Letters

Altruism: Methodological and Definitional Issues

Harry W. Power's claim (Reports, 11 July 1975, p. 142) to have demonstrated the absence of altruism in mountain bluebirds illustrates some of the logical, definitional, and methodological problems in the current research and theorizing on altruism.

Power suggests that the resolution of the debate concerning true altruism may affect social policy "insofar as officials assume that humans, like other animals, are basically either altruistic or selfish," and that it is therefore "important to assess the frequency of true altruism in nature." Officials have indeed been known to hold extreme views, but this has all too often been facilitated by scientists eager to overinterpret their findings. By phrasing the altruism-selfishness issue as an either-or question, and by the very mention of social policy in a report on bluebirds, Power shows a lack of caution, to say the least.

The claim that Power's study would help estimate the base rate of altruism is unreasonable, given that he has observed a small number of individuals of one species for a short period of time in one location. Such constraints and the dichotomous-choice analysis (presence versus absence of altruism) present serious problems, especially when the main hypothesis is stated in the negative (that is, absence of altruism). Given that specific stimuli to induce altruism versus selfishness were not presented in a controlled manner, few conclusions can be drawn about the absence of a behavior on the basis of short observation periods and a somewhat haphazard time-sampling method. This is particularly true of one of the behaviors in question—feeding the young—for which there is clearly a low probability of occurrence during relatively short observation intervals.

Power defines true altruism as the "promotion of other's reproductive success while reducing one's own inclusive fitness" and then claims that the "case for true altruism has been . . . weakened by Hamilton's . . . distinguishing kin altruism . . . from true altruism." Thus, Power seems to believe that the case for a phenomenon can be weakened by a (re)definitional fiat (*1*). One might ask which findings Power would have to obtain for him to conclude that true altruism exists. Even if a male consort did defend and feed its new mate's nestlings, one could argue that this was not an example of true altruism, since (i) the bird may have been a close relative of the nestlings' father, and/or (ii) the consort's behavior could be interpreted as increasing its chance of mating with the nestlings' mother. In either case, the consort could potentially increase, rather than decrease, its inclusive fitness by altruistic actions, which would, however, not be defined as "truly" altruistic by Power (or Hamilton).

The definitional chaos is augmented by the introduction of dualist and voluntarist assumptions. Thus, Stephen T. Emlen (Letters, 27 Feb., p. 808) criticizes Power's study on the grounds that consorts did not have a "true choice" of behaving altruistically or selfishly: Whereas they were physically able to feed the nestlings, they may not have been hormonally ready. Power (Letters, 27 Feb., p. 809) responds by proposing another dichotomy—the consorts' incapacity versus their refusal to foster the nestlings. On the basis of weak evidence, Power claims essentially that male consorts were physically able to feed the nestlings but did not want to (*2*).

To confuse matters further, R. D. Alexander (cited in Power's report) has suggested that male consorts were, after all, altruistic in that they did not kill the nestlings (thus, defining altruism as the *absence* of a *destructive* behavior). Power's explanation is that the killing of the nestlings would have prevented pair bond formation. If so, why was not pair bond formation prevented by the consorts' not feeding and not defending the nestlings?

In short, Power's methodology, formulation of the problem, and interpretation of the data seem to have obscured rather than clarified the definitional and empirical issues. Rather than attempt to demonstrate the presence or absence of "true" altruism, it would seem more fruitful to investigate the precise conditions which govern the occurrence and the degree of seemingly altruistic acts. Finally, with regard to social policy and generalizations to *human* altruism, Power omits mention of research that has been carried out on this topic by social psychologists during the last decade (*3*).

VLADIMIR J. KONEČNI
Department of Psychology,
University of California, San Diego,
La Jolla 92093

References and Notes

1. The problem in part stems from the acceptance of the propositions of the kinship theory, on the one hand, and the simultaneous use of the term "altruistic" for behaviors which are actually nonaltruistic in terms of that theory, on the other hand. If individuals are selected to maximize inclusive, rather than personal, fitness, then personal sacrifice in many situations (delineated by the theory) would be considered as a modal or "normal," rather than an altruistic, response.
2. In his reply to Emlen, Power also claims that his original conclusions had been largely based on the data concerning defending, rather than feeding, behavior. That this is not true can be ascertained by anyone carefully reading Power's original report.
3. J. Aronfreed, in *Altruism and Helping Behavior*, J. R. Macaulay and L. Berkowitz, Eds. (Academic Press, New York, 1970), p. 103; D. T. Krebs, *Psychol. Bull.* **73**, 258 (1970); L. G. Wispé, Ed., *J. Soc. Issues* **28** (No. 3) (1972).

The fundamental importance of determining the reality and frequency of altruism can be seen from the following argument.

Living things require resources (energy and material) to metabolize, grow, respond to environmental contingencies, and reproduce. This biological activity creates competition for limited resources, if not immediately at the time of the origin of life, then within a few generations, because reproduction increases the total demand on resources. Competition results in greater net reproduction by individuals that can sequester limited resources and eventual extinction of those that cannot. Sequestration of limited resources can be called reproductive selfishness. Thus, although it is derived, reproductive selfishness is a basic property of living things.

As living things, humans are necessarily reproductively selfish *unless* they have somehow evolved to altruism from a primordial state of selfishness. Various ideologies (economic, religious, scientific) assume humans to be altruistic, or at least fully capable of altruism, despite both the difficulty in understanding how altruism could have evolved and much historical evidence of selfishness. Because these ideologies do or would shape our lives, determining the truth or falsity of the assumption of human altruism may have vital consequences.

The assumption of altruism can be tested in both humans and other species.

Testing the same hypothesis in several species is the substance of the comparative method. Faith in the comparative method stems from overwhelming evidence for the theory of common descent and from the practical benefits obtained from its application in medicine and agriculture.

Studies of nonhuman species help determine the reality and frequency of altruism in nature, thereby providing a background for evaluating the human condition and perhaps providing clues as to how altruism could have evolved, if it is ever substantiated in humans. Such studies also test the core of the Darwinian theory (evolution through inter-individual selection); evidence of widespread altruism would refute its generality and require its abandonment or major modification.

In my study of altruism in mountain bluebirds, I attempted to prospectively test for altruism in one species. Others (see references 1–5 in my report) have retrospectively argued the altruism question with varying degrees of persuasiveness, but firm conclusions cannot be based on evidence gathered either under unknown conditions or by investigators not specifically testing a hypothesis. Whatever weaknesses my study may have, it is an attempt better followed by other experiments than by debate (*1*).

I concede none of Konečni's criticisms:

1) His implication that studies of bluebirds could have no relevance to human behavior effectively denies the value of the comparative method. Because all terrestrial species have a common descent, a discovery about one species can be profitably employed as a hypothesis (although not a conclusion) about another.

2) Konečni's denial that studying a single species helps estimate the frequency of altruism throughout nature effectively disavows a basic assumption of logic, that enumerating particulars leads to generalizations. This disavowal implicitly denies that nature has order and thus denies the possibility of science.

3) Konečni's assertion that I lack caution in mentioning social policy in a report on bluebirds and ought to have reviewed social psychology studies implies that I attempted to generalize results about bluebirds to humans. I wrote a report, not a review, and stated no conclusions about human behavior. I mentioned social policy to indicate the importance of resolving the altruism question.

4) Konečni's objection to my asking either-or questions implies that he considers all phenomena to be continuous. But many phenomena have discrete effects even if they can be measured along a continuum: a difference of a few nanometers determines the visibility of light, and small differences in physiological function determine ecological death. Altruism and reproductive selfishness form a dichotomy because the former decreases and the latter promotes inclusive fitness; incapacity to help and refusal to help (*2*) also form a dichotomy because they refer, respectively, to ability and use of ability. To insist a continuum is a functional dichotomy is to do as great a violence to truth as to capriciously insist a dichotomy is a continuum.

It is possible to resolve an either-or question by testing an either-or hypothesis. Thus I tested whether bluebirds would behave either altruistically or selfishly under specific conditions. Similarly, in statistical analysis groups are compared to determine whether they are the same or different.

5) Konečni's claim that I add to definitional chaos by distinguishing true, kin, and reciprocal altruism is incorrect. My separation actually enhances understanding by defining altruism with reference to its effect on inclusive fitness rather than its behavioral form. Unfortunately, I erred by not using even clearer terms: West Eberhard has used "beneficence" as a generic term for all helpful and seemingly helpful behaviors irrespective of their effect on the inclusive fitness of the benefactor, while Alexander has used "nepotism" as a substitute for "kin altruism" and "reciprocity" as a substitute for "reciprocal altruism" (*3, 4*). Their lexicon leaves "altruism" as a term exclusively describing acts of beneficence decreasing the benefactor's inclusive fitness (*5*).

6) Konečni's assertions that I failed to provide specific stimuli to induce altruism or selfishness and employed an inadequate observation schedule is also incorrect. I provided specific stimuli in the form of helpless, demanding nestlings, and myself as a potential nestling predator. I visited each nest as often and long as I could while simultaneously conducting an experiment on foraging behavior. This past summer I made further observations and witnessed two male consorts sequentially join a female at a nest where the resident male had been killed by a predator. During 293 minutes of observation in a 4-day period, when both a consort and resident female were present, the female fed her nestlings 52 times, removed 14 fecal sacs from her nest, and directed frequent alarm calls, often toward me (whenever I was not in a truck), and once at a passing coyote. Neither consort provided any care, yet male parents normally provide as much care at that time of the season as do females, and frequently are more aggressive (*6*).

7) Konečni's questioning what findings I would have to obtain to acknowledge the existence of altruism, and his implication that I could always explain evidence away, implicitly denies the value of considering hypotheses in a related sequence. In considering altruism, one must first test the hypothesis that a particular animal is beneficent (for example, consorts provide care to nestlings). If no beneficence occurs, no further hypotheses need be considered. But when beneficence occurs, one must consider two other hypotheses: (i) the benefactor is related to the beneficiary(ies); and (ii) the benefactor is reciprocally rewarded (for example, consorts have young through the surviving parent). These latter hypotheses consider the possibilities of nepotism and reciprocity, respectively. Therefore, I was not evading admitting altruism by considering these hypotheses in my discussion. The female consort that fostered young appeared to be behaving altruistically. But reproductive error can occasionally be expected and is no more evidence that altruism has specifically evolved in bluebirds than fractures are evidence that bones have evolved to break.

HARRY W. POWER

Department of Biology, Livingston
College, Rutgers University,
New Brunswick, New Jersey 08903

References and Notes

1. Most evolutionary biologists of my acquaintance find the evidence for altruism so weak that they reject the reality of altruism except in the form of an occasional action by an individual consequently disfavored in the process of selection. Yet most ideologues and social scientists of my acquaintance assume human altruism and so seek to devise schemes to make manifest our supposed innate altruism. Both sides have persistently erred by basing conclusions on anecdotal evidence and preferring polemics to experiments.
2. Konečni complains about voluntarist assumptions. But the most primitive organisms make choices in responding to stimuli, trees integrate environmental information in deciding when to begin the process of dropping their leaves in autumn, and computers choose among yes-no alternatives when processing information. Neither free will nor human consciousness are necessary in order to make decisions; how choices are made is interesting but irrelevant to the reality of decision-making itself.
3. M. J. West Everhard, *Q. Rev. Biol.* **50**, 1 (1975).
4. R. D. Alexander, *Annu. Rev. Ecol. Syst.* **5**, 325 (19 → *Behav. Sci.* **20**, 77 (1975).
5. In his first note Konečni creates definitional chaos by treating inclusive and personal fitness as opposing quantities, whereas personal fitness is a component of inclusive fitness. → W. D. Hamilton [*J. Theor. Biol.* **7**, 1 (1964)] and West Eberhard (*3*).
6. Konečni reintroduces Emlen's argument of hormonal determinism but without evidence. I encourage readers to closely compare my report with Emlen's comment and my reply.