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COMPARISON

OF

MAGNETIC STANDARDS

AT

BRITISH OBSERVATORIES

WITH A DISCUSSION OF

Various Instrumental Questions Involved

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A COMPARISON OF MAGNETIC STANDARDS AT BRITISH OBSERVATORIES WITH A DISCUSSION OF VARIOUS INSTRUMENTAL QUESTIONS INVOLVED

§ 1. Instruments and Methods of Comparison

THE standard magnetic instruments in use at Kew Observatory up to 1924 were a unifilar magnetometer by Jones and a dip circle Barrow No. 33, with needles 1/97 and 2/97 by A. W. Dover. During 1923 and 1924 these were compared with the standard instruments belonging to the observatories of Greenwich, Valencia, Stonyhurst and Eskdalemuir. Comparisons were also made in 1921, 1922 and 1923 with the coil magnetometer at the National Physical Laboratory.

Unifilar magnetometers serve to determine both declination (*D*) and horizontal force (*H*). The Valencia magnetometer was compared at Kew. In all other cases the *H* comparisons were effected indirectly through unifilar Dover 140, which was taken successively to Greenwich, Stonyhurst and Eskdalemuir. Comparisons of the Dover and Jones instruments were made at Kew before and after the comparisons at other stations. The *D* comparisons with the Stonyhurst and Eskdalemuir instruments were also made through Dover 140. The comparison with the Greenwich *D* standard depended partly on Dover 140, and partly on another unifilar Casella 181.

The Valencia and Stonyhurst dip circles were compared directly at Kew with the Barrow circle. The comparison between the Kew Barrow circle and the dip inductors in use at Greenwich and Eskdalemuir was effected through a dip circle Dover 74 belonging to Eskdalemuir.

At stations provided with magnetographs the comparison of magnetic instruments can be either direct or through the curves. For a direct comparison, it is a great convenience—in the absence of an observing tent, almost a necessity—to have two observation huts, as at Kew and Eskdalemuir. Each hut at these stations has three observation piers. A distant mark of known azimuth is visible from the central pier of each hut, so that observations, whether of *D* or *H*, can proceed simultaneously with two instruments. In the Eskdalemuir huts, and in the larger hut at Kew, the east and west piers are sufficiently far apart to admit of satisfactory simultaneous observations with two dip circles in the same hut.

If there is only one hut, and observations with the instruments under comparison have to be taken at different times, the comparison has to be effected through the curves. The difference between the two instruments can be derived from the difference between the base line values which they supply for the curves.

In the case even of simultaneous observations, comparison via the curves may have advantages, especially in the case of *H*. The time required for the vibration experiment is determined by the time of swing of the magnet, and so will usually differ for the two instruments under comparison. Even in the case of the deflections, it is always irksome to two observers to keep step, and it cannot be done at all when one deflects at two distances and the other at three.

§ 2. Sources of Uncertainty

A satisfactory comparison of instruments is by no means a simple matter. An uncertainty, which there is no obvious way of eliminating, is the possibility that the difference between two instruments may vary with the place of observation. If the two standard instruments are not confronted directly, but indirectly through a

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travelling instrument, there is in addition the risk that the travelling instrument may alter in the course of its journeys. To check this entails two sets of observations with the travelling instrument at one at least of the stations.

If the difference between two standard H instruments, belonging to stations where H is widely different, is to be derived through a travelling instrument, some assumption is necessary. The plan adopted by the Carnegie Institution of Washington is to assume that the difference between two instruments varies directly as H. The comparison supplies the constant by which H has to be multiplied. This view is demonstrably sound when the difference arises from error in the accepted value of the moment of inertia of a collimator magnet, and this is undoubtedly a common cause of part at least of the difference observed between magnetometers. But there are other errors, the consequences of which are more involved. Fortunately in the present case, the differences between the values of H at the several stations are comparatively small, and the point is for the moment rather of theoretical than practical importance.

A difference between the values of D obtained with two magnetometers might have an optical, a mechanical, or a magnetic source, and if a travelling instrument differed from two observatory standards by amounts which were substantial and substantially different, the conclusion to be drawn would be very uncertain.

In the case of a dip circle the readings obtained with two needles have been found to differ by different amounts when the dip is altered. The behaviour of a needle turns largely on the perfection of a very small part of the axle, the part used varying with the local dip.

A source of uncertainty affecting all magnetic comparisons is the possible presence of local disturbance. If there are two huts, as at Kew and Eskdalemuir, the values of D, H, or I (inclination), in the two should not be assumed identical. It is even desirable to have direct evidence of identity between the piers in a single house. In the case of Kew an elaborate set of simultaneous observations with exchange of the two huts was carried out in 1915, during a comparison with instruments belonging to the Carnegie Institution of Washington, in which Captain Kidson and Mr. Francis, the Kew observer, took part. According to their results the two huts were identical for D, H, or I, within the limits of observational error. But in a subsequent intercomparison in 1922, in which the observers were Mr. W. C. Parkinson, of the Carnegie Institution, and Mr. H. G. Harris, of Kew Observatory, the former's observations suggested a difference of 1′ between the values of D in the two houses. This was not confirmed by the simultaneous or later observations by the Kew observer. But in view of the suspicion thus aroused, observation sites were interchanged during all the intercomparisons made at Kew during 1923 and 1924. The same was done during the comparisons made at Eskdalemuir.

§ 3. Changes in Instruments

Another aspect of the case calls for comment. When an intercomparison is made of, say, two standards of length, we are comparing it is true two lengths which —quite apart from any relativity subtleties—are not strictly invariable. But the main cause of temporary variability, temperature, can be satisfactorily dealt with. Apart from temperature, the standard has probably some secular change, but if it is made of a suitable metal no change as large as 1 part in 100,000 is likely to occur in a long period of years. Thus, the relation between two standards of length, to the degree of precision ordinarily requisite, can be settled definitely by a single comparison, for at least a number of years. The length of the standard, it is true, may depend on how it is supported, and even to a slight extent on the atmospheric pressure to which it is exposed ; still what is a practically invariable standard of length, under a specified set of conditions, can be maintained at an observatory. Now this is exactly what cannot be assured in the case of the ordinary dip circle or magnetometer, whatever may be true of dip inductors or coil magnetometers.

The tendency to change is most easily recognized in the dip circle, because independent results are usually obtained with two needles. A difference between the needles on a single occasion may be ascribed to observational uncertainties. But if mean values be derived from a large number of observations, it will generally be found that one of the needles has a decided tendency to give a bigger dip than the other ; and if the observations be continued over a period of years, the difference between the two needles will usually show a decided change. Obviously, one at least of the needles cannot be giving the true dip, and it is at least unlikely that a mean from the two needles is correct. The probabilities are, moreover, that if a new pair of needles were substituted for the old, a change in the observed dip would ensue. It is, to say the least of it, hardly likely that the difference between the dips obtained with two dip circles will remain constant to 0′·1 for any great length of time.

In the case of the magnetometer, evidence of variability is harder to come by, especially in the case of *D*. Something may be learned from the observations taken with the numerous magnetometers which have been tested at Kew. Unfortunately, as a rule, time has allowed of only one or two *D* observations. The difference from the corresponding value of *D* derived from the curve, as standardized by the Jones' magnetometer, has been usually of the order 1′·0, and has fairly been assignable to observational error. If, as occasionally happened, the difference was markedly in excess of 1′, further investigation was made. In only a few cases was a difference decidedly in excess of 1′ substantiated, and, I think, in every such case some instrumental defect was discovered. The most usual defect was slight magnetism in some screw or other fitting. In one or two cases the glass window through which the distant mark is viewed was slightly prismatic. This latter source of error would tend to remain constant, unless the window were replaced. A magnetized part usually showed itself through change in the pointing of the *D* magnet when raised and lowered in the magnet box. As the height of the magnet has a considerable range consistent with satisfactory illumination of the scale, the presence of a magnetized fitting would naturally introduce variability into the results obtained at any one station. The magnetism of a defective piece, moreover, would be unlikely to remain constant, especially in the case of a travelling instrument.

§ 4. Magnetometer Constants

The case of *H* is the most troublesome. For perfect accuracy, perfection of workmanship must be combined with perfection in the determination of the constants. Invariability with time in the constants is a matter rather of faith than of knowledge. Errors in the constants have varying importance. For instance, if the observation temperature were invariable, error in the temperature coefficient of the magnet would not really matter. Again, an observer familiar with a particular magnetic pavilion can usually arrange that the temperature, if not constant, shall change at a nearly uniform rate. If it does so and he takes two complete *H* observations in the order vibrations, deflections, deflections, vibrations, he will secure very nearly the same mean temperature for the vibrations and deflections. If, however, the temperature gets out of control and varies erratically, the thermometer and the magnet are unlikely to be at the same temperature at the same time.

The terms depending on the induction coefficient are usually small, but it is difficult to feel assured that error in the induction correction is always trifling. The method of determining it assumes that temporary induction varies linearly with the field until the latter exceeds 0·45 C.G.S. units, and that it is independent of the strength or temperature of the magnet.

Error in the deflection distances may tell in two ways. The usual formula makes H vary as $r^{-\frac{3}{2}}$, where r is the deflection distance. Accuracy to 1γ means in England an accuracy of practically 1 in 20,000 in H, and so of 1 in 30,000 in r. When two deflection distances are used, the shorter is usually 30 cms. Thus, for an accuracy of 1γ in H accuracy to 0·001 cm. is required in r. Further, in actual use, the deflection bar bends, and a correction of at least 1 part in 10,000 is necessary to the lengths of the unbent bar as measured at the National Physical Laboratory. Unless the values accepted for the deflection distances are exact, an error ensues in the calculated value of m/H where m is the moment at 0° C. of the collimator magnet. This affects the values calculated for the distribution constant or constants in the usual expression $2mr^{-3}(1+Pr^{-2})$ or $2mr^{-3}(1+Pr^{-2}+Qr^{-4})$.

§ 5. Moment of Inertia of Collimator Magnet

The next source of error to be mentioned is more troublesome, as it tends to vary with time. Denoting by K the moment of inertia of the collimator magnet, the error ΔH in H due to an error ΔK in K is given by $\Delta H/H = 1/2 \Delta K/K$.

Usually K is calculated from the mass and dimensions of the inertia bar, which should be a perfectly homogeneous right circular cylinder. No other way was practicable prior to the construction by the late Professor W. Watson of an apparatus for comparing inertia bars directly. Even with the Watson apparatus, the comparison of two bars is tedious, and we are ultimately dependent on the homogeneity and the accuracy of the mass and dimension measurements of the bar accepted as standard. With the improved apparatus which became available after the institution of the National Physical Laboratory, it became possible to test the accuracy of form of the inertia bar which had long served as standard at Kew. The report was not wholly satisfactory, and with a view to a choice a new bar was ordered from each of the English firms known to construct magnetometers, special accuracy of construction being requested. At this time the Watson apparatus was not available, so the moments of inertia of the bars were compared by swinging each with the magnet of the standard Jones magnetometer. From each set of swings a moment of inertia was calculated for the magnet, which depended on the mass and dimensions of the particular bar. The results obtained with two of the bars D (by Dover) and E (by Elliott Brothers) for log $\pi^2 K$ at 0° C. were :—

From D	3·42376
From E	3·42372

The observations, which were taken at intervals from 1902 to 1904, were regarded only as a preliminary to a more complete determination. But the agreement found between these two bars being the closest, it was decided to accept them as the two standards. Their mass and dimensions, as determined in 1902 at the National Physical Laboratory, were as follows :—

	Mass.	Length at 0° C	Diameter at 0° C.
D	65·886 grammes	9·996 cm.	0·994 cm.
E	66·314 ,,	10·011 cm.	0·996 cm.

The desired opportunity for a more complete investigation of the moment of inertia did not arise for several years, and in the meantime no change was made, a value determined in 1891, viz. :—

$$\log \pi^2 K \text{ at } 0° \text{ C.} = 3\cdot42404$$

being adhered to.

A redetermination of the moment of inertia of the Jones magnet was eventually carried out in 1909–10. Assuming the inertia bars unchanged since 1902, the values obtained for log $\pi^2 K$ at 0° C. were :—

From D	3·423526
From E	3·423459
whence the accepted mean was	3·42349

The small difference between the two inertia bars, it will be noticed, is in the same direction as that observed in 1902 to 1904. The mean value thus obtained for log $\pi^2 K$ was less by ·00055 than the value in current use. It was used in the final calculations for the year 1910, and if it had been the sole correction applied would have entailed a discontinuous fall of 12γ (more exactly $11{\cdot}7\gamma$) in the value of H. This matter is further discussed in the Appendix (page 287).

Another re-determination took place in 1915. Assuming the inertia bars unchanged, there resulted for log $\pi^2 K$ at 0° C.—

From 12 observations with bar D	3·423292
From 12 observations with bar E	3·423253
Mean	3·42327

On the completion of the observations the bars were re-weighed at the National Physical Laboratory and each was found to have diminished 0·004 gramme in mass since 1902.

Assuming the decline in mass to have preceded the last determination of the moment of inertia, the revised mean value for log $\pi^2 K$ at 0° C. becomes 3·42325. This value was brought into use in the final calculations of H for 1915. If it had been the sole alteration in the constants, it would have entailed a discontinuity of -5γ (more exactly $-4{\cdot}8\gamma$) in the value of H. This is further discussed in the Appendix (page 287).

A final re-determination of the moment of inertia of the Jones magnet was made in 1922. Accepting the values obtained for the dimensions of the inertia bar in 1902, but the masses as found in 1915, the results obtained for log $\pi^2 K$ at 0° C. were—

From 10 observations with bar D	3·423050
From 10 observations with bar E	3·423013
Mean	3·42303

Before finally accepting this, it was decided to have the masses and dimensions of the inertia bars re-determined at the National Physical Laboratory. The results were as follows :—

	Mass.		*Length at* 0° *C.*	*Diameter at* 0° *C.*
D.	65·881 gramme	..	9·996 cm.	0·994 cm.
E.	66·305 gramme	..	10·008 cm.	0·996 cm.

With these revised values we find for the value of log $\pi^2 K$ at 0° C.—

By bar D, 3·42294 }
By bar E, 3·42272 } Mean 3·42283

The substitution of this value for that finally accepted for 1915, is equivalent to lowering H by $8{\cdot}9\gamma$. But of this, it should be noticed, a substantial part, viz., $4{\cdot}2\gamma$, is due to the acceptance of the values assigned to the masses and dimensions of the inertia bars in 1922 at the National Physical Laboratory in place of those assigned in 1915. The effect on the value of H is dealt with in the Appendix (page 287).

It will be noticed that with the latest laboratory measurements there is now a decided difference between the results obtained with the D and E bars. This has manifested itself equally in the case of several other collimator magnets or inertia bars which have been recently swung with both D and E. What this means is that while the original measurements of mass and dimensions made in 1902 gave results for the relative values of the moments of inertia of the bars D and E, which were in practical agreement with the results of direct comparison in the Watson apparatus, this is not true of the measurements made in 1922. This raises no presumption for or against the accuracy of either set of measurements. *A priori*, we should hardly expect absolute uniformity of density in any inertia bar.

A slight decline in mass in an inertia bar or a magnet in fairly common use is not unnatural, but a reduction in the length of an inertia bar from 10·011 to 10·008 cm. does seem surprising. As a matter of fact, two other inertia bars were re-measured in 1922. Each of the four bars measured appeared to have shortened since 1902, the change in bar D being the least, and the average change being 0·0025 cm. This is a matter for experts, but I must confess to a doubt of the reality of the changes. Possibly there may be some lack of perfection in the form of the bars, which leads to different results being obtained for the length, when the method of measurement is varied. The matter is of practical importance. If inertia bars can change appreciably in length in the course of 20 years, they do not afford the safeguard it has been supposed they do.

Supposing the change of length real, it is natural to suppose that most of it occurred in the earlier years. The real change in the calculated value of H arising from decline in the moment of inertia of the magnet between 1915 and 1922 was probably more like 5γ than 9γ.

It will be seen that each re-determination has necessitated the acceptance of a lower value for the magnet's moment of inertia. If the value obtained in 1891 had been adhered to, the values calculated for H in 1922 would have been too high by fully 25γ. Even since 1910 the average annual increment of error would have approached 1γ. The possibility of slight further loss in 1923 and 1924 should be borne in mind. It should also be noticed that the divergence between the D and E bars as measured in 1922 answers to a difference of about 4γ in the value of H at Kew.

§ 6. Distribution Constants

Another source of uncertainty remains to be noticed. The procedure which used to be practically universally followed with Kew-pattern magnetometers was to have two deflection distances, viz., 1·0 and 1·3 foot. When the C.G.S. system was adopted these distances were replaced by 30 cm. and 40 cm., respectively, and it was assumed that the Q of the deflection formula $2mr^{-3}\ (1+Pr^{-2}+Qr^{-4})$ was negligible. That procedure was still followed at Greenwich, Valencia and Stonyhurst, in 1923 and 1924. Three deflection distances, however, were in use at Kew and Eskdalemuir, viz., 22·5 cm., 30 cm. and 40 cm. at the former station, and 25 cm., 30 cm. and 40 cm. at the latter.

When Q is neglected, but is not negligible, the quantity calculated by the usual formula for P is not P, but is a quantity P' given by $P'=P+Q\ (r_1^{-2}+r_2^{-2})$, where r_1 and r_2 are the two deflection distances. The result obtained for H requires a correction ΔH given by $\Delta H/H = -\ (1/2)\ Q/r_1^2\ r_2^2$.

If a magnet is replaceable by two poles, the P and Q of a given pair of deflecting and deflected magnets can be expressed in terms of the pole distances. If this be done, and if Borgen's value 0·8 be accepted for the ratio of the pole distance to the length of a magnet, then the correction indicated above is in Britain roughly $+3{\cdot}5\gamma$ for a Dover unifilar, and $+5{\cdot}7\gamma$ for the Jones instruments in use at Kew and Stonyhurst. As a matter of fact, however, on an average of 15 years the neglect of Q in the Kew Jones would have led to an underestimate of H by fully 9γ.

Since 1910 the practice at Kew has been to deflect at 22·5 cm., 30 cm. and 40 cm., determining values for P and Q at the end of the year by combining all the observations of the year, with the occasional exception of one occurring during a highly disturbed time. Table I shows the values thus arrived at, also the corresponding values arrived at for the mean $\log_{10}\ (1+Pr^{-2}+Qr^{-4})$ from the three distances. This represents the correction factor applied to the mean log m'/H', where m'/H' represents the value obtained for m/H when P and Q are neglected. The table also shows the values of a quantity, $\Delta\ H$ representing the addition to be made to H as calculated from the P and Q of the individual year when one employs instead mean values of P and Q derived from the whole 15 years.

The collimator magnet was dropped in 1916, and that year was exceptional. It is omitted in calculating the 14-year means. Up to 1923 the practice was to calculate during the year provisional values of H based on the P and Q of the previous year, and at the year's end to introduce a correction which allowed for the difference between the P and Q of the current and the previous year. In 1923, however, it was decided to use in the final reductions a mean P and Q from the years 1917 to 1923. The value thus obtained for the mean $\log_{10}\ (1+Pr^{-2}+Qr^{-4})$ was $\bar{1}{\cdot}99955$. Curiously enough, 1924 supplied identically the same value for this logarithm as did

1923, an occurrence never experienced before. If we had used for 1924 a mean from 1917 to 1924 the value obtained for the logarithm would have been $\bar{1}\cdot99958$. The employment of this in place of the provisional value $\bar{1}\cdot99955$ would have entailed a correction of $+0\cdot5\gamma$ to H. It was decided, however, to adhere to $\bar{1}\cdot99955$. This is identical with the value given by the whole 15 years.

It will be observed that P and Q are opposite in sign, and generally increase or decrease numerically together. Thus, the fluctuations in $\log_{10}$ $(1+Pr^{-2}+Qr^{-4})$, which is the practically important quantity, are not so large as the inspection of the values of P and Q alone might suggest. Still, it is a matter of considerable importance what view is taken of the apparent fluctuations.

TABLE I.—"DISTRIBUTION CONSTANTS" CORRECTION

Year.	P +	Q −	Mean $\log_{10}(1+Pr^{-2}+Qr^{-4})$	ΔH
				γ
1910	0·882	1354	$\bar{1}\cdot99939$	+ 3·4
1911	0·832	1377	34	+ 4·5
1912	0·749	1286	37	+ 3·8
1913	1·504	1528	59	− 0·9
1914	1·226	1343	58	− 0·6
1915	0·778	1245	42	+ 2·8
1916	2·962	2044	96	− 8·7
1917	0·696	1236	38	+ 3·6
1918	1·683	1565	65	− 2·1
1919	1·496	1525	58	− 0·6
1920	0·970	1280	50	+ 1·1
1921	0·272	1054	30	+ 5·3
1922	1·809	1642	66	− 2·3
1923	2·240	1787	77	− 4·7
1924	2·084	1682	77	− 4·7
Mean, 15 years	1·346	1463	$\bar{1}\cdot99955$	—
Mean, 14 years	1·230	1423	$\bar{1}\cdot99952$	—

If everything instrumental except the magnets remained invariable, P and Q could change only with a change in the distribution of the magnetism in one or both magnets, i.e., some change equivalent to alteration in the pole distance. We should hardly expect a change to occur in the pole distance without an accompanying change of magnetic moment. The change of moment in the Jones magnet during the last 14 years has only been from about 648·9 to 640·2 C.G.S. units, a change of less than one-tenth of 1 per cent per annum. The change, moreover, has always apparently been continuous in one direction.

As the strength of the mirror magnet is not determined, it is possible, of course, that changes in it may have been the principal cause of changes in P and Q. As against this, however, is the fact that much the largest irregularity in P and Q occurred in 1916, a year when nothing special is known to have happened to the mirror magnet, while the collimator magnet was dropped and had to be re-balanced. The magnetic moment on this occasion did not seem to suffer, because the values calculated from m from the ten observations which preceded and the ten which followed the accident differed by only 0·1 C.G.S. unit. If, on the other hand, the high value in 1916 was due to some mechanical cause, its lack of permanency is curious.

§7. DISTRIBUTION CONSTANTS—*continued*

As there are usually fully 50 absolute observations in a year, we should naturally assume the annual mean values of P and Q to be free from any sensible accidental element. To obtain, however, some positive light on the subject, values were calculated for P and Q for the twelve months separately, for two groups of years, 1910–1915 and 1917–1923. It was recognised that some uncorrected temperature effect might exist. The observations within one calendar month should be fairly homogeneous, whether such an effect exist or not ; and if it does exist, it should be put in evidence through systematic differences between the winter and summer months.

Table II gives the resulting values of mean $\log_{10}(1+Pr^{-2}+Qr^{-4})$ and, in the last column, the algebraic difference between the means for the month and for the twelve months as a whole based on the whole thirteen years.

TABLE II.—VALUES OF $\text{LOG}_{10}(1+Pr^{-2}+Qr^{-4})$

Month or Season.	Group of Years, 1910–1915.	Group of Years, 1917–1923.	Mean from two Groups.	Difference from Mean. Unit 0·00001.
January	$\bar{1}$·99946	$\bar{1}$·99957	$\bar{1}$·99952	+ 2
February	33	24	29	−21
March	50	72	61	+11
April	57	41	49	− 1
May	24	40	32	−18
June	60	53	56	+ 6
July	40	67	54	+ 4
August	42	26	34	−16
September	26	64	45	− 5
October	38	54	46	− 4
November	71	65	68	+18
December	54	90	72	+22
Year	45	55	50	—
Winter	51	59	55	—
Equinox	43	58	50	—
Summer	41	48	45	—

It seems fairly certain that if there is any uncorrected temperature element in the calculated values of P and Q, it must be small. The winter (November to February) mean is higher than the summer (May to August) in both groups of years. But the month giving the biggest negative value is February, and so far as temperature is concerned February is much on a par with December, which supplies the largest positive value.

The results in the final column each depend on the observations of thirteen months, and so should suffer rather less from accidental errors than the observations of a single year. We seem driven to the conclusion that the observations of a single year are insufficient for the elimination of accidental causes. It was largely on this evidence that the decision was reached to employ in the reduction of the observations of 1923 and 1924 mean values of P and Q based on a number of years. This conclusion received support from the comparisons made between the Jones magnetometer and the coil magnetometer at the National Physical Laboratory in 1921, 1922 and 1923. The results (A) were obtained by using the individual year's values for P and Q. The results under (B) were derived from mean values from 1917 to 1923. The figures represent the excess in the value of H obtained with the coil magnetometer over that given by the Jones instrument.

Year.	1921.	1922.	1923.
(A)	$+10\gamma$	-1γ	-8γ
(B)	$+5\gamma$	$+1\gamma$	-4γ

The accuracy of the figures depends on the accuracy of the observations made at Teddington with the unifilar Dover 140, and on the absence of any change in that instrument due to transport between Richmond and Teddington : in view of the precautions taken, an uncertainty of 5γ seems more probable than one of 10γ.

§ 8. Comparative Results from Jones and Dover Magnetometers

We have still to face the question of the uncertainty actually entailed in indirect comparisons. In view of the proposed use of Dover 140, it was decided in 1923 to take weekly observations of H when possible with that instrument in the new hut at Kew, synchronously with the ordinary observations taken with the Jones instrument in the old hut. These observations were mostly taken by myself, as the prospective observer during the observatory comparisons. Deflections were taken at 30 and 40 cm. only, precisely as was done at the outside stations. Table III

Table III.—Differences between Jones and Dover Magnetometers

Month.	1923.			1924.			
	n	$10^5 \times$ difference in $\log_{10} (m/H)$.	Excess of Jones.	n	$10^5 \times$ difference in $\log_{10} (m/H)$.	Excess of Jones. A.	Excess of Jones. B.
			γ			γ	γ
January	3	107	+ 1	5	135	+ 4	0
February	4	181	−10	4	169	+ 4	0
March	2	141	− 9	3	147	+ 1	− 3
April	3	141	− 6	4	182	+ 5	+ 1
May	3	134	+ 3	5	176	+ 6	+ 2
June	11	130	− 1	4	174	+ 3	− 1
July	5	144	0	3	139	− 5	− 1
August	3	142	− 1	6	140	− 5	− 1
September	3	143	+ 2	10	145	− 4	0
October	3	159	+ 1	5	145	− 6	− 2
November	8	135	− 1	4	145	− 9	− 5
December	4	169	+ 5	5	148	−13	− 9
Totals and Means:	52	144	—	58	153	—	—

shows the mean results from each month of 1923, and 1924 for the excess in the value of H obtained with the Jones instrument over that obtained with the Dover. n denotes the number of observations.

The second column gives the excess of the value obtained for $\log_{10} m/H$, when P is neglected, from the deflections at 30 cm. over that obtained from the deflections at 40 cm. As a first approximation, P varies directly as this difference, and an approximate corresponding value of P could be obtained by multiplying each entry in the second column by 0·0475. A single value of P, +6·86, was applied to all the observations of 1923; it represented a mean which allowed equal weight to each observation. For 1924 two sets of values are given for the instrumental differences. The results headed (A) are calculated from a single value for P, +7·27, which allows equal weight to each observation of the year. The results under (B) treated the first and second six months independently, employing for the first six months the value +7·82 for P given by the observations of the first half-year, and for the second six months the value +6·85, given by the observations of the second half-year.

The irregularity in the figures for the first four months of 1923 may arise from the multiplicity (four) of observers. Of the outside observations during that year, those at Teddington were taken in July, and those at Greenwich in October and November. The figures in Table III do not suggest any sensible relative change between the two instruments from May to November, 1923. It will also be noticed that while the excess in the value of log m/H at 30 cm. fluctuates, the difference between its mean values from the first and second half-years would be small.

The outside observations in 1924 were taken during July and August, between the observations from which the figures for these months in Table III are derived. There is again no suggestion of any real change in Dover 140 during its journeys, which is so far satisfactory ; but the figures do suggest a change of some kind earlier in the year. If we assume P to have been really invariable throughout the year, we have an apparent sudden discontinuity at the end of June in the difference between the Jones and Dover instruments. This discontinuity disappears entirely if we apply to each half-year a P derived from its own observations. A small change in P occasioned by the journeys would not be surprising, as there was an undoubted loss of magnetism. The ten observations preceding the journey to Stonyhurst and Eskdalemuir gave a mean value of 604·6 for m, while the ten observations immediately succeeding the journey gave only 602·3 C.G.S. units. But the change in P, if any, clearly preceded the journey. Moreover, it is the value of P from the first six months that is high, and not the value from the second six months that is low.

The base values derived from the individual observations during December, 1924, were more than usually variable for both instruments and less weight than usual attaches to the results of that month.

§ 9. Comparative Results from Observations at Kew Observatory and at the National Physical Laboratory

A possibility which cannot be ignored is that the change in the relations of the Jones and Dover instruments, which the figures in Column A of Table III suggest, represents a real change in the Jones instrument. Fortunately, however, there is very strong evidence to the contrary. It had been arranged, partly with a view to such a contingency, that weekly observations should be taken at the National Physical Laboratory with the coil magnetometer for half-an-hour, synchronizing with part of the H observation at Kew. On each occasion some dozen readings were secured at Teddington. Measurements of the Kew H curves at the corresponding times gave a base value for the Kew curves. This base line value is lower than that derived from the corresponding measurements made at the time of observation with the Jones instrument by a quantity we shall call G, where—

G=Excess of coil instrument over Jones + excess of H at N.P.L. over H at K.O.

The train disturbances may produce a considerable effect on a single instantaneous value at Kew, but their effect on a mean value from an interval of 30 minutes appears to be trifling. Thus we should not expect the artificial disturbances to influence the station difference appreciably, unless the desire to secure readings at Teddington during the quietest moments introduced a selective effect. Even if it did, we should expect the consequences to be fairly uniform, as the observations were made near a fixed hour of the day, after the heavy morning traffic on the railways had subsided. Thus the variability in the difference between the two base line values should supply a check on the constancy of the instruments.

The base line value of the average force magnetograph shows a sensible drift—not infrequently a large drift—owing to weakening of the magnet, or other instrumental change. But the Kew H magnetograph has been for many years exceptionally stable, and after due allowance was made for the effects of temperature no certain change could be detected in the base line value throughout the seven months over which the comparison extended. Thus the simplest way of bringing out the facts is to record the base line values (at an invariable temperature) derived from each day's observations with the two instruments. Table IV gives these base line values in the first two columns. The third column (A) shows the departure of G from its mean value, $+60\gamma$, when each observation with the Jones is treated independently ; the fourth column (B) shows this departure when a mean base line value was calculated from all the observations of the month with the Jones instrument.

There was a range of 10° C. in the temperature of the magnetograph room during the observations and the temperature correction is $3{\cdot}1\gamma$ for 1° C. Obviously, a slight error in the temperature correction, or failure in the thermograph to show the true temperature of the magnet, might react considerably on the accuracy of the base line values. Any temperature uncertainty would, however, be without any practical effect on the *difference* between the base line values deduced from the observations made on the same day with the two instruments. The mean value of this difference for the whole 30 observations is 60γ, and the mean values are identical, 59γ, for June and December. It is sufficiently obvious that if we may assume the coil magnetometer and the station difference to have remained invariable, then we must conclude that no certain change occurred during the whole seven months in the Jones magnetometer, its observations being reduced with invariable values of P and Q.

TABLE IV.—WEEKLY COMPARISONS OF JONES AND COIL MAGNETOMETERS

Date, 1924.	Base Line Values.		Departure from Mean Difference.		Date, 1924.	Base Line Values.		Departure from Mean Difference.	
	Coil Instrument.	Jones.	A.	B.		Coil Instrument.	Jones.	A.	B.
	γ	γ	γ	γ		γ	γ	γ	γ
May 29	18131	18075	− 4	− 1	September 18	18136	18080	− 4	− 3
June 5	35	81	− 6	− 1	25	34	76	− 2	− 6
12	31	72	− 1	− 4	October 2	33	78	− 5	− 1
19	36	71	+ 5	0	9	32	72	0	− 2
26	35	77	− 2	− 1	16	37	78	− 1	+ 3
July 3	40	81	− 1	+ 1	23	38	71	+ 7	+ 4
3	35	—	—	− 4	30	45	72	+13	+10
10	34	77	− 3	− 5	November 6	31	72	− 1	− 2
17	38	83	− 5	− 1	13	38	71	+ 7	+ 6
August 7	34	75	− 1	0	20	35	71	+ 4	+ 2
14	35	71	+ 4	+ 1	27	35	76	− 1	+ 3
21	36	75	+ 1	+ 2	December 4	39	79	0	+ 2
28	33	70	+ 3	− 1	11	35	69	+ 6	− 3
September 4	38	81	− 3	− 2	18	30	76	− 6	− 8
11	36	80	− 4	− 3	31	44	83	+ 1	+ 6
					Mean ..	18136	18076	±3·5	±2·9

A number of sources of error may contribute to the entries in Column A. These include observational errors in the two absolute instruments, fluctuations due to Nature or to artificial disturbances in the station difference, errors in measurements of the magnetograms or in their scale value, imperfection in the temperature corrections and irregular fluctuations in the magnetograph. The entries in Column B have the same sources of error, but errors depending solely on the observations with the Jones instrument should be reduced. Errors, however, depending on actual changes in the magnetograph or on imperfect temperature compensation would be increased. The scale value of the magnetograms was taken as 1 mm. = 6·4γ throughout, this being the value obtained from scale value determinations both at the beginning and the end of the year. Curve ordinates cannot be measured to nearer than 0·1 mm., even under the most favourable conditions, while the observations were actually taken at times when the oscillations due to electric trains often exceeded 1 mm. Under such conditions instantaneous values are out of the question and it is difficult to estimate mean 5-minute or 10-minute ordinates of an oscillating curve to 0·1 mm.

If we assume everything else perfect, the entries in Column A would represent errors in individual observations with the Jones instrument. It will be seen that the entry exceeds 5γ on only six occasions, two of these occurring in December, a season at which light is apt to be insufficient in the magnetic hut.

If the difference between the coil instrument and the Jones magnetometer be assumed the same as in 1923, the station difference becomes 64γ. A substantial difference is not surprising since Rucker and Thorpe in their 1891 survey found *H* at Kew to be smaller by about 150γ than at Ranmore, some 15 miles to the south of it.

§ 10. Comparisons with the Instruments of the Royal Observatory, Greenwich

The unifilar magnetometer Dover 140 and the dip circle Dover 74 were taken to and from Greenwich in an R.A.F. motor car, so as to expose them to a minimum of risk from mechanical shocks or electric currents. Observations were taken on October 19, 22, 24, 25, 26, 29, 31 and November 2, 1923. There are, unfortunately, no regular facilities for rapid travel between Richmond and Greenwich. This made the observing day rather short, as the light was apt to fail by 4.30 p.m. There is only one magnetic pavilion at Greenwich, and the observatory instruments for declination and horizontal force, which are distinct, permanently occupy two of the observing piers. The observations of horizontal force and dip with the Kew instruments were made on a pier in the south-east end of the pavilion, near that occupied by the Greenwich horizontal force instrument and near the south window. This necessitated interchanging the unifilar and dip circle usually once a day. On some of the days, while dip observations were in progress, Mr. W. M. Witchell, of the Greenwich staff, took declination observations with Dover 140 on a tripod stand in the enclosure surrounding the magnetic pavilion. No distant mark of known azimuth being available, Mr. Witchell employed an object on the observatory building, determining its azimuth by observations on the sun or the pole star. The mean from four sets of observations by Mr. Witchell was—

$$\text{Dover 140} - \text{Greenwich standard} \quad = +0'\cdot 5,$$

the plus sign indicating a more westerly declination. A subsequent intercomparison at Kew gave—

$$\text{Kew standard} - \text{Dover 140} \quad = +0'\cdot 6,$$

whence we should deduce—

$$\text{Kew standard} - \text{Greenwich standard} = +1'\cdot 1.$$

The conditions under which Mr. Witchell observed were not very favourable, and the results obtained on the different days varied somewhat largely amongst themselves.

Subsequently, much more consistent results were obtained at Greenwich by the Greenwich staff for the difference between the Greenwich standard and a magnetometer, Casella No. 181, belonging to Greenwich, which had been tested at Kew shortly before. During the test three declination observations had been taken with the instrument by the Kew staff. The results of these two comparisons were as follows :—

$$\text{Greenwich standard} - \text{Casella 181} \quad = +0'\cdot 1,$$
$$\text{Kew standard} - \text{Casella 181} \quad = +0'\cdot 2,$$

leading to

$$\text{Kew standard} - \text{Greenwich standard} = +0'\cdot 1.$$

The observations made at Kew with No. 181 were undesirably few, but the later comparison seems to deserve most weight. Still, I think all we are entitled to conclude is that the difference between the two standards is probably less than 1′, and may be nil. This, moreover, assumes that no sensible local disturbance exists at Greenwich, as the positions of the instruments during the comparisons there were not interchanged.

The dip circle, Dover 74, was compared directly at Kew Observatory with the Kew standard dip circle by Barrow, both before and after the observations taken with it at Greenwich. The sites of the two instruments were interchanged and use was made of a comparatively new pair of needles belonging to circle Dover 239, as well as of the pair belonging to circle 74 itself. The results obtained at Kew were as follows :—

From 13 observations with needles of circle 74, Barrow–Dover 74 $= +0'\cdot72$,
From 8 observations with needles of circle 239, Barrow–Dover 74 $= +1'\cdot08$.

The comparisons made at Greenwich resulted as follows :—

From 5 observations with needles of circle 74, Greenwich standard—Dover 74 $= +0'\cdot70$
From 6 observations with needles of circle 239, Greenwich standard—Dover 74 $= +1'\cdot40$

Assigning equal weight to the results obtained with the two pairs of needles, the final result is :—

Greenwich standard (inductor) — Kew Standard (circle) $= +0'\cdot15$.

The values for the Greenwich inductor corresponding with the dips observed with the Dover circle were deduced from measurements of the Greenwich curves made by the Greenwich staff.

The comparisons made between the Dover and Jones unifilars at Kew made the two in agreement to $0\cdot5\gamma$ at the time of the visit of the former to Greenwich, the visit being without any certain effect on the readings of the instrument. We may thus treat the H observations at Greenwich as if they had been made with the Kew standard itself. The corresponding values for the Greenwich standard were derived by the Greenwich staff from measurements of the Greenwich curves, standardized by the Greenwich instrument, with a provisional value for the distribution constant P. The results of the several observations are given below. The dates and the values obtained for the magnetic moment m of the collimator magnet of Dover 140 are added, as they have a certain significance.

Date.	Oct. 22.	Oct. 25.	Oct. 26.	Oct. 29.	Oct. 31.	Nov. 2.
Greenwich standard—Kew standard	$+6\gamma$, $+6\gamma$	$+7\gamma$	$+6\gamma$, $+6\gamma$	$+12\gamma$, $+12\gamma$	$+11\gamma$, $+12\gamma$	$+7\gamma$, $+24\gamma$
m	605·0, 604·9	604·9	604·9, 604·9	604·6, 604·7	604·5, 604·8	604·7, 605·0

When two observations were made on one day they were taken in immediate succession, the programme followed being vibrations, deflections, deflections, vibrations. Every pair except the last appeared very consistent. The last were interrupted and had to be rather hurried, as the instruments had to be packed by a pre-arranged hour. It was clear from the Greenwich curves that one at least of the last two observations must be faulty, so it was decided to reject both. The mean from the other nine observations is—

Greenwich standard — Kew standard $= +9\gamma$.

Subsequently the moment of inertia of the magnet of the Greenwich standard was re-determined by the Greenwich staff, and the provisional value accepted for P was revised in the light of the results obtained for the complete year. The application of the two corrections thus required lowered the readings of the Greenwich standard by 18γ, leaving—

Greenwich standard — Kew standard $= -9\gamma$.

It may be added that a subsequent intercomparison of the inertia bars employed at Greenwich and Kew, made by Mr. Finch, of the Greenwich staff, with the Watson apparatus at Kew, showed that if the same inertia bars had been in use at Greenwich and Kew we should have had—

Greenwich standard — Kew standard $= -3\gamma$.

Also it should be noticed that if three deflection distances were used for the Greenwich standard, instead of two, we should expect an enhanced value of H. The increase might be large enough to alter the sign of the difference observed.

During some of the H observations at Greenwich bright sunshine was experienced. To prevent this interfering with the temperature of magnet and thermometer, while still securing sufficient illumination for the verniers, was rather a puzzle, and the apparent consistency between the results obtained on the same day was much above my expectations when actually observing. It will be noticed, however, that the mean of the four differences obtained on October 29 and 31 exceeds by 6γ the mean of the five differences obtained on October 22, 25 and 26. If the observations had stopped on October 26 the result for the difference between the two instruments would have been less by 3γ than the final mean.

§ 11. Comparison with the Instruments of Valencia Observatory

The comparisons with the Valencia instruments, magnetometer Dover 139 and dip circle Dover 118, were made under specially favourable conditions. The instruments were brought over from Ireland to Kew Observatory by Mr. C. D. Stewart, who took all the observations with them. The comparisons extended from March 5 to 12, 1924. Mr. Stewart took twelve observations of D, six in either hut. Whilst he observed in the one hut, Mr. Francis observed in the other with the Jones unifilar magnetometer. The base values for the curves derived from these observations were as follows :—

Place of Observation.	*By Dover* 139.	*By Jones.*	*Excess of Jones.*
Old Hut	13° 6′·2	13° 5′·6	− 0′·6
New Hut	13° 5′·0	13° 5′·3	+ 0′·3
		mean ..	− 0′·15

Mr. Stewart took eight dip observations with his two needles, while simultaneous observations with the Kew–Barrow circle were taken by myself. Use was made of the east and west pillars of the new hut, the positions of the instruments being interchanged. The results obtained for the algebraic excess of the Kew standard over the Valencia instrument were as follow :—

−0′·55, +0′·76, −0′·20, +0′·92, −0′·51, −1′·39, −1′·19, −0′·17, mean −0′·29.

Mr. Stewart took ten observations of H, six in the new hut and four in the old. On eight of the occasions Mr. Francis took an observation with the Jones magnetometer, observing four times in each hut. The observations with the two instruments were not absolutely synchronous, because the vibration times of the magnets were not identical, and deflections were taken at two distances only with the Valencia instrument (that being the practice at Valencia), as against three with the Kew instrument. Thus, the comparison was effected through the H curves. The difference between the base line values derived from Mr. Francis' observations in the two huts, when reduced to a common temperature of the magnetograph room, was only $0{\cdot}5\gamma$. This was accepted as confirmatory of the result of previous observations showing no certain difference between the two huts. Taking a mean base line value from Mr. Francis' eight observations as that representative of the Kew standard instrument, the base line values calculated from Mr. Stewart's several observations, employing a provisional value for P, led to the following values for the algebraic excess of the Kew standard :—

-4γ, $+8\gamma$, -1γ, -10γ, -7γ, -3γ, -10γ, -9γ, -18γ, -12γ, mean -7γ.

The substitution for the provisional value of P of the value subsequently derived from all the observations of the year 1924 made a rise of 4γ in the values obtained for H with the Valencia instrument, and thus lead to the final result :—

Kew standard − Valencia standard = -11γ.

§ 12. COMPARISON WITH THE INSTRUMENTS OF STONYHURST COLLEGE OBSERVATORY

The observations at Stonyhurst were taken on July 23–26, 1924. The curves were standardized by observations taken before and after my visit by Fr. Cortie and Fr. Rowland, with the Stonyhurst magnetometer by Jones.

Taking the base line value as invariable and as determined by the Stonyhurst observers, the eight declination observations taken with Dover 140 showed the following departure from the curve values :—

$+0'\cdot 1,\ +1'\cdot 1,\ -1'\cdot 3,\ +1'\cdot 2,\ +0'\cdot 5,\ +0'\cdot 1,\ +1'\cdot 5,\ +0'\cdot 2$, mean $+0'\cdot 4$.

As the subsequent observations at Kew made the Dover instrument read $0'\cdot 6$ lower than the Kew standard we deduce :—

Kew standard — Stonyhurst standard $= +1'\cdot 0$,

the plus sign indicating a more westerly reading.

The results from the nine H observations made the Dover value lower than that derived from the curves by :—

$53\gamma,\ 60\gamma,\ 63\gamma,\ 67\gamma,\ 49\gamma,\ 61\gamma,\ 56\gamma,\ 57\gamma$, and 52γ, mean 58γ.

As the comparisons[1] at Kew made the Dover instrument in practical agreement with the standard, we obtain :—

Kew standard — Stonyhurst standard $= -58\gamma$.

This accepted a provisional value for the distribution constant of the Stonyhurst instrument. With the value finally accepted at the end of the year the difference was reduced to 56γ.

On the return of Fr. Cortie from the British Association meeting in Canada, a re-determination of the moment of inertia of the Stonyhurst collimator magnet was carried out at Kew, using the inertia bars employed for the determination of the moment of inertia of the Kew magnet. This showed a substantial reduction to be necessary in the accepted value of the moment of inertia of the Stonyhurst magnet. One or two other minor instrumental corrections were found to be necessary, the outcome being a reduction of the instrumental difference to[2] :—

Kew standard — Stonyhurst standard $= -24\gamma$.

The magnetic pavilion at Stonyhurst is small, and to obtain a good light, while excluding direct sunshine on the instrument, requires skilful manipulation of some curtains. A strange observer has doubtless more difficulty and less success in dealing with these minutiae than the home observer, and with the sun shining he may be unable to avoid rapid changes of temperature. This presumably accounts for the somewhat large fluctuations in the difference between the value of H observed with the Dover instrument and the value derived from the curves.

The Stonyhurst dip-circle Dover 159 was compared directly at Kew against the standard Barrow circle on April 14, 15, 16 and 17, 1924. Ten simultaneous observations were taken, the observers being Mr. R. E. Watson and myself. The piers, instruments and observers were all interchanged, so as to eliminate any observational eccentricities or local differences. The values obtained for the excess of Dover 159 over the Kew standard were as follows :—

$+1'\cdot 24,\ +1'\cdot 08,\ +0'\cdot 57,\ +1'\cdot 90,\ -0'\cdot 39,\ +2'\cdot 04,\ -0'\cdot 35,\ +2'\cdot 12,\ +1'\cdot 34,$
$+2'\cdot 14$, mean $+1'\cdot 17$.

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[1] In reducing the observations taken in July and August, 1924, at Kew, Stonyhurst and Greenwich, use was made of a provisional value of P—derived from all the observations of 1923—which gave for H a value lower by 1γ than that given by the value obtained for P from the observations at Kew from July to December, 1924. This made Dover = Jones, *see* Table III.

[2] In a letter to the Director of the Meteorological Office Fr. Roland says—"in the case of the horizontal force the result cannot be taken as more than provisional, as it has not yet been possible to carry out certain supplementary observations, suggested by Dr. Chree, which may involve a further modification of the figure."

§ 13. Comparison with the Instruments of Eskdalemuir Observatory

The standard instruments at Eskdalemuir are a unifilar magnetometer Elliott No. 60, and a dip inductor. There are two magnetic huts at Eskdalemuir, similar in general character to those at Kew but considerably larger. An observer accustomed to the Kew huts finds them easier to work in than the pavilions at Greenwich and Stonyhurst. There is, however, a compensating disadvantage, so far as the comparison of ordinary instruments is concerned. The Eskdalemuir magnetographs record two rectangular components N and W, instead of D and H. Neither curve can have its base line value determined without absolute observations of both D and H, and observational error in either D or H affects both curves. On the first day of the comparisons, July 27, only a few D observations were made, and the unfortunate necessity of fitting a new suspension led to their not being synchronous with observations taken by Captain Absalom with the Elliott instrument. A comparison could have been made through the curves, but not very satisfactorily in the absence of the absolute observations of H requisite for the determination of base line values for the day. Under the circumstances it has seemed best to discard the observations taken on the 27th. On the four following days, July 28–31, observations of both D and H were taken on each day, the total numbers being 7 of D and 14 of H. The observations on July 28 and 29 were taken on pier No. 2, the central pier of the west hut, those of July 30 and 31 on pier No. 5, the central pier of the east hut. The regular Eskdalemuir observer, Mr. Beck, observed with the Elliott instrument, on the first two days on Pier No. 5, on the second two days on Pier No. 2. The observations with the two instruments were not strictly synchronous, and the comparison was effected through the curves.

To compare the instruments, it is only necessary to assume that the curve base line values were invariable throughout any one day. But if we assume in addition that they remained invariable throughout the four days, we can obtain estimates of the difference in the values of D and H at the central piers of the two huts from each of the instruments compared. The estimates thus obtained were as follows :—

	By Elliott 60.	*By Dover* 140.	*Mean.*
Excess of D in east hut	$+0'\cdot38$	$+0'\cdot68$	$+0'\cdot5$
Excess of H in west hut ..	$+4\cdot2\gamma$	$-1\cdot9\gamma$	$+1\gamma$

If the D observations taken with the Dover instrument on the 27th were included and the same curve base values were assumed for that day as for the subsequent days, the final mean value for the excess of D in the east hut would be reduced to $0'\cdot3$.

Observers of the Carnegie Institution of Washington, Captain E. Kidson and Mr. W. C. Parkinson, observing at Eskdalemuir with the Institution's instruments in 1915 and 1922, obtained from observations on the same two piers the following values for the differences between the two huts :—

	Kidson (1915).	*Parkinson* (1922).
Excess of D in east hut	$+0'\cdot8$	$+0'\cdot8$
Excess of H in west hut	$+\ 3\gamma$	$+0\cdot2\gamma$

The differences obtained on the several occasions have the same sign. But a more elaborate investigation would be required to justify a final conclusion even as to the sign of the difference.

So far as the intercomparison of the two instruments is concerned, it is immaterial whether a difference existed between the two houses, so long as it was the same throughout. Allowing equal total weight to the observations in the two houses, the final results obtained were :—

$$\text{Elliott 60} - \text{Dover 140} = +1'\cdot0 \text{ in } D$$
$$= +9\gamma \text{ in } H$$

Having regard to the results of the comparison of Dover 140 with the Kew standard, we thence obtain :—

$$\text{Kew standard} - \text{Eskdalemuir standard} = -0'\cdot 4 \text{ in } D$$
$$= -9\gamma \text{ in } H$$

In view of the conflicting nature of the results as to the difference between the two huts, and the complications arising from the special nature of the magnetographs, details are not given of the individual observations.

As already explained, the dip circle Dover 74 belonging to Eskdalemuir was compared at Kew against the Kew standard. A number of observations were taken with Dover 74 at Eskdalemuir by Captain Absalom, both with its own needles and with the needles of circle No. 239. Captain Absalom observed on a number of occasions, from January to July, 1924, on pier No. 3 in the west hut, and during September, 1924, on piers Nos. 5 and 6 in the east hut. The results he obtained for the *excess* of the dip given by circle 74 over that given by the dip-inductor, were as follows :—

		Mean.
Circle in west hut with its own needles	$+0'\cdot 81$	$+1'\cdot 00$
Circle in west hut with needles of circle 239	$+1'\cdot 19$	
Circle in east hut with its own needles	$+0'\cdot 77$	$+0'\cdot 90$
Circle in east hut with needles of circle 239	$+1'\cdot 03$	
Final mean		$+0'\cdot 95$

The observations made at Kew taking a mean from the two pairs of needles, gave :—

$$\text{Kew standard} - \text{circle 74} = +0'\cdot 90.$$

Thence, assuming no change in circle 74, we obtain :—

$$\text{Kew standard} - \text{Eskdalemuir inductor} = +1'\cdot 85.$$

The inductor is normally used on pier No. 6 in the east hut. If the difference $0'\cdot 1$, which the observations with circle 74 suggest between the two huts, were accepted as real, the excess of the Kew standard would be reduced to $1'\cdot 8$, but a more exhaustive comparison would be necessary to justify its acceptance.

The dip circle comparison represented a great deal of work, but the results are not wholly satisfactory. In the observations at Eskdalemuir the larger dip was obtained with the needles of circle 239, the mean excess being $0'\cdot 32$. In the observations at Kew, on the other hand, the larger dip was obtained with the needles of circle 74 itself, the mean excess being $0'\cdot 36$. Again, of the two needles of circle 74, No. 1 gave on the average the higher dip at Kew, but the lower dip at Eskdalemuir. Moreover, this does not appear to represent any idiosyncrasy in the observers at either station. It had been my practice during a succession of annual visits to Eskdalemuir to take observations with circle 74, and on these occasions, as later with Captain Absalom, needle No. 2 gave the larger mean dip.

It will be remembered that the observations made at Greenwich with the same circle 74 as intermediary, made the dip from the Greenwich dip-inductor exceed that from the Kew-Barrow circle by $+0'\cdot 15$. This suggests an excess of $2'\cdot 0$ in readings from the Greenwich inductor over those from the Eskdalemuir inductor. This seems surprisingly large, in view of the claims made for the accuracy of dip-inductors. If circumstances permitted, a direct comparison of the two inductors would be highly desirable.

§ 14. SUMMARY AND CONCLUSIONS

If we take the final results obtained with new values of moments of inertia, etc., the following are the results for the excess of the Kew standard over the corresponding standard at the station specified, a plus sign in D implying a higher westerly reading.

	Declination.	*Horizontal force.*	*Inclination.*
Greenwich	$+0'\cdot1$	$+\ 9\gamma$	$-0'\cdot15$
Valencia	$-0'\cdot15$	-11γ	$-0'\cdot3$
Stonyhurst	$+1'\cdot0$	-24γ	$-1'\cdot2$
Eskdalemuir	$-0'\cdot4$	$-\ 9\gamma$	$+1'\cdot8$

Many acknowledgments are due for observing facilities and for assistance: to the Astronomer Royal, Mr. Greaves and Mr. Witchell, at Greenwich; to Fr. Cortie and Fr. Rowland, at Stonyhurst; to Captain Absalom and Mr. Beck, at Eskdalemuir; to Mr. C. D. Stewart, of Valencia Observatory, and to Mr. R. E. Watson and Mr. B. Francis, members of the Kew staff. A large amount of extra observing was entailed at all the observatories, especially Stonyhurst and Eskdalemuir, and a very considerable amount of computing work, more especially at Eskdalemuir. At Stonyhurst I was accommodated as a guest in the College, and everything was done to make my visit a pleasant one. A like anxiety to lighten my labours was displayed by Captain Absalom at Eskdalemuir. Mr. Stewart had the burden of conveying the Valencia instruments from and to Valencia, and of observing with them at Kew.

In the course of the paper instrumental points have been discussed in considerable detail, partly for the guidance of those to whom it will fall to make similar comparisons in the future, and partly to enable the intelligent reader to judge for himself what the real significance is of the apparent differences between different instruments. The comparisons described in the present paper, it should be understood, were favoured by exceptionally quiet magnetic conditions. To myself the natural inference seems to be that when observations at a station are limited to a few days, the differences obtained between two ordinary unifilar magnetometers cannot really be relied on to 1γ in H or to $0'\cdot1$ in D. It appears doubtful, to say the least of it, whether changes of these amounts may not be expected to occur in most unifilars in the course of a few months. To apply corrections to 1γ in H or to $0'\cdot1$ in D to a unifilar as the result of a comparison with another unifilar, would seem to postulate higher accuracy than it is at present reasonable to expect.

APPENDIX

Uncertainties in Published Values of H at Kew Observatory

It is generally agreed that it is desirable that magnetic observations should be reduced shortly after being taken. If the result appears abnormal, a satisfactory explanation is most likely to be obtained whilst the observer's memory is fresh. If the observations of a year are to be reduced promptly and in a uniform way, the only plan is to employ the values of the constants available at the beginning of the year. This was the practice at Kew Observatory. For example, so long as the values accepted for P and Q depended on the observations of a single year, the values employed in the reductions were those of the previous year. When the values of P and Q for the year became available the individual calculations were not repeated, but a uniform correction was applied to all the values, monthly or annual, worked out for H. So again, if a revised value of the moment of inertia became available, in the course of a year, use of the old value was continued in the reductions, but the difference between this and the new value was allowed for at the end of the year. New values of the moment of inertia were thus utilized for 1910, 1915 and 1922, though they were not available at the beginning of these years.

In addition to the changes in the accepted values of P and Q and of the moment of inertia, there might be other changes to allow for. These included changes in the values accepted for the deflection distances. The deflection bar was measured in 1891 with the appliances—exiguous to modern ideas—then available at Kew Observatory, and again in 1904, 1908 and 1909, at the National Physical Laboratory with the following results :—

Nominal Distance.	Unbent Lengths.			Bent.
	1891.	1904.	1908 and 1909.	1909.
cm.	cm.	cm.	cm.	cm.
22·5	—	22·494	22·495	22·497
30	29·992	29·991	29·993	29·995
40	39·988	39·990	39·991	39·994

In the earlier years no allowance was made for the bending. The measurements made in 1908 and 1909 agreed to the nearest 0·001 cm.

Deflections were limited to 30 and 40 cm. until the middle of 1908. A third distance, 22·5 cm., was then introduced, with the intention of employing results from the three distances in 1909. But this was not actually done until 1910, so as to include in one correction the results of two new departures, i.e., the substitution of three deflection distances for two and the introduction of a new value for the moment of inertia. Allowance had been made in the final values of 1909 for alterations in the accepted deflection distances. This was explained in the Report for the year 1910, where it was stated that if use had been made in the earlier years of the same procedure and constants as for 1910, the published values of H would have been lowered by 6γ in 1908 and by 4γ in 1909. In this case the corrections partly neutralized one another, thus reducing the discontinuity -12γ which the change in moment of inertia, if taken alone, would have produced in 1910.

In 1915, on the other hand, the correction necessary for the change in P and Q from the previous year was of the same sign as that required by the change of moment of inertia, bringing the total correction up to $-8 \cdot 5\gamma$. In this instance -8γ was applied to the results of the first six months, and -9γ to those of the last six.

On the last occasion, 1922, when a correction for change of moment of inertia was required, the change in the values of P and Q since 1921 was exceptionally large, and the necessary correction was opposite in sign to that entailed by the change of moment, the balance being only -1γ.

The fact that the moment of inertia on each occasion showed a lower value than previously suggests gradual attrition through continued use. If we may judge from a long series of yearly values of moment of inertia from 1868 to 1904, published by Dr. N. A. F. Moos,[1] for the Bombay magnet, very substantial falls have occurred elsewhere. If the cause is wear and tear, the loss will naturally take place gradually, at a fairly uniform rate. Whether it was really so at Kew it is impossible to say. It is possible, for example, that a sensible proportion of the total change—equivalent to a fall of 9γ in H—observed between 1915 and 1922 may have been caused by the dropping of the magnet in 1916, which has been referred to in the text. The accident occurred after the observation on May 18, 1916; the magnet fell soft on floorcloth, and at the time was supposed to be none the worse. On the occasion, however, of the next observation it was noticed that the horizontal wire of the telescope did not cut the vertical scale of the magnet where it had done before, but nothing was thought of this until some weeks afterwards, when the Superintendent happened to take an observation. A careful examination was then made and a slight deformation was detected in the stirrup. This was rectified by Mr. Dover and the magnet was

[1] *Magnetic Observations made at the Government Observatory, Bombay, for the period* 1846–1905, *and their discussion,* Part I, pp. 19 and 20.

rebalanced. At the same time a small change was made in the way of setting the deflection carriage, so as to reduce the differences between the deflection angles obtained on the two arms of the bar. It was nearly the middle of September before these operations were concluded. A careful analysis was made of base line values, times of vibration and deflection angles, but no definite conclusion could be drawn except that the magnetic moment of the magnet had not suffered. Matters were complicated by the fact that abnormal values were obtained for P and Q from the year as a whole. The departure from the previous year was exceptionally large and it was the earlier months of 1916, prior to the known accident, which seemed mainly accountable for it. If it had been due to the changes intentionally introduced in 1916, we should not have expected the values of P and Q for 1917 to be almost identical with those for 1915, as was actually the case.

Perhaps the most important use of absolute values of H at an observatory is for the determination of secular change. So far as secular change from one year to the next is concerned, it might be better to neglect changes of moment of inertia altogether, if as slow as those at Kew, rather than have re-determinations every five or ten years, with the consequent introduction at these intervals of time of sensible discontinuities in H. But magnetic surveys are only made at intervals of time, and the values of secular change employed in the construction of charts usually represent mean values from a considerable period of years. It is thus probable that the balance of advantages, even for survey purposes, lies with the introduction of new values of the moment of inertia when they become available. The ideal thing, of course, assuming adequate staff time, would be to have during each year a sufficient number of inertia observations to furnish an accurate yearly value for the moment of inertia. An obstacle to this is that for inertia experiments it is necessary to employ a stouter suspension—with consequently enhanced torsion co-efficient—than is required for ordinary H observations. The desirability of keeping the torsion correction as low as possible will be recognized by all who appreciate the uncertainties entailed.

Of the following three sets of values for secular change since 1910, (A) assumes the published values of H ; (B) applies to the published values of H corrections reducing them to what they would have been if the values of P and Q had been assumed invariable ; (C) applies a further correction based on the hypothesis that the moment of inertia fell at a uniform rate during each period of years as calculated from the values observed in 1910, 1915 and 1922.

The unit is 1γ :—

	1910.	1911.	1912.	1913.	1914.	1915.	1916.	1917.	1918.	1919.	1920.	1921.	1922.	1923.
(A) ..		−1	−4	+7	−17	−25	− 6	−20	− 8	−13	−6	−11	− 5	0
(B) ..		0	−4	+2	−17	−21	−18	− 7	−14	−12	−4	− 7	−12	0
(C) ..		−1	−5	+1	−18	−17	−19	− 9	−15	−13	−5	− 9	− 4	0

So far as a mean value of secular change from, say, ten years is concerned, it is immaterial which procedure is followed. It cannot be said that any of the three procedures supplies a smooth secular change such as we should expect from the contemporaneous changes of declination.

Tables of the mean yearly values of H as published for the ten previous years, as well as the current year, will be found in the Year-books[2] for 1921 and 1922.

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[2] *British Meteorological and Magnetic Year-Book*, 1921, Part IV, p. 47 ; and *The Observatories' Year-Book*, 1922, p. 331.