On Aims and Methods of Cognitive Ethology[1]

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1. Introduction

In 1963 Niko Tinbergen published a paper, "On Aims and Methods of Ethology," dedicated to his friend Konrad Lorenz. This essay is a landmark in the development of ethology. Here Tinbergen defines ethology as "the biological study of behavior" and seeks to demonstrate the "close affinity between Ethology and the rest of Biology" (p. 411). Building on Huxley (1942), Tinbergen identifies four major problems of ethology: causation, survival value, evolution, and ontogeny. Concern with these problems, under different names (mechanism, adaptation, phylogeny, and development), has dominated the study of animal behavior during the last half century (Dawkins, et al. 1991; Dewsbury 1992).

With his emphasis on the importance of innate structures internal to animals, Tinbergen was resolutely antibehaviorist. Yet he remained hostile to the idea that ethology should employ any form of teleological reasoning or make reference to "subjective phenomena" such as "hunger" or the emotions. He wrote that teleological reasoning was "seriously hampering the progress of ethology" and that "[b]ecause subjective phenomena cannot be observed objectively in animals, it is idle to either claim or to deny their existence" (1951, p. 4).[2]

Since the 1976 publication of Donald Griffin's landmark book, *The Question of Animal Awareness*, a growing band of researchers has been attempting to study the cognitive states of nonhuman animals (for samples of this work see Bekoff & Jamieson 1990, and Ristau 1991). Although vigorous debate surrounds this research, cognitive ethology as a field has not yet been clearly delineated, adequately characterized, or sufficiently explained.

Our goal in this paper is to attempt for cognitive ethology what Tinbergen succeeded in doing for ethology: to clarify its aims and methods, to distinguish some of its varieties, and to defend the fruitfulness of the research strategies that it has spawned.

This paper is divided into five main parts. In the first part we briefly sketch the history of ethology and explain the motivation behind the cognitive turn. Next we discuss the groundbreaking work of Donald Griffin and the rise of cognitive ethology.

In the third section we distinguish two varieties of cognitive ethology ("weak" and "strong") and provide some reasons for preferring the latter to the former. The fourth part of the paper is a discussion of one area of research in cognitive ethology: social play. Finally we make some concluding remarks.

2. The Story of Animal Behavior

During the third quarter of the nineteenth century, Charles Darwin was the most important contributor to the foundations of animal behavior (Boakes 1984, Richards 1987). Darwin argued for mental continuity between humans and other animals, and claimed that "the lower animals, like man, manifestly feel pleasure and pain, happiness, and misery"(Darwin 1871, p. 448).[3] According to Darwin monkeys are capable of elaborate deceit (1896), insects can solve problems, and many animals can deliberate about what to do (1871, 1896).

Darwin's approach can be characterized as "anecdotal cognitivism." He attributed cognitive states to many animals on the basis of observation of particular cases rather than controlled experiments or manipulations. Darwin's follower, George Romanes, followed in the this tradition although he was more critical than Darwin of various cognitive attributions to nonhuman animals. Even Lloyd Morgan, mainly remembered for his canon—"in no case may we interpret an action as the outcome of the exercise of a higher psychical faculty, if it can be interpreted as the outcome of the exercise of one which stands lower in the psychological scale" (Morgan 1894, p. 53)—accepted the Darwin-Romanes view of the continuity of mental states. Indeed, as Rollin (1989) points out, Morgan's canon is not only consistent with the view that animals have mental states, it actually presupposes it.

Behaviorism arose in part as an attempt to overcome the anecdotal approach and to bring rigor to the study of behavior. Controlled experiments rather than field observations provided the primary data, and basic concepts were supposed to be grounded in direct observation. Against this background, animal consciousness came to be seen as ". . . mystical, unscientific, unnecessary, obscure, and not amenable to study"(Rollin 1989, p. 68).

Jacques Loeb, who was active from about 1890-1915, was an influential forerunner of behaviorism in biology. Although he believed that consciousness was an emergent property of higher organisms, he argued that all animal behavior could be explained nonteleologically in terms of tropisms (Pauly 1987). Throughout the 1920s, with the work of Watson and others, behaviorism became increasingly influential. By 1930 the behaviorist revolution was complete and anecdotal cognitivism had virtually vanished from mainstream science.

Classical ethology developed in Europe with the work of Lorenz and Tinbergen, and arrived in America in the post-World War II period (although as Dewsbury 1992 points out, there were contacts before the war). The roots of classical ethology were in the investigations of Darwin, Charles Otis Whitman, and Oskar Heinroth. Classical ethology signified a return to some of the ideas of Darwin and the early anecdotal cognitivists, especially in its appeals to evolutionary theory, the close association with natural history, and the reliance on anecdote and anthropomorphism in motivating more rigorous study.

Lorenz, who was trained as a physician, comparative anatomist, psychologist and philosopher, did little fieldwork but his knowledge of animal behavior was enormous. His method was to watch various animals, both domestic and wild, who lived near his

homes in Austria and Germany. He freely used anecdotes and did very little experimentation. Lorenz thought that empathy, intuition and emotion were important in understanding animals and that science should not be pursued "in the belief that it is possible to be objective by ignoring one's feelings" (Lorenz 1988/1991, p. 7). He attributed to animals such states as love, jealousy, envy, and anger.

Tinbergen complemented Lorenz's naturalistic and anecdotal approaches by doing elegant, simple and usually relatively noninvasive field experiments. Tinbergen also worked with Lorenz on several classical problems, including egg-rolling in geese.

Theoretically what was most important about Lorenz and Tinbergen was the emphasis they placed on internal states such as "instincts," "drives," "motivational impulses" and "outward flowing nervous energy." On their view behavior is typically caused by internal states; external stimuli mainly release or block behavior. This emphasis on internal states was in sharp contrast with the behaviorist tradition.

However by 1973 when Lorenz and Tinbergen were awarded the Nobel Prize (shared with Karl von Frisch), many thought that their grand theory was already in tatters (Kennedy 1992). As early as 1968 Patrick Bateson wrote that "[w]orship of the old gods and the intellectual baggage that went with it still survives quaintly in odd corners. But for the most part proponents of a Grand Theory have either been forced to close their eyes to awkward evidence or modify their ideas to the point of unfalsifiability" (p. 33). Marian Dawkins has written that "[m]ost contemporary textbooks on animal behaviour tend to dismiss 'instinct' altogether and attempt to consign it to honorable retirement" (Dawkins 1986, p. 67).

In recent years no grand theory has arisen to replace the Lorenz-Tinbergen theory of instinct. However the question of adaptation (survival value) has become increasingly central in animal behavior studies. Indeed, many researchers write as if a behavior is completely explained if it can be shown that it might contribute to inclusive fitness. This is surprising since adaptationist explanations are often radically underdetermined by empirical evidence; and when they are not, the availability of a good adaptationist story does not drive out other forms of explanation.

The Lorenz-Tinbergen theory of instinct was meant to be an account of the mechanisms of behavior. With the decline of the "grand theory" some researchers have turned to neuroethology as the replacement for the study of instinct. However, despite great advances in neuroethology, much of what we want to know about animals cannot be explained in these terms alone. If we want to know why Grete (the dog) barked at the postman, an explanation in terms of neural pathways may not be very helpful (Dennett 1987).

Like many of the animals it studies, animal behavior needs all four legs (mechanism, adaptation, phylogeny, and development). And perhaps as never before animal behavior needs to countenance a variety of forms of explanation. Cognitive ethology has the potential to make important contributions to our understanding in a number of areas, for the cognitive vocabulary can help to deliver important insights about animals that may otherwise not be available.

2. Griffin and the Rise of Cognitive Ethology

Many of the same forces that led to the development of cognitive psychology in the 1960s began to gather in animal behavior in the 1970s. Lorenz and Tinbergen had already made appeals to "unobservable" internal states respectable, and philosophers

such as Hilary Putnam (1960/1975) and Jerry Fodor (1968) had shown that material-
ism and mentalism could be made compatible. In addition, Jane Goodall and Dian
Fossey were popularizing the idea that the other African apes, including chimpanzees
and mountain gorillas (see Cavalieri & Singer in press), have rich cognitive and emo-
tional lives (Montgomery 1991).

The rise of cognitive ethology can conveniently be dated from the publication of
Donald Griffin's *The Question of Animal Awareness* (1976). In view of its historical
significance it is surprising that the expression 'cognitive ethology' occurs only twice in
the first edition of this landmark book, and then only in the last four pages. By 1978,
however, this term figured in the title of Griffin's *Behavioral and Brain Sciences* target
article. In each succeeding book (Griffin 1984, 1992) this expression has become more
frequent (on Griffin's development see Bekoff in press a, Hailman 1978).

One explanation for Griffin's apparent reluctance to use the term `cognitive etholo-
gy' is his hostility to cognitive psychology. This hostility may be surprising since, as
we have suggested, the cognitive turn in ethology can be related to similar develop-
ments in psychology. However Griffin appears to think of cognitive psychology as a
variety of behaviorism. Indeed, he claims that "conspicuously absent from most of
contemporary cognitive psychology is any serious attention to conscious thoughts or
subjective feelings"(Griffin 1984, p.11). Yet it is "conscious thoughts" and "subjective
feelings" that Griffin is most interested in exploring. Griffin writes that the challenge
of cognitive ethology "is to venture across the species boundary and try to gather satis-
factory information about what other species may think or feel"(Griffin 1984, p.12).

Griffin's picture is of a world of creatures with different subjectivities leading their
own individual lives. Trying to learn about the minds of other animals involves trying
to get "a window" on their minds (Griffin 1984, Chapter 8). Griffin seems to think
that communication offers such a window, and in his writings he focuses on the com-
munication systems of various animals.

Griffin's cognitive ethology has been attacked from several directions. Scientists,
especially those of a behaviorist persuasion, often argue that cognitive or mental con-
cepts cannot be operationally defined, thus there are no researchable questions in cog-
nitive ethology. On this view cognitive ethology should be banished from the citadel
of science and consigned to the scrapheap of idle speculation (for discussion see
Bekoff & Allen in press).

Griffin seems to be of two minds about this objection. In much of his work he
has been concerned to satisfy his critics by framing definitions. Yet he seems impa-
tient with the demand for definition and sometimes dismissive of it. In his early
work (1976, 1981) Griffin is concerned to define such terms as "conscious aware-
ness" and "mental experience." In Griffin (1982, 1984) he tries to define "mind,"
"aware," "intend," "conscious," "feeling," and "think;" but he is most concerned to
define "consciousness." Although Griffin seems to think that it is important to de-
fine these key terms, he never seems completely happy with the definitions that he
gives. In 1981 he writes that "almost any concept can be quibbled to death by exces-
sive insistence on exact operational definitions"(p.12). By 1991 he is claiming that
"it is therefore neither necessary nor advisable to become so bogged down in quib-
bles about definitions that the investigation of animal cognition and consciousness is
neglected altogether" (pp. 4-5). But despite his interest in getting on with it, even if
the central terms cannot precisely be defined, Griffin returns again and again to the
problem of definition.

In our view classical definitions cannot be given for key terms in cognitive ethology but it is not necessary to give them in order to have a viable field of research. Classical definitions preserve meaning and provide necessary and sufficient conditions for the application of a term. An area in which there is controversy is likely to be one in which the definitions of key expressions are contested. It is not only the application of cognitive terms that is contested, there are also competing definitions of such terms as 'fitness', 'recognition', communication', 'play', 'choice', 'dominance', 'altruism', and 'optimality'. With respect to mental concepts, a huge literature has developed over the years about whether or not it is part of the meaning of mental terms that what they refer to is private, introspectable, incorrigible, and so on. One result of scientific inquiry is to help fix and refine definitions. As science advances, definitions change and become more precise and entrenched. In order to get an area of inquiry going, what is needed is some common understanding of the domain to be investigated, not agreement about the meaning of key terms. Key terms in cognitive ethology are well enough understood to begin inquiry, even if classical definitions are difficult to come by.

Griffin's cognitive ethology is not sunk by the failure of definition. Yet it should be clear from this discussion that Griffin is tempted by some key assumptions of his critics. It is another assumption, one that Griffin shares with some of his critics, that is especially problematical for his version of cognitive ethology.

Griffin appears to accept a fundamentally Cartesian notion of the mind, at least with respect to its epistemological status. Although he formulates his central question in different ways, what Griffin really wants to know is whether animals are conscious. He assimilates the question of consciousness to the question of whether animals have subjective states. When the question is posed in this way, the link between mind and behavior seems highly contingent: two creatures may be in the same subjective (i.e. mental) state, but in only one does this have any objective (i.e. behavioral) consequence; two creatures may be in the same objective (i.e. behavioral) state, but in only one is the behavior caused by a subjective (i.e. mental) state. Knowledge of the minds of others is, on this view, inferential and probabilistic (Griffin 1992, p. 260). From our observations of objective states we make inferences to unobservable, subjective states. But since the connections between observable, objective states and unobservable, subjective states are weak and contingent, these inferences can be incorrect. On this view the passage from behavioral observations to the attribution of mentality is always uncertain and possibly treacherous. Nevertheless Griffin believes that many animals are conscious and he appeals to three sorts of evidence in support of his view.

The first sort of evidence can be viewed as a generalization of an argument given by Mill (1884) for the existence of other human minds. It involves noting that in my own case various forms of consciousness are associated with various behaviors, physical states and structures; and inferring that these behaviors, states, and structures are probably associated with various forms of consciousness in other creatures as well. It has often been pointed out that this argument fails in its goal of establishing the existence of other human minds; for generalizing to countless cases from my own involves a very large generalization from a very small sample (Rosenthal 1991, Part II.A.). When the analogies are weaker, as they are when drawn between humans and nonhumans, the induction is even more suspect.

Other arguments that Griffin gives involve appeals to novel or flexible behaviors. These appeals often have the rhetorical power of "gee whiz" stories. When people hear about the neat things that animals do they are often inclined to infer consciousness. But such inferences are open to the following objection. If flexible and novel behaviors can

fully be explained by reference to noncognitive states or processes whose existence is relatively uncontroversial, then it is reasonable to explain them in these noncognitive terms. In many cases such behavior can be explained in such noncognitive terms (e.g. see Galef 1990). In other cases it cannot, but Griffin's critics say that cognitive explanations are just temporary placeholders for the "real" explanations of which we are currently ignorant. Put in these terms, the dispute appears to be a standoff.

In the light of these difficulties with other forms of argument, it is not surprising that the evidence that Griffin most relies on involves communication. Just as Descartes placed a great deal of weight on the importance of language, so Griffin views communication as providing a window on other minds.

Communication can provide important evidence for various views about the nature of animal minds (see Cheney & Seyfarth 1990; Smith 1990, 1991). But this concept as it is used in the ethological literature has its problems (Philips & Austad 1990). Communication is not a transparent window that permits us to see into another "subjectivity." Thus facts about animal communication do not always provide support for views about the kinds of minds that Griffin believes that animals have.

So the objectors are right (in a way) but for the wrong reasons. They point out that the existence of Griffin-style minds in nonhuman animals is highly speculative and cannot convincingly be demonstrated by inferences from behavioral data. From this they conclude that animals do not have minds, or that if they do, they cannot systematically be studied. Instead the correct conclusion is that animals do not have Griffin-style minds, but for that matter neither do we. Our minds are closely tied to behavior and so are the minds of other animals. However our knowledge of other minds is not generally a matter of inference from behavior.

We agree with Griffin that many animals have mental states and that this belief is supported by close observations of their behavior. As we shall suggest in section four, minds that are closely tied to behavior can systematically be studied. In our view cognitive ethology is not only possible, but it is an active field of ongoing research.

In summary, Griffin's great contributions are to insist that questions about animal minds be addressed, to argue that what we say about animal minds must be continuous with our views about human minds, to bring a fully comparative perspective to bear on these questions, and to have motivated empirical research in a neglected area. However, despite his contributions and his immensely important historical role, cognitive ethology must develop more sophisticated conceptions of the mind and its relation to behavior, and develop research programs that are capable of answering some very specific questions. In the next two sections we will take some initial steps towards discharging these obligations.[4]

3. Two Concepts of Cognitive Ethology[5]

Cognitive ethology is an area that is undergoing growth and expansion. Among the different sorts of practices, two kinds of cognitive ethology can be distinguished. We will refer to them as "weak cognitive ethology" and "strong cognitive ethology," and discuss them in turn.

A. Weak Cognitive Ethology (WCE)

WCE is the most common form of cognitive ethology. WCE countenances the use of a cognitive vocabulary for the explanation of behavior, but not its description. The

following passage is a characteristic expression of WCE (although in this passage it is offered as a "definition" of cognitive psychology).

> [I]t is the study of the mental processes that result in behavior. These internal processes act on sensory input: transforming, reducing, elaborating, storing, retrieving, and combining. Because these processes are usually not directly observable, their characteristics and the information upon which they act are inferred from behavior. Hypotheses about internal events (i.e. cognitive theories) generate predictions of how environmental inputs will be transformed in the production of behavior (Yoerg 1991, p. 288).

WCE is an advance over behaviorism because it takes information processing seriously. Behaviorists typically treated organisms as "black boxes" whose internal states were irrelevant to the real job of science which involves mapping environmental inputs on to behavioral outputs. WCE pries the lid off the black box and treats its contents as important.

However the description of the contents of the black box often relies on fashionable computer metaphors. Indeed, one might say that WCE simply replaces the mechanical metaphors of the behaviorist tradition with the computer metaphors of cognitive science. It may be, as many think, that the computer metaphor marks a real advance over mechanical ones. Digital computers have impressive formal powers that old-fashioned machines that rely on gears and pulleys do not. But Griffin and others (e.g. Searle 1992) remain unimpressed. They say that something is left out even in these very sophisticated models (e.g. "consciousness," "intrinsic intentionality").

Whether or not something has been left out, there appears to be a double-standard between humans and nonhumans that is implicit in much work that is done in WCE. Nonhumans are often assimilated to computers in a way in which humans are not. But the significant border, if there is one, is not between animals and computers on the one hand and humans on the other; but between biological creatures and nonbiological entities. Both may process information but they seem importantly different. The capacity for having affective states is a feature of many biological creatures, but one that computers do not seem to share. Many biological creatures suffer pain, distress, fear, and can be happy or contented. WCE leaves out the affective states of biological organisms. Cognition may play a role in emotion, but emotional and affective states cannot simply be reduced to cognitive states.

Another weakness of WCE is that it attempts to protect the description of behavior from the cognitive vocabulary. Researchers in the tradition of WCE seem to share the behaviorist presumption that the behavior that is to be explained can and should be described in a cognitive-free language that makes reference only to bodily movements. Appeals to cognitive states enter only with attempts at explanation. We believe that a great deal of animal behavior cannot meaningfully be described without using cognitive and affective vocabularies. What distinguishes strong cognitive ethology from WCE (in part) is the willingness to deploy these vocabularies in the interpretation of behavior as well as in its explanation.

B. Strong Cognitive Ethology (SCE)

SCE underwrites a range of research programs in which both cognitive and affective vocabularies are willingly employed for purposes of interpretation and explanation. We will explain these concepts of interpretation and explanation in turn.

One important function of ethological investigation is to describe the behavior of animals. This role is not as highly prized as it was in the early days of ethology and is often dismissed as a hangover from natural history and sometimes likened to stamp-collecting. Yet any science must provide a description of its domain and it is important to know what animals do if we are to explain why they do it.

In recent animal behavior studies there has been a search for canonical descriptions that reflect the basic categories of behavior (e.g. Golani 1992; see also Purton 1978). The idea is that for any behavior it is possible to produce a description in a common vocabulary that is solely based on what is observable. Other descriptions of behavior, though they may be useful, involve "reading into the behavior" and are ultimately eliminable. This view is untenable for a number of reasons.

First, although we cannot argue the point in detail here, we believe that the search for basic nonhuman behaviors is doomed for the same reasons that the search for basic human actions is doomed. At time T1 Kelly presses the button, rings the door-bell, and displaces some molecules. Did Kelly do one thing or many things? If one thing, which thing? If many things, which thing is basic? Grete (the dog) may simultaneously engage in a play behavior, bow, bend her front legs, kick up some dust, and displace some molecules. The same questions arise about how many things Grete did and which they are. We believe that no plausible answers to these questions can be given that are independent of pragmatic factors. What an animal does and how this is conceptualized is a contextual matter.

A second reason why this approach is untenable is related to this point. In our view descriptions of behavior are intrinsically plural and multidimensional. What counts as "the best" description is relative to the questions being asked and the interests of the interrogator. It would be unfruitful and perhaps impossible to constrain all descriptions of animal behavior by a set of basic categories (Mason 1986). This point is perhaps most obvious with respect to primates. Primatologists virtually always describe the behavior of their subjects in highly abstract and functional terms. Later, often for purposes of publication, they may try to translate these descriptions into the vocabulary of bodily movements. But if primatologists were forbidden to use abstract, functional vocabularies, one wonders if they could describe the behavior of their subjects at all (Bekoff in press b). Indeed, what would be the title, or the subject for that matter, of a classic book like De Waal's *Peacemaking Among Primates*.

A third problem with this approach is that in many cases descriptions of an animal's behavior in the canonical language would deprive us of insights into the meaning of the behavior. Predator-avoidance may take many forms, and since nonhuman animals are no more infallible than human animals, such behavior may fail, or occur when no predator is within striking distance. In many cases we might be disposed to say that the animal is trying to avoid a predator, yet a description of the animal's behavior just in terms of her obervable bodily movements would not allow this insight.

Finally, an animal's behavioral repertoire is organized functionally as well as in other ways. The same bodily movements may have different meanings; and the same behavior, defined in functional terms, may involve different bodily movements. For example, the same bodily movements involved in canid play are also involved in aggression and reproduction (see section 4). And the same behavior from a functional point of view, for example predator avoidance, may involve tree climbing in one case and running in another.

For these reasons we believe that the search for canonical descriptions of animal behavior fails. This approach is rooted in the positivist dream of a value-free observation language that can be used to characterize the phenomena that covering laws are supposed to explain. Whatever the plausibility of this model for the physical sciences, it is highly implausible for ethology.

Because the attempt to describe behavior in a canonical vocabulary that reflects basic categories is unsuccessful, we favor the use of the term "interpretation" where others use the term "description." This acknowledges the fact that describing what animals do involves interpreting their behavior.

A central role of explanation is to specify why something happened. Although we cannot tell the story here, we would defend a view of explanation that is similar to our account of interpretation: explanations can be plural, noncompetitive, and occur at different levels of abstraction. In our view appeal to generalizations that involve cognitive and affective states can genuinely be explanatory.

However a word of caution is in order. We have tried to defeat a picture of ethology that leaves no room for cognitive and affective interpretations and explanations. But even if what we have said is correct, no one is compelled to employ such vocabularies. It is still open to someone to object that such vocabularies are illegitimate—neither suitable for interpretation nor explanation. The rejection of the "canonical description view" does not imply the legitimacy—much less the fruitfulness—of the SCE alternative. A second objection is weaker. It may be admitted that although cognitive and affective vocabularies can be employed legitimately in interpretation and explanation, we are not compelled to use them and indeed would do better if we did not.

With respect to the second objection, we concede that no one is driven to apply cognitive and affective vocabularies to animals on pain of logical contradiction. Quine and Skinner could write their autobiographies as narratives of their bodily movements without falling into logical inconsistency. No doubt the same would be true of Digit and Koko. But Quine's autobiography is boring: it lacks insight and inspiration. One has the feeling that much of what is important has been left out. In our view the same is true with respect to interpreting and explaining the behavior of many nonhuman animals: one can avoid cognitive and affective vocabularies, but as we will try to show in the next section, in many cases one does this on pain of giving up interesting and insightful perspectives.

With respect to the first objection, this charge most plausibly comes either from those who espouse a double standard with respect to humans and nonhumans (or languageless creatures and those with language [e.g. Carruthers 1989]), or eliminativists with respect to cognitive and affective vocabularies. We have argued elsewhere, as have many others, that a principled double standard cannot be maintained, so we will not repeat those arguments here (Bekoff & Jamieson 1991; Jamieson & Bekoff 1992). With respect to eliminativism, if it is true that cognitive and affective vocabularies will one day bite the dust, then SCE would cease to exist. But SCE is not singularly vulnerable. The elimination of cognitive and affective vocabularies would fell other scientific enterprises as well and be part of a radical revision of the way that we think about the world. It is enough here to defend SCE against those who are more modest in their claims.[6]

In this section we have distinguished two concepts of cognitive ethology, spoken in favor of one, and defended it against two objections. The heart of the case for SCE, however, rests with its fruitfulness as a conceptual guide to empirical research. In the next section we will discuss one area of research in cognitive ethology.

4. Social Play

Space does not allow us to cover the many areas of research (e.g. mate choice, habitat selection, individual recognition and discrimination, injury-feigning, assessments of dominance, foraging for food, caching food, various types of social communication, observational learning, tool use, imitation, teaching) in which cognitive ethological approaches have been useful in gaining an understanding of the behavior of animals (for examples see Griffin 1984, 1992; Mitchell & Thompson 1986; Byrne & Whiten 1988; Cheney & Seyfarth 1992; Bekoff & Jamieson 1990, 1991; Ristau 1990; Bekoff in press b). Here we will discuss only one area: social play.

Social play is a behavior that lends itself to cognitive studies, and poses a great challenge to researchers (Mitchell 1990; Bekoff & Allen 1992). In particular, the question of how mammals communicate their intention to engage in social play presupposes cognitive states, without which it would be difficult or impossible to describe the social encounter (Bekoff in press b).

The canid "play bow" is a highly stereotyped movement that seems to function to stimulate recipients to engage (or continue to engage) in social play (Bekoff 1977). When an animal performs a play bow she crouches on her forelimbs, leaves her hind legs fairly straight, and may wag her tail and bark. Such play-soliciting signals appear to transmit the message that "what follows is play." Play-soliciting signals are used to communicate to others that actions such as biting, biting and shaking of the head from side-to-side, and mounting are to be taken as play and not as aggressive, predatory, or reproductive behavior.

Play-soliciting signals appear to foster cooperation between players so that each responds to the other in a way consistent with play and different from the responses that the same actions would elicit in other contexts (Bekoff 1975). This cooperation may occur because each of the participants has a belief about the intentions of the other animals who are involved in the social encounter. For example, in coyotes the response to a threat gesture is very different if it is immediately preceeded by a play signal or if a play signal is performed at the beginning of the interaction (Bekoff 1975). The play signal can be viewed as altering the meaning of a threat signal by establishing (or maintaining) a "play mood." When a play signaler bites or mounts the recipient of a play signal, the recipient is not disposed to injure or to mate with the signaler.

It is difficult to describe canid play behavior without using a cognitive vocabulary. One and the same bodily movement can be aggression or play. The difference between a movement that is aggressive and one that is playful is naturally described in terms of one animal's intention and another animal's appreciation of the intention.

Similarly the cognitive vocabulary appears to provide the resources for explaining some play behavior. For example, suppose that we want to know why Grete permitted Jethro to nip at her ears. One explanation may be that Grete believes that Jethro is playing. This gives rise to further questions, such as whether Jethro believes that Grete believes that Jethro is playing. One of the challenges of research in cognitive ethology is to investigate the extent to which such questions are well-formed and what the possible answers to them might be.

In this section we have been able to provide only a brief summary of some questions about social play. Because of the brevity of this account, we have not been able to discuss behaviors in which the affective vocabulary gains a foothold. Nor did we

discuss what might be reasonable empirical constraints on cognitive interpretations and explanations.

It is important to remember that we are pluralists with respect to both explanation and interpretation. Cognitive explanations do not exclude other causal ones, nor do they rule out explanations that are adaptationist, phylogenetic, or developmental. In our view we need to employ a large range of conceptual resources in order to understand behavior.

5. Concluding Remarks

We have argued that cognitive ethology can be defended against its critics. In addition, we have discussed some of its varieties and forms and briefly sketched one area of research in cognitive ethology. Before closing, it is worth mentioning what cognitive ethology can contribute to cognitive studies generally.

Cognitive ethology can help to broaden the perspective of cognitive studies in two ways. First, cognitive ethology can help to situate the study of cognition in an evolutionary framework. It should be a necessary condition for postulating a cognitive state in a human that the existence of this state is at least consistent with evolutionary history. Although lip service is sometimes given to this constraint, talk of evolution in cognitive science is too often metaphorical. Cognitive ethology has the potential to make cognitive science take evolution seriously. Second, the fact that cognitive ethology is fully comparative can help to make cognitive science less parochial. Although there has been a great deal of concern about parochialism with respect to nonbiological systems, this concern has often coexisted with a surprising degree of "chimpocentrism" (Beck 1982). Many people are more willing to countenance cognition in computers or space aliens than in rodents, amphibians, or insects. Even in cognitive studies there is a tendency to view cognition as "essential" to humans and instantiated in various (lesser) degrees only in those who are phylogenetically close to humans. With its view of cognition as a strategic evolutionary response to problems that might have been faced by a variety of diverse organisms, cognitive ethology can help to overcome this form of parochialism.

There is no question but that the issue of animal minds is difficult and complex. Like questions about the human mind, it is tangled in issues of definition, conception, relation to behavior and so on. Yet in our view cognitive ethology is here to stay. For the adoption of cognitive and affective vocabularies by ethologists opens up a range of explanations, predictions, and generalizations that would not otherwise be available. As long as there are animals to behave and humans to wonder why, cognitive interpretations and explanations will be offered. In our view this is not only permissible, it is often enlightening. Sometimes it is even science.

Notes

[1]We are grateful to all those who participated in discussions of this material at the University of Wyoming and the 1992 Philosophy of Science Association meetings. We especially thank Colin Allen, Marc Hauser, David Resnik and Carolyn Ristau

[2]However Tinbergen seems to suggest only a page later that "the study of subjective phenomena" is "consistent in the application of its own methods" but that this study should be kept distinct from the study of causation (1951, p. 5).

[3]However there is a passage in Darwin (1871, Chapter 2) where he seems to suggest discontinuity between humans and other animals. Humans are dominant, according to Darwin, because of language, and language in part depends on human intellectual faculties. This suggests that discontinuities in power between humans and other animals may reflect discontinuities in intellect.

[4]There is an important strand in Griffin's work that we have not addressed: He wants to understand creatures from "the inside out;" he wants to know what it is like to be a bat (for example), and he assumes (following Nagel 1974) that such knowledge does not consist in knowing some set of "objective" facts about bats (for a contrary view see Akins 1990). If Griffin is right in supposing that such radical subjectivity exists, cognitive ethology as we understand it will not deliver a deep appreciation of it. Griffin's concerns about radical subjectivity may be of profound importance, but they go beyond the boundaries of science as it is currently understood.

[5]In what follows we make several simplifying assumptions including these: first, that cognitive ethology is directed towards explaining behavior rather than cognitive competencies; second, that for many organisms in many cases intentional interpretations and explanations count as cognitive ones; and third, that information processing in many organisms counts as cognitive activity. All of these assumptions warrant further discussion.

[6]As suggested in the text, the existence of a cognitive vocabulary is a necessary condition for the persistence of cognitive ethology. However cognitive ethology is not committed to "folk psychology." Cognitive ethology is committed to the view that the behavior of nonhuman animals can usefully be interpreted or explained in ways consistent with our best understanding of cognitive states, whether these involve folk psychological concepts or not. If our best understanding of cognitive states involves some alternative to folk psychology, then cognitive ethology should embrace the alternative.

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