

fifty species and subspecies of planktonic foraminifera found in the shaft as a further contribution to the intercontinental correlation of the Tertiary rocks.

REFERENCES

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31st August, 1958.

DEVELOPMENT OF LINEATION IN COMPLEX FOLD SYSTEMS

SIR,—Mr. P. A. Hill expresses the view in his letter (*Geol. Mag.*, 1958, xcv, 351), that a paper (*ibid.*, xciv, 1-24, 1957) by Clifford, Fleuty, Ramsay, Sutton, and Watson is, in parts, of "spurious accuracy", "statistically invalid", "over-written", and expressed in "jargon". When Hill goes on to add "that these remarks are in some ways unfair, as they apply to many other workers" I am tempted to comment that perhaps we are all out of step except George.

"Spurious accuracy." Hill criticizes "dip symbols of 31°, 59°, 71°, etc." on our fig. 8 in the paper "a map of country where variation in dip is extreme". There are in fact no dip symbols on this map, but I will take it that Hill means structural observations in general. I had thought that the view that it was worthless to make accurate measurements in such ground was dying out. Rightly or wrongly our procedure has been to take large numbers of observations and to make each as accurate as we could. By doing this one can hope to find out whether there is system in the variations. Ramsay's field map of the ground covered by fig. 8 contains over 4,000 structural measurements. The analysis and presentation of such large masses of data raises problems. In the paper we used three methods of presentation—stereograms (figs. 6, 10, 11); sketch-maps showing generalized lines of strike with the directions of dip indicated (figs. 3, 4, 6, 10), or generalized directions of lineations (figs. 7, 10, 11); maps showing observed values where there was some special reason for publishing these (fig. 8). One of the reasons for drawing fig. 8, criticized by Hill, was to show the *distribution* of two sets of linear structures, to demonstrate how one set survives near the axis of an early fold, but is largely destroyed on the limbs. It is disheartening to find that Hill, as a serious critic of the paper, states that these clearly labelled symbols for lineations represent dips of bedding planes or foliations.

Hill states that our fig. 10 is "statistically invalid". He claims that the distribution of 250 foliation readings shown on this figure is not given. The information appears, however, on p.16, lines 9-11. Hill also claims that the scatter of these readings is not shown. It is, however, shown by the stereogram, fig. 10b. This stereogram also indicates that the fold axis in the ground discussed by Fleuty is not bent in the manner Hill suggests. Although Hill has failed to grasp two of the facts shown by this stereogram he is content to end his letter with the *ex cathedra* remark (after no discussion of the matter at all) that stereographic projections are used as window dressing.

"Over-writing." Hill asks why 26 words are needed to say that a fold plunges steeply. This seems a reasonable question until one turns up the reference (p. 16, lines 1 and 2) and finds that the sentence says nothing about the amount of plunge but describes the form and attitude of a rather unusual fold.

“Jargon.” Here I am on delicate ground. What seems all right to one is jargon to another, but I am surprised at the terms Hill objects to. Bailey’s terms *antiform* and *synform* have been in use for a quarter of a century and describe succinctly the form of folds in ground where the rocks are inverted or the succession is not known. I am sorry they are strange to Hill. We used hinge in the sense it has been used by Bailey (*Tectonic Essays*, 1935, pp. 51, 91, 156) and as defined by de Margerie and Heim (*Les dislocations de l’écorce terrestre*, Zurich, 1888. French text, pp. 50 and 51).

I find it difficult to comment on Hill’s discussion of the main purpose of the paper as he seems so frequently to have got hold of the wrong end of the stick or to have missed the relevant passage in our text. Here are some examples of what I have in mind. Hill complains that we do not seriously consider “rotation simultaneously with or during the closing stages of folding.” Yet pages 5–8 of the paper are concerned with movements of varying complexity which we consider to have occurred simultaneously.

On page 8 (last para.) and on page 9 (see also fig. 5) we deal specifically with the effects of renewed deformation during the closing stages of folding. Incidentally Hill’s comments on our fig. 5 are particularly curious for we specifically state in the text that the wrinkles are not related to the fold depicted, but are later than it.

Hill appears sceptical about our recognition of earlier and later structures. When one set of structures cuts across another or is superimposed upon another, it appears to us reasonable to accept that one is later than the other. Pages 11 to 19 deal with such matters and the accompanying figures illustrate them. It appears to me that Hill has misread one of the most important figures in this part, as we have already seen (fig. 8); he says himself that he does not see the purpose of another (fig. 9) though this seems straightforward enough. He has failed to see the strike lines clearly indicated on fig. 6 and has missed the information fig. 10*b* provides. His note concerning the Tarvie syncline and our fig. 7, placed in brackets in his letter, suggests that a long passage (para. 3 of our page 12) dealing with the same matter has escaped him. How seriously can one take the comments of a reader who goes through a paper in so slap-dash a fashion?

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6th September, 1958.

DERIVED AMMONITES IN BASAL CRETACEOUS CONGLOMERATE

STR.—In the course of a recent field class in Northern Ireland, one of my students, Mr. J. A. Hirst, discovered in the Basement Conglomerate of the Cretaceous a *remanié* Middle Lias ammonite. The precise locality was the roadside section above Binvane Farm, Murlough Bay. The specimen has been identified by Dr. M. K. Howarth as *Pleuroceras transiens* (Frentzen) which in Britain is known only from a few feet of strata near the junction of the *margaritatus* and *spinatum* zones in the Middle Lias of Raasay. Although there is a record of derived Upper Lias fossils in the Cretaceous conglomerate (Hartley, J. J., 1933, *Irish Naturalists Journ.*, vol. iv, p. 238), this is, so far as I am aware, the first record of Middle Lias forms. The specimen is now in the Geological Survey Museum (GSM 96788).

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9th September, 1958.