COMMENTARY

The Museum Tradition in Ornithology—A Response to Ricklefs

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As a museum ornithologist, I found Ricklefs' (1980) commentary on the museum tradition in ornithology to be decidedly patronizing, and I do not consider the perspective it offers to be an accurate reflection of the needs or future of museum-based studies of birds. First of all, the museum tradition in ornithology is not dying—it is being killed; strangled by the pervasive attitude that only theoretical and experimental approaches to biology are valid, by university biology departments that have eliminated evolutionary and morphological biology from their curricula, and by museum administrators who fill curatorships with ecologists, ethologists, and chemists, while their collections languish. As the museum tradition is written off as "anachronistic and obsolete," the myths that the systematics of birds is better understood than that of other vertebrates and that fossil birds are virtually nonexistent live on and thrive, along with the misconceptions that museum specimens have discharged their function in "the pursuit of taxonomic and biogeographical investigations" (Ricklefs 1980: 206) and that collections no longer need be expanded.

Many ornithologists appear to believe that the higher-level systematics of birds is a closed book, the sequence of orders and families in their field guides being an immutable constant that was determined long ago according to some infallible principle. In reality, the present classification of birds amounts to little more than superstition and bears about as much relationship to a true phylogeny of the Class Aves as Greek mythology does to the theory of relativity. A glance at the Gadow-Wetmore classification now in use shows that there is still no concept in ornithology of what constitutes a primitive bird. Such a classification would be unthinkable in mammalogy or entomology, for example. Much of our "current" classification of birds is derived from often arbitrary decisions made in the last century by Hans Gadow (1893) that have not been seriously examined since. As a result, by my reckoning there are no less than five polyphyletic orders of birds, for instance Ciconiiformes (Olson 1979, Olson and Feduccia 1980a) and Gruiformes (Olson and Steadman in press). Furthermore, the relationships between genera within families, and between species within genera, have been well documented in only a very few instances.

Because the higher-level systematics of birds actually has a poorer foundation than in any other division of vertebrate zoology, research based on museum specimens will be absolutely essential to lifting this pallium of ignorance. In the past few years, the myological studies of Raikow and his students (e.g., Raikow 1978, Bentz 1979, Raikow et al. 1980) have provided valuable new information on avian relationships and have begun to put the systematics of certain passerine groups on a much sounder footing than ever before. There are many opportunities for students to contribute materially to our understanding of avian systematics through similar anatomical studies of other taxa. It is hardly possible that the "anatomical specimens in collections in the United States have largely served their intended purpose" (Ricklefs 1980: 206), when the vast majority of these specimens have never once been removed from their jars for examination.

Some of the most important new developments in systematic ornithology have come through fossil discoveries, yet, despite the frequency and significance of such discoveries and despite my own efforts (Olson 1976: xi) to chide authors not to repeat old nonsense, there seems to be no way to put an end to the idea that birds are poorly represented in the fossil record. It is still a favorite theme, as the following very recent example demonstrates: "so little fossil material exists. . . . that, for evolutionary problems, studies of birds are not attractive" (Ricklefs and Gill 1980: 121). The fact of the matter is that the quantities of undescribed fossil birds already on hand greatly exceed the lifetime working capacities of the few scientists who are competent to study them, and thousands more fossil birds are being recovered annually.

Fossils have recently not only revealed entirely new groups, such as the giant penguin-like Pelecaniformes of the North Pacific (Olson and Hasegawa 1979), or mosaics, like *Presbyornis,* that link two orders never before thought to be related (Olson and Feduccia 1980b), but in many cases also have a direct bearing on modern ecological and biogeographical theory. Fossil birds and other vertebrates from the West Indies (Olson and Hilgartner in press, Pregill in press) show clearly that many extinctions and relict distributions in the Antilles are the result of habitat changes wrought by alternating wet and dry

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periods in the Pleistocene. This explains the "stages" attributed by Ricklefs and Cox (1972, 1978) to the so-called "taxon cycle" and suggests that it is environments that are cycling in the West Indies, not taxa.

Since 1971, fossils obtained from Pleistocene dunes and more recent sinkhole deposits in the Hawaiian Islands have nearly *doubled* the number of species of endemic land birds known in the archipelago (Olson and James MS). Massive extinctions, even of small passerines, appear to be correlated with destruction of lowland habitats in Hawaii by Polynesians. The same story is likely true for the avifaunas of all the Pacific islands, where the need for paleontological studies is now strongly indicated, because the extant birds on these islands represent but a portion of those that were present only a few hundred years ago. With this perspective in mind, I am not much impressed with the data being used in studies of avifaunal turnover on islands nor with the value, predictive or otherwise, of MacArthur and Wilson's (1967) much discussed theory of island biogeography. The fossil record on islands contains concrete, factual information on the historical biogeography of birds. More people should be obtaining and using data from this source.

But it is not just anatomical and paleontological studies that need attention. There is valuable information yet to be had from studies of geographic and other variation using study skins. In contrast to the taxonomists of the past, we now have much more extensive information from geology and climatology that allows us to interpret geographic variation in addition to describing it. The approach of Haffer (1979) has shown the importance of viewing the process of speciation in a historical framework. Studies of patterns of intraspecific variation could lead to increasingly refined interpretations of the effects of past climatological and geological events on avian populations. Here, also, there is much need for new, museum-based research.

Systematic considerations have played a significant role in some of Ricklefs' own work; for example, in the comparisons between the competitive abilities of endemic versus nonendemic genera in the West Indies (Ricklefs and Cox 1978). Here, the authors are totally at the mercy of the systematists who made the decisions about generic limits. Only because of inadequacies in current systematics could Lack (1976: 193) make the statement that in land birds "there has been virtually no adaptive radiation within the West Indies." Actually, there is an entirely overlooked radiation of finches in the West Indies (D. W. Steadman and Olson in prep.) that provides many parallels with the renowned radiation of Darwin's finches in the Galapagos and that would make a fascinating subject for ecological research. In the absence of a systematic revision of this group, ecologists would be forever unaware that such an opportunity existed.

Examples of the need to expand collections are endless. In my own studies of geographic variation in Neotropical birds, I have repeatedly been thwarted and annoyed by gaps in collections, by the frequent paucity of specimens even of some common species, and by the predominance of old specimens with minimal, erroneous, or nonexistent data. Johnson (1980: 3) found that for widespread North American forms of *Empidonax,* the pre-existing series of specimens available in museums were quite inadequate for detailed systematic studies and had to be augmented by "considerable new material."

For anatomical specimens the deficiencies are even greater. Although virtually all historically known species of birds are represented in collections by study skins, there are a great many species for which not a single pickled specimen or skeleton exists. When I first attempted to identify the thousands of fossil birds I collected in Puerto Rico, I found that, despite the National Museum's extensive holdings of recent Puerto Rican birds, only four specimens of three species had been prepared as skeletons.

While Ricklefs may be correct in his statement that new museum material is being acquired "at the lowest rate this century," this does not mean that such inactivity is justifiable. In view of the rapid and inexorable destruction of habitat on a worldwide basis, it is more imperative than ever to have ornithologists in the field with the desire and ability to prepare specimens. We also need more ornithologists in sinkholes and sand dunes with shovels and screens and more ornithologists in fossil beds in lonely western mountainsides with pickaxes, plaster bandages, and gas-powered jackhammers. These tools can advance our knowledge of birds at least as much as sonograms and calorimeters.

The most serious threat to the museum tradition will come from the lack of qualified curators. Students with an interest in systematics and evolution of birds are hard put to find any encouragement to enter these fields, not to mention their being unable to obtain adequate training. How many so-called "biology" departments are there in this country that do not offer a course in comparative anatomy? Students who are not totally disillusioned and who continue to maintain an interest in biology other than in the currently fashionable trends later receive little incentive from their advisors. I well remember one professor telling me when I was a graduate student that there was no future at all in paleontology or systematics and that the only road to success was through the study of fat metabolism! Given the current imbalance, which seems worse in ornithology than elsewhere in vertebrate zoology, most prospective students of morphology and systematics either enter other fields or are forced to get their training more in spite of their education than because of it.

I am at a loss to understand what amounts almost to active antipathy towards systematics on the part of many biologists—as though biology could not encompass more than one line of endeavor and as though nonsystematists would not benefit from having the organisms they study properly classified. As imperfect as our knowledge of avian systematics and evolution may be, this knowledge has not been superceded by the other kinds of science now in vogue. The evolutionary approach certainly deserves a fairer representation than it has at present, as there are abundant opportunities for highly original and widely interesting research in morphology, systematics, and paleontology of birds. There is far more left to be done in these fields than has been done in all of the past. Some means should be devised to assure that students are at least made aware of this fact and that they are provided with a better chance to obtain appropriate training should they desire.

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