seven o'clock, stood at 30° immediately after totality; the keen breeze which was blowing before the sun was shadowed died completely away at the time of totality." I inclose a photograph which clearly shows the protuberances noticed by all the observers. KILLINGWORTH HEDGES

Westminster, October 30

An Earthquake Invention

THE object I had in view in my former communication to NATURE (vol. xxxii. p. 213) on this subject, has been attained, as the following quotations from Prof. Milne's letter in NATURE (p. 573) show: "I have no desire to claim the authorship of the aseismatic joint;" and again, "I am as yet in the dark as to who was the first inventor of the aseismatic joint."

Well, I can enlighten him, and I claim the invention for Mr. David Stevenson, whose paper describing it was read before the Royal Scottish Society of Arts in 1868, and published in their *Transactions;* whose firm designed, superintended the construction of, tested and sent out to Japan seven lighthouse apparatus, carried on tables 8 feet in diameter, fitted with this contrivance. Further Messrs. Stevenson designed two lighthouse *buildings*, iron towers 29 feet in diameter at the base and 46 feet in height, with an aseismatic joint *at their base*, which were constructed and *erected* in the work-yard of the contractors in Edinburgh, and finally, in 1869, shipped to Japan, but unfortunately they never reached their destination, as the vessel went down on the voyage out.

There are three points in Prof. Milne's letter on which I wish to make a few remarks. The first is to give the explanation Prof. Milne asks as to the part the late Mr. Mallet took in the invention of the aseismatic joint which I may observe Mr. Mallet never claimed for himself. Mr. Stevenson consulted with Mr. Mallet as to what was the exact *mécanique* of an earthquake shock, and how he thought it would affect the delicate apparatus usually placed in a lighthouse. This information Mr. Mallet furnished, but so far from suggesting a ball and plate joint, he expressed a fear that the superstructure, if placed on balls as proposed by Mr. Stevenson, would be thrown down, and in a letter dated March 14, 1868, acknowledging a copy of the *Scotsman* newspaper, containing a notice of Mr. Stevenson's paper, he says that if the balls and plates proposed are confined to the apparatus in the light-room, he "would augur much more favourably of the result being satisfactory," but that his "own notion for Japan or other shaky places would be to make all the towers rather of timber or of boiler plate work." This, I think, should put Prof. Milne's mind at rest on this point.

The second point is with reference to ball and plate seismographs. I never described a seismograph, but my brother did, in 1883, in NATURE, vol. xxviii. p. 117, though, so far from claiming the *idea* as original, he says: "The idea of the instrument I propose was suggested to me by the aseismatic arrangement designed by my father, Mr. David Stevenson, for averting damage to buildings and lighthouse apparatus in countries subject to earthquakes."

I entirely agree with Prof. Milne that the joint employed in ball and plate seismographs, lamp tables in Japanese lighthouses, model houses, and the Professor's own dwelling-house, all "involve the same principles, and they only differ in their dimensions," and my point is that Mr. David Stevenson was not only the original inventor of this contrivance, but, what is of far more importance, suggested and carried into practice *the* only known method of mitigating the effects of earthquake-shocks on buildings, and the astatic house of which Prof. Milne reported such good results to the British Association of 1885, which is described in NATURE, vol. xxxii. p. 527, as being "rested at each of its piers upon a handful of cast-iron shot each a quarter of an inch in diameter" placed "between flat iron plates," is obviously merely a modification of the same principle.

The third point is as to the success of the aseismatic joint. It does seem a little curious that Prof. Milne, in the *Transactions*, British Association of 1884, when he appeared to me and to others to claim the invention for himself, thought it perfection, though now he appears to have changed his mind. I do not think, however, it affects the question at issue, whether the aseismatic joint is a success or not; but that it is a success will be seen from Prof. Milne's own reports in the *Transactions* of the British Association, and from the following information which was supplied by Mr. Simpkin in 1884, who had just returned from Japan, where he was engaged in the lighthouse service. At Isuragisaki and Kashmasaki lighthouses the aseismatic tables were firmly strutted with timber to prevent any motion, as inconvenience was felt from the oscillations of the table when winding up the machine, the steadying screws sent out with the apparatus for the purpose of temporarily doing so having for some reason not been put in at these stations. These two are the only lighthouses at which any damage has been done by earthquake, while those stations at which the tables are in operation have never suffered at all, although they have been repeatedly subjected to shocks; but for full particulars as to this see NATURE, vol. xxx. p. 193, and vol. xxxii. p. 316.

Prof. Milne excuses himself on the ground that he was 10,000 miles away from a library and never saw Mr. Stevenson's paper, but surely NATURE finds its way out to Japan, and this subject has been referred to in your columns frequently; it was also discussed in 1876 before the Institution of Civil Engineers, and an account of it was published in their *Transactions;* but, after all, the apparatus was actually at work in Japan where he was living. D. A. STEVENSON

84, George Street, Edinburgh, October 19

The Mithun

I WAS glad to see in NATURE of July 16 (p. 243) that Mr. W. F. Blanford had drawn attention to the extraordinary mistake made by Dr. Kuhn in considering the gayal and gaur specifically identical, and their differences as due to domestication. If this latter were true we should see endless intermediate forms instead of two invariably distinct. To those who know them in their habitat the confusion must seem extraordinary, even though both are here called "Mithun." The gayal (*B. frontalis*, v. *gavœus*) is known (domestic only) all through these hills, and not in the plains; is pied black and white, with pink muzzle, white legs, and the tips of the horns point *outwards*. The gaur (*B. gaurus*, v. *cavifrons*) is only known wild, in the hills and also plains, never pied, has white legs, and the tips of adult horns invariably point *inwards*. The gayal domestic, and never known wild; the gaur wild, and never known domestic; and they do not cross. I have known both here now many years, and had good opportunities of observing and contrasting them. I have had a fine bull gaur feeding along beside me at twenty yards in short grass for over quarter of an hour, as I sat motionless in my Rob Roy canoe, an enormous Dontal (tusker) elephant at the same distance off on the opposite bank; each occasionally left off to sniff me, but resumed again, taking me, in brown-grey costume and grey-coloured canoe, for a snag in midstream (which stream was deep and stagnant). It is not always easy or possible to point out to such a man as Dr. Kuhn that the study of the "dry bones" of an animal is really but half the battle in comparing it with its allies. The study of specific distinctions should include the whole animal, alive as well as dead.

But the clearest proof that these two distinct forms are not due to domestication is that, instead of endless intermediate forms, we find absolutely none. S. E. PEAL

Sibsagar, Asam, September 26

On the Behaviour of Stretched India-rubber when Heated

I SHOULD like to make the following remarks with reference to the letter of Mr. H. G. Madan which appeared in the last number of NATURE:—

(*a*) Though the fact that india-rubber becomes *hot* when *stretched* might be, and no doubt is to be, *partly* attributed to molecular friction, we cannot thus account for the *cooling* which resulted from *contraction* in the experiments of Joule and Sir William Thomson.

(*b*) Text-books as a rule are not, I am afraid, sufficiently explicit as to whether the stretched india-rubber is contracted in *volume* when heated, or only in *length*. Thermodynamic theory does not require, in order that longitudinal pull should produce rise of temperature, that the *volume* should be diminished when the temperature is raised, and the results of Joule's experiments are in reasonable accord with theory.

(*c*) The real state of things seems to be that the effect of heating a stretched piece of india-rubber is to *lengthen* it if the tension is *small*, and to *shorten* it if the tension is large (Hr. Schmulewitsch, *Vierteljahrschrift der Naturforsch. Gesellschaft, Zürich*, xi. 202); thus, for a certain tension there will be neither elongation nor contraction, and my own experiments on the