has been misapplied to this specimen, which, as far as can be judged from the drawing, appears to be either *Ventriculites quincuncialis*, or one of the *Cephalites*, both quite different in outward appearance from the plain *simplex*. I know that it is often not so easy to distinguish the species of those preserved in flint as of those in chalk, but in this instance it is quite evident that it is not *simplex*.

My object in writing the above has been to vindicate my father's scientific accuracy, and to recall the facts he worked out. With regard to another point: it is stated by Prof. Thomson that some of the beautiful sponges discovered in the late deep-sea dredgings, especially the *Holtenia* and its allies, and the Ventriculites, "belong to the same family, in some cases to very nearly allied genera," or, as Dr. Carpenter puts it ("Good Words," October 1872, p. 703):—"Here we found the type of the old Ventriculites, which were supposed to be extinct, still living on in the deep sea." Much as my father would have delighted in the exquisite beauty of these new forms (the *Euplectella* he had examined in 1848), I do not think that he could have acknowledged the *Holtenia* as belonging to the ancient Ventriculidæ; nor, if the use of the word "type" depend for its force upon the character of structure, can it be truly said to be a type of that family. True, it possesses a silicious skeleton, but so does the Euplectella; and neither from Prof. Thomson's description ("Depths," pp. 70-72), nor my own examination, can I discover in the Holtenia any trace of or resemblance to the delicate structure and folded membrane of the Ventriculidæ. With great deference, therefore, to the opinion of these investigators (if I am wrong I will gladly learn), it appears to me that the modern type of the old Ventriculite is yet to be found.

I will add that the series of specimens figured in my father's book is in the Britith Museum, open to examination by students, together with a large portion of hi collection of the Ventriculidæ.

Highgate, Sept. 27 LUCY TOULMIN SMITH

"Deidamia"

I NOTICE in Prof. Wyville Thomson's extremely interesting papers the name *Deidamia* v. Willemoes-Suhm, used for a crustacean genus. This name must be changed, inasmuch as it is preoccupied in Articulata by Dr. Clemens in 1859. Dr. Clemens has used the title for a valid genus of North American Sphingidæ. I propose, therefore, for the genus in Crustacea, the name *Willemoesia*, in honour of its discoverer, with the two species *leptodactyla* and *crucifer*, the former the type.

AUG. R. GROTE,
Curator of Articulata, B.S.N.S.

Buffalo, U. S., Sept. 15.

Dr. Sanderson's Experiments and Archebiosis

IN a communication made to the British Association during its recent meeting at Bradford, Dr. Sanderson criticises the experiments of Prof. Huizinga, and also throws doubt upon the validity of the conclusions which I have drawn from experiments of my own. The "Note" appears in your columns this week; and seeing the nature of the conclusion drawn by Dr. Sanderson from his experiments, I am not a little surprised to find no mention in it of one most important point, viz., the temperature at which Bacteria are killed when immersed in fluids.

It must be obvious to all who understand the real nature of the question at issue, that no valid conclusion can be drawn by Dr. Sanderson from his experiments, unless he is able to argue from a definite conviction as to the temperature at which Bacteria are killed in fluids.

Now a study of Dr. Sanderson's writings would show the reader that up to the time of their publication he had every reason to believe that Bacteria were uniformly killed in fluids at a temperature of 100° C. If he still believes this to be true, he cannot (in the light of facts which he has learned concerning the productivity of previously boiled fluids in closed flasks) refuse his assent to my main proposition, viz., that Bacteria are capable of arising in fluids independently of living reproductive or germinal particles.

But the conclusion which Dr. Sanderson does draw from his experiments, and his imputation that facts do not warrant the conclusion of Prof. Huizinga and myself, would seem to imply that he is in possession of some new evidence subversive of his previous opinion, and tending to contradict views which I have recently published concerning the death-point of Bacteria in heated fluids. ("Proceedings of Royal Society," Nos. 143 and 145, 1873.)

As Dr. Sanderson is entirely silent upon this point, I venture to ask, both for my own information and for that of your readers, whether he still believes that Bacteria are killed by a temperature of 100° C. in fluids; and if not, upon what grounds he has changed his opinion?

In the face of his expressed intention (not a little contradicted, as I venture to think, by his public action) of taking no part in the "spontaneous generation" controversy, I ask Dr. Sanderson this question, because I cannot suppose that he would publicly throw doubt upon the validity of the conclusion which Prof. Huizinga and I have drawn from our experiments, in the absence of fresh evidence of his own upon the thermal death-point of Bacteria.

At present he has publicly expressed the opinion that we are not warranted in our conclusions, whilst he has given no sufficient information either to the world of science or to ourselves by which to test the correctness of his own conclusion. This seems neither just to us nor to himself.

H. CHARLTON BASTIAN

University College, Oct. 3

Mr. D. Forbes's Criticism of Mr. R. Mallet's Volcanic Theory

AFTER the lapse of half a year Mr. D. Forbes has recurred in NATURE for Sept. 4, 1873, to my remarks published in NATURE of March 20 last, to his remarks upon my Theory of Volcanic Energy and Heat contained in his review of my translation of Palmieri's "Incendio Vesuviano," which appeared in NATURE of February 6 preceding.

I pray your permission to make some remarks upon Mr. Forbes's last production. They are the last by which I shall prolong this unpleasant controversy.

Mr. Forbes affirmed that if anything was certain, it was that the ejecta of volcanoes in all ages and all over the world are *identical* chemically or mineralogically, and upon this assumption passes a summary condemnation upon my theory, which he predicts will never receive acceptance from anyone—chemist, or mineralogist, or geologist. This rash and I will now say discourteous prediction I at once disposed of by giving the names of two authorities, whose competence even Mr. Forbes could not question, who had already accepted my views.

To this Mr. Forbes now says, that, as these gentlemen possessed for their guidance in assenting to the bare statement of my views, no better information than that upon which he dissented from them, so they may have been mistaken and not he. How is Mr. Forbes sure they had no better information, and can it be possible that he is so dull in weighing the force of evidence as to see no difference in probability of error between two assumed equally competent men—one of whom can assent to a proposition upon his prior knowledge and without waiting for proof; and another, who dissents, before he has heard what can be advanced in favour of the proposition and against his own previous knowledge or supposed knowledge? This, however, is now immaterial except as an indication of Mr. Forbes's capacity for weighing evidence.

To Mr. Forbes's grand objection I replied that it is based upon error as to fact—that it is not true that all volcanic solid ejecta are identical at all times and everywhere.

While I denied, and do again deny, that identity, chemical or mineralogical, exists in those bodies, I admitted that they do present a great general resemblance—which is just what we should expect.

I added a very important remark, namely that whether it were true or false that all volcanic ejecta were identical, chemically or mineralogically—the fact, whether one way or the other, did not apply to or affect my theoretic views as to the nature and origin of volcanic energy and heat; one way or the other, the identity or dissimilarity between the ejecta as found at the surface must be the same, whether they be derived from materials already and constantly in fusion, or be fused by elevation of temperature locally and temporarily produced; the materials fused being the same in both cases.

This last objection, which is fatal to Mr. Forbes's criticism, whether the foundation on which he has rested it be true or false, he either has not noticed or finds it convenient now to ignore.

I illustrated the want of identity, chemical or mineralogical, and yet the great general similarity at all times and places of