

laminated shale". Again, no marine fossils had been detected. The 1-in. geological map (Sheet 123) shows, in the extreme N.E. corner, this Wall Grange grit as Third Grit, with a south-westerly dip of 20°

A few weeks ago I picked up on the floor of the quarry some fossiliferous shale which was kindly examined by Dr. C. J. Stubblefield, who reported that it contained "*Aviculopecten* cf. *losseni* (von Koenen), *Dunbarella* sp., *Anthracoceras* sp., *Gastrioceras* sp. with some fragments showing widely spaced crenulate ornament". He suspected that the horizon might be that of *G. cumbriense*, though further material would be required for confirmation.

On a later brief visit more shale was picked up, which though badly weathered appeared to contain *Gastrioceras cancellatum* and *Reticuloceras* sp. *Pterinopecten* was also present. Dr. F. Wolverson Cope examined the specimens and confirmed the presence of *Gastrioceras cancellatum* Bisat together with *Reticuloceras reticulatum* mut. *superbilingue* Bisat.

The *cancellatum* band and probably also the *cumbriense* band were inferred to occur somewhere in the quarry. A field meeting of the North Staffordshire Field Club visited the Wall Grange brick-pit on 31st May, and Dr. Cope soon found the *cancellatum* band. Good specimens of *Gastrioceras cancellatum*, *Reticuloceras reticulatum* mut. *superbilingue*, *Pterinopecten* sp., *Orthoceras* sp., fish spines and scales, were obtained from it. The *cumbriense* band was also located some 20 to 30 feet higher, but direct measurement was not possible because of shale scree. The finding *in situ* of these two marine bands, hitherto unrecorded in this brick-pit, proves quite definitely that the capping grit is the Rough Rock, or First Grit.

J. MYERS.

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NEWCASTLE,  
STAFFS.

9th June, 1951.

#### RED SEA RIFTING

SIR,—Mr. Arkell wrote a rather emotional and unfriendly article in the *Geological Magazine* of January, 1951, in which he accuses me of not giving credit to British Geologists in Egypt. This article reached me only to-day, due to continuous travelling during the last nine months, and would not have been written by Mr. Arkell if he had followed the international custom of sending me a copy of his complaints before publishing the article.

In my article on the macrostratigraphy of Egypt, which should have appeared in September, 1950, but is still in print in Cairo, I gave a selection of references (altogether 74), including three from Andrew, four from Ball, seven from Beadnell, eight from Hume, two from Moon and Sadek, and many others from British geologists. One of my first sentences in chapter 2 of my article ("Tromp 1950" referred to by Mr. Arkell) reads: "Since 1897 considerable reconnaissance work has been done by the Geological Survey of Egypt, in particular by the geologists Ball (bibl. nos. 17–20), Barron (bibl. nos. 21–3), Beadnell (bibl. nos. 25–31), Hume (bibl. nos. 48–9), Little (bibl. nos. 51–3), Lucas, Madgwick (bibl. nos. 45–7), Moon (bibl. nos. 45–7), Sadek (bibl. nos. 54, 55, and 55), and others. Local studies were made by Blanckenhorn, Zittel, etc., etc."

A little further: "Up to 1935 the best compilation of the geology of Egypt was written by W. T. Hume and his collaborators, which was published in four magistral volumes, incorporating an incredible amount of interesting facts on the geology of Egypt."

Still Mr. Arkell, who apparently did not try to contact me first, claims that I did not give sufficient credit to British geologists in Egypt.

Much of the geology of Egypt has been changed, however, during the last ten years, as a result of the work of the oil companies. Mr. Arkell will be convinced as soon as he receives a copy of my article. "The astonishing second sentence of my opening paragraph" (as Mr. Arkell calls it), refers to

Krenkel and many other non-Egyptian geologists (i.e. geologists who did not work in Egypt for many years) who developed theories on the Red Sea graben without knowing in detail the stratigraphy and structural features of Egypt.

Once again, a less emotional approach in future would help to improve international relations amongst geologists.

DR. S. W. TROMP,

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RIJNSBURGERWEG 159,

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4th June, 1951.

SIR,—I am much relieved to learn that Dr. Tromp has not overlooked the work of the Geological Survey of Egypt and that it is to receive due recognition in his forthcoming publication. My letter, however, referred necessarily to his published paper. If I was guilty of emotion, it was indignation, because in his paper Dr. Tromp attributed to himself a number of discoveries for which the credit belongs elsewhere.

W. J. ARKELL.

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10th June, 1951.

#### SCOURIE DYKES AND LAXFORDIAN METAMORPHISM

SIR,—In his paper "Scourie Dykes and Laxfordian Metamorphism" (*Geol. Mag.*, lxxxviii, 153) Sir Edward Bailey makes a number of severe comments based on abstracts of papers presented by us to the Geological Society (*Abstr. Proc. Geol. Soc.*, 1950, 51–3) concerning parts of the Lewisian of Scotland. These comments amount to a statement, firstly, that we had failed to acknowledge our debt to the great body of knowledge built up by previous workers and set out principally in the North-West Highland Memoir of 1907, and secondly, that we have produced neither new material nor new ideas.

The papers (which are now in the press and will appear as one paper in the *Q.J.G.S.*, cvi, 1951) begin with what we hope is an adequate outline of earlier work. We refer therein specifically to the pages of the North-West Highland Memoir dealing with the Torridon and Scourie areas now in question. When the papers were delivered, care was taken to give the substance of the quotations and references with which our paper opens and to outline the present state of knowledge of the two areas.

It is unfortunate that, as Sir Edward Bailey was not present at the meeting, and because the paper has not yet been published, his knowledge of its contents appears to be confined to what was set out in the printed abstracts. The manuscripts were deposited in August, 1949, with the Geological Society, where they were available for consultation on application. Had we known that Sir Edward Bailey held such strong views on the abstracts we should ourselves have been happy to send him a copy of the complete manuscript.

The prevalent conception of the Lewisian has been epitomized by Peach and Horne (*Chapters on the Geology of Scotland*, 1930, p. 24). They write: "The detailed mapping of that region [the western seaboard of Ross and Sutherland] by the Geological Survey has shown that the rocks are divisible into (1) a Fundamental Complex, consisting mainly of gneisses that have affinities, both chemically and mineralogically, with plutonic igneous products, and partly of crystalline schists which are evidently of sedimentary origin; (2) a series of igneous rocks intrusive in that complex in the form of dykes, sills, and irregular veins."

Sir Edward has recently written of our Abstracts: "Like the authors of these summaries, I agree with the bulk of the striking results set out by Teall and his colleagues." We cannot, however, agree with such a complete correlation of our opinion with that of the authors of the Memoir of 1907. Thus, we reject the view that the complex is mainly orthogneiss; we reject