

under the best circumstances in which nomenclatural decisions have ever been taken.

In England, more than twenty-five years ago, the British National Committee on Entomological Nomenclature formally proposed that a family name was not to be changed unless it or the name of its type genus was found to be a homonym. That Committee included such well known zoologists as Karl Jordan, S. A. Neave, and G. A. K. Marshall. Support for this solution of the problem has been growing in the last decade, and those who now wish to repeal it should realize the large amount of opinion on the other side. In the *Bulletin of Zoological Nomenclature* for July, 1953 (vol. 8, nos. 6/9), in which were published the views of Arkell and Moore, there are a number of statements in favour of *not* changing family names in cases of generic synonymy. The proposition of not changing was supported by several groups which gave special consideration to the major problems to come before the Copenhagen Congress—namely, the Nomenclature Committee of the Society of Systematic Zoology (W. I. Follett, Chairman: six out of eight members clearly expressing support), the American Committee on Entomological Nomenclature (C. D. Michener, Chairman), the Committee on Nomenclature of the American Museum of Natural History (John T. Zimmer, Chairman), and the Nomenclature Discussion Group of Washington, D.C. (R. E. Blackwelder, Secretary: approved by nearly two to one majority). Paleontologists are represented in three of those four groups. In a specialized field, the insect order Diptera, a questionnaire sent to dipterists all over the world in 1952 showed 69 per cent of 166 votes in favour of the solution as later adopted at Copenhagen. It thus appears that the Copenhagen vote was a fair sample of the views of zoologists.

(5) In the long view, if a great proportion of animal species remains to be discovered and named (estimates for neozoology alone range from 50 to 90 per cent), the number of generic and group names yet to be proposed and shuffled about with successive classifications and reclassifications will be considerable. Any rule that will render some name changing unnecessary for the future, as does clause 54 (1) (a), will be a great boon and should not be discarded.

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June, 1954.

THE CARIBBEAN "OLIGOCENE"

SIR,—Dr. Stainforth's comments on my recent paper on the Miocene-Oligocene boundary, especially in so far as they affect the Caribbean region (*Geol. Mag.*, xci, No. 2, pp. 175-6, 1954), suggest that he has missed the object of the paper. I am familiar with the selected references cited by Mr. Stainforth, as well as many more concerning the Central American region and many others dealing with marine Miocene and Oligocene faunas in other parts of the world: only a few selected recent papers having a fundamental bearing on the subject were quoted in my original paper probably for the same reason as Dr. Stainforth—because a comprehensive list would have been impracticable as it would fill a whole issue of this periodical.

A study of the more recent literature, referred to in part before, indicates that there is a growing school of workers who recognize that the upper part of the so-called "Oligocene" in the Caribbean region is probably of Lower Miocene age: this is the view that I fully endorse as a result of researches carried out in many parts of the world over a period of nearly thirty years. It is evident that two important and world-wide palaeontological changes are involved, one between the Eocene and the Oligocene, the other between the Oligocene and the Miocene, the latter occurring well below the so-called "Miocene/Oligocene" boundary as interpreted until recent years in the Caribbean region. Consequently, I am firmly convinced that the faunal

succession in the latter region is not out of phase with that in all other parts of the world, and that beds with *Miogypsina*, *Orbulina*, *Candorbulina* (for example) and their associated faunas, as now known, are all unquestionably of Miocene age. The evolutionary succession of *Miogypsina sensu stricto* (see Drooger) lies entirely within the Miocene.

Finally, I would point out that Dr. Stainforth, throughout his writing, is using the terms "Oligocene", "Miocene", and "Aquitainian"—all the stages being defined in Europe—as if they were unquestionably used correctly in the Caribbean region. This I maintain is not the case. It is imperative that subscribers to this view should purge their minds of all preconceived ideas and review and assess the palaeontological evidence from the Caribbean region again and compare it anew with modern and up-to-date evidence from other parts of the world. My conclusions have been based on such studies. Not all those who study the Caribbean Tertiaries appear to have carried out such investigations and this is hindering the promotion of a sound and orderly understanding of the evolution of Tertiary faunas and of their world-wide correlation.

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21st May, 1954.

REVIEWS

Mountain Building¹: An Essay Review

By W. B. HARLAND

In the first part of his recent book Van Bemmelen summarizes his undation theory developed over many years and connected with the ideas of Haarmann, Stille, Cloos, Bailey Willis, and many others in different ways. This hypothesis is generally known to the English reading world through his paper to the International Geological Congress in 1933.

In the second and larger part of the book he applies this to, and endeavours to support it from, a study of the orogenic evolution of the earth's crust in Indonesia. In so doing he summarizes parts of his massive *Geology of Indonesia* (1949) itself a synthesis and interpretation of a century of geological investigation by many Dutch geologists.

The second part with its chapters on stratigraphy, volcanism, tectonics, and geophysics, concludes with a summary of the geological evolution of Indonesia. All these chapters are elementary in the sense that much is devoted to definition and introduction, while the local detail is brief and not supported by references. From this there is little chance of assessing critically Van Bemmelen's hypothesis in terms of the evidence presented; on the contrary the regional description is presented as an illustration of what is implied in the full development of his hypothesis. In so far as the author has already made such a full and well documented account available in his *Geology of Indonesia*, this is not a serious criticism, and the book is thereby made conveniently brief and readable. It could have been further shortened by omitting some of the introduction to stratigraphy which ranges from an explanation of unconformity to several detailed schemes for dividing and correlating the Tertiary without any attempt at an assessment of their application.

However, this book will be of interest as a recent exposition and application of the undation hypothesis to the problem of mountain building.

¹ *Mountain Building*, by R. W. Van Bemmelen, pp. xii + 177, with 51 figs. Martinus Nijhoff, The Hague, 1954.