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 MALLETT

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 Δt 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 Δt 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 Reinnet 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