

# Wittgenstein and Ant-Watching

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**ABSTRACT:** Research in animal behavior begins by identifying what animals are doing. In the course of observation, the observer comes to see animals as performing a particular activity. How does this process work? How can we be certain that behavior is identified correctly? Wittgenstein offers an approach to these questions, looking at the uses of certainty rather than attempting to find rules that guarantee it. Here two stages in research are distinguished: first, watching animals, and second, reporting the results to other scientists. Certainty about what animals are doing, has different uses at each stage.

**KEY WORDS:** Animal behavior, certainty, description, observation, seeing.

Thus the theory of description matters most.  
It is the theory of the word for those

For whom the word is the making of the world,  
The buzzing world and lispng firmament.

Wallace Stevens  
"Description Without Place"

This is about research on animal behavior: what scientists say about the things that animals do; what they say, and don't say, about the basis of their understanding of what animals do; how one decides whether to believe a scientist's story about what animals do. I am interested in this because I do research in animal behavior. The first step in such research, mine or anyone's, is to say what the animals are doing. Without this step there wouldn't be anything about which to do research; this step establishes the domain of the research. This is generally true in scientific research. I don't mean to say that there isn't a real world out there for scientists to study. But the world doesn't necessarily fall into the pieces that scientists pick up and examine. Each piece has to be cut out of the world somehow by a process of identification and description. And the shape, size and texture of the piece is crucial to the rest of the research, because the characteristics of the piece set boundaries on the kind of questions that can be asked about it. Not only do the characteristics of the piece determine the avenues of subsequent research, but what the piece looks like, how it seems, is embedded in the practices of scientists as social beings. This has been said many times; here is a recent statement by Laclau and Mouffe (1987, p. 83):

To call something a natural object is a way of conceiving it that depends upon a classificatory system. Again this does not put into question the fact that this entity which we call stone exists, in the sense of being present here and now, independently of my will; nevertheless the fact of its being a stone depends on a way of classifying objects that is historical and contingent. If there were no human beings on earth, those objects that we call stones would be there nonetheless; but they would not be 'stones,' because there would be neither mineralogy nor a language capable of classifying them and distinguishing them from other objects.

When I began to do research I tried to keep track of how I was making the first step, that of describing. I chose some animals – a particular species of ants – and watched them. I wrote down what they seemed to be doing. I had a very long list of types of behaviors. I sorted the list into categories. I watched ants in different places and conditions. I added new items to the list, combined others, rejected others as misconceived. The list narrowed into 4 basic activities, with all the remaining initial items as instances of the 4 activities. The activities are: *foraging*, collecting food, mostly seeds, and carrying it back to the nest; *nest maintenance*, building the nest, clearing the foraging trails and maintaining the nest structure by moving sand around; *midden work*, carrying things to and rearranging the refuse pile, and *patrolling*. Patrollers are the first ants to emerge in the morning. They move slowly around the surface of the nest, and often stop to touch antennae with other ants, or to inspect the ground with their antennae. If they find a new source of food, they recruit other ants to come and carry it back to the nest. If they encounter a disturbance, they circle around, attack it if they can, and spread a wave of agitation through the ants outside the nest. In the course of arriving at this classification of activities, a change took place in the way I see these ants. The change was irreversible. Instead of seeing them as I did at first, as small dark randomly moving objects, I came to see them as ants doing particular things.

In the same way, I see people on the street doing things: a woman is driving a car, a man is greeting another man that he knows, and so on. I do not see a series of actions which I then put together into inferences about what is going on. That is, I do not see a woman inside a large metal object with her hands on a round wheel which turns when the object turns, etc.; what I see is a woman driving a car. There is a question of how we come to see what ants, or people on the street are doing. One topic of scientific talk about this process has to do with the possibility of being wrong. That is, the question of how we come to see animals doing something is related to the problem of how one decides whether a story about what animals are doing is a true story. The latter problem is related to the one Wittgenstein raises in *On Certainty* about whether one can identify and name an object with certainty. He says that what is true depends on a shared system, an agreement about what is true. A story about the real world – which is an extension of denoting things in the world – is not, and cannot be, constructed out of nothing from empirical sense-data. Instead, for example, a story about what a person on the street is doing is constructed out of a shared system of warranted beliefs about human behavior.

Are the observations true because they are shared? C. S. Peirce (1958) says yes: “The opinion which is fated to be ultimately agreed to by all who investigate is what we mean by the truth, and the object represented in this opinion is the real”. Wittgenstein says that because truth works this way, we need to re-examine the uses of the question whether the story is true. He does not offer a set of criteria against which a story can be checked to decide if it is true. This is because what is warranted about the story cannot be taken out of it and held up to the light. The system of truths is the “element within which the argument takes life”. In ant-watching, it is the system within which behavior is interpretable. And being interpretable, the quality of making sense, is not something that sits outside of the behavior, that can be distinguished from it. Behavior within the interpretable system *looks like* animals, or people, doing something.

Seeing and interpreting are intertwined. Suzanne Langer (1951) writes about the symbolizing function of the human mind. One of her examples of the way that, whenever we look at a thing, we see it *as* something, is the facial or emotive expressions people see in the shapes of furniture or cars. The interpretation of the way that things and people look is fundamental to everyday social competence. In *Frame Analysis* (1974), Erving Goffman sketches a theory of the interpretation of everyday experience, how it is that one knows “what it is that is going on”. He begins with the notion of a “framework” (pp. 21–22):

When the individual in our Western society recognizes a particular event, he tends, whatever else he does, to imply in this response (and in effect employ) one or more frameworks or schemata of interpretation of a kind that can be called primary... . A primary framework is one that is seen as rendering what would otherwise be a meaningless aspect of the scene into something that is meaningful... . Each primary framework allows its user to locate, perceive, identify and label a seemingly infinite number of concrete occurrences defined in its terms.

The notion of a framework is part of an attempt to structure the relation between what we see, and what we see it *as*. Appropriate behavior depends on sharing frameworks for interpreting experience; sharing frameworks makes agreement about reality possible.

When I watch ants, I see the ants doing various things. Out of my relationship with them as scientist-watcher emerges a story about what they are doing. There are junctures at which they either reinforce the story or force me to come up with a new one. The process is like that of “pattern-recognition” described by H. Margolis (1987, p. 28), involving: “ ‘jumping’ (reaching a response), ‘checking’ (taking a closer look if something sufficiently adverse [to the interpretation] is noticed after a jump), ‘priming’ (becoming predisposed to making certain jumps), and ‘inhibiting’ ” (becoming indisposed to making certain jumps).

The process of coming to see what the ants are doing is concurrent with, but not identical to, the process of testing hypotheses about what the ants are doing. For example: each morning when foragers start out on the trail towards the place where they will collect seeds, there are some ants that move directly along the trail, and others that zig-zag around much more, and don’t seem to get very far very quickly. The latter kind used to look to me like confused foragers. Later on

in the day, all the ants on the foraging trail go directly towards the end of the trail, where they fan out, walk around, and, if they find a seed, carry it back towards the nest. One summer I put out piles of birdseed at different times of day. I saw the early, apparently confused foragers pounce on the seeds, and carry them back to the nest. Soon a stream of ants would be doing the same. But if I put the seeds out later, when only foragers walking directly on the trail would encounter them, the foragers would walk past the seeds, and no stream of ants would appear to collect them. I came to see the “confused foragers” as patrollers, searching for new food. I saw the other foragers as already decided about their destinations, not available to be distracted by a new source of food.

Writing a paper, or talking to scientists about the ants is an activity whose rules differ from those of watching ants. The rules of this second activity entail a particular account of the first one, ant-watching itself. According to these rules, the story about what the animals are doing arises somehow from the accumulation of disconnected bits of sense-data received by the watcher. To the extent that the adoption of the story can be accounted for in this way, it can be accepted as true. If I say, ants are patrolling when they hold their abdomens at a particular angle and move around in a particular zig-zag trajectory, and stop to touch other ants or the ground with their antennae at particular time intervals – saying those things helps to create a situation in which no one in my audience objects that those ants are not really patrolling. When I say, I put dots of yellow paint on patrollers’ heads, and I saw them meander around on the foraging trail until they found and collected piles of seeds early the next morning, this tells the audience that patrollers, not foragers, really do look for new sources of food. But it would not be acceptable to say, ants are patrolling when I see them patrolling and I could show anyone in the audience how to see them that way too.

Bourdieu (1977) distinguishes practical and theoretical knowledge. Becoming familiar with a city from a map is theoretical knowledge; knowing one’s way around is practical knowledge. The transition from not-seeing to seeing what the ants are doing is a transition into practical knowledge, analogous to the transition into knowing one’s way around a city, coming to live there. There is an analogy between Bourdieu’s practical and theoretical knowledge, and the two activities of watching ants and talking to scientists about research in ant behavior. Talk among scientists involves acting as though watching animals were a form of theoretical knowledge. Reporting to scientists on animal – watching is like accounting for one’s practical knowledge of a city as if it were acquired exclusively through studying a map.

It seems to me that the way we report on what we see animals doing doesn’t correspond to the way it actually works. That is, I do not believe that the basis of ants looking to me like patrollers is the list of elements of patroller behavior that I myself report. I assume that this discontinuity, between what actually happens to me when animal-watching and what I report, is common to all animal-watching scientists. Why does this matter? Let us take it as given that a scientific understanding of animal behavior is valuable. How well we understand scientifically what animals do, depends in part on how well we identify what

they are doing, since such identification is the determining first step of any research. If we rely on an inauthentic version of how we go about seeing animals as behaving in a particular way, the most fundamental stage of our research is hidden.

The literature on ant behavior provides an example of the problems caused by an inadequate account of how we see what animals are doing. Ant colonies have been used to test optimal foraging theory, which predicts how foragers ought to behave in order to obtain the food items yielding the most energy, while expending as little energy as possible in searching for and collecting food. To decide if ants or ant colonies are optimal foragers, a measure of foraging success is needed: how much food is coming in per unit of foraging effort? To measure this, many observers have counted the total numbers of ants coming into the nest, and the numbers coming in carrying food. The proportion carrying food in is a measure of the foraging success of the colony. This assumes that all the ants coming into the nest not carrying food are unsuccessful foragers. The problem is that not all of the ants going in and out of the nest are foragers. For example, patrollers go out of the nest and come back in without carrying anything. Nest maintenance workers come out of the nest carrying a bit of soil, put it down, and go back into the nest. Midden workers may come out of the nest, go to the midden, move things around, and go back into the nest without carrying anything. All of these would be counted as unsuccessful foragers. But such counts are misleading, because some of the “unsuccessful foragers” were never searching for food at all. To answer the question whether colonies are foraging optimally, it matters very much what the ants are seen to be doing.

Consider an observer that is measuring foraging success by counting all the ants going into the nest, and the numbers of these that are carrying food. What does he see the ants doing? He sees the nest maintenance workers going in to the nest, because he is counting them, but he doesn't see them as nest maintenance workers – instead, he sees them as unsuccessful foragers. Perhaps he looks only at the area immediately surrounding the nest entrance, where he is counting, or at the area outside the nest mound, where foragers travel on trails, and manages not to see the midden workers, nest maintenance workers, or patrollers on the nest mound. How does he reconcile his view of the ants as unsuccessful foragers with the fact that, eyes glued to the nest entrance, he must sometimes see nest maintenance workers coming *out* carrying bits of soil? Perhaps in his view, some of the foragers sometimes carry something out, and only a small proportion manage to carry some food item back in.

We can learn nothing about how observers like this see from reading their articles on optimal foraging in ant colonies. In the studies that use this measure of foraging success, it is an unquestioned starting point that all ants outside the nest are foragers. This example is remarkable only in that it is so obvious that the observer's seeing affects the results. But the importance of the manner of seeing to the results is pervasive in all ethological research, and raises some interesting questions. Is it possible to say how we come to see what animals are doing? Is it possible to include this in our scientific reporting? How do we

decide if an account of an animal's behavior is a true one?

One of Wittgenstein's answers to questions like these is that one way we decide whether someone knows something is by deciding whether he is in a position to know. That is, we decide whether the system in the context of which a statement is true, is available to the one who makes the statement. Thinking about it this way, to decide if someone is telling me a true story about what animals are doing, I would have to know if she is in a position to know that story. That means as well as telling the story, she would have to tell me whether she is in a position to know it. How would I tell someone that I am in a position to know what the ants are doing? To answer this, I try to figure out first what it is that persuades me that I indeed am in a position to know. It is this: I watched the ants a long time, and sometimes try to see them doing something other than that which I first see them doing, in order to leave open the possibility of improving my seeing. If I believe that I have done this as well as I can, then I reach the point that it doesn't make sense to doubt that they are really doing what I see them doing. Because if I did doubt my seeing after trying what I judge to be long and hard enough, then I would have to doubt everything I see, and that is impractical.

But as Wittgenstein points out, the worst answer to the question, How do you know that  $x$  is true? is, Because I think I know it. He keeps reminding the reader that the resolution of these puzzles is not in introspection, but in sorting out what one can know about what other people say. All I can really say about my own knowing what the ants are doing is that it makes the practice of ant-watching possible. There are two activities to be distinguished here. The first goes on between the watcher and the animals, such as the one between me and the ants. The second takes place among scientists. Here observations of animals are reported and discussed. Hume, Popper, Lakatos and many others have discussed the impossibility of arriving at certainty by means of inductive reasoning, and the implications of this for the confirmation of scientific theories. This is a problem that arises in the analysis of this second activity, among scientists, the one in which theories are confirmed, and rejected.

But while I am watching ants, the question of whether my story is wrong is peripheral. The question comes up now and then, but it is not the basis of ant-watching. The rhythm of ant-watching is not determined by a series of hypotheses, inferences and confirmations. Instead it is a rhythm of inattention, absorption, and sudden transitions. Each transition is from not-seeing to seeing, in which a group of ants seem to click into a pattern and rhythm, like an invisible bit of choreography, which identifies itself to me as patrolling, or foraging; I see them doing something. It is not exactly that the ants suddenly come to life, but that what they are doing suddenly becomes the activity of a form of life I recognize.

Recognizing what animals are doing is related somehow to recognizing what people are doing. It is important to delineate the role of empathy in this process. As J. Durant (1981) has shown, early animal behaviorists described the process of animal watching, of coming to see what animals are doing, as a process of

reading their *gestures* as if they were human gestures. “The early ethologists shared a distinctive view of animal behaviour and of the way it should be studied. This view held that animals possess specific, innate ‘characters’ which can be understood, often by direct analogy with human character, on the basis of prolonged and sympathetic observation” (p. 5). For example, Heinroth (1959, p. 119) wrote that bird sounds are “like our laughing and crying”, and that “Man and animals certainly possess extraordinarily similar feelings which are often expressed in the same way... In jest, we usually express our point of view thus: animals are emotional people of extremely poor intelligence”. Animals were seen to be expressing simple human emotions.

But seeing what animals are doing, and imagining how they are feeling, are distinct activities. The practical knowledge of an ant-watcher is very different from the practical knowledge of an ant. I know how to watch ants but I would not know how to be one; that is, I am not able to invent, spontaneously, new ant behavior. If I want to know what ants will do in a new situation, I have to create that situation and watch them. In general, interpretation of behavior does not originate in empathy. I can see people doing something I have never done, for example, one person killing another by shooting him, and describe it, without having any sense of what it feels like to kill someone. My feelings, horror, fright, etc., are the feelings of a person watching the killing, not of the people engaged in it. I might imagine, though very inadequately, what it would be like to be an ant. But such empathetic fantasies are not the basis of my seeing what the ants are doing. In fact the obvious discrepancy between their capacity for emotion and mine reinforces my sense of their alien-ness, of the gap between us. When I watch ants empathetically, I usually imagine I would find it extremely frustrating and discouraging to do the things I see them doing. But no matter how many times they have to rebuild a damaged nest, or pick up an object and try to push it through an opening that’s too small, they apparently *never* get discouraged. One could be very wrong about how it feels to be an ant in a certain situation (e.g., discouraged), and still succeed in seeing a true story about what the ant is doing (e.g., rebuilding a nest).

The transition from not-seeing to seeing what the ants are doing occurs when the ants fit in to my human picture of the world. Wittgenstein: “But I did not get my picture of the world by satisfying myself of its correctness; nor do I have it because I am satisfied of its correctness. No: it is the inherited background against which I distinguish between true and false. The propositions describing this world-picture might be part of a kind of mythology. And their role is like that of rules of a game; and the game can be learned purely practically, without learning any explicit rules.” (O.C., p. 15). Suddenly what I see the ants doing, resonates with the “inherited background” I use to interpret behavior. This is a background I have inherited by living as a social being.

If we could describe the mythology or framework within which we see animals, we could describe how we come to see what animals are doing. There is a cultural gradient along which I would expect people closer to me to have the most similar story about what the ants are doing. For example, for me cartoons

are an element of the inherited background that goes into animal watching. Animation makes shapes animal-like, that is, makes them behave, by creating interpretable patterns in time and space. Learning to understand cartoons as a child meant learning tropes for identifying various dramatic personalities and the stories they belong in. When an ant looks like the cat going for the canary, I see the ant as a predator. Another general example of the relation between everyday and scientific mythologies of behavior is the way that the size of animals functions as a theme in animal behavior research. Behavior identified as “threat displays” often has to do with animals making themselves look bigger, by rearing up, puffing up feathers, standing on high places, and so on. In studies of fighting behavior, it is customary to ascertain whether the bigger animal usually wins. Here there is an obvious correspondence with the interpretation of human behavior, in which, in confrontations, greater size means greater power.

Much further from me on the cultural gradient are the other animals that watch the ants. Lizards eat the ants when they come out to forage. Their relationship with the ants is different from mine, and that gives rise to different kinds of seeing. The lizards wait alongside the trail of foraging ants and suck up ants as they go by; lizards rarely go up on to ant mounds. Do they see, or perceive somehow, a foraging trail as a foraging trail? What can I know about how lizards keep track of what the ants are doing, and how ant behavior functions in the social reality of lizards? I know something about their ecological relationship with the ants. A theory that could tell me how my stories derive from my role as ant-watcher, might also account for the relation between the lizard’s view of ants and its role as ant-predator.

A scientific approach to the study of the transition from not-seeing to seeing what animals are doing, would be to examine it in lots of people. One could imagine asking people from very different cultures to pinpoint what it is that resonates for them, the kinds of junctures at which seeing takes place. For example, one could ask a group of animal behavior researchers all to watch the same events and to locate their jumps of certainty. The results of this exercise would depend on a theory in which to organize the researchers’ accounts. Would researchers be able to report on moments of certainty?

In *Philosophical Investigations*, Wittgenstein uses the example of a drawing that looks either like a duck or a rabbit to explore questions of seeing. He discusses this in terms of what we say about what we see. The process of coming to see what the ants are doing is related to what he calls “the dawning of an aspect”. When I say the ants seem to click into a pattern that I identify, the experience is like the one Wittgenstein describes as: “it is as if an *image* came into contact, and for a time remained in contact, with the visual impression” (p. 207). Seeing that the ants are doing something in particular means seeing something about the way that their movements, and interactions with each other, are organized.

To explore the question of what it means to see an aspect of a picture, or to interpret an aspect of an experience, Wittgenstein uses an example of a man who is “aspect-blind”. When you ask such a man what he sees, he can describe the



physical characteristics of the object, but fails to see it *as*. Thus he might see two different photographs of a person perfectly well but fail to see them as photographs of the same person. There is a clear analogy to animal-watching. The observer is, at first, aspect-blind. The ants appear to be moving around randomly. After watching long enough, or when someone shows the new observer what the ants are doing, they *look* different. A friend of mine who teaches the sociology of science showed his class a TV wildlife program on monkeys, without the sound. The class saw a bunch of monkeys in a tree, waving their arms around. The soundtrack reported that one monkey acts as a “policeman”, overseeing the behavior of the other monkeys. With the soundtrack, the same piece of film looked like monkeys playing the roles of policeman and policed.

What different observers see the ants doing depends on the different frameworks, or techniques, or experience, that they bring to their seeing. But there are some things no observer would see the ants doing; no one would say that patrolling ants are doing the tango, or fencing, or buying new clothes. Such false seeings are impossible because they would be immediately ruled out. Though it is impossible to prove, on the basis of the method of seeing, that a particular aspect of behavior is really there, it is clear that some aspects can be eliminated. “Suppose there were imponderable evidence for the chemical (internal) structure of a substance, still it should have to prove itself to be evidence by certain consequences which can be weighed.” (Wittgenstein, P.I., p. 228).

There are different kinds of certainty. Wittgenstein’s way through the problem of certainty in seeing seems to me a very useful one. He suggests looking at what the certainty is *for*; what its uses are. In animal behavior research, certainty about what the animals are doing is used to make the rest of the research possible. For example, I classify the behavior of harvester ants outside the nest into four categories. In the course of several years of research, this version of what the ants are doing has been the basis of consistent results. In talking to or writing for scientists about research, consistent results, or lack of evidence that I am seeing improperly, is accepted as a solution to the problem of certainty in my ant-watching. But I know that this is not the source of certainty when I watch ants.

Can there be a theory of the practical knowledge that goes into good animal-watching? If we could specify what is necessary for being in a position to know what the animals are doing, then researchers could report on what they actually do. How could scientists be taught to talk about, and thereby do, their animal-watching? As it is now there is very little scientific training in seeing what animals are doing. Most of the training consists in showing students to see what the teacher already saw. Also some of the people that work in animal behavior may not ever have to make the transition from seeing behavior as indeterminate, to seeing it *as* particular acts, because instead they record instances of events that have been identified by someone else. They have been taught how to see from the first moment of watching, so the occasion for new seeing does not

arise. (At times this strikes me as impossible, an overly cynical speculation about my colleagues' behavior; at others I think it is obviously what must be happening.)

There is an article, famous among teachers of ethology, describing a laboratory exercise in which students are taught to collect data on the "behavior" of a toy that says one of several things when its string is pulled (Hailman and Sustare 1983). This article has been much cited, and the exercise repeated, which suggests that this method reflects a widespread view of how ethological research is done. Indeed, the language of technology permeates scientific talk about animal behavior. We speak of behavior as "hard-wired", of programming, efficiency, and neural circuitry. The article makes a point about the relation between observer and animal. If one can use the same techniques to observe machines and animals, then the relationship of the animal to the observer cannot matter, because a machine can have no such relationship. Surely, though, the relation between observers and live animals is actually very different from that between observers and stuffed toys.

Hearne (1987) discusses how our views of what animals are doing arise from out relationships with them. To show how we can see intentionality and other moral behavior in animals, she gives examples of a discourse, that of the animal trainer, in which seeing animals as moral agents is necessary. Of course, an important difference between training dogs and watching ants is that what the trainer thinks the dog is doing, always has a strong effect on what the dog does next. The training relationship requires the dog to respond to what the trainer sees it doing. This is not always true of my relationship with the ants. If I think an ant is about to sting me, I will try to brush it off. I do experiments that change what the ants do – put out objects that they carry away, for example. These experiments are designed around what I see them doing. But often, what I see them doing does not affect what they do. At times, my presence does not matter. Watching monkeys is somewhere between watching ants and training dogs in the amount of give-and-take that is required. Monkeys see me watching them, and I see them seeing me. They respond to what they see me doing; in turn I have to take a position as doing something in particular, for example, being an inobtrusive observer. With the ants I can be no more than a pair of boots; that is, for them, a pair of harmless hills that have unaccountably appeared in the landscape.

Different relations with animals are associated with different kinds of certainty about what they are doing. It may not be possible to construct a rigorous account that ensures that what I come to see the ants doing, is what they are really doing. But it is not possible to abandon the belief that what they are really doing matters. One tempting refuge is to believe in a natural or biological correspondence between what I see and what is really there. Then the transition from not-seeing to seeing can be presented as the moment when this natural correspondence clicks into place. This is the position adopted by H. Margolis (1987), whose discussion of pattern-recognition begins by accepting pattern-recognition, the process of seeing what is there, as a black box. He argues that

ways of seeing will have evolved to increase the fitness of the people that use them, and that humans could not have come so far in the history of their evolution if their abilities to interpret the world were seriously impaired. I am not convinced that in general, accurate seeing tends to guarantee people an advantage in reproductive success. To apply this to the way ethologists see animals, the development of current practices of animal behavior research may not necessarily select for correctly identifying what animals are doing. During the heyday of optimal foraging theory, it was to a scientist's advantage to publish a paper on it, even if this meant misrepresenting the activities of the foraging animals. The way that scientists see animals' behavior occurs in the context of the system for interpreting behavior that they live in, a system embedded in the social practices of a certain time and place.

Some philosophers of science seem to be converging on answers to questions about the certainty of perception and interpretation, taking positions that are described as some variant of realism and/or relativism. These include M. Grene (1966, 1985, Ch. 8), M. Polanyi (1958), R. Harre (1986), J. Margolis (1986, 1987), and the psychologist J. J. Gibson (1966), though there are important differences among these authors. They accept the impossibility of certainty but do not reject the possibility of good understanding through scientific work. This position could be summarized very broadly as follows. Stick to realism, that is, take it as certain that the world as we perceive it is really there. Admit to the possibility of being wrong: perception and the identification of objects, though immediate and involuntary, can be misleading. Next, admit to the influence of social history and context on perception. This means accepting some form of relativism, seeing the knower as an agent rather than as a mechanical measuring device. Do not try to think of all understanding as explicit and formal. Instead, look for a biological relation between seeing and the world, and take the social relation between what is seen and what it is seen as, as an extension of this.

This argument admits the possibility of wrong seeing, but makes a case for the possibilities of being correct, seeing what is really there. It says, let's start from seeing correctly, which happens often enough, and go from there to describe the construction of scientific theories as a rigorous (though not necessarily logical) process. In other words, it is saying, these bricks, seeings, are good enough, though not perfect, and with them we can construct a solid building. Wittgenstein goes further, because he rejects the business of justifying a procedure, such as the construction of scientific theories, by matching it up to a set of rules. To show that a building is properly built, one doesn't look at how well it conforms to a building code. One looks at what happens in it, whether it keeps the rain out, whether people tend to walk through the walls or not, and so on. To investigate the coherence of scientific theories, one would investigate what is done with them, how they function in scientific understanding, not the legitimacy of their component parts.

To call for new thinking about how we watch animals, is not to say we need new rules for correct animal-watching procedures. (Such rules may be impossible to derive anyway.) What happens when I watch ants is very different from

the standard account of what goes on during scientific animal-watching. It seems very likely that experiences like mine are usual, and that the standard account rarely corresponds to actual practice. This is important, and in arguing for its importance I am not making any claim that the real activity is somehow softer, nicer, or better than the cold objectivity professed in the standard account. It is important because what we see animals doing determines the outcome of our research. If we agree not to talk about how we watch animals, and instead to substitute an account we know to be untrue, we are obscuring the crucial first step in our work: saying what the animals are doing. Such obfuscation can only work against our attempts to understand why and how animals behave as they do.

My questions about ant-watching do not arise when I am watching ants. Most of the time I do not think much about certainty when ant-watching: I enact it. The questions do not come from doubts about the certainty of my seeing, but from the ways that doubts about the certainty of seeing have shaped scientific practice. The resolution of the problem would come from changes that would allow observers to talk about what they are actually doing when watching animals. Truthful accounts of scientific animal-watching don't yet exist. This truth is not yet formed because we have not yet learned to talk about it.

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