

Yet the migrants are a dominant portion of the community for variable lengths of time. Is it justified to ignore them? It is interesting to note that the habitats occupied by the nonbreeding species are significantly different in structure from the residents, which is alone a fact of biological interest. In response to their comment that my Audubon's Warbler data are ". . . contrary to many years' field experience by everyone else in southwestern deserts . . ." and that the birds ". . . never countering nor defend territories . . ." I can only state that "everyone else" was not in my study area during 1 May–30 June 1973, nor do I recall seeing any of the commentary signers there during those dates either. As I mentioned in the 1977 paper (and I still feel this way) the extremely late winter and accompanying heavy snowfall to the north likely inhibited normal northward migration, resulting in the rather atypical, yet still biologically interesting, data set upon which my reports were based. It should be noted that their criticisms apply to only 3 species (Audubon's, MacGillivray's, and Wilson's warblers) not to "5 or 6" as they state in their commentary.

The House Wren data are accurate, with 16 territories (not just song perches) being located in the study area, which includes more than just desert riparian habitat (mouth of Zion Canyon, Springdale ponds, cultivated fields?). As to their criticism of my literature citations I can only state that the two papers had different purposes and perhaps different audiences; one was oriented toward methods and a geographical comparison, while the second dealt more with the avian biology of the area. The statement that criticism of the first paper produced a change in the literature cited of the second is ludicrous, without basis in fact (none of the letter signers were reviewers on either of the papers), and totally conjectural. Of course I was familiar with all the relevant Virgin River publications as well as ". . . the most elementary ornithological literature . . ." before going into the field. As some of the signers surely know, the habitat in the Virgin River Valley has been changing rapidly, and the habitats in Behle's classic 1943 paper are thus vastly different from those when I was studying the area. While no one questions their accuracy, not even Wauer's mid '60's data are reliable indicators of the 1970–80 picture. After all, "any experienced naturalist knows that nature is disorderly."

In conclusion, I would like to comment on the rather unfortunate and highly emotional tone of their commentary. It seems a waste of time to pick on a person's literature cited section when so many questions of a biological nature are yet unanswered. If the signers are truly interested in the Virgin River Valley, why not contact me or request copies of my data (the originals of which are still squirreled away in one of my file cabinets), so that we can develop a *scientific* dialogue? And why wait for 6 years after the first paper was published to say anything? I agree with the signers that there needs to be careful documentation in all fieldwork and that the collection of specimens is often needed to verify identification. But why collect a series of 16 House Wrens when Utah museums are full of them? Moreover, even a specimen is not necessarily an accurate piece of field data. After all, dead birds tell no tales.

Finally, I would like to thank the signers for including me with Fretwell, James, Karr, Levin, MacArthur, Selander, Tramer, and Wiens as a "modern ecologist." Up until now I had thought of myself as an ornithologist who happened to know a couple of statistical tricks.

Response: Ornithology and Ecology as Sciences

JAMES A. MACMAHON AND PETER B. LANDRES¹

We are happy to have the opportunity to respond to the Commentary of Austin et al. It is a bit surprising that we are in the position of responding to a statement that contains much with which we agree and that we infer all avian ecologists or ecological ornithologists might agree. We fall into this former group, ecologists who find birds interesting and tractable as objects of study in an attempt to elucidate ecological principles. As such the "basic reference works and pioneer handbooks" of the people mentioned by Austin et al. and those similar works produced by many of the signatories of the Commentary are often starting points, indeed the very sources, for the development of ecological ideas that later lead to formalization into a fabric of ecological hypotheses. With this basic agreement one might suspect that we would tacitly accept all that Austin et al. proffer. This is not the case. We respond to their Commentary in two parts. First, we address their specific statements about our work, and then we take this opportunity to make some broader philosophical comments.

¹ Department of Biology, UMC 53, Utah State University, Logan, Utah 84322 USA.

First the two substantive comments. There is the possibility that we misidentified a flycatcher, included in our study, in the field. *Empidonax* flycatchers are difficult to identify, and we would not be the first to have problems with them. However, we noted vocalizations, looked at field marks, and even went to the bird collections at the University of Arizona to examine skins of animals from the general area of our site. All such data continued to suggest that we observed *Empidonax oberholseri* rather than any of the other forms. We were well aware that this was not in line with published works; empiricism, however, won out. We did not shoot voucher specimens because they were not abundant and because our study was designed to look at interactions of birds and vegetation architecture as well as birds with birds. Deleting an uncommon species would have introduced a source of error we chose not to deal with. As it turns out, other observers (e.g. David W. Steadman, University of Arizona, pers. comm.) point out that they have seen *E. oberholseri-hammondi* in similar areas of northern Sonora in the spring also. Thus our identification is not necessarily in error. The more likely error is that we assumed that a bird was a breeding member of the community based on song. As we now know that migrant or wintering *oberholseri* may call, we may be (in fact most certainly are) in error about the bird's breeding status. Because we were only interested in interactions over the time span of our study, the fact that the bird was there, regardless of its breeding condition, does not cause our results to be suspect. As important as a scientific name is to researchers, if we never named our bird and instead called the bird species "A" it would not alter our results, nor would the exclusion of this species from the data of the larger study affect our overall conclusions.

The second point of Austin et al., chastizing us for using weather data from Douglas, Arizona to suggest the parameters for our site 120 km away, is also unfounded. Douglas is within the same Climate Diagram zone as our site (Walter et al. 1975, Climate-diagram Maps, New York, Springer). Weather data are not critical to our study, and 2 yr of data won't tell the context in which the community develops, on the average, so we offered the data available as a suggestion so that other workers could visualize, in a general way, the weather of the study site. Nowhere in the paper do we use these data for interpretation of results.

Thus, we probably identified our flycatcher properly, but misassigned it to breeding status. We see no problem with our use of the weather data. In sum, the points indicated by Austin et al. do not alter any of our conclusions.

In a more philosophical vein, we have noted in the Commentary section of this journal during the last year that several museum-oriented ornithologists have exchanged verbal blows with avian ecologists. We believe that some of this commentary has provided more heat than light. Knowledge is not derived from only one habit of inquiry; rather the various approaches to knowledge are tailored to the topic of study. Thus, habits of inquiry in nuclear physics need not be, nor should they be expected to be, the same as in biology. This same divergence is true within avian biology. Highly accurate, detailed distribution maps based on valid identification are a needed part of science. But these and detailed regional lists are not the only form of data that constitute good science and avian biology. We might, for example, be interested in asking questions about nature that might be phrased as "just how far can I go toward elucidating generalizations about the relationship of birds to environmental components?" Such an approach may demand a broad-brush conceptualization of the problem and an attempt to collect very specific data to test explicit hypotheses. These approaches are not mutually exclusive but are rather complementary, even synergistic.

We suggest that ecologists should continue to recognize that without the details of the regional guides and the knowledge provided by museums, many of us would have more difficult tasks in our pursuits, and often the grist for our hypothesis-testing would be lacking. Conversely, more traditional ornithologists should be aware that ecologists often study their organisms very intensely, for varying periods of time, for specific purposes, in ways that break with tradition. Such studies do not purport to describe the total biology of an organism, but only its relations in the context of the specific study—though it is hoped that such studies will reveal patterns that will repeat themselves in nature and provide the foundations of a predictive science. Often the analysis of the volume of data collected in such intense studies is so great that only statistical analysis can unravel the complexities. The lack of need by some workers for hypotheses, statistics, or models for one type of work does not negate their value in other contexts. Some flycatchers, after all, are only distinguishable by "average" characters—a use of statistics.

Finally, both sides of the discussion we have noted in *The Auk* seem to suffer from hyperbole. What each of us should be doing is applying our various approaches to problems of avian biology as skillfully as possible, while at all times heeding information from other lines of inquiry. Such an approach will allow us to advance as a moving front; there is too much work to be done for us to become bogged down in a quagmire of discussion directed, often chauvinistically, toward the quality of another's type of science.