Changes in the Length of the Day.

By Prof. ERNEST W. BROWN, F.R.S.

AT the present moment the answer to the question as to whether the rate of rotation of the earth is fluctuating, depends almost wholly on the interpretation which shall be given to certain differences between the observed positions of two or three bodies in the solar system and their positions as calculated from the laws of motion and gravitation. Owing to the unavoidable errors of observation, usually greater in the past than now, a detailed discussion of the astronomical data is necessary in order to discover, if possible, the extent to which the astronomical evidence is reliable. To me it has appeared to be sufficiently good to warrant full consideration of its consequences and to suggest further search for confirmation of the results.

Although these fluctuations appear to be physically independent of the well-known progressive change due to tidal friction, the latter is included with the former in the astronomical observations, and this fact requires a knowledge of the amount of the progressive change in order to isolate the fluctuations.

The progressive change is mainly determined from a comparison of material deduced from ancient eclipses with modern observations. Dr. J. K. Fotheringham's latest determination of the frictional portion gives an amount the cumulative effect of which at the end of a century is to change the apparent longitude of the moon by $4''\cdot 5$; since it varies as the square of the time, the accumulation in a decade is less than a twentieth of a second of arc. An apparent displacement of the sun of the same character had been deduced earlier by Dr. P. H. Cowell; for this Dr. Fotheringham gets a century accumulation of $1''\cdot 5$, but states that any values between $1''\cdot 1$ and $1''\cdot 7$ will satisfy the observational material.

The hypothesis that these amounts were due to tidal friction was first placed on a numerical basis by Prof. G. I. Taylor, who showed that shallow seas must be mainly responsible, and that, in particular, the Irish Sea produced about one-fiftieth of the observed amount. Shortly after, Dr. H. Jeffreys, from such data on tides, currents, and ocean depths as were available, deduced in the same manner from all the shallow seas of the globe an amount not very far from that obtained by astronomical observation; in fact, the two values agreed well within the errors due to the uncertainty of the data.

The apparent solar effect bears, however, a relation to the apparent lunar effect, which depends mainly on the amounts of the friction which are to be attributed to the solar and lunar tides respectively. Jeffreys showed that if we accept Fotheringham's figure for the apparent lunar effect, the maximum apparent solar effect in a century, namely, $0''\cdot 9$, is obtained when we attribute all the friction to the lunar tides. This is somewhat less than the least figure, $1''\cdot 1$, admitted by Fotheringham.

While the discrepancy exists and is in need of explanation, too much stress should not be placed upon it in view of the uncertainty of much of the material used to deduce the results. The hypothesis of continental drift has some bearing on the question. Practically the whole of the ancient astronomical observations were made in a restricted area on the earth's surface. If this region had drifted relatively to the rest of the earth in the direction opposite to the tidal drift, that is, east, by a sensible fraction of the total tidal retardation, the discrepancy would disappear. In this sense the drift could scarcely be tidal; if its existence be postulated, an internal source seems to be necessary.

In extrapolating these results into the past or future, it is to be remembered that the astronomical observations cover less than 4000 years, while the frictional calculations depend only on modern data. The discussion of the latter by Jeffreys showed that two-thirds of the whole amount was due to the Bering Sea alone. This sea is bordered on the south by the Aleutian Islands, which now constitute a region of considerable seismic and volcanic activity. From what we know of changes of ocean depth in such regions, there is no security that changes in the depth of this sea may not have taken place, even within historic times, sufficient to modify sensibly the total amount of tidal friction. While a retardation of the rate of rotation by tidal friction has certainly existed throughout the geologic history of the earth, its amount has probably been subject to considerable variations, and the same may be said of the future.

The results contribute nothing to a hypothesis which demands a sliding of the crust as a whole over the nucleus if the assumed angular velocity of relative shift be constant. While a retarded or accelerated effect is not excluded, the substantial agreement of the observed and computed friction indicates that it is probably, if existent, quite small. The same reasoning can be applied to other frictional effects; for example, that of an assumed general average circulation of the atmosphere relative to the earth's surface.

We now come to the fluctuations in the earth's rate of rotation which are indicated by the astronomical evidence. There are accelerations and retardations of an order of magnitude quite different from that of the frictional effect. There have been decades in which the accumulated apparent change from uniform motion has been several seconds of arc, while that due to friction in similar periods is less than one-twentieth of a second, as pointed out above. Since the frictional effect has always the same sign, while these fluctuations have both signs, they cannot be due to variations in the amount of friction, or indeed to any frictional effect caused by attractions of bodies outside the earth. Before discussing their origin further, however, I shall give some indication of the evidence on which their existence is postulated.

Briefly stated, the astronomical evidence comes mainly from a comparison of the deviations of the moon and sun from their gravitational orbits, with confirmatory evidence from the transits of Mercury. The earth is the clock by means of which the rates of motion of these bodies are measured. If the rate of the clock varies, the apparent changes in the rates of motion of other bodies will be substantially proportional to the rates themselves. The best test of the clock is the fastest moving body which has been sufficiently observed, namely, the moon. The latter exhibits apparent variations which are much too large to be attributed to defects either in the observations or in the gravitational theory. When we examine the observations of the sun, after applying all known corrections, including the secular acceleration, we find a similar set of variations of the right order of magnitude. But as the angular motion of the sun is less than 1/13 that of the moon, the variations are less in this proportion, and consequently are observable with a corresponding degree of uncertainty. In fact, it is only by combining all the existing material that we can feel any degree of confidence in their existence.

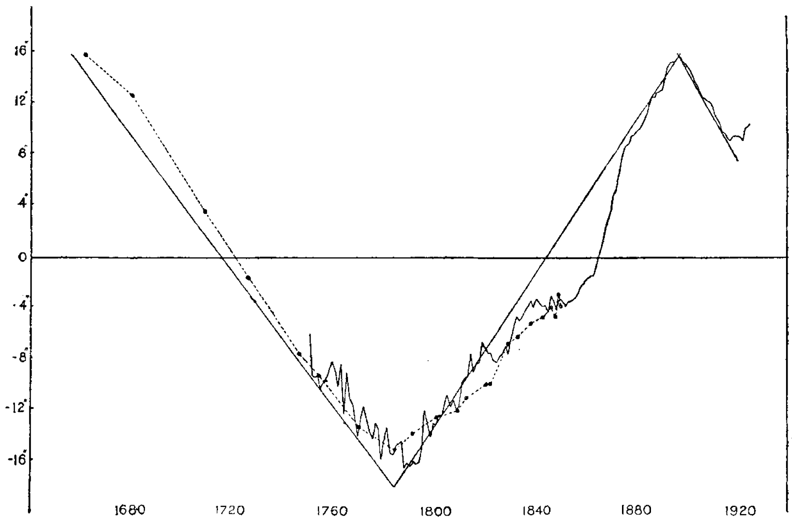

FIG. 1.—Fluctuations of the moon's mean longitude from Greenwich meridian observations (full line), and from occultations (broken line). The inclined straight lines are guides for the eye.

While the motion of Mercury round the sun is four times as fast as that of the earth, the relative lack of observational material more than offsets the advantage gained by the speed of motion. Nevertheless, the valuable work done by Mr. R. T. A. Innes in discussing these observations, the results of which led him to revive Newcomb's original hypothesis, constitutes an important addition to the evidence furnished by the moon and sun.

Fig. 1 exhibits the deviations of the moon in longitude from its gravitational orbit (which latter includes the frictional effect) as judged by the Greenwich meridian observations (full line) and the occultations (broken line); the inclined straight lines are merely guides for the eye. After 1850, the two sets and other observations so nearly agree on the scale of this figure that the Greenwich material is alone shown. The main outlines of the fluctuations with a minimum near 1790, a maximum close to 1898, and a marked change near 1917, are evident. Details before 1850, and in particular between 1810 and 1850, are doubtful; it seems that one set was subject to systematic errors which lasted for many years.

Fig. 2 shows (full line) the corresponding deviations of the sun as obtained from Greenwich, on a much smaller scale; the broken line is the full line curve of Fig. 1 with each ordinate divided by 13·3—the ratio to be used for the hypothesis of a change in the earth's rate of rotation. The coincident changes about 1898, 1917, are well marked; that at 1790 is doubtful, on account of the large deviations which follow this date. Comparison with observations from other places appears to indicate defects in the Greenwich material for the sun in this period, and, judging by the occultations, for the moon also. The same comparison indicates systematic errors of observation throughout the whole range which, while not large enough to change the principal correlations, are large enough to account for the differences between the second halves of the curve in Fig. 2.

The evidence thus favours the hypothesis of changes in the earth's rate of rotation rather than that of unknown forces affecting the motions of the bodies in this particular manner. If it be adopted, it is easy to prove that, owing to the nearly spherical distribution of the earth's mass, the causes of the change must almost certainly be internal. Since

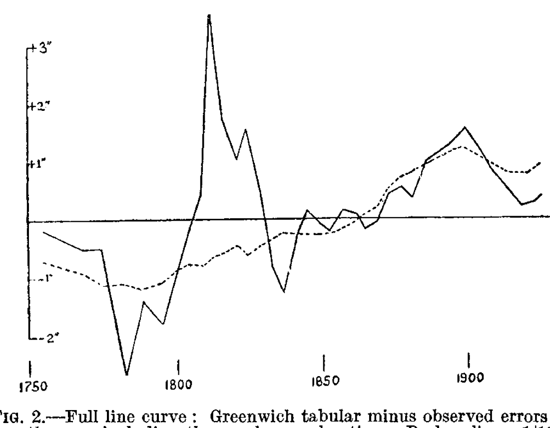

FIG. 2.—Full line curve: Greenwich tabular minus observed errors of the sun, including the secular acceleration. Broken line: 1/13·3 of the lunar fluctuations.

the angular momentum of the earth remains constant, we are reduced to the consideration of changes in the distribution of its mass, with respect to the axis of rotation. An idea of the magnitudes involved can be obtained from the supposition that the effects are due to successive raising and lowering of the whole crust. If the maximum change of the outer radius be twelve feet, the thickness of the crust which must be moved vertically by this amount, in order to explain the astronomical observations, is fifty miles. If uniform expansions and contractions throughout the whole earth be assumed, the maximum change of the external radius necessary is five inches. An assumption somewhere between these extreme limits appears to be the only way of explaining the fluctuations. It will be seen at once that, while local oscillations may be present, they cannot be invoked to account for the main phenomenon.

If oscillations of the whole crust take place, a natural procedure is to search for evidence of them in terrestrial phenomena. With a crust formed of uniform and unbroken material like that which composes the surface, detection would be difficult, because the resulting strains and pressures are far within the elastic limits. From the actual crust, of unequal heights, fissured in all directions and undergoing constant change from erosion, something observable may be expected. Adjustments, especially those needed for isostatic compensation, are continually taking place, and might be expected to be more frequent with or soon after a change of radius. The interpretation given here requires the earth's radius to be below its mean value from about 1790 to 1898, and a change near this latter date from the minimum to the maximum value taking taking place within a very few years, as well as a further sudden change to the mean value about 1917. Other sudden changes may have occurred in the past, but the observations lack the accuracy needed for definite statements.

Some attempts to correlate the observations with seismic phenomena have been made. Prof. H. H. Turner some years ago deduced a period of between 200 and 300 years from the records of Chinese earthquakes, and suggested that it might be related to the lunar deviations. In this connexion it must be remembered that the curve of Fig. 1 represents nearly all our present knowledge ; we do not know the extent to which the period or amplitude as deduced from this curve may represent what has taken place in earlier centuries. I have attempted a comparison with the frequency of British earthquakes from the material collected by Davison in his " History of British Earthquakes." There are indications of some correlation, but as they depend partly on the curvatures at different places of the curve in Fig. 1, there is doubt as to their reality. Correlation with the intensity of volcanic action was also briefly examined without success. The records of Kilauea as gathered by Dana and others would probably have served as a test if the material had been as complete and continuous as that gathered by Dr. Jaggar since 1911.

One remarkable correlation seems to be well founded, namely, the close correspondence between the frequency of British earthquakes on one hand and the difference between the Greenwich observations and the occultations on the other. Since the occultations are gathered from observations in widely different places and are nearly free from systematic error, it would seem that the Greenwich observations are subject to a small systematic error which depends on local earth movements. This same difference is also correlated with the lunar deviations. Closer examination of these correlations has not furnished any explanation for their occurrence.

The applications of the hypothesis, assuming that the oscillations have existed for long periods, to the formation of the surface features of the globe are far-reaching. Their source must obviously be sought in the physical and chemical conditions of the earth below the outer crust. It follows that a large supply of energy is available for external use. Adjustments to relieve the strains indirectly caused by erosion will be more frequent than without the oscillations, and the accumulated strains will be smaller. General statements of this character, however, do little more than furnish a basis for detailed investigations of specific problems. Can such oscillations, for example, constitute a factor of importance in the building of a mountain chain bordering a deep depression, as in the Pacific area ? Will there be vertical or horizontal differential movements of continental areas, and, if so, can such movements be detected in variations of sea-level or of deposition of sediments ? As to the latter, an inch of deposit in 200 years is 400 feet in 1,000,000 years, or 80,000 feet in 200,000,000 years, so that the magnitudes involved are of the right order for observation if the phenomenon exists. Many other questions of a similar character will occur to the geologist and the geophysicist.

The Principles of Biological Control in Economic Entomology.[1]

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I.

THE CONTROL OF INJURIOUS INSECTS.

IN considering the possibilities of success of the method of biological control of injurious insects, we have to take into account a number of factors, the most important of which are the climatic conditions and the amount of economic disturbance in the affected area. The most striking successes in control have been made in countries with a warm and equable climate, in which new beneficial insects can be introduced with greater ease and, when introduced, flourish and spread more rapidly, than in countries in which either a marked change of seasons or a severe winter has to be faced. But this climatic factor, important as it is, must rank only second to another factor, which may be termed the *amount of disturbance* of the affected

[1] From the Trueman Wood Lecture of the Royal Society of Arts, delivered on Oct. 27, 1926.