The Behavior Analyst 1991, 14, 107-110 No. 2 (Fall)

On the Difficulty of Studying "Basic" Behavioral Processes in Humans

Marc N. Branch
University of Florida

Barone, Perone, and Galizio are to be complimented for bringing to the readership of *The Behavior Analyst* the issue of studying basic conditioning processes in adult humans. They are to be complimented especially for the articulate and logical way they have crafted their presentation. There is much to agree with in the paper, however I shall not dwell on those points but rather on ones that I hope will challenge the authors.

At the outset let me reveal my biases so that the reader (as well as Baron, Perone, and Galizio) can evaluate my views in light of my prior inclinations. One, I am not convinced that there is a qualitative difference between basic conditioning processes that exist in humans versus in nonhumans. Obviously, this is an empirical question, and the jury is still out so far as I can see. Certainly, it is premature to claim such a difference. Two, despite this, I am skeptical about the utility of employing (at least as they have been employed, for the most part, so far) direct analogues of laboratory conditioning procedures with adult humans.

My reservations come from two sources to which I hope Baron, Perone, and Galizio can respond. The first has to do with just how closely procedures with humans actually mimic those used with nonhumans, and the second, deeper, issue has to do with the nature of behavioral control.

Baron, Perone, and Galizio note that attempts to extend principles identified in the laboratory with nonhumans as subjects to the "real world" have often resulted in successes. The whole of applied behavior analysis and the use of behavior-modification techniques in psychotherapy testify to the success of these extrapolations. Yet, almost paradoxically, when attempts at nearly direct interspecies replication have been made in the laboratory they often have failed. Does this mean that the successes in extending behavioral principles to the nonlaboratory environment represent one of the grandest sets of epiphenomena ever witnessed? That certainly is one view, and apparently is held by some critics. Another possibility, however, is that the attempts at direct replication have not been as direct as the investigators had hoped. Baron, Perone, and Galizio point to one of the ways in which this may be the case when they note that in many studies steady-state behavior has not been established. Another possible problem, which seems to have received little attention, lies in the realm of response definition. In research with nonhumans, the measured activities typically have been identified empirically as functional units of behavior (i.e., as operants). That is, the measured behavior has been shown—via experimentation that led to the standard sorts of apparatus that are employed—to produce the "smooth curves" that indicate that "natural lines of fracture" have been identified (cf. Skinner, 1935, 1938). What evidence is there that a single push of a button by a human in a laboratory is an instance of a functional unit of behavior? Interesting in this regard is that handwriting, rather than simple button pushing, was employed as the response in one of the studies on effects of schedules of reinforcement in which distinctly temporally patterned behavior was observed under fixed-interval schedules (Gonzalez & Waller, 1974).

In the laboratory with nonhumans, response classes have been chosen with an eye toward functionality but also with another consideration. Attempts have been made specifically to minimize intrusion by either phylogenetic or ontogenetic influences. The "arbitrary" response of the lever press in the rat, for example, is chosen because rats were not selected genetically in environments in which levers existed and presumably they have not experienced such levers during their lives before they get to the experiment. This second consideration is illustrative of the deeper issue that confronts those who would conduct simple extrapolations from the nonhuman-laboratory situation to laboratory research with humans: the issue of the role of pre-experimental history. It is to this second issue that I now turn.

Baron, Perone, and Galizio correctly point out that effects of unknown histories can (and presumably have) play(ed) a role in research with nonhumans. They also note that, despite the fact that pre-experimental history can influence results of experiments with nonhumans, critics of attempts to mimic animal-lab procedures with humans point to pre-experimental history as a virtually insurmountable problem. They "do not see the justification for such asymmetry." I would like to submit that there is a justification: one that is closely related to the distinction that Sidman (1960) drew between "intrinsic" and "imposed" variability.

In Sidman's view variability in behavior could be considered "intrinsic" or "imposed." Imposed variability is that due to directly identifiable influences, whereas the sources of intrinsic variability were not possible to identify. Science proceeds by assuming that all variability is imposed by discoverable factors and then searching out those factors. A difference, at least potentially, between research with nonhumans and adult humans is that pre-experimental history can be imposed with nonhumans but not with adult humans. Baron, Perone, and Galizio provide an excellent example of this when they describe the experiment by Thomas (1969). Thomas suspected that the failure to see orderly stimulus-generalization gradients for tone frequencies in pigeons (cf. Jenkins & Harrison, 1960) was due to the fact that pigeons have relatively little experience with pure tones of different frequencies. That is, he suspected a role for pre-experimental experience. He found that by *imposing* experience with such stimuli, pigeons came to exhibit orderly gradients of generalization across tone frequency.

Compare Thomas's procedure to one that we might try to use in understanding effects of exposing adult humans to schedules of reinforcement in the laboratory. Suppose we get results that seem to indicate the operation of pre-experimental history (e.g., the subjects tell us they were following some rule they abstracted from the procedure). How are we to impose the suspected variable, or how can we eliminate it from our experiment? As best I can see, given our current knowledge we cannot do either. The influence of pre-experimental history stands essentially as a kind of intrinsic variation. When we suspect influence by pre-experimental experience with nonhuman subjects we can do new experiments in which the suspected influence is either removed or explicitly provided. That is, we can make it a source of imposed variability. With adult humans we are not afforded that luxury.

Baron, Perone, and Galizio suggest that history effects can be minimized by establishing steady-state baselines. [Whether they are right or wrong on this point, it still remains an important research tactic to establish steady-state behavior, but that is another issue.] I am not sure I see how this is the case. Histories can be prevented or specifically arranged to occur, but never can be eliminated once they have happened. The hope is, apparently, that long exposure to current contingencies can override the effects of prior history. Whether this is true seems to me to be an empirical question, and, ironically enough, if what I have argued is true then to answer the question will require research with nonhumans. By programming (i.e., imposing) certain histories and then exposing subjects for long times to new contingencies, we may be able to determine if effects of the history can effectively be rendered so small as to be irrelevant. This will be no small undertaking, however, because assessing latent effects of history is not a simple task. Furthermore, the task is nearly impossible with adult humans who have histories that cannot be arranged.

If research with adult humans is compromised by effects of pre-experimental histories such that variability from these sources has to be treated as intrinsic, what is to be the role of such research, and how is it to be carried out? One answer to the latter question, one that is adopted by most experimenting psychologists, is that such variability ought to be treated as intrinsic, making it appear necessary to resort to the techniques and procedures involved in inferential statistics. That seems a defeatist's response to me.

I see three ways of dealing with the issue. Two of these have proved, apparently, to be unconvincing to many psychologists, but I believe they are very important anyway. We should continue, one, to attempt to apply findings from research with nonhumans to humans, and we should continue, two, to try to interpret human behavior (including human behavior observed in the laboratory) in terms of the principles identified in research with nonhumans. These, despite the criticisms of naysayers, are critical to the development of behavioral science. Just as explaining some apparently anomalous phenotype as a result of natural selection strengthens our belief in that theory of evolution, so does explaining some apparently intractable instance of human behavior increase our confidence in the behavioral principles. For example, when Stemmer (1990) shows how basic behavioral principles can account for being able to understand a novel passive sentence or a novel structure-dependent sentence, our confidence in the generality of the principles is increased. If, by contrast, it were not possible to generate an account (as has been asserted by some, e.g., Chomsky, 1975), then we would believe our account to be deficient. Successful interpretation is a necessary (but obviously not sufficient) ingredient in any successful theory.

Successful applications, too, suggest that our theory is on the right track. Interestingly, failed applications also can help to flesh out a theory of human behavior. Consider, for example an instance where a behavior-modification technique based simply on rearranging contingencies fails. Presumably, instances of this kind gave birth to "cognitive behavior modification." One reason that current contingencies fail to have a beneficial effect is that behavior established by former contingencies interferes. In cases where "cognitive behavior modification" is indicated it is presumed that stimulus control of behavior by self-verbalizations (for example) is one source of the problem. Research with humans can then be aimed at studying how such stimulus control can be modified. In this way, the theory is enriched by examination of new ways principles can be employed.

Baron, Perone, and Galizio note that Estes (1972) was pessimistic about the likely success of trying to manage human behavior by applying techniques used in the animal-conditioning laboratory. I am less so because of the third way that research in the laboratory with humans may contribute. Actually, I should say a combination of research with humans and nonhumans will be needed. I speak here of what might be referred to as the study of "layering" of behavioral principles. Much, if not most, research in the experimental analysis of behavior has focused on effects of contingencies "uncontaminated" by other influences, including exposure to other contingencies. That is, most of the time we arrange our experiments so that effects of prior experience are minimized (note discussion above about selection of responses). Only a few experiments have been aimed at examining effects of specific sequences of experiences (e.g., Barrett, 1977; Wanchisen, Tatham, & Mooney, 1989; Weiner, 1969). Obviously, shaping of behavior or stretching ratios involves these sorts of issues, but research aimed explicitly at such problems has been relatively rare. It seems clear that if we are to understand how sequences of experiences influence behavior we shall have to study them. I see no reason why some of this research cannot be done with adult humans. To the extent that we can make guesses about the histories that people have, we can then study effects of adding new contingencies. The quality of our guesses, of course, should be improved as basic research with nonhumans helps us identify important variables.

The study of how sequences of experiences interact is an enormously important enterprise from the point of view of trying to improve behavioral technology. I have heard people say that behavioral techniques work well enough with nonhumans, retardates, and young children, but that they often are inadequate when working with adults. To the extent that this is true, it probably is a result of our poor knowledge of how to "layer" experiences.

As a final point, the reader may have noticed that I have tried to employ the modifier "adult" before the use of "human" virtually throughout this paper. The purpose of this has been to emphasize that humans with relatively less potentially intrusive history (i.e., infants and young children) seem the best subjects for trying to examine directly the generality of basic conditioning principles that have been isolated with nonhumans. They may also be the only subjects in whom we really can examine the generality of any "layering" sorts of principles that are discovered with non-humans. And they surely represent the best subjects for studying effects of imposed verbal histories on later performace.

Again, Baron, Perone, and Galizio are to be thanked for attacking a difficult and very important problem. I look forward to their responses (and to other criticisms).

REFERENCES

Barrett, J. E. (1977). Behavioral history as a determinant of the effects of *d*-amphetamine on punished behavior. *Science, 198,* 67–69.

Chomsky, N. (1975). *Reflections on language.* New York: Pantheon Books.

Estes, W. K. (1972). Reinforcement in human behavior. *American Scientist, 60,* 723–729.

Gonzalez, F. A., & Waller, M. B. (1974). Handwriting as an operant. *Journal of the Experimental Analysis of Behavior, 21,* 165–175.

Jenkins, H. M., & Harrison, R. H. (1960). Effect of discrimination training on auditory generalization. *Journal of Experimental Psychology, 59,* 246–253.

Sidman, M. (1960). *Tactics of scientific research: Evaluating experimental data in psychology.* New York: Basic Books.

Skinner, B. F. (1935). The generic nature of the concepts of stimulus and response. *The Journal of General Psychology, 12,* 40–65.

Skinner, B. F. (1938). *The behavior of organisms: An experimental analysis.* New York: Appleton-Century.

Stemmer, N. (1990). Skinner's *Verbal Behavior,* Chomsky's review, and mentalism. *Journal of the Experimental Analysis of Behavior, 54,* 307–315.

Thomas, D. (1969). The use of operant conditioning techniques to investigate perceptual processes in animals. In R. M. Gilbert & N. S. Sutherland (Eds.), *Animal discrimination learning* (pp. 1–33). New York: Academic Press.

Wanchisen, B. A., Tatham, T. A., & Mooney, S. E. (1989). Variable-ratio conditioning history produces high- and low-rate fixed-interval performance in rats. *Journal of the Experimental Analysis of Behavior, 52,* 167–179.

Weiner, H. (1969). Controlling human fixed-interval performance. *Journal of the Experimental Analysis of Behavior, 12,* 349–373.