

"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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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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