EXPLANATION OF THE METHOD OF OBSERVING

WITH THE

TWO MURAL CIRCLES,

AS PRACTISED AT PRESENT AT THE ROYAL OBSERVATORY OF GREENWICH.

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THE method of observing with two mural circles, as now practised at the Royal Observatory of Greenwich, differs from all others hitherto described, inasmuch as it embraces two or three very material considerations, which formerly have been little attended to, or neglected altogether. All writers who have explained theoretically the different verifications, which have been thought applicable to astronomical circles, have considered these instruments to be what their name implies, strictly of a mathematical figure; at least very nearly so: and hence they conclude, that if any one known diameter of this figure, can by any mechanical contrivance be placed in a given position, the relative situation of all the other diameters will become known.

The Greenwich method, so far from admitting the truth of this proposition, is founded on the supposition that it is totally erroneous; and that it becomes absolutely necessary to take a much more extended view of the subject.

An Italian student (under examination) being asked to define a circle, replied that it was "*una cosa tonda;*"—now this is exactly the proper definition of an astronomical circle. Admitting that, at the time the instrument is divided, it may be absolutely perfect, (from the extreme care that is taken to keep it in an exactly uniform temperature) yet the first moment it is exposed to the action of currents of air of variable temperatures, unequally acting on different parts of its surface, it will cease to be the same instrument: all that can be expected of it is, that it will oscillate about the perfect state in which it was, when divided; but even this cannot be relied on with certainty; for the effect of the exposure of a large instrument to unequal temperature is, that every part of it endeavours to take that situation which it finds most expedient to its own repose. Now the principal object of the method about to be explained is, to diminish as much as possible the mischievous tendency of this fluctuation. It will however be advisable, in the first place, to notice a peculiarity in the Greenwich system of observation, which consists in rejecting assistance from the intervention of either spirit-level or plumb-line, or indeed of any previous verification whatever; for a very little consideration will show us, that the adoption of any of these ordinary resources would be a contravention of the very principle which it is so essential to enforce; it would be substituting a vague undefined arbitrary point, or rather diameter of departure, instead of aiming at a general investigation of every point of each instrument, which alone can afford effective and satisfactory results.*

The only preparatory step to a series of observations, is to place each circle *nearly* in the plane of the meridian, and *nearly* perpendicular to the horizon. For the sake of convenience likewise, it is desirable that the six microscopes should be placed *nearly* equidistant from each other; but nothing more is required to be known about them. Neither is it at all requisite to attend to the position of the telescopes, with regard to the divisions of their respective circles; since the process now to be described will be found abundantly sufficient to bring out the required results, from mechanical considerations alone, the whole of the requisite data being supplied by the observations themselves.

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* This principle has been acted upon at the Royal Observatory ever since the year 1812, and was not adopted without due deliberation and a patient trial of other methods.

It ought, however, to be distinctly understood, that nothing here stated, is meant to apply to the use of a plumb-line as employed in instruments which turn freely in azimuth. Nothing can be more correct and efficacious than the application of a plumb-line to instruments of this description. In this case it does not act as a point of departure, but ensures the verticality of the axis. The theory of an altitude and azimuth instrument, is simply this: The instrument measures *double* the angle which the observed object makes with the axis round which the instrument turns; and if by any mechanical contrivance this axis can be made to point to the zenith, then *half* the observed angle is the zenith distance required. The method of dividing and registering the observations made with this instrument, so as in appearance to give altitudes and zenith distances, has rather a tendency to conceal this simple theory.

† This paper was added to the Greenwich Observations at the request of some members of the Council of the Royal Society.

Each circle is likewise supposed to be provided with an artificial horizon of mercury, so contrived as to command the greatest possible portion of the reflected meridian.

The first step in the process is to observe a number of stars simultaneously with each instrument, either by direct vision or by reflection; the telescopes being parallel to each other in each observation.

The object of this part of the process is to determine the exact quantity that one instrument marks more or less than the other, when each is directed to the same object.

Theoretically this should be accomplished by a single observation: but keeping in mind the leading principle of the method, as already established, it will be evident that our object can only be satisfactorily effected by the examination of a variety of diameters; and in proportion as these examinations are numerous and diversified, so will the relative position of each instrument be accurately determined.

It is thus that the quantity, called in the Greenwich observations *the mean difference,* is obtained for every 24 hours when the weather will permit. The exact determination of this quantity is of the utmost importance; and it is in consequence of its being obtained by such a complete analysis of each circle, that the two instruments may be now combined as one, and employed for the determination of altitudes. It is this second part of the process that is next to be explained.

A series of stars may now be observed reciprocally; that is, the direct image of the star by one instrument, at the same time that its reflected image is observed with the other. Unassisted by our previous investigation, this observation could give no result whatever; but the mean difference, determined as above, serves precisely the same purpose as the index error of HADLEY's sextant; and it is almost needless to observe, that by the proper application of this correction the altitude is ultimately obtained. Now with the altitude, we obtain likewise the knowledge of the position of the horizontal diameter of each instrument: but keeping in view the principle so much insisted on in the former part of the operation, we must not rest contented with a single determination of one diameter; but must, in a similar manner, from altitudes observed on various points of the arc, and by taking sometimes the direct and sometimes the reflected observation, with the same instrument, endeavour by every possible variety to obtain the maximum of precision of which the method is capable.

The position of the horizontal diameter of each instrument being thus deduced from a mean of all the preceding experiments, sufficient data are obtained for computing the places of those stars that have been observed in the first part of the process, and employed in computing the mean difference: for it must be recollected that, without the knowledge of their horizontal diameters, the instruments, with respect to the stars in question, give nothing but differences of declination; but by means of the horizontal diameters, the altitudes of the observed objects can be accurately determined.

It may now be understood how, in this method, the observation of each star is not insulated from the rest, but bears its due proportion in the determination of all the others. It thus not unfrequently happens, that an observation of the moon contributes decidedly to the more exact determination of a star. Each of the above processes would be imperfect without the other, but united they form a most powerful combination.

If it be admitted that the process above described be founded on unobjectionable principles, the observer need not be alarmed at the discordances which may accidentally occur in the use of it, since they will probably arise from many small errors falling in one direction; for it is in the very nature of this method to place not only every error of the instrument, but the slightest negligence on the part of the observer, in the most prominent point of view.

Another circumstance deserving of notice is, that by mere inspection of the observations themselves, without having recourse to any astronomical calculations whatever, we may form an exact estimate of the power and accuracy of each instrument. Whatever irregularities and deviations from the mean may be found in the table of *horizontal points,* published with the Greenwich Observations, we may expect to find similar deviations and irregularities in the observed altitudes of the stars; and if the astronomical results should be subject to greater irregularities than the numbers in these tables, those irregularities may fairly be attributed to some other source, and not to any defect in the instruments themselves.