On the Determination of Time at Different Observatories.
By Professor R. A. Sampson. (Plates 4, 5.)

Publication of times of receipt of wireless signals at different observatories permits the comparison of determinations of time at the observatories themselves, the signal itself being eliminated. And when several observatories are linked together in this way, whether by means of the same signal or of different signals, it becomes possible by a simple step to compare the time determination of each observatory with the mean of all, and so to assign to each with some considerable certainty the amount by which its own determination on any occasion was astray, at any rate in so far as fluctuations from a datum line are concerned. When this correction is accepted, it may be carried back in criticism of the Transit Circle observations, from which the time determination in question was drawn, for the purpose of searching for the origin of the discrepancies to which such observations are liable.

In 1920 November (*M.N.*, 81, 1) I published in summary the results of comparison *Paris – Edinburgh* from 1914 March to 1920 October, taken by the rhythmic signals. In the present communication I carry these forward and give more detail regarding them. The Astronomer Royal has now given us the Greenwich times of receipt of Paris signals from 1912 August and the Nauen (Berlin) and Annapolis (Washington) signals also from 1920 January to 1921 September. He has kindly supplied me with a copy of his MS., which permits the Greenwich numbers to be incorporated in the following comparisons. In taking these, various methods were employed, but evidence is given that the material is effectively homogeneous, and I accept it as such for the purpose of this paper. Further, with the courteous consent of the Director of Uccle Observatory, MM. Philippot and Moreau, who are in charge of the time service, have placed at my disposal in full detail the times of receipt at Uccle of Paris rhythmic signals from 1920 April onwards. There is thus material for comparison, which we may denote as P – E, G – P, G – B, G – W, and U – P, and by an arithmetical step which involves no obliteration of individual features we obtain B – M, E – M, G – M, P – M, U – M, W – M where $M = \frac{1}{6}(W + E + G + B + P + U)$ and B – M, . . . denotes the amount by which the time of Berlin, . . . is fast on the mean. Putting these numbers side by side, we have a chart in which the inequalities peculiar to each observatory in turn are present as to five-sixths of their full amount, qualified only by one-sixth of the mean of the other five. This is an effective isolation of each observatory, by which its own errors are assigned to it. No such step was practicable before wireless signals supplied the necessary links. For the period anterior to 1920 I have only G, P, E, compared with their own mean, so that the numbers are less discriminative; and in the case of a few short gaps in the 1920–21 series one or other of the observatories has had to be omitted and adjustment made accordingly. The method by which the numbers P – E are derived is explained in my paper of 1920 November along with other details which need not be repeated here. In the following pages "Plotted" Clock Errors are employed. I have rearranged the material and followed the Astronomer Royal in taking means for weeks for the later period and means for months for the earlier one.

Monthly Means.

$$M = \tfrac{1}{3}\{E + G + P\} \ (1914\text{–}1919).$$

$$M = \tfrac{1}{5}(B + E + G + P + W) \ (1920\text{–}1921).$$

N.B.—G − M positive indicates Greenwich time fast on the mean. (The unit is 0·01 sec.)

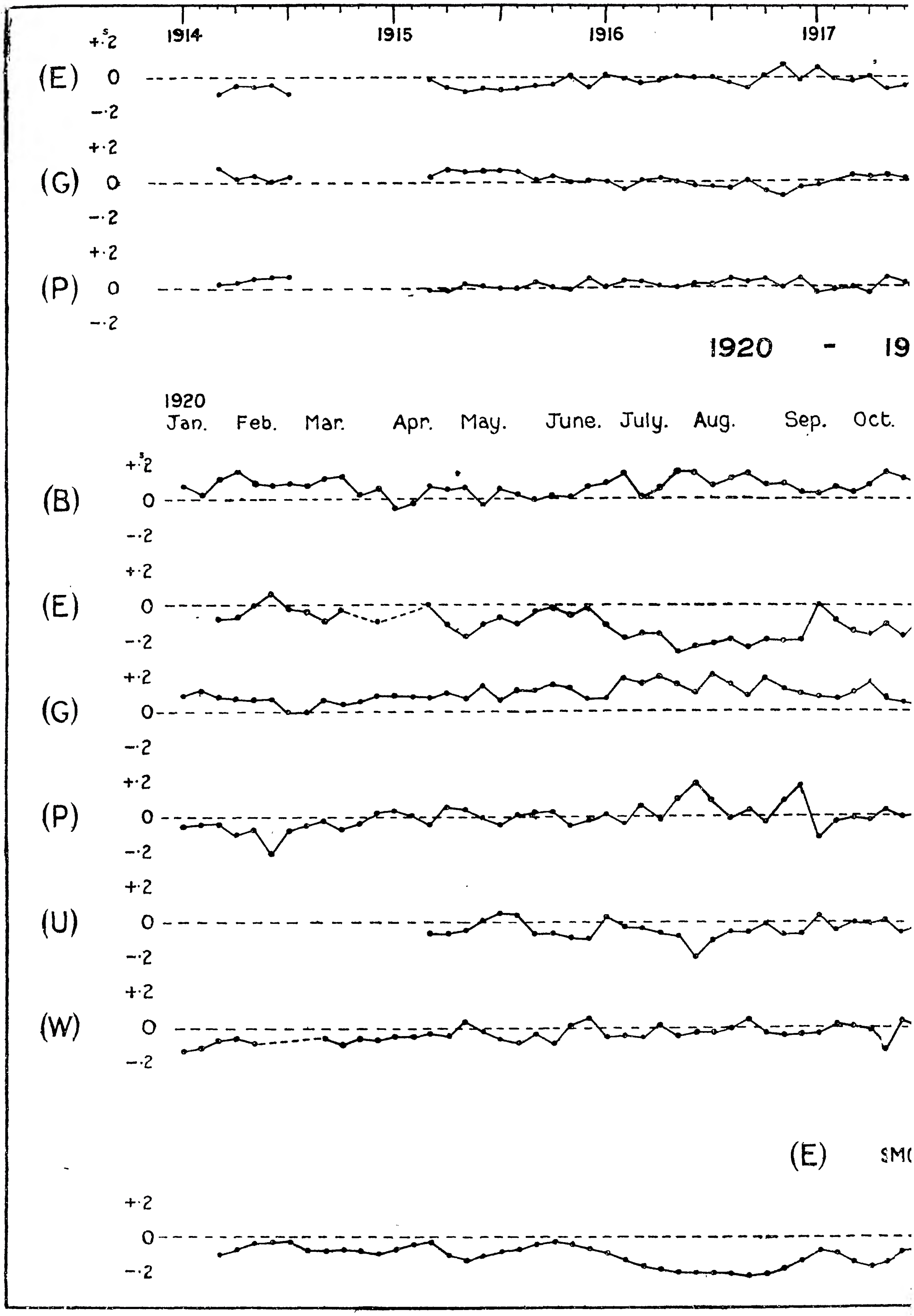
	Jan.	Feb.	Mar.	Apr.	May.	Jun.	Jul.	Aug.	Sept.	Oct.	Nov.	Dec.
1914												
E − M	...	...	−10	−6	−7	−6	−9	...	...	...	...	...
G − M	...	...	8	2	3	0	3	...	...	...	...	...
P − M	...	...	2	3	5	6	6	...	...	...	...	...
1915												
E − M	...	...	−2	−6	−8	−6	−7	−6	−4	−4	1	−6
G − M	...	...	3	7	6	6	7	6	1	3	0	1
P − M	...	...	−2	−2	2	1	0	0	3	0	−1	5
1916												
E − M	1	−1	−3	−2	0	0	0	−3	−5	1	7	−2
G − M	0	−4	1	2	1	−2	−2	−3	1	−5	−8	−3
P − M	0	4	3	1	0	2	2	5	3	5	0	5
1917												
E − M	5	−1	−2	0	−7	−5	−1	−3	−1	−7	−3	−3
G − M	−2	1	3	2	3	2	−2	1	1	6	4	5
P − M	−3	−1	0	−3	5	2	3	3	1	0	−2	−1
1918												
E − M	2	...	...	−7	−10	−12	−6	−11	...	−5	5	−4
G − M	−1	...	...	8	7	8	2	6	...	8	−3	2
P − M	−2	...	...	−2	2	4	4	6	...	−2	−3	1
1919												
E − M	−2	−4	−5	−8	...	...	−12	−6	−9	−7	−6	−6
G − M	−3	5	1	6	...	...	9	9	6	12	11	6
P − M	5	0	3	1	...	...	3	−2	2	−4	−4	−1
1920												
E − M	−6	1	−5	−3	−8	−7	−22	−25	−12	−15	−11	−7
G − M	10	5	5	7	11	9	15	15	9	8	7	10
P − M	−4	−9	−4	−1	−2	−3	5	0	1	−2	−5	−11
1921												
E − M	−4	−5	−3	−5	−16	−16	−17	−19	−16	...	...	...
G − M	12	7	10	8	12	10	15	13	13	...	...	...
P − M	−12	−4	−7	−2	2	0	−6	0	10	...	...	...

Weekly Means for 1920, 1921.

In forming these means the Greenwich numbers G – P, G – B, G – W from the Paris, Nauen, and Annapolis signals respectively are employed, in conjunction with P – E and U – P, which are derived from the receipt of the 10.30 a.m. Paris rhythmic series at Edinburgh and Uccle respectively. M denotes the mean $\frac{1}{6}$(B + E + G + P + U + W), but with adjustment when a gap occurs in the record of one or another observatory. (B) denotes B – M. (The unit is ·01 sec.)

Week commencing	(B).	(E).	(G).	(P).	(U).	(W).
1920 Jan. 5	8	...	9	− 5	...	−13
12	3	...	12	− 4	...	−11
19	12	− 7	8	− 4	...	− 7
26	17	− 6	7	−10	...	− 6
Feb. 2	9	0	7	− 7	...	− 9
9	8	7	7	−21	...	...
16	9	− 1	0	− 8	...	...
23	8	− 3	0	− 5	...	...
Mar. 1	12	− 8	7	− 3	...	− 6
8	13	− 2	5	− 7	...	− 9
15	3	...	6	− 4	...	− 6
22	6	− 9	9	2	...	− 7
29	− 6	...	9	3	...	− 5
Apr. 5	− 2	...	8	1	...	− 5
12	8	0	8	− 5	− 6	− 4
19	6	−10	10	5	− 6	− 5
26	7	−17	7	4	− 5	3
May 3	− 3	−10	15	− 1	1	− 3
10	6	− 6	6	− 5	5	− 7
17	3	−10	12	1	4	− 9
24	0	− 3	12	2	− 7	− 4
31	2	− 1	15	2	− 7	− 9
June 7	2	− 5	14	− 5	− 9	1
14	6	− 2	7	− 3	−10	5
21	9	−11	7	1	3	− 6
28	15	−18	19	− 4	− 4	− 5
July 5	1	−15	16	6	− 4	− 6
12	6	−15	20	− 3	− 6	1
19	16	−26	15	10	− 9	− 5
26	15	−23	11	19	−20	− 3
Aug. 2	8	−21	21	9	−11	− 3
9	12	−19	15	− 1	− 6	− 1
16	15	−24	9	4	− 6	4
23	8	−21	19	− 2	− 2	− 3

Week commencing		(B).	(E).	(G).	(P).	(U).	(W).
1920 Aug.	30	9	−20	14	9	−8	−4
Sept.	6	4	−19	10	18	−7	−4
	13	4	0	8	−13	3	−3
	20	7	−8	7	−3	−5	1
	27	4	−14	11	−1	−1	1
Oct.	4	8	−16	17	−2	−2	−2
	11	16	−11	7	4	0	−13
	18	12	−17	5	1	−6	4
	25	8	−8	4	1	−3	1
Nov.	1	13	−6	4	2	−9	−6
	8	11	−5	8	−2	−11	−2
	15	7	−10	12	−4	−4	...
	22	10	−10	10	−10	−9	12
	29	8	−16	2	6	−15	15
Dec.	6	5	−12	9	−5	−8	14
	13	13	−8	15	−10	−10	3
	20	11	−1	12	−14	−4	−2
	27	−6	3	19	−24	−4	12
1921 Jan.	3	−1	−1	17	−21	−5	13
	10	−5	8	12	−15	−9	12
	17	8	−1	13	−5	−18	4
	24	2	−8	13	3	−16	5
	31	2	−12	11	3	−19	16
Feb.	7	5	−2	11	−1	−11	6
	14	2	0	10	0	−16	4
	21	8	−1	6	−5	−6	...
	28	4	−6	13	−7	−2	...
Mar.	7	8	−1	11	−13	−3	...
	14	12	1	13	−11	−8	−8
	21	23	−7	0	0	−8	−14
	28	12	6	4	2	−1	−24
Apr.	4	13	−3	6	5	−9	−10
	11	11	0	9	2	−10	−14
	18	13	−3	12	−7	−5	−13
	25	16	−7	13	−6	−6	−12
May	2	16	−11	13	4	−8	−11
	9	7	−14	14	4	−7	−6
	16	13	−15	19	−1	−11	−7
	23	18	−18	12	5	−9	−5
	30	12	−13	9	3	−10	2



1914
1915
1916
1917
(E)
(G)
(P)
+·2
0
−·2
1920 - 19
1920
Jan.
Feb.
Mar.
Apr.
May.
June.
July.
Aug.
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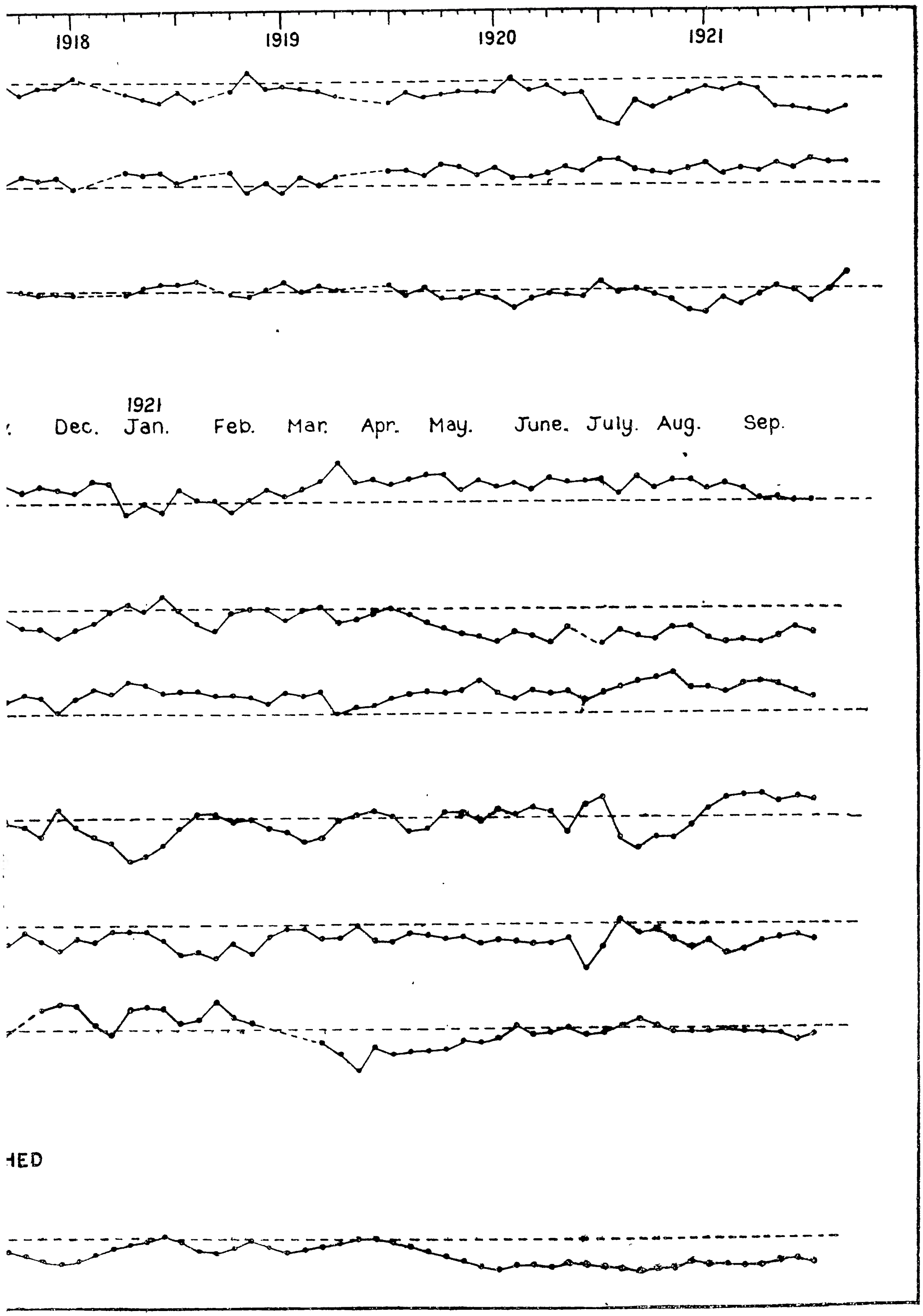
W.T. C(


1918
1919
1920
1921
1921
Dec. Jan. Feb. Mar. Apr. May. June. July. Aug. Sep.
HED

ɔarisons.

Week commencing	(B).	(E).	(G).	(P).	(U).	(W).
1921 June 6	8	−15	14	6	−11	−3
13	14	−19	12	4	−11	−2
20	12	−10	13	−8	−8	1
27	12	...	8	8	−25	−3
July 4	13	−20	14	12	−14	−2
11	5	−13	15	−11	2	3
18	15	−16	19	−16	−5	6
25	8	−17	21	−11	−3	2
Aug. 1	13	−11	23	−11	−9	−2
8	13	−10	15	−4	−14	−2
15	8	−17	15	6	−10	−2
22	11	−19	12	12	−17	−1
29	8	· 18	17	13	−15	−2
Sept. 5	2	−19	18	14	−10	−2
12	3	−16	17	10	−8	−3
19	1	−11	13	12	−6	−6
26	1	−14	10	10	−9	3

Examination of the charts (Plate 4) that express these data can hardly fail to cause considerable surprise, both on account of the sudden changes that take place in them, and, on the other hand, by the way a particular value is adhered to closely for some months and afterwards discarded for another different one. It must be remembered that each of these six observatories takes special pains to determine time. Each observatory has a different record which speaks for itself. Each one was quite unaware of peculiarities in its own record. As it happens, over this period the Paris record is perhaps the most disturbed. In order to verify that it did not unduly modify the mean with which the others are compared, I prepared a second trace in which Paris was eliminated, and the others were compared each with the mean of the remaining five. Certainly the traces become a little smoother, but the characteristic features of each persist. So little does it matter, that I have not thought it necessary to reproduce them. In particular, I would draw attention to the Annapolis (Washington) trace. Ten extremely smooth months are followed by three of rather pronounced oscillation; over the whole of this period the time shows a continuous drift, amounting in all to at least two-tenths of a second fast; then in two months more the whole of this accumulated drift is lost, and a new very consistent run begins and persists for six months, which also shows a definite drift in the original sense.

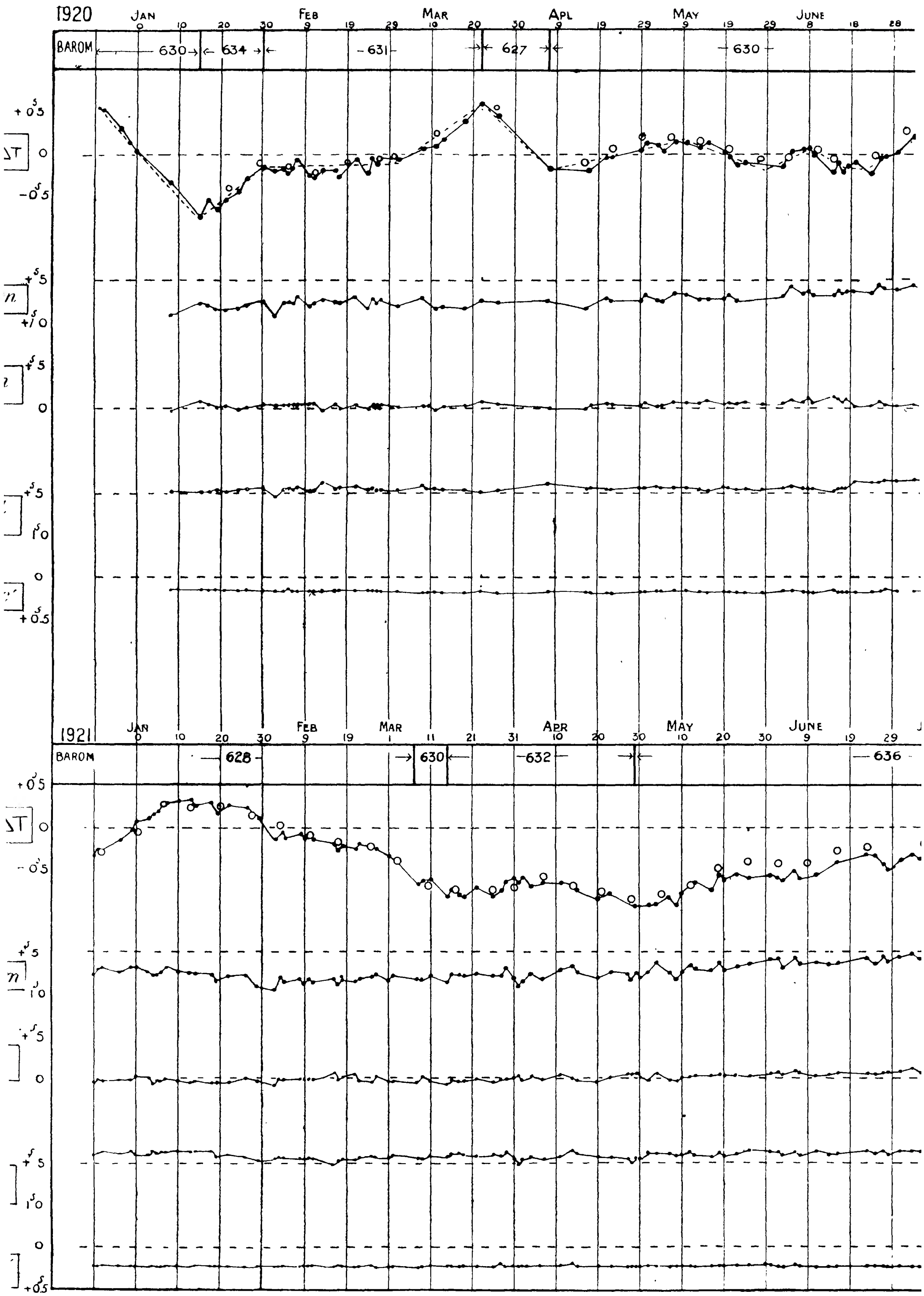
One serious conclusion I draw from these charts, namely, that they destroy the basis upon which all fine longitude work has hitherto been executed. Thus it has been the custom to take a pair of observers A, B from a place X, and a pair C, D from a place Y. During the first period A, C observe for time at X, while B, D observe at Y. After a certain interval the observation is reversed, A, C observing at Y, and B, D at X. The aim is, of course, to eliminate personality, but it appears to me that we can have no confidence that such would be the result, whether the two periods are separated by a short interval or a long one. As an illustration we have the recent Washington–Paris determinations, which gave respectively, with observers reversed, $5^h\ 17^m\ 36^s{\cdot}549$ and $36^s{\cdot}758$, a difference for which no explanation could be assigned.

I now turn to the Edinburgh record in particular, to examine what light it throws upon the faults of time determination, whether suspected or unknown.

I have added a smoothed version of the record, to bring out its systematic features. They might be described generally as a pronounced seasonal fluctuation with a minimum about July or August and a maximum about February.

To show how far this is a persistent feature, we have the monthly means for the period 1914–1921 for Greenwich, Paris, and Edinburgh alone. There is some evidence of such a fluctuation in 1919 and 1918; but before that, though possibly present, it is much suppressed. I shall refer further below to this base fluctuation and its possible source.

In Plate 5 I give also unsmoothed the results of the actual time determinations in the form of the ascertained correction required each night by clock Riefler. It is the practice to let this clock run between half a second fast and half a second slow, altering the air pressure from time to time to make the necessary change of rate. The barometric reading is therefore marked as well. The time comparisons brought into the foregoing discussion are "plotted" from Riefler's chart by representing the record by a series of straight lines, as described in my paper in *M.N.*, 1920 November. This crude method is designed, not to secure the closest agreement with the observations, but to leave outstanding and unsuppressed all discordances from the indications of a constant rate for further examination. It will be seen upon examination of these straight lines that upon unfavourable occasions some choice in their position is possible, and I would not say that occasionally a weekly mean so derived may not be off the truth by an amount running up to a limit of, say, $0^s{\cdot}1$. Therefore I do not attach much significance to the small notches in the recorded trace derived from W.T. comparisons, since they could all be removed by an allowable variation of the method of plotting. But the fluctuations which persist when the record is smoothed stand upon a different footing. Small circles on the chart mark the path which the plotted times should occupy in order that the differences *Edinburgh–Mean* may be reduced to zero. It will be seen that the departures are of a character which cannot be explained away by any ordinary theory of accidental error. And, as far as systematic error goes, the system on which the Edinburgh times were found was most carefully kept unvaried. The fluctuation is seasonal. I have at present no conclusion to offer as to its source. The instrumental constants, level, etc., show a similar seasonal fluctuation which would appear in the chart if they fitted badly to the observations. But it is hard to suppose that they fit badly, systematically, for several months together and in the same sense.



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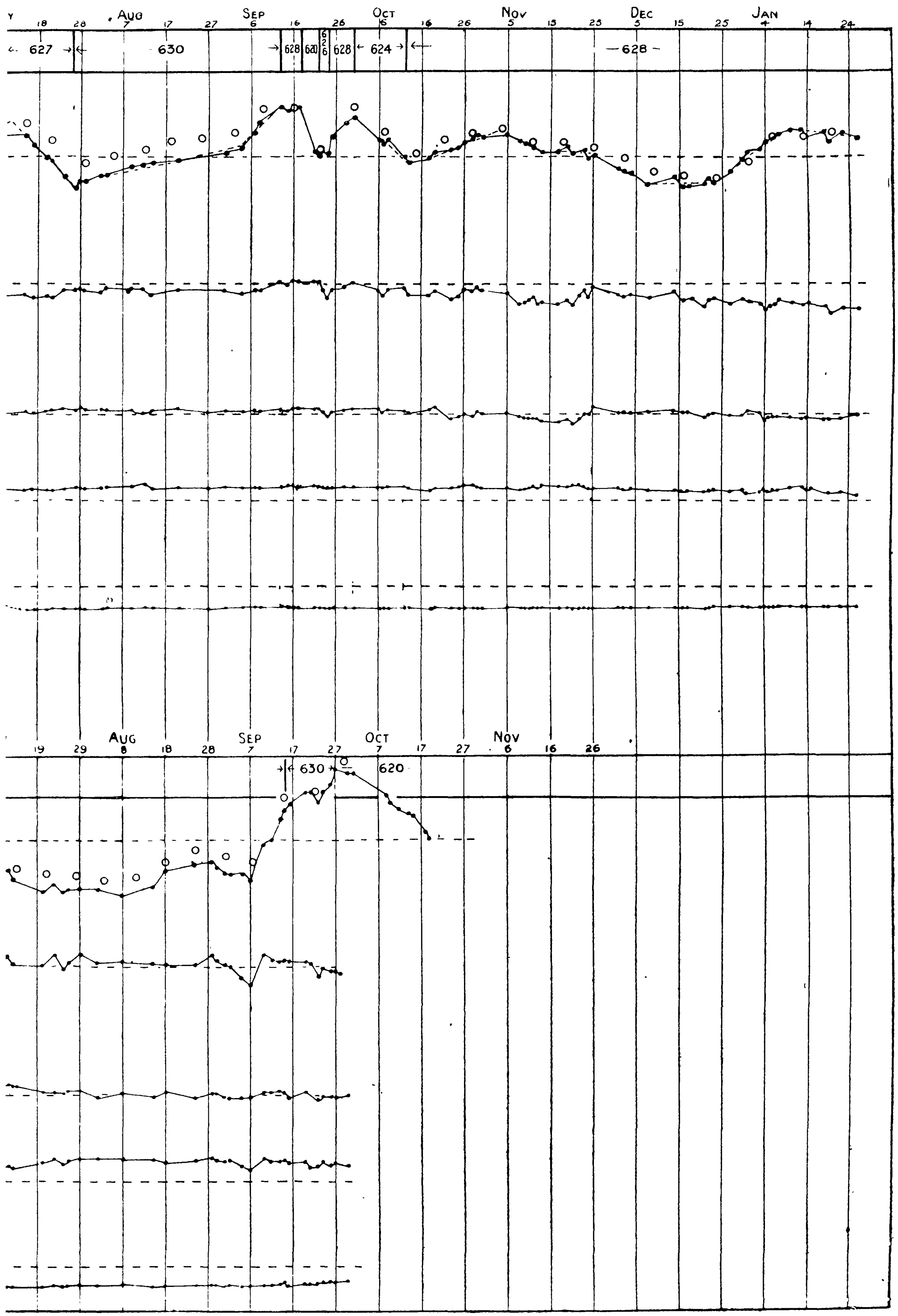

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In connection with this subject I may add the preliminary outcome of an inquiry as to how far one can assign the sources of change that are indicated in a chart of clock corrections. Below the clock corrections I have charted the values of m, n, l, c' employed in deriving them. If these were a correct fit to the observations, there should be no sign of correlation between their changes and the changes in the time trace. This is by no means the case. By comparing the detailed features it becomes perfectly clear that the irregular changes in m eliminate themselves very imperfectly from the trace of clock correction; they are, in fact, evidently responsible for most of its erratic changes. On examining further, it is seen that most of these changes in m are due to changes in n; that is, to observations of polar stars. Effects of changes in l, c are also to be traced, but much less frequently. It will, of course, be remembered that the changes shown in the traces of n, l, c' require to be increased by factors approximately 1·5, 1·8, 4·0 respectively before they compare with the changes in ΔT. If this is accepted, it leads to two important conclusions: (1) sudden erratic changes, which appear in the ascertained clock error and are reversed shortly after, have nothing to do with the clock or chronographic system, when these are properly managed, but are introduced into the record by the observations; and (2) such erratic features are due, as a rule, to the adopted constants for correcting the instrumental errors, and not to the time stars. Compare a similar conclusion reached by M. Bigourdan, "Corrections des Signaux Horaires," *Bull. Astron.*, 1 (1921), 260.

The time stars will differ by considerable amounts from one another; it may be taken that ten stars will not agree much within a range of $0^{s}\cdot1$, but the mean of their indications will contain little of systematic residual.

It is natural to inquire how far the atmosphere can be held responsible for these irregularities, by means of some anomaly in refraction such as a lateral component. The theory of refraction assumes that the strata of equal density in the atmosphere have a common normal which agrees with the direction of the geographical zenith. This corresponds to the case of an undisturbed gaseous envelope, in which the layers of equal density, pressure, and temperature coincide. In actual circumstances it must often be far from the truth, and the strata of equal density must be oblique to the vertical and in motion. The ray from a star, as it penetrates any stratum, will be refracted towards the zenith of that stratum by an amount $\delta\mu \times \tan$ (zenith distance) where $\delta\mu$ is the increase of refractive index, which is proportional to the increase of density, at the stratum. Now, disregarding at present anything except a deviation of the zenith in the line E – W, and supposing the star to be observed in the meridian, if the ray encounters a succession of strata progressively tilted over more towards the East, from the true geographical zenith Z to the surface inclination Z′, the successive elements of refraction will be the same in amount as in the ordinary theory, but will be directed in turn to points $Z_1, Z_2, \ldots$ between Z and Z′. If $\sigma, \sigma_1, \sigma_2, \ldots \sigma'$ be the successive directions of the ray, and if $\sigma\sigma'$ produced meets ZZ′ in ζ, then the effect of the displacement of zenith will be the same as if refraction took place, not towards Z, but towards ζ, where we need not distinguish between the angular distances σZ, $\sigma\zeta$.

If the image of the star is thus displaced to σ', the lateral amount will be $\sigma\sigma' \sin Z\sigma\zeta$

$$= 58''\cdot 2 \tan \sigma\zeta \sin Z\sigma\zeta = 58''\cdot 2 \sin Z\zeta \sec \sigma\zeta$$
$$= \rho \sec (\text{zenith distance}) \text{ say, where } \rho = 58''\cdot 2 \sin Z\zeta.$$

To give a lateral displacement of the order of 1″, the distance of ζ from Z would require to be about one degree.

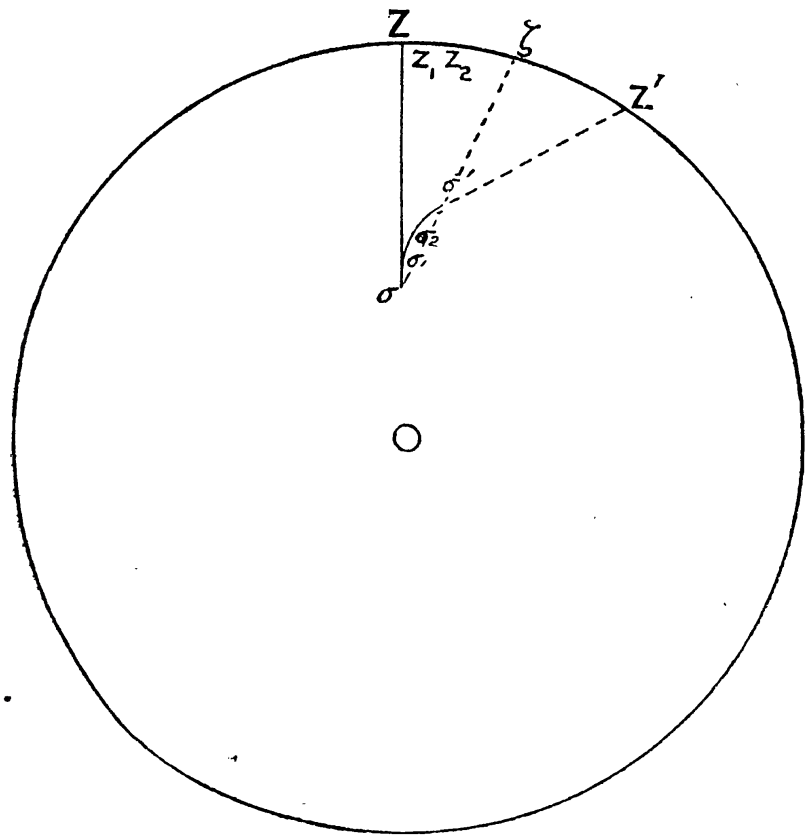

FIG. 1.

Now imagine that collimation and level are determined in the usual way and, let us say, corrected, and that the azimuth correction also happens to be zero, so that the path of the collimation axis of the telescope, which may be taken as a fixed spider thread in the field of view, actually describes the line PZQ, which is the geographical meridian. But in observing a polar at the catalogue time, when it is actually at σ_3 on the meridian, it is seen at σ_3' displaced towards the false zenith, and this is interpreted in the absence of other evidence as a fault in azimuth. Such is the actual process by which the constant n is derived, which is carried into the reduction of observations and used in particular to deduce m, subject to the condition that l, the level error, is zero, as here, or at least already known. With respect to the false meridian so introduced, the effect upon the transit of the stars will be twofold, first because a false azimuth correction is applied, and second, because the visible transit which is accepted as the observation is that of σ' and not that of σ.

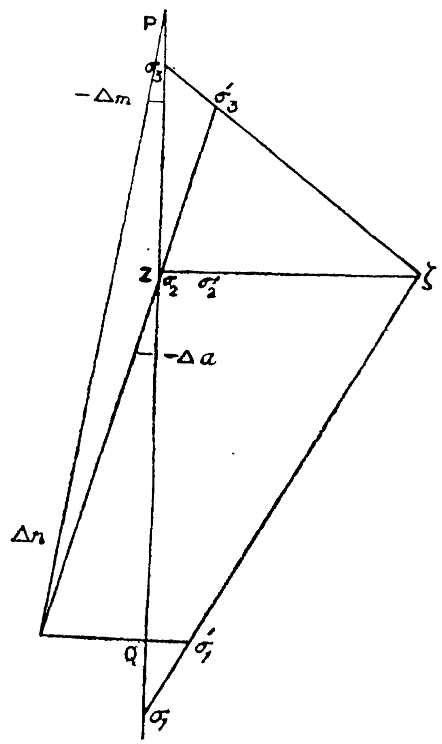

FIG. 2.

If n is taken from a close polar star it is easily seen that

$$-\Delta a = \rho / \sin\phi \cos\phi, \quad \Delta n = \rho/\sin\phi, \quad -\Delta m = \rho/\cos\phi.$$

Again, for an equatoreal star

$$Q\sigma_1' = \rho/\cos\phi.$$

That is to say, if lateral refraction were present, but the solutions were conducted in the usual way, ignoring it, an equatoreal star would seem to transit late by $2\rho \sec\phi$ (in arc) and a zenith star by $\rho\sec\phi$, being half the same amount. Or again, if ΔT_Q is the clock correction derived from equatoreal stars, and ΔT_Z that derived from zenith stars, the clock correction cleared from the effect of lateral refraction would be $2\Delta T_Z - \Delta T_Q$. The slower movement of zenith stars makes this last formula of little service, but by massing a number of observations together it may be seen whether, and by what amount, ΔT_Z stands out generally from ΔT_Q. With this object I have had stars which transit near the zenith regularly observed for clock error along with stars of low declination. The outcome is quite clear. About four times out of five the high stars indicate the clock slow relative to the equatoreal stars. The means, month by month, are as follows:—

Clock Error—High Stars minus Low Stars.

	J.S.	R.W.W.	E.A.B.	All.	No. of Obs.	Range.
	s	s	s	s		
1920 May . . .	+·03	+·03	+·04	+·03	11	·08
June . . .	·01	·02	·03	·02	14	·09
July . . .	·03	·02	·03	·03	9	·06
August . .	...	·01	·01	·01	6	·02
September . .	·01	+·02	·01	·01	19	·05
October . .	·02	−·01	·02	·01	15	·08
November . .	...	·00	·02	·01	18	·09
December . .	...	+·01	·03	·02	18	·12
1921 January . .	...	·01	·01	·01	17	·07
February . .	...	·01	·01	·01	23	·08
March . .	...	·00	·04	·02	17	·08
April . . .	+·03	·03	·00	·01	11	·07
May . . .	...	·01	·01	+·01	10	·05
June . . .	−·01	+·01	...	−·01	9	·06
July . . .	...	−·01	·00	·00	10	·07
August . .	...	+·01	·02	+·01	7	·05
September . .	·00	·00	·01	·00	15	·05

The observations are surprisingly steady.

The average range, measuring the difference between the greatest positive and lowest negative indication of each month, is $^s·07$, and is adhered to pretty closely.

The general mean lag for 229 observations is $^s·012$.

If this is interpreted as a lateral displacement of the atmospheric zenith, it gives $\rho = -0''·1$ or ζ permanently west of Z, the geographical zenith, by 6′. The fluctuations from one set of observations to another may reach, say, three times this quantity. These are not negligible amounts; whatever their origin, they would seem sufficient to account for the internal discrepancies of a single set of observations; but they appear to me to exclude most definitely any supposition as to atmospheric effect which would explain the systematic fluctuations, of amplitude about $0^s·2$, between the Edinburgh times and the mean of the six observatories.

An accidental displacement East in a polar star, from whatever cause it may arise, of amount $0''·3$, interpreted as a correction to the instrumental constants, would give

$$\Delta a = -0''·6, \quad \Delta n = +{}^s·024, \quad \Delta m = -{}^s·036,$$

and the whole determination of clock correction would be affected approximately by the amount of $-\Delta m$.

That anomalous refractions do occur, anyone can witness who watches the stars with a micrometer on an unsteady night. It may be verified by taking a photographic trail with a fixed telescope. And in fact it has been referred to frequently (cf. "Observations made with the Cookson Floating Zenith Telescope, 1911–18, Royal Observatory, Greenwich").

But there must be great doubt whether a displacement, even so large as $6'$, can be entertained between Z and ζ. The barometric gradient in intense storms is about 2·5 mm. of mercury per degree of latitude, which amounts to a gradient of $1'$ only in the height of the homogeneous atmosphere. We can hardly suppose a steeper gradient could maintain itself for any length of time or over an extended area. Probably the constant observed difference between high and low stars is no more than a form of personality belonging to the three observers in common, unless indeed it belongs to Boss's P.G.C., to which the places are reduced.

So far as this discussion leads to a definite conclusion it is this—the weakest elements in time determinations are the constants representing the instrumental errors. The clock and chronographic system can be brought to a point of order at which they hardly enter, and it is not difficult to take enough time stars to cancel the accidental error that may attach to any one. If the reduction constants could be made more reliable, a great improvement in smoothness would result. But what steps to take to avoid or to cancel the errors attaching to these constants is a much larger question, upon which I do not touch at present.

But even if another treatment of the instrumental constants reduced time determinations from day to day to complete smoothness, it would still leave unexplained, as far as one can see, the seasonal fluctuation shown by it with respect to the mean of the other observatories. It will be a matter of great interest to see how far the individual features in the traces for different observatories persist or disappear in subsequent years. The fluctuation in the Edinburgh times appears to be seasonal, and I recall that an examination of the temperature coefficients of the Edinburgh Transit Circle (*M.N.*, 1914 December) showed that when the superficial temperature changes were removed, very smooth curves of basic changes emerged which showed seasonal variation in both level and instrumental azimuth, presenting a maximum about February and a minimum about July or August; that is to say, with a lag as though controlled by the temperature a few feet below the surface of the rock. Further, the stellar azimuth derived from observation of polars showed a fluctuation of the same kind with respect to the instrumental azimuth, which was derived from settings upon the N. and S. collimators. These facts are suggestive, but I cannot at present show any reason why they should account for a parallel fluctuation appearing in the time record.

The Use of a Graduated Wedge in Stellar Classification and Parallax Work. By Major William J. S. Lockyer, M.A., Ph.D. (Plate 6.)

The classification of stars according to their spectra is based on the consecutive appearance and disappearance of metallic, ionised, and gaseous lines as the stars increase or decrease their temperature. This system has proved most useful up to the present time, but there remains still a certain amount of uncertainty in the determination of spectral types. A more refined method recently adopted is that in use at the Mount Wilson Observatory, which consists in determining the *numerical* relationships between the intensities of the lines compared, thus providing a more accurate basis for the classification. This method has been recently fully set out,* and need not be described here.

In the measurement of the intensities of the spectral lines, pairs of lines, fairly close together, are selected, the negatives being placed in a stereo-comparator.

Again, the spectroscopic method of determining stellar parallaxes also involves the measurement of the relative intensities of close pairs of lines in the spectrum, for it has been found that the absolute magnitudes of the stars are closely associated with the relative intensities of the spectral lines.†

The present work of the Norman Lockyer Observatory is now devoted to both of these researches by the above-mentioned methods. The Observatory, however, does not possess a stereo-comparator or similar instrument, but has one of the small standard photo-measuring micrometers (Model 13) by Messrs. Adam Hilger. The question arose how to adapt this machine for measuring the relative intensities of the lines to which reference has been made above.

It occurred to the writer that these differences in intensities of close pairs of lines could be determined by means of a graduated neutral-tinted wedge, and, with this idea in mind, a stepped wedge was built up by cutting a homogeneously exposed Kodak film into graduated size strips and placing them one over the other after the principle of an echelon grating.

With this primitive apparatus numerous determinations of the relative intensities of pairs of lines in a spectrum were made. The results were so promising that an order was given to Messrs. Adam Hilger to supply a more refined article.

The wedge was soon received. Its gradated area measured $3{\cdot}8 \times 1{\cdot}95$ cm. It was found, however, that its densest portion was not quite opaque enough to obliterate the strongest lines in some of the spectra measured, so it was determined to procure a further one with a steeper gradient of density.

Both these wedges have now been tested, and they have given such satisfactory results that the researches referred to above have been commenced.

* *Communications to the National Academy of Sciences, Washington*, No. 23, p. 113, 1916, by Walter S. Adams.

† *Ibid.*, No. 24, p. 118, 1916, by Walter S. Adams.