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## Conditioned Anti-anthropomorphism

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How should scientists react to anthropomorphism (defined for the purposes of this paper as the attribution of mental states or properties to nonhuman animals)? Many thoughtful scientists have attempted to accommodate some measure of anthropomorphism in their approaches to animal behavior. But Wynne will have none of it. We reject his argument against anthropomorphism and argue that he does not pay sufficient attention to the historical facts or to the details of alternative approaches.

Although his main target is anthropomorphism, Wynne also displays ambivalence towards current human cognitive psychology. He states that it has resurrected several mentalistic concepts, and while he allows that cognitive psychology uses introspection to generate experimentally testable hypotheses, he suggests, “there are grounds for criticizing this practice.” Nevertheless, he continues, “the issues are not as extreme as when this method is applied across species.” Like most scientists and philosophers, we agree that inferences about animal minds require special handling, but we believe that Wynne’s arguments commit him to the much stronger conclusion that all mental state attributions, even within our own species, are beyond the ken of “objective materialistic science.” We resist the temptation to speculate about why he thinks this. Instead, we look only at what he wrote. Consider the following claims:

1. “[a] anthropomorphism is a form of mentalism, and [b] as such is not amenable to objective study. [c] Labeling animal behaviors with everyday terms from lay psychology does not explain anything. Rather it is an example of the nominalist fallacy” (p. 125).
2. “Mentalism fails to qualify as a scientific explanation

for (at least) two reasons. [a] First, it uses ultimately non-material causes to attempt to explain behavior... [b] Second, mentalistic concepts are intrinsically private and thus by definition subjective, not objective” (p. 132).

We have defined anthropomorphism in such a way that 1a is true. We will deal with 1b, amenability to objective study, further below when we come to 2b. 1c is true but misleading. It is true because the act of labeling never explains anything. It is misleading for none of his main contemporary protagonist authors (Bekoff, Burghardt, and de Waal) has ever indicated that merely labeling animal behaviors mentalistically explains those behaviors. Nor do the direct quotations from these authors that Wynne supplies in his section on “Modern Anthropomorphism” establish his claim that they are guilty of the nominalist fallacy. We distinguish three possible roles for anthropomorphism in the science of animal behavior: (i) explaining animal behavior, (ii) as sources of hypotheses about the causes of animal behavior, and (iii) as targets of explanation in their own right. In discussing the nominalist fallacy, Wynne attacks the first of these. The three scientists mentioned by Wynne in this context consider anthropomorphism in its role (ii), as a source of scientifically testable hypotheses, and they endorse role (iii), treating mental states in animals as legitimate targets of scientific investigation and explanation. But it is only after such investigation that any of them would endorse the explanatory role (i). Theirs is no mere labeling strategy, but an attempt by thoughtful scientists (we assume they do have mental states!) to understand how their thinking about animals can be usefully informed by thinking about human mentality.

We are compelled to note that in his response to Bekoff, Burghardt, and de Waal, all of whom have distinguished records of scientific publication, Wynne nowhere assesses their claims in any detail. Instead, he writes as though it is sufficient to apply the label of *anthropomorphism* to their views while mounting some arguments against anthropomorphism. Insofar as thoughtful defenders of anthropomorphism endorse anything like an explanatory role for mentalistic terms,

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it is after going through hypothesis and experimental investigation. This is just as true of the historical figures Wynne criticizes as their modern counterparts. We will argue below that Wynne is no more successful in representing the history of anthropomorphism in comparative psychology than he is in making his case against anthropomorphism, which we turn to next.

Wynne's objections to mentalism in statement 2 above would, if correct, present equal difficulties for mental concepts whether used for humans or nonhuman animals. Words are not immaterial causes made flesh. What, then, of Wynne's reasons for banishing mental state attributions from science? With regard to 2a, his charge that mentalism entails immaterial causes, we can think of only two possibilities. Either Wynne would accuse all cognitive scientists of being dualists, or he is committing a version of the genetic fallacy. The scope of his reactions to cognitive science is difficult to discern given that he did not articulate his "grounds for criticizing" the use of mentalistic notions in human cognitive psychology. But given all the extremely thoughtful work that has gone into providing a materialistic underpinning for cognitive science over the past 50 years (not to mention the long history of materialistic theories of mind, including Descartes' contemporary Hobbes), we should not just take Wynne's word for it that mentalistic terms are unavoidably committed to immaterial causes. 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. This is the genetic fallacy. Just because the terms used to be understood in that way, it does not follow that we must regard them now as inadmissible. It would be similarly fallacious to claim that it is unscientific to consider hypotheses about the biological functions of the liver on the grounds that functions, pre-Darwin, were associated with now-discredited teleological, vitalistic, or creationist views. Wynne commits the very same fallacy when he writes that it is a mistake to try to use the word 'anthropomorphism' positively because of its origins in the 13th century as a label for a mistaken theological view. One might as well reject the term 'Big Bang' in physics because it was invented by Hoyle to mock a theory that he believed rested on a mistake.

Wynne's mention of the subjective/objective contrast (1b and 2b) provides the more interesting challenge: namely, how to bring mental states into the scientific fold. We will argue for two points about this. First, Wynne's history of anthropomorphism distorts the attempts of earlier thinkers to meet that challenge. Second, with respect to current science, this is a challenge that cognitive scientists and neuroscientists are vigorously pursuing. Wynne ignores the details of this scientific work. The result in both cases is that he commits the straw man fallacy.

Wynne presents the history of comparative psychology as one of decreasing appeals to anthropomorphism. But the story is more complicated than he lets on. For instance, although Darwin used a lot of anthropomorphic anecdotes in his effort to establish continuity, he also took mental states to be target of empirical investigation (role iii above). In his final book he described experiments aimed at understanding the intelligence of earthworms with respect to the petioles and other objects they use to plug their burrows (Darwin, 1881; see also Crist, 2002).

Romanes collected many anecdotes, but he also tried to justify this practice. Wynne describes nothing of Romanes's attempt to struggle with these issues, choosing instead to simply label him as credulous and gullible because he included reports from "persons bearing names more or less unknown to fame" (Romanes, 1883 as cited by Wynne). In fact, however, many of the more unbelievable anecdotes in *Animal Intelligence* come from known observers. It was Darwin who provided Romanes with an anecdote that snails are capable of communicating complex information to one another. Similarly, when a bishop reported a kind of trial of a jackdaw by rooks, Romanes felt obliged to publish (see Boakes, 1984, p.26). Wynne acknowledges Boakes as a careful commentator, but instead of stopping to analyze the history carefully himself, he jumps ahead ten years to Lloyd Morgan's 1894 publication of his *Introduction to Animal Psychology*.

The problem with this leap is that it causes Wynne to neglect the fact that during the 1880s Lloyd Morgan was one of the most important critics of Romanes and Darwin's uses of anthropomorphism. In 1886 Lloyd Morgan published a short article in the journal *Mind* in which he described the state of the science at that time as "a chaotic mass of anecdotal fact and fiction" (Lloyd Morgan, 1886, p. 174). He began by quoting an anecdote from Romanes in which an orangutan unties several knots in order to get a key. Lloyd Morgan criticizes Romanes's inference that the animal understood the nature of the problem. He rejects Romanes's anthropomorphic method of considering one's own mental states when performing similar activities. Lloyd Morgan argued that such a method carried a risk of error even when applied cross-culturally within humans, and he thought that the situation was worse for animals, partly because of the absence of language. He argued that the scientific study of animal intelligence should focus on the habits and activities of animals and concluded that knowledge of the subjective states of animals is unattainable.

By 1894, however, Lloyd Morgan was more willing to employ anthropomorphism. Missing from Wynne's story is any indication of why Lloyd Morgan would change his mind. Lloyd Morgan's reasons are still relevant to current research, and they can be summed up in one word: evolution. Lloyd Morgan may have been Romanes's biggest critic, but he was also Romanes's great friend. Romanes's use of anthropomorphism and his views about the subjective states

of animals were challenges to Thomas Huxley's automaton view of animals. Huxley (1874) believed that the subjective states of animals were causally irrelevant for understanding animal behavior: Consciousness was essentially an epiphenomenon. Huxley believed that human subjective states were more complex than those of animals. Romanes argued that if subjective states are becoming more complex and are different among various species, then they must be selected for. On this view, subjective states were causally efficacious and could therefore explain behavior.

Lloyd Morgan would come to accept Romanes's argument that the subjective states of animals were in fact important to the understanding of animal behavior, but not until he had experimental evidence. After publishing *Animal Life and Intelligence* in 1890, Lloyd Morgan was informed by T. Mann Jones that he had wrongly described many of the behaviors of young chicks (Boakes, 1984). Lloyd Morgan had relied on experiments that Douglas Spalding published in 1872 describing the pecking behavior of chicks as completely unlearned and totally instinctive. Upon repeating these experiments T. Mann Jones found that there was a learned component. Lloyd Morgan repeated the experiments himself and saw that he could not ignore the learned aspect of the chick's behavior—and he thought that he needed to appeal to the experience of the chick in order to explain this.

Lloyd Morgan's personal realization that seemingly instinctive behavior was fine tuned by learning would lead him to write his *Introduction to Comparative Psychology* (1903), where he first stated his now famous canon. The historical details of how Lloyd Morgan came to the canon help us to understand the acceptance of anthropomorphism by perhaps the most famous psychologist in Britain of the era. Wynne is right that Lloyd Morgan's canon is not a blanket prohibition against anthropomorphism. But he is wrong to insinuate that Lloyd Morgan commits the nominalist fallacy. The canon is better seen as a call for experimentation that can pick out where subjective states are causally important. Lloyd Morgan famously gives the example of his own dog opening a garden gate. The question that Lloyd Morgan poses is whether or not the dog understands how the gate works. Lloyd Morgan does not pretend to be able to know all of the qualities that might go into the subjective state of understanding something, but he can experiment and see whether the dog has general knowledge of how the gate works. He tries to get his dog to open the latch in different ways, but the dog is unable to open the latch except by using his nose in the way that it is accustomed to. Lloyd Morgan concludes that the dog does not understand how the latch works, but has associated lifting his nose against the latch with the opening of the gate. This is an application of the canon. However, had the dog been more flexible in its use of the latch, the canon would have supported the conclusion that the dog understands something about the way that the latch works. In this case, we would be speaking of the mental states of the dog.

Of course, critics of such inferences are free to propose (and test) alternative explanations. Detailed attention to actual experiments is where the discussion of anthropomorphism is most likely to be fruitfully located—not where Wynne takes it, with broad generalizations about the demerits of anthropomorphism. Had Wynne considered the works of Lloyd Morgan more carefully, he might have seen this. Instead, his comments on the absence of a science of remorse do nothing to show that anthropomorphism is generally unjustified. It fails to engage with any actual science. Instead, we are supplied with Wynne's own anecdote-driven speculations (“I have noticed that...”, p. 133) about the possibility (“perhaps... perhaps... perhaps...”, p. 133) of Pavlovian explanations for the submissive behavior of puppies in the presence of damaged objects.

We do not rule out the power of such approaches to explain some aspects of animal behavior. Neither do we deny that some pro-anthropomorphism scientists sometimes provide more rhetoric than substance. But, as we mentioned above, many scientists are vigorously pursuing the challenge of applying cognitive approaches to animal behavior, entirely within a non-dualistic framework. Specific scientific work which makes use of the attribution of mental states to animals 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. Metacognition, social play, affective neuroscience, and the study of mirror neurons are all areas where the attribution of mental states to animals has led to interesting experiments. Instead of blanket complaints against anthropomorphism, we enter a plea for more attention to the actual arguments of philosophers of science who have shown that anthropomorphism is not necessarily or always a logical mistake (e.g., Fisher, 1991; Sober, 1998; Keeley, 2004) and for more thoughtful engagement with those scientists who have attempted to articulate their own views about the various roles that anthropomorphism plays in their empirical investigations of animal mind and cognition.

### References

- Boakes, R. (1984). *From Darwinism to behaviorism*. Cambridge, UK: Cambridge University Press.
- Brosnan, S. F., & de Waal, F. B. M. (2003). Monkeys reject unequal pay. *Nature*, 425, 297-299.
- Crist, E. (2002). The inner life of earthworms: Darwin's argument and its implications. In M. Bekoff, C. Allen, & G. M. Burghardt (Eds.), *The cognitive animal: Empirical and theoretical perspectives on animal cognition* (pp. 3-8). Cambridge, MA: MIT Press.
- Darwin, C. (1881). *The formation of vegetable mould through the action of worms, with observations on their habits*. London: John Murray.
- Fisher, J. A. (1991). Disambiguating anthropomorphism: An

- interdisciplinary review. *Perspectives in Ethology*, 9, 49-85.
- Hunt, G. R., Rutledge, R. B., & Gray, R. D. (2006). The right tool for the job: What strategies do wild New Caledonian crows use? *Animal Cognition*, 9, 307-316.
- Huxley, T. (1874). On the hypothesis that animals are automata, and its history. *Nature*, 10, 362-366.
- Keeley, B. L. (2004). Anthropomorphism, primatomorphism, mammalomorphism: Understanding cross-species comparisons. *Biology & Philosophy*, 19, 521-540.
- Morgan, C. L. (1886). On the study of animal intelligence. *Mind*, 11, 174-185.
- Morgan, C. L. (1890). *Animal life and intelligence*. London: Edward Arnold.
- Morgan, C. L. (1903). *An introduction to comparative psychology* (Rev. ed.). London: Walter Scott Publishing.
- Romanes, G. (1883). *Animal intelligence*. New York: D. Appleton & Co.
- Sober, E. (1998). Morgan's canon. In D. D. Cummins & C. Allen (Eds.), *The evolution of mind* (pp. 224-242). New York: Oxford University Press.
- Spalding, D. A. (1872). On instinct. *Nature*, 6, 485-486.
- Weir, A. A. S., & Kacelnik, A. (2006). A New Caledonian crow (*Corvus moneduloides*) creatively re-designs tools by bending or unbending aluminium strips. *Animal Cognition*, 9, 317-334.