

THE MONASTIC REMAINS OF NORFOLK AND SUFFOLK. By C. J. W. MESSENT. Norwich : H. W. HUNT, 1934. Price 7s. 6d.

SOME years ago we reviewed an earlier book by the same author on old buildings in Norfolk because it contained some remarkably good descriptions of the various building-materials of the county. In the present work there is no geology, but it is of great interest in other ways and there are a number of charming sketches as illustrations, many of which are wisely chosen from the less well-known monastic remains of the district. In Suffolk there were about 100 monastic buildings and in Norfolk 180, the little town of Thetford alone having no less than thirteen entries in the list.

CORRESPONDENCE.

QUATERNARY CHANGES OF OCEAN LEVEL.

SIR,—Three articles by Cosmo Johns have appeared this year in your magazine. I did not pay much attention to the first (LXXI, p. 66) for three reasons. (1) It seemed to be an extension of W. B. Wright's hypothesis for Scandinavia, namely that the land recovered from isostatic depression as the ice melted and sometimes the sea rose faster than the land and formed the Yoldia and Tapes beaches. This hypothesis according to Wright includes a quicker recovery in the peripheral regions from isostatic depression (this is to explain the earlier recovery at Vendyssel and Uddevalla). The Ancyclus depression, however, was later in the north of the Gulf of Bothnia—well inside the peripheral regions—than in the south, so that Wright's hypothesis must be recast before being employed, much less extended. (2) If there was a general lowering of ocean level during the Quaternary at what times did the level rise to form the Quaternary beaches in North Spain at + 195, 150, 65, and 40 metres not to mention other sites? (3) An early lapse from logic. Mr. Johns says in effect (p. 67, 1st paragraph) "It could have been *a* or *b*. If it was *a*, I should not be able to prove what I want to prove, therefore it was *b*."

A fourth reason is now added by the recent discovery that the 100 ft. beach is not of the same age on both sides of Scotland—a fact which, I hope, finally explodes the theory that the raised beaches were formed as land ice melted into the ocean.

It would take too much space to go through the papers step by step, especially since Mr. Johns takes us through the domains of geophysics, glaciology, meteorology, and more with bewildering rapidity. Perhaps I might pick out one or two points here and there.

(*a*) The 100 ft. terrace is much earlier at Swanscombe than in the north: Acheulean implements (? pre-Riss) against Yoldia times (? Buhl). I might also point out that the lower Thames has

been twice or three times up to the 100 ft. level or near, i.e. the 100 and 85 ft. rock ledges, covered with gravel of Acheulean date, and dirty sand at 100–110 feet after Clacton-Levallois times—Burkitt has proposed ponding of the North Sea to account for the latter.

(b) In regard to the second paper (p. 176), the coral island problem is a ticklish one for anyone not personally acquainted with the region to tackle. There is an impartial discussion by Steers (*The Unstable Earth*, 1932) and the painstaking work of Chubb recently published (*GEOLOGICAL MAGAZINE*, LXXI, 189). If for the reasons given above anything to do with a 1,000 m. depression of ocean level is struck out as impossible, nothing much original is left.

(c) The third paper (p. 408) is a combination and amplification of the first two, and the geomorphology extremely speculative.

I must protest against Mr. Johns's description of his curve as a "Raised beach" curve. The only beaches he seems to know of are the 140,100 (these are grouped together. Why?) and 25 ft. beaches. Fig. 2 loses interest when it is known that the so-called "Yoldia sea submergence" did not occur contemporaneously up the east coast of Britain.

However, mere criticism is tedious, and I am glad that there are points on which one can agree with Mr. Johns's findings. One is that the major glaciation of Scotland can be placed in the second half of Penck's Mindel-Riss interval, another that there has been subsidence (though I do not agree to recovery) in the north Pacific.

Might I suggest that Mr. Johns is attacking a very interesting problem in the wrong way, namely by forming a hypothesis and selecting facts to fit it? Surely the scientific method is to discover first what were the earth movements (wide variations in ocean level can no longer be entertained), taking as a starting-point the raised beaches and river terraces round Europe, second, what caused them?

R. G. LEWIS.

BLOCK FAULTING IN THE WESTERN RIFT OF CENTRAL AFRICA.

SIR,—In a letter published in the June issue of the *GEOLOGICAL MAGAZINE*, Dr. E. O. Teale recorded observations by himself and Mr. G. M. Stockley to show that compression phenomena in the rocks along the margins of the Rift in Tanganyika Territory are very ancient and long antedate the present scarps, and that the Rift fractures are everywhere best interpreted as associated with block movements. Mr. Stockley refers particularly to pre-Rift overthrusting in the Livingstone Mountains, and to block-faulting there and in the Ruhuhu trough; both these areas lie on the eastern side of Lake Nyasa.