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What Is Behavior? A Philosophical Essay on Ethology and Individualism in Psychology, Part 1

In a recent seminar in the department of biobehavioral sciences at my university a lively controversy suddenly emerged from a sleepy discussion of experimental results. "Grooming behavior"? Surely that was a contaminated description, not a straight description of the experimental data. The *behavior*, the datum, was that the animal "scratched itself," a description containing no speculations about function. The speaker did not agree. There is nothing amiss, indeed everything right, he insisted, in classifying behavior in accordance with function, and there was every reason to believe, in this case, that grooming was the function of the behavior.

Meanwhile philosophy of psychology is engaged in a debate that has, as I will try to show, the same roots. Will a mature cognitive psychology need to characterize its subjects in ways that make reference to how they are imbedded in their environments? Or will it be "individualistic," making reference only to what supervenes on the structures of individual bodies and brains? The individualists argue that the behavioral dispositions of a person clearly depend only on that person's *inner* constitution, and hence that there can be no need to refer to the individual's relation to the wider environment in order to explain them. The anti-individualists argue that it is impossible even to describe much of the behavior that it is psychology's job to explain without reference to the environment. For example, "Jane pointed to the red block" and "Jane said that she was ill" are surely descriptions of behaviors requiring explanation (Burge 1986a), yet the first makes reference to a block in the

Special thanks to the animal-behavior scientists Colin Beer, Matthew Kramer, and Ben Sachs for help with this chapter.

environment, the second to the role within her language community of the sounds Jane made. Siding with the individualists, my colleague in biobehavioral science mutters that these latter descriptions are surely descriptions of the hypothesized *functions* of Jane's behavioral outputs, not uncontaminated descriptions of the form of her behavior.

These controversies stem, I believe, from the same misunderstanding. The confusion concerns what "behavior" is in the sense that it is the behavioral scientist's job to explain it. Classical ethologists believed that, in principle, all the behaviors of an organism could be described by an ethogram prior to making any assumptions about the functions of these behaviors.[1] Classical animal behaviorists, who concentrated on learning theory, believed the same. A proper description of sensory input and behavioral output for any organism would be just whatever description was needed to formulate regularities or input-output laws for the system. This has also been the stance of psychological individualists. The difference between the latter two is mainly that the contemporary individualist looks for laws that refer to states of *inner* mechanisms regulating behavior as well as to input and output. Let me lay my cards down on the table straightaway by contrasting this classical position on behavior with what I believe behavior, in the relevant sense, actually is.

Any animal's activities can be described in a potentially infinite number of ways, and hence classified under any of a potentially infinite number of categories of form. Behavior, I will argue, is the *functional* form of an animal's activity. Other forms of the animal's activity are not relevant to behavioral science. As such, behavior obviously cannot be isolated and described prior to speculation about function; to offer a description of behavior is to offer a hypothesis precisely as to what *has* a function.

Furthermore, because the functions of behaviors are to make specific impacts on the environment, behaviors cannot be isolated and described apart from reference to the environment. Etiological explanations of behavior concern mechanisms that tailor the forms of behaviors to the structure of the environment and/or strategically place these behaviors

1. For a contemporary defence of this view, see Schleidt and Crawley 1980 and Schleidt 1985.

within the environment so as to have appropriate impact. Hence, explaining the operation of these mechanisms requires describing the relations their operations normally bear to the environment. To take a central example, in order to understand how beliefs, desires, and other intentional states enter into the explanation of behavior, we must understand what relations these states bear to the environment when they have been properly induced and are functioning in a way that is biologically normal.

In this chapter I will explain and defend the claim that behavior is functional form for the general case of ethology. In chapter 8, I will show how the truth of this claim entails that behaviors extend far out into the environment, and I will show why etiological explanations of behaviors cannot proceed without continual reference to this wider environment.

What, then, is behavior, the core subject of ethology? I am using "ethology" broadly here to cover animal-behavior studies generally, and I am including humans among the animals. A behavior is, I suggest, at least the following:

1. It is an external change or activity exhibited by an organism or external part of an organism.
2. It has a function in the biological sense.
3. This function is or would be normally fulfilled via mediation of the environment or via resulting alterations in the organism's relation to the environment.

Requirement 1 gives us a rough way to distinguish behaviors from physiological processes.[2] Notice that it allows things other than movements to be behaviors, things such as emission of sounds (vocalization, sonar), of pheromones, of light signals (fireflies), of electric shocks (electric fish); things such as changes of color (octopuses and chameleons), emitting heat (incubating), and so forth.

Requirement 2 is the central one. Most of this chapter will explain and defend it. It may help the reader, in looking ahead to the human case, to recall that the mechanisms responsible for human purposive actions have emerged from a history of natural selection and have

2. For a different tradition on the use of the term "behavior," see, as a paradigm, Engel 1986.

biological functions (see chapter 2). If human purposes are a species of biological purposes or proper functions, then human actions are behaviors in the sense described. This position will be clarified in part 2 of this essay (chapter 8).

Requirement 2 excludes from the class of behaviors such things, taken in themselves, as loss of heat, emission of odors, nonfunctional changes in pallor (turning red when one is hot), and galvanomic skin responses. Requirement 3 excludes such things, considered by themselves, as excretion of waste (e.g., sweating merely *as* excreting, breathing CO_2 into the atmosphere), getting a sun tan, getting callouses on one's hands, and shivering, for although these events or processes have functions, the performance of these functions is not mediated by the environment. That is, these activities do not effect changes in, or in relation to, the environment in order that the environment should give a return on the investment.

The simplest forms of behavior are not environmentally induced or influenced, or if they are, this influence is not functional. Put simply, the organism does not strategically *place* these behaviors in the environment. Thus we breath, the clam passes sea soup through its digestive tract, the barnacle waves its foot, and the jellyfish drags its tentacles. Each of these is a behavior with a function, but none is strategically placed in response to the environment. Perhaps the barnacle or the clam slows down its activity when the water gets too cold, but if so, this will not be a strategic deceleration but a mere byproduct of the organism's chemistry. Similarly, our breathing speeds up or slows down in response to our bodily needs, but not in direct response to the environment so as to place it correctly in the environment.

More interesting behaviors are those that are advantageously placed in the environment so that they occur, tend to occur, or occur more often than randomly, when the environment is ready to cooperate. They are placed so as to effect their functions through the mediation of the environment, when and where the environment is ready to mediate. It is on these latter kinds of behaviors that I will concentrate. Animal and human psychology might be distinguished within the somewhat broader field of ethology by the fact that psychology too concentrates on the latter behaviors, emphasizing mechanisms of control of behavior by or partly by the environment.

The behavioral scientist with whom I began this essay took it that "grooming behavior" was a description of behavioral "function" in a sense in which descriptions of function go beyond straight descriptions of the experimental data to incorporate illicit speculations of some kind. My project is to argue that there is no such thing as a minimal, antiseptic, or unprejudiced description of the data, the behavior, that it is the job of the behavioral scientist to explain. But first, it will be well to understand this fear of infection by function. There are, I believe, several overlapping historical sources of this fear.

If we look to the history of behaviorism, we find a strong concern that the data for psychology should be intersubjectively observable data, in contrast, specifically, to data collected by introspection. One of the things that was traditionally thought to be known by introspection and, when the chips were down, by introspection alone, was what one's intentions or purposes were in action. It apparently followed that no reference to an organism's purpose in behaving should be made when describing behavioral data. To describe behavior by reference to its purpose would be to describe it by reference to hidden, possibly occult, causes in the organism, causes that, at the very least, could not be directly observed. It would be to build "mentalistic" notions or at least assumptions about hidden variables into the very description of one's data.

Out of the tradition of ethology came a parallel concern about the dangers of anthropomorphism. It is all too easy to read motives into an animal's behavior by analogy with what one's own motives would be. For example, Lehner (1979) cautions us that in describing a dove's behavior "as 'escape flying behavior' we are assuming that the dove was responding to a stimulus from which it *wanted* to escape" (1979, 46). But it may well be that nothing parallel to the motives of humans are to be found in such animals at all; certainly the ethologist should be careful not to prejudge such motives. And even if a label such as "grooming behavior" does not carry the implication that the animal has grooming as a personal motive, still by initially labeling the behavior as grooming rather than merely scratching, one may be blinding oneself to the true functions involved or to the necessity of seeking hard evidence for the functions one thereby assumes. According to a famous quote from Konrad Lorenz, "It is an inviolable law of inductive natural science that it

has to *begin* with pure observation, totally devoid of any preconceived theory and even working hypothesis. This law has been broken by one and all of the great schools of behavioral study" (1950, 232).[3]

Thus in the tradition of classical ethology, one begins the study of an animal by first constructing an ethogram.[4] The ethogram is a list of the units in the animal's behavioral repertoire, described, in the first instance, purely as a set of motor patterns. But it is sometimes recognized explicitly (more often implicitly) that progress cannot be made without also noting something about the context of occurrence of these motor patterns. For example, to describe a behavior as eating, jumping, bar pressing, or scratching is already to have moved beyond muscle contractions to the wider context of these. Indeed, Drummond (1981) argues that a complete description of a behavioral unit would include, besides "intrinsic properties" (e.g., motor patterns), also location, orientation, physical topography, and physical effects.

Drummond's inclusion of physical effects in a description of pure behavioral form is particularly interesting, since description of effects has been taken by others to be description of the function, as *opposed* to the form, of behavior. For example, Robert Hinde tells us that "there are two methods for describing behavior. One involves reference ultimately to the strength, degree and patterning of muscular contractions. . . . The other involves reference not to these changes but to their consequences" (1970, 10). On this Lehner (1979, 44–45) comments that the distinction between "empirical description—description of the behavior in terms of body parts, movements and postures—(e.g., baring the teeth)" and "functional description—incorporation of reference to the behavior's function—(e.g., bared-teeth threat)" is "nearly synonymous" with Hinde's distinction between describing muscle contractions and describing consequences of these. Similarly, Bastock (1967, 11) writes that "displays . . . are best defined in terms of their function. *Threat* displays tend to cause withdrawal on the part of the adversary; *appeasement* or *submissive* displays tend to reduce attacks" (taken from

3. Colin Beer and others call this "the doctrine of immaculate perception."
4. I am much indebted to Matthew Kramer for supplying me with a quick review of current literature on ethogram construction from chap. 4 of his dissertation (1989). The references in this and the next paragraph, except Hinde 1970 and Lehner 1979, were found through this source.

Purton 1978). Purton (1978) discusses what he considers to be the *mistake* of conflating functions with mere effects. My argument will be that *exactly the same considerations that distinguish functions from mere effects also distinguish behavioral forms from mere motions*, from incidental effluences of the organism, and from other incidental changes occurring on its surface. Nonfunctional activity forms have exactly the same status as do nonfunctional effects of behaviors. Neither is a proper subject matter or a part of the data that behavioral science must explain. Conversely, to distinguish those forms of motor pattern and other outputs of the organism that are proper data for behavioral science from those that are not is impossible without implicitly postulating the *existence* of some function or other for the output, if not always the specifics of the function.

Concerning the task of constructing an ethogram, the obvious questions have, of course, arisen about how to segment the motor patterns that an animal exhibits into chunks, how long these segments should be, and how similar to one another they must be in order to be grouped together as examples of the same behavior (Schleidt and Yakalis 1984). But the consensus seems to be that as ethologists become more familiar with the animal under study, this theoretical problem tends to subside, and practical agreement to emerge (Kramer 1989). I would like to convince you that the problem of how to segment motor patterns into chunks is in fact but the tiniest tip of a huge theoretical iceberg. The theoretical problem is theoretically huge. It is solved in practice only by either commonsensical or ethologically experienced implicit reference to function. Since common sense for the most part solves this theoretical problem, one can appreciate its magnitude only by withholding common sense. Please try not to flinch, then, as I proceed to rub your nose in the theoretical absurdity.

There is a tendency to think of the motions of an individual organism as constituting a straightforward set of manageable size. These are the "outputs" for individualist psychology, the items that must be explained as deriving from "inputs" to the sensory systems by references to the regulating mechanisms between. And a collection of these events, observed one by one and incorporated into a list, are supposed to constitute the basic data for ethology. The ethologist's initial problem is how to divide and classify these individual behaviors so as to put each

relevant *type* on the list just once. The individualist has a similar problem, for she wishes to *explain* the movement events, and events can be explained only under types. There is no such thing as explaining, simply, "so-and-so's current movements"; movements must be explained under general principles, and hence under general descriptions. The problem that emerges, then, is not just how to divide and count behavioral events. It is that the number of possible descriptions that might be given of any *one* movement event is completely unmanageable. Please try to keep common sense under control while I belabor this point.

Consider, for starters, that motions can only be described relatively, through mention of spatial and temporal relations to chosen relata. Relative to *what* should a given motion be described so as to classify or to explain it? Should we try to explain why Amos the mouse moves away from the cat, toward the kitchen clock, toward the waiting broom, toward London, or toward the North Pole? Should we explain why Amos's eyes blinked just before a piece of dust struck his closed eyelids, when the clock said 2:37:08, just as Amos's whiskers twitched, or just as the end of Amos's tail passed the fifth blue square of the kitchen linoleum? Indeed, did Amos blink, or was it just that his upper eyelashes removed themselves, in an arc, away from his eyebrows, or moved to point at his navel or his nose or his toes? Should we explain why muscle cell no. 237 in Amos's right biceps contracted at the same time that muscle cell no. 153 in Amos's left ear relaxed, or why it contracted at the same time that muscle cell no. 863 in his right *triceps* relaxed? Or would it be better to explain how it happened that *all* of the muscle cells in his body happened to coordinate so as miraculously to convey him across the floor, rather than leave him in a twitching heap in the middle? We might *attempt* to explain any of these things, and, in principle we might succeed with enough physics and chemistry, and a full chemical-physical state description of Amos and of a big enough piece of the world around him at a certain very exact time. But surely it is not the job of any life science to explain Amos's motions under every one of the uncountable number of descriptions that can be given of them. Under what descriptions, then, *is* it the behavioral scientist's job to explain Amos's motions? What is the *principle* involved here? This, I take it, is the same as the question of which of these descriptions describe *behaviors* of Amos, rather than mere motions, behaviors being the concern of the behavioral life scientist.

Motions are not peculiar with regard to the infinity of their possible descriptions. Amos can make squeaks, chattering sounds, sneezes, coughs, choking sounds, or he can be silent—silent except that, if you listen closely, he makes breathing sounds and little thumping sounds with his feet (danger signals or just foot patter?) and also with his heart. Which of these sounds and which silences are subject matter for behavioral science? How should the sounds be described? By pitch, inflection, duration, periodicity, harmonic structure, rhythmic structure, amplitude, or pattern of repetitions? Consider the sounds that a human makes. Some of these, such as screams and laughs, can be described relatively crudely. Others, the speech sounds, need to be described in great detail and in accordance with principles of such subtlety that they are not yet fully understood. Still other sounds, such as sounds made while choking or urinating, sounds made by the heart, and normally those made in breathing, do not need to be described at all. Sometimes silences need to be described, and sometimes they do not. Given the infinity of possible descriptions of emitted sounds and interspersed silences, what determines the descriptions that are relevant to behavioral science?

Does one look, perhaps, for repeated behavioral units, for patterns that recur? That mice run away from cats, for example, is a recurrent phenomenon, that they run toward waiting brooms is not. But the heart says "pit-a-pat" with wonderful regularity, every mouse eyeblink is a momentary movement of its eyelashes away from its eyebrows, every mouse foot touching the floor makes a minuscule thump, and choking is a distinctive and reliably reproducible sound under the right stimulus conditions. Yet none of these are behaviors, not in the sense we seek. None of these is the behavioral scientist required to explain (though explanations may fall out of behavioral science, of course, if these mouse outputs are shown to be accidental byproducts of other outputs that *do* require explanation).

The structure of the theoretical problem here may be clarified by comparing it with the better recognized problem concerning which *effects* of an organism's bodily motion require explanation. Hinde (1970) and Drummond (1981) suggest including physical effects of an

organism's movements as part of the description of the form of the organism's behaviors. Yet we know that not every effect of an organism's movement can be considered part of its behavior. One effect of Rattus-the-rat's current muscle contractions is that the bar in front of him is depressed. But a second is that the watching experimenter frowns or smiles, a third that an elongated shadow passes over the floor in front of the cage, a fourth that a food pellet enters his cage, a fifth that this pellet makes a rattle that alerts young Templeton in the cage next door, making his mouth water, and so forth. We know that it is not the job of the behavioral scientist to explain all of these happenings. The productions of these effects are not all behaviors.

In truth, very few things that an organism does are behaviors. "Doing" is a far more general notion than is "behaving." To "do," one need merely satisfy an active verb. Active verbs are for the most part noncommittal about whether or not what they describe is the realization of a function or purpose. In the case of a few verbs, for example, "fall," "trip," and "slip," function or purpose is definitely excluded. And there are a few verbs, for example, "hunt," "fish," "seek," "challenge," and "threaten," that tell *only* of function and not at all of form. Thus to know that an animal is hunting is to know that the function of its behavior is finding and/or catching, but it is not to know anything whatever about the form that this behavior takes. Hunting behaviors can be realized with walking behaviors, swimming behaviors, flying behaviors, eye movements, movements of the fingers through the yellow pages, or "mental movements" (hunting through one's memory for a name). To say that an animal hunts is to say nothing at all about either the form or the result of its behavior. But most verbs are just the opposite. They designate form or result and are noncommittal about function. For example, "He bumped me with his elbow" and "He stepped on my toe" are noncommittal about whether the doing was a function or purpose of his bodily movement or merely a result of it. It is even possible inadvertently to "sign a check," accidentally to "warn someone," or involuntarily to "raise one's arm," under prompting of a well-placed electrode. Indeed, one might even "say that one is not feeling well" without doing so purposefully. Perhaps one is asleep or reciting sentences in a foreign language. That is, each of these descriptions *can* be used as a description of the form or result of a doing that is not, however, a *behaving* in the sense that concerns behavioral science.

But I have been ignoring a loud clamor in the wings. The clamor is that *law*, not function, is what distinguishes those bodily motions, sounds, effects of these, and so forth, that it is the behavioral scientist's job to attend to. The motions to be explained must be described under whatever descriptions yield *laws* of behavior. For example, there are no *laws* of mouse behavior that determine mouse motions relative to kitchen clocks or the North pole, but likely there are laws that determine mouse motions relative to cats, or at least relative to the orientations on mouse retinas of catlike images. (By "laws" can be meant, of course, not only lawful dispositions of the whole mouse but also lawful dispositions of parts of inner mechanisms in the mouse, which laws add up, flowchart style perhaps, to an explanation of the mouse's outer behavioral dispositions.) Similarly, if there is some law of behavior under which falls Rattus's pushing the bar down but none under which falls his causing an elongated shadow, then ipso facto that Rattus pushes the bar down is a behavior that it is the psychologist's job to explain, whereas that Rattus causes an elongated shadow is not. Indeed, the actual history of psychology suggests that the psychologist may be able to predict the bar-pressing effect in accordance with laws of rat psychology without being able to predict the bodily motions that cause the bar-pressing, without, say, predicting whether Rattus will use both paws, the right paw, the left paw, or his nose to depress the bar. Some of the laws of rat psychology may be about behavior described just in accordance with effect, and hence not about bodily motions at all.

This classic move is premised, I believe, partly on a confusion between function and law. If one supposes that functions are in general lawfully performed, as opposed to being performed only under ideal conditions, then searching for functions is easily assimilated to searching for laws. Compare Bastock's assimilation, cited above, of the function category "threat display" to the category of "displays that tend to cause withdrawal on the part of the adversary." The move is based also on a misconception concerning science, on the belief that valid sciences always deal in laws. I discuss these two mistakes in chapter 9. Here I will try to show only that outputs that fall under laws are not always behaviors.

The chameleon has a disposition to turn brown when placed in a brown box. The mouse has a disposition to brown nicely when placed in an oven at 350 degrees Fahrenheit. The chameleon's color change exemplifies a law of behavior that it is the ethologist's job to study. Why does the mouse's color change not exhibit a law of behavior that it is the ethologist's job to study? Or if one prefers muscle contractions, why is it not a law of behavior that rigor mortis invariably sets in shortly after the ingestion of cyanide, or that muscle rigidity results from the right sort of encounter with tetanus. It has been demonstrated that male rats deprived of food for nine days copulate less frequently than rats not so deprived.[5] Was this the discovery of a behavioral law?

But perhaps you will object here that antecedents that break the system under study, antecedents that damage the organism, can't yield laws *for* or *of* the systems under study. For example, whether or not decrease in copulation after nine days of starvation is a rat behavior falling under a psychological law depends on whether the starvation does damage to the rat's insides so that it is no longer a proper subject for the study of rat psychology. But that objection is a cheating one. For what it *means* to say that the system broke down is exactly that it is not a function of the system or a byproduct of its functions to react in this way. The very subject matter of behavioral study, the *intact* animal, is defined by reference to proper or normal function. Behavioral dispositions are dispositions not just of any old chunk of warm matter but of a chunk having a normal constitution, where this is defined relative to its (historically defined) proper functions (chapters 1 and 2). Most of the dispositions of Amos and Rattus as chunks of matter are chemical and physical, not psychological. To find the psychological ones, we must make a necessary reference to the *functions* of Amos's and Rattus's dispositions.

To make this clearer, consider some lawful dispositions that may be realized without destroying the biological system. A strong enough electric shock administered to the body in one place contracts the muscles in another. Cockroaches become torpid when the temperature drops

5. Sachs 1965, as re-presented in Hinde 1970. In fact, Sachs's experiment was much more interesting than Hinde's discussion suggests. Even after nine days without food, rats mostly choose sex over food when offered both.

too low. A mild blow below the knee cap causes a kick. If spun around enough times in the same direction, children fall down. When a puff of air hits an open eye, it blinks. Which of these lawful consequences are behaviors? My suggestion is that the eye-blink reflex is the only one of these that is clearly a behavior. It is the only one, so far as we know, that has a function. The rest are probably "spandrels," results of the system's architecture that are accidental relative to its functional design (see chapter 2). Similarly, the eye-blink reflex is *properly* described as a blink or closing of the eyes, not as a movement of the eyelashes away from the eyebrows or toward the navel, nose, or toes, for only the covering of the eyes, as such, has a biological function. That the blinker's eyelashes move away from his eyebrows is a response that falls under laws, but it does not fall under behavioral laws.

I will also not accept as an objection that some behavioral scientists would call knee jerks or becoming torpid when too cold "behaviors." My claim is that if they believe that these happenings have no functions, then if they think about it carefully, they will see that they *shouldn't* call them behaviors, not with a capital "B." The impulse to call them behaviors rests on a confusion. It rests on the assumption that whatever an animal *does* is behavior. And it rests on a false belief about the data of science. It rests on the belief that not only must behavior, the basic data for the behavioral sciences, be observable but that it must also be observable, right on the surface, that it *is* behavior. I have been trying to show that this is not so, that there is no *surface* feature that distinguishes behaviors from other doings.

But now it will be asked, How can the behavioral scientist's initial *data*, what she is supposed to explain, be only forms of output that have functions when the fact that a form has a function is not an observable fact but a matter of theory? Well, how can the classical chemist's data, what she is supposed to explain, be only the behaviors of chemical elements and compounds and not also of mixtures when the fact that a substance is an element or compound and not a mixture is not an observable fact but only a matter of chemical theory? The philosophy of science has matured a great deal since it helped to give birth to behaviorism. Not only have anxieties about speculating on the contents of little black boxes been dispelled; so have anxieties about infect-

ing ones data-gathering with theory. If there is agreement on anything among current philosophers of science, it is on this: what the data for a given branch of science are and how those data must be described so as to connect with theory are matters that are adjusted along with theory and cannot be finally settled in advance. Theoretical science is, in this respect, always a bootstrapping operation.

Of course, it is true that ethologists spend much time putting down in their field notebooks descriptions of behaviors whose functions they do not yet understand. They make a point of trying to describe behaviors in ways that do not prejudge the issue of *specific* function. This practice makes eminent sense. In no science is it good to jump to conclusions. But the fact that the ethologist's preliminary field notes often turn out to be *useful* attests not to the fact that the behavioral data for ethology are recognizable prior to theory. It attests to the soundness of the traditions behind such data collecting and to the perspicacity of the trained field worker in separating out descriptions that are *likely* to be descriptions of functional forms from those that are unlikely to be. Thus it is that as ethologists become more familiar with the animal under study, the theoretical problems about how to "chunk" and classify behaviors tend to subside, and practical agreement to emerge (Kramer 1989).

This is not the place to explore the rich question by what signs and symptoms the ethologist discerns that a certain behavioral unit is bound to have some function or other. But it is very often true that the ethologist rightly perceives this long in advance of entertaining any specific hypothesis about what that function is. On the other hand, it is also true that the ethologist can sometimes be badly mislead. It is not always obvious what it is that an animal is doing that constitutes its true behavior. Reflect, for example, that few but trained linguists can even hear all the salient distinctions among sounds in human languages unrelated to their own, but these distinctions are crucial if one wishes to describe verbal behaviors. Similarly, Colin Beer (1975, 1976) tells an involved story about difficulties in discovering where the true behaviors lie within the vocalizations and within the "facing away" behaviors of laughing gulls. "In spite of the technical advances in data collecting and data processing . . . , one still has to start out with selection of one out of an infinite number of possible descriptive strategies, in accordance with whatever one's wits and experience offer as the best bet" (Beer 1973, 54).[6]

I have urged that the behavioral scientist's job is not to study just the properties of a chunk of living matter but to study the properties of a biological system, the properties, roughly, that have accounted for the proliferation and survival of the creature's ancestors. These properties figure in an explanation of how it happened that some critical proportion of historical embodiments of the system under study managed to avoid destruction and ultimately to reproduce themselves. What a biological system does *as a biological system*, and not merely as a pile of atoms, is what its ancestors have historically done that enabled them to survive and reproduce. *As a biological system*, it *does* only what it is its biological purpose, or "proper function" (see chapters 1 and 2), to do.

The behavioral sciences, considered as life sciences, are engineering sciences in reverse. The engineer begins with certain functions in mind that she wishes to see performed and then figures out how to build a device that will perform these functions. The behavioral scientist begins with a device that has already been designed to perform certain functions and then figures out what these functions are and how the device is constructed to perform them. It is not her job to notice or figure out any other things the device might do, like supplying one a good dinner (hens) or making a good alarm clock (roosters). Nor is it her job to notice any other dispositions it might have, like one's knee jerks and one's skin turning red in the sun. So understood, the life sciences do not include studies of how best to exterminate roaches, of breeding techniques, or of how to grow turkeys with more white meat. Nor do the behavioral sciences as life sciences include studies of animal-training techniques, of how to get chickens to lay more eggs, or of how best to keep pigs from rooting. This is not to hurt anyone's feelings but just to

6. That it is not always obvious what constitutes an animal's true behavior is one reason that the ethologist will note highly conspicuous recurrent outputs of an animal even when these are apparently functionless. The explanations of such behaviors as spandrels or as leftovers from an earlier phase in the animal's evolutionary history are, of course, also of interest. But compare the last two paragraphs of this chapter. Behaviors that are species-typical, and hence aid in distinguishing related species, are also noted in the ethologists notebook, of course.

make what I think is a needed distinction. The heart of the life sciences is to understand life, not what can be done with or to life.

Yet if the behavioral scientist studies not chunks of matter but functional systems, how does it happen that the behavioral experimentalist puts out one eye of *Armadillidium* and then reports in the literature that it proceeds to swim around in circles, or removes large portions of a cat's brain and reports on resulting abnormalities in the cat's paw-placing behavior, or presents a newly hatched chick with a mechanical toy in place of a mother and reports effects of the resulting abnormal imprinting, or attempts to teach an ape sign language? Surely this is legitimate research in behavioral science, but just as surely, it is not investigation of proper behaviors of the animals being studied. The point of experiments such as these is to probe into the mechanisms, the machinery, by which proper behaviors *are* produced. To know what will deflect a mechanism from proper performance of its tasks, how it will perform under abnormal conditions or when altered in certain ways, can yield strong clues about how it is constituted, how it works inside, and hence how it normally manages to produce *proper* behaviors. It goes (or should go) without saying that the experimentalist does not perform *random* experiments on the animal to be studied. Not any old facts about how the animal will behave if mutilated in random ways or subjected to random adverse conditions interests the scientist—only facts that cast light on the mechanisms behind proper functioning.

What distinguishes the core life sciences from the physical sciences is a difference not in the natural kinds being studied but in the point of departure for the study. What is logically first for the core life sciences is the study of proper or normal function. Of course, there is also abnormal physiology, abnormal psychology, and so forth, which are studies of common aberrations, common malfunctions, of biological systems. But these subjects cannot even be defined except by *contrast* with proper operation of these systems. The study of biologically proper behaviors is prior and foundational; the study of abnormal function is a study of departures from this norm. As these departures become more extreme, the study of abnormal function merges slowly into a study of mere chemistry and physics.

8

The Green Grass Growing All Around: A Philosophical Essay on Ethology and Individualism in Psychology, Part 2

In the last chapter, I claimed that behavior is the functional form of an animal's behavior. My job now is to spell out implications of this thesis for understanding mechanisms of behavior control. At the far end of the tunnel, I will emerge with a rough thesis concerning what it would be like to understand the role that intentional states—beliefs, desires, intentions, seeings, hopings, etc.—play in the control of human behaviors.

I can summarize the main points I have made and will make in terms of the currently popular image of the organism as an input-output device taking in stimulations and emitting behaviors. In the last chapter, I argued that each organism emits an uncountable number of outputs, each of which is describable in an uncountable number of ways. But the only output forms of interest for the study of the organism as a living system are those that have biological functions. The animals' outputs must be described according to their functional forms. Only these are forms that it is the business of life science to explain.

The rest of the argument is roughly as follows. The functions of behaviors are, by definition, functions performed through mediation of the environment. Indeed, the functions that define behaviors often reach very far out into the environment, both in time and in space. So behavioral-output forms must be described in relation to the organism's environment, both its proximate environment and sometimes its very remote environment.

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