is certainly in no way opposed to my view, nor does it limit the amount of uplift to this figure. It is interesting, as previously little definite evidence of uplift had been found on this island.

Professor Williams agrees with me that there has been extensive faulting, leading to the disappearance of parts of some of the islands. This involves either a downthrow of at least 2,000 or 3,000 feet on one side of a fault, or an upthrow of similar amount on the other side. The latter alternative seems to explain all the known facts. That such faulting has taken place does not support Professor Williams's conclusion that the area is a relatively stable one.

Professor Williams questions too my method of estimating the amount of the submergence suffered by the Marquesas Islands at a later stage in their history. I assumed that originally the valleys were " V "-shaped, and the slopes of their sides were the same as they are now; then, the width of an embayment being known, a simple calculation will give its rock-bottom depth. The first assumption is completely justifiable, as, except where they have been partially filled in with deltaic deposits since submergence, the valleys are still " V "-shaped and the drainage cannot have been more mature before submergence than it is now. Steepening of the valley sides since submergence is unlikely, as the tendency of subaerial erosion will have been to lower their gradients. The embayments may have been slightly widened by marine erosion, but as far as possible I allowed for this in my calculations.

I reckoned the amount of submergence of Hivaoa at 600 feet, and this is more likely to be an under- than an over-estimate. The post-glacial rise in sea-level can account for only a part of it.

L. J. CHUBB.

UNIVERSITY COLLEGE,
LONDON.
10th October, 1933.

BRECCIAS IN THE WARWICKSHIRE COALFIELD

SIR,—In a paper recently published in this Magazine,[1] the author makes a brief reference to a publication [2] of mine which may give, unintentionally, a misleading impression of some of the conclusions come to in the course of my work among the Red Rocks of the Midlands. He says (p. 474) that I stated " that the unconformity below the (Clent) breccia is greater in magnitude than that between it and the Bunter ". What I said was that the break between the Clent breccias and the overlying Bunter in the Birmingham area is generally not so pronounced as that at their base. I was dealing specially in that paper with the lithological evidence, and did not mean that the unconformity at the base of the Bunter in the Midlands

[1] F. W. Shotton, " New Evidence on the Origin of Breccias and Conglomerates in the Warwickshire Coalfield," GEOL. MAG., October, 1933.

[2] W. S. Boulton, " The Rocks between the Carboniferous and the Trias in the Birmingham district," *Quart. Journ. Geol. Soc.*, lxxxix, 1933.

generally is of less magnitude than that at the base of the Clent Beds. Still less did I mean to imply that the unconformity at the base of the Keuper is less, for the latter is seen to be unconformable to the Bunter in some places in the Midlands. As to whether the Clent Beds are of Carboniferous or Permian age, it is obvious that in the absence of fossils there can be no satisfactory proof. But I have always felt that the onus rests with those who link them with the Carboniferous to show good evidence for removing them from the Permian. In the Birmingham area, the distinctive composition and source of these deposits, and the pronounced unconformity at their base, seem to me to justify in the meantime their retention in the Permian.

W. S. BOULTON.

40 OAKFIELD ROAD,
SELLY PARK, BIRMINGHAM.
6th November, 1933.

ON THE PREPARATION OF GEOLOGICAL MANUSCRIPTS

SIR,—May I be permitted to welcome the Editor's remarks on the preparation of Geological Manuscripts and at the same time to offer a few comments.

As a palaeontologist, I am gratified to learn that " palaeontologists do seem to know their job " ; still the Editor's difficulty in understanding when the name of the author of a species should be enclosed in parentheses may possibly be the fault of those writers. The rule simply is that when a species has been transferred from the genus in which its original author placed it to another genus, then, and then only, is the name of its author placed in parentheses.

Some writers make a fetish of the author's name attached to a species name. The main reason for giving it at all is to avoid confusion with possible homonyms, and it may for the same reason be necessary to give the date as well. In any case the information is helpful to a reader who may wish to look up the species. But once this information has been given, it is useless pedantry to keep repeating it throughout a paper.

The principles laid down for the use of capitals in the names of rocks, minerals, and stratigraphical divisions seem exactly to meet the case. One occasionally meets with a use of the word " carboniferous " different from any of the examples given. When a rock of any age, e.g. Miocene, contains coal, it may therefore be called a carboniferous rock, although it does not form part of the Carboniferous system.

Coming now to the method of quoting previous publications, I ask leave to draw attention to the various Reports of the British Association's Committee on Zoological Bibliography and Publication, which in this respect apply equally to palaeontology and geology.