

THE MANY PERILS OF EJECTIVE ANTHROPOMORPHISM

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Anthropomorphism is the use of categories appropriate to humans for the study of animals or objects. Behavioral anthropomorphism is a very common feature of our day to day speech. Just in the last few days, I have heard a cat described as being "lonely," a computer described as "improvising," a gear box as "protesting," storm winds as being "angry," the stock market as "lacking willpower," and a tomato plant as looking "dejected." Do these anthropomorphic expressions merely entertain, or do they represent a kind of "street truth" about the things they describe that might form the basis for scientific insight?

There was a period when anthropomorphism was raised to the level of a scientific method. The chief engineer of this levitation was George Romanes (1882, 1883, 1884, and 1889), a turn-of-the-century Oxford comparative psychologist who was close to Charles Darwin and interested in developing Darwin's thoughts on mental evolution. Romanes, of course, has not been the only animal behaviorist to try to use ejective anthropomorphism as a sort of scientific method. Darwin himself used mental words to refer to animal behavior, as do some modern writers such as Donald Griffin (1976). I focus on Romanes' method here, because his was an early, influential, and highly self-conscious version of anthropomorphism, one for which the issues were exceedingly clear (Wasserman, 1984).

Romanes' Ejective Mentalism

Romanes' method was founded on a distinction between subjective and objective observation. When you or I want to know about our own mental states, Romanes' argument runs, we have only to make subjective observations. Subjective observations are those that we make directly of our own mental states via introspection. Introspection is regarded as a direct, veridical perception of states of our own mind: our thoughts, our feelings, our perceptions. The power of introspection is sometimes called "privileged access" because the mind's owner is thought to have a uniquely direct awareness of his or her own mental processes (Dennett, 1978).

Objective observations are those that you or I make of the mental activities of

AUTHOR'S NOTE:

I am indebted to Patrick Derr, Peter Lipton, Cheryl Logan, Robert Mitchell, Walter Wright, Debbie Moore, and Penny and Caleb Thompson for assistance in thinking up and putting down the thoughts contained in this paper. Address all correspondence to: Nicholas S. Thompson, Departments of Biology and Psychology, Clark University, Worcester, MA 01610; IN% "nthompson@clarku.vax.edu"

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another person. But we can only observe the manifestations of those mental activities in the behavior and verbalizations of that person. We can, of course, make objective observations of ourselves, just so long as we don't make use of our privileged access to our own minds. We can observe our own behavior and we can surmise our own mental states from that behavior as another person would. Thus we might infer that we are nervous from the fact that we spill a glass of water at a business lunch or that we don't like a place from the fact that we keep avoiding it. Since, according to Romanes, we have two sources of information about our own minds—our own observation of the objective consequences of our mental states *and* our direct observation of those mental states—and only one source of information about the mental states of others, we are better situated to understand our own mental states than understand the mental states of others.

From this postulate about the superior state of our information about our own minds comes, in Romanes' system, the opportunity to use knowledge of our own minds to understand the minds of others. Romanes' method involves putting together our knowledge about the relationship between the two sources of information concerning our own minds with our knowledge of objective information about others' minds to make an inference concerning the subjective state which accompanies their objective behavior. This intuiting does not result in subjective knowledge because we are not privy to others' minds; neither is the knowledge we derive strictly speaking objective, because we have made use of our own subjective experience to interpret the objective behavior of the other. To distinguish it from objective and subjective observation, Romanes calls his method *ejective*. Romanes explicitly regards his method as a form of analogical reasoning. From his knowledge of the relationship of his mental states to his acts, he can infer which mental states underlie the acts of others just because he believes that:

$$B_{\text{Romanes}} : M_{\text{Romanes}} :: B_{\text{Others}} : M_{\text{Others}}$$

In other words, he believes that the relationship between others' mental states and their own behavior is the same as the relationship between his own mental states and his own behavior.

When the ejective method is applied to animals, it becomes a systematic form of anthropomorphism in which the observer derives the anthropomorphic categories from the examination of her own personal experience. Romanes' goal in developing his ejective method was to provide an empirical basis for developing a theory of mental evolution. The study of mental evolution requires the comparative study of mental states in animals and that, in turn, requires some method for knowing about animal mental states. The ejective method was Romanes' way of gaining that knowledge.

Logically, the justification for Romanes' method stands on four premises. The first is a belief in an introspective capacity called *privileged access*. For Romanes' method to be useful, each of us must have a special kind of insight into his or her own mental activities. Otherwise, there is no reason to look into our own minds before we try to understand the minds of others. Why? Because, in the absence of privileged access, looking at one's own mind is just like looking at the mind of the

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other. And since comparative animal mentalists are interested in the minds of other animals, they would look directly at those minds, instead of pausing to look at their own minds along the way.

The second premise underlying Romanes' method is *explanatory mentalism*. Romanes himself might have disputed that he was interested in a causal account. He sometimes took the position of a psychophysical parallelist, arguing that mental states accompanied stimulus and behavioral events without being causally engaged with them (e.g., Romanes, 1882). It is difficult to take these disclaimers seriously. Romanes' analysis of particular cases makes free use of stimulus events to explain mental states and mental states to explain behavior. Furthermore, if one is going to make use of the ejective method to understand *why* animals do what they do, explanatory mentalism is essential. The scientist's introspection into his own mental states does not help explain behavior unless mental states are themselves causes. For instance, when I am trying to understand what causes my dog to scratch at the door, it does me no good to know that "the dog wants to come in," unless "wanting to come in" is a potential cause of door scratching. Therefore, for Romanes' method to be useful in explaining behavior, it must be the case that mental states can cause behavior.

The third premise is *egomorphism*. Romanes was interested in using information gained about humans to generalize to other animals. Yet introspection gave him information not about humans but about himself. Consequently, his project made little sense unless the information gained about himself in the course of introspection was generalizable to other human beings. The ejections of the scientist to the dog scratching at the door would have little value if they were entirely idiosyncratic. If in attempting to understand the dog's scratching, I looked into my own mind and found that I was moved to pass through doors by invisible angels suspending me by my armpits, my attribution of dog-angels as motivator of the dog's scratching behavior would not have much scientific usefulness. Although not stated by Romanes, it seems absolutely essential that the ejections be those that any human being would make, not simply the whims of an individual observer.

The fourth premise—and the fourth premise, only—is *simple anthropomorphism*. Stripped of all its introspectionism, mentalistic, and egomorphic implications, anthropomorphism means simply applying the categories appropriate to humans for the explanation of animal behavior. For Romanes' project to be consistent with this premise, categories derived from the study of human beings must be applicable to animals.

The defense of Romanes' ejective anthropomorphism is a logical chain, the conclusion being dependent on the integrity of each and every one of its parts. If *any* of the premises of the argument is false—introspection, causal mentalism, egomorphism, or simple anthropomorphism—then, the conclusion is unproven. That is, if introspection does *not* give us special access to our own mental events, *or* if mental events do *not* cause behavior, *or* if the causes of our own behaviors do *not* give us insight into the causes of the behavior of our fellow humans, *or* if knowing the causes of human behavior does *not* give insight into the causes of animal behavior, then Romanes' ejective anthropomorphism is illogical.

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Ejective anthropomorphism has recently come back into favor as a part of what some are calling cognitive ethology. My conclusion will be that ejective anthropomorphism does not provide the basis for any valid scientific method, although of course, like any flight of fancy, it can be the basis of conjectures that lead to scientific hypotheses. Philosophically sophisticated readers will instantly recognize that the project of *proving* that ejective anthropomorphism is scientifically invalid is much too complicated to attempt in so few pages. However, I hope at least to suggest to ethologists and comparative psychologists some of the pitfalls that await those who succumb to the intuitive seductions of ejective anthropomorphism.

Because the defense of ejective anthropomorphism is a logical chain, I will proceed by attacking its four premises. By eroding the reader's confidence in any of these premises, I hope to erode his or her confidence in the conclusion. In their logical order, the premises decline in controversiality. So, for rhetorical purposes, I will start with the last and least controversial, anthropomorphism, then work backward toward the first and most controversial, privileged access.

Anthropomorphism Proper

Making inferences about the behavior of animals from human behavior is a special case of generalizing from one animal species to another.

Interspecies Generalization

Given that species are different sorts by definition, any behavioral generalization from species to species must be based on some evidence or argumentation to counter the presumption that species are different. Usually that evidence is a specific observation that the two species display the same trait, but sometimes we are unable conveniently to observe one of the two species and want to generalize from known to an unknown species. What sort of evidence or argumentation would be sufficient?

Two kinds of argument are often offered. One is that the two species in question are taxonomically related. Since classification is by morphology, this procedure amounts to generalizing from morphological to behavioral similarity. The most familiar example is in the behavioral attributions that are made to fossilized animals on the basis of their morphological similarity to living creatures. Since the teeth of the tyrannosaur are similar to the teeth of living flesh-eating creatures, we assume that tyrannosaurs hunted and ate other animals.

Taxonomic generalization is assumed to be safer if the trait generalized is a conservative trait that unites the species being generalized from with the species being generalized to. For instance, mice are often used in studies of human toxic reactions because the two share basic mammalian physiology. But the same logic can also work against generalization. We are reluctant to make generalizations when the trait or process under consideration is distinctive. Thus, primates are often used in experiments on complex learning, because everything we know about the brains of mammals suggests that the primate brain is unique in important respects having to do with learning.

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The second kind of argument in support of interspecies generalization is ecological. Animals that live in the same circumstances, eat the same food, etc., are expected to have the same behavior patterns. Thus physical anthropologists have argued for the large group size of *homo erectus* on the basis of evidence that they hunted large animals.

While these sorts of generalizations often are correct, the very nature of the evolutionary process guarantees that they will often be wrong. To the extent that species respond to selection pressure by altering their behavior to fit new circumstances, an animal's behavior will be found to be badly predicted from its morphology, and to the extent that an animal's behavior is shaped by its morphology, ecology itself will be a bad predictor of behavior. So the best we can say about generalizations between species is that they sometimes work and that they work best when the species in question are both morphologically and behaviorally similar.

The Special Case of Generalizing from Humans to Other Animals

Generalizing from humans to animals is a special case of generalizing between species. Is there any reason to believe that generalizing from humans to other species is more or less sound than interspecies generalizations? Your answer will depend on the degree to which you regard the human species as unique. Much of human morphology is quite conservative; the shape of the hand, for instance, notwithstanding the opposable thumb, is a generalized character compared with the forelimbs of, say, bats, seals, and horses. But of all the parts of the human anatomy, the brain is unique, and it is, of course, to the structure of brain we would look most closely in deciding on the appropriateness of generalizing from human beings about behavior. Moreover, the human species is already known to be behaviorally unique in a wide variety of ways. It is the species with by far the proportionately longest period of immaturity, it is the only mammalian species to be complexly and socially organized on such a wide scale, it is the only species to develop communication to such a high degree, and it is the only species to make such extensive use of learning and traditional transmission in development. With all these unique behavioral features in mind, I would have to say that, unless I had powerful reasons for doing so, the human species would be one of the *last* I would use as a basis for behavioral generalizations to other species.

My reluctance stems from the strong influence of ethology on my thinking (Thompson, 1986, 1987), as opposed to comparative psychology. Because comparative psychology had its origins in 19th century psychology, comparative psychologists have traditionally focused on a relatively few mental processes. Ethology, by contrast, has its origins in comparative anatomy. Comparative anatomy has led many ethologists to make a strong distinction between the structure of a behavior and its function. As a consequence, ethologists have been more precise about the distinction between the goals toward which behavior is directed and the functional outcomes, such as reproduction, or aggression, or caretaking that explain it. In fact, much of ethological research consists in showing that goals and functions are not inherently connected and in describing the factors by which they become connected in the life of the animal. (Thompson, 1986) In deciding how to generalize

from human mental states to animal, readers are deciding whether to think like a classical comparative psychologist, who starts with the assumption that mental states are widely stable across the animal kingdom but find their expression in a variety of different activities, or whether to think like a classical ethologist, who assumes that species motivations are specific until they have been shown to be shared.

Ultimately this is a question of taste, but I think it is fair to say that interpreting behavior in terms of a few trans-specific mental states is throwing away most of what has been accomplished by ethology in its 50 years of achievement. The genius of the ethological way of looking at animals is that it focuses our attention on activities in the species's particular environment. When Tinbergen showed that a distant British postal truck was an appropriate aggressive companion for a stickleback fish, he directed our attention to something very particular about the fish: Whatever the function of this behavior, the fish's behavior is directed toward eliminating red stimuli from its territory; the fish will approach and attack until such stimuli are removed (Tinbergen, 1951). The highly specific nature of the fish's motivation is obscured when, in anthropomorphic fashion, the fish is said to be responding to "anger" or to an "aggressive drive."

Egomorphism

Inherent in every act of explanatory ejective anthropomorphism is a generalization from the self to the other members of the human species. Egomorphism is implicit in ejective anthropomorphism, because one could not use the insights gained by examining one's own mind to generalize from humans to animals unless one had first generalized from these insights to the minds of other human beings. Generalizing from self to other is simply a special case of generalizing from one member of the species to all other members of the species. And this, in turn, is a case of generalizing from a sample of one.

When is a sample of one useful? As always, it depends on the variability of the phenomenon under investigation. One can learn a lot from a sample of one if the characteristic under investigation is extremely stable across the species and almost nothing if it is not. Many visual and perceptual phenomena—such as color vision and depth perception—are stable across the human species and much of what we know about these functions was discovered by investigators who employed **only** themselves as subjects. But, as the literature on individual differences suggests, we would learn very little about the personality or intellectual development of humans by the study of a single arbitrarily chosen individual, unless, of course, we happened to capture an individual who stood near the central tendency of the species as a whole.

So one of the steps in deciding on the plausibility of explanatory ejective anthropomorphism is deciding how likely we think it is that mental states remain the same from person to person. Are thoughts, feelings, and needs something that we share with our fellow human beings, or are they things that each of us experiences uniquely and idiosyncratically? Putting aside for the moment the question of how we would know, I think most people would answer, "It depends!" Some mental states seem pretty stereotyped, like the feeling we experience when we strike a finger with a hammer. If we saw someone hit a thumb with a hammer we would have a pretty

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good idea what she would do and what she would later report about how she had felt. Other mental states, such as our feelings toward our loved ones, our jobs, the spaces in which we live, etc., seem more variable. Different people in apparently the same situation would report different feelings about these situations.

This observation suggests that if egomorphism is an essential step in ejective anthropomorphism, then we can usefully generalize from humans to animals only those mental states that do not reflect our individuality. Thus the requirement of egomorphism, while not forbidding ejective anthropomorphism altogether, still places important limitations on the kinds of mental processes that can be ejectively anthropomorphized.

Causal Mentalism

The problems with causal mentalism arise from what causal explanation is all about. Whatever else can be said about causal explanation, its satisfactions arise from making a connection between the event to be explained and some different antecedent event. It follows that an event which is explained as its own cause is not explained at all (Lipton and Thompson, 1988). Before we can explain an event we must know of another antecedent event which can serve as its cause. This analysis suggests that we have to have an independent source of knowledge about the causes of an event beyond the fact that the event itself is caused. In other words, if mental events are to be explainers of behavior then we must have some other way of knowing about them other than that they cause the kind of behavior we are trying to explain (Ryle, 1949; Kenny, 1989).

To see why this need for independent specification of causes poses a problem for mental states explanations, let's examine a particular case, the explanation of eating by reference to hunger. According to the simplest version of mental states explanation, the absence of food in time causes some physiological event, such as a drop in blood sugar, and this in turn causes the mental state of hunger. Hunger, in turn, causes us to seek out and eat food. The problem with this account is that while we have two sources of specification for hunger we also have two causal explanations, the causing of hunger by deprivation and the causing of eating by hunger. We can use deprivation as an independent specification of hunger and thus give meaning to the statement, "Hunger causes eating." But in so doing, we make circular the statement that deprivation causes hunger. Similarly, we can use eating as our independent specification of hunger and thus make the statement that deprivation causes eating into a meaningful explanation, but in so doing, we render circular the statement that hunger causes eating. Thus, to whichever external variable we turn to specify hunger, we convert into a meaningless truism one of the two explanatory statements that characterizes hunger as an explanation of eating behavior.

But what about some internal variable such as the drop in blood sugar? Could we now use the drop in blood sugar or some other intervening physiological variable as our specification of hunger? Well, yes we could. But do we really want to? Imagine a person whose blood sugar has fallen sharply, but one who has recently been well fed shows no heightened sensitivity to food and refuses stalwartly to eat.

Would you be willing to say that such a person is hungry? Only if you are willing to do so are you qualified to rescue causal mentalism through physicalism, the identification of mental states with physiological states.

After considering these and other issues I have come to the conclusion that mental states do not explain behavior; they describe it. The term *hunger* does not explain eating; it refers to the higher order pattern of behavioral design of which eating is part.¹ To say that an organism has a hunger for an object is not to speak of the cause of its eating but to make the claim that the animal's behavior is designed around consuming that particular object. If we say that foxes are hungry for rabbits, we are describing a fact about the behavior of foxes over time, viz., that the fox's behavior is organized around the search for rabbits, that foxes are sensitive to aspects of their environments that suggest the presence of rabbits, that foxes catch and eat rabbits, and that foxes return to the places where rabbits have been found to search for them again. All this is suggested by the idea that foxes have a hunger for rabbits. An analogous design in the behavior of rabbits is suggested by the statement that rabbits have a fear of foxes. It suggests that rabbits avoid places in their environment where foxes are to be found, that they flee when foxes approach, and that they relax and go about other matters in their lives only when foxes are absent.

Now, because "rabbit hunger" and "fox fear" are design properties of fox and rabbit behavior respectively, they are not available as explainers of that behavior. We cannot say that the fox chased the rabbit because it hungered for it or that the rabbit fled the fox because it feared it. Doing so would be like saying that a knife is sharp because it has a cutting design. A cutting design is not a cause of the knife's sharpness, it is simply a name for the suite of characters—among them sharpness—that constitute a design for cutting.

Does this mean that mental states are forever insulated from causality, as Rosenberg once argued (Rosenberg, 1978)? Not at all. We can make at least three kinds of statements relating design with causality. First we can describe the manner in which the design features of an animal's behavior embody causality. We can say, for instance, that it is characteristic of the design of the fox that the sight of the rabbit causes it to give chase. Or that it is characteristic of the design of the rabbit that the sight of the fox causes it to flee. That is, rabbits and foxes are designed so that certain events cause one another. We can also make statements about the consequences of the motivational designs. We can say, for instance, that the rabbit-hunger of foxes leads to a selection pressure for faster rabbits. Furthermore, we can say that the fox-fear of rabbits leads to a selection pressure for faster foxes. Finally, we can search for causal explanations of these motivational designs. We might say, for instance, that foxes have a rabbit-hunger because foxes with that motivational design have had more offspring than those without. We might also say that rabbits have fox-fear for the analogous reason.

This account of mental state terms helps us to understand why we use them even when they are not valid explainers of behavior, but it does nothing to rescue ejective anthropomorphism from its reliance on a causal mentalism in which behavior is explained by reference to mental states. To make such a rescue we must have a method of specifying mental states that is independent of the environmental events that cause those states and the behavioral events that those events cause.

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Why not specify mental states by our own personal awareness of them? Hunger then could be a state of awareness brought about by deprivation, a state of awareness that in turn causes eating. The problem with using introspection to provide independent specification is that it stands and falls on the validity of introspection as a form of observation.

Introspection

The fourth requirement for ejective anthropomorphism is introspection—a special capacity to look into our own minds and see there the causes of our behavior. For instance, wondering why we find ourselves poised in front of the refrigerator door at 3 a.m., we look into our minds and find that we are hungry for a small bit of the apple pie that we so appreciated the previous evening at dinner. This introspective insight at least provides an explanation for our reaching for the pie and perhaps even for our being at the refrigerator door in the first place. Like causal mentalism, the idea of introspection is deeply entrenched in our ordinary language and seems entirely natural. Like causal mentalism, however, introspection seems to become fraught with problems when it is closely examined.

Just as our doubts about causal mentalism flow from a careful consideration of what is meant by causation, so our doubts about introspection flow from a careful consideration of what is meant by “spection.” The root, *spect*, in latin refers literally to seeing. So, talk of introspection seems to suggest that when we introspect we learn something about our own minds that is analogous to what we learn when we look at something with our eyes. Whatever else may be said about this relation, it seems to require a mind to see and a mind to be seen—a very perplexing idea.

To grasp just what a genuinely perplexing idea this is, imagine how you would go about wiring a computer to introspect its own processes. At first, the project seems entirely feasible. Somewhere within the body of the computer would be a module dedicated to the task of analyzing the activity of the rest of the computer. It might have extensive computational capacities of its own or the ability to call upon idle capacity of the main computer in its task of analyzing the whole.

The first thing we know about such a module is that it would have to have sensors to bring in information about the whole. Unless the introspection module was to be as large as the whole that it is analyzing, those sensors would have to be limited to certain crucial localities which indicate the state of critical sub systems within the whole. The introspection module would analyze the information from these sensors and build a model of the activity of the whole machine. Because the information gathered by such a module would be partial, its model of the whole would necessarily be subject to error. Thus even in a computer introspection system, the introspection module would be subject to misinterpretation and error. So, immediately we have to give up on the idea that privileged access means that what we learn through introspection is necessarily true.

This may not seem to matter. Could it still be true that introspection, while fallible, gives information that is more reliable than the indirect information we get from observing ourselves in other ways? Maybe we don't always see our own minds accurately, but we still see them more accurately than we see the minds of others.

Unhappily, to think in this way is to miss the main point: Whatever the introspection module knows, either reliably or unreliably, it is almost certainly not something about itself. Like any information processing system it processes information about something other than itself. This same oddity appears in what we say about our own introspection. The I that speaks when I say that I am hungry is not the voice of the hungry entity but the voice of the introspection module speaking for the hungry entity. What this suggests is that introspection is not in principle a special sort of observation but a variant of extrospection in which the observing system is physically intimate with the observed system.

This way of characterizing introspection does seem to undermine the notion of privileged access. For if privileged access is simply the analysis of one system by another with the goal of providing a model of that system on the basis of partial knowledge of that system, then what precisely constitutes the privilege of privileged access? The only privilege that I can see remaining is in the sources of information available to the analyzing system. One might argue that my introspection module has such good sources of information about my behavior that it can be trusted to provide a more accurate understanding of that behavior than your extrospection module could provide. But is there any reason necessarily why this should be true? Could it not be true that another computer, not physically tied to the first computer but with different sensors might in fact be able to build a better model of the first computer than its own introspection module provides?

Perhaps. It depends. Or as philosophers say, "It is contingent." Whether a person's access to his or her mental states is privileged is an empirical question. No argument has been presented that says that my own view of my own mental states is necessarily useful. The flow of logic—from introspection, through causal mentalism, through automorphism, to anthropomorphism—is interdicted at its source because there is no information to be obtained through introspection that is *in principle* more accurate than other sorts of observation. (Churchland, 1984)

Conclusion and Afterword

In summary, ejective anthropomorphism is in doubt because there is no special source of information to be gained from it; if there were, it is unclear that source of information would inform us about the causes of behavior. Even if it did give information about the causes of our own behavior, it could not be generally relied upon to give us information about the causes of behavior in our fellow humans, and even if it did give us information about the causes of behavior in our fellow humans, it could not be generally relied upon to give us information about the causes of animal behavior.

If ejective anthropomorphism is such a weak, risky, and ill-founded method, then why do we use it, even in our day-to-day lives, even informally? And why have Romanes and other modern cognitive ethologists used it in their understanding of animals?

The answer, I think, is that he didn't and they don't. Our belief that we engage in ejective anthropomorphism is itself an excellent example of the fallibility of introspection. Our introspective account of how we arrive at our assessments of the

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mental states of other creatures is wrong if it tells us that we engage in ejective attribution of mental states to explain the behavior of animals. To see that this must be so, let's examine once again the example of the dog scratching at the door. According to Romanes's ejective anthropomorphism we know that the dog "wants to be let in" because the dog is doing the sort of thing that we would do when we want to come in. In other words, we know that

$$B_{\text{Dog}} : M_{\text{Dog}} :: B_{\text{Me}} : M_{\text{Me}}$$

And we know that $B_{\text{Dog}} = B_{\text{Me}}$ and that $M_{\text{Me}} = \text{Wants to come inside}$. Therefore, according to the logic of ejective mentalism, we can infer that $M_{\text{Dog}} = \text{Wants to come inside}$.

But wait a minute! How *did* we figure out that $B_{\text{Dog}} = B_{\text{Me}}$?

Only under the most extreme circumstances could a human being be induced to whimper and scratch at a door to get inside, and few of us can claim ever to have done so. Consequently, few people who have observed animals can ever claim to have observed in themselves the connection between scratching at a door and wanting to come indoors.

So the identity $B_{\text{Dog}} = B_{\text{Me}}$ is literally false. This problem of the literal dissimilarity of behavior is not limited to door scratching. In fact, there is very little in the behavioral repertoire of dogs that is literally similar to the behavior of human beings. They scratch for fleas with their hind legs with rapid repetitive motions, we with our front limbs with motions that are usually more irregular and less reflexive. They lap water, we drink it. They lick their pups, we cuddle ours. And so forth. Most readers will be hard pressed to think of a single unequivocal case where $B_{\text{Dog}} = B_{\text{Me}}$ is literally true.

So in what sense *is* $B_{\text{Dog}} = B_{\text{Me}}$ true? It seems to be true in a formal sense. That is, our knocking at a door to get in shares formal properties with the dog's scratching at the door, just as our scratching mosquito bites share formal properties features with the dogs scratching fleas, and our cuddling of our babies shares formal properties with the dogs licking of its puppies. These formal properties are the very behavioral design features by which we recognize mental states in ourselves and others. We know that the dog's scratching is "to get in" because the scratching behavior is designed around getting in: The behavior is likely to occur when there are reasons for the dog to be inside, and it terminates as soon as the door is opened; we know that our own knocking behavior is "to get in" for the same sorts of reasons. So we know that the two sorts of behavior are the same because they are instances of the same "mental state." Thus, in order to meet the starting conditions of the ejective method, we must first see the dog's behavior as the equivalent to our own. And to do this, we must first use objective methods to assign the dog a mental state. Only then can we apply the ejective method. But since we already know the dog's mental state before we apply the ejective method, the ejective method is superfluous.

So, in conclusion, not only is ejective anthropomorphism not useful as a formal scientific method, it is probably not even useful as an everyday intuitive method. In fact, it is probably not useful at all. Notice, however, that this radical conclusion in

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NO WAY precludes the utility of attributing mental states to animals nor of intuition in the study of animal behavior. The reason for this is that the conclusion presupposes that mental states are objective higher order design features of behavior, and also because there is no particular reason to believe that our knowledge of mental states comes exclusively from our knowledge of ourselves or even from our knowledge of our fellow human beings. I would assume, given the powerful role that commensal and domestic animals have played in human cultural and physical evolution, that our understanding of mind in general has always been formed in part from our experiences with animal mind. Hence, attributing mental states to animals is no more controversial than attributing those same states to human beings.

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NOTES

1. My belief in this position comes from my experience with two philosophical movements: (1) Neorealism (Holt, et al., 1912), which grew out of William James's pragmatism in the early 20 Century and influenced psychology through the work of E. C. Tolman (1951), E. B. Holt (1914), and J. J. Gibson (1978), and (2) philosophical behaviorism as developed by Gilbert Ryle (1949).