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.

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

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

and hear his explanation." Surely no such arrangement will satisfy "all the scientific men in the world." We contend that as Mr. Casella has publicly claimed the invention as his own, it ought to be decided with equal publicity whether he has done anything more than copy our instrument.

We again give the description of our thermometer (not in our own words, for we might be accused of shaping them to suit our purpose) but in the words of the late Admiral Fitzroy as they appear in the first number of Meteorological Papers, page 55, published July 5, 1857, in referring to the erroneous readings of all thermometers consequent on their delicate bulbs being compressed by the great pressure of the ocean, Admiral Fitzroy says:—

"With a view to obviate this failing, Messrs. Negretti and Zambra undertook to make a case for the weak bulbs which should transmit temperature but resist pressure. Accordingly, a tube of thick glass is sealed outside the delicate bulb between which and the casing is a space all round which is nearly filled with mercury. The small space not so filled is a vacuum into which the mercury can be expanded, or forced by heat or mechanical compression, without doing injury to, or even compressing the inner or much more delicate bulb," &c. &c.

Mr. Casella "did not wish to take up your valuable space to describe his thermometer." Well, it matters not; the late Admiral Fitzroy has done it for him. He described it sixteen years ago; and if the reader will take every syllable of the extract above quoted, and substitute the word "alcohol" for "mercury" (which colourable change was effected by Mr. Casella, to the detriment of the instrument), they will have a correct description of Mr. Casella's thermometer in the most minute details.

HY. NEGRETTI AND ZAMBRA

Rain-gauge at Sea

I BEG to send you a copy of a letter I received lately from Capt. Goodenough, of the Royal Navy, respecting the use of my rain-gauge at sea. (See NATURE, vol. vii. p. 202.)
Nov. 8 W. J. BLACK

"H.M.S. *Pearl*, lat. 6° S., long. 22 W.

"Dear Sir,—I should have taken an earlier opportunity of writing to you about the instrument which you were so good as to design for use on board ship, but have not had the good fortune to fall in with any rain up to the present time with which I could at all events in some measure test and chronicle the rain-gauge. It is odd that in a journey of twenty days I have had only '07 in. of rain, and that although I am at this moment in a district in which an average of seven hours' rain usually falls at this time of the year. On that one occasion '07 in. did fall and was duly caught in your instrument as well as in another mounted on gimbals, the measurements being exactly alike in each. I much prefer the mounting of your instrument, and will report to you as to the amount of weight it requires after some experimenting with it. The usually most steady instrument is one which is heavy, and whose centre of gravity is very near its centre of oscillation. I do not think it would be well to increase the size of the instrument, as it would become inconvenient to place, except for the use of a man who wishes to devote himself very much to that order of observation. Our poop is so high here that I do not anticipate any mixture of sea-spray in the gauge, but if it were so your table would be sufficient to clear it, supposing we had Carpenter's Hydrometer to test with, as we might not expect enough water to float an ordinary one.

"I remain, yours very truly,

"JAMES E. GOODENOUGH

"Captain R.N. Command H.M.S. *Pearl*, proceeding *via* the Cape to Australasia."

Glaciers

In a letter printed in your number for Oct. 16 (vol. viii. p. 506), Mr. J. H. Röhrs states that he believes that glaciers existed at or near the sea-level in central Hindustan in the glacial period. Glaciers undoubtedly existed in the Himalayas at a much lower elevation than at present; there are traces of their action in Sikkim in valleys, the bottoms of which are now only 4,000 ft. above the sea, and in the north-western Himalayas, Mr.

Medlicott, I think, considers that in some valleys, glaciers descended to within 1,000 ft. of the sea-level, but I have never heard of any marks of old glacial action in the Indian peninsula south of the Himalayas. There are no mountains in central Hindostan exceeding about 4,000 ft. in height, and a careful examination of the portions of the Nilgiri mountains in Southern India, which rise above 8,000 ft., has not afforded any proof of the former presence of ice. It is very probable that Mr. Röhrs possesses information upon this subject with which I am unacquainted, and it is without the least wish to express a doubt of the accuracy of his information, that I ask for any evidence he can produce in favour of his assertion, as the subject is one in which I am greatly interested.

W. T. BLANFORD

JOHANN NEPOMUK CZERMAK

JOHANN NEPOMUK CZERMAK was born June 17, 1828, in Prague. His father, Johann Conrad Czermak, was a medical practitioner of high repute in that city, and his uncle, Joseph Julius Czermak, enjoyed a considerable reputation as Professor of Medicine and Physiology, first at Gratz and afterwards at Vienna. Educated at the high school of his native town, Johann Czermak entered upon the study of medicine at the University of Vienna in 1845. In 1847 he moved to Breslau, where he had the great advantage of living with the distinguished physiologist Purkinje. From Breslau he passed on in 1849 to Würzburg, where in 1850 he received the degree of M.D., publishing on that occasion an inaugural dissertation on "The Microscopical Anatomy of the Teeth," in which he called attention to the larger "interglobular" spaces so often found in the upper part of the dentine. After a visit to England he settled at Prague, where he became assistant to Purkinje, who then held the chair of Physiology in that place. In 1855 he left Prague to take the chair of Zoology at Gratz; but zoology was not his proper province, and he gladly accepted in 1856 the offer of the Professorship of Physiology at Krakau, which however he left in the following year for the like chair in Pesth. In both these universities he established physiological laboratories and gave a decided impulse to physiological research; but the political agitations then rife made life distasteful to him there, and in 1860 he resigned his chair and returned to Prague. Such frequent changes must have interfered greatly with sustained research, but by this time Czermak had made his name known as well by several investigations in experimental physiology and in subjective vision, as especially by his researches on the laryngoscope, his treatise on which ("Der Kehlkopfspiegel und seine Verwerthung") embodying the results made known in various papers in 1858 and 1859, he published shortly before his return to Prague.

Here he resided some years, visiting at times England, Holland, and France, in order to make the value of the laryngoscope better known to his fellow-workers in science and medicine. There are many in England who retain pleasant memories of these visits.

The ample means brought to him by the gifted lady whom he had the happiness to marry, enabled him to build in Prague and furnish at his own expense a private laboratory for research, in which he not only worked himself, but which he also placed at the disposal of others. Many would have envied, and few would willingly have let slip, such an opportunity for quiet labour; but Czermak, conscious of the power he possessed of lucid exposition, delighted in teaching, and felt perhaps the want of the stimulus which pupils afford. Accordingly, when in 1865 he was offered the chair of Physiology in Jena, vacated by the removal of von Bezold to Würzburg, he at once accepted it. Here he continued until, in 1869, finding the disease to which he eventually succumbed (and the beginning of which he himself attributed to the irritation caused by the