LETTERS TO THE EDITOR.

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Fœtal or New-born Giraffes Wanted.

WILL you give me the opportunity of making a request through your columns to museum curators and African sportsmen? I am especially anxious to obtain for study, preserved in spirit or dry, the head (*not* the prepared skull) of a new-born giraffe or of a late fœtal individual in which the boney ossicusps of the horns are already formed. I should be able to return the specimen after examination to the owner if desired. I should be glad to examine several such heads were it possible to procure them. All expenses of transport would be paid by me. I venture to ask those who can help me to communicate with me without delay.

E. RAY LANKESTER.

Natural History Museum, Cromwell Road, London, June 23.

Seismometry and Gêite.

BEFORE making a few comments on Prof. Milne's second letter under the above title (NATURE, June 12, p. 127), I should like to express my warm appreciation of his devotion to seismological research, and the great impetus it has given to observational work. In pure seismology—apart from applications of elastic solids to earth problems—Prof. Milne's reading is doubtless more extensive than mine, but if he is correct in regarding my first letter as containing nothing new to seismologists, they must, as a class, be singularly prone to a policy of *meliora scio deteriora sequor*. Novelty in results is, of course, much a matter of opinion. When Prof. Milne says, however, that there is no occasion for my warning as to Young's modulus, I must in reply give a quotation from his first letter, relating to the material of his hypothetical core, "it follows that the density . . . is 5·96, or approximately 6. *The* elastic modulus for a core of this density which conveys vibrations with a speed of at least 9·5km. per second is 451×10^{10} C.G.S., or roughly speaking, a little more than twice the Young's modulus for Bessemer steel." The italics are mine. If "the modulus" is not Young's modulus, E, a comparison between it and the E for steel is misleading, because a comparison of numerical results naturally implies that they refer to the same physical quantity. On this view the statement is doubly misleading, because there are *two* wave moduli, viz. $m+n$ and n. If, as one would infer from Prof. Milne's second letter, "the" modulus was intended for the wave modulus $m+n$, the futility of the comparison becomes obvious when we remember that on the ordinary theory $(m+n)/\mathrm{E}$ may have any value between 1 and ∞, according to the value of Poisson's ratio. As a matter of fact, "the" modulus must, I think, have been intended at the time for Young's, though this must have escaped Prof. Milne's memory. If it were meant for $m+n$, we should have $(451 \div 5{\cdot}96)^{\frac{1}{2}}$ "at least" 9·5, whereas it is really only 8·7. If, however, we multiply 451×10^{10} by 6/5—which would be correct if 451×10^{10} were a Young's modulus in a material where Poisson's ratio had the uniconstant value 0·25—and substitute this, we deduce a wave velocity of 9·53km. per second.

Prof. Milne seems to have misunderstood my treatment of the two wave velocities in the *Phil. Mag.* (March, 1897, p. 199), and as it bears directly on the question at issue, I should like to make it clear. In previous papers I had advanced a variety of considerations pointing to the conclusion that, whilst all applications of elastic equations to the earth are more or less speculative, the mathematical and physical difficulties are enormously reduced when we suppose that the deep-seated material—about which we have no direct information—is nearly incompressible, *i.e.* has a Poisson's ratio approaching 0·5. Such a hypothesis, for one thing, rendered it unnecessary to assign to the rigidity and Young's modulus values largely in excess of anything yet encountered at the earth's surface. There remained, however, the fact of the high velocities observed in the more rapid earthquake waves, which had been generally supposed to imply enormously large Young's moduli, such, for instance, as the value 45×10^{11} given by Prof. Milne. The problem stood as follows:—

In an infinite isotropic elastic medium there are necessarily *two* wave velocities. If we know them both we can deduce all the elastic properties of the medium, provided we know the density; if we do not know the density, we can still deduce Poisson's ratio. If the medium is not infinite, but is bounded by a plane surface, then, as shown by Lord Rayleigh, there is a special type of surface wave the velocity of which, especially when the material is nearly incompressible, approaches closely to that of the slower or rigidity body wave natural to the material. If the bounding surface be not plane, but spherical or spheroidal, there is doubtless a wave answering to the Rayleigh wave, which within moderate distances of its origin may be expected very closely to resemble the Rayleigh wave in type, when the depth to which it penetrates and the wave-length are both very small compared to the central radius. If the medium have a Poisson's ratio of 0·25, the velocities of the two body waves must be in the ratio of $\sqrt{3}$(or 1·73) : 1.

In the earth there seems distinct evidence of only two types of waves. For the more rapid, supposing them to travel straight through, Prof. Milne himself would apparently take 10km. as the most probable value at depths below the immediate heterogeneous crust. It was important for my object not to understate this velocity, and I took the somewhat higher figure of 12·5km. The second type—which Prof. Milne terms the "large" waves—travel much slower. If they go straight through, their velocity is *less*, of course, than if they travel along the surface. On the former hypothesis, Prof. Milne might make them a trifle slower than the value I took, viz. 2·5km. per second. If, instead of 12·5 and 2·5, we took 10 and 2, we should obtain, of course, the same value of Poisson's ratio as before, 0·48 approximately, with a value for E somewhat *less* even than the very moderate value (about 10×10^{11} C.G.S.) obtained in my paper. If we took 10 and 2·5, or even 10 and 3, for the two velocities, we should get 0·47 and 0·45 for the values of Poisson's ratio.

The uncertainty as to whether the "large" waves were body waves or surface waves—or, as I thought more likely, a combination of the two—was not overlooked, as Prof. Milne's letter might suggest, but was dwelt on at some length in the paper. If they are *entirely* surface waves, the heterogeneous nature of the earth's crust, and the irregularities of mountain and ocean, are such as to introduce extreme uncertainty into any mathematical calculations. In this event it is doubtful whether any conclusion can be drawn either for or against the hypothesis of great incompressibility in the core; its explanation of the high velocity in the faster waves would, however, be unaffected.

The discussion of magnetograph results by Prof. Milne in the B.A. Reports for 1898 and 1899 (1888 is surely a misprint) was familiar to me as a contributor of data, but it did not seem to render my letter unnecessary. I suspect, however, that I partly misunderstood Prof. Milne's letter on this part of the subject, as I did not fully realise that he did not recognise the distinction between anomalous and merely high values of the horizontal force H. The fact that H is nearly twice as large at Batavia or Bombay as at Kew is natural, owing to their proximity to the magnetic equator. Whether the values at these stations are higher or lower than one would expect from their geographical position cannot be said with certainty until the completion of magnetic surveys. What my letter suggested was the advantage for critical purposes of records at a station where there is known to be a true *large* magnetic anomaly—*e.g.* in N.E. Ireland or the Scottish Highlands. Variations in the value of g are, relatively considered, trifling compared to those in H, and the larger gravitational anomalies present systematic features to which there seems no parallel in magnetics (*c.f.* Bourgeois' discussion of g in the "Rapports présentés au Congrès International de Physique," Tome iii., Paris, 1900). Apart from the question of the unit, I am a little puzzled by Prof. Milne's gravitational data for Kew, and I should warn him that there, as at some other stations, the agreement between different observers at different times has not been such as to warrant much reliance in any one observer's value for $g-\gamma$ (*i.e.* gravity observed less calculated). C. CHREE.