fishes, etc.; (7) tests of entomostraca; (8) quartz in subangular and well-rounded grains; (9) black granules, apparently of iron-oxides; (10) rich-brown, polished phosphatic concretions; and (11) dull-green grains, imperfectly rounded. These constituents are enumerated as nearly as possible in order of abundance.

The Winterbourne-Boxford phosphates have a known range in time considerably greater than those of Taplow. Their advent far down in the *cor-anguinum* zone is especially interesting; for in England, as Mr. Jukes-Browne has remarked, that subdivision of the Chalk almost everywhere "presents the appearance of having been quietly and continuously accumulated in water that was seldom disturbed by bottom currents," albeit a tendency to develop hard bands at one horizon, at least, is apparent in the western part of the London Basin. The Phosphatic Chalks of Winterbourne and Taplow evidently mark places on the sea-floor particularly liable to the impingement of strong currents, and may mark places above which the water commonly had a gyratory motion. In any case, their zonal range argues a marked degree of stability in the current-system of the body of water in which they were laid down.

CORRESPONDENCE.

THE TRIMMINGHAM CHALK AS A ZONE.

Sir,—I hope you will allow me at this late hour to reply to Mr. Jukes-Browne's letter in your February number. It raises some very large questions which cannot be adequately dealt with in a letter, but I should like to define my position.

It appears to me that the term 'zone' is applied indiscriminately to at least two different conceptions. One of them is what may be labelled the 'international zone.' This covers the great ill-defined subdivisions which persist over large areas embracing more than one natural province, and by which we correlate the equally ill-defined local territorial names such as 'Norwich Chalk,' 'Maestricht Chalk.' 'Meudon Chalk,' etc. These correlations are interesting, but of little practical value, as the zones themselves are rarely, if ever, accurately defined.

The other conception above referred to is what may be called the 'provincial zone.' Each of these zones contain such a thickness of sediment as from place to place over a natural province (whether made natural by modern geographical or ancient geological conditions) contains *throughout* (*pace* Mr. Jukes-Browne) some well-marked form which is comparatively scarce or altogether absent both above and below. These zones have upper and lower limits which are well defined either palæontologically or stratigraphically, often in both ways. They are therefore capable of practical application by the pit-worker, who most stands in need of help, and through him by allied

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sciences. The 'zone of *Marsupites*,' to which Mr. Jukes-Browne appeals, is one of the finest possible examples of what I mean. The 'zone of *Marsupites*,' introduced by Barrois, was an international zone. It was adopted from a country in which not much was known about it, and it had not even received a separate name; its upper boundary for England was wholly undefined, its lower boundary was only defined in the Margate area by a physical character which Barrois only thought he recognised again in the Sussex area, and there wrongly according to Dr. Rowe, and the type fossil only occurs in about 40 feet out of some 300 feet attributed to this zone by Barrois. I do, though I gather Mr. Jukes-Browne will disapprove, most strongly urge the limitation of the 'zone of *Marsupites*,' for the south of England at any rate, to the important bed of very uniform thickness and position in the series which contains *Marsupites* "in every foot," and outside which *Marsupites* is practically non-existent. Can there be any doubt as to which of these two zones is the more logical and practically useful?

Now it so happens that the international zones introduced by Barrois up to and including the zone of *M. cor-testudinarium* are sound provincial zones for the south of England, answering (except in the case of the zone of *B. plena*) very well to what Mr. Jukes-Browne considers such an unreasonable test, i.e. the occurrence of the type fossil in every foot of the zone. But that does not make it any the less desirable if we are establishing a new zone to establish the most accurately defined one that we can. Does Mr. Jukes-Browne's zone of *O. lunata* satisfy the reasonable requirements of scientific accuracy? How would he define its upper and lower boundaries? Clearly not by the appearance and disappearance of *O. lunata*, for there are at least 10 feet of chalk exposed at Trimmingham below the lowest occurrence of this species and anything from 25 feet upwards above the highest occurrence, and I cannot see any other possible criterion. The zone of '*Terebratulinæ*' which I propose begins where *Terebratulina gracilis* appears (and I am in hopes of satisfying myself that this is just above the hard yellowish bed, in which case the zone of *B. mucronata* would have at Trimmingham an upper boundary defined both physically and palæontologically), and will end where *T. gracilis* disappears, unless before that point is reached some other fossil worthy of being made a zone fossil comes in. The only objection I can see to my zone of *Terebratulinæ* will be removed when the characteristic fossil of the zone below that of *H. planus* is properly named, and I hope we shall not have to wait much longer for this.

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[Erratum.—In Mr. R. M. Brydone's article in February number, Geol. Mag., 1906, p. 77, line 33 from top of page, for 'blending' read 'banding.'—Edit. Geol. Mag.]

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