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Is anyone a cognitive ethologist?

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Everybody's got something to hide except me and my monkey[2]

Other contributions to this special issue have covered a lot of ground, including topics such as language, tool use, morality, conflict resolution, and culture in nonhuman animals, how to study animal minds, and how to avoid being guilty of the wrong kinds of anthropomorphism. They reflect a 'sea change' in attitudes towards thinking of animals as cognitively complex individuals whose mental capacities and biological heritage are continuous with those of humans.

Many philosophers are interested in these developments, and the term 'cognitive ethology' has become familiar to them. But philosophers' uses of this term show little respect for traditional differences among different approaches to the scientific study of animal minds. Over the past couple of years, I've encountered an increasing number of philosophers using 'cognitive ethology' to refer to the study of animal cognition generally. (See, e.g., Kornblith 2002, p. 28, fn. 1.) Perhaps this is as it should be, for part of the sea change that I just mentioned is an increasing willingness of scientists trained in a variety of theoretical perspectives to take seriously questions about animal minds (see, e.g., the diverse contributions to Bekoff et al. 2002). It might also be argued that the old distinctions between animal learning theory, comparative psychology and ethology are increasingly irrelevant to modern animal behavior scientists whose interests in animal cognition may lead them to combine approaches from different historical traditions (e.g., Balda et al. 1998; Shettleworth 2001). Why not call all of this activity cognitive ethology? But if everyone's a cognitive ethologist, then the term has ceased to mark some important distinctions in the study of animal cognition.

Despite the willingness of philosophers to apply 'cognitive ethology' generically to scientific studies of animal cognition, many of the scientists actually doing the work are reluctant to accept the label. One common reason for this is displayed by Alan Kamil in his essay 'On the proper definition of cognitive ethology' (Kamil 1998). Kamil laments the association of the term with

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[2]Like all the other section headings, this too is the title of a song recorded by the Beatles. If you sing all five of them you win.

apparently anecdotal approaches to allegedly intractable problems. The principal target of such complaints is Donald Griffin, who originated the term 'cognitive ethology' approximately 25 years ago. In a series of books he has urged scientists to consider questions of conscious awareness in animals. Resistance to Griffin's call has deep causes in the history of studies of the animal mind. I cannot hope to do full justice to all of that history here – any attempt within the constraints of this essay to give an historical account of how we got to this point in the scientific study of animal behavior is doomed to miss important details. There is a complicated story to be told about the relationship between Darwinism, 'American' behaviorism and comparative psychology, and 'European' ethology, that spans at least three continents and now spans three centuries from the 19th to the 21st. It is a history that contains many acrimonious disputes, often based on misunderstandings and mischaracterizations of opponents, sometimes due to ignorance and sometimes due to mistrust of the political motives of other scientists. Nevertheless I shall attempt to show in this paper that there are important issues which emerge from considering the distinctions among different approaches to studying animal cognition. In other words, I will attempt to convince you that this is no mere terminological exercise.

More than this, however, I also want to lay the groundwork for a proper epistemology of cognitive ethology – one which accommodates the ethologists' belief that observing animals in complex social and ecological situations provides a genuine source of knowledge about animal behavior. This proper epistemology will depend on having a grasp of the actual nature of cognitive ethology. Perhaps the simplest characterization of cognitive ethology is that it is the marriage of ethology and cognitive science. But most simple characterizations of any marriage should never be trusted, and this one masks some potential incompatibilities between the two partners. Ethology has typically been concerned with observational and experimental studies of animals behaving 'naturally' while cognitive science (especially cognitive psychology) favors experimental studies under controlled laboratory conditions. The tension between these approaches is implicit in the work of the three distinguished scientists who spoke at the University of Cincinnati's Taft/Philosophy colloquium, a meeting that formed the nucleus for this special issue of *Biology and Philosophy*. All three are appropriately regarded as 'liberal' when it comes to attributing mental capacities to nonhuman animals, but their work bears different relationships to Griffin's program, and to the classical ethology of Lorenz and Tinbergen. As in comparative biology itself, their differences may turn out to be as interesting as their similarities.

Marc Bekoff is primarily a field biologist whose predominant research technique involves quantifying the relationships between behavior and context derived by observing animals behaving spontaneously in relatively natural environments. His comparative studies of differences in play-related behaviors in various canid species are classic examples of ethological work. Frans de Waal has worked primarily with captive animals living in the semi-naturalistic conditions that are found in relatively enlightened zoos and primate research center. Like Bekoff his primary research technique is to observe and count the behaviors that emerge from spontaneous interactions among his subjects. Because he studies our nearest relatives, his investigations of chimpanzee and bonobo social behavior have captured wide attention for what they might tell us about human origins. (He was unable to provide a paper for this issue of *Biology and Philosophy*, but de Waal 1991 provides a useful overview of the approach.) In contrast to the other two, Sue Savage-Rumbaugh's research involves intensive interaction with captive animals, including extensive training of her research subjects. As she makes clear at the end of her paper, Kanzi and Panbanisha are 'enculturated apes' with unique capacities derived from a *Pan/Homo* cultural context. Their cognitive abilities are observed in daily interactions, but also probed in experimental situations very much like those used by cognitive and developmental psychologists. The latter methods are relatively distant from the observational studies conducted by ethologists. However, like de Waal, Savage-Rumbaugh's pioneering studies on language capacities in bonobos are of great interest to those interested in understanding the evolution of human cognitive capacities because of the close phylogenetic relationship between the two species.

As well as pursuing different kinds of studies, our three scientific specimens have different relationships to the label 'cognitive ethologist'.

Marc Bekoff started off by telling us that he is a cognitive ethologist (in his paper, he says 'I think I am'). Bekoff is indeed an ethologist, and although I've often caught him dozing off at meetings, he's usually cognitive, so at least in that sense he's a cognitive ethologist! Joking aside, in his paper he advocates an ethological approach to animal cognition that he calls 'biocentric anthropomorphism' which maintains that while we cannot do anything other than use our own terms to describe the mental states of animals, we must strive to do so in ways that reflect the animals' own perspectives. This approach leads him to conduct experiments such as his recent 'yellow snow' study (Bekoff 2001) to understand what dogs know about their own and others' urine. As Bekoff also demonstrates in his paper – picking up on themes in his recent books – he's not afraid to affirm the importance of trying to study topics such as emotions and consciousness in animals – topics that might be considered 'hard' by philosophers steeped in the mind–body problem, but considered 'soft' by scientists who favor definite conclusions based on repeatable measures. When Griffin first raised the question of animal awareness, such topics were widely regarded as beyond the pale of respectable scientific research. Griffin recounted numerous examples of apparently intelligent behavior in animals in an effort to urge scientists to stop looking the other way. Marc Bekoff certainly can't be accused of looking the other way, and he certainly makes ample reference to Griffin's work, but he is also somewhat critical of Griffin's views on the nature of consciousness and the centrality of questions about consciousness to cognitive ethology (see Allen and Bekoff 1997). But (as indicated by Kamil's dissenting view) it is by no means clear that an affinity to Griffin's objectives is the proper definition of cognitive ethology. One also might question whether experimental biocentric anthropomorphism yields the kinds of theories and specific models of cognition that are characteristic of cognitive science more generally. Neither is it the case that declaring yourself to be something, makes you one. So let us defer judgment on Bekoff's self-declaration.

What about Frans de Waal? Well, I have a feverish recollection of his response when I asked him the question a couple of years ago when we were both on the panel in a symposium session at a meeting of the Eastern Division of the American Philosophical Association, where I had the 'flu. I recall that there was a moment's hesitation before he said that he accepted the description of being a cognitive ethologist. That hesitation is revealing when placed alongside the fact that the term 'cognitive ethology' does not appear even once in his recent book *The Ape and the Sushi Master* (de Waal 2001), and Griffin rates neither a mention nor a citation. In the book, de Waal explicitly describes his links to the ethological tradition of Konrad Lorenz and Niko Tinbergen. He also explicitly takes on the mantle of sociobiology, a label that has been quite unpopular since Gould and Lewontin published their 'spandrels' paper with its famous critique of sociobiology and adaptationist thinking more generally (Gould and Lewontin 1979). So, taking on the similarly unpopular label of 'cognitive ethologist' would not be out of character, making it all the more noteworthy that he does not spontaneously do so.

And Sue Savage-Rumbaugh? Before the Cincinnati symposium I'd never before had the opportunity to ask her the question. I had suspected that her answer would be 'no', and although she revealed that she had been trained originally as an ethologist, her current research methods are not those of a classically trained ethologist. If employing an ethological approach is a necessary condition for being a cognitive ethologist, then Sue Savage-Rumbaugh is not in the club. Nevertheless, in common with those who declare themselves to be cognitive ethologists, issues of mind and anthropomorphism are at the forefront of Savage-Rumbaugh's work, and she is evidently sympathetic to cognitive ethology (having, for example, contributed a cover blurb to the book Bekoff and I wrote together, *Species of Mind*: *The philosophy and biology of cognitive ethology* (Allen and Bekoff 1997)).

The three scientists described above, who presented their work at the Taft colloquium, thus represent contrasting yet overlapping approaches to the study of animal cognition. They do not, of course, exhaust the range of approaches. Absent, for instance, from the conference were those coming from a traditional 'behavioristic' animal learning background, representing what I'll call *neobehaviorist* approaches. Cognitively-oriented neobehaviorists no longer reject all talk of animal minds, but continue to believe that the animal learning laboratory is where scientific progress can best be made towards understanding topics such as intentions (Heyes and Dickinson 1990) and thinking (Cook 2002; Timberlake 2002; Wasserman 2002) in animals. Consciousness remains beyond the pale for such scientists. Many of the psychologists working in this tradition also tend to be quite dismissive of the power of observational methods to reveal anything scientifically reliable about the mental causes of behavior in nonhuman animals (e.g. Heyes 1987; see Bekoff and Allen 1997).

It is not my intention in this paper to engage in arguments about which is the 'right' way to study animal cognition, or whether one approach is more 'scientific' than the other. That such disputes still go on should not be doubted, however. For example, one has only to read the commentaries published alongside Rendell and Whitehead's (2001) ethological study of culture in whales and dolphins to see the charge of 'pseudoscience' as well as numerous less inflammatory but similarly scathing criticisms. Particularly controversial is Rendell and Whitehead's acceptance of Guinet and Bouvier's (1995) observational reports that they take to indicate active teaching of hunting techniques to young killer whales by their mothers. Likewise, de Waal (2001) describes Guinet's observations as 'perhaps the strongest evidence for teaching' among nonhuman animals. Pseudoscience or strong evidence? At issue is whether to trust the interpretations provided by long-time observers of the animals. I shall return to this issue below, but for the purposes of this paper let us stipulate that different approaches to the study of animal behavior and cognition all have their advantages and disadvantages, and that there is no perfect set of methods for studying the cognitive capacities of nonhuman animals. Nevertheless, and this is one point of this paper, if philosophers, and other interested parties who are not themselves behavioral scientists, are to properly assess the claims that are made about animal cognition, they cannot do so without understanding the distinctions and relationships between observational and experimental approaches to studying animal behavior. We cannot build a proper epistemological account of cognitive ethology without taking into account its roots in ethology.

All you need is love

A number of years ago, Dale Jamieson and I visited the distinguished Dutch ethologist Adriaan Kortlandt at his home in Oxford. He told us that ethology owes its existence to the ready availability of cheap field glasses after the first world war. Thus equipped, thousands of dedicated amateur bird watchers were able to observe behavior more closely than ever before. Konrad Lorenz, in his book *The Foundations of Ethology* (Lorenz 1981), echoes the theme of the importance of amateur ornithologists, writing that 'it is no accident that so many of the fundamental discoveries in ethology were made within the zoological class of birds' (1981, p. 47). Far from regarding amateurs as amateur*ish*, Lorenz maintains that ethology requires love for animals – a theme that is echoed by both de Waal and Bekoff in their recent books. As Lorenz puts it, only those who love animals are willing and able to endure the 'simply prodigious amount of time, spent in presuppositionless observation' that is a necessary basis for understanding animals.

Nowadays, sophisticated as we are about the theory-laden nature of observation, we may smile at the phrase 'presuppositionless observation' although Lorenz certainly thought this notion could be justified epistemologically (Brigandt 2003; also, see section 4 below). Setting this issue aside, there is nevertheless a serious claim worth considering here about the role played in ethology of 'just watching' animals, outside the context of any experiment. One of the first tasks of an ethologist is the construction of an 'ethogram' – a catalog of behavioral elements that is characteristic of the members of a species, and applicable with a high degree of intersubjective reliability by other experienced observers. The elements of the ethogram may only be discernable after a considerable amount of time spent watching animals behaving freely. In stressing the importance of observation under free conditions, ethology is quite different from experimental psychology which reaches its apotheosis in the Skinner box. All the 'observations' that are made in a Skinner box – lever presses, intervals between stimulus presentation and response, etc. – can be recorded and tabulated by machine. Once the equipment has been set up, the experimenter may not, in fact, have to watch the animals at all. Modern comparative psychologists and cognitive psychologists preserve this tradition of separating researcher from subject in the experimental situation, even though they have abandoned the traditional behaviorist's opposition to inner causes.

Why might loving animals enough to watch them intensively matter? A suggestion is provided by Darwin when he writes: 'It is a significant fact, that the more the habits of any particular animal are studied by a naturalist, the more he attributes to reason, and the less to unlearnt instincts.' Darwin 1871 (1936 p. 453)) Darwin's view, then, is that extensive animal watching tends to lead to a cognitive account of the animal's behavior, in terms of learning and reasoning. Darwin's suggestion is borne out by some of the chapters in *The Cognitive Animal* Bekoff et al. 2002) where one finds ethologists giving cognitive interpretations of behavior in species as diverse as antelope, jumping spiders, hyenas, ground squirrels, dogs, snakes, earthworms (Darwin himself!), and prairie dogs – species that are well beneath the radar screen of most philosophers, who have been rather fixated on the behavior of so-called 'higher' animals, especially chimpanzees. If being a cognitive ethologist simply means being an ethologist who accepts cognitive accounts of animal behavior, then perhaps every ethologist worthy of the title would become a cognitive ethologist simply by doing what ethologists do.

No doubt many would dismiss talk of cognition in spiders or earthworms as arrant nonsense. Darwin's own willingness to accept cognitive accounts of animal behavior in a wide variety of species is, of course, rather notorious for its reliance on anecdotes. One of my favorite examples concerns the writings of myrmecologist Pierre Huber, who is cited approvingly by Darwin as having observed ants playing by 'chasing and pretending to bite each other like so many puppies' (Darwin 1871 (1936 p. 448)). Darwin uses this to stress mental continuity between humans and other animals. But we, of course, are inclined to be highly skeptical that ants could engage in anything as cognitively sophisticated as pretense, and therefore we think that Darwin is too ready to accept Huber's 'anecdotal' reports without experimental confirmation. Yet, who are we to second guess the expert Huber when we have not watched ants as thoroughly as he? Clearly Huber's extensive experience in watching ants puts him in a position to know things about ants that the rest of us do not know. Might it not also have put him in a position literally to see things about their intentions that we cannot see?

I ask these questions rhetorically not because I think we ought necessarily to be convinced by Huber's reports of pretense in ants, but because they help us to focus on the role of the expert observer's testimony in the science of animal behavior, and, in particular, on the role of experience of the human observer in the process of understanding animal behavior. Frans de Waal describes his frustration at the many scientists who are skeptical of findings about primate behavior, and he criticizes many of the leading skeptics for relying upon armchair speculations about possible causes to undermine the observational reports of scientists who have spent much time watching the animals. But aren't such scientists simply pointing out, quite legitimately, the availability of alternative hypotheses? De Waal thinks not, because anyone who knows the animals would realize that the suggestions were untenable. Naming a culprit, he writes (p. 61): 'What makes critics such as Heyes unfathomable to me is their total absence of humility when faced with a group of animals they have never worked with.' Here again we see a presupposition in favor of the interpretations of the expert observer, the ethologist who has spent the time watching the animals.

The crux of the matter is, of course, whether the judgments of patient, 'loving' observers are reliable. In the eyes of many skeptics they are suspect because love is equated with sentimentality, from which it is a short step to Disney-style anthropomorphism. Indeed, the confessions of the increasingly skeptical primatologist Danny Povinelli are explicit on his own past susceptibility to the Disneyfication of animals: 'My earliest impressions of chimpanzees were, to put it mildly, rather absurd,' he writes in the introduction to his book Povinelli 2000, p. ix), and he goes on to blame Disney and National Geographic for this. Although Povinelli has not given up on cognitive attributions to chimpanzees generally (or so he claims), he now rejects what seem to many expert chimpanzee observers to be perfectly defensible claims about chimpanzees' understanding of what others see, and about their understanding of tool use. Povinelli argues that he has been driven to this deflationary view of chimpanzee capacities on the basis of a series of rigorous laboratory experiments that favor an explanation of chimpanzee abilities in terms of sophisticated associative learning mechanisms, acquired very rapidly through trial and error learning on a case-by-case basis. According to Povinelli, the resulting knowledge is narrowly tied to specific cues in the stimulus situations and does not involve any genuine understanding of the causal principles underlying tools or other minds.

Povinelli puts great store in his cleverly designed experiments, which do indeed seem to show that *his* chimpanzee subjects do not explicitly represent certain kinds of abstract information about tools or vision (or, at least, they do not exploit such representations in his experiments). Hence these animals are prone to respond to novel situations in ways that are ineffective for accomplishing certain obvious goals that we would attribute to them (such as the goal of obtaining a banana). The trouble is that the particular rearing and learning histories of his chimpanzees are so different from those of other labs, not to mention wild groups, and the particular experimental situations are so far removed from normal conditions for chimpanzees, it is impossible to say how far Povinelli's results should be generalized to all chimpanzees Allen 2002).

Greater faith in experiments than in free observation is practically a hallmark of the difference between psychologists and ethologists. But in a very real sense, all animals maintained in laboratory research settings are artifacts. Although the traditional apparatus of conditioning experiments has great power to shape animal behavior, there are severe limitations in that power. This point is nicely illustrated in the following story from Daniel Lehrman, a comparative psychologist who nonetheless was skeptical of strictly behavioristic approaches. He writes:

> I vividly remember the occasion, some 15 or 20 years ago, when I first visited a major operant conditioning laboratory (which, as it happened, was the archetypal one). My host showed me a pigeon in a chamber, and a button which I could press to present the pigeon with a reinforcement which, to my untutored eye, looked like a piece of ordinary pigeon food, but which I was assured had magical properties. Following instructions, I spent a happy hour teaching the pigeon to turn around in a circle and then stand for 2 s with its side toward the food dispenser, before looking in the dispenser for the food. Suddenly, I was visited by a dazzling revelation. If *any* behavior could be shaped up in this chamber, perhaps I could teach a domestic pigeon to perform the courtship bow of a ring-dove, which is quite different from that of the domestic pigeon; perhaps I could teach the bird to court when it was immature, or not in the breeding condition; perhaps I could alter the frequency of occurrence of bowing and other instinctive behavior patterns, or cause them to be performed in other than the natural situation, and omitted in the natural situation. In short, I could use the operant conditioning technique to elucidate the origin and internal organization of instinctive behavior patterns! Eureka! Oh, wow!
>
> I explained these plans to my host, who quickly disillusioned me by saying, 'Well, I don't think that will work. We've tried that kind of thing

a little, and this technique doesn't work too well with what you might call 'bird behavior'.' (Lehrman 1971, pp. 467–468).

I was led to the paper from which this quote is taken by reading ethologist Colin Beer's (1975) eulogy to Lehrman, which was titled 'Was Professor Lehrman an ethologist?' Beer describes the suspicion with which Lehrman, as an American trained experimental psychologist, was regarded by European ethologists, until, at a conference, the ethologists discovered that Lehrman shared their passion for ornithology. Here was an experimental psychologist who actually liked watching animals! As Lehrman's anecdote reveals, he was profoundly skeptical of what could be learned about animals that are simply cogs in the experimentalist's machinery, insisting instead on the importance of a 'natural history orientation' for doing his kind of experimental psychology. In pursuing his question of whether Lehrman was an ethologist, Beer writes (1975, p. 959): 'The best ethological work, it seems to me, has a quality that is emergent from a combination of profound curiosity about, refined perception of, and exquisite feeling for the patterns of behavior shown by different kinds of animals in nature.' Beer ultimately concludes that the differences between Lehrman and the ethologists were significant, 'but in one respect at least Lehrman and Lorenz were of one kind: they could both 'spontaneously describe their attitude to their subjects in terms of love'. Beer 1975, p. 964; Beer is quoting from Lehrman 1971, p. 470.)

Love is not all you need, but it does make possible the kind of patient animal watching that ethologists believe can lead to genuine expertise, knowledge, and understanding of animal behavior.

Too much monkey business

I have been emphasizing the role of free (non-experimental) observation in animal cognition studies. From an experimentalist's point of view it might be quite reasonable to acknowledge the importance that such observation plays, but ultimately to regard it as subservient to experimental methods. Such an attitude might even be fostered by a reading of the sequence of events leading up to the development of Gordon Gallup's famous mirror test of self recognition in chimpanzees.

Gallup (2002) describes his early investigations of chimpanzees' ability to recognize themselves in mirrors. His initial approach was to present isolated naive chimpanzees with mirrors outside their cages and observe and count their responses over a 10-day period. He writes that 'The transition from social to self-oriented responding gave the impression that the chimpanzees had learned to recognize themselves' (2002, p. 325). But being unwilling to stop there, Gallup invented the mark test, which measures the response of the chimpanzee to a mark placed on the animal's forehead while under an anesthetic. Gallup was able to show that after recovering from the anesthetic a chimpanzee was much more likely to touch the mark when a mirror was present, if it had had prior experience with mirrors. In light of this sequence, it is tempting to see the role of the initial, relatively free observations described in Gallup's first experiment as merely suggestive – giving an 'impression' as Gallup puts it – with the epistemological heavy lifting being done by the mirror test itself. I think this account of the contribution of the initial observations to our understanding of chimpanzees is a mistake.

To see this, first let us look at ways in which the mirror test has been used by comparative psychologists studying a variety of species. The mirror test used an objective criterion for measuring the response (touching the mark) and offered a procedure that could be replicated across a variety of species. Consequently, it has spawned a substantial cottage industry among primatologists. According to the 'industry newsletter', chimpanzees get it, orang utans get it, and maybe educated gorillas get it, but no matter how long you wait, monkeys just don't get it. (Some numbers, as reported by Shumaker and Swartz (2002): 5 of 6 tested orang utans 'passed'; 6 of 23 gorillas; and 73 of 163 chimpanzees.)

Passing the mirror test is a trophy that many researchers would like to capture for their favorite species. But despite its apparent virtues as a comparative test, the mirror test paradigm reveals the limitations of experimental approaches to animal cognition in diverse species. First, the test may be not be fair to species that pay limited attention to faces, let alone to visual changes in appearance that are of questionable biological significance. (Imagine a hippopotamus or a pig caring about an odd mark on its body.) Second, gorillas and many monkeys avoid eye contact except during aggressive bouts, so the natural inclination to avert eyes may be hard for members of these species to overcome when confronted with their own mirror images. Furthermore, for many non-primate species the mark-touching criterion is inappropriate because of anatomical differences that make touching the mark impossible.

These problems have led researchers working with species other than chimpanzees to modify Gallup's original protocol. Working with cotton top tamarins, Hauser et al. (1995) dyed their prominent white hair tufts with dayglo colors, and claimed to record more touching of the dyed tufts in the presence of a mirror. These results have been strongly contested by Gallup and others, and Hauser himself was unable to replicate them in a second study Hauser et al. 2001). Hauser (in conversation) notes that not all chimpanzees 'pass' the test either, so this negative result could be a sampling effect. Working with dolphins, Reiss and Marino (2001) used marks on various parts of the dolphins' bodies, and used a visual inspection criterion. Interpretation of the 'looking at' seems quite compelling to these researchers, but it is clearly more open to interpretation than the direct touches of the mark counted by Gallup. Working with a gorilla, Shumaker and Swartz (2002) first trained the animal to associate touching a visible mark with a food reward. They found that in the presence of the mirror during testing, a mark on the face did not elicit a reponse, but a mark on the chest in a position that could only be seen in the mirror elicited self-directed touching. This approach might be taken to be similar to a widely criticized study done in Skinner's lab (Epstein et al. 1981) which used operant conditioning to get pigeons to peck at a mark on their own bodies that could only be seen in a mirror. But because of the involvement of the mirror during the actual training process, the Epstein study is not generally taken to support the attribution of self-recognition to the pigeons. Shumaker and Swartz did not use the mirror during training, and while they regard their result with a single subject as merely preliminary, they suggest that it points to motivational differences between chimpanzees and gorillas.

The proper cognitive interpretation of Gallup's original experiment is quite controversial (see Gallup 2002 for references). From a strict inferential perspective, each of these modifications of the original protocol introduces further uncertainty about the conclusion that members of these species are capable of recognizing themselves in mirrors. Yet this uncertainty is almost entirely expressed by those who haven't spent time watching these animals. In particular, in many cases, the researchers have ample prior experience watching their subjects interacting freely with mirrors. My suggestion is that this prior experience is a significant piece of the puzzle of explaining the different attitudes that skeptics and proponents have to the same experimental evidence. To put the point another way, my suggestion is that Gallup's original mirror test confirmed what he already strongly suspected and perhaps even already knew about chimpanzees, having watched them interacting with mirrors. It is typically only in the absence of such experience that skepticism about the cognitive interpretation of the behavior takes root.

If this suggestion is correct, then free observations are not just heuristically useful for the development of the 'critical' experiment. As the considerable controversy shows, the experiment itself remains open to variable interpretation. Rather, the free observations might be better regarded as an integral part of the case for a cognitive interpretation. The terms of the scientific debate among experimental psychologists leads, however, to the epistemological role played by free observation being downplayed. But with respect to the nonhuman animals they love to watch, the scientists represented at the Taft colloquium are far more convinced of the appropriateness of attributing relatively complex mental abilities by what they have seen these animals do outside any particular experiment, than they are by particular experiments done to satisfy the canons of scientific publishing. Thus, for instance, speaking off the cuff during his Taft lecture, and without any prompting from me, Frans de Waal, while describing his work on conflict resolution, said: 'Experiments on primates showed—well we knew it from the start—that …' In other words, the experiments only confirmed what he and his collaborators already knew.

I am the walrus

Given that ethologists frequently observe animals under conditions in which good experiments are very difficult if not impossible to design, the task of developing an epistemology of observation is especially important.

Konrad Lorenz justified the role of basic observation in ethology by appealing to Gestalt psychology. On his view, a coherent perception or understanding of what an animal is up to emerges from masses of data taken in while watching animals under unconstrained conditions. This process is not 'a rational induction', according to Lorenz, and it can take years or decades of 'unconscious accumulation of data' before the emergence of a Gestalt, 'often coming completely unexpectedly and like a revelation, but full of the power to convince.' (Lorenz 1981, p. 45) Brigandt (2003) argues that for Lorenz 'the ideas about Gestalt perception are intended to show that there is an important and cognitive mechanism that is able to get knowledge out of what has been observed' and thereby to justify 'the way of performing observations peculiar to classical ethology.' Regardless of whether the appeal to Gestalt psychology is a particularly useful way to think about the way in which basic observations are processed, it is hardly controversial to suggest that unconscious processes can lead to scientifically important revelations. Nor is this distinctive to ethology, for there are many anecdotes of scientists experiencing revelatory moments after extensive study of physical or biological phenomena. Such experiences are just as likely to occur among devotees of the Skinner box as among ethologists, although the content of the 'revelation' is likely to be markedly different in each case.

Lorenz also stresses the importance of basic observation as a prerequisite for good experimental design, and expresses regret that 'a very large proportion of the younger researchers who consider themselves ethologists show a deplorable lack of knowledge of animals' (1981, p. 53). But what is of particular interest for ethology is the *kind* of understanding of animal behavior that emerges from extensive observation. Lorenz's own example of emergent understanding involves Karl von Frisch, with whom he and Tinbergen shared the Nobel prize in 1970. Lorenz attributes von Frisch's discovery of 'the amazing 'computing apparatus' that is capable or calculating the position of the sun by using the plane of polarization of the light coming from a clear sky' Lorenz 1981, p.46) to a Gestalt experience that came about only after decades of observing bee behavior. The idea that bees are computing complex functions is indeed one that is likely to strike the non-expert as far-fetched, but this idea has been borne out experimentally.

Despite Lorenz's enthusiasm for Gestalt explanations of ethologists' insights into animal behavior, I'm inclined to think that a rather different mechanism is at work with respect to his own understanding of the animals whose lives he surrounded himself with. In his popular book, *King Solomon's Ring* Lorenz 1952), he describes giving free run of his house to various kinds of animals. Of course, giving these animals free run of the house is hard on his furniture, books, and china, yet, he writes:

> Is all of this absolutely necessary? Yes, quite definitely yes! Of course one can keep animals in cages fit for the drawing room, but one can only get

> to know the higher and mentally active animals by letting them move about freely. How sad and mentally stunted is a caged monkey or parrot, and how incredibly alert, amusing and interesting is the same animal in complete freedom. Though one must be prepared for the damage and annoyance which is the price one has to pay for such house-mates, one obtains a mentally healthy subject for one's observations and experiments. This is the reason why the keeping of higher animals in a state of unrestricted freedom has always been my specialty. (p. 22)

When Lorenz later describes his jackdaws as falling in love and getting married, those of us who have not spent much time with jackdaws are likely to protest that this is excessive anthropomorphism. Not so, says Lorenz. His experience observing these birds makes him secure in his claim, and the appearance of anthropomorphism is rather a reminder of the extent to which our own behavior shares a biological heritage with members of other species. The description, Lorenz would maintain, is the result of his immersion and absorption in the extensive details of the animals' lives. Lorenz conceived of the evolution of intelligent behavior as the replacement of instincts by more flexible learning mechanisms (Brigandt, in prep.), hence observations of individual instances of flexible behavior played an important role in his investigations. The approach is easily dismissed as anecdotal and anthropomorphic by those who are unfamiliar with the particular animals.

Can Lorenz's appeal to Gestalt psychology help justify confidence in his interpretations? I'm inclined to be skeptical, although a thorough investigation of Lorenz's epistemology would take us too far afield in this paper (but see Brigandt 2003). My goal in this paper is to identify some possible approaches to the problem of understanding the epistemological role of observation, of which Lorenz's Gestalt approach is one.

A second approach is to view observations of animal behavior as premises to an argument by analogy, specifically to infer the mental or cognitive causes of an animal's behavior by analogy to the causes of similar human behavior. This approach has been attacked by Povinelli and Giambrone (Povinelli 2000, ch. 1) in its application to even our closest animal relatives, chimpanzees. They argue that the argument has logical weaknesses and is based on untenable introspective premises. Both prongs of their critique miss the mark (see Allen 2002 for more details). First, by 'logical weakness' they mean only that the argument is deductively invalid, but no one ever supposed otherwise. Second, they conflate two different versions of the argument by analogy for other minds. One, exploited by Bertrand Russell in his response to the skeptical problem of solipsism, must essentially rely on introspectively derived premises if it is not to beg the question. The other, for the existence of other species of mind, is not beholden to the problem of solipsism and is therefore not limited to introspection as a source of information about cognitive processes. Still, it is true that the strategy of arguing by analogy faces significant epistemological difficulties that would need to be solved if we are to have more confidence in the connection between long term observation and reliable understanding of animal minds.

We need and lack an objective understanding of the effects of observing animals upon the observers themselves, and the consequences of those effects for their claims to knowledge. My own view is that such an understanding requires a naturalized epistemology of observers. Such an approach might try to understand the ways in which long term observation of animal behaviour produces changes in observers' brains as a first step towards assessing the reliability of the judgements that result.

A speculative suggestion about how such work might go is derived from current work on 'mirror' neurons suggesting that intentional action and perception of others' intentions are served by a common neural substrate that represents motor schemas for action. Gallese and his colleagues coined the term 'mirror neuron' after discovering neurons in the ventral premotor cortex of monkeys that are activated both when the monkey engages in purposeful grasping activities, and when it observes similar hand actions performed by another individual (see Gallese et al. 2002 for the review from which this description is drawn). Homologous mirror neurons have subsequently been identified in humans, and Gallese et al. maintain that 'By an implicit process of simulating action when we observe other individuals acting, we can immediately recognize them as goal-directed agents like us, because a similar neural substrate is activated when we ourselves attempt to achieve the same goal by acting.' Gallese et al. 2002, p. 458). Speculatively applying these ideas to the capacity of long-term animal observers to understand the intentions of their subjects, it is tempting to suggest that this capacity might be the result of shared motor schemas developed as a direct result of watching animals interact with each other freely. It is likely to be devilishly difficult to determine the reliability of the mechanisms that enable the observed actions of others to be mapped onto our own actions, especially when those mechanisms cross species boundaries, and even more so when it is difficult to get an independent fix on the intentions of the observed subjects. But the elucidation of such a mechanism at least provides something for a naturalized epistemology of cognitive ethology to pursue.

If I may be allowed to push the speculative aspects of this a bit further, it's interesting to note that long term observers of animals sometimes seem to take on certain behavioral aspects of their subjects. I'll leave to your imaginations what I have observed about the urination patterns of Marc Bekoff, who studies dogs. For a slightly more family-oriented example, I recently raised some of these ideas about shared motor schemas at a meeting where I was followed by comparative psychologist Sally Boysen, who has an enormous amount of experience with captive chimpanzees. The title slide of her presentation contained a photo, that she has used many times before, showing herself holding a young chimpanzee. In the picture, the chimpanzee is making a 'play face'. She started her opening spiel, describing the chimpanzee's play face, when suddenly she stopped mid-sentence to remark that she had just noticed that in the photo she too is making a chimpanzee play face. I have also heard Boysen say 'I *am* a chimpanzee' and describe – and, perhaps more interestingly, act out – a consequence of this 'fact' in an anecdote about helping an older female chimpanzee who was new to Boysen's lab escape a terrifying situation, by engaging with her in a conspiratorial fashion. The implied intentionality in Boysen's account is quite rich, but her tendency to interpret these animals in this way and her implicit capacity for acting like a chimpanzee plausibly result from the large amounts of time spent observing and interacting with chimpanzees outside of any experimental situation.

Are mirror neurons part of the mechanism for these abilities? That remains to be investigated. If they are, then a system that has so far been articulated as a basis for intentional understanding within a single species, may cross species boundaries. Chimpanzees, of course, provide the most likely candidates for such a transfer since they are our closest living relatives. Boysen, like Savage-Rumbaugh, is working with highly enculturated apes, which may not be representative of apes in the wild. Whether these scientists understand their subjects using the same neural mechanisms as ethologists, who interact with their subjects in very different ways, is an empirical question of just the sort that should be answered by a naturalized epistemology of animal cognition. It is even more speculative to wonder whether an ethologist who has spent a lifetime watching dogs might also have come to develop mirror neuron responses to their actions. Nevertheless, an understanding of the mechanisms at work in all such processes would help us to identify their strengths and limitations as sources of knowledge. Skeptics may be inclined to think the mechanisms very unreliable, especially when applied widely across species boundaries, but that is a matter for empirical investigation not armchair complaint.

The long and winding road

Rendell and Whitehead (2001) in their article on culture in whales and dolphins explicitly liken ethology to ethnology. I used to think that the similarity between the two words was merely typographical and of no particular significance – except as a source of annoyance when unknowledgeable copy editors made unauthorized spelling changes on my manuscripts. But I have come to believe that the two disciplines have quite a bit in common, and that a number of the methodological debates within ethnology have counterparts in ethology.

To write an ethnography it is necessary to spend considerable amounts of time with the people one is studying. Among cultural anthropologists, the extent to which one should immerse oneself in the daily lives of those one purports to study is controversial. Such immersion tends to change the ethnographer in ways that make it more difficult to maintain 'critical distance'. There is no consensus among cultural anthropologists about the extent to which it is necessary or desirable to maintain such distance, but it is agreed that immersion creates challenges for the ethnographer, who may be changed in ways that make communication difficult with the intended readers of the ethnography who have not had the same experiences (e.g., Rosaldo 1989). Abu-Lughod (1991) introduces the term 'halfie' to describe the anthropologist who has allegiances to two camps. Cultural anthropologists like Abu-Lugohd who are sensitive to the issue of the individuality of humans tend to oppose themselves to what they see as reductionist scientific approaches. Good ethnography, according to Abu-Lughod, consists in providing detailed descriptions of the experiences of individuals while avoiding any tendency to generalize about other cultures.

Of the three 2003 Taft Colloquium scientists discussed in this paper, Sue Savage-Rumbaugh may come the closest to implementing the ethnographer's approach by giving us a complete description of her animal-subject's life. But that life is one, as she concedes, that may have turned the subject itself into a *Pan*/*Homo* 'halfie'. An ethologist cannot immerse him- or herself as completely in the lives of wild animals as ethnographers can in the lives of their human subjects, or as completely as scientists such as Boysen or Savage-Rumbaugh can in the artificial lives of their subjects. Neither can ethologists make use of the interviews and questionnaires that are an essential part of the ethnographer's tool kit, nor can they engage in the language-based interactions that figure so prominently in the work of Savage-Rumbaugh or Boysen. Nevertheless, ethologists who spent a great deal of time watching, and sometimes interacting with, the same animals, day in and day out, typically do develop a great appreciation for their qualities as *individuals*.

Many ethologists limit their professional writings to statements about animals that will not raise charges of anthropomorphism. But ask almost any ethologist how smart their animals are, and just beneath the surface you will often find a wealth of observations supporting cognitive attributions in the Darwinian mold (as attested by many of the contributions to Bekoff 2000 and Bekoff et al. 2002). Their challenge is to report the knowledge derived from close observation back to those who have not had the relevant experiences, and who tend to be much more skeptical about the testimony of ethologists than they are of the expert testimony of ethnographers, even though the ethologists often have years more experience observing their subjects than anthropologists do. Ethnographers can embrace stories derived from their free observations of individuals as revealing something important about the diversity of human experience while rejecting a narrow view of 'reductionistic science' as the only way of knowing. They can also rely on power of testimony from the subjects themselves to persuade others of the legitimacy of their interpretations. Ethologists, however, are much more constrained by their conception of themselves as scientists, and the limitations posed by subjects who cannot speak for themselves. Consequently they must make a more circumscribed case for the significance of free observations for a proper understanding of animal behavior.

Is anyone a cognitive ethologist? Some are trying to make the marriage work, and there is very interesting work being done in many quarters to bring real ecological problems into the laboratory. But the eventual answer will be 'no' unless the partners in this marriage can be reconciled. On the cognitive side there is constant pressure towards laboratory experimentation in conditions that are often of questionable ecological validity (see also Allen 2004). On the ethological side there is a pull towards observing animals in conditions where experimental control is either very difficult or impossible. Any attempt to do something in the middle is vulnerable to criticism from both directions. With respect to claims to being 'scientific' however, there is certainly a power imbalance between the partners, with observational methods typically seen as playing a subservient role to experimental methods, and coming in for severe criticism when they do not. For those who are really determined to make the marriage work, it will be necessary to find a way to explain why others should trust ethologists' judgments about the cognitive attributes of the animals they so clearly love.

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