

Animal Cognition: Theory and Evidence

Review of *Species of Mind: The Philosophy and Biology of Cognitive Ethology* by Colin Allen and Marc Bekoff

William S. Robinson

Dept. of Philosophy & Religious Studies
402 Catt Hall
Iowa State University
Ames, IA 50011-1306
USA

wsrob@iastate.edu

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REVIEW OF: Colin Allen and Marc Bekoff (1997). *Species of Mind: The Philosophy and Biology of Cognitive Ethology*. Bradford (MIT Press). ISBN 0-262-01163-8. xii + 209pp. Price: \$US35.00; hbk.

Species of mind seeks primarily to establish the credentials of cognitive ethology as an exciting and above all respectable branch of scientific inquiry. Difficulties in achieving the aims of cognitive ethology are not downplayed, but it is argued that they can be overcome sufficiently to sustain an empirical research program. An important subgoal is the promotion of an interdisciplinary approach to the required research.

If cognitive ethology were a more mature discipline, a book with the above goals would hardly be necessary. It is partly because so many of its questions are unsettled that cognitive ethology is exposed to doubts and criticisms. One way of dealing with this situation would be to propound a single approach, method, and set of established results as a model for cognitive ethology, and argue that if the discipline is developed in the recommended way, it is respectable science. Allen and Bekoff reject this path and go out of their way to keep questions open. This way of proceeding often requires them to forego simple claims about actual advances in favor of complex, and somewhat weaker, claims that defend the empirical character or likely future productivity of various

approaches to cognitive ethology. This circumspection is on the whole commendable, although it sometimes leads to rather indecisive discussions.

An early concern in the book is whether investigation of animal mentality is compatible with naturalistic science. The authors reject narrowly reductionistic forms of naturalism, but leave the positive characterization of naturalism largely open. One point they do wish to make (p. 12) is that Darwinian continuity (of animal minds with human minds) is a more naturalistic view than the traditional Cartesian claim of discontinuity. Another early concern is to distinguish intentionality (in Brentano's sense of aboutness or representation, not the ordinary sense of purposiveness) from consciousness. The authors are concerned to show that many questions about the former can be pursued independently of views about the latter. Discussion of consciousness is deferred until relatively late in the book (chapter eight).

Chapter two is "A brief historical account of classical ethology and cognitive ethology." It provides a useful introduction to these subjects that will serve to prepare nonethologists for understanding the issues in the rest of the book.

Chapter three explores the difficulty raised by the fact that the same bodily motion may constitute different behavior in different contexts, and the same behavior may be achieved by different bodily motions on different occasions. Descriptions in terms of bodily motions may thus fail adequately to characterize behavior; but descriptions in terms of functions may draw charges of unwarranted mentalistic inflation. It is thus difficult to satisfy the desire for a way of describing activities of animals that can be used reliably by different investigators across various contexts and species. The authors "favor a pluralistic approach according to which it is an empirical question which schemes for categorizing behavior will turn out to be empirically most productive" (p. 47).

While the authors emphasize constructive suggestions for the development of cognitive ethology, their project evidently requires a certain amount of responding to critics of that enterprise. Chapter four takes on the fundamental complaints that animal mental states are unobservable and cannot be reported upon by their possessors. The authors hold that, barring extreme and implausible interpretations of "private", privacy does not entail having no effects. Thus, they argue, mental states in animals may be introducible as part of the best explanation for observable events. If this kind of introduction is to work, however, ethologists must clearly identify what needs explaining. Helpful in doing this will be a distinction between behavior that is relatively stimulus bound, and behavior that is relatively stimulus free. The authors explain this distinction, and defend its importance against a number of strategies that seek to dismiss "stimulus free" behavior as, e.g., merely bound to stimuli of nonobvious complexity, or as exhibiting chance.

In their pivotal fifth chapter, Allen and Bekoff argue for a thesis that is considerably stronger than the cautious pluralism of chapter three. Namely, they hold that eliminativism and behaviorism ignore content, and that this makes them "less suitable for the explanatory purposes of cognitive ethology than are approaches that ascribe content bearing states" (p. 69). Moreover, they hold that folk psychology is a "prototheory" of

behavior which contains terms for contentful states and generalizations of a causal-explanatory kind which "may be suitably refined and incorporated into a fully scientific theory of mind and behavior applicable to both humans and nonhumans" (p. 66).

Significant attractions of these theses for cognitive ethology are that descriptions in contentful (or, intentional) terms can be easily tied to evolutionary explanations (Millikan, 1984, is highly relevant background here), that they may permit generalizations across species, and that they are less difficult to find than either strictly behavioral or "syntactic" (in the sense of Stich, 1983) descriptions. But, of course, many will be suspicious that these benefits are illusory. Many discussions in the remainder of the book implicitly or explicitly respond to various aspects of this suspicion, particularly those raised by scientists. The focus of chapter five is on objections coming from philosophers, most prominently Stich (1983) and Dennett (1969, 1996). These writers have stressed the difficulty of determinately specifying particular content ascriptions. It is, for example, tempting to say that Fido, wagging his tail before his bowl, is expecting food. But food is material with the function of nourishment, and it is exceedingly doubtful that Fido has any such concept as "function" or "nourishment"; thus it seems that "food" cannot be an accurate term for what it is that Fido is expecting. This kind of difficulty will infect any casual attribution of intentional content to animal mental states.

Allen and Bekoff face this argument head on, making, among others, the following points. (1) For cognitive ethology to be a science, it need not achieve a level of precision that would support predictions of particular actions of individual animals. (2) The fact that accurate content ascription is difficult does not show that it is in-principle impossible. Fido may not have the concept of food, or any concept that corresponds to a single English word; but "there is still no reason to think that we cannot manipulate English so as to explain what the dog's concepts are" (p. 82). (3) Many of the difficulties put forward against content ascriptions for animals apply to adult humans (who make mistakes and have various lacunae in their knowledge) and to children (who may ask for food, for example, before having a concept of nourishment). But in these cases, we do not draw the conclusion that content ascriptions must be useless, or that they cannot lead to reasonable predictions.

Allen and Bekoff's discussion will not satisfy those who (like the present reviewer) have doubts that folk-psychological concepts are terms of a causal-explanatory theory. However, the authors' position that they are entitled to forego any attempt to allay *these* doubts is quite reasonable. If they can show that content ascription to animals is no worse off--or even not too much worse off--than content ascription to humans, they will have gone as far as can be expected of them to meet a fundamental philosophical objection to ascribing (intentional) content to states of animals. There remains, however, a large gap that must be filled by empirical work. For even if it is possible in principle for the resources of English to provide adequate descriptions of animals' concepts, it might not be practically possible for ethologists ever to find such a description. The view that (useful) intentional descriptions will be less difficult to find than behavioral descriptions might still be illusory, because it might be that any conceptual description that comes out of field work might be so inadequate as to prevent scientific advance. Whether this

problem will defeat the establishment of cognitive ethology as a science is, however, an empirical matter. Thus, the authors quite properly turn, in their next two chapters, to empirical studies.

Chapter six centers on a summary presentation of results from Bekoff's (1995) study of play bows in canids. (A play bow is a distinctive posture in which the rear stands, the forelegs "kneel" and the head is held low to the ground.) The apparent point of the summary is to invoke empirical support for the view that play bows can be legitimately assigned a meaning--namely, "I still want to play". The method of the study was, in brief, to film and analyze play episodes. Play bows were identified, as were four types of actions that occur outside of play, and that could be interpreted (by us or by the subjects) as aggressive. (For brevity, I will, unlike the authors, call these "possibly aggressive" actions.) A key question was whether play bows occurred randomly during play episodes, or whether they were especially likely to occur in connection with the possibly aggressive actions (thus helping to avoid misinterpretation that would lead to fighting instead of play). Unfortunately, the authors give such a cautious report that the answer to this question is not clear. The table that is reproduced from the paper lists the possibly aggressive actions, and shows mostly low percentages of these as being preceded or followed by play bows. Further, the results are reported very weakly: "bows may serve to provide information about other actions that follow or precede them" (p. 103). However, some relevant facts that are clear in the paper are not reported in the book's summary, to wit: of the play bows observed during the reported episodes, 88.8% occurred just before or just after one of the possibly aggressive actions in dogs, with 94.3% and 97.7% being the corresponding figures for infant wolves and infant coyotes, respectively. The preponderance of these bows occurred just before or just after the most "aggressive" of the actions--74%, 79% and 92% for dogs, infant wolves and infant coyotes, respectively.

The cited study thus does support the view that play bows during play episodes have some kind of signalling function that helps avoid degeneration of play into fighting. But this result leaves open the question of just how to characterize the "message" in play bows. For this purpose, one needs to tie the observational evidence to theories of intentionality. Toward this end, the authors provide interesting discussions of views of Rosenberg (1990), Dennett (1987), and especially Millikan (1984). There are, however, some difficulties. The authors refer to Dennett's "implausible assumption of perfect rationality" (p. 95). But, as the authors are clearly aware (p. 93), Dennett does not assume that any organism is (or even could be) perfectly rational; nor does he hold that lack of perfect rationality makes the intentional stance useless. It is thus unclear what assumption the authors wish to attribute to Dennett, and what their complaint about this assumption amounts to. (These questions surface again in Chapter 9.) After discussing Millikan's views, the authors conclude that "it is probably more correct to view [play bows] as intentional signals, a limiting case of intentional icons" (p. 108). Millikan sharply distinguishes intentional icons from beliefs--the latter but not the former involve "identification", and have participation in inference as one of their proper functions. The authors are aware of these differences; puzzlingly, however, they do refer (p. 109) to animals' beliefs about other animals' intentional states, and they offer a curious attempt (p. 97) to meld Millikan's approach with a thesis of Rosenberg's concerning higher-order

beliefs and desires involved in pretense. Readers who are suspicious of unwarranted intentionalistic inflation of behavioral descriptions will likely not be satisfied with these parts of the discussion. In fairness, however, the authors do not claim to offer a definitive conclusion about the correct analysis of the intentionality of animals' behavior, and they make a strong case that attempts to relate experimental work to theories of intentionality can be fruitful.

Chapter seven draws upon a large number of studies concerned with aspects of antipredatory behavior. Animals have a repertoire of different responses to predators, which may be called forth by different kinds of predators; and the correct and timely classification of predators depends upon vigilance behaviors. All these aspects of antipredatory behavior can be studied empirically. They involve complexities for which it is plausible to suppose that appeals to cognitive abilities are necessary, both to obtain the best explanations and to suggest hypotheses for fruitful further study. Some of the experimental results cited in this chapter suggest that the problem of which concepts to attribute to animals can be solved sufficiently to permit some solid scientific advance. The authors are, however, acutely honest about the difficulties of such attribution. They give an extended discussion of studies of effects of group size on the vigilance behavior of certain birds that raises (among other points) the question of what counts as a group, or what counts as being in a group of a certain size, from the point of view of the animals involved. Skeptics about our ability to find adequate descriptions for animals' concepts will find many points for their case here. These points, however, have emerged from a research program that is, at least, empirical; and the authors argue for the viability and fruitfulness of continuing this kind of research.

Allen and Bekoff regard the questions we have reviewed so far as pursuable (empirically and theoretically) independently of resolving the question of animal consciousness, and they consistently put off the latter question until chapter eight. In this chapter, they disclaim ability to answer Nagel's famous question as to what it is like to be a bat (or other nonhuman). They argue, however, that there can be an empirical inquiry into *whether* there is anything at all it is like to be a certain organism. Pivotal in their argument is the concept of an organism's ability to remain sensitive to an input, while discounting it, i.e., while avoiding responding to its normal meaning. An example of this ability is the separation of perception and judgment that we make when we see the lines in the Mueller-Lyer illusion as unequal in length, but judge them to be equal and treat them accordingly. The argument that is based on this ability seems to be as follows. There can be behavioral evidence that a (nonhuman, nonverbal) organism can be both subject to an illusion and able to discount it in responding. "If one takes seriously the idea that an organism can discriminate its appearance states from its judgments (beliefs) about the environment, then one is committed to the distinction between the way things appear to the organism and its beliefs about them. In our view, attributing conscious, subjective experiences may provide the best explanation for the ability of some organisms to make this distinction" (pp. 152-153).

A problem with this argument is that what perception provides to an organism can be given a purely informational description. Information can be regarded as being processed

in various ways, and in a complex information processor it can be discounted under certain circumstances. There are programs for nonmonotonic reasoning that could be described as "discounting" certain information, or no longer "taking it seriously", without merely erasing it, or making it forever irretrievable. But such programs are not convincing demonstrations of computer consciousness. Thus, we need more than what the authors give us, if we are to conclude that there is consciousness in an information-discounting organism, and not merely unconscious, but appropriately complex, processing. In fact, to get to a positive conclusion about consciousness from the authors' premises, we must *antecedently* accept that "appearance" *in its full-blooded "consciousness" sense* can legitimately be applied to what sense organs provide to animals. But the legitimacy of attributing full-blooded conscious appearances to animals is a way of describing exactly what an argument for animal consciousness is supposed to show. (By "full-blooded consciousness sense" of "appearance", I mean the sense in which, e.g., ties can appear blue (whether veridically or not) when looked at by a normal observer; there is a "what it is like" to have them appear that color. This is to be contrasted with the "bloodless" sense, in which one may say that it appears that Candidate X is going to lose. Here, there is no quality that the incipient losing presents to appearance; there is only a judgment that information in one's possession inconclusively supports the statement that X will lose.)

A further difficulty with the argument for animal consciousness is that even if conscious, subjective experiences (appearances in the full-blooded sense) were found to be *always* present in humans when they discounted perceptual information, it would by no means be clear how those experiences *explain* the ability to discount information; thus, invoking the idea of inference to the best explanation to help us support animal consciousness is suspect. The difficulties of the authors' argument for animal consciousness, however, should not obscure the significance of the first premise of that argument. *If* we presuppose (or find some other argument to show) that consciousness is an effect of all neural activities that are complex enough to do a certain job (e.g., discount information without merely becoming insensitive to it), then it is important that we can establish empirically that that job is accomplished in nonverbal animals.

Chapter nine is largely a defense of the value of field work, against the view that only laboratory studies can provide a proper approach to animal mentality. Much of the discussion takes the form of a detailed reply to Heyes and Dickinson (1990, 1995). Among the key points made are the following. (1) Field work is not all passive observation--subjects in the field can be induced to respond to experimentally contrived situations. (2) Not all intentional attributions need to take the form of beliefs; i.e., internal states may be needed that represent properties without being connected to inferences in the manner characteristic of beliefs. (3) Hypotheses about intentional states, like scientific hypotheses in general, cannot be tested in isolation; thus simple behavioral laboratory indicators are inadequate as criteria for intentional states. And (4) interpretation of laboratory controls and results can go wrong if broader considerations, available only from field work, are not taken into account.

Despite the length of this review, there are many topics that have had to be left without mention. *Species of mind* is a book that is rich in both its ethology and its philosophical ideas. As a philosopher, I found the ethology quite accessible; and I judge that most of the philosophy will be found accessible by ethologists. This is a book that is intended to open discussion rather than to close it, and it succeeds admirably in this aim. Those just coming to the subject of cognitive ethology and those who are already involved its debates will profit from this book, or pass it by at their considerable cost.

References

Bekoff, M. (1995). Play signals as punctuation: The structure of social play in canids. *Behaviour*, 132, 419-429.

Dennett, D.C. (1969). *Content and consciousness*. Routledge and Kegan Paul.

Dennett, D.C. (1987). *The intentional stance*. Bradford: MIT Press.

Dennett, D.C. (1996). *Kinds of minds*. Basic Books.

Heyes, C. & Dickinson, A. (1990). The intentionality of animal action. *Mind and Language*, 5, 87-104.

Heyes, C. & Dickinson, A. (1995). Folk psychology won't go away: Response to Allen and Bekoff. *Mind and Language*, 10, 329-332.

Millikan, R.G. (1984). *Language, thought, and other biological categories*. Bradford: MIT Press.

Rosenberg, A. (1990). Is there an evolutionary biology of play? In M. Bekoff & D. Jamieson (eds.), *Interpretation and explanation in the study of animal behavior*, vol. 1. Westview.

Stich, S. (1983). *From folk psychology to cognitive science*. Bradford: MIT Press.