Biogeography of Half the World

P. J. Darlington, Jr.

The Pacific Basin together with the land around it stretches almost from pole to pole and covers about half of the earth. The biogeography of this enormous area cannot be treated exhaustively in a single volume. Nevertheless **Pacific Basin Biogeography** [Bishop Museum Press, Honolulu, 1963 (*but actually issued 20 February 1964*). 572 pp. $12], edited by J. Linsley Gressitt, does cover the distribution and probable history of life, chiefly terrestrial life, around and across the Pacific remarkably well. Stress is necessarily placed on selected situations and special cases, but the geologic background is considered and pertinent general biogeographic problems are discussed. The volume is therefore a very important and useful one.

The following quotation from the preface summarizes both the contents and the organization of the book itself.

> This volume includes [the results of] an intersectional symposium and an interdivisional symposium organized by the Division of Zoology and Entomology of the Tenth Pacific Science Congress held in Honolulu, August–September, 1961. The first and major symposium, "Biogeography of the Pacific Basin," comprised three subsymposia, representing three geographical divisions of the Pacific Basin; northern (Bering Arc), central (Tropical Pacific), and southern (Antarctic). Thus it concerns not merely the island areas of the Pacific, but the continental relationships, particularly in the northern and southern portions where the Old and New Worlds come into greater or lesser proximity. The second symposium, "Modification of Biotic Balance of Island Faunas and Floras," constituted a single session devoted to various aspects of biotic and ecological change under changing insular conditions.

Section 1-A, on Bering Arc relationships, consists of 13 contributions, which cannot be discussed individually here. Emphasis is on exchange of terrestrial life across more or less recent Bering land connections, and on evidence that Alaska and eastern Siberia were *broadly* connected by land during Pleistocene glaciation and that the land was ice-free and tundra-covered and was a refugium for plants and animals of the tundra.

The Hooker-Darwin Dilemma

Section 1-B, on tropical relationships in and across the Pacific, includes 15 contributions and is, inevitably, less coherent than the section on the Bering Arc relationships. Running through much of this section and through the following one (1-C) is a debate that began more than 100 years ago, between Hooker and Darwin. This is the debate between persons who think that some plants and animals *cannot* cross salt water and that land connections must be postulated to every island where the organisms occur and those who think that the composition and distribution of the floras and faunas of many islands, including New Zealand, show that the plants and animals concerned *must have* crossed water gaps. This debate has been going on in West Indian biogeography too. A long generation ago most biogeographers interested in the West Indies thought that some plants and animals on the islands must have dispersed across land. Now, most of those working on West Indian biogeography think that somehow the entire biota has been derived across water. I think that the debate will probably go this way in the Pacific, and in the Antarctic too, in the end. But I cannot be sure, and in the meantime, some of the questions raised are fascinating and important. For example (from Corner's paper and the discussion following it), how can figs establish themselves across ocean gaps? Figs are pollinated apparently only by special fig insects, and even if the figs and insects dispersed together, the insects would have nothing to feed on until the newly established trees were large enough to fruit. Yet, in the western Pacific, endemic species of figs exist at least as far east as Samoa and New Caledonia. Did figs spread across land to these places? Or can their presence there be explained in some other way?

Section 1-C, on Antarctic situations, with ten contributions, considers some of the many relationships that undoubtedly do exist among the plants and invertebrates (but not terrestrial vertebrates) of southern South America, New Zealand, and the Tasmanian corner of Australia. The role of Antarctica in the history of southern terrestrial life is stressed. That Antarctica has played a role cannot be doubted, but it is not clear whether the continent was a major evolutionary center or just a stepping stone, nor whether Antarctica was connected by land with other continents. Persons interested in these questions will find this volume exciting.

The "summary discussion" of the preceding sections, by Elwood C. Zimmerman, is treated as a separate section, 1-D. This is not and cannot be a real summary, as Zimmerman notes in his first paragraph. It is rather a restatement of Zimmerman's own points of view and conclusions. But these are important. They are based on an unrivaled knowledge of the facts of Pacific biogeography. Those who find the present volume to their taste should, if they have not already done so, read the first volume of Zimmerman's *Insects of Hawaii* (Univ. of Hawaii Press, Honolulu, 1948), which is a discussion of basic situations in the Pacific and of biogeographic principles applied to the Pacific.

Section 2, with six contributions, is concerned with some of the limits and modifications imposed on island biotas by isolation and by insular environments, with emphasis on the influence of man and on repopulation after devastation.

On Publishing Symposium Volumes

This book is indeed important and useful, but some criticisms have to be made of it nevertheless. First, its publication was inordinately delayed. The papers were presented in August and September 1961, but, although the book is dated 1963, the actual date of publication was 20 February 1964. The result is that some of the contributions are out of date and might almost as well not have been published at all. For example, Brundin's paper is said by the author himself to be a precursory one already out of date in some details (footnote, p. 425). Van Steenis' paper has, I suppose, been largely superseded by his long and important paper, "The land-bridge theory in botany" [*Blumea* **11**, No. 2 (1962)]. And

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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-*