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EDITED BY WILLIAM E. DAVIS, JR.

EXXON VALDEZ OIL SPILL: FATE AND EFFECTS IN ALASKAN WATERS. Edited by Peter G. Wells, James N. Butler, and Jane Staveley Hughes. American Society for Testing and Materials, Philadelphia, Pennsylvania. 1995: ASTM Special Technical Publication 1219, 955 pp., introduction, 25 technical papers, author and subject indices. $55.00 (cloth).—This volume contains some of the papers presented at the Third Symposium on Environmental Toxicology and Risk Assessment held in Atlanta, Georgia on 26–28 April 1993. Most of the research reported in this volume was supported by the Exxon Company, USA, during the period 1989–1991, following the grounding of the T/V *Exxon Valdez* in Prince William Sound, Alaska, on 24 March 1989. The accident resulted in the release of an estimated 11 million gallons of crude oil from Alaska's North Slope into the sound; some of this oil was—and still is—deposited on beaches as far as 900 km to the southwest.

One of the truths of the *Exxon Valdez* oil spill (hereafter, EVOS) is that there is no single version of what happened, particularly in regard to long-term effects. My own perspective is that of someone who works for the state-federal EVOS Trustee Council, which was appointed to administer the damage assessment and restoration programs following the spill. The following comments reflect my own views and not those of the Trustee Council.

The Atlanta symposium was the first forum for public discussion of results from Exxon's post-spill science program. Exxon's contractors were invited to participate in a Trustee Council-sponsored symposium in Anchorage, Alaska on March 1993, but none presented papers. Several Trustee Council-sponsored investigators participated in the Atlanta symposium, but only one of their papers appears in this volume (McDonald et al., pp. 296–311). Proceedings from the Trustee Council symposium are now available as well (Rice et al. 1996). Together, these books frame the issues regarding initial and short-term (i.e., 1–3 y) injuries. As the results of the Exxon- and Trustee Council-sponsored studies find their way into the open literature and follow-up studies are conducted, there will be ample opportunity for contrast and synthesis.

Following an introductory chapter, "Introduction, Overview, Issues" (Wells et al., pp. 3–38), this volume is organized into sections on the "Chemistry and Fate of the Spill" (6 papers), "Shoreline Impacts of the Spill" (6), "Impact Assessment for Fish and Fisheries" (4), "Impact Assessment for Wildlife" (8), and "Impacts on Archaeological Sites" (1). The introduction is largely a summary of the contents, but the editors comment more broadly on lessons learned from EVOS. Their bottom line is that chronic effects of the spill on wildlife and fisheries are limited and, at the population level, blend with natural factors, resulting in variability in the abundance and distribution of species. An assertion that Exxon's studies, but not those sponsored by the Trustee Council, are "synoptic, covering unimpacted as well as impacted areas" (p. 6) is wrong. From the outset, studies sponsored by the governments made ample, contemporaneous use of unimpacted areas as reference sites (e.g., EVOS Trustee Council 1989).

The balance of this review addresses the bird studies in the "Impact Assessment for Wildlife" section. My intent is to highlight some of the issues for readers who plan to wade into the now burgeoning EVOS literature.

The paper by Day et al. (pp. 726–761), "Use of Oil-Affected Habitats by Birds After the *Exxon Valdez* Oil Spill," is innovative because it assesses impacts on and recovery of the birds' use of oiled habitats rather than the birds themselves. This approach complements that of the Trustee Council-sponsored boat surveys, which drew on limited historical data to look at impacts to and recovery of bird populations (e.g., Agler et al. 1994, Klosiewski and Laing 1994). Looking at the Exxon and Trustee Council surveys together, there is agreement that there were impacts with respect to several bird species, including, for example, Black Oystercatchers (*Haematopus bachmani*) and Harlequin Ducks (*Histrionicus histrionicus*) in Prince William Sound.

Day et al. defined impact as "a statistical difference in the abundance of a species among bays exposed to various levels of oiling, after habitat differences . . . have been taken into account" (p. 728). Recovery is achieved when a significant difference is no longer evident. Of course, not finding a difference can reflect either a real lack of difference or a lack of sufficient statistical power to detect a difference if one should exist. Day et al. present no analysis to quantify the latter, but their use of alpha levels of up to 0.20 increased the chance of detecting oiling effects.

The approach taken by Day et al. enabled the conclusion, for example, that in 1989 there was an initial, early-summer impact on Marbled Murrelets (*Brachyramphus marmoratus*), but that recovery in the use of oiled habitats was achieved by mid-summer of the same year. This tells us something about how quickly birds can reoccupy oiled marine habitat, but it reveals little or nothing about impact on or recovery of the murrelets themselves. I am not persuaded that reoccupancy of an oiled habitat necessarily is a "clear sign that . . . other types of recovery [demographic and reproductive . . .] can proceed" (p. 728). It is entirely possible that birds continue to use polluted habitats that are detrimental to short- and long-term survival and productivity. Day et al. twice cite Morrison (1986) in support of the idea that reoccupation means that recovery can proceed, but Morrison did not address this issue. In fact, the thrust of Morrison (1986) is that birds are not effective environmental indicators.

Two papers address the status of murres (Common, *Uria aalge,* and Thick-billed, *U. lomvia*), which accounted for 74% of the oiled bird carcasses recovered following the spill (Piatt et al. 1990). Exxon's approach to the issue of impacts on murres was (1) to compare their post-spill surveys of murres at colonies in the oil-spill area with historical data and the Trustee Council's post-spill surveys, and (2) to conduct more intensive studies on numbers of murres and productivity at one of two colonies in the Barren Islands, lower Cook Inlet.

Erikson (p. 780–819), "Surveys of Murre Colony Attendance in the Northern Gulf of Alaska Following the *Exxon Valdez* Oil Spill," takes the first approach and concludes that impacts on murre colony attendance were relatively short-term. Post-spill colony attendance estimates in 1991 by both Erickson and U.S. Fish and Wildlife Service staff (Nysewander et al. 1993a) were similar, giving some confidence that both sets of biologists can count murres! The real issue is how one interprets historical data in relation to these post-spill colony estimates.

Erikson points out the difficulties in estimating colony attendance and the errors in the historical data in the "Catalog of Alaskan Seabird Colonies" (USFWS 1991), but he was uncritical in selecting only the most recent counts for comparison in his test for a change in numbers caused by oiling. For example, without an accompanying note (Table 3, p. 800), Erikson used counts within the Chiswell Island group by Nishimoto and Rice (1987) that were obtained under marginal sea conditions. These counts were low relative to earlier counts by Bailey (1976), thus giving the impression of little change in pre- and post-spill numbers in the Chiswell I. The best approach might have been to conclude that neither set of counts provided a good basis for comparison.

Beyond the issue of which historical estimates are most appropriate, Erickson assigned murre colonies to risk categories (high, moderate, and low) depending on colony location relative to the oil. Since "the pelagic distributions of murres for the individual colonies is not known" (p. 786), however, these categorizations and the related ANOVA analysis are problematic. For example, colonies separated by only a few km are assigned different risks (e.g., Chiswell I.-high; Rugged I.-moderate), even though such distances are well within likely foraging ranges of birds attending colonies (Ainley and Boekelheide 1990), and murres on the water in the pathway of the spill may not yet have moved into colonies nearer shore (e.g., Rugged Island).

The most important group of murre colonies in the spill area is in the Barren I. The paper by Boersma et al. (p. 820–853), "Common Murre Abundance, Phenology, and Productivity on the Barren Islands, Alaska: The *Exxon Valdez* Oil Spill and Long-Term Environmental Change," again underscores the importance of the interpretation of historical data.

In Table 2 (p. 833), Boersma et al. present a confusing array of historical murre estimates—including preliminary and final estimates and even numbers acknowledged to be in error—from East Amatuli I.-Light Rock in the Barren I. group. For example, two estimates for Light Rock in 1978 consist of a preliminary estimate of 10,000–30,000 murres (Simons and Pierce 1978) and a final estimate of 20,000 that Manuwal (1978) derived from his field assistants' (Simons and Pierce 1978) preliminary figures. Why are these numbers presented? On the one hand, Boersma et al. use them to illustrate the poor quality and unreliability of the historical data. On the other hand, they use these data to show that their estimates of about 35,000 murres at East Amatuli I.-Light Rock during 1990–1992 fall within the range of pre-spill, historical estimates for the Barren I. Unfortunately, the historical range they proposed, 19,000–61,000, mixes an apple and an orange. The "apple" is the figure of 19,000, which was not reported by Manuwal (1978) and which Boersma et al. referred to as a "presumed" count (p. 843). This number must have been calculated from Simons and Pierce's (1978) preliminary estimate for birds on the cliffs at Light Rock and a number that was apparently doubled by Simons and Pierce to account for murres on the cliffs and at sea at East Amatuli I. The "orange" is Bailey's (1976) figure of 61,000, which was an attempt to estimate the total population of the East Amatuli I.-Light Rock colony by counting birds on the cliffs and water and in the air. The bottom line is confusion, and my conclusion is that the historical data do not provide a reliable basis for interpretation of post-spill estimates.

Boerma et al. question some of the early conclusions that the oil spill disrupted reproductive timing and synchrony in murres in the Barren I. (e.g., Nysewander et al. 1993b). I agree with them and so do Piatt and Anderson (1996). The case presented by Boersma et al., however, is less than compelling, since it is based on work at only one of the two Barren I. colonies (East Amatuli I.-Light Rock); and, within that one colony, their study focused on a single 5 × 5 study plot on a flat area of optimum habitat on top of Light Rock that is strikingly different from the narrower rock ledges on vertical cliffs on which most murres nest in the Barren I (D. Roseneau, pers. com). Finally, Boersma et al. do not address evidence of disruption of breeding murres at other colonies, such as at Puale Bay on the Alaska Peninsula (e.g., McCarthy and Dewhurst 1993).

Stubblefield et al. (pp. 665–692) present an "Evaluation of the Toxic Properties of Naturally Weathered *Exxon Valdez* Crude Oil to Surrogate Wildlife Species" and conclude that the exposure concentrations observed in Prince William Sound after 1989 would present negligible toxic risk to wildlife. The Mallard (*Anas platyrhyncos*) was their avian surrogate for a battery of tests for acute and subacute toxicity, but there was no discussion of whether Mallards in laboratories are good surrogates for Harlequin Ducks and other birds in harsh marine habitats. Holmes et al. (1979) established that under sheltered laboratory conditions Mallards can ingest large amounts of petroleum without showing signs of distress, but that mortality frequently occurred among Mallards subjected to even small environmental stresses (e.g., sustained mild cold). Stubblefield et al. do not cite this highly relevant paper (Holmes et al. 1979), nor did their tests include environmental stresses on the surrogates.

White et al. describe the "Density and Productivity of Bald Eagles [*Haliaeetus leucocephalus*] in Prince William Sound, Alaska, After the *Exxon Valdez* Oil Spill" (pp. 762–779). Their message is that there were no demonstrable effects of the oil spill on eagle density or reproduction one and two years after the spill. This agrees well with Bowman et al. (1995): Their surveys documented an initial impact on Bald Eagles, but they predicted recovery by 1992. Indeed, the Trustee Council listed the Bald Eagle as a "recovering" species in its restoration plan (EVOS Trustee Council 1994).

Wiens (pp. 854–893) concludes this volume's bird papers with a chapter on the "Recovery of Seabirds Following the *Exxon Valdez* Oil Spill: An Overview." He starts by noting that early concerns expressed by Trustee Council-sponsored investigators and others about the initial impacts of the spill on birds and predictions of extended recovery times "were not based on careful, scientific studies . . ." (p. 857). This is puzzling given that one of the most prominent, early predictions—that Common Murres might require recovery times of 20 to 70 years, or sooner (Piatt et al. 1990)—was in large measure based on modeling work by Wiens and his students (Ford et al. 1982).

Wiens proposes that if injury cannot be detected statistically, then no injury has occurred; recovery is the disappearance through time of statistically significant differences between oil-exposed and reference samples. The no-significant-effect-means-no-effect-existed contention is logical when data are ample, but the absence of a significant effect may reflect nothing more than the lack of sufficient power to detect an effect, which is often the case in view of limited historical data, natural variations in populations and productivity, and complicated interpretation of results at off-site controls. Peterson (1993:36) refers to the contention that no significant effect means no effect existed as a "recurring fallacy" and suggests that, in the absence of convincing analysis of power, definitive conclusions about no effects are unjustified.

Wiens begins a discussion of equilibrium and natural variation by noting that "recovery is often thought to have occurred when the system returns to its state before a disruption, such as an oil spill" (p. 862). This is something of a red herring in regard to EVOS. The federal Natural Resources Damage Assessment regulations (43 CFR, Subtitle A, Part 11) that served as an initial framework for the *Exxon Valdez* damage assessment make clear that recovery means a return to baseline conditions that would have existed had the spill not occurred--quite a different concept from the one Wiens describes. In the subsequent restoration program, pre-spill conditions are used as proxies because of the difficulty in predicting the conditions that would have existed had the spill not occurred. Further, there is explicit recognition that, in the case of species that had declined before the oil spill (e.g., Marbled Murrelet and harbor seal [*Phoca vitulina*]), this objective is not realistic (EVOS Trustee Council 1994).

Wiens criticizes Laing and Klosiewski's (1993) boat surveys in Prince William Sound for basing their comparisons of pre- and post-spill data "on the premise that differences in the state of the system before and after the spill are due to spill effects alone" (p. 867). This is incorrect: Laing and Klosiewski (1993; also Klosiewski and Laing 1994) also test whether marine bird populations in the oiled zone of Prince William Sound were less than expected given the pre-to-post-spill changes that had occurred in the unoiled zone, thus employing both geographic and temporal controls.

Wiens wonders why there are disagreements about the effects of EVOS and suggests that some studies, presumably those sponsored by the Trustee Council, focused on damages, while others, presumably Exxon's, also included recovery. To be sure, the studies sponsored by the Trustee Council during 1989–1991 were part of an injury assessment for the purpose of presenting a damage claim to the spillers of the oil, but determining the extent and rate of natural recovery of injured resources was integral to the process from the outset (e.g., EVOS Trustee Council 1989). Is it unfair to assume that the only reason that Exxon's studies emphasized recovery was to limit the legal and economic liability of the corporation? Perhaps, but it is even more unfair to imply that the governments' only interest was in documenting injury when the determination of recovery is essential to ecologically and commercially important management decisions and to organizing a restoration program (e.g., Strand et al. 1993).

Determining the biological significance of impacts and distinguishing between natural variation and spill effects are key themes in Wiens' paper. These themes are relevant, but I caution readers to be aware of what Peterson (1993:33) called the "fallacy of natural variation," in which it is inappropriately argued that because an impact is small in magnitude relative to the natural range of variation the impact is of no ecological consequence. Wiens does not discuss whether mortalities to birds from oil spills or other anthropogenic perturbations are additive or compensatory (e.g., Piatt et al. 1991) or the possibility of interactive, cumulative effects on populations (e.g., Ainley and Lewis 1974, Ainley and Boekelheide 1990). A series or combination of natural and anthropogenic events can reduce a population's natural resiliency, and, whatever the effects of EVOS were, they were superimposed on a decadal-scale period of change and decline for an entire suite of fish-eating marine birds and marine mammals in the north Pacific (e.g., Duffy 1991, Piatt and Anderson 1996). The interaction of spill effects and natural environmental change may be the long-term legacy of this oil spill.

In sum, this volume of mostly Exxon-sponsored reports is an early contribution to the scientific literature on EVOS effects. Readers should approach this volume with caution and an open mind, just as they should approach reports on research sponsored by the Trustee Council. The comments offered above are intended to flag some of the issues to consider as this story unfolds over the next decade or more. Thanks are due to the following individuals who either read a draft of this review or otherwise provided assistance: V. Byrd, C. Haney, D. Irons, M. Morrison, C. Peterson, J. Piatt, B. Rice, S. Rice, D. Roby, D. Roseneau, J. Senner, R. Spies, B. Wright, and an anonymous reviewer.—STANLEY E. SENNER

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NEARCTIC PASSERINE MIGRANTS IN SOUTH AMERICA. By Raymond A. Paynter, Jr. Publication of the Nuttall Ornithological Club, No. 25, Cambridge, Massachusetts. 1995: 126 pp., X + 72 range maps, 3 tables, $13.50 (cloth).—The tremendous recent surge of interest in the conservation of nearctic passerine migrants (or neotropical migrants, as they are more often called) has focused overwhelmingly on north temperate breeding grounds and wintering areas in Central America and the West Indies. This focus reflects the geographical distribution of scientists, the ease of reaching study areas, and greater abundance of migrants in Central America and the West Indies. As a result, we know very little about the distribution of migrants in South America, even though 68 passerine species winter on the continent. The species that winter mainly in South America include some species identified as being of high priority for conservation because of population declines and habitat loss, including the Olive-sided Flycatcher (*Contopus borealis*), Cerulean Warbler (*Dendroica cerulea*), Canada Warbler (*Wilsonia canadensis*), and Bobolink (*Dolichonyx oryzivorus*). Migrants such as swallows, Swainson's Thrushes (*Catharus ustulatus*) and Eastern Kingbirds (*Tyrannus tyrannus*) are abundant and conspicuous members of the tropical communities in which they winter. Before we can evaluate the extent to which these species are threatened by wintering ground habitat loss, we need better information on their winter ranges and habitat requirements.

The goal of this book is to provide range maps showing where each species occurs in South America. The range maps are based on a review of more than 500 publications (on file at the Museum of Comparative Zoology, Harvard University) on bird distribution at 1,260 different sites. The maps show the exact locations (points) at which a bird is known to have been collected and/or observed. No data are provided on abundance, habitat, or date of observation for each point. The text accompanying each species, however, usually attempts to summarize the seasonal pattern of occurrence, the general habitats occupied, and the abundance in different regions. The introductory section is brief (5 pages) and there is a concluding section on general patterns of distribution, relative abundance, and summer residents.

The book succeeds in providing useful data for conservation planners. The greatest concentration of migrants is in the Northern Andes region where human population pressure is great and habitat loss is proceeding rapidly. Many of the migrants in this region include such declining species as the Olive-sided Flycatcher and the Cerulean and Canada Warblers. The data in this volume lend support to the hypothesis that winter habitat loss is a particularly likely cause for their population declines.

One of the major limitations of this volume is the lack of data on the dates of occurrence for each point. As a result, it is difficult to determine which records likely pertain to passage migrants and which refer to locations where the species truly "winters." This is an especially severe problem with relatively late migrants such as the Veery (*Catharus fuscescens*), which is usually depicted as having a large winter range, but may in fact winter in only a small part of Southwestern Amazonia (J. V. Remsen, pers. comm.). The species accounts usually mention locations where the species may be just a passage migrant, but it would have been useful to have dates, or at least months, of collection or observation on the maps. Similarly, a more detailed map of South America showing the Andes would have helped readers interpret the winter ranges.

Based on my own experience, which is mostly in southeastern Peru, I found the maps to be generally accurate. There is a point missing for the Connecticut Warbler (*Oporornis agilis*) from the Manu National Park, but all other species that occur there are documented by point on the range map. I can envision many tropical ornithologists looking at these range maps and noticing other gaps; this should spur many of us to publish our distributional records in a more timely manner. Paynter argues that these maps provide the broad outline and that only the details will change as more data arrive. Perhaps he is correct, other than the need to separate migratory and wintering distributions for some species.

The book is restricted to passerine migrants, which includes most, but not all terrestrial migrants. The two *Coccyzus* cuckoos, Yellow-billed (*C. americanus*) and Black-billed (*C. erythropthalmus*), also winter mainly in South America, as do several raptors. The volume therefore is not a comprehensive coverage of terrestrial nearctic migrants, nor is it intended to be.

The concluding sections of the book provide syntheses of the data, including species diversity from each country, an exploration of vagrancy in nearctic migrants, a brief mention of habitat requirements, and extensive discussion of occasional breeding by nearctic migrants in South America. Paynter argues that distributions are well defined in spite of vagrant records. Many of the species listed winter primarily in Central America and the West Indies and occur only as stragglers in northwestern Columbia or along the Caribbean Coast.

This book is a bargain at only $13.50, as are most of the volumes in the Nuttall Ornithological Club Series. It will serve as a valuable reference for the growing community of biologists who are using nearctic passerine migrants to help direct conservation strategies in South America.—SCOTT K. ROBINSON

THE ECOLOGY OF MIGRANT BIRDS: A NEOTROPICAL PERSPECTIVE. By John H. Rappole. Smithsonian Institution Press, Washington, D.C. 1995: xvii+ 269 pp., 18 figures, 27 tables, 5 appendices. $35.00 (cloth).—The main purpose of this timely book is to argue that nearctic-neotropical migrants (hereafter "neotropical migrants") are essentially tropical birds that leave the neotropics only to breed. Rappole's book is intended to counter the belief, which he feels is general, that neotropical migrants are different from neotropical residents and, therefore, have different conservation needs, at least within the Neotropics. In a parallel vein, Rappole argues that migrants also form an integral part of bird communities during migration and have special conservation needs during these periods as well. Rather than viewing neotropical migrants as supreme generalists capable of exploiting a wide range of habitats and foods, Rappole believes that many species are nearly as specialized as tropical residents and fit well into tropical bid communities. This view again runs contrary to the view that migrants are opportunists that skim the surplus resources from the margins of habitats.

Rappole offers the following general lines of reasoning to support his main thesis. (1) Many studies have shown neotropical migrants defending territories in the interior of forest habitats and may even have displays adapted specifically for winter territorial defense. (2) Some species decline after tropical forest clearing. (3) Many individuals are forced to use marginal habitats such as early second-growth because they are competitively excluded from superior forest habitats. (4) Some species are less abundant in small tropical forest fragments. (5) Wandering, nonterritorial birds may experience higher mortality rates. (6) The diets of temperate and tropical birds are generally similar. (7) There are many "floaters" in winter. (8) Migrants show comparable or even greater site fidelity to winter quarters than to breeding territories. (9) Migrants are not necessarily subordinate to residents and those that are subordinate have not been proven to suffer reduced fitness consequences. (10) Many tropical fruits are exploited by and may even be coevolved with migrants. (11) Available evidence suggests a likely tropical origin of most neotropical migrants.

Rappole also identifies and tries to refute several lines of evidence that might support different views of the ecology and evolution of neotropical migrants. (1) He argues that studies showing migrants concentrating in second/growth edge habitats are methodologically flawed (e.g., overreliance on mist nets) and ignore the birds that do defend territories in forest. He mentions only briefly that most of these studies are from Panama and South America where neotropical migrants may really be a marginal part of forest communities. I doubt if anyone would argue that neotropical migrants play a major role in forest bird communities in the northern Neotropics where Rappole has done most of his work. (2) Studies showing migrant tolerance of very small fragments are again methodologically suspect and ignore the fact that some species are less abundant in small fragments. Rappole's best example is for flocking species such as the Black-and-white Warbler (*Mniotilta varia*), which avoids fragments that are too small to sustain stable flocks. (3) Arguments that the plumage dimorphism of most neotropical migrants is an adaptation for breeding grounds ignore the possibility that winter territoriality also plays a role, as Rappole advocates. (4) If neotropical residents and migrants differ in diet, then Rappole's central thesis may be suspect. Rappole, however, argues that there is tremendous dietary overlap, even though his data here (pp. 29–30) are anecdotal. It is too bad that Poulin and Lefebvre's (1995, Auk 113:277–287) data showing huge differences between the diets of migrants and residents weren't available. These data suggests that migrants consume prey items ignored by most residents (e.g., ants); therefore, even territorial species may be exploiting "marginal" foods. Similarly, the remarkably specialized diets of many tropical birds are not mentioned. (5) Rappole emphasizes the huge geographical differences between New World and Old World migration systems, which may explain why palearctic migrants, at least in Africa, occupy marginal habitats rather than the forest interior. He further argues that the studies showing palearctic migrants as peripheral members of communities are too localized in equatorial West Africa to be generalized over a large geographical area. (6) If migrants are different morphologically from tropical residents, then they may be better adapted to exploit temperate resources. Rappole argues that support for this thesis is flawed because it fails to control for phylogeny and the possibility that tropical birds adapted to exploit the dominant temperate insect resources (especially caterpillars) are the ones that would be most likely to evolve migration.

Central to Rappole's book is the hypothesis that neotropical-nearctic migrants have a tropical origin. He argues that forced dispersal from saturated tropical habitats would inevitably lead to the evolution of migration. This argument occupies most of Chapter 6. Personally, I found his argument to be reasonable, but then again, I do not view the evolution of migration as particularly surprising in a strongly seasonal world and the high mobility of birds. Being able to fly makes birds ideally suited to exploit temporally and spatially variable resources, regardless of their place of origin.

Rappole's central arguments have profound implications for conservation strategies, which are the focus of Chapters 8 and 9. If, as earlier argued, migrants are specialists on breeding, migratory, and wintering grounds, then they should be even more vulnerable to catastrophic habitat loss than residents. Rappole used a probabilistic model (page 137) to argue that they are roughly three times more likely to become extinct because catastrophic events can hit them at three different times and places (breeding, wintering, and stopover habitats). If, on the other hand, migrants are adaptable generalists with enormous global populations, then they are less likely to be affected by any one catastrophic habitat alteration. Similarly, if neotropical migrants are an integral part of tropical bird communities, then they may be good indicators for tropical conservation in general. If, on the other hand, they are mostly marginal members of bird communities, then they may be poor indicators for designing strategies for tropical bird community conservation. This latter view would lead to an emphasis on improving habitat for migrants in heavily agricultural landscapes, rather than saving tropical forests.

Rappole clearly recognized the importance of his central thesis, which may explain the overall tone of urgent advocacy. Rappole and his "group" (he often uses "we" in the text rather than the "I" pronoun more traditional in a single-authored book) have long advocated that conservation of neotropical migrants must first deal with tropical habitat alteration. For readers who share this view, this book will serve as a good, well-written review of the arguments in favor of this thesis. This book is also an excellent summary of the work of Rappole and his group.

For readers with different views, however, I doubt that this book will change their minds. There are too many gaps in the literature covered and oversimplifications of opposing views. The many papers showing relative insensitivity to habitat alteration in the tropics are ignored or selectively reviewed to emphasize the few species that were negatively affected. Methods of studies with opposing views are routinely challenged, but when the same methods are used to support Rappole's thesis (e.g., BBS, mist nets), they are accepted. The theory of the tropical evolutionary origin of migration ignores the likelihood that once migration has evolved, selection will continue to favor life-history and morphological traits that might lead to fundamental differences between migrants and residents.

In summary, Rappole's book will probably be cited as advocacy for the "tropics-first" view of neotropical migrant conservation rather than as a balanced review. This is partially appropriate, but it may lead readers to overlook the stimulating, but somewhat tangential sections on plumage evolution (pp. 19–21), GIS applications (p. 26), postbreeding ecology (pp. 77–78), habitat selection during migration (p. 97), austral migration (pp. 99–100), and pollution (p. 158). Perhaps time will vindicate Rappole's view; Table 9.3 provides hypotheses about which species will decline based on habitat alteration in the Neotropics.—SCOTT K. ROBINSON

THE BREEDING BIRDS OF QUÉBEC: ATLAS OF THE BREEDING BIRDS OF SOUTHERN QUÉBEC. By J. Gauthier and Y. Aubry (eds). Province of Québec Society for the Protection of Birds, Canadian Wildlife Service, Environment Canada, Québec Region, Montreal. 1996: 1302 pp., many color and black-and-white photographs $149.95.—This book is incredible. Not only is it the largest book in my library (it is 28 × 32 cm and must weigh 4 kg), but it has amazing content. Every page is packed with details, many of which are useful to readers not associated with Québec. Pages on breeding distribution are models of efficiency in presentation of a vast amount of data. The pictures are many and generally attractive. The range maps, descriptions of species, and biological information are all detailed and well done. The book contains great maps, beautiful satellite photos of the entire province, and a truly remarkable literature section. That the editors were able to bring all of this material together is a tribute to their skills in coordinating the efforts of an extremely large group of skilled ornithologists. Be sure to see a copy of this book because it must be seen to be believed.—C. R. BLEM

HANDBOOK OF THE BIRDS OF THE WORLD. VOLUME 3: HOATZIN TO AUKS. BY J. DEL HOYO, A. ALLIOTT AND JORDI SARGATAL (EDS.). Lynx Edicions, Barcelona, Spain. 1996:821 pp., 60 color plates, many color photographs $175.—There are so many things to like in this series that it is difficult to single out one feature. The illustrations are wonderful and the photographs generally sharp and informative. The text is a concise, rich lode of information. The bibliography is well researched and detailed. I had to search closely for something to criticize. For example, occasional pages are printed less boldly than the others and I think some of the wording in picture legends is not as accurate and succinct as it could be. Both of these observations are "picky"—they take little from the overall quality of the presentation. Skip a few lunches or movies and invest in this series if you are seriously interested in birds.—C. R. BLEM.

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