

which, except the fifth, is incontrovertible. He has himself pointed out that it is here that we differ, and that this difference may ultimately be traced to a difference in our doctrine as to the distribution of velocity among the molecules in any given portion of the gas. He assumes, as Clausius, at least in his earlier investigations, did, that the velocities of all the molecules are equal, whereas I hold, as I first stated in the *Phil. Mag.* for Jan. 1860, that they are distributed according to the same law as errors of observation are distributed according to the received theory of such errors.

It is easy to show that if the velocities are all equal at any instant they will become unequal as soon as encounters of any kind, whether collisions or "perihelion passages" take place. The demonstration of the actual law of distribution was given by me in an improved form in my paper on the Dynamical Theory of Gases, "*Phil. Trans.*" 1866, and *Phil. Mag.* 1867, and the far more elaborate investigation of Boltzmann has led him to the same result. I am greatly indebted to Boltzmann for the method used in the latter part of the sketch of the general investigation (see p. 535) which was communicated in a condensed form to the British Association on Sept. 20, 1873.

J. CLERK-MAXWELL

Mallet-Palmieri's Vesuvius

As I am assured that it would be most undesirable as well as unbecoming of me to continue a scientific controversy in the tone of Mr. Mallet's letter which appeared in *NATURE* of October 9, I would only beg those who have perused it to remember that my remarks were altogether directed to the assertions contained in Mr. Mallet's introductory sketch, and not comments upon his theory of volcanic energy of which, as he himself admits, we as yet know little or nothing. I would then ask them to compare its contents with the substance of my letter in *NATURE*, Sept. 4, and judge for themselves whether so far from its being any answer to my arguments, it does not, on the contrary, furnish additional "evidence of his confounding chemical constitution with percentage composition, &c.," the very keynote of this discussion.

Mr. Mallet writes—"Mr. Forbes appears to think that chemists, mineralogists, and geologists are the sole arbiters" of such questions; a remark he could not have made had he read some of my publications; yet I am quite willing to admit that I do place more faith in them collectively, than in any one physicist or mere mechanic whether theoretical or practical; and I believe I am correct in asserting that no theory of volcanoes will be accepted by the scientific world until its doctrines are proved to be fully in accordance with the facts brought forward by these sciences.

When the reasons for my delay in answering Mr. Mallet's criticisms were fully stated, is it not, to say the least, most unjust of him to harp on this string, after having already taken more than a month to produce a rejoinder the reverse of an answer, and the style of which, peculiar to himself, is in complete harmony with that of his introductory sketch, of which one of his favourable reviewers writes—"We do not cordially approve of his method of dealing with other writers. There is, if we may be excused the expression, a tone of bitterness all through his writing which gives the reader a most uncomfortable sensation, and leads a person altogether unbiassed to imagine a feeling of jealousy on the part of so distinguished a writer as Mr. Mallet which we are sure cannot exist in reality. After giving a sketch of the various authors who have ventured to give different and erroneous opinions on the subject of vulcanicity," &c. Another reviewer remarks that—"While objecting to most of the views of geologists, which, however, he frequently distorts, Mr. Mallet claims the character of physical truth for his own ideas," and adds, "what we chiefly object to in this portion of the volume is the assumption on Mr. Mallet's part of a conscious superiority to others, and a freely expressed contempt for all previous observers, especially for geologists." Need I add more?

DAVID FORBES

11, York Place, W. Oct. 20

Oxford Science Fellowships

As Mr. Perry's letter, in the last number of *NATURE*, contains assertions calculated to impede the progress of science here by deterring persons, not graduates of Oxford, from competing for appointments in colleges, and also involves charges of, to say the least, discourtesy to himself, I trust you will find space in your next number for the following explanation.

First, as to Mr. Perry's general assertion respecting fellowships. From the fact that a graduate of Belfast is ineligible for a Fellowship in *Merton College*, Mr. Perry infers that "outsiders are ineligible for *Oxford* Fellowships in Physical Science." This is clearly illogical, and it is also untrue.

Secondly, as to the special case of Mr. Perry.

The ordinances of Merton College state that "no person shall be eligible" for a fellowship "who shall not have passed all the examinations required by the University for the degree of Bachelor of Arts." It appears a possible interpretation that Cambridge and Dublin B.A.'s, who can at any time incorporate in this University, may be candidates. If this be so, the reply of the Warden of Merton, as Mr. Perry gives it (of the actual correspondence I know nothing), may be correct, though perhaps not sufficiently explicit. This, however, is a legal question, and the college is taking steps to obtain the opinion of an eminent counsel.

Mr. Perry was not left, as his letter would naturally lead readers to infer, without warning as to this difficulty; for in July I wrote to Mr. Perry strongly expressing my doubt as to his eligibility, but as I was away from Oxford I could not quote the words of the ordinance; I advised him to consult the sub-warden, but I believe he did not follow my advice.

Mr. Perry received my letter, and replied to it on July 27.

The great difficulties which Mr. Perry asserts to have been thrown in his way, simply arose from the fact that he only proposed to come to Oxford during the vacation. Now it is not to be expected that I should allow any person who chooses to apply to overhaul the physical apparatus of the University in my absence, and it is unreasonable to suppose that, to suit the convenience of such a person, I must give up engagements made long before, in order to assist him in a candidature for an office of emolument in a college.

It must be borne in mind that there are nineteen colleges, any one of which may at any time offer a fellowship for proficiency in physics, and consequently to have to be at the service of outsiders, who may wish to be candidates, during the long vacation (the only time I have for real study) might become a serious matter, and to ask for such assistance seems to me to make a most unreasonable request.

I must add that if Mr. Perry imagines he would have been at any appreciable disadvantage by not knowing the particular instruments in the University cabinet (which it is by no means certain would be used for a college examination), either he assumes that the examiners would be guilty of the absurdity and unfairness of puzzling candidates by new or peculiar apparatus, or he feels very uncertain about his own practical knowledge.

A Cambridge B.A. is a candidate for the Merton Fellowship, and I have every reason to think that he found the Oxford candidates on exactly equal terms with himself in the practical examination.

H. B. CLIFTON

Oxford, Oct. 18

P.S.—Since writing the above I have been informed that a Cambridge graduate has been elected to a Science Fellowship in Magdalen College, Oxford. This is a proof of the inaccuracy of Mr. Perry's statement as to the ineligibility of outsiders for Oxford Fellowships.

Harmonic Echoes

I BELIEVE the echo observed by W. J. M. is of a different nature from mine and more analogous to one described by Oppel (*Pogg. Ann.* xciv. 357, 530). Each bar of the railing, when struck by the aerial pulse, diverts a small portion, which is scattered in all directions, much as if the bar were itself the source of sound. These derived pulses reach the ear of the observer at approximately equal intervals, and accordingly blend into a musical note, whose pitch, however, may not be quite constant. Oppel discusses the effect of different positions of the original source and the observer with respect to the grating, on which alone the pitch and its variations depend. It is evident that an echo formed in this way is in no sense *selective*.

I have been asked several times how the Bedgebury echo would be affected by the *character* of the original sound. Of course, if my theory is correct, the octave could not be returned, unless it were originally present; but the intensity of the echo was too feeble to give any promise of a successful observation with such an instrument as the clarinet. The experiment would be most interesting if a more powerful echo of the same class can be found.

RAYLEIGH

Terling Place, Witham, Oct.