

RULES OF NOMENCLATURE

SIR,—My letter on clause 54 (1) (a) of the “Copenhagen Decisions” (*Geol. Mag.*, 1954, xci, p. 174) was not written without previous discussion with other workers, all of whom agreed with my view, and I have since had letters of support from institutions and individuals as far apart as the United States and New Zealand. Dr. Sabrosky (*Geol. Mag.*, 1954, xci, p. 325) is the first to suggest that we have “misread the decision and misjudged its effects”.

This contention appears to me to be quite untenable, for the decision taken by the Copenhagen Congress at the outset of its revision of the Articles of the Code relating to family names, namely the decision to revoke the existing Articles 4 and 5 (1953, Copenhagen Decisions on Zoological Nomenclature, page 32, decision 43) can only mean that the new provisions then adopted apply to all family names, irrespective of the date on which they were published. I therefore cannot accept Dr. Sabrosky's assertion that we have misjudged the effects of the Copenhagen decision.

Apart from this, clause 54 (1) (a) seems to me to be wrong in principle, for it states as a Rule that wrong names are to be used in preference to correct ones. We all recognize that any rules must at times produce an unfortunate result in particular cases, and that such cases should be adjusted by a special act of the Commission under its plenary powers. No limit has been set to the number of such cases that may be brought forward for individual legislation. It is not, however, the *correctly* formed names that should be suppressed by Rule and require this protection to survive, but the nomenclaturally incorrect names, i.e. those formed on junior synonyms. As remarked in my previous letter, the Rule should state the obvious, which is that the name of a family is to be formed on the valid name of its type genus.

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THE MIOCENE/OLIGOCENE BOUNDARY IN THE CARIBBEAN REGION

SIR,—The paper by F. E. Eames in this Magazine (vol. xc, No. 6, December, 1953), and the subsequent exchange of opinions between Drs. Eames and Stainforth (vol. xci, Nos. 2 and 4), are of unusual interest to students of Caribbean stratigraphy.

Eames certainly demands drastic changes in the Upper Tertiary stratigraphy of the Caribbean region but there is no sense in trying to shirk the issue at stake. We are not in a position to contradict his contentions but it seems prudent and scientific to consider seriously his conclusions. Is it only a coincidence that almost all specialists in larger foraminifera with intimate knowledge of faunal assemblages in Europe, North Africa, and the Near and Far East, were, and seemingly still are, inclined to attribute a younger age to post-Eocene larger foraminifera from the Caribbean region than is admitted by their colleagues in the Americas? Why the common reference of students of smaller foraminifera to the close relationship of Miocene-Pliocene assemblages of the Indo-Pacific with Oligocene ones of the Caribbean region? In the Paleocene and Eocene we accept without hesitation the fact that pelagic forms may be used as universal time markers. There is, therefore, no reason why the same or related genera should not have the same stratigraphic value in younger beds. At least until the end of the Oligocene there was free connection between Atlantic and Pacific provinces across the Darien isthmus (Woodring, 1954, p. 728). This being the case there was nothing to stop free interchange between the two areas at this period, let