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SYMPOSIUM ON THE EARTH: ITS FIGURE, DIMENSIONS AND THE CONSTITUTION OF ITS INTERIOR.

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IV.

VARIATIONS OF LATITUDE: THEIR BEARING UPON OUR KNOWLEDGE OF THE INTERIOR OF THE EARTH.

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To review even hastily the contributions that astronomy has made to our knowledge of the figure and dimensions of the earth and the constitution of its interior, would consume more time than I can fairly claim as my share this afternoon. Let me therefore pass over those points that are on accepted ground and are matters of general agreement from the different points of view represented in this symposium; and let me dwell instead upon certain recent developments especially in need of consideration, concerning which the astronomer desires the criticism and help of the geologist, the seismologist, the physicist, and the meteorologist. These developments have come to us directly or indirectly through a study of latitude variations, so that most of what I shall have to say will deal with this subject.

Although variations of latitude are in a sense a very recent addition to our knowledge, yet on the theoretical side, at least, we find the beginning more than a century and a half ago. In 1755 Euler considered "the rotation of solid and rigid bodies" in a memoir that is now recognized as the foundation stone for our edifice. He showed that if such a body is projected into space it will exhibit two kinds of rotation; the first of these is the familiar one that corresponds to the day in the case of the earth; the other is more subtle and corresponds to the variation of latitude. By reason of this the axis of the diurnal rotation is continually changing within the body, progressing in a regular way and coming back after a time to its earlier positions. An ordinary top gives us a simple example of this kind of rotation. The spinner imparts to the top a motion of translation as well as a rotation, and if we wish to study the rotation we must arrest the translation in some way. This we can do by letting the top fall upon a hard surface in which the iron peg soon wears a minute hole for itself, and the effect is to stop the translation of the top without modifying seriously the rotation. Then we can see that while the top is turning very rapidly around an axis, this axis is itself rotating in a comparatively leisurely way. Just the same thing is occurring with the earth: the point (or pole) at which the axis of the daily rotation pierces the surface of the earth is continually in motion. If we could take to the neighborhood of the pole a modern instrument, and if we could observe there at leisure and in comfort, we should have no particular difficulty in finding the position of the pole within a meter. But if we should repeat these observations a few months later we should find that the pole had wandered away to some distance. To be sure, this distance would not be great and all the wanderings of the pole that have thus far been observed could be plotted to true scale on the floor of a room not much larger than the one we are in. Of course if the pole is moving, so too is the earth's equator; and thus the latitudes of all points on the earth are varying. Such wanderings as these need not disturb the peace of mind of those gentlemen who like to discover the arctic or the antarctic pole. Under the circumstances that the polar explorer must work and with the meager instruments he can transport, he is glad to determine his latitude within half a mile of the truth.

We must understand that it is only in our time and only after the lapse of many years since Euler published his memoir, that latitude variations have actually been observed. There was nothing in Euler's theory to indicate how large a variation to look for, since this is a matter that depends upon the whole complex of "initial conditions," of which our knowledge is the very vaguest. But this theory does tell us what the period of the variation should be, since this depends upon the shape of the earth and the distribution of the material within it, and precisely the information that is here needed is afforded by a study of precession. Applying this information Euler was able to say that the period of the latitude variation should be ten months. Bessel at Königsberg in 1842, later Peters at Pulkova, Nyren also at Pulkova, Downing at Greenwich, and Newcomb at Washington, all searched their observations for evidence of a latitude variation having a period of ten months, but all in vain. Astronomers concluded that if latitude variations existed at all, their extent was too small to be detected by instruments of the precision that had then been attained.

Toward the end of the nineteenth century vague whisperings that this conclusion might be incorrect seem to have been in the air. But the first clear word to this effect came in 1888 from the lips of Küstner at Berlin. He had invented and applied a method for determining the amount of the aberration of light; but he found that his observations gave well-nigh impossible results, agreeing neither among themselves nor with earlier reliable observations. By a nice chain of logic he was able to exclude one possible explanation after another until there was left only the supposition that the latitude of his station had changed while his observations were in progress. Next he examined nearly contemporaneous observations made at other places, and when he found that he could account for certain puzzling discrepancies, he no longer hesitated to announce that latitudes were variable after all.

This announcement awoke the liveliest interest and encountered no little scepticism. Special observations were at once set on foot at various observatories in Europe and America, as well as at a station near Honolulu in the Sandwich Islands. These islands are about opposite in longitude to the European stations, and this was the reason for establishing a station there. For obviously if the pole is really changing its place then the changes in latitude for two opposite stations will be the reverse of each other. When in 1893 this was found actually to be the case, other possible explanations for the observed phenomena at once fell down, and latitude variations became for the first time a universally accepted fact.

Much time and effort have since been expended in attempting to formulate the "laws" of latitude variations and to give them a mechanical interpretation. But observation has shown that the variations are of unexpected complicity, and as a consequence we are still very far from having satisfactory knowledge of this subject. By the same token it is probable that an intensive study of these variations, particularly from points of view other than the astronomical, will teach us much concerning the interior of the earth as well as some of its surface phenomena.

It was the late Dr. Chandler, of Cambridge, Massachusetts, who took the lead in investigating the nature of latitude variations. By overhauling ancient observations (made of course without any reference to the present subject) he was able to trace the presence of the variations back to the time of Bradley in the middle of the eighteenth century. Thus it happens that at the very time that Euler was writing the first theoretical paper on the subject, Bradley had already begun making the observations from which the actual existence of latitude variations might have been proven at once. Chandler was able to gather similar evidence from other miscellaneous series of observations and thus to set down a tolerably continuous record of the variations during a century and a half. However interesting a fact this may be from an historical point of view, it does not help very much in a practical study of the subject. There are two reasons for this: first, it is only for European stations (and for the most part only for Greenwich) that we have any knowledge of these earlier variations; the other component of the wanderings of the pole, namely that in the meridian at right angles to the meridian of Greenwich, did not begin to be known until very recently. Again, these ancient observations were undertaken for certain definite purposes that they served as well as could be expected for their time; but they were not intended and are not well suited for precise determinations of the latitude. Close acquaintance with the subject has taught us that exceedingly delicate observations are necessary to define the variations with adequate accuracy. If I held in my hands two plumb lines half a meter apart, they would not be quite parallel to each other, though both are exactly vertical; if they were prolonged, they would meet somewhere near the center of the earth, 4,000 miles below. The angle between them is a little less than 0″.02 and represents approximately the accuracy that is demanded and that has recently been attained in latitude observations. This success is due chiefly to the International Geodetic Association which has organized an "international latitude service" of high efficiency, and to whose efforts and experience are due the improvements in instruments and methods that have made possible this extraordinary degree of precision. Since 1899, the Association has maintained six observing stations for this sole purpose, two of these being in our own country. One of the minor effects of the war that is now raging in Europe will be the discontinuance of some of these stations. One of the American stations has already been abandoned and the same fate will overtake the other in June, 1916, unless some independent means of maintaining it, at least temporarily, presents itself soon. An interruption of these observations would be a great pity, for this is one of the cases where a continuous record is highly desirable.

To return to Chandler and his work on these variations, perhaps the most important of his achievements was to show that the principal term in the variations, instead of having a period of ten months in accordance with Euler's theory, has in reality a period of fourteen months. This difference explains the failure of Bessel and all the others who preceded Küstner to find a latitude variation in their observations; for, relying upon Euler's results, they had all tested their observations for the ten-month variation and had sought for no other variation. For the same reason, Chandler's announcement of the longer period was received with incredulity in some quarters, and this feeling did not vanish until Newcomb pointed out that Euler had made a certain assumption regarding the interior of the earth that had in the meantime been universally discarded; his period of ten months applies in fact only to a perfectly rigid and unyielding earth. Newcomb showed that if the earth yields to deformation to the same extent as though it were composed throughout of steel, then Euler's period would be lengthened to about fourteen months. Here we have the first dependable determination of the rigidity of the earth, a result that has since been confirmed in several ways, particularly by a measurement of "bodily tides" in the earth.

The fourteen-month term (or the modified Eulerian term as it is now called) has been under accurate observation for a quarter of a century. The period can probably (though not certainly) be regarded as constant. This is what we should expect, for a change in this period would call for a sensible alteration in the distribution of the material within the earth, or a change in the rigidity of the earth. The amplitude of this term presents a very puzzling problem. Its usual value is about 0".27, but twice in recent years it has jumped to about 0".40. Such a change could be accounted for by supposing that the earth had received a severe blow or a succession of milder blows tending in the same direction. We are reminded that both Milne and Helmert have suggested that there might be a direct connection between latitude variations and earthquakes. This suggestion was originally made by Milne very early in this century when the astronomical data necessary to test it were still very meager. It is to be hoped that the question will be taken up again in the light of the information that has been added during the past ten or twelve years.

Though the Eulerian term is the largest part of the latitude variation, it is by no means the only important one. We have next an annual term with a maximum amplitude of about 0".20. We may say with some confidence that this term is seasonal and meteorological in its origin, but at present no more definite statement would be warranted. It was early suggested that ocean currents might cause this variation. These currents would have to vary greatly with the season, either in the volume or the speed of the flow, or in its direction; for an unvarying current would merely modify the Eulerian term once for all and would leave the latitude variations otherwise unchanged. A similar suggestion has been made with regard to air currents, and appeal has also been made to unequal deposits of snow and ice on two opposite hemispheres of the earth, to account for the annual term. It seems to me that these explanations have not been subjected to the critical numerical tests that are possible and desirable. The meteorological data are doubtless competent to enable us to compute at least the order of the effects in the latitude variations that we should expect from these various causes. Furthermore the annual term is probably variable in its amplitude, and it is important to ascertain how (if at all) these changes are related to the corresponding meteorological observations.

One other term must be mentioned in this brief summary. A few years ago Kimura of Japan made the important discovery (the most striking contribution to astronomy that has ever come out of Asia) that the latitudes of all stations are affected by a variation that does not depend upon the longitude but which is the same for all points in the same latitude. In other words there is present a variation that is not due to the wanderings of the pole. To ascertain more closely the nature of this term, the International Geodetic Association extended its latitude service temporarily to the southern hemisphere, with the result that the term was found to be of precisely the kind that would be caused by an annual wandering of the center of gravity of the earth to and fro along the axis of rotation. This must be regarded merely as an illustration and not as an explanation, for so great a change (about three meters) in the position of the center of gravity is excluded on other and very conclusive grounds. No plausible explanation for the Kimura term has as yet made its appearance, and as a consequence the reality of the term has been questioned from every possible point of view. Many explanations have been advanced, each of which sought to account for the term as merely an instrumental effect or the like, just as was the case twenty years earlier with the whole of the latitude variation itself. Against such attempts the Kimura term has held up very well. It is not too much to say that at the present time all but one of the numerous explanations of this class have been disposed of; this exception deserves a brief mention, particularly as it calls loudly for the attention of the meteorologist. Let us suppose that the layers of equal density in the atmosphere above a station are not horizontal, but that they are sensibly inclined. If this occurs without our knowledge, as it would under ordinary circumstances, then we should apply refraction to our observations in a slightly erroneous way and we should derive a value for the latitude that is not quite correct. Let us suppose further that this effect were a world-wide one and that in any given month there would be a pronounced tendency for the inclination to be in the same sense in all latitudes, north and south, as well as in all longitudes. Then we should have a set of circumstances that would account for the Kimura term as an atmospheric effect, and therefore it would be excluded as a real variation of latitude. So far as the astronomer is able to testify, the evidence is against the occurrence of such tilts in the atmosphere. The inclination required to account quantitatively for the amplitude of the Kimura term is over two minutes of arc, or a slope of about one part in fifteen hundred. Presumably in a few years we shall be able to say something more definite as to the possibility of the existence of such conditions. My own opinion is that this explanation, like so many others of similar character that have been suggested for the Kimura term, will be found untenable. Further I venture to think that latitude variations as a whole will find their explanations less on the surface of the earth and more in its interior than seems now to be the generally accepted opinion.

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