CORRESPONDENCE

ORIGIN OF FRACTURE AND SLATY CLEAVAGE

Sɪʀ,—W. K. Fyson (1962) states that the distance apart of bedding-plane partings and foliations influence the size of folds in the Devonian rocks near Plymouth, and infers that movements along these planes were important in the development of the folds. He considers that there was a similar mechanical origin for both the fracture and slaty cleavage developed during folding, and it is pointed out that the usual lack of displacement, the flattening, and the formation parallel to axial planes indicates that the cleavages lie in direction of extension and not in the directions of shear oblique to the compression. Whereas the soundness of these conclusions is not questioned, his belief that bedding-plane shear caused the cleavages associated with the folds appears to be unwarranted.

Fyson argues (p. 222) that with flexural slip along bedding planes in plastic material the maximum extension in cross-section within any segment of fold limbs is at 45 degrees to the bedding so that platy minerals and cleavage form (by mechanical rotation or recrystallization) in this direction. According to this argument orientation of mineral grains within the deformed material is not within or symmetrical to the transportation direction—which is difficult to accept.

A number of examples are figured by Fyson (p. 212) where folded confined and cleaved beds show considerable thickening and thinning, and he states that some beds often pinch out completely on the short limbs of upright and isoclinal folds. Using similar examples of folds the present writer has shown that within incompetent layers the effect of bedding-plane drag must be negligible, and considered that the cleavage formed in the direction of mass elongation, which is *also* the direction of plastic flow (1961, *a* and *b*). Clearly, this mechanism can be applied to any bed that has behaved in the least bit incompetently—even within folds where movements along bedding planes may have been important in the fold development.

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44 Sᴏʙᴇʀᴛᴏɴ Aᴠᴇɴᴜᴇ,
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Sɪʀ,—E. Williams disagrees with the concepts (*a*) that during folding of incompetent beds internal movement parallel to the bedding could be significant and that (*b*) this movement could lead to the development of oblique extension and hence cleavage.

Considering (*a*), Williams states he has shown that in incompetent beds (i.e. those that change in thickness) the effects of bedding-plane drag must be negligible. But his evidence is restricted to the folding of incompetent beds between those unchanging in thickness, with the mass movement of the former to or from the hinge (above reference 1961*b*, p. 318). This type of folding, which in its extreme form is expressed by markedly disharmonic folds, has been observed elsewhere by the writer to be unaccompanied by well-developed cleavage. Where cleavages are well-developed, however, as in the Plymouth area, beds of all lithologies change thickness around the folds and the folding is not disharmonic. Williams also ignores the important role that movement parallel to the fold axes has in producing these changes in thickness.

With regard to (*b*), it can be readily demonstrated, as shown in Text-fig. 7 in the writer's paper under discussion, that " one-sided " laminar movement or flow parallel to the bedding leads to extension of form ellipses oblique to fold limbs. Pushing over a pack of cards can illustrate the same principle. Only in the special case of perfectly equal movement in opposite senses does the direction of laminar flow coincide with that of extension. The cleavages, as Williams agrees, develop in the directions of extension oblique to the bedding.

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THE EVOLUTION OF *GRYPHAEA*

SIR,—While it is reassuring that Philip (1962), in his new analysis of *Gryphaea*, confirms my belief that the ratio of the periphery (P) to the length of the right valve (R) is an adequate measure of the tightness of coiling, the way he uses my measurements by treating their relationship as linear is open to serious objection. As he seems to betray a failure to appreciate the biological grounds which led me to use logarithms I shall have to be much more explicit than in my original paper.

When I made direct plots of P against R I did not find a simple linear relationship (text-figures 1 and 2). Considering firstly the best balanced sample in regard to size, from the *gmuendense* (= *reynesi*) Subzone of Glamorgan, it will be seen that a line drawn through the points up to an R value of about 25 mm. will miss most of the points for the larger specimens which fall on a slightly steeper curve. The beginning of a similar curvilinear upward trend is also seen in the upper *angulata* (= *complanata*) Subzone sample for the rest of England and there is no reason to believe that the remaining two samples exhibit different principles of growth. These graphs reflect what is clearly apparent on handling individual specimens, that the larger the size the tighter the coiling. These results suggested to me the possibility of a relationship of allometric type, which was confirmed when the logarithmic plots (text-figures 2 and 3 of my 1959 paper) were seen to conform much more closely to a straight line, indicating that the direct P/R relationship is probably exponential.

Now the study of change in shape with size, far from being heretical, has become eminently orthodox in evolutionary studies (Simpson 1953, pp. 25–9) and the large body of work undertaken since the pioneer investigations of Huxley (1932) on allometric or relative growth make imperative a proper evaluation of this factor whenever the slightest suspicion exists of deviation from simple linear relationships of different measures. Yet Philip shows no evident appreciation of its importance. His key equation (v) assumes an equiangular spiral, which *Gryphaea* manifestly is not, since if it were P and R would plot as a straight line, and his treatment of variation only in adult shells becomes pointless. It is not surprising that his P/R regression lines of the younger. larger forms show a highly significant increase in tightness of coiling compared with the older, but in the circumstances such results can have little meaning in terms of evolution.

Philip, of course, like Trueman, recognizes that tightness of coiling may increase during the life of at least some individuals but explains this as the consequence of selection for the more incurved forms. In proposing this explanation, he makes the statement that " the small individuals which died at an early age are usually loosely coiled and possess shell profiles not present in the initial portions of the adult specimens which survived to maturity ". The latter part of this statement does not accord with my own observations, nor is there a suggestion of anything but a continuum in the growth series at given horizons as plotted in text-figures 1 and 2.