

# Assumptions in animal cognition research

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For experiments designed to investigate whether an animal has a particular psychological, social, or normative property had by humans, researchers use Neyman & Pearson's (1928;1967) hypothesis testing methods, and generally formulate the null hypothesis such that the animal is said not to have the property in question. When a purportedly human property is attributed to an animal without prior methodologically sound investigation of this sort, that attribution is considered anthropomorphic. In the current debate about the role of anthropomorphism in animal cognition research, some scholars have raised the concern that the standard psychological methods result in a bias against attributing properties to animals when those properties are seen as somehow *special* human (Sober 2005, de Waal 1999)—special because they are psychological, social, or normative properties that have been identified as potential markers for human uniqueness.

One division among animal cognition researchers is between those who emphasize the similarities between humans and nonhuman animals (especially when it comes to the great apes), and those who emphasize the differences (who we will call “selective skeptics”<sup>1</sup>). Both camps justify their position by appeal to evolutionary considerations. The ethologist Frans de Waal, for example, argues that when we see similarities in behavior between humans and other apes, we should expect to see similarities in cognitive processes and functions, because the similarities in behavior suggest that the individuals derived from a common ancestor<sup>2</sup>. Other animal cognition researchers, such as the psychologist Daniel Povinelli, express great concern about this way of describing other species, worrying that some animal cognition researchers are too eager to undermine claims of human uniqueness<sup>3</sup>. For selective skeptics, the null hypothesis is that animals do not have human-like cognitive systems, social relations, or normative properties. Further, at least some selective skeptics claim that current research supports

such hypotheses (despite the methodological prohibition against affirming the null, which we will discuss below).

We argue that the special worry about anthropomorphism as expressed by selective skeptics is a bias. We do two things. First we challenge the idea that the special human properties can be unproblematically identified, and hence that the null hypothesis can be unproblematically stated. Second, we argue that the correct application of the Neyman & Pearson methodology does not justify a special worry about anthropomorphism over general worries about making false claims.

### **Human properties and the null hypothesis**

As animals, humans and non-human animals share a number of biological, morphological, and relational properties, as well as some psychological properties such as the ability to *fear* (e.g. a predator) or *desire* (e.g. food). No experimental study is needed to draw these conclusions, even if they are based on behavioral observations of animals—perhaps the same kinds of observations we use to justify the notion that other humans experience fear and desire. Other features are thought by some to be special kinds of human traits, including psychological states such as beliefs, personality traits such as confidence or timidity, emotions such as happiness or grief, social organizational properties such as culture or friendship, and moral behaviors such as cooperation or punishment. Why are these properties more problematic than the others? A recent answer is that the problematic properties are those identified via interpretation, or folk psychological properties (Penn & Povinelli 2007; Povinelli & Giambrone 1997; Povinelli & Vonk 2003, 2004). The “insidious role that introspective intuitions and folk psychology play” in comparative cognition research is identified as being at the heart of the anthropomorphic approach to the science (Penn & Povinelli 2007, 732). The worry appears to be that folk psychological concepts are introspective explanations for human behavior that are then attributed to animals in analogous situations. They are problematic attributions in the first place because they are based on possibly false folk account of the cause of *human* behavior. Then, to make matters worse, those same properties are attributed to animals.

We think that identifying the special human properties as folk psychological fails to do the work that the selective skeptics need it to do; the distinction between folk psychological concepts and scientific psychological concepts will not map onto the distinction between anthropomorphic human properties and shared properties. Consider that folk psychology is “(a) a set of attributive, explanatory, and predictive practices, and (b) a set of notions or concepts used in these practices” (Von Eckardt 1995, 300). The practices of folk psychology include things such as predicting, explaining, justifying, evaluating, and coordinating behavior. And the concepts of folk psychology include theoretical mental entities such as beliefs, desires, intentions, memories, emotions, sensations, goals, and personality traits. If the selective skeptics were to identify folk psychological terms as anthropomorphic, they would have to accept that application of any folk psychological term to an animal is impermissible. But the skeptics cannot claim that any use of folk psychological language is problematic, because they make great use of many folk psychological concepts in their scientific papers—concepts including memory, goals, desires, emotions such as fear, and even beliefs<sup>4</sup>. Since the selective skeptics help themselves to some folk psychological concepts in order to do science, they cannot consistently dismiss any use of folk psychology as unscientific. The selective skeptic cannot sustain a general worry about the use of folk psychology in animal cognition research, and so the *special* properties cannot be identified as coextensive with folk psychological properties.

Given that there is no systematic account of what makes certain human properties special, we conclude that there is no justification for forming the null hypothesis in terms of animals not having so-called “higher” properties, or in terms of animals not having folk psychological properties. To form the null hypothesis in such ways is to make a problematic assumption about the very issue under investigation.

## **Types of errors**

In animal cognition research, like psychology more generally, some kinds of errors are thought to be worse than others. Students of psychology are taught very early in their training that committing a Type-I error is worse than committing a Type-II error.

These errors are identified in terms of the null hypothesis being investigated, and are often understood such that a Type-I error is a false positive and a Type-II error is a false negative.

Sober suggests that the methodological position of preferring Type-II errors is the position of preferring the bias we call anthropectomy over the bias of anthropomorphism, and it seems the skeptic would agree with that analysis. We disagree.

Consider the typical definitions:

Type-I Error – Rejecting a null hypothesis when it is in fact true.

Type-II Error – Failing to reject a null hypothesis when it is in fact false<sup>5</sup>.

Here, the null hypothesis is taken to be the default situation; it is what is assumed unless and until investigation shows it to be false, and it can never be proven true. In the case of animal cognition, it is almost always the case that the null hypothesis is that animals do not share the special human properties. Thus, whereas a Type-I Error involves making a false claim (e.g. claiming that some Fs are Gs when in fact no Fs are Gs), a Type-II Error does not; it is merely a refusal to make a true claim (e.g. not claiming that some Fs are Gs when in fact some Fs are Gs).

In psychology, Type-I and Type-II errors are defined in terms of the null hypothesis in such a way that Type-I errors – of which mistaken anthropomorphism is an example – are false claims, whereas Type-II errors are not. The problem is that when investigators go out of their way to avoid Type-I Errors, they not only run the risk of committing Type-II errors, but they also run the risk of committing the much more serious Type-IIA error, which we define as claiming that no Fs are Gs when in fact some Fs are Gs. Anthropectomy involves a claim about the nonexistence of a property. It is not a position of agnosticism, and so it is a mistake to prefer anthropectomy to anthropomorphism.

To put the same point in a slightly different way, we think it is a mistake – a serious mistake having to do with the application of psychological methodology – to both:

- (a) hold that Type-I errors are more serious than Type-II errors, and
- (b) view Type-I errors as errors of anthropomorphism and Type-II errors as errors of anthropectomy.

To see why, consider again how we might define the two kinds of error:

Type-I error = rejection of a null hypothesis when it is in fact true.

Understood this way, a Type-I error is indeed a false claim to be avoided. So far, so good. The problem for the skeptic can be put in terms of a dilemma concerning the definition of Type-II errors. Either:

(Horn 1) Type-II error = failure to reject a null hypothesis when it is in fact false.

or

(Horn 2) Type-II error = acceptance of a null hypothesis when it is in fact false.

If the skeptic takes on (Horn 1) of the dilemma, then she has good reason to think that Type-II errors are less serious than Type-I errors, but she has no reason to think that the skeptical hypothesis about psychological properties of animals should count as the null hypothesis. In its original formulation, the null hypothesis is a hypothesis that reflects what is expected to be the norm, and against which the researcher is looking for a statistically significant discrepancy (Neyman & Pearson 1928; 1967). However, in many cases there is no data on the norm, and in that case a typical textbook rule for setting up a good hypothesis is to choose a “dull or disappointing out-come...a boring result” (Garner 2005, 140). It is difficult to see how or why Garner’s advice should be followed in the case of animal cognition research. It is true that some people would find it amazing if mere beasts shared our capacity to read others’ minds, for example. But others would find it equally amazing if nonhuman animals, especially those who are very closely genetically related to humans, lacked *all* of our psychological properties. Even if we were to try to follow the textbook advice, we would be at a loss in determining whether the skeptical or optimistic hypothesis should be the null hypothesis. We conclude the selective skeptic cannot accept (Horn 1) of the dilemma, because it does not permit identification of some properties as uniquely human.

If, on the other hand, the skeptic accepts (Horn 2), then she is free to deem the skeptical hypothesis the null hypothesis, but then she has no reason to think that Type-II errors are less serious than Type-I errors. Under the (Horn 2) definition, both types of error are false claims, and it is perfectly legitimate for a researcher to choose something like “animals do not have psychological properties” as her null hypothesis, but it is not legitimate for her to think that accepting this hypothesis when it is in fact false is better than rejecting it when it is in fact true. Now, the same researcher might choose “animals

*do* have psychological properties” as her null hypothesis. The lesson just is that once Type-I and Type-II errors are made symmetrical, neither error is worse than the other. And since the errors are defined in terms of the null hypothesis, it follows that there is no direct epistemic reason to choose the skeptical hypothesis as the null hypothesis and no reason to choose the optimistic hypothesis as the null hypothesis. It’s a wash.

Either way, the claim that the risk of anthropectomy is less troubling than the risk of anthropomorphism is unwarranted.

### **Choosing the null**

Like de Waal and Sober, we think that the methods of comparative cognition result in a bias against attributing special human properties to animals. However, we don’t think that this bias is the result of a *correct* use of the methodological rule of thumb that Type-II errors are to be preferred to Type-I errors so much as it is a problem with identifying the null hypothesis in the first place.

The selective skeptics may reply to our argument that anthropectomy is as bad an error as anthropomorphism by reaffirming their status as skeptics rather than slayers—they may claim that they do not deny the existence of special human properties in animals, and hence are not open to the charge of anthropectomy, but that they simply remain agnostic. However, that response isn’t consistent with the sorts of claims the selective skeptics make. Take, for example, the last two sentences in a recent paper coauthored by Povinelli: “...whatever “good trick” (Dennett 1996) was responsible for the advent of human beings’ ability to reinterpret the world in a symbolic-relational fashion, it evolved in only one lineage – ours. Nonhuman animals didn’t (and still don’t) get it” (Penn et al. 2008, 129). This isn’t a cherry-picked example, and such negative claims abound in selective skeptics’ writing. It shouldn’t be surprising that such claims are made, when selective skeptics such as Povinelli identify the division between themselves and other animal cognition researchers as a division between those who emphasize the differences and those who emphasize the similarities between humans and animals. The selective skeptics are unwarranted in making anthropectic claims based on the standards of the Neyman and Pearson hypothesis testing method, and by

making such claims they do a disservice to the science of animal cognition research.

Nonetheless, the worry about anthropomorphism can offer a useful corrective to animal cognition research. While no general prohibition against using folk psychology in animal cognition can be justified in the face of current practice, given the ubiquity of such terms in scientific psychology, the worry about folk psychology does point to the need to have well-defined terms in hypotheses as well as in interpretations of results. We must try to avoid using fuzzy language to describe animal behavior or cognition, even when the functions and the mechanisms of such behavior or cognitive capacities are not well understood in humans. When we use the same term to describe baboon friendship and human friendship, and the term has not been operationalized in the same way in its application to humans and nonhumans, its careless use may have unintended implications. Researchers can avoid unintended implications by carefully choosing the terms they use to interpret animal behavior, and by reminding us that some terms, like friendship, refer to a range of human relationships that differ from one another in innumerable ways (across age ranges, across cultures, etc.). This task is not unlike the task taken on by anthropologists who need language to describe culturally distinct human behavior (Andrews 2009).

Researches should set aside any worry about *special* human psychological, social, or normative properties, given the difficulty in even identifying what such properties might be. Rather, animal cognition researchers who want to make comparisons across species should carefully identify the property of interest in the comparison species before they can ever begin to ask whether it exists in the target species. Some properties, such as the capacity for theory of mind, are still so poorly understood in the human case that it isn't surprising that looking for them in animals has led to so much controversy. The better defined the question, the better the science. While that is a general principle that extends beyond animal cognition research, it is one that bears repeating in this context.

## Notes

1. We can categorize those skeptics who are particularly worried about anthropomorphism into two types: categorical skeptics who think that animal cognition research cannot be good science, and selective skeptics who think that some kinds of attributions are not justified. Many of the defenses of animal cognition research have addressed categorical skeptics, such as J.S. Kennedy (1992), who think that animal cognition research is an unscientific field of research. These skeptics think that animals are not the right sorts of things to apply the concepts to. For categorical skeptics, the charge of anthropomorphism is a pre-empirical one. They think that researchers in animal cognition are making a category mistake by asking whether animals have certain properties (for this critique see Bekoff & Allen 1997; Fisher 1990, 1991; Keeley 2004).
2. “The...cladistic rationale applied to humans and their close relative should lead us to adopt cognitive similarity as the default position, thus making anthropomorphism a virtual nonissue” (de Waal 1999, 259).
3. “If the dramatic resculpting of the human body and brain that occurred over the past 4 million years or so involved the evolution of some qualitatively new cognitive systems, then this insistence on focusing on similarities will leave comparative psychologists unable to investigate hallmarks of their own species—or chimpanzees, for that matter. It [seeking to find similarities across species] is an agenda that does justice to no one” (Povinelli & Bering 2002, 116). Similar concerns are shared to some degree by other animal cognition researchers (e.g. Shettleworth 2010a, 2010b, Silk 2002, Blumberg & Wasserman 1995, Wynn 2004, 2007).
4. Povinelli and Vonk accept that chimpanzees have beliefs; they write “everyone agrees that the chimpanzee’s mind contains mental representations” (Povinelli & Vonk 2003, 158).
5. See, for example, Garner 2005: “Type-I error is rejecting a null hypothesis that is true. Type-II (or beta) error is failing to reject a null hypothesis that is, in fact, false” (135). But note that this way of defining Type-II errors is not universal. See Fisher 1971, who defines “errors of the second kind” in terms of “*accepting* the null hypothesis ‘when it is false’” (17, emphasis added). For reasons that should become clear soon, it is of the utmost importance to determine whether Type-II errors should be defined in terms of “failing to reject” or in terms of “accepting”, for these phrases describe two entirely different doxastic states. Our assessment of the problem with much animal cognition research is that it is unclear whether this important distinction is made in actual practice by researchers.



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