

BOOK REVIEWS

EDITED BY WILLIAM E. SOUTHERN

The Audubon Society encyclopedia of North American birds.—John K. Terres. 1980. New York, Alfred A. Knopf. xxv + 1,110 pp. 875 color photos and 800 black-and-white figs. \$60.00.—An encyclopedia of birds? Written by one man? Yes indeed, that is what we now have in this imposing tome. Imposing is certainly the word, as the book measures $9\frac{1}{2} \times 11\frac{1}{4}$ in. and weighs a good 7 pounds. Not exactly the thing for "light" reading. John Terres, formerly editor of *Audubon*, aided by a galaxy of expert consultants, has worked 10 years on this offering. The obvious intent has been to provide a convenient source of information for the amateur bird student who is interested in going beyond listing and identification. But even if this is the obvious intent, the book also has value to the professional as a source of much obscure and hard-to-dig-out information.

North America has been defined here as in the 5th edition of the A.O.U. Checklist. Terres has included accounts of 847 species known from this area, grouped in families that are arranged alphabetically by the English name. Each species account begins with an explanation of the scientific name, and the English name if necessary, and a few introductory remarks about the bird. This is followed by a brief description, which in most cases is barely adequate for identification purposes. There follows a short paragraph touching on habitat, general behavior, and vocalizations. Detailed information is then given under the headings: Feeding Habits, Nest, Eggs, Incubation, Other Names, Accidents, Age, Host to Cowbirds, Hybrids, Weights, and Range. As is often the case in such works, the abbreviated range statements are sketchy to say the least, and are in some cases quite misleading for those northern species whose breeding ranges extend southward along the Appalachians and the Rockies.

Besides the species accounts there are some 600 essays of varying length on major topics such as Migration, Flight, Extinct Birds, Nomenclature, Nesting, Care and Feeding of Abandoned or Injured Wild Birds, etc. Many technical terms are defined, and there are short biographical sketches of ornithologists (none living) for whom birds were named or who named birds. The book concludes with a bibliography of over 5,500 references. The text material is copiously documented and extensively cross-referenced.

A major feature of the book is the set of colored photographs. The dust jacket claims that there are more than 875 (I have not counted them). Almost every species is represented, and some species have several pictures. Besides the usual "portraits" of resting birds and parent birds feeding young, there

are often interesting and instructive shots showing behaviors. A few of the pictures appear to be "studio" shots of captive birds (e.g. the Wren Tit on p. 1,046, which even so is a fascinating picture). One picture, Mountain Quail (p. 709), may be of a mounted bird, and the picture labeled Bewick's Wren (p. 1,043) is clearly of a Carolina Wren, and is a better picture than the one labeled for that species. All of these pictures are excellent, with most of them showing remarkable detail. The color reproduction has been outstanding, and in the review copy I did not notice any pictures out of register. As was the case in the Field Guide published by the same company a few years ago, it is extremely difficult to find out who took a given picture, although there is a section on photographic credits, arranged by photographer. May I add my name to the list of reviewers who have complained about having excellent color photographs printed across the two opposite pages of the open book, and hence being interrupted by the inner margins. In this case the narrow inner margins make this less distracting, but it is nevertheless a deplorable custom. Besides the colored plates there are many black-and-white drawings illustrating a few species for which no colorplates were available, as well as structural or behavioral features described in the text.

Many recently published large books have been criticized for wasting paper, but this one is innocent of that charge. Each page has three columns, although one is sometimes used for illustration. The margins are only $\frac{1}{2}$ inch all around and indeed the inner margins are so narrow that reading is often difficult. The book appears to be as sturdy as is required by its weight, but like the *Webster International* it almost needs a reading stand for its use.

An interesting feature not often encountered is a phonetic guide to the pronunciation of all scientific names and of many technical terms. Most ornithologists of my acquaintance tend to pronounce scientific names in an off-the-cuff every-man-for-himself fashion, and so the pronunciations given here may sound strange to some ears. However, those that I checked against other sources seem to be correct. On the other hand, I have never heard any ornithologist refer to the tail feathers of a bird as the "reck-TRY-seez" or to the songbirds as the "OSS-in-neeZ" as given here, although both of these pronunciations are sanctioned by my *Random House Dictionary*.

What, then, does this immense amount of material amount to in the final analysis? Can the amateur use the book as a reliable source of information about the birds he sees? Obviously I have not read nearly all the nearly one million words, but most of the subjects I have looked up are well done, and are rea-

sonably accurate. A number of minor errors came to my attention, however, and one can only wonder about other errors in material that was either not read or was unfamiliar. In most of the accounts the author has used a specific anecdotal technique to describe behaviors and other phenomena, citing a person, with reference, as having seen or reported a specific event. This probably makes the reading more interesting to the amateur than would a more scientific, and possibly duller, approach. There is often a "Guinness Book of Records" attitude about some of the data given, but I suppose that this is the sort of thing that many bird watchers might want in order to settle arguments, or simply to satisfy curiosity. Overall, I think that the answer to the question given above is "Yes." The amateur will find this a useful and worthwhile book. The collection of pictures alone makes it a useful addition to one's library. Besides the pictures, the professional will also find use for this book, as a handy, quick guide to such hard-to-find things as clutch size, longevity, weights, etc. This will be particularly useful to those who spend a part of their time answering telephoned questions from the public. Both the amateur and the professional will find the many citations to the literature useful.

Inevitably this book will be compared with "The New Dictionary of Birds" produced by Landsborough Thomson and the B.O.U. a number of years ago. The accounts in that book were all prepared by experts, and hence in some ways are more authoritative than the ones here, but that book in smaller size attempted to cover the birds of the world, with emphasis on European forms. There is much more information about the individual species covered in the present book, and given the difference in emphasis and purpose, this book stands up well compared with the earlier one.

There are, however, some factual errors, both of omission and commission, that came to my attention. As mentioned above, the range descriptions are sketchy to say the least, and some are misleading. Thus, no mention is made of skuas off the Pacific Coast; it is not pointed out that the eastern population of the House Finch has now expanded well into the middle west; the U.S. Rocky Mountain range of the Dipper is omitted; and a few others. The author has misinterpreted the shooting experiments done in Maine by the Fish and Wildlife Service personnel in the 1950s. The attempts to remove all the birds from a given area were not done in order to check for possible replacement from a floating population, but rather they were an attempt to measure the effectiveness of birds in controlling spruce budworm infestations.

A number of other factual errors of lesser importance caught my eye, and there are probably more. LeConte's Sparrow and LeConte's Thrasher were not

named for the same person. Kumlien's Gull was named for the younger Kumlien, and not his father as given. Perhaps I can be pardoned for discussing a minor matter of considerable local and personal interest. As far as I know no one except Ernst Mayr (1963) has ever referred to the presumed hybrid warbler named for George Sutton as the "Potomac" Warbler. Certainly that was not the intent of the describer. Terres, however, uses this as the primary name, and also perpetuates Mayr's error in identifying the putative parent species of this form that was locally uncommon at the type locality.

The greatest fault in this book, however, is the host of small, usually unimportant errors that indicate great carelessness on the part of the editors and publishers. These errors are of two kinds. With such a long time in the making, it is inevitable that some of the material would be out of date. The author (and the editors?) have made some effort to correct these; for example, the nomenclature used follows the two recent supplements to the A.O.U. Checklist. But a number of things have not been caught. The range of the House Finch mentioned above is probably in this category. Evening Grosbeaks now come south almost every winter rather than in the 2-year cycle, mentioned in the text, that was prevalent a decade ago. Such things as the ranges of expanding species are out-of-date (e.g. Five-striped Sparrow). In one place we are told that the *Condor* is a bimonthly publication (which it was) and in another we are told that it is a quarterly (as it now is). The most amusing error of this sort occurs when the change of the Great-tailed Grackle from the genus *Cassidix* to *Quiscalus* is given, but the pronunciation is still given for the older name.

The other type of error is simply carelessness on the editorial side. At the price asked for this book, it seems that more care should be given to such matters. A whole host of proper names are misspelled: Benson (= Bentsen) State Park, Amodon (= Amadon), P. O. Sclater (= P. L. Sclater), Gavan (= Gavin) de Beer, Viellot (= Vieillot), D. G. Elliott (= D. G. Elliot), and Alfred Russell (= Russel) Wallace. Admittedly those last two are toughies, but what, pray, are editors for? A paper by Janet Witzeman and others is both cited in the text and given in the bibliography as being by Sitzeman et al. Perhaps the worst example of this concerns six papers by H. C. Mueller and D. D. Berger (two also with G. Allez). In the bibliography one is cited correctly, three are attributed to H. C. Mueller and D. C. Berger, and one is cited as H. B. Mueller and D. D. Berger. I daresay that there are many more of these than caught my eye.

Overall, I would recommend that this book has a definite place in school libraries, as well as in the libraries of such bird watchers as can afford the price (and the space).—GEORGE A. HALL.

Vicariance biogeography.—Gareth Nelson and Donn E. Rosen (Eds.) 1981. New York, Columbia University Press. xvi + 593 pp. \$35.00.—This volume is the outcome of a symposium held in 1979 at the American Museum of Natural History “devoted to contrasting the theoretical positions of dispersalism and vicariance biogeography.” It contains 12 main papers, 23 discussion papers, and 10 responses by the main speakers.

In view of the considerable amount of uncertainty about the meaning of the two terms *dispersalism* and *vicariance*, the reader would expect an introductory chapter with precise definitions and a clear formulation of the opposing viewpoints. Alas, there is no trace of such an exposition. Neither is Croizat’s “panbiogeography” clearly defined, nor the term *vicariance biogeography*, apparently preferred by his latter-day disciples. The claim that it “derives its strength from the concept of relatedness, as expressed by . . . a cladogram” is evidently misleading because dispersalist explanations are just as much based on an analysis of relationship. There is no organization of the 12 chapters, but rather an almost random sequence of papers defending vicariance and defending traditional biogeography, papers presenting the best current geological thought (such as Hallam’s excellent summary of plate tectonics) and presenting highly unorthodox geological speculations on sunken continents, papers on methodology, and rather descriptive distributional papers that deal with the distribution of higher taxa (families and orders) or of discontinuously distributed species, etc. Consistent with a current fashion, no opportunity is missed to denigrate Darwin and Darwinism. Darwin’s biogeography, the first biogeography in which the consequences of dispersal were properly evaluated, is rejected by the vicarianists out of hand. I might illustrate this by some choice quotes: “Darwin’s argument . . . disqualifies him as a theorist worthy of attention,” “Darwin preached from ignorance,” “I judge that Darwin’s . . . zoogeography and phytogeography . . . are worthless as instruments of learning,” or “caught on his own hook, Darwin squirmed through long pages of the *Origin*.”

As the editors have failed to pinpoint the nature of the argument between the two schools and have badly misrepresented the views of their opponents, let me try to correct this deficiency.

The classical biogeographers (dubbed *dispersalists* by Nelson and Rosen) postulate that most biota (except, let us say, on newly formed oceanic islands) are a mixture of old elements and subsequently arrived colonists. The “old” elements, in certain groups, may have originated as far back as various periods of the Mesozoic or early Tertiary (it is futile to go back beyond the Permian Pangea). Prior to the discovery of plate tectonics and ocean floor spreading, biogeographers were forced to explain the old discontinuities as due to dispersal across the Bering

Straits Bridge or to island-hopping across the Indo-Australian Archipelagoes. Now we interpret the vicariant distribution patterns of some of the old elements as caused by the fracture of plates (secondary discontinuities). On all land masses, however, many layers of new colonists have been superimposed on the oldest faunal element owing to dispersal from various directions and throughout the time that the area has existed. For the classical biogeographer, a biota is thus always a mixture of various elements differing in age and origin.

The vicarianists, so far as I can judge from their disorganized writings (and disagreements with each other), postulate very widespread, sometimes virtually worldwide, ancestral ranges of taxa. The now-existing discontinuities between sister groups are explained by a later origin of the barriers now separating them. They reconstruct the postulated previous connections between vicariants by connecting areas of major endemism—the “tracks” of Croizat. The occurrence of colonization across barriers is minimized, if not denied altogether. Not only dispersal, but also extinction, is largely ignored.

The explanation of discontinuities is one of the decisive differences between the Darwinian and the Croizatian biogeographers. That there are different causes for distributional discontinuities was known and extensively discussed long before Croizat’s grandfather was born. Most of the earlier authors realized that there are two kinds of discontinuities: (a) primary ones produced by the establishment of a founder population across a geographical barrier, and (b) secondary ones, caused by the disruption of a previously continuous range by a geological event (a fracture of plates, a flooding of shelves, the rise of a mountain range, etc.) or by a climatic event (ice barrier, origin of savannahs or deserts, etc.). I have never been able to fight my way through the more than 3,500 pages of Croizat’s undisciplined verbiage but so far as I have gotten I gather that his major thesis is to question the significance of primary (dispersalists) discontinuities and to try to explain all discontinuities as secondary. Let me hasten to state that this is a completely legitimate hypothesis, as is any thesis that is open to falsification. This volume would have been a major contribution to biogeography if the followers of Croizat had falsified the theories of dispersal and the Darwinians had falsified or at least attempted to falsify the thesis of Croizat. Unfortunately, there is only a single paper dealing with theory testing (by Simberloff and coauthors), but it limits itself to an endeavor to test the MacArthur-Wilson theory of island biogeography and virtually ignores the problems of discontinuities in higher taxa.

For the Croizatians, as for nearly all true believers, everything is either black or white. There are no mixtures or combinations. If one accepts the existence of primary discontinuities (dispersal across

barriers), then, of course, one must deny the existence of secondary discontinuities. Since the latter are known to exist, "dispersalism. . . is no longer an issue, having already been rendered obsolete by the revolution in the earth sciences, if not previously by the synthesis of Croizat" (Nelson, p. 526).

I do not know a dispersalist who has not accepted that many discontinuities, particularly of higher taxa, are secondary. If one finds trogons in the Old World and New World tropics, no "dispersalist" proposes that they dispersed across the wide Atlantic. Surely there was once a continuity, in this case perhaps the Eocene North Atlantic connection between Europe and Greenland. Actually, even the vicarianists cannot help but admit that distributions over an entire or several continents must have been the result of dispersal.

It is obvious from the discussions that the argument is almost exclusively about the frequency of dispersal across barriers. The vicarianists virtually deny such dispersal or, at best, allow it grudgingly in a few cases. What is curious to an ornithologist is that anybody can deny dispersal, considering how frequently it has occurred even in historical times. Numerous species have colonized New Zealand, on their own power, in the last 100 years. The same has been recorded for the Shetlands and Orkneys, for the islands off the California coast, and for numerous other islands in the Atlantic and Pacific. The denial of dispersal forces the Croizatians into all sorts of eccentric geological theories. For instance, Croizat (p. 511) explains the Galapagos Islands as "a fragment of [continental] geological America, that "was recast [=fractured], apparently in early Tertiary times, into a number of islands beset by volcanism." The now-existing biota of the Galapagos is the product of evolution of the "ancient plant and animal life inherited as a whole from the American continent." This remarkable assertion, albeit the necessary consequence of panbiogeographical dogma, is so totally refuted by the geological and biological evidence that no further comment is necessary.

In a longish harangue against Darwin's conclusion that any new species originates in a circumscribed area, Croizat completely misses the fact that Darwin's argument was directed against authors like Louis Agassiz, who explained the discontinuous distribution of species by multiple independent creations. Darwin's major point was that such an appeal to the supernatural is not scientific. But Croizat also misses the point that in the process of a multiplication of species, a new species always originates at a circumscribed location, a truth that is equally applicable to such disparate modes of speciation as polyploidy, sympatric speciation, or peripatric speciation in founder populations. One would think that Croizat, after he had got several pages of maligning Darwin out of his system, would proceed to provide evidence for the falsity of Darwin's belief that "each

species has proceeded from a single birth place," but one searches in vain either for such a falsification or for a "superior" replacement theory.

For the cladists (and I believe all vicarianists are cladists, but not vice versa), Popper is the official philosopher. Not surprisingly, Platnick invokes Popper's falsification principle to silence his opponents. Yet, curiously, the Croizatians never attempt to falsify their own hypotheses. Interestingly, vicariance theory permits a number of clear-cut predictions. For instance, if two biota, now more or less widely separated, are the products of a secondary separation, then all components of this biota must have the same age. Hence they must also show more or less of an equivalent degree of divergence from each other. Furthermore, within each of the separated biota how much unity must prevail? Nowhere in the presentations of the vicarianists do I find even the beginning of such an analysis. I think I know the reason.

In several contributions one encounters exhortations that biogeographers should, in the future, test their theories by the Popperian method. These biogeographers, who apparently do not read any literature that is more than 10 years old, are unaware that classical biogeographers have again and again strictly applied the canons of the hypothetico-deductive method. They were able to show by this method that among two competing hypotheses one did and the other did not successfully withstand the test of falsification. Among scores of examples I could quote I will refer only to a single one. In the 1940s there were two hypotheses concerning the colonization by birds of the islands between the Sunda and the Sahul shelves. Rensch (1936) postulated former land bridges between the islands, while Stresemann (1939) favored dispersal across water gaps. The two hypotheses permitted numerous predictions based on the distances of the islands and depth of the water between them. In a thorough analysis, I was able to show (Mayr 1944) that the predictions of the land-bridge hypothesis could be falsified in every case while those of the across-water-dispersal hypothesis were thoroughly confirmed. I don't know of a single equally careful analysis by any latter-day Popperian.

Perhaps the greatest weakness of vicariant biogeography is its endeavor to explain the distribution of very different groups by a single ancestral process ("track") and, furthermore, to place the time of origin of the distribution patterns as early as possible, mainly Triassic-Jurassic, occasionally up to the mid-Cretaceous period. In that manner discontinuities can be attributed to geological events, permitting a neglect of dispersal. As more and more evidence shows, however, most of the higher taxa of terrestrial organisms, up to the rank of orders, originated only in the later Cretaceous and in the Tertiary, and hence cannot have been inhabitants of plates that had drifted apart at an earlier geological period.

The difficulties created by the attempt to explain

the distribution pattern of very different groups by a single geological event is well illustrated by the biota of New Guinea. The flora of that area is essentially Indo-Malayan, and phytogeographers find no borderline at Wallace's or Weber's lines, where the Oriental and the Australo-Papuan faunas meet. Most of the New Guinea insects that are associated with plants are likewise Malayan. The mammals, the birds, and some of the insect groups (Plecoptera) are clearly an Australo-Papuan element.

There are two major factors that determine the distribution pattern of a group—its dispersal abilities and the geological period when most of the dispersal took place. This is why earthworms and primary freshwater fishes will have totally different distribution patterns from birds or butterflies. In complete opposition to the claims of the vicarianists, biota are, therefore, highly heterogeneous, with each of the composing elements having a different history. The biogeographer is therefore forced to recognize different types of faunas and floras, and in an analysis of this problem I recognized six different types of biota (1965). Others might arrive at a different classification, but all unbiased biogeographers will at least agree on the extraordinary heterogeneity of all biota.

There is something strangely essentialistic about the behavior of Croizat's taxa. They move or are stationary as wholes. Every species does exactly the same as the higher taxon to which it belongs. Croizat's thinking is well illustrated by his interpretation of the origin of the bird faunas of Pantepui (Venezuelan highlands). They represent for him a pre-Cretaceous continuity, with the gaps between the various mesas and the Andes secondarily produced by erosion. Therefore the age of the allopatric subspecies and even of the nonvariable species is postulated to be greater than the length of the Tertiary. How can one take a biogeographer seriously who ignores the established ideas on the rates of evolution to such an extent?

The vicarianists make a great ado about combining patterns of distribution with cladograms, as if the study of distribution with reference to common descent were a new invention by Rosen, Nelson, Platnick, and their associates. Admittedly the verbiage is new, but a glance at the literature on discontinuity shows that explanations, even by pre-Darwinians such as Buffon, Zimmermann, Pallas, de Candolle, or Forbes, were based on inferred "relationship." The Hennigian methodology may be more rigorous than the frequently rather superficial analyses of earlier authors, but the basic approach is the same. Curiously, cladograms based on the analysis of sister groups are in most cases unable to resolve the problem of whether a discontinuity is due to dispersal or is secondary (vicariant). Let us assume, for example,

that a species of South American taxon A disperses across the Atlantic to Africa. There it establishes a taxon B that forms a sister group to A (or part of A). The resulting cladogram can only tell us what the nearest relative of B is, but not whether it reached Africa by dispersal or by a continuity that had existed prior to continental drift.

It is curious how little attention is paid in this volume to the unsolved problems of biogeography. Let me mention only a single one, the rate of faunal (biotic) turnover. Birds, for instance, seem to have a very rapid turnover, as indicated by the bird faunas of the Venezuelan highlands. Old elements become quickly extinct, particularly in areas that are relatively easily colonized, and are replaced by more recent invaders. As a result, there is hardly any endemism above the species level and more than half the bird fauna consists of recent colonists, while by contrast the flora has endemic families and numerous genera and comparatively few recent arrivals. Similar ancient floras are found in Southwest Australia and some parts of South Africa. Why do some groups have a rapid turnover and others such a slow one? What groups of organisms belong to either category? What is the causation of these differences? Enough is now known about the biogeography of many kinds of organisms to initiate a truly comparative biogeography. However, this cannot be done by authors who subscribe to a rigid dogma and whose whole endeavor is to prove the validity of that dogma instead of simply trying to find the truth.

What is the final verdict on this volume? There are a few excellent contributions, such as Hallam's review of the latest findings of plate tectonics. Solem presents a discussion of the Pacific distribution of snails that raises numerous problems, particularly in view of our still rudimentary knowledge of the former geological history of the Pacific. Brundin shows that one can be a confirmed cladist and yet see clearly that dispersal across barriers is an important causation of distribution patterns. The chapters by the vicarianists merely demonstrate how blind and biased even intelligent scientists can be. Because the emphasis in the volume is on distribution patterns established during the break-up of Pangea during the Mesozoic, there is less in it of interest for the ornithologist. Indeed, the evidence indicates that nearly all the distribution patterns of genera and species of birds were established after the crustal plates had reached positions not drastically different from the present. Evident exceptions are the old Australian element and the Eocene Holarctic element (trans-Atlantic). Reading the volume, however, made me hope that someday someone will undertake a truly rigorous analysis of the vicariant claims in order to demonstrate conclusively how flimsy they are.—ERNST MAYR.

Introduced birds of the world.—John L. Long. 1981. Sydney and Wellington, A. H. and A. W. Reed. 528 pp. (available in the U.S. from Universe Books, 381 Park Ave. S, New York, NY 10016; \$47.50 post-paid).—Very occasionally one will encounter a book that will be a lasting and important reference work. Such a volume is Long's. It provides a detailed account of every species of bird that has been introduced, reintroduced, transplanted, or escaped and established anywhere in the world.

The heart and bulk of the book are the species accounts. These include the natural range of the species, where it has been introduced, and the success or failure of such attempts. No matter what an ornithologist's specialty, sooner or later he or she will need to know the original range of a species, or the distribution of a widely introduced species. I have made the book a parlor game within recent months, challenging visitors to our home to find a species involved in an introduction, no matter how obscure, that is not documented in Long's book. Despite much searching, no one has yet been successful in finding an omission. This is not to say the book is without errors, but they are minor, and often ones of interpretation, or concerning very recent developments. Most species accounts are accompanied by maps showing the natural range and liberation sites. The drawings by Susan Tingay that grace many of these range maps are excellent, with a few minor exceptions. Text references to colored plates to be found in other publications are also helpful, especially for the more obscure species. Reintroduction, the relatively new management tool for endangered species that has been so successful in New Zealand, is well documented. The text is well referenced with a Literature Cited section of some 2,000 references.

The book contains little in the way of synthesis, leaving that to others. Table I presents the information, surprising even to me, that Hawaii has had more introductions tried (162 species), and more species established (45 definitely established and 25 probable), then any other area in the world. This includes such vast areas as North America (119 tried and 39 definite), and Europe (69 tried and 27 definite). New Zealand, with 133 tried and 38 definitely successful, is in second place behind Hawaii for this dubious distinction.

The author does summarize the effects of introductions and, not surprisingly, finds that in the vast majority of cases the consequences have been neutral to negative. The few exceptions are possibly game birds being of "benefit" to hunters. In this case, however, as with all introductions, the human urge to get something for nothing is generally preordained to failure.

I think the ornithological community can be truly grateful to John Long for his long and fruitful labors.

I can imagine very few ornithologists whose libraries do not require a copy of the book. It will rank right along side the indispensable works of whatever your specialty. I highly recommend it.—C. J. RALPH.

Systematics and biogeography. Cladistics and vicariance.—Gareth Nelson and Norman Platnick. 1981. New York, Columbia University Press. xi + 567 pp., about 390 figs. \$35.00.—The authors state that their book is concerned "with comparative biology, the science of diversity," but as clearly indicated in the subtitle what the authors are really concerned with is cladistic analysis and historical biogeography, particularly the explanation of distributional discontinuities; that is, of vicariant distribution patterns.

Like the founding pheneticists and the early Hennigians, Nelson and Platnick are disturbed over the seeming subjectivity of much of traditional systematics and biogeography, and want to give these fields methodological rigor. Throughout the volume, therefore, the authors make considerable efforts to exclude any kind of biological evaluation of the observed phenomena in order to produce a strictly formalistic-logical methodology. Species, for instance, are for them "the smallest detected samples . . . that have unique sets of characters . . . [including] as species the subspecies . . ." Consistent with the traditional methodology of cladistics, various well-known terms are transferred to new concepts; the function of the cladogram, for instance, is not to depict aspects of evolutionary genealogy, but to "depict structural elements of knowledge," whatever that means. In order to keep away from all references to evolution they adopt the vaguest and most circular definition of homology: homology simply means "that there are . . . some common properties of . . . [certain] structures as . . . might possibly serve as a useful basis for discussion." The major part of the volume is devoted to the topology of cladograms, of which they depict many hundreds. It is curious that so many scores of pages are devoted to topological exercises dealing with rather trivial problems, but that mosaic evolution is virtually ignored and so are those cases where one of two sister groups has acquired a great number of autapomorph specializations while the other has acquired few or none. Perhaps this is not surprising since cladistics does not know how to deal with such situations and solves the problem by quietly shoving it under the rug. Yet these are among the most important problems in classification and provide some of the most cogent reasons why so many taxonomists reject cladistic classification.

The last three chapters (pp. 357–543) are devoted

to biogeography. Although concentrating on the important milestones of biogeographical research, and in particular paying tribute to the pioneering but all-too-often slighted contributions of de Candolle, the historical survey of Chapter 6 neglects a great many important authors, such as Zimmermann, Gmelin, Willdenow, Pallas, and the superb previous review by von Hofsten (1916). More importantly, it fails to bring out the difference between two developing traditions, the regional and the faunistic (biotic) ones, which jointly dominated biogeography for the 100 years after de Candolle. The 57 pages of Chapter 7 are devoted to topological exercises. Although it seems that the authors had a good deal of fun constructing more than 400 dendrograms, I fail to find any evidence of a practical value. In their endeavor to be strictly objective, the authors are desperately anxious not to consider the taxa as entities of blood and flesh, but simply as entities that are no different from pieces of inanimate matter.

Let me illustrate this with an example. Hawaii has a great deal of endemism, including three plant genera, one with western, one with southern (New Zealand, etc.), and one with eastern relationships. The authors, after rejecting various other cladograms connecting these three genera, finally design some that they like. One now expects that the authors will give us the conclusions that can be read from these cladograms, but instead the subject is dropped. Not surprisingly, either, for the experts are near unanimous in their agreement that the three genera arrived on the Hawaiian Islands independently of each other by transoceanic colonization. The cladograms connecting them have no meaning whatsoever. The same can be said for most of their other cladograms.

The final chapter is devoted to biogeographic regions. A series of maps is presented that then are interpreted in a number of tentative cladograms. As an ornithologist I am struck that the useful concept of the "biogeographic species" is entirely ignored. If, for instance, a widespread Pacific genus occurs everywhere as a single species except for eight species in New Guinea and four in Hawaii, the thinking biogeographer would like to know how many of the multiple species in an area are allopecies. It totally changes the picture if, for instance, each of the four Hawaiian species were endemic on a different island, rather than all four on a single island. In no other chapter would it have been more important to make a clear separation between secondary discontinuities (separations owing to continental drift, etc.) and primary discontinuities (such as caused by over-water dispersal on volcanic islands). But the two kinds of discontinuities are consistently lumped together, depriving the analyses of any value. Not surprisingly the authors conclude that

"varied levels of endemism seem irrelevant," while, as every biogeographer knows, the analysis of the level of endemism is a most powerful tool in the study of dispersal. The possibility that dispersal could be the cause of a distribution pattern, rather than a secondary rupture of a previously contiguous range, is conveniently eliminated by saying that this interpretation "might simply be false, in the sense that the endemic forms could represent the results, not of dispersal and colonization, but rather of *in situ* development" (p. 512). That one could propose null hypotheses and test which of the two possibilities is more probable, as has been done repeatedly by the traditional biogeographers, never seems to occur to the vicarianists. In the 1930s many zoogeographers considered Samoa, Fiji, and the Solomons the remnants of a single land mass formerly united by land with New Guinea. To me these island groups seemed oceanic, and their bird faunas derived by over-water colonization. One can readily predict the composition of the bird faunas under either theory. I undertook such testing, and the actual composition was completely consistent with the "oceanic islands" theory (Mayr 1941).

The authors admit (p. 517) that Darwin was the champion of dispersal and that he initiated a tradition continued, up to the present time, in the work of such authors as Simpson and Darlington. Then, however, the authors claim that "what brought this tradition to an abrupt end was the development of continental-drift theory in its two modern forms: plate tectonics and earth expansion." The authors fail to see that the newer geology affects only the distribution patterns of old and poorly dispersing groups (like earthworms and true freshwater fishes) but does not affect the problem of dispersal across barriers. To use the changed geology as an excuse for ignoring dispersal makes the book of these authors virtually useless for a conscientious biogeographer.

Those who have attempted to explain patterns of distribution since pre-Darwinian days have almost universally used the method of searching for the nearest relatives (the cladists would say the "sister group") of a given endemic group. One can represent this in the form of cladograms, as was done quite successfully by Hennig in his analysis of the New Zealand insects. But to think that it would be a necessary and sufficient method for the explanation of the distributional history of animals and plants, to represent all distributions by cladograms, is a monstrous error. It would have hardly seemed necessary to write such a fat volume to prove such a simple point. I don't see how a biogeographer could learn anything from a volume that ignores so much of biogeographic causation and that is so biased in its judgments.—ERNST MAYR.