

EXPERIMENTAL ANALYSIS OF HUMAN BEHAVIOR BULLETIN

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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.

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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 John Crosbie, Department of Psychology, West Virginia University, P.O. Box 6040, Morgantown, WV 26506-6040, E-mail: jcrossbie@wvu.edu; submit research in progress, abstracts, and news to Cloyd Hyten, Department of Behavior Analysis, University of North Texas, P.O. Box 310919, Denton, TX 76203-0919, E-mail: hyten@scs.cmm.unt.edu (Phone: 940-565-4071) (Fax: 940-565-2467). Submit brief reports and technical information by September 15 and all other materials by October 15 for the Fall 1998 issue.

1998 STUDENT PAPER AWARD WINNERS

Five student papers were selected to receive the EAHB SIG Student Paper Awards for 1998. The award winners also presented their papers at the Association for Behavior Analysis Convention in Orlando. Congratulations to the following students:

- **Michael Clayton**, "A Comparison of Match-to-Sample and Respondent-Type Training of Equivalence Classes"
University of Nevada - Reno, Sponsor: Linda J. Hayes
- **Kimberly Epting**, "Formation and Maintenance of Equivalence Classes via Instructions versus Reinforcement Contingency Training"
University of North Carolina - Wilmington, Sponsor: Carol Pilgrim
- **Adam Goodie**, "Behavior Analysis and Bayesian Integration"
Max Planck Institute for Psychological Research, Germany, Sponsor: Ed Fantino
- **Elliot M. Paletz**, "Effects of Punishment on Human Choice: A Test of an Additive Model"
Auburn University, Sponsor: Tom Critchfield
- **Katherine Stewart**, "Will Subjects Demonstrate Same-Category Clustering in the Verbal Recall of Equivalence Class Members?"
University of North Carolina - Wilmington, Sponsor: Carol Pilgrim

Thanks to the following reviewers of the Student Paper Awards:

John Crosbie
Bill Dube
David Greenway

Tim Hackenberg
Steve Kemp
Jennifer O'Donnell

Carol Pilgrim
Kate Saunders
David Schaal

Richard Serna
Joe Spradlin
Dean Williams

BRIEF REPORT

EFFECTS OF SAMPLE-RESPONSE REQUIREMENTS ON MATCHING-TO-SAMPLE PERFORMANCE WITH HUMANS

Leo A. Carlin, Oliver Wirth, and Philip N. Chase
WEST VIRGINIA UNIVERSITY

Among the procedures used to study conditional discrimination learning in humans, the most common is the matching-to-sample (MTS) procedure. During MTS a subject selects from two or more stimuli one that bears some relation to a preceding stimulus. In the typical arrangement, a trial begins with a sample stimulus presented to the subject. Following a response of some kind to that stimulus (i.e., an observing response), two or more comparison stimuli are presented from which the subject selects one. Selection of a comparison stimulus ends the trial and initiates an inter-trial interval (ITI).

With nonhuman subjects several procedural variables, including sample-response requirements, affect MTS performance (see Mackay, 1991, for a review). For example, pigeons that were not required to peck the sample, took 3-5 times as long to reach criterion performance as pigeons required to peck the sample only once (Cumming & Berryman, 1961). Eckerman, Lanson, and Cumming (1968) also showed that MTS accuracy was reduced 10-25% when the sample-response requirement was removed.

Because similar effects may be found with humans, a sample-response requirement (e.g., touching or pointing to the sample) has become part of the standard MTS procedure with human subjects. We know of no study, however, that has tested the effects of a sample-response requirement with humans and some studies have successfully produced conditional discriminations without requiring sample responses (e.g., Wulfert & Hayes, 1988).

Although variations in the typical MTS procedure are common in studies with humans, and often have little impact on the performance under investigation, the demands of some experimental questions or applications can lead to procedural arrangements that may affect performance in important ways. For example, in response to a growing interest in empirical

investigations of fluency and rate-building procedures (see Johnson & Layng, 1992), we have attempted to develop a model for testing the differential effects of rate-building and practice using MTS procedures. In early MTS experiments in our laboratory, the number of trials completed per minute was limited by the latency to respond to both sample and comparison stimuli. We, therefore, began to present sample and comparison stimuli simultaneously, and subjects were required to respond only to comparison stimuli. Although that modification increased response rates, it adversely affected initial acquisition of conditional discriminations.

Figure 1 shows differences in acquisition performance obtained across conditions with different response-to-sample (RTS) requirements. After preliminary training, subjects received blocks of trials in which all the trained conditional relations were presented in each block (see Method below). Subjects required to respond to sample stimuli (SAV and JET) took 8-21 blocks (approximately 288-756 trials) to reach criterion performance, whereas subjects not required to respond to the sample (ELK and NMG) took 71-104 blocks (approximately 2500-3700 trials).

Those early results show that a sample-response requirement can greatly affect MTS performance, but the advantages and disadvantages of such a requirement need to be assessed. The purpose of this study was to provide such an assessment.

METHOD

Subjects

Three female (KAL, KEC, and RR) and one male (BCM) college students were hired as subjects through a subject recruitment board in the Psychology Department at West Virginia University as part of a larger experiment to investigate the effects of training and instructions on the development of equivalence relations. Subjects were paid .5¢ for each correct response in a block of 24 or 36 trials providing that the accuracy criterion was met. If the accuracy criterion was not met, they received no earnings for that block of trials. The accuracy criterion was 90% for each of the first 42 blocks of training trials, and 97% for each

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remaining trial block. In addition, subjects received a \$1 bonus per session for attending all scheduled sessions. Sessions were conducted 3 to 5 days per week, and lasted for 50 min.

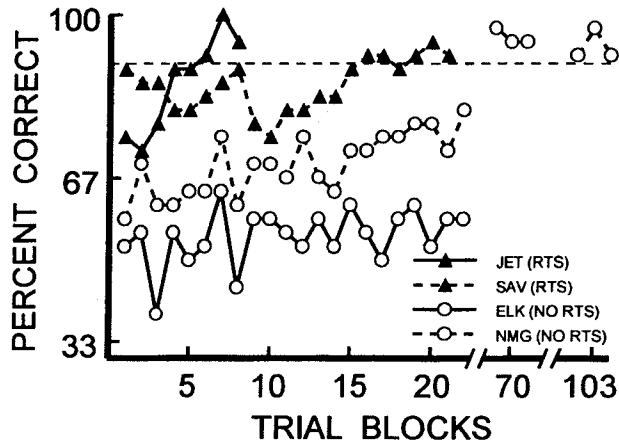


Figure 1

Apparatus and Stimuli

Subjects sat in a small windowless room and responded on a 40-MHz 386 computer. Stimuli were presented on a VGA monitor. On each trial a 0.5-cm x 0.8-cm white sample stimulus was located within a 2.0-cm x 2.5-cm red square above three blue squares (each 2.0 cm x 2.5 cm) containing the white comparison stimuli. One of two sets of stimuli was presented during various conditions of the experiment. Each set consisted of 21 consonant-vowel-consonant nonsense syllables.

Procedure

MTS procedures were used to train conditional relations consistent with three 7-member stimulus classes. Subjects were instructed to select the stimulus in the blue box that matched the stimulus in the red box. Correct conditional-discrimination trials produced the word "correct" at the top of the screen and a 900-Hz tone for 100 ms. Incorrect trials produced the word "wrong" and a 100-Hz tone for 100 ms. Both correct and incorrect responses were followed immediately by another trial. After a block of 24 conditional discrimination trials was presented, the video monitor displayed the number of correct responses, the percent correct, and the earnings, if any, that occurred in that trial block.

Eighteen conditional relations were introduced gradually across 42 trial blocks. Sets of three new conditional relations were presented across five trial blocks, followed by two trial blocks during which all previously-introduced relations were presented. The order of presentation of the conditional relations and

the location of the comparison stimuli were randomly determined within each trial block. After the first 42 blocks of trials, subsequent trial blocks consisted of 36 trials during which each of the 18 conditional relations was presented twice. Training continued until 97% or greater accuracy was obtained for 3 consecutive blocks of 36 trials.

For each subject, presentations of the sample and comparison stimuli were varied across two conditions. In one condition, subjects were not required to respond to the sample stimulus, and the sample and comparison stimuli were presented simultaneously. Presses on the left-, down-, or right-arrow keys selected comparison stimuli and started the next trial immediately (after a 0-s ITI). In the other condition, each trial began with the sample and comparison boxes empty. Presentation of the sample stimulus in the sample box occurred after a variable amount of time had elapsed. On average, presentation of the sample was delayed 1.5 s with delays ranging from 0 s to 3 s. After the sample stimulus appeared, a single response on the up-arrow key produced the three comparison stimuli. If a response occurred before the sample stimulus was presented, the sample delay was reset to 5 s. This arrangement prevented repeated sample-key responses before the stimulus was presented, and presumably maintained looking at the sample box until the sample stimulus was presented.

RESULTS

Figure 2 shows acquisition data when a response to the sample (RTS) was not required (open circles), and when a RTS was required (filled circles). The data are from trial blocks in which all conditional relations were presented. When the sample and comparison stimuli were presented simultaneously, acquisition was slow, and none of the subjects reached 90% accuracy for 3 consecutive sessions by trial-block 68. At this point, subjects were trained using a second set of stimuli with the sample-response contingency in effect. When presentation of the sample stimulus was delayed and subjects were required to respond upon its presentation, acquisition was rapid: subjects reached 90% accuracy in 3-15 trial blocks.

DISCUSSION

The present results show that conditional-discrimination performance improved when a RTS was required. These data are consistent with other studies that report improved acquisition with pigeons as a result of increased contact with the sample stimulus before presentation of the comparison stimuli (Eckerman et al., 1968; Nelson &

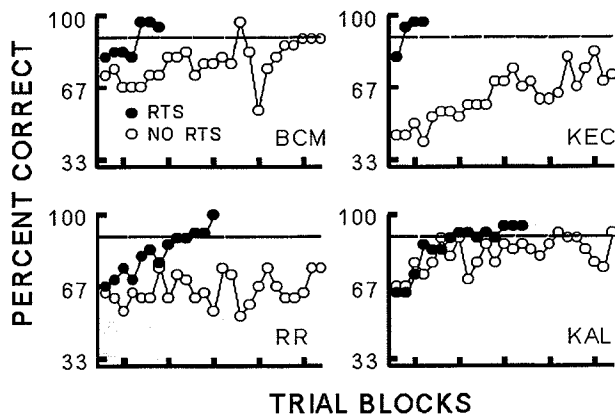


Figure 2

Wasserman, 1978), and with our earlier between-subjects comparison (see Figure 1).

When the subjects are pigeons and the response is pecking, visual contact with the stimulus is assumed when a peck occurs on the sample key. Similarly, when the subjects are humans and the response is touching the sample stimulus or using a computer mouse to move the cursor into the sample box, visual contact with the sample stimulus is more likely than when sample and comparison stimuli are presented simultaneously. In the present experiment, explicit contingencies were designed to induce looking at the sample box and produce visual contact with the sample stimulus when it appeared. Because the sample was presented at variable times, and premature responses delayed its presentation, it is assumed that subjects looked at the sample box and waited for the sample to appear before responding to produce the comparison stimuli. It is possible that this brief period of looking at the sample stimulus is responsible for the improved performance by the subjects who previously showed slow acquisition. Without such a contingency, contact might be minimized and discriminative control by the sample stimulus might be slow to develop.

Discriminative control by the sample stimulus may be facilitated further by arranging contingencies that increase the degree of contact with the relevant features of the stimulus. Sacks, Kamil, and Mack (1972) reported that the number of training sessions required to reach criterion levels of accuracy in a MTS procedure decreased as the fixed-ratio response requirement increased. Moreover, when different responses were required in the presence of each sample stimulus, MTS acquisition was faster than under conditions with nondifferential sample-response requirements (Cohen, Looney, Brady, & Aucella, 1976;

Sidman, Rauzin, Lazar, Cunningham, Tailby, & Carrigan, 1982).

Taken together, these results suggest that different sample-response contingencies might result in varying degrees of contact with relevant features of the sample stimuli. Increasing the duration of the sample presentation, requiring a single response to ensure visual contact, or requiring multiple responses to increase the duration of that contact, might serve to increase control by the conditional stimulus. When differential sample responding is trained, even more precise control by the sample stimulus might result.

Although the present results suggest that the sample-response contingency contributed to improved acquisition of the second set of conditional relations, further tests are necessary to determine if performance increased as a function of extended exposure to the training procedure, or if the conditional relations among the second set of stimuli were more easily learned. Arranging such a test within a single subject, however, might be difficult because observing responses may not be reversible. That is, when contingencies have been arranged for observing the sample stimulus, the observing response may persist even when the explicit contingency is removed. Once such a topography is established, contact with contingencies that reinforce accurate responding may be sufficient for its maintenance.

It also is possible that the ITI alone was responsible for improved performance with the second set of stimuli, and that increased contact with the sample stimulus was not a necessary feature of the sample response. This argument is weakened by the results shown in Figure 1. Differential MTS performance was found across conditions with and without a sample-response requirement, even though a 0-s ITI was arranged in both conditions. The role of the ITI might be tested by arranging a condition in which a variable ITI preceded the simultaneous presentation of the sample and comparison stimuli, and comparing acquisition performance to that obtained when a RTS was required.

Because the subjects in the present study were part of a larger experiment, conditional discrimination training with a programmed response to the sample was provided only when acquisition became a problem. The influence of the sample-response contingency on conditional discrimination acquisition might be tested further when another group of subjects shows acquisition difficulties. Subjects could be exposed to the second set of stimuli with no sample-response requirement in both conditions. Slow acquisition with both the first and second set of

stimuli would suggest that the programmed observing response was responsible for improved performance in the present study.

The present results show that contingencies can be arranged that produce adequate contact with the sample stimulus and thereby improve performance during conditional discrimination training. Although contact is difficult to operationalize, it is reasonable to suggest that conditional control is more likely if subjects are under discriminative control of the relevant features of the sample stimulus.

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RESEARCH IN PROGRESS

SUBTLE BUT POWERFUL DEMAND CHARACTERISTICS: A RESEARCH NOTE

Julie K. Clow and Thomas S. Critchfield
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Much has been written about the importance of minimizing demand characteristics in psychological experiments. Within behavior analysis, this perspective is expressed mainly as concern about the effects of instructions on human behavior, with a consensus that, unless instructions are the primary focus of investigation, subjects should be instructed minimally, if at all (Pilgrim & Johnston, 1988). Even minimal instructions can influence behavior, however, as we recently were reminded when examining data from a study on stimulus equivalence and transfer of function.

The responses of interest were mouse clicks to the 64 cells of a matrix of squares. Subjects responded to locations in the matrix under the discriminative control (e.g., click left vs. click right) of several stimuli that later became part of equivalence classes. Our interest was in whether discriminative functions would transfer to other equivalence-class stimuli to which no such function was directly trained. Subjects earned points, exchangeable for course credit, contingent on their performance. They first completed a baseline phase during which experimentally-novel stimuli were presented individually with the response matrix available. Before the first baseline session, subjects were told: "Your point earnings will be recorded, but you will not receive any feedback on your performance. Do the best you can." During the session, a message on the subject's screen, displayed adjacent to the response matrix, stated: "Click any square or squares of your choosing. Where you click will affect your earnings."

Figure 1 shows stimuli presented to, and data obtained from, a representative subject. The top row shows some stimuli presented during a typical baseline session, and the bottom row shows matrices that summarize response patterns during the session. Within each matrix, cells shown in black attracted responses and cells shown in white did not. Subjects distributed their mouse clicks across the matrix to reproduce the stimuli, with remarkable pictorial accuracy given that clicks left no visual record on the

screen. These data can be interpreted as support for the maxim that when experimental control is weak, other (e.g., historical) influences will predominate (e.g., Sidman, 1960). In the present case, it may be important that the instructions mentioned click location, and that the subjects (college students working on a college campus for course credit) had extensive educational histories of reinforcement for copying. The conjunction of these factors apparently produced functional instructions for copying the stimuli.

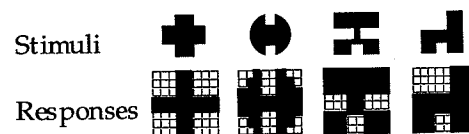


Figure 1

Such opportunistic control might be expected to dissipate once powerful experimental contingencies are in place, and, indeed, during the function-training phase of the study, we have found reinforced response patterns to predominate (e.g., reinforcement for clicking left-side cells has yielded mostly left-side responses). Despite this experimental history, however, when feedback was withheld during post-equivalence function-transfer tests, the patterns shown in Figure 1 re-emerged for several subjects. Overall, our experience in this study suggests that, even with "minimal" instructions and an "appropriate" experimental history, demand characteristics can arise any time that behavioral predispositions meet an accommodating experimental environment.

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AN ATTEMPT TO CHANGE INADVERTENTLY ESTABLISHED SAMPLE-S- CONTROL

Jennifer O'Donnell and Kathryn J. Saunders
UNIVERSITY OF KANSAS PARSONS RESEARCH CENTER

In a two-choice arbitrary matching to sample procedure (AMTS), the selection of one comparison stimulus, B1, is reinforced in the presence of one sample stimulus, A1, and the selection of the other comparison, B2, is reinforced in the presence of another sample, A2. Accurate AMTS performance, however, does not necessarily reflect the development of sample-S+ relations (i.e., A1-B1 and A2-B2). For example, selections of B1 in the presence of A1 might be under sample-S- control (i.e., reject B2 in the presence of A1).

Predominant or exclusive control by sample-S- relations may present problems when testing for emergent relations (see Johnson & Sidman, 1993). In the traditional AMTS task, sample-S+ and sample-S- control cannot be separated. In this preliminary study, we (1) demonstrated a failure to establish a sample-S+ relation in accurate AMTS, and (2) attempted to establish the sample-S+ relation using "biasing" procedures reported by Johnson and Sidman.

Our subject, KR, was a 43-year old man with moderate retardation and an extensive AMTS history (Saunders & Spradlin, 1993). He had learned 29 sets of two AMTS problems (i.e., AB & BC; or AB & CB), usually under "trial and error" procedures. Tests for symmetry and transitivity generally were positive. With several of the sets, however, accuracy could not be maintained when the two arbitrary matching problems (i.e., AB & CB) were intermixed in a session. Consequently, we conducted a more detailed analysis of KR's performance with one of these sets of stimuli. Here we present the results from the "CB" problem in this set.

KR responded on a pressure-sensitive monitor. The sample was presented in the middle of the screen and the comparisons were presented in the two lower corners, with the location of S+ balanced across trials. Correct comparison selections produced a tone and a

penny; incorrect selections produced a buzz and a 3-s black screen. The ITI was 5 s. (See Saunders & Spradlin, 1993, for complete details.) Session events were controlled by software designed by Dube (1991). Two 48-trial sessions were conducted consecutively each weekday.

To assess S+ and S- control, we used a procedure designed by McIlvane, Withstandley, and Stoddard (1984). In this "blank-comparison procedure," only sample-S+ control or sample-S- control could operate on each trial (see Figure 1). For each sample, there was one sample-S+ trial and one sample-S- trial. On sample-S+ trials, a black square replaced the S-, and S+ was the only basis for responding (i.e., a correct response was *selecting S+*). On sample-S- trials, the black square replaced the S+, and S- was the only basis for responding (i.e., a correct response was *rejecting S-* and selecting the black square). For KR, these four trial types were intermixed in a session, and differential reinforcement was provided.









Sample	S+	S-	Trial type
C1 	B1 	■	sample-S+
C2 	B2 	■	sample-S+
C1 	■	B2 	sample-S-
C2 	■	B1 	sample-S-

Figure 1

Preparation for blank-comparison probing was conducted with different stimuli in an identity-matching format, as described by McIlvane et al. (1987).

Data from the last 4 of 34 sessions are shown in the first panel of Figure 2. Accuracy on C1 sample-S+ trials, represented by the filled circles, was low (i.e., 20-50%). In contrast, accuracy on C2 sample-S+ trials and both types of sample-S- trials was 80-100%. Thus, KR had learned only one sample-S+ relation: C2-B2. Instead of the other sample-S+ relation (C1-B1), he had learned to reject B2 in the presence of another sample.

Our next goal was to determine whether a biasing

This research was supported by the National Institute of Child Health and Human Development, Grants HD 18955 and HD 27314.

We thank Joe Spradlin and Dean Williams for helpful comments.

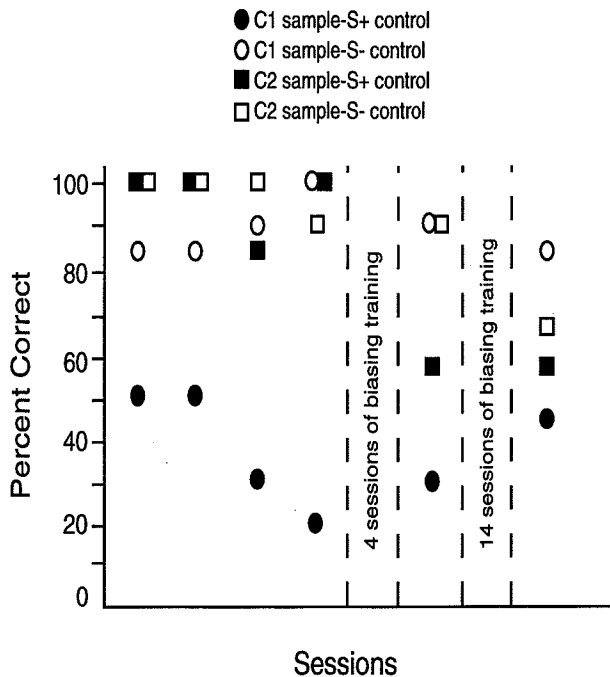


Figure 2

procedure reported by Johnson and Sidman (1993) would establish the C1-B1 relation. Figure 3 shows all trial types. In this procedure, the S+ remained the same but S- varied across trials. For example, in the presence of C1, the S+ always was B1, and either B2 (the original S-), X1, X2, or X3 served as S-. Each sample was presented on 24 of the 48 trials; the original S- was presented on six trials ("baseline"), and the remaining 18 trials were biasing trials (those with a novel S-), with an equal number of each S+/S- arrangement. Johnson and Sidman's rationale was that subjects would learn the fewest possible relations. If so, KR should learn the two sample-S+ relations rather than the eight sample-S- relations.

KR received 22 training sessions with the biasing procedure. Baseline-trial accuracy was $\geq 90\%$ except in four sessions. Biasing-trial accuracy always was $\geq 90\%$, and was 100% in most sessions.

To determine whether biasing training established the C1-B1 relation, blank-comparison probes with differential reinforcement were repeated after the first four biasing training sessions, and again after an additional 14 training sessions. Figure 2 shows that the biasing procedure did not establish the sample-S+ relation with C1. Furthermore, accuracy on sample-S+ trials with C2 and on both types of sample-S- trials decreased. A possible reason for the failure to establish the C1 sample-S+ relation is that

varying the S- across trials made sample observation unnecessary. That is, on the biasing trials, KR simply could have rejected the less-frequently presented comparison—a possibility acknowledged by Johnson and Sidman (1993).

To determine whether this happened, the comparison pairs shown in Figure 3 were presented without samples in a simultaneous, simple-discrimination format. One session was conducted after the 21st biasing training session. No tones or pennies were delivered during this session. In preparation, KR was told that he would receive his pennies at the end of the session, and in the immediately preceding session biasing training was

Sample	S+	S-	Trial type
C1	B1	B2	baseline
C1	B1	X1	biasing
C1	B1	X2	biasing
C1	B1	X3	biasing
C2	B2	B1	baseline
C2	B2	Y1	biasing
C2	B2	Y2	biasing
C2	B2	Y3	biasing

Figure 3

given without feedback and delivery of pennies. In that session, baseline-trial accuracy was 92%, and biasing-trial accuracy was 100%.

On baseline trials, KR selected B1 11 of 12 times (92%). On biasing trials, KR always selected B1 or B2. This suggests that the sample was irrelevant in biasing training and that KR simply rejected the least-frequent comparison. Two differences between our study and Johnson and Sidman's study might account for their relative success: their subjects were verbally sophisticated and they did not attempt to change previously existing stimulus control, as we did for KR.

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GRANT AWARDED TO EAHB SIG MEMBER

Grant Title: Generative Recombination at the Single Word Level

Principal Investigator: Kathryn J. Saunders

Affiliation: The University of Kansas - Parsons

Agency: NICHD

Dates: 01/01/98 - 12/31/2000

Amount: \$100,000

We will seek to determine the extent to which, in nonreading adults with mental retardation, "word attack" skills can be established by teaching relations between spoken words and letters, and between spoken words and printed words. The studies will address basic questions about the processes of abstraction, recombinative generalization, and reading by analogy. They will also address practical issues important to the development of computerized instruction of rudimentary reading skills. We have several aims. The first is to investigate the conditions necessary to establish abstraction of onset sounds, that is, the recognition that the same onset sound occurs in different words. The second is to determine whether recombinative generalization of within-syllable units occurs after training with whole syllables. The third is to determine whether generative oral reading skills can develop after participants learn to select printed words in response to spoken words. In individuals with mental retardation, these are largely unstudied questions. These studies will determine the feasibility of the approach and inform the development of a full research program.

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ASSESSING THE PRIMARY GENERALIZATION OF EQUIVALENCE ALONG THE DIMENSION OF STIMULUS HUE

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Several recent investigations have shown that equivalence-class membership can generalize to a range of stimuli that are physically similar along some dimension to a member of the class (Fields, Adams, Brown, & Verhave, 1993; Fields, Adams, Buffington, Yang, & Verhave, 1996; Fields, Reeve, Adams, Brown, & Verhave, 1997; Fields, Reeve, Adams, & Verhave, 1991). In such procedures, generalization tests of emergent relations are conducted following demonstrations of equivalence, in which stimuli that are perceptually similar to a member of each class are presented as sample stimuli. The extent to which such stimuli, denoted as dimensional variants, occasion class-consistent comparison selections is held to be the extent to which the variants are members of a generalized equivalence class (Fields et al., 1996). Thus, one condition under which a stimulus may become equivalent to other stimuli is when it shares formal properties with an equivalence-class member. These findings suggest that, due to primary generalization, symbolic relations can include a wide range of stimuli, such that an equivalence class may be of infinite size (Fields et al., 1997).

In the present experiment, three 3-member equivalence classes were established, then generalization was assessed by presenting dimensional variants as sample stimuli in place of one member of each class. This procedure differed from those used by Fields and colleagues in two important ways: (a) generalization of class membership was assessed with the stimulus dimension hue instead of line length to provide a systematic replication and extension of previous results; and (b) three comparison stimuli rather than two were available on all trials to ensure that subjects could not respond successfully on the basis of

exclusion, nor achieve 50% accuracy by chance alone (see Sidman, 1987).

METHOD

Participants

Five University of Nevada undergraduate psychology students participated for course credit, and were recruited through announcements made in those courses.

Apparatus and Stimuli

The experiment was controlled by an IBM-compatible personal computer running Microsoft Visual Basic 5.0 and equipped with a color monitor and a two-button mouse. Nine of the 12 stimuli were black arbitrarily-configured figures. The three stimuli that were designated as Stimuli B1, B2, and B3, were solid-colored trapezoids, either of yellow, blue, or red hue, respectively. Saturation and luminescence were identical for the three stimuli (240 and 120, respectively); only their hue varied. The respective hues of Stimuli B1, B2, and B3 were 40, 140, and 240. The 12 stimuli were arbitrarily divided into three 3-member classes.

During the generalization test, 18 dimensional variants of Stimuli B1, B2, and B3 were employed. Variants differed from Stimuli B1, B2, and B3 only along the hue dimension. The hue values of the 18 variants were all greater than the hue value of Stimulus B1, with the hue value increasing by 10 for each variant. Similarly, the hue values of the 18 variants were all less than the hue value of Stimulus B3. The hue value for Stimulus B2 was the median value in the range of hue values for Stimuli B1, B2, B3, and the 18 dimensional variants. Thus, the nine variants that fell between Stimuli B1 and B2 ranged in hue from yellow to blue, whereas the nine variants that fell between Stimuli B2 and B3 ranged in hue from blue to red. All stimuli used in the present experiment were created in Microsoft Photo Editor (3.0).

Procedure

The experiment consisted of three phases. Throughout all phases, sample stimuli were presented in the top center of the screen, then, 1 s later, three comparison stimuli were presented evenly spaced

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across the bottom of the screen. Subjects selected a comparison stimulus by clicking the mouse on it. Each sample stimulus presentation marked the onset of a new trial. Following correct matches during Phase 1, sample stimuli and matching comparisons became outlined in black for 1.5 s, after which the statement "Excellent! One point!" was displayed, and one point was added to the subject's current point total. Incorrect matches led to a new trial. All trials were separated by a 1-s intertrial interval. Point totals were displayed in the lower left-hand corner of the computer screen throughout Phase 1. Data were collected during one 30-min to 60-min session for all subjects.

Prior to the experiment, subjects were told that on each trial their task was to select one of the three comparison stimuli by clicking the mouse on that stimulus. Subjects also were informed that they would be given feedback regarding correct responses only at certain times during the experiment, and that they should perform at their best throughout the experiment.

Phase 1 consisted of the conditional discrimination training of six relations (A1-B1, A2-B2, A3-B3, A1-C1, A2-C2, and A3-C3). The A-B relations were trained first. Subjects selected one of three comparison stimuli (B1, B2, or B3) in the presence of one of three sample stimuli (A1, A2, or A3). Sample stimuli were presented in a random order with the constraint that each sample stimulus could be presented no more than five times per 15-trial block. After subjects had achieved a mastery criterion of 14/15 correct responses (i.e., 93% correct per 15-trial block) for A-B relations, A-C relations were trained. The procedure for training these relations was identical to that used to train A-B relations, except that C1, C2, and C3 were presented as comparison stimuli. After subjects had responded correctly on 14/15 trials, A-B and A-C relations were trained together. The procedure for training these relations was identical to that used to train A-B and A-C relations separately, except that, on a given trial, B1, B2, and B3, or C1, C2, and C3 could be presented as comparison stimuli. The order of sample-stimulus presentations was determined randomly. The phase ended when subjects had responded correctly on 28/30 trials (i.e., 93% correct per 30-trial block) of the mixed A-B and A-C trials.

In Phase 2 the emergence of six symmetry relations (B1-A1, B2-A2, B3-A3, C1-A1, C2-A2, and C3-A3) and six equivalence relations (B1-C1, B2-C2, B3-C3, C1-B1, C2-B2, and C3-B3) was assessed. The

order of stimulus presentations was determined randomly, but the emergence of each individual relation was tested no more than three times. No feedback was given for correct matches. Trials assessing baseline relations were not presented during this phase. The phase ended after 36 trials.

In Phase 3, a 108-trial generalization test was conducted to reassess the accuracy on test trials of the B-A and B-C relations assessed in Phase 2. In addition, the 18 variants of Stimuli B1, B2, and B3 were presented as sample stimuli on test trials in place of Stimuli B1, B2, or B3. Test trials for each of the generalized B-A relations were presented three times, and test trials for each of the generalized B-C relations were presented three times. Thus, there were 54 test trials for generalized B-A relations, and 54 test trials for generalized B-C relations. Test trials for B-A and generalized B-A relations were assessed first, followed by test trials for B-C and generalized B-C relations. Within each set of test trials, the order of test trials was determined randomly. Subjects received no feedback during this phase.

RESULTS

All subjects attained criterion during the A-B, A-C, and mixed A-B and A-C training sets of Phase 1 within at least five trial-blocks. In addition, all subjects performed with at least 90% overall accuracy during the equivalence test, thus demonstrating the emergence of three 3-member classes.

Of primary interest in this experiment was the proportion of class-consistent responses occasioned by each dimensional variant during the generalization test. Generalization gradients were produced and assessed for each subject. If a dimensional variant occasioned class-consistent responding on at least 90% of trials, the variant was considered to be a generalized member of the class.

Figure 1 shows for each subject (S1 to S5; see rows), the proportion of Class 1-, Class 2-, and Class 3-comparison selections (see columns) that were made in the presence of each variant as a function of hue value. Left, middle, and right columns show the respective proportions of Class 1-consistent, Class 2-consistent, and Class 3-consistent responding that was observed in the presence of dimensional variants from Stimuli B1 to B2, B1 to B3, and B2 to B3, respectively. All of the generalization gradients show that the variants most physically similar to class-members were most likely to occasion class-consistent comparison selections. Furthermore, as physical dissimilarity between class members and dimensional variants increased, the proportion of

class-consistent responses occasioned by the variants decreased. Figure 1 also shows individual differences between subjects with respect to which variants were observed to be generalized class members.

DISCUSSION

The present data show that equivalence-class membership can generalize to novel stimuli that are formally similar to a class member along the dimension of stimulus hue, and that generalized equivalence classes can be obtained in a procedure in which three stimulus classes are established. Thus, this experiment replicated and extended the results reported by Fields and colleagues. That equivalence-class membership can generalize to physically similar stimuli has profound implications for our understanding of the conditions under which equivalence classes emerge.

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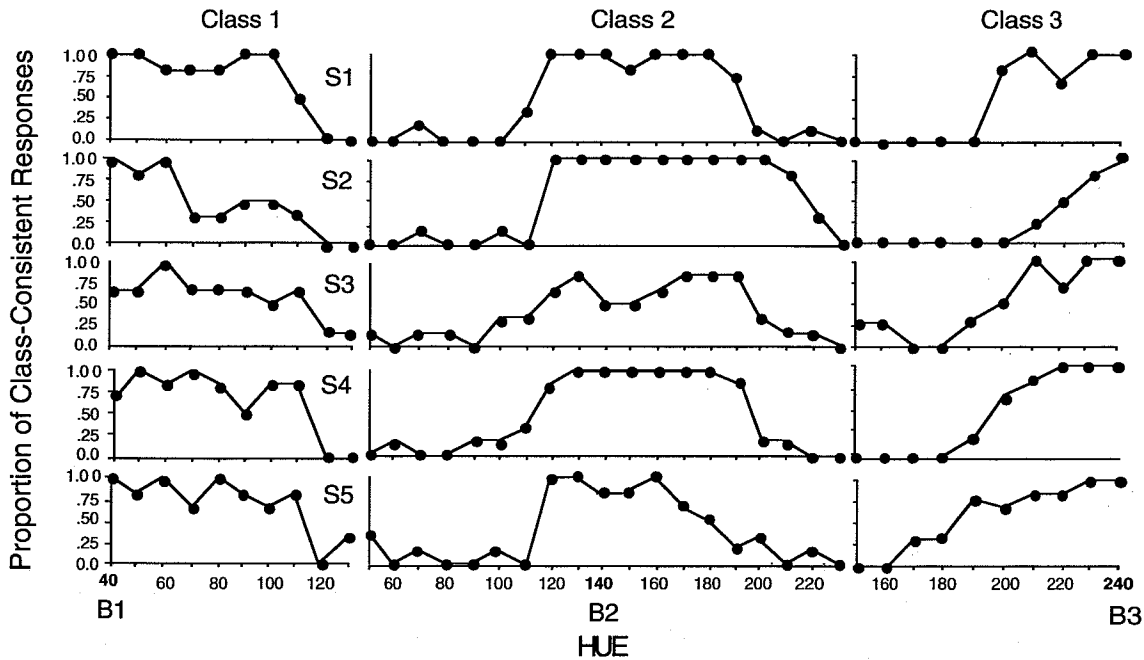


Figure 1

NONVERBAL ASSESSMENT OF LINE-ORIENTATION PERCEPTION IN INDIVIDUALS WITH MENTAL RETARDATION

Richard W. Serna, Stephen Oross III, & Nora A. Murphy
E. K. SHRIVER CENTER AND NORTHEASTERN UNIVERSITY

An often overlooked yet important prerequisite to establishing stimulus control is the perception or detection of stimulus differences that are defined as relevant by the experimenter/teacher. For our interest of establishing stimulus control in individuals with mental retardation, however, there is very limited published research about the perceptual capabilities of those with severe intellectual disabilities. This is understandable, in part, because the verbal requirements of many traditional psychophysical assessment tasks, including "yes/no" and "same/different" procedures, preclude their use with individuals who have limited language comprehension and expression skills. The present paper summarizes an initial feasibility study of psychophysical assessment using a nonverbal yes/no method of measurement.

A seemingly obvious alternative nonverbal method would be standard matching-to-sample (MTS) methods, in which the participant compares two or more different stimuli to a standard. Unfortunately, the forced-choice nature of these methods may set the occasion for stimulus control topographies (McIlvane & Dube, 1992; Ray, 1969) that are not consistent with the intent of the experimenter (Serna, Wilkinson, & McIlvane, in press). We believe that the best test of detection skills is one that asks the participant to make a judgment of whether or not two stimuli are the same or different from his/her perspective. To this end, we have adapted the blank-comparison matching-to-sample (BCMTS) method (McIlvane, Kledaras, Lowry, &

Stoddard, 1992) for use in assessing perceptual thresholds.

In BCMTS, participants view a MTS array that displays a sample, a comparison stimulus, and a black square (the blank comparison). On each trial, the participant indicates whether or not the display contains a comparison that matches the sample. If a matching comparison is available, the participant touches it (analogous to saying "yes"). If no matching comparison is available, the participant touches the blank key, in this case a black square (analogous to saying "no"). During training, trials with and without matching comparisons are presented equally often. The procedure has been used with success in several studies with individuals with severe mental retardation (e.g., McIlvane et al., 1992; McIlvane, Withstandley, & Stoddard, 1984; Serna, Dube, & McIlvane, 1997; Serna et al., in press) and also with young typically developing children (Wilkinson & McIlvane, 1997).

To assess the feasibility of our method, we obtained rough threshold estimates of line orientation with two individuals with mental retardation. Specifically, we asked at what point along a graded series of orientations away from a visual standard would participants detect a difference. We chose line orientation detection because of its relevance to discrimination of two dimensional forms, such as letters and numbers, critical to preacademic skills we strive to teach individuals with mental retardation. The assessment was conducted with two different standards: a vertical line and an oblique (45°) line. This allowed us to test for the "oblique effect" (Appelle, 1972, i.e., the ability to discriminate orientations) is better around the principle orientations (i.e., horizontal and vertical) than around oblique orientations. Thus, we hypothesized that participants would sooner detect differences between line orientations away from the vertical line than the oblique line. Another purpose of the study was to ask whether BCMTS testing could provide an assessment that was comparable to that obtained in a verbal version of the task.

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METHOD

Participants

Participants were two individuals with mental retardation, DDM, age-equivalent score: 4.4, CA: 9.11, and ALP, age-equivalent score: 7.6, CA: 18.10. To compare verbal and nonverbal versions of the assessment protocol, both participants were capable of responding to verbal instructions.

Verbal Assessment

In the verbal version of the assessment task, participants were first *pretrained* to verbally report whether two lines that appeared on a computer screen were the same or different. The stimuli used in pretraining were vertical (V) and horizontal (H) lines, and oblique lines rotated 45° to the left (OL) or the right (OR). The standard stimulus appeared in the center of the screen, and the comparison stimulus appeared in one of four positions in the corners of the screen. Participants were asked to look at the two stimuli and say whether they were the same or different. Both participants were able to do so when presented in a random order with several trials of the following pairs: V/V, H/H, V/H, H/V, OL/OL, OR/OR, OL/OR, and OR/OL. Pretraining was followed by a *baseline maintenance phase*. In addition to the pairs above, the following pairs were added: V/OL, V/OR, OL/V, OR/V, H/OR, H/OR, OL/H, and OR/H. Finally, in the *test phase*, probe trials were mixed with the baseline; no differential feedback was provided during this phase. Individual probe trial pairs consisted of (a) a vertical line (the standard stimulus) and a stimulus rotated away (clockwise or counterclockwise) from the vertical line, and (b) OR (the standard stimulus) and a stimulus rotated away

(clockwise or counterclockwise) from OR. The values of the test rotations away from the standard were based on double-octave steps. (Single- and double-octave step sizes are commonly used for initial screening in psychophysical research.) The values were 0.5°, 2°, 8°, and 32°, as shown in Figure 1. Each standard and test rotation was presented eight times across several sessions.

Nonverbal Assessment

The nonverbal assessment was identical to the verbal assessment, except that the task was presented in the blank-comparison format. In a standard IDMTS task, the participants were first trained to select the comparison stimulus that was physically identical to the sample, using combinations of stimuli presented during the baseline maintenance phase. Then, a fading procedure (McIlvane et al., 1992) was used to establish blank-comparison MTS. The participant learned to touch the line comparison if it matched the sample (a "yes" response) and the blank if it did not (a "no" response). As in the verbal assessment, probe trials designed to assess detection of line-orientations away from the sample were then presented, but in the blank comparison format. Both participants were presented with each task in counterbalanced order.

RESULTS AND DISCUSSION

Figure 2 shows the results. First, both participants maintained high baseline accuracy during the maintenance and test phases (not shown). Second, participants demonstrated consistent judgments within the test values assessed. Third, both participants demonstrated the oblique effect: participants detected differences in the lines earlier in the series of rotations when they rotated away from the vertical line. Finally, the results from the verbal vs. nonverbal versions of the task were very similar for DDM. Interestingly, ALP proved unable to complete the verbal version of the task, but did fine with the nonverbal version.

These preliminary results suggest that blank-comparison assessment of perceptual thresholds is feasible for individuals with mental retardation. Our plan is to conduct more fine-grain assessments of orientation thresholds and assessments of other dimensions relevant to discrimination of two-dimensional forms with frankly nonverbal individuals with mental retardation. We predict that the method will be successful with nonverbal participants, given that the blank-comparison procedure has been used successfully in several previous applications with individuals who have limited language. Our ultimate goal is to increase our understanding of the perceptual abilities of individuals with severe intellectual disabilities.

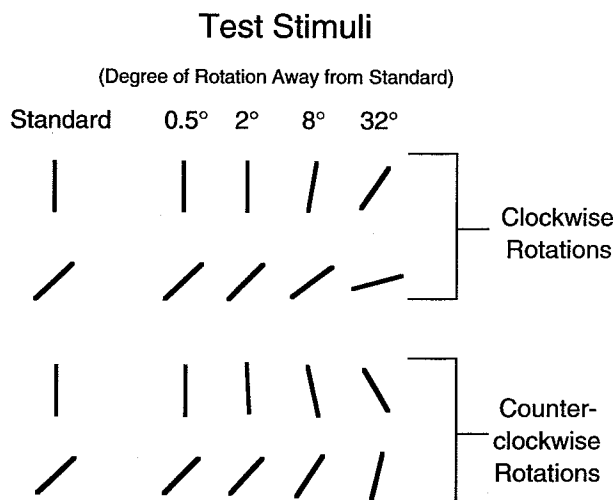


Figure 1

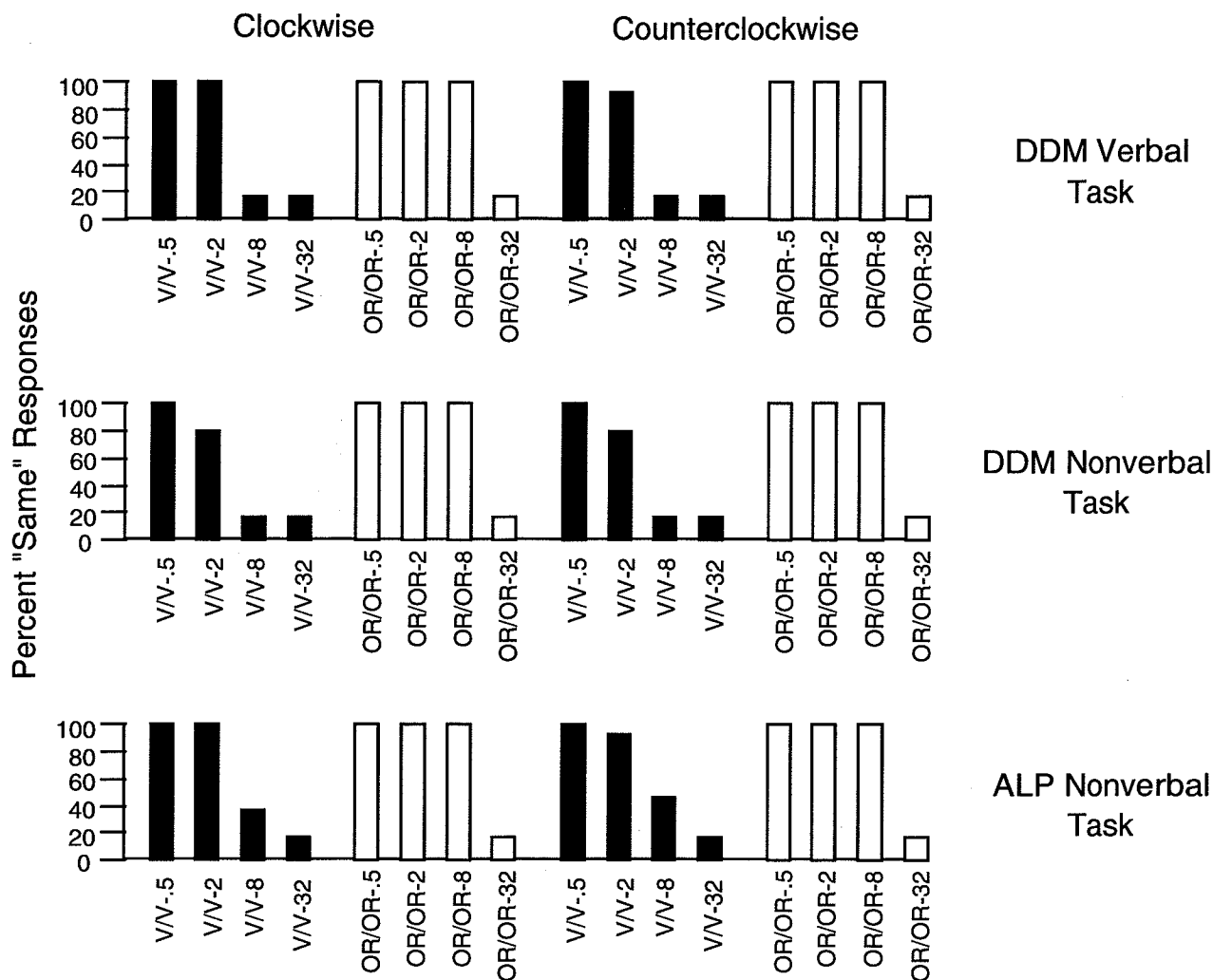


Figure 2

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Punishment Generalization Gradients After Two-Stimulus Discrimination Training

Jennifer O'Donnell and John Crosbie
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Previous studies with humans have shown that punisher delivery may mask control by schedule-correlated stimuli, thereby precluding discriminative control. Such masking occurs with both immediate and intermittent punishment, suggesting that for stimuli to become discriminative, punishers cannot be delivered contiguously with responses. The aim of the present experiment was to establish stimulus control of punished responding. Eight subjects earned points on a variable-interval schedule by pressing a lever or pulling a plunger in the presence of different horizontal-line lengths. In discrimination training,

each response in the presence of one of two stimuli also produced point loss. Point loss initially was delivered immediately, and then was delivered at the end of the session to remove suppressive effects of punisher delivery and thereby allow line length to acquire suppressive properties. After the discrimination had developed, either 10 (for four subjects) or 19 (for the other four subjects) different line lengths (including S+ and S-) were presented randomly. Gradients of punishment effects were obtained, confirming that line length had become functional. Results suggest that although punishment can come under stimulus control with humans, relatively complex procedures may be required, and that once a discrimination is trained generalization is likely to occur.

Association for Behavior Analysis, Orlando, FL, May, 1998.

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THANKS!

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The 1998 Convention of the Southeastern Association for Behavior Analysis will take place *October 15-17 in Asheville, NC*. The meeting offers a varied, single-track program of invited addresses that span all areas of behavior analysis. A call for posters will be issued during the summer. For information, contact Program Chair Dean Williams, University of Kansas, Parsons Research Center, Box 738, Parsons, KS 67357.

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