

tends towards a maximum and a single specimen might consist of many parallel or concentric layers varying in depth to less than $\frac{1}{2}$ mm. This is particularly noticeable in those varieties that encrust fossils or that display contortions and other irregularities without obvious encrustation. As regards the extent of the "tufa" I have seen masses several square feet in area on weathered surfaces, and I have observed large colonies of fasciculate lithostrotionids with the corallites cemented together by radiating fibrous calcite in concentric layers and with dark blue-grey calcite mudstone as a final infilling between the encrusted corallites where these are not in actual contact. Such features seem to me to be original, but if any recrystallization has occurred it has been confined to the encrusting material, whether organic or inorganic: the calcite mud when present is unaffected. If such encrustations were organisms their nature has not been determined. In some cases, as Black points out, bryozoans are associated with the bands and certain of them may in fact be partly recrystallized bryozoa. In this respect it is interesting to note that Dr. Black finds no evidence in support of Dr. J. E. Prentice's statement (*Quart. Journ. Geol. Soc.*, 1951, cvi, 171) that the fibrous bands represent "in recrystallized state the layered skeletons of the reef-forming calcareous algae". In view of the many references to these structures, notably by Hudson, Bond, and Parkinson, it is rather curious that Prentice does not refer to the prevailing view of their origin.

I do not follow Black in his argument that the formation of tufa necessitates extensive uplifts, since in reef sedimentation much of the growth is near sea level. (See also Bond, *Quart. Journ. Geol. Soc.*, 1950, cv, 157, and *Geol. Mag.*, 1950, lxxxvii, 267.)

Dr. Black's strongest evidence for the secondary nature of the layers is his discovery of fossils partly in calcite mudstone and partly in the fibrous material, which brings me back to my original point of the possible mixed origin of the bands. But whatever the truth of the matter the structures have one thing in common; they occur only in deposits to which the name reef limestone has been applied. This indicates that the layers are indicative of something inherent in the mode of accumulation of the sediments.

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FOSSILIFEROUS STRATA AT KAPP SCANIA, DAUDMANNSØYRA, VEST SPITSBERGEN

SIR,—During the Birmingham University Expedition to Spitsbergen, 1948 (leader M. F. W. Holland), which was engaged in the geological survey of the southern part of Oscar II Land, B. H. Baker collected some fossils, mainly corals, at Kapp Scania. This locality lies well within a belt extending along the west coast of Spitsbergen which has long been known to consist of practically unfossiliferous folded rocks referred to the Hecla Hoek system of Pre-Devonian age. (A. K. Orvin, *Skrifter om Svalbard og Ishavet* No. 78, 1940.)

Last year a second collection was made from the same area by a party (including Holland) from the Oxford and Cambridge Spitsbergen Expedition, 1951.

Both collections have now been examined at Cambridge by one of us (C. L. Forbes) and the corals are found to belong to *Caninia calophylloides* (Holtedah) and related species, while the brachiopods present are probably *Choristites* sp. (the specimens are of poor quality). There are also a few fusulinids, completely silicified, but showing traces of deeply folded septa. The fauna seems to be of high Middle or low Upper Carboniferous age. A comparison of this fauna is being made with the extensive collection of the Cambridge Spitsbergen Expedition, 1949, from the Billefjorden region at present being examined (by C. L. Forbes).

The fossiliferous series has highly metamorphosed rocks of pre-Devonian appearance both to the East and West. Baker, Weiss, and others of the Birmingham Expeditions of 1948 and 1951 have been working on the tectonic implications in conjunction with their work in the St. Jonsfjorden area further north.

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THE AGE OF THE MALVERN FOLDING

SIR.—Perhaps the provider of part of the stimulant (or irritant according to viewpoint) which encouraged Mr. W. Mykura to produce his note on "The Age of the Malvern Folding" (*Geol. Mag.*, Nov.–Dec., 1951), may be allowed to welcome the new facts now available. Even a suggestion that Middle Coal Measures may be represented in the Abberley district is important, because it shows how unwise it is to assume that Middle Coal Measures cannot be present beneath the Trias on the east side of the Malverns.

I think, however, that Mr. Mykura's treatment of the tectonics obscures the unknown. In his text-fig. 2 he boldly shows at Wallhouse a post-Triassic fault with a minimum downthrow of 750 feet to the east, without any supporting evidence. And between Woodbury Hill and Walsgrove Hill he has shown a most remarkable tectonic phenomenon—a *normal* fault which, according to his map (text-fig. 1) follows the bedding for one mile, parallel to the outcrops to the west, turning a 45° corner in the process. Even if such a fault were probable, it requires the easterly outcrop of Aymestry Limestone to be inverted, which should be readily demonstrable, and also the presence of Lower Ludlow on the east. No evidence is given to support either of these requirements, and I am not aware that any has been published. Without such evidence, Groom's original interpretation of the Silurian structure is preferable.

There appears to be a misprint in the middle of p. 390; the dip of 73° to E. 10° S. in the Coal Seam on Ladywood Common, the most important piece of tectonic evidence in favour of post Morganian folding, is shown on the map directed W. 10° S. This dip is placed immediately east of a fault downthrowing 300 feet to the east. Is this throw firmly based, or has it been introduced to allow sufficient room for the steep dips recorded in the Coal Measures to flatten out to the West?

I have often wondered whether some of the folding on the west side of the Malvern line may not be of post-Triassic age. Professor W. F. Whittard (*Geol. Mag.*, 1949, p. 375 and fig. 1) has described post-Triassic thrust fault towards the north-west in the Chepstow area, and Dr. J. S. Turner (*Proc. Geol. Assoc.*, 1927, p. 372) has published evidence for a post-Permian thrust in the Westmorland Pennines. It seems therefore possible that some of the complications with north-south and north-east-south-west strikes in the intervening area may have been produced by post-Triassic compressive movements.

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