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COMMENTARY

PERSONAL PERSPECTIVES AND LACK OF DATA UNDERLIE DIFFERENT INTERPRETATIONS OF INTERSPECIFIC AGGRESSION

This is a reply to the commentary by Murray (Condor 88:543, 1986a) on our earlier paper (Livezey and Humphrey, Condor 87:154–147, 1985a), that of Nuechterlein and Storer (Condor 87:87–91, 1985a), and the subsequent commentaries (Murray, Condor 87:567, 1985; Livezey and Humphrey, Condor 87:567–568, 1985b; Nuechterlein and Storer, Condor 87:568, 1985b). Unlike Murray, I am not convinced that the exchange has been particularly illuminating, but a few final remarks seem justified.

Murray (1986a) contrasted his philosophical approach from that of myself, Humphrey, Nuechterlein, Storer, "and most other ornithologists." Murray claims to (1) make assumptions (2) make "predictions," "statements," or a "search for patterns" that seem consistent with (or from which can be deduced) the available observations, and if the fit is acceptable, then (3) "infers" that the underlying mechanism has been discovered. Most of the rest of us, he writes,"... report their observations and offer an hypothesis to explain the observations." Although I cannot speak for other ornithologists, I contend that, at least concerning interspecific aggression, this distinction is unwarranted. Scientists generally are constrained by working assumptions, consider available data, and suggest possible causes for these observations, hopefully as testable hypotheses. As more data are collected, workers reject or modify current hypotheses, or propose new explanatory models. I see no evidence from Murray's published arguments (e.g., Ecology 52:414–423, 1971; Condor 78:518–525, 1976; Biol. Rev. 56:1–22, 1981; 1985; 1986a) or the philosophers he cites that he operates in a fundamentally different way. Even if one were persuaded by Murray's general thesis (Oikos 46: 145–158, 1986b), I feel that in this instance it provides no new insights or empirical power. Perhaps Murray has not failed to communicate his views, as he (1986a) believes, but rather his views fail to generate interest because they have few practical implications.

There are differences in perspective, however. Murray (1986a) stated that most ornithologists assume traits (including interspecific aggression) to be adaptive. I agree, but it should be emphasized that Murray makes the equally potent but opposite *assumption* that interspecific aggression is *not* adaptive (e.g., Murray 1971: 415), although he (1981:17) admits that it evidently is in some instances. For *Tachyeres,* I find Murray's choice less appealing because of the implausibility of such widespread, frequently injurious behavior being maintained in the face of opposing selection. The opposite assumption of Murray led him to a "mirror" inference—that the nonaggressiveness of subordinate species is an adaptation for coexistence with dominant species (Murray, Auk 86:199–231, 1969; 1971). The associated inference that such events entail mistakes in identification, in species obviously capable of recognizing not only conspecifics but of distinguishing their mates, is equally unpersuasive. One's approach is necessarily influenced by one's views on related issues; Murray's (1971, 1976, 1981, 1986b) is influenced by his distinctive opinions concerning the competitive exclusion principle, interspecific competition and character convergence, the relationship between territory size and food supply, interspecific territorality and counter-selection within populations, and the relative gravity of mistaking nonadaptation for adaptation or vice-versa.

Perhaps the most serious misinterpretation Murray (1986a) makes is that regarding the alternative hypotheses offered in our original paper (1985a). He claims that "... these are not five possibilities from which ... one hypothesis will eventually be shown to be correct." We do not know if any of the proposals will be borne out by future work (neither can Murray), but we guess that at least some would pass the test. Available data are simply inadequate for anyone to offer a "finished" model, and our hope that our alternatives be tested should be obvious from the experiments suggested in our commentary (1985b). Murray's use of "correct" with respect to our proposals is his own, and I do not endorse or claim to understand it. Equally misleading, I think, is Murray's (1986a) statement that interspecific aggression of steamer-ducks is a single phenomenon; an attempt to define away this possibly multifaceted problem does not constitute valid argument. His opposition to possible selective advantages of interspecific territoriality is surprising given his earlier (1981: 17) discussion of "fortuitous" consequences of such aggression.

If we are to be faulted for *ad hoc* hypothesizing, then so is Murray, but at the level of proximate cause. Murray (1971, 1976, 1981, 1985, 1986a) repeatedly refers to (but rarely details) the proximate stimuli that elicit aggression, and argues that these are proximate causes of intraspecific and, for other species sharing these stimuli, interspecific aggression. One could invent different combinations of possible stimuli to cover the targets (and exclude nontargets) of *each species* considered, and these could be revised speculatively to include and exclude species as new observations become available. One could contrive such a set for *Tachyeres*—stimuli shared by catfish, grebes, cormorants, a variety of anatids, and a rat, but not possessed by penguins or charadriiform shorebirds. Lacking desired observations, one could conjecture that predicted but undocumented targets simply have been overlooked or one could resort to imaginary experiments.

Finally, Murray's (1986a) claims regarding his theory merit brief consideration. I see nothing *illogical* in his arguments, although I question the plausibility of some. Murray's denial of the existence of proposed alternatives is surprising, given the present exchange of ideas and Murray's (1971, 1976, 1981) preoccupation with the alternative argument of food competition. As for the claimed lack of contradictory "facts," this is a matter of opinion and reflects in large part: (1) an undeniable paucity of data necessary for rejection of alternatives (not lists of anecdotal observations) and (2) the untestability of certain assumptions (e.g., the adaptiveness of a behavior *at the time of its origin*; Murray 1981:17). Advocacy of a single explanation—in the absence of critical data, by appeals to supposed philosophical differences, and in the name of generality (at the expense of realism)—is at best unproductive and at worst a deterrent to the design and performance of comprehensive, decisive tests.

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