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APPLIED IMPLICATIONS OF REINFORCEMENT HISTORY EFFECTS

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Although the influence of reinforcement history is a theoretical focus of behavior analysis, the specific behavioral effects of reinforcement history have received relatively little attention in applied research and practice. We examined the potential effects of reinforcement history by reviewing nonhuman, human operant, and applied research and interpreted the findings in relation to possible applied significance. The focus is on reinforcement history effects in the context of reinforcement schedules commonly used either to strengthen behavior (e.g., interval schedules) or commonly used to decrease behavior (e.g., extinction).

DESCRIPTORS: extraexperimental history, human operant, reinforcement history, transition states, translational research

Behavioral assessments and interventions are influenced by a participant's reinforcement history. By reinforcement history, we refer to a participant's exposure to various schedules or contingencies of reinforcement that are no longer in place. The notion of reinforcement history is central to the philosophical orientation of behaviorism; however, relatively little empirical work has focused directly on its influence with human participants in socially meaningful contexts (Salzinger, 1996; Wanchisen, 1990).

Most of the research on reinforcement history has been conducted in nonhuman laboratories, using operant chambers (e.g., Alleman & Zeiler, 1974; Baron & Leinenweber, 1995; Cohen, Pedersen, Kinney, & Myers, 1994; Cole, 2001; Doughty et al., 2005; Freeman & Lattal, 1992; LeFrancois & Metzger, 1993; Ono & Iwabuchi, 1997; Wanchisen, Tatham, & Mooney, 1989). An advantage of nonhuman research is that it allows more control over the reinforcement histories experienced by the subjects. For instance, most nonhuman subjects start experiments naive to the contingencies in effect or have only limited experience with the experimental environment. Nevertheless, nonhuman laboratory studies may have limited external validity.

Controlled laboratory experiments using human participants also have involved operant chambers. In these studies, the simple responses and contingencies (such as button pressing and points) often already exist in the participants' repertoires or environments. This may be more similar to applied problems than nonhuman experiments because participants in applied research often have an extensive history with the response or with complex reinforcement contingencies that maintain the response in the natural environment. Further, the effects of reinforcement history on humans may be dramatically different than the effects on nonhumans because of verbal behavior (Branch, 1991).

Although specific methods for determining the effects of reinforcement history have varied widely, the most common approach involves evaluating the effects of prior exposure to

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reinforcement schedules (e.g., Weiner, 1962). Typically, two or more reinforcement schedules are chosen as *history* schedules. The effect of a history with these schedules is later assessed on responding during the *target* schedule. Once stable rates or patterns of responding have been attained during the history schedule, responding is assessed on the target schedule to evaluate possible influences of the reinforcement history.

Applied studies characteristically have evaluated reinforcement history effects only indirectly insofar as those effects were not the central focus of the research (e.g., Mace, Neef, Shade, & Morrow, 1994; Vollmer, Roane, Ringdahl, & Marcus, 1999). In many applied experiments, for example, participants are referred specifically because they display some undesired response for a substantial period of time (e.g., self-injurious behavior) and thus already have an established reinforcement history for that response.

The purpose of the current paper is to examine literature on reinforcement history with an eye toward application. The discussion is not intended to be exhaustive, but rather will focus on the potential implications of reinforcement history for applied behavior analysis and the current state of the literature in relation to applied research. Given that only a handful of applied studies have directly examined reinforcement history effects, the bulk of the current discussion focuses on nonhuman research and its implications. The paper is organized by the effects of various histories on reinforcement schedules commonly used as interventions. To this end, the discussion is focused on the effects of reinforcement history on schedules commonly used for acquisition and maintenance of appropriate behavior (interval and ratio schedules) and for reduction of problem behavior (time-based schedules and extinction). Within each section, discussion is further divided into laboratory research and applied research with future directions.

EFFECTS OF REINFORCEMENT HISTORY ON RESPONSE ACQUISITION AND MAINTENANCE SCHEDULES

Effects of Reinforcement History on Interval Schedules

Laboratory research. The bulk of research on reinforcement history has examined the effects of history on subsequent interval schedules. Researchers frequently chose fixed-interval (FI) schedules as the target because responses during the interval do not affect the delivery of the reinforcer, permitting widely disparate response rates to result in similar reinforcement rates. Therefore, FI schedules may maximize the potential for evaluating reinforcement history effects because they do not select against particular rates or patterns of responding.

Early human operant research using FI target schedules characterizes the procedures (Weiner, 1962, 1964, 1965, 1969). In one study (Weiner, 1969), participants were divided into groups and exposed to one of three history schedules: fixed-ratio (FR) 40, differential reinforcement of low rate (DRL) 20 s, or DRL 20 s followed by FR 40. Then, all participants were exposed to an FI target schedule. Participants with any DRL 20-s history responded at low rates during the FI schedule, including those participants in the compound DRL 20 s FR 40 group, who had a more recent FR 40 history. By contrast, the participants without a DRL history responded at high rates during the FI conditions. This result was replicated across multiple experiments using different interval durations during the FI target schedule.

Weiner's (1969) findings suggested that certain effects of reinforcement history may influence behavior even when the organism has experienced an intervening history or has substantial exposure to the current reinforcement contingencies. Weiner's research used a human operant procedure, in which humans participated in experimental situations akin to traditional (nonhuman) operant chambers.

Other early human operant experiments showed that the effects of DRL and FR histories on FI responding can be extremely robust (e.g., Weiner, 1962). For instance, the effects of an FR schedule history persisted when the target schedule required low response rates and punished high-rate responding (e.g., Weiner, 1969). Thus, current reinforcement contingencies may influence response rate only in conjunction with previous reinforcement history, even if the prior history does not immediately precede the target schedule. That is, historical influences may persist in the face of current contingencies.

Like the human operant studies discussed above, most experiments using nonhuman subjects have examined the effects of schedule history on subsequent responding using interval schedules (e.g., Baron & Leinenweber, 1995; Bickel, Higgins, Kirby, & Johnson, 1988; Doughty et al., 2005; Freeman & Lattal, 1992; Johnson, Bickel, Higgins, & Morris, 1991; Lopez & Menez, 2005; Wanchisen et al., 1989). Although the overall effects found in the nonhuman literature also suggested that reinforcement history influenced responding, the specific findings differed from Weiner's (1962, 1969) results. In particular, some nonhuman research contradicted Weiner's (1969) assertion that reinforcement history effects are durable. For example, LeFrancois and Metzger (1993) systematically replicated Weiner's (1969) procedures by exposing one group of 3 rats to a DRL history schedule and another group of 3 rats to DRL followed by FR as a history schedule. Both groups of rats were later exposed to an FI target schedule. Those rats exposed to only DRL as the history schedule predictably responded at low rates during the FI target schedule, and those rats with a history of both DRL and FR responded at high rates during the FI target schedule. In other words, responding on the FI schedule was different following DRL alone than following DRL plus FR, with the more recent FR history exerting more control.

LeFrancois and Metzger (1993) attributed these findings to differences in experimental procedures, such as the method of training, the specific schedule parameters, or the use of primary instead of conditioned reinforcers. The authors also noted that differences in extra-experimental reinforcement history could contribute to the discrepant results. For example, the human participants used in Weiner's experiments may have had extensive histories with naturally operating schedules that resembled FI, FR, and DRL schedules, but half of the rats used by LeFrancois and Metzger were naive. However, a comparison of the cumulative records of the experienced and naive rats did not show any clear differences, suggesting that the extraexperimental reinforcement history could not account entirely for the obtained results.

An alternative explanation for the discrepant results is the use of schedule-correlated stimuli, which were used by Weiner (1969) but not by LeFrancois and Metzger (1993). It is this alternative explanation that carries direct implications for application. Remote reinforcement history may exert more of an influence when a distinct stimulus is correlated with the history schedule and is later presented during the target schedule. To investigate this possibility, Ono and Iwabuchi (1997) demonstrated that pigeons responded at higher rates in the presence of a stimulus that was previously associated with the differential-reinforcement-of-high-rate (DRH) schedule than in the presence of a stimulus that was previously associated with the DRL schedule, even when exposed to 15 sessions of VI between the history and test schedules (although these differences decreased across time). In a second experiment, pigeons were exposed to the same history schedules, but were removed from the experimental situation for 6 months before the target schedule was introduced instead of being exposed to the VI schedule. When reintroduced into the experimental setting, the pigeons still responded at higher rates in the presence of the stimulus previously associated with the DRH schedule than in the presence of the stimulus previously associated with the DRL schedule. With continued exposure to the new contingencies, the differences in response rate between the two stimulus conditions gradually decreased, but never reached equality.

Applied implications and future directions. The effects of reinforcement history on FI schedules may be important in applied work for at least two reasons. First, the naturally occurring reinforcement schedules that maintain behavior may share features with interval schedules (e.g., Critchfield, Haley, Sabo, Colbert, & Macropoulis, 2003). This may be particularly the case for individuals who have set daily schedules, in which a particular reinforcer (e.g., attention from a certain adult or a particular tangible item) may not become available until a certain amount of time has passed since the beginning of the day. Second, interval schedules may be used for the acquisition and maintenance of appropriate behavior, such as academic tasks or on-task behavior in classrooms (e.g., Henderson, Jenson, & Erken, 1986).

Weiner's (1962, 1969) experiments established that, under certain conditions, even distant reinforcement history can influence responding. These results have relevance to application in three ways. First, when different effects of a similar manipulation (e.g., a behavioral intervention) are observed across participants or within the same participant across time, differences in reinforcement history should be considered. Although researchers have hypothesized that behavioral history contributes to between-subjects differences (e.g., Wanchisen, 1990), applied research has not yet identified the degree to which individual differences observed during behavioral interventions may be related to differences in reinforcement history.

Second, Weiner's (1962, 1969) work implies that researchers could use reinforcement history effects to improve behavioral performance. For example, experimenters could assess the rate at which a child will complete a task on an FI reinforcement schedule. If the rate of responding is lower than desired, a history with a DRH schedule, which is essentially an FR or variable-ratio (VR) schedule with a certain time limit to complete the response requirement, could be provided for completing the task; then the FI schedule could be reintroduced. Results of human operant experiments (e.g., Weiner, 1969) suggested that responding should occur at much higher rates in the second FI exposure than the first because of the intervening DRH history. This type of manipulation may be useful if continually monitoring response rates, a necessary feature of DRH schedules, is impossible or undesirable (e.g., in a classroom where a teacher must attend to multiple students).

Third, Weiner's (1969) findings could have implications for interventions that include a differential-reinforcement-of-alternative-behavior (DRA) component, to the extent that interval schedules are easier to implement than ratio schedules (because responding need only be monitored at the programmed reinforcement interval). Treatments using DRA schedules typically involve extinction (reinforcers are withheld following problem behavior) of maladaptive behavior and reinforcement of some alternative behavior, with the characteristic effect of increasing appropriate behavior and decreasing problem behavior. To make DRA more practical for caregiver implementation, the alternative response is sometimes reinforced on an interval schedule rather than a ratio schedule (e.g., Marcus & Vollmer, 1996). Individuals who have a history with ratio-like DRA schedules may allocate more time to appropriate responding, even if the programmed contingencies for appropriate behavior and problem behavior are changed to equal-interval schedules. This type of effect may make it possible to effectively reduce problem behavior and increase appropriate behavior, at least temporarily, even when the treatment cannot be immediately implemented with ideal integrity in the natural environment. In a sense, a schedule history might be arranged to produce a bias toward appropriate behavior.

Yet, further research is needed to clarify the conditions under which Weiner's (1962, 1969) results are replicated. The results of Ono and Iwabuchi's (1997) and LeFrancois and Metzger's (1993) studies suggested that reinforcement history effects may not always be as durable as initially indicated by Weiner (e.g., Weiner, 1969). In both of the nonhuman studies, the effects of reinforcement history were eventually diminished by continued exposure to the current contingencies, although Weiner argued that history effects may be long lasting. The temporary reinforcement history effects support Sidman's (1960) assertion that history effects are part of and perhaps even define a transition state. The durability of reinforcement history effects has important implications for the development and implementation of effective behavioral interventions. If the effects of reinforcement history are short lived, they should be of less concern. The decrease in the history schedule's influence over time suggests that reinforcement history effects could be minimized in applied settings by introducing features of the natural environment into clinical treatment sessions before implementing the treatment in the natural environment (Stokes & Baer, 1977). For example, treatment sessions conducted by a therapist could include the presence of the client's caregivers or could be conducted in the client's home. Other features of the natural environment could be gradually added to the treatment procedure before the therapist concludes treatment services.

Applied researchers and practitioners should carefully consider the use of discriminative stimuli, given that the presence of these stimuli seems to influence the effects of reinforcement history (Ono & Iwabuchi, 1997). Specifically, reinforcement history may have very durable effects when the history schedules are associated with particular stimulus conditions (e.g., schedules of reinforcement in residential or hospital-based treatment programs vs.schedules of reinforcement in the home). To make the Ono and Iwabuchi findings more directly relevant to application, further research is needed in which the participants are exposed to a treatment schedule such as DRA, differential reinforcement of other behavior (DRO; reinforcers delivered contingent on the absence of behavior), or fixed time (FT; reinforcers are delivered a fixed points in time, regardless of responding) as the target schedule. In addition, applied research should be conducted on the influence of histories that are associated with particular stimulus conditions, such as a hospital facility or the home environment.

A starting point for such research may be to associate distinct discriminative stimuli with different schedules of reinforcement (as in a multiple schedule; Tiger, Hanley, & Heal, 2006) and then to examine the effects of those histories when schedules on all components are matched. For example, researchers could associate a DRH schedule with the presence of a red stimulus and a DRL schedule with the presence of a green stimulus for individuals working on academic tasks. After responding stabilized on these history schedules, a VI schedule could be introduced in the presence of both the stimuli. This type of procedure would be a systematic replication of the Ono and Iwabuchi (1997) study but with a socially relevant response. If this study replicated the Ono and Iwabuchi findings, research could begin to examine the effects of histories and target schedules that are more commonly used in applied work (e.g., ratio schedules).

Effects of Reinforcement History on Ratio Schedules

Laboratory research. Relatively few studies have examined the effects of reinforcement history on ratio target schedules. This paucity of research could be due to the characteristically high rates of responding produced by ratio schedules, making them inappropriate to demonstrate rate-increasing history effects. In addition, ratio schedules are likely to quickly override the effects of history schedules that typically produce low response rates because ratio schedules, unlike interval schedules, select against low response rates (e.g., Cohen et al., 1994). This suggests that reinforcement history effects, particularly those engendering low-rate responding, may be substantially less pronounced during ratio target schedules than during interval target schedules.

Research by Cohen et al. (1994) provides an example of the rapid dissipation of reinforcement history effects during ratio target schedules. Different groups of pigeons were exposed to FR, VR, or DRL histories and a progressive-ratio (PR; the number of responses required for reinforcement gradually increases) target schedule. An additional group of pigeons was exposed to the PR schedule from the beginning of the experiment to serve as a control for changes in responding that may simply be due to continued participation in the experiment. Response rates during the PR target schedule were highest initially for the group with the VR history. However, these effects were temporary and were evident only through the first several PR sessions, suggesting that the observation of reinforcement history effects may be less likely during ratio-based interventions.

Applied research, implications, and future directions. An applied implication of the rapid dissipation of reinforcement history effects is that consideration of history may be less pertinent when using ratio schedules in interventions than when using other schedule types. Unfortunately, this claim is highly speculative. Although many applied studies indirectly assess the effects of reinforcement history on responding during ratio schedules, no applied evaluations directly targeting the effects of history on FR targets have been conducted. However, some commonly used applied procedures are relevant to this discussion.

The high-probability procedure (e.g., Mace et al., 1988) provides an example of applied practices related to reinforcement history. This procedure involves repeated presentation of requests that are likely to result in (i.e., have a high probability of) compliance. Interspersed among these high-probability requests are a few requests that are unlikely to result in compliance, if presented alone (i.e., low probability). Typically, both the high-probability and low-probability requests are reinforced on some type of ratio schedule (e.g., FR 1). Therefore, although an unusual variation of the typical reinforcement history procedure, high-probability procedures could be conceptualized as a ratio history schedule and a ratio target schedule. The results of several studies have shown that using high-probability procedures can substantially increase compliance with otherwise low-probability requests (e.g., Ardoin, Martens, & Wolfe, 1999; Horner, Day, Sprague, O'Brien, & Heathfield, 1991; Mace & Belfiore, 1990; Mace et al., 1988).

Like other areas of reinforcement history research, the studies targeting high-probability procedures have revealed only short-lived effects of history. Mace et al. (1988) exposed a participant to two different interprompt times (IPT) to evaluate the importance of temporal contiguity between the high-probability request and the low-probability request. The experimenters compared a 5-s IPT to a 20-s IPT using multielement and reversal designs. The percentage of compliance to low-probability requests was substantially greater when the IPT was 5 s than when the IPT was 20 s. The results of this study demonstrated a limitation of the high-probability procedures: The duration of the reinforcement history effect demonstrated by increased compliance was extremely short lived, which may make the use of high-probability procedures impractical in some circumstances. Additional research is needed to test the parameters of and methods for increasing the IPT. A potentially interesting avenue for this research would be parametric evaluations of the relation between compliance and IPT in high-probability procedures. For example, it may be possible to increase the IPT with which the high-probability procedure is effective by changing either the history or target schedules in the procedure.

Additional research on the effects of reinforcement history on ratio targets in a variety of situations is warranted, particularly given the relative prevalence of ratio schedules in applied work (e.g., DRA on a ratio schedule). This type of research may be particularly beneficial in the examination of treatment integrity failures. As suggested by Vollmer et al. (1999), recent reinforcement history may influence the outcome of phases involving integrity failures, such that treatments withstand the detrimental effects of integrity failures better when the failure phases follow perfect treatment implementation than when they follow a baseline or degraded treatment phase. For example, integrity failures could be systematically introduced following both baseline and full implementation (e.g., 100% correct) treatment phases, using either reversal or multielement designs. Differences in responding during otherwise identical integrity failure phases would suggest influence of recent reinforcement history.

EFFECTS OF REINFORCEMENT HISTORY ON RESPONSE-REDUCTION SCHEDULES

Effects of Reinforcement History on Time-Based Schedules

Laboratory research. A few studies of reinforcement history have used time-based target schedules, in which reinforcement is delivered at certain points in time independent of behavior (Alleman & Zeiler, 1974; Doughty & Lattal, 2003; Lachter, 1971). These schedules may be of particular interest to applied behavior analysts because of their common use as a treatment component, typically referred to as noncontingent reinforcement (NCR). NCR procedures are used across a range of populations and target responses and have become popular, in part, due to ease of implementation (e.g., Vollmer, Iwata, Zarcone, Smith, & Mazaleski, 1993).

To evaluate the effect of DRL and FR history schedules on response patterning during FT schedules, Alleman and Zeiler (1974) first exposed 5 pigeons to DRL history schedules followed by FT target schedules. The pigeons then were exposed to FR history schedules, and responding on the FT target schedule was reassessed. Pigeons maintained low response rates during FT schedules when they had an immediate DRL history, but engaged in high rates of pecking during FT schedules when they had an immediate FR history. In addition, 2 pigeons were exposed to FR then DRL history schedules; these 2 pigeons still responded at high rates during the subsequent FT target schedules.

The results obtained by Alleman and Zeiler (1974) are important for several reasons. First, they contradict human operant findings using FR and DRL history schedules and FI target schedules. Recall that in studies by Weiner (1964, 1969), participants who were exposed to both FR and DRL history schedules responded at low rates (seemingly in line with the DRL history) during subsequent target schedule phases, even during those phases that directly followed an FR history. By contrast, Alleman and Zeiler showed that the FR history exerted more durable effects on responding than did the DRL history. That is, the pigeons responded at high rates during FT target schedule presentations that followed both history schedules. These discrepancies could have been due to species differences or extraexperimental history. A third possibility accounting for the discrepant results is the use of FT (by Alleman & Zeiler) instead of FI (by Weiner) target schedules. This possibility could be examined in future research comparing the effects of similar history schedules across multiple target schedules.

Histories with delay to reinforcement can also affect responding on FT target schedules. Doughty and Lattal (2003) assessed the effects of reinforcement history on variable-time (VT) target schedules, using histories with immediate versus delayed reinforcement. VT schedules were eventually effective at reducing response rates following histories of both immediate and delayed reinforcement, but more total responses were obtained during the VT target associated with the history of delayed reinforcement. These results suggested that the use of FT (or VT) schedules may be contraindicated (or at least less effective) for individuals who have a history with delayed reinforcement.

There may be historical conditions that would lead to ineffective or less effective NCR treatments. Lachter (1971) demonstrated one of these conditions by examining the effects of VR and VI histories on VT target schedules with pigeons. Response rates decreased after continued exposure to the VT schedule, but the rapidity of these decreases differed based on the parameters of the history schedule. The VI histories with interreinforcer intervals (IRI) that matched the IRI of the VT produced slower declines in response rates than when the IRIs between the history and target schedules were considerably different. Persistence of behavior may also occur when high-rate behavior operating under an FT schedule results in contiguous couplings of behavior and reinforcer delivery (for a potential example, see Vollmer, Ringdahl, Roane, & Marcus, 1997).

Applied research, implications, and future directions. Laboratory research suggests that there may be some conditions under which FT schedules are ineffective at reducing behavior. Specifically, behavior may persist during FT schedules if the response was previously reinforced on an intermittent ratio-like schedule or if the IRIs between the history and target schedules are equated (e.g., Lachter, 1971). This information may allow clinicians to better predict the efficacy of an FT schedule before its actual implementation. That is, if the problem behavior was known to be reinforced on intermittent ratio schedules, using an FT schedule as treatment may be contraindicated. Alternatively, clinicians could use information about reinforcement history effects on FT target schedules to specifically arrange baseline schedules that would facilitate treatment effects prior to implementing the FT schedule, such as using (i.e., intentionally imposing) DRL in place of intermittent FR schedules. Similarly, the use of FR 1 baselines (rather than intermittent FR schedules) may provide a sufficiently distinct baseline to treatment contrast. This assertion is supported by numerous applied studies in which baseline was FR 1 and treatment was a time-based schedule, yielding immediate decreases in response rates (e.g., Carr & Britton, 1999; Hagopian, Fisher, & Legacy, 1994; Marcus & Vollmer, 1996; Vollmer et al., 1993; Vollmer, Marcus, & Ringdahl, 1995).

Some applied studies evaluating the effects of FT schedules following ratio schedules have observed response maintenance when FT was implemented following VR (Carr, Bailey, Ecott, Lucker, & Weil, 1998; Dozier et al., 2001; Ringdahl, Vollmer, Borrero, & Connell, 2001), replicating nonhuman research findings (e.g., Alleman & Zeiler, 1974; Lachter, 1971). Rates of responding seem especially likely to persist when IRI (Dozier et al.; Ringdahl et al.) or the reinforcer magnitude (Carr et al.) was similar during exposure to FT and VR schedules.

Ringdahl et al. (2001) compared the response-decreasing effects of FT schedules that had similar or dissimilar IRIs to baseline with 3 children with developmental disabilities. Baseline schedules consisted of FR 1 for 2 participants and FI 30 s for the final participant. To establish an FT schedule with an IRI similar to the FR baseline schedule, the experimenters identified the mean IRI during the last five sessions of the immediately preceding FR schedule and used that value for the FT schedule. For the participant with an FI 30-s schedule as the baseline, the similar FT schedule was an FT 30-s schedule. Dissimilar schedules were determined by multiplying or dividing the similar FT schedule by 6. For all participants, the similar FT schedule was less effective at decreasing responding than was the dissimilar FT schedule, suggesting that the IRI between baseline and treatment may play a role in treatment outcome. For one participant, the similar FT schedule actually increased response rates above the previous baseline. These results suggest that practitioners should ensure that the IRI in NCR differs from the client's recent history of reinforcement rate when using NCR treatments.

Dozier et al. (2001) also manipulated the IRI between VR and subsequent FT schedules with 2 developmentally disabled children. As in Ringdahl et al. (2001), the timing of reinforcer deliveries during the FT was based on the mean IRI from the VR baseline. For both participants, the rate of responding during the yoked FT phase was approximately equal to or slightly greater than the rate of responding during the VR phase, suggesting that reinforcement history may play a role in the maintenance of responding during FT schedules in the context of application.

In contrast to Ringdahl et al. (2001) and Dozier et al. (2001), results of research implementing FT schedules following FR 1 schedules generally have not shown an elevation in response rates (e.g., Carr & Britton, 1999; Hagopian et al., 1994; Marcus & Vollmer, 1996; Vollmer et al., 1993, 1995). This effect could be due to dissimilarity between the rate of reinforcement during the ratio schedule and the rate of reinforcement during the subsequent FT schedule. These rates of reinforcement have not been reported in most studies, so the similarity in reinforcer rate during prior research cannot be assessed reliably. However, the rate of reinforcement in these studies can be estimated by comparing the rate of responding during FR 1 phases (which should equal the rate of reinforcement) to the programmed FT value used during treatment. A brief review of the current literature in which transitions were made from an FR 1 to an FT schedule shows that the IRI or rate of reinforcement between the FR 1 and FT schedules was dramatically different (typically shorter IRIs were used during the FT), especially during the initial FT implementation (e.g., Hagopian et al.; Marcus & Vollmer; Vollmer et al., 1993, 1995). Interestingly, however, Kahng, Iwata, DeLeon, and Wallace (2000) reported substantial decreases in responding despite equating reinforcer intervals between an FR 1 baseline and subsequent FT treatment for 3 participants. In this study, decreases in responding were approximately equal between the FT with equated IRI and an FT 10-s schedule.

It remains possible that the differences in results obtained by Ringdahl et al. (2001) and Dozier et al. (2001) in comparison to other studies of FT responding, including Kahng et al. (2000), have to do with differences in experimental procedures. In particular, both Ringdahl et al. and Dozier et al. examined the effects of similar baseline and FT schedules with relatively arbitrary responses (microswitch pressing and item sorting in the two studies, respectively). By contrast, Kahng et al. examined the effects of FT schedules on problem behavior, using reinforcers identified in a functional analysis. The extraexperimental reinforcement history associated with the problem behavior may have altered the treatment outcomes in Kahng et al. relative to the Ringdahl et al. and Dozier et al. studies. In particular, it is likely that the problem behavior examined by Kahng et al. was maintained in the natural environment on a reinforcement schedule unlike the FR 1 used as a baseline in the experiment. Thus, the FT schedule used, although similar in IRI to the experimental baseline, may have been dissimilar to the participants' extraexperimental histories of reinforcement for problem behavior. By contrast, the responses used by Ringdahl et al. and Dozier et al. were relatively novel and probably did not have an extensive extraexperimental reinforcement history. This may have resulted in an increased likelihood of response maintenance when similar IRIs were implemented in the baseline and FT schedules.

The possibility of extraexperimental histories increasing the efficacy of FT schedules with similar IRI to baseline schedules could be examined in future applied research. For example, researchers could reinforce one novel response on an intermittent schedule, similar to one that might maintain problem behavior in the natural environment, while a second novel response was on extinction. Following this history, a baseline schedule would be introduced for both responses. The baseline schedule could be similar to those used in prior studies, such as an FR or FI schedule, but would be distinct from the initial intermittent schedule. Finally, an FT schedule with an IRI similar to the baseline schedule would be implemented. The influence of extraexperimental reinforcement history would be suggested if the FT schedule was more effective for the response with the initial history of intermittent reinforcement than for the response with the initial extinction history.

In addition to IRI, similar reinforcer magnitudes from a VR baseline to an FT treatment result in maintained responding (Carr et al., 1998). Carr et al. used arbitrary, novel responses and the mean IRI during baseline to determine the FT schedule values, similar to the procedures used by Dozier et al. (2001) and Ringdahl et al. (2001). However, the experimenters examined the effects of these FT schedules with three different reinforcer magnitudes: low, medium, and high, in which the medium magnitude (e.g., one cookie) was three times greater than the low, and the high magnitude was two times greater than the medium. Responding persisted when the reinforcer magnitude during FT was similar to baseline for 4 of the 5 participants. Reinforcement magnitudes that differed from the baseline magnitude were effective at suppressing response rates for all participants. It is possible that, because of the participant's history with contingent reinforcement, a particular reinforcer magnitude became discriminative for responding even when the reinforcement schedule is changed. The results of Carr et al.'s experiment have implications for application because similar reinforcers (in terms of form and magnitude) are often used in clinical baseline and treatment conditions. If an FT treatment is ineffective at reducing response rates, changing the density or magnitude of reinforcement may increase its efficacy.

Effects of Reinforcement History on Extinction Schedules

Laboratory research. Ferster and Skinner (1957) published the first and most comprehensive program of research showing reinforcement history effects during extinction. They described responding during extinction (target schedule) following more than 15 different history schedules of reinforcement. Different courses of responding during extinction followed each type of schedule history, although all eventually showed the characteristic decreases in overall response rate.

Parameters influencing the course of extinction include the magnitude or delay of reinforcement prior to extinction (Lerman & Iwata, 1996), the schedule of reinforcement before extinction (Ferster & Skinner, 1957; Lerman & Iwata; Lerman, Iwata, Shore, & Kahng, 1996; Okouchi, 2003; Spradlin, 1996), the use of instructions (Martens, Bradley, & Eckert, 1997), and the length of the response-dependent reinforcement period before extinction (Ferster & Skinner; Lerman & Iwata). The manipulation of these variables prior to implementing extinction can alter the probability of response persistence, extinction bursts, spontaneous recovery, and behavioral contrast. Although these lines of research examine possible historical influences on extinction, we focus here on those areas that highlight how histories with different reinforcement schedules may lead to differential outcomes under extinction target schedules. For this reason, we focus discussion on partial reinforcement extinction effects (PREE), resurgence, and behavioral momentum.

The evaluation of response persistence during extinction is exemplified by research on PREE. Specifically, some researchers have argued that resistance to extinction is more likely to be reduced when extinction follows a continuous reinforcement schedule than when it follows an intermittent (partial) reinforcement schedule (e.g., Grosslight & Child, 1947; Jenkins & Rigby, 1950; Pavlik & Carlton, 1965; Rescorla, 1999). The effect is typically measured by the occurrence of more responses during extinction (e.g., Mowrer & Jones, 1945). In application, the effect of intermittent history schedules on responding during extinction may be of particular importance, in that most naturally occurring reinforcement schedules presumably operate on some intermittent basis.

Most of the early literature on PREE used between-subjects designs to demonstrate effects (e.g., Grosslight & Child, 1947; Jenkins & Rigby, 1950; Mowrer & Jones, 1945). These studies involved a wide variety of responses and reinforcement schedules and generally found that subjects exposed to a partial reinforcement history engaged in more responses during extinction than did those with a history of continuous reinforcement. A limitation of the above studies was that all comparisons were made between subjects using aggregated group data. Therefore, it is unclear whether the group data are representative of an individual's performance. The results of more recent within-subject studies on PREE have been mixed. Many studies examining within-subject PREE have actually found a reversed PREE; that is, intermittently reinforced behavior is less resistant to extinction than behavior that is consistently reinforced (e.g., Adams, Nemeth, & Pavlik, 1982; Nevin, Mandell, & Atak, 1983; Papini, Thomas, & McVicar, 2002; Pavlik & Carlton, 1965).

Studies have found evidence of both PREE and reversed PREE with different populations (e.g., Flora & Pavlik, 1990; Pavlik & Flora, 1993; Svartdal, 2000). For example, Svartdal compared varying levels of intermittent reinforcement both within subjects and between groups, using human participants. The results of the within-subject analysis showed a reversed PREE for all three groups. However, the between-groups analysis showed PREE; that is, the accuracy of the group who previously received reinforcement for 100% of correct responses diminished more quickly than did accuracy of the other two groups. Svartdal argued that the observation of reversed within-subject PREE depended not only on the absolute level of reinforcement during the history schedules but also on the relative levels between the two schedules that were presented. In other words, PREE and reversed PREE may be a function of the context in which the organism experiences extinction. This interpretation fits nicely with the results obtained by Pavlik and Carlton (1965), who also suggested that the context in which the history schedules are presented may affect PREE.

Although Svartdal's (2000) study provides interesting information about the within-subject and between-groups differences in PREE, several limitations should be noted. First, the author presented only aggregated data. The within-subject analyses conducted in this study seemed to be more within-group analyses using aggregates of the individual subjects' data for each group. For example, Svartdal compared the average rate of responding during extinction following the 100% schedule to the average rate of responding following the 60% schedule. It is possible that this type of aggregation skewed the results. Second, the use of an accuracy measure and a discrete-trial format makes this study different from most in the PREE literature. As previously mentioned, it is unclear what effect the trial format and choice of dependent variable had on the results. Finally, the component schedules could have interacted during the experiment, similar to a carryover effect. If this is the case, the results obtained in this study could be idiosyncratic to the combinations of schedules examined.

In addition to the relatively substantial body of research on PREE, studies have examined the effect of reinforcement history on resurgence. Although various techniques have been used to study resurgence (e.g., Epstein, 1983, 1985; Franks & Lattal, 1976; Leitenberg, Rawson, & Bath, 1970; Leitenberg, Rawson, & Mulick, 1969; Lieving & Lattal, 2003; Lindblom & Jenkins, 1981; Wilson & Hayes, 1996), most experimenters use a three-step procedure. First, responding is established and maintained by a response-dependent reinforcement schedule. Second, that response is eliminated usually while a second operant is reinforced. Third, a response-independent (e.g., FT) or extinction schedule is introduced for all responses. This typically results in reemergence of the response that was initially extinguished; this is also known as resurgence. The degree of resurgence seems to be a function of the recent reinforcement history of the organism (e.g., VI 120 vs. VI 30; Lieving & Lattal). Because response-independent schedules and extinction are commonly used as behavioral treatments, the notion of resurgence has important implications for application. For example, resurgence could be a problem at times when appropriate behavior cannot be reinforced following the implementation of a DRA procedure.

Research on behavioral momentum can also address the effect of various historical reinforcement schedules on responding during extinction. Research on behavioral momentum typically involves reinforcing responses on a multiple schedule with a rich component and a lean component, then disrupting performance through presession feeding, noncontingent food delivery during the session, or exposure to extinction (Nevin et al., 1983). The outcomes of this research in relation to extinction as a disrupter have shown that responding in the component associated with leaner schedule typically undergoes more rapid suppression during extinction than does responding in the component associated with the richer schedule (e.g., Grace, McLean, & Nevin, 2003; Nevin, 1988; Nevin & Grace, 2005; Nevin et al., 1983).

The findings from behavioral momentum seem at odds with those from the traditional PREE, which suggest that responses with an intermittent history are more resistant than responses with a continuous history when extinction is implemented. As a means of addressing these discrepancies, Nevin (1988) reanalyzed data from several studies on PREE and found that the PREE studies typically confounded the initial response rate during the history schedules with the slope of the extinction curve. When controlling for factors such as the change in rate of responding during extinction, the amount of preextinction exposure to the history schedules, and changes in schedule-correlated stimuli, Nevin found results from the PREE literature that were consistent with the momentum hypothesis. That is, variables other than the type of history schedule may be the primary influence on PREE. These results suggest that the method of analysis used to examine changes in responding during extinction may alter the conclusions obtained in those analyses.

Applied research, implications, and future directions. Lerman et al. (1996) evaluated extinction effects following intermittent or continuous reinforcement schedules with 3 individuals who had been diagnosed with mental retardation. To evaluate the effect of continuous and intermittent histories on responding during extinction, the experimenters exposed 2 participants to continuous reinforcement followed by extinction, then intermittent reinforcement followed by extinction. The 3rd participant was exposed to continuous and intermittent reinforcement in a multielement format, followed by extinction during all sessions, similar to the reinforcement history research using multiple schedules described earlier (e.g., Doughty & Lattal, 2003; Pavlik & Carlton, 1965; Rescorla, 1999). Each type of reinforcement schedule was associated with a different therapist to enhance discrimination among conditions.

During extinction, resistance was examined in three ways. First, the number of responses required to reach a predetermined extinction criterion were compared across extinction phases. Second, the number of experimental sessions required to reach the extinction criteria were compared across phases. Finally, the proportion of responding during extinction to responding during baseline was compared across phases. Results showed some evidence of reversed PREE, but there were discrepancies across the different types of analyses. Using the number of responses and number of sessions to criteria as the methods of analysis, 2 participants showed PREE; that is, more resistance was observed during the extinction phase following intermittent reinforcement than in the extinction phase following continuous reinforcement. The 3rd participant showed a reversed PREE (i.e., more resistance was observed following continuous reinforcement than following intermittent reinforcement). When the data were analyzed as a proportion of the baseline rate of responding, PREE was not obtained for any of the participants. Two participants showed a reversed PREE; the proportional rate of responding was greater in the extinction condition following continuous reinforcement than in the extinction condition following intermittent reinforcement. Equivocal results were obtained for the 3rd participant.

The authors attributed these differences to the more elevated rates of responding observed for all 3 participants during the intermittent reinforcement conditions than during the continuous reinforcement conditions. Because there was a greater difference between the rates during the intermittent reinforcement and extinction phases than during continuous reinforcement and extinction phases, the traditional analyses using number of sessions or responses until zero rates were obtained caused behavior to seem more persistent during intermittent reinforcement than during continuous reinforcement. Once the experimenters controlled for initial rates of responding, the difference in resistance was either eliminated or reversed.

Lerman et al. (1996) suggested that differences in resistance to extinction may be due to differences in the rate of responding before extinction is implemented and not necessarily due to other parameters of the reinforcement schedule. To test this assumption, experimenters could compare resistance to extinction following continuous reinforcement and following DRL. Using this procedure, response rate would be somewhat controlled, such that the response rate should be lower in the intermittent reinforcement condition than in the continuous reinforcement condition. The proportional rate of responding during the extinction and baseline conditions could be examined for PREE or reversed PREE. In addition, future research should compare PREE results obtained using both rate and accuracy measures to evaluate possible differences as a function of the dependent variable selected (e.g., Svartdal, 2000). Such a comparison of rate and accuracy measures may have important implications because of the use of discrete-trial methods in application (such as those used to teach children with autism), which commonly use percentage correct as the primary dependent measure (e.g., Grindle & Remington, 2005).

Resurgence is a second research area that reveals reinforcement history effects on responding during extinction. To examine the possibility of resurgence in clinical settings, Lieving, Hagopian, Long, and O'Connor (2004) reinforced several forms of problem behavior displayed by 2 participants with developmental disabilities, using the reinforcer identified through a functional analysis. During an initial FR 1 phase, both participants engaged in moderate to high rates of problem behavior. Following this phase, extinction was implemented for one form of problem behavior while other topographies continued to produce reinforcers on an FR 1 schedule. This manipulation decreased the rate of the topography on extinction while the alternative topographies either increased or continued at high rates. Finally, all response topographies were placed on extinction to test for resurgence. For both participants, rates of the response that was initially placed on extinction (in the second phase) increased above baseline levels.

The results obtained by Lieving et al. (2004) replicated and extended prior work on resurgence by showing that resurgence occurred with multiple topographies in the same response class, and that resurgence occurred with participants who engaged in problem behavior. This finding may have important implications for treatment integrity. Many behavioral interventions involve reinforcing an appropriate alternative response while placing problem behavior on extinction (e.g., DRA). If caregivers do not implement these procedures as prescribed, especially if they fail to reinforce appropriate behavior, resurgence of problem behavior may be likely to occur. This type of situation is directly analogous to the conditions used by Lieving et al.: Problem behavior was reinforced, then was placed on extinction while an alternative behavior was reinforced, and finally, during caregiver implementation of the procedures, both responses were placed on extinction. Determining the level of treatment integrity required on a DRA-type treatment would be a potentially interesting extension of the current applied research on resurgence.

In addition to PREE and resurgence, the effects of repeated exposures to extinction have been examined in several studies. Goh and Iwata (1994) showed that when extinction was reintroduced following a period of response-contingent reinforcement, the behavior was eliminated more quickly and with less bursting than during the initial exposure to extinction. Although this experiment included only 1 participant, the results suggest that reinforcement history effects associated with extinction may make it a desirable procedure when treatment integrity is a concern. For example, there may be cases in which a clinician knows that an individual will be exposed to some periods of reinforcement following the implementation of extinction. These reinforcement periods could be due to caregivers who do not view the undesired behavior as a problem; the individual's contact with untrained caregivers, such as new babysitters or teachers; or a host of other reasons. In these cases, extinction may be a preferable response-reduction procedure because of the reductions in bursting and other negative effects observed with multiple exposures.

Finally, the form of reinforcer used in the history schedule can influence responding during extinction (the target schedule). Martens, Bradley, and Eckert (1997) exposed 2 fourth-grade students to each of three different reinforcer types, including praise, praise plus redirection, and praise plus positive attention during brief (2-min) histories. Each brief reinforcement history was followed by an 8-min exposure to an extinction target schedule. Both students worked for a substantially lower proportion of time following the praise-positive-attention history than following a history with either the praise-only or praise-redirect reinforcer topographies. These results suggest that even brief histories can affect responding during subsequent extinction phases, but that conclusion is tempered by the brief exposure to the target schedule. It remains unknown whether the response persistence observed following the praise-only and praise-redirect phases would last long enough for the establishment of such a history to be useful to teachers. In addition, the reason for the differences across reinforcer topographies remains unexplored. It is possible that participants' extraexperimental histories established the functional properties of the conditioned reinforcers used in the study.

The effect of reinforcement history on responding during extinction could have important implications for teachers or for clinicians who work in settings where staffing ratios or other factors lead to relatively extended periods in which appropriate behavior does not contact reinforcement. The results of prior research suggest that the schedule of reinforcement, the history of prior extinction exposures, and features of the reinforcer (e.g., magnitude, delay, and topography) all can affect responding on subsequent extinction schedules. An interesting line of applied research may involve the development of a reinforcement history package designed to provide a history of reinforcement that would permit maintenance of responding during relatively long periods of extinction (for skill maintenance) and a counter package that could be implemented before extinction to reduce the number of responses emitted during extinction (for behavioral treatment).

CONCLUSIONS

Although most studies on reinforcement history effects have used human operant or nonhuman models, they have direct relevance to application because they use reinforcement schedules that are similar to those that maintain responding in skill acquisition and maintenance programs, such as ratio and interval schedules, and to those used as treatment for problem behavior, such as time-based schedules and extinction. Several standardized procedures have been developed to study reinforcement history effects, the most current of which involves the use of multiple schedules in a within-subject design (e.g., Ono & Iwabuchi, 1997). Future research on the effects of reinforcement history may further refine these procedures to better understand how reinforcement history affects responding in applied settings. For example, additional research could be conducted in which the organism is exposed to the target schedule before the history schedule to determine a baseline level of responding. Surprisingly, this control procedure is rarely a feature of reinforcement history studies. Perhaps control procedures are excluded due to concerns that a baseline exposure to a subsequent target schedule is itself a significant historical variable or because of questions regarding what constitutes an appropriate control condition (e.g., Thompson & Iwata, 2005).

The majority of studies on reinforcement history effects have used FI reinforcement schedules as targets. The results of research using FI target schedules suggest that parameters of the history and target schedules (e.g., the schedule type, schedule value, and duration of schedule exposure) may affect whether or not history effects are obtained. By contrast, few studies have examined the effects of reinforcement history on ratio schedules. Because of the frequency with which ratio schedules are used in applied settings, further examination of reinforcement history effects on those schedules is warranted. An initial line for that research could identify the likelihood of observing reinforcement history effects on ratio target schedules. It may be that little research exists because ratio schedules are relatively resistant to the effects of reinforcement history. To this end, experimenters should be encouraged to publish noneffects if they are obtained.

In application, reinforcement history effects may be inadvertently obscured for several reasons. First, reinforcement history effects can go unnoticed by applied researchers because of a focus on only one dimension of behavior (usually response rate) when the history is affecting another dimension, such as response patterning (Cole, 2001; Doughty & Lattal, 2003; Freeman & Lattal, 1992; Wanchisen et al., 1989). Second, observed reinforcement history effects may go unreported in the literature because of a perceived lack of experimental control. That is, reinforcement history effects may be considered a contamination of subsequent phases and may undermine the experimental design, making the results unsuitable for publication (Lattal & Neef, 1996). Finally, researchers may discard (and therefore fail to publish) potentially effective treatments because the participant's reinforcement history results in undesirable rates of responding during the initial stages of the treatment. Applied researchers should be aware of the possibility of reinforcement history effects and should attempt to understand the occurrence of these effects. In addition, if a data set seems possibly contaminated by reinforcement history, efforts could be made to identify the source and parameters of the history in an effort to gain control over as many relevant variables as possible.

For applied research, four strategies may be useful to increase our knowledge of reinforcement history effects. The first strategy involves experimental analyses of reinforcement history using applied procedures. For instance, researchers could provide clients with differential histories and then later examine the effects of those histories on target schedules, such as commonly used treatment schedules. Current human operant research using multiple schedules as histories may prove to be a useful guide for extension to applied research, but future research could explicitly arrange schedules commonly thought to maintain problem behavior as history schedules and could arrange schedules commonly used to treat problem behavior as target schedules. In addition, participants' extraexperimental histories should be reported in publications when available. Experimental analysis of these extraexperimental histories could be conducted, starting perhaps on inpatient treatment units where particular extraexperimental histories could be established to evaluate their effects on responding during experimental sessions. Preliminary research evaluating extraexperimental experiences has already begun (e.g., Kodak, Lerman, & Call, 2007; Roane, Call, & Falcomata, 2005). Applied reinforcement history research is particularly important because the results would have clear and immediate utility in the development of interventions for a variety of adaptive and problematic behaviors.

The suggestion to develop better models of reinforcement history in application leads directly to the second strategy to study history: Problems found in application could be studied in more detail using nonhuman procedures. Although researchers may not always be able to control the extraexperimental reinforcement history of human participants, a greater degree of control might be obtained over a nonhuman's extraexperimental reinforcement history (Wanchisen & Tatham, 1991). Noteworthy, however, is that even nonhumans are not immune to the influences of extraexperimental reinforcement history. Nonhuman subjects that are handled or housed differently outside the experimental chamber may experience extra-experimental histories that subsequently influence responding (Baron, Perone, & Galizio, 1991; Crabbe, Wahlstein, & Dudek, 1999). A potentially interesting line of future research would involve the manipulation of nonhuman subjects' extraexperimental histories to evaluate which variables influence later responding. For example, prior research has shown that manipulations of extraexperimental reinforcement history, such as the stimuli to which nonhumans are exposed when outside the experimental chamber, can later affect responding (e.g., Hebb, 1949; Thomas, 1969). Further investigations of this type, especially those that seek to replicate features of clients' extraexperimental histories, may advance our understanding of extraexperimental reinforcement history as a variable that influences responding.

The third strategy involves efforts to gain more information about clients' extraexperimental histories. No solid method for assessing extraexperimental reinforcement history has been developed to date, but procedures could include interviews with the client or the client's caregivers to ascertain pieces of distant reinforcement history, as well as direct observations (such as descriptive analyses; Mace & Lalli, 1991) of the client outside the sessions to identify typical ongoing extraexperimental experiences. For example, contact with the reinforcers used in the experimental sessions could be measured outside the sessions. Information about clients' extraexperimental histories may allow greater control over responding, especially through easier identification of reinforcers and greater knowledge of levels of deprivation for different reinforcers. In addition, the ability to classify clients according to features of their extraexperimental histories may prove to be useful in the identification of potential variables that influence reinforcement history effects.

The final strategy is to continue in a phase until behavior reaches steady-state responding. It is possible that many of the research findings reported in the area of behavioral history and in the assessment and treatment literature in general result from behavior that occurs in transition states. Conducting phases until steady-state responding is achieved has become a hallmark of nonhuman research in behavior analysis, but has attained lesser importance in applied research. To be sure, conducting phases until rates of behavior stabilize is often impractical and sometimes impossible because of external time constraints (e.g., the length of a school year or semester, or the time that the participant will spend in an inpatient facility). Also, conducting extended phases that involve high rates of problem behavior could be unethical. However, greater knowledge of steady-state behavior in applied settings would be useful because such behavior may show fewer influences of reinforcement history (Baron et al., 1991). One possible way to address the concerns with conducting steady-state research in application would be to develop better models of behavioral treatments using nonclinical populations as participants. Results from these models, including steady-state responding, could guide research with clinical populations, thereby reducing the need for steady-state results in later applied replications.

To date, reinforcement history studies have almost by chance investigated history and target schedules relevant to clinical application. Future research could explicitly select schedules commonly thought to maintain problem behavior as histories and commonly used treatment schedules and schedules used for response acquisition and maintenance as targets. There is currently little research on the effects of histories on schedules commonly used to reduce problem behavior, including DRO and DRA. In addition to using treatment schedules as targets, further research should select history schedules similar to those that operate in natural environments, such as those schedules that may maintain problem behavior. For example, researchers could select history schedules that included both response-dependent and response-independent reinforcer deliveries. This type of mixed history may be similar to the reinforcement schedule experienced by individuals who engage in attention-maintained problem behavior; attention may be delivered contingent on problem behavior throughout the day, but attention also can be delivered independent of problem behavior at other points. This type of complex-schedule research is needed to further extend the understanding of reinforcement history effects in application.

Additional research on reinforcement history could target other procedures commonly used in application. For example, the existing literature on reinforcement history typically uses primary reinforcers that are immediately delivered. With the exception of limited human operant and applied research, the effects of reinforcement history on responding maintained by conditioned reinforcers remains largely unknown. Knowledge of reinforcement history effects on conditioned reinforcement is important because of the wide use of conditioned reinforcers in application. Token systems are widely used in classrooms, and other conditioned reinforcers (e.g., money and certain forms of praise) are ubiquitous and suggest several potential questions regarding their use. It is unknown, however, if the same or a similar reinforcers need to be used in the reinforcement history and target schedules for history to affect behavior; if a history with conditioned reinforcement will have the same effects as a history with a primary reinforcer; and the extent to which delay of exchange interacts with reinforcement history.

With future advances in the methods for studying reinforcement history and increased knowledge of the effects of history, researchers could begin to use procedures that include history effects to study other phenomena. Already, methods used to study reinforcement history have also been used to test hypotheses about resistance to change (Doughty et al., 2005) and conditioned reinforcement in concurrent-chains schedules (Ono, Yamagishi, Aotsuka, Hojo, & Nogawa, 2005). Future research could use known history effects to further illuminate how basic processes (e.g., reinforcement, punishment, and extinction) operate in the natural environment. Clearly much remains to be learned about reinforcement history effects. The current review has focused specifically on reinforcement history, but parallel phenomena likely include punishment histories, histories with dynamic schedules of reinforcement (e.g., algorithm-based schedules, as suggested by Lattal & Neef, 1996), and stimulus control histories (including histories with instructions).

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