edition, is incomprehensible, saving the assumption that both with respect to his father's annual reports and other sources, the son was wholly incapable of doing his father justice. It is a pity that the task of preparing a second German edition was not entrusted to a competent botanist, because the original work, apart from the uncompromising antagonism to Evolution that pervades it, still occupies an undisputed position in modern botanical literature. As it is, the French edition is not merely an advance on the original German—it is incomparably better than the second German edition. It is only, however, fair that some justification of such assertions should be given. Taking the chapter on Oceanic Islands as an example, it may be confidently stated that no additional information is given; yet there is no branch of geographical botany that has advanced more during the last decade than insular. On the other hand Tchihatcheff embodies nearly all that was known up to date. One slight alteration observed in this chapter is—Madeira is stated to be 50 German geographical miles nearer Europe than the Azores, instead of 150, as in the original. Then certain unfounded statements in refutation of the arguments of other botanists concerning the relationships of insular floras remain uncorrected. Thus, in allusion to Sir Joseph Hooker's demonstration ("Insular Floras," p. 7) that the vegetation of St. Helena has, on the whole, its nearest affinities in South Africa, it is objected, on the authority of Roxburgh, that three out of the five genera named by Hooker were originally introduced into the island from the Cape of Good Hope, whereas an examination of Roxburgh's enumeration of the plants of St. Helena reveals the fact that the indigenous, and endemic, St. Helena species of the genera in question were unknown to him, and his remarks apply only to actually introduced species. Again, to repeat in 1884 such statements as that the vegetation of Juan Fernandez has little systematic relationship with that of the Chilian or Antarctic floras and that *Pringlea antiscorbutica* is restricted to Kerguelen Island is unpardonable, because the contrary is now historical. Defects such as those pointed out are numerous, but as they are mostly due to the state of knowledge fifteen years ago, the author of the work of that date is not to be blamed for them; rather the present editor and publisher for offering the public an old book as new.

W. Botting Hemsley

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Nomenclature in Elasticity

The word *stress* is used, sometimes in the sense of *load*, sometimes in that of *load per unit area*. Clearness, however, requires these two ideas to be kept perfectly distinct, and therefore to be denoted by separate terms. *Load* is surely expressive enough, or, if not, there is the more comprehensive word *force*: why then use *stress* synonymously? It would be far better to reserve *stress* to signify *load per unit area*. This Prof. Kennedy (p. 269) calls *intensty of stress*; but why not *stress* simply? The word *intensity* is not in itself suggestive of anything distinctive, and is therefore useless.

Pressure and *tension* are terms used in the same loose manner, though, when intended to represent *force*, they sometimes have the word *whole* prefixed. Is it not better to say *force* when we mean *force*? We can then reserve *pressure* and *tension* as vector-synonyms of *stress* in the sense of *force per unit area*, which is indeed their usual *rôle*.

Another misused term is *resilience*, which sometimes denotes the *work* done in producing *proof* strain in a body (Rankine's definition), sometimes the *work* done *per unit volume* in producing *proof* strain, sometimes the *work* done *per unit volume* in producing *any* strain. I prefer, myself, the third definition: the second would then be the *proof resilience*, and the first might be called the *strain-energy*.

However, whatever terminology is finally agreed upon, let it be perfectly definite and consistent.

In his Fig. 1 (p. 269) Prof. Kennedy writes: "Breaking load, 18·85 tons per square inch." According to his own nomenclature, he should surely say: "*intensity of* breaking *stress* 18·85 tons per square inch," and this I should prefer to call simply the breaking stress—premising that for *tons* I should write *tons' weight*. In this case, as the diameter is ¾ inch, and therefore the section ·442 square inch, the breaking *load* is 8·33 tons' weight. Similarly in the other figures.

Christ Church, Oxford Robert E. Baynes

Earthquake-Proof Buildings

Mr. Muir is quite correct as to the facts and date of the introduction of the aseismatic tables into Japan. In 1869–70 seven aseismatic tables for carrying the lighting apparatus were sent from here and erected in Japan, and Mr. Simpkins, who has recently returned from Japan, informs me that there are three in action at present. Two iron towers, 46 feet high, with this arrangement at their base, were also constructed and shipped for Japan, but the vessel was lost and no more were sent out, as the engineer in charge—Mr. Brunton—took an unfavourable view of their efficiency—his idea being that they would not work, as he considered that buildings of "great weight and solidity, thereby adding to their inertia and checking their oscillation, were best suited to meet the difficulty in Japan." Mr. Milne's experiments with aseismatic tables have borne out Mr. David Stevenson's original view as to their power of mitigating an earthquake shock. For fuller information see Nature, vol. xxx. p. 193. D. A. Stevenson

Edinburgh, August 3

A Mechanical Telephone

Having observed in this week's Nature a notice of a "mechanical telephone" said to be brought from America, I may state that so far back as 1878 I experimented on the transmission of sounds by wires, and communicated the results obtained, from a large number of experiments, to the Physical Society of London in March, 1878; the paper being afterwards published in the *Philosophical Magazine* for August, 1878. These experiments are referred to by the Count du Moncel in his book on "The Telephone," published in 1879. I found no difficulty in carrying on a conversation through wires laid in various ways from room to room of a house; and musical sounds, breathing, and whistling were also readily transmitted, and through most unlikely arrangements, such as a common wire fence. Various materials were tried for the transmitting and receiving ends—disks of cardboard set in deepish rims being found to give excellent results with a No. 16 copper wire. In one of my experiments I found that the disks were not required, the wire itself picking up and transmitting the sounds. The results obtained were most interesting; but as the range was necessarily limited, it did not seem to me that there was much scope for practical application. W. J. Millar

100, Wellington Street, Glasgow, July 31

Electrical Phenomenon

About ten o'clock in the evening of July 23 a party of four of us were standing at the head of the avenue leading to this house, when we saw a feebly-luminous flash appear on the ground at a distance of some thirty yards down the avenue. It rushed towards us with a wave-like motion, at a rate which I estimate at thirty miles an hour, and seemed to envelop us for an instant.