REVIEWS

EDITED BY WALTER BOCK

The sinking ark: a new look at the problem of disappearing species.—Norman Myers. 1979. New York, Pergamon Press. xiii + 307 pp. $9.95.—The problem of disappearing species is an urgent one for ornithologists and other biologists, for both selfish and altruistic reasons that are all too plain. In these circumstances we can each choose whether to become involved or to remain detached, but no one should be ignorant of the crisis. The causes of extinction, the economics and politics of preservation, and the size and configuration of nature reserves are all topics that must receive professional consideration of the highest quality. This is why a number of us looked forward to the publication of a book like "The Sinking Ark," and why its failure is a serious setback for the cause of species conservation.

"The Sinking Ark" is a shallow, silly, and confused book which does only one thing effectively: it dramatizes the magnitude of the present mass extinction of species. I question neither Myers' statistics nor his guesses (although it would be nice if they were more clearly differentiated from one another); anyone should be able to see that ecosystems and species are disappearing fast, perhaps a great deal faster than Myers' much-quoted guess of one species per day. But once this problem has been delineated, "The Sinking Ark" becomes indistinguishable from the "Titanic."

From the beginning, the author wants us to know that he is an expert and a professional. It is "switched-on scientists, not 'case-hardened eco-nuts'" who are now issuing the warnings, and he makes plain that he is one of the former group. Yet in spite of this indication of his professionalism, I confess that I cannot make heads or tails of Myers' genetics and evolution. Maybe I have missed something that readers of *The Auk* will understand, so I quote:

> To date, electrophoresis offers only partial and approximate measures of genetic variability. But as the technique is refined, it may prove useful to conservationists who agonize over whether to allocate funds towards saving species X while implicitly consigning species Y and Z to oblivion. Already electrophoresis has revealed that the average polymorphism and heterozygocity . . . for plants is 17 percent, for invertebrates 13.4, for man 6.7, for vertebrates in general 6.6, for reptiles 5, and for birds less than 5. Least variability appears among polar-region species . . . an intermediate amount among temperate-zone species, and the greatest degree among tropical species. These findings tend to confirm that the tropical rainforests, with their plethora of plant and insect species, deserve to be at the top of the conservationist's shopping list.

So much for birds, so much for Canada, Scotland, and Argentina. I think I know how Dr. Myers has confused heterozygosity, polymorphism, variability (at many levels), and species diversity in order to make his strange value judgment, but no matter. Let's go on.

> There are abundant good reasons why whales deserve the best efforts of conservationists. However, whales' longevity and low reproductive rates mean that it takes decades for one generation to give way to another. In turn, this means that whales' scope to respond to environmental changes via selection pressures and adaptive variations—the forerunner processes of speciation—is more limited than for almost any other form of life, except for similar higher animals such as elephants and rhinos, also man. So for purposes of evolutionary processes that lead to new species and greater genetic diversity, whales have little to offer as compared with virtually all other of earth's stock of species.

So much for whales. But let's go on to the inevitable conclusion.

> If conservation postulates the maintenance of genetic variety as a leading rationale for preservation of species, should we not lend a helping hand to the evolutionary processes for insects, along the lines of what we have been doing for dogs and their breeds?

I looked long and hard for some sign in all of this that Myers is playing a harmless little joke on the reader—but, alas, he seems quite serious. What bothers me most about this weird perversion of Darwinian theory is the obvious ulterior motive behind it: I can use genetics, evolution, and ecology to tell you which species should live and which must die, Myers implies. A kind of global eugenics is crawling out of the woodwork here, and this strikes me as more evil and more dangerous even than the extinction that is now going on. The species that are saved will be selected by experts on the basis of their "utilitarian benefits." But what expert is there who can calculate the net utilitarian benefit to humanity of any species (integrated over the time that humans and that species have left to coexist on the planet)? Who knows

the consequences, the pluses and the minuses, of keeping or losing any species? Not I, and certainly not Norman Myers.

Before describing what I believe to be the central defects of this book, I want to note several lesser but nonetheless irritating biases. First, for Myers, the only biome-type that really seems to count is the tropical moist forest. I also love and appreciate these forests; I was in one just a few days before writing this review. And I am aware of their peril. But they occupy only a few percent of the earth's surface—there are other ecosystems equally deserving of our concern. To equate species diversity with preservation value makes no sense, biologically or otherwise. Alaskan tundra and Idaho trout streams are just as important to keep intact. Every major climatic and altitudinal zone, every soil type, and every regional aquatic system must continue to have available a fair sample of the plants and animals that evolved there. Nothing less will do. After all, we are not about to recolonize a ravaged temperate zone desert with species that we have saved in any tropical moist forest.

Myers gives four "country profiles" in his book; in three the dominant natural ecosystem is tropical moist forest (the fourth is also tropical). Even so, for all his preoccupation with this ecosystem, he does not always seem on familiar ground. In his six-page discussion of Costa Rica, for example, he does not once mention the serious effect of banana cultivation on the moist forests of both the Caribbean and Pacific slopes.

A second minor criticism is that Myers seems reluctant to share the limelight in the text with people whose ideas he is using. The philosophical implications of the idea of conserving the smallpox virus are discussed, but Bernard Dixon's original and fascinating paper on this subject is not cited. A whole chapter is based largely on the ideas of Garrett Hardin and he is quoted directly, but I do not recall seeing his name mentioned except in the footnotes at the back of the book. Worse yet, Myers tends to slight the other side of controversial subjects: in citing Martin and Wright's classic and well-balanced work on Pleistocene extinctions, the climatic change hypothesis is relegated, without discussion, to the status of a minor factor.

But there are more significant flaws in the book. Although it hardly seems possible, Myers does not seem to have grasped the fundamental cause of modern extinctions. Leopold Kohr, who inspired E. F. Schumacher, said it clearly in his brilliant book, "The Breakdown of Nations," published nearly a quarter of a century ago: "Bigness, or oversize, is really much more than just a social problem. It appears to be the one and only problem permeating all creation. Wherever something is wrong, something is too big."

How true this is of species extinctions. There is the oversized human population, to be sure; and Myers knows that. But even more important is the oversized technology that can consume 100,000 ha of rain forest at a gulp, that can change the pH of the rainfall of a continent, that can release millions of tons of deadly and durable chemicals into the environment, and that can plan the rearrangement and usually the degradation of the ecosystems of whole nations.

What is Myers' solution to the problem of shifting cultivators in rain forests? Give them "intensive" agriculture (with its monocultures, fertilizers, massive irrigation and water diversion systems, herbicides, and insecticides) so that they will leave the forest alone. Tax the multinationals for conservation programs, regulate them, educate them (but don't hurt them), so that the massive power they wield, their global oversize, has no bad effects on rare species. Get the seed companies (nearly all owned by multinationals now) to preserve genetic variety and diversity. Develop biomass fuels on a grand scale (despite the fact that each gallon of corn-based ethanol for gasohol causes the loss of more than 100 pounds of topsoil). Get Americans to give up unnecessary medical operations, to stop drinking too much, and to stop wasting food, and we can use the "$100 billion" saved each year to preserve species. He even says that "there is no basic conflict between the economic interests of developing countries and those of developed countries." Business and growth as usual, but do it nicely.

It is Myers' refusal to come to grips with the nature and ecological consequences of bigness that causes this kind of silliness. In his preface he notes that his book was underwritten, in part, by the Rockefeller Brothers Fund. I doubt that they have found cause to regret their investment.

Finally, the basic defect of "The Sinking Ark" is that it has no moral underpinnings. As Aldo Leopold and Charles Elton have pointed out, there are better, tougher reasons for saving species than to keep them for exploitation. Myers claims to be a hard-nosed, "switched-on," scientific and economic realist. But is it realistic to believe that the cure for cancer is growing somewhere around the next bend of the Rio Negro, and that Volkswagen can be taxed, cajoled, and enlightened into saving the pristine rain forest it owns? Is it realistic to behave as if everything on this planet were meant to serve our ends—to believe that we both can and should figure out which species we can afford to throw away? A sorry kind of realism, I think, but no better than one can expect to result from a mixture of bad morality and bad ecology.—DAVID EHRENFELD.

Conservation biology: an evolutionary ecological perspective.—Michael E. Soulé and Bruce A. Wilcox (Eds.). 1979. Sunderland, Massachusetts, Sinauer Associates, Inc. 395 pp. $14.95.—The strength of this volume is mixed. The weak sections are those chapters in Parts I (Ecological Principles of Conservation, 4 chapters) and II (The Consequences of Insularization, 5 chapters) that deal with the application of island biogeography and ecological theory to conservation problems. Part III (Captive Propagation and Conservation, 5 chapters) is excellent and contains some fascinating information on the technology and problems of breeding endangered species in zoos. Part IV (Exploitation and Preservation, 4 chapters) is also strong and reviews large-scale preservation (African wildlife and rain forests) and the details of management, and closes with a call for change by Ehrlich.

The volume begins with a foreword by Thomas Lovejoy of the World Wildlife Fund, who reviews the destruction of our planet and argues that because of this the demands on science are large. Ehrlich, in the closing chapter, is closer to the truth: conservation has very little to do with science and very much to do with economics, politics, and sociology. And Ehrlich is right that unless there are fundamental changes in the way society views its relationship to Earth nothing else really matters. In the introductory chapter Soulé and Wilcox review conservation biology and conclude, among other things, that "conservation biology, strictly speaking, does not include the subject of economics." This revelation would be of interest to native Africans such as the Ik who are now nearly extinct as a result of their exclusion from their former hunting grounds, now the Kidepo Reserve (see Coe, Chapter 16). The editors continue reviewing conservation biology and are obvious supporters of the application of island biogeography (called insular ecology by Wilcox) to the planning and design of refuges. The editors and several of the contributors refer to some deleterious effects of isolation (extinction, inbreeding, etc.). I certainly sensed a deleterious effect of isolation while attending the conference, and it was isolation from reality. This feeling was particularly strong while I was sitting in an air-conditioned building with other white middle-class biologists urging nations located in the tropics to set aside huge areas for the preservation of the natural environment.

Another sort of isolation characterized the papers by Diamond, Wilcox, and Terborgh and Blair. Here the isolation was from standard scientific method in Popper's sense: that is, if an idea is not held up to possible falsification it is not science. To hear them speak and to read the contributions one would think that the ideas they proposed were not controversial. Simberloff (in press, J. Biogeography) has already commented on this but I would only point out that another way of looking at island biogeography and conservation design can be found in Abele and Connor (1979, Proc. First Conf. on Sci. Res. in the National Parks, Vol. I: 89–94, R. M. Linn, Ed.). It is somewhat disturbing that this paper, or any opposing viewpoint, for that matter, was not cited despite the facts that (a) the editors had a copy of it before the conference, and (b) I presented part of the paper at the conference as the introduction to a workshop on the design of nature reserves. Regardless of which view is correct, it is hardly in the spirit of scientific enquiry to protect an idea from challenge.

In chapters 2 and 5 Gilbert and Foster point out that a reserve must include successional stages, and both authors argue for managed disturbance. Eisenberg adds that mature tropical evergreen forests do not support as high a diversity of mammals as a mosaic of mixed second growth and tropical evergreen. Preserves are obviously going to require some management.

Franklin, Soulé, and Goodman make some very specific suggestions concerning the population sizes, genetics, and management of population growth. Franklin's comments that the effective population size should not be less than 50 for the short term and should be 500 for the long term bear directly on Eisenberg's data, which suggest that most large mammals in Wilpattu Park have populations below this value. Soulé provides additional information on the genetic diversity of small populations and, despite some disclaimers, recognizes the tenuous assumptions of the models. Goodman, with a very interesting example, provides details on managing populations under conditions of encouraged growth or culling.

The third section deals with captive propagation and conservation. The section begins with an overview by Conway, who points out that nearly one-twelfth of the species of birds and one-sixth of the mammals have been recently bred in zoos. He further reviews the four possible functions that captive-bred stocks could fulfill. Some of these have been successful, but the cost is fairly high and there has been inadequate support at best. Senner reviews a model of inbreeding depression in small closed populations and finds that the result is almost always extinction. His recommendations echo those of Franklin and Soulé: to avoid founders that are inbred and caution against an unbalanced sex ratio. Benirschke, Lasley, and Ryder present a fascinating report on the technology of captive propagation, including using endocrine studies to sex individuals and karyotyping individuals of the same "species" prior to mating attempts. The chromosome data are of much wider interest than captive propagation. They conclude optimistically concerning the availability of the necessary biomedical technology, but suggest that zoos can serve as a

last refuge for only a small number of vanishing species. Kleiman emphasizes the many complexities of sociobiology as it relates to captive propagation. These can include the process of mate selection that results from intense fighting, infanticide, the need for extended family groups in some species, and suppression of mating by male dominance. She also points out the need for zoos to publish their failures as well as their successes. It is difficult to avoid a certain amount of pessimism here. Earlier chapters have made a number of important recommendations concerning group size and inbreeding, for example. Yet it is apparent from Kleiman's work that it is going to be difficult to satisfy the sociobiological requirements of some species and at the same time avoid the problems associated with small populations. Campbell reviews reintroduction of captively-bred animals into the natural habitat. He cautions that the few successful introductions, such as the European bison, ibex, and chamois, should not overshadow the numerous problems, particularly those of economics. Campbell points out that the Peregrine Falcon program has already cost one million dollars and will probably run to 4 or 5 million dollars if it is maintained for another 10 or 15 years. While this may seem outrageous to some taxpayers, it is a small amount compared to the cost involved in the restoration of historic structures, as mentioned by Campbell. Ten problems associated with reintroduction are summarized, ranging from permits and laws to the chauvinistic attitude of Welsh nationalists who opposed the reintroduction of the rosy marsh moth into England.

The fourth section deals with exploitation and preservation. Coe begins this section with a detailed review of African wildlife resources. He reviews the use of game ranches and the relationship of Africans to their wildlife, especially as a source of protein. I find it difficult to be sympathetic to conservationists who ignore Coe's recommendation that national parks and reserves be relevant to the lives of rural Africans who live in the area. A "vehicles only" policy of parks can't help but antagonize the very people whose cooperation is vital to the survival of the park. Is the extinction of the Ik tribe less serious than the loss of another species? Whitmore deals with tropical rain forests of the world. He begins with the astonishing statistic that rain forests have disappeared at the rate of 21 ha per minute between 1964 and 1973. I would suspect that the rate has probably doubled since 1973. It is easy to agree with Whitmore that "adequate" (= large and representative) samples of virgin rain forest should be preserved, but the biological, social, and economic complexities of doing this in the nations where the forests are located appear overwhelming. Pyle deals briefly with the management of reserves, and his conclusion can be summarized in his own words—"The emphasis is on practicum." Despite the glowing optimism of "insular ecologists" the specific problems of management have much to do with everyday activities such as vandalism, pollution, and legal challenges, and virtually nothing to do with extinction coefficients that have half-times of thousands of years. These everyday problems will have done their damage long before the long-term effects of insularization are felt, claims to the contrary notwithstanding. As already mentioned, Ehrlich, in the closing chapter, argues that nothing short of radical changes in the behavior of the world's societies will save the Earth. He is right.—LAWRENCE G. ABELE.

Character variation and evolution of sibling species in the *Empidonax difficilis-flavescens* complex (Aves: Tyrannidae).—Ned K. Johnson. 1980. University of California Publications in Zoology, Vol. 112. Berkeley, University of California Press. x + 154 pp., 39 text figures + 3 plates. $9.50.—Picture a beginning graduate student, eager to select a research project that will make a mark, about to examine the contents of specimen cases in a university museum. If by chance the first case contained tyrannid flycatchers, and if, due to continuing misfortune, the first drawers opened were those containing species of *Empidonax,* most likely our student would quietly close the case, thank the curator, and select a research project in the physiology of chickens. Most members of *Empidonax* are remarkably similar in size, shape, color, and foraging behavior, in fact so much so that some are interspecifically territorial to a degree.

To the seasoned systematist, however, the *Empidonax* group presents a number of intriguing problems. The genus is rich in sets of superspecies. A number of species have remained allopatric long after the completion of geographic speciation. Ned Johnson has taken on this group, and in the present volume sets out to examine the early stages in the evolution of morphologically very similar species, the Western Flycatcher (*E. difficilis*) and the Yellowish Flycatcher (*E. flavescens*). The development of isolating mechanisms in the formation of sibling species, and the relationship between morphological stability and isolating mechanisms, are also examined. The analysis is based on morphological measurements from 1,284 bird skins and on 5,032 sonagrams of 208 individual songs. Fieldwork began in 1964 and continued through 1970.

As with many papers emanating from the Museum of Vertebrate Zoology that appear in the University

of California Publications in Zoology series, one gets a lot of basic biology along with information more central to the systematic theme. Johnson provides a comparative treatment of distribution, abundance, and habitat requirements of the populations and species being studied. Each species of *Empidonax* is reported to occupy a distinctive habitat for nesting.

An analysis of character correlations indicated that some skin and song characters were redundant, so the character set was reduced to minimize redundancies. The correlation matrix was presumably on the total sample of males (females were not so treated), but it could have been on locality means. This distinction is important, but the statement of methods is silent on this issue.

Comparison of the sexes showed that males tend to be larger than females in most characters. Greater sexual dimorphism was found for *E. difficilis* than for *E. flavescens*.

There were greater coefficients of variation for bill characters than for wing and tail measurements, a pattern found for many other avian species. Why bill size should be more variable than wing and tail size is puzzling, and a few authors have theorized about this. Rothstein (1973 Amer. Natur. 107: 796–799), for example, speculated that because of variation in food, bill dimensions are subject to more extreme yearly shifts in directional selection, resulting in the accumulation of greater variability. Johnson believes this is not the entire story, and points out that wing size in aerial foragers is tied to food gathering and therefore ought to show greater variability than it does. Actually, this is not a particularly good counterargument because even in highly aerial foragers, such as swifts, it is difficult to accept the idea that the wing is more of a "feeding structure" than in any other bird. No one yet has a good explanation for the character variability sequence. Processes more closely tied to the physical aspects of growth and development may be responsible.

In tests involving relative variability of individual characters there are no differences between the sexes, between *E. difficilis* and *E. flavescens,* nor between *E. difficilis* and *E. hammondi.* Overall comparisons, however, suggest that *E. hammondi* is more variable than *E. flavescens,* which is more variable than *E. difficilis.* Mention is made of other theories concerning variability and whether or not the *Empidonax* data agree. The data are not adequate, however, to develop any of these ideas successfully.

Each mensural, color, and song character was examined by an analysis of variance procedure that grouped sets of statistically nonsignificant locality samples. This, coupled with the clear graphic presentation on range maps, makes it easy to look for broad clines as well as abrupt steps, and even allows comparison of specific pairs of locality samples. Some of the clearest clines are seen with color (e.g., purity of breast color), while other characters show seesaw patterns of variation. Song is the basis for believing that *E. difficilis* and *E. flavescens* are separate biological species. The advertising songs of both species differ in obvious ways and are presumed to serve as premating isolating mechanisms. Song traits were measured from sonagrams and patterns tested by analysis of variance.

Multivariate analysis was also used. Presumably because complete data were lacking for all samples, five separate data sets were analyzed: one of 10 samples of male *E. difficilis* with 22 mensural, color, and song characters; two based on 22 size and color characters of 22 samples of males and 19 samples of females of *E. difficilis*; and two based on 22 size and color characters of 49 samples of males and 42 samples of females of *E. difficilis* and *E. flavescens* combined. Two multivariate procedures were used, principal components analysis of correlation matrices based on locality means, and cluster analysis of taxonomic distances, also calculated on locality means. The preferred statistical procedure would have been canonical variates analysis because it examines among-population differences relative to within-group differences. Nonetheless, Johnson's analysis does yield interpretable results. In both the principal components and cluster analysis there are clumps of samples that are distinguishable, and the overall impression is that the statistical analysis mirrors the geographic position of samples and combines populations according to relationships suggested by the existing subspecies. One of the problems with five separate data sets and two methods of analysis is that specific comparisons often do not match or are not replicable from one test to the next. This indicates one of the main difficulties in interpreting too literally the results of single statistical tests in systematics, but, taken together, a biologically realistic picture emerges.

Zones of contact between *E. d. difficilis,* the coastal subspecies, and *E. difficilis hellmayri,* an interior subspecies, came under scrutiny via discriminant function analysis. A single function made up of size and color characters, and another based on components of advertising song, were calculated to maximally separate the two races. The geographically intermediate sample came out morphologically and vocally intermediate, with a broader range of variation than either of the two "pure" samples. This not only seemed to demonstrate integration, but increased variability due to hybridization, recombination, and backcrossing as well.

Of all the characters examined, clear clinal variation exists in very few. Most vary in a seesaw fashion, and Johnson's attempt to explain this by climate and ecological correlates is admittedly limited by a lack of data. It seems that a variety of selective factors may be acting on different suites of characters. To unravel the selective forces will be difficult.

Johnson subjectively identified concordance among characters. "Whole complexes of features change more or less simultaneously over fairly narrow and well defined zones" (p. 102). The major regions with minimal geographic variation within them are: the Channel Islands, Pacific Coast, Rocky Mountains to central Oaxaca, nuclear Central America, and Costa Rica to Panama. There are also zones of abrupt character change between these regions, specifically between the southwestern California coast and the Channel Islands, along the crest of the Cascade Mountains, in north Mexico, at the Isthmus of Tehuantepec, and in the Nicaraguan depression. Generally, size and habitat choice vary concordantly, with strong gradients over short distances. Vocal characters change more gradually. No generalization was possible for color.

The geographic variation analysis supports the subspecies already established for both species. Johnson expresses assurance that the trinomial system should remain intact for this group. Two sets of subspecies are believed to be "megasubspecies," subspecies presumably approaching species status. The view is supported that *E. difficilis* and *E. flavescens* are truly allopatric in their breeding distribution, but that the two are distinct biological species.

To conclude, Johnson presents a hypothetical scheme for evolution in *Empidonax*. In Stage I there is rapid morphological character divergence between two sibling populations, with much slower change in vocalizations. In Stage II allopatric divergence continues for morphology, physiology, and color, and is paralled by divergence in song. Following a brief period of parapatry, sympatry between close relatives may occur, and here, in Stage III, divergence in vocalizations catches up to divergence in physical traits, and both increase at a greater rate than in Stage II. Several species pairs are plotted to show their position in this scheme. The model seems useful. One concern is that I do not know what to make of the fact that two subspecies of one species (*E. d. difficilis* and *E. d. hellmayri*) are farther along at the parapatry-sympatry border (Stage III), while two subspecies of different species (*E. difficilis occidentalis* and *E. flavescens salvinii*) are still in late allopatry phase (Stage II). One minor criticism is that Johnson labels one of the axes in his speciation graph "rate of character change," when it should read simply "character change" or "degree of divergence." The model depicts absolute change or divergence, not the rate of that change, at least in my interpretation.

Johnson's study has eliminated most of the taxonomic confusion about this group. It has been an interesting challenge that has been met by the author's solid understanding of the biology of *Empidonax* flycatchers, by multivariate morphometrics, and by an extensive analysis of vocalizations, all followed by good sense.—DENNIS M. POWER.

Avian orientation and navigation.—Klaus Schmidt-Koenig. 1979. London, Academic Press. x + 180 pp., 84 figures. $26.50.—During the last decade the increase in studies of avian orientation and navigation has been dramatic. This is particularly true for studies of avian navigation in homing pigeons. Schmidt-Koenig has been very active in this field, and most of his research has concentrated on various aspects of pigeon homing. In addition, he has been instrumental in planning and coediting the proceedings of two international symposia on animal migration, orientation, and navigation; one in 1970 at Wallops Island, Virginia, and the other at Tubingen, West Germany in 1977. In this volume Schmidt-Koenig provides a concise and authoritative summary of most of the studies of avian orientation and navigation that have been published to date, particularly those in the last decade.

The volume is divided into nine chapters. The first gives a much abbreviated introduction (8 pages) to bird migration, and the second covers briefly (13 pages) some of the major techniques used in studying bird migration in the field (field-glass observations, radar, and radio tracking). The third (42 pages) is devoted to laboratory studies of the basic and integrated sensory capabilities of birds that may be used in orientation and navigation as well as the orientation of caged birds, and in the fourth chapter, Schmidt-Koenig allocates 12 pages to the experimental fieldwork with wild birds (e.g. displacement experiments, establishment of breeding and wintering ranges). The fifth chapter is the longest in the book (55 pages) and not surprisingly covers homing experiments with pigeons. The hypotheses, theories, and concepts that have been advanced to explain how birds might navigate are discussed in chapter 6 (13 pages), and chapter 7 is a 1-page conclusion. The eighth chapter contains a brief and very valuable summary of all the preceding chapters and sections in the book. The final chapter is only 1¼ pages long and explains

the use of circular or two-dimensional statistics in the volume. The bibliography is excellent and up-to-date, and a 4-page index concludes the volume.

The chapter devoted to laboratory studies includes the recent findings that pigeons are sensitive to ambient pressure changes, infrasound, odors, polarized light, ultraviolet light, and magnetism. The sun, star, and magnetic compasses used in orientation are also discussed, and Schmidt-Koenig, like many others, feel that the compasses may be hierarchically arranged, that is, that the basic compass may be the magnetic compass and the sun and star compasses are aligned or calibrated relative to the magnetic one. Even if this is so, the term "hierarchical" is not really appropriate and may be misleading to those familiar with hierarchical theory. The compasses are alternatives and depend on the availability of the appropriate cue. As Schmidt-Koenig emphasizes, all three are established ontogenetically in different ways and operate differently with respect to vision and time compensation.

In reviewing the hypotheses of avian navigation, Schmidt-Koenig concludes that there is no experimental support for the hypothesis that a grid of isoclines of Coriolis force and the vertical component of the geomagnetic field is used for navigation. The theories of sun, star, and inertial navigation are discredited similarly. Olfactory navigation in pigeons has some experimental support, but conflicting results exist. Vector "navigation" involving direction and distance information is supported by experimental results and may explain how birds-of-the-year accomplish their first autumn migration, but the utility of this concept in bird navigation is limited.

The original Kramer concept of map and compass is still the basic frame of reference in avian navigation studies, and although much is known about the compasses birds use in orientation, virtually nothing is known about the map component. Consequently, we still do not really know how birds navigate and home after displacement to an unknown location. It is perhaps time to examine critically and revise the conceptualization of navigation in birds in terms of map and compass components.

Although the book is well written and illustrated it contains a few typographical errors (e.g. Waltschko instead of Wiltschko on page v, severel instead of several on page 21, and Clemson, N. C. instead of Clemson, S. C. on pages 159 and 175). Schmidt-Koenig has done an exceptionally fine job of summarizing the abundant and complex information that characterizes the rapidly growing area of avian orientation and navigation research. It is a pity that the book costs so much, because it should be on the bookshelf of every avian biologist.—SIDNEY A. GAUTHREAUX, JR.

Birds of the British Lower Eocene.—C. J. O. Harrison and C. A. Walker. 1977. Tertiary Research Special Paper Number 3. London, The Tertiary Research Group. 52 pp. **Birds of the British Upper Eocene.**—C. J. O. Harrison and C. A. Walker. 1976. Zoological Journal of the Linnean Society 59: 323–351.—Colin J. O. Harrison and Cyril A. Walker have coauthored at least 20 papers in avian paleontology since 1970. Harrison has been sole author on a similar number of paleontological papers during the same period. To date there has been no critical published review of their publications, and an assessment of the merits of these papers is warranted. Although the present review will focus only on two papers concerning the Eocene avifauna of Britain, the remarks below are equally applicable in concept to other studies of fossil birds published by these authors.

Because Lydekker (1891, Cat. Fossil Birds Brit. Mus. (Nat. Hist.), London) was the last author to review the Eocene fossil birds of Britain in a comprehensive manner, Harrison and Walker's research is timely and ought to be a significant improvement in our knowledge of Eocene birds. After short introductions and lists of taxa involved, both papers proceed with systematic descriptions and diagnoses of the fossils, followed by a brief discussion (1977) or no discussion at all (1976). The basic organization of these papers seems to be logical, but one is soon frustrated by the brevity or complete lack of interpretive information, and by the failure of Harrison and Walker to discuss the biological significance of their findings. They do not place the various taxa in any kind of evolutionary context. There are seldom even statements such as, "This is the earliest known fossil of the Hydrobatidae." Only once is reference made to the geology of the deposits from which the fossil birds were collected. There is no indication of the nature of the stratigraphic data that accompany each specimen, although one might expect that precise stratigraphic information would be lacking for the majority of specimens, many of which were obtained from a variety of collectors more than a century ago. Furthermore, Harrison and Walker never discuss or give reference to the associated flora or fauna of their fossil sites. Such information exists and must be readily available at the British Museum. Thus, no attempt is made to relate the Eocene birds of Britain to the environment in which they lived.

Each species account is divided into subsections such as "Diagnosis," "Material," "Occurrence," "De-

scriptions," "Measurements," and "Comments." Synonymies are provided for previously described taxa, and etymologies are given for newly named taxa. In these two papers alone, Harrison and Walker have named 2 new families, 14 new genera, and 18 new species from lower Eocene deposits, and 7 new genera and species from upper Eocene localities. They begin the systematic section of their lower Eocene paper with the description of *Marinavis longirostris* as a new family, genus, and species of supposed procellariiform affinities, the species being based on three scraps of a bill and an extremely fragmentary carpometacarpus. Their "diagnosis" of the Marinavidae (p. 6) does not mention any other family of Procellariiformes, so it is impossible to determine how the Marinavidae resembles or differs from other members of the order. Even if *Marinavis* possessed only characters that were clearly procellariiform in nature, the available material would simply be inadequate for the establishment of new taxa. Harrison and Walker's admission of the presence in *Marinavis* of characters that are suggestive of the Pelecaniformes and the Laridae, as well as others not found in any of the modern taxa considered, makes their ordinal assignment even more suspect. They designate a rostral hook as a paratype of *Marinavis,* but go on to say (1977: 8) that "the rostral hook and carpometacarpus might be referable to either order [Procellariiformes or Pelecaniformes]." They provide no justification at all for referring the carpometacarpus even to the order Procellariiformes, much less to their newly erected Marinavidae. The treatment of *Marinavis* can be cited further as an example of three characteristic features of Harrison and Walker's work: 1) the complete or nearly complete failure to mention which living species were used in their osteological comparisons; 2) the use of "descriptions" that are extremely lengthy and redundant, particularly when accompanied by an illustration; 3) the unjustified referral of a fossil fragment to a taxon that is otherwise based solely on a different skeletal element (which may be equally fragmentary and uninformative).

The description of *Precursor* (1977: 24), a new genus of supposed charadriiform relationship and possibly a glareolid, suffers from many of the same conceptual problems as that of *Marinavis* and contains additional kinds of errors as well. Harrison and Walker create three new species of *Precursor*: *parvus,* based on the distal end of a humerus and a paratypical proximal end of a humerus; *magnus,* based on the distal end of a tarsometatarsus only; and *litorum,* based only on the distal end of a humerus. All three holotypes are illustrated in Plate 6, with *parvus* enlarged 5×, *magnus* enlarged 3×, and *litorum* enlarged 2×. Direct comparison of the illustrations of *parvus* and *litorum* (both based on distal ends of humeri) is rendered nearly impossible by their separation on opposite sides of the tarsometatarsi of *magnus* and by the use of such different size scales. Virtually no comparable measurements are given for these humeri, so once again it is very difficult to compare the two specimens directly. Early in the "description" of each supposed species of *Precursor* is found the ubiquitous and frustrating statement alluding to the "good condition" of the fragment in question, despite its many flaws that are then described. Of *P. parvus* they say (p. 25), ". . . well-preserved but the palmar aspect of the condyles has been worn away and there is slight damage to the anconal tips of the ectepicondyle and entepicondyle"; of *P. magnus* (p. 26), ". . . in a very good state of preservation, but lacking the trochlea for the fourth digit and proximally broken a little before the metatarsal facet"; of *P. litorum* (p. 26), ". . . in a good state of preservation but the ectepicondylar ridge is eroded." I am not at all convinced that any of the specimens of *Precursor* are members of the Charadriiformes, much less the Glareolidae. A conclusive judgement on this matter requires better specimens.

The photographs of *Precursor* in Plate 6, mentioned above, do not stand alone in being misleading or incorrect. Plate 1: A–E illustrates *Primodroma bournei,* a new genus and species of storm-petrel whose name does not appear in the caption because line 5 is reproduced twice: once in line 1, and then in its correct place. One thus gets the initial impression that the humerus of *Primodroma* belongs to *Pseudodontornis,* which is misspelled in line 5. *Parvigyps praecox,* a new genus and species of Accipitridae, appears in Plate 7: D–J, and not Plate 7: D–H as stated on p. 31. The holotype tarsometatarsus of *Argillipes aurorum* appears in Plate 7 and is therefore isolated from the tarsometatarsi of the other putative galliforms that are shown in Plate 8. Plate 8: E illustrates the humerus of *Argillipes aurorum* and not *Argillornis longipennis,* as claimed on p. 20.

The main difference between the late Eocene study of Harrison and Walker (1976) and the 1977 paper is the younger age and the smaller size of the avifauna in the former. The same conceptual errors are evident throughout, and the reader is once again given few clues as to why a particular fossil was assigned to order or family. The description of *Colymboides anglicus* Lydekker 1891, occupies 1½ pages, yet says little that is not obtainable from the photographs in Plate 1. Statements such as (p. 327), "The bone [humerus] widens at the distal end . . ." are applicable to birds in general and are of no value in defining taxa. The synonymies in Harrison and Walker (1976) are inadequate and inconsistent. Publication dates are not given in the synonymies of *Colymboides anglicus* or *Piscator tenuirostris.* The authors also omit

any of the volumes of Brodkorb's *Catalogue of Fossil Birds* in the Literature Cited, despite using Brodkorb's name in certain of the synonymies.

They describe *Howardia eous* as a new genus and species of Anatidae (p. 337), but the generic name *Howardia* is preoccupied, being only one of no fewer than four preoccupied generic names already proposed by Harrison and Walker. In addition, *Howardia* is based on a single piece of sternum so fragmentary that it cannot possibly be accurately identified and diagnosed. When fossils are so damaged that identification even at the ordinal level is impossible or highly speculative, what knowledge is gained by describing these specimens as new genera and species? It is evidently Harrison and Walker's philosophy that every fragmentary fossil can and should be identified and given a name, as though the naming of new taxa were an end in itself. In pursuit of their philosophy, they have come to regard a paleontological species of bird as being constituted by a single end of a bone instead of an entire skeleton. Little attempt is made to refer elements of both the wing and leg to the same taxon, the result being a wild proliferation of new names. Of the 25 new species named in their two Eocene papers, only three have elements from both the pectoral and pelvic assemblages assigned to them. This taxonomic treatment has in effect made holotypes of the majority of early Tertiary fossil birds in the British Museum. Even when they do refer more than one element to a single species, this is often done arbitrarily and with little justification.

The naming of completely undiagnostic fossils would be less disturbing if most of their potentially diagnostic specimens were at least assigned to the correct order. From an examination of Harrison and Walker's figures and descriptions, Olson and Feduccia (1979, Proc. Biol. Soc. Washington 92: 494–497) have determined three of Harrison and Walker's new genera (*Parvicuculus, Procuculus,* and *Primoscens*) to have been incorrectly assigned to order. After examining the actual specimens involved, I am in full agreement with Olson and Feduccia. The description of an unassociated, damaged Eocene fossil as a new species in a modern family, just because it is reminiscent of one end of one bone in that family, is not a valid practice in other subfields of paleontology, and avian paleontology should not be an exception. Olson (1977, Smithsonian Contrib. Paleobiol. No. 35) has pointed out that *Limnofregata,* a primitive frigatebird from the lower Eocene of Wyoming, could be safely referred to the Fregatidae only after studying the entire skeleton. Familial misidentifications could easily have arisen had only isolated elements of its skeleton been found, because certain bones of *Limnofregata* have characters of other higher taxa. The fact that most early Tertiary birds cannot be diagnosed from single elements is sufficient to invalidate many of the systematic conclusions of Harrison and Walker; the majority of their newly named taxa from the Eocene of Britain can only be regarded as "Aves, incertae sedis."

In summary, the paleontology of Harrison and Walker consists of the application of names to what are usually undiagnostic or misidentified bones, without consideration of their geological or biological context. The inadequate diagnoses and unhelpful descriptions contain many nomenclatorial, typographical, and grammatical errors. Their publications are reminiscent of the 19th century precepts exercised by such workers as R. W. Shufeldt and C. W. DeVis. The correction of the past errors of these and other authors has been an important duty of avian paleontologists in the 20th century. Unfortunately, it appears that such remedial work is not only unfinished, but is being made increasingly necessary, thus impeding the genuine progess that has been made in avian paleontology in the past few decades. For someone with sufficient knowledge of avian osteology to make his own assessments, the photographs in Harrison and Walker's papers are of interest in that they illustrate specimens not figured elsewhere and not necessarily available for personal inspection at the British Museum (Natural History). I would have to advise anyone else simply to ignore these publications.—DAVID W. STEADMAN.

Biogéographie et écologie.—J. Blondel. 1979. Paris, Masson. x + 173 pp., 47 figures, numerous tables. Paper. 130 French francs.—For about 17 years now Jacques Blondel, who is a senior research worker with the Centre National de la Recherche Scientifique (CNRS) and a professor at the Technical and Scientific University of Languedoc in Montpellier, France, has carried out detailed and long-term investigations on the structure of bird communities in Mediterranean France and elsewhere. In this book Blondel not only summarizes his own studies and those of his coworkers, but, more importantly, integrates this work within the broader field of community ecology and geographical ecology. The result is a remarkable volume, small in size but rich in ideas and examples, and especially valuable in its detailed attempt at a synthesis of modern theory and extensive, often original field data.

Blondel approaches his subject fully within the framework of what he calls MacArthur and Wilson's paradigm. But instead of dwelling upon already existing theory or developing more theory through further mathematical refinements, Blondel resolutely tries to see to what extent predictions from theory can be verified after careful measurements of crucial parameters in nature.

The book is divided into five chapters. In the first (The Diagnosis of Distribution Patterns), the various levels of integration that one can use to approach the study of specific or community distribution patterns are discussed. Blondel recognizes five levels: continental, regional, sectorial, biotope, and sampling site. For each level he explains problems of definition and of analysis of diversity, using as the major example his own work in southern France, especially his distributional studies on Mount Ventoux. The second chapter (Composition and Structure of Communities) analyzes richness, abundance, and diversity of communities. Note that Blondel distinguishes two kinds of "communities," called respectively in French *communauté* and *peuplement*. The first corresponds to the biocenosis, the second to the community in English language publications. In chapter three (Strategies of Use of Ecological Space and Community Regulation), Blondel discusses causal factors of community distribution and structure. In the fourth chapter (Ecological Successions and Ecosystem Development) are examined the factors that contribute to community dynamics over time, with detailed examples from the long-term succession studies carried out by C. Ferry and B. Frochot on forest avifaunas in Burgundy, and from the gradient analyses by Blondel himself in Mediterranean avifaunas. In the last chapter (Insular Communities) the problems of richness, diversity, structure, turnover, and adaptive strategies are analyzed with special reference to Blondel's own studies (with coworkers B. Frochot and P. Isenmann) of avian communities in mainland France and the island of Corsica.

All the major questions being currently debated are discussed in this book, including among others niche breadth, niche width, niche overlap, alpha, beta, and gamma diversities, stability, habitat selection, *r*- and *K*-selection, and turnover. In spite of the occasional uncritical acceptance of some theoretical ideas or research results, which sometimes mars the originality of the work, this book seems to me to be one of the best statements of the current state of geographical ecology and community ecology available in any language. Besides the examples from work by the French school of avian ecologists, with which English-speaking workers are unfortunately largely unfamiliar, Blondel's book has the further merit of also integrating other European with American studies. If it had been written in English instead of French, this volume would have been perfect for the series of "Monographs in Population Biology" published by Princeton University Press; it would be well worth having this book translated into English and republished.—FRANÇOIS VUILLEUMIER.

Neural mechanisms of behavior in the pigeon.—A. M. Granda and J. H. Maxwell (Eds.). 1979. New York, Plenum Press. xvi + 436 pp. $42.50.—The past 25 years have witnessed a resurgence of studies in comparative neurobiology, characterized by an emphasis on brain-behavior relationships and involving a combination of anatomical, physiological, and behavioral techniques. Because they offer a rich repertoire of species-typical behaviors combined with a remarkable assortment of learning abilities, birds have been among the most common subjects for such studies. Our knowledge of avian neuroanatomy and sensory physiology is expanding rapidly, and we can begin to discern the outlines of some of the neural mechanisms underlying eating and drinking, vocalization, aggressive behavior, orientation, reproductive behavior, and perceptual processes in birds. A broad and authoritative review of this work would therefore be welcomed by the general reader, even at the price of this book. Unfortunately, the present volume is not such a review, despite its title. It reflects, instead, the proceedings of a conference devoted primarily to the study of visual processes; "Visual mechanisms of behavior in the pigeon" would thus be a more appropriate title. Like all conference proceedings, it suffers from a certain arbitrariness of inclusion (Keeton's chapter on navigation; Boord and Karten's review of anatomy of the vestibular system) and exclusion (e.g. interocular transfer). Its focus on a single species, while reflecting the state of the field, further limits its usefulness for the generalist, and a chapter on Columbiform taxonomy is so perfunctory as to be useless to either the specialist or generalist.

These necessary caveats disposed of, the book's many specialized virtues should be noted. The bulk of the book is devoted to visual psychophysics, particularly the psychophysics of color vision, with a subsidiary emphasis on the role of retinal oil droplets. These chapters are of interest not only for their substantive content but because they illustrate the variety and sophistication of the behavioral procedures available for the experimental analysis of the sensory world of birds. A second major group of papers deals with the anatomy and physiology of central visual structures, their possible homologies with mammalian structures, and their presumed role in various aspects of visual perception. They contain two very clear messages for the comparative neurobiologist. First, there is a surprising degree of similarity in the organization and function of visual structures in birds and mammals, but the differences may be quite illuminating. Second, even "simple" perceptual processes (e.g. optomotor reflexes) are likely to reflect widely distributed central neural processes.

While the general reader may not want to buy this book, its publication is a sign that traditional ornithologists will increasingly find themselves in the company of scientific colleagues of whose existence they formerly had only the vaguest knowledge. Collaboration between these two groups will ultimately add a new dimension to the study of birds.—H. PHILIP ZEIGLER.

Catálogo de las aves de Cuba.—Orlando H. Garrido and Florentino Garcia Montaña. 1975. La Habana, Cuban Academy of Science. 149 pp., one locality map. No price given.—The same authors in 1965 published a catalogue by the same title in which 312 species comprized the Cuban list. This has now increased to 329 native and 3 introduced species, not counting domesticated birds; nomenclatural and classificatory changes also warranted a new edition of the check-list. This whole book is written in Spanish in concise, easy sentences. Although no literature is cited (except for the scientific names used) the authors give in the introduction a narrative account of the history of Cuban ornithology and give credit to those ornithologists of the U.S.A. who recently contributed to Cuban and Antillean ornithology, especially James Bond, whose systematic list they confess to follow. The scientific nomenclature relies on Peters' and Hellmayr's catalogues, without consideration of the recent A.O.U. nomenclatural changes. Spanish and English vernaculars of the species are given, the former often created by the authors where no Cuban vernacular was found. Upon leafing through the check-list, we notice that besides the local avifaunistic role, the terminal dates of seasonal occurrence are also often given, and that these result quite commonly from the observations of author Sr. Garrido, especially when it comes to passerines. The principal supplier of new inshore and cay occurrences is Sr. Joaquín de la Vara, and numerous rare occurrences have been noted and collected by Srs. F. García and Telmo Naranjo. These seem to be the leading field ornithologists of Cuba today. We learn that senior author García has collected and described six new Cuban subspecies, viz. *Saurothera merlini santamariae, Centurus superciliaris sanfelipensis* and *C. s. florentinoi, Xiphidiopicus percussus monticola* and *X. p. gloriae* (the latter endemic to the Cantiles keys, at the south coast straight south of Habana and Santo Tomas), and finally *Contopus caribeus sanfelipensis,* on the keys of the same name. All of these were described between 1966 and 1972 in the publications of the Museum of the Cuban Academy of Sciences named after Felipe Poey (the annals being called *Poeiana*). This check-list definitely advances our knowledge of North American birds wintering or passing through Cuba and of those of accidental occurrence. Fifteen species have been collected only once, another 22 twice, and most of the increment of this Cuban check-list over the last edition is recruited from these accidentals. Of interest to avian geographers are the rarities Sr. de la Vara collected, viz. *Puffinus diomedea* and *P. griseus, Oceanodroma castro, Phaethon aethereus,* and *Xema sabini,* and also the accidental occurrence of *Oceanites o. oceanicus* and *Phaetusa simplex* (though both without date of collection). As the check-list tabulates the breeding species as well as the domesticated birds and hypothetical lists, it seems truly to represent the knowledge of avian faunistics and systematics of Cuba in 1975, and we hope for continuation of fruitful research and publication by our Cuban colleagues.—MIKLOS D. F. UDVARDY.

Fauna republicii socialiste Rômania. Aves (Păsări).—I. I. Cătuneanu (Ed.), I. Koródi Gál, D. Munteanu, S. Paşcovschi, and E. Vespremeanù. 1978. Bucureşti, Rumanian Academic Publisher. Vol. XV. Issue 1. 316 pp. with 152 figures. Price Lei 34.00.—Two-thirds of this book is introduction, the remainder a systematic treatment of the orders Gaviiformes, Podicipediformes, Procellariiformes, and Pelecaniformes, following Wetmore's system and Vaurie's lower classification. The entire book is written in the Rumanian language, aimed at the interested public of that country. Thus the introduction is a concise handbook of ornithology, with chapters (some shorter, others more extensive) on the history of ornithology in general and on Rumanian territory in particular, morphology and anatomy, ecology, paleontology, avian geography, economic and conservational considerations, and a brief introduction to the methods of avian research and systematics. The systematic part provides a key to the orders, and within each family there is a key to the species. The species descriptions follow the usual, exhaustive European handbook format: field marks, habitat, description of plumages, molts, mensural data, and voice. Systematic status and problems are touched upon but very briefly. Detailed are world (with full page map) and domestic (likewise) distributions, migration, seasonal occurrence, banding recoveries, breeding biology, food habits, behavior where known, and economic status, with a simple but well-executed (but less well-printed) line drawing.

It is welcome news in general that Rumania has now started a handbook series, which undoubtedly will stimulate more interest in bird studies within the country and make the results of these studies more

accessible to the rest of the world's ornithologists (Rumanian being a Latin language, is easily followed by those who master some of the other Neolatin tongues). In particular, the strong emphasis on avian geography is especially laudable in a faunistically oriented handbook. The 70-odd pages devoted to this discipline outline the Sclater-Wallace system of zoogeographic regions, with world distribution maps of 33 bird families given as examples (some of these, though, are incomplete [Phalaropodidae] or faulty [Bombycillidae], but they convey the general idea). The faunal elements of the Palearctic region (following B. Stegmann and K. Voous) are also discussed and presented in detail before the analysis of the Rumanian avifauna begins. This latter section consists of 253 breeding species that occupy the five major ecosystem groups that cover the country. The known distribution of each is mapped with the characteristic, accessorical, and ubiquitous species listed and mapped: the alpine tundra has 8 characteristic (exclusive) species, the montane coniferous forest contains 18, the deciduous forest 81, the prairies and cultivated grasslands (mostly cereal acres) 17 species, and wetlands harbor 75 "characteristic species." This adds up only to 209 species. The very widely spread species amount to 40, but the tally repeats certain species as secondary in several ecosystem groups, much as A. H. Miller did with the California avifauna in 1952 (which was not known to the authors of the Rumanian handbook). There is an extremely useful tabulation of the breeding avifauna that shows, besides zoogeographic affinities, whether the species is expanding, regressing, or stable in the territory concerned. To sum it up, whereas this neat handbook in many respects resembles a state or province monograph in North America, the faunistic and avigeographic treatment makes it far superior and exemplary.—MIKLOS D. F. UDVARDY.

Survey of the Balkan Peninsula bird fauna.—S. D. Matvejev. 1976. Beograd, Serbian Academy of Science and Arts. (Monogr. No. 491, Section Nat. Math. Sciences No. 46). Part I. Woodpeckers and perching birds, Piciformes and Passeriformes. 365 pp., one fold-out map, and 86 figures (mostly distribution maps). No price given.—Sergei Matvejev's works reflect the welcome and modern combination of ecological and taxonomical background that earlier resulted in such important volumes as his Ornithogeographia Serbica (1950), Biogeografija Jugoslavije (1961), and Catalogus Faunae Jougoslaviae, Aves (1973, with V. F. Vasić). As the previous ones, this book also contains, beside the main text in Serbo-Croatian, an extensive (22-page) summary in English that is recommended reading even for those faunists and taxonomists whose interests lie far away from the Balkan Peninsula of southeastern Europe.

Preparations for this book consisted of decades of fieldwork to complement the faunistic and taxonomic gaps of knowledge, and several years of meticulous microtaxonomical study in the course of which several thousand Yugoslavian skins were compared with the rich western Palearctic collection of the Soviet Academy of Sciences in Leningrad. The point is well taken by Matvejev when he explains that the biology of birds was considered in addition to morphological characters in researching infraspecific categories. Breeding habitat, ecogeographic affinities, quality, quantity, and direction of character changes, behavior, migrational characteristics, and vocalizations were decisive factors. Population groups thus discerned were often overlapping with previously described morphological subspecies. Others to which taxonomists such as Vaurie (1959) did not assign the rank of subspecies were found on ecological bases to be in the process of evolving into subspecies or else to be ecotypes. I find the recognition of ecotypes—"group occurrences of a nonhereditary demonstration of ecological variability, characteristic of the genotype of the species" (p. 328), especially noteworthy. Another group of infraspecific categories is also strongly emphasized by Matvejev, viz. hybrid populations between subspecies as well as good species in this biogeographically border area of several ecogeographical provinces of Europe.

Matvejev's field method of a faunistic survey seems to be a linear variant of the "minimiarea" concept of European vegetation surveyors. With the aid of a simple mathematical formula one can determine for each habitat type the minimal length of a transect that includes all very frequent, frequent, and common species, and this transect length can then be applied to the same habitat type in subsequent surveys. Besides abundance, sociability was also noted in the surveys. These two characteristics are given for each breeding species in each of their habitats—a remarkable novelty in a faunistic-taxonomic work!

Beyond the methodological introduction and the systematic descriptions, the book also gives a comprehensive summary of the biogeographical affinities of the Balkan avifauna. This truly transitional area has 228 breeding taxa (species and subspecies), of which 35 (19%) are endemic, 5 broadly distributed, and the remaining 184 forms evenly divided between the European and Mediterranean fauna types. Yet within each fauna type the affinities point toward the East rather than toward the West. This has some fauna-historical implications that Matvejev briefly introduces.

The English summary also contains full translation of the treatment of three sample species. It is beyond my competence to judge these taxonomically, but I congratulate colleague Dr. Matvejev for the

successful first part of a mammoth undertaking, and wish him good luck for continuing work on the nonpasserines.—MIKLOS D. F. UDVARDY.

A handbook of Audubon prints.—Taylor Clark and Lois Elmer Bannon. 1980. Gretna, Louisiana, Pelican Publishing Co. 122 pp. 18 figures. $9.95. **On the road with John James Audubon.**—Mary Durant and Michael Harwood. 1980. New York, Dodd, Mead. xiii + 638 pp., numerous black-and-white illustrations, end paper map. $19.95.—Of the making of Audubon books there appears to be no end. The first of these two very different volumes explores the painting, printmaking, and some of the publication work done by the naturalist, his sons, his colleague John Bachman, other family members, and various engravers. The various editions of the *Birds* and *Quadrupeds* are described, and differences between the several versions of the prints are discussed. Several tables list the individual plates in both works and the current prices for prints in excellent condition. The list of bird prints also indicates where each of the original paintings was completed, when each was done, and who painted the backgrounds.

While the price lists will be helpful to artists and collectors, most of the information in this very slim volume is available in much greater detail in the standard published sources. The text will probably be a useful digest of information about the man and his art to beginning collectors and those unfamiliar with the Audubon story. Ornithologists and those well versed in Auduboniana will find little to detain them here.

The Harwood and Durant book (Durant is Mrs. Harwood in private life) is the product of a 13-month, 56,000-km trip by auto over the American and eastern Canadian routes covered by Audubon between 1804 and 1843, with a concluding section about the artist's home on the upper West Side of Manhattan Island. Harwood and Durant are both professional authors and editors who served at one time on the staff of *American Heritage*. The method they have employed in this volume is to intersperse their comments about what Audubon did, saw, felt, and wrote with excerpts from Audubon's own writings, most of them previously published. Each entry is preceded by the initials of the author (Harwood, Durant, or Audubon).

Many of Harwood and Durant's observations are well written, and some of the comparisons that they draw between the land in Audubon's day and ours make interesting reading. Some of the information they present about Audubon's friends, relations, colleagues, and professional interests is not readily available elsewhere. They seem to have run into any number of people who had never heard of Audubon, a sobering thought.

There are, unfortunately, several problems with the book. The Harwoods sometimes appear confused as to whether they are to stick with their Audubon theme or write a chatty modern travelogue. Many of their digressions have no discernable connection with the Audubon story. Parts of the book are overwritten, and it is clear that a much more rigorous editorial hand should have been applied throughout. The three-way division of the text is not always followed, since Harwood and Durant often quote Audubon in their sections.

A relatively brief Author's Note at the back of the book lists the published and unpublished Audubon materials that Harwood and Durant consulted, but rarely are the Audubon entries keyed to particular sources, something that would have greatly enhanced the value of the book. In sum, the plan of this effort was an excellent one, but the execution of the project fell somewhat short of its objective.—KEIR B. STERLING.

ALSO RECEIVED

Verbreitungsatlas der Brutvögel der Schweiz (The atlas of breeding birds in Switzerland).—Swiss Ornithological Station. 1980. Schweizerische Vogelwarte. (CH-6204 Sempach, Switzerland.) 462 pp., illus., Sfr.58.00 + postage. **Bird atlas of Natal.**—Digby Cyrus and Nigel Robson. 1980. Pietermaritzburg, University of Natal Press. (North American distribution through Lawrence Verry, Inc., Box 98, Mystic, Connecticut 06355). xx + 320 pp., 544 black-and-white illustrations by Tony Clarkson. £15.00.—These two atlases both follow the approach to mapping distributions initially set forth by Sharrock (1976, The atlas of breeding birds in Britain and Ireland, Berkhamsted, T. & A. D. Poyser), but they differ in their scope and style. The atlas of Swiss breeding birds summarizes records gathered by observers in Switzerland during 1972–1976 and is confined to breeding birds. The distributions of 188 species are described. For each species, the breeding distribution, coded as possible, probable, or confirmed breeding, is mapped for 10 × 10-km squares covering Switzerland. The accompanying text (in both German and French) describes this distribution and comments upon habitat, density, recent changes, and migratory status. A stylized sketch accompanies each species account. Separate

chapters describe the altitudinal distribution of 12 selected species and compare the distributions of birds and plant associations.

The atlas of Natal birds is more comprehensive in its coverage of species, but provides somewhat less detail in the written species accounts. Observations made during 1970–1979 are used to map the distributions of 530 species in quarter-degree squares covering Natal. Coverage includes the entire year, and the monthly occurrence of species is coded for each square. Each species account includes a brief statement of the species' world distribution and its habitat affinities and breeding period in Natal, and is accompanied by a line drawing. Additional sections describe the mapping procedure, provide information on background environmental and habitat conditions, outline the distributions of 100 rarer species, and provide a checklist of birds recorded in Natal. Also included is a plastic sheet showing altitude and vegetation types that can be placed over the distribution maps; this is a nice idea, but the faint printing on the overlay makes it difficult really to see relationships.

Both books are well produced. Each will be of considerable value to individuals working in or visiting the areas covered. More important, perhaps, these distributional treatments provide definitive foundations to which future distributional changes may be related.—J. A. W.

How to write and publish a scientific paper.—Robert A. Day. 1979. ISI Press (3501 Market St., University City Science Center, Philadelphia, Pennsylvania 19104). xi + 160 pp. Paper. $8.95.—Many individuals involved in gathering scientific information devote considerable attention to the design of their research, the gathering of careful observations, and the most appropriate analysis and interpretation of the results they obtain, but their attentiveness seems to stop there, and when they prepare the material for publication, it comes out looking as if the entire project was done in a lost weekend. Part of the problem is that an unfortunately large number of such individuals seems not to have learned basic grammar or clear expression, but in many cases a manuscript is poorly organized and unconvincing simply because the author does not know how to go about preparing it properly. This book, written by an editor of a scientific journal, provides answers to many of the "how-to" questions that confront authors of scientific papers. The topics range from organization and writing style through design of tables and illustrations to dealing with editors (always an exhilarating experience!). The treatment is succinct and often humorous, and the lessons are quite worthwhile. If you're planning to prepare a manuscript for *The Auk* (or any other journal), you should get a copy and study it carefully; following its messages won't remedy bad science or equivocal results, but it will surely enhance the probability of getting good work accepted.—J.A.W.

The birds of The Gambia.—Jørn Vestergaard Jensen and Jens Kirkeby. 1980. Aros Nature Guides, Skolebakken 5-4.tv, DK-8000, Åarhus C, Denmark. 284 pp., illus. Paper. £11.80/D.kr148.00.—The Gambia is the smallest country in Africa. Located on the west coast of the continent, it extends inland some 300 km along the valley of the lower Gambia River. Despite its small size, however, The Gambia includes several major life zones, and these are reflected in the diversity of its avifauna. This book is a compilation of accounts of species recorded in The Gambia through 1979.

Introductory sections describe the geology, climate, and life zones of the country, followed by accounts of 37 birding localities, including local maps and species lists. The major part of the book is devoted to species accounts. For each of the 489 species recorded in The Gambia, status and distribution are described, accompanied by maps showing the dry season (November–April) distributions for all except the vagrant species. Additional brief information is provided on habitat affinities and breeding status (where known). The general West African distribution of the species is also described, using avifaunal zones (e.g. "rain forest") rather than listings of countries of occurrence. These species accounts are quite brief, but suffice to provide a general image of the distributional patterns of the species within The Gambia. Many of the species are illustrated with black-and-white photographs of varying resolution and quality. The book represents a good introduction to the distribution of Gambian birds and will be helpful for those anticipating travels to that country, but it will be of little help in field identification.—J.A.W.