(10) Adams, Joy, and Sanford, *Publ. A.S.P.*, **36**, 137, 1924; Joy, *Ap. J.*, **64**, 287, 1926; Schilt, *Ap. J.*, **64**, 215, 1926; Adams, Joy, Strömberg, and Burwell, *Ap. J.*, **53**, 94, 1921.
(11) *Annals of Harvard Coll. Obs.*, **84**, 157, 1920.
(12) *Communic. Nat. Ac. Sc. Mt. Wilson Obs.*, No. **63**, 1919.
(13) *M.N.*, **88**, 548, 1928.
(14) *M.N.*, **86**, 328 and 444, 1926.
(15) *Ap. J.*, **49**, 189, 1919.
(16) *Publ. A.S.P.*, **39**, 160, 1927.
(17) It has previously been shown that all real double stars of spectral type B have periods longer than $1^{d}\cdot3$ (*M.N.*, **86**, 63, 1925; *Ap. J.*, **66**, 117, 1927).
(18) *Publ. A.S.P.*, **32**, 315, 1920.
(19) The eclipsing variable V Puppis (Sp. B1p; P = $1^{d}\cdot454$; K = 302 km./sec.) closely resembles our hypothetical B star. According to Hellerich (*loc. cit.*) the rotational velocity for the brighter component of this star is 276 km./sec., and the corresponding line-width should be 7·9 Å. Miss Maury (*Harvard Annals*, **84**, 186, 1920) has measured the widths of several lines in this star and has found a mean of 5·78 Å.—an unusually high value for a B-type star.
(20) *Ap. J.*, **54**, 91, 1921.
(21) Eddington, *The Internal Constitution of the Stars*, p. 353, 1926.
(22) *Ap. J.*, **59**, 30, 1924.
(23) *Zeitschrift für Physik*, **44**, 793, 1927; **46**, 765, 1928.
(24) *Ap. J.*, **21**, 1, 1905.
(25) v. Klüber, *Zs. f. Phys.*, **44**, 481, 1927; Unsöld, *Zs. f. Phys.*, **44**, 765, 1928; Payne, *Proc. Nat. Ac. Sc.*, **14**, 399, 1928.
(26) The numerical results cannot be directly compared with those of Carroll, since he uses spectral lines of zero intensity, whereas our lines are taken from observational data, and their depths are very moderate
(27) *M.N.*, **86**, 63, 1925.
(28) In fig. 4 the stars α Camelopardalis and ξ Persei, the orbits of which are based on the detached calcium lines, have been excluded. These stars are peculiar and their velocity curves may not be due to orbital motion. Both have wide lines and small values of K. They would, of course, in no way contradict the above statement, even if they had been included. On the other hand, we have included the two stars V Puppis and μ^1 Scorpii, based on Miss Maury's measurements.
(29) *L.O.B.*, **11**, 141, 1924.
(30) *Veröff. der Berlin-Babelsberg Sternwarte*, Band v., Heft 6.

September 1928.

Shortt Clocks and the Earth's Rotation. By J. Jackson, M.A., D.Sc.

(*Communicated by the Astronomer Royal.*)

In *M.N.*, **88**, 465, a description was given of the performance of the clocks Shortt 3 and Shortt 11 from the time of their installation, 1924 November and 1926 May respectively, till the end of 1927. It was there shown that the going of these clocks over long intervals of time far surpassed that of earlier types of clock. The accuracy of the clocks indeed was such that it was found advisable to allow for the non-uniformity of sidereal time resulting from nutation—the principal term of coefficient $1^{s}\cdot06$ and period 18·6 years indeed varies so slowly that it causes no trouble, but the term of coefficient $^{s}\cdot08$ and six-month period is large enough and varies rapidly enough to be considered. The correction required to the clock time to reduce it to true sidereal time was therefore written in the form

$$\alpha + \beta t + \gamma t^2 + \delta\int(\mathrm{T} - \mathrm{T}_0)dt + \text{Nutation}.$$

The coefficient γ was found to be nearly steady throughout 1927, with a value of $1^s.84$ when t was measured in units of 100 days for Shortt 3; the value for Shortt 11 appeared to be of a similar amount, but to be gradually decreasing. The temperature coefficient, though small, was clearly defined of amount $^s.003$ per day per 1° F. For Shortt 3, the correction was given in formula (3), p. 469, as

$$7^s.775 + 8^s.3636t + 1^s.8400t^2 + 0^s.298\int(\mathrm{T} - 54^\circ.8)dt + \text{Nutation}$$

where t is measured in units of 100 days from 1927 December 1^d 0^h G.C.T.

The following table is a continuation of Table III. on p. 469, of vol. lxxxviii., and gives a comparison of the time determinations made with the reversible transit B in 1928 with the above formula which was deduced from observations made from 1927 March to December. The observations for the first half of the year were made by myself and for the second half by Mr. Finch. Our relative personal equation is negligible. It should be recalled that the formula satisfied the observations exactly on 1926 July 19, but left a residual of $0^s.76$ in October of that year. It satisfied the observations from 1927 March till December to $0^s.1$. Comparison with formula (3)′, which does not materially differ from (3), is not given here. The last column shows the number of stars observed south and north of the zenith.

TABLE I.

Comparison of Clock Errors observed with Transit B with those predicted in M.N., **88**, 469, *from Observations made from March to December* 1927. (*Multiples of* 10^s *have been dropped as they arise in the 3rd and 4th columns.*)

1928.		Temperature Integral.	Formula (3).	Obs.	O − C₃.	No. of Stars.
		s	s	s	s	
Jan.	6	+ ·036	+0·180	+0·176	−·004	4, 2
	19	·051	1·488	1·482	−·006	6, 4
Feb.	1	·060	2·869	2·838	−·031	6, 4
	13	·071	4·180	4·102	−·078	6, 4
	20	·078	4·994	4·897	−·097	5, 3
	28	·085	5·891	5·804	−·087	3, 3
Mar.	15	·074	7·773	7·746	−·027	4, 5
	17	·073	8·022	7·956	−·066	3, 3
	28	·074	9·376	9·252	−·124	3, 3
	30	·077	9·669	9·529	−·140	4, 2
Apr.	2	·082	0·027	9·832	−·195	4, 3
	18	·085	2·107	1·808	−·299	5, 6

TABLE I.—*continued.*

1928.	Temperature Integral.	Formula (3).	Obs.	O–C$_3$.	No. of Stars.
	s	s	s	s	
Apr. 24	·078	2·919	2·582	–·337	7, 5
28	·079	3·490	3·164	–·326	3, 3
May 9	·090	5·027	4·694	–·333	6, 5
18	·091	6·321	6·023	–·298	7, 4
24	·084	7·241	6·929	–·312	6, 4
30	·083	8·121	7·849	–·272	4, 3
June 6	·105	9·235	9·040	–·195	3, 2
11	·116	0·060	9·896	–·164	4, 3
15	·122	0·652	0·491	–·161	5, 3
22	·120	1·832	1·745	–·087	5, 4
27	·119	2·596	2·530	–·066	2, 1
July 1	·123	3·262	3·289	+·027	4, 3
7	·134	4·275	4·295	+·020	3, 1
11	·150	4·944	4·996	+·052	5, 1
13	·159	5·298	5·351	+·053	5, 1
17	·186	6·029	6·096	+·067	3, 2
21	·229	6·747	6·836	+·089	6, 4
25	·282	7·480	7·562	+·082	6, 5
Aug. 2	·425	9·020	9·045	+·025	3, 1
8	·533	0·153	0·049	–·104	6, 4
9	·549	0·349	0·303	–·046	3, 2
13	·615	1·147	1·068	–·079	3, 3
17	·684	1·921	1·742	–·179	4, 3
20	·738	2·502	2·292	–·210	5, 2
27	·867	3·907	3·621	–·286	5, 4
Sept. 1	·962	4·916	4·605	–·311	6, 5
4	1·016	5·502	5·160	–·342	5, 3
7	1·072	6·123	5·754	–·369	5, 4
11	1·146	6·968	6·607	–·361	5, 3
18	1·274	8·371	7·954	–·417	7, 3
25	1·398	9·881	9·382	–·499	5, 3
30	1·472	0·867	0·336	–·531	5, 2
Oct. 4	1·532	1·721	1·181	–·540	6, 3
9	1·603	2·786	2·218	–·568	3, 3
11	1·638	3·204	2·629	–·575	5, 4
17	1·763	4·552	3·974	–·578	5, 3
21	1·857	5·442	4·908	–·534	5, 3
25	1·951	6·322	5·850	–·472	6, 3
28	2·022	7·011	6·535	–·476	5, 3
Nov. 4	2·188	8·633	8·220	–·413	5, 4
8	2·282	9·563	9·152	–·411	5, 3

TABLE I.—*continued.*

1928.	Temperature Integral.	Formula (3).	Obs.	$O-C_3$.	No. of Stars.
	s	s	s	s	
Nov. 17	2·495	1·682	1·308	−·374	5, 4
23	2·622	3·137	2·732	−·405	5, 3
24	2·646	3·339	2·930	−·409	2, 1
Dec. 4	2·884	5·836	5·447	−·389	2, 1
7	2·956	6·521	6·177	−·344	5, 3
9	3·023	7·023	6·642	−·381	6, 3
14	3·122	8·337	7·993	−·344	5, 4
17	3·192	9·030	8·703	−·327	2, 2
26	3·406	1·304	0·953	−·351	5, 3
29	+3·479	+2·080	+1·734	−·346	4, 3

The residuals which are shown in fig. 1 reached a maximum of $0^s{\cdot}58$ in October, *i.e.* fully nine months later than the last observation used in the formation of formula (3). The turning points in the errors from prediction were

	s
April 24 . .	− ·337
July 21 . .	+ ·089
October 17 . .	− ·578.

The value on December 31 was $-{}^s{\cdot}346$, so that there was a total fluctuation in the year of $1^s{\cdot}66$. This gives an average departure of the predicted from the actual rate, independently of sign, of ${}^s{\cdot}0046$ daily, the greatest being about ${}^s{\cdot}01$ in August and September.

The residuals between the observed and predicted clock errors show oscillations of a kind which clearly cannot be explained by slight adjustment of the constants, but it was thought worth while to recompute them and at the same time those for Shortt 11, which has constants remarkably similar to those for Shortt 3. Consequently the residuals from the old formula for the period 1927 March to 1928 December were grouped together in intervals of somewhat less than a month and analysed. The resulting corrections to formula (3) are :—

Shortt 3.

$$-{}^s{\cdot}064 - {}^s{\cdot}0108t - {}^s{\cdot}0144t^2 - {}^s{\cdot}0158\int(T - 54^\circ{\cdot}8)dt.$$

Shortt 11.

$$-1^s{\cdot}007 + {}^s{\cdot}2761t - {}^s{\cdot}0858t^2 - {}^s{\cdot}0456\int(T - 54^\circ{\cdot}8)dt$$

where t is computed as previously from 1927 December $1^d\ 0^h$ G.C.T.

Applying these to formula (3) above we get the following corrections to the two clocks.

Shortt 3.

$$7^s{\cdot}711 + 8^s{\cdot}3528t + 1^s{\cdot}8256t^2 + 0^s{\cdot}2822\int(T - 54^\circ{\cdot}8)dt + \text{Nutation}.$$

Shortt 11.

$$6^{s}{\cdot}768 + 8^{s}{\cdot}6397t + 1^{s}{\cdot}7542t^2 + 0^{s}{\cdot}2524\int(\mathrm{T} - 54^{\circ}{\cdot}8)dt + \text{Nutation.}$$

In these formulæ t is computed from 1927 December 1.

If we bring forward the epoch by 500 days to 1929 April 14, and at

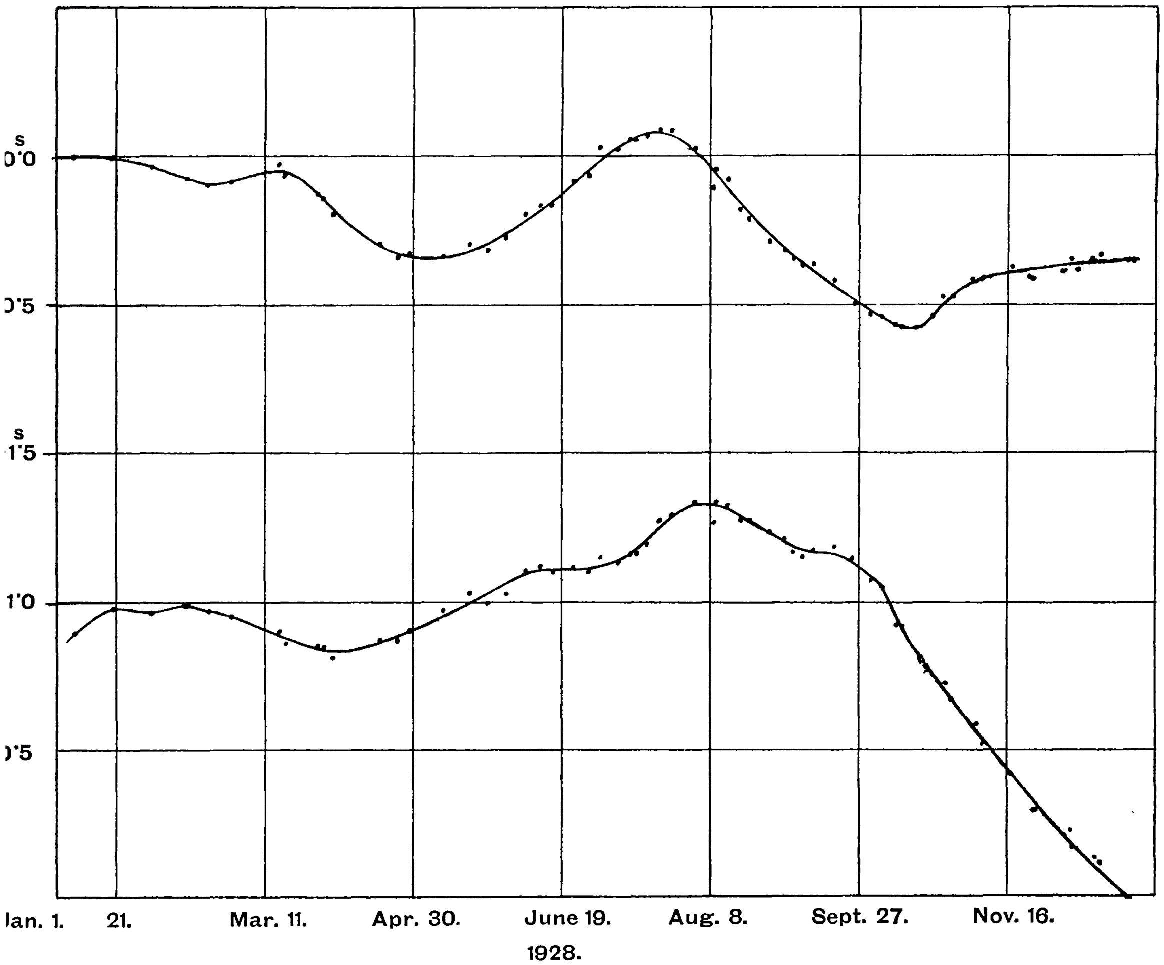

Fig. 1.—Comparison of the corrections to Shortt 3 and Shortt 11 with the formula (3) based on observations 1927 March to December referred to Shortt 3. The interval between the vertical lines is 50 days, between the horizontal lines 0·5 seconds.

the same time alter the standard temperature to 62°·8 F., at which the clock room is now controlled, we obtain the following equations numbered consecutively to those in the previous paper.

Shortt 3.

$$(5)\quad 100^{s}{\cdot}797 + 28^{s}{\cdot}8656t + 1^{s}{\cdot}8256t^2 + 0^{s}{\cdot}2822\int(\mathrm{T} - 62^{\circ}{\cdot}8)dt + \text{Nutation.}$$

Shortt 11.

$$(6)\quad 98^{s}{\cdot}905 + 28^{s}{\cdot}2002t + 1^{s}{\cdot}7542t^2 + 0^{s}{\cdot}2524\int(\mathrm{T} - 62^{\circ}{\cdot}8)dt + \text{Nutation.}$$

A comparison of these formulæ with the observed values is given in Table II.; the data before 1927 March depend on observations with the large transit circle, and were not used in the analysis.

Table II.

Comparison of Clock Corrections for Shortt 3 and Shortt 11 with Formulæ (5) and (6) respectively. (Observed—Computed.)

1926/1927.	No. 3.	No. 11.
	s	s
July 9	+0·41	..
19	+ ·20	..
29	− ·03	..
Aug. 8	− ·20	+7·94
18	− ·29	7·42
28	− ·41	6·87
Sept. 7	− ·47	6·31
17	− ·49	5·87
27	− ·56	5·26
Oct. 7	− ·58	4·72
17	− ·59	4·19
27	− ·58	3·70
Nov. 6	− ·60	3·09
16	− ·58	2·56
26	− ·52	2·18
Dec. 6	− ·44	1·91
16	− ·37	1·69
26	− ·38	1·41
Jan. 5	− ·32	1·13
15	− ·26	0·96
25	− ·21	0·77
Feb. 4	− ·09	0·69
14	− ·05	0·52
24	− ·06	0·36
Mar. 6	+ ·01	0·26
16	+ ·01	+0·12
26	+ ·13	..
Apr. 5	+ ·09	..
15	+ ·03	..
25	+ ·03	..
May 5	−0·05	..

1927/1928.	No. 3.	No. 11.
	s	s
Mar. 19	+0·026	+0·084
May 26	+ ·044	− ·350
June 12	− ·018	− ·354
July 23	− ·090	− ·133
Aug. 16	− ·046	+ ·190
Sept. 12	+ ·026	+ ·452
Oct. 2	− ·041	+ ·520
20	− ·051	+ ·401
Nov. 10	+ ·047	+ ·280
Dec. 12	+ ·070	− ·051
29	+ ·069	− ·161
Jan. 13	+ ·069	− ·152
Feb. 7	+ ·018	− ·151
24	− ·004	− ·189
Mar. 16	+ ·050	− ·292
30	− ·050	− ·350
Apr. 23	− ·207	− ·313
May 17	− ·185	− ·197
June 8	− ·054	− ·101
27	+ ·114	− ·053
July 12	+ ·217	+ ·007
25	+ ·259	+ ·173
Aug. 10	+ ·139	+ ·254
21	+ ·010	+ ·257
Sept. 6	− ·082	+ ·246
24	− ·184	+ ·296
Oct. 10	− ·237	+ ·161
25	− ·135	+ ·045
Nov. 10	− ·004	− ·098
Dec. 2	+ ·067	− ·182
21	+0·154	−0·222

It will be seen that the residuals are very similar to those deduced from formula (3). For Shortt 3 there is a range in the residuals from $+^{s}\cdot26$ to $-^{s}\cdot24$ between 1927 March and 1928 December, but when extrapolated backwards there was a residual of $0^{s}\cdot60$ in 1926 October, and the daily rate in 1926 July was erroneous by $^{s}\cdot022$. The performance of Shortt 11 was not so good, the range during the interval from 1927 March to 1928 December, was from $+^{s}\cdot52$ to $-^{s}\cdot35$, or approximately twice that of Shortt 3, while extrapolation backwards was seriously in error, and in 1926 August the error in rate was $^{s}\cdot05$ daily.

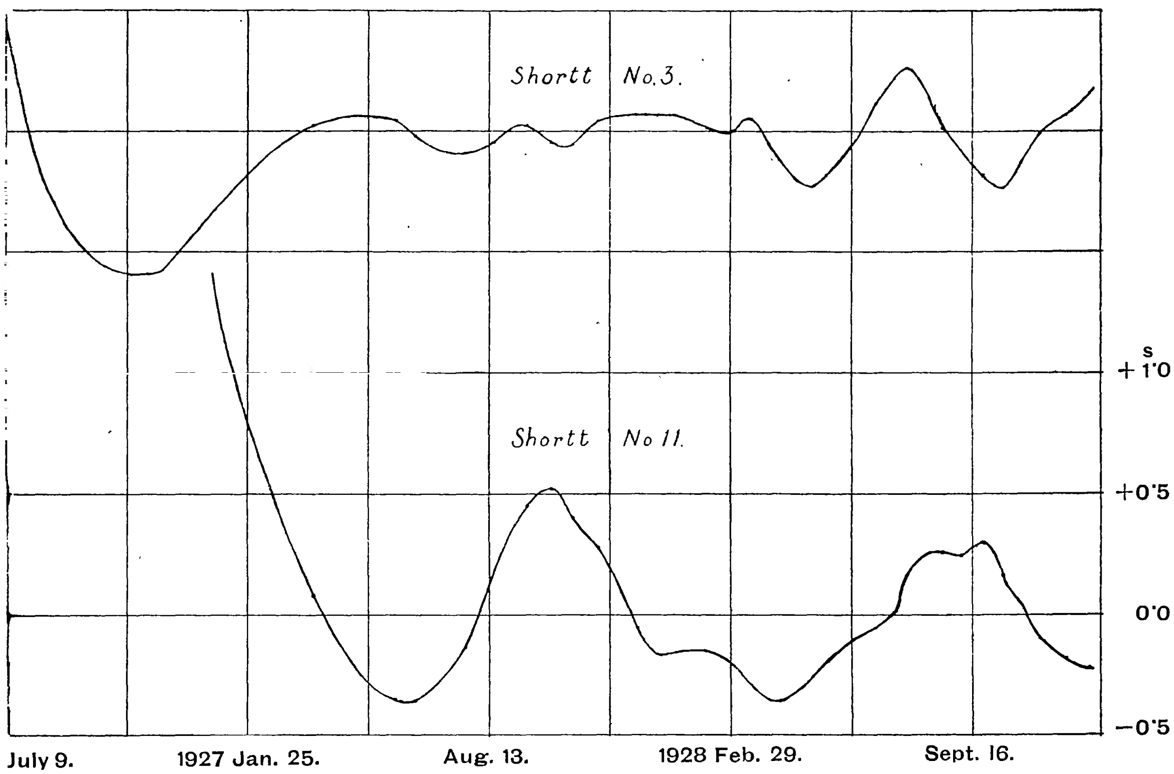

FIG. 2.—Comparison of observed clock corrections with those computed from formulæ (5) and (6). The vertical lines are at intervals of 100 days, and the horizontal lines at intervals of 0·5 seconds.

It will be seen from the diagram showing these residuals (fig. 2) that the ways in which the two clocks depart from the formulæ when extrapolated backwards are very similar. It would indeed be reasonable to expect the clocks to take some time to settle down after being started in 1926 July. Shortt 3 had previously had a run of $1\frac{1}{2}$ years and might have settled down more quickly than Shortt 11, which was only delivered at the observatory in 1926 May.

In considering the actual performance of the Shortt clocks, the form of the formula used must be first examined. For a perfect clock the coefficients γ and δ should be zero. But no mischief is done, apart from decreased weight in the result, if (1) they are accurately determined, (2) they can be accurately applied, which means if the temperature is accurately known, and what is most important (3) their permanency can be guaranteed.

Temperature.—The temperature term should in fact give no insurmountable difficulties. The coefficient δ is comparatively small, and it should be possible, if necessary, to further reduce it. Temperatures can now be controlled so that the variation is small, but no thermostat can act perfectly throughout the whole length of the pendulum. What is required, so far as the clocks at Greenwich are concerned, is to know the temperature, so that for one year compared with another, the error in the temperature should not systematically amount to $0^{\circ}\cdot 1$ F.

Variations in temperature may, however, give rise to much more serious troubles than have been considered in this paper. Several excellent clocks of the older patterns have been known to show large variations of daily rate for a difference of 1° C. between the top and bottom of the clock case. Whatever the cause of this may be, it cannot be connected with the escapement. The fact that the free pendulums have copper cases and that they are in rooms where the changes of temperature have been gradual should ensure that the stratification in the clock cases is not very large.

It is, however, desirable that such a possible source of error should be inquired into. The apparatus for experiments is not available at Greenwich, and in any case it is clearly undesirable to interfere at present with the clocks which have already had an undisturbed run of $2\frac{1}{2}$ years. An experiment which might be attempted would be to have two free pendulum clocks which could be compared to something like $^{s}\cdot 001$ or less, and determine the effect of raising the temperature of the top half of the case above the lower by, say, 1° C. for 24 hours.

The Secular Term.—It will be seen that the formula for the clock correction contains a secular term, which though unimportant for runs of a week or two assumes large dimensions in the course of a year. A variation of this term by only 1 per cent. of its amount would accumulate an error of a quarter of a second in a year. So long as the origin of this term is unknown, it must make conclusions based on the performance of these clocks over a period of years somewhat unconvincing. It has generally been assumed that it is due to a growth of the invar rod. This assumption is based on the known fact that invar grows for some time after its manufacture.

But the secular term in the going of the Shortt clocks would require a growth of the invar amounting to 1μ in 120 days. For the first nine months after Shortt 3 was installed its change of rate was three times its present value, then it suddenly changed to a little more than its present value, and for the last two years it has been practically constant. This in no way resembles the growth of invar generally recorded. Its total amount in 4 years far exceeds what would have been expected, and the uniform rate of the change in the last 2 years does not resemble the irregular slowly decreasing growth of invar. Observations concerning invar too, refer to a bar carefully preserved in a horizontal position. The pendulum rod, with a heavy bob at its lower end, is swinging through a small arc about the vertical, and every 30 seconds a small impulse with a component along the length is applied. It is therefore by no means certain that the growth of invar is the cause of the secular term.

It is easy to make other suggestions though not so easy to test them. The invar rod is attached to a suspension spring of elinvar. A change of 1μ per 120 days in the point of flexure of the spring could produce the observed effect. The stability of the bob is important. The bobs of these pendulums are made of type metal,* and are cylindrical in shape with a central hole. The bob rests on a small upright tube of invar (No. 3) or brass (No. 11) pinned to the pendulum rod. It is possible to think that the bob is slowly sinking on its support. Chemical changes appear incapable of producing a sensible effect. It is rather extraordinary how in 2 years the pressure inside the case of No. 3 has apparently decreased gradually by about 2 mm., from 36 mm. to 34 mm., according to the mercury gauge, while according to the oil gauge it has decreased by 10 mm. A graduated silver scale which was introduced into No. 11 for the accurate reading of the arc has become unreadable, apparently from the action of mercury vapour. It appears possible that some mercury may have amalgamated with the type metal of the bob. However, if we assume the pendulum to consist of a uniform invar rod of 500 gms. with a bob at the end weighing 5000 gms., deposition of ·01 gm. of mercury on the bob would only alter the daily rate by ^{s}·001, whereas the rate has actually altered in 2 years by ^{s}·27. There appears to be no reason why in future type metal should be used for the bob. The chief reason for its use appears to be its high density, but this is of no great importance where the pressure is very low. With the temperature accurately controlled it might also be worth trying a steel instead of an elinvar suspension spring.

A cause of the remaining irregularities in the clock may be irregular growth of the invar rod. Professor Boys advocates the use of fused quartz which has a very low temperature coefficient, and for which he believes that the mechanical difficulties of connecting the suspension spring, bob, and impulse wheel are surmountable.

The idea that the performance of the Cottingham clock might throw some light on the secular term led to an analysis of its errors. This clock has a pendulum quite similar to the free pendulums as regards the materials of its construction. The performance, however, has been rather spoilt by the fact that the temperature coefficient is large, about ^{s}·04 per day per 1° F. An analysis of its performance showed a secular term two-thirds that of the Shortt clocks, but in the opposite direction. Clocks in general tend to gain with age as the oil thickens, and it is quite likely that the secular term of the Cottingham clock is the result of an effect of this kind, so that it does not shed any light on the Shortt clocks which have practically no oil except a trace at the pivot of the wheel which receives the impulse, and the axes of the impulsing mechanism, although the latter are not connected with the pendulum in the same way as in an escapement clock with the usual train of wheels.

Forces.—It is assumed that the only force acting on the pendulum

* Eighty per cent. lead.

is the earth's gravitational attraction. It is unlikely that this is more variable than the earth's rotation. The direct attraction of the sun and moon are not large enough to produce sensible effects. The effect of tidal deformation on the earth's figure, and consequently on the earth's rotation and the gravitational attraction at any place have been investigated by Dr. Jeffreys in the *Geophysical Supplement*, vol. ii., p. 56, and found to produce as the largest term one similar to the semi-annual nutation, but only of one-tenth the amount, and therefore negligible. In countries where violent earthquakes take place, their effect on clocks must be considerable, but no such effects have been traced at Greenwich. It is possible that the irregularities of the order of $\pm$ ^{s}·01 shown in the comparisons of the two clocks may be of this origin.

It has been suggested that as invar is an alloy of nickel and steel it is magnetic, and that the rate of the pendulum may be influenced by the earth's magnetic field. Nickel-steel alloys have very little power of retaining permanent magnetisation, and the earth's magnetic field is too small for any permanent magnetisation to be produced. The effect of magnetic fields on the rates of chronometers and watches was carefully discussed, both from a practical and a theoretical point of view, by Chapman and Lewis in *M.N.*, **73**, 583, in which fields many times larger than the earth's field were used. They concluded that the only appreciable effect was in the temporary magnetisation of the steel bar of the balance wheel, and the analysis given is applicable to the pendulum rod. The formula (16) given by them shows that the effect of a field of strength F along the direction of equilibrium produces a change of daily rate of $42\mu T^2 \frac{A}{Ml} F^2 f(\theta_0)$ seconds, where μ is the permeability, T the period of a complete vibration, A the cross-section of the bar, M the mass of the pendulum, l its length, and $f(\theta_0)$ a function of the arc of vibration, which may be given the value 3·1 for very small arcs. The units are those of the C.G.S. system. If we put $\mu = 100$, $T = 2$, $A = 1$, $M = 7000$, $l = 100$ we get a daily change of rate of ^{s}·072F^2. The value of μ used is that used by Chapman and Lewis for steel—I have no knowledge of its value for invar in weak magnetic fields, but its value is probably not higher. If we take $F = \cdot 43$, the earth's vertical component, we get a rate of 0^{s}·013 which should not vary from year to year. We therefore deduce that magnetic influences on the clock are negligible. In all cases the effect of magnetisation would be to accelerate the clock, while in 2 years the daily rate of the clock has been retarded by ^{s}·27 as the result of secular changes actually taking place.

Other Shortt Clocks.—Although several other Shortt clocks have been in use for a few years, so far as I know no detailed accounts of their performance have been published except by Professor Sampson, who has given details of Shortt 0 and Shortt 4 in the *Proceedings of the Royal Society of Edinburgh.* The difficulties regarding time determinations, to which he pays considerable attention, appear to have been largely overcome at Greenwich by reversing the transit on all stars and by the use of a striding level. The short-period irregularities in the comparison of the two clocks of the order of $\pm$ ${}^{s}{\cdot}01$ are very similar to those shown by the Greenwich clocks. The discovery of the cause of these irregularities should form an important stage in the obtaining of still more regular clocks, and observations such as contemplated by Mr. Loomis with Shortt clocks in America may help in the solution. Mr. Loomis proposes to make a direct comparison of the master pendulums to much smaller quantities than ${}^{s}{\cdot}001$.

For Shortt 0, Professor Sampson's data show no secular acceleration for the first half of 1927, and less than half that of the Greenwich clocks for the second half. For Shortt 4, on the other hand, the secular term appears to have a constant value 50 per cent. in excess of that of the Greenwich clocks.

The principal difference between the clocks so far disclosed is that for Shortt 4, Professor Sampson finds the same change of rate when the pressure was reduced by ·3 inches of mercury from 1·2 inches as from 1·5 inches. The change of rate was ${}^{s}{\cdot}300$ per day for 1 inch of mercury. With Shortt 3, the change was only at half this rate when the pressure was reduced from 1·4 to 1·0 inches, and only an eighth when reduced from 1 inch to 0·4 inches. The actual performance of Shortt 4 appears to be about the same as Shortt 3 and of Shortt 0 as of Shortt 11.

The clock Shortt 16 was set up as a mean time clock at Greenwich at the end of 1927. It is corrected several times daily as it controls the time signals sent out from the observatory. Also the case was opened in 1928 July for alteration of the rate. The true performance of this clock is therefore subject to large uncertainty, but it appears that its secular term is only about one-third that of Shortt 3 or 11, while its temperature coefficient is about twice as large.

The Earth's Rotation.—In addition to a secular slowing down of the earth's rate of rotation, a considerable mass of evidence has been collected which has been interpreted as indicating fluctuations in the rate of rotation. As the most rapidly moving body, the moon should show such irregularities most easily, and, as is well known, there are unexplained irregularities of comparatively large amount between the actual motion of the moon and the best lunar tables.

The results have been carefully discussed by Professor E. W. Brown in *Yale Transactions*, vol. iii., part 6, and by Professor de Sitter in *B.A.N.*, 124. The data obtained from the moon are fairly well substantiated by that from other bodies in the solar system, but it must be admitted that the quantities to be observed for the latter are so small as to approach the limit of accuracy of the observations themselves. If indeed the differences between the observed and computed positions of the moon are due to irregularity in the earth's rotation, that rotation must have been gaining about $1^{s}{\cdot}6$ yearly round about 1870, and losing $0^{s}{\cdot}7$ yearly in 1905, while changes as great as 1^{s} yearly may have taken place quite suddenly.

The difficulty in testing such supposed changes in the rate of the earth's rotation by means of clocks is twofold—to get a clock which will (1) steadily maintain its rate within a fraction of a second a year, and (2) keep on doing this for a number of years. If indeed the rate of the earth's rotation changed suddenly, as has been suggested, it would only be necessary to keep the clock going for two or three years at the critical epoch. If it should be possible to construct a clock to surmount (1) above but not (2), the difficulty in correlating its rate with that indicated by the moon's motion would be the difficulty of obtaining monthly positions of the moon correct to, say, $\pm 0''{\cdot}05$. The moon in fact is not suitable for checking irregularities if they should exist of the kind shown in the diagrams of this paper.

Although the residual irregularities of Shortt 3 and Shortt 11 do not agree, they appear to indicate that the rotation of the earth in 1928 did not differ by 1^s from that in 1927. Now the residuals between the observed and computed longitude of the moon for 1921, 1922, and 1923 * were

1921·5	. . .	− 9·42″
1922·5	. . .	− 9·17
1923·5	. . .	− 10·73.

Should such a change be observed between 1927·0, 1928·0, and 1929·0, it will not be possible to attribute it to irregularity in the earth's rotation, as $1''$ in the moon's longitude corresponds to 2^s in the earth's rotation. These clocks should be able to decide whether erratic changes of $1''{\cdot}0$ in the moon's longitude of one year compared with the preceding and following years is due to the earth's rotation or not.

The Greenwich clocks have throughout been in the care of Mr. Bowyer.

The Solar Rotation derived from the H and K Lines in Prominences. (Second Paper.) By J. Evershed, F.R.S.

In *Monthly Notices*, **88**, 126, I summarised the results of measures of 92 spectra of prominences obtained in the years 1926–27. These yielded values of the solar rotation in four zones of heliographic latitude which are remarkable for the very large angular motion in each zone, as compared with values derived from spectra of the reversing layer, or from the motion of sunspots. In addition, the general shift towards red of the H and K lines was found to exceed the predicted relativity shift by about 0·003A, in approximate agreement with measures previously obtained with the same lines in the chromosphere, and with iron lines in the reversing layer in the same spectral region.

During 1928 the work has been continued on the same lines, but one change has been made in the method which greatly simplifies the process of measurement and reduction of the spectra. Owing to this change, and to the favourable season, a much larger number of plates

* It should be noted, however, that this is a rather erratic set of residuals, and the comparison of the observations with theory was direct in 1923 but indirect in 1921 and 1922. The data are taken from *Yale Transactions*, vol. iii., part 6.