

—too modest. A large number of forms found outside Florida are figured and described. In many respects Dr. Dall's work may be regarded as a species of text-book on the Tertiary Mollusca of the United States, having especial reference to Florida. In several instances all the known fossils of a genus occurring in America are alluded to under the description of that genus; and a good deal of critical revision is effected in almost every branch. It is the work of a thorough master of the subject, and we look forward with considerable interest to the appearance of Part III. in which the author hopes to begin and conclude the description of the bivalves, and to give a general summary and tables. G. F. H.

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CORRESPONDENCE.

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THE PEBBLE-RIDGE AT WESTWARD HO.

SIR,—The Pebble Ridge at Westward Ho has often been noticed by geologists, and was the subject of a paper by Mr. Pengelly, F.R.S., so long ago as 1868.<sup>1</sup> During a recent visit my attention was attracted to a peculiarity in the "pebbles," which, although not referred to by Mr. Pengelly, can scarcely have escaped observation. So far as I am aware, however, the said peculiarity has not been recorded. The feature referred to is the concentric lamination often induced in "pebbles" above a few inches in diameter. An outside coat about  $\frac{1}{16}$  of an inch in thickness peels off, and often underneath the first a second layer comes away. Thus the "pebbles" exhibit all the appearance of a concentric concretionary structure. But as the "pebbles" can be traced to their origin in rhomboidal or rectangular blocks of the local grits, the concentric structure, following the varying curves of the rounded "pebbles," must necessarily be induced and not original. Although this structure is very common I could not find a single instance in a "pebble" sufficiently small and light to carry away in my pocket. It is confined entirely to weighty stones. It would almost seem as though we had in these rounded "pebbles" a sort of pseudo-concentric cleavage caused by percussion, and if so we may possibly find in these "pebbles" a clue to the concentric concretionary structure often noticed by geologists, and which in certain cases may be due not so much to crystallization as to pressure acting equably on the surface of a nodule enclosed in softer strata. However, my object in writing is to call attention to a significant fact rather than to attempt to explain it.

A. R. HUNT.

TORQUAY.

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THE RELATIVE AGE OF FLINTS.

SIR,—I have to thank Mr. Jukes-Browne for his article on the Relative Age of Flints in your July issue. His paper is a valuable contribution, and will materially assist the workers in this most difficult subject by directing their attention to those points which most require elucidation.

I gather that, as regards tabular flint, he believes it was formed in

<sup>1</sup> Trans. Dev. Assoc. 1868, p. 415.

fissures, and that microscopically it has the same appearance as flint from the Nodular layers. At present I must confess to being ignorant of his reason for putting flint floors in a separate classification. Is it simply their wide extent? True, they are generally quite solid and free from the cavities so common in the more vertical sheets of flint, but this, I had supposed, was due merely to the horizontal position favouring the deposit of silica because of the slower current of water.

These horizontal layers, whatever their origin, show, with this exception, the same signs as the others of having been formed originally of two plates, and probably in open joint-planes. Should Mr. Jukes-Browne's suggestion that the flint was deposited in cracks during the first upheaval of the chalk be correct, we must believe that these cracks often extended at least through 100 ft. of soft chalk saturated with sea-water, and that many of these fissures were far from being vertical, frequently at an angle of only 30° with the horizon.

My suggested explanation of the absence of flint in the Lower Chalk only referred to the beds in our S.E. district, and I take it that Mr. Jukes-Browne's experience does not refute my belief that wherever there are nodules in the Lower Chalk they are also present in the Upper Chalk, but not *vice versa*. He is doubtless aware that it has been asserted that the total percentage of flint in different beds is roughly the same. If this be true, surely we must look to segregation as the cause of all the visible flint in the cretaceous strata, and the question is, when did this process take place?

The varying character of flints in successive zones is doubtless of importance, but does not prove that flint was contemporaneous with the chalk; though evidence of the traces of the *soft* parts of organisms in flint would probably be conclusive, and I should be glad to hear if such have been found.

The cores I spoke of are not tubular, and I still fancy are not formed round Doryderma, though probably confined to one particular zone. The outer coats of these flints are generally shaped like a potato, hollow inside, with a solid cylindrical core passing through from end to end. This is the simplest form, but sometimes other cores cross the first at various angles. I should be glad to submit specimens to Mr. Jukes-Browne if he would care to see them.

These cores, as well as some other flints I have, containing remarkable loose kernels, lead me to ask him to allow me to add "Mechanical Flints" to his classification, and to include concentric circles of flint with his Paramoudra division.

I can only briefly touch on the chemical influences involved, except that I doubt that Mr. Jukes-Browne's theory of the precipitation of silica through contact with carbonated water can be accepted; but his paper was all too short, and I should be glad if in some future communication he would give us his views as to the sources whence the large quantity of silica for Flint-floors, etc., was derived.

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TUNBRIDGE WELLS, Aug. 1, 1893.