

## THE CONTRIBUTION OF THE IMPERSONAL ASTROLABE TO FUNDAMENTAL ASTRONOMY

*George Darwin Lecture delivered by Professor André Danjon on 1958 May 9*

I wish to recall that Sir George Darwin, to whom this lecture is dedicated, devoted a part of his work to rotating celestial bodies. The title of his first paper was *On the Influence of Geological Changes on the Earth's Axis of Rotation*. Since then the discovery of the movements of the poles and, later, of the variations in the length of the day have aroused a renewed interest in the problems of practical astronomy related to these phenomena. My aim in this lecture is to describe the programme of observations now being carried out at the Paris Observatory and to mention the further developments that can be anticipated in the near future.

A few years before the outbreak of the last war, I had published the description of a new type of transit instrument for the determination of right ascensions. The results obtained at Strasbourg with a prototype of small dimensions (aperture 6 cm) having been very encouraging, a new series of tests was commenced at the Paris Observatory in 1946. Meanwhile, an instrument of similar design but of 12 cm aperture was constructed. The early observations made with this larger instrument revealed unforeseen difficulties, which could have been overcome by completely redesigning the mounting. But, for the time being, the study of this instrument has been discontinued and all the facilities available in Paris Observatory, including scientific and technical staff, laboratories and workshop, etc. have been concentrated on the construction of an instrument of an entirely different type: the impersonal astrolabe, which will be the main topic of this lecture.

I had moreover another reason for giving priority to the astrolabe, of which an experimental model had already been constructed at the Observatory in 1950 and progressively improved. This reason is that, in 1952, the International Council of Scientific Unions decided to organize the International Geophysical Year and to incorporate in its programme a determination of world longitudes and latitudes. The observatories participating in the operation were recommended to observe stars selected in the Fundamental Catalogue FK3 (or in the improved catalogue FK3R). It was agreed that preference should be given to modern instruments giving simultaneously and with the highest possible accuracy the two coordinates, time and latitude. The zenith telescope, indeed, gives both coordinates but requires a special catalogue limited to zenith stars, in which therefore non-fundamental stars have to be introduced. The astrolabe is the only instrument which permits both coordinates to be determined by the observation of fundamental stars alone.

A large size astrolabe was then ordered from the Société Optique et Précision de Levallois (OPL). The design of the instrument was worked out in close co-operation with the technicians of this firm. The new instrument differs from the experimental model mainly in its dimensions, which are larger, and in the improved arrangement of the mechanical parts.

The instrument was delivered in 1956 and put into service in July of that year. Since then it has been in use without interruption. More than twelve thousand transits have been observed, which is adequate for evaluating the qualities of the instrument, if not for establishing definitive results. The observations have been carried out mainly by L. Arbey, B. Guinot and Mlle Débarbat.

Under the most favourable atmospheric conditions, the OPL astrolabe permits the transit of a star, at a well determined zenith distance, differing very slightly from thirty degrees, to be recorded with an accuracy characterized by a standard deviation of  $0''.09$ , assuming that the errors of the catalogue have been corrected. The deviation increases with atmospheric scintillation, and its most frequent value is  $0''.17$ . For only six per cent of the nights is it larger than  $0''.26$ . But what is most important is that the observations have proved to be free from systematic errors, a result of outstanding importance for the use of the impersonal astrolabe as a fundamental instrument.

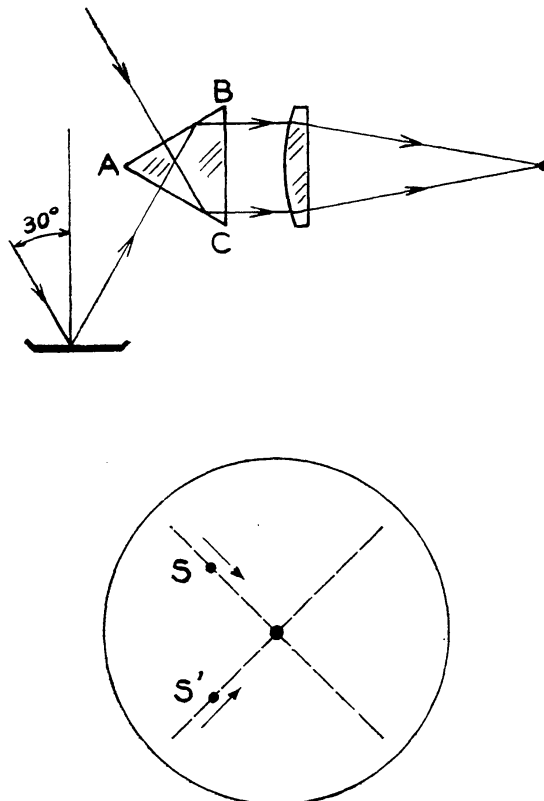


FIG. 1.

Indeed we very soon realized that this instrument was useful not only for the determination of time and latitude but that it could be used as well for the improvement of the FK3 catalogue and for the determination of the fundamental astronomical constants. The accuracy of the results is due partly to the intrinsic qualities of the instrument and partly to the application of the so-called "method of equal altitude of stars".

As for the instrument itself I wish first to recall its principle. Suppose that we fix an equilateral glass prism in front of a horizontal telescope in such a way that its edges are horizontal and one of its faces vertical (Fig. 1). A beam

of light coming from a star entering the face AB at right angles to the edge of the prism, is then reflected from AC and emerges through the lower half of BC. Another portion of the beam falls on AC after having been reflected from the surface of a bath of mercury. It is reflected by AB and emerges through the upper half of BC. Thus, looking through the telescope, two images of the star, S and S', are seen which appear to move in opposite directions, one moving upwards and the other downwards. They appear to coincide when the star is at a zenith distance of exactly  $30^\circ$  and the instant of coincidence is noted. As the prism angle is not exactly of  $60^\circ$ , there are three unknowns in the problem, namely: the correction of the clock, the latitude, and the excess of the zenith distance over  $30^\circ$ . Therefore at least three stars, in different azimuths, must be observed. In fact, many more than three stars are observed, and in general each of our groups contains twenty-eight stars. The plotting of position lines, a method familiar to navigators, gives an approximate graphical solution but as our observations are very accurate a solution by least squares should be applied.

The astrolabe in its original form was designed by MM. Claude et Driencourt\*. It dates back to 1900 and it was then a very simple instrument which consisted merely of a telescope, a prism, a mercury horizon and adjustment devices. The observer noted the time of coincidence of the two images of the star by the eye and ear method or by the hand tapping method. Thus, each star gave only a single determination of time, which was affected by personal equation. But it was intended to be an instrument for use in the field and not an instrument of high precision. There was indeed another source of error resulting from the images being displaced when the position of the eye-piece was changed even by a slight amount. The reason is that the images are formed by two pencils of rays the axes of which are not strictly parallel but are inclined to each other at an angle of two or three degrees. If the focusing is altered, the distance between the two pseudo-images changes, which produces the same effect as a variation in the prism angle. I will later show you that this angle has an astonishing stability, but, owing to the defective arrangement of the instrument, no full advantage of this remarkable property was taken.

Various astronomers have been successful either in correcting this optical defect or in rendering the astrolabe an impersonal instrument. The excellent results obtained by Paul Muller, the well known double star observer, with a micrometer containing a doubly refracting prism, led me to the concept of a more complete solution which I will now describe and which is the essential feature of the impersonal astrolabe†.

A doubly-refracting prism is placed at the focus of the telescope. For certain reasons, which would take too long to describe, I have selected a double Wollaston prism made of quartz. The arrangement of this prism is shown in Fig. 2. The separation angle of this prism is precisely equal to the angle between the axes  $a$  and  $b$  of the two pencils transmitted by the objective. The splitting of the pencil  $a$  gives two pencils  $a_1$  and  $a_2$ , as is shown in the figure, while the splitting of pencil  $b$  gives pencils  $b_1$  and  $b_2$ . It is at once seen that  $a_1$  and  $b_2$  are

\* A. Claude and L. Driencourt, *Description et usage de l'Astrolabe à Prisme*, Paris, Gauthier-Villars, 1910.

J. Ball and Knox Shaw, *A Handbook of the Prismatic Astrolabe*, Cairo, 1919.

See also: E. Chandon and A. Gougenheim, *Les Instruments pour l'observation des Hauteurs Egales en Astronomie*, *Revue Hydrographique*, **12**, 1, 1935.

† A. Danjon, L'Astrolabe impersonnel de l'Observatoire de Paris, *Bull. Astro.*, **18**, 251, 1955.

parallel and that therefore the distance of the two corresponding images  $\sigma$  and  $\sigma'$  no longer depends on the adjustment of the eyepiece. The two other images are eliminated by suitably placed screens. Moreover, the coincidence of the two images  $\sigma$  and  $\sigma'$  can be obtained by a displacement of the quartz prism parallel to the axis of the telescope. The coincidence thus obtained can be maintained

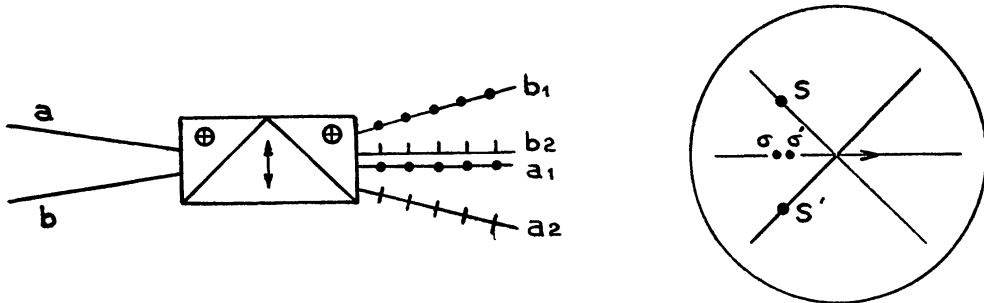


FIG. 2.

by traversing the prism at a rate proportional to the sine of the azimuth and to the cosine of the latitude. The micrometer screw which drives the prism, carries a disk with contacts connected to a chronograph. The astrolabe, already corrected for its optical defects, then becomes impersonal. The observer needs only to correct the movement produced by a motor, so as to maintain the coincidence of the two images.

In fact this coincidence is obtained not by superposing the two images, but by placing them side by side along the same horizontal line between two illuminated wires as shown in Fig. 3.

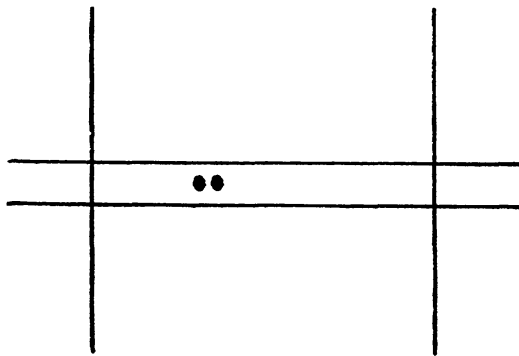


FIG. 3.

It would take too long to describe, in all details, the arrangements adopted for the component parts of the astrolabe\*. Figure 4 gives a very simplified layout of the instrument. The objective of the OPL model has an aperture of ten centimetres and a focal distance of one meter. Two equal lune-shaped holes are made in a diaphragm, giving diffraction images elongated along the vertical, with axes in the ratio of three to five. The faces of the prism are rectangles of 10 cm by 8 cm. The dimensions of the micrometer screw are rather unusual: 25 mm in diameter and 5 mm in pitch. If the star is observed exactly to the East or to the West, the screw makes a complete revolution in 2.61 seconds. The speed of the motor is constant and is adjusted to suit the latitude of the station.

\* A. Danjon, "L'Astrolabe impersonnel modèle OPL", *Bull. Astro.*, to be published in 1958.

A speed reducer automatically varies the speed of rotation of the screw proportionally to the sine of the azimuth, when the astrolabe is turned around its vertical axis to observe another star. To make the instrument as compact as possible, the beam of light is broken twice by mirrors made of fused quartz and located between the objective and its focus.

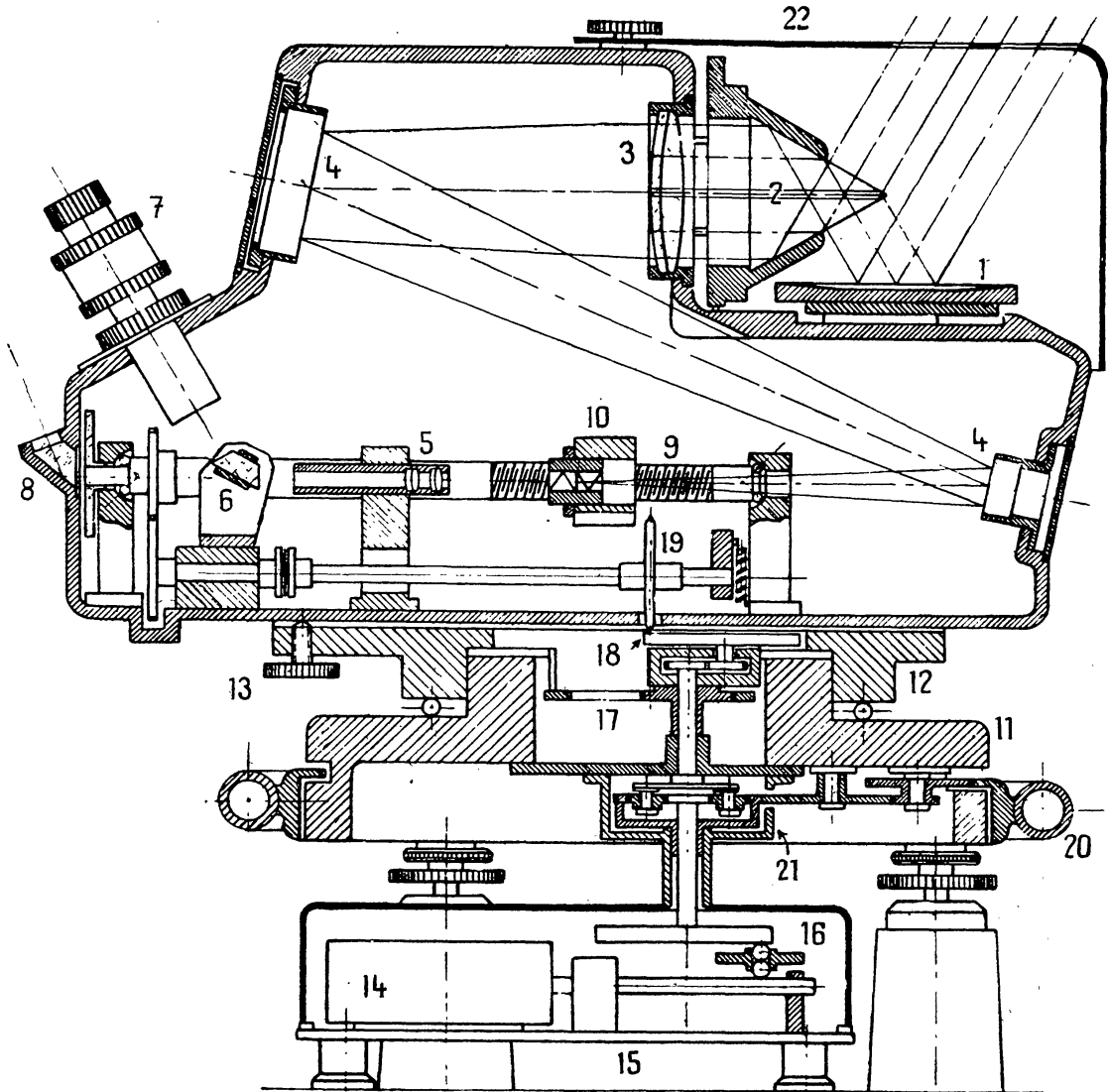


FIG. 4.

The accuracy of astrolabe observations is due to a large extent to careful manufacture by the Société OPL but equally to the working conditions and to the intrinsic properties of the equal altitude method. I shall consider these two points in turn. As the instrument rotates around a vertical axis, its flexures are not liable to vary with the azimuth. Its design provides protection against sudden changes of temperature. The results of time and latitude observations do not even depend on the absolute value of the prism angle; they could be affected only by a variation of this angle during the observation. But results obtained since July 1956 have clearly demonstrated that there is no systematic variation of this angle in the course of the night, not even in the course of a year,

and that the zenith distance remains invariable within a few hundredths of a second. Our observation programme includes twelve groups, each of twenty-eight stars chosen from the FK3 or its supplements. These groups are identified by letters a, b, . . . , k, l.

A single observer can observe, during a single night, at least two consecutive groups, often three, or even more\*. The computed zenith distance, corrected for refraction, would have the same value for all these groups if the prism angle were constant and if the catalogue were not affected by any systematic error. In fact, two consecutive groups observed in the course of the same night, give different values as is shown by Fig. 5. In the course of a year, differences a-b, b-c, . . . , l-a are obtained. The distribution of these differences does not reveal any systematic variation in the nature of a function of the right ascension, which indicates that the prism angle is independent of the annual variation of temperature and that errors in the catalogue are solely responsible for the differences obtained.

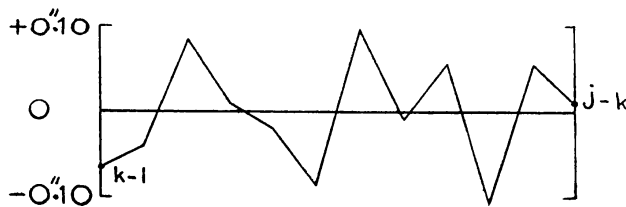


FIG. 5.

Even more, we have been able to verify that the zenith distance of transits does not vary systematically during the course of the night, although, as a general rule, the temperature of the instrument decreases from the evening to the morning. This is illustrated by the smallness of the closing error of the differences that B. Guinot has computed for a complete year. For instance, for group k this closing error was found as small as  $-0''.017 \pm 0''.104$ . At any rate it is not significant. It must be emphasized that the prism has been very carefully annealed, that its mounting does not exert any prejudicial stresses, and that these two conditions seem to be essential.

Let us now consider the method of equal altitude of stars and its advantages. Devised by Gauss about a hundred and fifty years ago for field operations, its application to problems of fundamental astronomy has so far been restricted to special cases such as, for instance, the determination of latitude by the Talcott method, which is still in use in the International Latitude Service, or the observation of transits in the prime vertical by the method of W. Struve. However, it is very valuable, in its general form, for linking together stars whose declinations differ by several tens of degrees and whose right ascensions differ by several hours.

The declinations of the stars that can be observed with the astrolabe are between  $\phi - 30^\circ$  and  $\phi + 30^\circ$ ,  $\phi$  being the latitude of the station. In Paris, these two limits are theoretically  $+18^\circ 50'$  and  $+78^\circ 50'$ , or practically  $+20^\circ$  and  $+78^\circ$ . Their hour angle at the time of the transit is comprised between the limits  $-H$  and  $+H$ ,  $H$  being the solution of the equation  $\sin H = \frac{1}{2} \sec \phi$ . For Paris  $2H = 6^h 36^m$  and for Herstmonceux,  $6^h 59^m$ . The observation of one group requires about one hour and a half so that it is possible to link together stars whose right ascensions differ by as much as 8 hours.

\* B. Guinot, "L'Astrolabe Impersonnel. Réduction des Observations. Étude des Résultats." *Bull. Astro.*, 18, 283, 1956.

When two transits of the same star are observed, one to the east and the other to the west, the mean of the observed times gives its right ascension, and its declination is easily computed from the difference of the same transit times. The determination of the right ascension is not very accurate when the declination of the star is near the limits and, on the other hand, the determination of the declination is not possible when the hour angle of the transit is close to  $H$ . To fill these gaps it would be advantageous, and for more than one reason, to set up astrolabe stations in several latitudes. In Paris for instance we obtain good values of the right ascensions for declinations comprised between  $+20^\circ$  and  $+78^\circ$ , and good values for the declinations in the intervals between  $+20^\circ$  and  $+50^\circ$  and between  $+68^\circ$  and  $+78^\circ$ . In these intervals the application of the method of equal altitude with a high precision instrument discloses at once the individual errors of the catalogue, as these errors are important compared to the accuracy of the observations. I have already mentioned that the average value of the standard deviation for one transit was  $0''.17$  and that, under very favourable atmospheric conditions, this deviation is as small as  $0''.09$ . As each group is observed about 20 times each year, the plotting of the position lines is correct within a few hundredths of a second. In a few years the precision of their determination will be as accurate as  $0''.01$ . Individual errors being expressed by a dispersion of the position lines in the diagram, some of them intersect the circle, whose centre and radius are obtained from a least squares adjustment, and some others remain outside the circle. As for the systematic errors of the catalogue they are sometimes revealed by an asymmetry of the diagram.

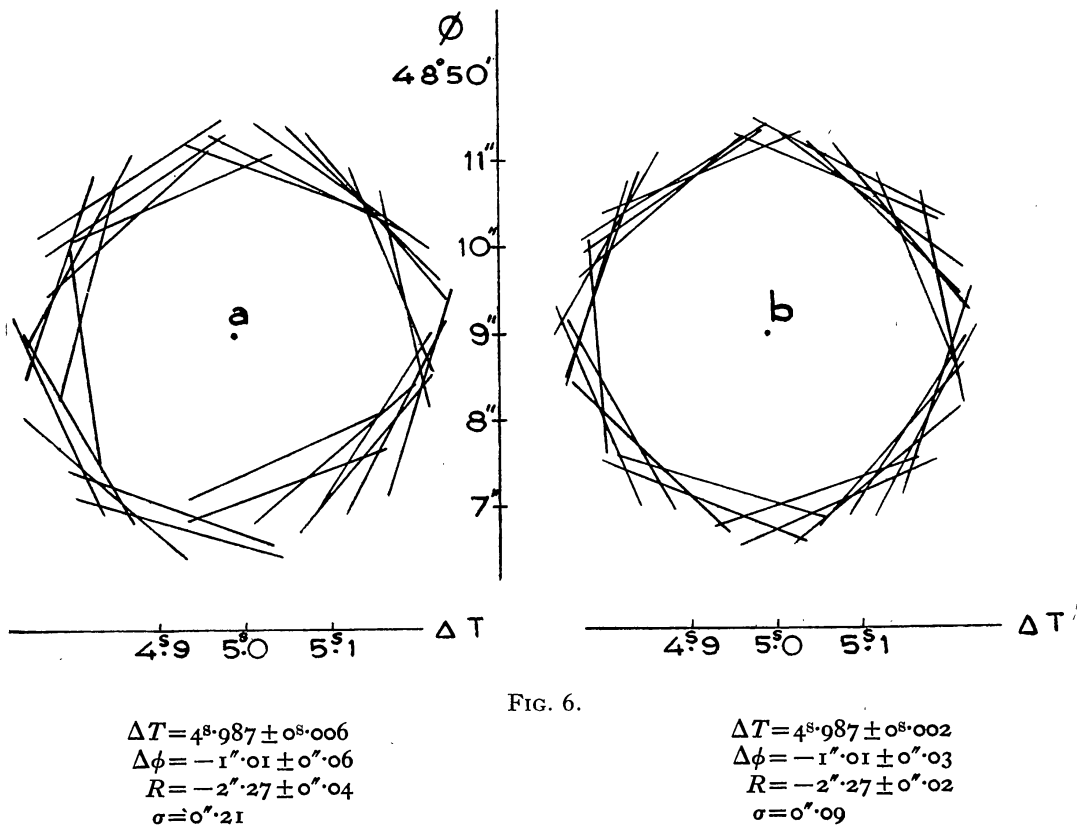


FIG. 6.

The diagram in Fig. 6(a) was drawn from the results obtained on 1958 January 22 on group d. The same group was observed in 1957 and the displacements to

be given to the position lines to make them tangent to the circle were already known. Fig. 6(b) shows the same diagram corrected from the observations obtained in the preceding year. Their quadratic mean error decreases from  $0''.21$  to  $0''.09$  and the asymmetry disappears. This example illustrates clearly the effectiveness of the method of equal altitudes in correcting the position of stars, but it must be remarked that this operation is merely an internal adjustment affecting each group independently and that it is by no means sufficient to correct the catalogue.

It is now essential to make a fundamental remark. Whatever be the instrument used, a single observation of a group of stars cannot disclose systematic errors of a catalogue which are equivalent to a change in the coordinate system and which do not affect the relative positions of the stars. Such are those produced by an error in the value of the precession or of the nutation. As the relative positions of the stars remain unchanged but the position of the pole and the origin of the coordinate system are erroneous, the diagram of the position lines is translated without distortion and therefore neither its symmetry nor the radius of the circle of the position lines is affected. But of course the coordinates of the centre of the circle are altered, and consequently so also are the time and the latitude. The method of equal altitude of stars, which reveals so clearly the individual errors and the localised errors affecting the various regions of the sky, cannot therefore provide any evidence of a systematic error in the reference system by a *single* observation of a given group. Errors of this sort can be found out only by a chain programme of observation, associated with group to group connections, covering at least one entire year. These errors will then appear in the group errors in the form of a variation depending on  $\sin \alpha$ .

I have said a moment ago that there was in this respect no privileged instrument. Suppose that we observe with a meridian circle the transits of a group of stars of limited range in right ascension. If the pole of the catalogue is at a distance  $\Delta\nu$  from the true pole of rotation, in consequence of an error in the precession, and if  $\alpha_m$  is the observed mean right ascension of the group, the azimuth of the meridian circle, computed from the observations, will have an error equal to  $\Delta\nu \sin \alpha_m \sec \phi$ , but nevertheless the representation of the observations by means of this faulty azimuth will still be excellent. Therefore no improvement of the catalogue can be expected. Here again the "chain adjustment" alone will enable the errors of the catalogue to be determined and corrected.

Theoretically the method of equal altitude of stars and the method of meridian transits are therefore equivalent in this respect. But in practice their possibilities are very different. In the first place, the stability of the astrolabe can be guaranteed with much greater certainty than that of the meridian circle. In particular, the astrolabe enables the value of the prism angle to be accurately controlled, and that is the only instrumental constant whose variation would be prejudicial to the accuracy of the observations. We now know by experience that the changes in the prism angle are negligible compared with the accidental or systematic errors of the fundamental catalogue. Moreover, the observations with the astrolabe are referred to a single terrestrial reference: the vertical. The discussion of the latitude observations have proved, as I will show you, that the daily variations of the zenithal refractions are imperceptible, at least in Paris.



The conditions are quite different in the transit instrument. The solid body which serves as the direction of reference is not now a simple glass prism but a telescope with its trunnions. The line of sight is determined by the optical centre of an objective, consisting of two lenses which cannot be linked rigidly together nor to their mounting, and by the spider's web carried by the movable frame of a micrometer. The objective and the micrometer are linked together by a metallic tube exposed to unceasingly variable conditions of temperature, as this tube radiates either from one side or the other as the star to be observed is to the north or to the south. The axis of rotation is defined by two trunnions carried by a heavy metallic part exposed to the same variations of temperature. Consequently the position of the instrument changes with respect to the vertical from one star to another, and as the flexures are not rigorously symmetrical with respect to the meridian plane, lateral deflections are unavoidable. To measure the collimation of the instrument and the inclination of its axis of rotation, the telescope is pointed towards the nadir in a direction quite different from the positions it occupies for the observation of stars. Who could say what happens to this collimation and inclination when the telescope is pointed towards a star? To obtain an accuracy of 1/10th of a second of arc, it is necessary that the mechanical or thermal deformations of the instrument are less than 1 micron. Experience shows that they are in general much larger than this limit. More than 70 years ago, Chandler already asserted that the method of equal altitudes of stars was better suited to give accurate results than meridian astronomy\*. We have reached this very conclusion by comparing the physical properties of the astrolabe and the transit instrument. But there is another reason, of a geometrical nature, which confirms this conclusion. With the method of equal altitudes, transits of stars can be observed all around a complete circle (almucantar) on the celestial sphere; and that is why the radius of the circle and its pole, which is the zenith of the station, are so well determined. Meridian observations provide different conditions, as the observations can only be made over 45° of zenith distance, that is along an arc which covers only one quarter of the circumference on the meridian; thus, the position of the pole of this arc, which defines the direction of the rotation axis of the transit instrument, is practically less accurately determined than that of the zenith of the astrolabe.

Even if it were perfect the meridian circle by itself could not conveniently provide for the "chain adjustment", because such adjustment cannot be effected without referring, as an intermediate step, to an azimuth mark or a collimator. I know nothing more disappointing than the observation of a mark. I have often observed those of the Strasbourg Observatory which were set up very carefully by Winnecke. Even when at first sight they appeared to be stable, they suffered every now and then from sudden variations amounting frequently to one second of arc. If the mark is located not far from the instrument then its azimuth is not well defined. If it is far from the instrument its direction is

\* S. C. Chandler, "The Almucantar", *Ann. of Harvard Coll. Obs.*, 17, 1887, 179-181.

"I think it can be successfully maintained that the almucantar system of observation constitutes a more simple and direct solution of the problem of determining the absolute time from the phenomena of diurnal motion, and consequently one more likely to be free from the systematic error, than that of the meridian transit." . . . "As regards the adaptation of the instrument to purely astronomical purposes, one line of usefulness seems to be clearly marked out, in the application to the class of position measurements for which meridian instruments have heretofore been exclusively employed."

frequently deviated by lateral refractions. But in any case a terrestrial mark is far from being as reliable as the prism of the astrolabe for defining a direction of reference. Its stability cannot even be assured through the course of a night, from evening to morning, and far less during day time.

In my opinion, better results can be expected from a collimator than from a mark, because the collimator can be installed in the interior of the meridian room and can then be efficiently protected against variations of temperature, as well as its pillar and even the ground all around the pillar. That is why I have used two collimators, one to the east and the other to the west, with the transit instrument that I mentioned at the beginning of this lecture and of which I will now recall briefly the principle (Fig. 7). In front of the objective of a telescope

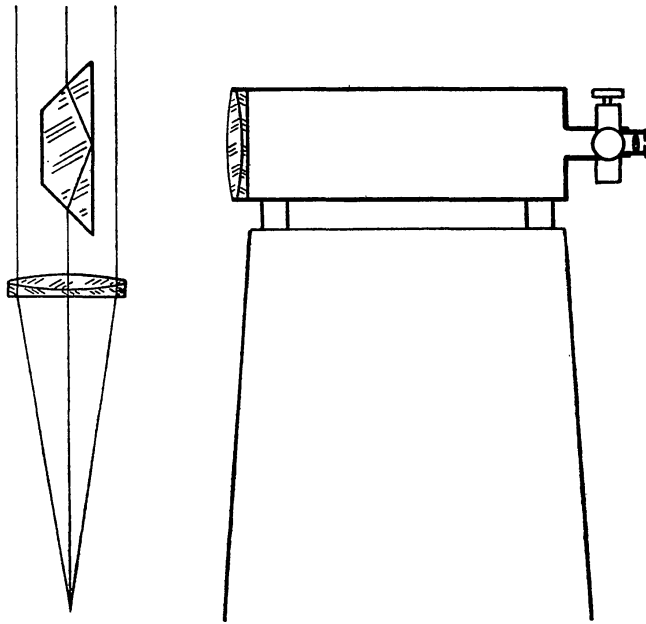


FIG. 7.

whose axis lies in the meridian plane, is mounted a reversing prism, which covers its surface with the exception of two segments which remain uncovered. In the field of the telescope two images of the star appear, which move in opposite directions. The two images coincide when the star transits the plane of the reflecting face, which differs but by a very small amount from the direction of the meridian plane. The position of the prism is then determined by auto-collimation, by means of the lateral collimator pointed towards its reflecting face. The readings made with this collimator provide the necessary correction for reducing all the transits to a same plane. The second collimator is used when the instrument is reversed. I will not mention the various operations which enable the horizontality of the collimator to be controlled by means of a mercury horizon and its azimuth to be controlled by the observation of fundamental stars. What I wish to stress is that the line of sight is much better defined by the prism than by the optical axis of the conventional meridian telescope. The major difficulties occurring in the meridian telescope are automatically eliminated, but indeed other problems arise which I am now endeavouring to overcome. If there is a noticeable difference in temperature between the interior and the exterior of the room, the reflected image of the

cross wires as seen in the collimator is unsteady. This is but a minor inconvenience which can be easily corrected. The most troublesome effect is caused by the position of the prism in front of the objective. If its temperature differs from that of the surrounding air even by a few tenths of a degree, it is enveloped by a layer of air about 1 cm thick within which there exists a marked temperature gradient. The part of the pencil of rays which crosses this layer becomes divergent if the prism is warmer than the air and convergent if it is cooler, and in both cases the image given by the free aperture is altered. If the air around the prism is stirred by means of a fan, this layer of air disappears and the image resumes its normal aspect. But if the fan is stopped, then this layer is at once formed again.

A similar effect is also produced in instruments in which metallic parts are placed across the path of the light. I have observed for instance, by a very sensitive interferometer method, that the bright extensions which surround the stellar images in a reflecting telescope are caused by the presence of the supports of the secondary mirror which, cooling off during the night by radiation, are surrounded by a layer of warmer air producing a refraction effect, and not a diffraction effect as is generally assumed. André Couder gets rid of this effect by covering the supports of the secondary mirror with sheets of polished aluminium.

The case of the reversing prism of my transit instrument cannot be treated so easily and I have had to adopt a more radical solution: the prism of the projected instrument will be placed in an atmosphere of helium, this gas having a refractivity ten times smaller than that of air and a conductivity six times higher. The idea is not a new one: Bernard Lyot had elaborated plans for a reflecting telescope of an aperture of 150 cm without a dome, to be installed at the Observatory of the Pic du Midi, the tube of which, sealed by a glass plate at its upper part, would have been filled with helium.

I have given these detailed explanations in order to recall, in a specific case, how different can be the actual properties of an instrument and its theoretical properties. When an inventor forgets this important truth, the actual use of the instrument soon reminds him of it. But let us come back to our subject and take up again the "chain adjustment". Years will elapse before we know if this new instrument is as well adapted as the astrolabe for this particular purpose.

I will not wait any longer to answer objections raised by various authors about this chain programme, the legitimacy of which has been disputed. Their criticisms are based, in particular, on the closing errors, sometimes quite large, found for certain latitude stations equipped with zenith telescopes\*. The mean value of the closing errors for the years 1956 and 1957, for the four stations of the International Latitude Service, was  $-0''.124$  between the evening group and the middle group and  $-0''.101$  between the middle group and the morning group. But the individual values show a very large scattering. The extreme annual means were  $-0''.345$  for Ukiah and  $+0''.162$  for Kitab. If these closing errors had the same value for all the stations they could be eliminated by correcting the aberration constant but they are very different from one station to the other. This dispersion can be assigned only to

\* G. Cecchini, *Relazione sul attività del Servizio Internazionale delle Latitudini*, I.U.G.G. General Assembly, Toronto, 1957.

instrumental or to meteorological factors. I am myself convinced that the first occur much more frequently and are in general more harmful.

It may also occur that the local terrain or the presence of large buildings in the immediate vicinity of the instrument will give rise during the night to atmospheric turbulence which produces appreciable accidental refractions. Such cases are no doubt exceptional as the site of an observatory is as a general rule selected with full knowledge of these facts. But a fall in the temperature in the course of the night affects the level and the micrometer screw of the zenith telescope, and these effects contribute to the closing error of the chain programme.

The astrolabe does not have such disadvantages. Dr Guinot, who has determined the connections of our groups, has found for the first year of observation a very small closing error. He first smoothed the observations, as I have already stated for the group of January 22, in order to free them from individual or local errors. Then he computed the differences between consecutive groups observed *by the same observer in the course of the same night*. He started with group k and came back to the same group after a complete year. Table I gives the differences in time and latitude with the related standard deviations.

TABLE I  
1956-57

Groups	Time		Latitude		n
	unit : $0^{\text{s}}.001$		Unit : $0''.01$		
j-k	+ 15.0	$\pm 4.1$	- 2.9	$\pm 3.1$	5
k-l	+ 3.2	2.2	+ 0.5	4.0	12
l-a	- 8.0	1.7	+ 3.0	3.6	10
a-b	- 10.0	2.5	- 3.1	2.9	11
b-c	+ 23.0	1.7	+ 5.7	1.8	10
c-d	- 12.1	1.7	- 3.8	2.0	12
d-e	+ 3.0	1.7	+ 1.9	2.0	13
e-f	+ 0.5	1.1	- 2.6	1.2	13
f-g	- 0.5	1.5	- 7.3	1.6	14
g-h	- 12.8	1.5	+ 7.4	2.5	13
h-i	+ 1.7	1.8	- 4.9	1.4	15
i-j	- 1.6	1.5	+ 2.6	2.5	16
Closing error	+ 1.4	$\pm 7.1$	- 3.5	$\pm 8.7$	

These results have been confirmed by those of the second year of observations which will end by July next.

Certain differences, mainly in right ascensions, reach surprisingly high values. In the course of the same night, groups b and c give clock-corrections differing by an amount of 23 milliseconds. But what is still more remarkable is the smallness of the closing errors *which are not significant*. We can therefore assume that the chain method of adjustment is permissible, and we can conclude that not only the astrolabe but also the atmosphere of Paris is very stable. If, contrary to my opinion, the closing errors observed elsewhere were not chiefly due to instrumental causes, we would then have to admit that, by bad fortune, these other stations had all been located in exceptionally unfavourable sites and that Paris enjoyed uniquely privileged conditions. Flattering though this last assumption may be for the pride of the French, I strongly refuse to believe in the poor quality of the other latitude stations and I persist in accusing the zenith telescopes, the control of which seems to me very difficult. By control I mean a permanent and absolute control such as that which is provided for the astrolabe,

at least twice per night, by the determination of the prism angle and not a mere testing of the instrument at long intervals.

Having conclusively established that the application of group corrections is permissible, Dr Guinot computes the corrections required to refer the determinations of time, latitude and zenith distance to a provisional system more consistent than that of the FK<sub>3</sub>.

TABLE II

	a	b	c	d	e	f	g	h	i	j	k	l
Time (0 <sup>s</sup> .001)	+0.1	-10.0	+12.9	+0.7	+3.6	+3.9	+3.3	-9.6	-8.0	-9.7	+5.1	+8.2
Latitude (0 <sup>''</sup> .01)	+1.5	-1.3	+4.7	+1.2	+3.6	+1.2	-5.8	+2.0	-2.1	0.0	-2.6	-1.8
Zen. Dist. (0 <sup>''</sup> .01)	-7.3	+1.4	+2.4	+0.8	-7.4	+2.3	+1.8	+7.4	-3.2	+2.4	+2.9	-3.5

Fig. 8 represents the group corrections for time and latitude reduced to nearly the same scale, as an arc of 1 millisecond at the zenith in Paris is nearly equal to 0<sup>''</sup>.01. It appears at first sight that the right ascensions of the FK<sub>3</sub> are less homogeneous than the declinations. These differences may be partly

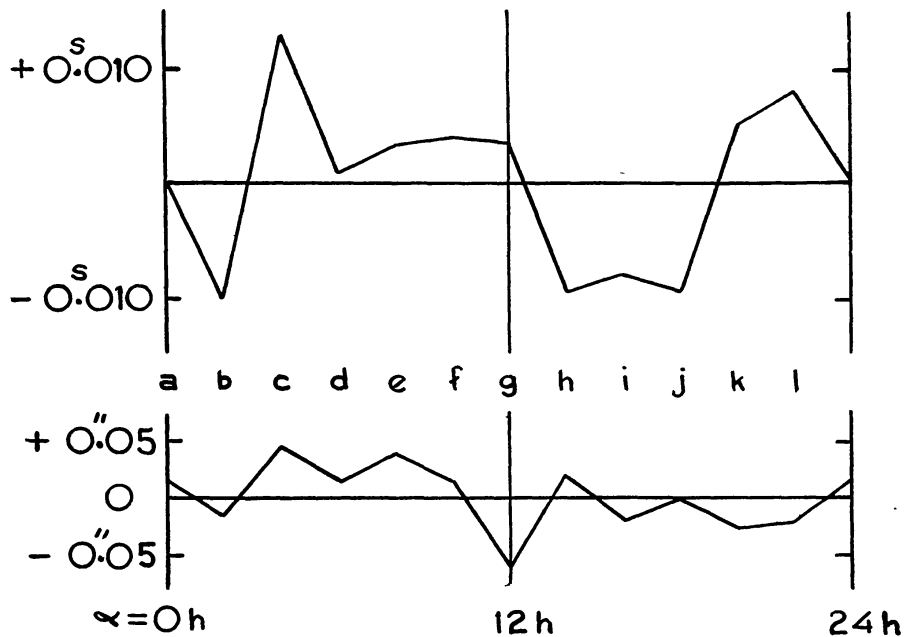


FIG. 8.

explained by the uncertainty of the proper motions, the computation of which depends to some extent upon meridian observations made before the introduction of the travelling wire micrometer. But that such deficiencies still exist in the FK<sub>3</sub> is a proof of the inability of conventional methods to link together stars with very different right ascensions.

The analysis of group corrections discloses an irregularity of small amplitude in right ascension depending upon  $\sin \alpha$ . The corresponding error in the declinations of the form  $\cos \alpha$  is still smaller. The pole of the provisional system, which practically coincides with that of the FK<sub>3</sub>, therefore differs but little from the

true pole. But the systematic errors of the FK3 are much smaller than the local errors, which concern areas of an extent comparable to that of the constellations.

We shall know in a few years whether the corrections given in Table II are stable or whether they vary in course of time. In the latter case their variations will enable corrections to the constants both of precession and nutation to be derived. But we shall have to wait for 19 years until we can accurately sort out these two constants.

It is well known that the constant of aberration  $k$  can easily be deduced from the closing error in latitude, but the value obtained from the first year of observations, equal to  $20''\cdot48$ , has only a low weight. B. Guinot has shown that the time determinations could also be used for this purpose. Group d can be observed in Paris for about six months. If the conventional value of  $k$  is too small by  $0''\cdot04$ , as has been suggested, then the clock correction given by this group will show a sensible variation compared with the determinations made on normal groups at the same dates. It will at first decrease, by a total amount of 3 milliseconds, and will then increase by the same quantity. It seems possible to obtain evidence of this effect.

It is not surprising that the determination of the constants requires long series of observations but apart from this long-term programme the data given in Table II have immediate applications: (1) to the determination of the irregularities of Universal Time; (2) to the study of the variation of latitude; and (3) to the establishment of a catalogue of stars referred to the provisional system of the astrolabe.

The first of these applications has been studied by Dr Arbey, the two others by Dr Guinot\*. My two associates have refrained from making any selection of the observational data. They have rejected no observations and have retained incomplete groups as well as those observed by personnel under training. But they have weighted the results according to their standard deviations by means of a suitably decreasing scale. They have then computed the means, including from 10 to 19 groups but more generally 13. All these means have nearly the same weight and they are shown by the points on the following figures derived from astrolabe observations grouped as just explained.

The diagram 9(a) was derived by Dr Arbey and represents the irregularities of the Universal Time TUO in Paris from 1956·5 to 1958·0.

These observations have been referred to the Uniform Time given by Dr Essen's atomic standard at the National Physical Laboratory which is at present the best tested of these standards. The time given by the crystal oscillators of Paris Observatory has been compared with Essen's standard through the intermediary of the MSF time signals. Dr and Mme Stoyko have shown that the definitive terrestrial time from 1955·5 to 1957·7 requires a correction of the form  $At^2$  to compensate for the effect of a progressive increase in the length of the day†. The Universal Time TUO represented on Fig. 9(a) has been corrected for this term. It is then directly comparable with the curve in Fig. 9(b), which represents the difference TUO-TU<sub>2</sub> computed by extrapolation by Dr Stoyko, in conformity with the instructions of Commission 31 of the International Astronomical Union for the rapid computation of the provisional time

\* A memoir by B. Guinot is in the press, to be published in *Bulletin Astronomique*.

† A. et N. Stoyko, *C.R. Acad. Sci. Paris*, **246**, 235, 1958.

TU<sub>2</sub>\*. Fig. 9(c) represents the difference of the ordinates of the curves 9(a) and 9(b), that is the provisional time TU<sub>2</sub>.

It is not possible at present to give an interpretation of this curve, as the final coordinates of the pole are not yet known, neither are the results obtained by other observatories which would enable the seasonal variations of the Earth's rotation to be computed. It can, however, be remarked that the rate of provisional TU<sub>2</sub> is systematic and in particular an annual variation can be recognized. I have endeavoured to smooth out the TU<sub>2</sub> irregularities and finally I have applied the following correction expressed in milliseconds:

$$\Delta = 6 \sin 2\pi(t + 0.15) + 2.5 \sin 4\pi(t + 0.05) + 5(t - 1957.5)^2$$

$t$  being the time expressed in years.

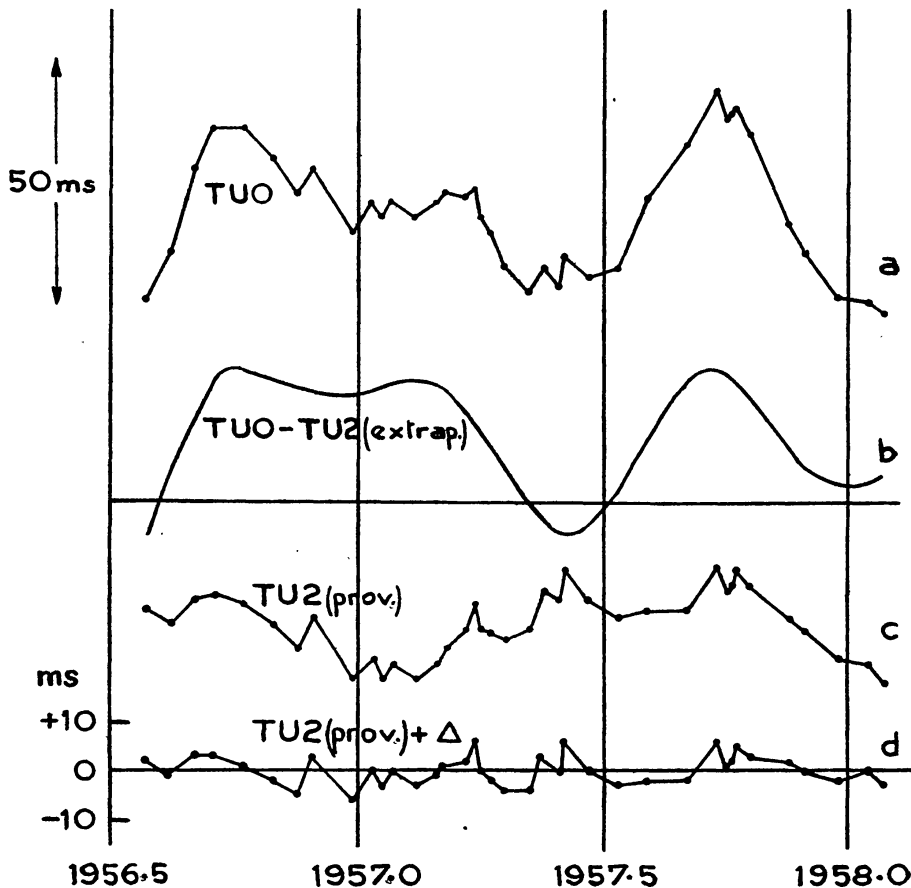


FIG. 9.

The residuals, shown on Fig. 9(d), show no systematic trend and their mean quadratic value is of 3.0 milliseconds. I wish to stress that the diagram includes 38 values for a period of 18 months, that is an average of 2 values per month.

What is the significance of the quantity  $\Delta$ ? The first two terms will possibly be justified when the final value of the difference TUO-TU<sub>2</sub> is known, though it is possible that a portion of these terms should be attributed to errors in the group corrections and to the annual variation of the vertical. A slow variation would not affect in any way the closing error and therefore could not be detected.

\* N. Stoyko, *Bull. Hor. du B.I.H.*, Série 4, 10, 203, 1956.

On this particular point the question remains open. As for the third term of the expression of  $\Delta$ , that is the term in  $t^2$ , it is to be added to the term identified by Dr and Mme Stoyko, the existence of which is confirmed by the present work. For the period covered by our observations, the gradual progressive rate of decrease of the terrestrial time is  $0.52 t^2$  milliseconds,  $t$  being here expressed in months (instead of  $0.484 t^2$ ).

It is not without interest to have been able to establish these empirical corrections, which make it possible to pass from the TUO time of Paris to the uniform time of the Essen's standard. When Ephemeris Time has been determined for the same period, the connection of the Essen standard to the conventional second of time will become easy. If we use the results of all the astrolabes now in operation throughout the world, including those to be delivered in the near future, it can be ascertained that this connection can be performed within 5 years with an accuracy of  $10^{-9}$ .

But of all the problems dealt with in this lecture that of the time is indeed the most difficult. To obtain good time determination it is not enough to have available a good atomic standard and a good optical instrument. The chronograph, the time signal receivers, the propagation time of radio waves can each give rise to sources of errors or uncertainties. Great progress remains to be made in the transmission of time. In addition, with the degree of accuracy that we endeavour to obtain, the personal equations of the various observers are not negligible.

I have endeavoured to show the personal equations by plotting separately the diagram of Fig. 9(d), for the three principal observers, Dr Arbey, Dr Guinot and Mlle Débarbat. For certain periods, the same details appear on these curves as, for instance, the low values of April–May 1957, which very likely are real. But for other periods the curves do not agree so well. For instance, in January 1957, we find  $-7$  milliseconds for the difference A–G while this difference is  $+4$  milliseconds at the end of June 1957. In general, a systematic difference seems to exist between the observers, as for the whole of the observations of 1956–1957 the following values are obtained:

$$A-G = -2.9 \text{ milliseconds}$$

$$G-D = -0.7 \text{ milliseconds.}$$

But these figures should not be interpreted as invariable personal equations. Indeed these differences are not stable; they vary fortuitously. Nowadays the name of “noise” is given to functions characterized by such random variations. We could therefore speak of “personal noise” instead of “personal equation”.

It is very difficult to explain the persistency of such a marked difference as that of January 1957 between two well-trained observers. On one hand it is rather unlikely that the deflection of the vertical takes different values for odd and even days. On the other hand the observations are carried out in such a way that a systematic error in the appreciation of the coincidence of the two images affects only the radius of the circle defined by the position lines, but not the position of its centre. It must therefore be assumed that the observer does not time the coincidence in the same way on the east as on the west and that the error therein involved varies from week to week or from month to month. For the time being this effect remains without a satisfactory explanation. I have asked M. André Lallemand to study a photo-electric receiver designed



especially for the astrolabe in order to increase the accuracy of the observations and to obtain with confidence that of one millisecond.

The determination of the latitude offers less complexity than that of the determination of time, as it is derived straight away from the observations themselves after the group junctions are completed. Fig. 10 was derived by Dr Guinot and represents the latitude of the Paris astrolabe since the middle of the year 1956. Our groups have been selected in such a way that they will be observable without any change until the year 2000. Homogeneous data will then be available for the determination of the secular wandering of the pole which is not the case at the moment.

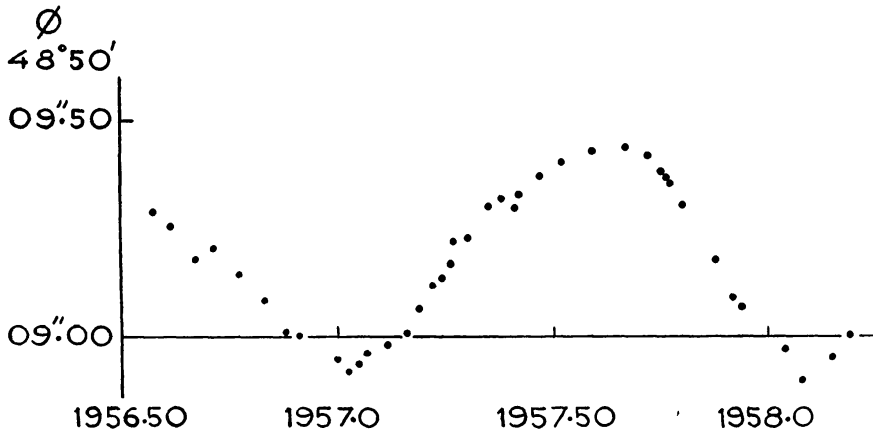


FIG. 10.

We have still to indicate what is the order of magnitude of the errors involved in both determinations. The average standard deviation of a group-observation is 4.3 milliseconds in time and of  $0''.062$  in latitude, including incomplete groups (the best determinations have given much smaller values, respectively 2.3 milliseconds and  $0''.025$ ). These figures refer to the consistency of the values within one group. If we compare the observations of groups between themselves by referring them to the smoothed curve, then the standard deviations take larger values, particularly for time, viz., 7.6 milliseconds and  $0''.073$ .

I shall now present the results of B. Guinot relating to the establishment of a catalogue of fundamental stars. In 1955 he presented to the General Assembly of the International Astronomical Union a note on the error  $\Delta\alpha_s$  of the FK3 derived from the results of the prototype astrolabe which was the only one in service at that time. Fig. 11 shows the similarity of the results obtained since then with the OPL astrolabe, although the observed stars are in general different in the two cases.

But B. Guinot has gone further. He has undertaken to determine with the astrolabe the coordinates of as many stars as possible, referred to the provisional system derived from the group corrections. He first computed the translation  $T$  which should be applied to each of the position lines to take into account:

(1) The internal smoothing of each group. It has been checked that these corrections were not affected by magnitude equations or by colour equations.

(2) The corrections of Table II.

He then derived from  $T$  the correction to the tabular coordinates of the star. If it has been observed at its two transits, east and west, and if  $T_E$  and  $T_W$  are

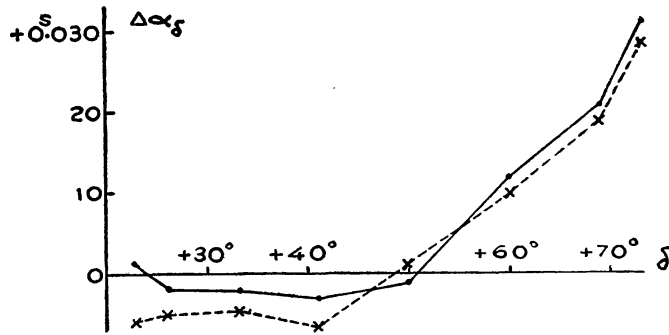


FIG. 11.— $\Delta\alpha_\delta$  (Astrolabe—FK3).  
Continuous line: experimental astrolabe (1954).  
Broken line: OPL astrolabe (1957).

the corresponding values of  $T$ ,  $A$  is the azimuth of a transit reckoned from the south, positively towards the west, and  $\eta$  is the parallactic angle, then the corrections to apply to the coordinates of the catalogue are

$$\Delta\alpha \text{ (Astrolabe - FK3)} = \frac{T_E - T_W}{2|\sin A| \cos \phi}$$

$$\Delta\delta \text{ (Astrolabe - FK3)} = -\frac{T_E + T_W}{\cos \eta}$$

In certain special cases, advantage can be taken of a single transit, for instance when the star is close to its maximum elongation. In that case it can be assumed that  $\cos \eta = 0$  and the right ascension is then given by the following relation

$$\Delta\alpha \text{ (Astrolabe - FK3)} = -\frac{T + \Delta\delta \cos \eta}{\sin A \cos \phi}$$

The same formula gives  $\Delta\delta$  for stars close to the southern limit in declination as it then can be assumed that  $\Delta\alpha \sin A = 0$ .

B. Guinot compiled in that way a first catalogue of 115 stars of which 47 are known by their two coordinates, 56 by their right ascension only and 12 by their declination only. The Institut Géographique National has lent to Paris Observatory a second OPL astrolabe, and special groups of stars, different from that of the routine programme, are now being observed in order to extend this catalogue to 300 fundamental stars, that is, more than half of those of the FK3 and of its supplement contained in the Paris zone. Already, interesting remarks can be made.

As for the declination, the astrolabe catalogue and the FK3 are in good general agreement. The average of the differences is

$$\overline{\Delta\delta} \text{ (Astrolabe - FK3)} = -0''.002$$

and is not significant. This is only a check of the adjustment. Fig. 12 shows the behaviour of  $\Delta\delta_\alpha$  for stars comprised in the zone  $+20^\circ$  to  $+30^\circ$ . Systematic differences are small but the statistics relate only to a limited number of stars. It is obvious that a constant error could affect the declinations of both catalogues.

In right ascension, the differences are larger and the distribution is more systematic. The chart in Fig. 13 gives a very expressive representation. Each star is there represented by a circle, whose diameter is proportional to the difference  $\Delta\alpha$  (astrolabe - FK<sub>3</sub>), black when the difference is positive and open when it is negative. When the difference is smaller in absolute value than 5 milliseconds the circle is replaced by a cross. We notice at a first glance a

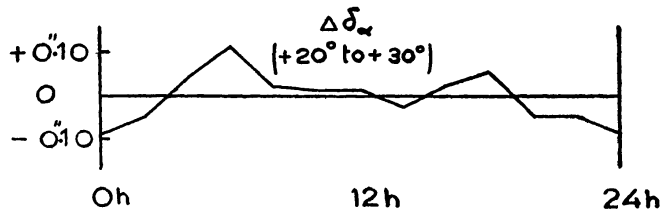


FIG. 12.

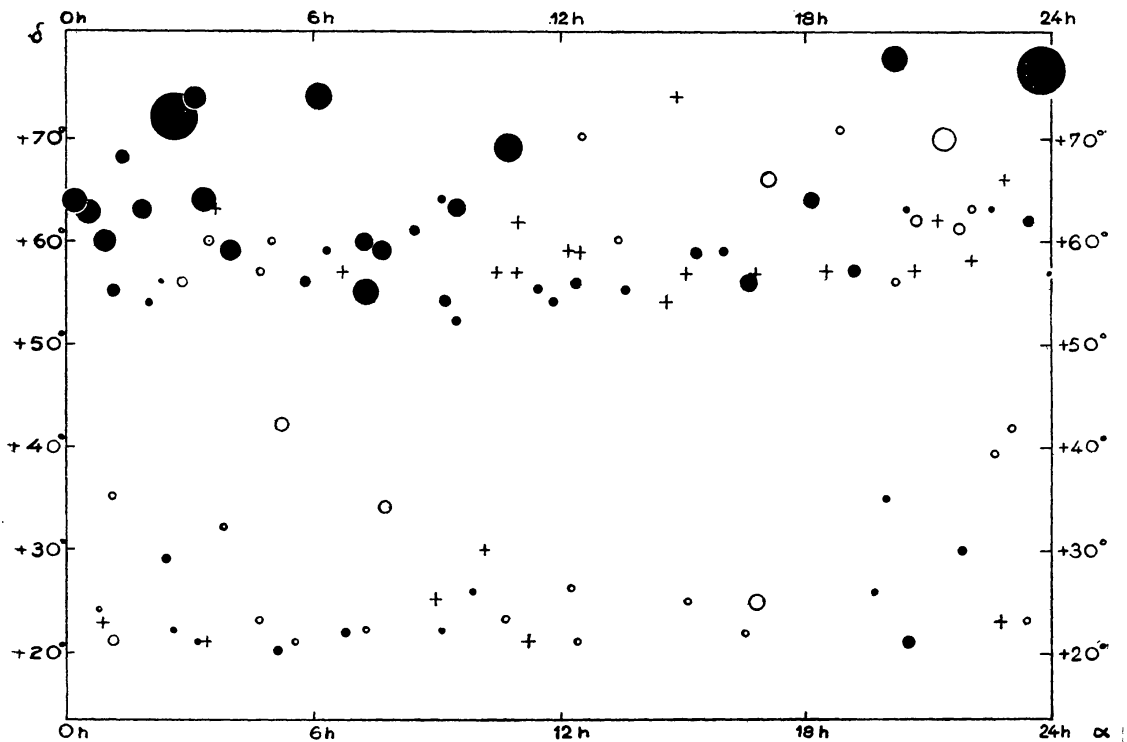


FIG. 13.

region of high declination, comprised between 23 hours and 11 hours, where the differences are very considerable and positive. It is indeed because of this anomaly that the difference  $\Delta\alpha_\delta$  increases so rapidly beyond  $+50^\circ$ . This illustrates the disadvantage of dividing  $\Delta\alpha$  into two terms. The FK<sub>3</sub> system is homogeneous between 11 hours and 23 hours. In this interval it would be sufficient to correct the individual positions to obtain a good catalogue with very small systematic errors for the coordinates as well as for the proper motions. On the contrary, to apply the correction  $\Delta\alpha_\delta$  in this interval would be injurious. If the correction were applied it would be necessary to compensate for it by applying another correction  $\Delta\alpha_\alpha$  of the opposite sign, this correction being a function both of  $\alpha$  and of  $\delta$ .

One may wonder why the astronomers have not hitherto realised the existence of these regional errors. The reason is very likely because it had become the practice to analyse the errors in an arbitrary way by means of a formula instead of considering very simply only  $\Delta\alpha$  and  $\Delta\delta$ . In my opinion, it would be advantageous to abandon this method which can in no way be justified and which is merely confusing. For the compilation of the FK4, it would be wiser to consider rather the individual errors, the local errors, and the systematic errors, the latter mostly arising from errors in the adopted positions of the pole and of the equinox.

Fig. 14 represents the mean values of  $\Delta\alpha \cos \delta$  between  $+60^\circ$  and  $+75^\circ$ . There are two conspicuous discontinuities at about 11 hours and 23 hours. The representation of such residuals by trigonometrical series would of course be nonsensical. The conclusion is that the conversion of the FK3 to the FK4 cannot be derived from harmonic analysis.

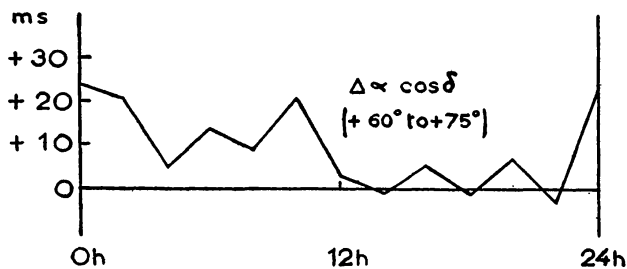


FIG. 14.

I will now make a last remark. In the case of meridian observations, the accuracy of which decreases from the equator as  $\cos \delta$ , the large discrepancies observed for high declinations can be allocated to random errors. But the accuracy of the measurements of right ascensions with the astrolabe depends only on  $\sin A$ . It is the same at Paris for the zone from  $+20^\circ$  to  $+22^\circ$  as for the zone  $+68^\circ.5$  to  $+73^\circ$ . Hence if the  $\Delta\alpha$  are much larger in the second zone than in the first, it is the catalogue that is responsible. The uncertainty of the group corrections could explain discrepancies of the order of 5 milliseconds but not of 20 milliseconds or more. The anomaly of the FK3 is found again, but to a lesser extent, in the N30 catalogue of H. R. Morgan. The discontinuities are smaller and less abrupt. However, positive corrections should still be applied to the right ascensions between 23 hours and 11 hours beyond  $+50^\circ$ . But the first astrolabe catalogue has as yet too few stars to allow for further detailed discussion.

If I have taken the liberty of presenting so many provisional results, perhaps too many, it is not so much for their intrinsic value, for I do not ignore that they have to be confirmed\*, as to emphasize the advantages of the method of equal altitude of stars. When this method is applied with the impersonal astrolabe, it provides for a permanent test, the zenith distance, which enables the quality of the results to be objectively evaluated. The several unknowns, namely time, latitude, corrections to the catalogue or to the fundamental constants, can be clearly separated from each other in a manner which cannot otherwise be

\* All the reductions will soon be revised using the high precision ephemerides which Dr Fricke, Director of the Rechen-Institut, has kindly undertaken to compute for the stars of the Paris zone.

attained in meridian astronomy. Instead of amending the catalogue hour by hour, it enables large areas to be dealt with, which are easily linked together. The results take an expressive form and their discussion leaves hardly any obscurities. It also gives to the computer an impression of confidence which I have observed with all my associates. I am convinced that this impression is not fallacious and that favourable prospects are really opened in the difficult field of fundamental astronomy, which is by no means a dead subject.