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 inserted page has information about joining the SIG and contributing to the *Bulletin*. 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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Guidelines for Submissions

Please send three copies of submitted 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*.

Please see the editorial in this issue for submission suggestions. We encourage submissions not covered in the editorial that are likely to be of interest to members.

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. We will reproduce figures and tables directly from what we receive from you, so please prepare them with the column or page width of the *Bulletin* in mind.

Preliminary Communications may be up to 1,000 words long.

EAHB members have a standing invitation to submit Abstracts from posters and presentations given at conferences that do not publish proceedings. Abstracts should be 200 words or less.

Submit by April 15 for the Spring, 1992 issue.

Editorial

Greetings from your new chairs. As our first official act, we want to thank Carol Pilgrim and Mark Galizio for 3 years of superb service. Happily, their legacy is a growing and financially stable SIG and a vital EAHB Bulletin--a truly excellent foundation on which to build.

Many changes have taken place in the experimental analysis of human behavior since the SIG was formed about a decade ago. Perhaps the primary reason for forming the SIG was that relatively little basic behavior analytic research using human subjects was being conducted at that time (a few noteworthy exceptions notwithstanding). The SIG was formed to "facilitate the growth" of EAHB. Happily, this development has come to pass. Edmund Fantino (1991) recently concluded his term as JEAB's editor by noting that about 1/3 of its papers now report research conducted with human subjects. Comparable growth is also evident elsewhere, in other journals that publish EAHB research and at the various regional and national conferences sponsored by behavior analytic organizations.

One response to this growth might be to disband the SIG because its stated goals have been met. Not surprisingly, we do not favor that course. In our opinion, the SIG goals are evolving to reflect the growth that has occurred. No longer are we concerned primarily with increasing output in the area. Now that output is up, our goals must be to sustain the growth, broaden our scope and base of support, and further encourage high quality research that will produce data of enduring scientific value. Thus, our main goal for the coming term is to develop further the information and support services provided by the SIG and the Bulletin. To that end, we will solicit submissions that will help the SIG accomplish these goals. What follows will summarize the types of submissions we seek, and the rationale for each.

Brief Reports Section

To encourage the increased use of the Bulletin for communicating empirical findings of interest to SIG members, we will have a section devoted to brief reports. The section will include descriptions of high quality work of limited scope, for example, methodological notes or limited direct or systematic replications. A newly formed Editorial Board will help review submissions to the Brief Reports section.

Why, one might ask, should one submit to the EAHB Bulletin, given the availability of JEAB, Psych Record, Analysis of Verbal Behavior, and the like? The most compelling reason is that none of these journals explicitly encourage communication in a "Brief Reports" format. Sections of this type are now featured in JABA and in journals in other areas, particularly in the neurosciences. The Bulletin can fulfill this function for the experimental analysis of human behavior.

We believe that efforts to communicate such work should be encouraged for a number of reasons. Perhaps the most compelling reason is that the expense and difficulty of doing research sometimes precludes pursuing interesting findings to the point of a full-blown research report. Many of us have collected data that could not support a full length article but that would nonetheless be of interest to others. We hope that the Bulletin's Brief Reports section will make it less likely that valuable data will be lost or buried for lack of an appropriate outlet. The Bulletin will typically not publish studies that have serious flaws in conception or execution. Of interest, however, are reports of interesting data resulting from studies that do not achieve their original purposes (e.g., Stikeleather & Sidman, 1990).

Preliminary Communications Section

We also want to continue to encourage other types of research communications for which minimal editorial review is desired. One might contribute a paper to the Bulletin, for example, to acquaint the SIG membership with ongoing research efforts, perhaps with the goal of generating broader interest in the topic or soliciting feedback from the SIG membership. Papers with these goals will be published in the "Preliminary Communications" section. This section will also house brief and extended abstracts of EAHB paper and poster presentations at conferences, particularly those at conferences other than ABA (e.g., regional behavior analysis conferences, Behavioral Pharmacology Society, the Gatlinburg Conference, etc.). Also, papers not judged appropriate for the "Brief Reports" section may, with modification, appear in this section. For example, an otherwise interesting study that lacks a needed control might be summarized here in an extended abstract. Finally, methodological innovations of value to

EAHB researchers may also be reported here.

Scientific and Professional Development Section

We plan a section that will explicitly encourage scientific and career development of SIG members. For example, one topic that concerns many SIG members is how to go about obtaining financial support for their research programs. We plan a series of articles that will provide some guidance in this area. Mike Cataldo's paper in this issue kicks the series off.

Another aspect of scientific and professional development is becoming better acquainted with the efforts of other disciplines in the behavioral and neurosciences. In 1992, scientific development is leading progressively to the removal of many philosophical and technical barriers to effective interdisciplinary communication. We plan a series of articles that will describe research in areas that are likely to interest SIG members, such as the study of brain function in relation to behavior. We believe this is one area in particular where behavior analysts can make material conceptual and methodological contributions. Also considered will be other approaches to topics studied within EAHB, including attention (stimulus control), conceptual development (stimulus classes), language (verbal behavior), and a variety of others. Suggestions for appropriate topics and contributions are explicitly invited.

We have two main goals for this series. The first is to help us keep informed about relevant work from outside of the EAB mainstream. The second goal is entirely practical. The continued growth of the experimental analysis of behavior may depend on our showing a broader scientific community how our discipline can produce data of enduring scientific value. We hope that this section will suggest to us ways that this can be accomplished without compromising the integrity of our field or diminishing the special contributions that behavior analysts can make to the study of human behavior.

Technical Information and Materials Exchange Section

This section will address one of the traditional goals of special interest groups, the informal sharing of solutions to problems. We know from direct experience, for example, the frustration that comes when research plans are thwarted by purely technical problems. For example, suppose one experiences an equipment failure with formerly reliable experimental apparatus and cannot obtain repair parts--because they are no longer available, because the repair cost exceeds the department budget, and so on. In this section, one can describe the need and perhaps find other laboratories willing to help. On the other hand, suppose that one has upgraded one's equipment, and that functional older equipment is gathering dust in a storeroom. This may be a case in which one individual's problem may be another's solution. Surplus equipment and the conditions for passing it along can be described in this section.

This section will focus additionally on another major concern of many SIG members--how to make better use of computing equipment in their research. It will be helpful, we believe, to publicize the availability of software that has proven useful in EAHB studies. This could include either commercially-available or "home brew" programs. Similarly, information on interfaces, feeders, stimulus display devices, or other commercially available or custom designed hardware can be presented in this section. Bill Dube has generously agreed to function as an ad hoc technical editor to review submissions, and he has prepared a description of his matching-to-sample software that is currently used in a number of EAHB labs. Along these lines, we also solicit brief articles that describe how computer authoring systems can be used to bypass the well-known problems of writing special-purpose software for research purposes.

William McIlvane and Kathryn Saunders
Editors

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TEACHING CONDITIONAL DISCRIMINATION TO YOUNG CHILDREN: SOME METHODOLOGICAL SUCCESSES AND FAILURES

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Michael J. Dougher
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Devany, Hayes, and Nelson (1986) reported the acquisition of visual-visual conditional discrimination and stimulus equivalence in normal 2-year-old children and in language-using, mentally retarded children matched to the 2 year olds by mental age. In addition, nonlanguage using retarded children learned conditional discriminations, but tests for equivalence were negative. The present study began as a systematic replication of the procedures of Devany et al. with normal 2-year-old children. To avoid the possibility of experimenter cuing, we used a computer for stimulus presentation; Devany et al. used a table-top matching task. In addition, our study differed in that three comparisons (one correct and two incorrect) were presented on each trial; Devany et al. presented only one incorrect comparison. Also, the stimuli that we used did not differ in color. Except for these differences, our training procedures replicated those used by Devany et al. This report focuses on the failure of these procedures to establish conditional discrimination in normal 2-year-old children.

Phase 1

Method

Four normally developing children served as subjects. They ranged in age from 2 years 3 months to 2 years 9 months.

Experimental sessions were 15-20 minutes long and took placed in a small room at a day care center. The number of trials per session was not fixed, but usually at least 20 trials were presented. Stimuli were presented and data recorded with a personal computer. The monitor was covered by a transparent, touch-sensitive screen. The child sat in front of the monitor, with the experimenter sitting directly behind him or her.

The stimulus figures, shown in Figure 1, were arbitrarily designated A1, B1, C1, A2, B2, and C2. The designations varied across subjects. In training relations between "class 1" stimuli, "class 2" stimuli served as incorrect comparison stimuli and vice versa. That is, when A1-B1 was trained, B1 was the correct comparison and B2 and C2 were the incorrect comparisons. When A2-B2 was trained, B2 was the correct comparison and B1 and C1 were the incorrect comparisons.

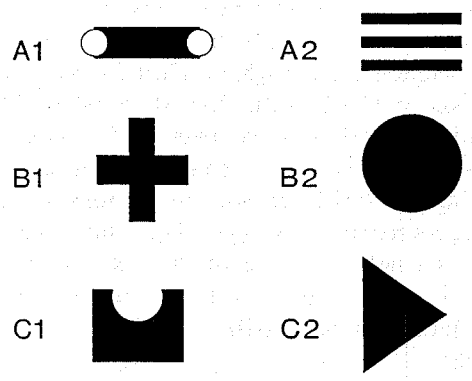

Figure 1

On each trial, the sample stimulus appeared on the upper center of the screen. The comparison stimuli appeared on the bottom half of the screen and were presented randomly in either the left, the right, or the center position (except that the correct comparison stimulus did not appear in the same position on two consecutive trials). At the beginning of each trial, the experimenter pointed to the sample stimulus and said, "See that? Which one goes with that one?" Correct responses were followed by a yellow truck with red wheels appearing on the monitor screen; music played as it moved across the screen. Correct responses also resulted in praise from the experimenter and tiny fish crackers, small fruit snacks, or the opportunity to blow soap bubbles. Incorrect responses were followed by the intertrial interval (ITI). ITI length was a minimum of 2 seconds.

When necessary, physical prompting (guiding the child's hand to the correct choice) and visual prompting (placing the experimenter's finger on the correct choice) were used. Visual prompting was used during a trial if subjects failed to make a response without physical prompting during the previous trial or if the previous two responses were incorrect. Physical prompting was used if the visual prompt failed to produce correct responding within approximately 5 seconds.

The general trial sequence for each relation was as follows: the stimuli for the first trial consisted of the sample stimulus and only the correct comparison stimulus, the second trial added one incorrect comparison, and the remaining trials for that relation consisted of the sample and three comparison stimuli. The trials with only one or two comparison stimuli did not count towards criterion. Subjects were first trained to select comparison stimulus B1 in the presence of sample stimulus A1 (A1-B1). Then subjects were taught to select B2 in the presence of A2 (A2-B2). Then these two trial types were intermixed for a minimum of 20 additional trials. The mastery criterion used throughout training was 9 of 10 consecutive unprompted correct responses for each trial type. Thus, the criterion on the mixed task was a total of 18 of 20 total trials correct, with 9 of 10 correct consecutive responses each for A1-B1 and A2-B2.

Results

The percentage of unprompted correct responses was plotted in blocks of 10 consecutive trials. Figure 2 shows that all subjects' performances reached the 90% correct criterion on the first trial type, A1-B1 and on the second, A2-B2. However, none of the subjects reached criterion when these two trial types were intermixed. Across subjects, the percent correct on this mixed task ranged from 51% to 57%. An informal error analysis revealed no consistent pattern in errors, either within or across subjects. That is, the subjects did not consistently choose any one incorrect comparison for any of the trial types.

Discussion

These results indicate the failure of a methodology for training and testing that would permit equivalence research with young children. This failure is in contrast to the results of the Devany et al. (1986) study, previously described.

It is possible that the subjects in this experiment would have eventually performed the conditional discrimination, given many more training trials on the mixed task than employed in the Devany et al. (1986) study. To address this question, another 2-year-old subject was recruited and given extended training to see whether her performance would eventually reach criterion on the mixed task. In addition, we examined possible differences in performance on these conditional discrimination tasks between 2-year-olds and older children by studying the performance of two older subjects, ages 4 and 6 years.

Phase 2

Method

The subjects were K.B., a girl aged 2 years 9 months; M.B., a girl aged 4 years 7 months; and M.D., a girl aged 6 years 4 months.

The materials and apparatus were identical to those in Phase 1. Experimental sessions were 10-30 minutes long and took place in a small room in the UNM Department of Psychology.

The general procedures were identical to those in Phase 1, except that one additional procedure was employed to facilitate training of the conditional discriminations. To direct attention to the sample stimulus on prompted trials, the experimenter occasionally covered the comparisons while asking, "See this?", then uncovered the comparisons while asking, "Which one goes with that one?".

Results

The percentage of unprompted correct responses was graphed in blocks of 10 consecutive trials (see Figure 2, bottom).

The two older subjects, M.D. and M.B., met criterion after 43 and 48 trials, respectively. The younger subject, K.B., met criterion on each trial type presented alone. Two hundred trials were then presented on the mixed (A1-B1, A2-B2) task, but her performance on this task did not meet criterion.

Discussion

Phase 2 subjects differed considerably in age, and accordingly demonstrated notable differences in performance.

Training with the 2-year-old subject, K.B., was terminated during the mixed task, as with all subjects in Phase 1. It was previously hypothesized that perhaps the Phase 1 subjects had simply required more training trials to learn these conditional discriminations. However, K.B. continued responding near chance level for 200 trials before training was terminated, as noted above.

The 6 and 4-year-old subjects, M.D. and M.B., learned the conditional discriminations almost errorlessly, requiring a total of 43 and 48 trials, respectively.

General Discussion

Given that Devany et al. (1986) had successfully trained four normal 2-year-old subjects to perform conditional discriminations, the results of this study are surprising. Based on their graphs, their four normal subjects required 50, 50, 70, and 120 trials to learn one conditional discrimination. The five, 2 year olds in this study received from 60 to 241

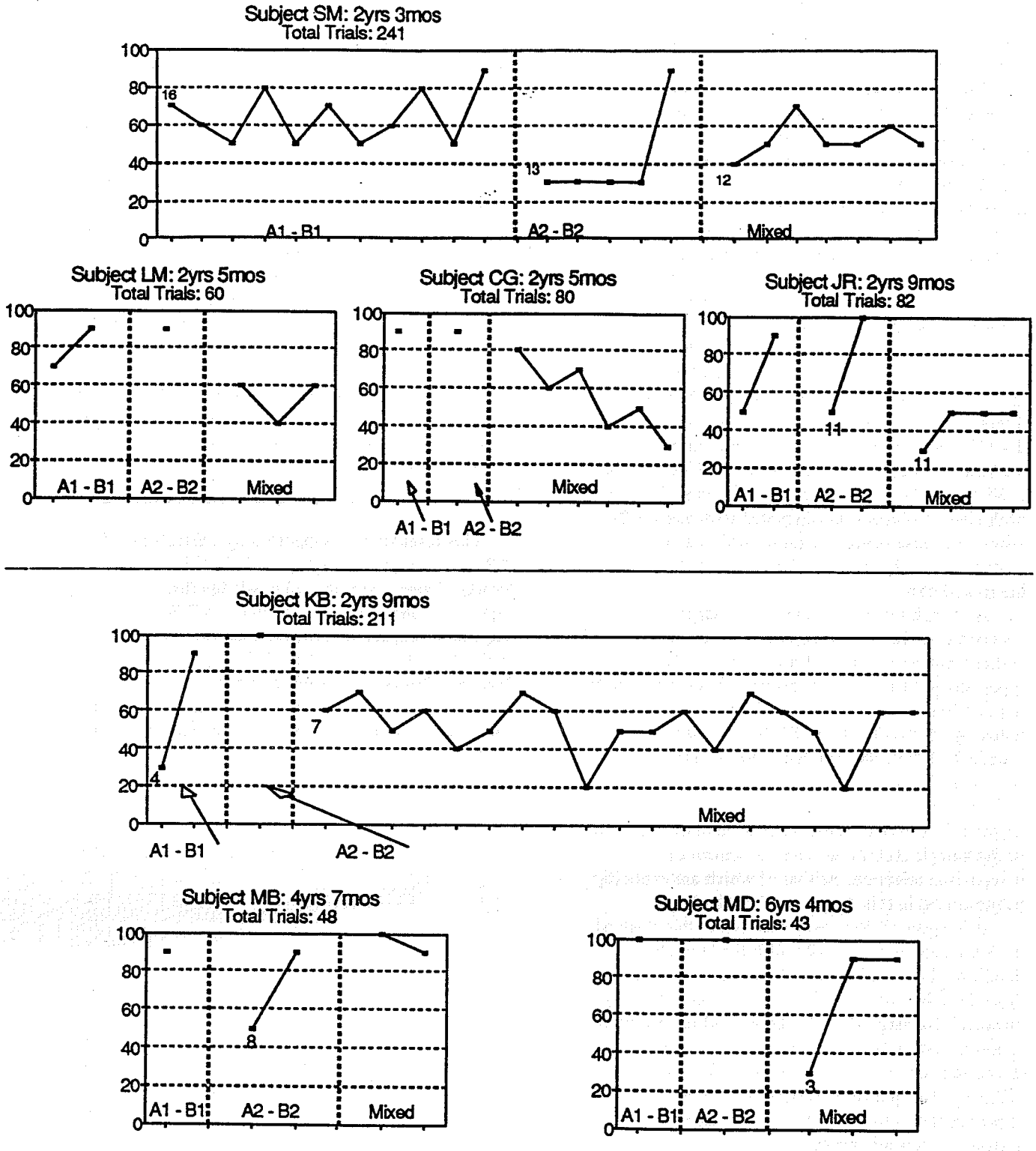

Figure 2. Individual training data. The data are presented as percentage of unprompted trials correct (vertical axis) across blocks of 10 trials (horizontal axis). When data points include other than 10 trials, the number of trials is noted below the data point.

trials, and none of them learned the conditional discrimination.

The present experiment differed from the Devany et al. (1986) study by using a computer task. Another difference in procedure was the use of the two initial trials without two incorrect comparison stimuli for each trial type. A third difference was that our study presented two incorrect comparison stimuli on each trial. It is possible that these methodological changes increased the complexity of the training task beyond the capabilities of this age group, but exactly why these procedures would do this is unclear.

Another difference between this study and that of Devany et al. (1986) is that the stimuli used in their experiment differed in color as well as shape. There is evidence that color may be a more salient basis for discrimination among stimuli than form, for young children (e.g., Catherwood, Crassani, & Freiberg, 1989). However, a follow-up replication of these procedures was conducted with another 2-year-old subject using stimuli of different colors. Criterion was not met in 150 trials, including 92 on the mixed task.

Although the present results are surprising in contrast to those of Devany et al. (1986), it is perhaps not surprising that presenting the two trial types independently before presenting the mixed task did not facilitate acquisition of the mixed task. As noted by McIlvane, Dube, Kledaras, Iennaco, and Stoddard (1990), the first two steps of training in this type of procedure require only the discrimination of the comparison stimuli. Success on the mixed task requires successive discrimination of the sample stimuli and sample control of comparison selection, neither of which are explicitly programmed in this "three-step" method.

It is apparent that many children at this stage of development have difficulty with the laboratory conditional discriminations characteristic of the typical equivalence paradigm. Given the relatively advanced language skills of these children, however, it seems certain that their behavior has come under the control of some contextual stimuli in natural settings. The question remains how to more appropriately study conditional discrimination learning in this age group.

Clarification of this issue is particularly essential because important determinants of stimulus equivalence remain as yet unspecified. These determinants need to be understood, given the clear relevance of stimulus equivalence as a theoretical framework for language, concept formation, categorization, and other "cognitive" phenomena prominent in early development.

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HOW NOT TO GET A RESEARCH GRANT

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Many pitfalls lie in the path of a productive career. Seemingly important activities that are time consuming and that compete with advancing our science and one's professional standing represent proverbial "behavioral traps." With reductions in grant funding, increased competition from biomedical research, and the outbreak of public media reports about scientific fraud, the writing of grants, and worse the receipt of an award, can be considered a trap to avoid. Despite the inherent logic of this observation, a scientist may encounter considerable pressure to compete for grant funds. Ergo, this brief bit of advice on how to not get a grant.

At the onset, a word of caution concerning my advice: take it with some care and seek additional counsel. I say this because I am not ideally suited to this topic. Over the past 15-20 years, I have been the recipient of over 20 million dollars in grant funds and at last count, my success rate at having a grant awarded was slightly over 80%. Despite this rather dubious record, I will do my best.

I offer the following based on numerous personal attempts to not get a grant and my experience at observing others doing likewise while I served as a reviewer or chairperson of review committees for NIH, NIMH, MCH, etc.

SUGGESTION ONE: *Don't Apply*

Following this suggestion is guaranteed to achieve the goal of not getting a grant. Further, it has added benefit of saving you 50-90 hours, which is what most good-sized grants cost, plus several thousands of dollars in support staff time. If you are submitting a large Center or Program Project Grant, the costs in professional time could conservatively exceed $50,000.

SUGGESTION TWO: *Ignore Your Audience*

Like almost all acts of verbal behavior, "grant getting" is a matter of audience control. There will be at least a primary and secondary reviewer each of whom will have their biases, points of view and possible publications in the field. The composition of a review group can often be determined while a grant is being written and an educated guess can be made as to the probable reviewers. For example, most review groups are like Noah's Ark: there are a few of every kind including at least two scientists considered to be knowledgeable about your area. If at all possible, you should write your proposal in a manner that at least is contrary to the reviewers' biases and points-of-view on the research area. Better yet, if you can offend them by either not citing their work or by misinterpreting or criticizing their work, you will most certainly have the primary and secondary reviewers using all their skills to ensure a less-than-fundable score. *Some* members of the review group will not have read your proposal, *most* will rely heavily on the primary and secondary reviewers' opinions and *all* get to vote; thus, the damage you do with the primary and secondary reviewers will get magnified.

SUGGESTION THREE: *Violate the Grant "Rules of the Road"*

In many aspects of life there are "rules of the road." For example, in California the pedestrian has the right-of-way. In New York City, vehicular assault appears to be sport. Beware the California tourist in New York City. In Arkansas, the car that is passing has the right-of-way. (Think about it!) On the open waters, there are two important rules on the right of way: *Rule 1*--the vessel under sail or to starboard has the right of way. *Rule 2*--if you ignore Rule 1 the larger vessel wins.

For grants there are some rules set down in writing by the agency. If you ignore them, no matter how much the reviewers or committee members like you and want to see you funded, you will succeed in not getting a grant. For example, one large grant request had the backing of a State Senator who was willing to argue for an increased appropriation for the granting agency, a prospect about which the agency was favorably disposed. In short, lots of folks were rooting for the applicant. Unfortunately, he/she ignored the guidelines and wrote a totally inappropriate application. The review process was literally painful for reviewers as they attempted to provide a valid appraisal yet continue to encourage the investigators. But the author was ultimately successful, as the application was not funded and all the work and hassle of

spending some 5 million dollars was avoided.

Informal rules also exist, those not published in the grant guidelines. One set of rules has to do with the necessity for proposing a scientifically valid study. Since most members of a committee are not familiar with your content area, they rely on the opinion of the primary and secondary reviewers and on the pieces of the proposal they can recognize. That is, the primary and secondary reviewers provide commentary on the *content*, while each reviewer assesses for him/herself the *form*. Considerations about form relate to some additional suggestions.

SUGGESTION FOUR: *Propose A-Theoretical Research Devoid of a Hypothesis*

Part of the "form" of a grant that scientists look for is the section that shows how the research will add to theory building. The NIH has even made it easy for reviewers to find this section by requiring you to write a few hundred words and label it the "Significance Section." Scientists can then easily recognize this section even if they do not understand the content. Also, they look for the hypothesis and the logical progression of how it will be tested. Many of us in Behavior Analysis apply research methods to show how we have "solved a problem of social relevance" rather than to test a hypothesis and add to theory. Writing a research grant application the way we write many of our studies will greatly contribute toward the objective of not getting a grant.

SUGGESTION FIVE: *Be Vague about the Subjects*

If you do this, members of the review group (whether they understand your content area or not) will argue that the research cannot be done if you do not know for whom the phenomena apply. Most scientists on a review group are accustomed to reading precise descriptions of human subjects in terms of inclusion and exclusion criteria. For animal research, the relevance of the species is expected to be argued. Ignore this history and reviewers will argue that you are a "bad" scientist. If the phenomena you wish to study apply to a broad set of subject classifications and therefore this distinction is either unimportant or your subject choices will show generality across types, by all means *do not* point this out as a rationale (and if you have followed Suggestion Two and alienated the primary reviewer, he/she won't argue this point for you).

SUGGESTION SIX: *Provide No Evidence on the Use of Your Measures*

Whenever possible, do not use standard methods of measuring the phenomena in question and then ram the point home by not providing any evidence of your experience with the novel measures you propose.

The only way you can better your chances of not getting a grant is to ignore altogether describing how you will measure a change due to the independent variable. I've seen this done in a number of applications to which the comment always made is: "Look, this applicant didn't even tell us how he/she is going to measure things!"

SUGGESTION SEVEN: *Avoid Including Pilot Data or Examples of Previous and Related Research*

A great way to convince a review group that you are worthy of funding is to show them a preliminary investigation. Therefore, if you don't want the money, don't present relevant pilot work and don't mention published studies on related topics that employ the same or similar methodology to that which you propose to use. Instead, try to describe many possibilities for your research but do not waste time on the specifics of methodological rigor. Reviewers are extremely fond of fishing and very good at observing attempts to put together an expedition; fortunately for you, fishing expeditions are not a fundable category.

Of course, presenting a lot of pilot data will not always doom you to receiving an award. If the studies you are proposing are essentially the same as work you've already done, or if the pilot work is so good that you've sufficiently answered the proposed questions, the reviewers will notice and the grant will not be funded.

SUGGESTION EIGHT: *Be Vague about Your Independent Variable, Design, and Analysis*

This is very much related to #6 above. I once observed a very senior scientist submit and defend a proposal wherein this suggestion was followed to a "T." So excellently was this done that the reviewers asked *three* times for clarification: "Dr. _____, exactly how do you propose to conduct this study? That is, could you please describe the experiment that will prove your hypothesis?" His/her reply was vague two times, and in response to the third request for specifics he/she replied that we all must be familiar with his/her other work and that he/she would conduct the proposed work in the same excellent manner. Indeed, those of us who knew him/her well were convinced he/she would somehow work it out and were further somewhat embarrassed that we'd asked, let alone three times.

The biomedical reviewers on the group who didn't know the investigator from Adam thought the whole matter ridiculous, killed the section and eventually the entire rather large multi-investigator application.

If you propose a time-series or single subject design, you can follow this suggestion by not defending the reason for your choice. Since this design approach will be unfamiliar to many members of a review group, they will not know what to make of the idea and this will be reflected in their score.

Similarly, if you are going to use a group design and statistical methods of analysis, it is good to be vague here, too. You should avoid describing the exact tests you are going to use and the rationale for choosing these tests over others. Opt instead for saying merely "The appropriate statistical tests will be used." This always "impresses" fellow scientists accustomed to analysis sections written by statisticians who are Full Professors and Co-Investigators. The contrast of your poorly thought-out approach will win you an unfundable score.

SUGGESTION NINE: *Do Not Provide Details on Exactly How You are Going to Accomplish All This: (e.g., Do Not Provide Specifics for How the Study Will be Done in the Time Required Nor Why You Need the Funds Requested).*

This suggestion pertains to the many details necessary to (1) make what you propose believable, (2) convince the reviewers that you have thought things through, and (3) convince them that you are the type of scientist who is capable of making appropriate changes if problems are encountered and that you have enough experience to accomplish what you propose.

Some crafty investigators provide a time line showing how each phase of their work can be accomplished in the funding period of the grant. Some even describe a greater scope of work on a longer time line and then show how the project proposed fits into the "Grand Plan." Bad idea. Something like this could get a proposal funded just because the investigator has a "broad view."

On some of my proposals which have failed miserably and *gotten funded*, I have spent hours describing in a budget justification how I arrived at each and every cost. Things are itemized in detail. Staff are accounted for in terms of the hours per week they would devote to each task.

In a grant review session, an embarrassingly small amount of time is spent on the budget and concern is inversely related to the degree of detail provided by the investigator. Cuts are made correspondingly at warp speed. I literally have seen budgets reduced by 50% or more in less than 10 minutes. So if you are worried and think you may fail and get the grant, provide little justification for the budget request and then perhaps you will get the next best thing: not enough money to do the work.

SUGGESTION TEN: *Argue with the Reviewers' Process, Judgment, and Decision if You are Given a Second Chance and Asked for Clarification.*

Occasionally (make that very rarely) a conflict will arise in a review and a decision which might result in a nonfundable score will be tabled pending clarification by the investigator. A corollary to this is when an investigator requests a reconsideration of an unfavorable decision. In both cases, potential grantees in search of "not getting a research grant" can almost guarantee lasting animosity of the review group by attacking them. One investigator would most certainly have gotten a sizable addition to a multi-project application if in the reply letter he/she had agreed with the reviewers' concerns, argued his/her point on academic grounds (to save face), but in the end agreed to *attempt* to address reviewers' concerns in his/her subsequent work if funded. Indeed, rules on the conduct of research after an award is made are very flexible; the applicant could have changed aspects of the research if necessary after the award was made. Changes in the research plan after the award would not be considered by the reviewers, as their role ends with an assignment of a score. In this particular case, the applicant attacked the integrity of the primary reviewer and the review process. The primary reviewer merely had to read to the committee the investigator's letter and say, "Gee, I don't understand why this investigator chose this approach in the reply." The committee did the rest--no money.

CONCLUSION: If you fail and somehow get funded, do not despair as the agency will expect you to submit another grant application (called a competitive renewal) at the end of the funding period so that your work can continue and thus you will have another chance to use these 10 suggestions on How *Not* to Get a Research Grant.

Helpful suggestions on these suggestions by Drs. Joseph Brady and Nancy Ator are gratefully acknowledged. I tried to solicit comments from less successful grant writers, but they declined fearing that such assistance would be a behavioral trap.

COMPUTER SOFTWARE FOR STIMULUS CONTROL RESEARCH WITH MACINTOSH COMPUTERS

William V. Dube

E. K. Shriver Center

The Shriver Center Behavioral Sciences Division research program includes several projects examining issues in stimulus control and discrimination learning, with emphasis on the problems encountered by individuals with mental retardation and autism (Stromer et al., 1991). Over the past 6 years, many of our studies have used a portable microcomputer-based apparatus. A chapter published a few years ago described our initial development efforts from 1984 to 1988 (Dube & McIlvane, 1989). This note will update that chapter.

Apparatus

We use Apple Macintosh computers fitted with Microtouch[1] touch-sensitive screens. (The software can also be run with the mouse or keyboard.) Microtouch currently manufactures a line of touchscreens that use the Apple Desktop Bus (ADB) input protocol. ADB touchscreens are relatively easy to install in newer Macs like the Classic and SE. Installation in older pre-ADB Macs like the 512E or Plus is more difficult because it requires adapting a touchscreen intended for PC-type computers. If appropriate technical support is available, however, the lower cost of used older Macs can make them attractive alternatives to new equipment.

Software Overview

This note will describe two general-purpose programs that run a variety of discrete-trial simple- and conditional-discrimination procedures. Both programs are written in THINK Pascal and compiled as Macintosh applications.

The "MTS" program includes simple simultaneous discrimination and simultaneous and delayed matching-to-sample procedures. Sample stimuli appear in the center of the screen and up to four comparison stimuli appear in the corners; see Dube, Kledaras, Iennaco, Stoddard, & McIlvane (1990), or McIlvane, Dube, Kledaras, Iennaco, and Stoddard (1990) for illustrations. The MTS program implements several stimulus-control shaping procedures (e.g., fading) and optional procedural variations (e.g., delayed sample presentation; McIlvane, Kledaras, Stoddard, & Dube, 1990) that are useful with less-capable experimental subjects.

The "SPELL" program includes standard and constructed-response matching-to-sample procedures. Sample stimuli appear at the top of the screen and comparison stimuli appear at the bottom. In standard matching, up to three comparisons can be displayed side-by-side. In constructed-response matching, the comparison area can display up to 10 stimuli that are selected sequentially. For example, given a sample picture of a cat, the comparison display could include the individual letters A, O, T, G, D, and C. As the subject touches the "C," then the "A," and finally the "T," the letters move across the screen to the sample area (below the picture) to spell the word CAT; see Dube, McDonald, McIlvane, and Mackay (1991), or Stromer and Mackay (1990) for illustrations. The SPELL program is intended for more capable subjects who have established matching-to-sample repertoires.

Strengths

Flexible, automated control of sessions. The software uses two types of input files; both types are plain text files prepared with a text editor. The lower-level file, a *stimulus file*, is analogous to a tray of slides; it contains trial-by-trial specifications for the stimulus displays for a block of trials (up to 120). A session can include any number of stimulus files. The higher-level *control file* specifies the events for one entire session. A control file lists the stimulus files that are to be run, and it may also include commands that specify or change independent variables at various points in the session.

Control file commands also control branching among stimulus files contingently upon the subject's performance. For example, a control file may be set up to do the following: The session begins with a block of 36 trials reviewing a previously learned performance; if accuracy is 90% or better, the program branches to a block of trials that teaches a new performance with a fading procedure; if accuracy is between 75% and 90%, the program repeats the review block; if accuracy is below 75%, the program branches to a remedial block.

[1]MicroTouch, 55 Johnspin Road, Wilmington, MA 01887

The two-level system of input files, with the command language in the higher-level file, allows the experimenter to program entire experimental sessions in advance.

Ongoing evaluation of performance. The software can evaluate several aspects of the subject's performance after every trial, and it can terminate the current block of trials when specified conditions are met. These conditions include accuracy criteria, numbers of errors, a specified step of a fading procedure (see below), or combinations of the three. When coupled with the branching options outlined above, this feature allows automated implementation of programmed instruction.

Stimulus-control shaping procedures. The software implements three types of fading procedures (all are described in Dube, Iennaco, Rocco, Kledaras, & McIlvane, in press). One gradually increases the intensity of S- stimuli by displaying them in 16 graded shades of gray. A second procedure gradually changes the relative sizes of S+ and S- stimuli over 16 steps. The third gradually increases the duration of S- display. All automatically advance and back up conditionally upon the subject's performance. A fourth type of procedure involves gradual changes in stimulus topography (Zygmont, Lazar, Dube, & McIlvane, 1992). Implementation of this procedure requires the preparation of special sets of stimuli (see below) and somewhat elaborate branching options in the control file.

Wide range of auditory stimuli. Both programs can present auditory samples prepared with Farallon's MacRecorder digitizer and SoundEdit software (about $150 for both). New auditory stimuli can be recorded by microphone or line level input. MacRecorder is very easy to use, and dictated spoken-word stimuli can be prepared in only a few minutes.

Weaknesses

Pre- and postsession time requirements. The high degree of control and flexibility described above comes at the cost of time spent in session preparation and data analysis.

The software cannot generate random sequences of trials. It is necessary to program every trial explicitly by preparing stimulus files. We make this task manageable by using sets of text-file templates. Each template is a block of trials balanced for position and distribution of stimuli. In order to avoid too frequent repetition of the same trial sequences, it is necessary to have several different templates for each type of trial block. Sessions are prepared by using a text editor to replace the generic stimulus tokens in templates with actual stimulus specifications.

Data analysis also can be time-consuming. The software does not provide descriptive statistics for sessions. The output files are plain text files, formatted as tables, showing trial-by-trial records of stimulus and response events. An optional tab- or comma-delimited output file format can be read by many spreadsheets (e.g., Microsoft Excel). This option will allow some automated data analysis via spreadsheet macros.

Limited visual stimuli. The range of visual stimuli is somewhat limited. The programs do not support color graphics (although they will run on Mac II computers). The current library of black-and-white stimuli includes letters, numbers, Greek letters (for classical equivalence research), about 200 arbitrary forms, and about 100 icon-type line drawings of common objects (see Dube et al., 1991 for examples of the latter). They are approximately 2 x 2 cm in the MTS program, and approximately 1 x 1 cm in the SPELL program. All visual stimuli are defined as characters in standard and specially prepared Macintosh fonts. In order to create new stimuli, it is necessary to create a new font or edit an existing font. We use the font editing tool in Apple's ResEdit program, but any commercial or public-domain font editor should do. Thus, the software is limited in that visual stimuli cannot be prepared with "draw" or "paint" programs or image scanners.

Software Availability

Software development was supported primarily by grants from NICHD. Our current policy is to provide a copy of the compiled software and documentation free of charge, under the conditions that it will be used for nonprofit research or educational purposes only, and that its source will be acknowledged appropriately in dissemination of research results.

Caveats. The potential user should understand that the software is not a commercial-quality program, but rather a tool whose evolution has been shaped by the specific requirements of our laboratory. The software has a few bugs, and it does not conform to Apple's Macintosh User Interface Guidelines. Unfortunately, our department does not have the resources to offer formal user support. If you get a copy, plan on investing some time fiddling with it.

The software requires 1mb RAM and System 6.0.5. Requests should include a blank disk and stamped return envelope. The documentation is in Microsoft Word 4.0; specify disk or print. Write to William V. Dube, Behavioral Sciences Division, E. K. Shriver Center, 200 Trapelo Road, Waltham, MA 02254.

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EAHB SIG ABSTRACTS: ABA 1991

Adult Humans' Choice Behavior in a Self-Control Paradigm: Independence of the Amount and Delay Ratios in the Generalized Matching Law

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Six experimentally naive adult human females chose between different relative amounts of reinforcement (points exchangeable for money) and between different relative delays of reinforcement, scheduled according to modified concurrent independent variable-interval reinforcement schedules. Each subject's data were tested for the combined effects on preference of the independent variables relative amount of reinforcement, relative delay of reinforcement, and their interaction. For all subjects the interaction term was not significant. In addition, the generalized matching law accounted for a significant amount of the variance in the choice behavior of each subject (between 82-98%). Human subjects' choices between different amounts and delays of reinforcement can be described by the generalized matching law.

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Variables Related to Self-Control in Children: Age, Language Ability, and Sensitivity to Reinforcer Amount and Delay

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Several researchers have found that self-control increases with age. The present two experiments examined two possible factors in this relationship: language ability and sensitivity to variation in amount and delay of reinforcement. In Experiment 1 the subjects, 21 3- to 5-year-old children, chose between different combinations of amount and delay of reinforcement scheduled according to a discrete-trials procedure. Language ability was also assessed. Experiment 2 used 18 3- to 7-year-old children and a procedure similar to that used in Experiment 1 except that sensitivity to both variation in amount and in delay of reinforcement was also assessed. The results suggest that, in support of previous studies, both self-control and language ability increase with age. The unique contributions to self-control of language ability and sensitivity to amount and delay of reinforcement are discussed.

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Self-reports of Matching-to-Sample Success: Bias, Sensitivity, and Base Rates of Target-Response Reinforcement

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The present research extends the investigation of humans' momentary self-reports using a modified matching-to-sample (MTS) task to generate target responses and a query about whether the last response met a conjunctive speed-accuracy contingency to generate Yes-No button-press self-reports. In previous experiments using these procedures the accuracy, bias, and sensitivity of self-reports all were related to (a) the complexity of MTS stimuli, and (b) the stringency of the time limit on MTS responses. Because similar effects obtained with two different methods of controlling the "difficulty" of the MTS target response, the critical feature of past manipulations probably was the base rate of target-response success (percent reinforced MTS responses) they produced. Described in this way, previous studies have examined self-reports under only a limited range of target-response base rates (usually, 50% or higher). In particular, this might be expected to influence report biases. The present investigation examined self-reports across a wider range of base rates, generated by manipulating the complexity of both sample and comparison stimuli in the MTS target task (in a few instances, the time limit on MTS responding also was manipulated). Accurate self-reports were intermittently reinforced. Six subjects participated in experimental conditions which produced MTS reinforcement rates typically in the range of 25% to 95%. Self-report accuracy and sensitivity were not systematically related to MTS base rates, but reporting biases were. When MTS success was frequent, biases were in the direction of reporting

success. When MTS success was infrequent, biases were less pronounced or, in some extreme cases, in the direction of reporting failure. Logarithmically-determined functions accounted for 75% to 99% of the variance in individual-subject functions. The concavity of the functions indicates a pervasive tendency to over-report target-response success that extends beyond a simple matching of self-reports to base rates.

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Self-reports of Matching-to-Sample Success: Further Characterization of a Bias for Reporting Reinforceable Responses

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The present research extends the investigation of humans' momentary self-reports using a modified matching-to-sample (MTS) task to generate target responses and a query about whether the last response met a conjunctive speed-accuracy contingency to generate Yes-No button-press self-reports. Previous experiments using these procedures have described a bias for reporting successful responses (that is, when self-reports are inaccurate, they typically occur in the form of reports of success following an unreinforceable response). Three studies further examined this tendency. Study 1 used probabilistically-generated consequences in a sham MTS task to show that subjects overestimated reinforcement frequency even when it was response-independent. Study 2 examined the acquisition of self-reports, both with and without extensive MTS pretraining, and found the bias to be generally present from the introduction of experimental tasks. Study 3 manipulated the wording of the self-report query and found no effects on the direction of bias. Taken together, the studies suggest that a bias for reporting successful responses, as previously reported, is pervasive and possibly extra-experimental in origin.

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"Self-knowledge" of Delayed Matching-to-Sample Performance: Differential Effects of the Number of Nonmatching Sample and Comparison Stimuli

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Ten undergraduates performed a delayed matching-to-sample (DMTS) task under moderate time pressure while the number of nonmatching sample and comparison stimulus items was manipulated across 8 experimental conditions. Trial-by-trial self-reports assessed "self-knowledge" of DMTS success (accurate and inaccurate self-reports produced intermittent point gains and losses, respectively). Although number of stimuli (both samples and comparisons) was inversely related to DMTS performance, only sample manipulation systematically influenced the overall accuracy of self-reports about that performance. A signal detection analysis (signal = successful DMTS response) revealed that reporting errors occurred almost exclusively as false alarms, producing a pervasive bias for reporting successful responses that was mediated by both sample and comparison manipulations. However, the two manipulations tended to have opposite effects on report sensitivity. The results prompt revision of a proposal that self-report accuracy is negatively related to DMTS reinforcement frequency (Critchfield & Perone, submitted). The results also help explain why sensitivity functions in another investigation did not covary systematically with DMTS reinforcement rates: By manipulating both sample- and comparison-stimulus number, the study intermingled effects of opposite direction (Critchfield, submitted).

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Auditory Stimulus Equivalence Classes

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This poster describes the extension of standard stimulus equivalence methodology (Sidman & Tailby, 1982) to the study of equivalence classes consisting solely of auditory stimuli. Stimuli were digitized arbitrary syllables (e.g., "cug," "vek") presented via a Macintosh microcomputer. Training and testing were conducted with a two-choice auditory successive conditional discrimination procedure: On each trial, samples and comparisons were presented successively. As each comparison was presented, a response location (a rectangle) appeared on the computer screen. After all stimuli were presented, subjects selected one of the response locations (all comparisons were presented with each response location equally often). Data from normally capable adult subjects demonstrated (a) the formation of two three-member auditory equivalence classes, and (b) expansion of the classes to four members each.

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Rule-Governed Behavior and Sensitivity to Remote Consequences

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When behavior is acquired through verbal instruction rather than by direct exposure to contingencies, it is often less sensitive, or sensitive in different ways, to the environment than is nonhuman behavior. The present research examined the role of instructions in adult humans' choices in situations with contrasting short-term and longer-term consequences. Instructions were sometimes consistent with, and sometimes inconsistent with, programmed contingencies, permitting a dissociation of rule-governed from contingency-governed behavior. Fourteen adult humans were given repeated choices between 2 time-based schedules of point delivery: a fixed-time 30-s (FT30) schedule and a progressive-time (PT) schedule that began at 0 s and increased by 5 s with each point delivered by that schedule. Choosing and then satisfying the requirements of either schedule produced a point (worth 4¢), followed by an immediate return to the choice phase. Under conditions labeled "Reset," choosing the FT not only produced a point, but also reset the PT schedule to its minimal value (0 s for some subjects, 15 s for others). These Reset conditions alternated with "No Reset" conditions, in which PT requirements were independent of FT choices. Some subjects were given no instructions regarding schedule requirements. Others were given instructions directing them to switch at the equality point--the point at which the PT and FT schedules were equivalent. This instruction was consistent with the programmed contingencies on the No Reset procedure, but inconsistent with those contingencies on the Reset procedure. Switching patterns of noninstructed subjects were clearly distinct under Reset and No Reset conditions, indicating sensitivity to programmed consequences. Six of nine instructed subjects followed inaccurate instructions at least half of the time, despite substantial losses in overall reinforcement rates. Thus, in contrast to previous reports of humans' maximizing overall reinforcement rates on choice procedures, the present experiment reveals circumstances under which sensitivity to direct consequences is overridden by verbal stimuli. When discrepancies between the instruction and programmed contingencies were greatest (i.e., when the PT schedule was reset to 0 s), compliance with the rule precluded effective contact with the contingencies. When this discrepancy narrowed (when the PT schedule was reset to 15 s), compliance sometimes occurred in spite of contact with those contingencies, suggesting that rule-following was controlled at least in part by socially mediated consequences.

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Exchange Delays and Impulsive Choice in Humans

Cloyd Hyten, Douglas Field, Gregory Madden,
Joel Greenspoon, and Kathryn Mistr
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When points/money are used as positive reinforcers in self-control choice procedures, there are at least three different delays involved: (1) POINTS DELAY--the delay to receipt of points; (2) EXCHANGE DELAY--the delay to the exchange of points for money; and (3) SPENDING DELAY--the delay to spending money to obtain other reinforcers. Previous studies that have obtained strong preferences for the self-control choice when using positive reinforcement have only manipulated points delays. We compared the common method of points delays with exchange delays in a discrete trials self-control choice procedure to see how preference was affected by both kinds of delays. All subjects showed exclusive self-control in the points delay condition, replicating findings obtained with similar procedures by previous researchers. Under the exchange delay condition, in which subjects received points immediately but exchanged them for money only later, all 6 subjects showed exclusive self-control at the minimum exchange delay (1 day), and 4 out of 6 showed exclusive (100%) impulsivity at either 3-week or 6-week exchange delays. This experiment demonstrated that exchange delays are a more powerful determinant of preference in self-control procedures than points delays. Exclusive impulsivity (rarely seen in human self-control studies) or exclusive self-control can be obtained by varying the size of the exchange delay using points/money as reinforcers.

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Synonymy and Stimulus Equivalence: Preliminary Investigations of Emergent Stimulus Control in Meaningful Responding

Andrew Mitchum, Scott Lawrence, & Richard Shull
The University of North Carolina at Greensboro

In a traditional memory design, one observes a list of words and is asked after a delay to judge whether items of a second list are "old" or "new." Semantic memory researchers have observed that synonyms of words from the first list are more likely judged "old" than novel items. In behavior analytic terms, synonyms are alternative response forms occurring under very similar stimulus conditions and having very similar effects on the listener. Experiment 1 attempted to mimic natural English performance by "installing" an artificial vocabulary of nonsense syllables via stimulus equivalence procedures and including these stimuli in a semantic memory paradigm. In opposition to predictions, most subjects showed a reliable decrement in the probability of falsely recognizing synonym analogues. This correlated with verbal reports of awareness of semantic relationships involved. Experiment 2 involved manipulations of subjects' awareness of relevant semantic relationships and of their motivation to respond counter to false recognition predictions. It was found that only under circumstances of intense motivation and blatant exposure to synonym pairs could subjects reverse the usual effect. Implications for the nature of recognition performances and for a more adequate methodology are discussed.

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Conditioning Precurrent Behavior: Within Subject Replication and Post-Session Verbal Report

David Polson
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Behavior is called "precurrent" when it affects the conditions controlling another behavior. The present study examined a precurrent contingency in which one (precurrent) behavior raised the probability of reinforcement for another (current) behavior from .02 to .08 for 15 s. Data were presented for one subject, an undergraduate student, who was exposed to an ABABA sequence of conditions. With the precurrent contingency present (A), precurrent responding was conditioned and maintained; with the precurrent contingency absent

(B), precurrent responding extinguished. Current responding occurred at a high stable rate regardless of the condition. In the post-session verbal reports, this subject did not provide an approximation of the precurrent contingency until late in the experiment, prior to which precurrent responding had already been conditioned and extinguished twice and subsequent to which accurate reporting did not continue. Because precurrent behavior does not produce the reinforcer directly, precurrent contingencies may escape the attention of both the behaving organism and the behavior analyst. Future research should examine the "conditionability" of precurrent behavior, and once a precurrent operant is established, its resistance to disruptive operations (e.g., punishment, satiation, response effort, etc.). This research may produce insights into complex phenomena such as self-control, problem solving, and autoclitics.

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Lorazepam and Triazolam: A Comparison of Effects on the Repeated Acquisition and Performance of Response Sequences in Humans

C. R. Rush, S. T. Higgins, W. K. Bickel, and J. R. Hughes

The acute effects of triazolam (0-0.75 mg/70kg) and lorazepam (0-6 mg/70kg) were examined with eight healthy male volunteers responding under a two-component multiple schedule of repeated acquisition and performance of response sequences. In each component of the multiple schedule, subjects completed a different sequence of 10 responses in a predetermined order across three keys. In the acquisition component, subjects had to acquire a new sequence each session, while the response sequence always remained the same in the performance component. Each compound dose-dependently increased overall percent errors and reduced overall rates of responding in both components. Effects were selective across the components of the multiple schedule, however, in that lower doses were necessary to increase overall errors in the acquisition component than in the performance component. Triazolam and lorazepam differed in terms of onset and duration of effect, but not in magnitude of effect. These results suggest that the liability associated with the use and abuse of triazolam and lorazepam are comparable when considering absolute magnitude of effect. The liability associated with the use and abuse of triazolam could be considered less than that of lorazepam, however, if the duration of action is emphasized.

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Effects of Diazepam and Secobarbital on Choice of Social versus Monetary Reinforcement

C. R. Rush, S. T. Higgins, W. K. Bickel, and J. R. Hughes

Drugs of abuse (e.g., alcohol and *d*-amphetamine) increase choice of social over monetary reinforcement. The present project assessed the generality of this effect on choice behavior to secobarbital (100-200mg/70kg) and diazepam (10-20mg/70kg). Across a 69-min test session, subjects made an exclusive choice between a social or monetary option every 3 min. In the social option, subjects could converse with a partner, but no extra money was available. In the monetary option subjects could earn extra money by emitting speech monologues under a VI 60-s schedule of reinforcement. Eight pairs of normal volunteers participated in Experiment 1, while nine pairs of normal volunteers participated in Experiment 2. Experiment 1 assessed the effects of only secobarbital, while Experiment 2 assessed the effects of diazepam and secobarbital. In Experiment 1, choice for the social over monetary option increased as an orderly function of secobarbital dose. In Experiment 2, diazepam and secobarbital increased choice for the social versus monetary option, but the effects were not particularly robust. Nevertheless, they were in the same direction as observed with other drugs of abuse (e.g., alcohol and *d*-amphetamine). Considered together, the present results and earlier findings are consistent with the position that control of behavior by abused drugs may be strengthened by their ability to increase the control exerted by social, and perhaps, other reinforcers.

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Some Effects of Altering Prerequisites for Equivalence

Kathryn Saunders, Richard Saunders, Dean Williams, and Joseph Spradlin

The prerequisites for emergent matching performances indicative of equivalence were altered under two different conditions. Six 8- to 13-year-old children participated, three in each condition. Both conditions began with the training and maintenance of 4 two-choice conditional discriminations (AB, BC, CD, and DE; e.g., D1->E1, D2->E2). In the "test-before-reverse" (TBR) condition, tests for all emergent relations involving the E stimuli were positive (AE, BE, etc.). In the "reverse-before-test" (RBT) condition, tests were not conducted. Next, under both conditions, reversed DE relations (D1->E2, D2->E1) were trained with feedback. In subsequent tests, the responses of two Ss in the TBR condition remained in accordance with original training and those of the third gradually corresponded to current training. In the RBT condition, the test trial responses of two subjects were in accordance with the DE reversal and the third subject responded inconsistently (at chance levels). These and previously reported findings indicate that the maintenance of emergent performances need not depend on current performance of prerequisites. However, prerequisite-reversal procedures, including the present ones, have produced inconsistent results across subjects. Because such procedures provide the basis for more than one set of performances on test trials (i.e., a stimulus can be in more than one class), the specific performance demonstrated is influenced by uncontrolled variables. Studies of contextual control suggest one way of gaining control over these outcomes.

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Discrimination Among Members of Stimulus Classes on the Basis of Nodal Distance

Joseph E. Spradlin and Richard R. Saunders

Three experiments were conducted in which 8 verbally sophisticated subjects were taught four, two-choice conditional discriminations [A-B, B-C, C-D, D-E]. When the subjects were able to perform 100% correctly on these four conditional discriminations without differential reinforcement during a session in which they were intermixed, they were presented massed test trials for equivalence class, and massed test trials for discrimination among stimuli from the same class on the basis of nodal distance between the sample and comparison stimuli. All 8 subjects demonstrated equivalence classes and 6 of 8 subjects selected comparison stimuli separated from the sample stimuli by the fewest nodes on over 50% of the nodal discrimination test trials. Percent correct data generally were consistent with the nodal distance hypothesis. However, latency data were generally inconsistent with the nodal distance hypothesis.

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Vietz, D.
van Haaren, F.
Wanchisen, B.
Woods, P. A.
Wulfert, E.
Wylie, A. M.

*We are publishing this list because some members have asked when they last made a contribution to the SIG. Also, we'd like to be sure our records are correct. Please let us know if they are not.

**denotes an exceptional contribution

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EQUIPMENT EXCHANGE
FREE CONTROL COMPUTING EQUIPMENT TO A GOOD EAHB HOME

The Behavioral Sciences division of the E. K. Shriver Center is phasing out DEC PDP-11 computing equipment from its research operations. As a consequence, there are now several spare processors (PDP 11/02's and 11/23's), 64k memory cards, RX01 and RX02 floppy disk controllers, serial and parallel interfaces, clocks, BDV11's, and other cards. All have been recently functional and probably remain so. We also have a spectacular number of BRS 200 series logic cards. Interested parties can contact Richard Serna, Shriver Center, 200 Trapelo Road, Waltham, MA (617) 642-0027.

NEW IDEAS IN THE ANALYSIS OF HUMAN BEHAVIOR: WINNERS OF THE 1992 EAHB-SIG STUDENT PAPER AWARDS

Thomas S. Critchfield
Auburn University

Five papers received awards in the 1991-92 Student Paper Competition sponsored by the EAHB-SIG. Awards were based on blind review by established members of the special interest group. The winning papers appeared to reflect a state of growth and good health for the experimental analysis of human behavior. Four of five winning papers represented departments that have not previously produced winners, and the papers receiving acknowledgement addressed diverse topics. **Karen G. Augustson** (University of New Mexico; sponsor Michael J. Dougher) reviewed the traditional literature on category learning in children and found possible functional parallels with recent work on stimulus equivalence. Her paper provided a conceptual argument for the use of stimulus equivalence as a framework to explain and extend the literature on category learning. **David Polson** (University of Victoria; sponsor J. Parsons) suggested a behavioral interpretation of precurrent operants, focussing on the concern that recent applied studies have shown precurrent operants to influence the probability of other responses, but have not directly examined the sensitivity of precurrent behavior to its consequences. His report included the results of a preliminary test of the interpretation using free-operant methods and unambiguous, readily measurable responses. **Kathleen M. Drake** (University of Kansas; sponsor Richard R. Saunders) described two experiments examining the formation and merger of equivalence classes in children. The studies addressed the question of whether differential response requirements can play a role in the emergence of untrained relations. **Gregory J. Madden** (University of North Texas; sponsors Cloyd Hyten and Sigrid S. Glenn) considered the proposition that operant conditioning and natural selection are analogous, both at the surface level and at the level of specific subprocesses. His conceptual analysis focused on whether processes such as reinforcement, extinction, and punishment, have true analogues in biological evolution. A fifth winner, **Michael Schlund** (University of Wisconsin-Milwaukee; sponsor Alan Baron), reported two experiments addressing the question of whether response modality is a factor in human performance on temporal schedules of reinforcement. The studies were based on the assumption that vocal and manual responses may have separate histories and dynamics that could interact differently with temporal schedule requirements.

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EAHB SIG ABA EVENTS

The EAHB SIG ANNUAL MEETING at ABA will be held on Tuesday, May 26, from 10:00 to 1100 A.M. in "Pacific H."

The EAHB SIG STUDENT PAPER AWARD SYMPOSIUM (see above) will be held on Wednesday, May 27, from 9:00 to 11:00 A.M. in "Pacific O."

EAHB SIG MEMBERSHIP INFORMATION

You can join the SIG or renew your membership by completing the form below and sending it along with a check. Current members: Check your MAILING LABEL, it shows the year through which your dues are paid.

DUES are $6 U.S. funds. Despite rising costs, the SIG is able to hold dues at a low level because (a) administrative costs are subsidized by the Parsons Research Center, University of Kansas, and (b) most of our members have generously added a voluntary contribution of $2 or more to their dues. If you can afford an extra $2, please send it--the SIG will put it to good use.

ADDRESS all correspondence to: Kate Saunders, EAHB Bulletin, Parsons Research Center, 2601 Gabriel, P.O. Box 738, Parsons, KS 67357.

Circle: New Member New Address Renewal

Amount enclosed (U.S. funds, payable to EAHB SIG): $6 $8 $10 $12 $____
Payment for: 1991 1992 1993

Name _____

If you are a new member, or have a new address, complete the following:

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