

Politics or metaphysics? On attributing psychological properties to animals

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Abstract Following recent arguments that there is no logical problem with attributing mental or agential states to animals, I address the epistemological problem of how to go about making accurate attributions. I suggest that there is a two-part general method for determining whether a psychological property can be accurately attributed to a member of another species: folk expert opinion and functionality. This method is based on well-known assessments used to attribute mental states to humans who are unable to self-ascribe due to an early stage of development or impairment, and can be used to describe social and emotional development as well as personality. I describe how instruments such as the Child Behavior Checklist, which relies on intersubjective expert opinion, could be modified to assess other species subjects. The measures are validated via the accuracy of the predictions that are derived, which is an example of the functionality of attribution. I respond to theoretical criticisms against use of this method, and argue that if the method counts as good science for infant cognition research, then it should count as good science for animal cognition research as well. Correspondingly, if the method doesn't count as good science for animal cognition research, then we must be very skeptical of its use with nonverbal humans.

Keywords Anthropomorphism · Animal personality · Attribution theory · Mental states · Emotions · Concepts · Language · Culture

Political correctness for animal researchers

In the scientific study of animal cognitive capacities, there are some words that are not used. Instead of describing two individuals as “friends”, many prefer to speak of their “affiliative relationship.” And rather than using emotional state descriptions such as “happy” or “sad” or “depressed” to describe an animal, jargon may be introduced. However, some terms are used without concern; discussions of hunger and thirst, stress,

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fear, and even uncertainty are considered acceptable attributions for some species. Terms from the first group are among those whose attribution to other species is thought by some to be a case of anthropomorphism, the attribution of uniquely human traits—usually psychological or agential traits—to a member of a nonhuman species.

In recent years, there have been a number of theoretical discussions about the charge of anthropomorphism itself (including the essays in Mitchell et al. 1997; Daston and Mitman 2005, and work by Fisher 1990, Kennedy 1992; Crist 1999; Rivas and Burghardt 2002; Keeley 2004). Much of this discussion has focused on what is meant by the charge of anthropomorphism. It can't be that humans and animals share no properties (since we share many biological, morphological, relational, and spatial properties with animals). Further, it can't be that humans and animals share no psychological properties (else the critics wouldn't condone talk about pain or fear response). Recent research suggests that we share cultural properties with some species as well. Evidence of social learning and transmission of group-specific behavior in great apes (Bonniot et al. 2006; Horner et al. 2006; van Schaik et al. 2003; de Waal 2001; Whiten et al. 1999, 2001; Boesch and Tomasello 1998) and cetaceans (Krutzen et al. 2005), undermines the possibility that all cultural attributions are anthropomorphic.

The empirical research on animal cognition aims to determine which attributions are truly attributable to different species, whereas the charge of anthropomorphism is a pre-empirical obstacle to this research. Rather than focusing on the obstacle, I suggest that we ignore it as prejudice, and instead work on developing methods for testing the applicability of specific properties. The central aim of this paper is to address this epistemological concern, and suggest a two-part method that can be used to make accurate judgments about some mental and agential properties of other species. The first part of the method involves extracting the expertise of caregivers—*folk expert opinion*. The second part of the method involves testing the usefulness of the resulting attributions—*functionality*. Both practices are part of the current methodology in human psychology, and are used to attribute properties to people who are unable to do so themselves.

Many psychological assessments rely on folk expert observer judgments in order to gauge features such as personality and social adjustment. Such measures appeal to the judgments of parents, teachers, or caregivers of the subject. The instruments that are designed to extract expert knowledge are calibrated in part on the basis of their functionality. If the attribution of a personality trait, emotional state, or relationship is useful for making predictions of behavior that one did not antecedently have, then that attribution can be tentatively accepted as accurate. The two main tasks of science, prediction and control, rely on the use of abstract terms in order to categorize objects and events. If the attribution of the concept is useful and if it coheres well with other observations that we accept, then, following standard practice, that should be enough to accept it as current theory.

We recognize the expertise of infant caregivers in the development of tests used to attribute psychological properties to infants, and I'm suggesting that we should recognize the expertise of animal caregivers when developing measures used to assess whether other species have particular psychological properties as well. If the method counts as good science for infant cognition research, then it should count as good science for animal cognition research as well. Correspondingly, if the method doesn't count as good science for animal cognition research, then we must be very skeptical of its use with nonverbal humans. What I'm suggesting is that acceptance of a methodology should be based on the same considerations, whether the subject is an infant human or a member of another species.

Folk expert opinion

When a human visits a zoo, or goes on safari, she may describe the animals that she sees in the same terms that she would typically describe a human being. A dolphin with her mouth open in what appears to be a toothy grin may be said to be smiling, but in fact the layperson confronted with the “smile” doesn’t know he is about to be attacked. A chimpanzee who marches over and grabs your hand might be seen as being friendly or curious, but may in fact be preparing to bite.

In the same way, an adult with no experience with children may marvel at the happy smiling baby, but mother knows that her infant is ready to release his bowels. It is the mother’s familiarity with her child that allows her to make the correct attribution to her child, and the tourists’ unfamiliarity with exotic species that causes them to make the wrong one.

Though most humans are not experts in the behavior of chimpanzees or dolphins, most humans with children are experts in child behavior, and humans in general are experts in adult human behavior. We gain this status as experts through our experiences interacting with other people, rather than through explicit instruction or formal training. It is a unique kind of expertise that does not depend on some standard pedagogy, and is at least implicitly understood even before one’s first psychology class. We know that people have psychological properties, and we know something about how these psychological properties are related to stimuli and behavior. What is learned in the psychology class is built upon this folk understanding of human psychology, and while students learn about mechanisms and breakdowns of normal mental events, and while they may learn that some parts of commonsense psychology are false, the science that led to the discovery of mechanisms, deficits, and failures of folk psychology are themselves based on the lay expertise humans have about human minds. Starting at a relatively young age, we come to learn that classes of behaviors can be described using a particular term, and that application of the term can help us to formulate predictions about future behavior, as well as to make sense of the behavior by embedding it into a larger explanatory network. For example, in the West we learn that behaviors such as taking the last piece of cake and talking loudly on a cell-phone during dinner are both described as selfish behavior, and that both indicate a lack of concern for other people. And once we know that someone is rude, we can use that information to predict what that person will do in other similar situations.

Just as typical humans gain status as experts in adult human psychology through their experiences with adult humans, parents and good teachers become experts about children, nurses and caregivers become experts about people with dementia or other geriatric mental disabilities, etc. The expertise developed by caregivers, nurses, teachers, and others through their daily experiences with patients or children is recognized by researchers. Psychologists make use of this expertise in order to form judgments about the applicability of psychological properties to individuals. In the development of such measures, one of the questions is whether the property will be applicable to members of the group. In such cases, the judgments are categorical ones (though rather than taking the category to be members of the species, it is taken to be a proper subset of the species). Once the measures have been tested for validity, the judgments that we make using them are situational and pick out individual differences.

There exist many psychological instruments that are used to attribute psychological properties to humans who themselves do not have the concepts or the words to describe the properties that are attributed to them. Parents and teachers assess children’s emotional and social development using *The Child Behavior Checklist* (Achenbach and Edelbrock 1983). *The Vineland Adaptive Behavior Scales* (Sparrow et al. 1984) determine the personal and

social skills of children and low-functioning adults based on caregiver interviews. There are other measures for humans near the end of life, such as the *Social-Adaptive Functioning Evaluation* (Harvey et al. 1997) which is used by nurses to rate social and functional impairment in geriatric patients. Instruments such as these are widely used to assess personality, social development, intelligence, emotional adjustment, communication skills, and other psychological factors among children who cannot speak for themselves and adults who are low functioning or suffering from dementia.

The assumptions behind these instruments are that caregivers have knowledge of their charges, and that this knowledge can be extracted. Just as the caregivers of human infants or low-functioning adults have expert knowledge due to their familiarity with such people, we should expect a similar expertise among the human caregivers of other species. Experts on animal behavior include human caregivers, technicians, and others who work with captive animals, as well individuals who have spent a significant amount of time observing the behavior of individual animals in the field.¹ The method I'm calling *folk expert opinion* takes advantage of the knowledge gained by this experience, by treating those who are familiar with the individuals in the same way standard tests of human children treat parents, teachers, and other caregivers. Though I am calling these people experts, it is important to note that they need not have any formal academic training in animal psychology or behavior. They are experts by virtue of their experience with animals, just as human parents become experts about their children's temperament, cognitive capacities, relationships, and so forth. Given this point, the kind of expertise that I am talking about is an expertise through acquaintance, rather than an expertise through training. Caregivers have acquaintance knowledge of their charges, a kind of knowledge that scientists or other formal experts may lack.

One way to acquire folk expert opinions on the applicability of mental properties to other species is to start with well-established measures of human temperament, personality, social cognition, relationships, and so forth. Instruments that measure these properties can be appropriately modified, given to zookeepers and other species experts, and tested for reliability using standard psychological methods. For example, The Caregiver-Teacher Report Form for Ages 1½–5 from the *Child Behavior Checklist* includes items such as "Avoids looking others in the eye" and "Apathetic or unmotivated" that may be directly applicable to other primates, but probably will not pick out individual differences among all mammal species. In other cases, relevant changes will have to be made given the different ecological considerations for the species being examined. For example, the same instrument also includes items such as "Doesn't know how to have fun; acts like a little adult" and "Speech problem" which may not be appropriate for measuring the psychological state of any other species. In the move from the human species to each nonhuman species, validity will have to be confirmed, and inter-rater reliability will have to be assessed.

¹ Others share this view. For example, Rollin (1997) writes "usually the best source of information about animal pain are farmers, ranchers, animal caretakers, trainers—in short those whose lives are spent in the company of animals and who make their living through animals" (p. 128), and he cites Morton and Griffiths (1985) as endorsing this view. However, we must note that not all caretakers have the same quality of relationship with their charges, and that those who have nurturing relationships may be in a better position to make such expert judgments than those who are not socially involved with the animal, or those who see the animals as commodities. The same will be true for humans as well; the child will not demonstrate her full range of cognitive abilities when confronted by someone she doesn't trust, e.g. The issue is that the starting position for attributing mental states to human and nonhuman animals will be different, unless the attributor is familiar with and has the same quality relationship with the animal as the human experts have with the children. Without antecedent argument against choosing the same starting point for investigating animal and human mental states, there is no reason to place a greater burden of proof on the animal case than we place on the human one.

The method of folk expert opinion has already been used in the development of personality factors for a number of different species. Orangutans, chimpanzees, cats, dogs, even octopi and guppies are among the species whose personality dimensions have been subject to analysis by means of the same methods used to construct the Five-Factor Model (FFM) of human personality (Gosling and John 1999). The human FFM was developed to describe the way attributions of trait terms group together into statistically significant clusters, and it organizes personality into five domains: Neuroticism, Extraversion, Openness to Experience, Agreeableness, and Conscientiousness. While there remains controversy about the accuracy of the FFM (and philosophical worries about the nature of personality more generally), supporters of this approach have argued that most individual differences can be described using this model (Digman 1990) and that there are underlying genetic factors related to these domains (Bouchard and Loehlin 2001 have a review).

When this model is applied to other species, subjects have typically been part of zoo or aquaria exhibits, rather than research subjects or wild animals. Psychologists ask experts familiar with the animal's behavior, such as zookeepers or other people involved in daily husbandry or training activities, to rate a subject using the same methods used in developing the human FFM. King and Figueredo (1997) and Weiss et al. (2006) used this method to assess personality in chimpanzees and orangutans, respectively. As with the development and implementation of the FFM, raters are given lists of adjectives and asked to rate an animal on a 7-point Likert scale (on which 1 indicates total absence of the trait and 7 indicates extremely large degrees of the trait). Adjectives and descriptions on the orangutan scale include:

DEFIANT: Subject is assertive or contentious in a way inconsistent with the usual dominance order. Subject maintains these actions despite unfavorable consequences or threats from others.

PROTECTIVE: Subject shows concern for other orangutans and often intervenes to prevent harm or annoyance from coming to them (Weiss, personal communication).

Raters' judgments are then assessed for statistical reliability both within and between raters. The results indicate some differences and many similarities between human, chimpanzee, and orangutan personality factors. For example, while human factors of personality are limited to the five factors listed above, the chimpanzees showed six factors of personality. The chimpanzee model includes correlates for all the human factors, plus an additional factor for dominance (King and Figueredo 1997).² Orangutans, on the other hand, showed only the orangutan correlates for Neuroticism, Extraversion, Agreeableness, and Dominance, but also showed a factor that is a combination of Conscientiousness and Openness that was called Intellect (Weiss et al. 2006).³

² It has been argued by de Waal (2006) that humans should also be seen as having a Dominance factor. His claim amounts to the charge that a reverse anthropomorphism is at work among psychologists who are unwilling to attribute to humans a property traditionally used to describe nonhuman species.

³ One might think that the animal versions of the FFM are designed so as to determine whether a particular property is applicable to a species, whereas an animal version of the Child Behavior Checklist may be seen as determining situational applicability, rather than categorical applicability of a property. However, if categorical applicability is to be seen as an empirical issue, as has been argued, then in order to determine categorical applicability one must look at individual behavior to determine whether there exists some animal who has the property in question. Thus, methodologically speaking, we must see a connection between situational and categorical approaches to attributing psychological characteristics to animals. For example, when developing an other-species version of the Child Behavior Checklist, the tests for reliability and validity serve as tests for the appropriateness of the property for the species under consideration. If reliability or validity for a particular feature cannot be reached, then there is reason to avoid making claims about that property.

While one might take issue with the factor analysis of personality in general, if the critic wants to undermine the chimpanzee and orangutan results, then, barring any unseen differences in the methods used for animals and those used for humans, she would be forced to criticize the human studies as well. That is, if the human studies and the animal studies are methodologically parallel, then their acceptability should rise and fall together. To reject these studies as inapplicable to animals because they are in a different category to begin with is to beg the question.

Nonetheless, since instruments such as the Child Behavior Checklist and The Orangutan Personality Trait Assessment rely on the judgments of individuals who are familiar with the target subjects, there may be concerns about justifying ascriptions based solely on consensus. For example, to ask a question about personality among members of a species is to assume that members of that species have personalities. The critic might note that one could put together a similar survey designed to ascribe personality traits to automobiles, but since automobiles clearly don't have mentalistic properties, the attribution of personality to animals is no indication that they have mentalistic properties.

In addition, one might be concerned that the expert opinions cohere with one another not because they have all noticed some objective feature of the subject, but because they are part of a human narrative, and constructed together as a group. Thus, when asked about an individual's flourishing or personality traits, a caregiver might respond based on how she hears others talk about the animal. A group of caregivers working with orangutans, for example, might have all agreed that one young male is the policeman, and another young male the thief. They may then talk about the animals using those terms and other associated concepts. Once it comes time to fill out a survey about the orangutans' personality or flourishing, each caregiver's answer will be affected by this practice of discussing the individuals as having these properties.

Such concerns might lead a critic to conclude that the expertise of the caregivers is a pseudo-expertise, which like the "expertise" of astrologers, doesn't correspond to any objective state of affairs. However, there is an important distinction between the two kinds of groups. The astrologers may agree that the position of planets when a person is born determines some of their personality traits, but this is because the astrologers read the same books or trained with teachers from the same school. Their expertise, like the expertise of other discredited movements, tends to be an expertise that is gained discursively, through academic (or pseudo-academic) training. The expertise shown by caregivers is of a different sort. Caregivers, for the most part, do not get training in personality, emotions, or mental states before they begin caring for their charges. Their expertise is non-academic, and non-discursively learnable. It is an expertise that comes from a familiarity through association with their charge. In the intersubjective expert opinion of caregivers, consensus is not reached because the experts trained together in how to correctly rate individuals on personality or flourishing surveys. Rather, the instruments are used to capture the kind of knowledge that is developed through hands-on experience with the subject.

Of course, the caregivers can be wrong about their charges, and they may sometimes attribute properties based on a folk narrative constructed by the caregivers as a group.⁴ This brings us back to the original concern—perhaps the properties that the caregivers perceive are nothing more than false attributions. Perhaps only humans have emotions, personality,

⁴ It's worth noting that the formal research on personality in different species is careful to avoid this criticism. Researchers collect data from different groups of humans in order to minimize the danger of collecting shared interpretive frameworks.

or mental states. After all, many mental states are thick functional terms that relate not only to other mental states, but to social roles and expectations as well.

A few things can be said in response to this worry. First, like many folk concepts, mental and agential concepts are umbrella terms, and there are no necessary and sufficient criteria for their applicability. There are kinds of friendship, degrees of selfishness, and forms of extroversion. For example, we might describe the relationship between an adult and a child as a friendship, but not expect that the relationship live up to the ideal of an adult friendship. In a time of need one wouldn't expect the child friend to offer financial or practical assistance, or much in the way of sophisticated emotional support. Yet calling a child a friend indicates that to some extent, some of the features typical of friendship exist in the relationship between the two individuals. Applicability of mental and agential terms are likely based on some kind of probabilistic operation that relies on prototypes or exemplars, and that can make sense of claims such as "My dog is a good friend."

There is also some biological support for the claim of continuity of the mental across at least some species, given what we are learning about the biological and genetic basis for some mental properties and personality traits. For example, humans and other primates produce cortisol when they are experiencing high levels of stress. The shared hormonal substrate between humans and some other species, and the correlation between cortisol levels and stress in humans have led to an acceptance among animal scientists to speak of stress in other species—and it has led to research programs investigating the stressors, such as dominance rank, that an individual must contend with (Abbott et al. 2003).

In addition, the hormonal studies, like the personality studies, are based on hypotheses about the psychological states of other species that were formulated after observing species typical behavior. Among primates, it was noted early on that there are individual differences in behaviors, and a flexibility of behavior, and the hypotheses are developed according to an inference to the best explanation strategy. It was the recognition of widespread individual differences that led to the development of the human FFM of personality, and it is the same observation that has led to the development of personality measures for other species.

When caregivers disagree about a specific attribution, the mistaken attribution need not entail that the trait cannot be accurately attributed to the species. Humans make errors in attributing mental states to one another, and while we may disagree about what a person is feeling, the disagreement is based on the shared commitment that people can have those sorts of feelings. There are many reasons for humans to make—and believe—false attributions about themselves and others, as we know from social psychology. It is for this reason that instruments were designed to extract caregivers' expert knowledge, rather than accepting any attribution a caregiver might make. Instruments that measure children's personality, social flourishing, intelligence, etc. assume that there is some knowledge in the caregiver that can be acquired using such methods, but genuine knowledge must be distinguished from pseudo-knowledge by looking at the functionality of the attributions.

Caregivers gain knowledge of their charges by looking for correlations, by implicitly making inductive generalizations, and testing predictions—the same thing humans do in the development of their everyday folk psychology. Functionality and folk expert opinion work together in folk psychology as well as in the science of psychological instruments. Our adult human folk psychology, just like a parent's folk child psychology or a zoo-keeper's folk orangutan psychology, is a strategy for understanding behavior that is adopted because it is functional. To determine whether it is meaningful to apply a mental term to a nonhuman animal, we must determine whether it is useful to do so.

Functionality of terms

When a term is deemed acceptable for use with a particular species, it is typically because the applicability is demonstrable given the term's predictive force. For example, dominance rank is a property that is widely used to describe individuals in many social animal species, and the use of that term allows researchers and caregivers to formulate predictions about how the individuals will respond to particular events.

When we use the criterion of functionality, what we see is that properties such as friendship, which are not considered scientific among some researchers (Silk 2002), can be used to make predictions that serve to improve conditions for animals in captivity. For example, while doing her dissertation research on stress in rehabilitant orangutans, Raffaella Commitante noticed that when orangutan friends were housed together, they were less stressed. When friends were split apart, or when enemies or bullies were placed together in a cage, stress levels, as judged by facial expressions and behavioral tendencies, increased. In her dissertation research, Commitante found correlations between levels of cortisol in urine and certain expressions and behaviors (Commitante 2005). She was able to use this correlation, along with her judgments about friendships between orangutans, to suggest that the orangutans at the Wanariset orangutan rehabilitation center should be housed in friendship groups rather than age groups. When the animals were moved as Commitante recommended, stress behaviors declined (Commitante, personal communication). While this may not be a surprising result, the finding is significant because Commitante generated her prediction about stress levels by using the term "friend". Silk argues that whether two animals are friends is not something that is measured using objective standards, such as amount of time spent in proximity, amount of time wrestling, etc., and that there are no necessary or sufficient conditions for friendship.⁵ Rather, friendship judgments are made about animals the same way they are made about humans—the friendship relationship is something that is noticed after spending enough time with the animals to understand species typical behavior, and the qualities of relationships that exist between individuals.

Use of the relationship property "friend" allowed the caregivers at Wanariset to implement a housing arrangement that lowered the stress behavior of the animals. This example of the use of "unscientific" language to make real, behaviorally and physiologically measurable differences in the orangutans' daily life suggests that the friend relationship is one that can be usefully applied to orangutans.

The expertise that caregivers develop, and the judgments that they make, can be corroborated not only through consensus with one another, and not just through informal predictions, but also via formal studies. To take one example, researchers who developed personality factors for chimpanzees examined the predictive force of those personality attributions (Pederson et al. 2005). In the study, human caregivers first described their chimpanzee charges using the six-factor chimpanzee personality model. The personality factors are described without reference to any specific behaviors. At a later date (between 2 weeks and 3 years later), an independent observer recorded the frequencies of the target chimpanzees' behaviors. When the behaviors were categorized into five social contexts (agonistic, submissive, affiliative, solitary, and public orientation), and the social contexts

⁵ Each of these methods of determining friendship between apes has its downsides, as Silk (2002) shows. While she is hopeful that some more objective measurement of the friendship relationship can be developed, at this time there is not an operationalized definition of "friend". Thus, judgments about friendships between nonhumans will presumably be generated in the same way as judgments about human friendships are.

were identified theoretically with the personality traits using folk judgments, it was found that the chimpanzee's frequency of behaviors were predictable based on the individual's personality traits.

When ascribing mental states to infants, researchers justify their claims based on the predictions that they can make from the ascriptions. Before an ascription is tested, the judgments of expert caregivers, familiar with infant behavior, are taken as reasonable interpretations. The caregiver's folk infant psychology can then be formally tested, at which point the mentalistic attributions may gain greater or lesser support.

The methods used to test for mental properties in nonhuman animals can parallel the methods used with infant humans. We begin with the expert judgments of those familiar with the group, and develop and test hypotheses based on those judgments. While this may sound like common sense, I advocate recognizing the store of unscientific folk knowledge had by those who spend their lives caring for animals. Most researchers have largely ignored this knowledge, and we should pay more attention to it. After all, the folk knowledge we have about human behavior serves unselfconsciously as the starting point for our research in human cognition. It is an obvious and perhaps necessary place to start, since most researchers have a folk expertise in human behavior. But most of us do not have a folk expertise in the behavior of other species. To gain the same foundation in animal cognition research as we have in human cognition research, we need to start from the same source—folk expert opinion.

Discussion

Relying on folk expert opinion and testing the functionality of those opinions comprises a method used to ascribe mental properties to infants and other humans who cannot verbally ascribe properties to themselves. If this is a reliable method in infant cognition research, then we might expect it to be a reliable method in the case of other species as well.

However, one might object to extending the method to animals based on the extent of the differences between humans and nonhuman animals. This criticism may suggest an argument from analogy justification of the attribution of mental states to humans; humans have mental states because they are relevantly similar to me, and I have mental states, whereas animals don't because they are not relevantly similar to other humans. Rather than reviewing the well-known problems with arguments from analogy, I will instead examine what those possible differences might be. For the criticism to hold any weight, it must articulate what the differences are and how they are relevant.

An obvious difference between humans and other species is that only humans use a natural language, and there is a long history of arguing that animals can't have psychological characteristics because they don't have language. However, in the context of this argument, the difference is irrelevant. The existence of psychological instruments to identify the psychological properties of pre-linguistic infants and adults with dementia implies that using language is not necessary for having mental or agential properties. While language use cannot be the relevant difference, we must take a related concern more seriously. One might suggest that the methods for ascribing mental properties to infants rely on human concepts, and the conceptual division of the world may differ between species. The criticism might go like this: While it makes sense to use expert opinions to judge well-being, personality, or relationships of humans, this is because humans are the ones with these concepts. Animals don't have such concepts and so it is incorrect to attribute those concepts to them. We know that animals don't have those concepts, because

in order to have them they would have to have a host of other concepts that we know they don't have, concepts that connect in a rich semantic web of related concepts. And even if they have some similar concepts, we cannot know what the content of those concepts are *for them*. By using our concepts to describe the agential or psychological properties of individuals across species, we may be implying too much.

This kind of concern appears to be widely held, though to different degrees. Stich, for example, writes, “nothing we could discover would enable us to attribute content to an animal's beliefs” (1979, 23). Others demonstrate a more tempered worry. Keeton writes:

Almost all of our words have some sort of human connotation, imply some sort of human motivation and purpose. But such motivation and purpose may have no relevance to the behaviour of other animals, and we must constantly guard against unwarranted attribution of human characteristics to other species...English (like all human languages) having been developed around human activities and human interpretations, invariably reflect these, often with a strong cast of supernaturalism.... You are cautioned, therefore, to recognize the pitfalls inherent in any application of human-oriented language to the activities of other animals... (op. cite Kennedy 1992, 1).⁶

Keeton's concern applies just as well to attributions made across cultures or to our hominid ancestors, and cognitive anthropologists and comparative linguists have accepted this caution. Fifty years ago, the anthropologist Ward Goodenough addressed this issue in his classic article “Residence Rules”. In this article, Goodenough tries to determine why the ethnographic data on the rules of residence in marriage for Trukese society taken by John Fisher differed significantly from his own data, even though they were collected in the same community within three years of each other (Goodenough 1956). His conclusion is that the differences were due to the different theoretical approach taken by himself and Fisher—the difference between ethnography and ethnology. When ethnographers look at a society, they are trying to discover its principles and concepts in order to describe group practices using the group's own framework. Comparative ethnologists look for similarities across groups, and use their own concepts to organize the society that they are investigating. These two methods can lead to different conclusions about how to describe a society.

When investigating the psychology of other species, researchers can take Keeton's caution to heart and attempt an ethnography of the species rather than an ethnology. Depending on the goals of the research, however, it may be necessary to attribute human concepts in order to make judgments about similarities and differences across species. In the later case, we are applying concepts to individuals that they wouldn't—and couldn't—apply to themselves. This theoretical approach is criticized not only by those concerned about anthropomorphism in animal cognition research, but also by the postmodern critics of anthropology who decry the hegemony of the West and criticize the attribution of “uniquely Western” notions to the oppressed peoples of the global south. Both groups agree that “we” ought not apply our traits and notions to “them.” The difference lies only in the referents of the pronouns.

Such concerns imply that different groups must share concepts in order to enjoy cognitive similarities. However, there is reason to reject this assumption. To take one example, Eckman's work on emotions shows extensive cultural consistency regarding the emotions

⁶ In his 1992 defense of neo-behaviorism, Kennedy uses Keeton's quote to describe the position that once was orthodox. Kennedy's view is that we ought to return to this position, and that the growing acceptance of an anthropomorphic bias is a dangerous flirtation.

of anger, disgust, fear, joy, sadness, and surprise (Ekman et al. 1969; Ekman 1970; Ekman and Friesen 1971). He found, in addition, that there is widespread similarity of facial expressions that correlate with these emotions, across cultures. Though there are different words used to describe the emotions, and different conceptual divisions and associations, the physiological similarities allow us to make emotional attributions across cultures.⁷ Physiological similarities across species can likewise be used to make attributions to animals, even if the concepts attributed are not part of the species' conceptual framework.

An additional reason for being unworried about attributing properties to an individual when they don't have the concept themselves is that we readily do so when it comes to human children. For example, we ascribe beliefs to children before they begin to have facility with the belief concept around age four, yet we largely accept that children younger than four have the beliefs that we ascribe to them. Current child psychology accepts that children have beliefs before they have the concept of belief, and the same is true of many other mental states. The first worry, that we cannot ascribe psychological properties to individuals who do not themselves have that concept, should be set aside.

The critic, however, may remain unconvinced. The relevant difference between humans and other need not be at the level of conceptual framework. Rather, the difference may be biological. With humans, we can be relatively confident that applying our concepts to people in different cultures is accurate, because, despite cultural differences, we share a common evolutionary history, common DNA, common life trajectory, common morphology, and common basic needs for survival. And, since the construction of language was a collaborative human activity, the notions that became words in human languages are ones that are applicable to us. Because of the biological similarities and similar histories, our mental states attributions across humans will tend to be correct. But across species, as biological, morphological, or behavioral differences increase, the justification of our attributions will decrease as well.

While this claim is likely true, the observation doesn't help defend the critic's position because it doesn't allow for a clear line to be drawn between humans and other species. Consider the different ways in which one might answer the question about who is more similar to a human neonate—a chimpanzee neonate or an unrelated adolescent human. In some ways we would expect the two neonates to be most similar. For example, the human and chimpanzee infants will go through many of the same developmental stages during the first weeks of life. Both chimpanzee and human infants engage in neonatal imitation soon after birth, and then around eight weeks neonatal imitation disappears in both species, to be replaced by social smiling and mutual gaze with the mother (Matsuzawa 2006). The similarity in developmental trajectory between human and chimpanzee neonates would, according to the similarity argument, justify attributing the same sort of psychological properties to the two infants.

In other ways—biologically or morphologically—the human neonate will be more similar to the adolescent human. Judgments of similarity are themselves pragmatic, and the critic who looks for a resolution to the question about the mental states of other species

⁷ I don't mean to suggest that there is no difficulty with making attributions across cultures; when applying a term or notion from our culture, we must be vigilant. For one, we shouldn't understand our concepts too narrowly when working across social groups, be they inter- or intra- one's own culture. What counts as molestation in one group may be seen as acceptable medical care in another. Cultural differences are real, as are species differences. But at the same time we cannot deny the very possibility of similarities, and we shouldn't deny that a property is applicable before we have done empirical investigation that is based on a foundation of folk expertise with a particular species.

through appeal to degrees of similarity and difference will, at least in some cases, be making a decision about which features are relevant when making the determination.

The pragmatism implicit in similarity judgments that stem from our folk human and folk other-species psychology is of the same kind. In both cases, we make reference to properties and entities that are not directly observable in order to predict and explain behavior. We tend to think our explanations are generally accurate, because our predictions are generally accurate. The folk expertise in different species can be used as a starting point to examine both the similarities and the differences between humans and other species. If the joint method of folk expert opinion and functionality serves to justify attributions in the human case, using the same method with other species will help us learn which attributions we should, and should not, make.

To gauge the respects in which we are similar and different to other species is to continue work in comparative cognition. Worries about anthropomorphism only get in the way of cognition research on other species, and in order to avoid false attributions and underattributions—in order to accurately determine which psychological properties a species does and does not have—research in comparative psychology and ethology is exactly what we need.

To answer the metaphysical question, we need to avoid the political wrangling. The design of experiments, the choice and use of ethological methods, the analysis of the data, the environment of the study—these are all issues open for debate, and such debates will keep the science of animal cognition fecund. The anthropomorphism debate, on the other hand, is a distraction from doing good science.

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