The Analysis of Human Behavior in Context

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Does the name of the special interest group, “The Experimental Analysis of Human Behavior,” imply that those who analyze the behavior of human animals must organize themselves apart from those who analyze the behavior of nonhuman animals? Is the use of nonhumans in experiments really not relevant to the analysis of the behavior of humans? If so, then something must have changed. Many differences exist, of course, between the behavior of humans and nonhumans—humans, for example, cannot fly under their own power—but have we really isolated differences in principle, differences that require separate organizations for the study of each? I will try to indicate why I believe this is a serious concern, where the concern comes from, and what, perhaps, might be done to maintain what was once a flourishing bidirectional relation between research with humans and nonhumans, in both basic and applied research.

Key words: human behavioral research, nonhuman behavioral research

Like Alan Baron, the first recipient of this award (The Experimental Analysis of Human Behavior Special Interest Group Distinguished Career Award) last year, whom I am honored to follow, I, too, came into research on the behavior of humans after more than 10 years of experimenting almost exclusively with nonhumans as subjects. Also like Alan, I am concerned with the growing lack of communication between those who do research with humans and those who work with nonhumans. I see the current status of that once-flourishing partnership as a problem. Unfortunately, I have no surefire solutions, but it might be worthwhile if I indicated where my concerns come from, and why those concerns are relevant to the current practice of both basic and applied research with humans, and to the current education of behavior analysts.

As I worked more and more with human subjects, and as I looked at

more and more complex behavior, it became increasingly clear to me that my earlier years of work, mostly with rats, cats, and rhesus monkeys (although others in my laboratory worked with pigeons, I, personally, never did), had been an apprenticeship. It actually taught me how to do research with people.

This conclusion will undoubtedly surprise some of today’s behavior analysts. Those most surprised are probably behavior analysts who have, themselves, never worked with nonhumans, and so have never seen that the same principles and behavior-change techniques they have found so effective with humans work also with the so-called lower organisms. How many behavior analysts have never even applied those techniques when teaching their own dogs, cats, birds, and other house pets? How many have even applied them when teaching their own children? Upon starting my research with humans, I found that the very same methods for generating and maintaining behavior in nonhuman subjects, and the very same underlying principles, continued to work effectively. To be honest, however, I have to admit that I, too, was at first surprised that my convictions about the generality of our earlier work turned out to be so completely justified.

But after my first 10 years of re-

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search with nonhumans, the next 25 or so years found me maintaining a laboratory in which studies with human subjects, both normal and brain damaged, went on side by side with experimentation in which mice, pigeons, and rhesus monkeys participated as subjects; during a sabbatical year, when I had the privilege of working in Joe Brady's laboratory at Johns Hopkins, I did experiments almost exclusively with baboon subjects.

Speaking of the relevance of non-human to human research, and vice versa, I must tell a little story about those baboons. At the time, researchers in Brady's laboratory were training baboons to increase their own blood pressure (e.g., Harris & Turkkan, 1981). Because the experimental procedures might also have affected various other bodily processes, the researchers regularly took blood samples from the animals, samples that then had to be analyzed, just as yours and mine are when our physicians want to know some important things about us. But when asked to do the standard tests with baboons, the hospital blood laboratory refused. To get around this refusal, Brady's laboratory then sent the samples to the blood laboratory not as baboons' blood but under names like John Boone, Mary Boone, Daniel Boone, and other members of the Boone family. No problem; the research was able to continue. As far as the blood laboratory was concerned, the baboon samples were not distinguishable from their regular human samples.

In my laboratory, too, I found that we could use the same procedures with human and nonhuman subjects. In some of the work, we even used exactly the same apparatus both for people and monkeys. Human subjects sat on a chair in front of the matrix of keys onto which stimuli were projected, and they selected stimuli in conformity with the reinforcement contingencies we had set up. Rhesus monkeys were brought into the same room in transport cages, which were attached to the

wall around the key matrix, and then the wall of the cage was lifted so that the animal would have access to the stimuli on the keys and to the reinforcement tray. We found, for example, that the fading procedures we were investigating at the time were effective in teaching monkeys and people the same stimulus discriminations. Between the two species, only the reinforcers varied. Looking at the data, you could not tell which kind of subject had produced them.

There were, of course, many early examples of extensions from nonhuman studies. There was Fred Keller's code-voice technique for teaching Morse Code (Keller, 1943), something I was exposed to during my military service before I knew anything about behavior analysis. (Imagine how astonished I was, several years later, when I found myself working with Keller on his first research grant, which funded continuing studies of the code-voice method.) Later, Og Lindsley's work with psychiatric patients opened up an important behavioral area to a scientific approach (Lindsley, 1956). Ayllon and Azrin's (1968) work with institutionalized people, particularly the development of the token economy, followed by Girardeau and Spradlin's (1961) application of similar techniques to those with mental retardation, began a revolution in the treatment of the handicapped. The influence of Marian and Keller Breland, who had left Skinner's laboratory to make their living applying operant techniques to the training of nonhuman animals, can still be seen in the splendid demonstrations by marine mammals, birds, and other animals in our theme parks, and in the worldwide applications of "clicker training" to household pets—dogs, cats, birds, horses, and other species. Greenspoon's original applications of reinforcement techniques to human speech (Greenspoon, 1955) and Bijou's experimental adaptations of positive reinforcement in research with children (Bijou & Sturges, 1959) emboldened many of the rest of us to ex-

tend our research to our own species. Those original studies paved the way for even more sophisticated applications in the areas of autism and related illnesses, in academic education, in business practices, and others.

At the time, those early successes were heady discoveries—showing that our behavioral principles and procedures were generalizable even to humans. I realize that we have gone beyond that discovery phase now, and I am not recommending that everybody go through the same history that I experienced. That is a mistake that too many old professors make. They feel that students should go through all the troubles—and triumphs—that they went through. The fact that behavioral principles and procedures that have been discovered in the nonhuman laboratory work with humans, too, is now well established, and needs not be rehashed. Nevertheless, that fact should not be forgotten, either. Work is still going on with nonhuman subjects that is likely to change the ways we look at much human behavior, too, and that work must not only be accepted but encouraged. No science can stand still; if it cannot keep the discoveries coming, the interpretations refined, the procedures sharpened, and the extensions broadened, it will wither away. Something more exciting and promising will take its place. Even though our ultimate concern may be the behavior of humans, it would be folly to cut off our interest in the nonhuman research that has been the major source of our scientific growth.

In addition to survival considerations, an often applied but rarely enunciated methodological practice in relating nonhuman to human studies is more than relevant here. Gisiner and Schusterman (1992) stated the principle of a “bottom-up” approach to research on complex behavior. They wrote,

Animal language research includes a variety of experimental studies of complex learning and cognition by nonhumans in which human language serves as a model for experimental design

and data interpretation. . . . However, the “top-down” approach of adapting terminology from the study of a very complex set of performances . . . to considerably less complex performances . . . has not been successful in . . . defining the learning abilities required for normal human language performance. . . .

We have, instead, adopted a “bottom-up” approach: starting with directly trained, differentially reinforced . . . relationships and moving up through increasingly complex relationships emerging from and consistent with earlier learning. This gives us an operational model, or “how-to” guide, for investigating the role of reinforcement-based learning skills in complex cognitive performances. (p. 643)

Schusterman’s own work (1967) on errorless reversal learning, and more generally, the whole area of errorless learning, is a case in point. Although that work reached its zenith in programmed instruction in such complex areas as crystallography (Chalmers, Holland, Williamson, & Jackson, 1965), neuroanatomy (R. L. Sidman & Sidman, 1965), psychology (Holland & Skinner, 1961), and many academic subjects, it started with much earlier nonhuman animal research by Skinner (1938) and Terrace (1963a, 1963b). That seminal work on errorless learning by rats and pigeons was then effectively extended “upward.” I have said before that there should be statues of Terrace’s pigeons in front of every school of education (M. Sidman, 1985).

Nevertheless, although we, as professionals, accept the relevance of nonhuman research to the behavior of humans, it is important to realize that most of the rest of the world does not. Those early discoveries, in which we found that what we had learned about the behavior of nonhumans could be applied directly to humans, were heady because they called for a major alteration to a prevailing viewpoint, namely, the view that human behavior goes beyond what scientific methodology can investigate and explain. Even today, the predominant belief in our culture is that human behavior is self-regulated through inner agencies like the will or the personality, not by observable, measurable, and controllable en-

vironmental determinants. Through those inner agencies, we are held to regulate ourselves. The notion that our behavior, like that of nonhumans, is capable of explanation through scientific study, that it is amenable to change by means of procedures that have been scientifically derived and validated, is still not generally accepted.

I suspect that this unwillingness to accept human behavior as being subject to environmental variables comes about, at least in part, because such a view seems to many to deny us the control of our own destinies, to remove us from our central position in the universe. I think there are more valid reasons for the central position that we do occupy in the animate world—that we occupy both for better and for worse, and perhaps only temporarily—but I need not get into that discussion here.

Applied behavior analysts, therefore, who often do not have—and need not have—any interest in becoming scientists, are not, as many of them have been taught to believe, simply practicing an accepted profession. They are doing something special, something that flies in the face of much currently accepted wisdom, something that represents a major shift in society's belief about what it means to be human. All behavior analysts, basic and applied, whether they work with humans or nonhumans, are out at the forefront of a continuing intellectual and social revolution. In their training, they should be made aware of this, and taught to communicate it to their students and their clients. In their continued development, they should often be reminded of their role as pioneers.

This is not just a matter of seeking prestige through inclusion among the natural sciences; it is a matter of maintaining a discipline because it is needed, because it is uniquely effective in solving important problems. If we do not successfully nurture within our culture the general acknowledgment that behavioral research and practice fall within the domain of the natural sciences, then we shall see what is cur-

rently mostly neglect of our actual and potential contributions transformed into active opposition—as has already happened, for example, in traditional clinical and cognitive psychology's rejection of behavior analysis.

There, then, is one constructive reason for the existence of a professional society that is devoted specifically to human behavioral research. Those researchers, along with practitioners, are in a special position with respect to a significant social movement—the placement of human behavior along with the behavior of other organisms within the realm of natural science.

In another direction: It is obvious that much human behavior does differ from the behavior of nonhumans, even from those most closely related to us. Nonhuman species also, of course, display dissimilarities to each other. Different environments, bodily structures, physiological processes, and inheritances have brought distinctive behavioral contingencies into play for different species. Such contingencies involve different reinforcers, response capabilities, sensory equipment, survival mechanisms, and so on. Some of those variations actually give nonhumans advantages over humans: Birds fly under their own power and we do not; many animals can run faster than we can, are better swimmers, are stronger, or have keener senses of sight, hearing, or smell. Such variations are themselves amenable to scientific study, and a complete science of behavior must take them into account. So far, however, to account for behavioral variations from one type of organism to another has not required differences in principle. The critical role of contingencies, along with general principles of reinforcement, stimulus control, and motivation, combined with principles and processes that other biological sciences have taught us, have so far sufficed to systematize much of what has been observed in the behavior of organisms.

Given that obvious differences do exist and are undoubtedly responsible

for the predominant position of humans in today's world, we still cannot claim that we have isolated special principles that require separate organizations for the study of nonhuman and human behavior. What is it about humans, therefore, that justifies the existence of a special interest group called "The Experimental Analysis of Human Behavior"?

Humans, of course, speak and have developed languages, behavior that we have seen no other kind of organism perform. Many have been tempted, therefore, to focus on language as a kind of behavior that requires special treatment. Although other organisms are capable of some vocal and gestural communication, none have been shown to behave in ways that our linguistic sophistication has made possible. Vocal and written speech do indeed make it possible for us to do things that no nonhuman has ever been observed to do. But have we found any behavioral principles that would not also apply to other forms of behavior if they, too, were characterized by such a large number of possible response variations? That is to say, are there anything more than quantitative differences between linguistic communication and other classes of behavior?

For example, our vocal apparatus permits us a huge vocabulary; there seems to be no restriction to the number of words we can say or write. Unlimited variation seems possible in the rate of our vocal output, in the sequences of speech units (what we call grammatical structures), in speech rhythms, in modulations of pitch and tone, and so on. Through such variations, infinite levels of referential and emotional meaning can be attributed to our utterances. Written words keep the past with us and bring the future into the present, permitting us to be influenced by events to which we have never been exposed, to draw diagrams of things that nobody has ever seen, to plan for the future, and to preserve the past. Our language permits us to invent and build machines—airplanes, sub-

marines, guns, and so on—that more than make up for many of the natural abilities in which nonhumans surpass us. Vocal and written language, therefore, does make it possible for us to behave in ways that no nonhuman can match.

Does any of the complex behavior that language makes possible, that nonhumans are not capable of, require explanatory principles beyond those that help to account for the behavior of nonhumans? Future developments may necessitate principles that account singularly for human behavior, but I do not believe that any principles unique to human behavior have yet been demonstrated, or even shown to be needed. Still, because we are humans, it is important for us to ask the question. Because of our predominant position in today's world, with no other species in a position to control our behavior, we have to do that job ourselves. We have to know how to control our own behavior. There, then, is another reason for a behavior-analytic specialization in human research. Can we control our own behavior by making use of what we have already learned from other organisms, or will we have to appeal to processes that have not yet been discovered?

I have had to face this problem head-on in my own research. It looked for a long time as though phenomena that I interpreted as instances of equivalence relations were unique to humans, and I had to remain prepared to acknowledge that possibility. Now, however, the elegant studies that Schusterman and his colleagues have reported with sea lions as subjects (Kastak & Schusterman, 2002; Kastak, Schusterman, & Kastak, 2001) have shown us that nonhumans, too, are capable of equivalence relations, and can be used as subjects to study the relevant phenomena. There is much more work to be done here, of course. If sea lions can show equivalence relations, other organisms must be similarly capable, and additional demonstrations will generate new discoveries. Equiv-

alence relations may well be involved, for example, in such phenomena as kin and species recognition (Schusterman, Reichmuth, & Kastak, 2000), imprinting, vocal communication by dolphins and others, and so on. "Bottom-up" research pathways are once again becoming possible in phenomena that for a while seemed exclusive to human behavior.

The predominance and importance of word-mediated communication in our lives does make it tempting for behavioral scientists to seek principles that rely on words—spoken, written, or subvocal words—as a special causative factor that may help us to understand much of our other behavior. Exponents of such a view feel, of course, that those interested in human behavior should form their own specialty groups. An alternate point of view, however, treats word-mediated communication not as a kind of fundamental causative factor, but rather as an important kind of behavior that, itself, needs explanation. That is why many of us view equivalence relations as particularly important in human behavior, even though they are not exclusive to human behavior. The observations that define equivalence relations help us to understand why spoken and written words are so important in our lives. The equivalence of words to other aspects of the world is a phenomenon that many have recognized. For example, in J. K. Rowling's *Harry Potter and the Sorcerer's Stone* (1998), the following conversation—about a being so evil that its name was not to be spoken—took place between the young Harry Potter and the elderly, wise headmaster, Dumbledore:

"Sir?" said Harry. "I've been thinking . . . Sir—even if the Stone's gone, Vol-, I mean You-Know-Who—"

"Call him Voldemort, Harry. Always use the proper name for things. Fear of a name increases fear of the thing itself."

In Shakespeare's play, *Henry VIII* felt it necessary to point out that "Words are not deeds," and a common theme

in opera, literature, and poetry is "Names live on immortally."

Clearly, Dumbledore understood the power of equivalence relations to extend the properties of things to words, and vice versa; Shakespeare, in stating that words are not deeds, was contradicting a popular belief to the contrary—as Skinner (1957) pointed out, we do not say "It is *not* raining" unless we first had some tendency to say "It *is* raining"; and writers in general take advantage of commonplace emotional reactions to names as living representatives even of people long dead—note also the almost universally effective emotive power of the names carved into the Vietnam Memorial in Washington, D.C.

And so, even though methods and principles derived from research with nonhumans also apply to humans, the behavior to which they apply is often exclusive to humans and needs, therefore, to be studied on its own. It is always necessary to ask whether behavior that is particularly human requires new scientific principles to account for it. So far, the answer to that question has been "no," but the possibility cannot be ruled out. The question, therefore, must be asked.

In addition, of course, we want to study human behavior because we *are* humans, even though many who oppose the scientific treatment of human behavior do so on the grounds that such treatment is inhuman. We want to know about ourselves and where we fit into the universe. The conflict here is between those who, like myself, are comfortable with the knowledge that we do fit, and those who have to believe that we occupy a special position, in control and not subject to natural law. Unfortunately, the latter are currently predominant, and in seizing their supposedly predestined control over both animate and inanimate parts of our world, they have led that world along a path of destruction. Special interest groups like this one, devoted to the experimental analysis of human behavior, are, therefore, doing something

more than just gathering and systematizing experimental data on human behavior. They are helping to place humanity within the world, not outside of and in conflict with it. I think it is worth keeping this broader social dimension of our work in plain sight.

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