

tained some better tokens of consciousness than the greater or less resemblance of the movements in question to such movements as our conscious selves are in the habit of executing.

M. FOSTER

Meyer's Exploration of New Guinea

Few persons can have read Dr. Meyer's account of his recent adventurous and very successful journey with more interest than myself; but I confess I was surprised to find that the translator of my book should have misunderstood what I had stated, and so create a difference between us where none exists. He says (speaking of Dorey) that I "have not given a correct impression of the natives of the surrounding hills and mountains, separating them in some way from the inhabitants of the coast, as smaller, uglier, not mop-headed," &c.; and that he finds on the other hand, that "there is no generic difference at all between the Papoos of the mountain and the Papoos of the coast, except such differences as we find everywhere between the highlanders and coast inhabitants of the same race." Now I say exactly the same thing: "From these (sketches) and the captain's description, it appeared that the people of Arfak were similar to those of Dorey." ("Malay Archipelago," 3rd Ed. p. 505.) Dr. Meyer however, probably refers to what I say of the people of one hill village, close to Dorey: "The inhabitants seemed rather uglier than those at Dorey village. They are, no doubt, the true indigenes of this part of New Guinea, living in the interior, and subsisting by cultivation and hunting. The Dorey-men, on the other hand, are shore dwellers, fishers, and traders in a small way, and have thus the character of a colony who have migrated from another district. These hillmen, or Arfaks, differed much in physical features. They were generally black, but some were brown like Malays. Their hair, though always more or less frizzly, was sometimes short and matted," &c. (p. 499). I can only suppose that the word "differed" in the above passage was taken to mean "differed from the Dorey people," whereas the context shows that it means "differed among themselves," or varied, which would have been a better word. In the preceding page I have stated of the inhabitants of Dorey: "The majority have short woolly hair;" so that there is no difference from them in that respect. In all I have written about the Papuans I have maintained that the people of New Guinea and of all the immediately surrounding islands are of one race, with very unimportant local differences; and I do not think my remark, that the people of one village were "rather uglier" than those of another, three miles off, justifies the idea that I supposed there was any "difference," in an ethnological sense, between them. I cannot find that I have said a word about difference of stature.

The great success of both Messrs. D'Albertis and Meyer in penetrating inland in New Guinea will, it is to be hoped, induce other travellers to attempt the exploration of the far larger and less known southern portion. Two Europeans, with a small steam launch and a Malay crew, would, no doubt, be able to penetrate a long way up some of the larger rivers, and establish a station from which exploration of the central mountains might be effected. There is now no portion of the globe so completely unknown as this, or which promises such great results for every branch of Natural History.

ALFRED R. WALLACE

Deep-sea Sounding and Deep-sea Thermometers

WITH reference to the discussion which has recently been carried on in NATURE as to the deep-sea thermometers, I hope that perhaps the following statement may tend to put the matter at rest.

One of Negretti's thermometers was exhibited at the Royal United Service Institution at a lecture, March 11, 1859, by Admiral FitzRoy, who then spoke of them "as thermometers peculiarly constructed, self-registering," &c. The construction of these thermometers had been fully described in the "First number of Meteorological Papers, 1857," and was subsequently given in a "Treatise on Meteorological Instruments," published by Negretti and Zambra in 1864. The peculiarity of these thermometers was mentioned in the Hydrographic Instructions to Captain Dayman of the *Cyclops* Sounding Expedition, dated May 29, 1857. These facts are sufficient to show the ample publication of the device in question for protecting the bulbs against pressure.

I know from Dr. Miller himself that he did not know of Negretti's plan. In his paper in the Royal Society Proceedings,

he calls the one which he describes a "simple expedient." I am not aware of any just claim on the part of Mr. Casella to the principle of the invention.

I consider that the practice of instrument makers designating by their names instruments which they have not *invented*, is most reprehensible.

ROBERT H. SCOTT

London, Dec. 9

[We have received a letter on this subject also from Mr. Casella, but as there is nothing in it bearing on the real point at issue, we do not print it. The above letter from Mr. Scott renders it clear to us, and it will doubtless be also clear to our readers, that the whole credit of the double bulb belongs to Messrs. Negretti and Zambra. We quite agree also with Mr. Scott's closing remarks. This correspondence must now cease.—ED.]

The Dutch Photographs of the Eclipse of 1871

ABOUT a year ago Dr. Schellen kindly sent me two paper copies of the Java photograph, one of them was stated to be of the size of the original negative and the other was an enlargement of about ten and a half diameters, with a delicately soft outline and much detail in the corona. On comparing this with the Indian photographs I found that though the outline of the corona corresponded depression for depression with the two Indian series, yet there was great difference in the detail of the lower parts. The question therefore arose, Was such difference to be regarded as proof of enormous change in the corona in the course of about an hour, during the passage of the totality shadow from India to Java?

I had carefully compared and catalogued the details visible upon the original negatives of the two Indian series, and had found no structure in the one that could not be traced in the other, but the details of the new Java photograph were quite of a different character, lumpy, and in more definite masses. On mentioning this to Lord Lindsay he informed me that he had other copies of the Java negatives which he had received directly from Prof. Oudemans and which were almost structureless. Mr. Davis undertook a critical comparison of the two Java photographs, and pointed out that in spite of the striking dissimilarity of the paper prints, they were evidently both taken from the same original, for they each showed a faint scratch and three minute photographic flaws in the same relative positions. It was impossible to assert that the one was a good print and the other a very bad one, for in the photograph with the delicate corona the moon's limb was soft and hazy, while with the poor corona the limb was perfectly sharp and definite. We had only one course left, and that was to infer that the softening and details had been produced artificially. Having detected manipulation in the corona, we naturally suspected it in the moon's limb, and thus arose my remark at the meeting of the Astronomical Society, that the sharp edges of the irradiation under the prominences might have been artificially produced by stopping out the moon, or rather by stopping out the hazy irradiation which presents so marked a feature, especially under the prominences in the Indian photographs, as well as in those taken in 1870.

There is still a little mystery which requires clearing up about the hazy irradiation. No trace of it is to be found in the copies of the Shelbyville photograph taken by Mr. Whipple in 1869, nor (as we now learn) in the Java photographs, although the action of the light has been greater in these than in some of the Indian and 1870 negatives, which show it as a very marked feature. We know that under ordinary circumstances hazy irradiation is produced by reflection at the hinder surface of the glass on which the photograph is taken, and that its amount may be greatly reduced by backing the plate, during its exposure, with wet paper, so as to produce a film of water instead of a film of air immediately behind the plate, thus causing nearly all the light to be transmitted instead of reflected at its back surface. Yet the Baikul photographs (and I understand also the Cadiz photograph of 1870) were backed with wet paper, and still show the irradiation very markedly.

The cause of the ellipticity of the dark moon touched upon by Prof. Oudemans seems to me to involve some very interesting questions. It is remarkable that the ellipticity does not occur in all eclipse photographs. After making allowance for the moon's motion during 40 seconds in the enlargement from the Cadiz negative, I may say that I have not been able to detect any difference between the polar and equatorial diameters in *any* of the 1870 photographs.

In No. 2 of the glass copies from the Ottumwa photographs, 1869, the moon is also apparently quite circular; but in No. 4, where the bright depths of the chromosphere are just appearing, the polar diameter is distinctly the longest. I have been led to conclude that the ellipticity is caused by an unequal eating over or irradiation at the polar and equatorial portions of the limb, and that in this lies proof that at the sun's equatorial regions the brighter layers of the chromosphere extend to a greater height than near the poles. We know from other sources that the corona generally, and probably also its lower portions, were not so bright in 1870 as in 1869 and 1871; hence the eating over between the prominences has been comparatively slight, and no detectable difference has been caused between the polar and equatorial diameters.

A. COWPER RANYARD

The British Museum

It is strange that such a statement as that advanced by Mr. W. Stanley Jevons in *NATURE*, Nov. 13, has so long remained unchallenged, viz. "that the British Museum exists not so much for the momentary amusement of gaping crowds of country people, who do not understand a single object on which they gaze, as for the promotion of scientific discovery, and the advancement of literary and historical inquiry." No one will dispute the truth of these statements, but substitute the word "instruction" for "momentary amusement," and I very much doubt if his views would meet with public approval. I have always looked upon the British Museum as the National Museum, and *pre-eminently* the Museum of the people, and, as such, the arrangement and labelling of the specimens should be of the most simple and instructive nature: nor is such an object opposed to, but perfectly coincident with, the highest interests of science. No wonder the Museum is filled with "gaping crowds" when nothing is done to instruct them as to the nature of objects of which Mr. Stanley Jevons himself admits they are ignorant, nor to provide them with a suitable and educational guide-book, without which they are as sheep without a shepherd. When the Trustees of this Museum can spare time, they may, perhaps, be able to direct attention to the fuller development of its scientific and educational functions; as regards the former, by the establishment of one exclusively British Department; and, as regards the latter, by carrying out the very obvious suggestions which I have advanced. The view that science, or rather scientific men, should have a monopoly of the benefits to be derived from this Institution is astoundingly selfish and narrow-minded. If such are the views of the Trustees, the British Museum had better be closed to the public.

S. G. P.

Moraines

I HAVE recently been visiting some of those spots which, according to Prof. Ramsay and other geologists, are marked by moraines of the ancient glaciers of North Wales, and several of which are supposed to form the retaining walls of lakes or tarns: and a question has arisen in my mind to which neither my own consideration nor any of the few books here at my command has afforded any answer.

A glacier which has retreated from its terminal moraine, is always the source of a stream of water, and this stream always cuts through the terminal moraine, and makes in it a gap often wide, and always reaching down to the level of the original soil. A terminal moraine from which a glacier has retreated is the rim of a saucer with a cleft in it, extending to the bottom of the saucer. It consequently cannot and does not act as a retaining wall, and the water from the glacier does not form a lake, but flows out as a stream. No better illustration of this fact occurs to me than the Rhone glacier, with its long series of terminal moraines, all intersected and cut through to the ground by the infant Rhone. How then can a terminal moraine ever form a lake? But if a terminal moraine alone cannot form a lake, a terminal moraine with a stopper put into its hole might. But how is the stopper to get there? Why should *débris* or stones or any other stopper stay in the one place in the whole line where there is no resistance?

Where the basin of the lake is supposed to be constituted by a rock basin and a moraine on its rim, what I have said has, of course, no application to the rock basin, but seems to me to apply to show that the moraine cannot constitute any part of the retaining barrier.

And again, where the retaining barrier is supposed to be constituted by a marine terminal moraine, *i.e.* by a moraine deposited under the sea, the observations I have made seem not to apply.

My questions apply to ordinary terrestrial terminal moraines. They are so simple and go so to the root of the whole notion that such moraines can form lakes that I presume they have been answered long ago by geologists. Can any of your readers tell me where such answers are given or what they ought to be?

Bryn Gwyn, Penmaenmawr, Oct. 13

EDW. FRY

The Elevation of Mountains and the Internal Condition of the Earth

I HAVE just read in *NATURE*, vol. ix. p. 62, Captain Hutton's letter to the Rev. Osmond Fisher on the "Elevation of Mountains and Volcanic Theories." I was also indebted some time since to the courtesy of Captain Hutton for a copy of his lecture on the Formation of Mountains, delivered at Wellington, New Zealand, November, 1872. Without entering at present into a discussion upon the particular theory which finds favour with him, I may be permitted to call attention to the fact that Sir William Thomson's views as to the rigidity of the earth have been distinctly called in question in a former number of this journal, which has probably not reached Captain Hutton. I refer to my communication entitled "The Rigidity of the Earth," printed in *NATURE*, vol. vii. p. 288. Captain Hutton expresses his belief that the theory of internal rigidity has probably a weak point somewhere. I venture to think that its weak points are so many as to make it a theory too brittle to form a support to any geological superstructure.

Dublin, November 28.

H. HENNESSY

METEOROLOGIC SECTIONS OF THE ATMOSPHERE

THE primary object of meteorology is to record the pressure, the temperature, the moisture, the electricity, and the movements of the atmosphere. It is desirable, however, that observations on these subjects should be combined with the elements of time and distance. At the general meeting of the Scottish Meteorological Society on June 26, 1867, I proposed the method, since generally adopted, of reducing the intensity of storms to a numerical value by the calculation of barometric-gradients, or in other words by dividing the difference of reading of any two barometers by the distances between the stations where such barometers are placed, thus introducing a nomenclature of universal application, by which the movements of any aerial current, and particularly the wind force of storms, may in every part of the world be reduced to one standard of comparison; and the calculation of thermometric, hygrometric, and electric gradients was subsequently proposed. Since then I suggested to the same society the extension of this system by the establishment of a series of barometers placed at short distances from each other in one or more than one direction in azimuth, so as to give horizontal atmospheric sections for pressure. By means of such lines of section the maximum gradient during storms might, from the nearness of the stations to each other, be ascertained, and thus the phenomena of local storms and other local atmospheric disturbances investigated with some hope of success; and since then a horizontal section extending landwards from the sea-shore has been proposed for temperature and moisture, chiefly with the view of determining the extension inland of the influence of the sea on climate.

It would be important were the system of meteorological sections extended to the vertical as well as the horizontal plane. If a string of stations were placed at short horizontal distances from each other and extending from the bottom to the top of a high hill or mountain, the section thus obtained would show the relative distribution at different times, of pressure, temperature, humidity, &c., in the vertical plane. In Scotland, the existing station of Drumlanrig is 191 feet, and that at Wanlockhead 1,334 feet above the sea, so that the difference in elevation is 1,143 feet. The horizontal distance between them is 9 miles, and in all probability the necessary number of intermediate stations could be established. In Hong Kong the town of Victoria is 1,666 feet below that of Blockhouse Victoria Peak, while in Switzerland