thesising the colours of the spectrum by reflection from seven moveable mirrors. With the mirrors placed at equal distances from each other, the spot of compound light is not white but yellow, that is to say, it is yellowish white, the colours lost between the mirrors being just those necessary to bring out the full white.

C. J. WOODWARD

Meteor

A FINE meteor was seen here to-night at about 9.10 P.M.; it was described to me as starting from near θ Orionis, and proceeding towards a point a little north of γ Eridani, when it was lost behind a belt of cloud.

J. M. WILSON

Rugby, Feb. 13

Snake Bites

IN NATURE of Dec. 22 I notice a note extracted from the *Pall Mall Gazette*, giving a return of the excessive number of deaths which take place annually in the Bengal Presidency, from the effects of snake-bite. That 11,416 persons die from this cause alone, "and that no efficacious means are adopted to check its ravages," are very startling announcements, and strike me as being well worth the attention of the readers of NATURE.

Upwards of a year ago Dr. Fayer recorded an elaborate series of experiments on snake poison, in the *Indian Medical Gazette*, from which he concludes "that if an animal, and probably a man, be fairly bitten by a fresh and vigorous cobra or daboia, it or he will inevitably succumb unless some immediate or direct method of arresting the entering of the poison into the circulation be practised." This direct method is to apply ligatures and cauterisation; for, says the same authority, "to conceive of an antidote, in the true sense of the term, to snake poison, one must imagine a substance so subtle as to follow, overtake, and neutralise the venom in the blood, or that shall have the power of counteracting and neutralising the deadly influence it has exerted on the vital forces." I remember reading some time ago of another doctor in India or Australia, who had tried ammonia as an antidote, but I cannot recollect with what result. It seems to me, however, that this real antidote has still to be found: and cauterisation, to prove effectual, must follow the course of the poison, which it cannot do; nor, indeed, is it possible for it to do much more than burn the walls of the wound, so it is not to be wondered at if some of those subjected to this powerful treatment do not recover.

I have long thought that the best cure for snake-bite would be powerful suction, applied to the wound by means of an instrument made for the purpose, and similar in principle to a boy's sucker. This would draw off a considerable quantity of the blood in the neighbourhood of the wound, and by so doing wash the poison out before it. Above the wound there might be a ligature applied, but sufficiently distant from it to ensure that the blood in the small vessels between the wound and ligature be competent to wash out the poison.

Where such an instrument is not at hand, and the assistance of a second party can be obtained, he might tie a ligature above the wound, and suck the latter with his mouth for a considerable time, spitting out all the saliva and blood which accumulates. It would be advisable, too, to make the wound a little larger before commencing to suck, with a sharp knife or otherwise, in order that the greater flow of blood may the better discharge the poison. The operation of sucking the poison into the mouth need not be feared, for even although a small portion of it were swallowed, it could do no harm. I believe I am correct in stating that a quantity of poison which will prove fatal on entering the circulation, would have no injurious effect when taken into the stomach. Where the bite is in such a part of the body that the party bitten can easily suck it himself, then he ought to do so: but unfortunately this is seldom the case, it usually being the lower extremities which are attacked.

In support of this method, I may say that I read, two or three years ago, an account of how the bites of snakes were counteracted in a woody portion of South America. The writer said when any one was bitten—and there were one or two almost daily—he was sure to die in thirty minutes to one hour afterwards if his wound was not immediately sucked. There, however, in order to make quite certain that the poison, when sucked out of the wound, would have no injurious effect on the sucker, the latter filled his mouth with olive oil before applying it to the

wound: and I imagine this would be a sure precaution, for it provides a plentiful supply of matter for the poison to diffuse in, without interfering much with the absorbents of the mouth. The result of this writer's experience was that the bitten person seldom, if ever, died; and he who sucked the wound never felt an injurious effect.

This subject seems so important that I have ventured to address you at this length upon it, in the hope of drawing from some of your correspondents further details concerning antidotes and methods of curing snake-bite.

T. L. PATTERSON

The Cretaceous Period

IN NATURE of Jan. 19, a letter appeared from Prof. Wyville Thomson defending the expressions, "we are still living in the Cretaceous epoch," "the chalk is being formed at present in the bed of the Atlantic." When first this announcement was made, it was followed up by various strong comments implying that the similarity of the Atlantic mud to the chalk in lithological character, and in many of the imbedded organisms * "would seem to unsettle much that has generally been accredited to geological science," would, in fact, revolutionise geological classification.

As these unfortunate expressions are again put forward, notwithstanding the protest of our most distinguished geologists,† Sir Roderick Murchison and Sir Charles Lyell,‡ it may be useful to consider what the question at issue really is. Simply stated, it is this: Have we sufficient evidence for drawing one of our strongest lines at the base of the tertiary deposits, and saying that it marks the commencement of a new epoch or period?§

In grouping the rocks, we have been obliged frequently to adopt an arbitrary classification. The thickness will not furnish the necessary tests, as accumulation is more rapid at one time and place than another. Lithological character will not do alone, as a bed often passes both vertically and horizontally into one quite different. The organic remains will not do alone, as the fauna migrated and re-migrated to suitable areas when favourable conditions occurred.

But sometimes we have the commencement of a new period well defined by seeing that the group of deposits which form the record of it rest unconformably on an older formation, which has been in part at least heaved up, denuded, and used to form the new series. At the bottom of this newer series we must infer a considerable break—a portion of unrepresented time.|| This time may be represented elsewhere, but we have a point in time well marked by the first grain of the new deposit laid upon the older denuded rocks.

So we are quite safe in saying that there had been a considerable lapse of time, and that new conditions prevailed over large areas when the Cambrian was laid unconformably upon the Laurentian, when the Upper Old Red was laid unconformably on the Lower Old Red and Silurian, when the Permian was laid unconformably on the Carboniferous and more ancient rocks.

Probably deposition went on longer over one area than another—very likely deposition has never in the earth's true geological history been entirely arrested, so that the connecting deposits between any two formations and intermediate forms of life may possibly still be preserved under the depths of ocean or on the vast still unexplored continents. It would be of course difficult under such circumstances to identify in a series of more or less continuous deposits the base line we have so well marked elsewhere by visible unconformity; and this difficulty occurs in the older rocks, as, for instance, in the case of the base of the Upper Old Red or Carboniferous in South Wales, of the Permian when the Rotheliegende is present, and many others, but this arises from our want of data. We might fairly hope that if we could find the continuous deposits after enormous intervals represented by known unconformities, we should read a wonderful story, and know, for instance, more clearly by what variation of forms and invasion of stronger life from adjoining areas we are left at the end of a long period with an ammonite instead of a goniatite, or a nautilus instead of either.

Now, to return to the particular case under notice. Have we at the bottom of the Tertiary formations evidence of a break so large, of a lapse of unrepresented time so long, of a change in conditions over large areas so great that, we are justified in saying that this is a convenient place to draw one of our strong lines?

* Carpenter, Lecture Royal Institution, Ap. 1869.

† Pres. Add. Geog. Sect. B.it. Assoc., Liverpool, 1870.

‡ Students' Elements of Geology, 1871.

§ See also an able article by Mr. Green, *Geol. Mag.*, Jan. 1870.

|| See Ramsay, Pres. Add. Geol. Soc., 1863-4.

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 your 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 *Iktin* ("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 Arc-turus 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.)