CHAPTER 16

BEYOND ANTHROPOMORPHISM: ATTRIBUTING PSYCHOLOGICAL PROPERTIES TO ANIMALS

KRISTIN ANDREWS

In the context of animal cognitive research, "anthropomorphism" is defined as the attribution of uniquely human mental characteristics to nonhuman animals. Those who worry about anthropomorphism in research are confronted with the question of which properties are uniquely human. As animals, humans and nonhuman animals[1] share a number of biological, morphological, relational, and spatial properties. In addition, it is widely accepted that humans and animals share some psychological properties such as the ability to fear or desire. These claims about the properties animals share with humans are often the products of empirical work.

Prima facie, one might think that in order to justify the claim that a property is uniquely human, it would be necessary to find empirical evidence supporting the claim that the property is not found in other species. After all, the goal of animal cognition is to determine what sort of cognitive abilities animals use. If scientists were to discover that a cognitive property wasn't found in any species except the human species, then the claim that some other animal had that property would be a false charge, and would be an example of anthropomorphism.

However, in practice anthropomorphic worries play a pre-empirical role. Research programs are charged with being anthropomorphic because they are

examining whether some species has some feature that the critic believes only animals can have, based on some pre-empirical consideration. This charge is sometimes defended by theoretical arguments about the nature of the ability or property being examined.

A number of features have been described as uniquely human on theoretical grounds, including psychological states such as beliefs and desires, personality traits such as confidence or timidity, emotions such as happiness or anger, social organizational properties such as culture or friendship, moral behavior such as punishment or rape. For convenience, I will refer to the members of the class as "psychological properties." J. S. Kennedy, a visible critic, includes feeling, purpose, intentionality, consciousness, and even cognition in his list of psychological properties that are incorrectly attributed to animals.[2] Among the critics, there is considerable disagreement about what counts as an anthropomorphic attribution, and this alone should raise questions about the charge.

We can identify two different questions about the attribution of psychological properties to animals in scientific contexts. First we can ask whether it is scientifically respectable to examine questions about the mental, psychological, cultural, and other such states of animals. Those who bemoan anthropomorphism think that we have no warrant for asking such questions. I will look at these worries and will argue that there is no special problem inherent in asking and answering such questions.

The second question arises with an affirmative answer to the first. After establishing that it is scientifically respectable to investigate whether an animal has a psychological property, we must then ask how such an investigation is to be carried out. In answer to the question of how we can study the psychological properties of animals, I will propose that we use an approach to the attribution of psychological features to animals that is based on the approach we use for prelinguistic children. A specific psychological attribution will be warranted if it takes into account the species and cultural normal behavior, it has predictive power, and it mirrors the attribution of a similar property in prelinguistic infants. This is not to say that nonhumans can have only the psychological properties that infants have. It *is* to say that the general approach, modified so as to be species appropriate, and the degree of evidence we use when studying infant psychology should be used when we study animal psychology. I will show how this method can be used to examine different kinds of psychological properties.

In some current research programs, researchers are following methods that fall on the side of the methods I will propose, but in other programs violations of these methods lead to what I will argue are false attributions of psychological properties to nonhuman animals.

Can We Study Animal Psychology?

One worry about allowing scientists to ask about the psychological states of animals is that the scientists' own subjective biases may affect the work. The worry has been

stated in different ways. For one, psychological attributions in general might be thought to be subjective interpretations of behavior. If psychological properties are in the eye of the beholder, then they are not appropriate objects of scientific study—except, of course, at a metalevel; an anthropologist might examine the behavior of attributing psychological states as a cultural practice. But if this worry is well-grounded, it holds just as well for research on humans as it does for animal research. If the critics have this in mind, they would be forced to reject most of contemporary cognitive approaches to human psychology since human psychology research works to find real human psychological properties. Giving up human psychology in order to avoid giving in to animal psychology is a price few would want to pay.

Another way of understanding the concern that scientists ought not examine the psychological properties of animals because it will lead to biased results is that humans are unable to control their tendency to see psychological properties wherever they look, so if they look for psychological properties in animals they will certainly "find" them. Humans begin to attribute intentionality at a young age, and overattribution is ubiquitous among small children. Those scientists who are willing to see animal behavior as intentional and explained by reference to psychological properties might be stuck in such a youthful developmental stage. This bias seems to be what G. H. Lewes had in mind when he criticized his contemporaries Charles Darwin and George Romanes for talking about animal psychology. He wrote that "we are incessantly at fault in our tendency to anthropomorphize, a tendency which causes us to interpret the actions of animals according to the analogies of human nature."[3] Kennedy writes that "anthropomorphic thinking about animal behavior is built into us. We could not abandon it even if we wished to,"[4] though he also believes that it needs to be corrected.

The critics seem to suggest that the scientist must avoid this bias by moving far in the other direction: the bias toward seeing all animal behavior as intentional can only be confronted by denying that any animal behavior is intentional. While I do not deny the existence of the bias, I do deny some features of the proposed response to the bias, which is overreactive. Humans are replete with biases that affect our ability to make accurate judgments, such as the gambler's fallacy (e.g., thinking that repeated losses in roulette indicate that there will be future repeated wins), observer-expectancy effect (e.g., reinterpreting your past expectation so that it matches with reality), or the primacy effect (e.g., accepting as most plausible the first explanation you hear). The critic who says that the existence of a bias makes it impossible for us to do science related to that bias would be forced to deny the possibility of science at all! Thus, while scientists need to acknowledge the bias, its existence does not entail the impossibility of scientifically investigating animal psychological properties. Rather, it speaks to the need for a scientific methodology designed to counter the bias.

There are two other theoretical concerns that motivate anthropomorphic worries. One is that having language is necessary for having many if not all psychological properties. The other is that all behavior can be explained by Thorndike's laws, associative learning, or classical conditioning. Both of these concerns, I think, are

unjustified, or at least are limited in scope and potentially misleading. Let's take the second concern first.

The behavioristic principles of learning are used to explain behavior—for example, we can explain why Pavlov's dog salivated at the sound of a bell by indicating that Pavlov presented the dogs with the bell before he presented the dogs with the food, that it is a natural reflex that food produces salivation in dogs, and that such training is an example of classical conditioning. The critic thinks that all animal behavior can be likewise explained by reference to one of the behavioristic laws. However, to defend that claim, behavior types must be examined one by one, and that requires that we first have a catalog of every behavior type for each species. Biologists, psychologists, and anthropologists regularly uncover new behaviors, and so any claim that behavioristic principles can explain all animal behavior are premature. In addition, there are many behaviors, such as chimpanzee insight learning[5] and capuchin monkey finger-in-eye games[6] that do not appear to admit of non-psychological explanations.

The worry that language is necessary for (many) psychological properties is similarly flawed. There is little concern about avoiding psychological research with prelinguistic human infants due to adultomorphic concerns (i.e., attributing adult psychological properties to children), and if we can ask about psychological properties in some individuals who lack language, having language cannot be a necessary condition for having any psychological properties. One might object, however, that since the child is a potential language-user, a scientist is more justified in ascribing psychological traits to an individual who will eventually use language. But there are at least two reasons to reject this response: not all infants gain language and using language is just one kind of behavior.

In addition, the critic who says that language is necessary for thought may be relying on an argument from ignorance by claiming that language is the only possible vehicle to support the cognitive processes required to explain how thinkers are able to make logical inferences between propositions. For example, a familiar argument against the view that animals have beliefs is that to have a belief one must be able to represent a propositional attitude, and the only way to represent a propositional attitude is through language. But this claim is based on a number of controversial assumptions. For one, it assumes that an external spoken, written, or gestural language is necessary to have an internal language of thought. It also assumes that belief requires representation, which is a view that has been challenged by recent work in philosophy and cognitive science.[7] Finally, the argument assumes that there are no alternative representational vehicles other than language, a view that has similarly been challenged.[8]

Rather than starting with these theoretical commitments, a scientist can remain agnostic and examine whether there are target behaviors that seem to be explicable only in terms of an animal having a belief. Such empirical work can help to promote the theoretical research by providing a larger class of relevant data. The critics who see animal cognition research as anthropomorphic want to end such research; they do not promote it. And while it is true that if scientists stopped investigating whether

animals have psychological properties, they would be less likely to make false claims, they would also be doing less in the way of science. The general principle that we should avoid false claims should not cause us to stop making claims altogether, since that would also result in making fewer true claims. The best scientific methods are those that will maximize the number of true claims over the number of false ones, not the methods that will avoid false claims altogether.

Finally, related to the above discussion there is a methodological worry about anthropomorphism in animal cognition research. The field of psychology has long embraced a methodological rule of thumb that might be seen as a conservative principle: one should always avoid the risk of making a type-1 error in favor of the risk of making a type-2 error. The errors are defined as follows:

> Type-1 error – Rejecting a null hypothesis when it is in fact true.
> Type-2 error – Failing to reject a null hypothesis when it is in fact not true.

The null hypothesis is what is assumed unless and until investigation shows it to be false. In the case of animal cognition, the null hypothesis is that animals lack the particular psychological property under investigation. For example, in what is known in psychology as the theory of mind research program, the null hypothesis is that animals do not have the ability to consider others' mental states, or to attribute beliefs and desires to themselves or others. In the literature, this ability to attribute mental states is called a "theory of mind." So, a type-1 error in this context can be seen as a false positive, whereas a type-2 error would be a false negative. If in fact chimpanzees do not have a theory of mind, and some researcher concludes that the chimpanzee does have a theory of mind, then the researcher is committing a type-1 error. Some critics of animal cognition studies take this methodological principle as reason not to accept animal psychological properties; because we fail to have the required evidence that, for instance, the chimpanzee has a theory of mind, we conclude instead that the chimpanzee does not have a theory of mind.

There are several problems with this line of reasoning. First, the methodological principle does not permit the inferences to the nonexistence of chimpanzee theory of mind; rather, it requires that we remain agnostic about chimpanzee theory of mind. From this it would follow that we don't know whether or not having a theory of mind is uniquely human, and hence, we don't know whether it is anthropomorphic to attribute a theory of mind to an animal.

Second, it has been argued that the acceptance of the methodological rule of thumb has resulted in a behavioristic bias for animal cognition research. One piece of evidence for the supposed behaviorist bias is that while false positives in animal cognition research have a widely recognized name ("anthropomorphism"), false negatives do not. Some have argued that not having a well-established name for false negatives in animal cognition research leads researchers to have a behavioristic bias, and terms have been introduced as an attempt to combat this worry. Frans de Waal calls the false negative error "anthropodenial,"[9] while Maxine Sheets-Johnstone calls it "reverse anthropomorphism."[10]

In his discussion of the role that type-1 and type-2 errors play in animal cognition research, Sober says that both errors are:

> maxims of "default reasoning." They say that some hypotheses should be presumed innocent until proven guilty, while others should be regarded as having precisely the opposite status. Perhaps these default principles deserve to be swept from the field and replaced by a much simpler idea—that we should not indulge in anthropomorphism *or* in anthropodenial until we can point to observations that discriminate between these two hypotheses. It is desirable to avoid the type-1 error of mistaken anthropomorphism, but it is also desirable that we avoid the type-2 error of mistaken anthropodenial.[11]

While I agree with Sober's analysis, I think that the worst error here is anthropodenial because it hinders the progress of science. As a part of the scientific process, one must be willing to make a claim that turns out to be false, whether that claim is one that is antecedently accepted or not. For progress in science to be possible, one must be open to being wrong, one must ask questions even when the answer turns out to be no, and one must challenge the null hypothesis in order to examine its accuracy. The willingness to be wrong is a willingness to make type-1 errors in the course of the acquisition of new knowledge. Scientific progress does not take a linear path; there are bumps and errors along the way. He who wants to avoid error at all cost ought not be a scientist.

This concludes my discussion of the common criticisms brought against the animal cognition research program and its investigation into the psychological properties of animals. In responding to those criticisms, I intend to have defended the scientific respectability of empirically studying the psychological properties of animals. Now that it is established that we *can* study animal psychological properties, the question that we must answer is *how* we can engage in such study.

How Can We Study Animal Psychology?

Given that we can and should investigate issues in animal cognition, are there general methodological principles that we can use to do so? To help answer this question, we can look at a respected field of psychology that shares many of the challenges of research in animal cognition discussed previously. Developmental psychology research on prelinguistic infants also deals with subjects who cannot tell them what they think or how they feel. When devising research programs, both investigators who study human infants and investigators who study nonhuman animals propose to examine the minds of their subjects without relying on linguistic behavior.

The fact that we can't talk to these subjects might be seen as a limitation of the research programs, but it should not be so regarded. In studying older children and adults, psychologists rely heavily on linguistic behavior. The measures are more subtle than the introspective methods of the nineteenth century, but current

research still assumes that language permits us a more direct window into the mind than does nonverbal behavior. Of course, linguistic behavior is still behavior, and the relationship between language and thought is still hazy at best. The idea that language unproblematically gives us a window to the mind ignores both the worries of the philosophers and the psychologists. W. V. O. Quine's discussion of radical translation points to the difficulty of coming to understand, from the perspective of one language-user, what someone from another language group is thinking.[12] Donald Davidson's notion of radical interpretation is based on similar worries about our ability to understand what others mean.[13] For both Quine and Davidson, to understand others we must begin by accepting the principle of charity and take the behavior we observe to be rational, noncontradictory, and derived from the same sorts of causes as our own behavior is. That is, to get the interpretative task off the ground we must see others as like us in an important sense; we must observe from a particular interpretive stance.

Psychologists too worry about using linguistic utterances as a window into the mind. In contrast to the old philosophical principle of privileged access, according to which individuals have private and privileged access to the contents of their minds, and in contrast to the old psychological method of introspection, the New Unconscious research program in social psychology is finding that in many cases, our reports of our own mental states are confabulations. In one of the first research papers in this field, Richard Nisbett and Timothy Wilson presented evidence that people do not know why they do the things they do and will make up stories to account for how they solved physical problems.[14]

If we want to learn about the mechanisms of mind, then both the philosophers and the psychologists warn us away from giving linguistic behavior too much of a privileged position. Rather than taking research on infant and animal minds to pose a special challenge, we could equally well treat it as more straightforward than research on humans, since when studying animals and pre-linguistic infants, we will not be misled (either intentionally or unintentionally) by the participant's linguistic utterances.

Despite several critics (e.g., John Newson's worry about "adultomorphism,"[15] and Robert Russell's worry that our attribution of psychological properties to children is a non-universal cultural practice not warranted by science[16]), research on infant cognition is flourishing, and scientists express little concern about ascribing psychological properties to human children. As an example of suitable ascription to infants, it is generally accepted in development psychology that children have emotions, beliefs, and desires and can communicate some of these mental states by one year of age.[17] This is so despite the fact that one-year-old children typically do not have the ability to string words together to form propositions, and often have not yet begun to produce any words, which they typically do between twelve and eighteen months.

Why is it that the adultomorphism concern has less effect on child development research than the anthropomorphism concern has on animal cognition research? One possible justification for the difference may be that most human infants do

eventually develop language, and this potentiality could be exploited by the researcher as justification for making an attribution to a preverbal infant. This attempt at justification fails, however, because the mere potential for language doesn't help either with the concerns about the vehicle of the mental state or with concerns about specifying the content of the mental state. If one does not yet have external language, one cannot be thinking in language—unless external language is not necessary for thinking in language, and in that case there would be no reason to exclude animals from the class of thinkers.

I think there is an explanation for the different standards for studying human infants and nonhuman animals, but that this explanation doesn't justify it. The difference can be explained in term of the kind of relationships investigators have with their subjects. The traditional approach to science involving animal subjects has been to keep a distance between the scientist and her research. Researchers who violate this principle are often thought not to be objective. For example, Jane Goodall's insistence on naming the chimpanzees she was observing was unconventional and caused some worry about her ability to remain objective, as did her use of gender pronouns to refer to the chimpanzees.[18] While naming ape subjects has now become standard, the rationale behind the criticism remains. Even today, the quest for scientific rigor and objectivity still strongly encourages researchers to take a position of detachment and neutrality toward their research subjects. The degree of success one manages depends at least partially on how the scientist interacts with her subject. When the subject is in a cage, there is metaphorical distance between the caged and the free individual that can have affective consequences in the researcher. When the subject is across a field being observed using binoculars, the physical distance can also cause a certain emotional distance. But when a researcher is working with a human child, it is almost impossible to avoid all forms of emotional response to the subject. Humans are wired to have emotional responses to infants (and, as Konrad Lorenz pointed out, to animals that resemble human infants by having big eyes, big heads, and little noses).[19]

The relationship between human researchers and human subjects is strengthened due to their shared physical and social world. Psychologists see human infants in their normal physical and social environment, and often have spent much time interacting with infants socially or as caregivers, teachers, or other similar roles during practical aspects of their training. Psychologists who plan to work on infants typically have a lot of lay expertise with children and develop commonsense views about infants that inform their research. I will argue that this lay expertise forms an undeniable and beneficial starting position for the researcher's future work.

For many working in animal cognition, there does not exist the same sort of shared social and physical environment between researcher and subject. Researchers who focus on experimental laboratory research may never see the subject in its typical ecological and social environment. They may not spend time with their subjects outside of the research context. They may, indeed, work hard not to develop an emotional or sympathetic relationship with their research subjects. In addition, those who are working with species who exist in very different contexts from us,

such as water-living mammals or avian species, are limited in their ability to develop the same sort of folk expertise given the difficulty with spending large chunks of time with individuals of those species outside of the research context. In support of this view is the finding that fieldworkers are more likely to attribute psychological properties to animals than are those working in a controlled environment such as a zoo or a lab.[20] While some might see this as evidence that field researchers are biased, it may also be evidence that field researchers have better access to the cognitive and affective capacities of their subjects than do researchers on captive animals.

What difference in methodology between field research and experimental research of a captive animal accounts for this difference? It may be that those who choose to do fieldwork are more prone to attribute psychological states to begin with. But there is another possibility: experience in the field may involve the development of a skill that makes such researchers more likely to understand their subjects, just as investigators who study infants develop skills associated with handling and regulating infants. Fieldworkers who engage primarily in observational studies typically spend much more time with their subjects than do experimentalists, and they have to learn how to observe before they begin to see what is going on. It isn't until after an observer learns how to see, and learns the typical behaviors of the group being studied that she can develop an ethogram—a catalog of species-normal behaviors, and the functional roles associated with them—and only then can she conduct the formal observational study. The pre-study period of observation allows the scientist to get to know her subjects and understand the individual differences in a group, so it also gives her a baseline of normal behavior. I suspect that this sort of experience results in the development of a skill that allows the fieldworker to notice intentional behavior, much as experts across fields come to notice saliencies that otherwise would have been perceived as noise. Researchers who are working with students know that when a student first enters the field she has to learn how to see, much as X-ray technicians have to learn how to read X-rays. Graduate students who are collecting data in the field for the first time will discard their first weeks or months of data, or not take data during that time, because they are still developing the skill of observing. In classical ethology, this preliminary stage of observation is called "reconnaissance observation," and new students are given exercises to develop skills in the art of seeing.[21]

Others who are not explicitly trained how to see can also come to develop an understanding of what behaviors mean by implicitly recognizing the context of the behavior. This is true both of species-typical behaviors, and individual differences. For example, when I was at Samboja Lestari Orangutan Rehabilitation Centre, there was a young male orangutan named Jovan who had a special trick: he would suck on his thumb to disarm the human working with him, and then grab something from the caregiver (a pen, a backpack, etc.) and run away with it. Someone who didn't know Jovan would fail to interpret his thumb sucking correctly and fall for the trick, whereas a caregiver who was familiar with Jovan knew to protect her gear, because she knew what he *wanted*. We might think of caregivers and nonacademic observers of animal behavior as being "trained" by the situation insofar as they receive feedback from the animals that may or may not match their expectations.

The fieldworkers' and caregivers' experiences are notably similar to the sort of experiences humans who work with children have. We live in the field, as far as the study of infant cognition goes. There are few difficulties conducting naturalistic observations of children; there is no need to travel, live in makeshift camps, or deal with unfamiliar environments. Children are everywhere, and child development researchers may have thousands of hours of watching before they even begin their formal studies. It is this experience with children that explains in part why there is little scientific worry about investigating the psychological properties of human infants, and I propose that what fieldworkers and infant researchers have greater access to than laboratory experimentalists is what I have called *folk expert opinion*.[22]

Folk expertise develops with experience with a taxon, a developmental stage, or an individual. It is what one has when one knows one's subject well. Most humans are folk experts on human behavior given their experience with others. Parents are folk experts of their children, and career nannies have folk expertise about children generally. Nurses and caregivers become experts about their charges with dementia or other geriatric mental disabilities. Caregivers have acquaintance knowledge of their charges, a kind of knowledge that scientists or other formal experts may lack. Folk experts on animal behavior include human caregivers, technicians, and others who work with captive animals, as well as individuals who have spent a significant amount of time observing the behavior of individual animals in the field.[23] A folk expert can also be an academic expert, who has studied the species formally, but for an academic expert to become a folk expert, she needs to gain additional knowledge through direct experience observing or interacting with member of the species. A researcher's stock of anecdotes can be seen as part of her folk expertise. For example, while I was interning as a dolphin trainer at The Kewalo Basin Marine Mammal Laboratory in the 1990s, I observed a male dolphin force copulation on a protesting female, and this observation became part of my knowledge about dolphins. Having seen that forced copulation happens in dolphins, and seeing that the female dolphin was struggling to obstruct the sex act, I saw that sex under these conditions was aversive to the female. With this kind of knowledge, caregivers can take precautions to minimize the risk that the female is subjected to the experience again.

Though most humans are not experts in the behavior of exotic animals, most humans with children are experts in child behavior, and most humans are experts in some areas of adult human behavior. We gain this status as experts through our experiences interacting with people rather than through explicit instruction or formal training; expertise is something we have to some degree even before taking our first psychology class. We know that people have psychological properties, and we know something about how these psychological properties are related to one's environment and behavior. The academic expertise that is gained through formal education builds upon the 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 is itself 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. A child can soon come to think that a mean child is one who will not share his toys, who pulls hair, and who doesn't wait his turn for the slide. Calling this individual "mean" helps the child to understand how to deal with him and to predict what his future actions are likely to be.

In research on humans, folk expertise is sometimes explicitly recognized and used, for example, in some psychological assessment instruments. Parents, teachers, and caregivers answer questions about the target individual's behavior, emotional state, and so forth, and this information can be used by researchers to make judgments about, for example, personality or social adjustment. Since not all the folk expert's knowledge is directly available to her, psychologists interested in this knowledge design instruments to extract the knowledge. Psychological instruments are calibrated in part on the basis of their functionality. The results of these instruments are functional if they produce novel accurate predictions, and if the prediction bears out, the attribution is deemed accurate.

For example, the "Caregiver-Teacher Report Form for Ages 1 1/2–5" from the *Child Behavior Checklist*, which is designed to measure children's emotional and social development, is a checklist that is presented to parents or teachers. The caregivers are taken to be folk experts with knowledge that can be extracted using these measures, and they are asked to rate children's behavior and traits.

Having folk expertise is only the first stage of doing good science with animals. Folk experts can be wrong, just as parents can be wrong about their children. My suggestion is not to forgo science in favor of the folk experts' common sense. The starting point for controlled study of infant behavior is much more robust than is the starting point for controlled study of animal behavior, and I propose that the science of animal cognition research will progress only if we are able to improve its foundations.

Nonetheless, some may feel quite uncomfortable with the role I am giving to folk expertise as the foundation for doing good science. The worry is that relying on folk expertise is a bias, and will lead to false conclusions about animals' cognitive abilities. For example, one might worry about people's folk expertise regarding their pets as a prime example of unwarranted psychological attribution. The dog owner who sees that her dog destroyed the furniture may interpret her dog's head-hanging behavior as expressing guilt, despite the possibility that there exists a more parsimonious explanation from associative learning: that the dog had been conditioned to expect a scolding after similar acts in the past. Parents are notorious for suffering from similar delusions as pet owners, and making over-attributions when simpler explanations suffice. Given these obvious problems with folk expertise, shouldn't we rather try to eliminate it from our research on human children, rather than bring it into our research on animals?

I answer this question with an emphatic "No," given the fecundity this method has had for our study of infant behavior. From our science, we have gained a greater understanding of when attributions are false and when they are accurate, and we have done this by using methods that capitalize on our folk understanding of infants. Consider for example the habituation-dishabituation method of studying infants.[24] The method involves showing a human infant a stimulus until she is habituated to it, as indicated by either eye-gaze or by reduced sucking on a pacifier. The infant is then shown a new stimulus that differs from the original in some subtle way. If the infant's eyes move back toward the stimulus, or if the sucking rate increases, researchers conclude that the infant notices the difference. This method gives us interesting results only because our folk expertise of infants allows us to conclude that children are interested in things they look at, and that a high rate of sucking indicates interest in the stimulus. This appeal to the child's interest is an appeal to a basic mental process that has not been determined by additional scientific investigation, but from our infant folk psychology.[25]

In order to make similar progress on animal cognition research, we must work toward first achieving folk expertise in the species to be studied. Fieldworkers, who spend years observing individuals, come close to gaining the kind of folk expertise that infant researchers gain so easily. Folk expertise about a species will include knowledge and understanding about stages of development, culture, and species-normal behavior. Folk expertise about an individual will include knowledge about the individual's typical behaviors, and the extent to which those behaviors reflect individual differences in the species or developmental stage. My claim is that animal cognition researchers should develop folk expertise of their subjects, and base their research programs on knowledge gained from experience with the species in its natural physical and social environment, just as human-infant researchers do.

I turn now to examples of animal cognition research to examine research programs that both followed and did not follow the advice I am giving.

Two Problematic Cases: Gorilla Mirror Self-Recognition and Chimpanzee Economic Games

Not having folk expertise can lead to poorly designed studies that don't take into account species-normal behavior. I will discuss two cases of studies that suffer from this problem.

One example of this problem can be seen in the early research on mirror self-recognition in gorillas. The research program on mirror self-recognition began with the work of Gordon Gallup. While there was anecdotal evidence that chimpanzees recognized themselves in mirrors, there was no formal test until Gallup introduced the mirror test for chimpanzees. He exposed four juvenile chimpanzees to a

mirror for eighty hours, during which time the chimpanzees first responded socially to the mirror image before they began using the mirror to explore their own bodies. Chimpanzees would examine their teeth and other parts of their body that are not accessible without the aid of a mirror. After this initial exposure, Gallup gave the chimpanzees the mark test by placing red marks on their faces while the animals were anesthetized. After they woke from the anesthesia, the chimpanzees were observed for some time and then exposed to a mirror. Gallup found that chimpanzees began to touch the marks on their face after being given the mirror, and that they touched the mark significantly more often in the presence of a mirror than when no mirror was present. Given that chimpanzees passed the mirror task in this way, Gallup concluded that chimpanzees understand that the image in the mirror is a reflection of the self and hence they have an understanding of self.[26]

This study was devised for use with chimpanzees, but the methods of Gallup's mark test were also used on other primates, including human children. While children of eighteen months and orangutans passed the task, early research on gorilla mirror self-recognition concluded that adult gorillas, unlike the other great apes, do not respond to their reflection. Given this negative finding it was suggested that gorillas might "be the only great ape which lacks the conceptual ability necessary for self-recognition."[27] And while subsequent studies confirmed the negative result, there is clear evidence that one gorilla does recognize herself in the mirror.[28] Koko, a gorilla who started learning sign language at one year of age, started to spontaneously show mirror-guided self-directed behaviors when she was about three-and-a-half years old, picking her teeth, combing her hair, and dressing up in wigs, hats, and makeup in front of the mirror. Using a variant of Gallup's task, Koko was tested when she was nineteen years old, and she, like the chimpanzees, touched the mark when exposed to the mirror significantly more often than when there was no mirror present. These findings were cross-validated by asking her questions about her reflection. In response to the "Who is that?" question, Koko signed in response, "ME THERE KOKO GOOD TEETH GOOD."

This evidence strongly supports the claim that Koko recognizes herself in the mirror and thus raises the question of why other gorillas fail the mark task. One might initially think that Koko's language training provided her with the kinds of concepts necessary for passing the mark task, but since human children can pass the test before they have language, it isn't clear why having a symbolic communicative system should aid in this task.

Drawing on their folk expertise with gorillas, Patterson and Cohen suggest another explanation for the failure of other gorillas to pass the mirror test: The gorillas found the presence of unfamiliar experimenters aversive. They write:

> It has been our experience that the presence of strangers profoundly affects gorilla behavior. We have found that it can take from several months to a full year for Koko and Michael to habituate to the presence of a new caretaker. . . . In each of the previous formal self-recognition studies with gorillas, experimenters who were not the gorillas' caretakers were in the room with them in very close proximity to the mirror. . . . Averting their gaze from strangers is a common

> behavior in gorillas. Observed social responses to the mirror may have been elicited by the experimenters, whereas mirror gazing and self-directed behaviors may have been inhibited by their presence.[29]

This explanation points to a difference between chimpanzees and gorillas: only gorillas tend to avoid the gaze of strangers. When first exposed to a mirror, the chimpanzees treated it socially, as if the reflection was a stranger. If gorillas find the gaze of strangers aversive, they have a strong motivation to avoid interacting with the mirror long enough to realize its function. That is, there is a difference in species-normal behavior between gorillas and other apes that can account for the early suggestions that gorillas don't have an understanding of the self. Modifications of the mark task, to account for the problem of motivation, found that gorillas do recognize themselves in mirrors.[30]

The lesson from the gorilla mirror self-recognition studies is that understanding the species is essential for devising studies to examine the psychological properties of an animal. Negative results are as important as positive results, and both need to be disseminated as part of the project of determining what psychological properties are attributable to a species or individual. But negative results are only valuable if they are based on a foundation of folk expertise. If the experimenter doesn't know that gorillas suffer from xenophobia and find the gaze of strangers aversive, then she might not take this variable into account when designing her study. But in such a case, the negative findings that result do not tell us anything about the gorilla.

A second example of a study that fails to follow the proposed method is the work on economic games in chimpanzees, research modeled on the groundbreaking work of Werner Güth and his colleagues.[31] In the original studies with humans, Güth and colleagues found that the traditional view of economic decision making, according to which people act according to the goal of maximizing resources for themselves and act rationally in pursuit of that goal, is false. Rather, by using an ultimatum game paradigm, the authors found that humans value the norm of fairness in the distribution of resources. In the standard version of the ultimatum game, two individuals are randomly assigned the roles of proposer and responder. The proposer is offered a sum of money and can decide to offer some portion of it to the responder. If the responder accepts the offer, both parties keep the money. However, if the responder does not accept the offer, then neither player gets anything. In humans, divisions that are perceived as unfair are often rejected, and the explanation for this is that humans have goals other than maximizing resources. Importantly, humans are sensitive to the interests and goals of others, and will make personal sacrifices in order to follow norms of fairness and cooperation, and punish transgressors.

It was found that chimpanzees and humans respond differently in ultimatum games. In order to test for fairness in apes, Keith Jenson and colleagues gave a version of the ultimatum game to a group of eleven chimpanzees in a controlled laboratory setting.[32] They found that the chimpanzees are more like the idealized rational man of traditional economic theory than are humans because chimpanzees, but not humans, fail to reject unequal divisions of resources. The conclusion is that

chimpanzees, unlike humans, are not concerned with fairness, and are much closer to Adam Smith's *Homo economicus* than humans turn out to be.

However, the normative conclusion of this research doesn't take into account species-normal behavior. For one, the conclusion that chimpanzees are not concerned with fairness is inconsistent with evidence from ethology and other research programs.[33] Just to give one example, Frans de Waal writes, "I once saw an adolescent female interrupt a quarrel between two youngsters over a leafy branch. She took the branch away from them, broke it in two, then handed each one a part."[34] Interventions such as this are common among chimpanzee societies, as is punishment of negative actions.[35]

In addition, in the human studies the experiments are based on species- (or at least cultural-) normal behavior. Plausibly, in our society there is a norm that when you fall into unexpected wealth, you share that wealth with others. For example, we seem to expect lottery winners to share their winnings, and in fact one survey of U.K. lottery winners found that 83% of those who won over 50,000 pounds in the lottery gave money to family members.[36] The existence of this norm is also demonstrated by the controversy surrounding Bill Gates's unexpected mega-wealth. Before he started the Bill and Melinda Gates Foundation, he was called a miser, and there was discussion about how the current class of the superwealthy differed from those of another time.

Given the existence of the norm "Unexpected wealth should be shared," when humans play the ultimatum game and their partner violates that norm, we should expect the actual result—that human players punish the partner who violates the norm. The poor offer to the partner is seen as unfair because it violates the norm of unexpected wealth, but if there were no such norm among humans, then we shouldn't expect the behavior to be seen as unfair or to be punished. The sentiment of fairness is based on a background expectation about normal behavior.

To claim that the chimpanzees do not have a sense of fairness simply because they fail a test based on the human norm of unexpected bounty is to assume that this human norm can be translated into chimpanzee societies. That is, we need to know whether species-normal behavior at the stage of development of the chimpanzee participants involves a norm about sharing unexpected bounties. And it seems that, for chimpanzees, there is no norm about sharing food resources; it is not part of their natural interactions.[37] While chimpanzees do share food in some circumstances, such as the meat that is acquired through cooperative hunting,[38] the ultimatum game does not reflect a norm about sharing jointly earned resources, and so the chimpanzee meat-sharing behavior cannot be seen as evidence for the existence of an unexpected wealth norm. That is, if this research had been based in folk-expertise on chimpanzee behaviors, researchers would never have asked whether chimpanzees have a concept of fairness by examining whether they accept inequitable distributions of goods.

When a human wins the lottery, she is expected to share some of her winnings with others. Nevertheless, when a human wins the sexual lottery and finds a good mate, she is not expected to share those winnings. To test the chimpanzee concept

of fairness by examining whether they share food is like testing the human concept of fairness by examining whether they share sexual access to mates. If an extraterrestrial researcher were to study the human sense of fairness by examining whether humans share their mates, the researcher might hastily conclude that humans don't have a sense of fairness. But the more sensitive researchers might try the study with different goods. Humans don't generally share things like sexual access to partners, toothbrushes, and so forth. Chimpanzees don't generally share food. If we want to know whether the chimpanzee has a sense of fairness, we first need to see whether there appears to be any relevant chimpanzee norm that could be tested or otherwise examined, and to do that relies on having some folk expert understanding of the species.

The failures of the early gorilla mirror-recognition work and the chimpanzee fairness studies both stem from a lack of knowledge about the species. To avoid such problems, I suggest that research on animal psychological properties must begin with folk expert opinion, just as our infant cognition studies are. What's good for the infant studies is good for the animal studies, so far as it goes. That is, if starting with folk expert opinion is a warranted starting point for human studies, it should be a warranted starting point for animal studies. Opinion based on folk expertise is a largely unacknowledged starting point for the human studies, and should be seen as comprising an important aspect of the methodology of infant studies. 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. I'm suggesting that acceptance of the methodology should be based on the same considerations, whether the subject is an infant human or a member of another species.

Successful use of this approach can be seen in some research programs. Let me present one area of research that begins with knowledge of species-normal behavior and relies on folk expertise in the attribution of psychological properties to animals: the research on personality traits.

A Successful Case: Animal Personality Traits

In human psychology, it is taken for granted that there are individual differences, and that these differences can be seen in terms of differences in personality traits. The Five-Factor Model (FFM) of human personality 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.[39] While there are some

theoretical and methodological worries about the FFM, supporters of this approach have argued that most individual differences can be described using this model[40] and that there are underlying genetic factors related to these domains.[41]

An individual's personality traits can be assessed using an instrument such as the Five-Factor Personality Inventory (FFPI). While the FFPI can be administered to the subject who is asked to make self-ratings, it is thought to be more accurate when it is given to a number of individuals who know the subject well.[42] Administering the instrument to people who know the individual well is seen as more accurate than administering the instrument to the target subject, because the responses to questions about oneself invoke social goals such as image control.

The use of third parties to assess a target subject is a common approach of psychological instruments. Children's emotional and social development is assessed using *The Child Behavior Checklist*.[43] *The Vineland Adaptive Behavior Scales*[44] is used to assess the personal and social skills of children and low-functioning adults. Geriatric patients' social and functional impairments are assessed using the *Social-Adaptive Functioning Evaluation*.[45] All these instruments rely on third-party judgments in order to evaluate a target subject, and since they require that the third parties know the subject well, they explicitly rely on the knowledge of a folk expert. In some cases, there is no option but to use a third party's response to make judgments about the target subject; from children with delayed language skills to elderly people suffering from dementia, such tests are relied upon in order to assess the personality, social development, intelligence, emotional adjustment, communication skills, and other psychological factors of children who cannot speak for themselves and adults who are low-functioning or suffering from dementia.

These instruments rely on the judgments of caregivers who do have language and the relevant concepts, and who are folk experts on the individual being examined. The assumptions behind these instruments are that caregivers have knowledge of their charges, and that this knowledge can be extracted. However, the instruments do not take the views of the caregivers at face value. Instead, the folk expert opinion is used as raw data. The use of such instruments follows the proposed methodology for animal cognition research: Begin with folk expert opinion, and then use established scientific methods to determine whether a psychological property is attributable to an individual.

Given the widespread use of these kinds of instruments to assess psychological properties of individuals based on the folk expertise of those who know the target subject well, it is a natural extension to use this method to investigate personality in nonhuman species. Several species have been studied, but I will focus on the research on personality in great apes. To assess the existence of ape personality traits, researchers spoke with folk experts such as zookeepers and others involved in daily husbandry or training activities in order to develop an instrument for assessing personality using the same methods used in developing the human FFM,[46] and used this method to assess personality in chimpanzees[47] 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 (according to 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.[48]

After administering the instrument to a number of raters, their responses were assessed for statistical reliability both within and between raters. It was found that the individual differences in chimpanzees and orangutans are grouped together by factor analysis just as they are in the case of humans. However, differences were found between species in the content of the factors. For one, six personality factors were found in chimpanzees; they found correlates for all the human factors, plus an additional factor for dominance.[49] 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.[50] Here we have one example of a research program that follows the general methodological suggestion outlined above: start with folk expertise, and then develop research paradigms along the lines of other paradigms used with pre-linguistic children.

Despite the elegant simplicity of the animal personality research, there are a number of concerns about this research program. First, one might have general problems with the factor analysis of personality. However, this problem is not for animal research per se, but rather a criticism that is equally applicable to the human research, so I here set that concern aside.

Another worry is that since instruments such as the Orangutan Personality Trait Assessment rely on the judgments of individuals who are familiar with the target subjects, one might worry that the folk judgments of a number of people could all be wrong; there may be concerns about justifying ascriptions based solely on consensus. In fact, the coherence of the folk experts' opinions might indicate the existence of an implicit collusion. The experts might, in their discussions of their charges, begin to speak about individuals in a certain way, and thus jointly construct narratives of individuals that consist of developed personality when no such personality actually exists. Given the existence of this socially constructed narrative, when the instrument is administered the caregiver might think more about the narrative than the animal.

However, this scenario is unlikely, because the narratives that are constructed about the individual animals are constructed because they are useful. With continued research that examines traits across sites, such worries can be eliminated. For example, the caregivers might talk of one orangutan as the policeman because he always intervenes in unbalanced fights and protects the more vulnerable members of the community. But this picture of the individual is given because it allows the caregivers to predict what the individual will do, and, for example, might be relied upon when deciding whether or not human intervention is required. The impressions

caregivers have of their charges is based on a familiarity through association with their charges. In the case of the intersubjective expert opinion of caregivers, consensus is not reached because the experts trained together in how to correctly rate individuals on personality surveys. The instruments are used to capture the kind of knowledge that is developed through hands-on experience with the subject. As a further protection against this worry, the researchers collect data from different groups of humans in order to minimize the danger of collecting shared interpretive frameworks.

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. Our adult human folk psychology, just like a parent's folk child psychology or a zookeeper's folk orangutan psychology, is not something learned at a teacher's knee. It is a strategy for understanding behavior that is adopted because it is pragmatically useful; it allows us to make predictions we couldn't make before, and it allows us to understand our charges, to find creative solutions to an individual's emotional or social problems. If it is useful to apply a personality trait to an animal, then it is meaningful to do so, just as it is in the case of human beings. But this emphasis on pragmatism shouldn't be interpreted as anti-realism about the traits being identified. Rather, such pragmatically useful methods of classification are means of uncovering objective features in the world; for example, we know that there are underlying genetic factors related to the identified traits in humans.[51] Further research may uncover the same in other species.

Implications for Other Studies: Theory of Mind

Understanding the species or the individual is the starting point for studies of human cognition, and if we hope to make progress in studies of animal cognition, we must attempt to gain the same degree of expertise with regard to the species under examination. Research on chimpanzee theory of mind is an area that could benefit from greater attention to the folk expertise of those who work closely with chimpanzees.

First, I will present a brief history of the theory of mind research program. This program began with David Premack and Guy Woodruff's investigation into whether a chimpanzee can attribute states of mind to others.[52] While research on chimpanzee theory of mind languished for two decades thereafter, it became an active research program with human children, where the emphasis was placed on the ability to attribute belief. The false-belief task, which was designed to test for belief attribution in children, was undoubtedly inspired by folk expertise. In his commentary on Premack and Woodruff's article, Daniel Dennett pointed out that young

children watching Punch and Judy puppet shows squeal for joy when Punch is about to push a box over a cliff; though Punch thinks that Judy is still in the box, the children know that Judy snuck out when Punch wasn't looking.[53] This folk knowledge of children was used to devise the false-belief task, in which participants watch a puppet show of Maxi hiding a piece of chocolate before leaving the room. While Maxi is out, his mother finds the chocolate and moves it to another location. Maxi returns to the scene, the show is stopped, and the participants are asked to predict where Maxi will go to look for his chocolate.[54]

While research on children's theory of mind thrived, the chimpanzee research largely failed to find experimental evidence that chimpanzees understand belief.[55] However, this negative result may be a result of not relying on folk expertise as a starting point. That is, the criteria of evidence need to be reconceived so that it is salient for chimpanzee subjects. Dennett, in his layman's interaction with children, noted that children find the false belief in the Punch and Judy show very entertaining. Tests of chimpanzee theory of mind that are modeled on the false-belief task and that fail to find evidence that chimpanzees understand belief are based on folk expertise about *children*, not chimpanzees. And chimpanzees are quite different from human children.

To construct a good chimpanzee theory of mind task, one that is founded on folk expertise about chimpanzees, researchers can look for natural behaviors in chimpanzees that are correlative to the children's behavior in the Punch and Judy show. Some research on theory of mind did just that, and recognized the importance of beginning with species-normal behavior. After countless studies suggesting that chimpanzees have nothing resembling a theory of mind, Hare and colleagues designed a competitive task to test whether chimpanzees understand what others can and cannot see, and he found that chimpanzees do understand such seeing.[56] Earlier research on the chimpanzees' understanding of seeing and theory of mind rely on cooperation with a human caregiver.[57] To explain the difference in findings, Hare and colleagues write,

> perhaps the communicative situations of these latter [cooperative] studies may be unnatural for chimpanzees, who have not evolved for this kind of cooperative communication over monopolizable food resources and who do not normally experience in their individual ontogenies others helping them to find food.…Chimpanzees' most sophisticated social-cognitive abilities may emerge only in the more natural situations of food competition with conspecifics.[58]

Here, the authors are acknowledging the importance of starting with species-normal behavior and using our knowledge of that behavior in devising experiments to test for psychological properties.

While the food-competition studies do not offer a definitive answer to the question of whether chimpanzees understand others' perceptual states, they are examples of how folk expertise can inform experimental design. While there are not yet experiments concerning chimpanzees' understanding of belief that are based on the expertise of caregivers, researchers who have worked closely with apes are often willing to attribute something like a theory of mind to their charges. Savage-Rumbaugh

writes, "there can be no doubt that Kanzi attributes intentions and feelings to others and that he recognizes the need to communicate things about his own mental state to others."[59] This judgment isn't based on the results of formal studies, but rather is the result of her thirty-five years of close work with bonobos. The judgment comes from what one might deride as anecdotes: that Kanzi plays pretend games, that he takes advantage of new caregivers by getting them to allow him to do things he is not normally allowed to do, and that he doesn't believe everything he is told. For example, Savage-Rumbaugh reports one incident in trying to educate Kanzi about the danger of electrical outlets. She writes:

> I have just tried to tell Kanzi that "shocks" come out of the wall—that the small hole in the wall is dangerous and can hurt him badly. It is clear that he understands something of what I have said to him, because he approaches the outlet with extreme caution, his hair on end. He smells it, he looks at it, he even throws something at it gingerly. The outlet just sits there. Kanzi stares at me with a rather incredulous look on his face—why, he wonders, do I think this thing is dangerous, and why did I lecture him so when he started to stick a screwdriver into it? . . . Waiting until I was not looking, he carefully hid the screwdriver under a blanket. Then, when I was thoroughly occupied . . . he removed the screwdriver from its hiding place and placed it directly in the outlet. . . . He stood ramrod straight, and his hair rose two inches. He yanked the screwdriver out of the socket and immediately burst into a series of emphatic "Waa" sounds.[60]

As a symbol-trained bonobo who is able to communicate with his human caregivers, Kanzi is certainly a special case, and the point is not to insist that Kanzi has a theory of mind or that a study of theory of mind in chimpanzees should start with an understanding of a single bonobo. Rather, the point is that Savage-Rumbaugh thinks that Kanzi has something like a theory of mind because she has a relationship with him that she describes as one of mutual empathy, and she knows who he is as an individual. Savage-Rumbaugh's special relationship with Kanzi may be used as a basis for hypothesis generation, and it can be used to design a formal test of theory of mind that would be appropriate for Kanzi. For tests of non-enculturated chimpanzees, the same sort of folk expertise is required.

Conclusion

I have argued that we must investigate hypotheses about the psychological properties of animals without prejudice. Researchers should report both positive and negative findings in order to determine whether we ought to reject the null hypothesis that animals don't have psychological properties. But negative findings are only valuable if they are based on a foundation of folk expertise. Experiments for determining whether or not an animal has some psychological property must be informed by the same folk expertise that informs our creation of experiments on human children. We ought not assume that an experimental

paradigm that works on one species or at one developmental stage will work on other species or at other stages.

Using folk expertise and knowledge of species-normal behavior as a starting point acknowledges the fact that psychology is a product of evolution, and that it evolved to cope with the natural social and physical environment of the species. If an animal has a psychological property, we should expect to see evidence of it in the animal's naturalistic interactions. And if the methods of ethology are not sufficient for determining the existence of a psychological mechanism, then we can use an experiment based on the naturalistic event in order to formally test for it. Naturalistic observations and folk expertise go hand in hand. Psychologists working with infants are utterly dependent on it, and so should those working on animal cognition be.

NOTES

1. Henceforth, all mention of "nonhuman animal" will be shortened to the less accurate but stylistically preferred term "animal."

2. John S. Kennedy, *The New Anthropomorphism* (New York: Cambridge University Press, 1992).

3. George Henry Lewes, *Seaside Studies at Ilfracombe, Temby, the Scilly Isles, and Jersey* (Edinburgh: William Blackwood & Sons, 1860), p. 385.

4. Kennedy, *New Anthropomorphism*, p. 5.

5. Wolfgang Kohler, *The Mentality of Apes* (New York: Harcourt, Brace, 1925).

6. Susan Perry, Mary Baker, Linda Fedigan, Julie Gros-Louis, Katherine C. MacKinnon, Joseph H. Manson, Melissa Panger, Kendra Pyle, and Lisa Rose, "Social Conventions in Wild White-Faced Capuchin Monkeys: Evidence for Traditions in a Neotropical Primate," *Current Anthropology* 44 (2003): 241–68.

7. For arguments against representationalism from cognitive science, see, e.g., Rodney A. Brooks, "Intelligence without Representation," *Artificial Intelligence* 47 (1991): 139–59; Esther Thelen and Linda B. Smith, *A Dynamic Systems Approach to the Development of Cognition and Action* (Cambridge, Mass.: MIT Press, 1994). Philosophical arguments against the representational nature of cognition include Andy Clark and Josefa Toribio, "Doing without Representing," *Synthese* 101 (1994): 401–31; Tim Van Gelder, "What Might Cognition Be, If Not Computation?" *Journal of Philosophy* 92 (1995): 345–81.

8. For arguments in favor of a cartographical representational system, see, e.g., Elisabeth Camp, "Thinking with Maps," *Philosophical Perspectives* 21 (2007): 145–82; David Braddon-Mitchell and Frank Jackson, *The Philosophy of Mind and Cognition* (Oxford: Oxford University Press, 1996); Michael Rescorla, "Cognitive Maps and the Language of Thought," *British Journal for the Philosophy of Science* 60 (2009): 377–407.

9. Frans B. M. de Waal, "Anthropomorphism and Anthropodenial: Consistency in Our Thinking about Humans and Other Animals," *Philosophical Topics* 27 (1999): 225–80.

10. Maxine Sheets-Johnstone, "Taking Evolution Seriously," *American Philosophical Quarterly* 29 (1992): 343–52.

11. Elliott Sober, "Comparative Psychology Meets Evolutionary Biology: Morgan's Canon and Cladistic Parsimony," in *Thinking with Animals: New Perspectives on*

Anthropomorphism, ed. Lorraine Daston and Gregg Mitman (New York: Columbia University Press, 2005), pp. 85–99.

12. Willard Van Orman Quine, *Word and Object* (Cambridge, Mass.: MIT Press, 1960).

13. Donald Davidson, "Radical Interpretation," *Dialectica* 27 (1973): 313–28.

14. Richard E. Nisbett and Timothy D. Wilson, "Telling More Than We Can Know: Verbal Reports on Mental Processes," *Psychological Review* 84 (1977): 231–59.

15. John Newson, "The Growth of Shared Understandings between Infant and Caregiver," in *Before Speech: The Beginning of Interpersonal Communication*, ed. Margaret Bullowa (London: Cambridge University Press, 1979), pp. 207–22.

16. Robert L. Russell, "Anthropomorphism in Mother-Infant Interaction: Cultural Imperative or Scientific Acumen?" in *Anthropomorphism, Anecdotes, and Animals*, ed. Robert W. Mitchell, Nicholas S. Thompson, and H. Lynn Miles (Albany: State University of New York Press, 1997), pp. 116–22.

17. John H. Flavell, Patricia H. Miller, and Scott A. Miller, *Cognitive Development*, 4th ed. (Upper Saddle River, N.J.: Prentice Hall, 2002).

18. Mary Midgley, "Being Objective," *Nature* 410 (2001): 753.

19. Konrad Lorenz, "Part and Parcel in Animal and Human Societies," in *Studies in Animal and Human Behavior*, vol. 2 (Cambridge, Mass.: Harvard University Press, 1971), pp. 115–95.

20. Alexandra Horowitz and Marc Bekoff, "Naturalizing Anthropomorphism: Behavioral Prompts to Our Humanizing of Animals," *Anthrozoos* 20 (2007): 23–35.

21. Philip N. Lehner, *Handbook of Ethological Methods* (Cambridge: Cambridge University Press, 1996).

22. Kristin Andrews, "Politics or Metaphysics? On Attributing Psychological Properties to Animals," *Biology and Philosophy* 24 (2009): 51–63.

23. Others also point out that there is great knowledge in those who work closely with animals. Bernard Rollin 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" ("Anecdote, Anthropomorphism, and Animal Behavior," in *Anthropomorphism, Anecdotes and Animals*, ed. Robert W. Mitchell, Nicholas S. Thompson, and H. Lynn Miles [Albany: State University of New York Press, 1997], p. 128). However, we shouldn't conclude that all those who work with animals are sensitive to their behavior. Being a folk expert requires having a certain quality of relationship, and it is likely that those caregivers who have a nurturing relationship with their charges are the ones who have developed the greater understanding of their personality, capability, and intentions.

24. Renée Baillargeon and Julie DeVos, "Object Permanence in Young Infants: Further Evidence," *Child Development* 62 (1991): 1227–46; Elizabeth S. Spelke, "Physical Knowledge in Infancy: Reflections of Piaget's Theory," in *Epigenesis of Mind: Studies in Biology and Cognition*, ed. Susan Carey and Rochel Gelman (Hillsdale, N.J.: Erlbaum, 1991).

25. The Habituation-Dishabituation Method has also been used to study primate concepts; for example, the number concept was studied in rhesus monkeys (Marc D. Hauser, Pogen MacNeilage, and Molly Ware, "Numerical Representation in Primates," *Proceeding of the National Academy of the Sciences* 93 [1996]: 1514–17) and cotton-top tamarins (Claudia Uller, *Origins of Numerical Concepts: A Comparative Study of Human Infants and Nonhuman Primates* [Cambridge, Mass.: MIT Press, 1997]).

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27. David H., Ledbetter and Jeffry A. Basen, "Failure to Demonstrate Self-Recognition in Gorillas," *American Journal of Primatology* 2 (1982): 307–10.

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29. Patterson and Cohn, *Self-Recognition and Self-Awareness in Lowland Gorillas*, p. 286.

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32. Keith Jensen, Josep Call, and Michael Tomasello, "Chimpanzees Are Rational Maximizers in an Ultimatum Game," *Science* 318 (2007): 107–9.

33. Marc Bekoff and Jessica Pierce, *Wild Justice: The Moral Lives of Animals* (Chicago: University of Chicago Press, 2009).

34. Frans B. M. de Waal, *The Age of Empathy: Nature's Lessons for a Kinder Society* (Toronto: McClelland & Stewart, 2009).

35. Frans B. M. de Waal and Lesleigh M. Luttrell, "Mechanisms of Social Reciprocity in Three Primate Species: Symmetrical Relationship Characteristics or Cognition?" *Ethology and Sociobiology* 9 (1988): 101–18.

36. Gary Finn, "Lottery Winners Reveal How Much They Gave Away," *The Independent*, November 15, 1999 at http://www.independent.co.uk/news/lottery-winners-reveal-how-much-they-give-away-1126242.html.

37. Brian Josep Call, Bryan Agnetta, and Michael Tomasello, "Chimpanzees Know What Conspecifics Do and Do Not See," *Animal Behaviour* 59 (2000): 771–85.

38. Christophe Boesch, "Cooperative Hunting in Wild Chimpanzees," *Animal Behavior* 48 (1994): 653–67; Christophe Boesch, "Cooperative Hunting Roles among Taie Chimpanzees," *Human Nature* 13 (2002): 27–46.

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42. A. A. Jolijn Hendriks, Willem K. B. Hofstee, and Boele De Raad, "The Five-Factor Personality Inventory," *Personality and Individual Differences* 27 (1999): 307–25.

43. Thomas M. Achenbach and Craig Edelbrock, *Manual for the Child Behavior Checklist and Revised Child Behavior Profile* (Burlington, Vt.: Queen City Printers, 1983).

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