cidents caused by movements a little more rapid than usual, or by the sudden slipping of large masses out of the position of unstable equilibrium into which the slow movements had brought them. If these large or primary movements were fault movements, one would almost expect to find the axis on the edge, and not in the middle or at the bottom of a steep trough in the ocean-bed. If the primary movement, on the other hand, was principally one of revolution about the axis, interrupted by an occasional sliding of the mass on one side of the axis upon the mass on the other, then we must look for secondary movements at some distance from the axis, where the displacement caused by revolution is naturally greater. Is it possible that the lesser shocks were more or less local movements of this character? It is curious to notice that the total intensity of the series of shocks amounts to 186,690 absolute units, or about 19 times the acceleration due to gravity. The large expenditure of energy implied by this total suggests at least a possibility of a very appreciable amount of movement in the land-mass of Tasmania and southeastern Australia. Though, so far as I know, there is no evidence of elevation or depression, one does not like to think of Mother Earth wasting so much of her strength for naught.

LETTERS TO THE EDITOR.

*** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as a proof of good faith.

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The Editor will be glad to publish any queries consonant with the character of the journal.

Volcanic Rocks in the Keewatin.

In view of the article published in *Science*, No. 571, entitled "Volcanic Rocks in the Keewatin of Minnesota," and the very numerous recent papers on the same subject, viz.: "Archæan Volcanic Rocks," it may be interesting to your readers, and, in any case, I think it is fair to myself, to publish the following letter of mine on the same subject, which was written fourteen years ago. Any comments by me are, I think, unnecessary.

Alfred R. C. Selwyn,
Deputy Head and Director,
Geological Survey Dept., Ottawa.

"Montreal, 9 December, 1879.

"My dear Professor Dana:

"I have just read your remarks[1] in reference to what I have ventured to call the Volcanic Group of the Quebec series of Sir W. E. Logan. I should like very much to know exactly what your views on this question are, and hope at some future time to hear them from yourself personally. In the meantime I would make a few explanatory remarks on the points you refer to in my paper. You say, 'The evidence of the general volcanic origin of the second group is not stated and the kind of rocks mentioned make a remarkable assemblage to be spoken of as these volcanic rocks.' This would seem as if I had meant to assert that all the rocks mentioned as constituting the group were of volcanic origin. I might certainly have made the matter plainer had I specified those rocks in the group which there were reasons for supposing to be of volcanic origin. It never occurred to me, however, that in giving a general description of a group of strata[2] of mixed volcanic and *ordinary* sedimentary origin it would be necessary to do so. As regards the evidence of a volcanic origin, of some of them I can only say now that it is of precisely the same kind as that which, in respect of similar British strata, has been considered to be conclusive by almost every British geologist of note, including De la Beche, Lyell,[3] Murchison, Sedgwick, Jukes,[4] Scrope and a host of others now living.[5] Further, that these conclusions, first arrived at by the most careful and minute geological investigations and mapping of the stratigraphy, have been, or are supposed to be, entirely confirmed by the, comparatively recent, microscopical and chemical investigations of these same rocks.

"It is now rather more than thirty years since I took an active part, under the geologist I have first named, in working out in all their intricate details the great Lower Silurian and Cambrian and older volcanic series of north Wales. Since then I have had abundant and world-wide opportunities of studying volcanic formations of all ages, recent, Tertiary, Mesozoic and Palæozoic, and I may say that it is on the result of this world-wide geological investigation and experience, and not on the occurrence of labradorite or any other particular mineral, that I have come to the conclusion that we have in Canada, as in Great Britain and elsewhere, good evidence of the existence of volcanic strata, and consequently of volcanoes, in Silurian or Cambrian and pre-Cambrian epochs. I am quite aware that most of the peculiar rocks, which, in common with a majority of British and some American geologists, I hold to be of volcanic origin, have heretofore been generally, and doubtless quite correctly, described simply as 'crystalline,' 'metamorphic,' or 'igneous' rocks. But this, it seems to me, does not refer so much to the question of their origin, as it does to that of their present condition and character, and if we carefully study their stratigraphical relations in the field, and their microscopic and physical characters, we at once find—at least, such has been my experience—that some other explanation of their origin and associations is required besides that of their being ordinary sedimentary deposits in a metamorphic condition. Indeed, your own and Mr. Hawe's careful and admirable investigations of the chloritic formation in the New Haven region seem to me to demonstrate the entire probability, to say the least, of the igneous and volcanic origin of the rocks you describe. It is, I believe, generally admitted that rocks having the mineral and physical peculiarities characteristic of many volcanic products would be more easily affected by metamorphic agencies, especially hydration, than those which are of ordinary and unmixed sedimentary origin and that these old volcanic rocks should have assumed these metamorphic or altered characters is, of course, what might be expected and their having done so certainly does not negative the supposition of their volcanic origin. It seems to me that *à priori* probabilities of the existence of volcanoes in Eozoic and Palæozoic epochs are very strong and that those who oppose any such idea should be prepared, like those who hold the opposite opinion, to state some good reason for their views, and also the particular geological epoch when, in their opinion, volcanic outbursts first occurred. If, on the other hand, the existence of volcanoes in these early geological epochs is admitted, then we may very naturally expect to find their products associated with the ordinary sedimentary rocks of the period, in the same manner as we do those of the volcanoes of recent and Tertiary times. And this is what British geologists generally claim to have done. I have no wish to dogmatise on this question and only desire the truth, whatever that may be; but at present I cannot help feeling that if I am in error, I am so in very excellent company, and that the views of such eminent geologists as I have named, and based, as I know

[1] *American Journal of Science*, vol. xv., 1879.
[2] Now, 1891, appropriately termed "*pyroclastic*."
[3] *Vide* Lyell's "Elements of Geology," 6th ed., pp. 695, 693.
[4] *Vide* Juke's "Manual," 2nd ed., p. 324.
[5] *Vide* Ramsay, "Memoirs," Geological Survey of Great Britain, vol. iii., chap. 5.

them to have been, from intimate personal acquaintance, both with the men and with the country to which they were applied on precisely the same evidence as is to be found in Canada, are at least entitled to be regarded as something more than 'fancy sketches.'

"I am, my dear Professor Dana,
"Very truly yours,
"(signed) ALFRED R. C. SELWYN."

Inquiry Regarding Fresh-water Pearl Fisheries.

I ENCLOSE herewith a circular of queries that I am very desirous of having answered to assist in carrying out an investigation in regard to the locations, yield and proper protection of fresh-water pearl fisheries in the United States. All correspondence on this subject should be addressed to me as follows: G. F. K., care of United States Commission of Fish and Fisheries, Washington, D. C.

GEORGE F. KUNZ.

LIST OF QUESTIONS.

The pearl-bearing mussels: 1. Nature of stream in which found, kind of bottom, character of water; 2. Geological character of the district as to rock, soil, etc.; 3. General abundance of mussels; 4. Size, shape, and position of the mussel beds; 5. Local names of mussels; 6. Habits of mussels; 7. Enemies and fatalities to which mussels are exposed, nature and extent of destruction by muskrats, hogs, freshets, etc.; 8. Size, shape, and color of mussels; 9. Species of mussels in which pearls are most common; 10. Proportion of mussels in which pearls occur; 11. Sizes, or other peculiarities, of shells in which pearls are found. The pearls: 12. Nature and origin of pearls; 13. Position in mussel; 14. Size, shape, and color of pearls; 15. Relative value of pearls of different sizes, shapes, and colors; 16. Markets for pearls; 17. Prices for pearls. The fishery: 18. Method of taking the mussels; 19. Description of apparatus used in taking mussels and in opening the shells; 20. Methods of extracting the pearls; 21. Treatment of pearls when found; 22. Utilization of mussels after extraction of pearls or after opening; 23. Principal occupations of mussel fishermen; 24. Statistics of fishery in 1893: Fishermen, number; Boats, number, value; Apparatus, number, value; Pearls, number, value; 25. Statistics, complete or partial, for previous years; 26. Period when pearl fishing was of greatest importance in district; 27. History of origin and growth of fishery; 28. Exhaustion of mussel beds, causes, rapidity; 29. Do exhausted beds become replenished, and in what time? 30. Is State protection of beds desirable or necessary?

Psuedo-aurora.

THIS phenomenon has again, this winter, been of unusual beauty and brilliancy over lights in this city. The following is an explanation of the occurrence: The phenomenon is not a shadow effect, as explained by Mr. Hazen in a number of last year's *Science;* neither has it any electrical significance, as has been indicated by several writers. It has a true light effect and consists of an apparent bright shaft of light extending upwards to the zenith over bright lights. If the lights are swung high it may also be seen extending from the light to the earth. It only occurs under certain atmospheric conditions, which are about as follows: Cold, cloudless nights, with but slight wind, if any, following upon an atmospheric humidity approaching saturation. Under these conditions, small, almost solid, flat, hexagonal ice crystals fall in a constant mist, glistening in a strong light, like particles of diamond dust. It is in the peculiar flat, light form of these crystals, associated with the manner in which they fall, that explanation of the phenomenon rests.

Leaves falling from trees, during a calm, preserve, during the chief part of the descent, the horizontal position. This is found by careful observation to be alike true of the fall of the flat, hexagonal ice particles at the times when pseudo-aurora occur. The rays of light from the lamp or other brilliant light, striking the under flat surfaces of the crystals, are simply reflected to the eye of the observer; and the eye receives rays reflected from all particles of ice within the vertical plane through the eye and through the lamp or source of light, referring the grand final light effect to the vertical.

That it is not of an electrical nature is evident in that some of the most brilliant shafts occur over bonfires, and when the moon is low in the horizon, on nights showing the aurora, it is seen to cause the same phenomenon.

H. L. BOLLEY.

Fargo, North Dakota.

Notes and Queries.

Rubus strigasus is rather common in Waterbury, Conn., but I have never found it bearing perfect fruit (except as noted below). The canes are quite as strong and thrifty as any found in northern New England, and plenty of flowers are produced, but rarely more than two or three drupelets in a berry ever mature.

My observations extend over a period of more than thirty years. At present the plant is much more abundant than formerly, but the failure of the fruit to mature is the same as at first. Asking Dr. Asa Gray, at a meeting of the Botanical Section of the A.A.A.S., several years ago the cause of this, he remarked that "he should like to see the flowers" before answering.

Of course it is because they are not *fertilized*, but why are they not? Honey bees here, as further north, find the raspberry blossoms a rich source of honey, and any part they may take in the fertilizing process ought to be as effective here as in other sections. The exception to this sterility makes the sterility still more singular. Along the ditches through some of our shallow peat bogs this red raspberry grows, sometimes, very rank and tall, and it is not rare to find, in September and October, the terminal portion of the year's growth full of blossoms and perfect but unripe fruit. I have seen these berries fully grown, very large, but none quite ripe, though they probably do ripen in warm seasons. There is a cause for this failure of the summer crop, but what is it? I believe the cultivated species and varieties of the raspberry do as well here as anywhere. It is well understood that the failure of the first crop of red clover to produce many seeds is because the bumble-bees, the only insects that frequent its flowers for honey, are too few to transfer the pollen.

This cannot be the case with the raspberry. It may be found that the honey-bee, wholly intent on gathering honey, neglects the pollen altogether and that the pollen gatherers prefer that of some other flowers found here. The interrelations between plants and insects are not all yet found out, but the suggestion made above may have no foundation in fact.

Thirty years ago the farm yards and road sides in western Connecticut were white in the summer from the abundance of the flowers of the May weed (*Maruta Cotula*). A few years later—but just when no one can tell, for it passed without observation—this plant disappeared entirely. For years not a single specimen could be found. It is now slowly reappearing, but not yet abundant. Over how large a part of the country this disappearance took place I cannot say, for my observations did not extend far beyond Waterbury and the adjacent towns.

Here, as in numberless other cases, the ever-recurring *why* appears. Certainly no *apparent* cause banished this