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 volcanic ejecta, by the analogy of the blast furnace, in which the same materials in the same proportions do not even in any one furnace, or at all times, produce identical slags.

What is Mr. Forbes's reply? That the *intention* of the iron master is to produce slags always the same, as the indication that the furnace is working well.

Doubtless the intention and desire of the iron-master is to produce good iron, and at all times as nearly as he can such a slag as indicates that he is doing so. But, as a matter of fact, he is not able to reach this. He can only approximate to constancy in the chemical or mineralogical constitution of his slags, which are never identical, even for short periods. Is this substitution of the intentions of the iron-master for the actual facts of the blast furnace slags, on Mr. Forbes's part, worthy of the candour of the searcher for truth; or does it not rather resemble the dialectic wriggle of the advocate?

Complete identity between any two rocky masses, ejected or otherwise, can only exist where the same elements in the same proportions are combined in the same way, and in the same molecular aggregation. If the mere presence in greater or less proportion in the mass, of certain crystallised minerals in any variable proportion, such as felspar, pyroxene, or leucite, in the magma of lavas, were enough to constitute identity, then nearly all the known rocks of the world, crystalline, igneous, and sedimentary, might be viewed as identical, for all consist of a few elements and of a few prevailing simpler minerals.

While still seeming to maintain his original statement, Mr. Forbes now substitutes for *identity*—a great *similarity* in all volcanic rocks. Further discussion is therefore needless—nor indeed would discussion of my views as to volcanic heat, &c., lead to any good result—with a gentleman whose notions of scientific method are such, that after six months' consideration he holds any distinction between hypothesis and theory to be mere hair-splitting, and whose notions of physico-mechanics are of that confused character, that he views pressure and work to be quite the same, and that it is matter of indifference whether we talk of "pressure converted into its equivalent, heat," or of work transformed into heat.

Would Mr. Forbes enlighten your readers by stating in figures what is the equivalent in heat, of the pressure of a weight of ten pounds, resting upon a rigid level plane?

Were Mr. Forbes of any real authority upon volcanic subjects, there might have been more ground for his sweeping and anticipatory condemnation of my views as to volcanic energy, which, however, in that case, he would never have uttered; but on looking down the list of his published papers, I do not find any treating of vulcanology simply, nor am I aware that he has ever enlarged the boundaries of our knowledge in that department by a hair's breadth.

Mr. Forbes appears to think that chemists, mineralogists, and geologists are the sole arbiters of all questions as to the nature and origin of volcanic heat and energy. Whatever they may have done to add to our knowledge of the visible and tangible phenomena of volcanic vents or cones, they have as yet contributed really nothing to discovering the nature and origin of volcanic heat itself, if we except some valuable negative evidence drawn from the gaseous emanations by chemists of late years, subversive of the older theories of the chemical origin of volcanic heat, still not quite extinct. It is much more to the physicist and theoretic mechanician dealing largely with the *physique du globe*, that we must look for further light, and whose province it will be to decide when the right key shall have been found to that enigma of ages, the true nature and origin of volcanic heat and energy.

I am done, sir, with this controversy, unwillingly entered upon, not in irritation, as Mr. Forbes states, but because I felt justified in protesting against new and I believe important views being obscured *in limine*, by objection based only on error.

My paper containing those views will ere long be before the world. My 100 separate copies (as author) from the "Phil. Trans." are already in the hands of or on the way to many men of science. The volume itself of the "Transactions" will no doubt appear before the end of the year, and to the verdict of the real men of science of the world, versed in the subject and competent to judge of it, I leave the result.

London, Oct. 6 ROBERT MALLET

## On the Equilibrium of Temperature of a Gaseous Column subject to Gravity

FROM Mr. Clerk-Maxwell's reply to my note on this subject which appeared in your columns a short time since, it would appear that he does not profess so much fully to explain the difficulty suggested by me as to show that it is capable of explanation, referring your readers to his other works for further information. I would not, therefore, have troubled you further on the subject had it not occurred to me on reading Mr. Maxwell's letter that I could state the case in such a way as to render clearly apparent the grounds for taking different views on this point.

Let a vertical column of gas, subject to gravity and in a state of equilibrium as to pressure and temperature, be divided by a horizontal plane P into two parts, A above and B below.

In the time $\Delta t$ let a mass $M_1$ of particles pass in their free course from A to B, and a mass $M_2$ from B to A.

Let the portion of A from which the particles composing $M_1$ proceed be called the upper stratum, and the corresponding part of B the lower stratum, then the following consequences may be deduced :—

1. From the equilibrium of density

$$M_1 = M_2$$

2. From the equilibrium of temperature the amounts of work in $M_1$ and $M_2$ while passing through P are equal.

3. From the effect of gravity the work in M while in A reckoning from the commencement of the free course of each particle composing $M_1$, is less than at P, while that in $M_2$ is greater.

4. Whence it follows that of the two equal masses $M_1$ and $M_2$ in the upper and lower strata respectively $M_1$ contains less work than $M_2$.

5. The work in $M_1$ while in the upper stratum reckoned as before, is the same as that of any other equal average mass in that stratum, and the same is the case also of $M_2$.

6. The average amounts of work in equal masses in the two strata, and the consequent temperatures of the strata are unequal, the lower stratum having the higher temperature.

I suppose Mr. Maxwell would deny the truth of statement (5). I presume he would argue as follows :—

"Of all the particles in the lower stratum which in the time $\Delta t$ have at the commencement of their free course a velocity and direction such as would take them through P, gravity in selecting those which compose $M_2$ excludes those whose velocities are insufficient to overcome the effects of their weights, while in forming $M_1$ particles of low velocity are selected (included?), which, but for the effects of gravity, would not have cut P in their free courses, consequently the particles in $M_1$ have an average velocity less than that of the upper stratum from which they come, while the particles of $M_2$ have a greater average velocity than that of the lower stratum, and consequently the inequality of the average velocity of the particles in the two strata cannot be inferred from the inequality of the average velocities of the particles composing $M_1$ and $M_2$ while in those strata."

This argument, therefore, assumes the theory that in a given mass of uniform temperature there are particles moving with every velocity from nothing upwards to a certain limit, and mixed in certain proportions. That this is actually Mr. Maxwell's view I own I might have remembered, but I suppose I overlooked it from an impression in my own mind that the molecular motion was to be regarded as being of a planetary (or in the case of gases a cometary) nature. That in masses of the same temperature velocities were to be regarded as practically uniform, except in so far as affected by the distance of the particles apart, and that the so-called impacts of particles were more properly to be regarded as perihelion passages of bodies moving among each other in hyperbolic orbits. If this view is the more accurate one, then obviously the argument which I have assumed that Mr. Maxwell would use, falls to the ground.

Is there no possibility of testing the nature of the thermal equilibrium of a column of still air? The result would at any rate throw an unexpected light on the nature of molecular motion.

Graaff Reinet College, July 19 F. GUTHRIE

## The Sphygmograph

DR. GALABIN, in his letter published in your last number, criticises my explanation of the cause of the small wave in the first part of the sphygmograph trace, which he calls the tidal wave. In his criticism he does not take into consideration the hæmodromograph traces of Chauveau, on which my explanation