LETTERS TO THE EDITOR.

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Alexander Agassiz and the Funafuti Boring.

PROF. POULTON has directed attention (NATURE, February 26, p. 712) to the fact that "very little has been said" concerning the evidence on coral-reef formation obtained by the boring at Funafuti, and he also refers to the views upon the subject held by the late Prof. Alexander Agassiz.

It will be remembered that the very successful borings at Funafuti were carried out by Profs. Sollas and Edgeworth David, and their assistants, under the auspices of the Royal Society, with valuable aid from the Admiralty and the Government of New South Wales. The place for the experiment was selected by a committee of the Royal Society, on which every shade of theoretical opinion was represented, this committee having the invaluable assistance of the late Admiral Wharton, who recommended Funafuti as perhaps the most typical atoll that could be found on the globe.

The very complete set of cores, with all the other materials obtained during these borings, were, by the permission of the Board of Education, received in the geological laboratories of the Royal College of Science at South Kensington, where they were studied by the members of the staff, with the invaluable assistance of Dr. G. J. Hinde, much aid being also given by the officials of the British Museum (Natural History) and of the Geological Survey.

Those who were responsible for the preparation of the report on the undertaking, published by the Royal Society in 1904, felt it to be outside their duty to advocate any particular theory of the origin of coral-reefs; their aim was simply to place on record the evidence obtained; and it may be added that this evidence is always open to examination and criticism from the circumstance that the halves of all the cores are now deposited in the British Museum, with the sections and other specimens, while duplicate halves of the cores have been sent to Sydney University.

During the eight years that the work of studying the materials from Funafuti was in progress, I received many visits from my friend, Prof. Alexander Agassiz, and gladly profited by his advice and suggestions. He showed his confidence in the manner in which the work was being carried on by entrusting to me the materials he collected from the upraised coral-reefs of the Pacific, with the request to have them examined side by side with the Funafuti cores, the result being published at his own expense.

I should not be justified in trying to reproduce the views of Agassiz as communicated to me in our frequent friendly discussions—everyone who knew him will accept my statement that they were always candidly and fairly expressed. But, fortunately, in the work published since his death, his position in the controversy is very clearly indicated. His own researches had demonstrated that, over very considerable areas in the Pacific, elevation, often to the extent of 1000 ft. or more, had taken place. Agassiz maintained that the masses of coral-limestone in the upraised islands—which were much altered, like the limestones in the lower part of the Funafuti boring—were Tertiary rocks, and that the lower cores of Funafuti were of the same age. His views are very clearly illustrated in a diagram reproduced in the "Letters and Recollections," p. 343, together with their relation to the views of others, as understood by himself.

I may add that the most careful study of the Funafuti limestones did not supply any evidence of such a change in the fauna as would justify their being assigned to any of the Tertiary periods. But even if such evidence had been found, the geologist would have been justified in arguing that this would only prove that subsidence had taken place with extreme slowness, or had been subjected to long interruptions. On the other hand, the fact, which Agassiz so fully demonstrated, that certain areas in the Pacific have undergone elevation in recent times, would suggest to every geologist, taking into account what we know of "the warping of the earth's crust," that other areas must simultaneously have been undergoing subsidence, and this was the view maintained by Darwin.

We are entitled then to say that a boring, initiated and carried out under the direction of representatives of all the rival theories on coral-reef formation, was attended with brilliant success. In an island selected as a very typical atoll, the main boring was carried down more than 900 ft. below the lowest depth at which, as all naturalists agree, reef-forming corals can flourish. The materials from top to bottom yielded only those organisms that thrive near the surface of the ocean, often in the position of growth. In opposition to the view that the boring may have penetrated only a talus on the side of the reef, it must be pointed out that two additional borings were made *in the very centre of the lagoon*, which revealed, down to the depth of 100 ft. below their limit of growth, the same reef-forming corals. Finally, in this very typical atoll, all idea of solution going on at the bottom of the lagoon was negatived by the luxuriant masses of the delicate calcareous alga, Halimeda, which, with the thinnest shelled Foraminifera, everywhere abounded in a perfectly uncorroded state.

With Prof. Poulton, then, we may fairly say that while the theory of subsidence is not of "universal application"—and Darwin in all his later writings candidly admitted that such was the case—yet the "validity" of this theory of subsidence is fully established in the case of the only atoll in which the test by boring has been carried out. JOHN W. JUDD.

Kew, February 28.

An X-Ray Absorption Band.

FOR some time past I have been trying to make accurate comparisons of the intensities of the various orders of X-ray spectra reflected by crystals. The purpose of the inquiry is to make experimental tests of the theoretical discussions by Debye and Sommerfeld in relation to the influence of molecular motions upon reflecting power. Of some of their predictions I have found it easy to obtain confirmation which is at least roughly quantitative. For instance, the intensities of the higher order spectra are much more affected by rise of temperature than the lower, and the amount of the change is of the right order of magnitude; also rock salt and sylvine show greater changes than fluorspar.

In one case, however, the results have been puzzling. The relative intensities of the spectra of the diamond at ordinary temperatures are quantities of much importance. Now the diamond which I use is a thin flake which intercepts only a fraction of the incident primary ray, a fraction which diminishes as the diamond is set at a greater angle to the primary beam in order to obtain the higher order reflections. It would appear, therefore, to be necessary to make allowance for this waste of reflection opportunities, a correction which would not be necessary in the case of the