The overlap and irregular occurrence of the Tertiary on various parts * of the Cretaceous deposits, the immense banks of flints containing Cretaceous fossils in the Tertiary beds, point to an enormous amount of change and denudation between the consolidation of the Cretaceous and accumulation of the Tertiary deposits. This is accompanied by an almost entire break in the higher forms of life.† It is true that the researches of Dr. Carpenter and his colleagues have brought to light many forms which have survived from the Cretaceous to our own time ; but these discoveries are only of the same kind as the discovery in recent times of the genus Lingula, or of forms allied to Encrinites. When we trace back to a remote antiquity ferns and other plants not very unlike those of our own day, Crustacea differing but little from our King Crab, Paludinas hardly distinguishable from recent forms—that does not throw doubt upon the useful grouping of the rocks from carboniferous to recent times.

Species are continually being found common to two beds known to be separated by enormous intervals of time. Upon this fact Barrande founded his theory of the Colonies. But the classification into Mesozoic and Tertiary depends upon evidence that cannot be shaken by the discovery of a few more forms common to the two. The wonder always was that the break in life was so complete as it appeared to be at the close of the Cretaceous period, and the deep-sea dredging expeditions confirm what was *a priori* almost a necessary inference, that deep-sea conditions prevailed somewhere during the whole of the period from the Cretaceous age to our own, and that some forms of life have not been destroyed or developed into anything else during that period ; but that is a very different thing from saying that there is not sufficient reason for holding that the base of the Tertiaries marks the commencement of a new epoch.

T. M'K. HUGHES

Insulation of St. Michael's Mount, Cornwall

HAVING read Mr. Peacock's letter in your publication of the 2nd inst., I beg, through the same medium, to show that his reasons for supposing "the mount could not have been an island in 1086" are groundless.

He begins by giving the measurement of "Domesday Book," date 1086, of "the Land of St. Michael," and afterwards writes as follows :—

"There is an entire absence in 'Domesday Book' of any mention of island or islands on any of the coasts of Cornwall. . . . In short, the mount could not have been an island in 1086, because it contained at least eight times as much land as it does at present, probably connecting it with the main land, from which it is even now only one-third of a mile distant. . . . The present area of the mount is only thirty acres, so that there are 210 acres missing . . . since 1086 ; and in 1099, thirteen years after, we have a record of a catastrophe which would fully account for the loss"—that being the great irruption of the sea in 1099, as recorded in the Anglo-Saxon Chronicle.

When yonr correspondent quoted the measurement of "The Land of St. Michael" above referred to, he evidently imagined St. Michael's Mount, with the Church or Monastery on its summit, to have been like a nobleman's seat in the midst of a large park, with the sea at a great distance from the centre—and all this to have been comprehended in "The Land of St. Michael." The fact, however, is that in 1086 the Mount was, as it still is, a rock about five furlongs in circumference at its base, and insulated by every tide, whilst the two parishes on the mainland nearest to it—viz., those of St Hilary and Perran-Uthnoe (which may be identical with "The Land of St. Michael"), were then holden by the Church or Monastery of the Mount."‡ As the mount, however, is now almost universally allowed to be the *Iktin* of Diodorus Siculus, we may be sure that it was long before the commencement of the Christian era insulated daily as it is at present. I have written very fully on this subject in my work already referred to, published in 1862, and also in a paper printed in the Transactions of the Plymouth Institution for 1867-68 (pp. 17-37), in both of which I have exposed the error of all the translators of Diodorus in calling the mount *Iktis* instead of *Iktin*, and have also shown that the Mount, which was called in the Cornish language *Bre*-tin ("Tin-*Mount*") as well as *Ik*-tin ("Tin-*Port*"), has given its name, not only to *Mount's Bay*, but probably also to the whole of *Britain*.

R. EDMONDS

Plymouth, February

P.S.—I had written the above before I saw Mr. H. Michell Whitley's letter in your last number, which states that instead of "*Keiwal holds the Church of St. Michael*," as Mr. Peacock has translated the passage in Domesday Book (p. 2), it should have been "*The Church of St. Michael holds Keiwal*" (or Treuthal, as it is also called on p. 11), which is the name of a manor in the parish of St. Hilary. This confirms what I have above written, although I have adopted a different way of disproving Mr. Peacock's theory.

Aurora Borealis

A FINE Aurora was seen last night, or this morning, from 1 to 3 A.M. It first appeared as a *transverse* band from N.E. to S.W., and passed in that course far South of the Zenith, or between Arcturus and Mars. Subsequently it spread laterally and upwards ; presently radiated from near the Zenith to all azimuths ; and at 2.30 A.M. some of the rays N.E. were strongly pink.

In the spectroscope, the usual green line was gloriously bright. I saw it first, with a hand spectroscope, in the darkened light of the rough glass panels of a stair door. There were also faint lines more refrangible over the regions of E, b, and F. Rather to my astonishment, I was totally unable to see a red line, even when looking at rays abundantly pink to the naked eye. This was a disappointment, to say the least of it, because I had prepared, and had in the lower part of the field of view, red chemical lines to compare with anything red that should appear in the Aurora ; and I had seen the red line perfectly well in the fine auroras of last autumn, but then I had no such checks on its place.

However, my spectroscope is still a very rough, home-made affair ; and I am living in hopes of something better when Government supplies this Observatory at last with its long-desired, long-delayed equatorial.

Royal Observatory, Edinburgh, Feb. 13 C. P. S.

*THE THEORY OF GLACIAL MOTION**

MR. CROLL'S papers on Ocean Currents are a powerful application of the modern theory of heat and force, to show the fallacy of Captain Maury's explanation of the causes of oceanic circulation. They also discuss other matters of great interest, but as the concluding part is not yet published, we shall say no more about them at present, but that they well deserve careful study.

The other paper is a criticism of the Rev. Canon Moseley's supposed proof that glaciers do not descend by the force of gravity, and of the arguments of Messrs. Ball and Matthews on the other side. It will be remembered, that Canon Moseley determined by experiment the "shearing" force of ice, that is, the force required to fracture it by parallel pressure. A plug of ice of known cross-section is fitted into a hole through two smooth boards, and the force required to break the ice by sliding the boards over each other is the "shearing" force. Increasing this in proportion to the dimensions of a glacier, or of any large portion of one, it was calculated that the force required to cause the different parts of a glacier to slide over each other (as they must do in descending a valley of constantly varying form and size) was at least thirty times greater than the force of gravity on a slope such as glaciers easily descend. Canon Moseley came to the conclusion that expansion and contraction of the ice by heat and cold was the moving power ; and the fact that the glaciers move slower by night than by day, and in winter than in summer, was supposed to prove conclusively that heat is the cause of motion.

Mr. Croll believes that Canon Moseley has demonstrated that gravity alone does not cause glaciers to descend, but he completely demolishes the theory of contraction and expansion. He admits that heat aids the motion, but maintains that it does so by acting on the molecules of

* See Lyell, Student's Elements of Geol., pp. 258, 261, where attention is called to higher cretaceous beds than those on which the Tertiaries rest in England.

† Lyell, Op. cit. p. 256.

‡ See my "Land's End District—its Antiquities and Natural History," p. 166.

* "On Ocean Currents." By James Croll, of the Geological Survey of Scotland (3 parts). "On the Cause of the Motion of Glaciers." By the same author (Extracted from the *Philosophical Magazine* of 1870.)

the ice, which it loosens momentarily from their mutual cohesion, and allows to be re-arranged under the influence of gravity. Heat, he says, is the *condition*, gravity the *cause* of the motion which takes place, molecule by molecule rather than in masses. It seems very doubtful, however, if this theory is more tenable than the one it is intended to supersede. If heat entering the glacier loosens the molecules in its passage and enables them to move insensibly into new positions, it is difficult to understand what causes the numerous longitudinal and transverse fissures of a glacier, the production of which is often attended by loud reports, and which indicate movements of masses, not of molecules. And how could molecular motion lead to that heavy grinding of the ice over its bed, which scores and wears down the hardest rocks, and whitens great rivers with the finely triturated mud?

None of the opponents of Canon Moseley have noticed what seems to the present writer to be a radical fallacy in his argument about "shearing force." He assumes that, whatever the bulk or weight of the glacier, or of any portion of it to which the formula of the shearing force may be applied, the whole mass shears at once by the action of gravity on the same mass, and does not recognise the possibility of one portion of a glacier acting by its weight to shear another and much smaller portion. But this must inevitably occur; for, owing to the excessive irregularity of the bed in which every glacier moves, the mass must be every where in varying states of tension and compression, and must contain at each instant certain lines and planes of least resistance, the extent of which lines and surfaces may be very small compared with the dimensions of the glacier itself. At any moment, therefore, the whole descending weight of a portion of the glacier containing perhaps thousands of cubic yards of ice, may act so as to cause the shearing of a few superficial feet where the tension is greatest. This being effected, a partial equilibrium is produced there; but the points or surfaces of greatest tension are shifted, and another small shear or fracture occurs; and by this process and the continued regelation of fractured surfaces brought into contact, it may easily be seen that the glacier as a whole would be gradually moulded to its bed, which it would descend as surely as if it were a viscous mass. Another source of motion not taken into account either by Canon Moseley or Mr. Croll is the irregular melting away of the under surface of the glacier by terrestrial heat, which would often form unsupported hollows till a fracture occurred, and every such fracture must result in a downward motion of a portion of the glacier. The observed difference of the rate of motion between winter and summer, day and night, is more probably due to the different quantities of water which descend the crevasses into the bed of the glacier at those periods, than to any direct action of the heat. It is well known that in the higher portions of a glacier the supply of water from melting snow diminishes during the night, as it does in a still greater degree during the winter; and the large quantity of water that flows beneath every glacier in the summer must greatly assist its motion, both by melting away its lower surface, and by, to some extent, buoying it up.

Mr. Matthews's important experiment of the bar of ice which gradually curved by its own weight, should be tried again in an atmosphere kept at the freezing point. This would settle the question whether heat is an essential condition for the curvature or motion of ice by gravitation; but so far as the facts lead us at present, the arguments of Canon Moseley and Mr. Croll by no means *prove* that glaciers do not descend by the force of gravity alone.

ALFRED R. WALLACE

[The publication of this article has been delayed. It was in our hands before the appearance of Mr. Ball's paper in the *Philosophical Magazine* for February, where a view almost identical with Mr. Wallace's is ably advocated.—ED.]

AN ACCOUNT OF THE ECLIPSE AS SEEN FROM VILLASMUNDA BY AN UNSCIENTIFIC OBSERVER

THOSE set in authority over the branch of the Eclipse Expedition stationed at Agosta having decided against depending only upon observations to be made from the Observatory there, deputed Mr. Ranyard to proceed to another point upon the line of totality, and selected me as his coadjutor. Accordingly we set off, accompanied by Jarvis and Burgoyne, two of Colonel Porter's Sappers, at half-past nine in the morning of the eventful day; and, after driving some eight miles inland, we attained about eleven o'clock a point which appeared to my companion to present advantages for our object. Leaving the road, we went into the middle of a field of springing oats, on the highest point of a rocky ridge at an elevation of 600ft. above sea level, and of 520ft. above the *glacis* of Fort Agosta, where were posted the rest of our friends. The spot which Mr. Ranyard selected as the most suitable lay about a hundred

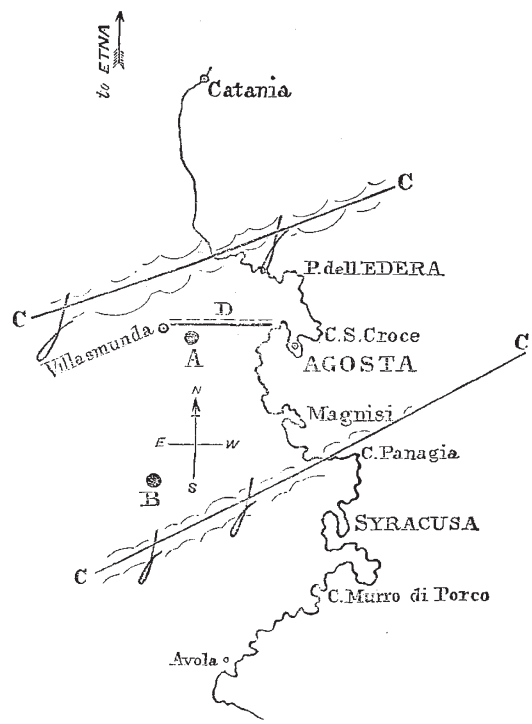

FIG. 1.
A our position; B the sun; CC, CC the lines of cloud; D the road to Agosta.

yards from a roadside farmhouse, called Casa Vecchia, upon the property of that friend of Science, the Marchese di Sangiuliano, and about two miles distant from the village of Villasmunda. A keen wind was blowing with considerable violence from the north-west, and the situation we had chosen being exposed to its full fury, we at first felt very uneasy with regard to our probable success, for we feared every moment that the telescope would be overturned and injured. A happy thought, however, soon extricated us from our dilemma. Causing our luckless coachman (who wept true Sicilian tears over the imaginary danger to his springless vehicle) to drive it, in the cause of Science, over the rock-sprinkled field, we utilised our carriage as a temporary shelter for the precious instrument, and were ready some time before