I have to thank you for sending me your paper on the Elevation of Mountains, which I have read with great interest. You and Mr. Mallet have done great service to geology by exploding the old-fashioned idea of cavities existing in the interior of the earth. I quite agree with you that a cooling earth must give rise to great pressure in the outer consolidated layers, and that this pressure must crush the rocks composing it; but I cannot think that this crushing is the cause of the elevation of mountains. My reasons for disagreeing with you are the following:—

1. The pressure from a shrinking globe must be uniform, and the lines of least resistance, once chosen, should remain always the same, and the elevation should be continuous. All minor differences would be insignificant in comparison with the flatter arch at the poles. These areas, therefore, would subside, and mountain chains should have had from the first an east and west direction. I see no provision for changing the localities of movement.

2. Where deposition was going on the rocks would be heating and no contraction could occur below them. But mountain chains have been always formed where the deposits were the heaviest, and where, therefore, uplifting would not be likely to occur.

3. All mountain chains are not formed on the same system, but can be divided into two groups, as I have pointed out in my lecture on this subject.

4. Whether a glacial epoch has ever extended over the whole earth or not, it is certain that the northern parts of America and Europe are much warmer now than they were in the Pleistocene period, consequently the rocks under them could not have contracted, and yet we know that extensive movements are even now going on in this area.

5. In order to produce a strain on the surface, the lower contracting rocks must be solid, consequently there would be nothing to support a large anticlinal, and no rocks to pass into the liquid state; the result would be a general small crumpling all along the surface. The relief also to the compression of the upper rocks could not be obtained by a single rising at a point, or along a line, without a horizontal movement of one bed over another, which appears to me to be impossible. Consequently I do not think that the shrinking could produce the observed effects, more especially as the Himalayas, &c. are of tertiary age, and the contraction of the globe, since the cretaceous period, cannot have been very great. These remarks apply also to Prof. Shaler's theory (Proc. Bost. Soc. Nat. Hist. 1866). Mr. Medlicott's section of the Himalayas is, to my mind, physically impossible. It is inconceivable that the beds could be engineered into the positions in which he has placed them.

6. The theory does not account for the numerous minor oscillations of level that coal measures often prove to have taken place.

7. The theory makes no provision for tension in the rocks. But it is a fact not sufficiently dwelt upon by geologists, that faults just as surely prove tension in rocks as contortions prove compression.

I have also a few objections to your theory of Volcanoes, and also to that of Mr. Mallet. They are as follows:—

1. The density of the crust has been shown by General Sabine to increase in volcanic regions, while, by your theory, it should decrease. Mr. Mallet's theory would account for this, as also would the one proposed in my lecture.

2. To cause a volcano the heat must go to the water, for the water cannot go to the heated rock, as your theory would require.

3. Volcanoes are not found in contorted countries, or where great lateral pressure has existed. In the older volcanic districts (*e.g.* North Wales) the eruptions occurred before the folding of the strata. This is also a strong point against Mr. Mallet's theory.

4. By Mr. Mallet's theory the crushing must be very sudden, or the heat would be conducted away, and as each eruption would require a fresh accession of heat, it ought to be preceded by elevation or subsidence on a large scale. The earthquakes that precede eruptions are just as likely to be effects as causes.

5. Faults show no heating where considerable crushing has taken place.

Such are the objections that occur to me, but, after all, we cannot well burke the question as to the state of the interior of the earth, and I must confess that the "Viscidists" appear to me to have a better position than the "Rigidists."

Mr. Hopkins' argument, drawn from precession and nutation, has proved untenable, and the only stronghold that the "Rigidists" now retain is the absence-of-internal-tide argument of Sir W. Thomson. This has not yet been assaulted, but it probably has a weak point somewhere, for its author has allowed that the interior of the earth is probably "at, or very nearly at, the proper melting temperature for the pressure at each depth," which seems hardly consistent with its being "more rigid than glass." On the other hand, the "Viscidists" have a very strong point in the fact that faults are known with throws of several thousand feet (which apparently must penetrate into some yielding material), as well as some minor positions, such as the supposed effect of the moon on causing earthquakes, the composition of volcanic rocks (which contain more alkali than could be obtained by merely melting sedimentary rocks), and the mode of occurrence of granitic rocks, none of which have been seriously attacked by the "Rigidists."

At this distance I cannot take part in a discussion, as I must always be five months behind hand, but if you think that a preliminary skirmish in the pages of NATURE would do good, although it did not bring on a decisive battle, you are quite welcome to publish this letter.

F. W. HUTTON

Wellington, N. Z., July 21

P.S.—At the time of writing my paper on Elevation and Subsidence (*Phil. Mag.* Dec. '72), I was not aware that Mr. Scrope had been the first to suggest* the theory there developed, or I should certainly have mentioned his name, and not proposed to call the theory after Herschel and Babbage. I feel that I owe Mr. Scrope some apology for my inadvertence.

* "Volcanoes," 1st ed. 1826, p. 30.

Deep-Sea Sounding and Deep-Sea Thermometers

WE have again to claim your indulgence for occupying space for a few comments on Mr. Casella's reply to our letter.

It is not true that we abstained from drawing attention during the lifetime of Dr. Miller to the fact that he had plagiarised our invention; on the contrary, we wrote to Dr. Miller as soon as we were told that he had read a paper before the Royal Society on his supposed invention, and we have before us Dr. Miller's answer, dated Nov. 23, 1869, wherein he writes:

"I am sorry if I have inadvertently done anything which may fairly be considered an injustice to you in respect to the deep-sea thermometer," &c.

We believe Dr. Miller did not know of our thermometer, but Mr. Casella did, having had one or more in his possession years previously, and as a fact our thermometer was well known in the trade; therefore he as the workman employed by Dr. Miller ought to have acquainted that gentleman with the fact. It is most likely that we should not have taken any further notice had the thermometer retained the modest title given to it by Dr. Miller, viz. the "Miller-pattern." This, however, did not suit Mr. Casella. Mr. Miller died—"mors tua vita mea,"—and forthwith the thermometer is styled the Miller-Casella, then by a little "progressive development," the instrument is brought out at the British Association as the Casella-Miller, and to day we have it in Mr. Casella's letter as "*my thermometer*."

On reference to the Royal Society's Proceedings, vol. xvii. p. 482, we find no mention of Mr. Casella's name except as the workman who took Dr. Miller's instructions, and we have yet to learn what right a workman has to appropriate to himself an instrument made for Dr. Miller, or any other customer, supposing, even for argument's sake, that we had no priority in its invention.

Mr. Casella asks "What has Negretti and Zambra's thermometer done that it should be known?"

In the first place it served him as a pattern, it showed him how the best deep-sea thermometer was constructed, and how to make others on the same principle; and we contend that had our instruments been placed in the hands of skilful, careful, and trained observers, such as are now engaged in the *Challenger* Expedition, they would have given results equal to those now obtained with the instruments supplied by Mr. Casella, and obviously so, their principle being precisely the same.

Mr. Casella talks about our thermometers having failed. Can Mr. Casella point out where are recorded any of the failures? Was Mr. Casella able to make them fail when he tried by placing one of them in his hydraulic press in the presence of gentlemen connected with the Meteorological Office? But this is not the point at issue, the sole question is, are the thermometers supplied to the expedition the same in principle as ours, or are they not?

Doubtless it would be much more agreeable to Mr. Casella that these questions should be decided by himself in private, hence his invitation to your readers "to go to his establishment