

Determination of Longitude by Wireless Telegraphy. (*Geophysical Discussions*, 1920 May 7.) By Professor R. A. Sampson, F.R.S., Astronomer Royal for Scotland. (Plate 11.)

In opening what is, I believe, the final chapter in the long story of the determination of longitude, one cannot refuse a brief glance, in review, at its refractory history. We know how Aristotle said that Morocco was probably contiguous to India because there were elephants in both, and how the names of the West Indies and La Chine Rapids fix a mark on the expectations of their discoverers; but none the less the essentials of the problem were perfectly understood by early explorers of the globe, it was only methods of execution that defeated them. In Hakluyt's *Voyages*, what I take to be the first successful determination of a long arc is recorded. It was made in the Straits of Magellan, in 1578, by Mr. John Winter, a consort of Sir Francis Drake, in a "good and newe ship called the Elizabeth, of 80 tunnes in burthen"—the H.M.S. *Queen Elizabeth* of those days; it runs as follows:—

"The 15 of September the moone was there eclipsed, and beganne to be darkened presently after the setting of the sunne, about 6 of the clocke at night, being then Equinoctial vernal in that countrey. The said eclipse happened the 16 day in the morning before one of the clock in England, which is about sixe houres difference, agreeing to one quarter of the world, from the Meridian of England towards the West."

Both observation and calculation were evidently done in his head, so we must not cavil if he gave 90° W. in place of 70° , for his reasoning is sound in all its details.

John Winter was able to bring off his determination because he had the fortune to find a celestial time-signal common to Patagonia and England, in the form of an eclipse; but the problem of providing such signals, whenever and wherever they were wanted, has proved by its difficulty an extraordinarily active stimulus to research. The Lunar Theory, the Theory of Jupiter's Satellites, both owe a large part of their lives to it. The study of the Moon, once acclimatised for that purpose at Greenwich, has been carried far beyond the point where it interests navigators. We may connect with it the fact that the Admiralty are the publishers of Hansen's *Lunar Tables*. In another field we owe to the premium offered by them the effectual encouragement of the admirable school of British clock-makers of the eighteenth and early nineteenth centuries, leading to the evolution of that wonderful piece of craftsmanship, the marine chronometer. It is only to be regretted that the study of horology—which is no less than the laboratory study of the rotation of the Earth—has been left at its craftsman stage and has never been taken forward as a purely scientific interest, as that of the Moon has been.

These remarks may seem discursive, but my purpose is to

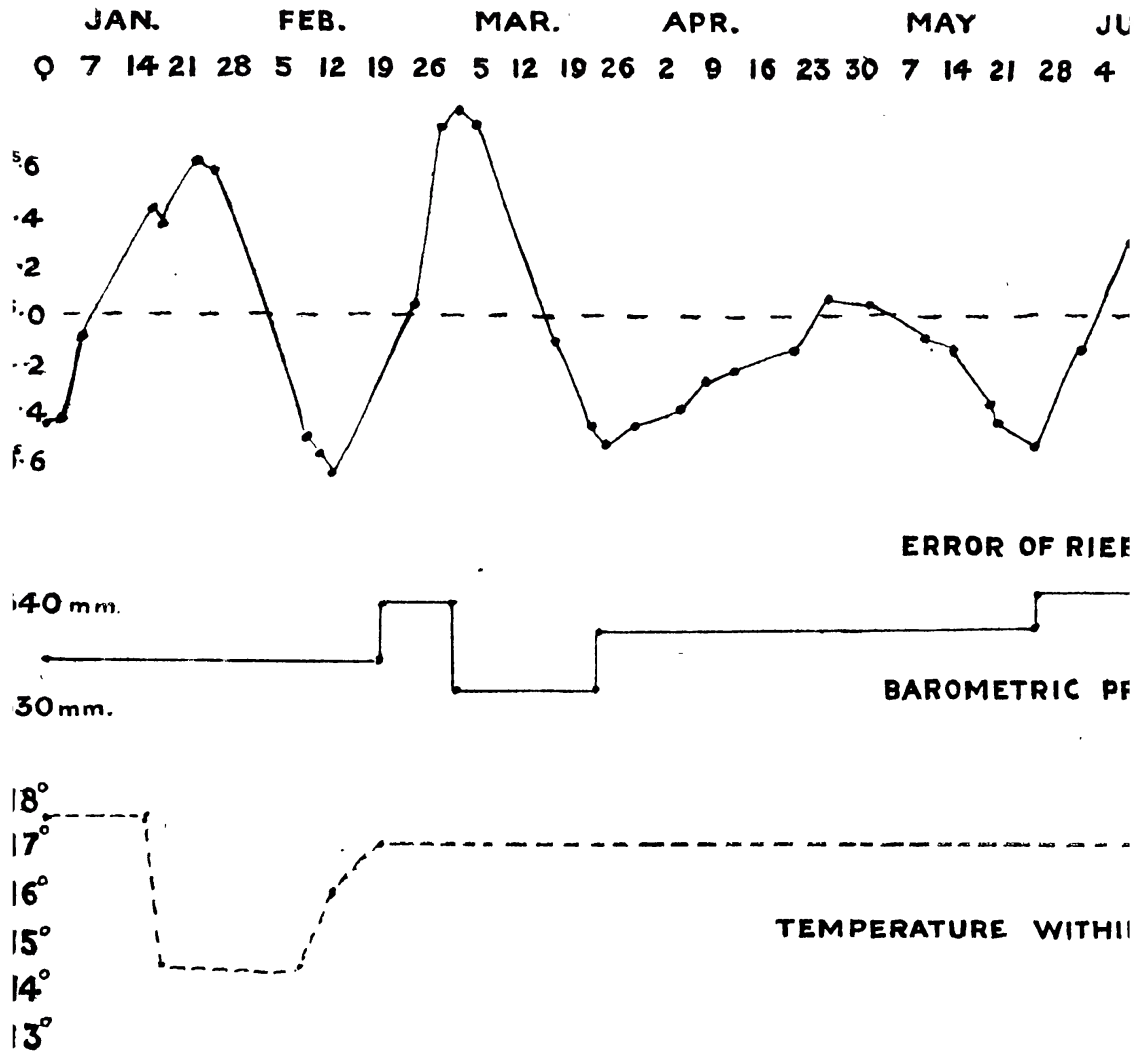
show, to anyone who has a feeling for history, how deeply implicated the research of longitude has been in the progress of our knowledge of the Earth, and also of the sky. It should, of course, be recognised that to-day what we don't know about it ranks as an abstract scientific research. I recollect that on the conclusion of the last Paris-Greenwich longitude determination, a newspaper man who had become aware of it tried to convey it to his public as "settling a mere point of honour in pure mathematics." Whether the pure mathematician of to-day would describe it so, we need not ask. You take his meaning. If anyone supposes that there is any outcome of value, in the material sense, in sight in the research, he will be disappointed. As Burns says, "Let him turn and flee." We shall go forward without him.

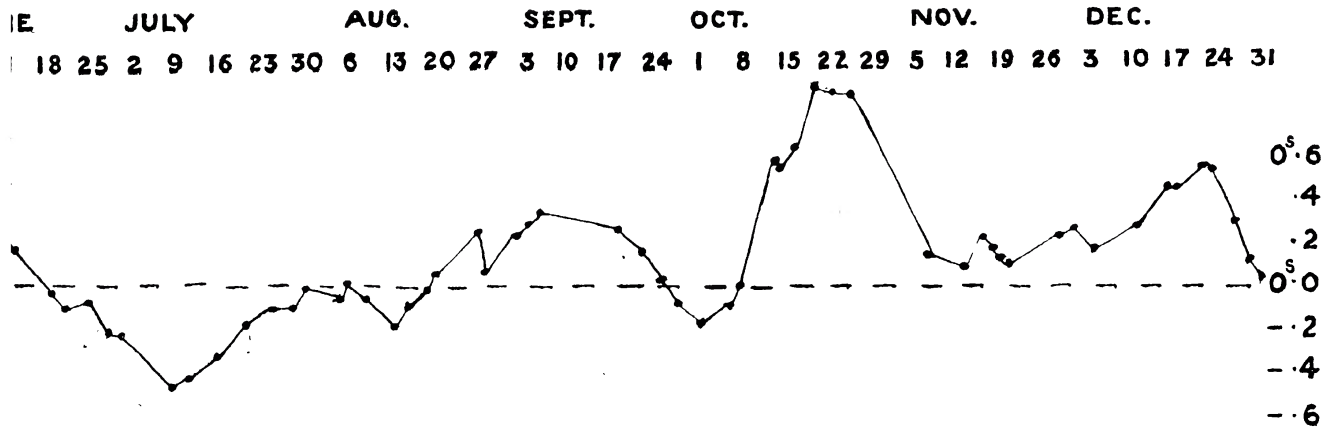
Now, in my opening words I referred to the application of W.T. to longitude as the final chapter in its long history. By that I mean that we have in it an ideal answer to the search for a common signal. Longitude has lagged painfully behind latitude in its advance to knowledge because the sky supplied in profusion for the latter celestial marking points such as the Sun, visible to separated stations, from which the elevation of the pole could be derived. Now here we have at last a world signal available for the other co-ordinate also. We shall be dull indeed if we do not exploit it forthwith for all that it is worth.

What I propose to do in this address is to examine what such a project involves; our present position in respect to astronomical preparedness to undertake it; some indication of the instrumental position; an outline of what questions of scientific interest it involves; the organisation the research would require; and the co-operation outside of astronomical resources to which we must look, if any complete treatment is attempted. In this I shall confine myself to the problem of the long arc alone. And as I dislike to occupy any equivocal place, I should like to say that I approach the subject as an astronomer and have no pretensions to be an expert either in geodesy or in wireless telegraphy; I shall be grateful to those who are if they will correct or amplify any defect they may notice in my remarks.

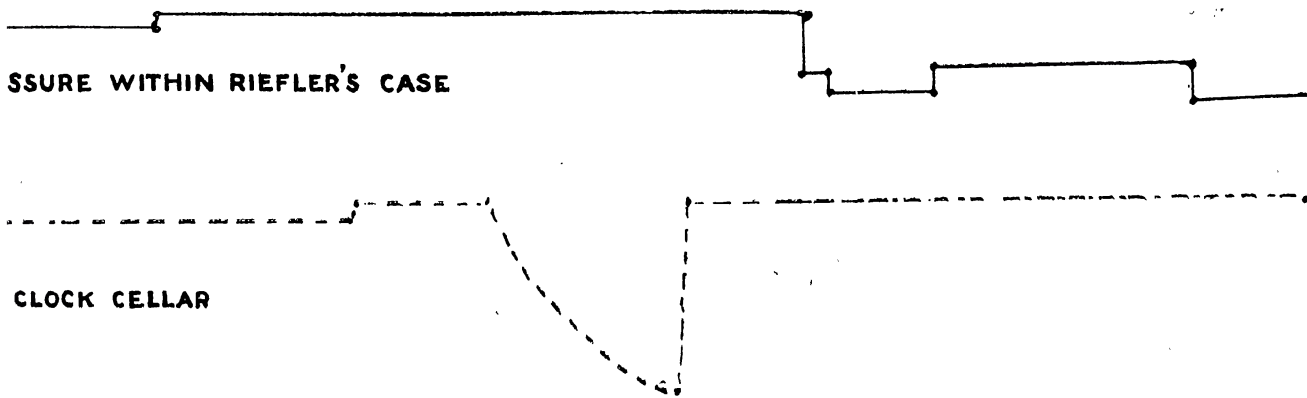
We have heard a great deal recently about the nature of time as a fourth dimension, and so forth, but to the plain astronomer time is just another name for the phase of the Earth's rotation. The Earth's rotation is his standard of time, and his clocks are simply instrumental means of indicating it. If we marked φ , δ round the hours dial of his sidereal clock in place of 0^h , 2^h , the pointer should tell at any moment the star that is on his meridian. Now it is plain that in stellar observations, and largely in nearly all celestial observations of any kind, a reference to the Earth's phase of rotation only enters to be eliminated again as promptly as possible; and this, in fact, is what is done. Thus, chiefly by long-continued systematic massed observations at Greenwich, the system of R.A. of stars, round the whole circle, is known almost as

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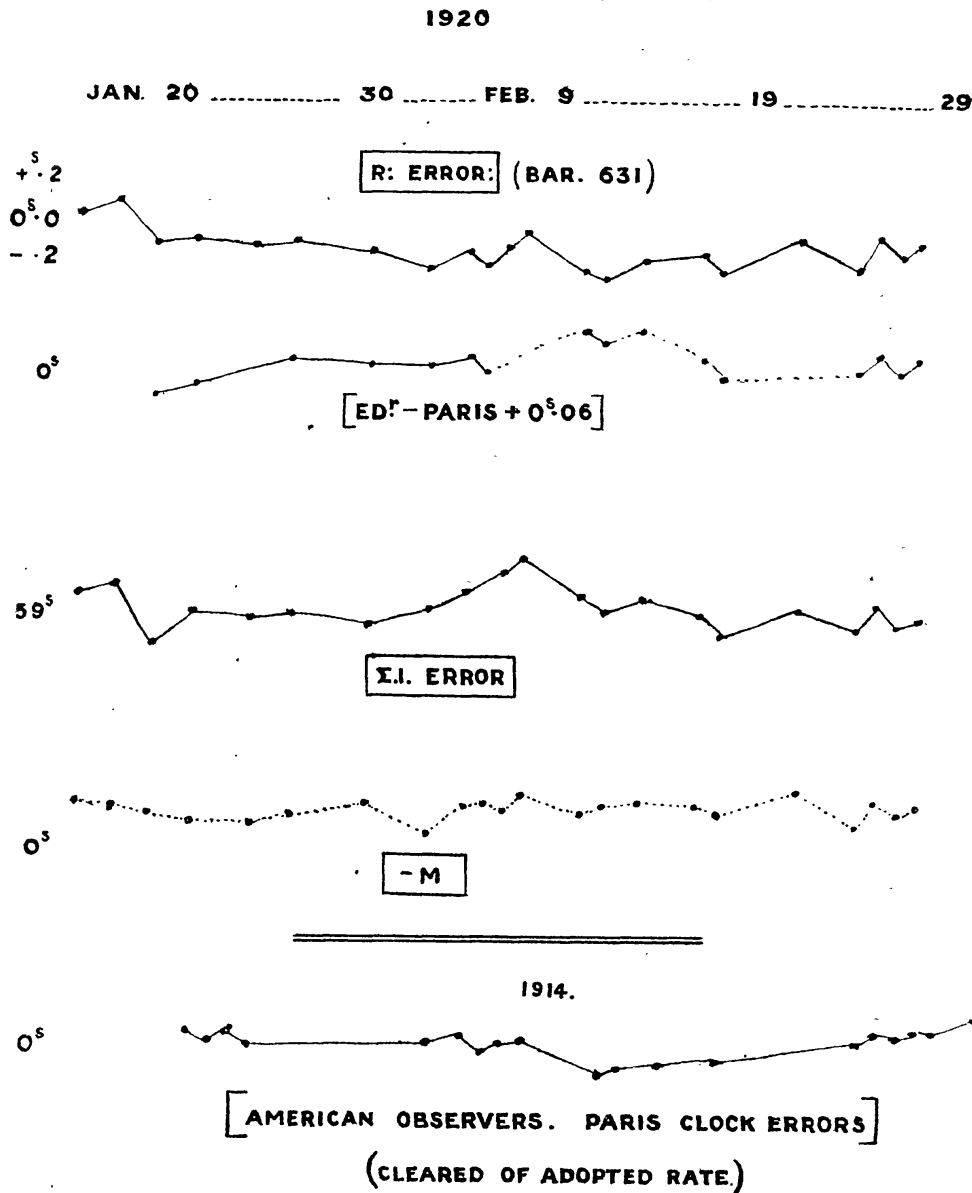
closely as we can observe the location of any special star between its immediate neighbours. It seems clear that there is no systematic distortion of the sphere of stars amounting to more than about $0^s.02$ anywhere. The performance of the clock has been made a secondary matter. This is a marvellously fine result, but it should not content us. It belongs to the stellar region only. Wherever any bodies enter, like the Moon or Jupiter's Satellites, which themselves constitute a natural clock or alternative measure of time to the Earth's rotation, we are forced to incorporate the readings of the same clock at different moments, or those of separated clocks. In other words, the problems of instrumental time-keeping and of longitude are inevitable. It will not do for the astronomer to treat them merely as a branch of geodesy. So far as he is interested in fundamental work he must spare no effort to make those regions as exact and reliable as care can make them. Nor, if Greenwich is to remain in fact as well as in name the prime meridian, can we ignore the circumstance that the probable error attached to the Paris-Greenwich longitude is very much greater than that claimed for the Paris-Washington longitude recently executed by wireless telegraphy.

I want then to survey briefly the present position in its different directions, and first I would take that of time determination at a single station. In speaking of time-keeping I shall draw my remarks from two sources: the published account of the determination of the Washington-Paris longitude by wireless, and the experience of my own observatory at Edinburgh. It is now several years since I selected problems of time-keeping and the rotation of the Earth as suitable to the circumstances and equipment of the observatory. A good deal remains to be done, and much was interrupted by the war, but my equipment is fairly effective. I have a clock cellar of which the temperature is controlled automatically, a Riefler clock and two others, which, if not as constantly reliable as Riefler, are yet able often for spells of months together to compete fairly with it. I have given a good deal of care to the chronographic system. I should be the last to suggest that my observations are made with any more skill than elsewhere, but I think I can fairly say that the instrumental precautions are nearly as thorough as they can be made. I shall put a few figures before you; if they strike you as less accordant than you expect, I venture to say that you will not easily outdo them at the present moment.

The chart shows the run of Riefler's error for about a year. This error is generally kept between $\pm 0^s.5$. When it reaches these limits it is brought back by the barometer. Reducing the indicated rates in runs of, say, twenty days or a month to standard barometer, say 630 mm., we are left with a slowly fluctuating base-rate running in this period from $-^s.12$ to $+0^s.03$, and known with hardly any uncertainty to $0^s.01$ per day. This may be an oil effect, or something obscurely associated with temperature of the nature of stratification in the chamber, if it belongs to the clock at

all. Removing this base-rate, and remembering that there are one or two instances where the base-rate is caught changing, we find besides that we have for each observation a residual erratic feature. The mean of these erratics for Riefler is $\pm 0^s.05$.

I have also regularly taken for several years the scientific or



rhythmic time-signals of the Eiffel Tower, and thereby obtained a comparison of my clocks with those of the Observatory of Paris. These signals are arranged as a species of time-vernier; with a carefully arranged ticking attachment at the Observatory it is possible to fix the coincidences to a single beat, that is, to $0^s.02$. Recently these have been taken only on the nights when I had a determination of time, so that there was virtually no rate to carry

forward. The mean of these comparisons, now between 400 and 500, shows Edinburgh slow on Paris by $0^s.06$, or the adopted longitude of Edinburgh $12^m 44^s.2$ W.—too small by that amount. The erratic residuals outside this mean contain the faults in time determination at both observatories; their average is $\pm 0^s.07$, which would agree with an individual erratic feature in the French time-signal equal to that which I assign to the Edinburgh clock, provided this *erratic feature belongs to the observing system and not to the going of the clock*. For it is evident that it would only appear in the comparison of the two clocks if it were *incorrectly* ascribed to the clock error that is employed.

Hence it becomes a question of importance to obtain confirmatory evidence of the origin of this erratic feature. Taking for exhibition a run in the present year, January 15–February 27, when the determination of errors was very frequent and the erratics slightly more pronounced than the average and consequently the oscillations readily noticeable, by comparison with Paris, and also with the clock Synchronome I, cleared of a calculated portion of its rate so as to make its chart more speaking, it is pretty evident that the strongest features are common to all three, and therefore belong to the observations. Indeed, if further we chart the T.C. errors, it happens that we are able to assign a substantial part of the error to the quantity m which enters in the same way for all stars, and thus, if wrong, goes straight into the clock error.

The clock error is found with a hand-driven transit micrometer, and appears to be practically impersonal. Each is derived from nine or ten stars, with two or three polars; and the average discrepancy of each star from the indicated mean is much less than the established erratic element of the whole, being $\pm 0^s.02$ only.

Since the essence of longitude work is the correct determination of time, one naturally asks whether the anomalies unexplained above are escaped by other systems and other observers. Taking the Washington-Paris determination as an example of the scrupulous observance of precautions—in which, for instance, the azimuth of the azimuth marks, and the level, were read between every two observations and the instrument reversed in the middle of every observation,—we may say that they are not. The range of their individual night's observation is greater than mine; there still remain occasional complete obscurities as to what the clock is doing, of which I show a specimen. These erratic features the American observers attribute not to the clock but to the observations, though without more than tentative explanation.

I shall return a little later to the question of the value of elaborate precautions.

It is perhaps not the occasion to enter into further particulars. If, as I suppose, and as the Americans suggest, these erratics belong, not to the best modern clocks, but to the observations, a joint treatment of time by many observatories would be a powerful method for clearing them away.

I would make a remark upon the determination of time. It is, I think, sometimes dropped out of mind how many links there are in its chain. Speaking still of our practice at Edinburgh and starting from the stars, there may be catalogue errors; the geometrical errors of the instrument are certainly not easy to place beyond suspicion; there is conceivably a personal error with the observer, sending his signal to the chronograph; the chronograph is a fairly elaborate instrument, its zero depends upon the true return to its original set of a marker which pricks a paper fillet, and the unvaried movement of the armature of an electromagnet which starts from rest. We then start another chain before we get to the mean-time clock. Taking its pendulum as the real standard, its signals are made by a wheel on the scape-wheel arbor, and I have shown that with a micro-chronograph the inequalities of these wheels are measurable; this signal operates a P.O. relay, introducing an electromagnet with armature starting every second from rest; this in turn actuates a second pricker of the chronograph which is open to the same criticisms as the first. The time is taken by a coincidence of the two pricks, followed by a calculation based on the *Nautical Almanac*. The errors of such a system require examination. By setting another clock to give the signal in place of an observer, they can easily be estimated. I do this every week, taking a run of coincidences of M.T. and sidereal clocks each hour for nine consecutive hours. The average discrepancy from a run completely smooth numerically is ± 0.010 , on the whole a somewhat surprising result, since an error of a single unit in marking the occurrence of coincidence would give half that amount, and there are two P.O. relays involved, working near their limit of operation, and finally the rounded *N.A.* figure may be anything up to ± 0.005 in error. But at any rate there is the chain, and if we are taking a wireless signal we must add to it the lag and accidental error of the apparatus that supplies the clock ticks—in my case the stroke of a little contact-making hammer agitated by the stroke of a pendulum controlled by Riefler's current. There are many of the links of the chain that might be called and are called, in different connections, the time; but none must be taken for another, and the problem of establishing their relative lags with security is fundamental to the research. It can be done, but only done, I think, with the oscillograph, as I have shown in numerous cases in a paper in *Monthly Notices*, 1918 June, on measurement of clock times to 0.001 .

Turning now to the question of the correctness of the R.A. system, or absence of deformation in the reputed sphere of the stars. The method of using a common wireless signal at two widely separated stations has a bearing upon this that in my judgment should not be overlooked. If both observers take the common signal as their standard and at the same time observe what stars are on their meridian, this is exactly parallel to the method of testing a divided circle with two fixed microscopes.

By going over and over the circle systematically, the separation of the microscopes—that is, in this case, the longitude difference of the stations—and also the errors of division, viz. the systematic errors in R.A.'s of stars, will be certainly found. It may be that this method will not easily supersede the methods which have already yielded so much to patient application, but it is another and unused method, and it offers advantages that the other has not; it eliminates all reference to clock rate, and its determinations are cumulative in weight.

So much for the question on its side of technical astronomy. As to its features of technical radio-telegraphy, it is fortunately unnecessary for me to speak. Sir Henry Jackson is one of the earliest pioneers on the subject in its use in the Navy, and at the present date he is chairman of the Admiralty Radio Research Board. Besides that, General Ferrié, Director of Telegraphs of the French Army, whom I informed of our intention to hold this discussion, has most kindly contributed a paper which will be read presently. He has directed his remarks to the latest technical advances, which are great, very great indeed, since the Paris-Washington longitude was determined in 1913 with a crystal detector, a wave-length of 2200, and emission from Paris at 18 kw. To-day I am able to show you, by General Ferrié's kindness, the very perfect autographic records that are now taken at the Eiffel Tower, or by Professor H. Abraham, of various distant signals, including those from Arlington, Maryland.

My impression as an outsider regarding these rapid changes is that anyone who wishes to keep abreast of the latest interests must be prepared to scrap and replace his apparatus pretty frequently. And always, I suppose, at greater and greater expense, as the appliances get cleverer and more elaborated.

Let us now turn again to the goal of our efforts, the ideal way to use the world signal which W.T. provides—a true world signal even though we decide to use it like the Sun, only for half the world at a time. I take it the very heart of the project is a network, a chain embracing the whole globe, with a few long arcs, all determined with the same definite standard of precision, and providing an automatic check by the application of a closing error. If this were once done it might be improved upon afterwards, but from the beginning it could not fail to be the standard for reference of any other systems.

Let us make a rough survey of the present resources, and the minimum requisite for such a plan.

It may be taken at present that both emission and reception should be in the hours of darkness. Allowing some margin, this would point, say, to three main arcs AB, BC, CA, no one of which exceeded, say, 150° , and three emitting stations, S, T, U, between them. The arc AB would be found from emissions at S, BC from those of T, and CA from those of U. For this A, B, C must be observatories fully equipped for determining time and receiving radio signals, S, T, U must be powerful radio stations which can

be heard for at least one quarter round the world. There is no need for these to be provided with appliances for time determination of a high order.

If these conditions cannot be fulfilled, we must fall back upon more closely placed points, with the disadvantage that there are more elements to contribute errors.

Starting from the meridian of Greenwich (A), California (B) is 120° W., Western Australia is 120° E. and Victoria (C) 140° E. I learn from Mr. Cooke, Commonwealth Astronomer, that the Australian observatories are united in keen interest in this matter, and cannot doubt that the observatories of the Pacific Coast would be equally interested. Choosing stations from the map, we would put Annapolis (90° W.) at S to serve A and B, Honolulu (160° W.) at T to serve B and C, and say Mauritius (60° E.) at U to serve C and A. But in such matters any actual experience is of more weight than symmetrical designs from a map, and I am fortunate to have a very interesting letter from Mr. Cooke on the subject.

Independently of the present project, Mr. Cooke had formed a plan to link himself by wireless, eastward and westward from Sydney, with Greenwich, and had constructed two signal clocks for the purpose, sending them to Darien and Aden respectively, supposing these to be suitable stations.

However, he finds that with the best receiving set the Commonwealth Radio Service could supply, he hears Darien (100 kw., distance 8700 miles) very rarely. Honolulu (350 kw., distance 5000 miles) he receives unequally, and doubts if the time-signals are sent with full power. Certainly they are not seldom omitted. Aden proved unsuitable, as did also Cairo. On the other hand, Lyons in night transmission (distance 10,600 miles) can be read with the phones on the table; and now, by the courtesy of General Ferrié, he is examining if time dots can be received. The 9 a.m. transmission from Lyons he cannot get. Cavite, Philippines (distance 3600 miles), he hears beautifully at all times.

There are, no doubt, capricious features in reception from different stations which hereafter technicians will clear up and probably remove for us.

Whatever the best solution may be, it is evident we are not very far from being able to organise a longitude net at the present day. It is perhaps idle to speculate further upon details, but I would suggest that we would naturally wish to duplicate the ties by adding a net of observatories in opposite latitudes, as the Cape (20° E.) to the Greenwich zone, Cordoba (60° W.) and Valparaiso (65° W.) to the Pacific zone, and Tokio (40° E.) to the Australian zone. One would thus surround the globe with a net of about six points roughly equidistant, the longitudes of which would be consistently determined with high accuracy.

What would such a scheme cost in time and money? Take the observatories first. It is a prime question with them. Their re-

sources are, as a rule, either small or are already occupied. From correspondence which I have had with members of the Commission de l'Heure, I gather that there is in some quarters fear that it might lead to an extensive paper programme, burdensome to the staff, disturbing routine, and not substantially advancing astronomy. Personally, my view is that if a joint treatment of the rotation of the Earth by the observatories of the world is possible, it is so important that it cannot be treated as a secondary problem, but must be organised alongside other matters even at the cost of some trouble and expense. But just how much would be the trouble and expense?

- (1) It would require modern long-distance receiving apparatus, perhaps but not certainly, preferably, autographic.
- (2) It would demand a thoroughly equipped time-keeping system, continuously studied, including all questions of chronographic lag and personality; but this should be considered a part of the system of any fundamental observatory, and not an additional impost.
- (3) The actual receptions should, I think, be made at least on every occasion when time is determined, and when more trust can be placed in the clocks, on every day. The calculations are very short.
- (4) There would be two stations to receive. Two half-hours a day might cover it, but these might be, one, say, at 8 p.m., and the other at 4 a.m. That is the only difficult feature.
- (5) As to discussing the observations, usually an observatory would prefer to discuss its own, but it need not do so, and in any case they should be communicated to the Bureau de l'Heure, which should issue a joint discussion.

A governing question in settling the cost is whether it is essential to use identical duplicate instruments at different stations, and to interchange observers, in order to eliminate personality.

I would make some remarks upon this point. It has certainly been the custom in past longitude work to make every instrument in duplicate, and to reverse every step that can be reversed. This is, I think, carried to an extreme, *e.g.* in the Washington-Paris determination, where the instrument is reversed in its Y's in the middle of the transit of each star. This only abrogates the collimation error, and that at the cost of introducing a new "contact error" from the micrometer. But there is substantial evidence for doubt whether the advantages of such a plan outweigh its disadvantages. The latter are:—

- (1) that the instruments must be small to be portable;
- (2) they are not, in fact, identical; (*e.g.* the two levels in Washington-Paris example gave permanent differences);
- (3) they are for exceptional use, and their behaviour and faults are therefore not so well known as the fixed prime instruments;

- (4) the longitude expedition is a disturbing event, destroying the routine of habit and continuous work;
- (5) and the requirements imply added cost.

Now we notice that the elimination of personality is not, in fact, secured. The interchange of American observers and instruments, end for end, Washington and Paris, changed the result by $0^s.21$. Each set was beautifully accordant with itself, being (from plotted clock corrections)

$5^h 17^m$	$36^s.549$	$\pm .0051$
	$36^s.758$	$\pm .0027$

In the face of this discrepancy discussion is quite helpless. Of course the mean is adopted, but no better conjecture is given of the origin of their difference than diurnal variation of rate of the two Riefler clocks, which is not confidently prescribed, and is not sufficient, and, I venture to say, certainly does not exist.

But if we do not, in fact, gain what we aim at by this course, I suggest that we should try another one, viz. to build upon the very thorough study which the great fixed transit circles enjoy, organise the work so as to allow of continuous approach to our end, with cumulative evidence, and for the elimination of systematic error trust to variety and succession of normal observers, rather than transport men for short expeditions to unfamiliar circumstances. Among other advantages this plan would allow of execution without disturbing the regular work of the observatory or adding to its expense.

The work should, in my opinion, continue permanently, or at any rate for several years. A certain number of nights would be lost because there was no time observation at one or other of the two stations, and no reliable clock error carried forward. Besides, the most important conclusions would be cumulative; once the proper precautions were known and observed, they would tend speedily to become of irresistible weight.

As regards the radio stations, each would require the automatic clockwork for making the spaced dots, gaining, say, 1 in 50 seconds, to be put into operation at an approximately known but really arbitrary moment. This would be the only additional expense, not surely a material one to a station of the magnitude contemplated.

As regards the time required for emission, this would be, say, ten minutes once a day, in the neighbourhood of midnight, at an agreed-upon hour. No special technical staff would be required.

To sum up the position, what we stand to gain is:—

- (1) a more exact knowledge of longitudes, the final settlement of the most obstinate of geodetical problems;
- (2) consistency in time determination throughout the world;
- (3) elimination of small remaining errors in systematic R.A. of stars;

- (4) improvement in knowledge of clocks as timekeepers and as chronographic instruments;
- (5) with some reaction to the benefit of radio research.

As to a practical proposition, it is understood that there will be a meeting of the Commission de l'Heure in 1922. These and cognate questions will probably supply its chief matter. There are some old international agreements as to ordinary wireless time-signals to rehandle, and one would expect a pretty strong international attendance when it meets. I would suggest that the British members should go to that meeting prepared with a well-thought-out plan, commensurate in scope with the responsibilities which our world position in all nautical matters implies. We should hope that the Admiralty would look with favour on this project. A great private company like the Marconi Company certainly could aid most powerfully, if we had the goodwill of their highly skilled staff, in advice and in keeping the observatories up to date in equipment. The commercial shipping interests of the country can hardly be said to be involved in any material way, but we hope that they would not be indifferent to its success, and would be able in some way to smooth its course. Upon astronomers the most responsible labour will fall. The consideration of perfecting their individual time systems is always before them. They might direct it with this end in view.

Note sur les Procédés actuels d'Emploi de la T.S.F. dans la Détermination des Longitudes. Par le général Ferrié, C.M.G., LL.D.

Le rôle de la T.S.F., dans la détermination des longitudes, est de fournir un signal instantané qui donne, pour chacune des stations dont on veut déterminer les différences de longitude, l'instant précis t auquel il faut noter l'heure de son garde-temps afin d'avoir, par différence avec les heures notées dans les autres stations, sa comparaison avec les autres garde-temps à cet instant. Son emploi est très avantageux, car il suffit que les stations considérées se trouvent dans le rayon de portée d'une même station radiotélégraphique, ce qui, maintenant, avec la multiplicité des grands postes de T.S.F. existants, se trouve à peu près réalisé pour des points quelconques à la surface du globe.

L' "état" E de chaque garde-temps sur le temps du lieu au même instant t est déterminé par ailleurs à l'aide d'observations astronomiques qui font connaître l'état E_0 à une époque t_0 voisine de t et la "marche" m , d'où l'on déduit

$$E = E_0 + m(t - t_0).$$

Quand on n'a pas besoin d'une précision supérieure au dixième de seconde, ce qui est le cas notamment dans la détermination du "point" à la mer ou en exploration, on se contente d'utiliser les