A paper of exceptional value and interest, on the morphology and biology of the green apple aphis (*A. posni*), is contributed by A. C. Baker and W. F. Turner to the Washington *Journal of Agricultural Research* (vol. v., No. 21). This is the "common apple aphid" in North America, as well as in these countries, and the whole life-cycle is passed on the apple. Very full and careful descriptions of the structure of the various forms are given by the authors, who, in the course of their season's work, examined no fewer than 75,000 specimens. Stages in the embryonic development are described, from which it appears that the embryo, after five days' growth, has a long resting period through the colder season of the year, lying in the centre of the winter egg. Of all the results obtained, however, the tracing of the succession through the spring and summer of a number of forms derived from a single stem-mother is the most important. Among the daughters of the stem-mother there may be one winged insect, and interesting "intermediates"—virgin females with rudimentary wings—appear together with the usual winged and wingless aphids. Sexual individuals may appear in the eleventh generation from the stem-mother, the earlier ones appearing as brothers and sisters of parthenogenetic females. The authors believe that temperature is by far the most important factor in determining the appearance of the sexual insects.

A paper by J. R. Malloch, on Chironomidæ and other Diptera from Illinois (Bull. Ill. State Lab. Nat. Hist., vol. xi., 4), is noteworthy because the systematic descriptions of the midges and flies are accompanied by detailed, well-illustrated accounts of the larvæ and pupæ of many genera of Mycetophilidæ, Asilidæ, Bombilidæ, Syrphidæ, and other families. G. H. C.

CHILIAN METEOROLOGY.[1]

ALTHOUGH Chile, in common with other South American countries, has suffered greatly from the conditions brought about by the European situation, the large budget of memoirs recently issued by Dr. Knocke shows little, if any, restriction in the work of the Central Meteorological and Geophysical Institute during 1915. No. 13, part i., of the Meteorological Year Book gives *in extenso* the tri-daily observations carried on at thirty stations during the year 1913, the data comprising barometric pressure, air temperature humidity, wind direction and force (the latter both in Beaufort and by anemometer), cloud, rainfall, evaporation, and exposed temperatures.

In No. 15, part ii., of the Meteorological Year Book the data are summarised in great detail from records kept at fifty-two stations, daily, monthly, and annual abstracts being given. As the stations cover more than 35° of latitude, and range in altitude from 4 to more than 3500 metres, all varieties of climate are to be found among the records. The warmest station, apart from Easter Island in the Pacific, was Arica, mean temperature 19·4° C. (66·9° F.), and the coldest Punta Arenas, 6·3° C. (43·3° F.). The absolute maximum was 38·3° C. (100·9° F.) at San Felipe, lat. 32° 40′ S., height 635 m., and the absolute minimum −8·0° C. (17·6° F.) at Punta Arenas. The effect of the cold Humboldt current in keeping down the temperature is well shown in the data for Arica (lat. 18½° S.) and San Felipe, the mean daily maximum values at the latter station on the mean of the year being 1·7° C. higher than at Arica, 14° nearer the equator, and situated at sea-level. A comparison of the temperature data from Ollagüe, at a height of 3695 metres, with those from Iquique shows a fall of 1° C. for each 323 m., both stations being close to lat. 20½° S.

[1] Instituto Central Meteorológico y Geofisico de Chile, Santiago, Dr. W. Knocke, Director. No. 13, "Anuario Meteorológico de Chile, 1913." Pp. 339. No. 14, "Medidas de agua caída en 1913." Pp. 71+plates. No. 15, "Anuario Meteorológico de Chile." Segunda parte. Pp. 134+plates. No. 16, "Valores horarios de los elementos meteorológicos en Los Andes, 1911 y 1912." Pp. 81+plates. No. 17, "Valores horarios de los elementos meteorológicos en Santiago, 1914." Pp. 91+plates.

Great variations in the mean amount of cloud are to be found, the mean annual values ranging from 0·9 at Calama in the north to 8·8 at Evangelistas, near the Pacific entrance to Magellan Straits. At the former station there were 327 clear days (cloud amount less than 2) and not a single cloudy day (cloud amount more than 8), while at Evangelistas only 2 days were clear and 305 cloudy. It is of interest to note that at the island of Juan Fernandez the barometric indications are very frequently an index of those taking place twenty-four hours later on the Chilian coast in about the same latitude.

No. 14 gives the daily rainfall recorded at 112 stations for the year 1913, arranged in parallel columns, thus exhibiting the distribution of the rain throughout the whole length of the country. The wettest station was Cape Raper, lat. 46° 49′ S., long. 75° 36′ W., with 4607 mm. (181·38 in.), the values for December being interpolated. At Calama and Copiapó in the north no rain fell, and ten other stations, all to the north of 30° S., had less than an inch. Hourly rainfall values are given *in extenso* for seven stations. From these records it is seen that torrential rains are uncommon, there being only two instances of more than an inch (25·4 mm.) falling in an hour, the maximum hourly fall being 40 mm. at Contulmo.

We are glad to see that in No. 16 Dr. Knocke continues to give hourly values of all the elements, the station selected in this instance being Los Andes, situated at the foot of Aconcagua, at a height of 820 metres, where the Chilian section of the Trans-andine railway begins. Los Andes enjoys an admirable climate—cool in summer and temperate in winter. Although 300 metres higher than Santiago, the mean temperature is slightly higher, while peaches and walnuts flower a fortnight earlier than in the Chilian capital. No. 17 of the memoirs contains the hourly values for the year 1914 of the principal climatic elements at Santiago, including earth temperature and the electric conductibility of the air observed once daily by means of a Wulff electroscope.

R. C. M.

THE MOVEMENTS OF THE EARTH'S POLE.[1]

MORE than a century ago it was shown by the mathematician Euler that if the axis round which the earth was rotating were not coincident with the axis of figure, which latter in the case of a spheroidally flattened earth is the shortest axis that can be drawn, the axis of rotation will revolve about the axis of figure in a period which, upon certain assumptions, can be precisely predicted. The time of one revolution of the pole of rotation around the pole of figure depends only upon the shape and degree of elasticity of the earth. In Euler's days the supposition that the solid earth had any appreciable elasticity was so far outside the range of experience that it was not considered by him. He calculated the period of the polar rotation on the assumption that the earth was perfectly rigid, and showed that this period would be about 305 days.

If we determine the latitude of a point on the

[1] Discourse delivered at the Royal Institution on Friday, May 19, by Col. E. H. Hills, C.M.G., F.R.S.

earth's surface by observations of the stars, we are in effect measuring the angular distance between the axis of rotation of the earth and the vertical line, or line through the zenith, at the point of observation. If, now, this axis of rotation moves, the observed latitude of the place will change, and if we prolong the observations over a sufficient time, we ought to find that this observed latitude fluctuates backwards and forwards about a mean value with the same periodicity as that in which the earth's pole of rotation moves round the pole of figure.

Every observer who is engaged in making observations to determine the precise positions of the stars, a class of observation which up to a few years ago occupied a very large fraction of the time and energies of astronomers, is actually continually determining and redetermining the latitude of his instrument. There is thus an enormous mass of latitude observations available for examination, and it should prove a not too difficult task to analyse these with the object of detecting a periodic variation. Two causes, however, militated against success in this inquiry: first, the very small magnitude of this variation; and, secondly, the fact that the earth is by no means rigid, and hence that the true period of the precessional rotation differs very substantially from the Eulerian period of 305 days.

All the earlier attempts to find evidence of this variation were, in fact, hampered by this preconceived notion of the ten-month period; the observations were carefully scrutinised with the view of detecting it, a process, as we now see, foredoomed to failure. It would be a useless task to recount here the various attempts that were made. Two of these, however, I should not like to pass over without notice, those of C. A. F. Peters, at Pulkowa, and Clerk Maxwell in this country.

Peters in his great and classic memoir on the parallax of the fixed stars devoted one section to a discussion on the variability of the latitude in a ten-month period. He found that the actual variation derived from the observations was of so minute a magnitude that it was well within the limits of unavoidable sources of error, and he therefore concluded that if there was any separation of the two poles it was too small to be detected by observation.

Clerk Maxwell examined the Greenwich observations of Polaris in 1851–4, and thought he found some small indications of maxima at about ten-month intervals, but he considered the results as very doubtful, and that more observations would be required to establish the existence of so small a fluctuation.

Substantially the same result was derived by other inquirers. Astronomers were therefore satisfied, up to the year 1884, that the earth's axis of figure was so nearly coincident with its axis of rotation that the difference between the two was inappreciable to the most refined observations. All methods of observation and all principles of the reduction of observations, both of astronomers and of geodesists, were tacitly based upon the idea of absolute coincidence between the two axes.

In 1884 the subject was independently reopened by two men—Chandler in America, and Küstner at Bonn—and entirely fresh light was thrown upon it. Their work was simultaneous and quite independent. I will take Chandler's first.

In 1884–5 he took a thirteen-month series of observations at Harvard with an instrument of his own devising, to which I will revert later. These observations showed a progressive change in the derived latitude, which appeared to him of a greater magnitude than could be accounted for by any instrumental errors. He, however, hesitated to ascribe it to a real change in the latitude without further confirmatory observations, which he could not then make. He therefore put these observations aside, and was, six years later, drawn to re-examine them by the publication of some of Küstner's results, which were also only explicable on the hypothesis of an actual variation in the latitude of the place of observation. It was, however, quite obvious to Chandler that his series of observations contained no warrant for an Eulerian period of ten months, and he therefore, to quote his own words, "deliberately put aside all teachings of theory, because it seemed to me high time that the facts should be examined by a purely inductive process; that the nugatory results of all attempts to detect the existence of the Eulerian period probably arose from a defect of the theory itself, and that the entangled condition of the whole subject required that it should be examined afresh by processes unfettered by any preconceived notions whatever." This bold rejection of theory and appeal to observation alone was rewarded with immediate success, and Chandler was able to show that his observations of 1884–5 contained unmistakable evidence of the rotation of the one pole about the other in a period of, not 305 days, but 428 days. Wherein, then, lay the deficiency of Euler's investigation? As already hinted, this arose from the assumption of rigidity, and it was shown first by Newcomb, and afterwards, more completely, by Hough, that the 428-day period was fully in accord with a degree of elastic yielding of the earth quite consonant with probability. Hough showed that if the earth were as rigid as steel the period would become 440 days; that the actual period is somewhat shorter than this means that the earth as a whole is decidedly more rigid than steel, a result which accords perfectly with other known phenomena which depend upon the earth's elasticity, such as the rate of propagation of earthquake waves.

Immediately following on this initial success Chandler undertook a prolonged and most laborious examination of old observations and reached results which have not completely borne the test of subsequent review. He was confident that the whole movement of the pole might be explained as the superposition of two rotations, one circular, with a 428-day period, and one elliptical, with a period of a year. He thought, further, that there was evidence that the longer period had varied in past times, and that in 1770 it was less than a year. This last result was traversed by Newcomb, who showed its extreme improbability. While fully bearing in mind the lessons of past experience as to the unwisdom of relying too closely upon pure theory, we cannot resist the conclusion that to accept any large change in the 428-day period within recent years would be to set aside the whole dynamical justification for accepting this period as a reality, it being quite impossible to admit that the elastic constants of the earth can be subject to any appreciable alteration within such time as a century or so.

As regards an annual period, we should now prefer to say that, while there are doubtless seasonal transfers of material upon the earth, such as the accumulation and melting of Arctic ice, which may produce a movement of the pole with an approach to a yearly periodicity, the part of the movement due to a true annual period is very small, and is quite masked by large, irregular disturbances. We shall be on safe ground if we say that the observed polar motion is compounded of a precessional rotation in a period of something very near 428 days at an average distance of 20 ft. from the mean pole, with an irregular movement superimposed on it; this irregular movement having sometimes the effect of modifying the rate of precessional rotation and sometimes of changing its amplitude—that is to say, altering the distance between the pole of rotation and the mean pole—according as it is acting parallel to, perpendicular to, or at any intermediate angle to the direction of the precessional rotation. I shall revert to this question later, and show how it is possible by a simple graphical construction to separate out this irregular motion and construct a diagram of it which should be helpful in elucidating its cause.

While it is thus to Chandler that the credit of discovering the 428-day period should be ascribed, it is to Küstner that we owe the first real proof that there is an actual variation in the latitude of a point upon the earth.

Küstner's observations were made in the same years as Chandler's, 1884–5, and were designed to determine the constant of aberration, a class of observation identical with those which would be used to determine the latitude of the place. Upon reducing these observations the results were at first sight anomalous in that they gave an impossibly small value of the aberration constant. The anomaly was not due to any instrumental cause; it could not be due to any seasonal change in the refraction, as the morning observations of 1884 were not accordant with the morning observations of 1885, nor could it be explained by any possible error in the proper motions of the stars. Küstner was thus enabled to state positively that the latitude of the place of observation had actually changed. It must be admitted that the years 1884–5 were particularly favourable ones, and that both these astronomers were in a sense lucky in having chanced upon them. The movement of the pole happened at that time to be exceptionally rapid. I do not, however, mention this as detracting in any way from the merit of their achievements; they deserve to be remembered as simultaneous but independent discoverers of this important and interesting phenomenon, and should be honoured, Chandler especially for his courageous rejection of mathematical theory, and Küstner for the very high skill and exquisite refinement of his observational work.

The importance of Küstner's discovery was at once recognised upon the Continent, and a proposal was made to the International Geodetic Conference to establish a chain of stations for carrying on a series of simultaneous observations and thus deducing the true law of this latitude variation. The suggestion was soon carried into effect. Six stations were chosen, all at the same latitude, 39·1° N.—Carloforte, in an island close to Sardinia; Mizusawa, in Japan; Gaithersburg in Maryland, and Ukiah in California—all new stations, where special observatories had been built for the purpose; a new observatory, established by the Russian Government at Tschardjui, in Russian Asia; and the existing observatory at Cincinnati. The reason for selecting stations at the same latitude was that identical sets of stars could be observed at each place, and thus any errors due to defective knowledge of star places are similar for all. These began work in 1899. Later, two stations in the southern hemisphere, at latitude 31·5° S.—Bayswater in Western Australia, and d'Oncatwo in the Argentine—were added.

The results were reduced and discussed by Prof. Albrecht at the Geodetic Institute, Potsdam, and published with a diagram showing the actual polar movement as deduced from the mean of the observations at all the stations, from time to time.

It was not long before these observations yielded a result of the highest interest. The observatory which devoted itself most whole-heartedly to the work, and at which the observations were most extensive and most precise, is that in Japan. This was under the able direction of Prof. Kimura. By a searching discussion of the whole series of observations he showed that they became far more consistent if a new term were introduced into the expression for the latitude variation, this term having an annual period, but being independent of longitude and having the same value for all the stations at the same date.

It will be readily seen that this term differs completely from those we have been considering hitherto. It is not a shift of the earth's axis or a movement of the pole of rotation; as it affects all places along a parallel of latitude equally the pole evidently does not move, but something which has an effect exactly the same as if the centre of gravity of the earth were shifted a few feet up and down, northward and southward, from its mean position.

The great difficulty in elucidating the Kimura term lies in its extremely small magnitude and in the consideration that there are so many possible sources of error affecting observations of this class which might have annual periodicities that their separation and evaluation are extraordinarily complicated questions. This is not the place to attempt any complete discussion, but a mention of some of the lines along which a solution has been sought may detain us for a few minutes.

The magnitude of the term at the latitude of 39° is about 6/100ths of a second of arc, or 6 ft. on the earth's surface. It has the same value and phase for every station on the same parallel and is zero on about March 9 and September 12, and maximum and minimum on June 10 and December 10, *i.e.* about ten days before the equinoxes and solstices respectively. It cannot be accounted for as a real shift of the earth's centre of gravity. It is true that in the alternate melting and accumulation of ice and snow at the two poles we have a periodic factor at work which does do this, but the amount is far too small. It was pointed out long ago by Van de Sande Bakhuysen that to fit in with the observed value of this term the apparent path of the centre of gravity must have an amplitude of 3 metres, which, if translated into terms of polar ice, would mean that a cap of ice one kilometre thick and 244 square degrees in area would have to form and disappear each year. This is obviously quite impossible. There are certain possible errors in the accepted values of the proper motions and parallaxes of the fixed stars which might produce an apparent variation in the observed latitude of this nature. As all parallaxes are based upon differential measures we cannot with certainty say that such errors are impossible; we can only say that they appear to us very unlikely, and that, if they were actually proved to exist, our ideas of the stellar universe would be profoundly modified.

If there were a yearly term in the refraction which had the effect of a periodic change in the apparent zenith we should get a corresponding periodicity in the observations. If, for example, there were a solar atmosphere, even of a quite tenuous nature, which extended into space beyond the earth's orbit, we should get a seasonal change due to the varying angular distance of the sun from the zenith of the place of observation. An atmosphere which could bend rays of light to the requisite amount, though undoubtedly extremely rare, would, however, be dense enough to offer an amount of resistance to a planet, or *a fortiori* to a comet, inconsistent with observed facts. It is, however, quite possible that the changing declination of the sun may curve or tilt the mean isobaric surfaces in the upper atmosphere in such a way that the apparent zenith moves north and south about its mean value, and that it is to this cause we owe the greater part, if not the whole, of the Kimura term. Such a displacement of the isobars is highly probable, and the phase times of the latitude variation—nil at equinoxes, maximum northward at summer solstice, and maximum southward at winter solstice—fits in perfectly with this explanation. The observations made in the southern hemisphere should form a crucial test. If this is the true cause the apparent latitude of a southern observatory will be shifted in the same direction as that of its northern counterpart, *i.e.* northward in June and southward in December. We have only a short series of observations from southern stations, but so far as they go they appear to conform. There is thus fairly strong evidence in favour of this explanation.

It must not, however, be assumed that the matter is settled beyond dispute. More observations are necessary, and especially observations at widely different latitudes. The international-stations are, as to the northern ones, almost exactly on a parallel, and, as to the southern ones, on a parallel differing only by $7\frac{1}{2}^{\circ}$ from the northern. This uniformity, highly advantageous for securing a precise record of the motion of the earth's pole, is disadvantageous for solving the riddle of the Kimura variation, and other places should join in the attack. Unfortunately the observations are very laborious and require the almost exclusive attention of an observer. There is, therefore, a very real want of an instrument which shall demand something short of the whole time of a skilled astronomer. With this object, and also with the intention of eliminating certain sources of error, instruments of new form have been devised. A short account of these will be of interest.

I shall not here attempt any description of the methods of observation used. It will be sufficient to say that, as what we want to find is the direction of the zenith at the place, all methods ultimately depend either upon a level, giving us the horizontal plane, or upon a plumb-line, giving us the vertical, and that of these two the level is the one that has almost exclusively been employed by the astronomer. The level is an instrument capable of a high degree of precision, but it has the disadvantage of being very susceptible to temperature changes, and, as both the glass tube of the level and the spirit with which it is filled are bad conductors of heat, it is impossible to ensure that it is at an even temperature throughout. Irregularities are thus produced which the reading of both ends of the bubble only partially eliminates. The mere fact of an observer standing near a sensitive level to read it may seriously vitiate its accuracy.

Some of these errors may be avoided, and such errors as are due to faulty reading of the level graduations by the observer entirely eliminated, by making the level an integral part of the instrument by floating the whole in liquid. The first application of this principle to an astronomical instrument was by Chandler, who carried out his series of latitude observations, already mentioned, with an almucantar, being a transit telescope floated in a trough of mercury. The name "almucantar" means a small circle of the heavens parallel to the horizon, and it will be sufficiently obvious that if the telescope can be set at any angle with the float, then as the instrument is rotated in the trough, or the whole trough itself is turned, the line of sight of the telescope will move round such a circle. With this instrument the stars are observed, not as in a transit circle crossing a vertical line, but crossing a horizontal circle of constant altitude. For convenience of calculation this horizontal circle would generally be selected as that through the celestial pole at the place. Chandler's instrument was purposely designed so as to differ as little as possible from the ordinary visual type, and must have been a most difficult instrument to use. The fact that he got such excellent results from it is no small tribute to his manipulative skill. The use of this form of instrument cannot be said to have found great favour among astronomers; there is only one example of it in this country, and, so far as I know, none on the Continent. The one we have is at the Durham University Observatory, and was designed by the present Astronomer Royal for Scotland, in co-operation with the late Dr. Common. It marked a very decided advance upon the earlier type. In two points specially, the screen of the floating part from wind disturbance, and the attachment of the eyepiece to the fixed part, the designers had the idea of a movable instrument, which a slight touch or a puff of wind would set vibrating to such an extent that no observation would be possible for a minute or two, clearly before them. The almucantar method of observation, meaning by this, not the use of a floating type of instrument, but the observing of stars crossing a horizontal circle, though appropriate for the particular class of observation we are here concerned with, those for determination of latitude, is not absolutely the best that can be used. To reduce every possible source of error to a minimum, particularly those due to refraction of the atmosphere, we want to observe stars as near the zenith as possible.

The floating principle has been applied with great success to a zenith instrument in the Cookson floating zenith telescope now at Greenwich, designed by the late Bryan Cookson, whose early death was a great loss to astronomy.

It is a photographic instrument, with a telescope or camera tube attached to a circular float which floats in a ring-shaped trough of mercury. The angle between telescope and float can be altered so that it can be clamped to point either vertically upwards or at any angle, up to about 30°, from the vertical. It is used in the well-known Talcott method. A pair of stars is selected which cross the meridian within a few minutes of each other at nearly the same zenith distance, one north and one south of the zenith. The instrument is set so as to include the first star in the field, the lens is opened, and as the image of the star moves across the plate it traces a fine line or trail. After the star has crossed the meridian, the telescope is turned through 180°, leaving tube and float clamped in the same relative position, and the second star traces out its trail. The distance between the two trails on the plate, which is small if the difference of their zenith distances is small, when the appropriate corrections are applied, gives the observed difference of zenith distance of the two stars, and, therefore, the observed position of the zenith, and hence the latitude of the observer. By repeating the observation with a number of pairs of stars a very precise determination of the latitude is made.

Recently a zenith telescope, designed, not on the floating, but on the hanging principle, finding the vertical line by virtue of its free suspension in a gimbal ring, has been constructed, and would have been at work by now had it not been for the interruption caused by the war. Though it has thus not yet been tested by practical experience, a few words on it may not be out of place. The method of observation will be the same as I have just described, except that there is no arrangement for clamping the instrument at an inclination to the vertical; it is intended to be used only in the vertical position, and the angle covered by the photographic plate will be a few degrees from the zenith on each side. Exactly how far we can go from the zenith depends upon the qualities of the lens, and no confident statement can be made until this has been tested, but it is hoped that star trails perfectly sharp for measurement will be secured up to an angular distance of 3° from the centre. This gives us as available for our purpose the stars over a belt 6° wide down to the sixth, and possibly the seventh, magnitude. The actual work of observing will be very simple, and will only mean that the whole instrument is rotated through 180° at certain prearranged times, and that the lens is opened after twilight and covered before the dawn. It would be possible for this to be done by mechanism controlled by a clock.

As the telescope hangs freely always in a vertical position, we entirely get rid of one of the astronomer's anxieties, the risk of error due to flexure or bending of his telescope, for though the tube can be made apparently very rigid, the excessively minute degree of bending sufficient to introduce appreciable errors is difficult, if not impossible, to avoid in a telescope which has to be used in different positions. Then, again, the errors due to changes of temperature inside or close to the instrument should almost disappear in this form. First, no temperature changes affect the suspension; so long as the body of the telescope remains undistorted the position of the true vertical in regard to the optical axis remains constant. Secondly, as the whole hanging part of the instrument is perfectly symmetrical about the vertical axis, with the trifling exception that the plate-carrier and photographic plate are not circular, but rectangular, no temperature change should distort the axis. Any distortion that can take place will, in fact, be the very small change of scale that will result from the difference in the expansion of the glass plate and the brass tube. Thirdly, it is possible, and in this instrument has been done, to enclose the whole in an outer case which can be made air-tight and kept at a constant temperature by a thermostat. In order to close the instrument in front it is necessary to have a plane parallel glass of slightly larger aperture than the lens. As this glass has to be worked with the same refinement as a lens, and as a plane surface is more troublesome to work than a curved one, this is rather a costly addition. Whether, as a matter of fact, it is worth while keeping the instrument at the same temperature, or whether it will be better to reduce the temperature change to a minimum by covering the whole with non-conducting material, and then apply the very small corrections necessary to the measurements made on the plate, is a question for experience to decide.

As a heavy hanging mass would be liable to long-continued vibrations when disturbed, a four-armed vane attached to a rod at the base is immersed in a dash-pot or bath of glycerine. This rod must be centred in prolongation of the vertical axis, otherwise the capillarity between rod and liquid will introduce a force deflecting the telescope from the true vertical. While it would thus appear that in this form of instrument most of the familiar sources of error are minimised, it is interesting to note the introduction of one possible cause of error, quite unfamiliar to astronomers, namely, the deflection that might be due to the attraction of the earth's horizontal magnetic force upon the hanging part. If the telescope-tube were, as is customary, made of iron or steel, this would reach a serious magnitude, and even if a proportion only of the suspended weight were of iron a perceptible deviation might result. It would, in fact, not be safe to allow this proportion to exceed one-tenth of the whole weight, and it therefore seemed better to exclude the use of iron or steel altogether. There is accordingly none, with the exception of the four thin flat pendulum springs which form the gimbal suspension.

In detaining you with these short descriptions of recently devised instruments, I may appear to have been wandering rather far from my subject, the wanderings of the earth's pole. You will, however, appreciate that in reality they follow very closely from it, being instruments designed with the special object of solving the particular problem we are discussing.

We will now revert to the diagram of the observed polar motion, and I will indicate how it is possible to analyse this so as to separate the irregular movements from the more orderly fourteen-month preces-

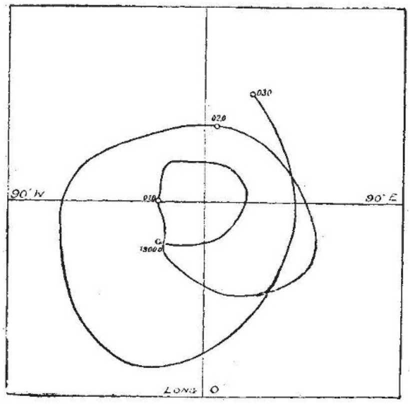

FIG. 1.—Track of polar movement, 1900–3.

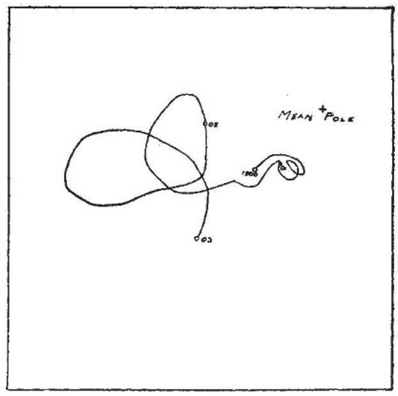

FIG. 2.—Same track referred to axis rotating in the earth with a fourteen-month period.

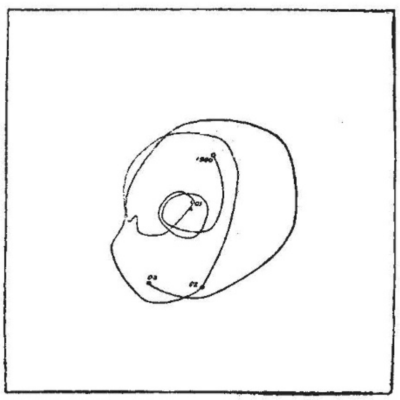
FIG. 3.—Hodograph of Fig. 2.

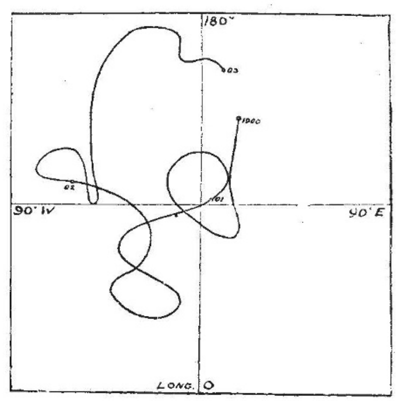

FIG. 4.—Hodograph referred back to axes fixed in the earth, or torque diagram.

sional rotation. We are justified in assuming that this free precessional period is constant in duration and therefore determines the average rate of rotation of the pole of revolution. If, therefore, we take a diagram of the polar movement, which will naturally have its axes of reference fixed in relation to the earth, and convert it into another diagram, showing the same movement, referred to axes rotating in the earth at the average rate of the precessional rotation, we obtain a graph of the irregular part of the polar path. If this irregular part has any well-marked annual period, such period ought to be apparent on inspection of the converted diagram. In the actual diagrams obtained there seems little or no evidence of the existence of a yearly term.

We now take the second diagram, and by the well-known process construct its hodograph, the curve which gives us a measure of the amount and direction of the force which could have caused the movement recorded in diagram No. 2. This will still be referred to the moving axes, so is not directly available for deducing the true direction of these forces in the earth. Before we can do this we must refer the diagram back again to axes fixed in the earth. Thus, finally, we obtain our diagram No. 4, which may be called the torque diagram, as it represents in direction and relative magnitude the torque or twisting force which has been acting upon the earth to produce the observed movement of the pole.

The interpretation of such a diagram is a somewhat complex matter, and has not yet advanced far. The causes that seem to be at work producing the irregular shift are either movements of the earth's crust, slow or rapid, as in an earthquake; the transfer of Arctic ice from one point to another, or its accumulation and disappearance so far as this takes place unsymmetrically with respect to the earth's axis; and possibly extensive barometric changes extending over considerable areas.

Of these the transfer of ice is the largest factor and is probably the one to which most of the irregular polar movement may be ascribed. An earthquake, even of gigantic dimensions, would have an almost negligible effect. The late Prof. Milne estimated that a very large earthquake might displace ten million cubic miles of earth through a distance of 10 ft. horizontally or vertically. Such a vast cataclysm would only change the position of the pole by a few inches.

In conclusion it will be an act of natural curiosity to inquire whether there is any evidence of the amplitude of these polar wanderings having been greater in past times than at present, and whether there is any likelihood of their being greater in the future. To both these questions the answer is "No." The axis of rotation is always kept near the axis of figure by internal friction, and it would require a large change in the distribution of mass to move the axis of figure very far.

As regards the future, the probabilities point still more strongly in the same direction. Each shrinkage of the earth, whatever its immediate effect on the position of the axis of rotation may be, tends ultimately to bring it nearer to the axis of figure or principal axis of inertia, and therefore tends to reduce the average amplitude of the polar path. The distance of the pole of rotation from the mean pole will therefore gradually decrease as the world grows older, while at the same time, as the earth cools and becomes less elastic and more rigid, the rate of rotation will quicken.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

THE Marquess of Crewe has been appointed President of the Board of Education, in succession to Mr. Arthur Henderson, resigned.

THE honorary degree of doctor of laws has been conferred upon Dr. Otto Klotz, of the Dominion Astronomical Observatory, Ottawa, by the University of Pittsburgh.

AN explanatory circular respecting the programme for technical schools and classes for the session 1916–17 has been issued by the Department of Agriculture and Technical Instruction for Ireland. The regulations which were in operation during the session 1915–16 will continue in force with some few alterations, among which we note that a school will not be recognised as a technical school under the conditions of section ii. (*a*) of the programme unless there are at least twenty approved introductory and specialised course students in attendance in any session, of whom not less than 50 per cent. are specialised course students. Teachers recognised for grants under the conditions of the third paragraph of the explanatory circular will not be recognised for this purpose as specialised course students. The case of schools of a special character will receive special consideration, and, if it is thought desirable, this regulation may be modified in the case of such schools. Grants will not be paid upon the attendance of a student at more than one lesson in the same syllabus on the same day, unless there is an interval of at least 15 min. between each lesson. Instruction in the first-year syllabus of a subject of a specialised course will not be permitted to be given concurrently by the same teacher with instruction in any other syllabus or subject.

A REPORT on Indian education, 1914–15, by Mr. Sharp, educational commissioner with the Government of India, has recently been received. The report is a very brief narrative of the main lines of Indian educational progress, and consists of twenty-seven pages (quarto) of letterpress and fifty-seven pages of tables. In addition, something like fifty interesting illustrations are given of educational buildings of different grades and classes which have been completed during the twelve months under review, and of the arrangements in such buildings. When it is considered that all forms of education are dealt with, from university standards down to primary schools, with an area about fifteen times as large as the United Kingdom, with a number of pupils of between seven and eight millions, and at a cost of eleven crores of rupees (that is, more than 7,200,000*l.*), it will be understood that a volume of the size mentioned represents almost the utmost limits of condensation. The effects of the war in Europe have been very distinctly felt in India, in the first place, in the desirability for economy, though even here it was found that the expenditure for the year under review was about 90 lakhs (nearly 600,000*l.*) higher than in the year previous to the war. The increase appears to have been mainly due to the rapidly increasing number of pupils in the schools, etc.; for in the five years up to 1914 the numbers had increased by no fewer than one and a third million pupils. The war, however, has affected the higher educational institutions more than the lower, for a good many of the British professors in colleges, etc., are now on military service. It is noticeable that pamphlets, such as "Why Britain is at War" and others, have been widely distributed in several of the Indian vernaculars to pupils. Also other means, such as lectures, etc., have been taken