

Response: In the Eye of the Beholder

ELLIOT J. TRAMER¹

I welcome this opportunity to reply to the commentary by Austin et al. Included among the signees are names I have admired all my professional life. Thus I am especially gratified to see my work cited above; if one fails to stimulate his peers he will be left severely alone. My gratification is dampened somewhat, however, by careful reading. It appears the commentary isn't about my work at all; rather, it's a revelation of the disciplinary and philosophical predilections of the authors. Hence I will respond to comments on my own work in the briefest terms and then proceed to what I believe to be the real substance of the commentary, and thus of this reply.

Concerning my latitudinal gradients paper (1974, *Condor* 76: 123), the literature citations are obviously skewed toward the authors whose works bear on the main issues at hand. As I am not sure what the other objections of Austin et al. are, I can only conclude that I failed to write the paper they would have written had they been me. Surely I need not provide a discussion of what is, or is not ecology. Most readers of *The Auk* already know, and for those in doubt the past 10 years have seen a proliferation of excellent textbooks on the subject.

Austin et al. "suspect" I misidentified "about 20% . . . in part or in whole" of the species listed in my paper on migrants in Yucatan (1974, *Condor* 76: 460). I suspect I didn't.

But the main issue here is one of appreciating the value of *all* the diverse approaches that contribute to Ornithology, or, for that matter, to any field of scientific inquiry. Ornithologists have much to gain from one another. To feel threatened by the growth of a new discipline is to miss an opportunity. The dependence of population ecologists on the work of taxonomists is obvious; one reason birds have been so attractive to ecologists is that avian systematics and distribution are so well known (although the "true" distributions, migrations, and ecology of birds are constantly changing as avian habitats are altered). The fruits of ecological inquiry also offer opportunities to those in other disciplines. For example, the work of Crowell, Grant, Selander, and others has revealed morphological differences that accompany diversity variations between island and mainland avifaunas. It is not clear whether these morphological changes result from a relaxation of interspecific competition on islands, in part because we understand so little about functional morphology. For example, no one is sure whether a higher population variance in some beak dimension really allows that population to capture a wider array of food items—we cannot interpret these morphological features in a way that clarifies their adaptive significance to the birds. Here is a fertile area for an anatomist with a flair for mechanics!

In sum, what is interesting Science is in the eye of the beholder. What is good Science spans a far broader spectrum.

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Response: Ornithology as Science

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I would like to thank Austin et al. for including me with such notable company as Tramer and MacMahon in their tirade on "modern ecology," and for calling attention to two of my papers on the multivariate analysis of bird communities in the Virgin River Valley. Owing to the highly regarded status of some of the commentary signers, I feel obliged to answer their criticisms.

At no time, in either paper, did I state or imply that all of the birds for which habitat data were collected were breeding birds. On the contrary, I did acknowledge that Audubon's (Yellow-rumped) Warblers were known to migrate through the area, as were several of the other species. The birds did sing full songs (not chip notes), however, showed intense intraspecific aggression, and remained in specific areas for days at a time. Based on these criteria, I felt justified in taking vegetation measurements in their habitats. The problem with studying desert birds is that they tend to be early breeders and to get an early enough start on data collection one must be in the area while migrants are still going through.

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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.

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