NOTES TO ACCOMPANY SOME THEOREMS IN THE DYNAMICS OF GEOLOGY.

By T. W. Kingsmill, F.G.S., &c.

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More than ten years ago my faith in some of the more generally received dogmas in geology commenced to give way. I had been brought up to the science mainly on the east coast of Ireland, and was there thrown in daily contact with phenomena which no observant student could fail to recognise as clearly the result of glacial conditions. As a boy, and years before I had made a study of geology, my attention had been irresistibly attracted by the huge erratic blocks which lay scattered over the Wicklow hills, and I gladly accepted the theory of the "Glacial Epoch" as a perfect explanation of the difficulties which had arisen in my mind. With such surroundings, the uniformatist system of Lyall had not unnaturally a special charm for me; and when later one of the stumbling blocks in the way of the interpretation of the history of the past was removed by the grand discovery of Darwin, that in the succession of life, as in the succession of geologic progress, there are no breaks, I hailed the new doctrine as little less than a revelation.

With such antecedents I arrived nearly a quarter of a century ago in China. I was informed that there were profound traces of the "Glacial Epoch" in Hongkong, and that the superficial deposits of the island were a veritable boulder clay. The boulders certainly were there, as likewise they were to be found everywhere along the granite hills characteristic of the coast ranges of southern China. My previous experiences soon showed me that the phenomena was not glacial, and I was able to convince myself that the so-called boulders were simply the more silicious spherules which had originally formed an integral portion of the solid granite rock, the more basic magma of which had become decomposed to considerable depths. The incident would have been scarcely worth recording had it not been that apparently similar rocks had been accepted by geologists like Belt and Agassiz as evidences of a glacial period within the tropics, and geologists were asked on such insufficient evidence to accept the theory of a general lowering of the earth's superficial temperature during a protracted period. If, however, these theories could be established it might naturally be expected that some of the mountain ranges of central China would afford some evidence in their favour. The evidence all tended in the opposite direction : diligent search showed that all denudation in these regions of which any trace remained had been essentially aqueous. More, the accumulating geological evidence went to show that Northern Asia in latitudes where in Northern Europe glacial conditions are unmistakeable, had not undergone, in modern geologic times at least, a glacial stage.

These conditions naturally excited doubts in the minds of a few of the more advanced students of geology, doubts, however, which in the face of the evident glacial traces visible to the most superficial observer in Northern Europe and America, it was difficult to substantiate by actual proof. Dr. John Evans in his presidential address before the Geological Society of London in 1876 stated as his belief that "as long as there was a possibility, not to say probability of the geographical position of the poles having changed, it is premature to invoke intense glacial periods to account for all the glacial phenomena which have been observed." Towards a similar conclusion my own studies in Eastern Asia had been gradually leading me, and in an address in February 1877 to the North China Branch of Royal Asiatic Society I stated my grounds in full.

Up to this period it was generally held that the interior of the earth was a mass of molten matter not yet cooled from its pristine condition, and the phenomena of earthquakes and volcanoes were readily attributed to the presence of this great mass in a state of fusion. As the mass cooled down gradually by radiation into space it shrank in dimensions, and the crust contracting on this became wrinkled and corrugated, producing the effects of earthquakes and volcanoes, and shaping the surface into continents and oceans with their concomitants of mountain ranges and valleys.

The first serious objections to this theory came from the Mathematicians—notably Sir William Thomson, who pointed out that the theory of internal fluidity was not compatible with known facts, such as the phenomena of the tides and the precession of the equinoxes. The actual amount of radiation, and the consequent secular contraction was found to be too small to account for the observed phenomena, while chemists pointed out the difficulty in supposing a central mass in a state of igneous fusion to be compatible with the existence of a comparatively rigid envelope. The accepted theories were thus found to be at variance with ascertained facts, while it was amply demonstrated that recent geological movements pointed to some deforming force. To refer this force to internal changes was only repeating the Hindu argument of resting the earth on the back of a tortoise; for after all what supported the tortoise in the one case, or produced the internal changes in the other?

About the same time, in the course of other studies, my attention was drawn to M. Henri Tresca's experiments on the behaviour of ordinary materials under compression, and his conclusion that under a certain amount of pressure, varying according to the material, all substances with which we are acquainted become plastic and flow as if they were liquid. Such a pressure we might expect to obtain at a depth of one or two hundred miles from the surface of the earth, and this condition, removed on the one hand from the perfect mobility of a liquid, and on the other from the assumed rigidity of a solid, seemed compatible with some of the phenomena adduced by Sir William Thomson. If, however, an inert mass incapable of internal reaction were to be substituted for a mobile fluid, it was necessary to look for some external force to account for the observed movements, and the force of gravity, accepted as the cause of oceanic tides, presented itself. The amount of deformation which could apparently be attributed to this cause on close examination seemed insufficient to account for the facts. It seemed, even at that time, to be an ascertained fact that there was a periodicity in the number and intensity of seismic disturbances corresponding with the motion of the earth from aphelion to perihelion, but the difference in the direct differential action of gravity due to the variation in distance was so small as to afford no reasonable explanation of the phenomena. Even increasing the difference to its possible extreme at periods of maximum eccentricity seemed to show so slight an increment as to be altogether out of proportion to the result. By no means, however, could the observed attitude of the oceanic tide be made to fit in with these calculations, and it became evident that a portion of the movement must be absorbed by the earth itself, but a portion still too small to be of any great effect as a disturbing cause.

Putting aside then for a time my calculations as unsatisfaction, I took up again Croll's "Climate and Time." Many geologists of repute had become dissatisfied with the enormous lapse of time demanded by the ultra-uniformatists for the growth even of the later tertiary formations, and were willing to accept his proposed chronology which referred the "Glacial Epoch" to a period of great eccentricity some 200,000 years ago. But a close study of the geology of Asia had confirmed my impression that the glaciated area did not extend to the northern lands of that continent. If a secular lowering of temperature were to be attributed solely to increased eccentricity, Mr. Croll had proved too much, for in such a case the lowering of the temperature must have been more or less constant along the parallels of latitude, a contention certainly not borne out by the facts, for actually it was confined to a comparatively small portion of the earth's surface. But if the evidence as to the extent of the glacial climate were insufficient, there seemed good reason to believe that the period was marked by frequent changes of level. Two if not three oscillations had to be accounted for in the British Isles, and elsewhere contemporaneous deposits borne similar testimony. If then the glaciation of northern Europe took place during a period of great eccentricity of the earth's orbit, we had at least equally strong evidence to prove a contemporaneous period of geologic change. Was there reason to associate this period of alternate elevation and depression with the condition of eccentricity?

The fall of an apple is said to have suggested the theory of gravitation. If instead of an apple a ball of liquid holding together by its internal attraction towards its own centre of gravity should fall towards the earth, is there evidence to indicate that its shape would be altered in the descent? A little thought suggested that as the forward portion of the ball was subjected to a greater pull than the upper, and that this differential attraction increased as the ball descended, the ball must naturally become elongated in the direction of the line of descent; and that if the fall were from a very great height it would eventually reach the earth as a stream of liquid. Now during a period of eccentricity of the orbit the earth does actually fall towards the sun in its course from aphelion to perihelion; at present the fall during the six months is about three million miles, but (accepting Mr. Croll's chronology as correct) during the European glacial period it must have fallen some ten and a half millions. It is true the ball did not fall in a straight line, but for the matter of that neither did the moon in falling towards the earth the exact distance which confirmed the comparison of the falling apple, and we know besides that a body falling down a perfectly smooth course acquires at any moment the same velocity it would have attained had it descended a like distance in a vertical line. If the earth were a rigid body it could not alter in shape, but a little more consideration will lead to the conclusion that in such a case the waters of the ocean would pile up at the poles of syzygy as the earth approached its perihelion. Experience tells us this does not occur, and we have only the alternative of believing the laws of gravity untrue, or of acknowledging that the earth is not such a rigid body as we supposed.

In the case experimental proof is out of the question; but the laws of gravity, though often complicated in their results, are essentially simple in principle. *One body attracts another directly in proportion to its mass, and inversely as the squares of the distances separating them.* Was it in this instance possible to apply those laws? The calculation fortunately was not very difficult, and it was found that approximately the prolongation of the earth's axis in syzygy would under present conditions amount to about one-third of a mile by the time it had arrived at perihelion. Tresca's experiments had proved the possibility of the earth assuming a new shape under the action of external deforming forces, and our deforming force had at last been found capable of producing a sensible effect. The fancy of Diogenes of Apollonia had come true, and in the earth the philosopher could behold a living entity which possessed its lungs in the stars, and inspired and exhaled in obedience to their attraction.

Although we may not be able to put a theorem to the crucial text of actual experiment, we may arrive at practically the same end by observing whether or not it is capable of offering a reasonable explanation of phenomena hitherto unexplained. The interior of the earth we may assume then to be in a state of plasticity under pressure, while experience teaches us that most of the substances of which the crust is composed are essentially more or less brittle. An explanation was thus at once offered for the curious fact that the majority of earthquakes hitherto observed had occurred in the winter half of the year, which happened in the vast majority of those recorded at perihelion. When once the form of gravity again was accepted as capable of affecting the shape of the earth the position and distance of the moon had likewise to be taken into consideration, and a further explanation was afforded of another observed fact—that somehow or other there was a relation between earthquake shocks and the state of the tides. It thus became easy to form an opinion as to the true cause of earthquakes and volcanic eruptions. The earth is a plastic mass surrounded with a brittle crust; in swinging round in its orbit it pants and palpitates like a soap bubble, the crust is very unequal in its capacity to bend, and is moreover here and there weakened by old cracks. Sometimes then the brittle crust snaps, and the snap is accompanied by the sudden vibration known as an earthquake shock; sometimes the yielding of the crust is accompanied by so much friction that the temperature of the adjacent rocks is raised to the melting point, and the phenomenon known as a volcanic eruption is the result.

Every earthquake and volcanic eruption then of necessity uses up some definite amount of force, and we can readily see that the only reservoir of force available is the *vis inertiæ* of the daily rotation. Astronomy teaches us that the period is by slow degrees becoming greater, and that some future period the earth may be reduced to the condition of the moon and present always the same hemisphere to the sun. True, the decrease in the velocity of rotation is at the present time extremely slow, amounting to scarcely more than a second in the length of the day in a thousand centuries. Even so small a factor may become of importance when it is capable of acting during the life-time of a planet. Now we can readily understand that if a shock occur on the medial line of rotation, the moments at either side being equal the sole effect will be an instantaneous stagger and a retardation of the daily period in proportion to the amount of energy withdrawn. If, however, the shock occur north or south of the equatorial line, the moments of rotation north and south being instantaneously unequal, the earth will shift round its axis of rotation, and the poles and equator assume a new geographical (not necessarily astronomical) position.

Now, one of the difficulties with which I started, and which equally struck so eminent a geologist as Dr. Evans, was the occurrence in some localities of glaciated conditions at distances from the poles inexplicable except on the assumption of considerable depressions of temperature, while in other localities at little distances from the poles there was no evidence of any more depression, or in some cases there was apparently evidence of the contrary change. If then we have evolved a machine capable of shifting the earth about its axis of rotation, it may so happen that each point on its surface may at one time or other have been within such distance of the pole as to become subject to glacial conditions, while the temperature and climate of the globe remained constant. It is the opinion of the astronomer in chief at the Russian observatory at Pultowa that a progressive change in the latitude of the building can be observed, while at Greenwich the contrary opinion prevails. Even at Greenwich the latitude concluded at the moment varies from that assumed from the observatory in the last century, and the question has naturally arisen in connexion with the Russian observations whether the difference is due to actual change, or is only apparent, resulting from instrumental errors not detected at the time.

This subject leads, however, to the threshold of another proposition more geological perhaps than seismological,—namely the possibility of timing the position of ancient poles and equators and the discussion of the arguments of the varying intensity from age to age of the manifestations of geoplastic energy. It is impossible in the limits of a single paper to give even an outline of the arguments, most of which are strict corrollaries from the preceding. There does, however, seem sufficient evidence to justify the assertion that geologic energy is not constant, but varies from age to age; and that while long periods, even during the tertian epoch, have been marked by frequent and violent changes in the distribution of oceans and continents, the present is an age of marked quiescence.

DISCUSSION.

Dr. Cargill G. Knott, who had the privilege of reading Mr. Kingsmill's paper beforehand, entered into a pretty full criticism of certain positions taken up by the author, of which the following is an abstract:—

The portion of the paper which is of special interest to the members of the Seismological Society, is where Mr. Kingsmill suggests an explanation of the annual periodicity of earthquakes. He invokes the aid of what might be called a differential tidal action. Already * the Society has listened to a discussion of the probable relations between earthquakes and tides or tidal stresses in the earth's substance. The present paper takes up the same question, but from an altogether different point of view. The argument is based upon the manner in which a liquid ball might be expected to behave as it fell towards an attracting, tide-producing body. According to Mr. Kingsmill's calculations, quite a large tide should be produced in the waters covering the earth as the earth falls from aphelion to perihelion. There is, of course, such an annual tide, but it is insignificant compared to the diurnal tides. Furthermore, if this annual solar tide is to be at all effective, surely the monthly lunar tide will also have an

*See Paper on *Earthquake Frequency*, by C. G. Knott, D. Sc., F.R.S.E. (Trans. Seismo. Soc. of Japan, Vol. IX. Part 1).

appreciable effect. When Mr. Kingsmill places us in the dilemma of believing the law of gravitation to be untrue, or of believing the earth to be plastic, his reasoning appears far from sound. He evidently expects an annual tide of huge dimensions; but, because there is no evidence of such, assumes that the earth must be itself yielding. Further, although he does not say so explicitly, the only logical end of his argument is that the earth must yield to the whole amount looked for in the ocean movements but not found there. He seems to be quite ignorant of the valuable calculations made with regard to this very subject by G. H. Darwin.

That the annual tidal stresses, small though they are, may have an influence on earthquake frequency is quite a legitimate surmise; but it cannot explain the fact of the annual periodicity of earthquakes. For statistics show that the maximum of earthquake frequency occurs, not at perihelion but at winter time; so that in the southern hemisphere the maximum falls at aphelion. In other words earthquake frequency is largely conditioned by meteorological phenomena; and, so far, there is little evidence that cosmical phenomena have any great effect. Thus, even granting the annual tide to have a seismic influence, we cannot regard it as the cause of the *winter* maximum in earthquake frequency. Here, in short, the theory is irreconcileable with the facts.