the other one is "*Felis* — 14. Femur Right side." There is no record in the catalogue by whom they were presented, nor of any of the circumstances of their *gisement*. The specimens, in fact, have no history whatever, and I can only say that I found them in close juxtaposition with a large series of red-deer bones from Holderness, with which they perfectly agree in their mineralogical condition. I have no doubt that they are *bonâ fide* from the Holderness Peat.

Their identification as bones of *F. leo* (variety *spelæa*) is also certain.

Hull Royal Institution C. CARTER BLAKE

Eozoön Canadense

SINCE reading some of the communications on the Eozoön, which have appeared from time to time in NATURE, I have felt constrained briefly to give the results of my examination of the "Eozoic" limestone in Eastern Massachusetts. I am the more disposed to do this, hoping that a new line of investigation will be suggested to observers in other localities.

Last autumn I visited for the first time the quarries of "Eozoön" limestone in Chelmsford, under the guidance of my friend Mr. Burbank, of Lowell, Massachusetts, who has furnished many microscopists with specimens for sections. Having been long engaged in the study of the foliated series of rocks, and having years ago discovered indubitable evidence that portions of the included limestone are of vaporous origin, I was prepared to recognise the same feature in the Chelmsford "Eozoic" rock. I was accordingly not surprised on examination to find, what the advocates of the organic nature of the Eozoön seem never to have suspected, that the limestone in question is not a "sedimentary rock;" that it occupies, or rather occupied, (for it has been for the most part removed) pockets or oven-shaped cavities, which were once plainly overarched by gneiss; that it is foliated, there being a regular succession of leaf-like layers from the walls toward the centres of the cavities, witness to which is borne by a like succession of different minerals; that in some places it ramifies the surrounding rock in a vein-like way, while in others it exactly conforms with the most abrupt irregularities of surface; that in one locality, which I have repeatedly examined, it conforms with the uneven portions of a mass of syenite, with which it is so associated as to reveal its more recent origin; and that, therefore, it is not of nummulitic derivation, but was deposited in a vein-like form, the materials having been probably forced up into the cavities from below while in a vaporous state.

Such, in few words, is the result of my examination—a result which tends to show that the "Eozoön" of Eastern Massachusets is not organic, and that thus it belongs to the department of Mineralogy, and not to that of Palæontology. Waving additional particulars for the present, I may simply add that I propose in due time to give a detailed exposition of the relations of this famous "Eozoic" rock.

Cambridge, Mass., April 15 JOHN B. PERRY

THICKNESS OF THE EARTH'S CRUST

I SEE that at p. 296 of your journal for February last, which has recently reached Calcutta, you print a lecture by Mr. David Forbes "On the Nature of the Earth's Interior," in which reference is made to the Mr. W. Hopkins's method of determining whether the thickness of the earth's crust is great or small when compared with the whole radius, and to M. Delaunay's objection to it.

The lecturer refers to me as having approved of Mr. Hopkins's method, which I always have done and do still, and then makes the following apparently crushing remarks to annihilate Mr. Hopkins and all who approve of his method and of the result to which it leads, viz., that the crust is very thick. He says:—"M. Delaunay, an authority equally eminent as a mathematician and an astronomer, was induced to undertake the reconsideration of the problem; a labour (!) which has resulted in altogether reversing the above decision and demonstrating the complete fallacy of the premises upon which so much elaborate reasoning had been expended."

As the lecturer had condescended to mention my name in connection with the subject, I wonder why he has taken no notice of my letter in reply to M. Delaunay, which was printed in your journal for July 1870, six months before the lecture was delivered, and which also appeared about the same time in the *Philosophical Magazine* and the *Geological Magazine*. In this I showed that M. Delaunay had evidently misconceived the problem, and that Mr. Hopkins's method is altogether unaffected by his remarks.

So much has been said about profound mathematical calculations in connection with Mr. Hopkins's investigation, that I conceive many have shrunk from attempting to understand the question at issue, from a feeling that they would not be able to comprehend it were they to attempt to do so. But this is quite a mistake. Anyone with an ordinary degree of knowledge of popular astronomy and of mechanical action is quite competent to form a good opinion on the point in dispute. What Mr. Hopkins did may be divided into two parts. He first conceived an idea, which was to be the basis of his calculation; and then he made his calculation. It is the *calculation* that calls for the "profound mathematics." But it is not this that is the matter of dispute. It is the *idea*, on which the calculation is based, which M. Delaunay calls in question.

I think I can make the matter sufficiently plain to your readers to enable them to form their own opinion.

Everyone having a knowledge of popular astronomy is aware that the earth revolves round an axis, which is fixed in the earth's solid crust, but shifts very slowly in space, producing what has been known ever since the days of Hipparchus by the name Precession. On this fact as his ground-work Mr. Hopkins reasoned as follows; and so got to his *idea*, which formed the basis of his calculation. Suppose the earth has a solid crust, the interior being filled up with fluid. If the axis remained steady in space and the crust revolved round it uniformly, no doubt, although the crust and fluid may have moved differently at one time, yet in the lapse of ages friction and viscosity in the fluid would cause the fluid at last to revolve with the crust just as if the whole were one solid mass. This being the case, suppose a slight horizontal push is given to the two poles, in opposite directions, so as slightly to shift the axis in space; what would happen? The revolving crust, by this new and additional motion, would slip over the surface of the revolving fluid, through a small space proportionate to the push given to the poles. The fluid could not possibly acquire in an instant this new motion, however small it might be, because the fluid is not rigidly connected with the crust. Suppose a second, and a third, and a succession of slight horizontal pushes to be given to the poles in a continually altering direction, the effect will be that the revolving crust will be continually slipping over the revolving fluid which has not time to acquire these new motions given instantaneously to the solid crust. These successive slight pushes given to the poles, and so to the solid crust, represent the unceasing action upon the crust of the force which causes the motion of precession in the earth's axis, and arises from the attraction of the sun and moon on the protuberant parts of the earth about the equator.

Mr. Hopkins having reasoned thus far, went a step farther, and so came to his fundamental idea. He saw that the thinner the crust the smaller would be the mass which the disturbing force producing precession would have to move, and therefore the greater would be the motion caused, that is, the precession. Here, then, he discerned a connecting link between the amount of precession of the earth's axis and the thickness of the earth's crust. This was the *idea* I have alluded to.

Starting from this idea he entered upon a profound calculation and obtained a formula, which gives the thickness in terms of the amount of precession. This amount is a matter of observation; and the thickness can therefore be deduced by the formula from the observed pre-