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THE TIDES OF THE ATLANTIC OCEAN

(*George Darwin Lecture, delivered by Professor J. Proudman, F.R.S., on* 1944 *October* 13)

It is appropriate, I think, that I should lecture on a subject connected with the tides of the ocean, for Sir George Darwin himself gave a great deal of his attention to this subject. From about 1882 until the end of his life in 1912, he was generally regarded as the greatest living authority on ocean tides.

I shall be mainly concerned with the discovery of the distribution of the tides over the open Atlantic Ocean, by the application of the principles of dynamics. This will involve a large measure of dynamical explanation of the tides as they have been observed on the coasts of the continents and islands. Owing to the great difficulty of measuring tides in mid-ocean, it is at present only by theoretical methods that we can expect to discover their distribution over the open oceans. A method depending on the tidal analysis of barometric observations has been put forward by E. Barkow (1911) and J. Bartels (1926), but has not been successfully employed, and to me the difficulties seem very great. Up to the present time the leading features of the tides have not been determined in any really convincing way. I propose to give a progress-report on an attempt which I have recently been making.

It was Newton who laid down the fundamental principles on which the tides are to be explained, and thus reduced investigations to problems in mathematics. A great deal of attention has been paid to these problems, some of it by the ablest of mathematicians, including Laplace and Poincaré. Much progress has been made in the determination of the tides in ideal geometrically simple basins, notably in recent years by A. T. Doodson and G. R. Goldsbrough. I shall show that some progress has been made with the actual basin of the Atlantic, but practically none has been made with the basins of the Pacific and Indian Oceans.

A question often asked is: Why are the tides of the Mediterranean so small? The answer is: The tides of the Mediterranean are just about of the magnitude that the simplest dynamical theory indicates. The much more interesting question is: Why are the tides of the Atlantic so large?

In order to introduce a fair amount of precision into the argument, I am going to restrict myself very largely to the principal harmonic constituent of the tides. This is the oscillatory motion with a period of 12 hours 25 minutes which is commonly denoted by M_2. For most places in the Atlantic it may be regarded as the average tide. In this constituent only the generating forces of the Moon are taken into account, and the Moon may be taken to remain in the plane of the Earth's equator and at a fixed distance from the Earth.

From the results of the analysis of observations the amplitude of this constituent has been evaluated at a large number of stations both on the continental coasts and on islands. A very convenient standard for the order of magnitude is provided by the equilibrium-form of the tides. This equilibrium-form gives what the tides would be if the water could immediately take up the position corresponding to the tide-generating forces of the Moon. Now the amplitude of the equilibrium-form of the tides is a function only of latitude and is given by $25 \cos^2 \phi$ cm., ϕ denoting the latitude, the mean value of which along a meridian is 12·5 cm. It is often not realised that the tide-generating forces are so small. Taking the mean amplitude of the equilibrium-form as a standard, there is at Brest a 16-fold magnification, at Boston, Mass., an 11-fold magnification, and at the oceanic islands a magnification of 4-, 3-, and 2-fold. This degree of magnification indicates that there is some form of resonance in the basin of the Atlantic.

The longer curves of Fig. 1 show the distribution of the tidal elevation along the continental coasts, including a line just outside the West Indies. The full lines show

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the Greenwich lunar time of high water and the broken lines the amplitude. The data for this figure have been obtained by taking mean values along stretches of the open coasts, each covering 5° of latitude. We notice that there is a progression northwards

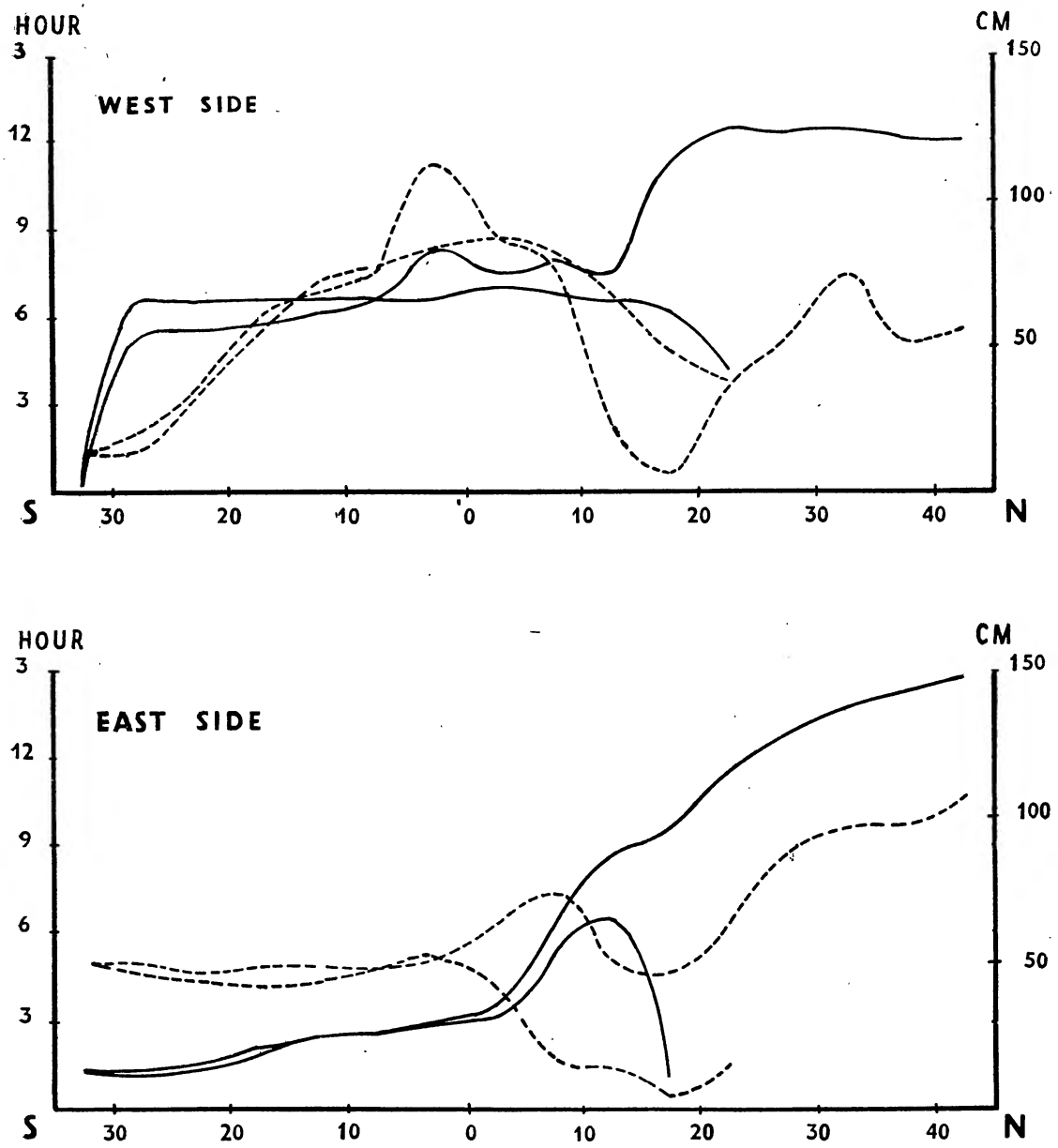


FIG. 1.—*Coastal elevations for M_2 constituent.*
Observation (longer lines) and synthesis (shorter lines).
Full lines give Greenwich lunar hour of high water.
Broken lines give amplitude in cm.

up both coasts of the South Atlantic and along the eastern coast of the North Atlantic. On the west side, at about 32° S. and 17° N. the range is very small and the time changes very rapidly; from about 2° S. to 14° N. and again along the U.S.A. from 22° N. to 42°·5 N. the times change very little, the hours being 7 and 12 respectively. Our problem is to find the theoretical distribution of the tides over the ocean which agrees with these coastal observations, and then we can check the theory by comparing the theoretical distributions with observations on the islands.

I shall now give a short historical review of the attempts that have been made to

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account for the chief features of the semi-diurnal tides of the Atlantic. In 1833, William Whewell published the first *co-tidal charts*, and Fig. 2 shows his chart for the Atlantic.* The idea of a co-tidal chart is that along each line high water occurs at the same time; in Whewell's case the numbers give the solar times of high water on days of New Moon and Full Moon. The reasoning behind this chart is largely non-dynamical. Only the

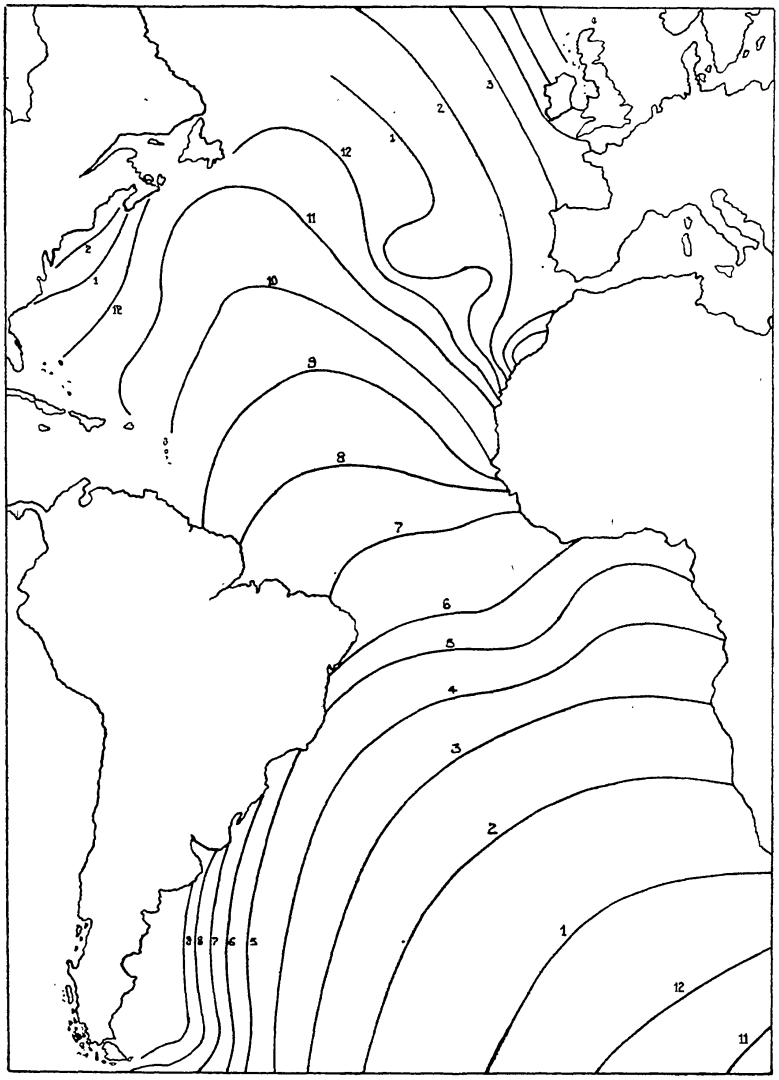


FIG. 2.—*Whewell's chart for the tides on days of New Moon and Full Moon. The numbers give the Greenwich solar hour of high water.*

forward curvature of the co-tidal lines is derived from dynamics, and this derivation is wrong. The lines just connect up the various points on the coasts at which high water has been observed to occur at the times indicated. In order to produce his charts Whewell organised an extensive series of observations. On the west, he got the simultaneity along the coast of the U.S.A., but he did not get the crowdings of co-tidal lines near 32° S. and 17° N. This particular chart led to the hypothesis that the tides of the Atlantic are waves derived from the Southern and Pacific Oceans. In 1842, Sir George Airy stated that he could not accept this hypothesis, and by 1848 Whewell himself had very largely abandoned it; but it lived for a long time in textbooks. Whewell's hypothesis raised the question as to whether the tides of the Atlantic are generated mainly within or mainly without the basin of the Atlantic itself. According to the hypothesis

* W. Whewell, *Phil. Trans. Roy. Soc.*, 1833, 147.

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they would be generated mainly without the basin of the Atlantic. The question requires more precise statement, and when this is done it is seen that the problem of obtaining a complete answer is more difficult than that which I have already formulated. Of course, such a complete answer has not yet been given.

In 1874, William Ferrel calculated, on dynamical principles, what the tides would be in canals along parallels of latitude and stretching from one side of the Atlantic to the other.* For a canal between Ireland and Newfoundland he thought that there might be an approach to the conditions of resonance. He considered that if there were a dyke extending from the Cape of Good Hope to the coast of South America, the tides of the North Atlantic would most probably be very nearly the same as they actually are. In Ferrel's case there were no meridian components of tidal current, but I have considered zonal bands in which there is no lateral constraint. I have taken into account both longitudinal and transverse components of generating force, the variations over each band in the direction of the meridian being determined by the variation of the generating forces. Some of the amplitudes are quite large; larger than those actually observed. This indicates that large tides are generated in the basin of the Atlantic itself. On the coast of the United States high water in these bands occurs at about 6 hours in lunar time. But there is so much discontinuity between the tides in two contiguous bands that it is a very difficult problem to find the connecting links.

In 1900, R. A. Harris introduced a series of hypotheses on the basis of which, in 1904, he constructed charts of co-tidal lines for all the oceans and seas.† These hypotheses are:

1. That the tides are dominated by the forms of simple free oscillations which have the period of 12 hours 25 minutes.
2. That these forms can be determined without paying attention to the absence of lateral constraint.
3. That these free oscillations can be determined without reference to the rotation of the Earth.

For the tides of the Atlantic, Harris used four simple free oscillations. The times of these oscillations are supposed to follow from the times of the various phases of the generating forces of the Moon, but in this I believe Harris was three hours in error. In combining the oscillations of overlapping areas, we see that at the intersection of two nodal lines there is still no rise and fall of the water, and in this way we arrive at an amphidromic point. Also, the openings cause progressive waves. Fig. 3 shows Harris's co-tidal chart, and the times on this do agree with observations. The nodal lines have been replaced by crowdings of co-tidal lines ; in particular there are crowdings of co-tidal lines on the west at 32° S. and 17° N. We notice that there is a progression from east to west across the north of the ocean. Harris's work may be subjected to some serious criticisms:

1. For the South Atlantic he neglects the resonance of transverse oscillations.
2. He gives no adequate consideration of the effect of the water in neighbouring regions.
3. He gives no adequate consideration of the dynamical effects of the Earth's rotation.
4. His work is very largely only qualitative; he gives no absolute amplitudes of tide.
5. His times do not follow from the generating forces.

His chart was not accepted by Sir George Darwin.

In 1910, Poincaré discussed the energy implications of progressive waves. An influx of energy into any region must be balanced by:

1. The dissipation of energy by the friction of tidal currents on the bottom of the ocean and connected seas.

* W. Ferrel, *Tidal Researches*, 237, 1874. † R. A. Harris, *Manual of Tides*, Part IVB, 1904.

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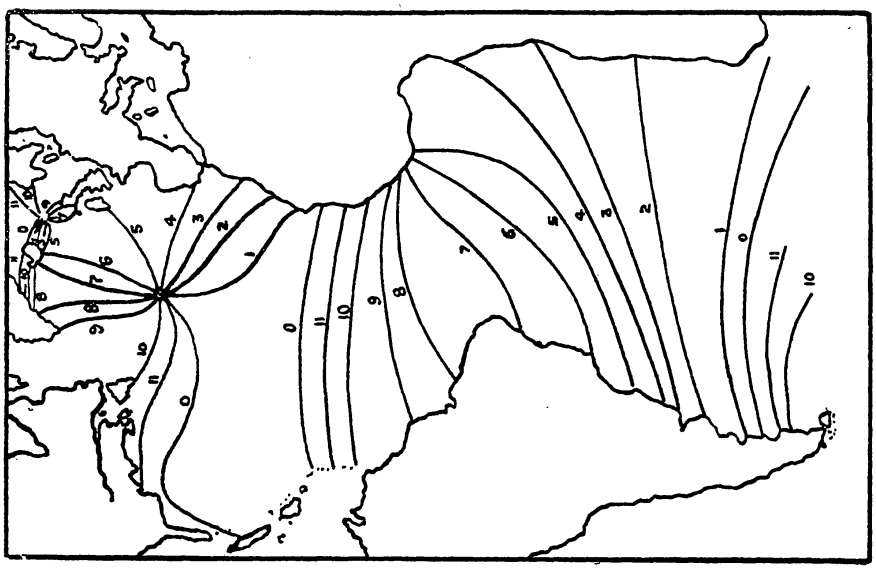


FIG. 5.—*Defant's chart for the semidiurnal tides. The numbers give the Greenwich hour of high water.*

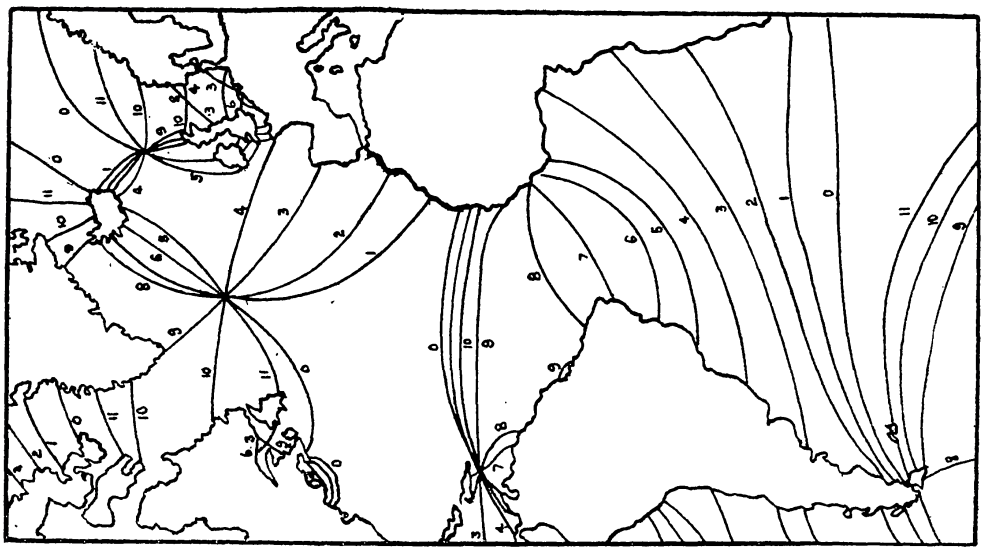


FIG. 4.—*Sterneck's chart for the tides on days of New Moon and Full Moon. The numbers give the Greenwich time of high water in hours which are one-twelfth of the period.*

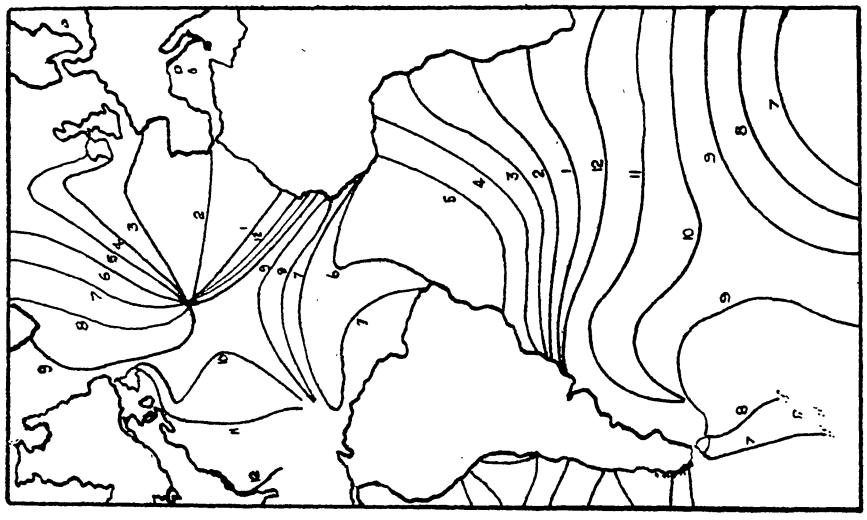


FIG. 3.—*Harris's chart for the M_2 constituent. The numbers give the Greenwich lunar hour of high water.*

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2. The work done against the generating forces.
3. The flow of energy out of the region.

In applying this principle to the Arctic seas, Poincaré assumed No. 2 to be negligible on account of the smallness of the generating forces in the Arctic, and No. 3 to be negligible on account of the smallness of Bering Strait. He assumed that No. 1 is always negligible. He thus deduced that the Arctic tides do not consist of a wave progressing away from the Atlantic. Incidentally, it would also follow that the energy passing into the Atlantic from the Southern Ocean must be given up to the tide-generating forces acting on the water of the Atlantic. But friction is not negligible, as was shown in 1918 by G. I. Taylor; and the progression of co-tidal lines does not necessarily involve a progressing of energy. For the M_2 constituent in the northern hemisphere and for a northerly progression of co-tidal lines, if the range increases towards the east sufficiently rapidly then the current at the time of high water may be directed towards the south, and in this case there will be a mean progression of energy towards the south.

In 1920, H. Jeffreys continued Taylor's work on the dissipation of energy in shallow seas, and in 1923 he showed that this friction must have very important effects on the distribution of the tides. There is another strong argument for the important effects of friction on the distribution of the tides, but for this we have to go beyond M_2 and consider also the corresponding solar harmonic constituent, S_2. One of the most striking facts about the actual tides is that, almost universally on the open coasts of the ocean, spring tides occur sometime *after* New and Full Moon. This can be accounted for by the dissipation of energy in marginal seas. It was in fact one of the points in favour of Whewell's hypothesis, for the S_2 constituent having a shorter free wave-length than the M_2 constituent, there would be an increasing lag in the time of S_2 high water behind the time of M_2 high water as we proceed northward up the ocean. But apart from this hypothesis it is clear that marginal dissipation must form a part of any adequate dynamical explanation of the observed tides.

In 1920, Sterneck published co-tidal charts for all the oceans.* These charts are not based on dynamical principles, but only on observations. He took all the observations available, resolved them into two phases differing by a quarter period, and then estimated the positions of lines of zero elevation in each phase. The intersection of a line of zero elevation in one phase with a line of zero elevation in the other phase gives an amphidromic point in the combination. By joining these amphidromic points to various points on the coasts he produced his co-tidal charts. For the semidiurnal tides of the Atlantic, his co-tidal chart, Fig. 4, shows the same general features as that of Harris, but the co-tidal lines for the North Atlantic have a more natural shape. He has the crowding of co-tidal lines on the west near 17° N. but not near 32° S.

In 1924, A. Defant gave a dynamical treatment of the spring tides of the Atlantic and Arctic regarded as one basin.† He applied to the combined basin of the Atlantic and Arctic the same methods which had been successful for such narrow seas as the Red Sea and the Adriatic. He considered two phases of a longitudinal oscillation. In each phase the transport of water is assumed to vanish at the northern end and the elevation to agree with observations at the southern end. All the generating forces are assumed to be allowed for in one of these phases, so that the other phase is a co-oscillation with the water of the Southern Ocean. Defant makes no allowance for the dissipation of energy. With one exception, the nodes of one phase are very near to the nodes of the other phase, so that their combination is practically a standing oscillation. There can be very little progression either northwards or southwards. When the rotation of the Earth is allowed for, transverse surface gradients are introduced, except quite near to the equator. In the North Atlantic the surface rises to the right of the current, while in the

* R. Sterneck, *Sitz. Akad. Wiss. Wien*, IIa, 129, 131, 1920.
† A. Defant, *Ann. Hydrog. Marit. Meteorol.*, 52, 153, 1924.

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South Atlantic the surface rises to the left of the current, and of course in both cases it oscillates with the current. We thus have a series of amphidromic regions, the co-tidal lines going round in the anti-clockwise direction in the North Atlantic and Arctic, and in the clockwise direction in the South Atlantic. This means that a wave progresses northwards up the coast of South America and of Western Europe, and that a wave progresses southwards down the coast of North America and West Africa. There is, of course, no south-going wave down the coast of West Africa. But then Defant has not yet allowed for the transverse generating forces, and this difficult calculation he does not do adequately. His forced transverse oscillations are chosen so as to make the combination of all his oscillations agree with observations on the coasts. Fig. 5 shows Defant's co-tidal chart, but it may be subjected to some very serious criticisms:

1. He assumes that longitudinal generating forces produce only longitudinal currents, and that transverse generating forces produce only transverse currents. Owing to the Earth's rotation, these assumptions are not valid.
2. His total neglect of dissipation is very serious.
3. His forced transverse oscillations are without value.
4. The large range which he obtained on the arctic coast of Canada is not in agreement with observations.
5. The small range which he obtained at the Azores is not in agreement with observations.

In 1932, Defant abandoned his work of 1924 and confined his attention to the neighbourhood of the central line of the ocean and also to the part of the Atlantic south of Iceland.* He assumed:

1. That the rotation of the Earth does not affect tidal conditions on the central line.
2. That the transverse generating forces do not affect tidal conditions on the central line.
3. That there is no dissipation of energy to the south of Iceland.

His method does allow for the dissipation of energy outside the region which he considered, and in fact he stressed the importance of friction in the Arctic Sea. He considered two phases, differing by a quarter period, such that they agree with observations at the Azores and at Tristan da Cunha. This time the node of one phase coincides with the loop of the other, and their arrangement indicates that there is a northerly progression throughout the region considered. His results show good agreement with the observations at the islands. Defant's assumptions cannot be more than rough approximations, but it does seem as if conditions on the central line can be largely accounted for by means of longitudinal oscillations. Defant's longitudinal current-phases also increase from south to north. They appear to be in good agreement with the results of the "Meteor" expedition of 1925–27. But his longitudinal current-amplitudes are only about one-third of those observed by the "Meteor". I am not disposed to attach very much importance to this disagreement, because it is very difficult to obtain an adequate depth-mean of current when the observations are much affected by the presence of internal tides. But if we discredit the disagreement shown by the amplitudes we must not attach much importance to the agreement shown by the phases, because the accurate determination of current-phases from observations is subject to the same difficulty.

I now propose to describe an attack on the subject which I started some years ago, but which is still only in its early stages. As a preliminary, let us examine a series of precisely formulated mathematical problems. Consider a region of the Atlantic bounded by two parallels of latitude, namely those for 45° N. and 35° S. Suppose that we are given either:

* A. Defant, *Wiss. Ergeb. "Meteor"*, 7 (1), 273, 1932.

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1. The general conditions to be satisfied along the coasts and the actual meridian-components of currents across the bounding parallels; or
2. The general conditions to be satisfied along the coasts and the actual tidal elevations on the bounding parallels; or
3. The general conditions to be satisfied at the coasts and the actual meridian-components of currents on one bounding parallel and the actual elevations on the other bounding parallel;

then the tidal elevations and currents are mathematically determinate all over the region considered, including their values on the coasts. For example, we might imagine the meridian-components of currents at 45° N. and 35° S. to be produced by suitably moving flexible pistons, and then in the first case it is clear that we have a definite physical problem. But we do not know either the actual currents or the actual elevations on the bounding parallels, whereas we do know the coastal elevations. Hence we consider the problem the other way round. Given the general conditions along the coasts and the actual values of the coastal elevations, the tides are mathematically determinate over the whole region considered, including the elevations and currents on the bounding parallels. This is my problem, but I propose to solve it by considering a series of other subsidiary problems of still different type.

Given the general conditions along the coasts and both the meridian-components of currents and the elevations along *one* bounding parallel, the tides are still mathematically determinate over the whole region considered.

This double condition along one bounding parallel has great advantages from a computational point of view. We can work steadily away from that parallel until we cover the whole region. Of course, in each subsidiary problem we have to take arbitrary distributions of current and elevation along the bounding parallel from which we start, but the object is to take such a series of distributions that a suitable linear combination of them will represent the actual distributions, whatever these may turn out to be.

I consider then a number of subsidiary oscillations. Each of these oscillations is a possible motion of the waters of the Atlantic under the conditions prescribed. These prescribed conditions include the general coastal conditions in all cases, and in each case they also include sufficient conditions along the parallel of latitude 35° S. to make the motion completely determinate.

I have not, as yet, introduced any dissipation of energy within the region considered, but this is quite consistent with any amount of dissipation of energy outside the region considered. My reasons for this neglect within the region are as follows:—

1. Jeffreys' total dissipation for the whole ocean did not include any contribution from parts of this region.
2. Defant's latest investigation did not involve any dissipation in this region.
3. It is desirable to keep the first attempt as simple as possible, and the introduction of coastal dissipation does complicate the details, though the general method allows for any amount of this.

I do not propose to describe the mathematical details by which these individual oscillations have been calculated. We proceed from one parallel of latitude to another 5° to the north of it, and much use has been made of an expansion-theorem, specially evolved for the purpose, which is being published by the London Mathematical Society.

It is possible that my treatment of the great irregularities of the basin formed by the Gold Coast and the north-east coast of Brazil may not yet be adequate. It is also possible that I have not yet taken sufficient terms in the series which represent the individual oscillations. I have still to examine these questions.

Fig. 6 shows the co-tidal and co-range lines of a forced oscillation. In this oscillation both the longitudinal and transverse generating forces are taken into account, and both the elevations and the meridian-components of currents are taken to be zero on latitude

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35° S. The oscillation is thus independent of the tides to the south of latitude 35° S The conditions satisfied along the southern bounding parallel secure that there is nc passage of energy into or out of the Atlantic. But they put on a much bigger restrictior than that, and keep the whole oscillation very small for some distance north of 35° S This does not mean that only a small part of the tides of the Atlantic are generated withir the Atlantic itself. If we put a barrier along any parallel of latitude, the cutting out

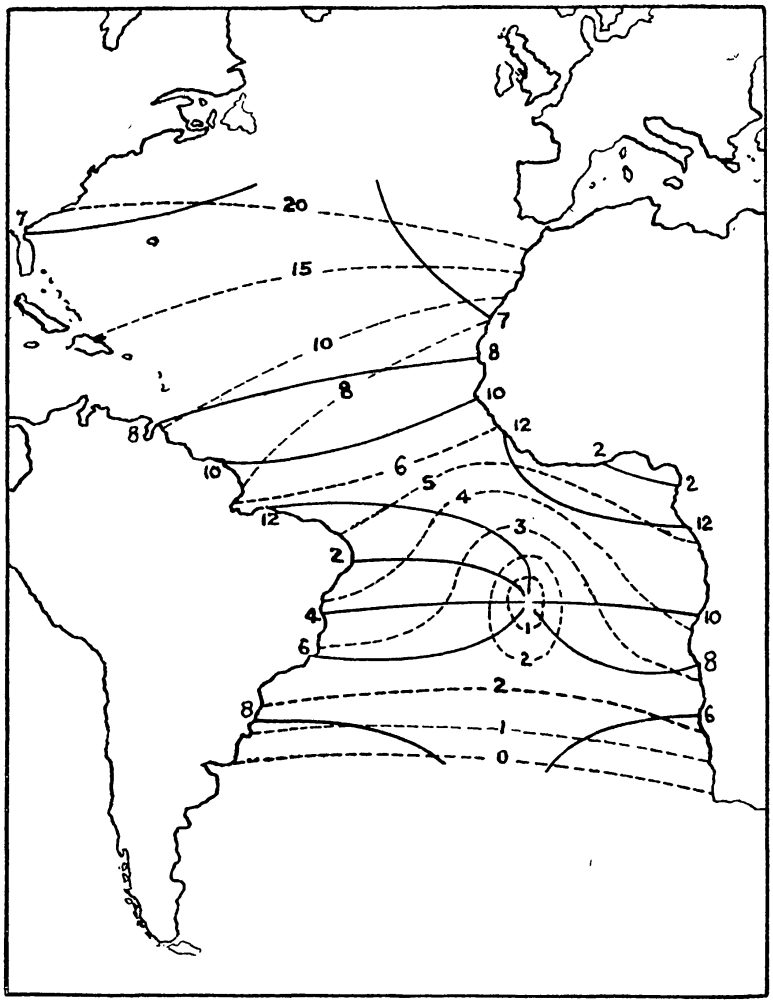


FIG. 6.—*Forced oscillation for M_2 constituent.*
Full lines give Greenwich lunar hour of high water.
Broken lines give amplitude in cm.

of the currents of this forced oscillation would require large multiples of the other oscillations.

The other subsidiary oscillations could be maintained by tides to the south of latitude 35° S. and are independent of the generating forces acting on the water of the Atlantic itself.

Fig. 7 shows an oscillation which I have called a north-going Kelvin-wave. It corresponds to a certain mean rate of northward progression of energy. As there are no generating forces and as there is no dissipation, this mean rate of progression of energy must be the same for all parallels of latitude. The particular type of motion assumed along the parallel 35° S. is that first investigated in 1879 by Sir W. Thomson for a canal on a flat Earth. The currents are directed along the meridians and both the elevations and the currents increase exponentially towards the west. No complication is produced by the curvature of the Earth along the parallels of latitude, but much

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complication is produced by the curvature of the Earth along the meridians, by variations in the width of the ocean and by variations of depth. It is these complications which make the distribution shown in Fig. 7 vary from the simple conditions along the parallel of latitude 35° S. Continuous reflection along the coasts is allowed for, but if a complete barrier were put across the Atlantic, then of course the wave would arrive at an impossibility. We notice that there are large amplitudes in the south-west and in the north-east.

Fig. 8 shows the co-tidal and co-range lines of a south-going Kelvin-wave. This time there is a certain mean rate of southward progression of energy, which is the same for all parallels of latitude. Again, the currents in latitude 35° S. are along the meridian, but this time both the elevations and currents increase exponentially towards the east. This time it is not a question of following up the wave, but of finding what it must have been on any parallel of latitude in order to reach given values 5° to the south. We notice that there are large amplitudes in the north-west and the south-east.

Fig. 9 shows the co-tidal and co-range lines in what I have called a north-going Poincaré-wave. The type of motion along the parallel of 35° S. is next in simplicity to that of a Kelvin-wave; it was first studied by H. Poincaré in 1910 for the same canal as was considered by Sir W. Thomson. Instead of the transverse currents being zero, they now follow a sine-distribution from coast to coast. For a uniform flat canal there is a line of zero range parallel to the sides of the canal; the co-tidal lines run straight across the canal, but the time of high water changes by six lunar hours on crossing the line of zero range. These simple conditions hold in our case only along the parallel of latitude 35° S. On account of the curvature along the meridians and of the variations of width and depth, a line of zero range is too artificial and becomes replaced by a special form of amphidromic region. We notice that in the centre of the ocean the amplitudes remain small.

Fig. 10 shows the co-tidal and co-range lines of a south-going Poincaré-wave. This corresponds to a certain mean rate of southward progression of energy. The conditions in latitude 30° S. are similar to those for the north-going Poincaré-wave.

These Poincaré-waves are quite different from anything in Defant's work. They are the two simplest of an infinite series of oscillations, and it will probably be necessary to consider more. When in 1919 G. I. Taylor gave his determination of tidal motions in a rectangular gulf, he used both Kelvin-waves and the infinite series of Poincaré-waves.

It is now a question of assigning an amplitude and a time-origin to each Kelvin- and Poincaré-wave, so that when the four are added to the forced oscillation, the coastal elevations in the synthesis will agree with observations as far as is possible. As all the oscillations have been computed from prescribed conditions at the southern boundary of the region considered, it is desirable to make the agreement between synthesis and observation as good as possible in the southern part of the region, and then to discover where it begins to break down. As there are four free waves, I have made the sum of all the waves agree with observations at four points on the coasts, two on the east coast and two on the west coast. Two of these points I have chosen in latitude 32°·5 S. and the other two in latitude 7°·5 S., *i.e.* south of the Gulf of Guinea and Cape San Roque where the major irregularities of the basin begin.

I then find, as expected, that the north-going Kelvin-wave is the most important. I calculate that this wave must have an amplitude of 52 cm. and a high-water time of 1·8 hour at the south-west corner of the region considered, where it is greatest. The next most important wave is the south-going Poincaré-wave. This must have an amplitude of 35 cm. and a high-water time of 9·1 hours at the south-west corner, where it also is greatest. The other two free waves have each an amplitude of 22 cm. at the south-east corner, where they both are greatest in the south. The high-water time of the south-going Kelvin-wave at this point is 10·8 hours, and that of the north-going Poincaré-wave 0·5 hour.

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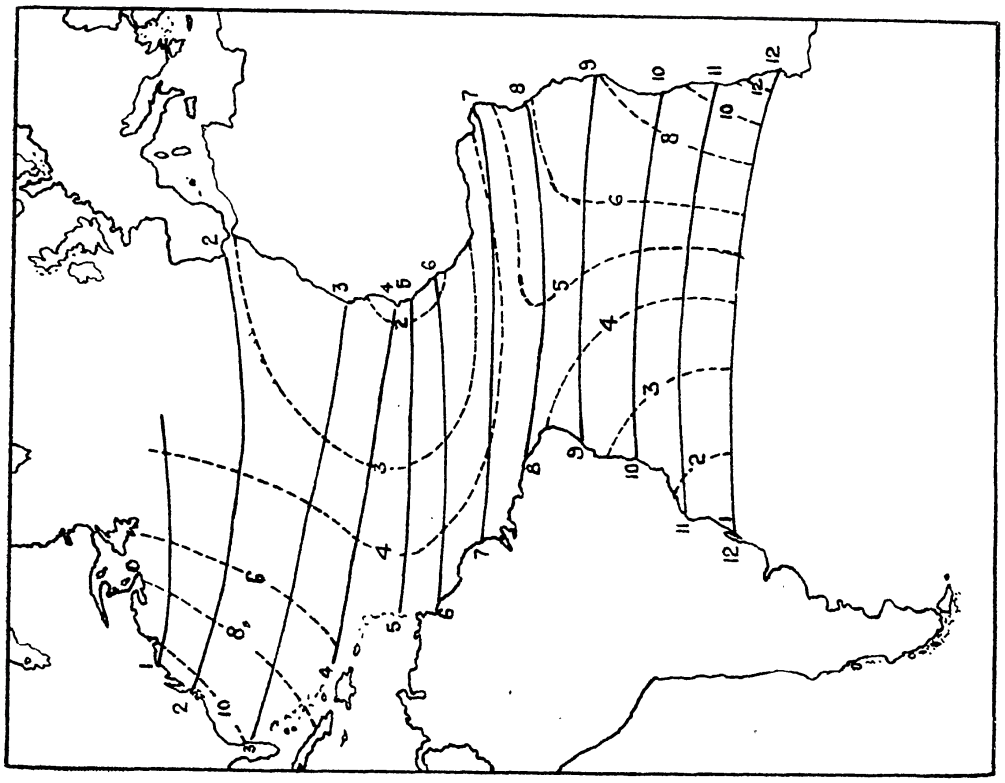


FIG. 8.—*South-going Kelvin-wave for M_2 constituent. Full lines give lunar hour of high water, origin arbitrary. Broken lines give amplitude, scale arbitrary.*

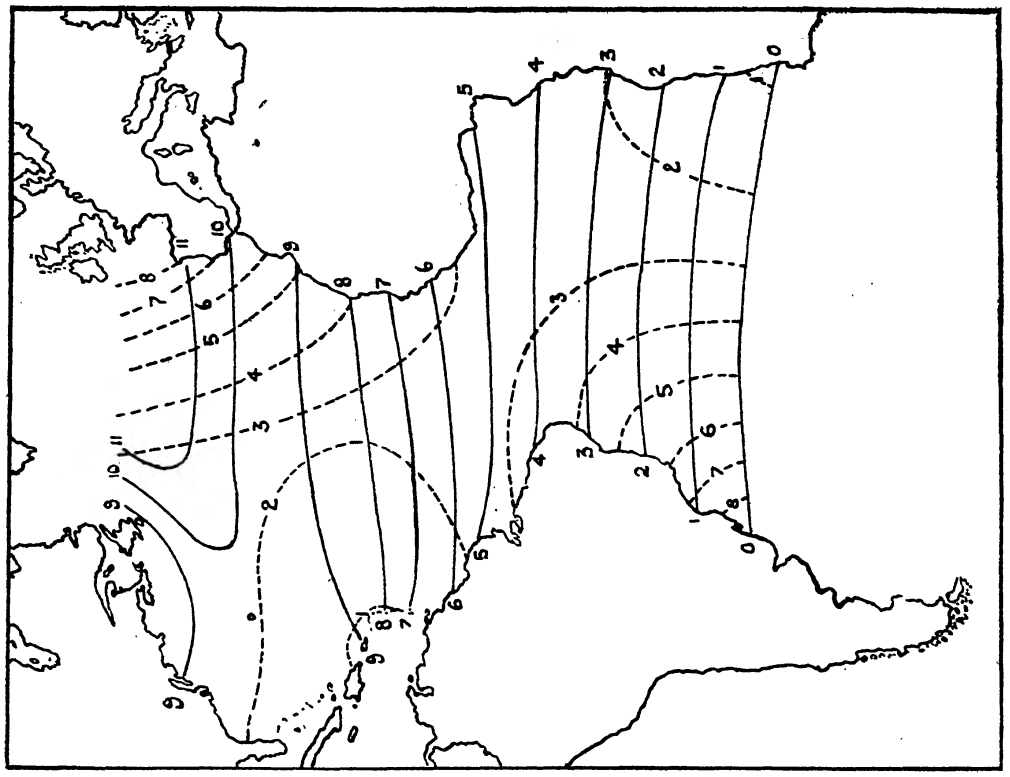


FIG. 7.—*North-going Kelvin-wave for M_2 constituent. Full lines give lunar hour of high water, origin arbitrary. Broken lines give amplitude, scale arbitrary.*

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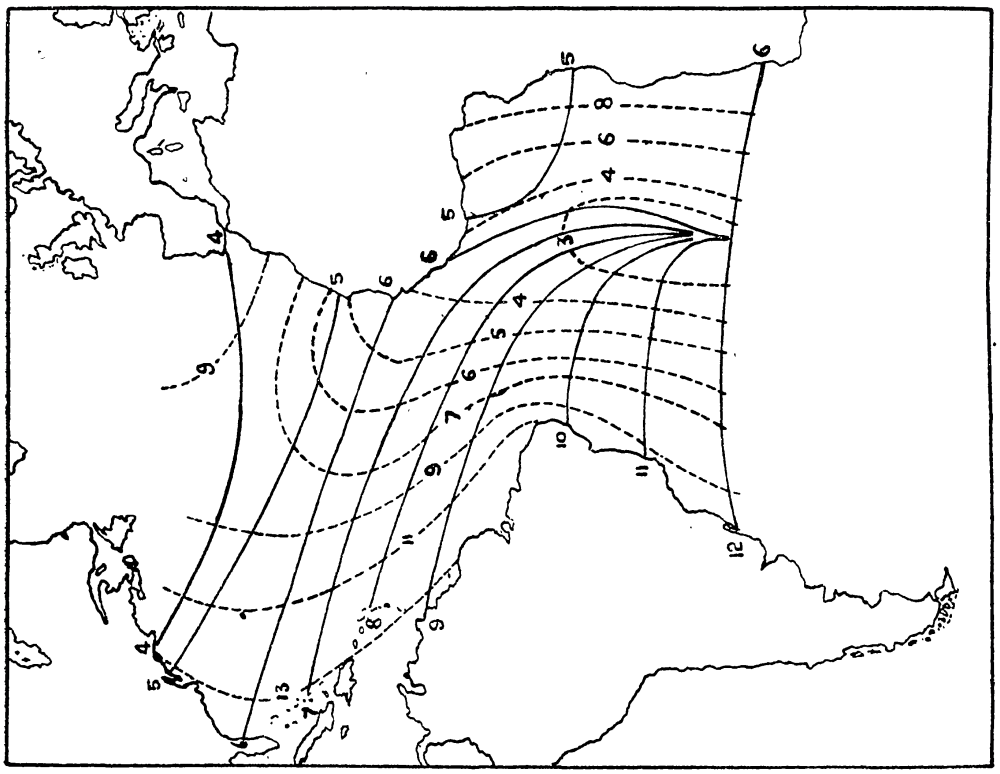


FIG. 10.—*South-going Poincaré-wave for M_2 constituent. Full lines give lunar hour of high water, origin arbitrary. Broken lines give amplitude, scale arbitrary.*

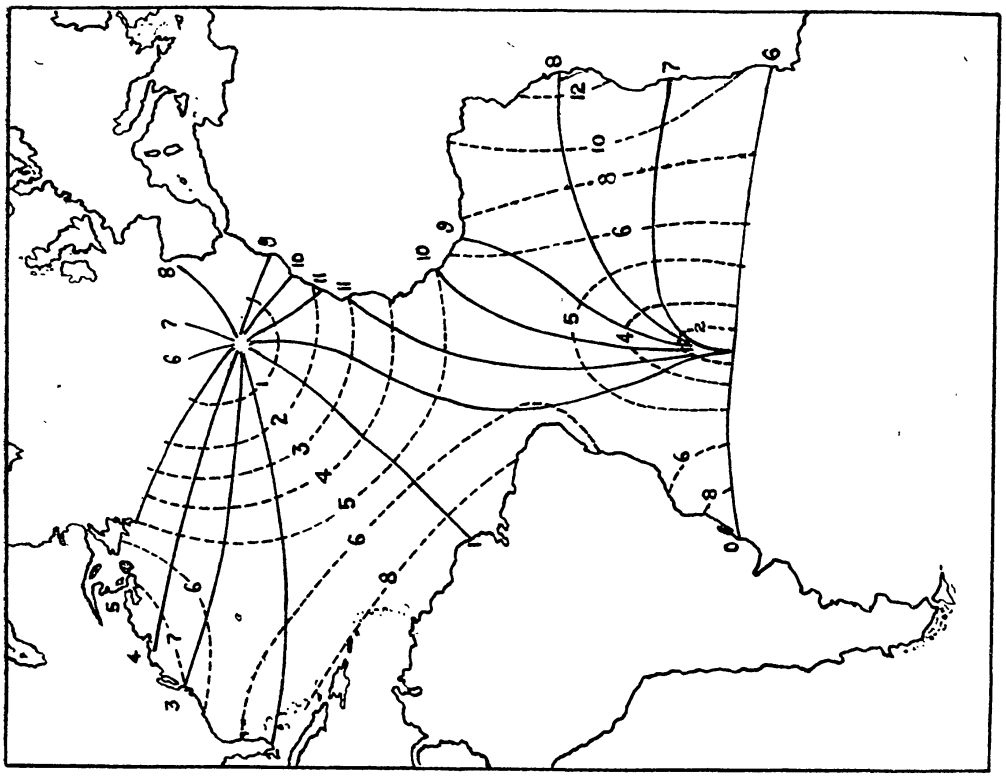


FIG. 9.—*North-going Poincaré-wave for M_2 constituent. Full lines give lunar hour of high water, origin arbitrary. Broken lines give amplitude, scale arbitrary.*

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The shorter lines of Fig. 1 show the lunar times of high water and amplitudes in the synthesis. They show fairly good agreement with the results of observations to the south of the equator, but they diverge very considerably to the north of the equator. Part of this divergence may be due to the deficiencies of the individual waves, as already indicated, but it may also be due to my not having taken a sufficient number of Poincaré-

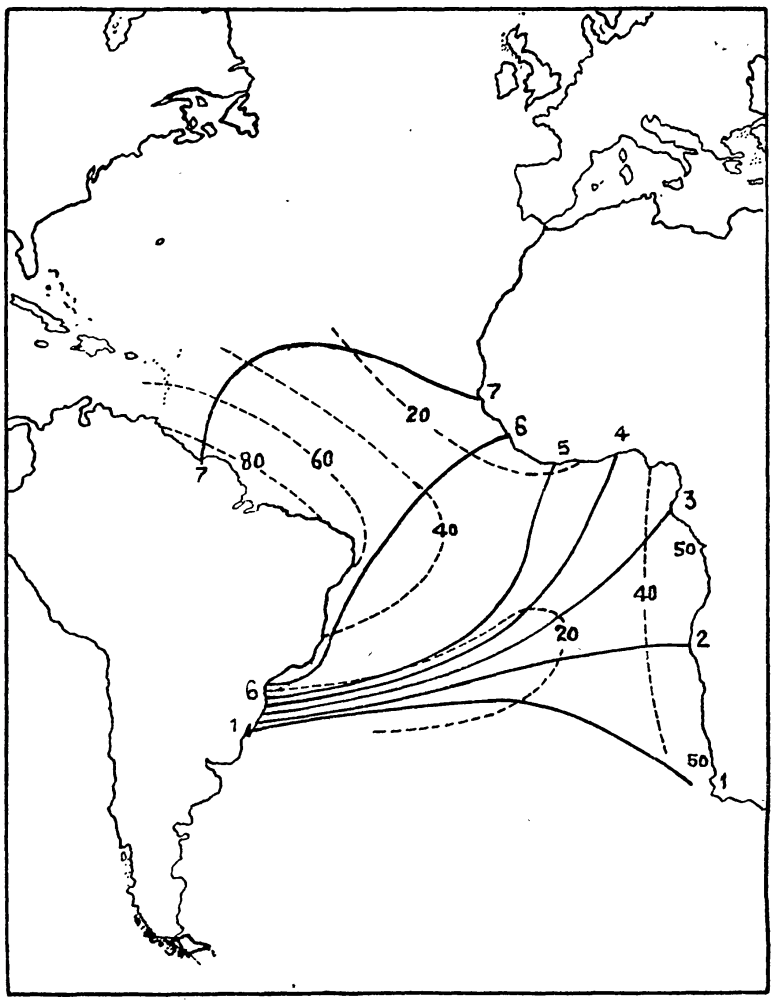


FIG. 11.—*Synthesis of subsidiary oscillations for M_2 constituent.*
Full lines give Greenwich lunar hour of high water.
Broken lines give amplitude in cm.

waves. The first two Poincaré-waves are somewhat special in that, for the particular dimensions of the Atlantic, at the southern boundary they progress along the meridians. All the others, at the southern boundary, progress along parallels of latitude.

Fig. 11 shows the distribution of co-tidal and co-range lines over the southern part of the region considered, according to the synthesis of waves at which I have arrived. I believe that this is the first time that co-range lines have been drawn right across the Atlantic. They show a trough of low amplitude reaching the coast of South America at about 32° S.; this trough contains a point of minimum amplitude in the middle of the South Atlantic.

By improving the determination of the individual subsidiary oscillations and by increasing the number of these oscillations, I hope to obtain a solution of the dynamical equations which will agree with the observations along greater lengths of coasts.

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