be complicated by a local upheaval of somewhat horizontal strata into a curve or arch; that then, while still upheaved and so distended laterally, a subsidence may take place towards the crown only of the arch, letting down the keystone, so to speak, as a 'trough' fault; then, on an extension of the same subsidence over a larger area, the arch, being keyed up afresh by the occurrence of the trough fault, can only give way by rupture of the nature of an overlap or reversed fault. These overlaps are sometimes on a considerable scale. Within a few miles of the place whence I write, there is a well-defined and proved and, as it happens, easily measured fault of this kind, in which the amount of movement is no less than 101 fathoms measured in the plane of the fault, the amount of throw being seventy-four fathoms vertical, and about sixty-nine fathoms horizontal. The hade of this fault is very nearly the same as Mr. Hebert's experimental ones, being 47°. I would also call attention to the fact that the *downward* vertical pressure P (*vide* his diagram) can in no case exceed the actual simple weight of the mass above the fault, and that in actual nature it is impossible (*vide* Fig. 2) for the left-hand portion to subside unless there be room for it to subside into. This room can, generally speaking, only be got by the horizontal *separation* of the masses on both sides of the portion subsiding. It would thus appear probable that all direct faults are of the nature of trough faults, that is to say, that either near or far off there is a somewhat parallel fault with an opposite hade, contemporaneous as to date of occurrence, and that this pair of faults meet sooner or later in depth. I would thus suggest that in the case of direct faults Mr. Hebert should in his inference substitute horizontal *tension* for vertical pressure (which is a secondary effect), and that the rule should be stated thus:—direct faults are indicative of horizontal tension, reversed faults of horizontal pressure.

STON EASTON, NEAR BATH. H. E. H.

PROF. MANTOVANI AND THE 'MIOLITHIC' PERIOD.

SIR,—Prof. Mantovani, in your last issue, proposes the term "Miolithic" for a period intermediate between the Palæolithic and the Neolithic. The term appears to be formed upon the "Miocene" of Lyell, which, of course, does not mean Middle Tertiary. Should the Italian Professor establish his new period, he would more appropriately substitute "Mesolithic" for "Miolithic." It is to be presumed he uses his terms in a purely local sense, for his Italian Miolithic age is represented as being contemporaneous with an age which produced "beautiful vessels of perfect work, resembling those of the ancient Etruscans," and was, therefore, probably post-lithic. The teachers of our science should not forget, for the sake of beginners, that the words "Palæolithic," "Neolithic," etc., represent, not absolute epochs of time, but stages in human development. CHARLES CALLAWAY.

WELLINGTON, SALOP,
Oct. 4th, 1877.