EXPERIMENTAL ANALYSIS OF HUMAN BEHAVIOR BULLETIN

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1995 WINNING STUDENT PAPER SUMMARIES

INVITED SYMPOSIUM: THE EXPERIMENTAL ANALYSIS OF HUMAN SOCIAL BEHAVIOR

RESEARCH IN PROGRESS

BRIEF REPORT

ANNOUNCEMENTS

THE EXPERIMENTAL ANALYSIS OF HUMAN BEHAVIOR BULLETIN

The *EAHB Bulletin* is published twice yearly, in the Spring and Fall, by the Experimental Analysis of Human Behavior Special Interest Group (EAHB SIG), a group organized under the auspices of the Association for Behavior Analysis (ABA). Articles in the *Bulletin* represent the views of the authors. They are not intended to represent the approved policies of the SIG or ABA, or the opinions of the membership of the SIG or ABA. The inside back cover has information about joining the SIG. Publication costs are paid by the dues of the SIG members and by the Parsons Research Center of the University of Kansas.

We would like to thank Donna Dutcher, Mark Johnston, and Julie McEntee for help with this issue.

Guidelines for Submissions

Please send three copies of brief reports and one copy of other materials. In addition, send one clearly labeled reproduction quality copy of each figure or table. For general information on preparing materials for publication in the *Bulletin*, we encourage authors to consult the author guidelines in the January issue of the *Journal of the Experimental Analysis of Behavior*. If possible, send text and figures of final versions on disk.

Brief Reports and *Technical Information* should be no longer than 2,000 words. They can be written in APA style (without an abstract) or in summary form. Please prepare figures and tables to fit the column or page width of the *Bulletin*. Incorporate information typically included in figure captions in the text.

Research in Progress may be up to 1,000 words long.

Laboratory Descriptions (as in Spring, 1990, 1991, and 1993 issues) may be up to 2,000 words long (including publication list).

EAHB SIG members have a standing invitation to submit *Abstracts* from posters and presentations given at conferences. Abstracts should be 200 words or less. Please include, on the same page as the abstract, the name and address of a contact person and a full citation for the presentation.

Please submit brief reports, technical information, and laboratory descriptions to Tom Critchfield (Department of Psychology, Auburn University, Auburn, AL 36849-5214); submit research in progress, abstracts, and news to Dean Williams (Parsons Research Center, P.O. Box 738, Parsons, KS 67357).

Submit brief reports and technical information by September 15 and all other materials by October 15 for the Fall 1995 issue.

SUMMARIES OF STUDENT PAPER COMPETITION WINNING PAPERS

STIMULUS EQUIVALENCE AND THE TRANSFER OF ANXIETY RESPONSES: THE EFFECT OF ALTERING STIMULUS EQUIVALENCE CLASS MEMBERSHIP

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This study investigated the effect of altering stimulus equivalence class membership on the acquisition of respondent elicitation. Recent research (Dougher, Augustson, Markham, Greenway, & Wulfert, 1994) has shown the transfer of respondent elicitation and extinction through equivalence classes. Recent criticisms of conditioning-based theories of anxiety disorders makes these findings particularly relevant. In addition to its implications for the acquisition of anxiety responses, stimulus equivalence processes may have some relevance for understanding how verbal therapies reduce anxiety reactions. In particular, it may provide an answer to the question of how is it that verbal interventions such as systematic desensitization or certain cognitive therapies reduce anxiety. Verbal interventions may work, at least in part, by altering the equivalence class membership of anxiety producing stimuli. Being told that a stimulus is not dangerous may put that stimulus in a new class: a class of nondangerous stimuli. What is unclear is whether altering the class membership of a stimulus will have this effect. This was the focus of the present study.

Four students (3 female and 1 male) at the University of New Mexico served as subjects for this study. The study consisted of five phases. The first involved shock level selection and subject selection based on skin conductance responsivity. Phase 2 consisted of training and testing three 5-member stimulus equivalence classes through a conditional discrimination task. The third phase involved classical conditioning, and a test for transfer of conditioning to the other members of the equivalence class. One stimulus from one equivalence class served as a CS+ and one stimulus from another class served as a CS-. Brief mild electric shock applied to the right forearm served as the US. Following the conditioning trials, the other members of the equivalence classes were presented in isolation as probe trials in a test for the transfer of the elicitation function. Conditioned elicitation and transfer were measured by changes in subjects' skin conductance level at the offset of stimulus presentations. Phase 4 consisted of retraining the stimulus equivalence classes so that the class membership of the C1 and C2 stimuli was switched. Phase 5 was a replication of Phase 3 with the retrained equivalence classes presented in isolation as probe trials. The retraining in Phase 5 affected the transfer of respondent elicitation. Specifically, after C2 was retrained as a member of Class 1 it elicited a conditioned response. In addition, subjects' responses to C2 and D1 were greater than their responses to either C1 and D2.

The present data suggest that this class alteration can change the function of relevant stimuli. Still to be determined, of course, is whether verbal therapies actually modify the equivalence class membership of targeted stimuli. One particularly relevant issue is whether instructions concerning class membership will affect the transfer of function in the same way as does the direct training of the class. This question is particularly relevant to the treatment of anxiety disorders which in many cases rely on instructions or verbally-based interventions as the method of altering the equivalence class membership.

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Student Paper Session and Award Presentation at ABA '95

The annual symposium of award winning student papers will take place at 10:30 - 11:50 AM Tuesday, May 30 in Independence B. Come to the symposium to hear these promising young scientists and scholars present their outstanding work in person and to contribute to the future of Behavior Analysis by reinforcing our students' fine work.

HUMAN MATCHING IN THE LABORATORY: A SECOND LOOK AT CONCURRENT VARIABLE-INTERVAL SCHEDULES

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Since the publication of R. J. Herrnstein's (1961) paper on relative and absolute response strength, much effort has been devoted to exploring the quantitative relationship between relative reinforcement rate and relative response rate and time allocation. This relationship has been examined across species, response classes, and reinforcers with a similar conclusion reached in all cases: that behavior tends to vary in a systematic manner with the rate of reinforcement by which it is maintained (Davison & McCarthy, 1988). Pierce and Epling (1983) reviewed 17 matching studies which involved only humans as subjects and concluded that humans match relative rates of behavior and reinforcement, both in terms of time allocation and response rate. However, when scrutinized, the means by which these conclusions were drawn raise doubt about the ubiquity of human matching. Bradshaw and Szabadi (1988) conducted a similar review using more sophisticated analytical techniques to conclude that human concurrent schedule performance may vary from that of nonhuman species. The present paper reviews human matching studies by employing the analytical strategies used by Baum (1979) in his review of primarily nonhuman studies in order to draw more quantitatively-based conclusions regarding the phenomenon of human matching. In addition, a number of methodological variables were examined to determine their contributions to the variability involved in human concurrent schedule performance. In all, 12 studies were reviewed, representing 115 individual matching functions.

Although a number of variables were examined across studies, two received the most attention. First, the percentage of variance accounted for (VAF) in the data by the matching function was examined in each study as a measure of how well the matching equation (or some derivative of it) describes the data. The other major dependent variable in the analysis was the slope, *a*, of the fitted regression line between reinforcement rate and response rate or time allocation. This variable allows for an examination of how well the behavior of humans adheres to strict matching (a=1) or if it deviates in some direction from 1 ($a <> 1$).

A number of the data sets in the human matching studies had slopes of less than one (less than 0.5 in many cases) and VAF values of less than 80% (below 20% in many cases). It is noteworthy that in Baum's analysis, none of the data sets fell below the 50% value in terms of VAF. With respect to concurrent schedule performance, therefore, human performance has been more variable than that of nonhumans. The extent of this variability both across and within studies suggests that the conditions under which the matching law describes human behavior reliably remain to be delineated.

In an effort to determine possible causes for some of the differences between human and nonhuman species with respect to matching, a number of methodological variables were subjected to simple *t*-test analyses. Reinforcers *other* than points exchangeable for money, responses *other* than simple button presses, and the absence of a changeover delay were all found to be variables which resulted in matching slopes significantly closer to 1 for the human studies examined. Experimental procedures which were different from a progression of VI-VI values also produced matching functions which accounted for significantly more of the variance than more standard procedures.

The present review suggests that human concurrent schedule performance remains to be understood as well as that in nonhuman species. Variability of methods across studies makes comparisons difficult but suggests that there may be dimensions which can systematically affect the relationship between human behavior and the reinforcement rates maintaining that behavior. Future research in the area should strive to produce more systematic and replicable work as well as extending laboratory findings to more naturalistic settings.

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CHILDREN'S EMERGENT RELATIONS OF EQUIVALENCE VIA EXCLUSION RESPONDING

JACQUELINE J. SCHENK
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Matching-to-sample is a procedure in which an individual is taught to select a particular comparison stimulus from two or more comparisons in the presence of a particular sample stimulus. In the simplest case, there are two sample stimuli (denoted A1 and A2), which are alternately presented, and two simultaneously presented comparison stimuli (B1 and B2). Reinforcement for comparison selection is conditional upon the presented sample: if A1, selecting B1 is reinforced; if A2, selecting B2 is reinforced. The controlling sample-comparison relation A1-B1, for example, may be based on the subject selecting B1 in the presence of A1 (S+ control) and/or rejecting B2 in the presence of A1 (S- control). A more general form of S- control is called control by exclusion. The term exclusion describes the tendency of subjects to select undefined (frequently novel) over defined comparisons (i.e., by the reinforcement contingencies) when the sample stimuli are also undefined (cf. Dixon, 1977). For example, when a subject has learned to match A1-B1 and A2-B2, he or she could select the undefined comparison Y1 and/or reject the defined comparison B2 in the presence of the undefined sample X1, and select Y2 and/or reject B1 in the presence of X2. Control by exclusion generally can be described as: If not A1, then do not select B1; if not A2, then do not select B2 (cf. Tomonaga, 1993). In two-choice conditional discriminations, control by exclusion logically refers to the same phenomenon as S- control; if only A1 and A2 appear as samples, "not A1" necessarily refers to "A2" and vice versa.

The present study further examined children's emergent arbitrary-matching performances via exclusion. Stimuli involved visual stimuli (Sets A, B, C, and D) and verbal stimuli (Set N). Experiment 1 served as a standard equivalence experiment. Eight children were taught to select A1 conditionally upon the experimenter's questions "Which one is correct?" (N1) and A2 upon "Which one is incorrect?" (N2) (N-A training), followed by N-B training and A-A identity-matching training. On subsequent B-A and A-B probes, all children conditionally related A1-B1, A2-B2, B1-A1, and B2-A2. In Experiment 2, eight other subjects received N1-A1, N1-B1, and A-A training. When subjects maintained N1-A1, N1-B1, and showed N2-A2 and A2-B2 during testing, they received A-B and B-A probes. On these probes, 3/8 subjects showed the expected A-B and B-A matching performances. In Experiment 3, Experiment 1 was replicated with eight other children. When they showed A-B and B-A discriminations, they received N1-C1 training and N2-C2 testing. On A-C, C-A, B-C, and C-B probes, the results of seven subjects indicated the formation of ABCN stimulus classes. In Experiment 4, eight other children received N1-A1, N2-A2, N1-B1, A-A, N1-C1, and A1-D1 training. On probes involving D1 and D2, the results of 7/8 subjects were indicative of the formation of ABCDN stimulus equivalence classes. In conclusion, the results suggest that preschool children are able to derive novel arbitrary conditional relations via exclusion and the trained and emerged conditional relations are relations with equivalence properties.

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Call for Nominations for Board of Student Editors

Because graduate students are an important part of the *Bulletin's* audience, and because an important part of the *Bulletin's* mission is to nurture graduate students, it is now *Bulletin* policy to include a graduate student referee in the peer review process for most submissions. Nominations for members of a Board of Student Editors are being sought until August 1, 1995. Desired qualifications include interest and experience in the experimental analysis of human behavior and good writing skills at both the conceptual and mechanical levels. The term of a student editor will end upon graduation or upon completion of additional training (e.g., postdoctoral fellowship). Send nominations, including the students' curriculum vitae, to Tom Critchfield, Department of Psychology, Auburn University, Auburn, AL 36849. A list of board members will appear in the Bulletin beginning with the Fall, 1995, issue.

TIME-OUT POSTPONEMENT IN HUMANS WITH NO REDUCTION IN OVERALL TIME-OUT FREQUENCY

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In traditional free-operant avoidance procedures (e.g., Sidman, 1953), two variables are confounded: (1) overall shock frequency reduction, and (2) postponement of individual shocks. Hineline (1970) devised a procedure that separated the effects of these variables. He found that, with overall shock frequency held constant, shock-postponement maintained avoidance behavior in rats.

The present experiment was a systematic replication of Hineline's (1970) experiment, using human subjects. Instead of shock, the aversive events consisted of brief interruptions of a continuously running video program. As in Hineline's study, responding affected the temporal position of aversive events within a trial but had no effect on their overall frequency or duration.

Three college students from the University of Florida, one female and two males, participated as subjects. Subjects sat in an enclosed chamber facing a television monitor recessed into one wall. The audio portion of the program was delivered through headphones. A small red light was placed in view of the subject. The subject was able to respond by depressing a small button on a hand switch.

Prerecorded video programs were shown during experimental sessions consisting of twenty, 120-s trials. Five-s inter-trial intervals (ITIs) separated trials. The video and audio signals were interrupted for a 15-s period of timeout in each trial. During the postponement procedure, however, responding affected the temporal position of the timeout within the trial.

Trials began with 15 s of time-in (television on) in the presence of a red light. The light signaled time periods during which button presses were effective. If no button presses occurred during this 15-s period, the light went dark and the 15-s timeout ensued. After the timeout, the audio and video signals returned for 90 s, followed by the ITI. A button press in this first 15 s both extinguished the light and postponed the timeout for 75 s (i.e., to the 90th s of the trial). After this timeout, the audio and video signals returned for 15 s, followed by the ITI.

An extinction procedure was put into effect after stable responding was established. Here, button presses neither turned off the light nor had any effect on the timeout (i.e., the timeout occurred 15 s into the trial, irrespective of responding). When responding became low and stable, the postponement procedure was reinstated.

The study showed that with overall timeout frequency held constant, responding that postponed timeouts was maintained in three college students. Figure 1 shows the percentage of trials per session with a response during the first 15 s of each trial. In the timeout-postponement conditions, button-pressing occurred at a level which postponed nearly all of the timeouts in every session. When responses no longer affected the temporal positioning of the timeouts, responding quickly dropped to low levels. Upon reinstating the timeout-postponement contingencies, postponement responding recovered.

The current procedure allows two variables that may affect responding in avoidance procedures to be dissociated. With overall timeout-frequency reduction held constant, timeout postponement was sufficient to maintain responding.

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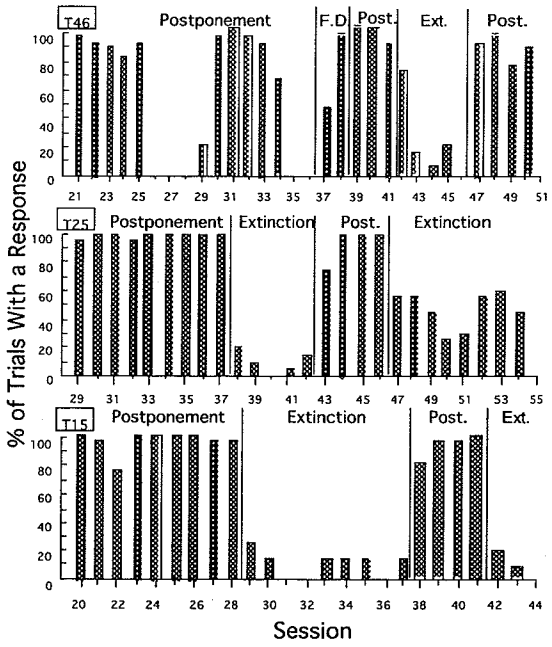

INVITED SYMPOSIUM: THE EXPERIMENTAL ANALYSIS OF HUMAN SOCIAL BEHAVIOR

OPPORTUNITIES LOST AND FOUND: INTRODUCTION

THOMAS S. CRITCHFIELD
AUBURN UNIVERSITY

If one views number of publications as a bellwether, these are good times for the experimental analysis of human behavior (*EAHB*) (e.g., Hyten & Reilly, 1992). Every silver lining has a cloud, of course, and it can be argued that the growth in *EAHB* has been limited to a relatively few research topics (e.g., Dougherty, 1994). The preponderance of recent *Bulletin* articles addressing conditional stimulus control may help to illustrate this point.

The appropriate role for *EAHB* in a science of behavior has been much debated, but few people dispute Don Hake's (1982) argument that *EAHB* can, and should, explore phenomena that are typically viewed as "uniquely human." Curiously, and sadly, the most "uniquely human" phenomena of all--including social behavior, verbal behavior, and idiosyncratic aspects of human development--are among the least investigated areas in *EAHB*. Apparently, by default, some compelling research areas are being ceded to researchers in other theoretical and methodological traditions.

This series of invited papers is an attempt to promote the taking stock of our collective efforts in the analysis of social behavior, and to begin to insinuate social behavior back into our collective pool of research topics. Several experienced researchers in this area were asked to address issues that currently occupy their thinking. Happily, the result is not far from a coherent whole. **Tom Sherburne** and **Bill Buskist** comment on the extent to which social behavior has been an orphaned topic in the evolution of *EAHB*. **Dave Schmitt** analyzes some contingencies that have led to this state of affairs and in the process identifies some research topics that may be worthy of our attention. **Bernard Guerin** takes this latter theme a step further as a way of suggesting that we modify our traditional levels of analysis to do empirical and theoretical justice to compelling social phenomena. Finally, on an optimistic note, **Don Cherek** provides a case study of how attention to social variables can be integrated into an existing research program and, in the process, provide a valuable bridge between basic and applied concerns.

In the interest of continuing this important discussion, I invite the submission of brief comments on these papers, or on the general topic of social behavior, for future issues of the *Bulletin*.

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TAKING STOCK OF THE EXPERIMENTAL ANALYSIS OF HUMAN SOCIAL BEHAVIOR

THOMAS R. SHERBURNE AND WILLIAM BUSKIST
AUBURN UNIVERSITY

Although social psychology traces its roots to the late nineteenth century (Hilgard, 1987), the experimental analysis of human social behavior did not emerge until the 1950s. Skinner's (1953) *Science and Human Behavior* was the first to thoroughly explore the possibilities of studying human social behavior using operant procedures. In 1956, Azrin and Lindsley published a paper in the *Journal of Abnormal and Social Psychology* reporting an operant analysis of cooperative behavior in children. Perhaps the most

often cited early operant study of human social behavior is Lindsley's 1966 paper on "the experimental analysis of cooperation and competition." In this article, Lindsley described his now-famous plunger manipulandum and a social task that later facilitated other behavior-analytic social research (e. g., Schmitt & Marwell, 1968, 1971).

While traditional social psychology has flourished, especially since the 1950s, the experimental analysis of human social behavior has floundered. By our count, the *Journal of the Experimental Analysis of Behavior (JEAB)* has published fewer than 20 empirical reports of human social behavior since its inception in 1958 (about 6% of all human operant papers published in the journal). *The Psychological Record* has published fewer than 10 reports since 1980 (about 7% of the relevant papers). Even more disconcerting is the fact that almost 75% of the papers reporting research on human operant social behavior published in *JEAB* have emanated from the laboratories of only two scientists: Dave Schmitt and the late Don Hake.

Traditional social psychology spans the study of a tremendously vast array of social phenomena, but about 60% of human operant social behavior studies in *JEAB* and *The Psychological Record* have addressed a small handful of topics: cooperation, competition, sharing, or the preference of one these response modes over the others. Table 1 summarizes the relevant papers.

Many reasons may be offered for the dearth of experimental analyses of human social behavior, but in the end, only one reason counts, and that is lack of interest. Recall what Skinner (1956) said about following one's nose: "When you find something interesting, drop everything else and study it." It seems obvious that behavior analysts have not found human social behavior sufficiently interesting to drop everything else and study it.

What can be done to increase behavior analysts' interest in the study of human social behavior? This series of papers on the topic is a good start--certainly readers of the *Bulletin* will now be more informed of the importance of this kind of research. But this is not enough. How many readers will drop what they are presently doing in their labs and start conducting social research? Probably few, if any.

The level and variety of behavior analytic social research will not increase until someone, for whatever reason, conducts an absolutely fascinating study or two and publishes his or her results. Others, seizing upon the importance of these findings, will suddenly look at social research differently. They will set up labs for running dyads, triads, and n-ads. Before long, other labs will spring up looking at all sorts of social phenomena. More and more symposia at ABA will focus on human social behavior. Books will be written and careers launched. We'll be in the midst of a "social movement."

At some point in the revolution, we'll pause for a moment and find ourselves amazed that it took us so long to discover that social behavior is truly interesting. Of course, our amazement (and the revolution, of course) might come sooner rather than later if we would pay more attention to what's going on in the rest of psychology, especially social psychology. After all, they've been doing this stuff for nearly a century and they show no signs of slowing down. Perhaps they know something we don't.

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SUBMIT ABSTRACTS, ARTICLES, CHAPTERS, AND BOOKS PUBLISHED, AND GRANTS RECEIVED FOR THE NEXT ISSUE

To keep current with member activities we would like to publish abstracts from conference presentations, articles published or in press, and grants received in every issue. Please send abstracts from ABA, Behavioral Pharmacology, and other conferences. Abstracts (including those published as part of "Grants Received") should be no more than 200 words; those longer than 250 words will be returned to you for editing. Send to Dean Williams by October 15, 1995.

Table 1. Summary of empirical studies of human and social behavior appearing in *The Journal of the Experimental Analysis of Behavior* (*JEAB*) and *The Psychological Record* (*TPR*).

Authors	Journal	Year	Subjects[a]	Behavior	Results
Schmitt & Marwell	*JEAB*	1968	C students	Cooperation	Cooperation when social stimulus was present but none when it was not present.
Schmitt & Marwell	*JEAB*	1971	C students	Cooperation	Cooperation was disrupted when one of a pair took $1 from other.
Schmitt & Marwell	*JEAB*	1971	C students	Cooperation	Cooperation high when avoidance of taking available, but low when not.
Hake & Vukelich	*JEAB*	1973	C students	Cooperation	Cooperation high when reinforced but low when punished.
Hake et al.	*JEAB*	1973	HS students	Auditing others' scores	Self-audits increased when coactor was present.
Vukelich & Hake	*JEAB*	1973	HS students	Auditing others' scores	Auditing was highest when scores were equal between members.
Hake et al.	*JEAB*	1975	HS students	Cooperating	Subjects chose to share by alternating the taking of matching to sample problems.
Hake et al.	*JEAB*	1975	HS students	Choice: cooperate or compete	Increased FR requirement, decreased competition, and increased cooperation.
Shimoff & Matthews	*JEAB*	1975	C students	Cooperation	Inequity of reinforcer magnitudes resulted in decreased cooperation.
Schmitt	*JEAB*	1976	C students	Choice: cooperate or compete	Competition was chosen only when it was profitable.
Emurian et al.	*JEAB*	1976	C students	Cooperation	Cooperation was higher when required to cooperate to use group area.
Olvera & Hake	*JEAB*	1976	HC students	Choice: cooperate or compete	High ratio group switched to cooperation sooner than low ratio group.
Weiner	*JEAB*	1977	Adults	Altruism	Giving was lower and extinguished faster than earning or point responding.
Emurian et al.	*JEAB*	1978	C students	Cooperation	Cooperation was highest when triad had to cooperate for access to group area.
Morris & Smith	*TPR*	1980	Children	Preference between adults	Children preferred adults who displayed affection.
Hake & Schmid	*JEAB*	1981	C students	Trust	Trusting behavior developed when requirement for cooperation was high.
Schmid & Hake	*JEAB*	1983	C students	Trust	Trusting developed when requirement for cooperation increased.
Buskist et al.	*TPR*	1984	C students	Competition	Instructions were not necessary for control of high rates of competitive behavior.
Barrett et al.	*TPR*	1987	C students	Stereotypy	When experimenter was present subjects' response rates and variability increased.
Buskist & Morgan	*JEAB*	1987	C students	Competition	FI competition increased response rates and break and run patterns developed.
Schmitt	*JEAB*	1987	C students	Choice: cooperate or compete	Cooperation was preferred over competition, competition resulted in lower rates.
Higgins et al.	*TPR*	1989	Children	Button pressing	Subjects imitated a confederate responding on a VT schedule.
Cherek et al.	*TPR*	1991	Adults	Aggression	Aggressive responding increased with increased provocation.
Dougherty & Cherek	*JEAB*	1994	Adults	Choice: compete or respond alone	Competitive responding was insensitive to reinforcer magnitude and probability.

[a]C students refers to college students and HS refers to high school students.

THE EXPERIMENTAL STUDY OF SOCIAL BEHAVIOR: THE PAST AND THE FUTURE

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In a 1994 retrospective view of Darley and Latane's 1970 award-winning monograph *The unresponsive bystander: Why doesn't he help?* Dan Batson describes the 60s and early 70s as a "golden age of civilization" in experimental social psychology (Batson, 1994). Practitioners of that era had a vision and confidence in what they were studying and what they were finding. It was a period, too, in which funding and jobs were plentiful. While mainstream works in social psychology received the most attention, the period also saw the flowering of interest in social phenomena in behavior analysis. Here, too, there was vision and confidence. The experimental study of single organisms was several decades old. Apparently lawful behavior was everywhere in evidence. Experimental methods and laboratory control equipment were in place. It seemed time to see if familiar social processes could be subject to the same kind of experimental control. Azrin and Lindsley's (1956) ground-breaking experimental study of cooperation with children pointed the way. Nonbehavioral colleagues were skeptical. Certainly little that they had seen earlier suggested that people would sit in a lab and press buttons or pull knobs for hours on end, let alone in a predictable manner. But indeed they did. They cooperated, competed, exchanged, shared, and responded to inequity in ways that were quite predictable from the conditions the experimenters created. Such success was not always received enthusiastically by nonbehavioral colleagues. Some, in fact, saw an incipient behavioral revolution that needed to be reckoned with, if not repelled. In sociology, for example, George Homans, Skinner's colleague at Harvard, had published *Social behavior: Its elementary forms* in which the second chapter argued the relevance of Skinner's work with pigeons for understanding human behavior (Homans, 1961). And Homans later became president of the American Sociological Association. Under the guidance of Charles Ferster, Elliot McGinnies (1970) published a social psychology text, *Social behavior: A functional analysis*, using behavior analysis as its base.

History shows the golden age to be short lived. Although numerous settings were developed and a variety of social behaviors brought under experimental control, the laboratory study of social behavior has not emerged as a major sub-area in social psychology, and not even in behavior analysis (although some types of social behavior have been of interest in applied behavior analysis). Why? I have some speculations. Before addressing these, however, there is another possibility to be considered. Even though a sub-area of social behavior has not emerged, the study of social behavior might be flourishing under other headings. The best case for this can be made for verbal behavior, an area in which research has burgeoned in recent years. But while the study of verbal behavior is by most definitions social, it is probably not useful to treat the entire area as a subfield of social behavior. Traditionally, the term social behavior in social psychology has had particular referents. Major areas historically have included cooperation, competition, exchange, problem-solving, power, influence, helping, aggression and the like, and these are the ones I will focus on here. In various of these areas, of course, verbal behavior plays an important role.

DEFINITIONS AND SETTINGS

Key social behaviors such as cooperation and exchange were defined and operationalized in various ways. For example, cooperation between two people can be defined very restrictively to include only joint consequences that result from combined face-to-face behaviors, or less restrictively to include only correlated consequences within some time period. These definitions are so different that generalization of findings from settings using various definitions is problematic (Hake & Olvera, 1978). No standard setting emerged for studying social behaviors (for a review of the various settings see Schmitt, in press). A standard setting, however, is a virtual prerequisite for cumulative research across labs. In behavior analysis, the matching-to-sample setting in stimulus equivalence research can be added to Skinner's operant chamber as prime examples of the advantages of standardization. In social psychology, the Asch conformity setting and the Prisoner's Dilemma matrix are good cases.

Another setting issue is the almost exclusive reliance on points or money as reinforcers. Although reliance on a single reinforcer type can be limiting (as noted below), a more general concern is consequence

strength. It has been noted frequently that human and animal laboratory procedures diverge on this point. Deprivation usually assures that food is the preeminent reinforcer in an animal's life. A few extra dollars (which are neither received nor used immediately) are of unknown importance for an undergraduate student.

GRANT SUPPORT

Grant support has declined dramatically. In the 50s and early 60s various government agencies were interested in group processes. The Office of Naval Research, for example, was a prime supporter of social psychological research. Research on social behavior is costly to conduct. It may take years of testing to develop settings in which variables of interest are controlled. Reversal designs typically require multiple sessions for stable behavior to develop. Studying more than one subject simultaneously greatly increases costs not only because of number. Behavior may take longer to stabilize and groups are lost when one person quits. How attractive is an area which researchers must personally support substantial subject costs?

LOSS OF INFLUENTIAL FIGURES

Key players were lost. Og Lindsley, who had designed several innovative settings, moved on to other interests. Don Hake, who studied various social phenomena and wrote extensively on the topic, died at the height of his influence.

RESEARCH AGENDA

The research agenda has remained cloudy. Much of the excitement of the early studies derived from the evidence that rates of rudimentary social behavior could be studied experimentally, and were sensitive to various reinforcement conditions. The prime elements were two people's behaviors that had consequences for oneself and/or the other person. Typically, behaviors were simple knob pulls or button presses, and consequences were counter points exchanged later for money. Following an era dominated by attitudinal and psychodynamic approaches, this demonstration of lawfulness was news. The most creative attempt to sketch a larger agenda based on the relation between behavior and consequences was made by Kelley and Thibaut (Kelley & Thibaut, 1978; Thibaut & Kelley, 1959). Although this work is not strictly behavioral, it is a brilliant analysis of consequence interdependence--the way a person's consequences can derive from others' behaviors of the combined effect of own and others' behaviors. For the first, consequences in a situation depend solely on the other's behavior (termed fate control by Kelley & Thibaut). For example, one person may give reinforcers such as approval, assistance, or money to another. These can be contingent on some behavior or given noncontingently. For the second, consequences depend in some manner on responses made by both people (termed behavior control by Thibaut & Kelley). For example, a person's consequences can depend completely on combined actions (e.g., two people lifting a heavy object accomplish what neither can do separately). Or consequences can be augmented when responses are combined (e.g., a person may enjoy playing the piano, but enjoys playing much more if another person plays guitar as part of a duet). Skinner (1953), too, noted that the reinforcing effect of an individual act is often increased enormously when a person is part of a group--as when a person cheers in a crowd or jeers in a mob. In these examples the consequences for people making the responses are correspondent (or positively correlated). The coordinated responses produce positive consequences for all participants. Consequences, however, can also be noncorrespondent (or negatively correlated). In the two-person case, the coordinated responses produce positive consequences for one person and negative (or less positive) ones for the other (e.g., only the winning runner gets a trophy). Analyses of interdependence in everyday settings are complex because social behaviors are often controlled by both socially-mediated and individual consequences (ones that depend only on one's own behavior). For example, the meal for two which one person prepares can have positive consequences for the preparer and for the guest. Two people may enjoy watching a movie singly, but enjoy it much more in the other's presence. In some instances, then, the social component only partially affects the consequences for each person. For researchers interested in consequence interdependence, Kelley and Thibaut's work is foundational. However, most of their speculations remain to be examined experimentally.

There are major challenges in studying many of these ideas experimentally. In everyday life, for example, interaction usually focuses on more complex situations: people are dependent on each other in multiple ways; the reinforced response is uncertain or problematic; verbal interaction is crucial. More complex problems have been studied (very loosely) in social psychology (e.g., discussion problems), but not in behavior analysis. The problems in identifying, controlling, and measuring relevant variables become immense as behaviors become more complex and

groups increase in size. In groups larger than two, simple matching formulations might appear to be relevant for understanding the frequency of interaction among group members, based on relative reinforcement rates. This is probably true, however, only in situations where reinforcers are of the same type (e.g., points or money). The more complex dependencies of everyday relations include varied reinforcers such as praise, favors, assistance, etc., and these are probably not commensurate (Green & Freed, 1993).

Within sociological social psychology, an experimental tradition has investigated the exercise of power in groups ranging in size from 3 to 8. The key independent variable has been network type, where subjects are able to communicate only with certain others over the division of points for each of a series of trials. Although these interactions are described as exchanges, they are more accurately seen as negotiations, in which subjects make offers of points from a menu of alternatives. They do not allow unilateral actions or rates of behavior to be studied. Results indicate that an individual's behavior and payoffs are strongly affected by network type and location in the network (for a review see Molm & Cook, 1995). Several formal models have been developed to predict power and exchange within such networks (for an overview see Willer, 1992). These studies generally use between-groups designs, and are conducted within a single session. Variability is often considerable, and it is unlikely that steady-state behavior is being assessed.

The most daunting of the complexities of social settings is verbal interaction. The role of verbal behavior in the form of rules has dominated attention in the past decade. In the study of rule governed behavior, distinctions emphasized by Skinner (1969) and Hayes and his associates (Hayes, Zettle, & Rosenfarb, 1989) correspond to traditional interests in the study of influence in social psychology. Rules are termed commands or plys where consequences for rule following are delivered by the rule-giver; they are termed advice or tracks where the consequences come from the environment. The distinction is similar to one between power and authority in social psychology (Homans, 1961). Here, various bases of power have been investigated, and these might be useful for an elaborated account in behavior analysis. Malott (1989) has proposed a further complication in rule effects. He contends that the consequences from either the rule-giver or the environment are rarely directly acting--they are too delayed to have strong effects on the behavior in question. His candidate for the direct acting contingency is a reduction in an aversive emotional state that is initiated when a rule is stated and removed when the rule is followed. Malott suggests that many instances where human behaviors are apparently under the control of reinforcers such as money, favors, and the like are more appropriately treated as analogs of reinforcement. Guerin (1994) has also emphasized the role of rules and delayed consequences for almost any social behavior. Rules are crucial in the conduct of experiments (e.g., promise of money) even when verbal behavior is purposefully excluded as the behavior of interest (e.g., as when cooperation or competition are studied with subjects unable to communicate). Earlier explanations of laboratory social behavior might have to be rethought in these terms.

What might the future hold? It seems true that the area of social behavior is less easily defined than it was 25 years ago, and that high-profile studies are few in number. But it is certainly not true that little progress has been made. With insights into the functions of verbal behavior come new research topics and reinterpretations of old ones. Particularly informative for this purpose is Bernard Guerin's *Analyzing social behavior: Behavior analysis and the social sciences* (1994). But will the area reestablish an identity? Weighing against it is the lack of funding and the absence of texts or monographs that describe and update a common set of topics in behavioral terms. But history also tells us that areas move in fits and starts. Agenda-setting books appear. Topics can leap to prominence when counter-intuitive findings appear or an accessible procedure is developed. Social psychology, for example, has been energized periodically in such ways in areas such as conformity, dissonance, the Prisoner's Dilemma, bystander intervention, and over-rewarding. What might affect the likelihood of such events in behavior analysis? It is most certainly related to the number of us working on and writing about problems in this area.

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GENERALIZED SOCIAL CONSEQUENCES, RITUALLY REINFORCED BEHAVIORS, AND THE DIFFICULTIES OF ANALYZING SOCIAL CONTINGENCIES IN THE REAL WORLD

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There are a number of complexities that make analyzing human behavior in everyday life very difficult: long and involved contingency histories; multiple contingency effects from large contingency repertoires; many avoidance and escape behaviors that are difficult to recognize in practice; and very complex stimulus discriminations. In this brief note, I wish to make a few comments about two of the trickiest aspects of analyzing the social behavior of real life: generalized social consequences and the shaping of ritual behaviors.

This paper was written while visiting the Psychology Department at Auburn University. I wish to thank everyone there for their friendly Southern hospitality and for the resources they made available. I want to thank Bill Buskist, Tom Sherburne, and Dave Schmitt for very helpful comments that greatly improved the paper. Thanks also to my PG 790 class at Auburn University for discussing some of these ideas, and especially MJ for making her wonderful reply. Correspondence to: Bernard Guerin, Department of Psychology, University of Waikato, Private Bag, Hamilton, New Zealand. (email: bguerin@waikato.ac.nz)

GENERALIZED SOCIAL CONSEQUENCES AND THEIR METHODOLOGICAL PROBLEMS

While the experimental analysis of behavior has so far concentrated on researching the effects of making specific consequences contingent upon behavior, I have argued that social behavior typically involves generalized consequences that are also generalized over persons, situations, and time (Guerin, 1992, 1994). There are regularities in the generalizations, however, that allow some experimental control.

Let us consider the famous Asch (1956) experiments. Solomon Asch brought strangers into a laboratory of whom only one in each group was really an experimental participant, the rest being confederates of the experimenter. The groups were shown a standard line and three comparison lines and the task was to state which of the comparison lines was the same length as the standard line. After a few trials, an easy example was given in which one of the three comparisons was clearly the correct answer. However, all the confederates (who gave their answers first) chose another line for their answer. It was then the turn of the real participants to give their answer.

Asch found that a surprising proportion of participants "conformed" with the majority and gave the clearly incorrect answer. Having established this baseline (between groups design) of conformity, he methodically explored many variables that might increase or decrease conformity. For example, if the answers were given privately to the experimenter on a piece of paper, conformity decreased. If just one other member of the group gave the obviously correct answer (did not conform) then typically the real participant also did not conform.

What is happening here from a contingency analysis point of view and what implications does it have for analyzing social behavior? Recently, while doing a verbal (pretend) reconstruction of this experiment with a class when discussing Asch's work, I asked a participant what she thought would have happened if she had *not* gone along with the majority. What would be the anticipated consequences? She replied, "Well, lots of things."

This nicely captures the "generalized" nature of the consequences involved. Nothing in particular always happens but many verbal and nonverbal social punishments have happened in the past. She might be laughed at, excluded from friendships, disregarded, smirked at secretly, and so on. Notice three things: these consequences are generalized over time, persons, and acts; these consequences have already been learned through socialization (a contingency history); and these sorts of consequences involve participation in groups—they are social. Social consequences usually have properties that come from being massively generalized (in the same sense of "massively parallel"; Hills, 1992) and do not necessarily behave like simple, presented reinforcers would. To repeat, social contingencies are usually not specific (cf. Delin & Baumeister, 1994).

Because acting in groups has been learned in the contingency history of the participants, even a group of strangers is a powerful source of consequences, albeit nothing we can pinpoint exactly (cf. Chase, 1988; Wanchisen & Tatham, 1991). What this means is that Asch was actually doing something quite reasonable from a contingency analysis point of view. Asch clearly defined the behavior to be studied and manipulated many stimulus contexts to see their effects on the behavior. The consequences were uncontrolled but were probably constant because they were generalized (Nevin, 1966; Wenrich, 1963). His research question translates into: given this (historical) generalized source of motivating social consequences, what happens when I change the stimulus context?

An animal (rat) analogue of Asch's research question might be as follows: we know that our experimental rats have all had a long history in one particular experimental chamber that has many colored lights present. The reinforcer has always been food, so we keep that constant. We know that the animals have learned many discriminations such that when complex combinations of lights are presented, food is delivered if the bar is pressed. For example, if the red light goes on for exactly one second (and not two) and then off and if this event is immediately followed by a blue light being lit, then food is delivered contingent on a bar press, but not if a green light also comes on. But if the green light was on during the whole red/blue sequence, then food will still be delivered (contingent on a bar press, of course) unless the yellow light was also on. So for our starting point, like our human participants, we know that our rats have learned some tough discriminations in the past.

Our task as experimenters, to mirror Asch's research question, is to discover what discriminations our rats have previously learned under these historical conditions. This is not often done in behavior analysis, so how would we go about this? We likely would keep the food constant as reinforcer and vary all possible (or at least theoretically plausible) combinations of light conditions and see the effect. Given this situation, what Asch did was quite reasonable.

Now, as good behavior analysts, we could place our rats in the chamber and immediately teach them a new discrimination, that stops them behaving as they have previously learned, but this is not the research question in this case. Likewise, we could offer the real participants in an Asch experiment $10,000 or a mega-truckload of M&Ms® to always give what they consider to be the correct answer, and this would surely change their behavior; but again, this was not Asch's research question, nor the common question of social psychological research. Social psychologists often take an already learned system and analyze how to control it using variables already in that system. Behavior analysts usually start from scratch, but if we cannot do this (Wanchisen & Tatham, 1991) then we might need additional methods for research.

There were still problems, of course, with Asch's experiment: the group design and the loose definition of conformity for example. Asch also only dealt with conformity which could apply to a group of strangers or loosely associated people. When specific groups are used, the behaviors shaped become more specific,

albeit ritualized. More recent advances in conformity research therefore include a "social identity" group or verbal community as a crucial variable in both the antecedent conditions and the consequences (Abrams, Wetherall, Cochrane, Hogg, & Turner, 1990; Guerin, 1994; Hogg & Abrams, 1988).

RITUAL CONTINGENCIES IN ORDINARY LIFE

Another problem when analyzing human social behavior involves ritual behaviors (Guerin, 1992). This does not mean exotic behaviors, which appear bizarre to western culture, but includes many social behaviors of our ordinary lives (Goffman, 1967).

Loosely following Durkheim (1915/1912; Guerin, 1992, 1994), ritual behaviors are those for which the shaping consequence is that the group is more likely to keep together. Ritual behaviors, therefore, are setting events for all the many and varied (generalized) reinforcing consequences that arise from belonging to groups. Those who do not perform the ritual behaviors of their groups become excluded from the group and lose the many reinforcing opportunities of being in that group.

Ritual behaviors are difficult to analyze because the immediate consequences arising from the behavior might have little or nothing to do with what is shaping the behavior. Let me give examples of two different verbal communities. First, if we pass a stranger in the street we often will remark something like, "Hello," "Konnichi wa," or "Good afternoon." The stranger's reply is not a positive or negative consequence in any usual sense of the word. Rather, such greetings are ritual behaviors because maintaining a community of people who speak in this fashion makes possible the many other benefits of such a loose community. This level of community is usually called a "neighborhood," and research on benefits shows that members typically will help with emergencies, with short term problems, with local problems, and will exchange a small range of resources over short periods (e.g., Litwak & Szelenyi, 1969). These very generalized consequences have no other connection with saying "Hello" or replying to "Hello."

As a second example, members of religious communities have many ritual behaviors that establish maintenance of the group and hence establish many reinforcement opportunities, even away from the church environment. For instance, economic business has historically been favoured among members of religious groups (Shapiro, 1987; Zucker, 1986).

Depending upon the historical conditions in which they developed, the rituals maintaining a group can be as bizarre as you like (Masonic handshakes), because the direct effects (from both the social and physical environments) are not shaping the behaviors (or at least not maintaining them). The form of the ritual behavior in principle should be substitutable for other forms, given that the source of control is unrelated to the immediate outcomes of the behavior (we do not immediately perform emergency help behaviors every time a neighbor says "Hello" in the street). In practice, however, anthropologists find that certain parts of the ritual easily change--raising your hat to someone passing in the street--but other parts do not--greeting the person in the currently acceptable form (Bloch, 1992).

Because the shaping consequences are massively generalized and come from an entire community of people, the ritual can be "felt" as a very powerful experience. The powerful consequences coming from a whole community are what will maintain behaviors like altruism, heroism, and dedication to the pursuit of truth and science (Rorty, 1991): the consequences are enormous because they could potentially be given afterwards by all members of that community. This makes the social reality of a large community a very powerful reinforcer.

This point was recognized by Durkheim, when writing about the religion and social structure of indigenous Australians:

> Religion ceases to be an inexplicable hallucination and takes a foothold in reality. In fact, we can say that the believer is not deceived when he believes in the existence of a moral power upon which he depends and from which he receives all that is best in himself: *this power exists, it is society* [italics added]. When the Australian is carried outside himself and feels a new life flowing within him whose intensity surprises him, he is not the dupe of an illusion; this exaltation is real and is really the effect of forces outside of and superior to the individual. (Durkheim, 1915/1912, p. 257)

Indeed, if we make acceptance by, or further participation in, such communities contingent upon specific actions, we can probably make members of large groups do almost anything, including the killing of people in war, the killing of themselves as suicides, or knowingly being killed as martyrs.

Like generalized social consequences, when analyzing the contingencies for ritual behaviors we must look beyond what is present in the immediate circumstances. To do this systematically, we need additional methods for studying such ritual behaviors,

and developing those methods, probably from methods already used in the social sciences but with the rigor of a contingency analysis, will be one task of the future for behavior analysts interested in social behavior.

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BRIEF REFLECTIONS AND PREDICTIONS REGARDING THE EXPERIMENTAL ANALYSIS OF HUMAN SOCIAL BEHAVIOR

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Although the experimental analysis of human social behavior was underway some 30 years ago (e.g., Lindsley, 1963), this field has remained a very small and not well nourished foster child of operant psychology. In fact, several years ago Miller (1983) presented the view that the entire field of human experimental analysis of behavior had remained rather obscure. Fortunately, a few investigators contributed to different aspects of the analysis of human cooperation and competition (Don Hake, Bill Buskist, and David Schmitt), and have shown that these human social behaviors can be brought under schedule control and that such responding is sensitive to environmental manipulations.

My brief comments in this present venue are intended to support the premise that the relative obscurity of the study of human social behavior will not endure, because such studies will provide the opportunity to bridge the traditional gap between experimental and applied research. Mace (1994) has presented the view that basic and applied branches of behavior analysis have gone their separate ways for the most part, with little or no interaction. The study of human social behavior under controlled laboratory conditions presents an excellent opportunity to coalesce applied and basic research. This transition from the laboratory to applied research will be catalyzed by the insistence that researchers establish

the external validity of the human social behavior they are studying in the laboratory. This "search for truth" will come from funding agencies and our peers, who will insist on scientific evidence that we are in fact studying what we say we are.

As an example, I wish to relate my own attempts at studying human social behavior and where this pursuit has lead me over the past 15 years. Pursuing an interest that I had since graduate school, I set out to establish a methodology to study human aggressive responding. I went to the social psychology literature, and found that all existing procedures operationally defined aggressive responding as the actual or ostensible delivery of an aversive stimulus, typically electric shock, to another person. Then the general principles of experimental analysis of behavior were applied to a new methodology which substituted point subtraction or loss as the aversive stimulus, added a response option maintained by point presentation, provided an aggressive response which ostensibly delivered a point subtraction instead of an electric shock and later added an escape option. Aggressive responding was generated by subtraction of points from the subjects' earnings and attributed this to another fictitious person. Aggressive responding was maintained by response contingent initiation of periods of time when subjects did not have points subtracted from their earnings.

The social context of the situation was established through instructions (i.e., points that you lose have been taken away by another person, and when you respond on a particular button points will be taken away from this person). The free-operant nature of aggressive responding in this procedure provided a degree of sensitivity and experimental control which had been lacking in other procedures. An important aspect of such investigations, is also providing instructions which establish the context as nonsocial and contrast such responding with contexts established as social. If one can establish that responding differs in the social and nonsocial contexts, then this supports the labeling of such responding as social.

After several years of investigating a menu of drugs, my peers in an NIH grant review committee, provided the incentive I needed to pursue external validation of the aggression procedure, namely do it or else. After that, we conducted two studies with male parolees that demonstrated that parolees with violent criminal histories emitted more aggressive responses than very similar parolees with nonviolent histories. This established link between prior criminal history and aggressive responding in our methodology supported the external validity. What we were calling aggressive responding in the laboratory was related to violent aggressive criminal acts occurring out in the real world.

A new collaborator brought our laboratory into contact with some ADHD children who were receiving methylphenidate (Ritalin). We were able to demonstrate that methylphenidate produced dose-dependent decreases in aggressive responding in five of these ADHD children. Two ADHD children displayed the opposite effect, that is, they increased aggressive responding following methylphenidate. These two children were also not responding clinically to methylphenidate. Subsequently, one of these children was taken off drugs, and managed with a token system, and the other child was placed on antidepressants.

As a result of these findings the following question arose, can we predict the clinical response of the ADHD children to methyphenidate in the laboratory? In order to determine the effects of methylphenidate on aggressive responding in these ADHD children, we conducted 4 days of testing involving a total of 12 hours. In contrast, a clinical evaluation of methylphenidate therapy based upon parent and teacher reports typically takes several months. Thus, the use of our behavioral technology could potentially save a great deal of time and improve treatment. We are now conducting a study to determine if we can predict a favorable or unfavorable treatment response to methylphenidate in ADHD children.

Our current research has lead us to consider the following questions: (1) Can we predict the probability of aggression in an adult or child by laboratory testing? (2) Can one predict clinical response to medication in ADHD children by laboratory testing? Such questions may or may not be answerable, but other future questions will inevitably come to establish the interface between experimental and applied research as a fertile area of research. In the future, laboratory evaluations of human behavior and/or estimates of the probabilities of certain classes of human behavior or evaluations of changes in human social behavior following behavioral or pharmacotherapy may be as common place as blood drawings and x-rays are now. I believe that such applications of our behavior analysis technology are possible.

I wish to encourage readers to consider the study of human social behavior. Since the social context is established via instructions, you are only limited by your own imagination.

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RESEARCH IN PROGRESS

S+/S- REVERSAL PROCEDURES MAY NOT RESULT IN FUNCTIONAL EQUIVALENCE

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When two or more stimuli control the same response class, they are said to comprise a functional stimulus class (Dube, McIlvane, Callahan, & Stoddard, 1993; Goldiamond, 1962, 1966; Skinner, 1935). However, in another sense, sharing a behavioral function may not be enough to establish functional classes. If the functional substitutability of stimulus class members extends such that a novel change in the function of one class member results in a derived transfer of function to other class members, then it may be useful to call this a functional equivalence class (Dougher & Markham, 1994). In previous research, we found that simply giving a set of stimuli similar respondent functions was insufficient to establish the set as a functional equivalence class. Given this result, we wondered whether covariation of some shared stimulus function might be required (Dougher, Augustson, Markham, Greenway, & Wulfert, 1994). One method which covaries stimulus function in order to examine functional equivalence has been the S+/S- reversal procedure (Dube, McDonald, & McIlvane, 1992; Sidman, Wynne, Maguire, & Barnes, 1989; Vaughan, 1988). Here, an S+ function is trained for some stimuli and an S- function for others in a simultaneous discrimination. When a performance criterion is satisfied, the functions are reversed, then reversed until eventually, a subject makes one or two errors immediately after a reversal of function, but responds correctly to the remaining stimuli. The present study looked at whether a S+/S- reversal procedure that covaries a shared function would result in the transfer of novel discriminative functions among members of two functional classes.

METHOD

Subjects, Apparatus, and Stimuli

Ten undergraduates in psychology classes participated for course credit. They were fully informed before and debriefed after the experiment. Subjects sat at a table in a small experimental room. On the table was an IBM personal computer. The computer presented stimuli on a 14 inch color monitor and recorded responses from the keyboard. Stimuli consisted of six geometric symbols designated as A1, B1, C1, A2, B2 and C2. All stimuli were white on a gray background.

Procedure

Reversal procedure. The following instructions appeared on the screen for 10 seconds, "Choose the left or right symbol using the '1' and '2' keys. Press '1' to choose the left, and '2' to choose the right symbol." Then, three 2-choice simple simultaneous discriminations were randomly presented within three-trial blocks. The three-trial types were A1/A2, B1/B2, and C1/C2. The stimuli designated as ones functioned as S+'s, and the twos as S-'s. Choices of A1, B1, or C1 were followed by the printed word, "Correct," and choices of A2, B2, or C2 were followed by the printed word "Wrong." After a series of 24 trials, performance was evaluated. If two or fewer errors had occurred, then the functions reversed and the stimuli that had been S+ were now S- and vice versa. If more than two errors occurred, then another 24 trials were conducted. This continued until five consecutive reversals occurred.

Novel discriminative function training. The following instructions appeared on the screen for 10 seconds, "Press either the 'Q' or 'P' key for each symbol that appears on the screen." Then, presentations of A1 and A2 randomly alternated in two-trial blocks. In the presence of A1, presses on the "Q" key were followed by the printed word, "Correct," presses on the "P" key were followed by the printed word, "Wrong," and presses on any other keys were ignored. In the presence of A2, presses on the "P" key were followed by the printed word, "Correct," presses on the "Q" key were followed by the written word, "Wrong," and presses on any other key were ignored. After a series of 24 trials, performance was evaluated. Training ended after two consecutive 24-trial series with no errors.

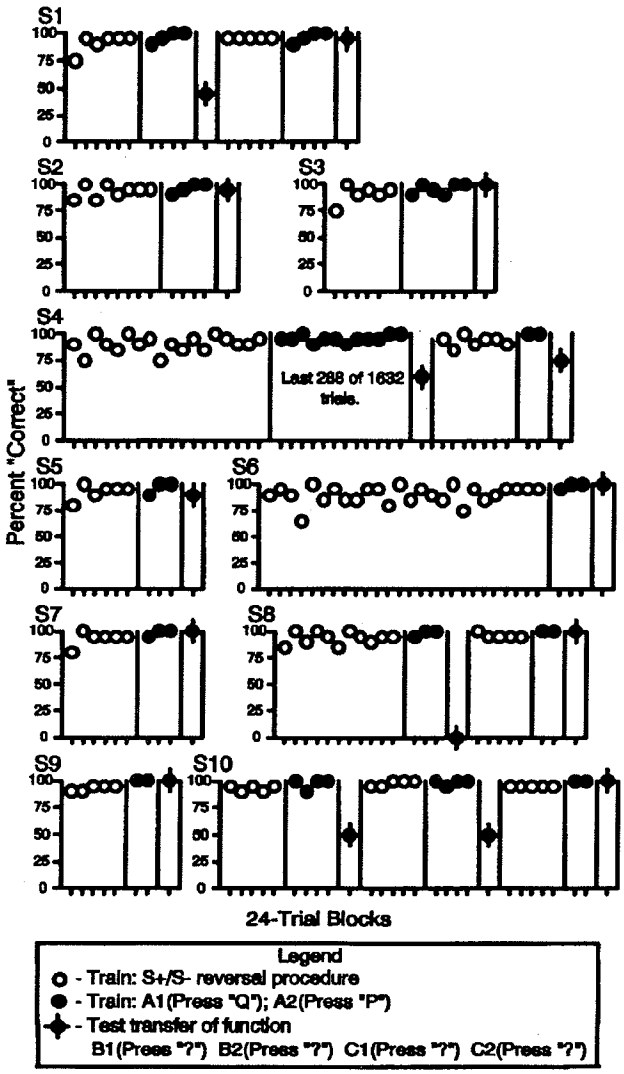
Transfer of function testing. The same instructions as in the novel function training above appeared on the screen. Then, presentations of each of all six stimuli randomly alternated in six-trial blocks. No programmed consequences followed responses. Testing ended after five blocks of probes.

Retraining. Subjects not demonstrating the transfer of function to the B and C stimuli during testing were retrained and retested.

RESULTS

Data for all subjects are presented in the figure below. Data points for the transfer of function tests represent the B and C stimuli only. All subjects met criterion for completion of the contingency reversals and novel function training of the A stimuli. Six subjects demonstrated immediate transfer of the novel function from the As to the Bs and Cs. However, four subjects (Ss 1, 4, 8, and 10) failed to show transfer in the first test. Subjects 1 and 8 required retraining once

and Subject 10 twice before showing transfer of function. Subject 4 declined further participation after retraining once.



DISCUSSION

There are at least two possibilities why the trained functions did not transfer among members of the established functional classes for some subjects. First, testing trials were conducted in extinction, and it is conceivable that this lack of feedback may have resulted in a gradual deterioration of performance. However, an examination of the data did not bear this out. Errors were consistent across the six blocks of test probes. Second, the reversal procedure itself may have induced a repertoire that precluded the demonstration of the expected transfer. After training the new discriminations to the A stimuli, the beginning of the test for transfer may have occasioned a reversal for some subjects. This notion is supported by the subjects' pattern of errors. Subjects' 1, 4, and 10 responded "correctly" to one pair (e.g., B1, B2) but "incorrectly" reversed responses to the other pair (e.g., C1, C2) of test stimuli. Subject 8 "incorrectly" reversed responses to all four test stimuli. More robust evidence for this notion would be the reversal of the newly trained discriminative functions to the A stimuli during the test for transfer. In fact, this was found for two (Subjects 4 and 8) of the four subjects who failed the test for transfer of function. Subject 4 reversed the functions of the A stimuli in 6 of 12 test probes, and Subject 8 in 12 of 12. A definitive explanation, of course, awaits further experimental analysis.

The present results show that S+/S- reversal procedures can result in functional classes that may or may not be functional equivalence classes. This supports the usefulness of maintaining the two different terms. What is critical at this point is to determine the conditions under which functional and functional equivalence classes form. We believe that this may be important in refining definitions of basic stimulus functions, and in the evaluation of theories of stimulus equivalence. In any case, research looking at functional equivalence should train additional behavioral functions and test for their untrained transfer instead of relying on reversal procedures alone.

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BRIEF REPORT

RESTRICTED STIMULUS CONTROL IN DELAYED MATCHING TO COMPLEX SAMPLES: A PRELIMINARY ANALYSIS OF THE ROLE OF NAMING

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On tasks like matching to sample, the performances of people with mental retardation may be called "restricted" or "overselective" because only one of two elements of a complex sample stimulus controls responding (cf. Litrownik, McInnis, Wetzel-Pritchard, & Filipelli, 1978). For example, a recent study included 0-s delay matching trials in which touching a two-element (forms) sample removed it from the display at the time a pair of single-element comparisons appeared (Stromer, McIlvane, Dube, & Mackay, 1993). The positive comparison was identical to one of the sample elements. The procedure is noteworthy because reinforcement could be maximized only if both of the sample stimuli exerted discriminative control (cf. Cox & D'Amato, 1982; Maki, & Leith, 1973; Maki, Riley, & Leith, 1976). Despite this, however, delayed matching performances with single-element samples were more accurate than with two-element samples.

The present study extended Stromer et al. (1993) by first examining whether restricted stimulus control would characterize the performances of persons with mental retardation when common pictures rather than forms were the sample stimuli. In addition, we assessed the effects on delayed matching performance of orally naming these sample stimuli. The purpose was to replicate studies by Constantine and Sidman (1975) and Bonta and Waters (1981) whose subjects (also with developmental deficits) used oral and signed names for sample stimuli, respectively. These subjects' delayed matching performances with single-picture samples improved when they were required to name the sample stimuli. This study asked in addition whether the sample naming would improve performances involving two-picture sample stimuli. Successful outcomes would contribute to the sparse literature about procedures seeking to broaden restricted stimulus control (cf. Burke, 1991).

In an extended discussion we address the broader issues that surround the study of naming in delayed matching. For example, the positive effects of naming are like the effects of nonverbal differential sample responding (e.g., Parsons & Ferraro, 1977; Parsons, Taylor, & Joyce, 1981; Torgrud & Holborn, 1989). At issue is whether the outcomes may be attributable to discriminative control by the samples or by stimuli produced by the differential responding. The methods may also be used to evaluate potential mediating functions of sample naming (Constantine & Sidman, 1975; Mackay & Ratti, 1990). Doing this may contribute to applied analyses of mediation tactics for promoting the generality of behavior change (Stokes & Baer, 1977).

METHOD

Subjects

The subjects were two females and three males (aged 33 to 59; mean = 48 years) functioning in the moderate range of mental retardation.

Apparatus and Setting

A Macintosh computer with a touch-sensitive screen presented stimuli and recorded data. The screen displayed five white "keys" (4.5 X 4.5 cm) on a gray background. Sample stimuli appeared on the center key and comparison stimuli appeared on the four outer keys. Sessions were conducted in a quiet area at the subject's day program or residence. Names spoken by the subject and verbal instructions given by the experimenter were tape recorded.

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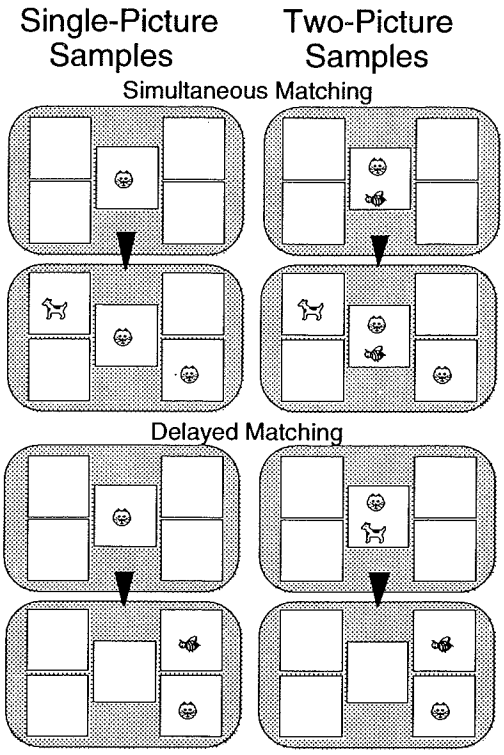

Figure 1

General Procedures

Matching to sample. One or two sessions occurred two or three days per week, each taking about five minutes. Trials began with a single-picture (e.g., cat, dog, or bee) or two-picture sample (top element/bottom element: cat/dog, cat/bee, dog/bee, dog/cat, bee/cat, and bee/dog). A pair of single-picture comparisons appeared after the sample was touched, one of which was identical to a picture that had appeared as sample. Touching the identical comparison was reinforced with auditory-visual feedback and the delivery of a penny; touching the nonidentical comparison resulted in a dark screen for 3 s. The keys were blank for 1.5 s between trials. The top of Figure 1 shows that on simultaneous matching trials, the sample remained on the screen when the comparisons appeared; the bottom shows that on 0-s delay trials, the sample disappeared after it was touched.

Naming test. Some subjects received a naming test prior to the matching trials (see below). On each trial a picture was displayed on the computer screen and the subject was asked to name it. The experimenter pressed the computer keys "K" and "J" to record correct and incorrect trials, respectively but there were no scheduled consequences. All responses were followed by the 1.5-s intertrial interval. Each of the three pictures in a stimulus set was presented three times during a session and there were four such sessions. These sessions were tape recorded and scored for reliability; scores by the experimenter and independent observer always agreed.

Phase-1 Procedure and Results

Phase 1 examined the simultaneous and 0-s delay performances of all five subjects. Each session consisted of 24 trials with single-picture samples and 24 with two-picture samples. Sample type and comparison positions varied unsystematically. The simultaneous and delayed matching conditions alternated in blocks of six sessions. Subject GR received the simultaneous condition twice and the delayed condition once; the other subjects received each condition twice.

Results. Figure 2 shows that accuracy was typically high for simultaneous matching with both single-picture and two-picture samples. In 0-s delay

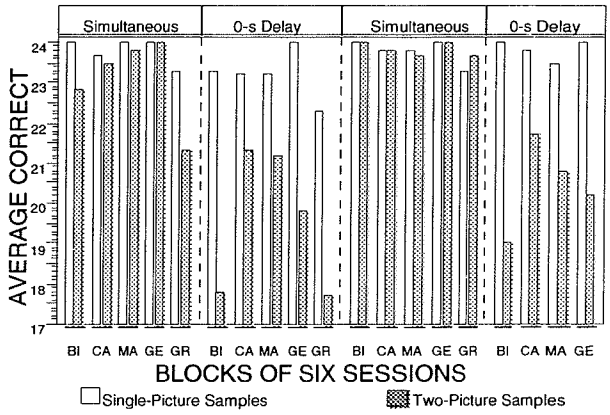

Figure 2

matching, accuracy stayed high with single-picture samples but declined with two-picture samples. For GR, this decrement on two-picture matching trials occurred despite highly accurate naming performance during the initial naming test.

Phase-2 Procedure and Results

Phase 2 assessed the effects on matching accuracy of instructions to name stimuli orally. Sessions consisted of 24 two-picture matching trials presented in 0-s delay. Four of the subjects who participated in Phase 1 were exposed to one or both of the following conditions: (a) No Instruction, in which the sessions were conducted as in Phase 1, and (b) Instruction, in which the experimenter told the subject to name the two sample pictures aloud before selecting a comparison. Subject CA began with the No Instruction

and then the Instruction conditions, followed by No Instruction, Instruction, and No Instruction again. Except for absence of the initial No Instruction condition, BI was the same. Subject MA received Instruction then No Instruction conditions; GE was given the Instruction condition once. Exposure to a condition continued until performance stabilized.

All sessions in Phase 2 were tape recorded. Recordings of the naming sessions were scored by an independent observer. The experimenter and observer showed virtually perfect agreement with respect to the accuracy of a subject's naming.

Results. All subjects named the sample pictures accurately as soon as instructions to name were introduced. Analyses of the recordings indicated a general lack of naming during the No Instruction sessions; the exception was BI during the final No Instruction condition. Figure 3 shows that initial

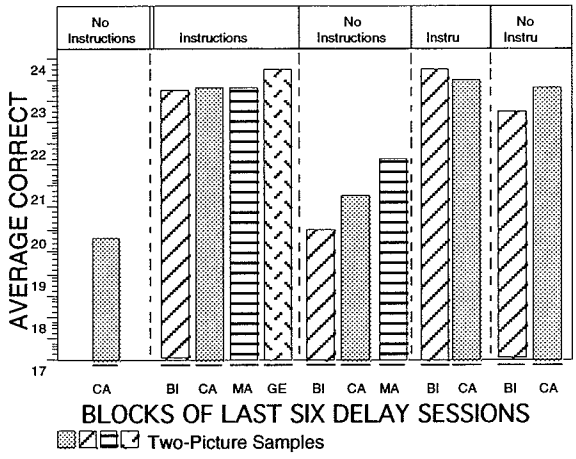

Figure 3

delayed matching performances were most accurate in the Instruction condition and relatively poorer in the No Instruction condition (for GE see Figure 2, far right). For BI and CA, high accuracies even occurred in the final No Instruction condition.

Phase-3 Procedure and Results

In Phase 3, the matching performances of BI and CA were assessed at longer delays using the original dog, cat, and bee pictures and a new set (bus, key, and eye). Each session was arranged in three blocks of trials: 12 trials with 0-s delay, 24 trials with 5-s or 10-s delay, then 12 trials with 0-s delay. Initially, the original pictures were used to examine performance with 5-s delay. The delay was then increased to 10 s. Finally, performance in 10-s delay was examined with the new pictures. A test was given to verify that subjects could name the new pictures prior to this

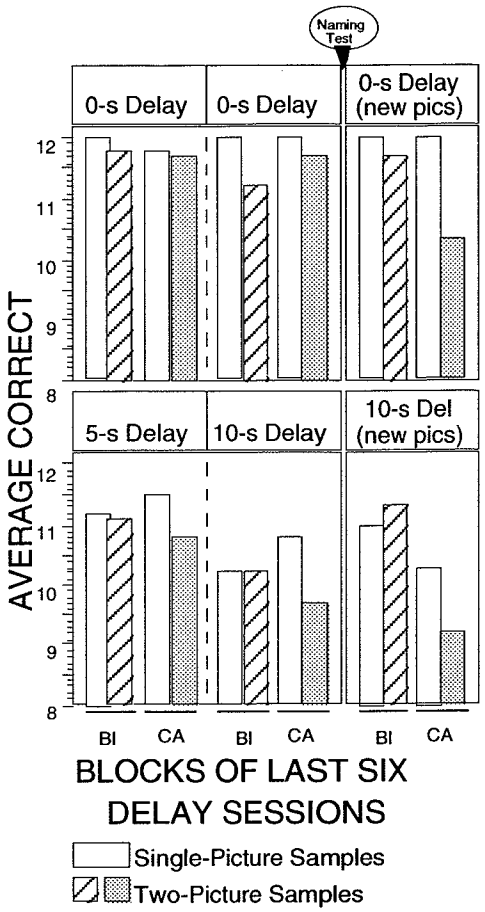

Figure 4

final delay condition. Each block contained an equal number of single-picture and two-picture trials.

Results. Figure 4 shows that accuracy scores on both single-picture and two-picture trials generally stayed high during 0-s matching with the original stimuli. The scores declined with longer 5-s and 10-s delays for both single-picture and two-picture trial types. During these longer delays, BI's single- and two-picture performances declined to comparable levels; CA's two-picture performances were always slightly lower than with single-picture samples.

Both subjects named the new pictures accurately. Afterwards, BI's 0-s delay performance (Figure 4, upper right) was comparable to that shown with the original pictures. BI's 10-s delay performance (Figure 4, lower right) was slightly lower, but above that with the original pictures. In contrast, CA's scores with the new single-picture samples were perfect; however, scores with the two-picture samples declined to the level of those in Phase 1 (Figure 2) and the beginning of Phase 2 (Figure 3). CA's 10-s delay scores with the new pictures were slightly lower than those with the original pictures.

DISCUSSION

These preliminary data raise issues about the stimulus control involved in delayed identity matching and the interpretation of the effects of naming on such performance. The results of Phase 1 replicate Stromer et al. (1993), suggesting restricted stimulus control in the delayed matching of persons with mental retardation. For simultaneous matching with single-picture and two-picture samples, accuracy scores were relatively high. In 0-s delay matching, scores stayed high on single-picture tasks but declined on two-picture tasks. Apparently, restricted stimulus control may occur even when subjects can name the sample stimuli.

Analyses of error patterns help to clarify the nature of the restricted stimulus control demonstrated by each subject (cf. Bickel, Richmond, Bell, & Brown, 1986; Bickel, Stella, & Etzel, 1984). For example, Stromer et al. (1993) found that such impoverished performance may reflect consistent control by a particular element or shifts in stimulus control from one element to the other across trials. Preliminary analyses of the present data suggest the occurrence of consistent selective losses of conditional control by the samples. Shifts in control by elements to irrelevant features of the task (e.g., position of stimuli in the display) are also prominent (cf. Sidman, 1969).

The errors in delayed rather than simultaneous matching may be traced to differences between these procedures in the stimulus control functions of samples and comparisons. In simultaneous matching, the discriminative control exercised by the samples was limited to the situation in which all stimuli were present. The samples were presented successively and a nondifferential touch added the two comparisons to the display. Selection of one comparison, conditional upon the current sample, was then required for reinforcement. In contrast, insertion of a delay demanded that the samples exercise another form of stimulus control. The successive discriminations required among the sample stimuli had to supply the bases for later simultaneous discriminations among the comparisons, even though the sample on each trial was absent at the time of choice. For the present subjects, this readily occurred with single-picture samples in 0-s delayed identity matching but not with two-picture samples where only partial identity of samples and comparisons was involved.

Phases 2 and 3 of the present study explored possible relationships between these delayed matching performances and subjects' picture naming. The findings demonstrate that naming may be used to broaden the discriminative control shown by elements of the samples (cf. Stromer & Dube, 1994). In Phase 2, accuracy on 0-s delay trials improved when subjects were told to name both sample pictures. These data extend previous studies showing that spoken or signed naming may improve performance on delayed matching tasks involving single-picture samples (Bonta & Waters, 1981; Constantine & Sidman, 1975). Particularly interesting were the present observations of improved matching performances even without instructions to name the pictures. In prior studies, which used single-picture samples, performances declined when the instructions to name were withdrawn. Perhaps the more extensive exposure of the present subjects to the contingencies of both naming and no-naming conditions contributed to the improved outcomes.

In Phase 3 we first assessed subjects' matching performances at 0-s, 5-s, and 10-s delays with the original stimuli. Accuracy scores for both subjects stayed high during 0-s delay matching but declined with 5-s and 10-s delays. Features of these data were replicated with 0-s and 10-s delays and new pictures. Notably, BI's high 0-s delay scores with the new pictures may reflect a generalized improvement in delayed matching with prolonged exposure to two-picture samples (cf. Stokes & Baer, 1977). In contrast, the decline in CA's 0-s delay performance with new two-picture samples suggests a lack of such generalization. These individual differences require additional analyses. For example, ongoing research is examining whether naming can be used to remediate problems of restricted stimulus control in 0-s delay matching in a generalized way. The potential of naming to improve performances at longer delays will also require additional study.

Further research will be needed to clarify how the naming may have enhanced delayed matching performance. There are several ways in which this could have occurred: First, the sample naming may have enhanced the successive discriminative control by the pictures. In other words, the positive effects of differential naming may have arisen because it merely ensured discrimination of (or attention to) the particular sample stimuli involved. These effects may resemble those produced by nonverbal procedures used to enhance delayed matching of two-element (Stromer & Dube, 1994) as well as single-element samples (Parsons & Ferraro, 1977; Parsons et al., 1981; Torgrud & Holborn, 1989).

A second possibility, related to the first, is that the sample naming facilitated performance by serving

on each trial to limit the number of stimuli that might be positive and specifying which would be negative (cf. Dixon & Dixon, 1978; Stromer & Osborne, 1982; Stromer & Stromer, 1989). This outcome would be helpful in the present procedure in which only three stimuli were repeatedly displayed across sessions. In the procedure, the naming of only two of the three stimuli on a trial may have promoted the partition of the set into two positives and a negative. Such a partition would have facilitated performance at the time of choice between the two comparisons presented either because the simultaneous discrimination was made easier, because the negative stimulus could be excluded, or some combination of these. Further analysis of these potential sources of the stimulus control would clarify how sample stimuli may function prospectively to select the discrimination required on a given trial (cf. Cumming & Berryman, 1965; Honig & Thompson, 1982).

Third, the positive effects of naming may reflect an entirely new basis for performing the task: The names produced by the subject, rather than the picture samples, may have exerted conditional control of comparison selection; thus, performance may have been based on delayed auditory-visual rather than visual-visual conditional relations. This possibility merits consideration because prior research suggests that delayed auditory-visual matching performance may surpass visual-visual matching performance (Constantine & Sidman, 1975). Our current data do not rule out this receptive account of the effects of naming. Matching performance, of course, may be based on only one or both of these sources of control within or across sessions.

Fourth, naming the sample pictures may have been followed within a trial by the production of those same names when the comparison pictures appeared; the latter names then provided a supplemental stimulus that linked the sample and comparison whose selection satisfied the reinforcement contingencies. The intriguing possibility is that if naming functioned in this way, highly accurate matching performance might occur even with delays longer than those expected on the basis of the three preceding accounts. For example, some of BI's accuracy scores at 5-s and 10-s delays along with informal observations of spontaneous naming during the delay and at the time of comparison selection, suggested this kind of control. Features of this account recommend it as a way of analyzing the potential mediating functions of naming in delayed matching to sample, a possibility developed further below.

Further Analysis of Mediation

Skinner (1968) reminds us of the importance and complexity of the reinforcement contingencies operating in situations involving mediating behavior. In Skinner's terms, mediating events may be viewed as *precurrent* behavior (Parsons & Ferraro, 1977; Parsons et al., 1981; Polson & Parsons, 1994; Torgrud & Holborn, 1989). Precurrent behavior is indirectly related to the prevailing contingencies because its occurrence increases the likelihood that some other *current* behavior will be reinforced. In this respect, complexity in delayed matching arises because the contingencies of reinforcement apply to discriminative behavior involving two different situations, one occurring at the time of sample presentation and a second occurring at the time of comparison presentation. The contingencies thus require discriminative performances based on events differing in location, timing, and potential sources of competing stimulus control. If the contingencies are to be satisfied, the discriminative control required in one situation must be compatible with that required in the other. In short, each sample (or sample element in the present study) must be capable of functioning also as the positive comparison and under other circumstances as the negative comparison, regardless of whether names play roles like those described earlier.

The basis for such compatibility may lie in the possibility that each sample and its related comparison may be substitutable for one another, a notion implicating class formation in certain delayed matching preparations. For example, if spoken names are to function as mediators, they may have to belong in classes with their respective visual referents; indeed, such classes may prove to be equivalence classes (cf. Sidman, 1994, pp. 363-364). The role of stimulus classes may be clear in some applications, for example, when written names (lists) mediate performances requiring later retrieval of their corresponding objects (e.g., Stromer, Mackay, Howell, McVay, & Flusser, 1995). Less obvious, perhaps, is how stimulus classes that include spoken names may be involved in delayed identity matching of pictures. The analysis would have to focus on the sample and comparison events that occur within a particular trial and recognize the symmetrical relations among the stimuli controlling the receptive and expressive performances that may be exhibited during that trial (cf. Dugdale & Lowe, 1990; Hayes, 1991; Mackay & Sidman, 1984). As noted earlier, the assumption is that (a) both sample and comparison displays occasion expressive naming and

that (b) those names function receptively as conditional stimuli in the presence of the comparisons. Further research notwithstanding, such symmetry suggests the existence of stimulus classes and may be critical in whether supplemental naming serves positive mediating functions.

To conclude, delayed matching methods may be useful tools for investigating ways to separate the possible mediating function of behavior from its observing function (cf. Constantine & Sidman, 1975; Mackay & Ratti, 1990). Parallels to such practical problems as establishing and maintaining instruction-following justify the thorough examination of mediating behavior in laboratory and field settings. Moreover, when naming is involved, the methods may be relevant to analyses of topics like self-instruction, self-management, and correspondence training (e.g., Agran & Martella, 1991; Baer, 1990; Browder & Shapiro, 1985; Ferretti, Cavalier, Murphy, & Murphy, 1993; Hughes, 1991; Hughes & Lloyd, 1993). The methods also lend themselves to other attempts to "mediate generalization" of the effects of intervention (Stokes & Baer, 1977; cf. Kirby & Bickel, 1988), for example, in analyses of written naming as a supplemental source of stimulus control (Stromer et al., 1995).

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MESSAGES FROM DR. SIG!

Guest Reviewers for 1994-95

Erik Augustson, University of New Mexico
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Eric Jacobs, University of New Mexico
Barry Lowenkron, California State University - Los Angeles
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Buskit, Sherburne, & Critchfield - A home for human operant research: A tribute to *The Psychological Record.*
Roche & Barnes - The establishment and electrodermal assessment of conditioned sexual responses.
Augustson - Comments on electrodermal assessment as a measure of classical conditioning in humans.

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