

Ornithology as Science¹

Ornithology is defined as the scientific study of birds. Anyone unaware that the natural sciences are man's accumulated knowledge of nature's facts, or truths, obviously has never read any of the many great classics that fill libraries. It was therefore something of a shock to read the Editor's description (1980, *Auk* 97: 409) of this search for simple truths as "sterile and empty" ("endless gathering of natural history observations with no guiding questions or concepts" being thus condemned and equated with "theorizing with no knowledge of the real world"). Similarly disturbing commentaries appeared from others; for example: "We never know the truth in science" (Fretwell, idem: 421). Further, we read that "the common quest of all ecologists as we develop our science" is "the detection of emergent patterns of order out of what seems chaos" (Levin, idem: 425). Any experienced naturalist knows that nature is disorderly; why cannot self-termed "ecologists" accept and study nature's truths as they are?

Fretwell's statement in particular must have started the entire past membership of the A.O.U. spinning in their graves. This sort of "modern ecology" conflicts too directly with basic ornithology. The basic reference works and pioneer handbooks of Florence M. Bailey, Bent, Blake, Brodtkorb, Chapman, Conover, Coues, Hellmayr, Hoffmann, Palmer, Peters, Ridgway, Skutch, Taverner, Wetmore, and others have stood the test of time; they are not untruthful, nor do we consider them more sterile and empty than "modern ecology." Neither are the solidly based contributions of Brewster, Burleigh, Godfrey, Grinnell, the several Howells and Millers, Jewett, Kilham, Louise deK. Lawrence, Lowery, Mayfield, Munro, Nelson, Nice, Nolan, Stone, Sutton, Swarth, Van Tyne, Walkinshaw, Willett, and the innumerable other searchers for nature's truths. Such "modern ecology" is not ornithology; and some of it is not even true ecology, by any stretch of the imagination, but is mere playing with figures and models.

One example of this is Tramer's "On latitudinal gradients in avian diversity" (1974, *Condor* 76: 123). Here we find winter "species density" plotted for North and Middle America by squares (500 km on a side), totally ignoring the basic principles of ecology long ago set forth by Merriam and others. Surely any informed naturalist knows that the fauna and flora of the high volcanoes of México, Guatemala, and Costa Rica are different from those of the nearby coasts, and that still other forms occur at intermediate levels. We are told (p. 128) that "the winter ranges of land-birds given in the A.O.U. Check-list (1957) were used to compile the gradient"; yet the A.O.U. does not mention the purely Meso-American species at all, nor does Tramer's bibliography mention the works of Alvarez del Toro, Blake, Dickey and van Rossem, Eisenmann, Friedmann et al., Griscom, Howell, Land, Miller et al., Monroe, Paynter, Russell, Schaldach, Smithe, or even Wetmore (although loaded with Cody, Karr, Klopfer, and MacArthur)! Is this ecology?

Let us consider some other "ecological" papers of recent years. Not exceptional, we fear, are Whitmore's two on the (presumably breeding) passerines of the Virgin River Valley, southwestern Utah. The first (1975, *Wilson Bull.* 87: 65) was based entirely on "plots with a singing territorial male bird as the center"; yet it includes 5 or 6 (out of 24) species that surely do not breed there. The bibliography, like Tramer's cites Cody, Klopfer, and MacArthur (also Beals, James, Selander, Sibley, and Sokal and Rohlf) with no mention of the A.O.U. Check-list, Behle, Bent, R. Hardy, Wauer, Woodbury, etc. Evidently there was criticism of this, for the second paper (1977, *Wilson Bull.* 89: 253) does cite some of the relevant literature. Nonetheless, all the doubtful species are again listed and only two are discussed: the House Wren is defended because Wauer and Carter (1965) had recorded it "in the riparian woodland during the breeding season"; what Whitmore does not mention is that they specified "above the desert," which eliminates his study areas! Of Audubon's (Yellow-rumped) Warblers, he reports "large numbers" without specifying dates—sometime between 1 May and 30 June. He adds, "I observed them actively countersinging and defending territories through June," implying quite clearly that they were still numerous. "I do not know if Audubon's Warblers ever left during the breeding season and if they did leave the effect of competitive release on the other warbler species." We can only say that this is contrary to many years' field experience by everyone else in the southwestern deserts; Yellow-rumped Warblers all leave by late May or early June, rarely sing, and never countersing nor defend definite territories. (This is all equally true of two others of Whitmore's warblers, MacGillivray's and Wilson's.)

Similar inaccuracies invalidate another Tramer paper purporting to give "Proportions of wintering North American birds in disturbed and undisturbed dry tropical habitats" (1974, *Condor* 76: 460). In about 20% of the species, there is reason to suspect misidentification, in part or in whole. For example, both the coastal *Zenaida aurita* and *Leptotila jamaicensis* are reported from interior Yucatán, but not the common *L. verreauxi*; *Amazilia tzacatl*, a rain-forest hummingbird, is reported widely (even to the

¹ Authors and their addresses are given at the conclusion of this essay.

arid outer beaches), but not the common *A. yucatanensis*. *Polioptila caerulea* is listed as a migrant, although the race *deppei* breeds on the peninsula (its type locality); the only gnatcatcher listed from 5 km south of Umán, inland, is *P. alboris*, primarily an outer-beach bird. All but one of the *Myiarchus* are listed as *yucatanensis*, which is actually less common than the unlisted *tuberculifer*. Not listed, either, are the common *Euphonia affinis* or *Vireo pallens* (although Tramer does report the somewhat similar but casual or accidental Orange-crowned Warbler, *Vermivora celata*). As usual in these "ecological" papers, there are no specimens, no documentation, nothing on identification, and no useful references at all; in fact, only 8 papers are cited, including 1 by Karr and 2 by Leck. Obviously this sort of undocumented "ecology" can only mislead.

Finally, we now read Landres and MacMahon's "Guilds and community organization: analysis of an oak woodland avifauna in Sonora, Mexico" (1980, Auk 97: 351). By plotting birds on territories, these authors conclude (among other inaccuracies) that two pairs of Dusky Flycatchers (*Empidonax oberholseri*) breed per 10 ha in open oak woodland in one area in eastern Sonora. There is the usual complete lack of documentation, and no indication of how any flycatchers were identified. Perhaps those they called *E. oberholseri* were dark-chested, a character of *E. hammondii* but also of *Myiarchus tuberculifer*, a small flycatcher that does breed in the oaks of Sonora and that is not mentioned. Even if really *Empidonax*, the birds are not likely to have been *oberholseri*, which is distinctly less numerous during the study period (which was only from 28 March to 1 May) than either *hammondii* or *E. d. difficilis*. We note, too, that although the study area is in Upper Sonoran Zone woodland, temperature range and precipitation are given only for Douglas, Arizona, a Lower Sonoran Zone (desert grassland) locality 120 km away!

With its limited coverage (all too characteristic of "modern ecology"), poor choice of dates, and dubious, undocumented identifications, this sort of "ecology" is unqualified for serious ornithological journals, for the same reasons as the various papers discussed above—particularly failure to consult the most elementary ornithological literature. The slightest glance at the A.O.U. or Mexican check-list, van Rossem, or even any adequate field guide would have told the authors that Dusky Flycatchers do not breed anywhere in México, and a bit of research in really useful ecological sources (such as the various state books on birds of the southwestern United States) would have warned that the study area was totally unsuitable ecologically for breeding *Empidonax* of almost any kind.

May we then suggest that ornithological journals, as permanent scientific documents, reject undocumented papers that confuse and alter the true distributions, migrations, and ecology of birds so carefully worked out by so many dedicated ornithologists in the past? Regional papers should be supported by some evidence, and should preferably be by authors thoroughly familiar with local birds (and preferably also plants) and with years (not days or weeks) of experience in the region. Let others publish elsewhere, if they must.

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