RESOLUTIONS

1. The conference pauses, in respect and admiration, to pay tribute to the devoted and productive career of the late Dirk Brouwer, and to recognize his leadership and his many contributions in the field of astrometry.

2. The conference recommends the standardized use of the Bonner and Cordoba Durchmusterung numbers to designate stars whenever possible in all publications and machine-readable data files. The ambiguity at $-22°$ shall follow the convention established in the Henry Draper Catalogue, Harvard College Observatory, Vol. 91, p. 12.

3. The conference recommends for consideration by Commission 8 of the International Astronomical Union the introduction of a general, standardized form of designation for all stars outside the Durchmusterungs (e.g. $\pm^{\circ}\!.\,^{\prime}\!.\,^{\prime\prime}\!.$, $^{h}\!.\,^{m}\!.\,^{s}\!.$, $^{mag}\!.$) including provision for the assignments.

4. The Atkinson mirror transit circle has great potential value for fundamental astronomy. The conference would welcome the completion of instruments of this type, and their use in fundamental observing programs.

5. The conference records its sense of the importance of the programs of the U. S. Naval Observatory for the improvement of the fundamental systems of reference stars in both hemispheres.

6. The conference urges that the southern counterpart of the AGK 3 photographic program be completed as soon as possible, both for astronomical and geodetic requirements.

7. The conference recognizes the need for further study of the methods of overlapping plate reduction techniques, with a view to reaching an optimum, practical mode of operation.

8. The conference records its appreciation of the careful and eminently successful efforts of the Organizing Committee, of the excellent facilities provided by our host, the University of Maryland, and of the support of the National Science Foundation, which enabled us to deliberate upon the current problems in the construction and use of star catalogues.

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Systematic Errors of Meridian Instruments*†

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With a conventional transit circle a number of systematic errors are possible, and although they might seem unlikely there are often clear indications that in fact they are present. The differences between different catalogues confirm this unmistakably, but the data are never sufficient to pinpoint them adequately. Some of the terms are undiscoverable by any methods so far used. Fourier analyses of the results can, however, clarify the position somewhat, and in the case of systematic division errors may fully determine even those coefficients which do at first sight appear undiscoverable. The flexure situation is very much better with the "mirror transit circle"; the flexure in right ascension is readily observable in full and is small in the axis made for the Royal Greenwich Observatory; there is every reason to expect that the flexure in declination would be considerably smaller again, and almost certainly less than $0\overset{''}{.}01$.

AMONG the systematic errors which afflict meridian instruments, flexures of various kinds are perhaps the most difficult to analyze properly. The flexure in declination has been studied extensively, but rather inconclusively; it is certainly not negligible, although it appears at first sight as though it should be relatively easy to make it so because "flexure" is here an abbreviation for differential flexure. If the central "cube" of the instrument is machined (inside as well as outside) to proper symmetry, if the two tubes are machined to be identical, and if the lens assembly and micrometer assembly have equal weights and have their centers of gravity at equal distances from their mounting flanges, then the two halves of the telescope should bend equally under their own weights; if they do, and if, in addition, the plane of the micrometer and the second nodal point of the lens are at equal distances from their respective mounting flanges, the line joining the micrometer (midpoint) to the nodal point will remain parallel to itself, and the differential flexure will be zero.

In practice, some of these requirements are considerably easier to satisfy adequately than others. The short distances from the micrometer and the lens to their respective flanges are evidently not very critical, since nearly half the focal length has to be added to

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† A preliminary form of this paper was presented at the 1966 College Park Conference. The view that many of the main results dealt with should probably be ascribed to division errors, rather than to flexure, came later. It seemed best to rewrite the paper in this sense; the abstract has been modified, but the title is still entirely appropriate.

each. The over-all lengths of the tubes can be made equal, to close tolerances, without difficulty, and their common axis can also be made to pass very nearly through the line of pivots. Their wall thicknesses (which largely determine their weights as well as their stiffnesses) are more difficult, but the greatest difficulty is probably provided by the internal dimensions of the central cube. Admittedly, even when no internal surfaces of either the cube or the two tubes are machined at all, the horizontal differential flexure (measured in the standard way by the two collimators) is generally less than 1″, and when the interiors are machined, as they are, for example in the Washington 6-in., the value can be further reduced; the observed value for this instrument, averaged over a few years, was sometimes less than 0″.1, though not in all arrangements of the components. But this is still undesirably large, as a semipermanent systematic term attaching to one of the best of the few fundamental instruments in use in the whole world. Judged by modern needs, it ought to be reduced; it ought also to be more constant, and especially ought to be unaffected by interchanging the lens and micrometer. (In addition, as we may see, the true significance of the parameter actually measured is uncertain.)

In principle, one could feed the relevant dimensions, weights, elasticities, etc., of any instrument into a computer and actually predict the flexure. The answer, if one fed in the specified values of a correct design, would clearly be zero, this procedure being one test for "correctness." One could then also feed in the specified tolerances, and obtain limits within which the flexure ought to lie. What one needs to know, however, is where it actually does lie, and this involves feeding in the true dimensions. They will fall well within the specified tolerances in some cases, barely within them in some, and probably outside them in some if the final acceptance inspection has been inadequate; there may also be dimensions, important or even vital for the computation, which were taken for granted and were not actually specified at all. (For example, the stresses at re-entrant angles, whether on external surfaces or on internal ones, can become extremely large if the angles are very sharply machined, and guns have blown their breech ends off, owing to neglect of this factor, even when all the more obvious stresses had been quite adequately allowed for. The curvatures of *all* re-entrant "angles" should be finite and specified.) Almost any structure that is firmly bolted together is also likely to have regions where the stresses exceed the elastic limit locally, and these regions cannot be known in any detail and may upset the theory. And since computation is thus unreliable both because a full theory would be very difficult and because some essential dimensions (and elasticities) are usually lacking, it is in practice necessary to measure the flexure directly.

This, however, has its drawbacks too. Observations of the collimators (Bessel) can readily give the term proportional to $\sin z$, where z is the zenith distance north, if we assume that this term is the only one; but it is often found that the value so obtained does not fit the astronomical determination of the equator point. And if we assume a complete Fourier series in z we find on investigating in detail that any one method usually yields values for only half the coefficients. Reflection of star images in a mercury pool, for example, cannot reveal any flexure terms that are antisymmetrical about the horizon, so that the coefficients of the odd cosines and even sines of the zenith distance remain unknown; reversing the instrument leaves all the sine coefficients unknown; and so on. Combination of various methods naturally fills some of the gaps, but not all; no combination of well-known methods will give the coefficient of a term in $\sin 2z$, if this is present (Atkinson 1955). [This coefficient should readily be determinable by the pentag method (Atkinson 1960), but this has never actually been done.] More serious, however, than this incompleteness is the fact that (in general) instruments fail to give repeatable results, where such tests are continued or repeated over lengthy periods, and in the last analysis it is sometimes decided, in effect, to jettison all these test data and rely only on the astronomical fact that the pole and the equator (which emerge, independently, from the observations) are 90° apart; this suffices to determine the coefficient of $\sin z$ if that is the only term, but if the value differs from the Bessel one this cannot well be true. The "astronomical" determination is probably the more serviceable of the two, since stars are not observed at zero altitude, but it is not really satisfactory; what is required is a knowledge of all relevant Fourier coefficients.

Where any long series of tests has been made, at least on a conspicuously good instrument such as the Washington 6-in., a Fourier analysis taking proper account of the effects of reversal, interchanging eye and object ends, and so on, may be valuable, even though some coefficients cannot be explicitly obtained. There is, in fact, more repeatability in the behavior of this admirable instrument than may at first sight appear, and such an analysis can to some extent pinpoint the nonrepeatable residues even though it cannot discover those Fourier coefficients which are "immune" to the tests used.

For the purpose of making such an analysis, we write the flexure as positive if the observed zenith distance north is greater than the true ("true" being as the rays actually reach the instrument, i.e., after correction for refraction and variation of latitude, as well as for apparent place). This convention is used no matter whether the instrument is arranged clamp east or clamp west; we have in all cases

$$\Delta z \equiv z_{\text{obs}} - z_{\text{true}}, \tag{1}$$

with z positive north of the zenith. When the instrument is arranged clamp east we also define the flexure

TABLE I. Differences $\frac{1}{2}$ (E–W) in declination, Washington 6-in., 1910–1918.

δ °	z °	Pos. II, 1 ″	Pos. I ″	Pos. II, 2 ″
− 25	−64	+0.08	+0.26	+0.32
− 15	−54	+0.20	+0.27	+0.23
− 5	−44	+0.24	+0.26	+0.34
+ 5	−34	+0.14	+0.24	+0.28
+ 15	−24	+0.14	+0.14	+0.24
+ 25	−14	+0.16	+0.04	+0.28
+ 35	− 4	+0.10	+0.10	+0.07
+ 45	+ 6	+0.13	+0.12	−0.06
+ 55	+16	+0.15	−0.03	+0.16
+ 65	+26	+0.12	+0.18	+0.08
+ 75	+36	+0.21	+0.11	+0.18
+ 85	+46	+0.26	+0.22	+0.08
+ 95	+56	+0.18	+0.14	+0.21
+105	+66	+0.20	+0.18	...

as

$$\Delta z_E=(z_0-z_t)_E=a_0+a_1\cos z+a_2\cos 2z+\cdots +b_1\sin z+b_2\sin 2z+\cdots, \quad (2)$$

where the arguments of the cosine and sine terms need not, of course, be known with any accuracy; even a "large" error in the nadir reading would be quite immaterial there (though it would affect the a_n). If we now suppose the instrument, after setting on some star at z, to be rotated 180° about a vertical axis, the physical flexure must be unchanged, but its effect on the zenith distance north has changed sign; in addition, to get back to the same star, z must be changed to $-z$ in all the Fourier arguments. Accordingly, for clamp west we must write

$$\Delta z_W=(z_0-z_t)_W=-a_0-a_1\cos z-a_2\cos 2z-\cdots +b_1\sin z+b_2\sin 2z+\cdots. \quad (3)$$

Thus if we average the clamp east and clamp west observations for any star (*straight* mean of the mean E and mean W results, even if the weights are unequal), we have

$$\tfrac{1}{2}[z_0(E)+z_0(W)]=z_t+b_1\sin z+b_2\sin 2z+\cdots, \quad (4)$$

and the true value is falsified by the error

$$E_1=b_1\sin z+b_2\sin 2z+\cdots, \quad (5)$$

which is not eliminated by reversal. (If the E and W weights are unequal, for some star, and if the *weighted* mean of E and W is taken as its true place, even the a_n are not eliminated.)

If all the coefficients in (2) are zero except b_1, and if we set on the two collimators in turn, after they have been set on each other, we have

$$z_0(N)-z_0(S) =180°+b_1\sin(90°+\epsilon)-b_1\sin(-90°+\epsilon) \quad (6)$$

if ϵ is the tilt of the collimators. ϵ will be small, so that

$$\tfrac{1}{2}[z_0(N)-z_0(S)]-90°=b_1. \quad (7)$$

This, the standard way of determining the "horizontal flexure," is still valid if the coefficients of the cosine terms are not zero, since $\cos nz=\cos(-nz)$. If, however, the higher sine harmonics are not zero, we have

$$\tfrac{1}{2}[z_0(N)-z_0(S)]-90°=b_1-b_3+b_5-\cdots, \quad (8)$$

and if the observations are corrected on the assumption that this observed quantity is the coefficient of $\sin z$ we are left with the error

$$E_2=(b_3-b_5+\cdots)\sin z+b_2\sin 2z+b_3\sin 3z+\cdots, \quad (9)$$

which still contains all the b coefficients except b_1, and even contains a term in $\sin z$ if other odd harmonics are present.

The suggestion (also due to Bessel) that the full form of the flexure function could in principle be discovered if similar observations were made with pairs of collimators inclined at large tilts to the horizontal is not quite correct; one is always concerned with two z values 180° apart, and the subtraction always removes all the even harmonics, both sines and cosines, whatever their coefficients. Thus while the purely astronomical test is sufficient if there are no harmonics above the first, tilting the collimators (which does not seem ever to have actually been undertaken) still leaves half the higher coefficients undiscovered, if they exist.

One can perhaps make an estimate of the probable magnitude of the b coefficients if one considers in more detail the a_n which are eliminated; any quantity which is eliminated by taking the mean of two observations can be directly evaluated by taking their difference, and it seems reasonable to believe that the b_n should in general be of the same order of magnitude as the a_n determined by this means.

We consider therefore the differences $z_0(E)-z_0(W)$, where evidently

$$\tfrac{1}{2}[z_0(E)-z_0(W)]=a_0+a_1\cos z+a_2\cos 2z+\cdots. \quad (10)$$

Since all the unknown z_t have now canceled out, (10) is formally an equation of condition for determining any selected set of a_n, though the observations do not run over a sufficient range of z to make the selection itself unique.

We apply (10) to the E–W differences published for the Washington 6-in., for the period 1910–18 (U. S. Naval Observatory 1927, p. 75); the values are given in Table I.

In Table I, values of $\delta > 90°$ refer, as usual, to subpole observations, and the δ values thus represent also zenith distances north except for a constant correction equal to the latitude; we have inserted the z values, taking the latitude as +39°. The designations I and II indicate exchanges of the lens and eye end; "Position I" was the arrangement used from 1913 October to 1917 June; "Position II, 1" was used from 1910 October to 1913 October; and "Position II, 2" (the same as II, 1 so far as eyepiece and lens are concerned) was used from

1917 June to 1918 August. (This third period was only worked to fill gaps, and the weight is low.)

As they stand, the figures suggest a nearly random scatter, rather than any clear trend; the three series also appear rather discouragingly different, if there is any trend. But the position is improved when we note that if (10) is in fact to hold, the value for any $-z$ must be the same as for the corresponding $+z$; reflecting the values about $\delta=39°$ should therefore put all points of any one series onto a single curve. Figure 1 shows the result of this reflection for the first series (Position II, 1), and it is evident that the north and south points do indeed lie practically on the same curve. The series

$$\tfrac{1}{2}[z(E)-z(W)]=25.2-10.3\cos z-1.0\cos 2z-1.8\cos 4z$$
$$-0.6\cos 6z+1.6\cos 8z-3.2\cos 12z, \quad (11)$$

(unit 0″.01) which was reached by trial and error, represents the observations with residuals whose sum (for 14 terms) is $v]=13$. However, this is probably not the best series to adopt; the sixth harmonic (a relic of earlier tries) is clearly unimportant, and there is good reason to suppose that the first harmonic is unreal, since observations with the vertical collimator gave the "vertical flexure," for the period in question, as only $-0″.01$. (The "vertical flexure" is just a_1 if there are no odd harmonics above the first; if there are, the observed quantity is $a_1+a_3+a_5+\cdots$.) Since, then, $-10.3\cos z-0.6\cos 6z$ can be pretty well represented, over the range 0–60°, by a series involving only a_0, a_2, a_4, and a_8, it was decided to make a fresh solution, involving only these four coefficients and a_{12}, and to do it by fitting exactly the five normal points given by the means for $z=4°$ and 6°, 14° and 16°, 24° and 26°, 44° and 46°, and 64° and 66°. The result is

$$\tfrac{1}{2}[z(E)-z(W)]=18.5-4.7\cos 2z-2.2\cos 4z$$
$$+1.5\cos 8z-2.9\cos 12z, \quad (12)$$

and the sum of the 14 residuals is $v]=15.8$ (with two fewer unknowns than before). This curve is plotted in Fig. 1; only four of the residuals exceed 1.0, and the observations were only given to the nearest unit anyway. The two largest residuals (-2.4 and $+2.7$) are on opposite sides of the steepest region, and it is clear that a full least-squares solution would produce no appreciable further improvement unless one decided to introduce further terms again. It seems improbable that the good fit can be an accident.

Comparison of the two series leaves little doubt that one cannot get any satisfactory series unless one admits either a first harmonic or a second one; and since the vertical collimator results indicate that (12) is to be preferred to (11) we conclude that this instrument had (over the period in question) a $\cos 2z$ term in its flexure which is fairly well determined. This does not, of course, prove that there is also an appreciable $\sin 2z$ term, but it leaves it a very real possibility. (Indeed, owing to the general shape of the instrument, b_1 is always considerably larger than a_1, and it would not be surprising if all the b_n were larger than the corresponding a_n; if there were complete N–S symmetry when the instrument is vertical, *all* the a_n would be zero.)

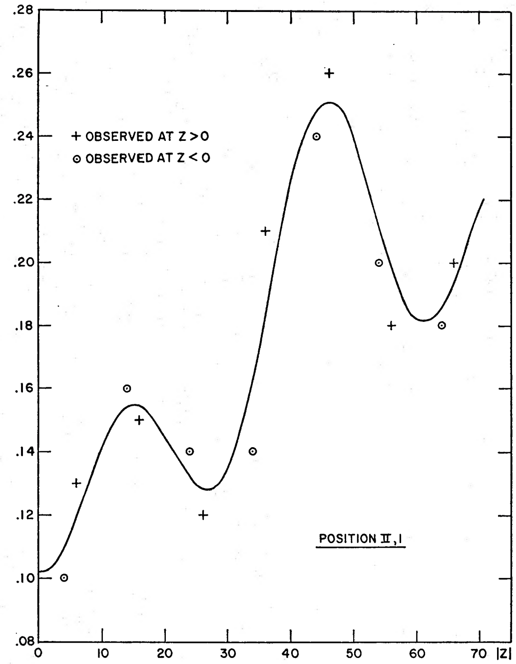

FIG. 1. Observed $\frac{1}{2}(\delta_E-\delta_W)$, Washington 6-in., 1910–13.

The next most important harmonic is plainly the twelfth; no matter what even harmonics one includes, a term $-0″.03\cos 12z$ in the E–W differences will be needed, and its reality can hardly be questioned in view of the good agreement between north and south results. It does, however, seem improbable that the flexure of even a very complicated structure should have so high a harmonic as the twelfth standing out in this way, and after some consideration it was realized that the term might arise, alternatively, from an error in the adopted division errors. Everything that has been said, above, about the behavior of flexure coefficients on reversal applies equally well to division errors, except that since there were four microscopes, spaced at 90° intervals, the division errors should repeat after 90°, and only harmonics of order $n=4m$ can arise in this way. The possibility that in fact they all do so is considerably strengthened when we consider the results for "Position I" and "Position II, 2"; these are plotted in Fig. 2, with the values for negative z reflected about $z=0$ as before. The conclusions are less clear in these cases, because there are now substantial N–S discrepancies and (10) evidently does not hold. But there is still evidence for a term with roughly a 30° period, and we see that the change from Pos. II, 1 to Pos. I reversed its sign, and the change from Pos. I to Pos II, 2 reversed it again. This could not happen, with any even harmonic however arising, if there were no difference between Pos. I and Pos. II except the interchange of eye end

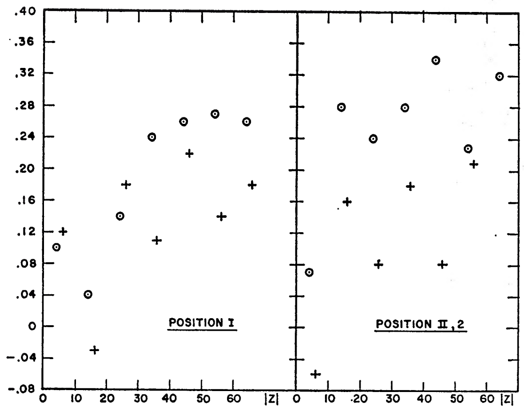

FIG. 2. Observed $\frac{1}{2}(\delta_E-\delta_W)$, Washington 6-in., 1913–17 and 1917–18.

and lens; but in fact the circle was also moved at the dates of these exchanges. In response to an enquiry, Dr. Adams very kindly looked up the old records and reported that the nadir readings, for the three periods in question, were

Pos. II, 1	Pos. I	Pos. II, 2
179°56′	45°2′	330°54′

(i.e., just the values of the parameter o quoted on p. 74 of the Publication), and clearly so far as a 12th harmonic in the division errors is concerned the two Pos. II situations were indeed equivalent and the Pos. I one was half a period out of phase.

One would expect that the circle shift of 180°+45°, as between Pos. II, 1 and Pos. I, would also reverse a fourth harmonic, and would leave an eighth one unchanged. However, the N–S difference which is evident in Pos. I (but seems to vary with z) makes even the twelfth harmonic hard to determine here, and the smaller fourth and eighth ones cannot even be estimated with any assurance. It seems fair to say, though, that these results are at least as consistent with the assumption that all three harmonics are purely due to division errors, as with the assumption that they are to a small extent due to flexure. The comparison with Pos. II, 2 is less rewarding, partly because the results here were of low weight but partly also because the circle shift of 150° (compared with II, 1) means not only that the two cosine coefficients should be multiplied by cos600° and cos1200°, i.e. $-\frac{1}{2}$, but also that ±0.866 of the two totally unknown sine coefficients b_4 and b_8 should now appear as cosine ones; the effect of the actual division errors is thus unpredictable for Pos. II, 2. At least, however, these results also do not forbid the assumption that the fourth and eighth harmonics, as well as the twelfth, arise purely from slightly wrong corrections for the division errors. We cannot exclude the possibility of small odd harmonics in the flexure, since they can be adequately replaced over this range by combinations of even ones, and the N–S discrepancies have still to be accounted for; but provisionally the most likely conclusion seems to be that the flexure contains no appreciable harmonics at all above the second, but does contain that one, perhaps in both the sines and the cosines.

Since rotating the circle by a quarter period (of any one harmonic) changes sines to cosines and vice versa, one has an interesting possibility of improving the larger periodic terms in the division errors, whenever they need this. The cosines, of course, are immaterial for the star places, since they go out on reversal; but two series of observations, with the circle moved one-quarter period between them, give both of the sine terms (for that harmonic), since each appears as a cosine term (and therefore discoverable) in the other series. In particular, if this instrument were now used with the circle displaced some odd multiple of $7\frac{1}{2}°$ from the 1911–1918 positions, and if the same division errors were still used, the results ought in principle to be corrected by $\pm0\overset{''}{.}03\sin12z$, the sign depending on just which phase change is made; in addition, if the halved E–W differences in that condition showed a twelfth harmonic of amplitude a_{12}, the 1911–1918 results ought to be corrected by $\pm a_{12}\sin12z$. One can do the same for more than one harmonic at a time in principle (as long as both the cosines and the sines of the various phase changes are appreciable), but the weight is then reduced. Flexure, of course, cannot be treated in this way; if it contains a term $-0\overset{''}{.}05\cos2z$, it may well contain a sin2z term of comparable size or larger; but only tests with a pentag would discover this.

If we grant that the north–south consistency shown by Fig. 1 is real and shows what this instrument really is capable of, the N–S discrepancies appearing in Fig. 2 are difficult to explain. At any specified zenith distance north, with clamp east (say), the intrinsic flexure situation is exactly the same as it is at the same zenith distance south, with clamp west. Some other flexure situation will hold for those two zenith distances with clamp west and east, respectively, and again only one flexure situation is involved. Subtracting the two, for any one star, always in the sense E–W, will give the same answer for $+z$ and $-z$ in so far as the flexure is symmetrical (algebraically, not arithmetically) about the zenith, and so far as the flexure is antisymmetric it will give zero; a north–south dissymmetry in the E–W differences should not ever arise. This is still true if we include possible shifts of the micrometer, or of the lens, or actual looseness of the tubes or the circle, under the term "flexure." Figure 2 implies a flexure term which changes, as we go from $+z$ to $-z$, in some definite way with clamp E, and does not change in the same way, as we go from $-z$ to $+z$ with clamp W; and yet the effects of gravity should be the same for both clamps, whenever the instrument's attitude in relation to gravity is the same; thus there really does not seem any way to account for Fig. 2 on the basis of flexure, even including looseness or departures from Hooke's law under this heading.

It is, however, conceivable that division errors (taking these, now, as broadly as possible) might account for these effects too. For the microscopes are not reversed with the instrument, and it might be the case that those on the east pier have some difference in illumination or in focus from those on the west which would interact with the divisions differently in different parts of the circle. We are concerned, here, with displacements that are small compared with the thicknesses of the individual rulings on the circle, and if each engraved line has a slight "turned edge" (in the mirror-maker's sense) throughout some 20° or so the circle, on one edge only, it is conceivable that the east microscopes might see these divisions displaced (say by 1% of their thickness) from the positions in which the west ones see them (Atkinson 1959). Such turned edges can arise from very slight wear in a cleaning operation, more especially since the metal used is often rather soft; they might, however, have been there from the first. It is noticeable that the twelfth harmonic is fairly strong (though variable) for positive z in Pos. I, and is almost absent for $z<-20°$, and indeed the curve (before reflection at the zenith) shows no tendency to repeat after 90°, whereas if the second harmonic is removed the curve for Pos. II, 1 naturally does repeat every 90°, as well as being symmetrical about the zenith in any case. A failure to repeat after 90°, so far as division errors alone go, suggests strongly that even on one pier the microscopes do not all react in the same way to the detail of the division edges, and that this detail varies round the full circle.

However this may be, there can be no doubt that as soon as any program reaches the point where the run of the z_E-z_W can be reliably inferred from the star observations, if these differences are not symmetrical about the zenith there is something wrong; there is a case for an immediate investigation. If the fault can be discovered and rectified, every arrangement of the tubes, micrometer, etc., should lead to curves as self-consistent as that of Fig. 1, though it must be admitted that not every transit circle is certainly as good intrinsically as the Washington 6-in. It is also evident that the divisions should be as clear-cut as possible, and there should be no chance that even after many years an incautious cleaning could blur their edges. In this respect the circles of the Cooke T. C. at the Royal Greenwich Observatory appear safer than any engraved metal ones can be; they are of plate glass, with the divisions etched and the etched surface silvered and heavily lacquered over the silver, and they are viewed through the glass. They cannot be touched at all, and do not need cleaning.

It should be added that if indeed there are differences, as between one microscope and another, in the interaction between optical (and focusing) errors and peculiarities of the rulings, the errors need not repeat after 90°, and even a second harmonic might be due to "division errors" and not to flexure. There is some case for determining the division errors both with clamp east and with clamp west, and for directly verifying that they do indeed repeat after 90°, if they should; also, it is clearly important to have all microscopes set at the same focus with high precision, since otherwise a small common change will improve some and make some worse. Common changes are probable, when large changes of temperature have to be accepted; they can be appreciably reduced if the east–west locating pressure on the axis is always towards the pier which carries the microscopes in use at the time.

If we now consider flexure in right ascension, no observations have ever been made, because no reliable method of making them has ever been worked out, or is in prospect even now, with instruments of the conventional pattern. Such flexure, however, may well exist. Over-all flexure of the *E–W* axis is bound to be present, and has been studied on the Cooke Transit Circle at the Royal Greenwich Observatory by autocollimation observations on a mirror clamped to the flat end of each pivot in turn (Atkinson, Symms, and Blackwell 1966). The amplitude of the main term was about $\pm 0\overset{''}{.}2$ in this case; and while this will not affect the star places if it is distributed along the axis with *E–W* symmetry about the plane through the telescope axis, the symmetry of the *E–W* axis of this instrument is in fact far from complete. It is also possible for the telescope tubes themselves to bend laterally under their own weight, if their east–west symmetry is imperfect (as in fact it is). The effects of all such flexures have to be combined with those due to pivot errors, to get the true corrections in right ascension; the pivot errors can be determined by several standard methods, but the flexure terms (which may well be quite comparable) are unknown. Even a pentag mounted at the center of the central cube, so that either the right ascension wire or a spot on the lens could be observed with the pivot telescope, would not be reliable; the second-order errors arising from cross tilts of the pentag (Atkinson 1960) can probably be kept constant enough, but since the light is not parallel, even parallel displacements of the pentag will matter, and these can arise from over-all *E–W* axis flexure, as well as from specific weaknesses of the actual pentag mounting. Reversing the instrument eliminates some effects, of course; but (as in the case of declination) only half the Fourier coefficients can be removed (or determined) in this way. The "Mirror Transit Circle" (Atkinson 1947 and 1961) makes the true resultant (on the mirror itself) of the pivot errors and axis flexure (*all* terms) directly visible in the pivot telescope; since it also directly eliminates telescope flexure, lens and micrometer movements, and various other disturbing influences, it offers a greater chance of freedom from undiscovered errors than any conventional design. The actual sum of pivot errors and *E–W* flexure, for the axis designed and made at Herstmonceux, had an amplitude of $\pm 0\overset{''}{.}071$, although there was at that stage an *E–W* couple on the axis which would become at least 100 times smaller when the weight of the circles and circle casings is added; the pivot errors, though small, could also be further reduced if one wished. But the important feature of the observed curves is their repeatability; the mirror was not loose in the axis, by stiff interference standards, and was counterpoised so that its own flexure was quite negligible; and since there was no couple in declination it seems improbable that there would be any appreciable flexure in this coordinate.

The design, for the complete instrument, which was worked out and left on file at Herstmonceux, allowed autocollimation in the main face of the mirror to be done without any change of the eyepiece optics, so that a single autocollimation by one telescope could be injected into the observing program at any point and would take no longer than an observation of one star. Since the mercury pool was also to be left permanently in place (covered, when not in use), a level and nadir observation would be equally quick (if the pavilion opened and shut rapidly enough). Taking such observations in both telescopes (not necessarily in immediate succession) would readily keep a running watch on the instrument's stability, and combining these with observations of one telescope by the other would provide information on the flexure in declination, if this is large enough to be detected at all. It seems somewhat improbable that any alternative design, even if complicated, could have as complete a set of self-checks; and the cost of any radically new design is always unpredictable. (If the instrument works as originally designed, the cost is necessarily near the estimated figure, since there really is no way to exceed that substantially if one does not overrun a properly estimated delivery date. But it is not unusual to find that alterations—politely called "development"—of the original design are essential, and the delivery time and cost will then increase unpredictably.) The Mirror Transit Circle has in this respect the advantage that the only new and difficult part, namely the axis and mirror assembly, has already been made and successfully tested; everything else can be standard practice, except that in many respects the requirements are much less rigorous than in the conventional case; the telescope tubes do not move and the lenses and micrometers have to work in one attitude only. Since the parts of the instrument which have not yet been made can thus be "routine," they should scarcely take longer than the previously untried parts which already exist; these took 15 months, from placing the contract to completion of tests.

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DISCUSSION

SCOTT inquired about the status of other mirror transit projects.

ATKINSON replied that he knew little of the Portuguese project, except that there had been difficulties with it. The Ottawa instrument, of which pictures were shown in Moscow, is still not operational. He stated that he had no recent information on this instrument, but he did know that there had been troubles with the mounting of the mirror.

KLOCK reported that right-ascension observations were now carried out with the Pulkovo horizontal transit instrument, which he had seen during his visit in 1965. He mentioned that the mirror was surfaced on the main casting made of stainless steel. The circles were read photoelectrically, but declination observations had not been started as far as he knew.

ATKINSON remarked that it did not appeal to him to machine and grind the mirror into the axis, because if the axis flexed, the mirror would also do so, which was not the case with his instrument. He asked if the autocollimation was done at the end of the pivots.

KLOCK replied that this was the case, by means of small flats on the ends of the pivots. Since the counterpoises were close to the center, he believed the mirror would be nearly free from flexure.

ATKINSON inquired whether they had published any results of the collimation on the pivots, because he felt it was vital to know how the mirror behaved before actual observations were carried out.

KLOCK replied that he did not know of any, but he did know they had published results on a model horizontal instrument which they had operated for several years before the new instrument was built.

CLEMENCE remarked that he thought it most regrettable that the Atkinson instrument was in mothballs.