VARIATIONS OF LATITUDE.[1]

"ALL astronomy," says Laplace, "depends upon the invariability of the earth's axis of rotation upon the terrestrial spheroid and upon the uniformity of this rotation." He adds that "since the epoch when the application of the telescope to astronomical instruments gave the means of observing terrestrial latitudes with precision, no variations in such latitudes have been found which could not be attributed to errors of observation, which proves that since this epoch the axis of rotation has remained very near the same point on the terrestrial surface." ("Mécanique Céleste," tome v. page 22.) Admitting then the position of the earth's axis, and consequently the values of terrestrial latitudes, to be sufficiently invariable for the purposes of the astronomer, the question has been many times raised whether this conclusion represents more than a kind of first approximation to the truth.

As this subject, or something very much like it, was receiving more or less attention on the part of the ancient geographers two thousand years ago or more, we can hardly claim for it the charm of novelty. An important feature of the geography of Eratos Thenis, written between 200 and 300 B.C., was a critical review of the work of his predecessors. His map of the world, which represented the best and latest information of his day, had as a sort of base line, or axis of reference, a parallel of latitude drawn from the pillars of Hercules towards the east, passing north of the island of Sicily, across the southern part of the Peloponnesus, and eastward across the continent of Asia. The positions of many places with reference to this line differed very considerably from those assigned by his predecessors. At the time of Ptolemy—400 years later—it was known that the map of Eratos Thenis failed in many particulars to conform to the then existing order of things. The conclusion was obvious; evidently changes had taken place in the relative positions of a number of prominent places on the earth; nor were these changes simply the trifling fractions of a second with which men are struggling so valiantly in these degenerate days, but such satisfactory and tangible quantities as three, four, or five degrees. Ptolemy's geography furnished the basis for comparisons and discussions of this kind for fifteen hundred years. Some few of his latitudes, as that of Alexandria, were determined with such precision as was possible in those days, while the foundation of very many was little more than guess-work. Comparisons from time to time with later determinations brought to light discrepancies which served to keep the question open and to furnish material for speculation.

In this connection we shall stop only to mention Dominique Maria de Ferrare, who enjoys the distinction of having had as a disciple the illustrious Copernicus. This philosopher believed that the evidence showed conclusively a progressive change in the position of the pole, and that in time the torrid and frigid regions would in a manner change places.

So far as the latitudes of Ptolemy were concerned it was pointed out[2] that the discrepancies were in part due to the method employed in their determination—that of the gnomon which gave the altitude of the sun's upper limb, and consequently a value of the latitude too small by a quarter of a degree.

Two or three hundred years ago much interest was taken in this question. We find associated with it the familiar names of Tycho, Rœmer, Hevelius, Picard, Cassini, and many others. As greater accuracy in methods and instruments prevailed, it became evident that the rough positions of Ptolemy could not be employed with any confidence in discussions of this character. In connection with these more exact methods also a new phenomenon began to manifest itself, viz., changes of short period.

Christopher Rothman, a contemporary of Tycho, found systematic differences between the determinations of the latitude of his observatory made in summer and winter. Tycho's observations at Prague showed a like peculiarity. Rœmer also discovered it. He attributed it confidently to periodic changes in the position of the earth's axis, and hoped in time to give a complete theory of the same.

A memoir by J. D. Cassini,[3] published in 1693—200 years ago almost precisely—gives a very complete summary of the state of the problem at that day. After a detailed examination of the evidence he concludes:—"Notwithstanding all these apparent variations, we may say that not only has no extraordinary change in the altitude of the pole or in the meridian altitude of the sun occurred in recent times, but that the heavens have at all times occupied the same position with regard to the earth as during the past century. For there is reason to believe that all these variations which have been mentioned came from several defects which occur in observation." He then goes over in detail those sources of error which are so familiar to us—instrumental errors and defects in theory—one only having a somewhat unfamiliar appearance, viz., we may reasonably suppose that variations in the direction of the plumb line occur similar to those of the magnetic needle. Nevertheless he says it is very probable that from time to time small changes in the altitude of the pole actually do occur, but they are periodic in character and do not exceed two minutes in amount. Thus, instead of several degrees which were conceded by the astronomers of previous centuries, only a paltry two minutes was now allowed, but with improved instruments, with the discovery of aberration and nutation, and the perfection of the theory of refraction, even this modest allowance was gradually reduced to a vanishing quantity.

Meanwhile new arguments were found for a reconsideration of the question. Geology had taken its place among the sciences. In the investigation of the fossil remains of plant and animal life abundant evidence was found of a former temperate or subtropical climate within the Arctic circle. It was also evident that at one time considerable portions of Europe and North America had been covered with glacial ice. Laplace mentions the argument for a change in the position of the earth's axis, founded on the existence of the fossil remains of elephants in Northern Siberia, but believes that the discovery of the remains of one of these animals preserved in ice, the body of which was covered with thick hair, turns the argument against those who employ it (M.C. v. p. 20).

In the *Quarterly Journal of the Geological Society* for 1848 is found a communication from a mathematician and astronomer, Sir John Lubbock, on changes in climate resulting from changes in the earth's axis of rotation. He suggests a mathematical discussion of the problem in order to determine, as he says, "under what hypothesis a change of the position of the axis of rotation is possible or not." The President of the Association, Sir Henry T. de la Beche, in the annual address of 1849, deals at some length with this subject. Again, in 1876, we find Sir John Evans, then president of the Society, discussing the problem (*Quarterly Journal of the Geological Society*, 1876, p. 60). He describes with much detail the fossil remains found in Spitzenbergen and Greenland belonging to the Miocene, upper and lower Cretaceous, Jurassic, and other geological periods, all of which point to a former temperature much above the present. Thus, among the Miocene plants of Spitzenbergen Prof. Nordenshiäld mentions the swamp cypress, now found in Texas, siquoias of great size, limes, oaks, and even magnolias. So in the Lower Cretaceous period Prof. O. Heer distinguished seventy-five species, including ferns, Cycadeæ and Coniferæ, many of which are closely allied to species now found in sub-tropical regions. From these remains Prof. Heer infers that the climate of Greenland and Spitzenbergen during the Cretaceous period was very much the same as that which now prevails in Egypt and the Canary Isles. The existence of beds of coal, of mountain limestone formed of the remains of corals and bryozoa, and shells of marine molluscs, the remains of Ammonites, Nautili, and even a Saurian—the *Ichthyosaurus polaris*—all point in the same direction. While, as Prof. Houghton remarks, the arguments from the presence of Ammonites and Coalplants strengthen each other, the one demanding heat, the other light.

Sir John Evans sums up the arguments as follows:—"The three points which it appears to me are most important to bear in mind with regard to the article of flora are (1) that for vegetation such as has been described there must, according to all analogy, have been a greater aggregate amount of summer heat supplied than is now due to such high latitudes. (2) That there must have been a far less degree of winter cold than is in any way compatible with the position on the globe; and (3) that in all probability the amount and distribution of light which at present prevail within the Arctic circle are not such as would suffice for the life of the trees."

He afterwards supposes a hypothetical case of possible

[1] Address before Section A (Astronomy) of the American Association for the Advancement of Science, at Madison, Wisconsin, by Prof. C. L. Doolittle, of South Bethlehem, Pa., President of the Section.

[2] Delambre, "Histoire de l'Astronomie au Dix-huitième Siècle," p. 155.

[3] S'il est arrivé du changement lans Lautein du pole au dans la Cones du Soleil? (*Memoires de l'Academie*, tome x. p. 360.)

elevation and depression, to which he invites the attention of mathematicians to determine whether it would not produce a change of 15″ or 20° in the position of the pole.

The invitation was duly accepted by Sir Wm. Thompson—now Lord Kelvin—and by Prof. G. H. Darwin. The former, by a process which he does not explain, convinced himself that a *vera causa* existed in the distortion of the earth, as shown by geological and other evidence, sufficient to produce large deviations in the position of the axis. To quote his own eloquent words, "Consider the great facts of the Himalayas and Andes, and Africa, and the depths of the Atlantic, and America, and the depths of the Pacific and Australia; and consider further the ellipticity of the equatorial section of the sea-level, estimated by Capt. Clarke at about one-tenth of the mean ellipticity of meridianal sections of the sea-level. We need no brush from the camel's tail to account for a change in the earth's axis; we need no violent convulsions, producing a sudden distortion on a great scale, with change of axis of maximum moment of inertia, followed by gigantic deluges; and we may not merely admit, but assert as highly probable, that the axis of maximum inertia and the axis of rotation, always very near one another, may have been in ancient times very far from the present geographical position, and may have gradually shifted through 10, 20, 30, or 40 or more degrees, without at any time any perceptible sudden disturbance of either land or water." (British Association Reports, 1876, Sections, p. 11).

Prof. G. H. Darwin has made this the subject of an elaborate mathematical investigation (*Phil. Trans.* 1877, p. 271). As the basis he takes the earth as we find it, assuming that the elevations of the continents and depressions of the ocean represent the kind and amount of distortion to which the earth has been subjected in the course of its past history. The mean elevation of the continents being about 1100 feet, and the mean depth of the oceans about 15,000 feet, it follows that in order to convert an ocean bed into a continent, or *vice versa*, an elevation or subsidence of 16,000 feet must have taken place. This would not, however, correctly represent the distortion of the earth, for the waters of the ocean flowing into the depressions would considerably modify the result. Taking into account the density of water as compared with the surface rocks, it appears that an extreme elevation of 16,000 feet from the bottom of the ocean to the surface of the supposed continent would be equivalent to an effective elevation of about 10,000 feet on a sealess globe. In case of a perfectly rigid globe, the only deformation which could take place would be that due to a redistribution of the surface materials. For a given elevation with a corresponding depression the maximum effect upon the position of the earth's axis would be produced when the elevations occurred in latitude 45° in two diametrically opposite quarters of the earth with corresponding depressions in the remaining quarters. In such a globe Prof. Darwin's analysis showed that the pole could never have wandered more than 3° from its original position as a consequence of the continents and oceans changing places. If, however, the earth is sufficiently plastic to admit of readjustment to new forms of equilibrium by earthquakes or otherwise, possible changes of 10° or 15° may have occurred.

This would, however, require such a complete changing about of the continents and oceans, with maximum elevations and depressions in precisely the most favourable places, as has certainly never occurred within geologic time. In fact, the evidence indicates that the continental areas have always occupied about the same position as now.

It would appear, therefore, that the geologist must give up this hypothesis of great changes in latitudes as a factor in the earth's development, unless, indeed, some other cause can be found of sufficient potency to produce the desired result. Such an agency is, perhaps, alluded to by Prof. Arthur Schuster in his address before Section A of the British Association a year ago (Nature, 1892, Aug. 4, p. 327). He propounds this question: "Is there sufficient matter in interplanetary space to make it a conductor of electricity?" He adds that he believes the evidence to be in favour of this view; but the conductivity can only be small, for otherwise the earth would gradually set itself to revolve about its magnetic poles. If such an action were admitted, we must suppose the poles of revolution and magnetic poles would long since have been brought into practical coincidence, unless this consummation were frustrated by changes in the position of the latter.

However all this may be, the question before the practical astronomer is this—Have we any reliable evidence showing that progressive changes in the position of the pole are now taking place? If this question were submitted to a jury composed of twelve good men and true from the astronomical profession, the chances would be largely in favour of a verdict in agreement with Laplace's decision seventy years ago.

At the International Geodetic Conference held in Rome ten years ago, Mr. Fergola brought forward a plan looking to a systematic study of this and other questions connected with changes of terrestrial latitudes. This plan, which was favourably received, consisted in a scheme for simultaneous series of observations at pairs of observatories on nearly the same parallel of latitude, but differing widely in longitude. The instruments were to be prime vertical transits, and the same stars to be employed at each of the two stations. Several pairs of observatories were designated by Fergola as being favourably situated for the purpose. Among others, Washington and Lisbon, the difference of latitude being 11′ 7″, that of longitude 4h. 31m. It is understood that efforts in this direction were made at Washington, but the necessary cooperation at the other end of the line was not secured, and the plan came to naught. It has not come to my knowledge that the scheme was at that time seriously considered at any of the other points selected.

Fergola gave a tabular statement which at that time seemed to show small but progressive diminutions of latitudes in Europe and North America. This table, with some additions—the latter enclosed in brackets—is as follows:—

		°	′	″
Washington ...	1845	38	53	39·25
	1863			38·78
	[1883			38·94]
Paris	1825	48	50	13·0
	1853			11·2
	[1891			10·95]
Milan	1811	45	27	60·7
	1871			59·19
Rome	1810	41	53	54·26
	1866			54·09
Naples	1820	40	51	46·63
	1871			45·41
Königsberg ...	1820	54	42	50·71
	1843			50·56
Greenwich ...	1838	51	28	38·43
	1845			38·17
	1856			37·92

In all these cases there is an apparent diminution during the present century. A similar tendency is shown by the observations of Peters, Gyldèn, and Nyrèn at Pulkowa, also by my own observations at Bethlehem since 1875. Instances are not wanting, however, where this diminution fails to manifest itself. Possibly most of the discrepancies shown here may be referred to periodic changes, the existence of which is no longer in doubt. It is by no means impossible or improbable that small local changes of latitude may occur due to slipping of the superficial strata of the earth's crust. That such lateral movements have taken place in times past in connection with mountain upheavals is, without doubt, true. That they are still going on in certain localities is probable; whether they are of sufficient magnitude to admit of measurement can only be determined by observation.

When we remember how few points there are on the surface of the earth, whose latitude was determined even no longer ago than fifty years, within one or two seconds of the truth, probably we should suspend judgment for the present with reference to the whole subject of progressive changes.

We come now to a phase of our subject with reference to which we can speak with some confidence, viz. periodic changes.

That in the case of a perfectly rigid earth, theory points to the existence of such a periodic change, completing its cycle in about ten months, has been long understood. In connection wlth the general problem of the motion of a free body under the action of any system of forces, the consideration of which was suggested by the problems of the solar system, we find the names of the leading mathematicians of the last century, d'Alembert, Segner, and Euler, not to mention others. It was the latter who, in 1765, in a work entitled "Theory of the Motion of Solid and Rigid Bodies," gave the equations the final form which Laplace declares seem to him the most simple which can possibly be obtained. (M. C.V. p. 284.)

The elegant form of these equations was due to the employment of the principle discovered by Segner, viz. that at every point of a body there are at least three principal axes of inertia at right angles to each other, which possess some very important properties. One of these properties is this—that if the body be set revolving about one of these axes which passes through its centre of inertia, and is understood by outside forces, it will continue to revolve about this axis for ever. If, however, it be started in its revolution about some other axis, the condition of things will be different.

In the first approximation to the solution of Euler's equations when applied to the earth, we meet with two constants of integration, whose values depend upon the position of the axis of revolution with respect to the principal axis of inertia (from which it can never differ greatly) at the instant which we take as the starting point of our integration. We further find that the presence of these quantities in our equations shows a revolution of the instantaneous axis of rotation about the principal axis of inertia. This rotation is in the same direction as the diurnal motion, the angular velocity y being expressed by the formula

$$y = \frac{C - A}{A} u$$

Where u is the velocity of diurnal rotation, C and A are the principal moments of inertia of the earth, the first with respect to the polar axis, the second with respect to an equatorial axis, the figure being regarded as that of an ellipsoid of revolution. The ratio

$$\frac{C - A}{A}$$

is found from the value of the constant of nutation, the degree of accuracy being such that the theoretical period of this rotation is known probably within one or two days. The value given by Oppolzer is 304·8 mean solar days. We shall assume it to be 305 days.

The angular distance between the two axes, evidently very small in case of the earth, can only be determined by observation, and will manifest its existence by fluctuations in the latitude having a period of 305 days. The first attempt to find by observation whether or not this movement was appreciable was by Bessel. This method was not well adapted to the purpose, and the result was negative or inconclusive.

The first quantitative determination which seemed worthy of confidence was made by Dr. C. A. F. Peters, of Pulkowa ("Recherches sur la Parallax des Etoiles Fixes," p. 146), in 1842. From a careful series of meridian circle observations carried on for thirteen months he found for the angle between the two axes ·071″ ± 017. Nyrèn followed with a careful discussion of the value given by the observations of Peters, Gyldèn, and himself with the same instrument. The results were ·101″, ·125″, and ·058″. Downing found from the Greenwich observations from 1868–77 ·075″ (*Monthly Notices, R.A.S.* March, 1892), while Newcomb found the somewhat smaller value ·04″ from the Washington prime vertical work.

These results are in reasonably good accord, and at first sight seem to show conclusively a real separation of the two axes, but as pointed out by Hall ("American Journal of Science," March, 1885, p. 223), the form of the expressions for determining the quantity is such that an apparently real value will always be obtained. If we assume a uniform rotation of one pole about the other our equations will contain two unknown quantities, x and y, where $x = \rho \cos \xi$. $y = \rho \sin \xi$, therefore whatever values we may find for x and y . ρ will always have a real and positive value. This may, therefore, be nothing more than a function of the errors of observation. The true test was therefore to be sought in the agreement of the values of ξ when reduced to a common epoch. These were found to be quite discordant, so much so as to throw doubt upon the reality of the results. The truth, as we now understand it, being that Euler's theory, perfect as it is, does not apply without modification to the present problem—the earth not being strictly a rigid body. Doubts as to the absolute rigidity of the earth had been expressed by more than one investigator, and the matter was discussed in 1876 by Lord Kelvin (British Association Reports, 1876, Sections, p. 11), and in 1879 by Prof. George Darwin (*Phil. Trans.* 1879), in relation to the problems of precession, nutation and tidal action—the conclusion being that the rigidity of the earth is probably between that of steel and glass. The bearing of this upon the present investigation was first pointed out by Newcomb (*Monthly Notices*) *Royal Astronomical Soc.*, March, 1892), viz. that in consequence of the elastic yielding of the earth as a whole the period of this rotation would be lengthened.

Before considering this matter in detail, however, the exigencies of historical continuity require us to glance at some remarkable results of observation.

In the spring of 1884 Dr. F. Küstner, of Berlin, began a series of observations, the results of which were destined to awaken a widespread interest in this subject, or, perhaps more properly, to crystallise the interest which already existed. His original purpose was sufficiently modest. The great meridian circle of the observatory requiring some repairs, he proposed to employ the interval while it was out of service in making a limited series of observations with another instrument, the universal transit, according to the Horrebow-Talcott method for the investigation of the constant of aberration. His purpose was not so much that of deriving a new and definitive value of this constant, which should be entitled to rank with the excellent results previously obtained, as to test practically the applicability of the method to this purpose, and to acquire the experience which at a future time might lead to a favourable result in a more complete series. Possibly it would be overstraining a time-worn simile to compare the modest investigator with Saul, son of Kish, who, going forth to seek his father's asses, found a kingdom; but certain it is that his results were vastly more important and far-reaching than anything which he could have anticipated in his original programme. His observations, not numerous, but of the first order of excellence, led to a value of the constant of aberration which appeared to be wholly inadmissible. Many an investigator would have been discouraged with this apparent failure, and the world would have known nothing of it. Not so with Küstner. Instead of abandoning the experiment as a failure he set himself resolutely to work to discover the cause of the anomaly. After examining the various causes which might be supposed to have contributed to such a result, personal, instrumental, and refractional, he announced without hesitation that it was due to a change in the latitude itself, viz., that from August to November, 1884, the latitude of Berlin had been from 0.2″ to 0.3″ greater than from March to May in 1884 and 1885. This conclusion was materially strengthened by the examination of a considerable amount of collateral evidence, particularly Nyrèn's elaborate series of observations at Pulkowa from 1879 to 1882, employed by the latter in discussing the constant of aberration. This somewhat bold hypothesis naturally provoked much discussion, and many were sceptical as to its truth; but instead of resorting to polemics, and quoting the authority of Aristotle and the sacred Scriptures on the one side or on the other, means were promptly found for testing it. These comprised both a re-examination of old observations and new ones, undertaken for this express purpose. Among the latter were special series of latitude determinations extending over an entire year or more at Berlin, Potsdam, Prague, and Bethlehem, all by Talcott's method. All of these agreed most satisfactorily in showing the reality of the fluctuation during the years 1888, 1889 and 1890. But the final test which should determine whether the changes observed were due to movements of the earth's axis required observations to be carried on simultaneously at points differing widely in longitude. A latitude campaign instituted for this purpose was therefore entered upon in the summer of 1891, under the auspices of the International Geodetic Association, operations being carried on at Berlin, Prague, Strassburg, Rockside, San Francisco, and Waikiki.

Some of the results have been in possession of the public for several months, and they show in the most conclusive manner that we are dealing with a movement of the earth's axis.

A series of latitude observations was also carried on at Paris from December, 1890, to August, 1891; part of the time two different observers were employed using different instruments, their results agreeing almost exactly. (*Comptes Rendus*, 1892,

p. 895.) Science acknowledges no national allegiance, but it is interesting to note that this series fails to show any trace of the periodic change; considering the smallness of the quantity in question and the limited scope of the series this failure proves nothing *pro* or *con.* Yet Admiral Mauchez expressed the opinion that the fluctuations which the Germans had been attributing to changes of latitude were due to some other cause (*Comptes Rendus*, 1892, p. 862.) It is also noteworthy that the value of the latitude found at this time is 0·8″ smaller than given by the elaborate investigation of M. Galliot in 1879, in which he employed 1077 observations by ten different observers. (*Comptes Rendus*, vol. lxxxvii. p. 684.) In this discussion an annual period, having a semi-amplitude of 0·20″ manifested itself somewhat obscurely; but M. Galliot placed on record his opinion that this had its origin in some cause other than a change in the latitude.

We have seen how it came about that the reality of periodic fluctuations in the earth's axis was placed beyond dispute. As to the true nature and law of these fluctuations we should probably now be groping in darkness but for the services which Dr. S. C. Chandler has rendered in the way of solving the mystery. Before Dr. Chandler attacked the problem no one appears to have called in question the applicability of Euler's theory to the case of the earth. The impression was indeed quite general that the changes were for the most part of a fortuitous character, produced by precipitation of rain and snow, by ocean currents and aerial currents acting unequally in different hemispheres, and therefore in so far as they might manifest a periodicity, this would be annual in its character. As early as 1876 Lord Kelvin expressed the opinion that the causes were sometimes sufficient to produce change of half a second in the course of a year. (British Association Reports, 1876, Sections p. 11.) It seemed therefore beyond question that any periodic change must conform to the 305 day period of Euler, or to an annual period, or a combination of the two. The latter hypothesis was worked out very completely by Messrs. R. Radeau (*Comptes Rendus*, vol. iii. p. 568) and F. R. Helmert (*Astronomische Nachrichten*, vol. cxxvi. p. 217).

Matters were in this condition when in 1891 Chandler attacked the problem. The main features of this investigation are given in a series of seven remarkable papers published in the *Astronomical Journal*, written from time to time while the work was still in progress, and when, as a matter of course, the final result could not be known. Like Kepler, the author carries us with him along the successive stage of the investigation, we share with him his triumphs and disappointments, and rejoice with him when well-merited success crowns his efforts. As to his methods and purpose, these are given in his own words. "I 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 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. . . . The problem which I therefore proposed to myself was to see whether it would not be possible to lay the numerous ghosts in the shape of various discordant residual phenomena pertaining to determinations of aberration, parallaxes, latitudes, and the like, which had heretofore flitted elusively about the astronomy of precision during the century; or to reduce them to some tangible form by some simple consistent hypothesis. . . . It was thought if this could be done, a study of the nature of the forces as thus indicated by which the earth's rotation is influenced might lead to a physical explanation of them."

From May 29, 1884, to June 25, 1885, almost exactly the time covered by the observations of Küstner, at Berlin, Chandler was observing at Cambridge with the Almucantar. The resulting values of the latitude shared a progressive change, for which there seemed no explanation unless the change were that of the latitude itself. At that time this seemed too radical an hypothesis, so the results were printed as they appeared, leaving the explanation to the future. The close agreement of Küstner's results, the verification by the subsequent work at Berlin, Pulkowa, Potsdam, and Prague seemed to warrant the expenditure of the labour involved in a thorough investigation of the entire subject. He began with Küstner's work at Berlin, the vertical circle observations of Gyldèn and Nyrèn at Pulkowa, and the precise vertical observations of α Lyrae at Washington 1862–66. These agreed in showing a period of 427 days. The examination of observations of circumpolar stars at Melbourne, and of Polaris at Leyden, partially confirmed the result.

Next came the observations of Bradley at Kew, Wanstead, and Greenwich. Here a very puzzling phenomenon appeared, the period being only about one year, with an amplitude of nearly an entire second. In discussing the observations of Brindley at Dublin, made during the early part of the present century, an opportunity occurred to wrestle, and that successfully, with one of the ghosts before referred to, viz., the singular results which Brindley had obtained for the parallaxes of a number of stars, and which led to an interesting discussion between Pond and himself.

Thus series after series was analysed with results in the main encouraging, frequently puzzling, and sometimes disappointing. The law, if such existed, did not appear on the surface. The secret could only be discovered by an elaborate analysis of the material. Accordingly, forty-five different series, extending from 1837 to 1891, comprising more than 33,000 observations, were examined, from which an empirical law was deduced as follows.

The velocity of rotation of the pole was a maximum about 1774, the period being about 348 days. Since then the velocity has diminished at an accelerated rate, the period in 1890 being 443 days.

During the last half century the semi-amplitude has remained sensibly constant at 0·22″.

Only three of the forty-five series examined, and these among the least precise, intrinsically gave results contradictory of the general law. The next step in the process was to analyse the observations in a different manner, to discover whether the deviations from the provisional law were real, also in what manner the variations of the period were brought about. For this purpose the results were tabulated chronologically at twenty-day intervals, all reduced to the meridian of Greenwich. As a result the real nature of the phenomenon was most distinctly revealed, and was as follows.

The observed value of the latitude is the resultant curve arising from two periodic fluctuations superposed upon each other. The first of these, and in general the more considerable, has a period of about 427 days, and a semi-amplitude of about 0·12″. The second has an annual period with a range variable between ·04″ and ·20″ during the last half-century. The maximum and minimum of this annual component of the variation occur at the meridian of Greenwich about ten days before the vernal and autumnal equinoxes respectively, and it becomes zero just before the solstices.

As the resultant of these two motions, the variations of the latitude is subject to systematic alterations in a cycle of seven years' duration, resulting from the commensurability of the two terms. According as they conspire or interfere, the total range varies between two-thirds of a second at a maximum to but a few hundredths of a second at a minimum.

Accompanying the paper is a diagram showing the relation between this theory and the observations of the fifty-four years on which it is based. The agreement, at times almost perfect, at other times shows deviations, apparently systematic, which are perhaps due to imperfect knowledge of the constants, or to erratic deviations of meteorological origin.

Dr. Chandler finds the general outcome full of promise for the astronomy of precision, showing that observations are free from defects of a systematic character to a much greater extent than has heretofore been supposed.

As the results of which we have been speaking were announced from time to time they did not pass unchallenged. The reality of the 427 day period was very promptly called in question on account of its supposed conflict with dynamic laws.

Prof. Newcomb, who at first ranked as a sceptic, soon found a very plausible explanation by assuming that the earth is not a rigid body as required by Euler's theory. The question whether the earth as a whole should be regarded as a rigid body has long been more or less an open one. Certainly the waters of the ocean introduce an element of mobility, but the investigations of Lord Kelvin and Prof. Darwin of the bodily tides in a viscous spheroid when applied to the earth, gave very little, if any, evidence of yielding in case of the latter to external forces.

Laplace had discussed with negative results the effect upon the earth's motion of the mobility of the ocean. (M.C., tome v. p. 76.) Euler's equations had been modified by Liouville for the case of a body which is slowly changing its form from internal causes (*Liouville's Journal*, 2nd series, tome iii. 1858, p. 1), and these modified forms had been employed by Darwin in the discussion of the influence of geological changes in the earth's axis of rotation. (*Phil. Trans.* 1877, p. 271.)

No suspicion, however, seems to have entered the brain of any of these investigators that any modification of Euler's 305-day period would result either from the mobility of the ocean, or the elastic yielding of the earth as a whole.

Newcomb shows in a very simple manner how this result might follow (*Monthly Notices R.A.S.* March 1892, p. 336), for in consequence of this elastic yielding the pole of figure would be brought towards the pole of the instantaneous axis by the centrifugal force.

Let us call the undisturbed position of the pole of figure the fixed pole, the actual position at any instant the movable pole, and the pole of the instantaneous axis the pole of rotation. The movable pole is therefore constantly moving towards the pole of rotation, describing a sort of curve of pursuit; the instantaneous velocity of the latter about the former is that of Euler's period, but the effect of the motion of this movable pole is to diminish the velocity with respect to the fixed pole in the ratio of its distance from the latter to the distance from the pole of rotation.

Lord Kelvin remarks that this supplies a new and independent method of determining the effective rigidity of the earth. As will readily appear, in this distortion work is being done against resistance, and unless the earth be perfectly elastic, which is certainly not true of that part accessible to observation, the two axes would in time be brought into practical coincidence. The tidal action set up in the oceans would also tend to produce the same result. Apparently, then, the continued existence of this term requires a constantly recurring series of impulses.

Gylden remarks that the hypothesis of elasticity is not the only one which will explain the Chandlerian period. (Astronomische Nachrichten, Band 132, p. 193.) He also concludes as the result of a mathematical analysis that we must look for the impelling cause to concussions going on in the interior cavities of the globe.

Aside from the fact that these discussions are in need of explanation to an extent quite equal with that of the phenomenon itself, it is an open question whether any explanation is called for. We have no proof of the perpetuity of this term. We are in possession of no observations accurate enough to throw any light on this subject before the time of Bradley, nor can it be asserted that so small a coefficient has remained constant during the interval of 150 years; possibly it may be on the road to extinction.

As to the annual term, it seems to have no foundation in theory except as the result of meteorological causes, in which case we can hardly hope for more success in dealing with it than in predicting the weather on which it depends. For further improvement in our knowledge of this subject we must look to continued observation at a number of points carried on for this express purpose, and so conducted as to eliminate, if possible, all systematic errors. If, as seems probable, the coefficients—at least that of the annual term—partake of the erratic nature of meteorological phenomena, it will be necessary to keep this work up perpetually.

A plan is under discussion for establishing four permanent latitude stations on the same parallel of latitude, at intervals of 90° in longitude as nearly as may be. These will presumably be equipped with identical instruments of the most approved form, and the same stars employed at all of them. Until this plan, or some modification of it, is in working order—and probably for some time after—careful determinations at other points will continue to furnish valuable data, especially in settling the question of progressive changes, local or otherwise.

The instrument hitherto employed in special observations for this purpose is the zenith telescope. The possibility of determining latitude by measurement of the small difference of zenith distance of two stars properly situated—one culminating north, the other south of the zenith—was pointed out by Horrebow in his *Atrium Astronominæ* in 1732. (Wolf, "Geschichte der Astronomical," p. 608.) Possibly he may have made a practical application of the principle; if so, any account of it has escaped my notice. The method, however, was employed by Father Hell—otherwise not unknown to fame—in determining the latitude of his transit of Venus station at Wardoehume in 1769. He appears to have been unacquainted with Horrebow's previous suggestion, and determined his latitude in this way, as he says, from necessity.

The idea seems to have lain dormant until about 1834, when it was hit upon independently by Talcott in America, and Pond in England. The latter, in employing the zenith telescope—which had then been recently mounted at the Royal Observatory for the special purpose of observing γ Draconis—found that a fifth magnitude star passed the meridian thirty minutes later at nearly the same distance on the opposite side of the zenith.

By observing these two stars, reversing the instrument between them, he found certain advantages now well known to be inherent in the method. (*Phil. Trans.*, vol. cxxiv. p. 209.) Pond states that the same method may be employed with Altazimuths, and other portable instruments, but the communication appears to have attracted no attention, and apparently he made no attempt to develop it farther.

In striking contrast is the immediate success which attended the employment by Talcott of an instrument constructed to carry out this principle. The first practical application of it was in 1834, in the survey of the northern boundary of Ohio. (*Journal Franklin Institute*, October, 1838.) Its merits were very promptly recognised by the officers of the U.S. Coast Survey, where it received a number of modifications and improvements suggested by experience, making it practically the instrument which we have to-day. It was many years, however, before it came into use to any considerable extent on the eastern side of the Atlantic.

To America undoubtedly belongs the honour of practically introducing this important improvement in latitude determination.

But although Americans practically introduced the instrument to the world, it was reserved to the Germans to teach us how to use it. It is due in great measure to refinements and improvements devised by German observers and instrument makers that the probable error of a single determination is now ·12″ or ·15″, instead of three times these amounts, with which we were formerly satisfied. The essential features of this instrument are the micrometer and the level. Unless these are of a high degree of excellence first-class results cannot be obtained; especially is this true of the level, of which two are commonly employed with the best class of instruments. Only those who have experienced it are aware how difficult it is to procure levels of the necessary quality. Moreover, changes of form are liable to occur, rendering what was a good level worthless. The method so frequently employed by determining the value once for all, and continuing to use it for years without farther examination will not answer here.

This uncertainty of the level has led to devices for dispensing with it. One of these, which seems promising, is the floating Zenith telescope, invented by Fathers Hagan and Fargie. In this instrument the telescope, with its accessories, floats on the surface of a trough of mercury, the trail of the star as it crosses the field being recorded on a photographic plate, which may be measured at leisure. Possibly a way may be formed for making these exposures automatically, thus furnishing means for keeping a record continuous in so far as absence of daylight and of clouds will permit. With four stations established as described above, equipped with automatic instruments, data will be rapidly accumulated for settling the questions still remaining doubtful. It will not, however, be a work of merely one, two, or three, but of many years.

Is it too much to hope that within five or ten years we may see some such system as this in full and successful operation?

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

A PARLIAMENTARY paper has just been issued in which is given an abstract of returns furnished to the Department of Science and Art, showing the manner in which, and the extent to which the councils of counties and county boroughs in England and Wales, and the county councils, town councils, and police commissioners of police burghs are devoting funds to the purposes of science, art, and technical and manual instruction. The returns were made by these bodies in response to a letter sent to them in December, 1892, by the Education Department. Much of the information contained in them was noted in these columns on August 28 (p. 404). It is remarked in the present returns: "A noticeable feature with regard to the work of the