

on the subject, and am anxious to state one or two matters which I learned from him concerning questions raised in my review. Prof. Marsh has visited Brussels since I was there, and since M. Dollo's memoirs referred to by me were in print, and has examined the Iguanodon skeletons with M. Dollo, this being the second occasion on which he has seen the collection. After having examined the specimens now available he is of opinion that the question whether the bones considered by M. Dollo to be sternal are in reality such, or clavicles, is still an open one. The form of the bones, which are undoubtedly identical with those in the British Museum specimen determined by Prof. Marsh to be clavicles, is exactly that of clavicles and unlike that of any known sternal bones. There can be no doubt that they belong to the pectoral arch, but the position in which they have been found in two Bernissart specimens points to their belonging rather behind than in front of the coracoids. It is, however, Prof. Marsh believes, just possible that they may have fallen forward into the position in which they there occur, and he awaits the results to be attained from their examination in the other Bernissart specimens before making up his mind. In the closely-allied Hypsilophodon the sternum is a single broad-keeled plate. In the case of the British Museum specimen one of the bones is attached to the scapula. At all events, he points out that, should these bones really prove to be sternal, it does not follow that Iguanodon had no clavicles at all, for there is a process on the scapula indicating the presence of a clavicle, and such a bone, possibly very small and rudimentary, may yet be found to exist.

The statement of Prof. Marsh that the post-pubis in Iguanodon is long and slender, and *incomplete*, is correct; the conclusion that it was not so arose from a misunderstanding of the exact meaning of the term *incomplete*, as used. It denotes that in Iguanodon the bone does not extend, as it does in some Dinosauria (Hypsilophodon), backwards as far as the ischia, or farther, as in some birds, and this, as will be seen by reference to the figure is the case in Iguanodon, in which the post-pubis does not extend much further back than just beyond the ischial tuberosity. The fact is proved clearly by British specimens as well as by those of Bernissart. Prof. Marsh has observed that in two or three of the Bernissart skulls sutures are distinctly to be seen.

H. N. MOSELEY

Prof. Henrici's Address at Southport

THOUGH a member of many years' standing of the British Association, I have not had the advantage of being present at the current meeting, and am altogether indebted to the report in NATURE for a knowledge of Prof. Henrici's opening address in Section A.

It is much to our advantage to have our educational deficiencies in certain points indicated to us in so candid and, at the same time, so kindly a manner as Prof. Henrici has done on this recent as well as on former occasions; and I hope we shall profit by such friendly criticism. Had I been present, however, I should have ventured to remark on two heads of the address, that I thought Prof. Henrici underrated (1) the extent to which the modern geometry has been cultivated in these countries by many who have not been fettered by the "slavery of examinations" (an expression in which I entirely sympathise), chiefly under the influence of the great geometer Chasles' works; (2) the character of the instruction our youth receive in decimal arithmetic, the abbreviated methods of processes in which being certainly found in our better class of text-books, notably in that of the late Prof. de Morgan, dating back some fifty years, may be assumed to be taught generally in our higher-grade schools, as I certainly know to be the case in several. Other remarks, turning rather on matters of opinion than of fact, which occur to me, would be considered, probably, out of place here.

J. J. WALKER

Scientific Aspects of the Java Catastrophe

YOUR excellent leading article on this great event omits to call attention to a factor which I have long maintained to be of the greatest interest and importance from the point of view of meteorology and geology in general. I allude to the quantity of gases or vapour emitted during the eruptions. This must bear a direct relation to the quantity of matter emitted (whatever its form) and also to the height and distance to which the matter may be ejected or carried.

Now I hold that such vast quantities of gases as must have

been liberated on this occasion cannot be passed over or taken as having no action on our atmosphere. Whatever the addition made, temperature and currents are influenced by it either locally or over great extents of the earth's surface, and if it were possible to take account of the height attained by the gases, their temperature of liberation, and the point of the surface of the globe whence proceeding, some judgment might be attempted of their action. In the present state of meteorology we know nothing of these quantities, but it is justifiable to assume that the upper currents of the air may be thus profoundly influenced, and that in certain cases cyclones may thus be generated. The present very fine dry weather we are enjoying here, with the high and steady barometer, may be a result of the great eruption, and it will be worth while to note if any abnormal conditions of atmosphere be found to prevail during the coming months.

J. P. O'REILLY

Dublin, September 16

"Elevation and Subsidence"

MR. YOUNG appears to think that I hold the view that rocky matter will melt at a lower temperature when under greater pressure. I did not intend, in my letter of August 24th, to express such an opinion as my own, but only to say that this was not a settled question; quoting the experiments upon which the doubt was founded.

Again, I merely mentioned the hypothesis that the matter of the nucleus may be above its own critical temperature as "conceivable." To all Mr. Young's present queries I should be disposed to answer in the affirmative, except to the second—"Do not the 'rigidity' calculations incontestably show that the earth is extremely rigid, *i.e.* solid?" As a geologist I do not concern myself anxiously about the nucleus. But to hold that the superficial parts are rigid I assert to be absolutely contrary to the known facts of geology. Perhaps it will be said that they ought to be, and therefore so much the worse for the facts.

Again, I say that mere plasticity of the upper layers will not explain the phenomena. The arrangement of rocks in the interior of mountain chains shows that the crust has been pushed over the surface towards them. It must, therefore, rest on a lubricating substratum. Again, mountains tend to rise and sedimental plains to sink. If mere plasticity were all, the reverse would happen.

As I understand it, the tidal argument for rigidity amounts to this. If the earth were not rigid, the fortnightly tide would be inappreciable. But Prof. Darwin, after most laborious and involved reductions of observations made at the instance of the Indian Government, has come to the conclusion that such a tide can be detected—not of its full amount, however (so far negating absolute rigidity), but something less than three-quarters of that. The undiminished amount ought to be $4\frac{1}{2}$ inches only. The barriers caused by my "roots of the mountains," which, as noticed by Mr. Gardner, would break up the continuity of the substratum, would, as I have elsewhere pointed out, be great obstacles to the formation of tides in it.

O. FISHER

Harlton, Cambridge, September 20

A Complete Solar Rainbow

ALTHOUGH I quoted Capt. Winchester's figures as to the diameter of the circumsolar bow, mentioned in my letter on p. 436, I may add that this measurement was checked by that of the chief officer (Mr. Grant), who took the distances from the horizon to the inner rim of the bow on both sides, and subtracted them from 180° . In the case of the Captain's measurements, in the first instance, he measured from the inner rim of the bow to the edge of the sun. This was doubled, and the diameter of the sun added to it. Under these circumstances I can hardly believe there could have arisen the mistake suggested by Dr. Ingleby.

September 17

D. MORRIS

C. M. INGLEBY (p. 489) is clearly mistaken in supposing that D. Morris's description in NATURE (p. 436) referred to a real rainbow, for he makes no mention of any rain, the phenomenon being on a thin film over the sky. It must have been a solar halo, differing from an ordinary one only in being more distinctly coloured than usual. I have on rare occasions seen small portions of an ordinary halo very brilliantly coloured, but never saw a complete one so.

T. W. BACKHOUSE

Sunderland, September 24