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Abstract. In order to assist other observatories working on improvements in meridian circle techniques, some of the difficulties encountered at the Dominion Observatory in its Mirror Transit Circle program are described. Some satisfactory aspects of the design, applicable perhaps to other instruments, are noted. An outline is given of the circumstances leading to abandonment of the project.

Résumé. Afin d'apporter une aide à d'autres observatoires dans leurs travaux d'amélioration de la technique du cercle méridien, l'auteur décrit quelques difficultés auxquelles l'Observatoire fédéral a dû faire face dans son programme relatif au télescope des passages. Il mentionne quelques aspects satisfaisants de l'instrument susceptibles d'être appliqués à d'autres. Il décrit brièvement les circonstances qui ont amené l'abandon des travaux.

Introduction

In 1954 plans were initiated for the replacement of the Ottawa meridian circle, installed in 1905, whose performance, particularly in declination, was falling increasingly behind modern standards. A proposed mirror transit-circle (Atkinson, 1947) offered among other advantages, much greater freedom from flexure, a fundamental control of any variation of collimation with altitude, the possibility of increasing aperture and scale, and greater facility for incorporating automated impersonal methods of registration. The disadvantages noted by Atkinson (double the effect of circle errors on the declination, increase in the effect of pivot errors on the right ascensions, difficulty of providing azimuth marks, and others) did not appear decisive.

Design considerations

Because of favourable experience with our photographic zenith tube of 25-cm aperture at f/17, the same dimensions were chosen, permitting the observation of PZT stars and asteroids. Photographic registration was intended, but as it was not certain that the instrumental constants could be so determined, provision was made for visual observations also. To minimize the circle reading disadvantage, a 76-cm-diameter circle and six long-focus microscopes rather elaborately mounted were specified. Observing slit and room refraction problems were to be obviated by nearly open-air operation. Atkinson's idea for observing all contributions to effective pivot errors was followed. Control of systematic errors down to ±0.″10 or better was aimed at; accidental errors two or three times as large could be tolerated as still superior to our meridian circle. If these last two goals could be achieved, a valuable contribution could be made to fundamental astronomy.

Brief history of the project

Construction, begun by 1959 on a temporary site adjacent to the observatory made available for 10 years, was almost completed by 1961. Brealey (1961) provides a good description, with additional detail in Brealey (1963). Fulfilment of the meridian circle commitment to AGK3R at the end of 1962 freed enough staff to commence operations in 1963. It soon became clear that the circle graduations were much too ill-defined for our requirements. The United States Naval Observatory undertook to regraduate the circle; while this was being done, a series of test observations in right ascension of stars selected from FK4 was begun in August 1963. Observations in both co-ordinates were resumed in February 1965 on this list supplemented by Ottawa zenith stars and several high polars ($\delta > 89°$). This work was halted in 1967 because of an increasingly evident instability of the axis. A right ascension list involving a few fundamental stars, repeated high polars, and doubled observations of zenith stars before and after transit was substituted for it. This program, intended to show as directly as possible the effects of the modifications undertaken, continued until suspension of activity in 1969.

Some of the problems encountered

An enumeration of some of the principal scientific problems encountered during these efforts follows.

1. The systematic accuracy of ±0.″05 required of the divided circle was not attained. The graduations, at 3′ intervals on a gold band, although greatly superior to the originals and closely similar to ones found quite satisfactory at USNO, were seen to shift in apparent position when illumination and exposure were varied. The six cameras in regular use (two 45° above, two 45° below the horizontal pair) made it possible to determine the relative division errors of four diameters at 45° in the course of the numerous regular readings at settings 0°, 45°, 90° etc., made for determining the instrumental constants. These relative errors were found to differ by amounts of the order of ±0.″10 (on the circle) in changing from the circle west set of cameras to circle east. These discrepancies were not removed by a fairly elaborate regulation of the obvious variables, and therefore a fuller determination of division errors was never undertaken.

Further complications probably resulted from the fact that the gold background of the filled graduations was not of a uniform aspect all around the circle. The automatic measuring engine for the circle films, on the other hand, could only be got to give properly repeatable results on a rather narrow range of film density. Visual measurement was found to be too inaccurate, and of course very tedious. Mechanical film-processing and photo-electric control of exposures were introduced to get satisfactory automatic measuring. It might be mentioned that the investigation of circle problems was made easier by the great stability (order of 0".10 relative motion over a six-hour session) of the long microscope tubes with 5X magnification. Even changing the film magazines would alter the pointing by only a few seconds of arc.

2. The long horizontal air path, over 13 m from north to south collimator prime focus, about 1.5 m above grade, led to generally poor local "seeing". This was evident both in the millisecond duration exposures for instrumental constants, where successive images of the same graticule were often displaced by a second of arc (20μ), and on the repeated 15- to 30-second exposures of the high polars for azimuth, where the star images were not infrequently similarly displaced.

In the special series of observations of zenith stars twice per transit, where nearly all variables except a minimum of seeing can be eliminated, the typical discordance in right ascension (which includes the relatively small photographic and measurement errors) was 40 ms for two 40-second exposures with centres one minute apart. This corresponds to a typical seeing displacement of about 0".30 in a single image. A strictly comparable figure for the nearby PZT with 20-second exposures is 0".20. In the case of the PZT this is halved by the four exposures available; for the mirror transit the seeing errors increase with zenith distance, are worse in the declination co-ordinate, and multiple exposures on a single transit are not generally feasible. The effect on the instrumental constants other than azimuth can be reduced by multiplying the readings, and the azimuth uncertainty reduced by using more stars in its determination, but with an evident loss of efficiency.

Various combinations of building and tube fans were tried, and insofar as their use could mitigate the temperature differences displayed by the thermistors at the central section and the collimators, with beneficial result. But the observers often found this impossible; it was often necessary to close the building to get a readable set of the instrumental constants taken every hour.

3. The above difficulty was obviously compounded by the almost open-air operation. Rolling away the roof and walls exposed the whole instrument to a near-hemisphere of sky, with subsequent radiative cooling, and to the passage, with the breeze, of inhomogeneous parcels of air close to the ground. Collimation readings were usually taken at the half aperture above and below the mirror (directed at the zenith then the nadir for this purpose), and these often differed systematically by over 1" in the vertical plane. A more striking phenomenon was the occasional doubling of the return image at auto-collimation or nadir. Both were ascribed to layering of the air in the proximity of the massive central section.

Ventilation, radiative shielding and thermal lagging were tried; the "over-under" difference was never wholly eliminated, thus casting doubt on the reliability of the declinations deduced. From the practical point of view, the roof opening mechanism was among the most vulnerable of the many subsystems of the mirror transit, and many months of observing were lost by failures in it.

4. A perplexing instability in the azimuth of the mirror axis, which became increasingly evident after 1966 (although perhaps not entirely absent previously) was not wholly removed. Variations of over 100 ms during the night (corresponding to relative motion of 7μ at the vees) were found eventually to correlate roughly with temperature changes. At the same time the level of the axis as well as the line of collimation between north and south telescopes would remain constant to 10 ms, barely above the error in their determination. As the old meridian circle had never displayed such symptoms, the mode of attachment of the superstructures was suspect first. Modifications to attach the massive base plates more directly and firmly to the reinforced concrete piers did not remove the fault. The portions of the piers above ground were heavily insulated as well as the superstructure, with rudimentary thermostatting around the base plates; these steps brought about some improvement as the experiments ended. It should be mentioned that changes in the line of collimation in the vertical plane, consonant with vertical motion in the independent north-south piers, were also evident, so that changes would have had to be made there too if measures in declination had been resumed.

The foregoing problems prevented the attainment of the goals mentioned earlier. Although several hundred nights of observations of fundamental and zenith stars were made, no useful positions have resulted; in declination for want of division corrections, in right ascension because of the dispersion. This could rarely be brought down to 20 ms secδ even in a differential reduction of the results of a single session, so that the number of satisfactory observations of even the most frequently observed stars was too small to be valuable.

Some satisfactory features of the design

Turning now to features of the Ottawa design found useful, the following are noted.

1. On the whole, photographic registration of stars and constants worked well. Visual observations were made only for such purposes as checking the focus, seeing or adjustment. The system of flashing the graticule at known five-second intervals during transit did away with any need of a chronograph, ensured that the star image was always close to a reference mark, and allowed the accuracy of tracking to be verified. Normally 40 s exposures gave well-measurable images of tenth magnitude stars. All stars were screened to appear between seventh and tenth magnitude; this range apparently caused no difficulty. The dimensional stability of the 35 mm film (Estar base) during exposure, processing and measuring was generally better than 1/1000, and observations were made so that the distances to be measured were usually under 1 mm. The overall contribution of film and measuring errors was about ±0".10 (±2μ) for the best defined

images. The fact of not working at the prime focus seemed to introduce no problems, probably because of the narrowly differential method of measurement. It is regrettable that no way was found to image all the stars of an observing session together in a common frame as is done with the PZT.

2. The lateral collimators advocated by Atkinson for measuring the effective pivot errors were incorporated in the design at 75 mm aperture, f/24. The consequent enlargement of the hollow pivots was not found to be detrimental, nor was the use of "ears" cemented to the sides of the main mirror. The first tests of the lateral collimators revealed clearly the presence of an oscillation of 15° period and several microns amplitude at the pivot, the result of chatter in the grinding of the latter. After this had been lapped off, the only remaining departure of the normal to the main mirror from a conical path on rotation was less than 15 ms, from a slight pivot ellipticity. The systematic accuracy attained in the course of a repeated series of measurements for this departure was about ±3 ms, but continuous monitoring of the mirror to such accuracy would have been impractical, had it been necessary, as the lateral collimators had hourly drifts several times as large. It was found desirable to align the "ear" normals very closely with the axis of rotation to reduce the excursions of the autocollimated test ray to a minimum.

3. The main mirror, on a simple six-point support held in by springs, showed no detectable flexure or motion in its cell with changes in orientation or temperature. Instead of attempting to maintain a constant pressure of the mirror on its supports by a system of counterpoises, the pressure was made several times what gravity would provide, so that its proportional variation on rotating the axis would be small. The central axis had been designed to be very stiff, and the location of the counterpoising with respect to the vees chosen to minimize flexure at the mirror supports. It was gratifying, then, that no evidence of lateral flexure was found with the autocollimators; that is, it could not have exceeded a few milliseconds. The figure of the mirror could be examined directly at nadir and autocollimation by Foucault testing; no change could be seen after the 45° rotation. While no direct evidence could be got for non-rotation of the mirror in its cell about an east-west axis, the constancy of Atkinson's β_1 (the angle between the plane of the mirror and the axis of rotation), to within the error of observation of a few milliseconds, over periods of months, was reassuring.

4. High polars (specifically BD 89°01, 02, 03, 38, plus Polaris and λUMi) furnished satisfactory azimuth control. Two or three were usually in the extended field (±20′) at all times, and three or four exposures could be made in a few minutes. Since these stars were allowed to trail, measurability depended on their speed as well as magnitude; 89°02, at $10^{m}\!.8$ with about 30′ polar distance, was generally unusable. A more careful determination of the scale of the photographs (from the known scale of the graticule markings) was necessary in these frames, where the star, up to 25 mm off the optical axis, might be 5 mm from the nearest fiducial mark. The north-south collimators were sufficiently stable to be used as short-term azimuth marks, but the necessity of altering their focal settings from month to month precluded their use as long term controls.

5. The remote-control, servo-setting features of the design were eventually made to work well and reliably. With observing largely reduced to pushing buttons, and the subsequent drudgery of film measurement greatly reduced by automation, no questions of personal equation, or of the effects of the proximity of the observer on the instrument could arise. There was some concern about some of these mechanisms as heat sources close to the instrument, but no difficulties were in fact encountered.

Circumstances leading to abandonment

The final section of this paper outlines the circumstances leading to the abandonment of the project. The disappointingly slow progress in solving the remaining difficulties was discouraging, but no serious flaws in the mirror transit principles had been found. Plausible solutions can be suggested for each problem: a glass circle with direct photo-electric read-out; much taller piers thermostatted throughout; evacuation or helium filling of the collimators; much better isolation of all parts from ambient fluctuations, and so on.

But the time was approaching to vacate the temporary site. Two men closely connected with instrumental development had been sent to the Dominion Astrophysical Observatory, Victoria, to work on the Queen Elizabeth II telescope project. It was hoped that they would resume their first task when the mirror transit was relocated in British Columbia as part of an intended Institute for Astronomy. The Institute plan was rejected, and the Observatories Branch, faced with the prospect of a costly local re-installation of an instrument needing a good deal more time, money and expertise than were available to ensure its success, suspended the operation.

Although there was some thought of storing components to await a more favorable time for resuming the program, subsequent reorganization of the federal government's responsibilities in astronomy, and re-evaluation of all Branch projects, led to the dismantling of the instrument and the dispersal of its constituents in 1970.

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