

so that the Carboniferous goniatite zonal scheme can be used in Devon to establish the succession and hence the structure. It is a fact worth emphasizing that so far this coarser-grained group has nowhere yielded goniatites, so that all correlations of the different outcrops of this group are as yet purely on a lithological basis. However, I have recently collected *Homoceras* in shales near the River Inny north of Stoke Climsland—that is, in an area of Dr. Simpson's Ugbrooke Group (p. 204).

The author remarks in conclusion (p. 207) on the fact that in the Tavistock Sheet (337) of the Geological Survey unmetamorphosed Culm Measures are shown *well within* the metamorphic aureole of the Bodmin Moor granite near Altarnun. From this he argues the probable post-granite age of the Ugbrooke Group. Surely the answer is more likely to be that this is a draughtsman's error, especially as there appears to be no mention of the matter anywhere in the accompanying memoir (Reid and others, 1911). That such errors can occur is seen from the 1913 edition of the Newton Abbot Sheet (339) in which a small patch of metamorphosed Permian is shown adjacent to the Dartmoor granite at Woolley near Bovey Tracey. One could argue from this the post-Permian age of the Dartmoor granite!

In any case, Dr. Coles Phillips showed in 1928 (Fig. 1) that the strip of Culm Measures near Altarnun definitely occurs *outside* the outer limit of the thermal aureole of the Bodmin Moor granite.

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THE SUPPOSED EVOLUTION OF *GRYPHAEA*

SIR,—I regret that Professor Swinnerton chose to word his criticisms of my work in such intemperate language; I shall nevertheless endeavour to reply briefly to the relevant points he raises.

There is surely a *non sequitur* in his opening argument for he states, referring to gryphaeid coiling, that "If such coiling be indeed a function of size then some of the giant forms which lived later in the Jurassic should be as closely coiled as an ammonite." I fail to see that there is anything in my argument that implies this. In regard to my choice of an index of size, the work of MacLennan and Trueman shows that the length of the right valve is satisfactory in this respect and that no substantially different result is to be expected if width is measured. I did incidentally measure width as well and my findings confirmed this. Depth cannot reliably be used as a size index as it is too intimately bound up with coiling.

Professor Swinnerton goes on to suggest that as my work on *Liostrea* is not supported by statistics there is a marked hiatus in my argument. Though I did a laborious analysis on *Gryphaea* I followed in the case of *Liostrea* the principle that no statistics are better than weak statistics, because rejection

of the statistics might obscure some fundamental observational truth. In the case in question, I am confident that I could demonstrate the lack of a gryphaeoid trend up the succession to an unprejudiced observer in any well-exposed Hettangian succession in Britain. This conclusion is not to be lightly disregarded, for I have made careful observations over a far wider area than either Trueman or Swinnerton considered. My observations can be readily checked and compared with Trueman's by any interested party; the pre-Planorbis oysters provide particularly promising material for this. I cannot deny that a particular conclusion of mine "cannot possibly have any experimental or directly observational proof", but, of course, the same consideration applies to Trueman's own hypothesis.

In the case of *Gryphaea* I measured and plotted *all* the specimens I collected, so readers may judge for themselves whether a high proportion had areas of attachment large enough to cause wide scatter. In fact, specimens with areas larger than 2 mm. never approached anything like 40 per cent of my material in any of the areas that I studied. It is not true that I made no attempt to locate Trueman's original collection, for I made abortive enquiries at a large number of likely town and university museums. Moreover, I have had a good look at Trueman's localities.

My methods of sampling are attacked on the grounds that I lumped together collections from different areas and different subzones. Actually I split my samples for the size frequency histograms and found that in respect to mean and maximum size geographical variations were negligible. But I consider that Swinnerton's geographical variation argument is invalid anyway, because it has always been assumed (and this is why it is so important) that the evolution of *Gryphaea* was *general* over Britain at least. If there were a genuine evolution in Trueman's area there must elsewhere have been subtle reversals of this trend to give a resultant "no evolution" in the general picture. Would Professor Swinnerton seriously subscribe to some such *ad hoc* hypothesis? Similarly, I was quite justified in lumping together collections from the *bucklandi* and *gmuendense* Subzones for the particular matter under consideration, which was to decide if the facts supported Trueman's suggestion that there was a significant difference between the horizon represented by these adjacent subzones and a lower horizon, the *angulata* Subzone, three subzones below. I wish to mention here that my raw data, for each region considered, were excluded from my paper for reasons of space, but are available in a dissertation I am shortly to present, together with details on collecting localities.

There is a certain irony behind many of the remarks Swinnerton directs at my methods, for they imply criticisms of Trueman's original work of 1922, which is the only relevant comparison in the present argument. If one examines this all-important work one finds surprisingly little information on collecting localities and exact horizons and virtually no statistical data. In fact it is not evident from the paper, which is all we have to go on, that Trueman made careful measurements on large numbers of oysters, as is generally assumed. If I accused Trueman of bias in his collecting it was because I am convinced that his results are quite at variance with reality; I am pained at the insinuation that in doing so I have been insulting to Trueman's honour. I respect Trueman as an imaginative worker who, like Professor Swinnerton, has done much to stimulate palaeontological thought, but I honestly believe that in his early work on *Gryphaea* he did not check an interesting idea with sufficient observation and measurement. Even so, I object less to this pioneer effort than to the uncritical acceptance it has subsequently received.

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