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Anthropomorphism and its Discontents

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Arguing about names for things is fun, and arguments about history are captivating and educational, but, if there is one thing all four commentaries and I can agree on, it is that what really matters is how best to move a scientific field forward.

Anthropomorphism Pro or Contra: A Scientific Beauty Contest?

The usefulness of an approach in science is a difficult beast to capture. Attempts to quantify the fertility of anthropomorphism can quickly degenerate into something akin to a scientific beauty contest (at worst) or a matter of counting publications or citations of protagonists (at best). Blumberg asks "whether individuals who explicitly engage in anthropomorphism have a track record of scientific discovery that exceeds those who do not[?]" and suggests that the "apparent usefulness of anthropomorphism … is an illusion" (p. 145).

Timberlake broadly concurs with Blumberg and me when he writes that the "…primary dependence on unshackled anthropomorphism for our knowledge about other species is not a promising direction for science to go" (p. 140).
On the other side of the argument, Goodrich and Allen point out that among the scientists who consider anthropomorphism useful are several, such as "Bekoff, Burghardt, and de Waal, all of whom have distinguished records of scientific publication" (p. 147). Goodrich and Allen characterize mentalistic cognitive scientists as "extremely thoughtful" (p. 148). Burghardt also sees value in careful critical anthropomorphism.

I sincerely thank the commentators for sharing their responses to my piece. I honestly believe that it is through argument and debate that science progresses.

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I have no desire to argue with the reputations of pro-anthropomorphs, many of whom have made significant contributions to our science. It was partially for that reason that I presented a hypothetical case of anthropomorphic explanation of the "remorseful" behavior of a dog, rather than a critique of a specific study by a pro-anthropomorph, in my target article. I was also concerned that whatever published study I might select could be dismissed by pro-anthropomorphs as somehow not the right example to have chosen. However, by outlining an hypothetical example, though I had tried to make clear that I was only sketching an approach – not actually carrying out the study – I invited the criticism of Burghardt that I was trying to explain "the most complex behavior of animals… without formal study or testing" (p. 137). (Anyone interested in collaborating in a study of this kind is invited to contact the author.)

I am therefore very grateful to Goodrich and Allen for identifying, "specific scientific work which makes use of the attribution of mental states to animals [that] would be worthy of analysis: for instance de Waal's experiments on fairness in monkeys (Brosnan & de Waal, 2003) or the experiments by Hunt, Rutledge, and Gray (2006) and Weir and Kacelnik (2006) to test the understanding of tools by New Caledonian crows" (p. 149). I am very happy to consider the usefulness of anthropomorphic thinking as evidenced in these three papers.

Brosnan and De Waal (2003) reported that monkeys demonstrated a sense of "fairness" (or "inequity aversion") when they rejected a less preferred reward under conditions in which they saw another monkey receive a more preferred reward for the same effort. This anthropomorphic claim is undermined however by the fact that the monkeys were just as likely to refuse the less preferred reward in a control condition in which the more preferred reward was simply made visible but without any other monkey present in the experimental room. As I stated in a previous critique of this experiment:

> There can be nothing iniquitous about receiving a nonpreferred reward if nobody is receiving anything better. In the [control] condition the monkeys are refusing the nonpreferred reward simply because they can see that a better reward is potentially available. This is therefore the most parsimonious explanation for their refusal to accept the nonpreferred reward when they see another monkey receive a better reward (Wynne, 2004, p. 140).

By jumping to an anthropomorphic and mentalistic conclusion, Brosnan and de Waal (2003), failed to establish what variables might have controlled the behavior of their subjects.

The other two papers nominated by Goodrich and Allen as exemplifying the usefulness of anthropomorphism are among the latest reports on the surprisingly sophisticated abilities to construct and use tools by New Caledonian (NC) crows.

Weir and Kacelnik (2006) presented some novel problems to a laboratory-housed NC crow ("Betty"), already experienced in the bending of pieces of wire to extract rewards from tubes. In three experiments, Betty was given the challenge of obtaining food rewards from traps through the manufacture of tools out of flexible strips of aluminum. The traps were familiar to Betty, as was the use of flexible wire to construct hooks to extract rewards from them (Weir, Chappel, & Kacelnik, 2002), but the aluminum strips were novel to the bird. The aluminum differed from wire most crucially in that it is only flexible in one plane (wire can be bent in any direction). In two of the three tasks Betty became progressively more successful in bending the aluminum strips to extract the food reward. One of the two tasks on which she was successful required the bending inwards of the aluminum strip (shortening it into a hook); the other required her to straighten out already bent aluminum (to make a longer probe). On the first task, her latency to extract food declined across trials; her rate of holding the tool by the modified end (a more successful strategy than holding the unmodified end) increased across trials, as did the (human-rated) quality of the tool. On the second task, Betty only completed four trials before dying unexpectedly. On the first trial, she squeezed the aluminum strip in on itself in order to make a tool thin enough to insert into the tube. Although the experiment had been designed so that a tool bent in this way would not be long enough to secure the meat, Betty was somehow able to defeat the apparatus and gain the reward. On Trial 2, Betty poked unsuccessfully with the unmodified tool and did not reach the reward. On Trials 3 and 4 she prodded the tube for some time with the unmodified tool, before pushing the aluminum strip back against the lip of the tube, thus unbending it and enabling access to the meat.

Betty's behavior is undoubtedly fascinating. But how is it to be understood? Weir and Kacelnik (2006) state their aims thus: "The experiment," they explain, "…addressed three inter-related questions: 1. What did the subject *know about* the relationship between tool shape and success at retrieving the bucket… 2. What did she *understand* about the link between modification technique and tool shape…? 3. To what extent was she *aware* of the connection between (1) and (2) above…?" (p. 320: emphasis added). I am not sanguine that we will ever understand the understanding of another species; nor that we can know what it knows about, or become aware of the extent to which it is aware. In any case, Weir and Kacelnik acknowledge that Betty's behavior is more parsimoniously comprehended as the result of simpler processes. They note that she repeatedly mandibulated the aluminum strip as she had previously treated wire, and she "nearly always attempted to probe for the bucket [with the unbent strip of aluminum] before modifying the material" (p. 326). Even when she modified the tool, her first probes were with the unmodified end. Nonetheless, they feel that reinforcement learning is unlikely to be an adequate explanation of the crow's behavior for two reasons. First, because the acquisition was too rapid; and second, because "…we suspect it is impossible for a robot equipped exclusively with associative learning algorithms to solve these tasks with a similar amount of experience..." (p. 332).

These are very weak grounds to reject parsimony. Taking their first argument first: In most learning models acquisition rate is governed by a free parameter. Thus rate of learning cannot render a learning model invalid. Weir and Kacelnik's (2006) second argument to reject reinforcement learning as an explanation of Betty's performance is an example of what Dawkins calls the "Argument from personal incredulity" (Dawkins, 1986). The inconceivability to Weir and Kacelnik of an associative robot completing the tasks as Betty did is an extremely weak ground to reject an objective parsimonious explanation of behavior in favor of a vague mentalistic one.

To be fair to Weir and Kacelnik, they acknowledge that "…progress might come when we can replace terms such as understanding (which we feel compelled to maintain for the time being) by precise hypotheses about the operations the subject makes in the course of generating solutions to novel problems." (2006, p. 332).

Thus I think they and I are in agreement that the real question at issue here is: What can these birds do and under what conditions? But whereas they wish to hang on to an ill-defined mentalistic anthropomorphic conceptualization, I would break the fundamental question into smaller operationalizable parts: What are the stimulus conditions necessary to show these remarkable performances? What prior histories of experience (both extrinsically reinforced and not) are required? To ask about "understanding" is to move away from empirical science towards an approach to animal behavior where we judge the processes controlling behavior by our intuitive response to what we see (e.g., "the bending [of the aluminum strip] did not appear 'deliberate'" (Weir &

Kacelnik, 2006, p. 325). This cannot be a route to an objective understanding.

Hunt et al. (2006) studied the response of wild-living NC crows to food presented in holes. These holes were constructed so as to be somewhat deeper than the tools the birds were in the habit of forming were adequate to extract from. The tools are made by the birds from locally-growing leaves. Hunt et al. observed that over several trials of experience with deeper holes, the crows developed the habit of forming longer leaf tools. Returned to shallower holes they returned to forming shorter tools. The researchers conclude that the crows have a default length of tool that they bring to any baited hole. Trial and error learning is not considered a possible mechanism for the development of longer tools because that "would have produced random variation around the average length of first tools [and] not consistently longer second tools" (p. 314). However Hunt et al. are unable to distinguish between a previously developed associative learning rule ("if a tool fails make a longer one," p. 308), and what they call a "delayed causal inference" (p. 308: an inference based on perceiving the depth of the hole and inferring the length of tool required by observation).

Though Goodrich and Allen cite this paper as an example of anthropomorphism at work, I am not so convinced that Hunt et al.'s (2006) work suffers from mentalistic anthropomorphism. Though their language tends towards mentalism at times, they operationalize their terms adequately to permit experimental test. One can argue with these specific predictions (why should an associative rule demand tools a constant extent longer on each trial?), but they are not, in my view, intrinsically anthropomorphic descriptors of behavior.

Thus two of the three papers nominated by Goodrich and Allen as examples of the usefulness of attributing mental states to animals in fact exemplify different ways that mentalistic anthropomorphism can impede our science (and the third is not significantly anthropomorphic on my reading). Brosnan and de Waal's (2003) mentalistic thinking inhibits them from recognizing that the performance in their control condition contradicts their anthropomorphic account. Weir and Kacelnik (2006) can see what their bird is doing, but nonetheless hang onto vague mentalistic anthropomorphic interpretations in preference to analyzing the stimuli controlling the bird's behavior. In both cases, anthropomorphic thinking has impeded progress in understanding animal behavior.

Anthropomorphizing the Brain

Although I do not want to stray too far from my remit – which is animal behavior – I do feel compelled to acknowledge that Goodrich and Allen are quite correct in identifying that my objection to mentalistic anthropomorphism could apply to mentalistic explanations in the cognitive sciences more broadly. Burghardt also criticizes my approach by drawing attention to the thriving state of the study of the neural bases of awareness and consciousness in nonhumans as exemplified by Baars (2005).

Goodrich and Allen feel that they have identified a weakness in my criticism of anthropomorphism because I would be dismissing "…all the extremely thoughtful work that has gone into providing a materialistic underpinning for cognitive science over the past 50 years…" Furthermore, they argue:

> If it is perfectly consistent to think, as many scientists do, that mental states can be understood in neurofunctional terms, then Wynne's complaint comes down to the dubious claim that we should now throw out mentalistic terms because they were originally associated with a dualistic worldview (p. 148).

I hope I hesitate with due humility before the thoughtful work of 50 years of cognitive science. I count many cognitive scientists among my friends. But there are problems with certain kinds of mentalistic research done under that banner. It is just as unhelpful to anthropomorphize the brain – even of a human being – as it is to anthropomorphize a nonhuman animal.

Bennett and Hacker (2003) – neuroscientist and philosopher – explore some philosophical confusions in neuroscience in the "Philosophical foundations of neuroscience." They identify as the "Mereological fallacy" the error of assigning to the brain the actions and powers of sentient human beings. Brains do not, "believe," "interpret," "know," or even "represent information" (all qualities that distinguished cognitive- and neuro-scientists have ascribed to it). Brains simply have action potentials and other neurological events which exist at the level of brains but not, of course, at the level of sentient beings.

Space does not permit a detailed rebuttal of Baars (2005) but, fundamentally there are two fallacies in the comparative neuroscience of consciousness. The first is believing that finding neural activity in a human that correlates with some mental state constitutes an objective basis for accepting that mental state as a scientific fact. Neural activity correlates with everything people do: If people in a brain scanner are found to show particular patterns of neural activity when thinking of Campbell's tomato soup this does not turn "thinking of Campbell's soup" into a fundamental unit of cognitive science. Second, the fact that there may be neural events or organs which correlate with conscious mental activity in the human, and that similar neural events or organs may be observed in nonhuman species, does not prove that these other species are capable of conscious mentation. The brains of other species have evolved to enable them to solve their own problems of survival and reproduction – we cannot assume that similar neural activity will produce similar outcomes in diverse species.

In so far as some cognitive scientists (not all) use mentalistic terms which are poorly operationalized, they are as unlikely to make progress in an objective science of human behavior and cognition as mentalistic pro-anthropomorphs are to make progress in the study of animal behavior and cognition.

It 'is' Your Grandfather's Anthropomorphism

As I outlined in the target article, for some five decades (until 1976), though comparative psychologists and ethologists argued about many things, there was a cross-party consensus that mentalism could not aid the study of animal behavior and cognition.

But anthropomorphism runs deep and seems to require repeated weeding out. Though the term has only been used to characterize an approach to animal behavior for the last 150 years, something like what we now call anthropomorphism can be identified in Aesop's fables from the sixth century BC.

Mentalistic anthropomorphism is on the resurgent again. I take Burghardt's point that his "critical anthropomorphism" is not Bekoff's or de Waal's (or Romanes's) anthropomorphism. And I wholeheartedly agree with Burghardt that the "failure to consider that other animals have a different world from ours" is a failure with dire consequences for our science. If the approach I sketched in my remorseful dog story "*is* critical anthropomorphism" as Burghardt states, then we are only arguing about the names for things. I do think the names for things matter: The name "anthropomorphism" has a seven century history of standing for an error of thinking (where "big bang" was only used derisively for about a decade). But it is not as important as stamping out mentalism and keeping our science objective.

What is wrong with mentalism? Mentalism is bad when it hides causes inside imaginary structures that cannot be operationalized in objective observable phenomena. Some mentalism is just a harmless *facon de parler* – a use of everyday terms in order to make one's descriptions of behavior more colorful and to communicate with a lay audience. But there is a growing resurgence of ol' time anthropomorphism: the anthropomorphism of Romanes and (much as it pains me to say it) Darwin. In just the last month prestigious journals have reported the development of a "self awareness" in elephants (Plotnik, de Waal, & Reiss, 2007), and "mental time travel" in corvids (Raby, Alexis, Dickinson, & Clayton, 2007).

If a science of animal behavior and cognition is to grow, we have to inhibit our spontaneous deep-seated anthropomorphic tendencies and grasp the challenge of objective descriptions of behavior. Ultimately – ironically – this is the only way we will ever discover higher-level cognitive abilities in animals.

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