

two *Goliathiceras*) from the Elsworth Rock which only occur (elsewhere) in the Upper Oxford Clay and the Lower Calcareous Grit, I am afraid that I cannot alter the views that I have already expressed.

Nor am I disposed to accept whole-heartedly Dr. Arkell's corrections of the identifications given in my Kachh Memoir, for example, of the Indian species which he does not know. But, since the personal element often influences identifications, it would weary your readers to pursue the subject. Suffice to say that only the latest elements of the Elsworth Rock ammonite fauna, i.e. those which date the bed are of *transversarium* age, and that in my opinion this greatly condensed rock also includes ammonites derived from older deposits.

What I objected to principally was that Dr. Arkell quoted from my text without reference to the table round which it was written, and which would have prevented any ambiguity about the various zonal names. Such terms as "transitional" or "confirmatory" lose their significance apart from the table, where the true position of *Cardioceras rouillieri*, for example, is clearly indicated.

The phrase about the Elsworth Rock at Upware being in close association with a coral reef is a quotation and should have been printed as such. I was concerned with the ammonite succession and not with questions as to where the "rock" is supposed to be; but I did establish the *transversarium* age of the coral reef the same as I did the Elsworth Rock. The "transition beds" with *C. rouillieri*, etc., are clearly not the Elsworth Rock but the beds transitional from Oxford Clay to the (now missing) Lower Corallian, which, at Elsworth, supplied some derived elements of the fauna. There is, then, in my view, no doubt that the Elsworth Rock is really a "mixed deposit".

L. F. SPATH.

BRITISH MUSEUM (NATURAL HISTORY),
16th March, 1938.

THE FLOOR OF THE ARABIAN SEA.

SIR,—We have read with interest the recent criticism by Dr. G. M. Lees (*GEOL. MAG.*, 1938, p. 143) of a paper on the Floor of the Arabian Sea published by us, and we would like to take this opportunity of replying to the points raised in that letter.

First, it is incorrect to claim that we state on page 223 of the *GEOLOGICAL MAGAZINE* for 1937 that on the Kuria Muria Islands a granite is intrusive into an overlying sandstone of *Miocene Age*. We do state that the granite is intrusive into a sandstone formation, but there is no mention on this page, or anywhere else in the paper, that the sandstone on the Kuria Muria Islands is of *Miocene Age*. That the granite is intrusive was inferred by one of the authors (R. B. S. S.) both from the upward tilting of the practically horizontal strata on approaching the granite contact, and from its

fragmentation at the contact. Perhaps Dr. Lees has been confused, since later on we mention that some of the sandstones and limestones along the south-east coast of Arabia are almost certainly the same age as the Makran group of sandstones; this claim was based on the report by Dr. R. D. Oldham, of the Geological Survey of India, that in the Sur Hinterland, to the west of Cape Ras al Hadd, there are thick deposits of Miocene age—a statement that Dr. Lees himself quotes in his paper (*Quart. Journ. Geol. Soc.*, lxxxiv, 610).

Secondly, Dr. Lees says that the discovery of a double ridge is an insufficient basis for the postulation of a Rift Valley. Here again there is a discrepancy between the original paper and Dr. Lees' interpretation. In the original paper we referred to the *apparent* similarity that exists between the *topography* of the floor of the Arabian Sea and the region to the west of it that is characterized by the Great Rift Valley, but we did *not* state that the double ridges are rifts.

Thirdly, we would be the first to agree that there are possible alternatives to fault scarping along the Arabian Coast, but we consider that faulting fits the facts, as they are known to-day, better than any other hypothesis. We pointed out that the soundings along the Arabian Coast are irregular and may possibly indicate submarine valleys, but since many authors consider that submarine valleys may be formed by means other than sub-aerial erosion, it would seem unwise (even if their existence had been definitely proved) to put this forward as an objection against faulting.

Fourthly, Dr. Lees claims that the ridges on the floor of the Arabian Sea undoubtedly "follow the trend of older fold systems, which according to evidence in Oman, may be of any age from Upper Cretaceous onwards". This may be so, but we would like to point out that there is no submarine evidence that the Murray Ridge has any connection with the Oman Range. The soundings in this region indicate that the Murray Ridge does not join the Oman Range at Ras al Hadd; a gap of some 70 miles with a depth of water of 1,750 fathoms separating the two.

We are in full agreement with Dr. Lees when he states that the area would repay further investigation and we are quite aware of the importance of geophysical investigations and the necessity for close collaboration between the various sciences involved. Finally, we think it is obvious to those who realize the scarcity of available data regarding the oceans, covering as they do nearly three-fourths of the surface of the earth, that any paper must of necessity be of a preliminary nature and not be regarded as a final synthesis.

R. B. SEYMOUR SEWELL.
JOHN D. H. WISEMAN.

9th April, 1938.