The Behavior Analyst 1991, 14, 139-144 No. 2 (Fall)

Behavioral History: A Promising Challenge in Explaining and Controlling Human Operant Behavior

Barbara A. Wanchisen
Baldwin-Wallace College

Thomas A. Tatham
Uniformed Services University of the Health Sciences

Behavior analysis is fractionated over attempts to explain discrepancies observed in the behavioral patterns of humans and those of other animals on schedules of reinforcement. There is a continuum ranging between two extreme positions, that is, eschewing versus embracing the traditional animal paradigm (the use of operant chambers in a laboratory setting) in terms of applying it to human subjects as Baron, Perone, and Galizio have aptly characterized. The major concern for researchers who either reject or seriously question the utility of using animal methodology seems to revolve around the existence of uncontrollable history effects attendant with human beings. We will address how history might be more clearly defined and argue that minimizing history effects may be exceedingly difficult given the present state of behavioral history research.

A clear distinction needs to be made between extra-experimental and experimental history effects (Wanchisen, 1990) so that the relative importance of each may be assessed. Baron et al. present Reynolds' (1961) experiment and suggest that it is perplexing that one pigeon in a discrimination task would attend to the color of the triangle and the other to the shape, and suggest that it must have been something in their "pre-experimental histories" that caused this difference. As these birds were experimentally naive, they probably were exposed to something different in their living environments (thus they were affected by extra-experimental influences). However, there are numerous cases where researchers use the same pigeons (or monkeys) in a series of experiments under the assumption that there are no residual effects, thus providing experimental histories which may or may not change present behavior. These two concerns, extra-experimental versus experimental history, can be important concerns for researchers studying either human or non-human species and further research could illuminate the effects each may have on subjects in operant experiments.

Baron et al. point out that subjects, human and non-human, spend only a small amount of time in an experiment and that their environments (extra-experimental conditions) can exert a powerful influence on their behavior in an experiment. They suggest that this extra-experimental history is equi-potent for humans and other animals: "Thus, while concern about extra-experimental factors led Shull et al. (1989) to oppose the use of traditional methods with humans, they did not oppose using the methods with animals. We do not see justification for such asymmetry."

Traditional techniques should be used on all species, but it must be recognized that there *are* differences in degrees of the extra-experimental histories of laboratory animals and humans. Specifically, laboratory animals have minimal extra-experimental influences compared to humans. First, genetic lineage is strictly controlled with laboratory animals. Second, their living environments, diet, and pharmacological histories are nearly identical (especially when caged individually). Third, laboratory animals typically have no previous experience with the reinforcer to be provided by the ex-

Address correspondence to the first author at the Department of Psychology, Baldwin-Wallace College, Berea, OH 44017.

periment (especially where shock or special pellets are concerned) and the reinforcer is virtually always a primary one. Fourth, the animals usually are taught arbitrary operants (typically lever- or treadle-pressing).

These conditions cannot be equated with the sample of humans who enter our laboratories with very different genetic make-ups and varied living conditions. Probably even more importantly, humans arrive with a rich and complex exposure to contingencies of reinforcement outside of the laboratory and have typically been exposed to the operant under study (button-pressing) and have definitely been exposed to the reinforcer (money), which is virtually always a secondary one. While normal humans also have a history of rule-following which may result in insensitivity to contingencies (Bernstein, 1988), it is equally plausible, and favored here, that exposure to various contingencies is a key determinant of such insensitivity (see Hineline & Wanchisen, 1989).

Baron et al. argue that some history effects may be controlled by experimental manipulations. They suggest a variety of strategies for reducing and accounting for inter-subject variability when humans are used as subjects. Several of those suggestions will be addressed here: 1) Providing suitable behavioral histories, in the laboratory, may reduce inter-subject variability by generating comparable baselines. 2) Long-term exposure to experimental contingencies may reduce the influence of historical variables. 3) Obtaining detailed demographic measures should help to explain inter-subject differences and suggest what factors need to be controlled in subsequent studies through either subject selection procedures or via other means, such as providing the appropriate experimental histories.

Their suggestions are useful and should lead to improvements in the quality of human experimentation. Particularly important is the emphasis upon explaining anomalous behavior in terms of environmental factors, for this emphasizes species continuity and de-emphasizes explanations couched in terms of uniquely human processes, such as verbal behavior. While an emphasis on historical factors in human research, coupled with long-term exposure to contingencies, should become more common and would be welcome improvements to human experimental methodology, such an approach is not easy and is fraught with complexities to be addressed here.

The history effects with which the authors deal most extensively are those of Thomas (1969) and Weiner (1964, 1969). In the former case, it was demonstrated that extensive exposure to a pure tone was crucial to the development of orderly auditory decremental gradients to that tone when it was subsequently used as a training stimulus. Perhaps the best known of behavioral history research within behavior analysis, that of Weiner, demonstrated that the rate of human fixed-interval responding could be experimentally controlled by exposing subjects to high- or low-rate schedules prior to exposure to the fixed-interval schedule.

In both cases it is interesting to note that there is a fairly obvious relationship between the history manipulation and the subsequent behavior. Weiner selected schedules in which there was a very tight correlation between response rate and reinforcement rate. Behavioral patterns persisted when the contingencies were shifted to a schedule (fixed-interval) which tolerates wide variations in response rate with minimal impact on reinforcement rate. Stated differently, response rate during the "test" condition was functionally related to response rate during the "history" phase; the experimental manipulation manifested changes during both phases in the same dependent measure and in the same response modality. One may think of such effects as creating insensitivity to the changed contingencies. Similar experiments with rats have yielded either similar results (Alleman & Zeiler, 1974; Bickel, Higgins, Kirby, & Johnson, 1988; Urbain, Poling, Millam, & Thompson, 1978) or ones in which there was a more complex relationship (Wanchisen, Tatham, & Mooney, 1989).

Experiments in which a "carryover" effect from one condition to another is exploited are undoubtedly relevant to understanding and conceivably controlling inter-subject differences in human operant experimentation and should be considered when designing, conducting, and analyzing such research, as suggested by the authors. A concern, though, is whether most history effects are as easy to conceptualize and identify as those discussed above. It may well be the case that history effects are operative in many human experiments but that identification and analysis of these effects will not be at all straightforward. This may be especially likely when "masked" history effects are involved. As used here, masked history effects are those which are revealed only as the result of additional manipulations. In the most basic masking paradigm, two groups of subjects receive different contingencies in their first experimental condition, then all subjects receive the same contingencies in the second condition, continuing to respond until all emit indistinguishable patterns. This seemingly eliminates the effects of the initial conditions. However, when a third manipulation is then introduced, the two groups behave radically differently, which is attributable to the differing initial conditions, and hence these effects are unmasked. Egli and Thompson (1989) refer to the effects of the initial condition as lying dormant or "latent."

Perhaps the most thoroughly explored history phenomenon has not been derived from schedule history research, but rather arose serendipitously from a perplexing issue in behavioral pharmacology; it illustrates the complexity and counterintuitive nature of some history effects. Specifically, responding suppressed by punishment is not typically increased by *d*-amphetamine (Geller & Seifter, 1960). This finding was a major exception to the rate-dependent effects generally observed with *d*-amphetamine (e.g., Dews & Wenger, 1977) in which low rates of responding are increased while higher rates are decreased. This anomaly led to a series of studies with squirrel monkeys in which it was shown that *d*-amphetamine could dramatically increase shock-suppressed, food-reinforced fixed-interval responding, provided that the subjects were exposed to avoidance contingencies. This effect was first shown in a "context" design in which food-reinforced fixed-interval responding in one component of a two-ply multiple schedule was suppressed by a superimposed fixed-ratio schedule of shock delivery; extinction was in effect in the alternate component (McKearney & Barrett, 1975). As expected, *d*-amphetamine did not increase responding in the punished component. Subsequently, the extinction component was changed to a shock-postponement schedule and the effects of *d*-amphetamine were redetermined. Importantly, *d*-amphetamine now produced dramatic response rate increases, demonstrating the importance of the larger behavioral context in determining drug effects.

This was followed by a conceptually important study in which shock-punished fixed-interval responding was established (but not as a ply of a multiple schedule) and the effects of *d*-amphetamine were determined (Barrett, 1977). The punishment schedule was then discontinued and replaced by a shock-postponement procedure. After several weeks the shock-postponement contingency was removed and the punishment procedure was reintroduced. After approximately 3 weeks of daily exposure to the punishment procedure, response rates stabilized at rates comparable to those displayed prior to exposure to the shock-postponement procedure. The effects of *d*-amphetamine were then redetermined and shown to produce large increases in responding. This study illustrates an important characteristic of history effects—they appear sometimes to be masked and are only revealed under special circumstances. Examination of cumulative records and response rates on the punishment schedule following exposure to the avoidance contingency might reasonably have led to the conclusion that exposure to avoidance produces temporary effects on response rate, but that extended exposure to the punishment schedule overcomes the effects of the avoidance history. It was only after an additional condition, in which *d*-amphetamine was administered, that persistent effects of the avoidance history were revealed. This study shows that although the primary dependent measure under baseline conditions may indicate no evidence of residual effects of an intervening "history" effect, special circumstances may reveal (unmask) an ongoing effect of a previous manipulation.

Confronted with anomalous human behavior it may be exceedingly difficult and time-consuming to ascertain what sort of history might account for the anomalies and to then experimentally verify the hypothesis. The authors' suggestion for extended exposure to experimental conditions is a good one and may in many instances reduce inter-subject variability. It is not clear, though, the extent to which this benefit may be attributable to attenuation of history effects versus other, unspecified, processes. Whether history effects eventually diminish as a correlate of extended exposure to the conditions under which the history effect is revealed is essentially an open question within both the human and non-human behavioral history literature. Weiner (1969), unfortunately, typically exposed his subjects to a few sessions under any particular condition prior to either terminating the subject's participation or changing conditions. Several studies with rats showed differences in response rates on FI schedules as a function of exposure to inter-response-time > t- versus FR schedules that persisted throughout the duration of the experiment (Bickel et al., 1988; Urbain et al., 1978). Even when a condition is extended for a very long time (e.g., Wanchisen et al., 1989), the possibility remains that despite apparent comparability of response rates and behavioral microstructure among subjects with diverse behavioral histories, an as-yet-experimentally-unexplored possibility remains that appropriate unmasking manipulations might reveal latent history effects. For example, it may be possible to see differential effects of *d*-amphetamine on response rate among rats trained with high- versus low-rate engendering schedules, despite a lack of ongoing differences in response rate under non-drug conditions (as the trends with the data of Bickel et al., 1988; Urbain et al., 1978; and Wanchisen et al., 1989 show occurring with extended FI exposure). It simply is not known whether (or how much) extended exposure to an experimental condition is adequate to override history effects.

Given the potential difficulties in minimizing historical factors by extended exposure, an alternative strategy might be to attempt to "actively" eliminate the history effect. There are data that weakly suggest that the influence of an avoidance history on the effects of *d*-amphetamine on a punished responding paradigm may be eliminated by repeatedly "unmasking" the history effect (Bacotti & McKearney, 1979). Employing a protocol similar to that of Barrett (1977), the investigators repeatedly injected each monkey's peak rate-increasing dose of *d*-amphetamine prior to daily punishment sessions. *d*-Amphetamine dependent rate increases were rapidly eliminated in one monkey, declined to control levels gradually in another monkey and were largely unaffected in a third. These data suggest that historical influences on behavior may be overcome by extended exposure to unmasking manipulations, but counter-interpretations may be offered. It was not ruled out, for example, that the diminution of the effects of *d*-amphetamine with repeated testing were due merely to the passage of time or to extended exposure to the punishment schedule (as distinct from the effects of repeated schedule and *d*-amphetamine exposure). Coupled with the large inter-subject variability, there is, at present, little evidence that manipulations designed to counter the effects of avoidance histories are effective.

The identification and analysis of history effects are further complicated by findings indicating that histories "transfer" across topographically distinct responses. Transfer was demonstrated in a study in which morphine was initially shown to decrease the rate of shock-postponement maintained chain pulling in squirrel monkeys (Barrett & Stanley, 1980). The chain and avoidance contingency were then removed and the monkeys were exposed to a schedule of fixed-interval response-produced shock in which the manipulandum was a lever. Next the lever was removed, the chain and avoidance schedule reinserted and the effects of morphine redetermined. Following the shock-maintained lever-pressing history, morphine increased avoidance-maintained chain-pulling. In terms of human beings, if transfer effects occur, then the number of potential sources of influences from history are substantially multiplied.

We do not take exception to the authors' suggestions that the quality of human operant research could be improved by sensitivity to the possible presence of confounding behavioral histories in the repertoires of experimental subjects. The point is also well-taken that history-minimizing manipulations such as extended exposure to experimental contingencies coupled with judicious employment of experimentally-provided histories could play a role in reducing inter-subject variability and, possibly, in reducing the discrepancy between findings obtained with humans versus other species. As noted, though, this is not an easy undertaking, for relatively few history effects have been documented and very little is known about how to meliorate their effects. Experimental evidence addressing whether the most fully-studied history effects are attenuated through extended exposure to experimental conditions has not yielded clear evidence, nor has an explicit attempt to eliminate a history effect been particularly successful. To the extent that prolonged exposure to contingencies does prove effective in minimizing the effects of experimental history, it appears as though the duration of exposure to the contingencies may need to be so long as to make the research impractical. The phenomena of masked history effects and transfer across responses will further complicate attempts to identify and control history effects.

These issues point not to any fundamental shortcoming in the authors' approach to human operant research, but instead underscore the need for further direct investigation of history effects, with both humans and with other animals as experimental subjects. Despite our contention that there is presently a decided lack of effective and efficient history-minimizing manipulations, there does not seem to be any fundamental reason why such a technology cannot be developed. This will be facilitated if investigators follow the suggestion of Baron et al. and gather detailed information about the pre-experimental backgrounds of their subjects. This information may provide hints regarding profitable avenues of research whereby promising possible historical influences may be experimentally analyzed. A tighter loop among identification of potential history effects in humans, their detailed exploration in non-human animals, and translation back into history-controlling manipulations in humans may prove quite profitable, albeit time-consuming. Rather than viewing humans' rich pre-experimental histories as a nuisance, it may be possible to view human behavior as a fertile source of inspiration for studies with non-humans. With sufficient research into history effects, in both humans and other animals, the authors' position should prove fruitful.

REFERENCES

Alleman, H. D., & Zeiler, M. D. (1974). Patterning with fixed-time schedules of response-independent reinforcement. *Journal of the Experimental Analysis of Behavior, 22,* 135–141.

Bacotti, A. V., & McKearney, J. W. (1979). Prior and ongoing experience as determinants of the effects of *d*-amphetamine and chlorpromazine on punished behavior. *Journal of Pharmacology and Experimental Therapeutics, 211,* 80–85.

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

Barrett, J. E., & Stanley, J. A. (1980). Effects of ethanol on multiple fixed-internal fixed-ratio schedule performance: Dynamic interactions at different fixed-ratio values. *Journal of the Experimental Analysis of Behavior, 34,* 185–198.

Bernstein, D. J. (1988). Laboratory lore and research practices in the experimental analysis of behavior: Designing session logistics—How long, how often, how many? *The Behavior Analyst, 11,* 51–58.

Bickel, W. K., Higgins, S. T., Kirby, K., & Johnson, L. M. (1988). An inverse relationship between baseline fixed-interval response rate and the effects of a tandem response requirement. *Journal* *of the Experimental Analysis of Behavior, 50,* 211–218.

Dews, P. B., & Wenger, G. R. (1977). Rate-dependency of the behavioral effects of amphetamine. In T. Thompson & P. B. Dews (Eds.), *Advances in behavioral pharmacology: Vol. 1.* (pp. 167–227). New York: Academic Press.

Egli, M., & Thompson, T. (1989). Effects of methadone on alternative fixed-ratio fixed-interval performance: Latent influences on schedule-controlled responding. *Journal of the Experimental Analysis of Behavior, 52,* 141–153.

Geller, I., & Seifter, J. (1960). The effects of meprobamate, barbiturates, *d*-amphetamine and promazine on experimentally induced conflict in the rat. *Psychopharmacologia, 1,* 482–492.

Hineline, P. N., & Wanchisen, B. A. (1989). Correlated hypothesizing and the distinction between contingency-shaped and rule-governed behavior. In S. C. Hayes (Ed.), *Rule-governed behavior: Cognition, contingencies, and instructional control* (pp. 221–268). New York: Plenum.

McKearney, J. W., & Barrett, J. E. (1975). Punished behavior: Increases in responding after *d*-amphetamine. *Psychopharmacologia, 41,* 23–26.

Reynolds, G. S. (1961). Attention in the pigeon. *Journal of the Experimental Analysis of Behavior, 4,* 203–208.

Shull, R. L., Lawrence, P. S., Tota, M. E., Sharp, J. A., Drusdow, M. A., Torquato, R. D., & Soyars, V. A. (1989). [Review of Davey and Cullen's *Human operant conditioning and behavior modification*]. *The Behavior Analyst, 12,* 69–77.

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.

Urbain, C., Poling, A., Millam, J., & Thompson, T. (1978). *d*-Amphetamine and fixed-interval performance: Effects of operant history. *Journal of the Experimental Analysis of Behavior, 29,* 385–392.

Wanchisen, B. A. (1990). Forgetting the lessons of history. *The Behavior Analyst, 13,* 31–37.

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. (1964). Conditioning history and human fixed-interval performance. *Journal of the Experimental Analysis of Behavior, 7,* 383–385.

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