Fleming's paper surely now takes second place to his very important review of the geologic and biogeographic history of New Zealand—"New Zealand biogeography: A paleontologist's approach" [*Tuatara* **10**, No. 2 (1962)]. Are long delays in the publication of complexly organized symposium volumes unavoidable, and, if so, is this not a strong argument against such publications? Might it not be better to encourage, and perhaps to subsidize, separate publication of important papers rather than to organize publication so that everyone must wait for the slowest contributor or for the slowest editor?

A second criticism of this volume is that there is no index, except the "Author Index," which is less than a page long. Lack of a detailed index in a book like this is a serious fault that should be emphasized by reviewers. Without an index, how can interested readers find all that is said about, for example, southern beeches (*Nothofagus*), or Tasmania, or speciation, or wind dispersal?

There are also cases in this book of what might be called the breakdown of rigorous scientific treatment when competent scientists turn from what they really know to marginal details or biogeographic generalizations. Why do so many specialists think that biogeography can be treated more casually than their own specialties?

For example, how is an uninformed reader to know that, although Menard and Hamilton speak with authority about the atolls and sunken guyots of the Pacific, their comment on the paleogeography of bordering continents, South America and Australia, is (to put it kindly) less authoritative? The geologic history of South America has been reviewed recently by Harrington [*Bull Am. Assoc. Petrol. Geol.* **46**, No. 10 (1962)], and his maps do *not* show the Cretaceous seaways across South America that Menard and Hamilton show without question (their Fig. 3), except that sea did cover the northwestern corner of South America. And as for Australia, most of the western part of that continent was probably land in the Cretaceous. Do Menard and Hamilton (Fig. 3) deliberately leave this part of Australia blank, or is it done accidentally? My objection to this map is not that it is necessarily wrong, but that it ought to have been conspicuously labeled "hypothetical" to distinguish it from real situations treated in the same paper.

Another example of less-than-rigorous presentation is Usinger's map on page 256. This map shows faunal divisions of the Pacific in a reasonable way, so far as insects are concerned. (Many plants probably are distributed this way too, but vertebrate animals are not.) But what do the arrows on this map mean? They are not explained, and they do not seem to conform to any consistent hypothesis. An arrow suggests that New Guinea has received its fauna mainly from Australia, although the faunal boundaries place New Guinea in the Oriental Region. And heavy arrows seem to show an enormous amount of dispersal out of Antarctica but nothing going into that continent. Can this really be what the author means? And, if so, what is the evidence of it? Did an editor delete the explanation of these arrows in an effort to keep the whole volume within a given size? If so, this is another argument against symposium volumes.

Nevertheless, this *is* an important and useful volume, one that all who are interested in biogeography must have. But earlier publication of these papers and in some cases more careful attention to some aspects of their presentation would have resulted in a still more useful contribution.

## Old World Higher Primates: Classification and Taxonomy

Elucidation of classification and taxonomy refines language and communication. In a variety of ways **Classification and Human Evolution** (Aldine, Chicago, 1964. 381 pp. $7.50), edited by Sherwood L. Washburn, does this for the Old World higher primates. As such, it is an important contribution, for clarification of the language by which the history of life is recounted must remain of fundamental importance as long as men wish to speak about the substrate from which they arose.

In the 17 papers included in this volume, the perennially fascinating problems of naming and interrelating man and his close relatives are covered from a catholic range of approaches. Not only are the contributors outstanding authorities in their respective fields of science, but each has much that is new and pertinent to say regarding the genesis of man and allied higher Primates of the Old World. This pertinence is derived from the remarkable series of recent advances in the study of primate behavior, biochemistry, morphology, and paleontology, advances that are authoritatively summarized in this volume.

Nevertheless, the momentum provided by an almost explosive increase in laboratory and field research in primatology, which has occurred since about 1950, makes this contribution more of a milestone along the way to further discovery than a final statement. The rapid growth in research pertinent to classification and human evolution is evidenced by the papers cited in the useful bibliographies included in this volume—more than 75 percent of these papers were published after 1950 and about 40 percent after 1960. This is true despite the fact that the study of fossil and living primates has fairly ancient scientific roots. Clearly this compendium will be of fundamental value as a sourcebook for students and as a stimulator of further research. That a full understanding of human origins and evolution has by no means been entirely worked out is shown by the refreshing variety and difference of opinion indicated by this broad spectrum of authors.

In an excellent initial chapter, G. G. Simpson deals with the meaning of taxonomic statements with particular reference to the classification of hominid species and allied apes, past and present. As an accomplished student of mammalian phylogenesis, Simpson is able to emphasize the most relevant points for the interpretation of the family tree of men and apes. The conceptual basis of mammalian taxonomy has seldom, if ever, been better illustrated, and I suspect that, with careful study, this chapter will long remain a taxonomic guide in human paleontology, not only for the general reader but also for the other contributors to this symposium and professionals generally. Simpson's analysis of classification, moreover, is reinforced from several additional points of view in two other significant papers on this general subject—"The taxonomic evaluation of fossil hominids" by Ernst Mayr and a paper on genetic entities among hominids by Theodosius Dobzhansky. It is well known that categories above the level of species are not subject to exact definition, except that the need for generic distinction (or the lack of it) can sometimes be tested by so-called "intergeneric" crosses such as the now well-known crosses, *Ursus* × *Thalarc-*

*tos, Papio* × *Macaca*, and *Bos* × *Bison*. If successful, such crosses, whether sterile or not, are now often taken by mammalogists to indicate generic identity, whereas intergeneric crosses that fail to produce viable offspring, such as those which have been attempted between species of *Capra* and *Ovis*, tend to sustain generic separation based on morphology. However, this method of determining genera may not have much applicability outside the Mammalia. In this context, it is interesting that Simpson here places the *Gorilla* species in the genus *Pan* on grounds of morphological similarity, as Ernst Mayr and others suggested earlier. This stresses again the extreme importance of attempting to secure a chimpanzee × gorilla hybrid. If this cross is successful, it will strengthen the probability of their generic identity and provide a most valuable laboratory animal for psychological research, if the hybrid exhibits a modicum of the extroverted chimpanzee temperament operating out of a larger gorilla-like brain. Of course, too much optimism on this point is perhaps unwarranted, but the possibility of such a cross has not been tested sufficiently, so far as I can determine.

The probable synonymy of *Gorilla* with *Pan* is most pertinent for taxonomic interpretation of the few available specimens of early and middle Pleistocene australopithecines. Clearly, skulls and dentitions of *Paranthropus* and *Australopithecus* differ much less than those of *Pan* and *Gorilla*, while the crania of *Zinjanthropus* and *Paranthropus* do not differ more than variant individuals selected from limited series of *Pan*—for example, those figured by Adolph Schultz in his important essay on age, sex, and variability in Primates (pp. 85–115). It is imperative that anyone who attempts to devise a reasonable classification of the extinct higher Primates fully understand the implication of Schultz's work on primate variability.

An example of the relevance of understanding hominid species variability will illustrate the point. Schultz reports that in a series of 58 full-grown male gorillas the internal volume of the braincase varied from 423 to 752 cubic centimeters. In his contribution to this symposium, L. S. B. Leakey discusses two main finds of early hominids from Bed I, Olduvai Gorge, Tanganyika. These are the skull of *"Zinjanthropus boisei"* and, a little lower in the section, the mandible (and apparently associated parietals) of the "pre-*Zinjanthropus* juvenile," which was recovered with adult foot bones. Leakey's discussion in this volume clearly indicates that, in his opinion, the "juvenile" represents a species different from the species represented by the *Zinjanthropus* skull, one more like the species of *Homo* than those of *Australopithecus* (s.l.). Endocranial volume estimates for these "two" varieties are not provided in the article, but, on the basis of available data, it would seem unlikely that either falls outside the range of brain volume indicated by Schultz for the gorilla. Consequently, it is probable that discrimination of two distinct species at Olduvai will have to be made on characters other than brain-size. If two species did exist contemporaneously at Olduvai, for any appreciable length of time, some interesting problems of ecologic overlap are raised. Both forms are most likely to have been wide-ranging open-country foragers. Even if the more "advanced" form was the only tool-maker at Olduvai, it would seem that the other must have been at least an occasional tool-user. It is difficult to avoid the conclusion that the dentition of *Zinjanthropus* indicates a tool user, for the greatly reduced canine-incisor series shows that this hominid could not have been a herbivore with feeding habits like members of modern *Pan*. Early relatives of man surely must have used their hands much more extensively in food preparation than the modern apes use theirs. It is also difficult to see how two species of hominids could have coexisted for long in the Olduvai region without one displacing the other, particularly if only one was a tool maker and if it preyed, however sporadically, on the other. Presumably such problems can be resolved only by collecting an additional range of hominid fossils from these beds.

Contributions by Campbell, on hominid quantitative taxonomy, and by Harrison and Weiner, on the philosophy of phylogeny, go far toward sharpening thinking on the taxonomy of Pleistocene hominids and on the logical systems of interrelating fossils into evolutionary trees of descent; these papers are well worth careful study. Articles of interest to the morphologist include Biegert's detailed study on the use of characters of skull, hands, and feet in primate taxonomy and Straus's thorough analysis of the anatomy of the Tuscan fossil primate *Oreopithecus*. This article appears to establish firmly the placement of this most interesting genus in the Hominoidea, alongside the living and fossil apes and men. Straus concludes that *Oreopithecus* should be placed in a family of its own or, alternatively, considered a primitive, aberrant member of the Hominidae. Napier's interesting contribution to this symposium consists of a thoughtful analysis of the locomotor functions characteristic of Hominidae. He draws the provocative conclusion that *ad hoc* tool using may have begun in the lineage leading to man as far back as the Miocene. This deduction seems entirely plausible, in view of the reduced anterior dentition of the *Ramapithecus-Kenyapithecus* group, which dates back to this period.

Stages in the behavioral evolution among higher Primates can be recapitulated to some extent by studying the habits of living species. In two studies (by Hall and by DeVore) the behavior of living apes is contrasted with the behavior of monkeys. The approach to primate evolution that combines an understanding of behavioral adaptations with their morphological consequences is well summarized by S. L. Washburn in a study entitled "Behavior and human evolution"; Washburn's analysis is complimented by Anne Roe's treatment of the psychological definitions of man.

Contributions to the understanding of primate taxonomy on the infracellular or molecular levels form a separate section of this volume. Klinger and his collaborators present up-to-date information on chromosome number and morphology among great apes and monkeys. These studies indicate the similarity of both chimpanzee and gorilla to man, and confirm earlier evidence of gross anatomy. The gibbons appear to be more monkey-like in chromosome pattern. This is in line with the paleontological and anatomical evidence that Hylobatidae must have had considerable antiquity of divergence from the pongid-hominid stem. Goodman's and Zuckerkandl's papers, on the more molecular aspects of primatology, reflect the rapidly growing interest in this field. Although such studies have no time dimension and consequently provide no information about the times of main phyletic branching among primates, they are already powerful taxonomic tools for neontologists; doubtless, they will become more so in the future.

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