, a southerly current acquires a westward trend. Conversely, when water is carried northward of its proper latitude, it leaves the earth behind and is carried eastward. Hence the water cannot oscillate northward and southward, without at the same time oscillating eastward and westward. Since in an ocean not partitioned into canals, the water must necessarily move not only east and west but also north and south, it follows that tidal movements in the ocean must result in eddies or vortices. The eddying motion of the water must exist everywhere, but it would be impossible, without mathematical reasoning, to explain how all the eddies fit into one another in time and place. It must suffice for the present discussion for the reader to know that the full mathematical treatment of the problem shows this general conclusion to be correct.

The very difficult mathematical problem of the tides of an ocean covering the globe to a uniform depth was first successfully attacked by Laplace. He showed that whilst the tides of a shallow ocean are inverted at the equator, as proved by Newton, that they are direct towards the pole. We have just arrived at the same conclusion by considering the tide wave in a canal in latitude 60°. But our reasoning indicated that somewhere in between higher latitudes and the equator, the tide would be of an undefined character, with an enormous range of rise and fall. The complete solution of the problem shows, however, that this indication of the canal theory is wrong, and that the tidal variation of level absolutely vanishes in some latitude intermediate between the equator and the pole. The conclusion of the mathematician is that there is a certain circle of latitude, whose position depends on the depth of the sea, where there is neither rise nor fall of tide.

At this circle the water flows northward and southward, and to and fro between east and west, but in such a way as never to raise or depress the level of the sea. It is not true to say that there is no tide at this circle, for there are tidal currents without rise and fall. When the ocean was supposed to be cut into canals, we thereby obliterated the northerly and southerly currents, and it is exactly these currents which prevent the tides becoming very great, as we were then led to suppose they would be.

It may seem strange that, whereas the first rough solution of the problem indicates an oscillation of infinite magnitude at a certain parallel of latitude, the more accurate treatment of the case should show that there is no oscillation of level at all. Yet to the mathematician such a result is not a cause of surprise. But whether strange or not, it should be clear that if at the equator it is low water under the moon, and if near the pole it is high water under the moon, there must in some intermediate latitude be a place where the water is neither high nor low, that is to say, where there is neither rise nor fall.[1]

Now let us take one more step towards actuality, and suppose the earth's equator to be oblique to the orbits of the moon and sun, so that they may sometimes stand to the north and sometimes to the south of the equator. We have seen that in this case the equilibrium theory indicates that the two successive tides on any one day have unequal ranges. The mathematical solution of the problem shows that this

[1]The mathematician knows that a quantity may change sign, either by passing through infinity or through zero. Where a change from positive to negative undoubtedly takes place, and where a passage through infinity can have no physical meaning, the change must take place by passage through zero.

conclusion is correct. It appears also that if the ocean is deeper at the poles than at the equator, that tide is the greater which is asserted to be so by the equilibrium theory. If, however, the ocean is shallower at the poles than at the equator, it is found that the high water which the equilibrium theory would make the larger is actually the smaller and vice versa.

If the ocean is of the same depth everywhere, we have a case intermediate between the two, where it is shallower at the poles, and where it is deeper at the poles. Now in one of these cases it appears that the higher high water occurs where in the other we find the lower high water to occur; and so, when the depth is uniform, the higher high water and the lower high water must attain the same heights. We thus arrive at the remarkable conclusion that, in an ocean of uniform depth, the diurnal inequality of the tide is evanescent. There are, however, diurnal inequalities in the tidal currents, which are so adjusted as not to produce a rise or fall. This result was first arrived at by the great mathematician Laplace.

According to the equilibrium theory, when the moon stands some distance north of the equator, the inequality between the successive tides on the coasts of Europe should be very great, but the difference is actually so small as to escape ordinary observation. In the days of Laplace, the knowledge of the tides in other parts of the world was very imperfect, and it was naturally thought that the European tides were fairly representative of the whole world. When, then, it was discovered that there would be no diurnal inequality in an ocean of uniform depth covering the whole globe, it was thought that a fair explanation had been found for the absence of that inequality in Europe. But since the days of Laplace much has been learnt about the tides in the Pacific and Indian oceans, and we now know that a large diurnal inequality is almost universal, so that the tides of the North Atlantic are exceptional in their simplicity. In fact, the evanescence of the diurnal inequality is not much closer to the truth than the large inequality predicted by the equilibrium theory; and both theories must be abandoned as satisfactory explanations of the true condition of affairs. But notwithstanding their deficiencies both these theories are of importance in teaching us how the tides are to be predicted. In the next chapter I shall show how a further approximation to the truth is attainable.

Authorities.

The canal theory in its elementary form is treated in many works on Hydrodynamics, and in *Tides*, "Encyclopædia Britannica."

An elaborate treatment of the subject is contained in Airy's *Tides and Waves*, "Encyclopædia Metropolitana." Airy there attacks Laplace for his treatment of the wider tidal problem, but his strictures are now universally regarded as unsound.

Laplace's theory is contained in the *Mécanique Céleste*, but it is better studied in more recent works.

A full presentment of this theory is contained in Professor Horace Lamb's *Hydrodynamics*, Camb. Univ. Press, 1895, chapter viii.

Important papers, extending Laplace's work, by Mr. S. S. Hough, are contained in the *Philosophical Transactions of the Royal Society*, A. 1897, pp. 201–258, and A. 1898, pp. 139–185.

CHAPTER X

TIDES IN LAKES—COTIDAL CHART

If the conditions of the tidal problem are to agree with reality, an ocean must be considered which is interrupted by continental barriers of land. The case of a sea or lake entirely surrounded by land affords the simplest and most complete limitation to the continuity of the water. I shall therefore begin by considering the tides in a lake.

The oscillations of a pendulum under the tidal attraction of the moon were considered in Chapter VI., and we there saw that the pendulum would swing to and fro, although the scale of displacement would be too minute for actual observation. Now a pendulum always hangs perpendicularly to the surface of water, and must therefore be regarded as a sort of level. As it sways to and fro under the changing action of the tidal force, so also must the surface of water. If the water in question is a lake, the rocking of the level of the lake is a true tide.

A lake of say a hundred miles in length is very small compared with the size of the earth, and its waters must respond almost instantaneously to the changes in the tidal force. Such a lake is not large enough to introduce, to a perceptible extent, those complications which make the complete theory of oceanic tides so difficult. The equilibrium theory is here actually true, because the currents due to the changes in the tidal force have not many yards to run before equilibrium is established, and the lake may be regarded as a level which responds almost instantaneously to the tidal deflections of gravity. The open ocean is a great level also, but sufficient time is not allowed it to respond to the changes in the direction of gravity, before that direction has itself changed.

It was stated in Chapter V. that the maximum horizontal force due to the moon has an intensity equal to $\frac{1}{11{,}664{,}000}$ part of gravity, and that therefore a pendulum 10 metres long is deflected through $\frac{1}{11{,}664{,}000}$ of 10 metres, or through $\frac{1}{1{,}166}$ of a millimetre. Now suppose our lake, 200 kilometres in length, runs east and west, and that our pendulum is hung up at the middle of the lake, 100 kilometres from either end. In fig. 31 let C D represent the level of the lake as undisturbed, and A B an exaggerated pendulum. When the tide-generating force displaces the pendulum to A B′, the surface of the lake must assume the position C′ D′. Now A B being 10 metres, B B′ may range as far as $\frac{1}{1{,}166}$ of a millimetre; and it is obvious that C C′ must bear the same relation to C B that B B′ does to A B. Hence C C′ at its greatest may be $\frac{1}{11{,}664{,}000}$ of half the length of the lake. The lake is supposed to be twice 100 kilometres in length, and 100 kilometres is 10 million centimetres; thus C C′ is $\frac{1}{1.1664}$ centimetre, or $\frac{9}{10}$ of a centimetre. When the pendulum is deflected in the other direction the lake rocks the other way, and C′ is just as much above C as it was below it before. It follows from this that the lunar tide at the ends of a lake, 200 kilometres or 120 miles in length, has a range of $1\frac{3}{4}$ centimetres or $\frac{2}{3}$ of an inch. The solar tidal force is a little less than half as strong as that due to the moon, and when the two forces conspire together at the times of spring tide, we should find a tide with a range of $2\frac{1}{2}$ centimetres.

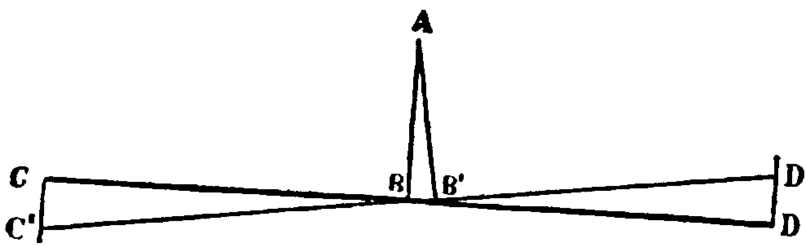

FIG. 31.—THE TIDE IN A LAKE

If the same rule were to apply to a lake 2,000 kilometres or 1,200 miles in length, the range of lunar tide would be about 17 centimetres or 7 inches, and the addition of solar tides would bring the range up to 25 centimetres or 10 inches. I dare say that, for a lake of such a size, this rule would not be very largely in error. But as we make the lake longer, the currents set up by the tidal forces have not sufficient time to produce their full effects before the intensity and direction of the tidal forces change. Besides this, if the lake were broad from north to south, the earth's rotation would have an appreciable effect, so that the water which flows from the north to the south would be deflected westward, and that which flows from south to north would tend to flow eastward. The curvature of the earth's surface must also begin to affect the motion. For these reasons, such a simple rule would then no longer suffice for calculating the tide.

Mathematicians have not yet succeeded in solving the tidal problem for a lake of large dimensions, and so it is impossible to describe the mode of oscillation. It may, however, be asserted that the shape, dimensions, and depth of the lake, and the latitudes of its boundaries will affect the result. The tides on the northern and southern shores will be different, and there will be nodal lines, along which there will be no rise and fall of the water.

The Straits of Gibraltar are so narrow, that the amount of water which can flow through them in the six hours which elapse between high and low water in the Atlantic is inconsiderable. Hence the Mediterranean Sea is virtually a closed lake. The tides of this sea are much complicated by the constriction formed by the Sicilian and Tunisian promontories.

Its tides probably more nearly resemble those of two lakes than of a single sheet of water. The tides of the Mediterranean are, in most places, so inconspicuous that it is usually, but incorrectly, described as a tideless sea. Every visitor to Venice must, however, have seen, or may we say smelt, the tides, which at springs have a range of some four feet. The considerable range of tide at Venice appears to indicate that the Adriatic acts as a resonator for the tidal oscillation, in the same way that a hollow vessel, tuned to a particular note, picks out and resonates loudly when that note is sounded.

We see, then, that whilst the tides of a small lake are calculable by the equilibrium theory, those of a large one, such as the Mediterranean, remain intractable. It is clear, then, that the tides of the ocean must present a problem yet more complex than those of a large lake.

In the Pacific and Southern oceans the tidal forces have almost uninterrupted sway, but the promontories of Africa and of South America must profoundly affect the progress of the tide wave from east to west. The Atlantic Ocean forms a great bay in this vaster tract of water. If this inlet were closed by a barrier from the Cape of Good Hope to Cape Horn, it would form a lake large enough for the generation of much larger tides than those of the Mediterranean Sea, although probably much smaller than those which we actually observe on our coasts. Let us now suppose that the tides proper to the Atlantic are non-existent, and let us remove the barrier between the two capes. Then the great tide wave sweeps across the Southern ocean from east to west, and, on reaching the tract between Africa and South America, generates a wave which travels northward up the Atlantic inlet. This secondary wave travels "freely," at a rate dependent only on the depth of the ocean. The energy of the wave motion is concentrated, where the channel narrows between North Africa and Brazil, and the height of the wave must be augmented in that region. Then the energy is weakened by spreading, where the sea broadens again, and it is again reconcentrated by the projection of the North American coast line towards Europe. Hence, even in this case, ideally simplified as it is by the omission of the direct action of the moon and sun, the range of tide would differ at every portion of the coasts on each side of the Atlantic.

The time of high water at any place must also depend on the varying depth of the ocean, for it is governed by the time occupied by the "free wave" in traveling from the southern region to the north. But in the south, between the two capes of Africa and South America, the tidal oscillation is constrained to keep regular time with the moon, and so it will keep the same rhythm at every place to the northward, at whatever variable pace the wave may move. The time of high water will of course differ at every point, being later as we go northward. The wave may indeed occupy so long on its journey, that one high water may have only just arrived at the northern coast of Africa, when another is rounding the Cape of Good Hope.

Under the true conditions of the case, this “free” wave, generated in and propagated from the southern ocean, is fused with the true “forced” tide wave generated in the Atlantic itself. It may be conjectured that on the coast of Europe the latter is of less importance than the former. It is interesting to reflect that our tides to-day depend even more on what occurred yesterday or the day before in the Southern Pacific and Indian oceans, than on the direct action of the moon to-day. But the relative importance of the two causes must remain a matter of conjecture, for the problem is one of insoluble complexity.

Some sixty years ago Whewell, and after him Airy, drew charts illustrative of what has just been described. A map showing the march of the tide wave is reproduced from Airy’s “Tides and Waves,” in fig. 32. It claims to show, from the observed times of high water at the various parts of the earth, how the tide wave travels over the oceans. Whewell and Airy were well aware that their map could only be regarded as the roughest approximation to reality. Much has been learnt since their days, and the then incomplete state of knowledge hardly permitted them to fully realize how very rough was their approximation to the truth. No more recent attempt has been made to construct such a map, and we must rest satisfied with this one. Even if its lines may in places depart pretty widely from the truth, it presents features of much interest. I do not reproduce the Pacific Ocean, because it is left almost blank, from deficiency of data. Thus, in that part of the world where the tides are most normal, and where the knowledge of them would possess the greatest scientific interest, we are compelled to admit an almost total ignorance.

The lines on the map, fig. 32, give the Greenwich times of high water at full and change of moon. They thus purport to represent the successive positions of the crest of the tide wave. For example, at noon and midnight (XII o'clock), at full and change of moon, the crest of the tide wave runs from North Australia to Sumatra, thence to Ceylon, whence it bends back to the Island of Bourbon, and, passing some hundreds of miles south of the Cape of Good Hope, trends away towards the Antarctic Ocean. At the same moment the previous tide crest has traveled up the Atlantic, and is found running across from Newfoundland to the Canary Islands. A yet earlier crest has reached the north of Norway. At this moment it is low water from Brazil to the Gold Coast, and again at Great Britain.

The successive lines then exhibit the progress of the wave from hour to hour, and we see how the wave is propagated

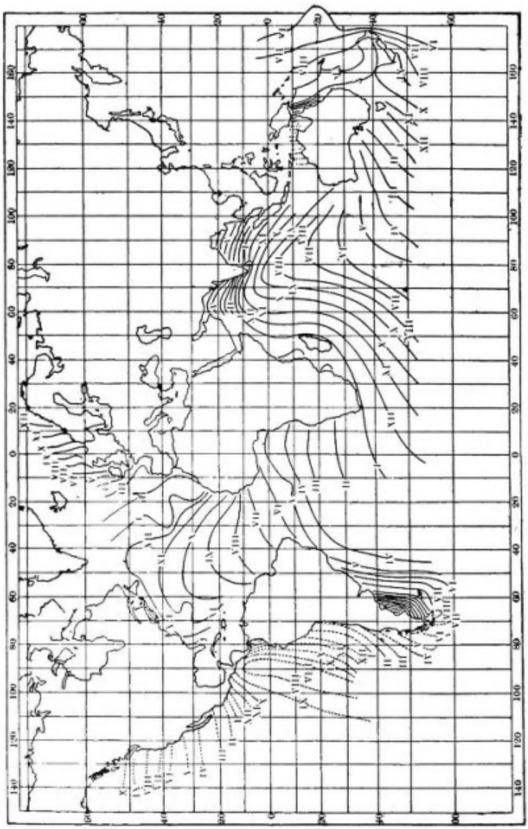

FIG. 32.—CHART OF COTIDAL LINES

into the Atlantic. The crowding together of lines in places is the graphical representation of the retardation of the wave, as it runs into shallower water.

But even if this chart were perfectly trustworthy, it would only tell us of the progress of the ordinary semidiurnal wave, which produces high water twice a day. We have, however, seen reason to believe that two successive tides should not rise to equal heights, and this figure does not even profess to give any suggestion as to how this inequality is propagated. In other words, it is impossible to say whether two successive tides of unequal heights tend to become more or less unequal, as they run into any of the great oceanic inlets. Thus the map affords no indication of the law of the propagation of the diurnal inequality.

This sketch of the difficulties in the solution of the full tidal problem might well lead to despair of the possibility of tidal prediction on our coasts. I shall, however, show in the next chapter how such prediction is possible.

Authorities.

For cotidal charts see Whewell, *Phil. Trans. Roy. Soc.* 1833, or Airy's *Tides and Waves*, "Encyclopædia Metropolitana."

CHAPTER XI

HARMONIC ANALYSIS OF THE TIDE

It is not probable that it will ever be possible to determine the nature of the oceanic oscillation as a whole with any accuracy. It is true that we have already some knowledge of the general march of the tide wave, and we shall doubtless learn more in the future, but this can never suffice for accurate prediction of the tide at any place.

Although the equilibrium theory is totally false as regards its prediction of the time of passage and of the height of the tide wave, yet it furnishes the stepping-stone leading towards the truth, because it is in effect a compendious statement of the infinite variety of the tidal force in time and place.

I will begin my explanation of the practical method of tidal prediction by obliterating the sun, and by supposing that the moon revolves in an equatorial circle round the earth. In this case the equilibrium theory indicates that each tide exactly resembles its predecessors and its successors for all time, and that the successive and simultaneous passages of the moon and of the wave crests across any place follow one another at intervals of 12 hours 25 minutes. It would always be exactly high water under or opposite to the moon, and the height of high water would be exactly determinate.

In actual oceans, even although only subject to the action of such a single satellite, the motion of the water would be so complex that it would be impossible to predict the exact height or time of high or of low water. But since the tidal forces operate in a stereotyped fashion day after day, there will be none of that variability which actually occurs on the real earth under the actions of the real sun and moon, and we may positively assert that whatever the water does to-day it will do to-morrow. Thus, if at a given place it is high water at a definite number of hours after the equatorial moon has crossed the meridian to-day, it will be so to-morrow at the same number of hours after the moon's passage, and the water will rise and fall every day to the same height above and below the mean sea level. If then we wanted to know how the tide would rise and fall in a given harbor, we need only watch the motion of the sea at that place, for however the water may move elsewhere its motion will always produce the same result at the port of observation. Thus, apart from the effects of wind, we should only have to note the tide on any one day to be able to predict it for all time. For by a single day of observation it would be easy to note how many hours after the moon's passage high water occurs, and how many feet it rises and falls with reference to some fixed mark on the shore. The delay after the moon's passage and the amount of rise and fall would differ geographically, but at each place there would be two definite numbers giving the height of the tide and the interval after the moon's passage until high water. These two numbers are called the tidal constants for the port; they would virtually contain tidal predictions for all time.

Now if the moon were obliterated, leaving the sun alone, and if he also always moved over the equator, a similar rule would hold good, but exactly 12 hours would elapse from one high water to the next, instead of 12 hours 25 minutes as in the case of the moon's isolated action. Thus two other tidal constants, expressive of height and interval, would virtually contain tidal prediction for the solar tide for all time.

Theory here gives us some power of foreseeing the relative importance of the purely lunar and of the purely solar tide. The two waves due to the sun alone or to the moon alone have the same character, but the solar waves follow one another a little quicker than the lunar waves, and the sun's force is a little less than half the moon's force. The close similarity between the actions of the sun and moon makes it safe to conclude that the delay of the isolated solar wave after the passage of the sun would not differ much from the delay of the isolated lunar wave after the passage of the moon, and that the height of the solar wave would be about half of that of the lunar wave. But theory can only be trusted far enough to predict a rough proportionality of the heights of the two tide waves to their respective generating forces, and the approximate equality of the intervals of retardation; but the height and retardation of the solar wave could not be accurately foretold from observation of the lunar wave.

When the sun and moon coëxist, but still move in equatorial circles, the two waves, which we have considered separately, are combined. The four tidal constants, two for the moon and two for the sun, would contain the prediction of the height of water for all time, for it is easy at any future moment of time to discover the two intervals of time since the moon and since the sun have crossed the meridian of the place of observation; we should then calculate the height of the water above some mark on the shore on the supposition that the moon exists alone, and, again, on the supposition that the sun exists alone, and adding the two results together, should obtain the required height of the water at the moment in question.

But the real moon and sun do not move in equatorial circles, but in planes which are oblique to the earth's equator, and they are therefore sometimes to the north and sometimes to the south of the equator; they are also sometimes nearer and sometimes further from the earth on account of the eccentricity of the orbits in which they move. Now the mathematician treats this complication in the following way: he first considers the moon alone and replaces it by a number of satellites of various masses, which move in various planes. It is a matter of indifference that such a system of satellites could not maintain the orbits assigned to them if they were allowed to go free, but a mysterious being may be postulated who compels the satellites to move in the assigned orbits. One, and this is the largest of these ideal satellites, has nearly the same mass as the real moon and moves in a circle over the equator; it is in fact the simple isolated moon whose action I first considered. Another small satellite stands still amongst the stars; others move in such orbits that they are always vertically overhead in latitude 45°; others repel instead of attracting; and others move backwards amongst the stars. Now all these satellites are so arranged as to their masses and their orbits, that the sum of their tidal forces is exactly the same as those due to the real moon moving in her actual orbit.

So far the problem seems to be complicated rather than simplified, for we have to consider a dozen moons instead of one. The simplification, however, arises from the fact that each satellite either moves uniformly in an orbit parallel to the equator, or else stands still amongst the stars. It follows that each of the ideal satellites creates a tide in the ocean which is of a simple character, and repeats itself day after day in the same way as the tide due to an isolated equatorial moon. If all but one of these ideal satellites were obliterated the observation of the tide for a single day would enable us to predict the tide for all time; because it would only be necessary to note the time of high water after the ideal satellite had crossed the meridian, and the height of the high water, and then these two data would virtually contain a tidal prediction for that tide at the place of observation for all future time. The interval and height are together a pair of "tidal constants" for the particular satellite in question, and refer only to the particular place at which the observation is made.

In actuality all the ideal satellites coëxist, and the determination of the pair of tidal constants appropriate to any one of them has to be made by a complex method of analysis, of which I shall say more hereafter. For the present it will suffice to know that if we could at will annul all the ideal satellites except one, and observe its tide even for a single day, its pair of constants could be easily determined. It would then only be necessary to choose in succession all the satellites as subjects of observation, and the materials for a lunar tide table for all time would be obtained.

The motion of the sun round the earth is analogous to that of the moon, and so the sun has also to be replaced by a similar series of ideal suns, and the partial tide due to each of them has to be found. Finally at any harbor some twenty pairs of numbers, corresponding to twenty ideal moons and suns, give the materials for tidal prediction for all time. Theoretically an infinite number of ideal bodies is necessary for an absolutely perfect representation of the tides, but after we have taken some twenty of them, the remainder are found to be excessively small in mass, and therefore the tides raised by them are so minute that they may be safely omitted. This method of separating the tide wave into a number of partial constituents is called "harmonic analysis." It was first suggested, and put into practice as a practical treatment of the tidal problem, by Sir William Thomson, now Lord Kelvin, and it is in extensive use.

In this method the aggregate tide wave is considered as the sum of a number of simple waves following one another at exactly equal intervals of time, and always presenting a constant rise and fall at the place of observation. When the time of high water and the height of any one of these constituent waves is known on any one day, we can predict, with certainty, the height of the water, as due to it alone, at any future time however distant. The period of time which elapses between the passage of one crest and of the next is absolutely exact, for it is derived from a study of the motions of the moon or sun, and is determined to within a thousandth of a second. The instant at which any one of the satellites passes the meridian of the place is also known with absolute accuracy, but the interval after the passage of the satellite up to the high water of any one of these constituent waves, and the height to which the water will rise are only derivable from observation at each port.

Since there are about twenty coëxistent waves of sensible magnitude, a long series of observations is requisite for disentangling any particular wave from among the rest. The series must also be so long that the disturbing influence of the wind, both on height and time, may be eliminated by the taking of averages. It may be well to reiterate that each harbor has to be considered by itself, and that a separate set of tidal constants has to be found for each place. If it is only required to predict the tides with moderate accuracy some eight partial waves suffice, but if high accuracy is to be attained, we have to consider a number of the smaller ones, bringing the total up to 20 or 25.

When the observed tidal motions of the sea have been analyzed into partial tide waves, they are found to fall naturally into three groups, which correspond with the dissections of the sun and moon into the ideal satellites. In the first and most important group the crests follow one another at intervals of somewhere about 12 hours; these are called the semidiurnal tides. In the second group, the waves of which are in most places of somewhat less height than those of the semidiurnal group, the crests follow one another at intervals of somewhere about 24 hours, and they are called diurnal.

The tides of the third group have a very slow periodicity, for their periods are a fortnight, a month, half a year, and a year; they are commonly of very small height, and have scarcely any practical importance; I shall therefore make no further reference to them.

Let us now consider the semidiurnal group. The most important of these is called "the principal lunar semidiurnal tide." It is the tide raised by an ideal satellite, which moves in a circle round the earth's equator. I began my explanation of this method by a somewhat detailed consideration of this wave. In this case, the wave crests follow one another at intervals of 12 hours 25 minutes $14\frac{1}{6}$ seconds. The average interval of time between the successive visible transits of the moon over the meridian of the place of observation is 24 hours 50 minutes $28\frac{1}{3}$ seconds; and as the invisible transit corresponds to a tide as well as the visible one, the interval between the successive high waters is the time between the successive transits, of which only each alternate one is visible.

The tide next in importance is "the principal solar semidiurnal tide." This tide bears the same relationship to the real sun that the principal lunar semidiurnal tide bears to the real moon. The crests follow one another at intervals of exactly 12 hours, which is the time from noon to midnight and of midnight to noon. The height of this partial wave is, at most places, a little less than half of that of the principal lunar tide.

The interval between successive lunar tides is $25\frac{1}{4}$ minutes longer than that between successive solar tides, and as there are two tides a day, the lunar tide falls behind the solar tide by $50\frac{1}{2}$ minutes a day. If we imagine the two tides to start together with simultaneous high waters, then in about 7 days the lunar tide will have fallen about 6 hours behind the solar tide, because 7 times $50\frac{1}{2}$ minutes is 5 hours 54 minutes. The period from high water to low water of the principal solar semidiurnal tide is 6 hours, being half the time between successive high waters. Accordingly, when the lunar tide has fallen 6 hours behind the solar tide, the low water of the solar tide falls in with the high water of the lunar tide. It may facilitate the comprehension of this matter to take a numerical example; suppose then that the lunar tide rises 4 feet above and falls by the same amount below the mean level of the sea, and that the solar tide rises and falls 2 feet above and below the same level; then if the two partial waves be started with their high waters simultaneous, the joint wave will at first rise and fall by 6 feet. But after 7 days it is low solar tide when it is high lunar tide, and so the solar tide is subtracted from the lunar tide, and the compound wave has a height of 4 feet less 2 feet, that is to say, of 2 feet. After nearly another 7 days, or more exactly after $14\frac{1}{2}$ days from the start, the lunar tide has lost another 6 hours, so that it has fallen back 12 hours in all, and the two high waters agree together again, and the joint wave has again a rise and fall of 6 feet. When the two high waters conspire it is called spring tide, and when the low water of the solar tide conspires with the high water of the lunar tide, it is called neap tide. It thus appears that the principal lunar and principal solar semidiurnal tides together represent the most prominent feature of the tidal oscillation.

The next in importance of the semidiurnal waves is called the "lunar elliptic tide," and here the crests follow one another at intervals of 12 hours 39 minutes 30 seconds. Now the interval between the successive principal lunar tides was 12 hours 25 minutes 14 seconds; hence, this new tide falls behind the principal lunar tide by $14\frac{1}{2}$ minutes in each half day. If this tide starts so that its high water agrees with that of the principal lunar tide, then after $13\frac{3}{4}$ days from the start, its hollow falls in with the crest of the former, and in $27\frac{1}{2}$ days from the start the two crests agree again.

The moon moves round the earth in an ellipse, and if today it is nearest to the earth, in $13\frac{3}{4}$ days it will be furthest, and in $27\frac{1}{2}$ days it will be nearest again. The moon must clearly exercise a stronger tidal force and create higher tides when she is near than when she is far; hence every $27\frac{1}{2}$ days the tides must be larger, and halfway between they must be smaller. But the tide under consideration conspires with the principal lunar tide every $27\frac{1}{2}$ days, and, accordingly, the joint wave is larger every $27\frac{1}{2}$ days and smaller in between. Thus this lunar elliptic tide represents the principal effect of the elliptic motion of the moon round the earth. There are other semidiurnal waves besides the three which I have mentioned, but it would hardly be in place to consider them further here.

Now turning to the waves of the second kind, which are diurnal in character, we find three, all of great importance. In one of them the high waters succeed one another at intervals of 25 hours 49 minutes $9\frac{1}{2}$ seconds, and of the second and third, one has a period of about 4 minutes less than 24 hours and the other of about 4 minutes greater than the 24 hours. It would hardly be possible to show by general reasoning how these three waves arise from the attraction of three ideal satellites, and how these satellites together are a substitute for the actions of the true moon and sun. It must, however, be obvious that the oscillation resulting from three coëxistent waves will be very complicated.

All the semidiurnal tides result from waves of essentially similar character, although some follow one another a little more rapidly than others, and some are higher and some are lower. An accurate cotidal map, illustrating the progress of any one of these semidiurnal waves over the ocean, would certainly tell all that we care to know about the progress of all the other waves of the group.

Again, all the diurnal tides arise from waves of the same character, but they are quite diverse in origin from the semidiurnal waves, and have only one high water a day instead of two. A complete knowledge of the behavior of semidiurnal waves would afford but little insight into the behavior of the diurnal waves. At some time in the future the endeavor ought to be made to draw a diurnal cotidal chart distinct from the semidiurnal one, but our knowledge is not yet sufficiently advanced to make the construction of such a chart feasible.

All the waves of which I have spoken thus far are generated by the attractions of the sun and moon and are therefore called astronomical tides, but the sea level is also affected by other oscillations arising from other causes.

Most of the places, at which a knowledge of the tides is practically important, are situated in estuaries and in rivers. Now rain is more prevalent at one season than at another, and mountain snow melts in summer; hence rivers and estuaries are subject to seasonal variability of level. In many estuaries this kind of inequality may amount to one or two feet, and such a considerable change cannot be disregarded in tidal prediction. It is represented by inequalities with periods of a year and of half a year, which are called the annual and semiannual meteorological tides.

Then again, at many places, especially in the Tropics, there is a regular alternation of day and night breezes, the effect of which is to heap up the water in-shore as long as the wind blows in-land, and to lower it when the wind blows off-shore. Hence there results a diurnal inequality of sea-level, which is taken into account in tidal prediction by means of a "solar diurnal meteorological tide." Although these inequalities depend entirely on meteorological influences and have no astronomical counterpart, yet it is necessary to take them into account in tidal prediction.

But besides their direct astronomical action, the sun and moon exercise an influence on the sea in a way of which I have not yet spoken. We have seen how waves gradually change their shape as they progress in a shallow river, so that the crests become sharper and the hollows flatter, while the advancing slope becomes steeper and the receding one less steep. An extreme exaggeration of this sort of change of shape was found in the bore. Now it is an absolute rule, in the harmonic analysis of the tide, that the partial waves shall be of the simplest character, and shall have a certain standard law of slope on each side of their crests. If then any wave ceases to present this standard simple form, it is necessary to conceive of it as compound, and to build it up out of several simple waves. By the composition of a simple wave with other simple waves of a half, a third, a quarter of the wave length, a resultant wave can be built up which shall assume any desired form. For a given compound wave, there is no alternative of choice, for it can only be built up in one way. The analogy with musical notes is here complete, for a musical note of any quality is built up from a fundamental, together with its octave and twelfth, which are called overtones. So also the distorted tide wave in a river is regarded as consisting of simple fundamental tide, with over-tides of half and third length. The periods of these over-tides are also one half and one third of that of the fundamental wave.

Out in the open ocean, the principal lunar semidiurnal tide is a simple wave, but when it runs into shallow water at the coast line, and still more so in an estuary, it changes its shape. The low water lasts longer than the high water, and the time which elapses from low water to high water is usually shorter than that from high water to low water. The wave is in fact no longer simple, and this is taken into account by considering it to consist of a fundamental lunar semidiurnal wave with a period of 12 hours 50 minutes, of the first over-tide or octave with a period of 6 hours 25 minutes, of the second over-tide or twelfth with a period of 4 hours

17 minutes, and of the third over-tide or double octave with a period of 3 hours 13 minutes. In estuaries, the first over-tide of the lunar semidiurnal tide is often of great importance, and even the second is considerable; the third is usually very small, and the fourth and higher over-tides are imperceptible. In the same way over-tides must be introduced, to represent the change of form of the principal solar semidiurnal tide. But it is not usually found necessary to consider them in the cases of the less important partial tides. The octave, the twelfth, and the upper octave may be legitimately described as tides, because they are due to the attractions of the moon and of the sun, although they arise indirectly through the distorting influence of the shallowness of the water.

I have said above that about twenty different simple waves afford a good representation of the tides at any port. Out of these twenty waves, some represent the seasonal change of level in the water due to unequal rainfall and evaporation at different times of the year, and others represent the change of shape of the wave due to shallowing of the water. Deducting these quasi-tides, we are left with about twelve to represent the true astronomical tide. It is not possible to give an exact estimate of the number of partial tides necessary to insure a good representation of the aggregate tide wave, because the characteristics of the motion are so different at various places that partial waves, important at one place, are insignificant at others. For example, at an oceanic island the tides may be more accurately represented as the sum of a dozen simple waves than by two dozen in a tidal river.

The method of analyzing a tide into its constituent parts, of which I have now given an account, is not the only method by which the tides may be treated, but as it is the most recent and the best way, I shall not consider the older methods in detail. The nature of the procedure adopted formerly will, however, be indicated in Chapter XIII.

Authority.

G. H. Darwin, *Harmonic Analysis of Tidal Observations*: "Report to British Association." Southport, 1883.

An outline of the method is also contained in *Tides*, "Encyclopædia Britannica."

CHAPTER XII

REDUCTION OF TIDAL OBSERVATIONS

I HAVE now to explain the process by which the several partial tides may be disentangled from one another.

The tide gauge furnishes a complete tidal record, so that measurement of the tide curve gives the height of the water at every instant of time during the whole period of observation. The record may be supposed to begin at noon of a given day, say of the first of January. The longitude of the port of observation is of course known, and the Nautical Almanack gives the positions of the sun and moon on the day and at the hour in question, with perfect accuracy. The real moon has now to be replaced by a series of ideal satellites, and the rules for the substitution are absolutely precise. Accordingly, the position in the heavens of each of the ideal satellites is known at the moment of time at which the observations begin. The same is true of the ideal suns which replace the actual sun.

I shall now refer to only a single one of the ideal moons or suns, for, *mutatis mutandis*, what is true of one is true of all. It is easy to calculate at what hour of the clock, measured in the time of the place of observation, the satellite in question will be due south. If the ideal satellite under consideration were the one which generates the principal lunar semidiurnal tide, it would be due south very nearly when the real moon is south, and the ideal sun which generates the principal solar tide is south exactly at noon. But there is no such obvious celestial phenomenon associated with the transit of any other of the satellites, although it is easy to calculate the time of the southing of each of them. We have then to discover how many hours elapse after the passage of the particular satellite up to the high water of its tide wave. The height of the wave crest above, and the depression of the wave hollow below the mean water mark must also be determined. When this problem has been solved for all the ideal satellites and suns, the tides are said to be reduced, and the reduction furnishes the materials for a tide table for the place of observation.

The difficulty of finding the time of passage and the height of the wave due to any one of the satellites arises from the fact that all the waves really coëxist, and are not separately manifest. The nature of the disentanglement may be most easily explained from a special case, say for example that of the principal lunar semidiurnal tide, of which the crests follow one another at intervals of 12 hours 25 minutes $14\frac{1}{6}$ seconds.

Since the waves follow one another at intervals of approximately, but not exactly, a half-day, it is convenient to manipulate the time scale so as to make them exactly semidiurnal. Accordingly we describe 24 hours 50 minutes $28\frac{1}{3}$ seconds as a lunar day, so that there are exactly two waves following one another in the lunar day.

The tide curve furnishes the height of the water at every moment of time, but the time having been registered by the clock of the tide gauge is partitioned into ordinary days and hours. It may, however, be partitioned at intervals of 24 hours 50 minutes $28\frac{1}{3}$ seconds, and into the twenty-fourth parts of that period, and it will then be divided into lunar days and hours. On each lunar day the tide for which we are searching presents itself in the same way, so that it is always high and low water at the same hour of the lunar clock, with exactly two high waters and two low waters in the lunar day.

Now the other simple tides are governed by other scales of time, so that in a long succession of days their high waters and low waters occur at every hour of the lunar clock. If then we find the average curve of rise and fall of the water, when the time is divided into lunar days and hours, and if we use for the average a long succession of days, all the other tide waves will disappear, and we shall be left with only the lunar semidiurnal tide, purified from all the others which really coëxist with it.

The numerical process of averaging thus leads to the obliteration of all but one of the ideal satellites, and this is the foundation of the method of harmonic analysis. The average lunar tide curve may be looked on as the outcome of a single day of observation, when all but the selected satellite have been obliterated. The height of the average wave, and the interval after lunar noon up to high water, are the two tidal constants for the lunar semidiurnal tide, and they enable us to foretell that tide for all future time.

If the tide curve were partitioned into other days and hours of appropriate lengths, it would be possible by a similar process of averaging to single out another of the constituent tide waves, and to determine its two tidal constants, which contain the elements of prediction with respect to it. By continued repetition of operations of this kind, all the constituents of practical importance can be determined, and recorded numerically by means of their pairs of tidal constants.

The possibility of the disentanglement has now been demonstrated, but the work of carrying out these numerical operations would be fearfully laborious. The tide curve would have to be partitioned into about a dozen kinds of days of various lengths, and the process would entail measurements at each of the 24 hours of each sort of day throughout the whole series. There are about nine thousand hours in a year, and it would need about a hundred thousand measurements of the curve to evaluate twelve different partial tides; each set of measured heights would then have to be treated separately to find the several sorts of averages. Work of this kind has usually to be done by paid computers, and the magnitude of the operation would make it financially prohibitive. It is, however, fortunately possible to devise abridged methods, which bring the work within manageable limits.

In order to minimize the number of measurements, the tide curve is only measured at each of the 24 exact hours of ordinary time, the height at noon being numbered 0 hr., and that at midnight 12 hrs., and so on up to 24 hrs. After obtaining a set of 24 measurements for each day, the original tide curve is of no further use. The number of measurements involved is still large, but not prohibitive. It would be somewhat too technical, in a book of this kind, to explain in detail how the measured heights of the water at the exact hours of ordinary time may be made to give, with fair approximation, the heights at the exact hours of other time scales. It may, however, be well to explain that this approximate method is based on the fact, that each exact hour of any one of the special time scales must of necessity fall within half an hour of one of the exact hours of ordinary time. The height of the water at the nearest ordinary hour is then accepted as giving the height at the exact hour of the special time. The results, as computed in this way, are subjected to a certain small correction, which renders the convention accurate enough for all practical purposes.

A schedule, serviceable for all time and for all places, is prepared which shows the hour of ordinary time lying nearest to each successive hour of any one of the special times. The successive 24 hourly heights, as measured on the tide curve, are entered in this schedule, and when the entry is completed the heights are found to be arranged in columns, which follow the special time scale with a sufficiently good approximation to accuracy. A different form of schedule is required for each partial tide, and the entry of the numbers therein is still enormously laborious, although far less so than the re-partitions and re-measurements of the tide curve would be.

The operation of sorting the numbers into schedules has been carried out in various ways. In the work of the Indian Survey, the numbers have been re-copied over and over again. In the office of the United States Coast Survey use is made of certain card templates pierced with holes. These templates are laid upon the tabulation of the measurements of the tide curve, and the numbers themselves are visible through the holes. On the surface of the template lines are drawn from hole to hole, and these lines indicate the same grouping of the numbers as that given by the Indian schedules. Dr. Börgen, of the Imperial German Marine Observatory at Wilhelmshaven, has used sheets of tracing paper to attain the same end. The Indian procedure is unnecessarily laborious, and the American and German plans appear to have some disadvantage in the fact that the numbers to be added together lie diagonally across the page. I am assured by some professional computers that diagonal addition is easy to perform correctly; nevertheless this appeared to me to be so serious a drawback, that I devised another plan by which the numbers should be brought into vertical columns, without the necessity of re-copying them. In my plan each day is treated as a unit and is shifted appropriately. It might be thought that the results of the grouping would be considerably less accurate than in the former methods, but in fact there is found to be no appreciable loss of accuracy.

I have 74 narrow writing-tablets of xylonite, divided by lines into 24 compartments; the tablets are furnished with spikes on the under side, so that they can be fixed temporarily in any position on an ordinary drawing-board. The compartments on each strip are provided for the entry of the 24 tidal measurements appertaining to each day. Each strip is stamped at its end with a number specifying the number of the day to which it is appropriated.

The arrangement of these little tablets, so that the numbers written on them may fall into columns, is indicated by a sheet of paper marked with a sort of staircase, which shows where each tablet is to be set down, with its spikes piercing the guide sheet. When the strips are in place, as shown in fig. 33, the numbers fall into 48 columns, numbered 0, 1, ... 23, 0, 1, ... 23 twice over. The guide sheet shown in the figure 33 is the one appropriate for the lunar semidiurnal tide for the fourth set of 74 days of a year of observation. The upper half of the tablets are in position, but the lower ones are left unmounted, so as the better to show the staircase of marks.

Then I say that the average of all the 74 numbers standing under the two 0's combined will give the average height of water at 0 hr. of lunar time, and the average of the numbers under 1, that at 1 hr. of lunar time, and so forth. Thus, after the strips are pegged out, the computer has only to add the numbers in columns in order to find the averages. There are other sheets of paper marked for such other rearrangements of the strips that each new setting gives one of the required results; thus a single writing of the numbers serves for the whole computation. It is usual to treat a whole year of observations at one time, but the board being adapted for taking only 74 successive days, five series of writings are required for 370 days, which is just over a year. The number 74 was chosen for simultaneous treatment, because 74 days is almost exactly five semilunations, and accordingly there will always be five spring tides on record at once.

In order to guard the computer against the use of the

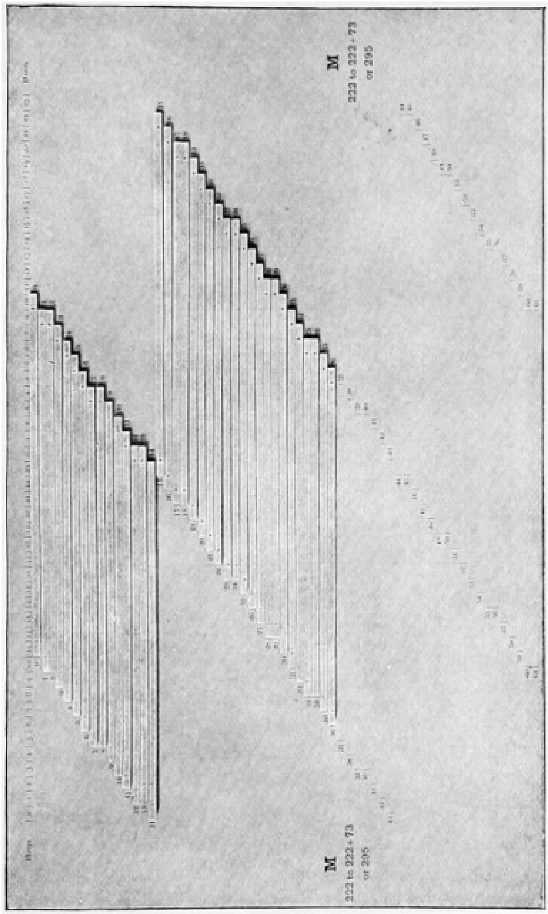

Fig. 33.—Tidal Abacus

wrong paper with any set of strips, the guide sheets for the first set of 74 days are red; for the second they are yellow; for the third green; for the fourth blue; for the fifth violet, the colors being those of the rainbow.

The preparation of these papers entailed a great deal of calculation in the first instance, but the tidal computer has merely to peg out the tablets in their right places, verifying that the numbers stamped on the ends of the strips agree with the numbers on the paper. The addition of the long columns of figures is certainly laborious, but it is a necessary incident of every method of reducing tidal observations.

The result of all the methods is that for each partial tide we have a set of 24 numbers, which represent the oscillations of the sea due to the isolated action of one of the ideal satellites, during the period embraced between two successive passages of that satellite to the south of the place of observation. The examination of each partial tide wave gives its height, and the interval of time which elapses after its satellite has passed the meridian until it is high water for that particular tide. The height and interval are the tidal constants for that particular tide, at the port of observation.

The results of this "reduction of the observations" are contained in some fifteen or twenty pairs of tidal constants, and these numbers contain a complete record of the behavior of the sea at the place in question.

Authorities.

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G. H. Darwin, *On an apparatus for facilitating the reduction of tidal observations*: "Proceedings of the Royal Society," vol. lii. 1892.

CHAPTER XIII

TIDE TABLES

A TIDE TABLE professes to tell, at a given place and on a given day, the time of high and low water, together with the height of the rise and the depth of the fall of the water, with reference to some standard mark on the shore. A perfect tide table would tell the height of the water at every moment of the day, but such a table would be so bulky that it is usual to predict only the high and low waters.

There are two kinds of tide table, namely, those which give the heights and times of high and low water for each successive day of each year, and those which predict the high and low water only by reference to some conspicuous celestial phenomenon. Both sorts of tide table refer only to the particular harbor for which they are prepared.

The first kind contains definite forecasts for each day, and may be called a special tide table. Such a table is expensive to calculate, and must be published a full year beforehand. Special tide tables are published by all civilized countries for their most important harbors. I believe that the most extensive publications are those of the Indian Government for the Indian Ocean, and of the United States Government for the coasts of North America. The Indian tables contain predictions for about thirty-seven ports.

The second kind of table, where the tide is given by reference to a celestial phenomenon, may be described as a general one. It is here necessary to refer to the Nautical Almanack for the time of occurrence of the celestial phenomenon, and a little simple calculation must then be made to obtain the prediction. The phenomenon to which the tide is usually referred is the passage of the moon across the meridian of the place of observation, and the table states that high and low water will occur so many hours after the moon's passage, and that the water will stand at such and such a height.

The moon, at her change, is close to the sun and crosses the meridian at noon; she would then be visible but for the sun's brightness, and if she did not turn her dark side towards us. She again crosses the meridian invisibly at midnight. At full moon she is on the meridian, visibly at midnight, and invisibly at noon. At waxing half moon she is visibly on the meridian at six at night, and at waning half moon at six in the morning. The hour of the clock at which the moon passes the meridian is therefore in effect a statement of her phase. Accordingly the relative position of the sun and moon is directly involved in a statement of the tide as corresponding to a definite hour of the moon's passage. A table founded on the time of the moon's passage must therefore involve the principal lunar and solar semidiurnal tides.

At places where successive tides differ but little from one another, a simple table of this kind suffices for rough predictions. The curves marked Portsmouth in fig. 34 show graphically the interval after the moon's passage, and the height of high water at that port, for all the hours of the moon's passage. We have seen in Chapter X. that the tide in the North Atlantic is principally due to a wave propagated from the Southern Ocean. Since this wave takes a considerable time to travel from the Cape of Good Hope to England, the tide here depends, in great measure, on that generated in the south at a considerable time earlier. It has therefore been found better to refer the high water to a transit of the moon which occurred before the immediately preceding one. The reader will observe that it is noted on the upper figure that 28 hours have been subtracted from the Portsmouth intervals; that is to say, the intervals on the vertical scale marked 6, 7, 8 hours are, for Portsmouth, to be interpreted as meaning 34, 35, 36 hours. These are the hours which elapse after any transit of the moon up to high water. The horizontal scale is one of the times of moon's transit and of phases of the moon; the vertical scale in the lower figure is one of feet, and it shows the height to which the water will rise measured from a certain mark ashore. These Portsmouth curves do not extend beyond 12 o'clock of moon's transit; this is because there is hardly any diurnal inequality, and it is not necessary to differentiate the hours as either diurnal or nocturnal, the statement being equally true of either day or night. Thus if the Portsmouth curves had been extended onward from 12 hours to 24 hours of the clock time of the moon's passage, the second halves of the curves would have been merely the duplicates of the first halves.[1]

But the time of the moon's passage leaves her angular distance from the equator and her linear distance from the earth indeterminate; and since the variability of both of these has its influence on the tide, corrections are needed which add something to or subtract something from the tabular values of the interval and height, as dependent solely on the time of the moon's passage. The sun also moves in a plane which is oblique to the equator, and so similar allowances must be made for the distance of the sun from the equator, and for the variability in his distance from the earth. In order

[1]Before the introduction of the harmonic analysis of the tides described in preceding chapters, tidal observations were "reduced" by the construction of such figures as these, directly from the tidal observations. Every high water was tabulated as appertaining to a particular phase of the moon, both as to its height and as to the interval between the moon's transit and the occurrence of high water. The average of a long series of observations may be represented in the form of curves by such figures as these.

to attain accuracy with a tide table of this sort, eight or ten corrections are needed, and the use of the table becomes complicated.

It is, however, possible by increasing the number of such figures or tables to introduce into them many of the corrections referred to; and the use of a general tide table then becomes comparatively simple. The sun occupies a definite position with reference to the equator, and stands at a definite distance from the earth on each day of the year; also the moon's path amongst the stars does not differ very much from the sun's. Accordingly a tide table which states the interval after the moon's passage to high or low water and the height of the water on a given day of the year will directly involve the principal inequalities in the tides. As the sun moves slowly amongst the stars, a table applicable to a given day of the year is nearly correct for a short time before and after that date. If, then, a tide table, stating the time and height of the water by reference to the moon's passage, be computed for say every ten days of the year, it will be very nearly correct for five days before and for five days after the date for which it is calculated.

The curves marked Aden, March and June, in fig. 34, show the intervals and heights of tide, on the 15th of those months at that port, for all the hours of the moon's passage. The curves are to be read in the same way as those for

Portsmouth, but it is here necessary to distinguish the hours of the day from those of the night, and accordingly the clock times of moon's transit are numbered from 0 hr. at noon up to 24 hrs. at the next noon. The curves for March differ so much from those for June, that the corrections would be very large, if the tides were treated at Aden by a single pair of average curves as at Portsmouth.

The law of the tides, as here shown graphically, may also be stated numerically, and the use of such a table is easy. The process will be best explained by an example, which happens to be retrospective instead of prophetic. It will involve that part of the complete table (or series of curves) for Aden which applies to the 15th of March of any year. Let it be required then to find the time and height of high water on March 17, 1889. The Nautical Almanack for that year shows that on that day the moon passed the meridian of Aden at eleven minutes past noon of Aden time, or in astronomical language at 0 hr. 11 mins. Now the table, or the figure of intervals, shows that if the moon had passed at 0 hr., or exactly at noon, the interval would have been 8 hrs. 9 mins., and that if she had passed at 0 hr. 20 mins., or 12.20 P.M. of the day, the interval would have been 7 hrs. 59 mins. But on March 17th the moon actually crossed at 0 hr. 11 mins., very nearly halfway between noon and 20 mins. past noon. Hence the interval was halfway between 8 hrs. 9 mins. and

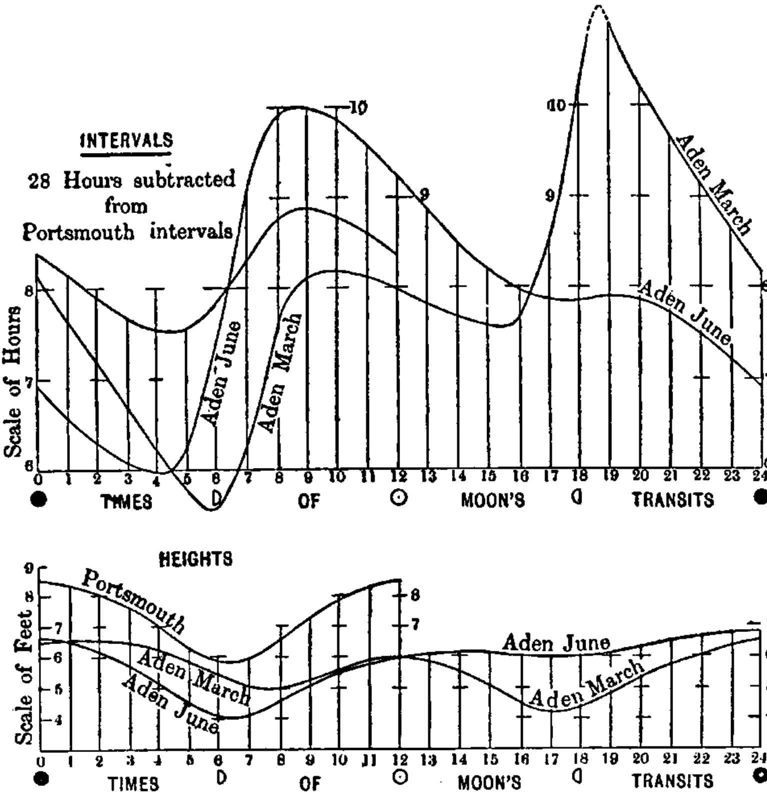

Fig. 34.—Curves of Intervals and Heights at Portsmouth and at Aden

7 hrs. 59 mins., so that it was 8 hrs. 4 mins. Accordingly it was high water 8 hrs. 4 mins. after the moon crossed the meridian. But the moon crossed at 0 hr. 11 mins., therefore the high water occurred at 8.15 P.M.

Again the table of heights, or the figure, shows that on March 15th, if the moon crossed at 0 hr. 0 min. the high water would be 6.86 ft. above a certain mark ashore, and if she crossed at 0 hr. 20 mins. the height would be 6.92 ft. But on March 17th the moon crossed halfway between 0 hr. 0 min. and 0 hr. 20 mins., and therefore the height was halfway between 6.86 ft. and 6.92 ft., that is to say, it was 6.89 ft., or 6 ft. 11 in. We therefore conclude that on March 17, 1889, the sea at high water rose to 6 ft. 11 in., at 8.15 P.M. I have no information as to the actual height and time of high water on that day, but from the known accuracy of other predictions at Aden we may be sure that this agrees pretty nearly with actuality. The predictions derived from this table are markedly improved when a correction, either additive or subtractive, is applied, to allow for the elliptic motion of the moon round the earth. On this particular occasion the moon stood rather nearer the earth than the average, and therefore the correction to the height is additive; the correction to the time also happens to be additive, although it could not be foreseen by general reasoning that this would be the case. The corrections for March 17, 1889, are found to add about 2 mins. to the time, bringing it to 8.17 P.M., and nearly two inches to the height, bringing it to 7 ft. 1 in.

This sort of elaborate general tide table has been, as yet, but little used. It is expensive to calculate, in the first instance, and it would occupy two or three pages of a book. The expense is, however, incurred once for all, and the table is available for all time, provided that the tidal observations on which it is based have been good. A sea captain arriving off his port of destination would not take five minutes to calculate the two or three tides he might require to know, and the information would often be of the greatest value to him.

As things stand at present, a ship sailing to most Chinese, Pacific, or Australian ports is only furnished with a statement, often subject to considerable error, that the high water will occur at so many hours after the moon's passage and will rise so many feet. The average rise at springs and neaps is generally stated, but the law of the variability according to the phases of the moon is wanting. But this is not the most serious defect in the information, for it is frequently noted that the tide is "affected by diurnal inequality," and this note is really a warning to the navigator that he cannot foretell the time of high water within two or three hours of time, or the height within several feet.

Tables of the kind I have described would banish this extreme vagueness, but they are more likely to be of service at ports of second-rate importance than at the great centres of trade, because at the latter it is worth while to compute full special tide tables for each year.

It is unnecessary to comment on the use of tables containing predictions for definite days, since it merely entails reference to a book, as to a railway time table. Such special tables are undoubtedly the most convenient, but the number of ports which can ever be deemed worthy of the great expense incidental to their preparation must always be very limited.

We must now consider the manner in which tide tables are calculated. It is supposed that careful observations have been made, and that the tidal constants, which state the laws governing the several partial tides, have been accurately determined by harmonic analysis. The analysis of tidal observations consists in the dissection of the aggregate tide wave into its constituent partial waves, and prediction involves the recomposition or synthesis of those waves. In the synthetic process care must be taken that the partial waves shall be recompounded in their proper relative positions, which are determined by the places of the moon and sun at the moment of time chosen for the commencement of prediction.

The synthesis of partial waves may be best arranged in two stages. It has been shown in Chapter XI. that the partial waves fall naturally into three groups, of which the third is practically insignificant. The first and second are the semidiurnal and diurnal groups. The first process is to unite each group into a single wave.

We will first consider the semidiurnal group. Let us now, for the moment, banish the tides from our minds, and imagine that there are two trains of waves traveling simultaneously along a straight canal. If either train existed by itself every wave would be exactly like all its brethren, both in height, length, and period. Now suppose that the lengths and periods of the waves of the two coëxistent trains do not differ much from one another, although their heights may differ widely. Then the resultant must be a single train of waves of lengths and periods intermediate between those of the constituent waves, but in one part of the canal the waves will be high, where the two sets of crests fall in the same place, whilst in another they will be low, where the hollow of the smaller constituent wave falls in with the crest of the larger. If only one part of the canal were visible to us, a train of waves would pass before us, whose heights would gradually vary, whilst their periods would change but little.

In the same way two of the semidiurnal tide waves, when united by the addition of their separate displacements from the mean level, form a single wave of variable height, with a period still semidiurnal, although slightly variable. But there is nothing in this process which limits the synthesis to two waves, and we may add a third and a fourth, finally obtaining a single semidiurnal wave, the height of which varies according to a very complex law.

A similar synthesis is then applied to the second group of waves, so that we have a single variable wave of approximately diurnal period. The final step consists in the union of the single semidiurnal wave with the single diurnal one into a resultant wave. When the diurnal wave is large, the resultant is found to undergo very great variability both in period and height. The principal variations in the relative positions of the partial tide waves are determined by the phases of the moon and by the time of year, and there is, corresponding to each arrangement of the partial waves, a definite form for the single resultant wave. The task of forming a general tide table therefore consists in the determination of all the possible periods and heights of the resultant wave and the tabulation of the heights and intervals after the moon's passage of its high and low waters.

I supposed formerly that the captain would himself calculate the tide he required from the general tide table, but such calculation may be done beforehand for every day of a specified year, and the result will be a special tide table. There are about 1,400 high and low waters in a year, so that the task is very laborious, and has to be repeated each year.

It is, however, possible to compute a special tide table by a different and far less laborious method. In this plan an ingenious mechanical device replaces the labor of the computer. The first suggestion for instrumental prediction of tides was made, I think, by Sir William Thomson, now Lord Kelvin, in 1872. Mr. Edward Roberts bore an important part in the practical realization of such a machine, and a tide predicter was constructed by Messrs. Légé for the Indian Government under his supervision. This is, as yet, the only complete instrument in existence. But others are said to be now in course of construction for the Government of the United States and for that of France. The Indian machine cost so much and works so well, that it is a pity it should not be used to the full extent of its capacity. The Indian Government has, of course, the first claim on it, but the use of it is allowed to others on the payment of a small fee. I believe that, pending the construction of their own machine, the French authorities are obtaining the curves for certain tidal predictions from the instrument in London.

Although the principle involved in the tide predicter is simple, yet the practical realization of it is so complex that a picture of the whole machine would convey no idea of how it works. I shall therefore only illustrate it diagrammatically, in fig. 35, without any pretension to scale or proportion. The reader must at first imagine that there are only two pulleys, namely, A and B, so that the cord passes from the fixed end F under A and over B, and so onward to the pencil. The pulley B is fixed, and the pulley A can slide vertically up and down in a slot, which is not shown in the diagram. If A moves vertically through any distance, the pencil must clearly move through double that distance, so that when A is highest the pencil is lowest, and vice versa.

The pencil touches a uniformly revolving drum, covered with paper; thus if the pulley A executes a simple vertical oscillation, the pencil draws a simple wave on the drum. Now the pulley is mounted on an inverted T-shaped frame, and a pin, fixed in a crank C, engages in the slit in the horizontal arm of the T-piece. When the crank C revolves, the pulley A executes a simple vertical oscillation with a range depending on the throw of the crank.[1] The position of the pin is susceptible of adjustment on the crank, so that its throw and the range of oscillation of the pulley can be set to any required length—of course within definite limits determined by the size of the apparatus.

The drum is connected to the crank C by a train of wheels, so that as the crank rotates the drum also turns at some def-

[1] I now notice that the throw of the crank C is too small to have allowed the pencil to draw so large a wave as that shown on the drum. But as this is a mere diagram, I have not thought it worth while to redraw the whole.

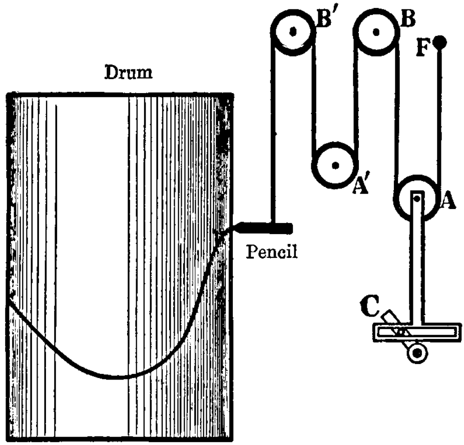

FIG. 35.—DIAGRAM OF TIDE-PREDICTING INSTRUMENT

initely proportional rate. If, for example, the crank revolves twice for one turn of the drum, the pencil will draw a simple wave, with exactly two crests in one circumference of the drum. If one revolution of the drum represents a day, the graphical time scale is 24 hours to the circumference of the drum. If the throw of the crank be one inch, the pulley will oscillate with a total range of two inches, and the pencil with a total range of four inches. Then taking two inches lengthwise on the drum to represent a foot of water, the curve drawn by the pencil might be taken to represent the principal solar semidiurnal tide, rising one foot above and falling one foot below the mean sea level.

I will now show how the machine is to be adjusted so as to give predictions. We will suppose that it is known that, at noon of the first day for which prediction is required, the solar tide will stand at 1 ft. 9 in. above mean sea level and that the water will be rising. Then, the semi-range of this tide being one foot, the pin is adjusted in the crank at one inch from the centre, so as to make the pencil rock through a total range of 4 inches, representing 2 feet. The drum is now turned so as to bring the noon-line of its circumference under the pencil, and the crank is turned so that the pencil shall be $3\frac{1}{2}$ inches (representing 1 ft. 9 in. of water) below the middle of the drum, and so that when the machine starts, the pencil will begin to descend. The curve being drawn upside-down, the pencil is set below the middle line because the water is to be above mean level, and it must begin to descend because the water is to ascend. The train of wheels connecting the crank and drum is then thrown into gear, and the machine is started; it will then draw the solar tide curve, on the scale of 2 inches to the foot, for all time.

If the train of wheels connecting the crank to the drum were to make the drum revolve once whilst the crank revolves 1.93227 times, the curve would represent a lunar semidiurnal tide. The reason of this is that 1.93227 is the ratio of

24 hours to 12 h. 25 m. 14 s., that is to say, of a day to a lunar half day. We suppose the circumference of the drum still to represent an ordinary day of 24 hours, and therefore the curve drawn by the pencil will have lunar semidiurnal periodicity. In order that these curves may give predictions of the future march of that tide, the throw of the crank must be set to give the correct range and its angular position must give the proper height at the moment of time chosen for beginning. When these adjustments are made the curve will represent that tide for all time.

We have now shown that, by means of appropriate trains of wheels, the machine can be made to predict either the solar or the lunar tide; but we have to explain the arrangement for combining them. If, still supposing there to be only the two pulleys A, B, the end F of the cord were moved up or down, its motion would be transmitted to the pencil, whether the crank C and pulley A were in motion, or at rest; but if they were in motion, the pencil would add the motion of the end of the cord to that of the pulley. If then there be added another fixed pulley B′, and another movable pulley A′, driven by a crank and T-piece (not shown in the diagram), the pencil will add together the movements of the two pulleys A and A′. There must now be two trains of wheels, one connecting A with the drum and the other for A′. If a single revolution of the drum causes the crank C to turn twice, whilst it makes the crank of A′ rotate 1.93227 times, the curve drawn will represent the union of the principal solar and lunar semidiurnal tides. The trains of wheels requisite for transmitting motion from the drum to the two cranks in the proper proportions are complicated, but it is obviously only a matter of calculation to determine the numbers of the teeth in the several wheels in the trains. It is true that rigorous accuracy is not attainable, but the mechanism is made so nearly exact that the error in the sum of the two tides would be barely sensible even after 3,000 revolutions of the drum. It is of course necessary to set the two cranks with their proper throws and at their proper angles so as to draw a curve which shall, from the noon of a given day, correspond to the tide at a given place.

It must now be clear that we may add as many more movable pulleys as we like. When the motion of each pulley is governed by an appropriate train of wheels, the movement of the pencil, in as far as it is determined by that pulley, corresponds to the tide due to one of our ideal satellites. The resultant curve drawn on the drum is then the synthesis of all the partial tides, and corresponds with the motion of the sea.

The instrument of the Indian Government unites twenty-four partial tides. In order to trace a tide curve, the throws of all the cranks are set so as to correspond with the known heights of the partial tides, and each crank is set at the proper angle to correspond with the moment of time chosen for the beginning of the tide table. It is not very difficult to set the cranks and pins correctly, although close attention is of course necessary. The apparatus is then driven by the fall of a weight, and the paper is fed automatically on to the drum and coiled off on to a second drum, with the tide curve drawn on it. It is only necessary to see that the paper runs on and off smoothly, and to write the date from time to time on the paper as it passes, in order to save future trouble in the identification of the days. It takes about four hours to run off the tides for a year.

The Indian Government sends home annually the latest revision of the tidal constants for thirty-seven ports in the Indian Ocean. Mr. Roberts sets the machine for each port, so as to correspond with noon of a future 1st of January, and then lets it run off a complete tide curve for a whole year. The curve is subsequently measured for the time and height of each high and low water, and the printed tables are sold at the moderate price of four rupees. The publication is made sufficiently long beforehand to render the tables available for future voyages. These tide tables are certainly amongst the most admirable in the world.

It is characteristic of England that the machine is not, as I believe, used for any of the home ports, and only for a few of the colonies. The neglect of the English authorities is not, however, so unreasonable as it might appear to be. The tides at English ports are remarkably simple, because the diurnal inequality is practically absent. The applicability of the older methods of prediction, by means of such curves as that for Portsmouth in fig. 34, is accordingly easy, and the various corrections are well determined. The arithmetical processes are therefore not very complicated, and ordinary computers are capable of preparing the tables with but little skilled supervision. Still it is to be regretted that this beautiful instrument should not be more used for the home and colonial ports.

The excellent tide tables of the Government of the United States have hitherto been prepared by the aid of a machine of quite a different character, the invention of the late Professor Ferrel. This apparatus virtually carries out that process of compounding all the waves together into a single one, which I have described as being done by a computer for the formation of a general tide table. It only registers, however, the time and height of the maxima and minima—the high and low waters. I do not think it necessary to describe its principle in detail, because it will shortly be superseded by a machine like, but not identical with, that of the Indian Government.

Authorities.

G. H. Darwin, *On Tidal Prediction.* "Philosophical Transactions of the Royal Society," A. 1891, pp. 159–229.

In the example of the use of a general tide table at Aden, given in this chapter, the datum from which the height is measured is 0.37 ft. higher than that used in the Indian Tide Tables; accordingly $4\frac{1}{2}$ inches must be added to the height, in order to bring it into accordance with the official table.

Sir William Thomson, *Tidal Instruments*, and the subsequent discussion. "Institute of Civil Engineers," vol. lxv.

William Ferrel, *Description of a Maxima and Minima Tide-predicting Machine.* "United States Coast Survey," 1883.

CHAPTER XIV

THE DEGREE OF ACCURACY OF TIDAL PREDICTION

The success of tidal predictions varies much according to the place of observation. They are not unfrequently considerably in error in our latitude, and throughout those regions called by sailors "the roaring forties." The utmost that can be expected of a tide table is that it shall be correct in calm weather and with a steady barometer. But such conditions are practically non-existent, and in the North Atlantic the great variability in the meteorological elements renders tidal prediction somewhat uncertain.

The sea generally stands higher when the barometer is low, and lower when the barometer is high, an inch of mercury corresponding to rather more than a foot of water. The pressure of the air on the sea in fact depresses it in those places where the barometer is high, and allows it to rise where the opposite condition prevails.

Then again a landward wind usually raises the sea level, and in estuaries the rise is sometimes very great. There is a known instance when the Thames at London was raised by five feet in a strong gale. Even on the open coast the effect of wind is sometimes great. A disastrous example of this was afforded on the east coast of England in the autumn of 1897, when the conjunction of a gale with springtide caused the sea to do an enormous amount of damage, by breaking embankments and flooding low-lying land.

But sometimes the wind has no apparent effect, and we must then suppose that it had been blowing previously elsewhere in such a way as to depress the water at the point at which we watch it. The gale might then only restore the water to its normal level, and the two effects might mask one another. The length of time during which the wind has lasted is clearly an important factor, because the currents generated by the wind must be more effective in raising or depressing the sea level the longer they have lasted.

It does not then seem possible to formulate any certain system of allowance for barometric pressure and wind. There are, at each harbor, certain rules of probability, the application of which will generally lead to improvement in the prediction; but occasionally such empirical corrections will be found to augment the error.

But notwithstanding these perturbations, good tide tables are usually of surprising accuracy even in northern latitudes; this may be seen from the following table showing the results of comparisons between prediction and actuality at Portsmouth. The importance of the errors in height depends of course on the range of tide; it is therefore well to note that the average ranges of tide at springs and neaps are

13 ft. 9 in. and 7 ft. 9 in. respectively.

TABLE OF ERRORS IN THE PREDICTION OF HIGH WATER AT PORTSMOUTH IN THE MONTHS OF JANUARY, MAY, AND SEPTEMBER, 1897.

Time		Height	
Magnitude of error	Number of cases	Magnitude of error	Number of cases
		Inches	
0^m to 5^m	69	0 to 6	89
6^m to 10^m	50	7 to 12	58
11^m to 15^m	25	13 to 18	24
16^m to 20^m	10	19 to 24	6
21^m to 25^m	11	—	—
26^m to 30^m	7	—	—
31^m to 35^m	4	—	—
52^m	1	—	—
—	177	—	177

N. B.—The comparison seems to indicate that these predictions might be much improved, because the predicted height is nearly always

Errors in Height for the Year 1892, Excepting Part of July

Magnitude of error	Number of cases
Inches	
0 to 6	381
7 to 12	228
13 to 18	52
19 to 24	8
31	1
—	670

above the observed height, and because the diurnal inequality has not been taken into account sufficiently, if at all.

In tropical regions the weather is very uniform, and in many places the "meteorological tides" produced by the regularly periodic variations of wind and barometric pressure are taken into account in tidal predictions.

The apparent irregularity of the tides at Aden is so great, that an officer of the Royal Engineers has told me that, when he was stationed there many years ago, it was commonly believed that the strange inequalities of water level were due to the wind at distant places. We now know that the tide at Aden is in fact marvelously regular, although the rule according to which it proceeds is very complex. In almost every month in the year there are a few successive days when there is only one high water and one low water in the 24 hours; and the water often remains almost stagnant for three or four hours at a time. This apparent irregularity is due to the diurnal inequality, which is very great at Aden, whereas on the coasts of Europe it is insignificant.

I happen to have a comparison with actuality of a few predictions of high water at Aden, where the maximum range of the tide is about 8 ft. 6 in. They embrace the periods from March 10 to April 9, and again from November 12 to December 12, 1884. In these two periods there were 118 high waters, but through an accident to the tide gauge one high water was not registered. On one occasion, when the regular semidiurnal sequence of the tide would lead us to expect high water, there occurred one of those periods of stagnation to which I have referred. Thus we are left with 116 cases of comparison between the predicted and actual high waters.

The results are exhibited in the following table:—

Time		Height	
Magnitude of errors	Number of high waters	Magnitude of errors	Number of high waters
		Inches	
0^m to 5^m	35	0	15
5^m to 10^m	32	1	48
10^m to 15^m	19	2	28
15^m to 20^m	19	3	14
20^m to 25^m	5	4	11
26^m and 28^m	2	No high water	1
33^m and 36^m	2	—	—
56^m and 57^m	2	—	—
No high water	1	—	—
	117		117

It would be natural to think that when the prediction is erroneous by as much as 57 minutes, it is a very bad one; but I shall show that this would be to do injustice to the table. On several of the occasions comprised in this list the water was very nearly stagnant. Now if the water only rises about a foot from low to high water in the course of four or five hours, it is almost impossible to say with accuracy when it was highest, and two observers might differ in their estimate by half an hour or even by an hour.

In the table of comparison there are 11 cases in which the error of time is equal to or greater than twenty minutes, and I have examined these cases in order to see whether the water was then nearly stagnant. A measure of the degree of stagnation is afforded by the amount of the rise from low water to high water, or of the fall from high water to low water. The following table gives a classification of the errors of time according to the rise or fall:—

Analysis of Errors in Time.

Ranges from low water to high water	Errors of time
Nil	—
6 in. to 8 in.	22, 26, 28, 56, 57 minutes
13 in.	36 minutes
17 in.	22 "
19 in.	33 "
2 ft. 10 in.	22 "
3 ft. 9 in.	23 "
3 ft. 11 in.	20 "

There are then only three cases when the rise of water was considerable, and in the greatest of them it was only 3 ft. 11 in.

If we deduct all the tides in which the range between low and high water was equal to or less than 19 inches, we are left with 108 predictions, and in these cases the greatest error in time is 23 mins. In 86 cases the error is equal to or less than a quarter of an hour. This leaves 22 cases where the error was greater than 15 mins. made up as follows: 18 cases with error greater than 15 mins. and less than 20 mins. and 3 cases with errors of 20 mins., 22 mins., 23 mins. Thus in 106 out of 108 predictions the error of time was equal to or less than 20 minutes.

Two independent measurements of a tide curve, for the determination of the time of high water, lead to results which frequently differ by five minutes, and sometimes by ten minutes. It may therefore be claimed that these predictions have a very high order of accuracy as regards time.

Turning now to the heights, out of 116 predictions the error in the predicted height was equal to or less than 2 inches in 91 cases, it amounted to 3 inches in 14 cases, and in the remaining 11 cases it was 4 inches. It thus appears that, as regards the height of the tide also, the predictions are of great accuracy. This short series of comparisons affords a not unduly favorable example of the remarkable success attainable, where tidal observation and prediction have been thoroughly carried out at a place subject to only slight meteorological disturbance.

If our theory of tides were incorrect, so that we imagined that there was a partial tide wave of a certain period, whereas in fact such a wave has no true counterpart in physical causation, the reduction of a year of tidal observation would undoubtedly assign some definite small height, and some definite retardation of the high water after the passage of the corresponding, but erroneous, satellite. But when a second series of observations is reduced, the two tidal constants would show no relationship to their previous evaluations. If then reductions carried out year after year assign, as they do, fairly consistent values to the tidal constants, we may feel confident that true physical causation is involved, even when the heights of some of the constituent tide waves do not exceed an inch or two.

Prediction must inevitably fail, unless we have lighted on the true causes of the phenomena; success is therefore a guarantee of the truth of the theory. When we consider that the incessant variability of the tidal forces, the complex outlines of our coasts, the depth of the sea and the earth's rotation are all involved, we should regard good tidal prediction as one of the greatest triumphs of the theory of universal gravitation.

Authorities.

The Portsmouth comparisons were given to the author by the Hydrographer of the Admiralty, Admiral Sir W. J. Wharton.

G. H. Darwin, *On Tidal Prediction.* "Philosophical Transactions of the Royal Society," A. 1891.

CHAPTER XV

CHANDLER'S NUTATION—THE RIGIDITY OF THE EARTH

In the present chapter I have to explain the origin of a tide of an entirely different character from any of those considered hitherto. It may fairly be described as a true tide, although it is not due to the attraction of either the sun or the moon.

We have all spun a top, and have seen it, as boys say, go to sleep. At first it nods a little, but gradually it settles down to perfect steadiness. Now the earth may be likened to a top, and it also may either have a nutational or nodding motion, or it may spin steadily; it is only by observation that we can decide whether it is nodding or sound asleep.

The equator must now be defined as a plane through the earth's centre at right angles to the axis of rotation, and not as a plane fixed with reference to the solid earth. The latitude of any place is the angle[1] between the equator and a line drawn from the centre of the earth to the place of observation. Now when the earth nutates, the axis of rotation

[1] This angle is technically called the geocentric latitude; the distinction between true and geocentric latitude is immaterial in the present discussion.

shifts, and its extremity describes a small circle round the spot which is usually described as the pole. The equator, being perpendicular to the axis of rotation, of course shifts also, and therefore the latitude of a place fixed on the solid earth varies. During the whole course of the nutation, the earth's axis of rotation is always directed towards the same point in the heavens, and therefore the angle between the celestial pole and the vertical or plumb-line at the place of observation must oscillate about some mean value; the period of the oscillation is that of the earth's nutation. This movement is called a "free" nutation, because it is independent of the action of external forces.

There are, besides, other nutations resulting from the attractions of the moon and sun on the protuberant matter at the equator, and from the same cause there is a slow shift in space of the earth's axis, called the precession. These movements are said to be "forced," because they are due to external forces. The measurements of the forced nutations and of the precession afford the means of determining the period of the free nutation, if it should exist. It has thus been concluded that if there is any variation in the latitude, it should be periodic in 305 days; but only observation can decide whether there is such a variation of latitude or not.

Until recently astronomers were so convinced of the sufficiency of this reasoning, that, when they made systematic examination of the latitudes of many observatories, they always searched for an inequality with a period of 305 days. Some thought that they had detected it, but when the observations extended over long periods, it always seemed to vanish, as though what they had observed were due to the inevitable errors of observation. At length it occurred to Mr. Chandler to examine the observations of latitude without any prepossession as to the period of the inequality. By the treatment of enormous masses of observation, he came to the conclusion that there is really such an inequality, but that the period is 427 days instead of 305 days. He also found other inequalities in the motion of the axis of rotation, of somewhat obscure origin, and of which I have no occasion to say more.[1]

The question then arises as to how the theory can be so amended as to justify the extension of the period of nutation. It was, I believe, Newcomb, of the United States Naval Observatory, who first suggested that the explanation is to be sought in the fact that the axis of rotation is an axis of centrifugal repulsion, and that when it shifts, the distribu-

[1] They are perhaps due to the unequal melting of polar ice and unequal rainfall in successive years. These irregular variations in the latitude are such that some astronomers are still skeptical as to the reality of Chandler's nutation, and think that it will perhaps be found to lose its regularly rhythmical character in the future.

tion of centrifugal force is changed with reference to the solid earth, so that the earth is put into a state of stress, to which it must yield like any other elastic body. The strain or yielding consequent on this stress must be such as to produce a slight variability in the position of the equatorial protuberance with reference to places fixed on the earth. Now the period of 305 days was computed on the hypothesis that the position of the equatorial protuberance is absolutely invariable, but periodic variations of the earth's figure would operate so as to lengthen the period of the free nutation, to an extent dependent on the average elasticity of the whole earth.

Mr. Chandler's investigation demanded the utmost patience and skill in marshaling large masses of the most refined astronomical observations. His conclusions are not only of the greatest importance to astronomy, but they also give an indication of the amount by which the solid earth is capable of yielding to external forces. It would seem that the average stiffness of the whole earth must be such that it yields a little less than if it were made of steel.[1] But the amount by which the surface yields remains unknown, because we are unable to say what proportion of the aggregate change

[1] Mr. S. S. Hough, p. 338 of the paper referred to in the list of authorities at the end of the chapter.

is superficial and what is deep-seated. It is, however, certain that the movements are excessively small, because the circle described by the extremity of the earth's axis of rotation, about the point on the earth which we call the pole, has a radius of only fifteen feet.

It is easily intelligible that as the axis of rotation shifts in the earth, the oceans will tend to swash about, and that a sort of tide will be generated. If the displacement of the axis were considerable, whole continents would be drowned by a gigantic wave, but the movement is so small that the swaying of the ocean is very feeble. Two investigators have endeavored to detect an oceanic tide with a period of 427 days; they are Dr. Bakhuyzen of Leyden and Mr. Christie of the United States Coast Survey. The former considered observations of sea-level on the coasts of Holland, the latter those on the coasts of the United States; and they both conclude that the sea-level undergoes a minute variability with a period of about 430 days. A similar investigation is now being prosecuted by the Tidal Survey of India, and as the Indian tidal observations are amongst the best in the world, we may hope for the detection of this minute tide in the Indian Ocean also.

The inequality in water level is so slight and extends over so long a period that its measurement cannot yet be accepted as certain. The mean level of the sea is subject to slight irregular variations, which are probably due to unequal rainfall and unequal melting of polar ice in successive years. But whatever be the origin of these irregularities they exceed in magnitude the one to be measured. The arithmetical processes, employed to eliminate the ordinary tides and the irregular variability, will always leave behind some residual quantities, and therefore the examination of a tidal record will always apparently yield an inequality of any arbitrary period whatever. It is only when several independent determinations yield fairly consistent values of the magnitude of the rise and fall and of the moment of high water, that we can feel confidence in the result. Now although the reductions of Bakhuyzen and Christie are fairly consistent with one another, and with the time and height suggested by Chandler's nutation, yet it is by no means impossible that accident may have led to this agreement. The whole calculation must therefore be repeated for several places and at several times, before confidence can be attained in the detection of this latitudinal tide.

The prolongation of the period of Chandler's nutation from 305 to 427 days seems to indicate that our planet yields to external forces, and we naturally desire to learn more on so interesting a subject. Up to fifty years ago it was generally held that the earth was a globe of molten matter covered by a thin crust. The ejection of lava from volcanoes and the great increase of temperature in mines seemed to present evidence in favor of this belief. But the geologists and physicists of that time seemed not to have perceived that the inference might be false, if great pressure is capable of imparting rigidity to matter at a very high temperature, because the interior of the earth might then be solid although very hot. Now it has been proved experimentally that rock expands in melting, and a physical corollary from this is that when rock is under great pressure a higher temperature is needed to melt it than when the pressure is removed. The pressure inside the earth much exceeds any that can be produced in the laboratory, and it is uncertain up to what degree of increase of pressure the law of the rise of the temperature of melting would hold good; but there can be no doubt that, in so far as experiments in the laboratory can be deemed applicable to the conditions prevailing in the interior of the earth, they tend to show that the matter there is not improbably solid.

But Lord Kelvin reinforces this argument from another point of view. Rock in the solid condition is undoubtedly heavier than when it is molten. Now the solidified crust on the surface of a molten planet must have been fractured many times during the history of the planet, and the fragments would sink through the liquid, and thus build up a solid nucleus. It will be observed that this argument does not repose on the rise in the melting temperature of rock through pressure, although it is undoubtedly reinforced thereby.

Hopkins was, I think, the first to adduce arguments of weight in favor of the earth's solidity. He examined the laws of the precession and nutation of a rigid shell inclosing liquid, and found that the motion of such a system would differ to a marked degree from that of the earth. From this he concluded that the interior of the earth was not liquid.

Lord Kelvin has pointed out that although Hopkins's investigation is by no means complete, yet as he was the first to show that the motion of the earth as a whole affords indications of the condition of the interior, an important share in the discovery of the solidity of the earth should be assigned to him. Lord Kelvin then resumed Hopkins's work, and showed that if the liquid interior of the planet were inclosed in an unyielding crust, a very slight departure from perfect sphericity in the shell would render the motion of the system almost identical with that of a globe solid from centre to surface, although this would not be the case with the more rapid nutations. A yet more important deficiency in Hopkins's investigation is that he did not consider that, unless the crust were more rigid than the stiffest steel, it would yield to the surging of the imprisoned liquid as freely as india-rubber; and, besides, that if the crust yielded freely, the precession and nutations of the whole mass would hardly be distinguishable from those of a solid globe. Hopkins's argument, as thus amended by Lord Kelvin, leads to one of two alternatives: either the globe is solid throughout, or else the crust yields with nearly the same freedom to external forces as though it were liquid.

We have now to show that the latter hypothesis is negatived by other considerations. The oceanic tides, as we perceive them, consist in a motion of the water relatively to the land. Now if the solid earth were to yield to the tidal forces with the same freedom as the superjacent sea, the cause for the relative movement of the sea would disappear. And if the solid yielded to some extent, the apparent oceanic tide would be proportionately diminished. The very existence of tides in the sea, therefore, proves at least that the land does not yield with perfect freedom.

Lord Kelvin has shown that the oceanic tides, on a globe of the same rigidity as that of glass, would only have an apparent range of two fifths of those on a perfectly rigid globe; whilst, if the rigidity was equal to that of steel, the fraction of diminution would be two thirds. I have myself extended his argument to the hypothesis that the earth may be composed of a viscous material, which yields slowly under the application of continuous forces, and also to the hypothesis of a material which shares the properties of viscosity and rigidity, and have been led to analogous conclusions.

The difficulty of the problem of oceanic tides is so great that we cannot say how high the tides would be if the earth were absolutely rigid, but Lord Kelvin is of opinion that they certainly would not be twice as great as they are, and he concludes that the earth possesses a greater average stiffness than that of glass, although perhaps not greater than that of steel. It is proper to add that the validity of this argument depends principally on the observed height of an inequality of sea level with a period of a fortnight. This is one of the partial tides of the third kind, which I described in Chapter XI. as practically unimportant, and did not discuss in detail. The value of this inequality in the present argument is due to the fact that it is possible to form a much closer estimate of its magnitude on a rigid earth than in the case of the semidiurnal and diurnal tides.

It may ultimately be possible to derive further indications concerning the physical condition of the inside of the earth from the science of seismology. The tremor of an earthquake has frequently been observed instrumentally at an enormous distance from its origin; as, for example, when the shock of a Japanese earthquake is perceived in England.

The vibrations which are transmitted through the earth are of two kinds. The first sort of wave is one in which the matter through which it passes is alternately compressed and dilated; it may be described as a wave of compression. In the second sort the shape of each minute portion of the solid is distorted, but the volume remains unchanged, and it may be called a wave of distortion. These two vibrations travel at different speeds, and the compressional wave outpaces the distortional one. Now the first sign of a distant earthquake is that the instrumental record shows a succession of minute tremors. These are supposed to be due to waves of compression, and they are succeeded by a much more strongly marked disturbance, which, however, lasts only a short time. This second phase in the instrumental record is supposed to be due to the wave of distortion.

If the natures of these two disturbances are correctly ascribed to their respective sources, it is certain that the matter through which the vibration has passed was solid. For, although a compressional wave might be transmitted without much loss of intensity, from a solid to a liquid and back again to a solid, as would have to be the case if the interior of the earth is molten, yet this cannot be true of the distortional wave. It has been supposed that vibrations due to earthquakes pass in a straight line through the earth; if then this could be proved, we should know with certainty that the earth is solid, at least far down towards its centre.

Although there are still some—principally amongst the geologists—who believe in the existence of liquid matter immediately under the solid crust of the earth,[1] yet the arguments which I have sketched appear to most men of science conclusive against such belief.

Authorities.

Mr. S. C. Chandler's investigations are published in the "Astronomical Journal," vol. 11 and following volumes. A summary is contained in "Science," May 3, 1895.

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Simon Newcomb, *On the Dynamics of the Earth's Rotation*, "Monthly Notices of the R. Astron. Soc.," vol. 52 (1892), p. 336.

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H. G. van de Sande Bakhuyzen, *Ueber die Aenderung der Polhoehe*, "Astron. Nachrichten," No. 3261.

A. S. Christie, *The Latitude-variation Tide*, "Phil. Soc. of Washington, Bulletin," vol. 12 (1895), p. 103.

Lord Kelvin, in Thomson and Tait's "Natural Philosophy," on the Rigidity of the Earth; and "Popular Lectures," vol. 3.

G. H. Darwin, *Bodily Tides of Viscous and Semi-elastic Spheroids, &c.*, "Philosoph. Trans. of the Royal Society," Part. I. 1879.

[1] See the Rev. Osmond Fisher's *Physics of the Earth's Crust*.

CHAPTER XVI[1]

TIDAL FRICTION

THE fact that the earth, the moon, and the planets are all nearly spherical proves that in early times they were molten and plastic, and assumed their present round shape under the influence of gravitation. When the material of which any planet is formed was semi-liquid through heat, its satellites, or at any rate the sun, must have produced tidal oscillations in the molten rock, just as the sun and moon now produce the tides in our oceans.

Molten rock and molten iron are rather sticky or viscous substances, and any movement which agitates them must be subject to much friction. Even water, which is a very good lubricant, is not entirely free from friction, and so our present oceanic tides must be influenced by fluid friction, although to a far less extent than the molten solid just referred to. Now, all moving systems which are subject to friction gradually come to rest. A train will run a long way when the steam is turned off, but it stops at last, and a fly-wheel will continue to spin for only a limited time. This general law renders it certain that the friction of the tide, whether it consists in the

[1] A considerable portion of this and of the succeeding chapter appeared as an article in *The Atlantic Monthly* for April, 1898.

swaying of molten lava or of an ocean, must be retarding the rotation of the planet, or at any rate retarding the motion of the system in some way.

It is the friction upon its bearings which brings a fly-wheel to rest; but as the earth has no bearings, it is not easy to see how the friction of the tidal wave, whether corporeal or oceanic, can tend to stop its rate of rotation. The result must clearly be brought about, in some way, by the interaction between the moon and the earth. Action and reaction must be equal and opposite, and if we are correct in supposing that the friction of the tides is retarding the earth's rotation, there must be a reaction upon the moon which must tend to hurry her onwards. To give a homely illustration of the effects of reaction, I may recall to mind how a man riding a high bicycle, on applying the brake too suddenly, was thrown over the handles. The desired action was to stop the front wheel, but this could not be done without the reaction on the rider, which sometimes led to unpleasant consequences.

The general conclusion as to the action and reaction due to tidal friction is of so vague a character that it is desirable to consider in detail how they operate.

The circle in fig. 36 is supposed to represent the undisturbed shape of the planet, which rotates in the direction of the curved arrow. A portion of the orbit of the satellite is indicated by part of a circle, and the direction of its motion is

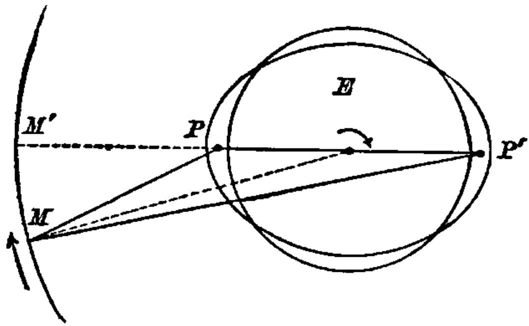

FIG. 36.—FRICTIONALLY RETARDED TIDE

shown by an arrow. I will first suppose that the water lying on the planet, or the molten rock of which it is formed, is a perfect lubricant devoid of friction, and that at the moment represented in the figure the satellite is at M′. The fluid will then be distorted by the tidal force until it assumes the egg-like shape marked by the ellipse, projecting on both sides beyond the circle. It will, however, be well to observe that if this figure represents an ocean, it must be a very deep one, far deeper than those which actually exist on the earth; for we have seen that it is only in deep oceans that the high water stands underneath and opposite to the moon; whereas in shallow water it is low water where we should naturally expect high water. Accepting the hypothesis that the high tide is opposite to the moon, and supposing that the liquid is devoid of friction, the long axis of the egg is always directed straight towards the satellite M′, and the liquid maintains a continuous rhythmical movement, so that as the planet rotates and the satellite revolves, it always maintains the same shape and attitude towards the satellite.

But when, as in reality, the liquid is subject to friction, it gets belated in its rhythmical rise and fall, and the protuberance is carried onward by the rotation of the planet beyond its proper place. In order to make the same figure serve for this condition, I set the satellite backward to M; for this amounts to just the same thing, and is less confusing than redrawing the protuberance in its more advanced position. The planet then constantly maintains this shape and attitude with regard to the satellite, and the interaction between the two will be the same as though the planet were solid, but continually altering its shape.

We have now to examine what effects must follow from the attraction of the satellite on an egg-shaped planet, when the two constantly maintain the same attitude relatively to each other. It will make the matter somewhat easier of comprehension if we replace the tidal protuberances by two particles of equal masses, one at P, and the other at P′. If the masses of these particles be properly chosen, so as to represent the amount of matter in the protuberances, the proposed change will make no material difference in the action.

The gravitational attraction of the satellite is greater on bodies which are near than on those which are far, and accordingly it attracts the particle P more strongly than the particle P′. It is obvious from the figure that the attraction on P must tend to stop the planet's rotation, whilst that on P′ must tend to accelerate it. If a man pushes equally on the two pedals of a bicycle, the crank has no tendency to turn, and besides there are dead points in the revolution where pushing and pulling have no effect. So also in the astronomical problem, if the two attractions were exactly equal, or if the protuberances were at a dead point, there would be no resultant effect on the rotation of the planet. But it is obvious that here the retarding pull is stronger than the accelerating one, and that the set of the protuberances is such that we have passed the dead point. It follows from this that the primary effect of fluid friction is to throw the tidal protuberance forward, and the secondary effect is to retard the planet's rotation.

It has been already remarked that this figure is drawn so as to apply only to the case of corporeal tides or to those of a very deep ocean. If the ocean were shallow and frictionless, it would be low water under and opposite to the satellite. If then the effect of friction were still to throw the protuberances forward, the rotation of the planet would be accelerated instead of retarded. But in fact the effect of fluid friction in a shallow ocean is to throw the protuberances backward, and a similar figure, drawn to illustrate such a displacement of the tide, would at once make it clear that here also tidal friction will lead to the retardation of the planet's rotation. Henceforth then I shall confine myself to the case illustrated by fig. 36.

Action and reaction are equal and opposite, and if the satellite pulls at the protuberances, they pull in return on the satellite. The figure shows that the attraction of the protuberance P tends in some measure to hurry the satellite onward in its orbit, whilst that of P′ tends to retard it. But the attraction of P is stronger than that of P′, and therefore the resultant of the two is a force tending to carry the satellite forward in the direction of the arrow.

If a stone be whirled at the end of an elastic string, a retarding force, such as the friction of the air, will cause the string to shorten, and an accelerating force will make it lengthen. In the same way the satellite, being as it were tied to the planet by the attraction of gravitation, when subjected to an onward force, recedes from the planet, and moves in a spiral curve at ever increasing distances. The time occupied by the satellite in making a circuit round the planet is prolonged, and this lengthening of the periodic time is not merely due to the lengthening of the arc described by it, but also to an actual retardation of its velocity. It appears paradoxical that the effect of an accelerating force should be a retardation, but a consideration of the mode in which the force operates will remove the paradox. The effect of the tangential accelerating force on the satellite is to make it describe an increasing spiral curve. Now if the reader will draw an exaggerated figure to illustrate part of such a spiral orbit, he will perceive that the central force, acting directly towards the planet, must operate in some measure to retard the velocity of the satellite. The central force is very great compared with the tangential force due to the tidal friction, and therefore a very small fraction of the central force may be greater than the tangential force. Although in a very slowly increasing spiral the fraction of the central force productive of retardation is very small, yet it is found to be greater than the tangential accelerating force, and thus the resultant effect is a retardation of the satellite's velocity.

The converse case where a retarding force results in increase of velocity will perhaps be more intelligible, as being more familiar. A meteorite, rushing through the earth's atmosphere, moves faster and faster, because it gains more speed from the attraction of gravity than it loses by the friction of the air.

Now let us apply these ideas to the case of the earth and the moon. A man standing on the planet, as it rotates, is carried past places where the fluid is deeper and shallower alternately; at the deep places he says that it is high tide, and at the shallow places that it is low tide. In fig. 36 it is high tide when the observer is carried past P. Now it was pointed out that when there is no fluid friction we must put the moon at M′, but when there is friction she must be at M. Accordingly, if there is no friction it is high tide when the moon is over the observer's head, but when there is friction the moon has passed his zenith before he reaches high tide. Hence he would remark that fluid friction retards the time of high tide.

A day is the name for the time in which the earth rotates once, and a month for the time in which the moon revolves once. Then since tidal friction retards the earth's rotation and the moon's revolution, we may state that both the day and the month are being lengthened, and that these results follow from the retardation of the time of high tide.

It must also be noted that the spiral in which the moon moves is an increasing one, so that her distance from the earth also increases. These are absolutely certain and inevitable results of the mechanical interaction of the two bodies.

At the present time the rates of increase of the day and month are excessively small, so that it has not been found possible to determine them with any approach to accuracy. It may be well to notice in passing that if the rate of either increase of element were determinable, that of the other would be deducible by calculation.

The extreme slowness of the changes within historical times is established by the early records in Greek and Assyrian history of eclipses of the sun, which occurred on certain days and in certain places. Notwithstanding the changes in the calendar, it is possible to identify the day according to our modern reckoning, and the identification of the place presents no difficulty. Astronomy affords the means of calculating the exact time and place of the occurrence of an eclipse even three thousand years ago, on the supposition that the earth spun at the same rate then as now, and that the complex laws governing the moon's motion are unchanged.

The particular eclipse referred to in history is known, but any considerable change in the earth's rotation and in the moon's position would have shifted the position of visibility on the earth from the situation to which modern computation would assign it. Most astronomical observations would be worthless if the exact time of the occurrence were uncertain, but in the case of eclipses the place of observation affords just that element of precision which is otherwise wanting. As, then, the situations of the ancient eclipses agree fairly well with modern computations, we are sure that there has been no great change within the last three thousand years, either in the earth's rotation or in the moon's motion. There is, however, a small outstanding discrepancy which indicates that there has been some change. But the exact amount of change involves elements of uncertainty, because our knowledge of the laws of the moon's motion is not yet quite accurate enough for the absolutely perfect calculation of eclipses which occurred many centuries ago. In this way, it is known that within historical times the retardation of the earth's rotation and the recession of the moon have been at any rate very slow.

It does not, however, follow from this that the changes have always been equally slow; indeed, it may be shown that the efficiency of tidal friction increases with great rapidity as we bring the tide-generating satellite nearer to the planet.

It has been shown in Chapter V. that the intensity of tide-generating force varies as the inverse cube of the distance between the moon and the earth, so that if the moon's distance were reduced successively to $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, of its original distance, the force and the tide generated by it would be multiplied 8, 27, 64 times. But the efficiency of tidal friction increases far more rapidly than this, because not only is the tide itself augmented, but also the attraction of the moon. In order to see how these two factors will coöperate, let us begin by supposing that the height of the tide remains unaffected by the approach or retrogression of the moon. Then the same line of argument, which led to the conclusion that tide-generating force varies inversely as the cube of the distance, shows that the action of the moon on protuberances of definite magnitude must also vary inversely as the cube of the distance. But the height of the tide is not in fact a fixed quantity, but varies inversely as the cube of the distance, so that when account is taken both of the augmentation of the tide and of the increased attraction of the moon, it follows that the tidal retardation of the earth's rotation must vary as the inverse sixth power of the distance. Now since the sixth power of 2 is 64, the lunar tidal friction, with the moon at half her present distance, would be 64 times as efficient as at present. Similarly, if her distance were diminished to a third and a quarter of what it is, the tidal friction would act with 729 and 4,096 times its present strength. Thus, although the action may be insensibly slow now, it must have gone on with much greater rapidity when the moon was nearer to us.

There are many problems in which it would be very difficult to follow the changes according to the times of their occurrence, but where it is possible to banish time from consideration, and to trace the changes themselves, in due order, without reference to time. In the sphere of common life, we know the succession of stations which a train must pass between London and Edinburgh, although we may have no time-table. This is the case with our astronomical problem; for although we have no time-table, yet the sequence of the changes in the system can be traced accurately.

Let us then banish time, and look forward to the ultimate outcome of the tidal interaction of the moon and earth. The day and the month are lengthening at relative rates which are calculable, although the absolute rates in time are unknown. It will suffice for a general comprehension of the problem to know that the present rate of increase of the day is much more rapid than that of the month, and that this will hold good in the future. Thus, the number of rotations of the earth in the interval comprised in one revolution of the moon diminishes; or, in other words, the number of days in the month diminishes, although the month itself is longer than at present. For example, when the day shall be equal in length to two of our actual days, the month may be as long as thirty-seven of our days, and then the earth will spin round only about eighteen times in the month.

This gradual change in the day and month proceeds continuously until the duration of a rotation of the earth is prolonged to fifty-five of our present days. At the same time the month, or the time of revolution of the moon round the earth, will also occupy fifty-five of our days. Since the month here means the period of the return of the moon to the same place among the stars, and since the day is to be estimated in the same way, the moon must then always face the same part of the earth's surface, and the two bodies must move as though they were united by a bar. The outcome of the lunar tidal friction will therefore be that the moon and the earth go round as though locked together, in a period of fifty-five of our present days, with the day and the month identical in length.

Now looking backward in time, we find the day and the month shortening, but the day changing more rapidly than the month. The earth was therefore able to complete more revolutions in the month, although that month was itself shorter than it is now. We get back in fact to a time when there were 29 rotations of the earth in a month instead of $27\frac{1}{3}$, as at present. This epoch is a sort of crisis in the history of the moon and the earth, for it may be proved that there never could have been more than 29 days in the month. Earlier than this epoch, the days were fewer than 29, and later fewer also. Although measured in years, this epoch in the earth's history must be very remote, yet when we contemplate the whole series of changes it must be considered as a comparatively recent event. In a sense, indeed, we may be said to have passed recently through the middle stage of our history.

Now, pursuing the series of changes further back than the epoch when there was the maximum number of days in the month, we find the earth still rotating faster and faster, and the moon drawing nearer and nearer to the earth, and revolving in shorter and shorter periods. But a change has now supervened, so that the rate at which the month is shortening is more rapid than the rate of change in the day. Consequently, the moon now gains, as it were, on the earth, which cannot get round so frequently in the month as it did before. In other words, the number of days in the month declines from the maximum of 29, and is finally reduced to one. When there is only one day in the month, the earth and the moon go round at the same rate, so that the moon always looks at the same side of the earth, and so far as concerns the motion they might be fastened together by a rigid bar.

This is the same conclusion at which we arrived with respect to the remote future. But the two cases differ widely; for whereas in the future the period of the common rotation will be 55 of our present days, in the past we find the two bodies going round each other in between three and five of our present hours. A satellite revolving round the earth in so short a period must almost touch the earth's surface. The system is therefore traced until the moon nearly touches the earth, and the two go round each other like a single solid body in about three to five hours.

The series of changes has been traced forward and backward from the present time, but it will make the whole process more intelligible, and the opportunity will be afforded for certain further considerations, if I sketch the history again in the form of a continuous narrative.

Let us imagine a planet attended by a satellite which revolves so as nearly to touch its surface, and continuously to face the same side of the planet's surface. If now, for some reason, the satellite's month comes to differ very slightly from the planet's day, the satellite will no longer continuously face the same side of the planet, but will pass over every part of the planet's equator in turn. This is the condition necessary for the generation of tidal oscillations in the planet, and as the molten lava, of which we suppose it to be formed, is a sticky or viscous fluid, the tidal oscillations must be subject to friction. Tidal friction will then begin to do its work, but the result will be very different according as the satellite revolves a little faster or a little slower than the planet. If it revolves a little faster, so that the month is shorter than the day, we have a condition not contemplated in fig. 36; it is easy to see, however, that as the satellite is always leaving the planet behind it, the apex of the trial protuberance must be directed to a point behind the satellite in its orbit. In this case the rotation of the planet must be accelerated by the tidal friction, and the satellite will be drawn inward towards the planet, into which it must ultimately fall.

In the application of this theory to the earth and moon, it is obvious that the very existence of the moon negatives the hypothesis that the initial month was even infinitesimally shorter than the day. We must then suppose that the moon revolved a little more slowly than the earth rotated. In this case the tidal friction would retard the earth's rotation, and force the moon to recede from the earth, and so perform her orbit more slowly. Accordingly, the primitive day and the primitive month lengthen, but the month increases much more rapidly than the day, so that the number of days in a month increases. This proceeds until that number reaches a maximum, which in the case of our planet is about 29.

After the epoch of the maximum number of days in the month, the rate of change in the length of the day becomes less rapid than that in the length of the month; and although both periods increase, the number of days in the month begins to diminish. The series of changes then proceeds until the two periods come again to an identity, when we have the earth and the moon as they were at the beginning, revolving in the same period, with the moon always facing the same side of the earth. But in her final condition the moon will be a long way off the earth instead of being quite close to it.

Although the initial and final states resemble each other, yet they differ in one respect which is of much importance, for in the initial condition the motion is unstable, whilst finally it is stable. The meaning of this is, that if the moon were even infinitesimally disturbed from the initial mode of motion, she would necessarily either fall into the planet, or recede therefrom, and it would be impossible for her to continue to move in that neighborhood. She is unstable in the same sense in which an egg when balanced on its point is unstable; the smallest mote of dust will upset it, and practically it cannot stay in that position. But the final condition resembles the case of the egg lying on its side, which only rocks a little when we disturb it. So if the moon were slightly disturbed from her final condition, she would continue to describe very nearly the same path round the earth, and would not assume some entirely new form of orbit.

It is by methods of rigorous argument that the moon is traced back to the initial unstable condition when she revolved close to the earth. But the argument here breaks down, and calculation is incompetent to tell us what occurred before, and how she attained that unstable mode of motion. If we were to find a pendulum swinging in a room, where we knew that it had been undisturbed for a long time, we might, by observing its velocity and allowing for the resistance of the air, conclude that at some previous moment it had just been upside down, but calculation could never tell us how it had reached that position. We should of course feel confident that some one had started it. Now a similar hiatus must occur in the history of the moon, but it is not so easy to supply the missing episode. It is indeed only possible to speculate as to the preceding history.

But there is some basis for our speculation; for I say that if a planet, such as the earth, made each rotation in three hours, it would very nearly fly to pieces. The attraction of gravity would be barely strong enough to hold it together, just as the cohesive strength of iron is insufficient to hold a fly-wheel together if it is spun too fast. There is, of course, an important distinction between the case of the ruptured fly-wheel and the supposed break-up of the earth; for when a fly-wheel breaks, the pieces are hurled apart as soon as the force of cohesion fails, whereas when a planet breaks up through too rapid rotation, gravity must continue to hold the pieces together after they have ceased to form parts of a single body.

Hence we have grounds for conjecturing that the moon is composed of fragments of the primitive planet which we now call the earth, which detached themselves when the planet spun very swiftly, and afterwards became consolidated. It surpasses the power of mathematical calculation to trace the details of the process of this rupture and subsequent consolidation, but we can hardly doubt that the system would pass through a period of turbulence, before order was reëstablished in the formation of a satellite.

I have said above that rapid rotation was probably the cause of the birth of the moon, but it may perhaps not have been brought about by this cause alone. There are certain considerations which make it difficult to ascertain the initial common period of revolution of the moon and the earth with accuracy; it may lie between three and five hours. Now I think that such a speed might not quite suffice to cause the primitive planet to break up. In Chapter XVIII. we shall consider in greater detail the conditions under which a rotating mass of liquid would rupture, but for the present it may suffice to say that, where the rotating body is heterogeneous in density, like the earth, the exact determination of the limiting speed of rotation is not possible. Is there, then, any other cause which might coöperate with rapid rotation in producing rupture? I think there is such a cause, and, although we are here dealing with guesswork, I will hazard the suggestion.

The primitive planet, before the birth of the moon, was rotating rapidly with reference to the sun, and it must therefore have been agitated by solar tides. In Chapter IX. it was pointed out that there is a general dynamical law which enables us to foresee the magnitude of the oscillations of a system under the action of external forces. That law depended on the natural or free period of the oscillation of the system when disturbed and left to itself, free from the intervention of external forces. We saw that the more nearly the periodic forces were timed to agree with the free period, the greater was the amplitude of the oscillations of the system. Now it is easy to calculate the natural or free period of the oscillation of a homogeneous liquid globe of the same density as the earth, namely, five and a half times as heavy as water; the period is found to be 1 hour 34 minutes. The heterogeneity of the earth introduces a complication of which we cannot take account, but it seems likely that the period would be from $1\frac{1}{2}$ to 2 hours. The period of the solar semidiurnal tide is half a day, and if the day were from 3 to 4 of our present hours the forced period of the tide would be in close agreement with the free period of oscillation.

May we not then conjecture that as the rotation of the primitive earth was gradually reduced by solar tidal friction, the period of the solar tide was brought into closer and closer agreement with the free period, and that consequently the solar tide increased more and more in height? In this case the oscillation might at length become so violent that, in coöperation with the rapid rotation, it shook the planet to pieces, and that huge fragments were detached which ultimately became our moon.

There is nothing to tell us whether this theory affords the true explanation of the birth of the moon, and I say that it is only a wild speculation, incapable of verification.

But the truth or falsity of this speculation does not militate against the acceptance of the general theory of tidal friction, which, standing on the firm basis of mechanical necessity, throws much light on the history of the earth and the moon, and correlates the lengths of our present day and month.

I have said above that the sequence of events has been stated without reference to the scale of time. It is, however, of the utmost importance to gain some idea of the time requisite for all the changes in the system. If millions of millions of years were necessary, the theory would have to be rejected, because it is known from other lines of argument that there is not an unlimited bank of time on which to draw. The uncertainty as to the duration of the solar system is wide, yet we are sure that it has not existed for an almost infinite past.

Now, although the actual time scale is indeterminate, it is possible to find the minimum time adequate for the transformation of the moon's orbit from its supposed initial condition to its present shape. It may be proved, in fact, that if tidal friction always operated under the conditions most favorable for producing rapid change, the sequence of events from the beginning until to-day would have occupied a period of between 50 and 60 millions of years. The actual period, of course, must have been much greater. Various lines of argument as to the age of the solar system have led to results which differ widely among themselves, yet I cannot think that the applicability of the theory is negatived by the magnitude of the period demanded. It may be that science will have to reject the theory in its full extent, but it seems unlikely that the ultimate verdict will be adverse to the preponderating influence of the tide in the evolution of our planet.

If this history be true of the earth and moon, it should throw light on many peculiarities of the solar system. In the first place, a corresponding series of changes must have taken place in the moon herself. Once on a time the moon must have been molten, and the great extinct volcanoes revealed by the telescope are evidences of her primitive heat. The molten mass must have been semi-fluid, and the earth must have raised in it enormous tides of molten lava. Doubtless the moon once rotated rapidly on her axis, and the frictional resistance to her tides must have impeded her rotation. This cause must have added to the moon's recession from the earth, but as the moon's mass is only an eightieth part of that of the earth, the effect on the moon's orbit must have been small. The only point to which we need now pay attention is that the rate of her rotation was reduced. She rotated then more and more slowly until the tide solidified, and thenceforward and to the present day she has shown the same face to the earth. Kant and Laplace in the last century, and Helmholtz in recent times, have adduced this as the explanation of the fact that the moon always shows us the same face. Our theory, then, receives a striking confirmation from the moon; for, having ceased to rotate relatively to us, she has actually advanced to that condition which may be foreseen as the fate of the earth.

The earth tide in the moon is now solidified so that the moon's equator is not quite circular, and the longer axis is directed towards the earth. Laplace has considered the action of the earth on this solidified tide, and has shown that the moon must rock a little as she moves round the earth. In consequence of this rocking motion or libration of the moon, and also of the fact that her orbit is elliptic, we are able to see just a little more than half of the moon's surface.

Thus far I have referred in only one passage to the influence of solar tides, but these are of considerable importance, being large enough to cause the conspicuous phenomena of spring and neap tides. Now, whilst the moon is retarding the earth's rotation, the sun is doing so also. But these solar tides react only on the earth's motion round the sun, leaving the moon's motion round the earth unaffected. It might perhaps be expected that parallel changes in the earth's orbit would have proceeded step by step, and that the earth might be traced to an origin close to the sun. The earth's mass is less than $\frac{1}{300,000}$ part of the sun's, and the reactive effect on the earth's orbit round the sun is altogether negligible. It is improbable, in fact, that the year is, from this cause at any rate, longer by more than a few seconds than it was at the very birth of the solar system.

Although the solar tides cannot have had any perceptible influence upon the earth's movement in its orbit, they will have affected the rotation of the earth to a considerable extent. Let us imagine ourselves transported to the indefinite future, when the moon's orbital period and the earth's diurnal period shall both be prolonged to 55 of our present days. The lunar tide in the earth will then be unchanging, just as the earth tide in the moon is now fixed; but the earth will be rotating with reference to the sun, and, if there are still oceans on the earth, her rotation will be subject to retardation in consequence of the solar tidal friction. The day will then become longer than the month, whilst the moon will at first continue to revolve round the earth in 55 days. Lunar tides will now be again generated, but as the motion of the earth will be very slow relatively to the moon, the oscillations will also be very slow, and subject to little friction. But that friction will act in opposition to the solar tides, and the earth's rotation will to some slight extent be assisted by the moon. The moon herself will slowly approach the earth, moving with a shorter period, and must ultimately fall back into the earth. We know that there are neither oceans nor atmosphere on the moon, but if there were such, the moon would have been subject to solar tidal friction, and would now be rotating slower than she revolves.

Authorities.

See the end of Chapter XVII.

CHAPTER XVII

TIDAL FRICTION (CONTINUED)

It has been shown in the last chapter that the prolongation of the day and of the month under the influence of tidal friction takes place in such a manner that the month will ultimately become longer than the day. Until recent times no case had been observed in the solar system in which a satellite revolved more rapidly than its planet rotated, and this might have been plausibly adduced as a reason for rejecting the actual efficiency of solar tidal friction in the process of celestial evolution. At length however, in 1877, Professor Asaph Hall discovered in the system of the planet Mars a case of the kind of motion which we foresee as the future fate of the moon and earth, for he found that the planet was attended by two satellites, the nearer of which has a month shorter than the planet's day. He gives an interesting account of what had been conjectured, partly in jest and partly in earnest, as to the existence of satellites attending that planet. This foreshadowing of future discoveries is so curious that I quote the following passage from Professor Hall's paper. He writes:—

"Since the discovery of the satellites of Mars, the remarkable statements of Dean Swift and Voltaire concerning the satellites of this planet, and the arguments of Dr. Thomas Dick and others for the existence of such bodies, have attracted so much attention, that a brief account of the writings on this subject may be interesting.

"The following letter of Kepler was written to one of his friends soon after the discovery by Galileo in 1610 of the four satellites of Jupiter, and when doubts had been expressed as to the reality of this discovery. The news of the discovery was communicated to him by his friend Wachenfels; and Kepler says:—

" 'Such a fit of wonder seized me at a report which seemed to be so very absurd, and I was thrown into such agitation at seeing an old dispute between us decided in this way, that between his joy, my coloring, and the laughter of both, confounded as we were by such a novelty, we were hardly capable, he of speaking, or I of listening. On our parting, I immediately began to think how there could be any addition to the number of the planets without overturning my "Cosmographic Mystery," according to which Euclid's five regular solids do not allow more than six planets round the sun.... I am so far from disbelieving the existence of the four circumjovial planets, that I long for a telescope, to anticipate you, if possible, in discovering *two* round Mars, as the proportion seems to require, *six* or *eight* round Saturn, and perhaps *one* each round Mercury and Venus.'

"Dean Swift's statement concerning the satellites of Mars is in his famous satire, 'The Travels of Mr. Lemuel Gulliver.' After describing his arrival in Laputa, and the devotion of the Laputians to mathematics and music, Gulliver says:—

"'The knowledge I had in mathematics gave me great assistance in acquiring their phraseology, which depended much upon that science, and music; and in the latter I was not unskilled. Their ideas were perpetually conversant in lines and figures. If they would, for example, praise the beauty of a woman, or of any other animal, they describe it by rhombs, circles, parallelograms, ellipses, and other geometrical terms, or by words of art drawn from music, needless here to repeat.... And although they are dexterous enough upon a piece of paper, in the management of the rule, the pencil, and the divider, yet in the common actions and the behavior of life, I have not seen a more clumsy, awkward, and unhandy people, nor so slow and perplexed in their conceptions upon all subjects, except those of mathematics and music. They are very bad reasoners, and vehemently given to opposition, unless when they happen to be of the right opinion, which is seldom their case.... These people are under continual disquietudes, never enjoying a minute's peace of mind; and their disturbances proceed from causes which very little affect the rest of mortals. Their apprehensions arise from several changes they dread in the celestial bodies.

For instance, that the earth, by the continual approaches of the sun towards it, must, in the course of time, be absorbed, or swallowed up. That the face of the sun will, by degrees, be encrusted with its own effluvia, and give no more light to the world. That the earth very narrowly escaped a brush from the tail of the last comet, which would have infallibly reduced it to ashes; and that the next, which they have calculated for one-and-thirty years hence, will probably destroy us. For if, in its perihelion, it should approach within a certain degree of the sun (as by their calculations they have reason to dread,) it will receive a degree of heat ten thousand times more intense than that of red-hot glowing iron; and, in its absence from the sun, carry a blazing tail ten hundred thousand and fourteen miles long; through which, if the earth should pass at the distance of one hundred thousand miles from the nucleus, or main body of the comet, it must, in its passage, be set on fire, and reduced to ashes. That the sun, daily spending its rays, without any nutriment to supply them, will at last be wholly consumed and annihilated; which must be attended with the destruction of this earth, and of all the planets that receive their light from it.

"'They are so perpetually alarmed with the apprehension of these, and the like impending dangers, that they can neither sleep quietly in their beds, nor have any relish for the common pleasures and amusements of life. When they meet an acquaintance in the morning, the first question is about the sun's health, how he looked at his setting and rising, and what hopes they had to avoid the stroke of the approaching comet.... They spend the greatest part of their lives in observing the celestial bodies, which they do by the assistance of glasses, far excelling ours in goodness. For although their largest telescopes do not exceed three feet, they magnify much more than those of a hundred with us, and show the stars with greater clearness. This advantage has enabled them to extend their discoveries much further than our astronomers in Europe; for they have made a catalogue of ten thousand fixed stars, whereas the largest of ours do not contain above one-third of that number.... They have likewise discovered two lesser stars, or satellites, which revolve about Mars; whereof the innermost is distant from the centre of the primary planet exactly three of his diameters, and the outermost, five; the former revolves in the space of ten hours, and the latter in twenty-one and a half; so that the squares of their periodical times are very near in the same proportion with the cubes of their distance from the centre of Mars; which evidently shows them to be governed by the same law of gravitation that influences the other heavenly bodies.'

"The reference which Voltaire makes to the moons of Mars is in his 'Micromegas, Histoire Philosophique.' Micromegas was an inhabitant of Sirius, who, having written a book which a suspicious old man thought smelt of heresy, left Sirius and visited our solar system. Voltaire says:—

"'Mais revenons à nos voyageurs. En sortant de Jupiter, ils traversèrent un espace d'environ cent millions de lieues, et ils côtoyèrent la planète de Mars, qui, comme on sait, est cinq fois plus petite que noire petit globe; ils virent deux lunes qui servent à cette planète, et qui ont échappé aux regards de nos astronomes. Je sais bien que le père *Castel* écrira, et même plaisamment, contre l'existence de ces deux lunes; mais je m'en rapporte à ceux qui raisonnent par analogie. Ces bons philosophes-là savent combien il serait difficile que Mars, qui est si loin du soleil, se passât à moins de deux lunes.'

"The argument by analogy for the existence of a satellite of Mars was revived by writers like Dr. Thomas Dick, Dr. Lardner, and others. In addition to what may be called the analogies of astronomy, these writers appear to rest on the idea that a beneficent Creator would not place a planet so far from the sun as Mars without giving it a satellite. This kind of argument has passed into some of our handbooks of astronomy, and is stated as follows by Mr. Chambers in his excellent book on 'Descriptive Astronomy,' 2d edition, p. 89, published in 1867:—

"'As far as we know, Mars possesses no satellite, though analogy does not forbid, but rather, on the contrary, infers the existence of one; and its never having been seen, in this case at least, proves nothing. The second satellite of Jupiter is only $\frac{1}{43}$ of the diameter of the primary, and a satellite $\frac{1}{43}$ of the diameter of Mars would be less than 100 miles in diameter, and therefore of a size barely within the reach of our largest telescopes, allowing nothing for its possibly close proximity to the planet. The fact that one of the satellites of Saturn was only discovered a few years ago renders the discovery of a satellite of Mars by no means so great an improbability as might be imagined.'

"Swift seems to have had a hearty contempt for mathematicians and astronomers, which he has expressed in his description of the inhabitants of Laputa. Voltaire shared this contempt, and delighted in making fun of the philosophers whom Frederick the Great collected at Berlin. The 'père Castel' may have been le père Louis Castel, who published books on physics and mathematics at Paris in 1743 and 1758. The probable origin of these speculations about the moons of Mars was, I think, Kepler's analogies. Astronomers failing to verify these, an opportunity was afforded to satirists like Swift and Voltaire to ridicule such arguments."[1]

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[1] *Observations and Orbits of the Satellites of Mars*, by Asaph Hall. Washington, Government Printing Office, 1878.

As I have already said, these prognostications were at length verified by Professor Asaph Hall in the discovery of two satellites, which he named Phobos and Deimos—Fear and Panic, the dogs of war. The period of Deimos is about 30 hours, and that of Phobos somewhat less than 8 hours, whilst the Martian day is of nearly the same length as our own. The month of the inner minute satellite is thus less than a third of the planet's day; it rises to the Martians in the west, and passes through all its phases in a few hours; sometimes it must even rise twice in a single Martian night. As we here find an illustration of the condition foreseen for the earth and moon, it seems legitimate to suppose that solar tidal friction has retarded the planet's rotation until it has become slower than the revolution of one of the satellites. It would seem as if the ultimate fate of Phobos will be absorption in the planet.

Several of the satellites of Jupiter and of Saturn present faint inequalities of coloring, and telescopic examination has led astronomers to believe that they always present the same face to their planets. The theory of tidal friction would certainly lead us to expect that these enormous planets should work out the same result for their relatively small satellites that the earth has produced in the moon.

The proximity of the planets Mercury and Venus to the sun should obviously render solar tidal friction far more effective than with us. The determination of the periods of rotation of these planets thus becomes a matter of much interest. But the markings on their disks are so obscure that the rates of their rotations have remained under discussion for many years. Until recently the prevailing opinion was that in both cases the day was of nearly the same length as ours; but a few years ago Schiaparelli of Milan, an observer endowed with extraordinary acuteness of vision, announced as the result of his observations that both Mercury and Venus rotate only once in their respective years, and that each of them constantly presents the same face to the sun. These conclusions have recently been confirmed by Mr. Percival Lowell from observations made in Arizona. Although on reading the papers of these astronomers it is not easy to see how they can be mistaken, yet it should be noted that others have failed to detect the markings on the planet's disks, although they apparently enjoyed equal advantages for observation.[1]

If, as I am disposed to do, we accept these observations

[1] Dr. See, a member of the staff of the Flagstaff Observatory, Arizona, tells me that he has occasionally looked at these planets through the telescope, although he took no part in the systematic observation. In his opinion it would be impossible for any one at Flagstaff to doubt the reality of the markings. There are, however, many astronomers of eminence who suspend their judgment, and await confirmation by other observers at other stations.

as sound, we find that evidence favorable to the theory of tidal friction is furnished by the planets Mercury and Venus, and by the satellites of the earth, Jupiter and Saturn, whilst the Martian system is yet more striking as an instance of an advanced stage in evolution.

It is well known that the figure of the earth is flattened by the diurnal rotation, so that the polar axis is shorter than any equatorial diameter. At the present time the excess of the equatorial radius over the polar radius is $\frac{1}{290}$ part of either of them. Now in tracing the history of the earth and moon, we found that the earth's rotation had been retarded, so that the day is now longer than it was. If then the solid earth has always been absolutely unyielding, and if an ocean formerly covered the planet to a uniform depth, the sea must have gradually retreated towards the poles, leaving the dry land exposed at the equator. If on the other hand the solid earth had formerly its present shape, there must then have been polar continents and a deep equatorial sea.

But any considerable change in the speed of the earth's rotation would, through the action of gravity, bring enormous forces to bear on the solid earth. These forces are such as would, if they acted on a plastic material, tend to restore the planet's figure to the form appropriate to its changed rotation. It has been shown experimentally by M. Tresca and others that even very rigid and elastic substances lose their rigidity and their elasticity, and become plastic under the action of sufficiently great forces. It appears to me, therefore, legitimate to hold to the belief in the temporary rigidity of the earth's mass, as explained in Chapter XV., whilst contending that under a change of rotational velocity the earth may have become plastic, and so have maintained a figure adapted to its speed. Geological observation shows that rocks have been freely twisted and bent near the earth's surface, and it is impossible to doubt that under altered rotation the deeper portions of the earth would have been subjected to very great stress. I conjecture that the internal layers might adapt themselves by continuous flow, whilst the superficial portion might yield impulsively. Earthquakes are probably due to unequal shrinkage of the planet in cooling, and each shock would tend to bring the strata into their position of rest; thus the earth's surface would avail itself of the opportunity afforded by earthquakes of acquiring its proper shape. The deposit in the sea of sediment, derived from the denudation of continents, affords another means of adjustment of the figure of the planet. I believe then that the earth has always maintained a shape nearly appropriate to its rotation. The existence of the continents proves that the adjustment has not been perfect, and we shall see reason to believe that there has been also a similar absence of complete adjustment in the interior.

But the opinion here maintained is not shared by the most eminent of living authorities, Lord Kelvin; for he holds that the fact that the average figure of the earth corresponds with the actual length of the day proves that the planet was consolidated at a time when the rotation was but little more rapid than it is now. The difference between us is, however, only one of degree, for he considers that the power of adjustment is slight, whilst I hold that it would be sufficient to bring about a considerable change of shape within the period comprised in geological history.

If the adjustment of the planet's figure were perfect, the continents would sink below the ocean, which would then be of uniform depth. But there is no superficial sign, other than the dry land, of absence of adaptation to the present rotation—unless indeed the deep polar sea discovered by Nansen be such. Yet, as I have hinted above, some tokens still exist in the earth of the shorter day of the past. The detection of this evidence depends however on arguments of so technical a character that I cannot hope in such a work as this to do more than indicate the nature of the proof.

The earth is denser towards the centre than outside, and the layers of equal density are concentric. If then the materials were perfectly plastic throughout, not only the surface, but also each of these layers would be flattened to a definite extent, which depends on the rate of rotation and on the law governing the internal density of the earth. Although the rate at which the earth gets denser is unknown, yet it is possible to assign limits to the density at various depths. Thus it can be proved that at any internal point the density must lie between two values which depend on the position of the point in question. So also, the degree of flattening at any internal point is found to lie between two extreme limits, provided that all the internal layers are arranged as they would be if the whole mass were plastic.

Now variations in the law of internal density and in the internal flattening would betray themselves to our observation in several ways. In the first place, gravity on the earth's surface would be changed. The force of gravity at the poles is greater than at the equator, and the law of its variation according to latitude is known. In the second place the amount of the flattening of the earth's surface would be altered, and the present figure of the earth is known with considerable exactness. Thirdly the figure and law of density of the earth govern a certain irregularity or inequality in the moon's motion, which has been carefully evaluated by astronomers. Lastly the precessional and nutational motion of the earth is determined by the same causes, and these motions also are accurately known. These four facts of observation—gravity, the ellipticity of the earth, the lunar inequality, and the precessional and nutational motion of the earth—are so intimately intertwined that one of them cannot be touched without affecting the others.

Now Édouard Roche, a French mathematician, has shown that if the earth is perfectly plastic, so that each layer is exactly of the proper shape for the existing rotation, it is not possible to adjust the unknown law of internal density so as to make the values of all these elements accord with observation. If the density be assumed such as to fit one of the data, it will produce a disagreement with observation in others. If, however, the hypothesis be abandoned that the internal strata all have the proper shapes, and if it be granted that they are a little more flattened than is due to the present rate of rotation, the data are harmonized together; and this is just what would be expected according to the theory of tidal friction. But it would not be right to attach great weight to this argument, for the absence of harmony is so minute that it might be plausibly explained by errors in the numerical data of observation. I notice, however, that the most competent judges of this intricate subject are disposed to regard the discrepancy as a reality.

We have seen in the preceding chapter that the length of day has changed but little within historical times. But the period comprised in written history is almost as nothing compared with the whole geological history of the earth. We ought then to consider whether geology furnishes any evidence bearing on the theory of tidal friction. The meteorological conditions on the earth are dependent to a considerable extent on the diurnal rotation of the planet, and therefore those conditions must have differed in the past. Our storms are of the nature of aerial eddies, and they derive their rotation from that of the earth. Accordingly storms were probably more intense when the earth spun more rapidly. The trunks of trees should be stronger than they are now to withstand more violent storms. But I cannot learn that there is any direct geological evidence on this head, for deciduous trees with stiff trunks seem to have been a modern product of geological time, whilst the earlier trees more nearly resembled bamboos, which yield to the wind instead of standing up to it. It seems possible that trees and plants would not be exterminated, even if they suffered far more wreckage than they do now. If trees with stiff trunks could only withstand the struggle for existence when storms became moderate in intensity, their absence from earlier geological formations would be directly due to the greater rapidity of the earth's rotation in those times.

According to our theory the tides on the seacoast must certainly have had a much wider range, and river floods must probably have been more severe. The question then arises whether these agencies should have produced sedimentary deposits of coarser grain than at present. Although I am no geologist, I venture to express a doubt whether it is possible to tell, within very wide limits, the speed of the current or the range of the tide that has brought down and distributed any sedimentary deposit. I doubt whether any geologist would assert that floods might not have been twice or thrice as frequent, or that the tide might not have had a very much greater range than at present.

In some geological strata ripple-marks have been preserved which exactly resemble modern ones. This has, I believe, been adduced as an argument against the existence of tides of great range. Ripples are, however, never produced by a violent scour of water, but only by gentle currents or by moderate waves. The turn of the tide must be gentle to whatever height it rises, and so the formation of ripple-mark should have no relationship to the range of tide.

It appears then that whilst geology affords no direct confirmation of the theory, yet it does not present any evidence inconsistent with it. Increased activity in the factors of change is important to geologists, since it renders intelligible a diminution in the time occupied by the history of the earth; and thus brings the views of the geologist and of the physicist into better harmony.

Although in this discussion I have maintained the possibility that a considerable portion of the changes due to tidal friction may have occurred within geological history, yet it seems to me probable that the greater part must be referred back to pre-geological times, when the planet was partially or entirely molten.

The action of the moon and sun on a plastic and viscous planet would have an effect of which some remains may perhaps still be traceable. The relative positions of the moon and of the frictionally retarded tide were illustrated in the last chapter by fig. 36. That figure shows that the earth's rotation is retarded by forces acting on the tidal protuberances in a direction adverse to the planet's rotation. As the plastic substance, of which we now suppose the planet to be formed, rises and falls rhythmically with the tide, the protuberant portions are continually subject to this retarding force. Meanwhile the internal portions are urged onward by the inertia due to their velocity. Accordingly there must be a slow motion of the more superficial portions with reference to the interior. From the same causes, under present conditions, the whole ocean must have a slow westerly drift, although it has not been detected by observation.

Returning however to our plastic planet, the equatorial portion is subjected to greater force than the polar regions, and if meridians were painted on its surface, as on a map, they would gradually become distorted. In the equatorial belt the original meridional lines would still run north and south, but in the northern hemisphere they would trend towards the northeast, and in the southern hemisphere towards the southeast. This distortion of the surface would cause the surface to wrinkle, and the wrinkles should be warped in the directions just ascribed to the meridional lines. If the material yielded very easily I imagine that the wrinkles would be small, but if it were so stiff as only to yield with difficulty they might be large.

There can be no doubt as to the correctness of this conclusion as to a stiff yet viscous planet, but the application of these ideas to the earth is hazardous and highly speculative. We do, however, observe that the continents, in fact, run roughly north and south. It may appear fanciful to note, also, that the northeastern coast of America, the northern coast of China, and the southern extremity of South America have the proper theoretical trends. But the northwestern coast of America follows a line directly adverse to the theory, and the other features of the globe are by no means sufficiently regular to inspire much confidence in the justice of the conjecture.[1]

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[1]See, also, W. Prinz, *Torsion apparente des planètes*, "Annuaire de l'Obs. R. de Bruxelles," 1891.

We must now revert to the astronomical aspects of our problem. It is natural to inquire whether the theory of tidal friction is competent to explain any peculiarities of the motion of the moon and earth other than those already considered. It has been supposed thus far that the moon moves over the earth's equator in a circular orbit, and that the equator coincides with the plane in which the earth moves in its orbit. But the moon actually moves in a plane different from that in which the earth revolves round the sun, her orbit is not circular but elliptic, and the earth's equator is oblique to the orbit. We must consider, then, how tidal friction will affect these three factors.

Let us begin by considering the obliquity of the equator to the ecliptic, which produces the seasonal changes of winter and summer. The problem involved in the disturbance of the motion of a rotating body by any external force is too complex for treatment by general reasoning, and I shall not attempt to explain in detail the interaction of the moon and earth in this respect.

The attractions of the moon and sun on the equatorial protuberance of the earth causes the earth's axis to move slowly and continuously with reference to the fixed stars. At present, the axis points to the pole-star, but 13,000 years hence the present pole-star will be 47° distant from the pole, and in another 13,000 years it will again be the pole-star.

Throughout this precessional movement the obliquity of the equator to the ecliptic remains constant, so that winter and summer remain as at present. There is also, superposed on the precession, the nutational or nodding motion of the pole to which I referred in Chapter XV. In the absence of tidal friction the attractions of the moon and sun on the tidal protuberance would slightly augment the precession due to the solid equatorial protuberance, and would add certain very minute nutations of the earth's axis; the amount of these tidal effects, is, however, quite insignificant. But under the influence of tidal friction, the matter assumes a different aspect, for the earth's axis will not return at the end of each nutation to exactly the same position it would have had in the absence of friction, and there is a minute residual effect which always tends in the same direction. A motion of the pole may be insignificant when it is perfectly periodic, but it becomes important in a very long period of time when the path described is not absolutely reëntrant. Now this is the case with regard to the motion of the earth's axis under the influence of frictionally retarded tides, for it is found to be subject to a gradual drift in one direction.

In tracing the history of the earth and moon backwards in time we found the day and month growing shorter, but at such relative speeds that the number of days in the month diminished until the day and month became equal. This conclusion remains correct when the earth is oblique to its orbit, but the effect on the obliquity is found to depend in a remarkable manner upon the number of days in the month. At present and for a long time in the past the obliquity is increasing, so that it was smaller long ago. But on going back to the time when the day was six and the month twelve of our present hours we find that the tendency for the obliquity to increase vanishes. In other words, if there are more than two days in a month the obliquity will increase, if less than two it will diminish.

Whatever may be the number of days in the month, the rate of increase or diminution of obliquity varies as the obliquity which exists at the moment under consideration. If, then, a planet be spinning about an axis absolutely perpendicular to the plane of its satellite's orbit, the obliquity remains invariable. But if we impart infinitesimal obliquity to a planet whose day is less than half a month, that infinitesimal obliquity will increase; whilst, if the day is more than half a month, the infinitesimal obliquity will diminish. Accordingly, the motion of a planet spinning upright is stable, if there are less than two days in a month, and unstable if there are more than two.

It is not legitimate to ascribe the whole of the present obliquity of $23\frac{1}{2}^\circ$ to the influence of tidal friction, because it appears that when there were only two days in the month, the obliquity was still as much as 11°. It is, moreover, impossible to explain the considerable obliquity of the other planets to their orbits by this cause. It must, therefore, be granted that there was some unknown cause which started the planets in rotation about axes oblique to their orbits. It remains, however, certain that a planet, rotating primitively without obliquity, would gradually become inclined to its orbit, although probably not to so great an extent as we find in the case of the earth.

The next subject to be considered is the fact that the moon's orbit is not circular but eccentric. Here, again, it is found that if the tides were not subject to friction, there would be no sensible effect on the shape of the moon's path, but tidal friction produces a reaction on the moon tending to change the degree of eccentricity. In this case, it is possible to indicate by general reasoning the manner in which this reaction operates. We have seen that tidal reaction tends to increase the moon's distance from the earth. Now, when the moon is nearest, in perigee, the reaction is stronger than when she is furthest, in apogee. The effect of the forces in perigee is such that the moon's distance at the next succeeding apogee is greater than it was at the next preceding apogee; so, also, the effect of the forces in apogee is an increase in the perigeal distance. But the perigeal effect is stronger than the apogeal, and, therefore, the apogeal distances increase more rapidly than the perigeal ones. It follows, therefore, that, whilst the orbit as a whole expands, it becomes at the same time more eccentric.

The lunar orbit is then becoming more eccentric, and numerical calculation shows that in very early times it must have been nearly circular. But mathematical analysis indicates that in this case, as with the obliquity, the rate of increase depends in a remarkable manner upon the number of days in the month. I find in fact that if eighteen days are less than eleven months the eccentricity will increase, but in the converse case it will diminish; in other words the critical stage at which the eccentricity is stationary is when $1\frac{7}{11}$ days is equal to the month. It follows from this that the circular orbit of the satellite is dynamically stable or unstable according as $1\frac{7}{11}$ days is less or greater than the month.

The effect of tidal friction on the eccentricity has been made the basis of extensive astronomical speculations by Dr. See. I shall revert to this subject in Chapter XIX., and will here merely remark that systems of double stars are found to revolve about one another in orbits of great eccentricity, and that Dr. See supposes that the eccentricity has arisen from the tidal action of each star on the other.

The last effect of tidal friction to which I have to refer is that on the plane of the moon's orbit. The lunar orbit is inclined to that of the earth round the sun at an angle of 5°, and the problem to be solved is as to the nature of the effect of tidal friction on that inclination. The nature of the relation of the moon's orbit to the ecliptic is however so complex that it appears hopeless to explain the effects of tidal action without the use of mathematical language, and I must frankly give up the attempt. I may, however, state that when the moon was near the earth she must have moved nearly in the plane of the earth's equator, but that the motion gradually changed so that she has ultimately come to move nearly in the plane of the ecliptic. These two extreme cases are easily intelligible, but the transition from one case to the other is very complicated. It may suffice for this general account of the subject to know that the effects of tidal friction are quite consistent with the present condition of the moon's motion, and with the rest of the history which has been traced.

This discussion of the effects of tidal friction may be summed up thus:—

If a planet consisted partly or wholly of molten lava or of other fluid, and rotated rapidly about an axis perpendicular to the plane of its orbit, and if that planet was attended by a single satellite, revolving with its month a little longer than the planet's day, then a system would necessarily be developed which would have a strong resemblance to that of the earth and moon.

A theory reposing on *veræ causæ* which brings into quantitative correlation the lengths of the present day and month, the obliquity of the ecliptic, the eccentricity and the inclination of the lunar orbit, should have strong claims to acceptance.

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CHAPTER XVIII

THE FIGURES OF EQUILIBRIUM OF A ROTATING MASS OF LIQUID

The theory of the tides involves the determination of the form assumed by the ocean under the attraction of a distant body, and it now remains to discuss the figure which a rotating mass of liquid may assume when it is removed from all external influences. The forces which act upon the liquid are the mutual gravitation of its particles, and the centrifugal force due to its rotation. If the mass be of the appropriate shape, these two opposing forces will balance one another, and the shape will be permanent. The problem in hand is, then, to determine what shapes of this kind are possible.

In 1842 a distinguished Belgian physicist, M. Plateau,[1] devised an experiment which affords a beautiful illustration of the present subject. The experiment needs very nice adjustment in several respects, but I refer the reader to Plateau's paper for an account of the necessary precautions. Alcohol and water may be so mixed as to have the same den-

[1] He is justly celebrated not only for his discoveries, but also for his splendid perseverance in continuing his researches after he had become totally blind.

sity as olive oil. If the adjustment of density is sufficiently exact, a mass of oil will float in the mixture, in the form of a spherical globule, without any tendency to rise or fall. The oil is thus virtually relieved from the effect of gravity. A straight wire, carrying a small circular disk at right angles to itself, is then introduced from the top of the vessel. When the disk reaches the globule, the oil automatically congregates itself round the disk in a spherical form, symmetrical with the wire.

The disk is then rotated slowly and uniformly, and carries with it the oil, but leaves the surrounding mixture at rest. The globule is then seen to become flattened like an orange, and as the rotation quickens it dimples at the centre, and finally detaches itself from the disk in the form of a perfect ring. This latter form is only transient; for the oil usually closes in again round the disk, or sometimes, with slightly different manipulation, the ring may break into drops which revolve round the centre, rotating round their axes as they go.

The force which holds a drop of water, or this globule of oil, together is called "surface tension" or "capillarity." It is due to a certain molecular attraction, quite distinct from that of gravitation, and it produces the same effect as if the surface of the liquid were enclosed in an elastic skin. There is of course no actual skin, and yet when the liquid is stirred the superficial particles attract their temporary neighbors so as to restore the superficial elasticity, continuously and immediately. The intensity of surface tension depends on the nature of the material with which the liquid is in contact; thus there is a definite degree of tension in the skin of olive oil in contact with spirits and water.

A globule at rest necessarily assumes the form of a sphere under the action of surface tension, but when it rotates it is distorted by centrifugal force. The polar regions become less curved, and the equatorial region becomes more curved, until the excess of the retaining power at the equator over that at the poles is sufficient to restrain the centrifugal force. Accordingly the struggle between surface tension and centrifugal force results in the assumption by the globule of an orange-like shape, or, with greater speed of rotation, of the other figures of equilibrium.

In very nearly the same way a large mass of gravitating and rotating liquid will naturally assume certain definite forms. The simplest case of the kind is when the fluid is at rest in space, without any rotation. Then mutual gravitation is the only force which acts on the system. The water will obviously crowd together into the smallest possible space, so that every particle may get as near to the centre as its neighbors will let it. I suppose the water to be incompressible, so that the central portion, although pressed by that which lies outside of it, does not become more dense; and so the water does not weigh more per cubic foot near the centre than towards the outside. Since there is no upwards and downwards, or right and left about the system, it must be symmetrical in every direction; and the only figure which possesses this quality of universal symmetry is the sphere. A sphere is then said to be a figure of equilibrium of a mass of fluid at rest.

If such a sphere of water were to be slightly deformed, and then released, it would oscillate to and fro, but would always maintain a nearly spherical shape. The speed of the oscillation depends on the nature of the deformation impressed upon it. If the water were flattened to the shape of an orange and released, it would spring back towards the spherical form, but would overshoot the mark, and pass on to a lemon shape, as much elongated as the orange was flattened. It would then return to the orange shape, and so on backwards and forwards, passing through the spherical form at each oscillation. This is the simplest kind of oscillation which the system can undergo, but there is an infinite number of other modes of any degree of complexity. The mathematician can easily prove that a liquid globe, of the same density as the earth, would take an hour and a half to pass from the orange shape to the lemon shape, and back to the orange shape. At present, the exact period of the oscillation is not the important point, but it is to be noted that if the body be set oscillating in any way whatever, it will continue to oscillate and will always remain nearly spherical. We say then that the sphere is a stable form of equilibrium of a mass of fluid. The distinction between stability and instability has been already illustrated in Chapter XVI. by the cases of an egg lying on its side and balanced on its end, and there is a similar distinction between stable and unstable modes of motion.

Let us now suppose the mass of water to rotate slowly, all in one piece as if it were solid. We may by analogy with the earth describe the axis of rotation as polar, and the central plane, at right angles to the axis, as equatorial. The equatorial region tends to move outwards in consequence of the centrifugal force of the rotation, and this tendency is resisted by gravitation which tends to draw the water together towards the centre. As the rotation is supposed to be very slow, centrifugal force is weak, and its effects are small thus the globe is very slightly flattened at the poles, like an orange or like the earth itself. Such a body resembles the sphere in its behavior when disturbed; it will oscillate, and its average figure in the course of its swing is the orange shape. It is therefore stable.

But it has been discovered that the liquid may also assume two other alternative forms. One of these is extremely flattened and resembles a flat cheese with rounded edges.

As the disk of liquid is very wide, the centrifugal force at the equator is very great, although the rotation is very slow. In the case of the orange-shaped figure, the slower the rotation the less is the equatorial centrifugal force, because it diminishes both with diminution of radius and fall of speed. But in the cheese shape the equatorial centrifugal force gains more by the increase of equatorial radius than it loses by diminution of rotation. Therefore the slower the rotation the broader the disk, and, if the rotation were infinitely slow, the liquid would be an infinitely thin, flat, circular disk.

The cheese-like form differs in an important respect from the orange-like form. If it were slightly disturbed, it would break up, probably into a number of detached pieces. The nature of the break-up would depend on the disturbance from which it started, but it is impossible to trace the details of the rupture in any case. We say then that the cheese shape is an unstable figure of equilibrium of a rotating mass of liquid.

The third form is strikingly different from either of the preceding ones. We must now imagine the liquid to be shaped like a long cigar, and to be rotating about a central axis perpendicular to its length. Here again the ends of the cigar are so distant from the axis of rotation that the centrifugal force is great, and with infinitely slow rotation the figure becomes infinitely long and thin. Now this form resembles the cheese in being unstable. It is remarkable that these three forms are independent of the scale on which they are constructed, for they are perfectly similar whether they contain a few pounds of water or millions of tons.[1] If the period of rotation and the density of the liquid are given, the shapes are absolutely determinable.

The first of the three figures resembles the earth and may be called the planetary figure, and I may continue to refer to the other two as the cheese shape and the cigar shape. The planetary and cheese shape are sometimes called the spheroids of Maclaurin, after their discoverer, and the cigar shape is generally named after Jacobi, the great German mathematician. For slow rotations the planetary form is stable, and the cheese and cigar are unstable. There are probably other possible forms of equilibrium, such as a ring, or several rings, or two detached masses revolving about one another like a planet and satellite, but for the present I only consider these three forms.

Now imagine three equal masses of liquid, infinitely distant from one another, and each rotating at the same slow speed, and let one of them have the planetary shape, the second the cheese shape, and the third the cigar shape. When

[1] It is supposed that they are more than a fraction of an inch across, otherwise surface tension would be called into play.

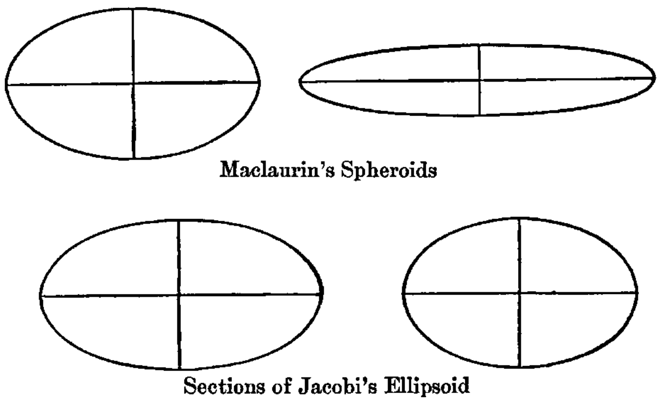

FIG. 37.

the rotations are simultaneously and equally augmented, we find the planetary form becoming flatter, the cheese form shrinking in diameter and thickening, and the cigar form shortening and becoming fatter. There is as yet no change in the stability, the first remaining stable and the second and third unstable. The three figures are illustrated in fig. 37, but the cigar shape is hardly recognizable by that name, since it has already become quite short and its girth is considerable.

Now it has been proved that as the cigar shape shortens, its tendency to break up becomes less marked, or in other words its degree of instability diminishes. At a certain stage, not as yet exactly determined, but which probably occurs when the cigar is about twice as long as broad, the instability disappears and the cigar form just becomes stable. I shall have to return to the consideration of this phase later. The condition of the three figures is now as follows: The planetary form of Maclaurin has become much flattened, but is still stable; the cigar form of Jacobi has become short and thick, and is just stable; and the cheese form of Maclaurin is still unstable, but its diameter has shrunk so much that the figure might be better described as a very flat orange.

On further augmenting the rotation the form of Jacobi still shrinks in length and increases in girth, until its length becomes equal to its greater breadth. Throughout the transformation the axis of rotation has always remained the shortest of the three, so that when the length becomes equal to the shorter equatorial diameter, the shape is not spherical, but resembles that of a much flattened orange. In fact, at this stage Jacobi's figure of equilibrium has degenerated to identity with the planetary shape. One of the upper ovals in fig. 38 represents the section of the form in which the planetary figure and the cigar figure coalesce, the former by continuous flattening, the latter by continuous shortening. The other upper figure represents the form to which the cheese-like figure of Maclaurin has been reduced; it will be observed

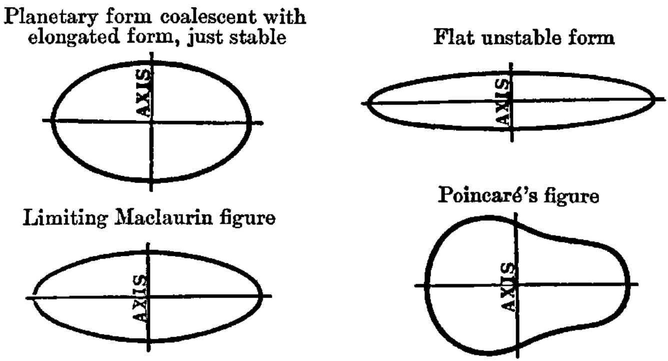

Fig. 38.

that it presents some resemblance to the coalescent form.

When the rotation is further augmented, there is no longer the possibility of an elongated Jacobian figure, and there remain only the two spheroids of Maclaurin. But an important change has now supervened, for both these are now unstable, and indeed no stable form consisting of a single mass of liquid has yet been discovered.

Still quickening the rotation, the two remaining forms, both unstable, grow in resemblance to one another, until at length they become identical in shape. This limiting form of Maclaurin's spheroids is shown in the lower part of fig. 38. If the liquid were water, it must rotate in 2 hours 25 minutes to attain this figure, but it would be unstable.

A figure for yet more rapid rotation has not been determined, but it seems probable that dimples would be formed on the axis, that the dimples would deepen until they met, and that the shape would then be annular. The actual existence of such figures in Plateau's experiment is confirmatory of this conjecture.

We must now revert to the consideration of the cigar-shaped figure of Jacobi, at the stage when it has just become stable. The whole of this argument depends on the fact that any figure of equilibrium is a member of a continuous series of figures of the same class, which gradually transforms itself as the rotation varies. Now M. Poincaré has proved that, when we follow a given series of figures and find a change from instability to stability, we are, as it were, served with a notice that there exists another series of figures coalescent with the first at that stage. We have already seen an example of this law, for the planetary figure of Maclaurin changed from stability to instability at the moment of its coalescence with the figure of Jacobi. Now I said that when the cigar form of Jacobi was very long it was unstable, but that when its length had shrunk to about twice its breadth it became stable; hence we have notice that at the moment of change another series of forms was coalescent with the cigar. It follows also from Poincaré's investigation that the other series of forms must have been stable before the coalescence.

Let us imagine then a mass of liquid in the form of Jacobi's cigar-shaped body rotating at the speed which just admits of stability, and let us pursue the series of changes backwards by making it rotate a little slower. We know that this retardation of rotation lengthens Jacobi's figure, and induces instability, but Poincaré has not only proved the existence and stability of the other series, but has shown that the shape is something like a pear.

Poincaré's figure is represented approximately in fig. 38, but the mathematical difficulty of the problem has been too great to admit of an absolutely exact drawing. The further development of the pear shape is unknown, when the rotation slackens still more. There can, however, be hardly any doubt that the pear becomes more constricted in the waist, and begins to resemble an hour-glass; that the neck of the hour-glass becomes thinner, and that ultimately the body separates into two parts. It is of course likewise unknown up to what stage in these changes Poincaré's figure retains its stability.

I have myself attacked this problem from an entirely different point of view, and my conclusions throw an interesting light on the subject, although they are very imperfect in comparison with Poincaré's masterly work. To understand this new point of view, we must consider a new series of figures, namely that of a liquid planet attended by a liquid satellite. The two bodies are supposed to move in a circle round one another, and each is also to revolve on its axis at such a speed as always to exhibit the same face to its neighbor. Such a system, although divided into two parts, may be described as a figure of equilibrium. If the earth were to turn round once in twenty-seven days, it would always show to the moon the same side, and the moon actually does present the same side to us. In this case the earth and the moon would form such a system as that I am describing. Both the planet and the satellite are slightly flattened by their rotations, and each of them exercises a tidal influence on the other, whereby they are elongated towards the other.

The system then consists of a liquid planet and liquid satellite revolving round one another, so as always to exhibit the same face to one another, and each tidally distorting the other. It is certain that if the two bodies are sufficiently far apart the system is a stable one, for if any slight disturbance be given, the whole system will not break up. But little is known as yet as to the limiting proximity of the planet and satellite, which will insure stability.

Now if the rotations and revolutions of the bodies be accelerated, the two masses must be brought nearer together in order that the greater attraction may counterbalance the centrifugal force. But as the two are brought nearer the tide-generating force increases in intensity with great rapidity, and accordingly the tidal elongation of the two bodies is much augmented.

A time will at length come when the ends of the two bodies will just touch, and we then have a form shaped like an hour-glass with a very thin neck. The form is clearly

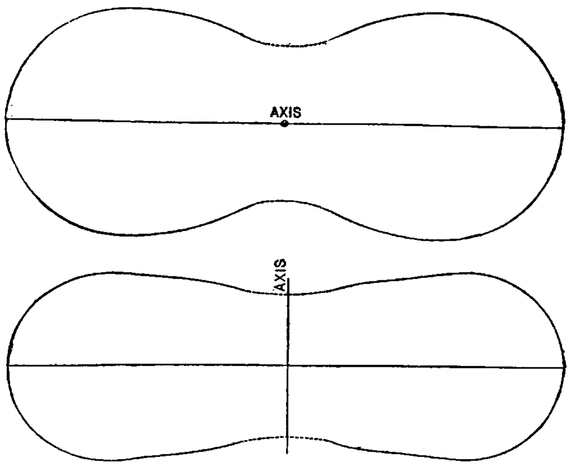

FIG. 39.—HOUR-GLASS FIGURE OF EQUILIBRIUM

Poincaré's figure, at an advanced stage of its evolution.

The figure 39 shows the form of one possible figure of this class; it arises from the coalescence of two equal masses of liquid, and the shape shown was determined by calculation. But there are any number of different sorts of hourglass shapes, according to the relative sizes of the planet and satellite which coalesce; and in order to form a continuous series with Poincaré's pear, it would be necessary to start with a planet and satellite of some definitely proportionate sizes. Unfortunately I do not know what the proportion may be. There are, however, certain indications which may ultimately lead to a complete knowledge of the series of figures from Jacobi's cigar shape down to the planet and satellite. It may be shown—and I shall have in Chapter XX. to consider the point more in detail—that if our liquid satellite had only, say, a thousandth of the mass of the planet, and if the two bodies were brought nearer one another, at a certain calculable distance the tidal action of the big planet on the very small satellite would become so intense that it would tear it to pieces. Accordingly the contact and coalescence of a very small satellite with a large planet is impossible. It is, however, certain that a large enough satellite—say of half the mass of the planet—could be brought up to contact with the planet, without the tidal action of the planet on the satellite becoming too intense to admit of the existence of the latter. There must then be some mass of the satellite, which will just allow the two to touch at the same moment that the tidal action of the larger on the smaller body is on the point of disrupting it. Now I suspect, although I do not know, that the series of figures which we should find in this case is in fact Poincaré's series. This discussion shows that the subject still affords an interesting field for future mathematicians.

These investigations as to the form of rotating masses of liquid are of a very abstract character, and seem at first sight remote from practical conclusions, yet they have some very interesting applications.

The planetary body of Maclaurin is flattened at the poles like the actual planets, and the degree of its flattening is exactly appropriate to the rapidity of its rotation. Although the planets are, at least in large part, composed of solid matter, yet that matter is now, or was once, sufficiently plastic to permit it to yield to the enormous forces called into play by rotation and gravitation. Hence it follows that the theory of Maclaurin's figure is the foundation of that of the figures of planets, and of the variation of gravity at the various parts of their surfaces. In the liquid considered hitherto, every particle attracted every other particle, the fluid was equally dense throughout, and the figure assumed was the resultant of the battle between the centrifugal force and gravitation. At every part of the liquid the resultant attraction was directed nearly, but not quite, towards the centre of the shape. But if the attraction had everywhere been directed exactly to the centre, the degree of flattening would have been diminished. We may see that this must be so, because if the rotation were annulled, the mass would be exactly spherical, and if the rotation were not annulled, yet the forces would be such as to make the fluid pack closer, and so assume a more nearly spherical form than when the forces were not absolutely directed to the centre. It may be shown in fact that the flattening is $2\frac{1}{2}$ times greater in the case of Maclaurin's body than it is when the seat of gravitation is exactly central.

In the case of actual planets the denser matter must lie in the centre and the less dense outside. If the central matter were enormously denser than superficial rock, the attraction would be directed towards the centre. There are then two extreme cases in which the degree of flattening can be determined,—one in which the density of the planet is the same all through, giving Maclaurin's figure; the other when the density is enormously greater at the centre. The flattening in the former is $2\frac{1}{2}$ times as great as in the latter. The actual condition of a real planet must lie between these two extremes. The knowledge of the rate of rotation of a planet and of the degree of its flattening furnishes us with some insight into the law of its internal density. If it is very much less flat than Maclaurin's figure, we conclude that it is very dense in its central portion. In this way it is known with certainty that the central portions of the planets Jupiter and Saturn are much denser, compared with their superficial portions, than is the case with the earth.

I do not propose to pursue this subject into the consideration of the law of the variation of gravity on the surface of a planet; but enough has been said to show that these abstract investigations have most important practical applications.

Authorities.

Plateau, "Mémoires de l'Académie Royale de Belgique," vol. xvi. 1843.

Thomson and Tait's *Natural Philosophy* or other works on hydrodynamics give an account of figures of equilibrium.

Poincaré, *Sur l'équilibre d'une masse fluide animée d'un mouvement de rotation*, "Acta Mathematica," vol. 7, 1885.

An easier and different presentation of the subject is contained in an inaugural dissertation by Schwarzschild (Annals of Munich Observatory, vol. iii. 1896). He considers that Poincaré's proof of the stability of his figure is not absolutely conclusive.

G. H. Darwin, *Figures of Equilibrium of Rotating Masses of Fluid*, "Transactions of Royal Society," vol. 178, 1887.

G. H. Darwin, *Jacobi's Figure of Equilibrium*, &c., "Proceedings Roy. Soc.," vol. 41, 1886, p. 319.

S. Krüger, *Ellipsoidale Evenwichtsvormen*, &c., Leeuwen, Leiden, 1896; *Sur l'ellipsoïde de Jacobi*, "Nieuw Archief voor Wiskunde," 2d series, 3d part, 1898. The author shows that

G. H. Darwin had been forestalled in much of his work on Jacobi's figure, and he corrects certain mistakes.

CHAPTER XIX

THE EVOLUTION OF CELESTIAL SYSTEMS

Men will always aspire to peer into the remote past to the utmost of their power, and the fact that their success or failure cannot appreciably influence their life on the earth will never deter them from such endeavors. From this point of view the investigations explained in the last chapter acquire much interest, since they form the basis of the theories of cosmogony which seem most probable by the light of our present knowledge.

We have seen that an annular figure of equilibrium actually exists in Plateau's experiment, and it is almost certainly a possible form amongst celestial bodies. Plateau's ring has however only a transient existence, and tends to break up into globules, spinning on their axes and revolving round the centre. In this result we saw a close analogy with the origin of the planets, and regarded his experiment as confirmatory of the Nebular Hypothesis, of which I shall now give a short account.[1]

The first germs of this theory are to be found in

[1] My knowledge of the history of the Nebular Hypothesis is entirely derived from an interesting paper by Mr. G. F. Becker, on "Kant as a Natural Philosopher," *American Journal of Science*, vol. v. Feb. 1898.

Descartes' "Principles of Philosophy," published in 1644. According to him the sun and planets were represented by eddies or vortices in a primitive chaos of matter, which afterwards formed the centres for the accretion of matter. As the theory of universal gravitation was propounded for the first time half a century later than the date of Descartes' book, it does not seem worth while to follow his speculations further. Swedenborg formulated another vortical cosmogony in 1734, and Thomas Wright of Durham published in 1750 a book of preternatural dullness on the same subject. It might not have been worth while to mention Wright, but that Kant acknowledges his obligation to him.

The Nebular Hypothesis has been commonly associated with the name of Laplace, and he undoubtedly avoided certain errors into which his precursors had fallen. I shall therefore explain Laplace's theory, and afterwards show how he was, in most respects, really forestalled by the great German philosopher Kant.

Laplace supposed that the matter now forming the solar system once existed in the form of a lens-shaped nebula of highly rarefied gas, that it rotated slowly about an axis perpendicular to the present orbits of the planets, and that the nebula extended beyond the present orbit of the furthest planet. The gas was at first expanded by heat, and as the surface cooled the central portion condensed and its temperature rose. The speed of rotation increased in consequence of the contraction, according to a well known law of mechanics called "the conservation of moment of momentum;"[1] the edges of the lenticular mass of gas then ceased to be continuous with the more central portion, and a ring of matter was detached, in much the same way as in Plateau's experiment. Further cooling led to further contraction and consequently to increased rotation, until a second ring was shed, and so on successively. The rings then ruptured and aggregated themselves into planets whilst the central nucleus formed the sun.

Virtually the same theory had been propounded by Kant many years previously, but I am not aware that there is any reason to suppose that Laplace had ever read Kant's works. In a paper, to which I have referred above, Mr. G. F. Becker makes the following excellent summary of the relative merits of Kant and Laplace; he writes:—

"Kant seems to have anticipated Laplace almost completely in the more essential portions of the nebular hypothesis. The great Frenchman was a child when Kant's theory was issued, and the 'Système du Monde,' which closes with the nebular hypothesis, did not appear until 1796. Laplace, like Kant, infers unity of origin for the members of the so-

[1] Kant fell into error through ignorance of the generality of this law, for he imagined that rotation could be generated from rest.

lar system from the similarity of their movements, the small obliquity and small eccentricity of the orbits of either planets or satellites.[1] Only a fluid extending throughout the solar system could have produced such a result. He is led to conclude that the atmosphere of the sun, in virtue of excessive heat, originally extended beyond the solar system and gradually shrank to its present limits. This nebula was endowed with moment of momentum which Kant tried to develop by collisions. Planets formed from zones of vapor, which on breaking agglomerated.... The main points of comparison between Kant and Laplace seem to be these. Kant begins with a cold, stationary nebula which, however, becomes hot by compression and at its first regenesis would be in a state of rotation. It is with a hot, rotating nebula that Laplace starts, without any attempt to account for the heat. Kant supposes annular zones of freely revolving nebulous matter to gather together by attraction during condensation of the nebula. Laplace supposes rings left behind by the cooling of the nebula to agglomerate in the same way as Kant had done. While both appeal to the rings of Saturn as an example of the hypothesis, neither explains satisfactorily why the planetary rings are not as stable as those of Saturn. Both

[1] "The retrograde satellites of Uranus were discovered by Herschel in 1787, but Laplace in his hypothesis does not refer to them."

assert that the positive rotation of the planets is a necessary consequence of agglomeration, but neither is sufficiently explicit. The genesis of satellites is for each of them a repetition on a small scale of the formation of the system.... While Laplace assigns no cause for the heat which he ascribes to his nebula, Lord Kelvin goes further back and supposes a cold nebula consisting of separate atoms or of meteoric stones, initially possessed of a resultant moment of momentum equal or superior to that of the solar system. Collision at the centre will reduce them to a vapor which then expanding far beyond Neptune's orbit will give a nebula such as Laplace postulates.[1] Thus Kelvin goes back to the same initial condition as Kant, excepting that Kant endeavored (of course vainly) to develop a moment of momentum for his system from collisions."[2]

There is good reason for believing that the Nebular Hypothesis presents a true statement in outline of the origin of the solar system, and of the planetary subsystems, because photographs of nebulæ have been taken recently in which we can almost see the process in action. Fig. 40 is a reproduction of a remarkable photograph by Dr. Isaac Roberts of the great nebula in the constellation of Andromeda. In

[1] *Popular Lectures*, vol. i. p. 421.

[2] Becker, *Amer. Journ. Science*, vol. v. 1898, pp. 107, 108.

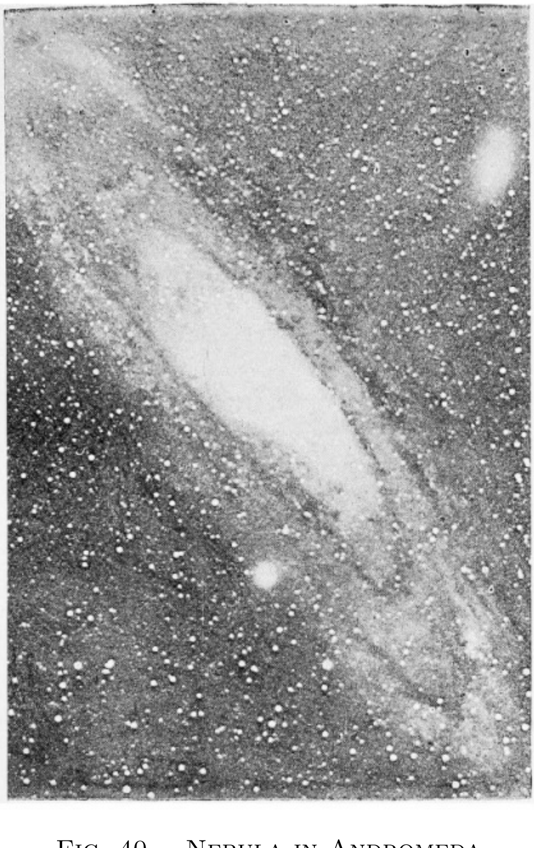

Fig. 40.—Nebula in Andromeda

it we may see the lenticular nebula with its central condensation, the annulation of the outer portions, and even the condensations in the rings which will doubtless at some time form planets. This system is built on a colossal scale, compared with which our solar system is utterly insignificant. Other nebulæ show the same thing, and although they are less striking we derive from them good grounds for accepting this theory of evolution as substantially true.

I explained in Chapter XVI. how the theory of tidal friction showed that the moon took her origin very near to the present surface of the earth. But it was also pointed out that the same theory cannot be invoked to explain an origin for the planets at a point close to the sun. They must in fact have always moved at nearly their present distances. In the same way the dimensions of the orbits of the satellites of Mars, Jupiter, Saturn, and Neptune cannot have been largely augmented, whatever other effects tidal friction may have had. We must therefore still rely on the Nebular Hypothesis for the explanation of the main features of the system as a whole.

It may, at first sight, appear illogical to maintain that an action, predominant in its influence on our satellite, should have been insignificant in regulating the orbits of all the other bodies of the system. But this is not so, for whilst the earth is only 80 times as heavy as the moon, Saturn weighs about 4,600 times as much as its satellite Titan, which is by far the largest satellite in the solar system; and all the other satellites are almost infinitesimal in comparison with their primaries. Since, then, the relationship of the moon to the earth is unique, it may be fairly contended that a factor of evolution, which has been predominant in our own history, has been relatively insignificant elsewhere.

There is indeed a reason explanatory of this singularity in the moon and earth; it lies in the fact that the earth is nearer to the sun than any other planet attended by a satellite. To explain the bearing of this fact on the origin of satellites and on their sizes, I must now show how tidal friction has probably operated as a perturbing influence in the sequence of events, which would be normal according to the Nebular Hypothesis.

We have seen that rings should be shed from the central nucleus, when the contraction of the nebula has induced a certain degree of augmentation of rotation. Now if the rotation were retarded by some external cause, the genesis of a ring would be retarded, or might be entirely prevented.

The friction of the solar tides in a planetary nebula furnishes such an external cause, and accordingly the rotation of a planetary nebula near to the sun might be so much retarded that a ring would never be detached from it, and no satellite would be generated. From this point of view it is noteworthy that Mercury and Venus have no satellites; that Mars has two, Jupiter five, and that all the exterior planets have several satellites. I suggest then that the solar tidal friction of the terrestrial nebula was sufficient to retard the birth of a satellite, but not to prevent it, and that the planetary mass had contracted to nearly the present dimensions of the earth and had partially condensed into the solid and liquid forms, before the rotation had augmented sufficiently to permit the birth of a satellite. When satellites arise under conditions which are widely different, it is reasonable to suppose that their masses will also differ much. Hence we can understand how it has come about that the relationship between the moon and the earth is so unlike that between other satellites and their planets. In Chapter XVII. I showed that there are reasons for believing that solar tidal friction has really been an efficient cause of change, and this makes it legitimate to invoke its aid in explaining the birth and distribution of satellites.

In speaking of the origin of the moon I have been careful not to imply that the matter of which she is formed was necessarily first arranged in the form of a ring. Indeed, the genesis of the hour-glass figure of equilibrium from Jacobi's form and its fission into two parts indicate the possibility of an entirely different sequence of events. It may perhaps be conjectured that the moon was detached from the primitive earth in this way, possibly with the help of tidal oscillations due to the solar action. Even if this suggestion is only a guess, it is interesting to make such speculations, when they have some basis of reason.

In recent years astronomers have been trying, principally by aid of the spectroscope, to determine the orbits of pairs of double stars around one another. It has been observed that, in the majority of these systems, the masses of the two component stars do not differ from one another extremely; and Dr. See, who has specially devoted himself to this research, has drawn attention to the great contrast between these systems and that of the sun, attended by a retinue of infinitesimal planets. He maintains, with justice, that the paths of evolution pursued in the two cases have probably also been strikingly different.

It is hardly credible that two stars should have gained their present companionship by an accidental approach from infinite space. They cannot always have moved as they do now, and so we are driven to reflect on the changes which might supervene in such a system under the action of known forces.

The only efficient interaction between a pair of celestial bodies, which is known hitherto, is a tidal one, and the friction of the oscillations introduces a cause of change in the system. Tidal friction tends to increase the eccentricity of the orbit in which two bodies revolve about one another, and its efficiency is much increased when the pair are not very unequal in mass and when each is perturbed by the tides due to the other. The fact that the orbits of the majority of the known pairs are very eccentric affords a reason for accepting the tidal explanation. The only adverse reason, that I know of, is that the eccentricities are frequently so great that we may perhaps be putting too severe a strain on the supposed cause.

But the principal effect of tidal friction is the repulsion of the two bodies from one another, so that when their history is traced backwards we ultimately find them close together. If then this cause has been as potent as Dr. See believes it to have been, the two components of a binary system must once have been close together. From this stage it is but a step to picture to ourselves the rupture of a nebula, in the form of an hour-glass, into two detached masses.

The theory embraces all the facts of the case, and as such is worthy of at least a provisional acceptance. But we must not disguise from ourselves that out of the thousands, and perhaps millions of double stars which may be visible from the earth, we only as yet know the orbits and masses of a dozen.

Many years ago Sir John Herschel drew a number of twin nebulæ as they appear through a powerful telescope. The drawings probably possess the highest degree of accuracy attainable by this method of delineation, and the shapes present evidence confirmatory of the theory of the fission of nebulæ adopted by Dr. See. But since Herschel's time it has been discovered that many details, to which our eyes must remain forever blind, are revealed by celestial photography. The photographic film is, in fact, sensitive to those "actinic" rays which we may call invisible light, and many nebulæ are now found to be hardly recognizable, when photographs of them are compared with drawings. A conspicuous example of this is furnished by the great nebula in Andromeda, illustrated above in fig. 40.

Photographs, however, do not always aid interpretation, for there are some which serve only to increase the chaos visible with the telescope. We may suspect, indeed, that the complete system of a nebula often contains masses of cold and photographically invisible gas, and in such cases it would seem that the true nature of the whole will always be concealed from us.

Another group of strange celestial objects is that of the spiral nebulæ, whose forms irresistibly suggest violent whirlpools of incandescent gas. Although in all probability the motion of the gas is very rapid, yet no change of form has been detected. We are here reminded of a rapid stream rushing past a post, where the form of the surface remains constant whilst the water itself is in rapid movement; and it seems reasonable to suppose that in these nebulæ it is only the lines of the flow of the gas which are visible. Again, there are other cases in which the telescopic view may be almost deceptive in its physical suggestions. Thus the Dumb-Bell nebula (27 Messier Vulpeculæ), as seen telescopically, might be taken as a good illustration of a nebula almost ready to split into two stars. If this were so, the rotation would be about an axis at right angles to the length of the nebula. But a photograph of this object shows that the system really consists of a luminous globe surrounded by a thick and less luminous ring, and that the opacity of the sides of the ring takes a bite, as it were, out of each side of the disk, and so gives it the apparent form of a dumb-bell. In this case the rotation must be about an axis at right angles to the ring, and therefore along the length of the dumb-bell. It is proper to add that Dr. See is well aware of this, and does not refer to this nebula as a case of incipient fission.

I have made these remarks in order to show that every theory of stellar evolution must be full of difficulty and uncertainty. According to our present knowledge Dr. See's theory appears to have much in its favor, but we must await its confirmation or refutation from the results of future researches with the photographic plate, the spectroscope, and the telescope.

Authorities.

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CHAPTER XX

SATURN'S RINGS[1]

TO the naked eye Saturn appears as a brilliant star, which shines, without twinkling, with a yellowish light. It is always to be found very nearly in the ecliptic, moving slowly amongst the fixed stars at the rate of only thirteen degrees per annum. It is the second largest planet of the solar system, being only exceeded in size by the giant Jupiter. It weighs 91 times as much as our earth, but, being as light as cork, occupies 690 times the volume, and is nine times as great in circumference. Notwithstanding its great size it rotates around its axis far more rapidly than does the earth, its day being only $10\frac{1}{2}$ of our hours. It is ten times as far from the sun as we are, and its year, or time of revolution round the sun, is equal to thirty of our years. It was deemed by the early astronomers to be the planet furthest from the sun, but that was before the discovery by Herschel, at the end of the last century, of the further planet Uranus, and that of the still more distant Neptune by Adams and Leverrier in the year 1846.

The telescope has shown that Saturn is attended by a

[1]Part of this chapter appeared as an article in *Harper's Magazine* for June, 1889.

retinue of satellites almost as numerous as, and closely analogous to, the planets circling round the sun. These moons are eight in number, are of the most various sizes, the largest as great as the planet Mars, and the smallest very small, and are equally diverse in respect of their distances from the planet. But besides its eight moons Saturn has another attendant absolutely unique in the heavens; it is girdled with a flat ring, which, like the planet itself, is only rendered visible to us by the illumination of sunlight. Fig. 41, to which further reference is made below, shows the general appearance of the planet and of its ring. The theory of the physical constitution of that ring forms the subject of the present chapter.

A system so rich in details, so diversified and so extraordinary, would afford, and doubtless has afforded, the subject for many descriptive essays; but description is not my present object.

The existence of the ring of Saturn seems now a very commonplace piece of knowledge, and yet it is not 300 years since the moons of Jupiter and Saturn were first detected, and since suspicion was first aroused that there was something altogether peculiar about the Saturnian system. These discoveries, indeed, depended entirely on the invention of the telescope. It may assist the reader to realize how necessary the aid of that instrument was when I say that Saturn, when

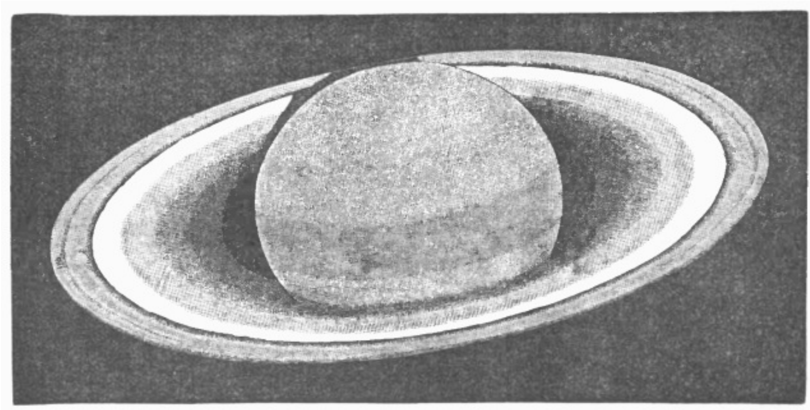

FIG. 41.—THE PLANET SATURN

at his nearest to us, is the same in size as a sixpenny piece held up at a distance of 210 yards.

It was the celebrated Galileo who first invented a combination of lenses such as is still used in our present opera-glasses, for the purpose of magnifying distant objects.

In July of 1610 he began to examine Saturn with his telescope. His most powerful instrument only magnified 32 times, and although such an enlargement should have amply sufficed to enable him to make out the ring, yet he persuaded himself that what he saw was a large bright disk, with two smaller ones touching it, one on each side. His lenses were doubtless imperfect, but the principal cause of his error must have been the extreme improbability of the existence of a ring girdling the planet. He wrote an account of what he had seen to the Grand Duke of Tuscany, Giuliano de' Medici, and to others; he also published to the world an anagram which, when the letters were properly arranged, read as follows: "Altissimum planetam tergeminum observavi" (I have seen the furthest planet as triple), for it must be remembered that Saturn was then the furthest known planet.

In 1612 Galileo again examined Saturn, and was utterly perplexed and discouraged to find his triple star replaced by a single disk. He writes, "Is it possible that some mocking demon has deceived me?" And here it may be well to remark that there are several positions in which Saturn's rings vanish from sight, or so nearly vanish as to be only visible with the most powerful modern telescopes. When the plane of the ring passes through the sun, only its very thin edge is illuminated; this was the case in 1612, when Galileo lost it; secondly, if the plane of the ring passes through the earth, we have only a very thin edge to look at; and thirdly, when the sun and the earth are on opposite sides of the ring, the face of the ring which is presented to us is in shadow, and therefore invisible.

Some time afterwards Galileo's perplexity was increased by seeing that the planet had then a pair of arms, but he never succeeded in unraveling the mystery, and blindness closed his career as an astronomer in 1626.

About thirty years after this, the great Dutch astronomer Huyghens, having invented a new sort of telescope (on the principle of our present powerful refractors), began to examine the planet and saw that it was furnished with two loops or handles. Soon after the ring disappeared; but when, in 1659, it came into view again, he at last recognized its true character, and announced that the planet was attended by a broad, flat ring.

A few years later it was perceived that there were two rings, concentric with one another. The division, which may be easily seen in drawings of the planet, is still named after Cassini, one of its discoverers. Subsequent observers have detected other less marked divisions.

Nearly two centuries later, namely, in 1850, Bond in America and Dawes in England, independently and within a fortnight of the same time, observed that inside of the well-known bright rings there is another very faint dark ring, which is so transparent that the edge of the planet is visible through it. There is some reason to believe that this ring has really become more conspicuous within the last 200 years, so that it would not be right to attribute the lateness of its detection entirely to the imperfection of earlier observations.

It was already discovered in the last century that the ring is not quite of the same thickness at all points of its circumference, that it is not strictly concentric with the planet, and that it revolves round its centre. Herschel, with his magnificent reflecting telescope, detected little beads on the outer ring, and by watching these he concluded that the ring completes its revolution in $10\frac{1}{2}$ hours.

This sketch of the discovery and observation of Saturn's rings has been necessarily very incomplete, but we have perhaps already occupied too much space with it.

Fig. 41 exhibits the appearance of Saturn and his ring. The drawing is by Bond of Harvard University, and is considered an excellent one.

It is usual to represent the planets as they are seen through an astronomical telescope, that is to say, reversed. Thus in fig. 41 the south pole of the planet is at the top of the plate, and unless the telescope were being driven by clockwork, the planet would appear to move across the field of view from right to left.

The plane of the ring is coincident with the equator of the planet, and both ring and equator are inclined to the plane of the planet's orbit at an angle of 27 degrees.

A whole essay might be devoted to the discussion of this and of other pictures, but we must confine ourselves to drawing attention to the well-marked split, called Cassini's divi-

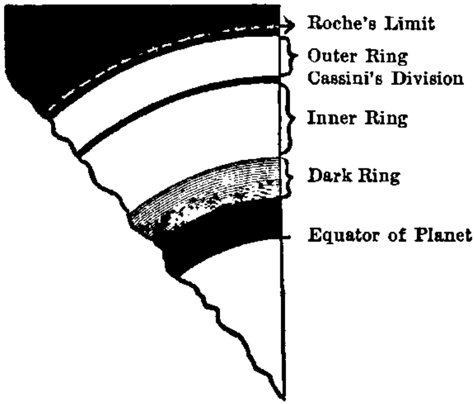

Fig. 42.—Diagram of Saturn and his Rings

sion, and to the faint internal ring, through which the edge of the planet is visible.

The scale on which the whole system is constructed is best seen in a diagram of concentric circles, showing the limits of the planet's body and of the successive rings. Such a diagram, with explanatory notes, is given in fig. 42.

An explanation of the outermost circle, called *Roche's limit*, will be given later. The following are the dimensions of the system:—

Equatorial diameter of planet	73,000	miles
Interior diameter of dark ring	93,000	“
Interior diameter of bright rings	111,000	“
Exterior diameter of bright rings	169,000	“

We may also remark that the radius of the limit of the rings is 2.38 times the mean radius of the planet, whilst Roche's limit is 2.44 such radii. The greatest thickness of the ring is uncertain, but it seems probable that it does not exceed 200 or 300 miles.

The pictorial interest, as we may call it, of all this wonderful combination is obvious, but our curiosity is further stimulated when we reflect on the difficulty of reconciling the existence of this strange satellite with what we know of our own planet and of other celestial bodies.

It may be admitted that no disturbance to our ordinary way of life would take place if Saturn's rings were annihilated, but, as Clerk-Maxwell has remarked, “from a purely scientific point of view, they become the most remarkable bodies in the heavens, except, perhaps, those still less *useful* bodies—the spiral nebulæ. When we have actually seen that great arch swung over the equator of the planet without any visible connection, we cannot bring our minds to rest. We cannot simply admit that such is the case, and describe it as one of the observed facts of nature, not admitting or requiring explanation. We must either explain its motion on the principles of mechanics, or admit that, in Saturnian realms, there can be motion regulated by laws which we are unable to explain."

I must now revert to the subject of Chapter XVIII. and show how the investigations, there explained, bear on the system of the planet. We then imagined a liquid satellite revolving in a circular orbit about a liquid planet, and supposed that each of these two masses moved so as always to present the same face to the other. It was pointed out that each body must be somewhat flattened by its rotation round an axis at right angles to the plane of the orbit, and that the tidal attraction of each must deform the other. In the application of this theory to the system of Saturn it is not necessary to consider further the tidal action of the satellite on the planet, and we must concentrate our attention on the action of the planet on the satellite. We have found reason to suppose that the earth once raised enormous tides in the moon, when her body was molten, and any planet must act in the same way on its satellite. When, as we now suppose, the satellite moves so as always to present the same face to the planet, the tide is fixed and degenerates into a permanent distortion of the equator of the satellite into an elliptic shape. If the satellite is very small compared with its planet, and if it is gradually brought closer and closer to the planet, the tide-generating force, which varies inversely as the cube of the distance, increases with great rapidity, and we shall find the satellite to assume a more and more elongated shape. When the satellite is not excessively small, the two bodies may be brought together until they actually touch, and form the hour-glass figure exhibited in fig. 39, p. 331.

The general question of the limiting proximity of a liquid planet and satellite which just insures stability is as yet unsolved. But it has been proved that there is one case in which instability sets in. Édouard Roche has shown that this approach up to contact is not possible when the satellite is very small, for at a certain distance the tidal distortion of a small satellite becomes so extreme that it can no longer subsist as a single mass of fluid. He also calculated the form of the satellite when it is elongated as much as possible. Fig. 43 represents the satellite in its limiting form. We must suppose the planet about which it revolves to be a large globe, with its centre lying on the prolongation of the longest axis of the egg-like body in the direction of E. As it revolves, the longest axis of the satellite always points straight towards its planet. The egg, though not strictly circular in girth, is very nearly so. Thus another section at right angles to this one would be of nearly the same shape. One diameter of the girth is in fact only longer than the other by a seventeenth part. The shortest of the three axes of the slightly flattened egg is at right angles to the plane of the orbit in

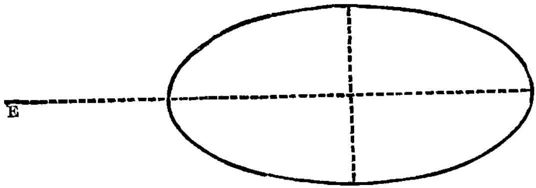

Fig. 43.—Roche's Figure of a Satellite when elongated to the utmost

which the satellite revolves. The longest axis of the body is nearly twice as long as either of the two shorter ones; for if we take the longest as 1,000, the other two would be 496 and 469. Fig. 43 represents a section through the two axes equal respectively to 1,000 and to 469, so that we are here supposed to be looking at the satellite's orbit edgewise.

But, as I have said, Roche determined not only the shape of the satellite when thus elongated to the utmost possible extent, but also in its nearness to the planet, and he proved that if the planet and satellite be formed of matter of the same density, the centre of such a satellite must be at a distance from the planet's centre of $2\frac{11}{25}$ of the planet's radius. This distance of $2\frac{11}{25}$ or 2.44 of a planet's radius I call Roche's limit for that planet. The meaning of this is that inside of a circle drawn around a planet at a distance so proportionate to its radius no small satellite can circulate; the reason being that if a lump of matter were started to revolve about the planet inside of that circle, it would be torn to pieces under the action of the forces we have been considering. It is true that if the lump of matter were so small as to be more properly described as a stone than as a satellite, then the cohesive force of stone might be strong enough to resist the disruptive force. But the size for which cohesion is sufficient to hold a mass of matter together is small compared with the smallest satellite.

I have said that Roche's limit as evaluated at 2.44 radii is dependent on the assumption of equal densities in the satellite and planet. If the planet be denser than the satellite, Roche's limit is a larger multiple of the planet's radius, and if it be less dense the multiple is smaller. But the variation of distance is not great for considerable variations in the relative densities of the two bodies, the law being that the 2.44 must be multiplied by the cube root of the ratio of the density of the planet to that of the satellite. If for example the planet be on the average of its whole volume twice as dense as the satellite, the limit is only augmented from 2.44 to 3 times the planet's radius; and if it be half as dense, the 2.44 is depressed to 1.94. Thus the variation of density of the planet from a half to twice that of the planet—that is to say, the multiplication of the smaller density by four—only changes Roche's limit from 2 to 3 radii. It follows from this that, within pretty wide limits of variation of relative densities, Roche's limit changes but little.

The only relative density of planet and satellite that we know with accuracy is that of the earth and moon. Now the earth is more dense than the moon in the proportion of 8 to 5; hence Roche's limit for the earth is the cube root of $\frac{8}{5}$ multiplied by 2.44, that is to say, it is 2.86 times the earth's radius. It follows that if the moon were to revolve at a distance of less than 2.86 radii, or 11,000 miles, she would be torn to pieces by the earth's tidal force.

If this result be compared with the conclusions drawn from the theory of tidal friction, it follows that at the earliest stage to which the moon was traced, she could not have existed in her present form, but the matter which is now consolidated in the form of a satellite must then have been a mere swarm of loose fragments. Such fragments, if concentrated in one part of the orbit, would be nearly as efficient in generating tides in the planet as though they were agglomerated in the form of a satellite. Accordingly the action of tidal friction does not necessitate the agglomeration of the satellite. The origin and earliest history of the moon must always remain highly speculative, and it seems fruitless to formulate exact theories on the subject.[1]

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[1] Mr. Nolan has criticised the theory of tidal friction from this point

When we apply this reasoning to the other planets, exact data are wanting. The planet Mars resembles the earth in so many respects that it is reasonable to suppose that there is much the same relationship between the densities of the planet and satellites as with us. As with the case of the earth and moon, this would bring Roche's limit to 2.86 times the planet's radius. The satellite Phobos, however, revolves at a distance of 2.75 radii of Mars; hence we are bound to suppose that the density of Phobos is a very little more nearly equal to that of Mars than in the case of the moon and earth; if it were not so, Phobos would be disrupted by tidal action. How interesting it will be if future generations shall cease to see the satellite Phobos, for they will then conclude that Phobos has been drawn within the charmed circle, and has been broken to pieces.

In considering the planets Jupiter and Saturn, we are deprived of the indications which are useful in the case of Mars. The satellites are probably solid, and these planets are known to have a low mean density. Hence it is probable that Roche's limit is a somewhat smaller multiple than 2.44 of the radii of Jupiter and Saturn. The only satellite which is in danger is the innermost and recently discovered satellite

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of view (*Genesis of the Moon*, Melbourne, 1885; also *Nature*, Feb. 18 and July 29, 1886).

of Jupiter, which revolves at 2.6 times the planet's mean radius, for with the same ratio of densities as obtains here the satellite would be broken up. This confirms the conclusion that the mean density of Jupiter is at least not greater than that of the satellite.

We are also ignorant of the relative densities of Saturn and its satellites, and so in the figure Roche's limit is placed at 2.44 times the planet's radius, corresponding to equal densities. But the density of the planet is very small, and therefore the limit is almost certainly slightly nearer to the planet than is shown.

This system affords the only known instance where matter is clearly visible circulating round an attractive centre at a distance certainly less than the theoretical limit, and the belief seems justified that Saturn's rings consist of dust and fragments.

Although Roche himself dismissed this matter in one or two sentences, he saw the full bearing of his remarks, and to do him justice we should date from 1848 the proof that Saturn's rings consist of meteoric stones.

The theoretical limit lies just outside the limit of the rings, but we may suspect that the relative densities of the planet and satellite are such that the limit should be displaced to a distance just inside of the outer edge of the ring, because any solid satellite would almost necessarily have a mean density greater than that of the planet.

Although Roche's paper was published about fifty years ago, it has only recently been mentioned in text-books and general treatises. Indeed, it has been stated that Bond was the first in modern times to suggest the meteoric constitution of the rings. His suggestion, based on telescopic evidence, was however made in 1851.

And now to explain how a Cambridge mathematician to whom reference was made above, in ignorance of Roche's work of nine years before, arrived at the same conclusion. In 1857, Clerk-Maxwell, one of the most brilliant men of science who have taught in the University of Cambridge, and whose early death we still deplore, attacked the problem of Saturn's rings in a celebrated essay, which gained for him what is called the Adams prize. Laplace had early in the century considered the theory that the ring is solid, and Maxwell first took up the question of the motion of such a solid ring at the point where it had been left. He determined what amount of weighting at one point of a solid uniform ring is necessary to insure its steady motion round the planet. He found that there must be a mass attached to the circumference of the ring weighing $4\frac{1}{2}$ times as much as the ring itself. In fact, the system becomes a satellite with a light ring attached to it.

"As there is no appearance," he says, "about the rings justifying a belief in so great an irregularity, the theory of the solidity of the rings becomes very improbable. When we come to consider the additional difficulty of the tendency of the fluid or loose parts of the ring to accumulate at the thicker parts, and thus to destroy that nice adjustment of the load on which the stability depends, we have another powerful argument against solidity. And when we consider the immense size of the rings and their comparative thinness, the absurdity of treating them as rigid bodies becomes self-evident. An iron ring of such a size would be not only plastic, but semi-fluid, under the forces which it would experience, and we have no reason to believe these rings to be artificially strengthened with any material unknown on this earth."

The hypothesis of solidity being condemned, Maxwell proceeds to suppose that the ring is composed of a number of equal small satellites. This is a step towards the hypothesis of an indefinite number of meteorites of all sizes. The consideration of the motion of these equal satellites affords a problem of immense difficulty, for each satellite is attracted by all the others and by the planet, and they are all in motion.

If they were arranged in a circle round the planet at equal distances, they might continue to revolve round the planet, provided that each satellite remained in its place with mathematical exactness. Let us consider that the proper place of each satellite is at the ends of the spokes of a revolving wheel, and then let us suppose that none of them is exactly in its place, some being a little too far advanced, some a little behind, some too near and some too far from the centre of the wheel—that is to say, from the planet—then we want to know whether they will swing to and fro in the neighborhood of their places, or will get further and further from their places, and whether the ring will end in confusion.

Maxwell treated this problem with consummate skill, and showed that if the satellites were not too large, confusion would not ensue, but each satellite would oscillate about its proper place.

At any moment there are places where the satellites are crowded and others where they are spaced out, and he showed that the places of crowding and of spacing out will travel round the ring at a different speed from that with which the ring as a whole revolves. In other words, waves of condensation and of rarefaction are propagated round the ring as it rotates.

He constructed a model, now in the laboratory at Cambridge, to exhibit these movements; it is pretty to observe the changes of the shape of the ring and of the crowding of the model satellites as they revolve.

I cannot sum up the general conclusions at which

Maxwell arrived better than by quoting his own words.

In the summary of his paper he says:—

"If the satellites are unequal, the propagation of waves will no longer be regular, but the disturbances of the ring will in this, as in the former case, produce only waves, and not growing confusion. Supposing the ring to consist, not of a single row of large-satellites, but of a cloud of evenly distributed unconnected particles, we found that such a cloud must have a very small density in order to be permanent, and that this is inconsistent with its outer and inner parts moving with the same angular velocity. Supposing the ring to be fluid and continuous, we found that it will necessarily be broken up into small portions.

"We conclude, therefore, that the rings must consist of disconnected particles; these may be either solid or liquid, but they must be independent. The entire system of rings must therefore consist either of a series of many concentric rings, each moving with its own velocity, and having its own system of waves, or else of a confused multitude of revolving particles, not arranged in rings, and continually coming into collision with each other.

"Taking the first case, we found that in an indefinite number of possible cases the mutual perturbation of two rings, stable in themselves, might mount up in time to a destructive magnitude, and that such cases must continually occur in an extensive system like that of Saturn, the only retarding cause being the possible irregularity of the rings.

"The result of long-continued disturbance was found to be the spreading out of the rings in breadth, the outer rings pressing outward, while the inner rings press inward.

"The final result, therefore, of the mechanical theory is, that the only system of rings which can exist is one composed of an indefinite number of unconnected particles, revolving round the planet with different velocities according to their respective distances. These particles may be arranged in a series of narrow rings, or they may move through each other irregularly. In the first case the destruction of the system will be very slow, in the second case it will be more rapid, but there may be a tendency towards an arrangement in narrow rings, which may retard the process.

"We are not able to ascertain by observation the constitution of the two outer divisions of the system of rings, but the inner ring is certainly transparent, for the limb (i. e. edge) of Saturn has been observed through it. It is also certain, that though the space occupied by the ring is transparent, it is not through the material particles of it that Saturn was seen, for his limb was observed without distortion; which shows that there was no refraction, and therefore that the rays did not pass through a medium at all, but between the solid or liquid particles of which the ring is composed. Here then we have an optical argument in favor of the theory of independent particles as the material of the rings. The two outer rings may be of the same nature, but not so exceedingly rare that a ray of light can pass through their whole thickness without encountering one of the particles."

The last link in the chain of evidence has been furnished by recent observations made in America. If it can be proved that every part of the apparently solid ring moves round the planet's centre at a different rate, and that the speed at each part is appropriate at its distance from the centre, the conclusion is inevitable that the ring consists of scattered fragments.

Every one must have noticed that when a train passes at full speed with the whistle blowing, there is an abrupt fall in the pitch of the note. This change of note is only apparent to the stationary listener, and is caused by the crowding together of the waves of sound as the train approaches, and by their spacing out as it recedes. The same thing is true of light-waves, and if we could imagine a colored light to pass us at an almost inconceivable velocity it would change in tint as it passed.[1] Now there are certain lines in the spectrum of

[1]This statement is strictly correct only of monochromatic light. I might, in the subsequent argument, have introduced the limitation that the moving body shall emit only monochromatic light. The qualifica-

sunlight, and the shifting of their positions affords an excessively delicate measure of a change which, when magnified enormously, would produce a change of tint. For example, the sun is a rotating body, and when we look at its disk one edge is approaching us and the other is receding. The two edges are infinitesimally of different colors, and the change of tint is measurable by the displacement of the lines I have mentioned. In the same way Saturn's ring is illuminated by sunlight, and if different portions are moving at different velocities, those portions are infinitesimally of different colors. Now Professor Keeler, the present director of the Lick Observatory, has actually observed the reflected sunlight from the several parts of Saturn's ring, and he finds that the lines in the spectrum of the several parts are differently displaced. From measurement of these displacements he has concluded that every part of the ring moves at the same pace as if it were an independent satellite. The proof of the meteoric constitution of the ring is therefore complete.

It would be hard to find in science a more beautiful instance of arguments of the most diverse natures concentrating themselves on a definite and final conclusion.

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tion would, however, only complicate the statement, and thus render the displacement of the lines of the spectrum less easily intelligible.

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