

LETTERS TO THE EDITOR.

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Carpenter on Eozoon.

THE scientific world is deeply indebted to Dr. Dallinger for his excellent new edition of Carpenter's invaluable work on the microscope, and among other things for his retaining unchanged the description of *Eozoon canadense*, as a monument of an important research up to a certain date.

Dr. Carpenter devoted much time to the study of Eozoon, and brought to bear on it his great experience of foraminifer forms, and his wonderful powers of manipulating and unravelling difficult structures. After having spent years in studying microscopic slices of Eozoon and the limestones in which it occurs, I have ever felt new astonishment when I saw the manner in which, by various processes of slicing and etching, and by dexterous management of light, he could bring out the structure of specimens often very imperfect. Not long before Dr. Carpenter's death I had an opportunity to appreciate this in spending a few days with him in studying his more recently acquired specimens, some of them from my own collections, and discussing the new points which they exhibited, and which unhappily he did not live to publish. Some of these new facts, in so far as they related to specimens in our cabinet here, have since that time been noticed in my *résumé* of the question in the *Memoirs of the Peter Redpath Museum*, 1888; but I hope my friend Prof. Rupert Jones may yet be able to complete Dr. Carpenter's work.

Those who know Dr. Carpenter's powers of investigation will not be surprised that later observers, without his previous preparation and rare insight, and often with only few and imperfect specimens, should have failed to appreciate his results. One is rather surprised that some of them have ventured to state with so great confidence their own negative conclusions in a matter of so much difficulty, and requiring so much knowledge of organic structures in various states of mineralization. For myself, after working for fifty years at the microscopic examination of fossils and organic rocks, I feel more strongly than ever the uncertainties and liabilities to error which beset such inquiries.

As an illustration in the case of Eozoon: since the publication of my memoir of 1888, which I had intended to be final and exhaustive as to the main points, and in so far as I am concerned, I have had occasion to have prepared and to examine about 200 slices of Eozoon from new material: and while most of these have either failed to show the minute structures or have presented nothing new, a few have exhibited certain parts in altogether unexpected perfection, and have shown a prevalence of injection of the canal system by dolomite not previously suspected. Since that publication also, the discoveries of Mr. Matthew in the Laurentian of New Brunswick, and the further study of the singular Cambrian forms of the type of *Cryptozoom*, have opened up new fields of inquiry.

I think it proper to state, in reference to Dr. Dallinger's footnote on the recent paper of Mr. Gregory, that it must not be inferred from it that Mr. Gregory had access to my specimens from Madoc and Tudor, though he no doubt had excellent material from the collections of the Canadian Geological Survey. It might also be inferred from this note that I have regarded the Madoc and Tudor specimens as "Lower Laurentian." The fact is that I was originally induced in 1865, by the belief of Sir W. E. Logan at that time that these rocks were representatives in a less altered state of the middle part of the Laurentian, to spend some time at Madoc and its vicinity in searching for fossils, but discovered only worm-burrows, spicules, and fragments of Eozoon, which were noticed in the *Journals of the Geological Society* for 1866. (The more complete specimen from Tudor was found by Vennor in 1866.) On that occasion I satisfied myself fully that the beds are much older than the Cambro-Silurian strata resting on them unconformably; but I felt disposed to regard them as more probably of the age of some parts of the Huronian of Georgian Bay, which I had explored with a similar purpose under Logan's guidance in 1856. As my work was not official, and was paleontological rather than stratigraphical, it did not seem proper to express any dissent from what were at the time the probable conclusions of strati-

graphical work; but I was quite prepared to assent to the new views afterwards adopted.

In conclusion, the new material bearing on Eozoon is accumulating so rapidly that I cannot hope to be able to master it in detail, but shall be glad to aid others who may have more time; but I hope to be able, in a work now in preparation, at least to present the facts up to date in a popular form.

J. WILLIAM DAWSON.

McGill College, Montreal, February 3.

The Samoan Hurricane.

REPLYING to the communication in *NATURE* of December 17, 1891 (p. 161), signed "H. F. B.," relative to my preliminary report on the Samoan hurricane of March 1889 (published in the *Proceedings*, U.S. Naval Institute, vol. xvii., No. 2, and in the *American Meteorological Journal*, July 1891), I would submit the following statement.

First of all, I wish to acknowledge Mr. Blanford's appreciation of the difficulties involved in the consideration of the subject, owing to the meagreness of the data; and at the same time to express my own appreciation of the fact that he himself has not had access even to such data as we have succeeded in collecting, but only to my necessarily brief discussion thereof, and the conclusions that I have drawn therefrom.

Mr. Blanford's explanation, as I understand it, is as follows: the vortex of the hurricane formed north or north-east (on the equatorial side) of Apia on the afternoon of the 15th, within a "region of disturbance" that had already caused stormy weather throughout the Samoan Islands and a decided barometric depression at Apia. The first effect of this formation was (by adding slightly to the normal evening rise of the barometer) to cause a decided rise of pressure, which, however, decreased again as the vortex slowly approached the harbour, thus causing the second minimum (the afternoon of the 16th), the duration of the storm being explained by the usual slow motion of the newly formed hurricane.

To the above explanation it is necessary to make a correction, I think, owing to the fact that the shifts of wind at the time of and immediately after the first minimum show that the centre of the disturbance then passed to the westward of Apia, and as the wind thereafter remained from north to north-east, the centre (or vortex) evidently remained to the southward and westward. This fact, however, merely introduces a change in the position where the new vortex formed, according to the theory under discussion.

Revising Mr. Blanford's explanation, then, in the light of this correction, it appears that the track of the depression is about as I have drawn it, but that a vortex formed slightly to the southward and westward of Apia, thus causing a slight rise of pressure at first, succeeded by a second fall, and the slow motion of this newly formed vortex caused the duration of the northerly gale.

Now, I must here take exception to one of Mr. Blanford's statements, which is as follows (referring to the theories given in my paper): "None of these explanations seem to take account of the circumstances that attend the formation of tropical cyclones, which, as we have elsewhere pointed out, differ in many respects from the storms of the temperate zone." A reference to my paper will show, I think, that I took into consideration the special peculiarity to which Mr. Blanford calls attention, and went so far as to insert a plate in order to illustrate two types of storms—namely, (1) the characteristic tropical hurricane type (where there is a decided vortex, or "centre of aspiration"); and (2) the type where there is a comparatively wide central region surrounded by an annular space where there are steep barometric gradients and correspondingly high wind velocities, but without any decided vortex, properly so called. I said also that "it will be seen that the indications are that the Samoan hurricane (on the 15th and 16th, at least) was of the second type, although during the 17th and 18th it doubtless became more like the first." In a word, I said (both explicitly and by means of the varying strength of the track drawn on the chart) that the depression passed Apia on the afternoon of the 15th, recurved (increasing in intensity and delaying whilst recurving, each of which is to be expected), and then moved off to the southward and eastward. I do not intend to convey the impression that I made any definite statement as to just when or where the vortex formed, nor am I wholly prepared to hazard such a statement even at