COGNITION, LANGUAGE and CONSCIOUSNESS: INTEGRATIVE LEVELS

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3 Reflections on Thinking in Animals

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As recently as two or three years ago, I would have gone to great lengths to try to convince my reader (a) that the utility of the concept of cognition is questionable, and (b) that the mind doesn't even exist. The first claim conceivably falls within the realm of scientific discourse: If someone claims that the concept of cognition has scientific value, then one should examine the available data, see how the concept is employed, and dispute it where more powerful alternatives are at hand. The second topic, the existence of mind, is an issue in the realm of pure philosophy. Scientific inquiry should, I now believe, proceed without paying it any mind.

Instead of arguing against the utility of the concept of mind, I will argue that the original argument is pointless. I will do so by drawing a distinction between the study of behavior and behaviorism, by examining some of the traditional arguments against mentalism in psychology, and by examining some of the ways in which the theory of evolution has been used with respect to thinking in animals.

BEHAVIORISM VERSUS PRAXICS

The ism that has been most concerned with the utility of the concept of mind is behaviorism. Behaviorism began early in the century as a movement for reform in psychology. Its founder, John B. Watson, like many psychologists of the day, was disillusioned with the progress that had been made in the investigation of mind—psychology's traditional subject matter. He argued that the subject matter of the field should be changed to the behavior of organisms and that all investiga-

tions of mind and consciousness should cease (e.g., Watson, 1913). The study of mind held its own, and, with the advent of computers and the various alliances that have been formed between psychologists, philosophers, linguists, and computer scientists, the study of mind has become, once again, the primary subject matter of the field.

Consistent with the dominance of a concern with mind, behaviorism as a movement has died. The few reformers still around today are largely ignored by the field. As I have discussed in detail elsewhere (Epstein, 1984a), however, the movement had substantial outcomes: First, it helped to convince many people that the behavior of organisms was a legitimate subject matter, and second, it produced a school of philosophy, which today is the appropriate referent of the term "behaviorism." Others (e.g., Bergmann, 1956) have argued that the behavioristic movement left an indelible mark on traditional psychology itself: Modern cognitivists are now almost pathologically concerned with objectivity in method and terminology, an outcome, in part, of early behaviorism.

Behaviorism as a movement is dead, but the philosophy survives. And, for a variety of reasons, it is important to distinguish the philosophy from the scientific discipline that the philosophy helped to inspire—to distinguish the *ism* from *the study of behavior*. The latter has been labeled "praxics," a blend of "physics" and the Greek *praxis* for "behavior" or "action" (Epstein, 1984a, 1985c, in press-a). Modern behaviorism makes many controversial claims about the nature of mind, feelings, language, perception, and free will (Zuriff, 1985), but it is not in the nature of science to make such claims.

Note, too, that you can be a behaviorist without being a praxist, and vice versa. You may subscribe to the philosophy but not study behavior. Conversely, you may study behavior and not believe in the philosophy. You may study behavior *and yet believe strongly that feelings are important, that mind exists, and even that people have free will.* There is no contradiction here. One's ability to determine lawful relationships between events in behavior and the environment—to discover how behavior varies as a function of genes, physiology, anatomy, conditioning, sleep deprivation, drugs, evolutionary history, nutrition, and so on—should not be affected at all by such beliefs. Physics has been advanced, and is still advanced, by thousands of individuals who believe in mind, free will, feelings, and, sometimes, even God.

Yet for many years only true believers were allowed to work in behavioral laboratories. Others have made their way in, but they have been known to skulk. What a waste of talent! For the science of behavior to grow and flourish, it must break free of the ism, and the laboratory doors must be opened to all.

Lately, I find I am much less concerned with behaviorism and far more concerned with praxics. I remain relatively convinced by behaviorism, especially by the arguments that behaviorists make regarding mind and cognition. But I have come to regard the arguments, or at least my own participation in them, as unproductive and superfluous for several reasons: First, people's opinions on

these issues are very, very hard to change. You can, sometimes, change someone's opinion about the theory of evolution by pointing to a very impressive body of evidence which supports the theory. But no data of which I am aware will convince someone that he or she lacks a mind or free will.

Second, I have found that whenever one debates about free will, mind, or feelings—the traditional triumverate in philosophical psychology and modern behaviorism's triumverate of controversial concerns—emotions turn up. I have seen red faces, I have seen shouting, I have seen spittle, and I have been insulted. On one occasion (and I have this on tape) I was challenged to a duel.

Third, debating these matters now seems to me to be a kind of academic backpedalling. When you backpedal on a modern bicycle, the gears do not engage. You don't go anywhere. It is fun to backpedal—everyone does it—especially when one is coasting down a hill and one's leg muscles are atrophying. When I first started investigating novel behavior in animals, I was invited to debate, to be in symposia—not with praxists, but with cognitive psychologists, some of whom were troubled by my work. As one might expect, I was rarely called on to clarify my procedures or to defend my theories but rather to defend *behaviorism*! In other words, I was spending a lot of time backpedalling—defending a school of thought which, as I now see it, no datum can defend. And I was being kept from moving forward in my research. That is the point. Science and scientists suffer when philosophy intrudes.

Fourth, the counterarguments to the standard behavioristic positions are not entirely absurd. There is stuff inside the heads of most people, and the nature of the stuff is not yet well understood. Not all behavior is predictable at this point—in fact, a great deal still seems fairly mysterious. The role that genes and physical maturation play in language acquisition and other complex behavioral phenomena is not yet clear. In short, the data are not yet in.

Finally, no one can deny that abstract models, the coin of modern cognitive psychology, have proved useful historically, even models as simplistic as Rutherford's atomic solar system.

COGNITION

Let me now backpedal by reviewing some of the arguments—mainly behavioristic arguments—against the use of the concept of cognition in the interpretation of animal behavior. I do this reluctantly. It was Hans Reichenbach who said that the history of philosophy would advance ever so much faster "if its progress were not so often delayed by those who have made the history of philosophy the subject of their research" (Reichenbach, 1951, p. 15). I suggest further that our understanding of the behavior of organisms might advance ever so much faster if we spent more time studying it and less time philosophizing about it.

I present five traditional arguments:

1. *The search for variables.* An appeal to mind can obscure the search for the controlling variables of the behavior for which mind is said to be responsible. You see some extraordinary behavior, attribute it to "insight," "reasoning," or "the self-concept," and further inquiry ceases. The contribution of other variables—genes, physiology, anatomy, nutrition, conditioning, sleep deprivation, and so on—is not determined, and, even worse, an illusion of understanding is sometimes created. In attributing the behavior to a "self-concept," we might mistakenly assert that we now understand that behavior.

2. *The reification error.* Sometimes the kinds of concepts that are introduced in discussions of mind or cognition are unjustifiably reified. One commits the so-called "reification error," or "hypostatization," or what Reichenbach (1951) labeled "the substantialization of abstracta." This is often done with "self," "memory," "will," "unconscious," and so on. That is, sometimes people mistakenly use these terms as if they refer to things when they do not. "Will" is more like "love" or "democracy" than like "brain" or "table." That is not to say that such concepts are necessarily useless. But it is a mistake to treat an abstract noun as if it refers to a thing.

3. *Property as explanation.* We sometimes commit an error about which Isaac Newton warned in his *Principia*: We attribute some phenomenon to a property of that phenomenon. For example, we see a child striking other children on a playground—that is, acting "aggressively." Aggressiveness is a property of or description of the behavior. We ask, "Why is the child behaving in this way?" and the answer, sometimes, is "because the child is aggressive." But aggressiveness does not explain the child's behavior, and, at least in this instance, it sheds no light whatsoever on the behavior. We shed more light by identifying variables of which the behavior is a function: The child's parents strike each other or strike their children, the child effectively controls the behavior of other children by striking them, the child lacks sleep or certain nutrients in his or her diet, and so on. To attribute *blank*ful behavior to *blank* tells us nothing about where the behavior comes from.

4. *The teleological error.* In spite of all the caveats and reminders, we all tend, from time to time, to explain current phenomena by referring to *future* events. "Why are you going to the dentist?" "*To have a tooth pulled.*" Why did the chicken cross the road?" "*To get to the other side.*" We excuse ourselves for making such statements by distinguishing between "reasons" and what are sometimes still called "causes." My reason for visiting the dentist was to have the tooth extracted, but there are many possible variables that contributed to the occurrence of the behavior: My tooth was decayed, a nerve was damaged, my wife instructed me to go, in the past under similar circumstances a visit to the dentist removed a source of pain, and so on. Explanations of current events must be in terms of past or concurrent events.

5. *Post hoc ergo propter hoc.* Finally, we tend to rely on sequence to tell us what caused what, but as Hume, Michotte, and many others have noted in various ways, sequence can be misleading. If, on the approach of a tiger, one first feels fear and then runs, one might attribute the running to the fear. James and Lange turned the causal sequence around: Incipient running, they claimed, produces changes in your body that you feel as fear. And indeed most people have had at least a few dramatic experiences in which motor behavior very obviously preceded a rush of emotion. Remember the time you veered your car away from a child in the road and *then* experienced a panic reaction? But to attribute a causal role to either emotion or motor behavior on the basis of such incidents is unnecessary and misleading. Rather, as Skinner has pointed out, the tiger can be said to have produced *both* the emotional and motor behavior. The sequence in which these occurred was presumably determined by other identifiable variables.

To all of these criticisms the cognitive psychologist—that is, *the psychologist*—has answers, and, indeed, I now believe that praxists and behaviorists have been at fault by not listening to the answers more closely. I am not saying that we should be *persuaded* by the answers. Nor am I suggesting that praxists should abandon their concern with the concrete, measurable, and manipulable variables that determine behavior. Rather, I am saying that no psychologist worth his or her salt would deliberately commit any of the five errors I have listed above. Both the methods and theories of many modern experimental psychologists are fairly sophisticated. The sloppy thinking that behaviorists sometimes attribute to cognitivists is, I'm afraid, often in the eye of the beholder.

The Controlling Variables. Many psychologists are simply not interested in the controlling variables of behavior, and why should they be? The key is subject matter. The subject matter of the psychologist is *mind.* The effect that some drug or reinforcement history has on *behavior* is beside the point. Events in behavior and the environment are of interest only to the extent that they shed light on *mental life.* I ask my readers, especially those who have an interest in behavior per se, to pretend that they have a strong and sincere interest in a *science of mental life* (I have to struggle to do this, but it is an invaluable exercise). The determinants of behavior are now beside the point. You are interested in the nature, "structure," and, perhaps, the "function" of *mind.* Does it work like a computer? Is it a serial processor or a parallel processor? Does it have different types of memories? Will one general set of laws account for all of its operations?

The assertion that mind is a legitimate subject matter—or, in other words, that psychology is a legitimate field—leads to "interesting questions." It has "heuristic value." It leads (perhaps serendipitously—but that is the case in all of the sciences) to "interesting discoveries" that often have "practical applications." End of exercise.

The study of behavior can be similarly defended (Epstein, 1984a, 1985c, in press-a).

Reification and Explanation. One *can* talk about such things as "memory," "self," and "mind" without committing the reification error. "Self," for example, has been used by people who know enough not to commit this error (e.g., Kagan, 1981) as a hypothetical construct that unifies a variety of apparently unrelated phenomena in early childhood: A variety of "self"-related behaviors all seem to emerge within a few months of each other, suggesting some underlying, possibly causal, process that is common to all (Lewis & Brooks-Gunn, 1979; cf. Epstein & Koerner, 1986). If a variety of self-related behaviors indeed appear in lockstep fashion, and if they prove to be insensitive to variations in the environment, then one may eventually find the actual physical embodiment of "self"—maturational changes in the brain which somehow facilitate the emergence of self-related behaviors. (Note that the mentalist would not be satisfied with the changes in anatomy and physiology alone. "Self" may be correlated with those changes, but it is *not* those changes.)

Cognitive concepts may be abused: They may be reified or used to explain behavior from which they are merely inferred. But they may also be used conservatively and constructively: They may be used to order apparently disorderly data; they may be used as tentative explanations for which, ultimately, some sort of physical instantiation could be found.

Purpose. One can also talk about purpose and goals without committing the teleological error. In fact, I have never met a psychologist who believed that the future controls the present, so what is all the fuss about purpose?

Purposeful behavior need not always have a purpose. A little boy may rummage through a box of toys without a goal "in mind." Does a goal make any difference? Is the search behavior any different if he is envisioning a little yellow truck? Is the behavior any different if he says to himself "I want the yellow truck" as he rummages? Very likely it is. But the behavior we speak of as indicative of his goal is just more behavior: It is not in the future; it is current—concurrent with the search behavior (which is also indicative of "a goal"). It demands its own explanation (did his playmate just ask him for the yellow truck?). *It probably also makes a difference in the behavior we observe,* although its sole function may be discriminative—which means, in a sense, that it is a kind of reminder: It helps keep the search behavior from drifting to other objects or other behaviors. Cognitivists very often mean little more than concurrent behavior of this sort when they speak of purpose.

Sloppy thinking turns up in every discipline, but cognitivists, I now believe, have no special claim on it. Psychologists have, from time to time, committed each of the errors I have just listed, but they may still study what they call "mind" without committing any of them. *Psychologists and praxists are interested in different subject matters, not different standards of research or analysis.*

THE IRRELEVANCE OF EVOLUTON

How, if at all, can the theory of evolution help us to shed some light on "thinking" in animals? Can it tell us whether the notion of cognition in animals is useful?

I regret to say that, unlike other authors in this volume, I believe the theory of evolution cannot help us, for, when it comes to cognition, evolution can easily be twisted to suit one's needs. The theory of evolution has been used in three different ways with respect to the concept of cognition in animals: to support it fully, to support it with reservations, and to reject it entirely (Epstein, 1984b). Moreover, I believe that none of these applications of the theory is unreasonable.

Support for Cognition

The first pertinent exemplar is the position taken by George J. Romanes (Romanes, 1888), a 19th-century naturalist who, prompted by simple problem-solving behavior or even facial contortions in animal subjects, spoke freely about the mental world of animals; that is, he interpreted animal behavior in traditional human terms, a practice sometimes called "anthropomorphism." Romanes justified this practice by appealing to the theory of evolution, which, after all, drew attention to the fact that species are not quite as separate and diverse as many people had believed. There is continuity among species.

In his new book *Animal Thinking,* Griffin (1983) uses the same approach. From instances of human-like animal behavior he infers the existence of mind, feelings, and intentions in animals: Vultures use stones to break open ostrich eggs. A green heron takes bread from a picnicker's table and strews it on the surface of a nearby creek in order to catch fish that rise for the bait. Polar bears throw large chunks of ice at seals to wound or kill them. I know of no way to discredit the kinds of speculations Griffin makes about such animals, and I see no point in doing so. But—and Griffin does not express interest in this in the book—I think we have come a long way toward understanding and predicting the kind of *behavior* about which he speaks, and an effective understanding of the behavior seems to me to be as valuable an achievement as an effective understanding of "mind."

Behavior. Epstein and Medalie (1983), for example, showed that a pigeon with relevant skills could "spontaneously" use a box as an extension of its own beak to reach an object behind a wall. More important, we offered a detailed

account of the emergence of the performance in terms of empirically-validated principles of behavior. The pigeon was first taught to push a flat, hexagonal box toward a green spot placed at random positions around the base of a large cylindrical chamber. Then a Plexiglas wall was installed in the bird's chamber. At the base of the wall was a gap, at the center of which was a small metal plate. In the absence of the box, we taught the bird to peck the plate. Then we gradually moved the plate behind the wall, so that the bird had to stretch its head beneath the wall to peck the plate. The box was then added to the chamber on the bird's side of the wall, and we continued to reinforce pecks to the plate while extinguishing behavior with respect to the box.

Finally, after there were no signs of pecks to the box for five consecutive daily sessions, we conducted the following test: With the box still in the chamber on the bird's side of the Plexiglass wall, we moved the plate just out of the bird's reach on the other side of the wall. The resulting performance resembled that of a young child faced with a comparable problem. The bird stretched repeatedly toward the plate, it pushed up against the wall, it scraped its feet on the floor, stretched again, and so on. After about 30 seconds, it approached the box, pecked it weakly, turned and approached the wall again, and then stretched again toward the plate. After 90 seconds, it began, quite suddenly, to push the box directly toward the wall. It pushed the box under the wall, thrust it against the plate and *then began to peck the box repeatedly, which was now in contact with the plate.*

The bird's performance can be accounted for in terms of empirical principles. For example, the bird's first pushes seem to be the result of a phenomenon called *resurgence* (Epstein, 1983, 1985b): When, in a given situation, behavior that was recently effective is no longer effective, other behaviors that were previously effective under similar conditions tend to recur. The bird's stretches toward the plate are ineffective; they are undergoing extinction. Therefore, behavior with respect to the box should become more likely, and, indeed, the frequency of behavior with respect to the box steadily increases, and the bird finally starts to push.

Even more striking were the performances described by Epstein, Kirshnit, Lanza, and Rubin (1984): They showed that pigeons with appropriate training histories could solve one of Köhler's (1925) classic box-and-banana problems in an insightful, human-like fashion. They also showed that different training histories make a difference. To solve the problem insightfully, a bird must have learned (a) not to jump and fly toward the target (a small facsimile of a banana suspended just out of the bird's reach), (b) to push a box toward targets at ground level, and (c) to climb onto a fixed box and to peck a banana overhead. Birds solved the problem clumsily if (a) jumping and flying had not been eliminated before the test or (b) they had learned to push but never to push toward a target. Subjects could not solve the problem at all if they had never learned to push or if they had never learned to climb.

In the test the birds were faced with the banana out of reach and the box elsewhere in the chamber. Birds with appropriate histories at first seemed confused when confronted with this scenario; they oriented toward the banana, turned in circles, oriented toward the box, and so on. Then, in a continuous series of movements, they pushed the box toward the banana, stopped when the box was beneath the banana, climbed, and pecked. More complicated performances have also been generated (e.g., Epstein, 1985a, in press-b), and, in each case, detailed moment-to-moment analyses have been offered (also see Epstein, 1981, 1984c, 1985c, 1986, and Epstein, Lanza, & Skinner, 1981).

Experiments of this sort have led to a formal theory, called Generativity Theory, of the emergence of ongoing behavior in the natural environment, and Epstein (1985c) presented equations and a computer model based on this theory which have proved reasonably successful in predicting ongoing, intelligent performances in human subjects.

Skepticism

The theory of evolution has been used not only to justify the concept of animal mind but also to question it in various ways. For example, Lloyd Morgan, another naturalist, used evolutionary theory to justify a conservative position. Today we attribute to him a variant of the principle of parsimony, sometimes called Morgan's Canon (Morgan, 1894). Though some speak of it as if it rules out the study of mind, in fact it does not. Morgan said merely that the theory of evolution implies not only continuity among species, but also gradations and differences among species. Humanlike behavior in an animal, he said, must be interpreted economically. When applied to cognition, that means that one must not refer to higher mental processes when lower ones will do. To my knowledge, Morgan never questioned the existence of cognition in animals; he merely raised a question of degree (Epstein, 1984b).

Rejecting Cognition

The third use of evolutionary theory arose as a result of advances in laboratory research on behavior during the first decades of this century. Thorndike's first puzzle box experiments were published in a monograph in 1898 (Thorndike, 1898) and then in book form in 1911 (Thorndike, 1911), and Pavlov's work came to light during the first decade. A variety of progress was made in accounting for animal behavior, at least under laboratory conditions, in terms of conditioning—in objective, cold, mechanistic terms, without reference to mind.

Once again, the theory of evolution was applied to the question of cognition, and this time, cognition was rejected entirely: If there is continuity among species, the argument went, and if nonhuman animal behavior is explainable by laws of conditioning, then the behavior of *human beings* should also be explaina-

ble by laws of conditioning. Thus mind is a spurious concept, even in humans, or, to put the matter crudely, *we err not only by anthropomorphizing with animals but also by anthropomorphizing with people*. This argument is typical of early behaviorism.

All of these views have weaknesses, but I will comment only on the last. The assertion that animal behavior is explainable by laws of conditioning was not supported by data in the 1920s when it was first made with any vigor, and it is still not fully supported today. One also has the extrapolation problem: Though there is continuity, there are also differences and gradations. Thus, even if all nonhuman behavior is explainable this way, it is not necessarily true that all human behavior is explainable this way.

Regrettably, the theory of evolution, which has been so illuminating in our understanding of behavior, sheds no light on the concept of mind.

CONCLUSIONS

Backpedalling is excusable if you have nowhere to go. But cognitivists seem confident that they are travelling new and exciting roads and that great destinations await. Behaviorists are philosophers, and since philosophers have been known to get airsick simply by detaching themselves from their armchairs, we need not worry about how far they will travel. Finally, laboratory praxics is furthering our understanding of the *behavior* people associate with cognitive processes.

So why backpedal?

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