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*RECENT PROGRESS IN GEODESY.**

THIS report is not based upon a complete examination of the recent literature of geodesy. The writer has been too busy to devote much time to the special research necessary to justify any claim to completeness. His regular duties have, however, kept him in touch with recent operations in geodesy, much more closely in touch, however, with operations in the United States than with those of Europe. The sketches of recent progress in each of several sciences called for by the rules of the Society are, as I conceive, to be written primarily, not for those well versed in that particular

* Forming a part of the Report of the Standing Committee on Mathematical Science for 1899. (Read before the Philosophical Society of Washington, February 3, 1900.)

science, but for the general information of others working on other lines. To this end a bird's-eye view of the present state of the particular science, together with a statement of the more important and interesting recent developments, will serve better than a detailed catalogue of the separate steps which make up recent progress.

By studying the recent reports of the International Geodetic Association and the geodetic reports from various countries, one is gradually convinced that there is well-sustained activity in geodetic operations in many countries, that geodetic facts are being steadily accumulated, and that steady progress is being made in improving methods and instruments. Having realized this, one naturally looks for published collections of results, and for some reasonably complete and well-digested scientific papers showing the relation between existing theories and results. One expects to find a considerable portion of the accumulated facts published in convenient form for the use of the hydrographer, topographer, the physicist, and the engineer. But this expectation is not realized. Of the great store of accumulated facts only a small part are as yet published in any complete or systematic way. The remaining ones are published, if at all, in such a fragmentary and disconnected form as to lose much of the value which they would otherwise have. Again, one looks in vain for any comprehensive study in recent years of the earth's figure and size. Since the publication by Colonel Clarke, some twenty years ago, of his values for the polar and equatorial radii, no corresponding comprehensive investigation has appeared in print.

It is not easy to understand why publication and discussion should lag so far behind the measures in the field. One consideration presents itself, however, as a partial, though insufficient, explanation. To adjust a network of triangulation requires an amount of expert computing which, as to time and cost, seems disproportionate to the field-work. Consequently, there is a decided tendency for the computations to be several years behind the corresponding field operations. By the time computations are finished for a given area new results of observations are available in that area or in adjacent connecting areas. Thus it always seems that the time for publication has not come, since whatever might be published would of necessity be incomplete.

It may be admitted that in many cases the computations made have been more complex and laborious than was warranted by the observations. The problem is one which lends itself to many theoretic refinements leading to long computations.

In what is above written as to delays in publication and computations needlessly prolix the writer must not be understood as referring chiefly to work in United States. Publication is apparently no further behind in this country than in other countries, with the possible exception of Great Britain. In simplifying the computations in office to correspond with the accuracy attained in the field, this country is easily foremost.

The years 1898 and 1899 mark an epoch in the history of geodesy in the United States. In 1898 the last of the field operations on the great transcontinental arc, extending from the Atlantic to the Pacific along the thirty-ninth parallel, were completed. During the same year the field measures of the oblique arc parallel to the Atlantic coast were also completed. In 1899 the necessary observations to complete an oblique arc in California, extending from Point Arena to the Mexican boundary, were brought to a close. In 1899 also, but little more than a year after the completion of the last field measurements, the computations connected with the arc of the thirty-ninth parallel were completed and the results put in form for the printer.

To appreciate the full force of the above statements one must have a realizing sense of the great length of the above arcs. The combined length of all the arcs used by Colonel Clarke in 1880 to deduce the figure of the earth was equivalent to 89 degrees of a great circle. The arc of the thirty-ninth parallel is equivalent in length to 37 degrees of a great circle; the Atlantic oblique arc is 22 degrees long, and the California oblique arc 8 degrees; and the combined length of these three arcs is 67 degrees, or more than two-thirds the total length used by Clarke.

The most interesting recent development in geodesy has been the investigations of variations of latitude. It may be asked, "Why is this investigation classed as geodetic rather than astronomic?" The answer is: It belongs in both classifications. Obviously it is an astronomical investigation, but it concerns the geodesist also; it concerns him directly, because the astronomical latitudes with which he must deal are now known to be a function of the time of observation. It is interesting to note also that many of the observations upon which our knowledge of the latitude variation depends were made with the zenith telescope, an instrument which the geodesist claims rather than the astronomer.

For ten years past there has been marked activity in investigating this question along two lines. One class of investigators worked by the inductive method, and slowly built up empirical mathematical expressions for the observed facts as to the variations, independently of any theory as to their causes. For this purpose they used many series of old observations, made at various fixed observatories, but not primarily for this purpose. In addition they have used many modern series of observations, made with zenith telescopes, for this special purpose. It has been shown that the motion of the pole in the past may be represented with considerable accuracy as a combination of two motions, each circular, one with a mean period of about 428 days and the other with a period of one year. A still closer approximation is obtained by assuming that the amplitude and epoch of each these motions is subject to a periodic variation. A somewhat closer agreement between the mathematical curves and the observations is secured when elliptical paths are substituted for the circular paths above referred to. The net result of the investigations by the inductive method has been certain mathematical expressions which closely represent the known facts of the past, but there is great uncertainty as to how far into the future these mathematical expressions may be extended, because their basis is wholly empirical.

The progress made along this empirical line of investigation has been due to Professor S. C. Chandler, of Harvard, more than to any other one man or, possibly, any group of men. His results have been published as the investigations proceeded in the *Astronomical Journal*, and form an exceedingly interesting series, not only on account of the remarkable success attained, but because of the ingenuity and skill shown in devising methods of investigation which are independent of any theoretical basis.

Another class of investigators has worked from the standpoint of pure theory. Their endeavor has been to furnish an adequate explanation for the observed facts. Up to the time when the latitude was shown, by direct observation, to be subject to periodic variations, one of the fundamental assumptions upon which astronomical computations were based was that the latitude of a given point on the earth's surface is invariable. Pure theory, indeed, indicated that the latitude of a point should be subject to a periodic variation of small amplitude with a period of about 305 days. Special investigations had failed to reveal a variation having that period, and astronomers had fallen back to the dogma of invariability. Theorists are now confronted with the necessity of accounting for a period of about 428 days instead of 305. It is known that any movements of the earth's crust and of the water and air in response to a displacement of the pole of figure from the pole of rotation have the effect of lengthening Euler's period. It has been shown that any difference in the equatorial radii also has a tendency to increase the theoretical period to correspond more nearly with the fact. The known atmospheric and oceanic currents have also been appealed to for an explanation. It has also been shown that it is not impossible that such impulsive forces as are concerned in earthquakes and volcanoes may produce some of the effects observed. The net result, however, of the investigations of theorists has been a series of partial explanations, no one of which stands all the tests which have been applied to it. Each fails, either by furnishing a law of motion which differs essentially from the observed facts or by being quantitatively inadequate.

This was the situation when the International Geodetic Association formulated and adopted a plan for determining the actual motion of the pole during a series of years with the highest attainable accuracy. The observations, in conformity with the plan devised by them, were actually begun in the latter part of 1899, and it is proposed to continue these observations uninterruptedly for at least five years. The plan of operations is that the actual motion of the pole shall be determined by simultaneous observations at four stations widely separated in longitude, at each of which an instrument of the highest precision is to be so used as to guard, as fully as possible, against all known sources of systematic error. The details of the plan have been worked out very carefully, and it is admirable in every respect.

The four stations selected are Gaithersburg, in Maryland, in longitude + 77; Ukiah, California, in longitude + 123; Mizusawa, in Japan, in longitude + 219, and Carloforte, on the small island of San Pietro, just west of Sardinia, in longitude + 351. One of these stations occurs in each of the four quadrants of longitude, reckoned from Greenwich. All are within three seconds of the parallel of 39° 8′ 10″. The conditions considered in selecting them, other than those indicated above, were that there should be nearly symmetrical conditions as to the character of the surface northward and southward of the station to avoid unsymmetrical refraction; that the hygienic, social, and climatic conditions should be such that the observer might remain healthy, comfortable, and contented; and finally in Japan and at the European station the region was carefully studied with reference to the probable frequency of earthquakes.

Two extra volunteer stations have now been added to these four, one at the Cincinnati observatory, which happens to be nearly on the parallel of the four selected stations, and one at Tschardjiu, in Turkestan.

The four principal observatories have been constructed with the utmost care, under specifications furnished by the International Geodetic Association, specifications designed to insure that observations shall not be vitiated by local differences of temperature. The observatory proper is surrounded by lattice-work to protect it from direct sunlight and the roof is double. During observations the roof is rolled back to leave an observing slit two meters wide. The walls are so low that the telescope projects above the roof during observations. This fact, together with the unusual width of the observing slit, puts the instrument virtually in the open air during observations.

The instruments used at the principal stations are specially designed zenith telescopes, by Wanschaff, of Berlin. The telescope has a focal length of about 51 inches, an aperture of $4\frac{1}{4}$ inches, and a magnifying power of 104. Aside from other minor peculiarities, two are especially noticeable. The barrel of the telescope proper is protected by an outer thin metallic tube, which is connected at but few points with the telescope proper, and serves merely to protect it against sudden changes of temperature. This false tube is pierced at various points to permit circulation of air in the space between it and the tube proper. The eyepiece is furnished with a reversing prism of peculiar construction, such that all observations may be made with the observing line apparently vertical and with the star apparently moving either upward or downward at the will of the observer. The observation upon one star consists of four pointings, two taken while the star is moving apparently upward in the field of view and two with the star moving apparently downward, the reversing prism being turned 180 degrees between these observations. If, then, the observer has a personal tendency to place the observing line too far to the right, this will have contrary effects in the two pairs of bisections, and the personal equation will be eliminated from the mean result. In so far as accidental errors are concerned, the few observations already made indicate a high degree of accuracy, the probable error of a single observation of a pair being about $\pm 0''.10$. Few series of observations yet made can show probable errors as small as $\pm 0''.16$.

The computed motion of the pole will be nearly independent of the errors in the assigned declinations of the stars, the effects of the errors of declinations being eliminated by the well-known group method. The stars to be observed are divided into twelve groups, and each group is observed for about two months (50 to 80 days, according to the time of year). During the first half of the period when group No. 2 is being observed group No. 1 is also being observed, and during the latter half of the period group No. 3 is being observed at the same time as group No. 2. Similarly, group No. 3 is observed first in connection with No. 2, and then in connection with group No. 4, and so on. The difference between the mean latitude from group No. 1 and the mean from group No. 2 during the period when both groups are being observed is obviously due to the difference of the mean error in declination of the stars of the two groups and to accidental errors of observation. This method of observation, after being extended throughout the whole cycle until group No. 12 overlaps group No. 1, furnishes a means of determining the declination correction to each group to reduce it to the mean of all the groups, and thus to eliminate the declination errors from the computed change in latitude.

As an additional precaution, the same list of stars is to be observed at all the stations.

The effect of an error in the assigned proper motions of the stars observed will be to make the latitude of any one station appear to increase or decrease with lapse of time, but will have no appreciable effect on the value finally derived for the motion of the pole; for, the same list being used at all stations, all the latitudes will appear to increase or decrease together. Both the Japan and the Maryland station, would appear, say, to have increasing latitudes, although they are nearly on opposite sides of the pole, and therefore this result could not be mistaken for an actual motion of the pole.

Aside from the precautions already indicated against abnormal refractions due to local conditions, the observations themselves have been planned so as to guard against errors arising from this cause. Each group contains six pairs which are used directly for computing the latitude variation, and of which very few have zenith distances exceeding 20 degrees. Each group also contains two pairs introduced for the special purpose of studying the actual refraction, and of which the zenith distance is about 60 degrees. The normal refraction at a zenith distance of 60 degrees is about four times that at 20 degrees; hence the two refraction pairs will furnish an effective method of determining any peculiarities of refraction which are sufficiently great to produce any appreciable effect upon the latitude pairs, provided such effect is one which increases with the zenith distance.

Any refraction effect which is analogous to a displacement of the apparent zenith, by a persistent barometric gradient, for example, will not be put in evidence by this test. To eliminate such an error, dependence is placed upon the fact that the final result is based upon observations at several stations varying greatly in longitude and in the surrounding climatic and local conditions.

It may seem at first sight that an annual variation in refraction would produce an apparent annual motion of the pole. This would be true if the motion of the pole were derived from observations at one station only. It will be seen, however, on further reflection that annual variations in refraction would tend to make all the latitudes along one parallel apparently increase and decrease together, and that therefore the computed motion of the pole would not be appreciably affected if these annual variations were of about the same magnitude at the different stations distributed around the pole.

To sum up, the discovery of the periodic motions of the pole was first made by a purely inductive method. The laws governing those motions have been slowly and painfully deduced by a continual application of the same method to old series of observations and to many new series made for the special purpose. Now a new campaign of observations, promising results more accurate than any hitherto obtained, has been commenced. The mean position of the pole for each fortnight of the next five years will probably be known within a radius of five feet. There is little prospect for improvement of the observational side of this question for many years. The new observations will furnish material for new triumphs of the inductive method in furnishing a still closer mathematical approximation to the unknown laws of motion of the pole. The interesting feature of this investigation is now that the theorists are at sea, so to speak. Will they at the end of the five years be able to furnish an adequate explanation for the new facts observed or, indeed, for those already known? It is on this theoretical side of the investigation that new energy is needed. Here is a golden opportunity for some one well versed in mechanics, astronomy, and geodesy.

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THE PLANKTON OF FRESH WATER LAKES.*

For some years I have been interested in the subject of the fauna of our fresh water lakes. This interest was first aroused in regard to the animals of the deeper parts of the lakes. The results of the explorations of the depths of the ocean were just becoming known. I had read in the older works that while the sea was densely populated along shore, and had what has become to be known as a 'pelagic' fauna and flora in the open sea, remote from land, the depths were a barren region utterly devoid of both animal and vegetable life. But later it had been shown that there was, even in the

* Address of the retiring President of the Wisconsin Academy of Sciences, Arts and Letters.