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COMMENTARY

THE INFLUENCE OF PHILOSOPHY ON THE INTERPRETATION OF INTERSPECIFIC AGGRESSION

My commentary (B. G. Murray, Jr., Condor 87:567, 1985) on the papers of Nuechterlein and Storer (Condor 87:87–91, 1985a) and of Livezey and Humphrey (Condor 87: 154–157, 1985a) regarding interspecific aggression in steamer-ducks (*Tachyeres* spp.) and the responses to it (Nuechterlein and Storer, Condor 87:568, 1985b; Livezey and Humphrey, Condor 87: 567–568, 1985b) are probably more instructive than they appear to be. I think they illustrate the fundamentally different philosophical approaches taken by me (essentially a theoretical biologist) and by my colleagues (essentially empirical biologists) to solving problems, and these differences seem deserving of explicit elucidation.

In my 1971 (Ecology 52:414–423, 1971) and 1981 (Biol. Rev., 56:1–22, 1981) papers I presented two hypothetico-deductive theories regarding the adaptive and nonadaptive origins of interspecific territoriality. The predictions of these theories seemed consistent with the available reports of interspecific territoriality in birds and other groups of animals.

With this method, when one has a series of assumptions that leads to a series of predictions, which are consistent with observations, one usually *infers* that he has discovered the cause-and-effect relationships that explain the facts, at least until an alternative set of assumptions explains the facts as well (or better) or until other facts are discovered that are contrary to prediction (I. M. Copi, Introduction to Logic, 3rd ed., Macmillan, New York, 1968; T. S. Kuhn, The Structure of Scientific Revolutions, rev. ed., Univ. Chicago Press, Chicago, 1972; K. R. Popper, Objective Knowledge, rev. ed., Oxford Univ. Press, Oxford, 1979). To my knowledge, no one has challenged the logic of my arguments or the facts I have used in testing my arguments, no alternative theories have been proposed to explain those facts, and no facts have been discovered that are contrary to the predictions of my theories.

In contrast, Nuechterlein and Storer (1985a, b) and Livezey and Humphrey (1985a, b) exemplify the method, which I believe predominates in ornithology, of the empirical biologists. They report their observations and offer an hypothesis to explain the observations. For example, they observe that steamer-ducks are aggressive toward birds of other species that may be eating the same food items; therefore, they hypothesize, the advantage of interspecific aggression in this case is elimination or reduction of the effects of food competitors. Or, they observe that steamer-ducks are aggressive toward birds of other species with which competition for resources seems most unlikely; therefore, they hypothesize, in this case interspecific aggression is "sexually selected ritualized behavior for assessment of males by females" or "practice for intrageneric combat." Epistemologically, these are ad hoc hypotheses, each of which accounts "*only* for the particular fact or facts it was invented to explain and has no other explanatory power, that is, no other testable consequences" (Copi 1968, emphasis in original). To some philosophers (e.g., Copi 1968; Popper 1979) ad hoc hypotheses are unsatisfactory and uninteresting scientific explanations. With such an approach we end up with a series of ad hoc hypotheses and no generalizations. Indeed, between them, Livezey and Humphrey (1985b) and Nuechterlein and Storer (1985b) offered five different hypotheses to explain a single phenomenon—interspecific aggression of steamer-ducks. These are not five possibilities from which the authors believe one hypothesis will eventually be shown to be correct. Rather they are five explanations to cover the range of targets attacked by steamer-ducks, each explanation being "correct" in a particular situation. Whether this is a desirable state of affairs is a matter of personal preference, although it should be noted that (by some definitions, at least) science is the search for generalizations (J. Brownowski, Science and Human Values, rev. ed., Harper and Row, New York, 1965; Copi 1968; Popper 1979).

These fundamental and apparently unrecognized philosophical differences in approach have led to a failure by me to communicate to others an understanding of what I have been trying to do for the past 20 years—to find general explanations with the hypothetico-deductive method. Thus, because I believe that an understanding of the proximate stimuli for interspecific aggression plays an important role in evaluating the origin of the behavior, both Livezey and Humphrey (1985b) and Nuechterlein and Storer (1985b) contend that I am concerned only with proximate stimuli whereas they are concerned with the ultimate factors leading to the evolution of interspecific behavior. In fact, my theories of interspecific territoriality are clearly concerned with the evolution of this behavior (Murray 1971, 1981). I have explicitly considered the question, has interspecific aggression evolved as an adaptation, or is interspecific aggression a nonevolved consequence of two (or more) species exhibiting the same proximate stimuli that elicit intraspecific aggression? What is at issue is not that Livezey, Humphrey, Nuechterlein, and Storer are trying to solve a different problem from the one I am, but that we have philosophically different ways of trying to find the solution.

As empirical biologists, Livezey, Humphrey, Nuechterlein, Storer, and most other ornithologists make observations and ask, How do we explain our observations? Traits are often assumed to be adaptive, and the question becomes, What is the selective advantage of this trait? In contrast, as a theoretical biologist, I search for patterns and ask, What statements can I make from which I can deduce the observed patterns? The respective answers to these questions are often different, and which answer is acceptable depends almost solely on one's scientific philosophy rather than on the correspondence between hypothesis and fact (Kuhn 1972; Murray, Oikos 46:145–158, 1986).

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