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Critical Anthropomorphism, Uncritical Anthropocentrism, and Naïve Nominalism

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The main title of Clive Wynne's essay poses the question: "What are animals?" My initial thought was that the second part of the title, "Why anthropomorphism is still not a scientific approach to behavior," should be the sole title, since a screed against anthropomorphism seems the main point of the paper. But, upon further reflection I now see that Wynne is questioning what type of animals we are, what kinds of behavior and underlying processes we share with other creatures, and how we should go about finding out about these similarities. Good questions, uncritical answers.

The first part of Wynne's essay reviews the history of anthropomorphism in interpreting animal behavior. Although useful, there is little new here. The main focus of the essay is a critique of Frans de Waal's, Marc Bekoff's, and my own versions of what Wynne considers current, but still illegitimate anthropomorphism that should be eliminated from the study of behavior. As a primary target of the essay, I will expand upon my earlier brief, and apparently unsuccessful, attempt to clarify my position (Burghardt, 2004). Wynne's arguments, once a rallying cry against anthropomorphism, are ineffective against modern views that acknowledges both that science is a human endeavor and that we are human animals.

To reduce misstatements I will use direct quotations freely, beginning with the first page of Wynne's article:

Though there are differences between these approaches [Burghardt, Bekoff, de Waal], they share the belief that projecting oneself into the situation of a member of another species can lead to the production of useful hypotheses for further scientific study.

John C. Malone made useful comments on earlier versions of this commentary.

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This is a fair, though incomplete, characterization. But Wynne continues:

... anthropomorphism, even of the reformed varieties, should have no place in an objective science of comparative psychology ... because anthropomorphism is a form of mentalism, and as such is not amenable to objective study. Labelling animal behaviors with everyday terms from lay psychology does not explain anything. Rather it is an example of the nominalist fallacy – the belief that naming something explains it.

Notice that Wynne goes from a claim for a heuristic method to develop testable hypotheses to a claim that anthropomorphism is incompatible with objectivity. The clear implication is that there is no value in the scientific testing of the hypotheses generated by the anthropomorphic stance. Wynne mainly emphasizes two points: Anthropomorphism, however qualified, is beyond redemption as a word, for it is irredeemably mentalistic, and mental processes are incapable of being studied scientifically. I think that he is wrong. There is a large literature in opposition to Wynne's version of the objective-subjective dichotomy in modern cognitive science, neuroscience, philosophy, and even behaviorism (such as the writings of Skinner on the study of 'private events' or private experience; c.f., Burghardt, 1997). Wynne also seems to deny that anthropomorphism, the attribution of human type traits to nonhuman entities, can itself be studied. There exists fascinating research on how and what traits people attribute to other species and even neurological studies (Heberlein & Adolphs, 2004) on the brain mechanisms involved.

Wynne reminds us that naming something does not explain it. This is true: Naming something as instinct, learned, genetic, even as a conditioned stimulus, does not explain anything and neither does calling a response anger, fear, guilt, or remorse. Even labeling something as food, a predator, a mate or even a reinforcer also only sets the stage for further analysis. Wynne does not give one example of my

using a term from lay psychology, let alone one that commits the nominalist fallacy. But then Wynne seems to fall into the nominalist fallacy himself by suggesting that labeling anthropomorphism mentalistic explains, as well as dismisses, it! By the end of the essay, Wynne has suggested that describing an anthropomorphic concept of remorse in his dog as perhaps a conditioned response, without any data, explains canine remorse. He wants us to return to the time when a few basic learning processes were thought to explain the most complex behavior of animals and all this without formal study or testing of these alternatives. Wynne is emulating B.F. Skinner who, in an influential paper, gave a long, detailed explanation of the behavior of honeyguides (small birds that lead larger animals to resources they can later exploit) without data, except a few second hand anecdotes (Skinner, 1966). Plausibility was enough, so why go to the field and actually learn something about the species? Should Wynne not actually set up some canine experiments based on his pet observations? The ethological revolution was about taking animals and their natural behavior seriously, which meant dealing with diverse species and not armchair *post hoc* explanations (in Skinner's case, to explain instinct). After more than 40 years, I expected better arguments.

So what did I really try to do? Griffin's writings on cognitive ethology reinstated an explicit concern for the experiences and inner processes accompanying behavior, arguing that classical behaviorism too often restricted our analysis of complex behavior by its limited conceptual and methodological scope. He pointed out that the complex behavior and decisions seen in many animals strongly suggested some mental experiences comparable, though not identical, to those of humans, and such an assumption was evolutionarily parsimonious. He raised the issues of awareness and consciousness in animals, now a most thriving endeavor among neuroscientists (e.g., Baars, 2005). I was fortunate to have worked at Rockefeller University with Griffin the year his book appeared (1976-77). During my stay, I realized that he had not adequately examined the earlier 19th century movements to understand the animal mind and why they petered out. Thus, my 1985 paper (Burghardt, 1985) was a critique of the kinds of mentalism and anthropomorphism Wynne rightly opposes. But I also wanted to show that using our stance as a sentient being was valuable, if not essential, to the study of behavior, and many innovative scientists had realized this. I developed the concept of "critical anthropomorphism" to recognize the multiplicity of information needed for an effective science of comparative psychology. We need to use all our scientific and natural history knowledge about a species, including its physiology, ecology, and sensory abilities to develop testable hypotheses, which may indeed be based on 'hmm, what would I do if I were in a similar situation to the other species?'

Wynne considers Timberlake's "animal centered" theromorphism "a viable step forward" as compared to critical

anthropomorphism. I challenge Wynne to find an essential difference between us on this point. The real culprit is anthropocentrism. My chapter on critical anthropomorphism, cited by Wynne, opened with a 1909 quotation by Jacob von Uexküll (Burghardt, 1991, p. 53): "Our anthropocentric way of looking at things must retreat further and further, and the standpoint of the animal must be the only decisive one." It is only through a critical anthropomorphism, I believe, that we can reach the point of escaping anthropocentrism. Wynne tells us nothing about how to reach the theromorphic stage.

Not only is critical anthropomorphism useful in developing hypotheses, an unreflective objectivism is bad science: in this case anthropomorphism by omission, an idea developed in another essay cited by Wynne but misrepresented (Rivas & Burghardt, 2002). By dismissing our own status as animals evolved to deal with the problems of living that other species also have to face, and attempting to be completely objective, we fall into serious errors as readily as through being naively anthropomorphic. In our essay we discuss examples such as foraging in snakes, aposematic coloration, courtship in *Drosophila*, cats hunting mice, language, zoo exhibits, and conservation planning where scientists were too anthropocentrically objective to do the careful thinking that would have avoided premature or erroneous conclusions. Wynne addressed none of these examples; a refutation of them would have been evidence of the errors of the Rivas and Burghardt analysis. We write (Rivas & Burghardt, 2002, pp. 10-11):

Anthropomorphism by omission is the failure to consider that other animals have a different world than ours. We can, without realizing it, attribute human traits to other species by failing to consider that many species perceive the world in a different manner than do we. . . . An important component of this approach, though often understated, is to consider the animal being studied as an active participant, with the researcher trying to put him or herself in the animal's situation. Timberlake and Delamater proposed that to understand the behavior of an animal, "Experimenters not only need to put themselves in the subject's shoes, they need to wear them – walk, watch, hear, and act like the subject" (Timberlake and Delamater, 1991, p. 39).

Furthermore, we pointed out that (Rivas & Burghardt, 2002, p. 15):

It is not enough to avoid an anthropomorphic vocabulary and claim to be strictly objective. Anthropomorphism is like Satan in the Bible – it comes in many guises and can catch you unawares! Lockwood (1989) pointed out some of the guises. The most easily recognized are not the problem; the conceit that one is immune to them is more often the problem. . . . scientists are not immune; lurking unseen it can compromise efforts in many areas. By using critical anthropomorphism and wearing the animals' "shoes" we

can overcome part of our natural bias and obtain a more legitimate understanding of other species . . .

Wynne neither effectively challenges the view that critical anthropomorphism or its variants produces useful testable hypotheses nor counters the argument that its neglect is a dangerous anthropocentrism that has led to erroneous science and conclusions, by unwittingly assuming that man is the measure of all things. In fact, his discussion of canine remorse *is* critically anthropomorphic, since he not only suggests processes that can be tested, but begins with the putative similarity between human and canine behavior. I ended my 1991 paper with a series of eight conclusions, and this was the final one:

Reductionistic methods are critical for testing and evaluating ideas, but the history of animal behavior has shown that the most paradigm-shaking insights have come about by refusing to be bound by the accepted mechanistic views of the day. Thus the ultimate paradox too rarely appreciated. We must be open to new phenomena considered improbable by current scientific wisdom, yet use all we know of current rigorous scientific methods to test these seemingly unlikely possibilities. Critical anthropomorphism provides a way to combine our human characteristics and abilities with various kinds of knowledge and keep the question-asking in bounds but still creative” (Burghardt, 1991, pp. 86-87).

Finally, it is simply wrong to claim that critical anthropomorphism is mentalism and that all mentalism is based on non-material (supernatural) causes. Darwin makes clear in his notebooks that he was a thoroughgoing materialist, and Griffin and other scientists make similar statements. Methodology should not be confused with metaphysics or the study of mental processes with the ghost in the machine. The deeper question is why Wynne continues his single-minded quest. I certainly cautioned against woolly thinking in 1985 (p. 916), quoting Ernest Hilgard in my epilogue:

Opening the doors to a freer exploration of mental activities, although a virtue to those who are disciplined to scientific procedures and values, may turn into a vice for those who see the new freedom as an opportunity for free-floating uncritical fantasies about mental life.

Contrary to Wynne’s strong intimations, I do not think I have attempted to foster bad science or been unaware of the dangers of the techniques I have advocated. Instead, it is time to take on real targets—critiquing the science done and the explanations offered, not the way the hypotheses were generated.

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