REFERENCES

1 in. Geological Survey Maps, Sheets 342, 343; and 6 in. Dorset Sheet, lv, S.W. (2nd edition), 1903.

(1) STRAHAN, A., 1898. "The Geology of the Isle of Purbeck and Weymouth," *Mem. Geol. Surv.*, p. 3 (Introduction).

(2) DAVIES, A. M., 1935. *The Dorset Coast*, p. 89.

(3) ARKELL, W. J., 1936. "The Tectonics of the Purbeck and Ridgeway Faults in Dorset," GEOL. MAG., LXXIII, 56.

(4) BURY, H., 1920. "The Chines and Cliffs of Bournemouth," GEOL. MAG., XVII, 71.

(5) BURTON, E. ST. J., 1932. "A Peneplain and Re-excavated Valley Floors in Dorsetshire," GEOL. MAG., LXIX, 474.

REVIEW.

THE SHONKIN SAG LACCOLITH. By J. D. BARKSDALE. *Amer. Jour. Sci.*, xxxiii, 1937, 321–359.

FOR many years past the Shonkin Sag laccolith has been regarded as a classical example of differentiation in place in a single intrusion. An examination based on much detailed field and laboratory work has revealed many difficulties in this explanation, and the author accounts for the marked horizontal layering of the different rock-types by three separate intrusions. These have probably been derived by deep-seated differentiation from one primary magma, though there is nothing to show by what process this primary separation was effected. It is suggested that the second intrusion was injected along the still liquid centre of the preceding one, and the third along the base of the second. The channel of intrusion has not been found.

CORRESPONDENCE.

THE PALISADE SILL OF NEW JERSEY.

SIR,—The Palisade diabase sill of New Jersey has been widely cited as one of the most convincing examples of gravitational differentiation ever since the very complete description of J. V. Lewis in 1907. In the course of a re-investigation of the intrusion, however, I have found that its mode of differentiation is probably more complex than previous accounts would lead one to believe. The following questions, for instance, arise :—

(1) There is a marked discrepancy in composition between the chilled contact-modifications of the sill (which are holocrystalline, and contain about 2 per cent of olivine but no quartz) and the sill

as a whole. If all the olivine of the sill be considered to have combined with some of the free quartz, there will still be at least 7 per cent of quartz left if averaged over the whole sill. Is this not a strong case for syntexis with the adjacent arkose ? In this connection an irregular 3 in. vein was found (near the base of the Englewood Cliff section) which appeared to consist of altered quartzose sediment.

(ii) The olivine ledge occurs about 60 feet from the base of the sill on top of normal diabase quite as coarse as that immediately above the ledge. Is not this position very high for a stratum supposed to rest on the chilled contact phase, even in spite of the great thickness (800 feet) of the intrusion ?

(iii) The percentage of olivine in the olivine ledge is at a maximum of 17 (by volume) at the centre, and falls off to 11 per cent at the top and bottom. How may this be reconciled with the settling of early formed olivine crystals through a fluid magma on to the solid contact phase ? Should not the percentage of olivine show a definite downward increase under such conditions ? There is, moreover, a complete absence of the packing of the olivine crystals which might be expected if the settling took place in a fluid magma. If, on the other hand, increasing viscosity ended the settling, why does not the olivine ledge show a gradational instead of a sharp upper contact ?

(iv) The hard " dikes " and bands in the olivine ledge, which appear to be intrusive into it, have the same composition as the olivine-rich diabase, with the addition of a considerable proportion of late biotite. May they not, therefore, represent the paths of juvenile and hydrothermal solutions which caused the development of the mica and a hardening effect upon the olivine-diabase ?

Another point of interest is the detection of abundant hypersthene and pigeonite besides the normal augite. I hope before the close of the year to complete a survey of the sill, which will find an answer to these questions. While this letter was being drafted, I learned that Dr. E. B. Bailey had a paper on the olivine ledge about to go to Press. Dr. Bailey very kindly allowed me to read the typescript, but, as his conclusions differ from mine in some particulars, discussion may conveniently be postponed until his paper is in print.

FREDERICK WALKER.

GEOLOGY DEPARTMENT,
UNIVERSITY OF ST. ANDREWS.
28th June, 1937.