UNIVERSITY OF MANITOBA

LEARNED HELPLESSNESS IN A SOCIAL SITUATION

BY

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A Dissertation submitted to the Faculty of
Graduate Studies in partial fulfillment for the
Degree of Master of Arts

Winnipeg, Manitoba
October 1978

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MASTER OF ARTS

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Abstract

The present experiment examined human response to two aspects of an uncontrollable enironment: (1) instructions relating to the potential for control, and (2) position in a two-person situation. All participant pairs were exposed to aversive noise. They were given one of four sets of instructions manipulating their expectations that they could reduce the amount of noise. Two of these sets of instructions contained no suggestion that the noise was subject to the participants' control, so these conditions functioned as control groups. The instructions to the experimental groups led participants to believe that they could reduce their own or their and their partner's amount of noise by solving puzzles. These puzzles were, in fact, largely insoluble. Position in a two-person situation was manipulated by giving feedback to half the participants that their partners were more successful than they. The subsequent test task consisted of proofreading a manuscript.

The findings provide only weak evidence for the existence of an interference effect (i. e., learned helplessness) in humans. Analysis of the manipulation checks, however, suggests

that the absence of debilitated performance was due to participants' failure to believe instructions. There was no support for the hypothesis that social situation influences the saliency of uncontrollability manipulations.

Introduction

The "learned helplessness hypothesis" has been proposed to account for the behaviour shown by animals who have been exposed to events beyond their control (Seligman, Maier, & Solomon, 1971). According to this hypothesis, the organism learns that its responding is independent of its reinforcement and comes to show motivational, cognitive, and affective deficits even in subsequent performance situations where events are controllable. This proactive interference with responding in animals has been shown to be due to the experience of uncontrollability and not to confounding factors (Maier, 1970; Seligman & Maier, 1967).

Research on the effects of uncontrollability in humans has attempted to both duplicate the animal procedures and adapt the procedures to humans. The ethical considerations in research with humans prevent researchers from employing conditions of true uncontrollability (since subjects must be allowed to leave the experiment). Further, experimenters cannot control the subjects' pre-experiment experience, which has been shown to affect susceptibility to the interference effect (Seligman & Groves, 1970; Seligman & Maier, 1967). Researchers are also ethically prevented from producing the severe, non-reversible behavioral deficits observed in animals. Researchers employing human subjects must thus engineer uncontrollability conditions so that any subsequent effect on performance is slight and can be dissipated by debriefing.

Given the ethical restrictions in human research, it is hardly surprising that studies in this area have shown mixed evidence for the existence of the interference effect in humans. The strongest evidence for the existence of the effect in humans was provided by Hiroto and Seligman (1975). These investigators employed two different uncontrollable pretreatments. The first was an instrumental noise-avoidance task (a human adaptation of the animal uncontrollability pretreatment procedure). The second was a cognitive task involving visual discrimination problems which were insoluble. Both of these pretreatments produced performance deficits on a subsequent task. Similar results were reported by Glass and Singer (1972, pp. 80-90 & 110-120), Miller and Seligman (1975), and Thornton and Powell (1974). There is an additional group of studies which found response deficits in support of the learned helplessness hypothesis, but whose designs failed to employ necessary control groups (Benson & Kennelly, 1976; Dweck & Repucci, 1973; Dyck, Breen, & Eckelman, Note 1; Glass, Reim, & Singer, 1971; Glass, Singer & Friedman, 1969; Hiroto, 1974; Roth & Kubal, 1975). These studies showed deficits in the uncontrollability groups, but either

(1) assessed these deficits by comparing to groups experiencing controllable--instead of neutral--conditions, or (2) failed to include a control group which was yoked to the uncontrollability group for amount of aversive stimulation.

There are, however, other studies which have shown facilitated performance in humans previously exposed to uncontrollability (Roth & Bootzin, 1974; Roth & Kubal, 1975; Thornton & Jacobs, 1972; Wortman, Panciera, Shusterman, & Hibscher, 1976). Wortman and Brehm (1975) have attempted to unify these findings by proposing that humans initially react to uncontrollability with heightened performance and become debilitated only upon continued exposure to the uncontrollable situation. This account was supported by Roth and Kubal's (1975) finding that small amounts of uncontrollability pretreatment resulted in facilitated performance, while large amounts resulted in debilitation.

A more general explanation for the above mixed findings is that factors which increase the saliency of the pretreatment manipulations also increase the probability that humans will exhibit debilitated subsequent performance. There are several manipulations used in human uncontrollability research which may function to increase pretreatment saliency, while remaining within the ethical bounds of human research. Such manipulations have generally increased saliency by inducing subjects to believe that the events in the pretreatment are potentially controllable, so that subjects attribute the failure to exercise control to themselves.[1] In the existing human uncontrollability literature, two classes of these manipulations have been employed: (1) instructions concerning the nature of the pretreatment task, and (2) introduction of a second person to the procedure.

Instructions

A nearly universal means of maximizing pretreatment salience has been employment of inctructions telling the experimental uncontrollability subjects that the pretreatment stimuli are potentially controllable when, in fact, they are not controllable. In several important studies of uncontrollability

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1. The hallmark of learned helplessness is inappropriate generalization from an uncontrollable situation to another situation which is controllable. In many studies, the test task situation is similar to the pretreatment situation in one or more respects. Subjects who experience uncontrollability and go on to exhibit debilitated performance in a second situation not clearly different from the first, may, in fact, be making an appropriate generalization. Such experimental procedures encourage the subject to account for his/her pretreatment experience by factors external to him/herself. While the experimental results may show performance debilitation, this deficit is not necessarily supportive of the learned helplessness hypothesis (Roth & Bootzin, 1974; Roth & Kubal, 1975; Wortman & Brehm, 1975). Experimental manipulations which encourage the subject to hold himself or herself responsible for failure to control the environment are thus likely to decrease the probability of "pseudodebilitation" and increase the probability of a true interference effect.

(Glass & Singer, 1972, pp. 80-90 & 110-120; Hiroto & Seligman, 1975) the group so instructed showed more debilitated performance than comparison groups which received the same objective uncontrollability (i.e., randomly presented aversive noise or electric shock) without the expectation of control instructions. This is a purely cognitive effect: the test task debilitation must be seen as a result, not of uncontrollable events *per se*, but as a result of uncontrollable events in the context of belief by subjects that control is possible.

A second means of instructionally manipulating subjects' perceptions of the pretreatment task involves presentation of the task as a measure of academic ability. Such instructions encourage college student subjects to attribute their task performance to an aspect of themselves which is presumably very important to them. Roth and Kubal (1975) found that people perceiving the pretreatment task as a measure of their academic ability showed debilitation relative to those subjects perceiving the task as a measure of "concept formation". Again, the experimental effect shown was a result of the subjective meaning of the failure to exercise control.

Presence of a second person

Another method of maximizing the saliency of uncontrollability manipulations involves inclusion of a second person in the experiment.

This person is presented as able to exercise control over the pretreatment events. Glass, Reim, and Singer (1971) found that subjects exposed to such a perceived-control peer showed debilitation relative to both subjects to whom the peer appeared to be equally powerless and to subjects who participated in the experiment alone. Wortman et al. (1976) found that presence of a perceived-control peer induced subjects to attribute their pretreatment task failure to themselves, but also produced facilitated test task performance. Dyck, Vallentyne and Breen (in press) also used a perceived-control peer to manipulate subjects' attributions for performance. They found an interactive relationship between length of pretreatment and the attribution manipulations.

Social psychological theory and research provides a basis for expecting that inclusion of a second person would affect both causal attributions and subsequent performance. Festinger's (1950, 1954) social comparison theory states that people use others performances to evaluate their own level of success. Several studies have shown that people seek to compare their performances with those of others (Hakmiller, 1962, 1966; Gordon, 1966; Thornton & Arrowood, 1966; Sheeler, 1966). Additionally, Vreven and Nuttin (1976) have shown that, at least for females, comparison with more successful others tends to lower subjects estimation of their own performance. Festinger's theory also predicts that maximum social comparison when the second person is perceived as a peer, as is the case in the aforementioned uncontrollability studies.

While social comparison theory states that people evaluate their performance by comparison with others, attribution theory is specific concerning the consequences of such comparison. It predicts that (1) knowledge that one's performance is different from others' leads one to hold oneself responsible for one's performance (Frieze & Weiner, 1971; Weiner & Kukla, 1970), and (2) belief in one's low ability is associated with decrements in responding (Weiner, Frieze, Kukla, Reed, Rest, & Rosenbaum, 1971; Weiner, Heckhausen, Meyer, & Cook, 1972). The foregoing two predictions do not, however, posit a direct link between presence of a second perceived-control person and subsequent debilitation. The subject could hold his/her low effort rather than lack of ability responsible for his/her performance. Dweck and Repucci (1973) demonstrated that attributions to low effort result in persistent performance, while attributions to low ability result in impaired performances. The Wortman et al. (1976) findings may have been due to subjects' causal attributions reflecting low effort rather than low ability.

Equity theory (Homans, 1961) holds that, in any human system, individuals seek to equalize their ratios of inputs to rewards. The subject in an uncontrollability experiment, presented with a second person who has more control than he/she does, has limited means of restoring equity. The subject can either (1) decrease his/her own inputs by expending less effort, or (2) make a self-derogating cognitive adjustment (such as adopting a belief in his/her own low ability level). If the aforementioned relationship between attributions of low ability and subsequent performance holds, then both of these alternatives would result in performance deficit.

These social psychological theories taken together, then, would predict that presence of a perceived-control peer would lead subjects to view their own performances in terms of internal factors--low effort or low ability. Attributions to low effort would lead to facilitated performance, while attributions to low ability would lead to debilitated performance. A perceived-control peer would thus have an effect on subsequent performance, but the direction of the effect would be determined by the attributions made by the subject.

The present study

The present study examined two aspects of the uncontrollability pretreatment: (1) instructions relating to the potential controllability of aversive stimulation, and (2) position in a two-person situation. All participants in the experiment were exposed to aversive noise. They were given one of four sets of instructions manipuating their expectations that they could reduce the amount of noise. Half of the participants were led to believe that their partners' task performances were similar to their own. The other half were led to believe that their partners were advantaged in some respect. The precise nature of that advantage was determined by the set of instructions mentioned above.

The following hypotheses were advanced: (1) People perceiving the onset of noxious events to be a result of their own task failure will show subsequent impaired performance relative to people given no reason to expect the noxious events to be subject to their control. (2) People perceiving themselves to be disadvantaged compared to their partners will show subsequent impaired performance. (3) The degree of debilitation due to disadvantaged social position will become more marked with increasing levels of potential controllability. People's evaluations of their own and their partners' pretreatment task performance were expected to affect their subsequent performance beyond the aforementioned hypothesized effects of the experimental manipulations. No hypotheses were made regarding which of these evaluations would be most important or how each would affect subsequent task performance.

Method

Overview: The design of this study employed a 2 x 4 factorial arrangement, diagrammed in Table 1. The first (four-level) factor denoted degree of perceived controllability. The four conditions were: (1) aversive noise, (2) aversive noise plus insoluble problems, (3) aversive noise presented as controllable via solution of problems which were, in fact, insoluble, and (4) aversive noise presented as controllable by self or by partner. Since the first two conditions involved no instructions that the noise was potentially controllable, these conditions were intended to be neutral in controllability. Conditions (3) and (4) were designed to induce perceptions of uncontrollability in the face of expected control. The second (two-level) factor denoted position relative to partner (same as partner or disadvantaged compared to partner). The dependent measure in this experiment was subsequent performance on a proofreading task.

Participants Participants in this experiment were 88 female undergraduate students taking a course in introductory psychology. The two members of each participant dyad were recruited from different classes. Participants were excused from the experiment if they were personally acquainted with each other or if their first language was not English.

Table 1

Diagram of the Experimental Design

		Uncontrollability			
		Noise Only	Noise plus Problems	Uncontrollability for Own Noise	Uncontrollability for Partner's Noise
Social Position	Symetrical	NO-S	NP-S	UO-S	UP-S
	Disadvantaged	NO-D	NP-D	UO-D	UP-D

Apparatus: One set of stereo audio headphones was provided for each participant. These headphone sets were connected to a cassette tape recorder in a room separated from the experimental room by a one-way mirror. Tapes were played in the pretreatment to give instructions and then to deliver bursts of 3,000 Hz noise at 90 dB (measured at the earphones). To introduce the intersession and test phases, other instruction tapes were played. Then, in the test phase, participants were played a recording of a human voice reading a series of random numbers.

In addition to the headphone sets, each participant had an information console consisting of a metal box with two lights. The left light was labeled "Partner's Success", the right labeled "Your Success". There was a plywood barrier between the two participants to prevent them from seeing each other's consoles or activity and to discourage communication between them.

Procedure: All participants were run in pairs. When both participants had arrived at the experimental room, the experimenter introduced herself, conducted them into the laboratory room, and let them be seated in the two chairs. Booklets which included the experimental tasks had been placed inside the desk. Participants were told that they were about to participate in an experiment about the "effects of noise on your performance on some simple tasks" and were given a rationale about the need to know about the effects of noise in our urban environments. Both were then given a sample of the noise and allowed to withdraw from the experiment. If neither withdrew, they were told to put on their headphones and listen for further instructions.[2] The experimenter then left the room. Up until that time, she was blind to the uncontrollability condition to which the dyad would be assigned.

Pretreatment: From the adjoining room, the experimenter randomly selected and activated the instruction tape(s) for one of the four uncontrollability conditions. With the exception of the Noise Only condition (groups NO-S and NO-D), the instructions were identical for both members of the dyad. Instructions for participants in the Uncontrollability for Pair's Noise condition (groups UP-S and UP-D) were as follows:

> Just inside your desk is a small booklet. Please take it out and place it on the desk in front of you. This booklet contains some puzzles. Now, look at the first puzzle labeled "sample". Your task here is to trace over all the lines in the diagram with your pen. You may not trace over any line twice and you may not lift your pen from the paper. Try it now for the sample diagram (Pause). The puzzles you will be working when the experiment starts will be more difficult than the sample, but we

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2. Three potential participants withdraw from the experiment because they found the noise too aversive. Their partners were excused with them.

have found that they are well within the ability of 80 percent of the college students we have tested. Put down your pen now while you listen to the rest of the instructions.

As you know, from time to time in this experiment, a loud noise will come on over your headphones. All the noise bursts you hear will sound the same. Their intensity and duration are determined by our apparatus. However, the number of bursts you hear is determined by you and by your partner. When one of you succeeds at correctly solving a puzzle, that person's next two noise bursts will
(1) not occur, and her partner's next single noise burst will not occur. So, if you are the one to succeed at solving a puzzle, you will notice a long period of time during which there is no noise. If your partner succeeds at solving a puzzle, you will notice a moderate period of time where there is no noise. However, if both of you fail to successfully complete the puzzle, the delivery of noise is inevitable.

One of the things we are exploring in this study is the effect of experiencing these conditions with another person. Because we must standardize conditions in the experiment, we must limit the communication between you and your partner. This is why there is a barrier between you--so you cannot see each other during the experiment. We also ask that you not talk or signal to her at any time during the experiment. The console in front of you is there to give you information about how your partner is doing on the puzzles. When she correctly solves a puzzle, the orange light on your console will go on like this. (Demonstration) Similarly, when you correctly solve one of your puzzles, her orange light will go on. The other light, the one on the left side of your console, represents your own success at puzzle solution. Once I see that you have successfully solved a puzzle, I will signal my acknowledgement to you by turning on your

> light like this. (Demonstration) So, when you are successful, the light on the right of your console and your partner's orange light will both go on. When she is successful, the appropriate lights will go on on both your consoles in a similar manner.
>
> I know these instructions are long, so let me repeat the important parts. You will be hearing bursts of noise and working at puzzles. Your success at the puzzles will avoid the next two scheduled noise bursts for you. Your partner's success
> (2) will result in your hearing one less noise burst than you would otherwise hear. The lights on your console indicate puzzle-solving success. Now, when I say "begin", you may turn the page and work on the first diagram. Remember not to retrace any lines or lift your pen from the paper.

Participants in the Uncontrollability of Own Noise condition (groups UO-S and UO-D) received the same instructions except for the underlined passages. For these groups, those passages read:

> However, the number of bursts you hear will be determined only by you. If you succeed at correctly solving a puzzle, the next scheduled noise burst will not occur. In fact, after you solve a particular
> (1) puzzle, you will notice a period of time during which there is no noise. On the other hand, if you fail to successfully complete the puzzle, the delivery of noise is inevitable.
>
> (2) Your success at the puzzles will avoid the next scheduled noise.

Participants in the Noise plus Problems condition (groups NP-S and NP-D) received the same instructions as above except that the underlined passages were excluded. For the Noise Only condition, the social position factor was introduced directly in the instructions. Participants in the Noise Only--Symetrical group (NO-S) received the following instructions:

> As you know, from time to time in this experiment a loud noise will come on over your headphones. All the noise bursts you hear will sound the same. Their intensity and duration are determined by our apparatus.
>
> One of the things we are exploring in this study is the effect of experiencing these conditions with another person. Both you and your partner are receiving the same amount of noise. Because we must standardize conditions in the experiment, we must limit the communication between you and your partner. This is why there is a barrier between you--so you cannot see each other during the experiment. We also ask that you not talk or signal to her at any time during the experiment. The console in front of you is not part of this experiment, so please disregard it. Now, when I say "begin", the experiment will start.

Participants in the other Noise Only group (group NO-D) received the same instructions except the underlined sentence read "Your partner is receiving about half the noise bursts that you are receiving."

When the instructions were complete for both participants, the experimenter activated a second tape. This tape started with the spoken word "begin" followed by 18 bursts of noise. Following Glass and Singer's (1972, pp. 80-90) study, the length of each noise burst was 9 seconds, and the interburst intervals varied in number of seconds as follows: 23, 100, 40, 55, 60, 120, 35, 25, 175, 60, 110, 15, 25, 75, 30. This noise pattern took 20 minutes. The booklets provided contained geometric patterns adapted from Feather (1961). Some of the problems were identical to those used by Glass, Singer, and Friedman (1969), and others were constructed for this study. 85 percent of the puzzles were insoluble.

The lights on the feedback console served to introduce the social position factor for most groups. Participants in the Symetrical groups saw their partners' lights go on only as many times as they themselves succeeded at the puzzles. Participants in the Disadvantaged groups saw their partners' lights go on 12 more times than the number of their own puzzle successes.

Intersession: Upon completion of the pretreatment phase, participants were instructed to complete the intersession questionnaire (see Appendix A). This contained a space to record their grade 12 English marks and questions concerning: (1) estimation of success at the puzzles, (2) causal attributions of their performance, and (3) expectation of success on the next task. All questions appeared once for the participant's own performance and once for her partner's performance. Participants in the Noise Only condition did not complete any of the above questions.

Test phase: The test task consisted of the proofreading task used by Glass and Singer (1972), presented in Appendix B. Participants in all conditions received the following taped instructions:

> In this part of the experiment, you will be proofreading a manuscript. You are to read each page and carefully note errors in the text. When you detect an error, underline the word containing the mistake and put a checkmark in the margin. Do not try to correct the error. While you are proofreading, you will be listening to a series of numbers. When you hear the number "2", place a slash mark in the text. Now, turn the page of your booklet and look at the page labeled "sample". The error in the spelling of "would" is underlined and a checkmark as been placed in the margin. The person doing this task would have heard a "2" between the words "after" and "the" in the text. Your two tasks, then, are to note errors in the text and to mark your text with a slash whenever you hear a "2". You may begin when you hear the first number over your headphones.

After these instructions, the tape continued with a male voice reading a series of random digits. After ten minutes, the participants were instructed to stop reading, underline the last line they had read, and put down their pencils.

Postsession: The experimenter then re-entered the laboratory room. She thanked them for their participation and asked them to complete a second questionnaire (see Appendix C). Three of the questionnaire items were manipulations checks, and the remaining eight were questions exploring secondary experimental effects. Four of the latter group (questions 7 through 11) were questions adapted from Byrne's (1971) Interpersonal Judgment Scale. When both participants had completed their questionnaires. they were debriefed, thanked, and excused.

Results

Overview: Three families or sets of analyses were performed on the present data. The first family tested the effects of the experimental manipulations on the proofreading task. The cognitive states measured in the intersession questionnaire were treated as mediators of this relationship. Both univariate and multivariate techniques were employed as parallel tests of the main experimental hypotheses. The second family of analyses tested the effects of the group manipulations on the aforementioned cognitive variables, treating the latter as dependent variables. The third family of analyses tested the effects of the group manipulations on the variables measured in the postsession questionnaire.

Family 1--Test task performance

Univariate analysis: Univariate tests of effects of group manipulations and cognitive variables on proofreading performance was accomplished through the use of multiple regression procedures. As pointed out by Cohen (1968), multiple regression is a general procedure of which analysis of variance (or covariance) is a special case. When multiple regression is used to analyze effects of group membership, dummy variables are used to transform the categorical variables denoting group membership into integer values. Multiple regression format was employed for this analysis because it can accommodate tests of both group manipulations and mediating cognitive variables within a single prediction equation.

A participant's mark in her grade 12 English course had been obtained for use as a covariate of proofreading performance. Its hypothesized utility as a covariate was established by its high correlation with performance on the proofreading task ($\underline{r}$ = .33, $F_{1,86} = 10.45$; $p < .01$). Incorporation of English mark as an initial predictor of proofreading performance removed from the following analyses what would otherwise have been considered "error variance".[3]

Analysis of the effects of experimental manipulations was accomplished by the use of dummy variables to transform the categorical variables of group membership into the integer values needed for multiple regression. Of the

3. An important assumption of analysis of covariance is that the scores on the covariate variable are equal among the experimental groups. The test for homogeneity of regression coefficients yielded a non-significant $\underline{F}$-ratio ($\underline{F}_{7,72} = 1.28$; $p > .05$), so this assumption was met.

available coding techniques, orthogonal coding was used to incorporate planned comparisons into the tests of effects (Kerlinger & Pedhazur, 1973). For the eight-cell factorial design, seven coded vectors were required. The first vector represented the main effect of social position (i.e., it contrasted all Symetrical cells with all Disadvantaged cells). The test of the influence of this vector represents a test of Hypothesis 2--that social position affects task performance. The next three vectors, taken together, represent the main effect of uncontrollability. One of these vectors reflected the test of Hypothesis 1--that expectation of noise controllability followed by failure to control the noise would lead to debilitated performance. The last three vectors, taken together, represent interaction in the overall factorial. Tests of the vector reflecting interaction in the experimental cells (UO and UP cells) and the vector reflecting interaction in the comparison cells (NO and NP cells) relate to Hypothesis 3--that seriousness of the social disadvantage would be reflected in test task performance.

The effects due to the addition of single vectors to the prediction equation represent planned comparisons.

Vectors were also added to the equation in groups, so that the familiar factorial main effects and interaction could be tested. The results of these vector additions are shown in Table 2, and the cell means are shown in Table 3. None of the vectors representing planned comparisons caused any significant change in the multiple $\underline{R}^2$. Further, no significant effects were observed when vectors were introduced in the groupings typical of the more common ANCOVA analysis. The manipulations differentiating the groups do not appear to have resulted in differential performance on the proofreading task, and Hypotheses 1, 2, and 3 were not supported.

Testing the mediating properties of the cognitive variables involved casting them into the prediction equation along with the covariate and group membership vectors. Because the cognitive variables all referred to or were based on task performance, they were collected only on the six groups performing the pretreatment task.[4] The prediction equation including both the cognitive variables and the vectors representing the group manipulations could

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4. Several participants in these six groups failed to complete questions regarding their experimental partners. There were five such instances of missing data, each in a different group. These cases, along with another randomly chosen from the sixth group, were discarded, leaving ten observations per cell in the design.

Table 2

Effect of Experimental Manipulations
on Percentage of Errors Detected

(Eight group case)

Source of variation	F	df	p<	F	df	p<
Social position factor	.048	1,79	.8254	.048	1,79	.8254
UO cells vs. UP cells	.003	1,79	.9565			
NO cells vs. NP cells	.738	1,79	.3930	1.030	3,77	.3851
NO,NP cells vs. UO,UP cells	2.343	1,79	.1298			
Interaction in experimental cells	.138	1,79	.7113			
Interaction in comparison cells	.149	1,79	.7005	1.064	3,77	.3695
Complex interaction	2.951	1,79	.0897			

Overall significance of group manipulations: $F_{7,79} = .9031$; $p < .5085$

Table 3

Percentage of Errors Detected

Means, Corrected Means,* and Standard Deviations

		Uncontrollability			
		Noise Only	Noise plus Problems	Uncontrollability for Own Noise	Uncontrollability for Partner's Noise
Social Position	Symetrical	44.8	48.7	38.5	39.8
		<u>44.2</u>	<u>47.1</u>	<u>38.0</u>	<u>39.7</u>
		(12.1)	(12.7)	(7.0)	(22.0)
	Disadvantaged	43.4	45.5	44.1	44.0
		<u>42.7</u>	<u>44.6</u>	<u>43.5</u>	<u>43.4</u>
		(15.8)	(9.8)	(10.7)	(15.0)

Total N = 88

Corrected means underlined, standard deviations in parentheses.

*The corrected means are the hypothetical values of the group means if all groups had had identical values on the covariate variable.

therefore be based only on these six groups. The previous calculations on the effects of the group manipulations were therefore repeated for these six groups.

The results of the analysis of group manipulations in the 2 x 3 design were substantially the same as those for the 2 x 4 design. The correlation between the covariate and the proofreading measure remained .33. The only important shift in the tests of significance of the group manipulations occurred in the test of the uncontrollability manipulation (c.f. Table 4). In the analysis of the 2 x 3 design, the test of the vector representing this contrast approached significance ($F_{1,53} = 3.422$; $p < .07$). This result constitutes weak evidence for Hypothesis 1 concerning the debilitating effects of uncontrollability given expectation of control.

The cognitive variables which were added to the prediction equation were those assessed by the intersession questionnaire. These variables were perceptions of own and partner's success at the puzzle task, causal attributions for self and partner, and expectations for success at a subsequent task for self and partner. Because the experimental manipulations were designed to produce perceptions of contrast between self and partner for half of the participants, the scores on variables requiring assessment of partner were

Table 4

Effect of Experimental Manipulations
on Percentage of Errors Detected

(Six group case)

Source of variation	F	df	p<	F	df	p<
Social position factor	.066	1,53	.7982	.066	1,53	.7982
UO cells vs. UP cells	.142	1,53	.7078	1.748	2,53	.1918
NP vs. UO,UP cells	3.422	1,53	.0699			
Interaction in experimental cells	.163	1,53	.6880	1.259	2,53	.2923
Complex interaction	2.353	1,53	.1310			

Overall significance of group manipulations: $F_{5,53} = 1.2193$; $p < .3130$

transformed into difference scores by subtracting them from their corresponding self ratings.

The ten cognitive variables were allowed to enter the prediction equation according to their order of importance in the data.[5] Among the ten, the first variable to enter the equation was the causal attribution of one's own ability (i.e., the proportion of the total responsibility for performance that could be attributed to ability). This variable did not contribute significantly to the prediction of test task performance ($F_{1,47} = 1.65$; $p > .05$). Since this variable was selected from the set of ten because it would effect the greatest change in the multiple R^2, all of the subsequently entered variables showed even less significant contributions to the prediction equation. Cognitive states related to the pretreatment task thus did not appear to have any relationship with the subsequent performance on the test task.

Multivariate analysis: The foregoing multiple regression analysis employed a single criterion as a measure of test task performance. A parallel multivariate analysis was performed in order to use three aspects of proofreading performance as dependent measures. The variables used were (1) the percentage

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5. "Importance" in this context was the "F to enter" criterion: the change in the multiple R^2 that would be effected by entry of a variable into the equation.

of detected manuscript errors (the variable used in the foregoing regression analysis), (2) the proportion of mistakenly noted errors, and (3) the total number of pages of the manuscript that were read.

The utility of the covariate was maintained with the multivariate composite ($\underline{F}_{3,77} = 4.039$; $p < .01$) in spite of its low correlation with the second dependent measure ($\underline{r} = .036$). The MANCOVA assumption of homogeneity of repression hyperplanes yielded a non-significant $\underline{F}$-statistic ($\underline{F}_{21,201} = .8354$; $p > .05$). English mark was therefore used as a covariate in the multivariate analysis.

The results of the multivariate analysis on the group manipulations are presented in Table 5, and the cell means for variables (2) and (3) appear in Table 6. Again, both planned comparisons and factorial main effects and interaction were tested. The findings of the multivariate analysis of experimental manipulations parallel the findings of the foregoing univariate analysis. There were no group differences on test task performance, and Hypotheses 1, 2, and 3 were again not supported.

The mediating properties of the cognitive variables were tested by ascertaining their relationship to the variables representing proofreading performance. The canonical correlation

Table 5

Effect of Experimental Manipulations

on Composite Proofreading Performance

Source of variation	F	df	$p <$	F	df	$p <$
Social position factor	.460	3,77	.711	.460	3,77	.711
UO cells vs. UP cells	.313	3,77	.816			
NO cells vs. NP cells	1.173	3,77	.326	.781	9,187	.635
NO,NP cells vs. UO,UP cells	.855	3,77	.468			
Interaction in experimental cells	.779	3,77	.509			
Interaction in comparison cells	.579	3,77	.631	.877	9,187	.547
Complex interaction	1.290	3,77	.284			

All F statistics based upon Rao's approximation of Wilks' Λ

Table 6

False Detections and Number of Pages Read

Means, Corrected Means, and Standard Deviations

		Uncontrollability							
		Noise Only		Noise plus Problems		Uncontrollability for Own Noise		Uncontrollability for Partner's Noise	
		* FD	**NPR	FD	NPR	FD	NPR	FD	NPR
Social Position	Symetrical	.797	4.591	1.912	4.227	1.027	5.273	.997	4.318
		.802	4.569	1.925	4.221	1.023	5.275	.996	4.321
		(.394)	(1.578)	(2.253)	(1.367)	(.759)	(1.232)	(1.162)	(1.401)
	Disadvantaged	.660	4.864	.847	4.773	1.039	4.591	1.216	4.727
		.654	4.889	.839	4.777	1.044	4.572	1.211	4.750
		(.775)	(1.380)	(.634)	(1.587)	(.788)	(1.446)	(1.414)	(1.367)

Total N = 88

Corrected means underlined, standard deviations in parentheses.

* False detections
**Number of pages read

analysis relating cognitive mediators with proofreading performance showed no significant relationship between the two sets of variables (Rao's $F_{30,138.63} = .7382$; $p < .50$). Three canonical variates were extracted which, taken together, accounted for only 13% of the variance in the dependent variable composite. Because neither the set of group membership variables nor the set of cognitive variables demonstrated a significant relationship with test task performance, no attempt was made to cast both sets of predictors into a single multivariate equation.

The first family of analyses relating group manipulations to proofreading performance showed that the experimental manipulations had little effect on subsequent performance. The cognitive factors which were hypothesized to mediate this relationship also had no effect on proofreading performance.

Family 2--Intersession questionnaire

In the foregoing family of analyses, the intersession questionnaire responses were analyzed as predictors of proofreading performance. Their relationship to the proofreading measure was directly tested, but their relationship to the group manipulations was only indirectly tested (via the simultaneous roles of both as predictors of proofreading performance). A multivariate analysis of variance was therefore employed to directly test the influence of the group manipulations on the intersession questionnaire responses. This analysis is not independent of the foregoing analyses predicting test task performance. Since the degree of redundancy is unknown, the true statistical probabilities of a Type I error cannot be assessed. Interpretation of the following results is therefore undertaken with caution.

The results of the MANOVA relating group manipulations to the cognitive variables (i.e., perceptions of success, causal attributions, and expectancies of future success) are presented in Table 7, and the group means of these variables are shown in Table 8. The main effect due to the social position factor was highly significant ($\underline{F}_{10,45} = 6.195$; $p < .0001$). Neither the main effect due to degree of uncontrollability nor the interaction showed significant effects. Table 9 presents the standardized weights in the social position discriminant function. The variables weighted most heavily in the social position discriminant function were those reflecting (1) difference between own success and partner's success, (2) own success, and (3) own task difficulty. The univariate test (separate ANOVA results on each variable are also presented in Table 9) is consistent with the discriminant function weights with respect to the large

Table 7

Effect of Experimental Manipulations
on Intersession Questionnaire Items

(MANOVA)

Source of variation	F	df	$p <$
Social position factor	6.195	10,45	.0001
Uncontrollability factor	1.064	20,90	.4009
Interaction	.662	20,90	.8527

All F statistics bsed upon Rao's approximation of Wilks' λ.

Table 8

Intersession Questionnaire Variables

Means and Standard Deviations

Social Position		Uncontrollability: Noise plus Problems	Uncontrollability for Own Noise	Uncontrollability for Partner's Noise
Symetrical	Success--own	2.9 (1.45)	2.8 (1.62)	3.7 (1.25)
	Success--difference	- .1 (.32)	-.2 (.79)	- .2 (1.55)
	Ability--own	8.5 (3.41)	7.9 (4.48)	7.5 (3.66)
	Ability--difference	- .6 (1.58)	- .6 (3.44)	- .4 (1.90)
	Effort--own	9.0 (2.91)	6.8 (3.99)	9.4 (5.29)
	Effort--difference	- .5 (1.90)	- .1 (2.47)	- .4 (1.58)
	Task difficulty--own	11.0 (3.16)	12.5 (9.07)	12.1 (6.83)
	Task difficulty--diff.	.6 (1.78)	-3.3 (8.81)	.9 (1.85)
	Expectation--own	3.7 (.67)	3.7 (1.16)	4.2 (.63)
	Expectation--difference	- .2 (.42)	- .4 (.96)	- .1 (.32)
Disadvantaged	Success--own	3.1 (1.29)	2.3 (.94)	2.3 (1.16)
	Success--difference	-2.20(1.62)	-2.8 (1.81)	-3.1 (1.37)
	Ability--own	7.3 (3.06)	7.6 (4.50)	8.9 (4.25)
	Ability--difference	-2.0 (3.06)	-1.5 (4.25)	-1.3 (2.41)
	Effort--own	9.0 (3.98)	7.5 (4.99)	8.5 (2.83)
	Effort--difference	-2.5 (4.53)	.1 (4.82)	- .4 (3.44)
	Task difficulty--own	10.7 (4.76)	13.0 (7.16)	9.5 (4.81)
	Task difficulty--diff.	3.7 (3.16)	2.1 (3.41)	2.6 (5.97)
	Expectation--own	4.0 (.81)	3.1 (.88)	3.2 (.92)
	Expectation--difference	- .8 (.63)	-1.4 (1.51)	-1.7 (1.16)

Total N = 60

Table 9

Social Position Factor:
Discriminant Function Weights
and Univariate ANOVA statistics

Variable	Weight	F	df	$p<$
Partner's success	-1.2218	52.9369	1,54	.0001
Own success	.5424	2.8364	1,54	.0980
Own task difficulty	.3702	.2441	1,54	.6233
Own ability	.2800	.0011	1,54	.9740
Expectation of own success	-.2043	3.7742	1,54	.0573
Partner's ability	-.1521	2.0043	1,54	.1626
Own effort	-.0940	.0039	1,54	.9502
Partner's effort	-.0826	.4783	1,54	.4922
Partner's task difficulty	-.0428	7.3567	1,54	.0090
Expectation of partner's success	-.0132	19.6085	1,54	.0001

effect of social position on perceptions of difference between own and partner's success. The cognitive variable reflecting expectation of partner's future success showed high significance when tested with a univariate ANOVA, but had almost no importance in the multivariate composite. This discrepancy reflects the redundancy of that variable with the other variables in the set.

Family 3--Postsession questionnaire

The postsession questionnaire contained eleven questions, some of which were manipulations checks and others dependent variables of secondary interest. The two questions regarding attraction to the other participant were additively transformed into one variable, as were the two regarding attraction to the experimenter. The resulting nine variables were analyzed by separate univariate tests. The form of each statistical test was determined by the nature of the variable. The family-wise error rate, set at .05, was divided evenly among the nine tests, so that the alpha level for each hypothesis was set at approximately .005.

Responses to the question "How unpleasant did you find the noise?" were subjected to a 2 x 4 ANOVA, the results of which appear in Table 10. The cell means are presented in Table 11. All F-statistics show values less than 1, indicating no group differences on responses to this question.

Table 10

Ratings of Noise Unpleasantness

Source of variation	Sum square	Mean square	F	df	p<
Social position factor	.284	.284	.330	1	.567
Uncontrollability factor	.307	.102	.119	3	.949
Interaction	1.580	.527	.611	3	.610
Explained	2.170	.310	.360	7	.923
Residual	68.909	.861		87	

Table 11

Ratings of Noise Unpleasantness

Means and Standard Deviations

Social Position		Uncontrollability: Noise Only	Noise plus Problems	Uncontrollability for Own Noise	Uncontrollability for Partner's Noise
	Symetrical	2.91 (.944)	2.64 (.809)	2.91 (1.045)	2.73 (1.191)
	Disadvantaged	2.91 (.831)	3.00 (.633)	2.64 (1.120)	3.10 (.701)

Total N = 88

Standard deviations in parentheses.

The question "To what extent did you feel that you and your partner were treated equally in this experiment?" was designed to tap the subjective reactions to the social position manipulation. It was therefore analyzed by a two-group one-way ANOVA.[6] Results show a trend toward a group difference, indicating that the participants in the Disadvantaged level felt they were treated worse than their partners ($F_{1,86} = 6.214$; $p < .05$). The group means appear in Table 12.

Participants in all but the Noise Only groups responded to the question "To what extent did you feel that your performance on the first task (the puzzles) influenced your performance on the second task (the proofreading)?". Responses to this question were analyzed in a 2 x 3 ANOVA. Results of this analysis, presented in Table 13, show no evidence of group differences. The overall mean was 2.09, indicating a "slight degree" of influence as the average response, but, as seen previously, there was no evidence of this slight influence in the analysis of test task performance. The cell means are presented in Table 14.

All participants rated the amount of noise they received. The 2 x 4 ANOVA results, presented in Table 15, indicate

6. The test was used only for the sake of consistency. The obvious choice of tests is the two-sample t-test. This is statistically equivalent to the test used: $F = t^2$.

Table 12

Equality

		Mean	Standard deviation
Social position	Symetrical	4.000	0.000
	Disadvantaged	3.773	.605

Table 13

Ratings of Dependency between Tasks

Source of variation	Sum square	Mean square	F	df	p <
Social position factor	.970	.970	1.151	1	.288
Uncontrollability factor	1.727	.864	1.025	2	.365
Interaction	2.212	1.106	1.313	2	.277
Explained	4.909	.982	1.165	5	.337
Residual	50.545	.842		60	

Table 14

Ratings of Dependency between Tasks

Means and Standard Deviations

		Uncontrollability		
		Noise plus Problems	Uncontrollability for Own Noise	Uncontrollability for Partner's Noise
Social Position	Symetrical	1.73 (.786)	2.45 (1.036)	1.73 (.786)
	Disadvantaged	2.27 (1.009)	2.18 (.751)	2.18 (1.079)

Total N = 66

Standard deviations in parentheses.

trends toward group differences, but the significance levels all fall short of the .005 alpha level. The cell means appear in Table 16.

The variable "attraction to partner" was formed by combining responses to a question concerning "liking" and a question concerning "working together in another experiment". Results of the 2 x 4 ANOVA performed in this variable are presented in Table 17, and the cell means appear in Table 18. The degree of uncontrollability factor shows a nonsignificant trend ($p < .025$). The significant interaction effect ($p < .004$) which is graphed in Figure 1, was due to a reversal in the Noise plus Problems groups of the tendency for Symetrical participants to report high liking for their partners. In the Noise plus Problems condition, where puzzle failure did not imply lack of control over the noise, Disadvantaged participants (i.e., those who perceived their partners as competent at puzzles) demonstrated more liking for their partners than Symetrical participants.

The two questions forming the previous variable, with the substitution of "experimenter" for "partner" were used in forming the variable "attraction to experimenter". The results of the 2 x 4 ANOVA performed on this variable, shown

Table 15

Ratings of Number of Noise Bursts Received

Source of variation	Sum square	Mean square	F	df	$p <$
Social position factor	1.375	1.375	2.079	1	.153
Uncontrollability factor	5.216	1.739	2.629	3	.056
Interaction	5.580	1.860	2.812	3	.045
Explained	12.170	1.739	2.629	7	.017
Residual	52.909	.661		80	

Table 16

Ratings of Number of Noise Bursts Received

Means and Standard Deviations

Social Position		Uncontrollability: Noise Only	Noise plus Problems	Uncontrollability for Own Noise	Uncontrollability for Partner's Noise
	Symetrical	3.55 (.687)	2.91 (.831)	3.27 (1.191)	3.36 (.505)
	Disadvantaged	3.18 (.982)	3.27 (.467)	4.27 (.786)	3.36 (.809)

Total N = 88

Standard deviations in parentheses.

Table 17

Attraction to Partner

Source of variation	Sum square	Mean square	F	df	p <
Social position factor	2.557	2.557	.920	1	.340
Uncontrollability factor	27.398	9.133	3.286	3	.025
Interaction	40.670	13.557	4.877	3	.004
Explained	70.625	10.089	3.630	7	.002
Residual	222.362	2.780		80	

Table 18

Attraction to Partner

Means and Standard Deviations

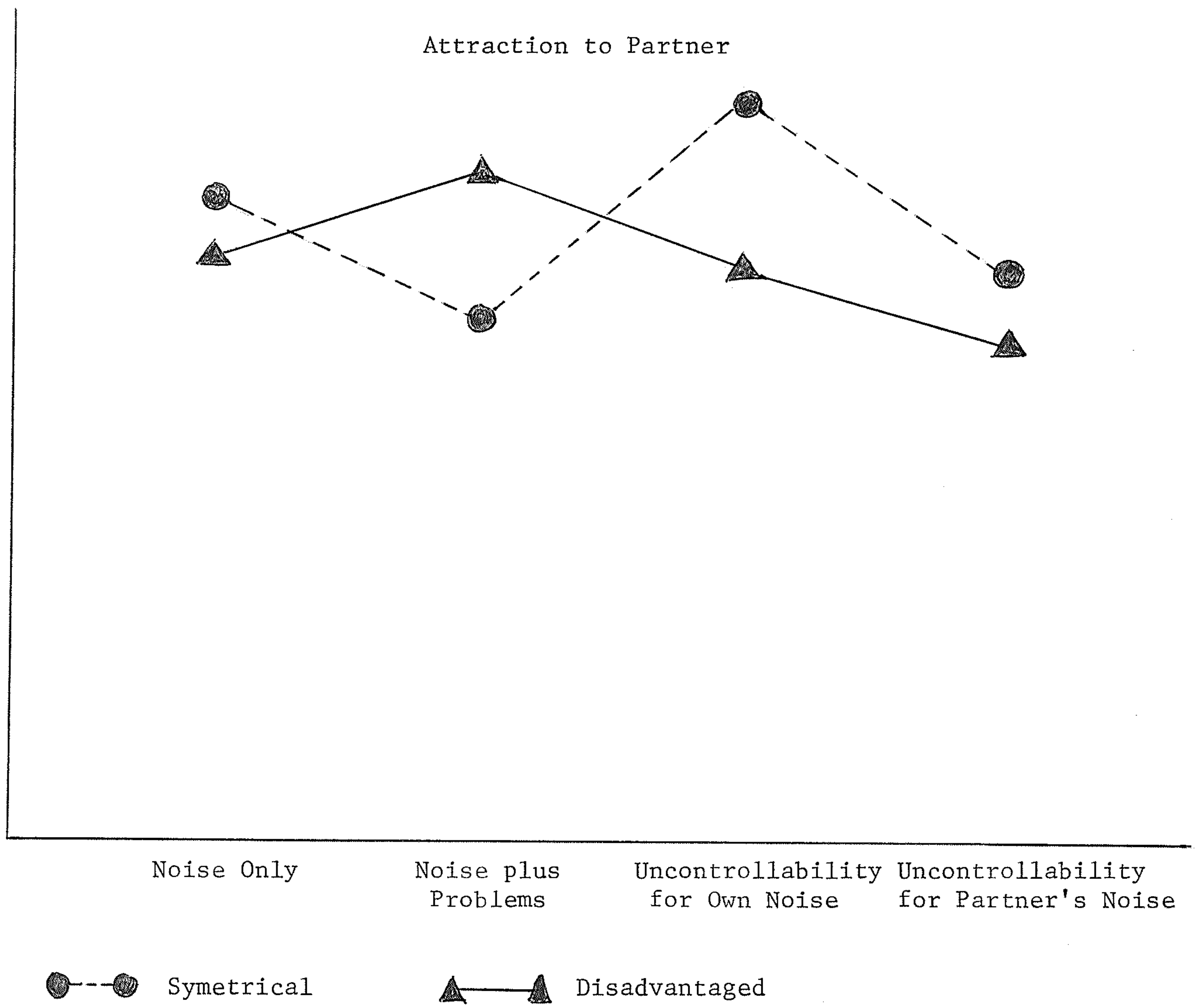
		Uncontrollability			
		Noise Only	Noise plus Problems	Uncontrollability for Own Noise	Uncontrollability for Partner's Noise
Social Position	Symetrical	11.27	9.91	12.18	10.27
		(1.348)	(1.700)	(1.834)	(2.453)
	Disadvantaged	10.63	11.82	10.45	9.36
		(2.809)	(1.079)	(1.968)	(1.567)

Total N = 88

Standard deviations in parentheses.

Figure 1

Attraction to Partner



in Table 19, reveal no significant main effects and a non-significant trend in the interaction ($p < .018$). The pattern of means giving rise to this interaction, graphed in Figure 2 and tabled in Table 20, was highly similar to that found in "attraction to partner".

The last three postsession questionnaire variables were checks on the experimental manipulations. For the first of these checks, participants were asked to report their perceptions of the difficulty level of the puzzles. The overall mean rating was 4.07, corresponding to a rating of "very hard". This indicates some suspicion on the parts of participants, since ratings of 5 or 6 indicated that at least some of the puzzles were impossible (means appear in Table 21). The results of the 2 x 3 ANOVA, shown in Table 22, indicate no group differences on perceptions of puzzle difficulty.

The second manipulation check tested was participants' perceptions of a difference between the number of noise bursts they received and the number their partners received. In all but the Noise plus Problems condition, participants in the Disadvantaged position experienced events designed to induce beliefs that they were receiving more noise bursts than their partners. The results of the one-way ANOVA on these six relevant groups, however, failed to achieve the .005

Table 19

Attraction to Experimenter

Source of variation	Sum square	Mean square	F	df	$p <$
Social position factor	1.375	1.375	.400	1	.529
Uncontrollability factor	1.943	.648	.189	3	.904
Interaction	36.580	12.193	3.551	3	.018
Explained	39.898	5.700	1.660	7	.131
Residual	274.724	3.434		80	

Table 20

Attraction to Experimenter

Means and Standard Deviations

Social Position		Uncontrollability			
		Noise Only	Noise plus Problems	Uncontrollability for Own Noise	Uncontrollability for Partner's Noise
	Symetrical	11.09	9.73	11.36	10.82
		(2.212)	(2.054)	(2.629)	(2.089)
	Disadvantaged	10.64	11.55	9.64	10.18
		(2.063)	(.820)	(1.850)	(1.722)

Total N = 88

Standard deviations in parentheses.

Figure 2

Attraction to Experimenter

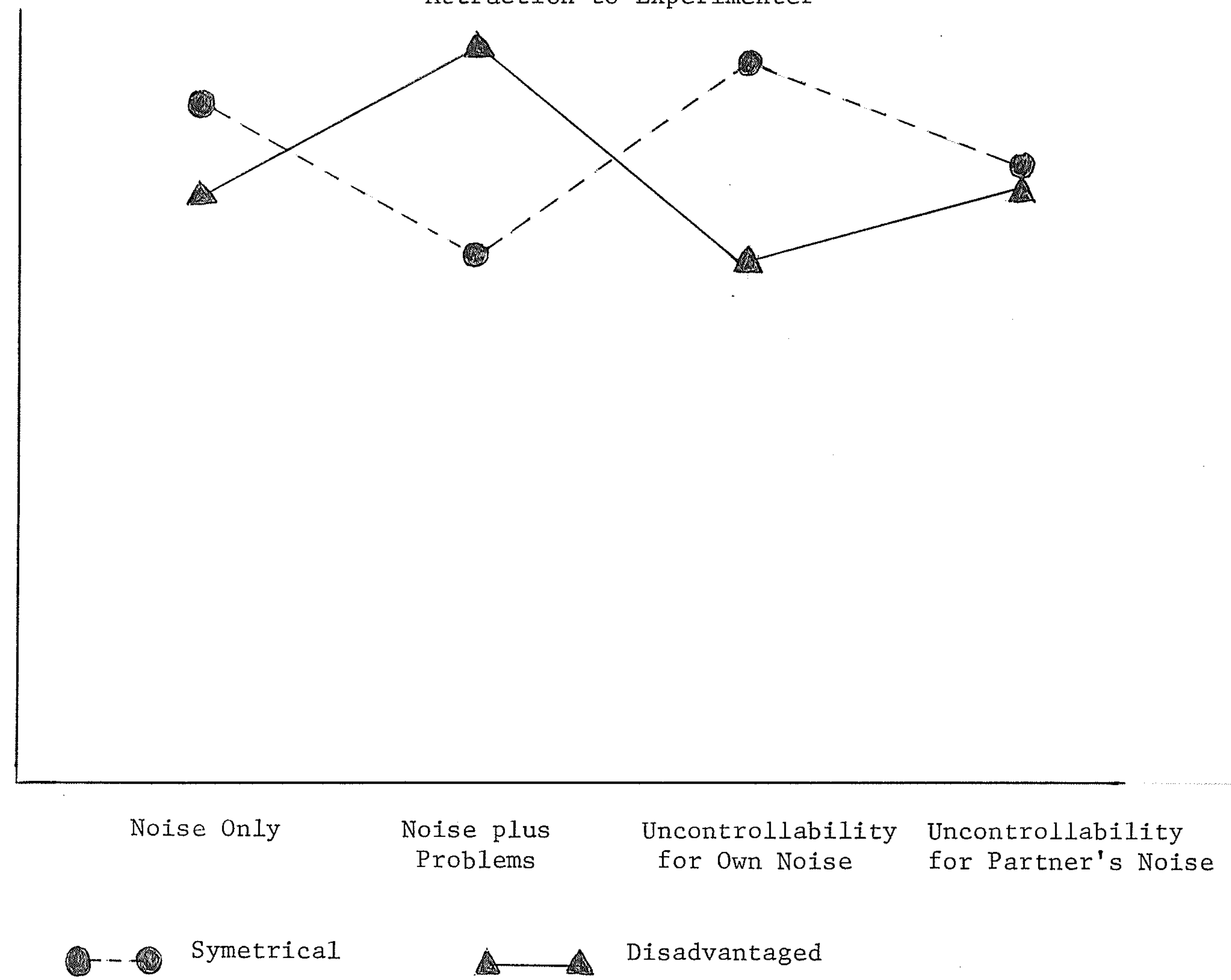


Table 21

Puzzle Difficulty

Means and Standard Deviations

		Uncontrollability		
		Noise plus Problems	Uncontrollability for Own Noise	Uncontrollability for Partner's Noise
Social Position	Symetrical	3.455 (.688)	4.273 (1.104)	4.000 (.633)
	Disadvantaged	4.455 (.820)	4.182 (1.079)	4.091 (.701)

Total N = 66

Standard deviations in parentheses.

Table 22

Puzzle Difficulty

Source of variation	Sum square	Mean square	F	df	p <
Social position factor	1.833	1.833	2.490	1	.120
Uncontrollability factor	.848	.424	.576	2	.565
Interaction	3.758	1.879	2.551	2	.086
Explained	6.439	1.288	1.749	5	.137
Residual	44.181	.736		60	

significance level ($\underline{F}_{1,64} = 6.61$; $p < .012$). The graph of the cell means shown in Figure 3 (means tabled in Table 23) suggests that participants in the Noise Only group failed to believe the instructions regarding their partners' noise (the relative positions of the Symetrical and Disadvantaged means were the opposite of expectation). A second one-way ANOVA was performed excluding the Noise Only groups. The results of this analysis showed a highly significant effect ($\underline{F}_{1,43} = 22.345$; $p < .000025$). It appears, then, that participants in the uncontrollability conditions made the intended connections between puzzle failure and number of noise bursts, at least as applied to their perceptions of their partner's performances.

The final manipulation check represents another test of the degree to which the puzzle-noise dependency was believed by the participants. Participants in all groups working puzzles assessed the degree to which they felt the noise to be dependent on puzzle performance. Participants in the Noise plus Problems condition were expected to report complete independence and those in the uncontrollability conditions some degree of dependency. Since there was no reason to expect differences on the social position factor, a one-way ANOVA

Table 23

Difference between Own and Partner's Amount of Noise

Means and Standard Deviations

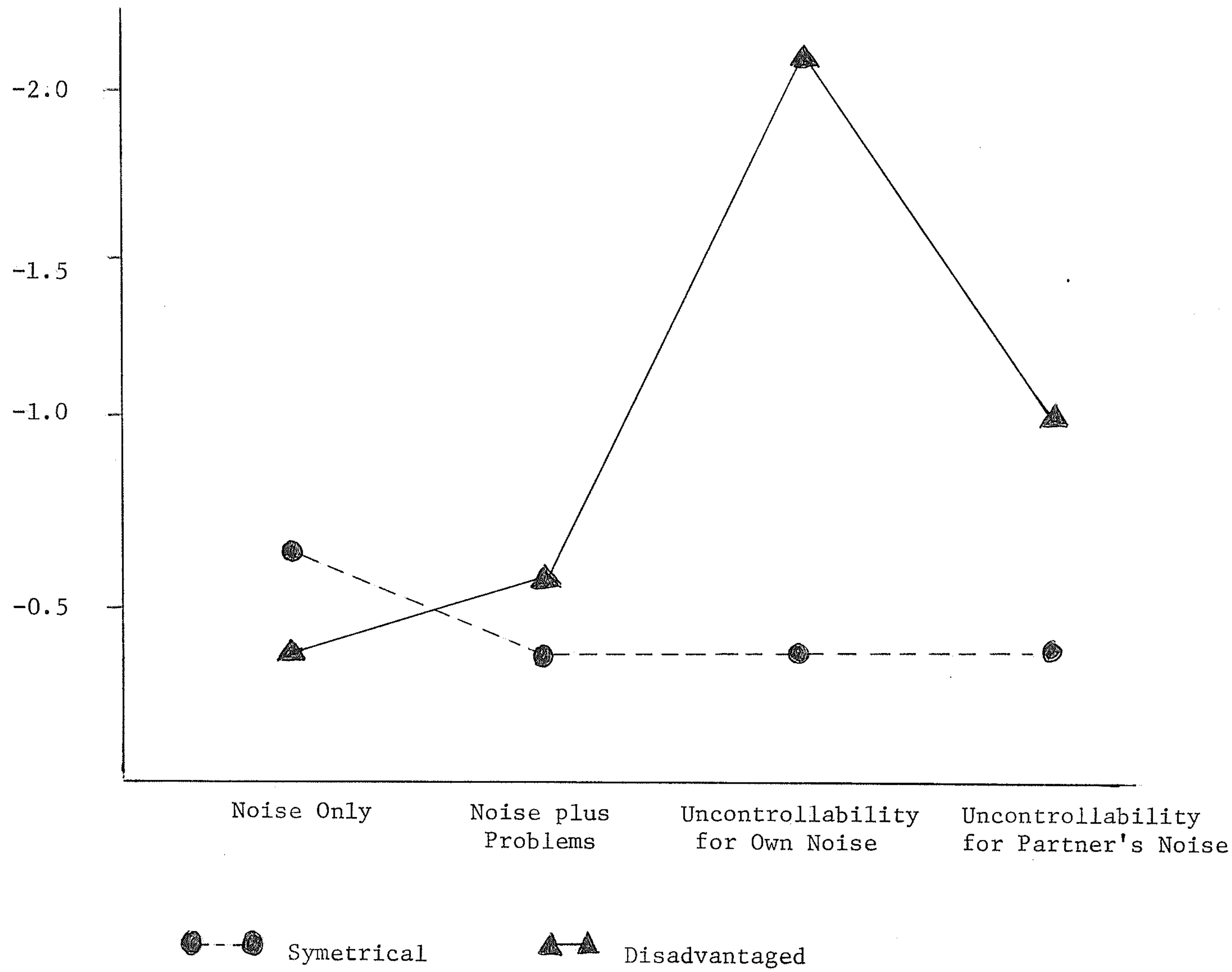
		Uncontrollability			
		Noise Only	Noise plus Problems	Uncontrollability for Own Noise	Uncontrollability for Partner's Noise
Social Position	Symetrical	- .636	- .364	- .364	- .364
		(1.286)	(1.206)	(1.206)	(.924)
	Disadvantaged	- .364	- .546	-2.182	-1.091
		(.809)	(.934)	(1.251)	(1.045)

Total N = 88

Standard deviations in parentheses.

Figure 3

Difference between Own and Partner's Noise



was employed to test the differences between the three levels of uncontrollability. Contrary to expectation, there was no evidence that these three groups differed on assessment of puzzle-noise dependency ($\underline{F}_{2,65} = 832$; $p > .05$). All group means (presented in Table 24) fell between 2 (slight degree of dependency) and 2.5 (where 3 = dependency). This result indicates not only that some participants failed to believe instructions relating puzzle failure to uncontrollable aversive events, but also that others construed such a connection in the absence of instructions.

The failure of the instructions manipulation which is indicated by the above findings would have serious consequences for the production of group differences on the proofreading test task. It appears that participants, instead of forming beliefs according to instructions, made individual judgments about the degree to which puzzle failure implied increased number of noise bursts. This variable (belief in puzzle-noise dependency) was therefore entered into the regression equation discussed earlier as a predictor of proofreading performance. Its inclusion produced a significant increment in the multiple $\underline{R}^2$ ($\underline{F}_{1,47} = 5.672$; $p < .05$), indicating that, beyond the effects due to the covariate and those due to group membership, belief in puzzle-noise dependency was associated with a deficit in test task performance.

Table 24

Puzzle-noise dependency

Means and Standard Deviations

	Uncontrollability		
	Noise plus Problems	Uncontrollability for Own Noise	Uncontrollability for Partner's Noise
Combined Social Position	2.136	2.455	2.182
	(.774)	(1.011)	(.853)

Total N = 66

Standard deviations in parentheses.

Discussion

The findings of this experiment provide only weak support for the existence of the interference effect in humans, and no support for the hypothesis that social situation influences the saliency of uncontrollability manipulations.

Uncontrollability effects

The only evidence for the interference effect in this study was the marginally significant contrast between the Noise plus Problems condition and the two Uncontrollability conditions. The strengthening of the effect when the Noise Only condition was eliminated from the contrast may suggest that the uncontrollability groups were debilitated, but the Noise Only condition was not an optimal comparison group for the detection of this debilitation. Participants in the Noise Only groups had to passively listen to bursts of loud noise for twenty minutes. The attenuation of boredom provided by the addition of the puzzles may make the Noise plus Problems condition closer to the desired neutral pretreatment.

The analyses of the manipulation checks suggest that the failure to produce the interference effect was due to failure of the manipulations. The pretreatment instructions for the uncontrollability conditions were designed to induce the belief that puzzle failure resulted in failure to control the onset of potentially controllable aversive noise. The post-session question which asked the participants to assess this dependency for their performances showed no differences on this belief between the uncontrollability and the comparison groups. On the other hand, the postsession question indirectly assessing the same beliefs about puzzle-noise dependency via perceptions of the number of noise bursts experienced by partners indicated that participants in the uncontrollability conditions did perceive their partners' puzzle performances related to their number of noise bursts.

There are several possible explanations for these contradictory findings. One explanation grows out of the fact that endorsements of the puzzle-noise dependency for a partner implied a belief in the partner's ability to control the noise, while a participant's endorsement of the puzzle-noise dependency with regard to her own performance implied a belief in her own inability to control the noise. It may have been easier for participants to accept another person's ability to control her stimuli than to accept their own inability to control their stimuli.

A second explanation for the apparent discrepancy between responses to the two postsession questions presumes that undergraduates are suspicious of instructions in psychology experiments. Participants were perhaps willing to "play along" with the experimental instructions when asked to report their acceptance of instructions in an indirect manner. When questioned more directly on their acceptance of the puzzle-noise dependency, they reported their own individually-formed beliefs.

In any case, the results of the analyses on the manipulation checks indicate that the differents sets of instructions did not induce differential beliefs regarding the relationship between one's own puzzle success and the attenuation of aversive noise. The experimental manipulation hypothesized to induce subsequent performance deficits was the participants' failure to control an event which they believed to be controllable. Those participants who believed the noise to be potentially controlled by puzzle success did, in fact, show the hypothesized debilitation on the test task. This finding is consistent with the early theories of W.I. Thomas (Volkart, 1951) that people behave according to their own definitions of situations, which may or may not correspond to the definitions intended by the experimenter. The finding suggests that if the instructions had been accepted by all participants, that the hypothesized effect would have occurred. This evidence is correlational, however, so that another, third, factor also correlated with belief in the potential controllability of the noise (such as a personality variable) may be the true "cause" of any debilitation on the test task.

The failure of several of these experimental procedures contrasts with the success of the same or similar procedures in several experiments conducted by Glass and his associates (Glass & Singer, 1972). The juxtaposition of geometric puzzles and aversive noise, where instructions established a relationship between them, was used in two experiments reported by Glass and Singer (1972, pp. 80-90 & 110-120). These investigators found that people given instructions establishing puzzle-noise dependency and who then experienced a series of insoluble puzzles did relatively poorly on the proofreading test task. The failure of the present experiment to produce similar results can be accounted for by (1) minor differences in the pretreatment procedures, and (2) the use of different comparison groups.

There are several known discrepancies between the pretreatment procedures reported by Glass and Singer and those employed in the present experiment.[7] First, the format of the puzzle task was altered. In the Glass-Singer studies, participants worked on each puzzle design until they either

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7. The complete details of the Glass-Singer experimental procedure were unavailable at the time of the present study.

solved the puzzle or gave up. In the present study, puzzles were presented in a randomized order in a booklet. This altered procedure may have had any of the following consequences: (1) participants experienced less sense of failure with the randomized puzzle presentation; (2) participants experienced a high ratio of puzzles solved to puzzles failed; (3) participants became suspicious that some of the puzzles were insoluble; or (4) the juxtaposition of noise onset with puzzles was altered in a way which decreased the participants' subjective sense of the dependency between puzzle failure and noise onset. Any of these consequences could have decreased the saliency of the uncontrollability manipulation in the experimental pretreatment.

A second procedural alteration was in the format of the instructions. In the present experiment, the instructions were adapted to the booklet format of puzzle presentation. They were also complicated by the additions necessary to establish the social position factor (i.e., instructions concerning the feedback on the partner's puzzle performance). The complexity of the instructions in the present study may well have diluted their impact. The instructions were modified several times in pilot work because of confusion or uncertainty on the parts of participants. Even the final forms of the instructions were probably so complex to be confusing to at least some participants. Furthermore, the rationale for the experiment (effect of noise on performance)and the instructions (effect of performance on noise) gave participants contradictory information, presenting the noise as first cause and then effect. The dual role thus assigned to the noise could well have exacerbated the perceived complexity of the instructions and minimized the extent to which participants held themselves responsible for the apparent uncontrollability of the noise.

The present experimental procedure was more highly mechanized, involving less personal contact with the experimenter than did those reported by Glass and Singer. The decrease in personal contact may have decreased the saliency of the failure experience. The dependency between puzzle success and avoidance of noise could also have been less believable when the noise was controlled from another room.

The final alteration in the pretreatment format was the use of a different noise source. The Glass-Singer studies employed noise bursts consisting of sounds in a city environment (typewriters, people speaking foreign languages, etc.). The present study, while it used the same timing of noise bursts as in the Glass-Singer procedure, duplicated the noise source used by Hiroto and Seligman (1975). That the latter study found debilitation is reason to believe that the alternate noise source was as effective as the one used by Glass and Singer (1972).

While the procedure used in the present experiment was substantially the same as that used in the two similar studies reported by Glass and Singer, the departures from the former procedure may have been significant. In particular, the changes in the format of the puzzles and the increased complexity of the instructions could account for the decreased effectiveness of the intended uncontrollability manipulation.

A second potential source of difference between the results obtained by Glass and Singer and those obtained in the present study is the experimental design. In the Glass-Singer experiments there were four groups: (1) noise plus insoluble puzzles, where participants were instructed that puzzle solution would leas to fewer noise bursts, (2) noise plus soluble puzzles with the same instructions, (3) insoluble puzzles only, and (4) soluble puzzles only. These investigators found people in group (1) debilitated when compared to people in the other three groups. These comparisons established, first, that perceived lack of control was more debilitating than perceived control, and, second, that experience with insoluble puzzles alone did not produce performance deficits. The present study, on the other hand, replaced the above comparisons with (1) a comparison between perceived lack of control with a neutral degree of control, and (2) a comparison between simple presentation of aversive stimulation and insoluble puzzles with a presentation which induced an expectation that puzzle solution led to control of the aversive stimuli. In the present study, then, comparison groups were yoked for aversive stimulation and experienced neutral instead of positive degrees of controllability. The comparisons in the present study are thus more adequate and more rigorous tests of the effects of exposure to uncontrollability than were the Glass-Singer comparisons. The absence of relative debilitation in the present study may indicate that previous debilitation findings were based on use of methodologically faulty comparison groups.

As Wortman and Brehm (1975) have pointed out, research in the effects of uncontrollability in humans has not come to grips with the issue of what characteristics define a proper control group. The negative findings of the present study, which employed rigorous controls, adds weight to this concern.

While the procedures used in this study were highly similar to those used in Glass's and Singer's work, they were quite different from those used by Seligman and his associates. One property of the present procedure which received comment by several participants was that the test task (proofreading) did not incorporate a success/failure dichotomy. Several participants remarked that such a dissimilarity between the pretreatment and test tasks minimized the influence of the pretreatment task upon test task performance. It is possible that the previous debilitation findings by the Seligman group were predicated on a similarity between pretreatment and test tasks. Task dissimilarity allows a more valid test of the learned helpless hypothesis (c.f., footnote 1 on page 4) and the absence of positive findings in the present study provides another reason to question the validity of previous findings of laboratory-produced interference effects in humans.

Social position effects

The social position manipulation, where one member of the participant dyad was led to feel disadvantaged in one or more respects, did not produce the hypothesized effects on test task performance. Such an absence of effect is surprising in light of previous studies showing either debilitation or facilitation effects using similar experimental procedures. Silverstein and Staff (cited in Glass & Singer, 1972, pp 92-98) found debilitation using procedures similar to the Noise Only condition. Failure to replicate Silverstein and Staff's findings can, however, be traced to manipulation failure: participants did not believe their instructions concerning differential noise. The reasons for the absence of a social position effect in the uncontrollability conditions are less clear.

One explanation for the absence of social position effects is the failure of the experimental manipulations to influence the cognitive variables which have been suggested in previous literature to mediate a social position effect. These two factors, attribution of ability and attribution of effort, were the least affected by the social position manipulations of all the ten cognitive variables assessed in the intersession questionnaire. It is quite possible that if the manipulations had had stronger effects on these two factors, the social position effects on test task performance would have occurred. Dyck et al. (in press), however, found that attributions concerning ability were extremely resistant to experimental manipulation.

The absence of social position effects on performance could be explained by the results on the postsession question concerning "equal treatment". There was a trend here indicating that some participants in the disadvantaged social position felt that they were not treated as well as their partners. These people may have (accurately) held the experimenter responsible for the social position differences instead of viewing themselves and their partners as the loci of the social position discrepancy. This kind of attribution probably would have diluted the saliency of the social position manipulation.

The absence of social position effects in the present study could reflect a mixture of the two opposing predictions made from previous studies. Some participants in the disadvantaged social position could have responded with facilitated performance, while others responded with debilitated performance. If some participants' responses to social disadvantage were in one direction while others' responses were in the opposite direction, however, one would expect to find greater variability in the Disadvantaged groups than in the Symetrical groups. There is no evidence for this variability difference.

Social psycholgical theories have suggested that such opposite effects would occur and that the direction of effect could be predicted by the specific causal attributions concerning ability and effort. One of these attributions, own ability, did emerge from the total set of cognitive variables as the most predictive of test task performance, but its absolute predictive ability was minimal. This minimal power could be related to the dissimilarity between the pretreatment and the test tasks. Attribution research considers ability to be an internal, stable factor, but it may also be conceived of as skill at a particular kind of task (Abramson, Seligman, & Teasdale, 1978). A participant with a history of poor mathematical-spatial performance and good verbal performance could easily hold her ability responsible for her poor puzzle performance. Such an attribution could well have little influence on her performance at the verbally-oriented proofreading task. This explanation could not account for the weakness of the other variable predicted to determine direction of test task effect, namely attribution of own effort.

There are, then, several plausible explanations for the absence of social position effects in this experiment. The most parsimonious explanation is, of course, that social disadvantage does not affect subsequent task performance. Other explanations relate to the procedures used in this experiment: (1) the important cognitive mediators of the hypothesized effect were not effectively manipulated; and (2) participants saw the experiment or experimenter as the true source of the social position discrepancy.

Supplementary data

The data on attraction to the experimenter was collected pursuant to the suggestion by Wortman and Brehm (1975) that participants in uncontrollability experiments showed debilitation effects because they got angry and stopped cooperating with the experimenter. The questions concerning attraction to the other participant were included for completeness and for exploratory interest: no preliminary hypotheses were made. The significant interaction effect of interpersonal attraction to one's partner suggests that another person's degree of control over the environment and his/her degree of skill both contribute to one's attraction to that person. While, in general, research has shown that liking for a person increases with his/her perceived competence, Mettee and Wilkins (1972) have demonstrated the complexity of this relationship. They found that liking decreased with increasing difference between the subjects' and objects' competence. In the present study, the implication that competence also implied control could have functioned like a large competence difference, making the relationship between social position, uncontrollability, and attraction complex and interactive.

Summary

The experimental manipulations in the present study failed to produce significant group differences in performance. There are several possible reasons for such failure. The absence of an uncontrollability effect may have been due to (1) ineffective instructions, (2) inappropriate comparison groups, or (3) use of a performance task which was clearly dissimilar from the pretreatment task. The absence of a social position effect may have been due to the artificiality of the manipulations, as participants did not apparently "internalize" the performance discrepancy between themselves and their partners.

The failure to find significant effects in the present experiment does not necessarily imply that further research should discard the procedures used in this study. The experiment was designed to be a valid test of the learned helplessness hypothesis, but several characteristics which made it valid probably also contributed to the absence of positive findings. It would be tempting for researchers to continue using comparison groups given positive control experiences and test tasks which are similar to pretreatment tasks, because such procedures maximize the probability of positive findings. It is much more important, however, to discern the true effects of uncontrollability on humans than to continue producing positive findings which can be attributed to confounds or inadequacies in the experiments.

The absence of positive findings in the present study illustrates the difficulty of experimentally inducing perceptions of uncontrollability and interpersonal differences. Such manipulations are especially difficult to design if one also attempts to use comparison groups which are adequately yoked to experimental groups for possible confounds. Researchers in this area will have to experiment with a variety of laboratory procedures. It may be, however, that laboratory manipulations are simply ineffective in producing a true interference effect in humans. Researchers interested in the effects of uncontrollability in humans would be well advised to also investigate naturally occurring instances of the uncontrollability phenomenon. Such research necessitates use of less rigorous designs. It would also, however, help us to determine what constitutes uncontrollability for a human being and what factors determine whether or not an uncontrollability experience is followed by debilitated performance.

Literature Review

Most experiments in learning explore the effects of contingencies of reinforcement. When organisms are exposed to an environmental contingency, they learn that their responses influence the probability of an environmental event. There has recently been work exploring the effects on an organism of a complete absence of a contingency between the organism's behavior and its environment. This absence of a contingency has been formulated as the condition where the probability of reinforcement given a response is equal to the probability of reinforcement given no response (Seligman, Maier, & Solomon, 1971). When this condition is present, the organism is said to have no control over its environment.

The first studies investigating lack of control involved administration of electric shock to mongrel dogs (Overmier & Seligman, 1967; Seligman & Maier, 1967). These dogs were first given inescapable shocks while they were immobilized in a Pavlovian hammock. When they were placed in a two-way shuttlebox where escape from shock was possible, about two-thirds of these dogs made no attempts to escape. Even when they made an escape response, they reverted to passively taking the shock, apparently failing to learn from their experience. These results indicated that the experience in a contingency involving uncontrollable aversive stimulation interfered with learning in subsequent controllable contingency situations.

Further work has been done to rule out alternative hypotheses which would account for the interference effect. In an effort to show that uncontrollability and not aversive stimulation itself was responsible for interference in active responding, Seligman and Maier (1967) introduced a triadic design. They yoked subjects who could escape the shock to subjects who could not escape so that both groups received identical patterns of shock. The third group in the triad received no shock. Decrements in responding appeared only in the dogs who could not escape shock, supporting the hypothesis that lack of control and not aversive stimulation produced the passivity. A second alternative hypothesis was that the dogs had been directly reinforced for inactivity so that they had subsequent difficulty in making the required active response. To dispel this hypothesis, Maier (1970), using the triadic design, conditioned a passive response in the escapable group. The results confirmed the hypothesis that the uncontrollable contingency and not conditioning of a passive response was responsible for the passive behaviour of the dogs. The group pretreated with escapable shock and the no pretreatment control group performed the active shuttle response in the test situation, while the group pretreated with inescapable shock failed to learn in the escapable situation.

These findings led Seligman and his associates to propose the existence of a particular kind of proactive interference called "learned helplessness." According to the learned helplessness hypothesis, when an organism learns that its responses are independent of its reinforcers, the organism exhibits the following characteristics: (1) lowered motivational state indicated by the lack of response initiation, (2) changes in cognitive state indicated by the failure to learn appropriate responding, and (3) aroused emotional state indicated by the high frequency of fear symptoms, weight loss, and ulcers. More generally, learned helplessness is inappropriate response generalization from situations which are uncontrollable to situations where control is possible. The learning paradigm which has been found to produce the interference effect bears strong resemblence to extinction procedures. Manning and Jackson (Note 3) have classified extinction procedures into two types. The first, which has been the most commonly used, involves the withdrawal of all reinforcement. The second eliminates the contingency altogether by causing reinforcement to be independent of behaviour. Manning and Jackson, calling these procedures classical and operant extinction respectively, noted the similarity between operant extinction and the procedures used by Seligman and associates. The procedures used in uncontrollability research thus fall within the broad category of extinction, but produce behaviours which are distinct from those produced by the more commonly used extinction schedules.

The learned helplessness hypothesis (Maier, Seligman, & Solomon, 1969; Seligman, Maier, & Solomon, 1971) states that motivational, cognitive, and emotional deficits result from exposure to uncontrollability and not from other factors. There is currently controversy in the animal learning field about whether uncontrollability or confounding factors in the design of experiments are responsible for the behavioural deficits which have been observed. Some of the possible confounding factors are norepinephrine depletion resulting from shock (Weiss, 1971), the adventitious reinforcement of "freezing behaviour" (Glazer & Weiss, 1976), and peculiarities of the triadic design (Levis, 1976). These questions stem from the specific experimental procedures used in animal studies, and their answers will be found with refinement of the animal methodology. The existence of the interference effect and its properties in humans can only be determined by examination of the specific procedures which have been applied to humans.

<u>Overview of the literature on uncontrollability in humans</u>

Experiments which are cited as part of the literature on human helplessness all employ the same general format. Subjects are given a pretreatment involving a task on which they experience some degree of uncontrollability. They are then given a second task. The independent variable is the level of pretreatment controllability and the dependent variable is performance on the second task.

Uncontrollability research in humans, being very recent, has no accepted methodology beyond that just described. However, human research has retained several characteristics of the animal methodology in (1) consistently employing some form of aversive paradigm, and (2) usually attempting a triadic design. Some researchers have also employed experimental tasks similar to the animal tasks, while others, in consideration of the properties of the human species, have used cognitive, problem-solving tasks.

Because methodological problems are present in many studies, a brief review of the basic requirements of uncontrollability research is in order. To show effects due to uncontrollable contingencies, designs must include at least one group pretreated in an uncontrollable contingency and at least one group not exposed to uncontrollability. The latter group in the prototype animal study (Seligman & Maier, 1967) was simply not exposed to any pretreatment conditions. Many studies using human subjects have instead exposed the latter group to controllable contingencies. As Wortman and Brehm (1975) have pointed out, controllability may itself produce effects which are distinguishable from simple absence of uncontrollability. Therefore, comparisons made between groups exposed to uncontrollable and controllable contingencies cannot separate effects due to uncontrollability from the confounding effects due to controllability. Therefore, at least one group receiving a pretreatment neutral in controllability/uncontrollability or receiving no pretreatment is a requirement for testing the basic hypothesis about the effects of exposure to uncontrollability.

A second basic requirement for research in uncontrollability involves care concerning the confounding of reinforcers with contingencies. In order to show that experimental effects are due to the uncontrollable contingency and not a result of the amount or type of reinforcement, designs must include a group receiving the same reinforcement as the group exposed to the uncontrollable contingency. Most researchers, including Seligman and Maier (1967), accomplish this task by yoking the group experiencing the uncontrollable contingency to a group receiving a controllable contingency, so that both groups receive identical patterns of reinforcement. Because effects due to reinforcement or aversive stimulation are known to be powerful, inclusion of such a yoked group is necessary for uncontrollability research. This point is particularly important in the light of Jenkins and Ward's (1965) finding that humans tend to associate high frequency of reinforcement with controllable contingencies. Experimental procedures where there is clear separation between reinforcer and contingency lend themselves to yoking designs. Procedures employing "cognitive" pretreatments, however, use the experience of failure to indicate both uncontrollability and aversive stimulation (the cessation of which would be negative reinforcement). Use of such procedures incorporates inherent weakness into designs, as elimination of the confound of reinforcement is not possible.

Designs in uncontrollability research, then, must include at least these three groups: (1) the experimental group which is exposed to uncontrollable contingencies, (2) a group receiving no pretreatment or a pretreatment neutral on the dimension of controllability, and (3) a group receiving the same pattern of reinforcement but a different degree of controllability as the main experimental group. It is possible to combine the two comparison groups into one group. Such a group would be given the same pattern of reinforcement as the experimental group, but would experience a neutral amount of control. Such a comparison group experiences the same environmental conditions as the main experimental group, but receives different instructions. The possibility of this all-purpose comparison group brings up important issues which will be discussed later.

There are many studies in the area of human uncontrollability which are difficult to interpret because of failure to include the crucial three groups. Several studies have attempted to ascertain effects due to uncontrollability by comparing the main experimental group to a group receiving a controllable contingency (Krantz, Glass, & Snyder, 1974; Wortman, Panciera, Shusterman, & Hibscher, 1976). Some investigators have discussed results in this way when the proper comparison group was, in fact, included in the design (Roth & Bootzin, 1974; Thornton & Jacobs, 1971). Hiroto (1974) neglected to include the necessary group yoked in amount of aversive stimulation, even though controllability and aversive stimulation were separable. Shaban and Welling (cited in Glass & Singer, 1972) pretreated people by exposing them to an uncontrollable bureaucracy. They included no group which was exposed to an aversive, but controllable bureaucracy. Studies using "cognitive" pretreatments (Benson & Kennelly, 1976; Dweck & Reppucci, 1973; Dyck, Breen, & Eckelman, Note 1; Eckelman & Hartsough, Note 2; Roth & Kubal, 1975) could not include the necessary groups because of the procedural inseparability of contingency and reinforcement. Many of these studies provide information of interest, but none provides strong evidence for the effects of uncontrollability that is free from important confounds.

Several studies do provide good support for the general hypothesis that exposure to uncontrollability produces deficits in performance in humans. Hiroto and Seligman (1975) exposed humans to two types of pretreatments. The "instrumental" pretreatment involved pushing buttons to escape aversive noise. The "cognitive" preatreatment was a visual discrimination task. The groups pretreated with uncontrollability performed worse than the two comparison groups on both instrumental and cognitive test tasks. These findings were replicated in a similar experiment

(Miller & Seligman, 1975). Thornton and Powell (1974), using a reaction time task, report some evidence for impaired responding in groups pretreated in uncontrollable contingencies. These investigators used a number of different kinds of neutral pre-treatment compairson groups, so that conclusions about the effects of uncontrollability depend on which of these groups is used for the comparison.

Experiments whose main effects are confounded by reinforcement and/or aversive stimulation provide some support for the above findings. Hiroto (1974) used procedures similar to the previous studies and obtained results indicating that people pretreated with inescapability performed poorly relative to a no-pretreatment comparison group. Shaban and Welling (cited in Glass & Singer, 1972) demonstrated that exposure to uncontrollable bureaucracy produced poor performance on a proofreading measure. Several studies have shown decrements in problem-solving in group experiencing failure on a cognitive task (Benson & Kennelly, 1976; Dweck & Reppucci, 1973; Dyck, Breen, & Eckelman, Note 1; Roth & Kubal, 1975).

Glass and Singer (1972) have reported a series of studies on the effects of uncontrollability. This line of research has conceptualized uncontrollability slightly differently than have those researchers whose interest grew out of animal conditioning studies.

Glass and Singer were concerned about the effects of perceived, but not necessarily actual, controllability/uncontrollability. These experimenters directly manipulated beliefs about control by instructions. Experiments by Seligman and his associates exposed people to materials which induced them to conclude for themselves that their tasks were controllable or uncontrollable. Since the learned helplessness hypothesis gives a central place to cognition as a mediator of behavioural deficits, experiments which directly manipulate cognitions are relevant to the hypothesis.

Several experiments have shown that humans prefer situations where they perceive some degree of control (Corah & Boffa, 1970; Geer, Davison, & Gatchel, 1970; Pervin, 1963; Stotland & Blumenthal, 1964). In an experiment by Glass, Singer, & Friedman (1969), people were instructed to work on a proofreading task while listening to loud noise. The group which believed it could terminate the noise performed better than the group which believed the noise was uncontrollable. In a subsequent study, Glass, Reim, and Singer (1971) found that access to another subject who could terminate the noise produced similar facilitation effects. While these results certainly indicate the salience of perceived control, the designs employed do not allow separation of controllability effects (facilitation) from uncontrollability effects (debilitation).

There are two studies of perceived control reported by Glass and Singer (1972, pp. 80-90 & 110-120) which employ adequate comparison groups to test the effects of uncontrollability. The two studies are nearly identical except in the modality of aversive stimulation (noise or electric shock). Two groups of people were administered the same series of aversive stimuli and both groups were instructed that success at solving puzzles would avoid the next aversive stimulus. One group received insoluble puzzles while the second group received soluble puzzles. Two comparison groups received insoluble and soluble puzzles. Results indicated that the people who had lack of perceived control over the aversive stimulation performed worse on a subsequent proofreading measure than those in the perceived control or problems-only groups. The procedure of these experiments is similar to that of Seligman and his associates. The main difference is that in these studies, subjects in the yoked comparison group experienced perceived, but not true, control. However, since the yoking, and not the degree of control, is the important factor here, this difference is of no consequence.

Sherrod and Downs (1974) used the Glass-Singer procedures to investigate helping behaviour. People in the uncontrollability group did a proofreading task while attending to random numbers and loud background noise. A second group were told they could terminate the background noise, while a third group received no background noise. The dependent measure consisted of a number of arithmetic problems worked as a favor to a stranger. Results showed overall differences between the three groups, with the uncontrollability group doing the least number of problems. While this study employs a pretreatment pertinent to the learned helplessness hypothesis, the degree of support it provides depends on one's interpretation of the dependent measure. Working relatively few problems could indicate debilitated performance, but it could also indicate lessened compliance, evidence of stress, or other factors extraneous to the learned helplessness hypothesis.

There are, then, a number of studies which support the existence of the interference effect in humans. The findings show that exposure to uncontrollable situations results in decrements in subsequent performance. There is no evidence, however, that humans display the dramatic behaviour deficits seen in dogs. The effect in humans appears to be similar to the decrements in responding seen in rats previously exposed to uncontrollability (Maier, Albin, & Testa, 1973; Seligman & Beagley, 1975) rather than the complete absence of responding found in dogs.

Further, results of experiments on human uncontrollability are not unequivocal. Thornton and Jocobs (1971) failed to show evidence for the debilitating effects of uncontrollability. Several studies have shown association between exposure to uncontrollability and facilitated performance (Roth & Bootzin, 1974; Roth & Kubal, 1975; Thornton & Jacobs, 1972; Wortman et al., 1976). Thornton and Powell (1974) suggest that the properties of the neutral pretreatment comparison group can explain these findings. They argue that several of the comparison groups used for uncontrollability research are equivalent to groups exposed to uncontrollability, rendering comparisons made with them misleading. Wortman and Brehm (1975) suggest that humans initially react to uncontrollability with heightened performance and only upon continued exposure become debilitated. This account is consistent with Seligman's (1975). In an experimental test of this theory, Roth and Kubal (1975) found that small amounts of pretreatment resulted in facilitated performance, while large amounts resulted in debilitation.

Problems in human research

There are several problems in applying the paradigm of animal uncontrollability to humans, many of which stem from the ethics of human experimentation. The first problem in human uncontrollability research is that total restriction of the subjects' experiences is not possible. Several studies have indicated that prior experience influences the susceptibility of organisms to the interference effect (Seligman & Groves, 1970; Seligman & Maier, 1967; Seligman, Marques, & Radford, Note 4). Experimenters using humans cannot control the pre-experimental experience of their subjects. Intersubject variability should therefore be high and the magnitude of effects may be small.

Experimenters are also unable to create conditions of true uncontrollability. Ethical standards dictate that human subjects be informed of their rights to terminate their involvement in an experiment at any time. Thus, the human subject is never in a position of complete uncontrollability, but only lack of control by his or her own consent.

Ethical considerations also dictate that experimenters should not produce the interference effect in a strength near to that seen in dogs. The dogs which showed the effect were severely debilitated in a setting different from the pretreatment setting. Seligman (1974, 1975) has suggested that the interference effect in humans is recognized as clinical depression. The severity and extent of this effect prohibits its experimental production in humans. Experimenters are thus limited to producing less profound, reversible behaviour changes in humans.

One of the crucial issues in human research concerns generalization. In the animal studies, subjects were pretreated in one apparatus and subsequently tested in a different apparatus. The situational cues and the contingencies of reinforcement both changed. The behaviour pattern developed in the first setting was clearly inappropriate in the second setting. This inappropriate generalization, the hallmark of learned helplessness, has been shown in very few situations with humans.

Several investigators, recognizing this issue, have presented the pretreatment and test tasks as separate experiments. Such a procedure would enable one to claim inappropriate generalization because people were generalizing from one experiment to another experiment. One such study (Roth & Kubal, 1975) showed debilitation due to prior uncontrollability in subjects receiving a long series of pretreatment tasks, but two of these studies showed facilitation effects (Roth & Bootzin, 1974; Wortman et al., 1976). Eckelman and Hartsough (Note 2) showed debilitation when pretreatment and test were presented as part of the same experiment, but no debilitation when they were presented as separate experiments. The case for humans exhibiting inappropriate generalization of passive behaviour is, according to these studies, very weak.

Even if these studies had shown strong evidence for transfer of debilitation from one experiment to another, several further objections could be made. First, the two situations are not completely different from one another. People debilitated in the second experiment could be making an inappropriate generalization because the experiments were different, or they could be making an appropriate generalization involving psychological experiments. In answer to this objection, several studies could be cited which have used unobtrusive procedures. Sherrod and Downs (1974) found that exposure to uncontrollable noise produced decreased effort in helping by employment of a confederate who approached the subject as he was leaving the experiment. The test task in this study was unobtrusively presented. Shaban and Welling (cited in Glass & Singer, 1972) found significant effects using the unobtrusive pretreatment of exposure to an unyielding bureaucracy. These two studies show the apparent transfer of debilitation between situations where one is not perceived as a psychological experiment.

The second objection to the two-experiment studies is that people could be making a generalization concerning a class of personnel -- namely, experimenters. The only study which varied the personnel while holding constant the situation was conducted on schoolchildren by researchers who were presented as teachers (Dweck & Reppucci, 1973). One teacher consistently presented the children with soluble problems, while the other consistently presented insoluble problems. When the latter teacher began presenting soluble problems, shildren were unable to work them, even though they easily worked similar problems presented by the soluble teacher. While the unusual procedure in this study reduces its comparability with other research, it illustrates that humans are sensitive to the association between other persons and uncontrollability experiences. Research purporting to demonstrate the interference effect in humans should show that people generalize their responses to uncontrollability from a person associated with uncontrollability to another person who is clearly different.

Given the ethical constraints on human experimentation, it is hardly suprising that experiments in this area show only weak evidence for inappropriate generalization of the part of persons pretreated with uncontrollability. Experimenters are prohibited from producing debilitated behaviour which would generalize beyond the experimental setting. How, then, have any extra-experimental effects been shown? The answer to this question lies in examination of the tacit assumption among researchers that the interference effect in humans is sensitive to instructional control and therefore can be easily dissipated by debriefing. This assumption has received some experimental support. Hiroto (1974) has shown that people who were merely informed that their responses were unrelated to their outcomes showed subsequent debilitation relative to persons unformed that there was a contingency between their responses and their outcomes. Similarly, Thornton and Powell (1974) easily produced dramatic "recoveries from helplessness" by informing their subjects that conditions had changed fron uncontrollable to controllable. Some studies have used the all-purpose comparison suggested earlier, where one group was both yoked for aversive stimulation and administered a supposedly neutral pretreatment. The only difference between the group exposed to uncontrollability and the conparison group was the instruction given to each. The fact that Hiroto and Seligman (1975) obtained an experimental effect comparing these groups indicates a strong instructional component to the interference effect in humans. Another experiment

(Glass, Singer, Leonard, Krantz, Cohen, & Cummings, 1973) showed that instructions could also cause facilitation effects. It is clear from these procedures that instructions regarding control significantly augment the actual experience of controllability/ uncontrollability conditions.

There are other ways in which instructions have been used to maximize the significance of exposure to no-control conditions in the human research. Roth and Kubal (1975) informed some people that the pretreatment task was a measure of academic ability. Those people so informed showed significant debilitation relative to people who believed they were simply doing an experimental task in "concept formation." Dyck, Breen, and Eckelman (Note 1) used similar instructions to maximize the saliency of the pretreatment.

Instructional manipulations apparently increase the saliency of the pretreatment. Production of experimental effects which are largely dependent on instructions are easily reversed by subsequent instructions. Reliance on instructional control, then, enables researchers to produce strong effects but stay within the ethical bounds of human research.

Presence of a second person

Human behaviour is strongly influenced by the presence and characteristics of other humans. The introduction of other persons to the uncontrollability situation can therefore serve as a second means of maximizing the saliency of the pretreatment.

Several studies of uncontrollability have shown effects due to the influence of a second person in the experiment.

Glass, Reim, and Singer (1971) subjected people to aversive noise. In three conditions, a confederate posed as another subject. People in the condition where they had no access to a confederate who had the power to turn off the noise (relative deprivation condition) did worse on a subsequent proofreading task than did people who had access to the confederate (indirect control condition) and people for whom the confederate was equally powerless. This result demonstrates (1) that the presence of a second person is only debilitating if that person is comparatively powerful, and (2) that the presence of a second powerful person is only debilitating when one has no opportunity to influence that person. While the findings from this experiment are striking, they must be considered tentative in light of a second study which failed to replicate the original results (Gruzen & Shapiro, cited in Glass & Singer, 1972, pp. 74-78).

Another uncontrollability study using presence of a second person employed a similar procedure. Instead of manipulating the perceived power or control of the second person, Silverstein and Staff (cited in Glass & Singer, 1972, pp. 92-98) manipulated perceptions of the aversiveness of stimulation experienced by the second person. People who believed that they were receiving more intense aversive noise than the other person demonstrated subsequent debilitation of a proofreading measure. This study indicates that the presence of a second person who is perceived as more fortunate also contributes to debilitation effects.

A third uncontrollability study investigated the effects of presence of a second person varying in perceived ability and control (Wortman et al., 1976). These investigators reasoned that presence of a second person of high ability would increase the tendency for the persons experiencing uncontrollability to attribute their failure to control to themselves and not to the experimental task. Results of this experiment indicated that presence of a more competent second person did increase attributions to ability. While the investigators expected this to increase debilitation on subsequent measures of performance, the obtained results instead indicated facilitated performance. These findings indicate that when a more powerful second person is also seen as having more ability, people do not show the debilitation that would otherwise be expected.

These findings taken together show that introduction of a second person to the uncontrollability pretreatment has different effects which appear to depend upon the perceived characteristics of that second person. The second person appears to augment debilitation effects when seen as more powerful or more fortunate, but appears to reverse debilitation effects when seen as having higher ability. There are many other factors in these studies which could also be responsible for these results, so these conclusions must be considered preliminary.

Social psychological analysis of second person effects

The effects of a second person in uncontrollability research can perhaps be better understood by examination of the social psychological literature. To this end, the three research areas of social comparison theory, attribution theory, and equity theory will now be examined.

Social comparison theory. Festinger (1950,1954) founded this theory by proposing that (1) there is a drive to evaluate one's ability, (2) when objective means of evaluation are not available, people evaluate their own abilities by comparing their performances with the performances of others, and (3) the most useful sources of evaluation are people known to have similar ability levels. Some of the research on this theory has tested proposition (1), which is of little concern to uncontrollability research. Studies investigating proposition (2) above have shown that, at least under some circumstances, people seek to compare their performances with those of others (Hakmiller, 1962, 1966; Gordon, 1966; Thornton & Arrowood, 1966; Wheeler, 1966). Vreven and Nuttin (1976) demonstrated that, at least for females, comparison with more successful others tends to lower subjects' estimation of their own performances. The Wortman et al. (1976) experiment described earlier supports the existence of this contrast effect. Discussion of the influence of lowered self-estimation upon subsequent performance is left to the forthcoming review of attribution theory.

Festinger's third proposition was that people would compare themselves with others similar to themselves. Although some studies support this claim (Hakmiller, 1966; Gordon, 1966; Wheeler, 1966), the absence of an operational definition of "similarity" confuses the testing of this hypothesis. Some studies using the existing societal milieu have shown that individuals evaluate their own situations by comparing them to the situations of people or groups with whom they identify (Campbell, Converse, Miller, & Stokes, 1966; Centers, 1949; Patchen, 1961; Stouffer, Suchman et al., 1949). Other studies show that social class can define the bounds of "similarity" in demonstrating that people compare themselves to others in the same social class, but not with others of different social classes (Runciman, 1961; Stern & Keller, 1953).

Comparing outselves with others gives us information concerning what we can expect from or for ourselves. We all lead lives made up of some situations over which we have a high degree of control and others where we have little or no control. We probably do not become debilitated as a result of experience in situations where we never expected to have any control. One of the major means of assessing our potential control in a given situation is to observe the degree of control exhibited by other people in that situation. Most experiments in human uncontrollability give people no opportunity to make such social comparisons. The only possible social comparison in most studies is that between the subject and the experimenter. Since there exist a priori status and power differences between experimenter and subject, studies indicate that such a comparison is unlikely to be salient for the subject once he/she is outside the experimental situation. The social comparison literature suggests that the presence of a second similar person in the experimental situation may affect the degree of debilitation resulting from uncontrollable experience.

Attribution theory. The original formulation of attribution theory by Heider (1958) suggested that people perceive their performance to be a function of personal characteristics or situational factors. Each of these possibilities was again broken down into two categories. One could perceive one's outcomes to be a function of ability or effort (personal characteristics), or of task difficulty or luck (situational factors). Weiner, Frieze, Kukla, Reed, Rest, and Rosenbaum (1971), noting the similarity between Heider's formulation and Rotter's (1966) work on locus of control, cast the four Heider factors into a 2 x 2 matrix with locus of control on one dimension and stability on the other dimension. This conceptual schema is now the predominant one in attributional research.

While there is a large body of research attempting to delineate the factors which determine attributions, many of these involve the pattern of prior reinforcement. There are some studies, more germane to the present problem, which show that knowledge of social norms affects the degree to which performance is attributed to task difficulty (Feather, 1961; Weiner, 1970). Some researchers have suggested that performance which diverges from social norms is likely to be attributed to the internal factors of ability or effort (Frieze & Weiner, 1971; Weiner & Kukla, 1970). While information about social norms is usually conveyed in experimental instructions, the introduction of a second person to the experimental situation could have the same effect. The Wortman et al. (1976) study used this rationale for its two-person procedure.

Recent attribution research has focused on the ways in which causal attributions affect subsequent performance. There are a good number of empirical findings relating attribution to achievement motivation (Weiner, 1974; Weiner, Heckhausen, Meyer, & Cook, 1972). Of particular interest in this review is the literature relating causal attributions to resistance to extinction, since the interference effect is a special case of extinction of responding. In several studies, humans were exposed to repeated failure while measures of performance and causal attributions were taken (Weiner et al., 1971; Weiner et al., 1972). In both studies there was a positive association between decrements in responding and attributions to stable factors (low ability or high task difficulty). Conditions which lead to attributions to these stable factors appear to be conditions which are very susceptible to extinction, and, by implication, susceptible to the interference effect.

Only one study has attempted to separate the two factors of ability and task difficulty. The Wortman et al. (1976) study found facilitated performance associated with attributions of low ability. The procedure used in this study did not, however, separate the two internal factors which could have been affected by the introduction of the second person. Instead, these two factors, ability and effort, were combined into "competence." The facilitated performances by people who rated themselves low on "competence" could well represent their increased effort. The precise relationship between attributions of low ability and subsequent performance must separate ability from both effort and task difficulty. The importance of this separation is underscored by Dweck and Reppucci (1973). They found that children attributing their failures to lack of ability showed impaired performances, while children attributing their failures to effort were much more persistent.

Attribution theory would predict the effects of the introduction of a second person according to the causal attributions prompted by that person's characteristics and by other elements of the situation. Attributions to high task difficulty will result in low levels of performance on that task, but not necessarily on different tasks. Attributions to low effort will result in subsequent facilitated performance (assuming that the situation is important enough to be "worth the effort"). Attributions to low ability will result in decrement in performance on subsequent tasks in the same situations or in different situations. The latter is the mechanism which most closely fits the learned helplessness hypothesis.

Equity theory. Equity theory was first proposed by Homans (1961), who held that a human system would tend toward "distributive justice." This condition is achieved when every person's ratio of inputs to rewards is equal to the ratio for others in that situation. Research in equity theory has employed procedures which induced perceptions of unequal inputs or unequal rewards. The subjects' efforts to equalize the inputs/rewards proportion become the dependent measures.

Equity theory is concerned with the effects of perceived inequity, and therefore would make no predictions about experimental situations where people perceive each other to have equal input/reward proportions. When the second person is perceived as different from the original subject in inputs and/or rewards, equity theory can be helpful in making predictions. Discussion of equity theory will be restricted to the behavioural results of inequity (see Walster, Berscheid, and Walster, 1976, for a complete discussion). Because the chief interest in in augmentation of the interference effect, inequity will be discussed with regard to the behaviour of the "victim."

According to Adams (1965), inequity in a dyad can be rectified by changing the inputs or the rewards of both people. A victim of inequity cannot, however, affect the inputs of another person. There are thus three options available to such a person: (1) increase his or her own rewards, (2) decrease the other person's rewards, or (3) decrease his/her own inputs. Since the victim has minimum control over the allocation of rewards, the first alternative takes the form of demands for restitution. Several studies have shown that victims behaved in this manner (Leventhal & Bergman, 1969; Marwell, Schmitt, & Shotola, 1971). Other studies show that victims choose the second alternative, retaliating against the other person (Berscheid, Boyd, & Walster, 1968; Ross, Thibaut, & Evenbeck, 1971).

The tendency for victims to restore equity by reducing their own inputs has been investigated in experiments involving payment for a performance. Inequity was induced by informing people that they were qualified, underqualified, or overqualified to receive the forthcoming rate of pay. The latter condition was the "victim" condition. People in this condition restored equity by reducing the amount of work they did by lowering its quality (Evan & Simmons, 1969; Lawler & O'Gara, 1967; Pritchard, Dunnette, & Jorgenson, 1972).

A fourth option is open to victims of inequity: they can cognitively restore equity by distorting the situation. This distortion takes the form of either derogating their own performances (Lerner & Matthews, 1967) or reducing the importance of the rewards (Weick & Prestholdt, 1968).

In order to examine the predictions that equity theory would make in uncontrollability experiments, the procedures used in these experiments must be taken into account. Procedures do not allow participants in these experiments to change their own contingencies. That is, they can only affect their own outcomes by changing their own inputs. In the current studies which employ a second person in the procedure, the second person's contingencies are also beyond the influence of the original participant. Uncontrollability studies, then, allow subjects to restore equity in only two ways: by changing their own inputs and by making cognitive adjustments. Borrowing from attribution theory, inputs in this situation can be categorized into ability and effort. A person placed in an inequitable situation (that is, where the other person is getting more rewards) can be expected to either (1) decrease his/her own inputs by expending less effort, or (2) thinking his/her ability to be low and the other person's ability to be high. These behaviours are identical to the behaviours seen in the effect labeled "learned helplessness."

It should be noted here that equity theory makes no predictions about behaviour antecedent to the establishment of the input/reward ratios for both parties. Research designed specifically to test equity theory establishes these proportions at the beginning of the experimental procedure via instructions. Uncontrollability research allows people to make their own discoveries about their input/reward ratios (i.e., contingencies of reinforcement) and thus has no means by which to detect the point at which these proportions become established. The facilitation effects obtained by Wortman et al. (1976) may thus reflect behaviour antecedent to the establishment of the input/reward proportions.

Control by a peer

The social psychology literature was reviewed in order to assess the effect of introducing a second (more competent) person into the uncontrollability experimental procedure. Further exploration of the two-person experimental situation leads to consideration of the case where one person has control over the other. Studies in social comparisons discussed above show that comparison to similar others is more salient than comparison to others who are not perceived as similar. The same hypothesis could be made about uncontrollability; namely, that to have no control *vis a vis* a similar other is more salient than having no control *vis a vis* another person who is dissimilar.

Experiments which have employed procedures allowing one subject control over a second subject have all been conducted as experiments in cooperation. Hake and Vukelich (1972) have classified cooperation research into interdependent procedures and dependent procedures. In the former, responses from both individuals are necessary to produce reinforcement, where in the latter, one person's reinforcers depend only on the other person's responses. Since the present interest in in uncontrollability, the experiments employing dependent procedures are the relevant ones.

One group of such studies (Sidowski, Wyckoff, & Tabory, 1956; Sidowski, 1957) employed procedures where participants were given two buttons and instructed to try to accumulate points. One of these buttons delivered shock to the other person, while the other button gave points to the other person. Results indicated that stable rates of responding were established when the point-giving button was operative. These stable rates of responding were independent of the participants' knowledge that they were working with partners. Another study (Rabinowitz, Kelley, & Rosenblatt, 1966) used similar procedures to study two kinds of dependence: (1) where both people could control the other's outcomes, and (2) where the first person controlled the other's outcomes and the second person controlled the other's behaviours. Results indicated that both situations produced stable rates of responding.

Boren (1966) placed two sets of monkeys in an apparatus where the operant responses of one monkey led to delivery of food to the other monkey. He found that stable rates of responding occurred when the monkeys were signaled such that they alternated responses. Under free operant conditions, however, the reciprocity between the monkeys degenerated. This two-organism contingency pattern, called a "backscratch contingency" (Powers & Powers, 1971) has been used with retarded children (Powers & Powers, 1971; Williams, Note 3; Williams, Martin, McDonald, Hardy, & Lambert, 1974). The contingency is desirable with this population because it apparently leads to increases in social interaction between children. One can speculate that the reason for this increased social interaction is that it is the only means of control available to the children.

In all of these procedures, the two subjects experienced lack of control over their own reinforcers. Studies using humans all obtained stable response rates, indicating either (1) that uncontrollability *vis a vis* a peer does not lead to responding decrements when the uncontrollability is mutual or (2) that control over a peer's reinforcers constutites some degree of control over one's own reinforcers. These studies attempt to analyze a very complex dyadic behaviour system. Procedures which control some of the inter-subject dependencies are needed to ascertain the functional mechanisms at play in these dyads.

Summary and conclusions

The research on the interference effect demonstrates some degree of support for the existence of the effect in humans. Examination of this research reveals that much of it has employed cognitive manipulations. Such procedures increase the saliency of exposure to uncontrollability, allowing production of the interference effect within ethical bounds. Social psychology and operant research suggest that introduction of a second person to the setting of uncontrollability research will also increase the saliency of the pretreatment and thereby augment the interference effect in humans.

APPENDIX A

Intersession Questionnaire

1. How successful were you at this task?

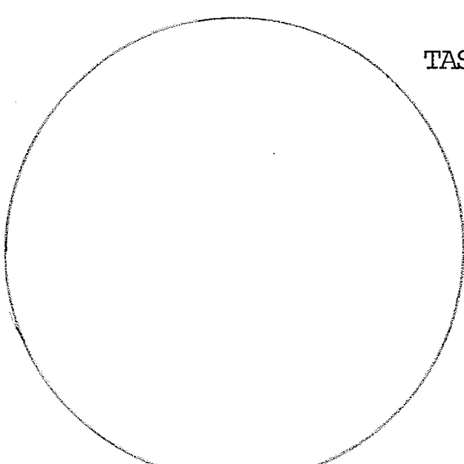
_____Completely successful
_____Successful
_____Slightly successful
_____Neither successful nor unsuccessful
_____Slightly unsuccessful
_____Unsuccessful
_____Completely unsuccessful

2. How successful was your partner at this task?

_____Completely successful
_____Successful
_____Slightly successful
_____Neither successful nor unsuccessful
_____Slightly unsuccessful
_____Unsuccessful
_____Completely unsuccessful

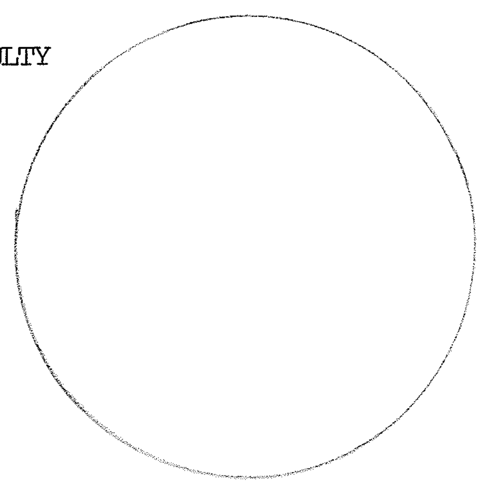
3. People attribute their performances to several different kinds of causes. For this question, consider performance on the task you just did as due to one or more of the following four factors: ability, effort put into the task, the difficulty of the task, and just plain luck. Think of your performance on the puzzles and decide how much each of these four factors was responsible for your performance. Now divide up the first circle below (the one on the left) according to the relative weight you give to each of the four factors. Do it as if you were dividing up a pie. Divide up the other circle (the one on the right) according to the weights of the four factors in your partner's performance on the puzzles. The four factors are listed in the center so you can label the "pieces of the pie" for both circles. Inside your desk is a ruler to help you draw straight lines.

YOUR PERFORMANCE



LUCK
TASK DIFFICULTY
EFFORT
ABILITY

PARTNER'S PERFORMANCE



4. How well do you expect to do on the next task?

_____Extremely poorly
_____Poorly
_____Slightly below average
_____About average
_____Slightly above average
_____Well
_____Extremely well

5. How well do you expect your partner to do on the next task?

_____Extremely poorly
_____Poorly
_____Slightly above average
_____About average
_____Slightly above average
_____Well
_____Extremely well

APPENDIX B

Test task

SAMPLE

The administration woold have filled the request, but after/the strike such a move was impossible.

Slums and their populations are the victims (and the perpetruators) of seemingly endless troubles that reinforce each other. Slums operate as vicious circles. In time, these vicious circles enmesh the whole operation of cities. Spreading slums require even greater amounts of public money--and not simply more money for publicly financed improvment, or to stay even, but more money to cope with ever widening retreat and regression. As needs grow greater, the wherewithal grows less.

Our present ur ban renewal laws are an attempt to break this particular linkage in the vicious circles by forthrightly wiping away slums and their populations, and replacing them with projects intended to produce higher tax yields, or to lure back easier populations with less expensive public requirements. The method fails. At best: it merely shifts slums from here to there, adding its own tincture of extra hardship and dirsumption. At worst, it destroys neighborhoods where constructive and improving communities exist and where the situation calls for encourcgement rather than destruction.

Slum shifting fails because it tries to overcome causes of trouble by diddling with symptoms. Sometimes even the very symptoms that preoccupy the slum shifters are, in the main vestiges of former troubels rather than significant indications of future ills.

Conventional planning approaches to slums and slum dwellers is thoroughly peternalitsic. The trouble with paternalists is that they want to make impossible profound changes, and they choose impossibly superficil means for doing so. To overcome slums, we must regard slum dwellers as people capable of understanding and acting upon their own self-interests, which they certainly are. We nned to discern, respect and bui upon the forces for regeneration that exists in slums themselves, and that demonstrably work in real cities. This is far from what is done to day.

Vicious circles, to be sure, are hard to follow. Cause and affect become confused precisely becauce they do link and relink one another in such complicated ways.

Yet there is one particular link that is crucial. If it is broken (and to break it is no simple matter of supplying better housing), a slum spontaneously unslums.

The key link in a perpetrual slum is that too many people move out of it too fast--and in the meantime dream of getting out. This is the link that has to eb broken if any other efforts at overcoming slums are to be of the least avail. This is the link that actually was broken and has stayed broken in places like North End, or the Back-of-the-Yards in Chicago, or in North beach in San Francisco, or the unslummed former slum in which I live. If only a handful of American city slums had aver managed to break this link, we might regard them skeptically as grounds for hope. These places might be freaks. More significant are the great number of slum neighborhoods in which unslumming starts, does unrecognized, and too often is discouraged or destroyed. The portions of East Harlem in New York which had proceeded far along in unslumming were first discouraged by unavailability of ne money; then where this slowed the unslumming process but wtill did not bring regression to slum conditions, most of these neighborhoods were destroyed outright--to be replaced by projects which became almost pathological displays of Slum broubles. Many areas in the Lower East Side which have started unslumming have been destroyed. My own neighborhood, as recently as the early 1950's was saved from disastrous amputation only because its citizens were able to fight city hall and even at that, only because the officials were confronted with embarrassing evidence that the area was drawing in newcomers with money, although this symptom of its unslummed status was possibly the least significant of the constructive changes that had occurred unnoticed.

Herbert Gans, a sociologist at the University of Pennsylvania, has given, in the Frebruary 1959 Journal of the American Institute of Planners, a sober butpoingant portrait of an unrecognised unslumming slum, the West End of Boston, on the eve of its destruction. The West Ends, he points out, although regarded offocially as a slum, would have been more accurately described as a "stable, low-rent area". If, writes Gans, a slum is defined as an area which"because of the nature of its social environment can be proved to create problems and pathologies', than the West End was not a slum. He speaks of the intense attachment of residents to the district, of its highly developed informal social control, of the fact that many residents had modernized or improved the interiors of theor apartments--all typical characteristics of an unslumming slum.

Unslumming hinges, paradoxically, on the retentionof a very considerable part of a slum population within a slum. It hinges on whether a a considerable number of the residents and businessmen of a slum find it both desirable and practical to make and carry out their own plans right there, or whether they must virtually all move elsewhere.

I shall use the designation of "perpatual alums" to describe slums which show no signs of social or economic improvement with time, or which regress after a little improvement. However: of the condition for generating city diversity can to intorduced into a neighborhood while it is a slum, and if any indications of unslumming are encouraged rather than thwarted, I believe there is no reason that any slum need to perpetual.

The inability of a perpetual slum to hold enough of its population for unslumming is a characteristic that starts before the slum itself starts. There is a fiction that slums in forming malignantly supplant-healthytissue. Nothing could be farther from the truth.

The first sign of an incipent slum, long befoer visible blight can be seen, is stagnation and dullness, Dull neighborhoods are inevitably deserted by their energetic, ambitious, or affluent citizens, and also by their young people who can get away. They inevitably fail to draw newcmers by choice. Furthermore, aside from these selective desertions and the selective lack of vigorous new blood, such neighborhoods eventually are apt to undergo rather sudden wholesale desertions by their nonslum populations.

Nowadays, the wholesale desertion by nonslum populations which give a slum its initial opportunity to form, are sometimes blamed on the proximity of another slum (especially if it is a Negro slum) or on the presence or proximity of Italian or Jewish or Irish families. Sometimes the desertion is blamed on the age and obsolescence of dwellings, ot on vague, general disadvantages such as lack of playgrounds or proximity of factories.

However, all suvh factors are imaterial. In Chicago, you can see neighborhoods only a block or two blocks in from the Lakefront partkland, for from the settlements of minority groups, well endowed with greenery, quiet enough to make one's flash creep, and composed of subs tantial, even pretentious buildings. On these neighborhoods are the literal signs of the desertion; "For Rent," "To Let," "Vacancy" Rooms for permanent and Transient Guests," "Guests Welcome," "Sleeping Rooms," "Furnished Rooms," "Unvurnished Rooms," "Apartments Available." These buildings have trouble drawing occupants in a city where the colored citizens are cruelly overcrowded in their shelter and cruelly overchanrged for it. The buildings are going begging because they are being rented or sold only to whites--and whites, who have so much more choice, do not care to live here. The beneficiaries of this particular impasse, at least for the moment, turn out to be immigrating hillbillies, whose economic choice is small and whose familiarity with city life are still smaller. It is a dubious benefit they receive: inheritance of dull and dangerous neighborhoods whose unfitness for city life finally repelled residents more sophisticated and competent then they.

Sometimes to be sure, a deliberate conspiracy to turn over the population of a neighborhood does exist--on the part of real estate operators who make a racket of buying houses cheaply from paniced white people and seeling them at exorbitant prices to the chronically housing-starved and pusshed-around colored population. But even this racket work s only in already stagnated and low vitality neighborhoods. (Sometimes the racket perversely improves a neighborhood's upkeep, when it brings in colored citizens more competent in general and more economically able than the whiles they replaced; but the exploitative economics sometimes results instead in replacement of an uncrowded, apathetic neighborhood with an overcroded neighborhood in considerable turmoil.)

If there were no slum dwellers or poor immigrants to inherit city failures, the problem of low-vitality neighborhoods abandomed by those whith choice would still remain and perhaps would be even more troubling. This condition can be found in parts of Philadelphia where "decent, safe and sanitary" dwellings go empty in stagnated neighborhoods, while their former populations mroe outward into new neighborhoods which are little different, intrinsically, from the old except that they are not yet embedded by the city.

It is easy to see where new slums are spontaneously formin g today, and how dull, dark and undiversė are the streets in which they typically form, because the process is happening now, What is harder to realize, because it lies in the past, is the fact that lack of lively urbanity has usually been an original characteristic of slums. The classic repfrm literature about slums does not tell us this. Such literature--Lincoln Steffens' Autobiography is a good example--focused on slums that had already overcome their dull beginnings (but had aquired other troubles in the mean time). A teeming, bustling slum was pinpointed at a moment in time, with the deeply erroneus implication that as a slum is, so it was--and as it is, so it shall be, unless it is wiped away root and branch.

The unslummed former slum in which I live was just such a teeming place by the early becades of this century, and its gang, The Hudson Dusters, was notorious thoghout the city, but its career as a slum did not begin in any such bustle. The hisoty of the Episcopal chapel a few blocks down the street tells the tale of the slum's formetion, almost a centory ago in this case. The neighborhood had been a place of farms, village streets and summer homes which evovled into a semisuburb that became embedded in the rapidly growing city. Colored people and immigrants from Europe were surrounding it; neigher physically nor socially was the neighborhood equipped to handle their presence--no more, apparently, than a semisuburb is so equipped today.

Out of this quiet residential area--a charming place, from the evidence of old pictures--there were at first many random desertions by congregation families; those of the congregation who remained eventually panicked and departed en masse. The church building was abandoned to Trinity parish, which took it over as a mission chapel to minister to the influxof the poor who inherited the semisuburb. The former congregation re-established the church far uptown, and colonized in its neighborhood a new queit residential area of unbelievable dullness; it is now a part of Harlem. The records do not tell where the next preslum was built by these wanderers.

The reasons for slum formation, and the processes by which it happens, have changed surprisingly little over the decades. What is new is that unfit neighborhoods can be deserted more swiftly, and slums can and do spread thinner and farther, than was the case in the Days before automobiles and government-quaranteed morgages for suburban developments, when it was less practical for vamilies with choice to flee neighborhoods that were displaying some of the normal and inevitable conditions that accompany city life such as presence of strangers) but none of the natural means for converting these conditions into assets.

At the time a slum first forms, its oppulation may rise spectacularly. This is not a sign of popularity, however. On the contrary, it means the dwellings are becoming overcrowded; this is happening because people with the least choce, forcced by poverty or discrimination to overcrowd, are coming into an unpopular area.

The density of the dwělling units themselves may or may not increase. In old slums, they customarily did increase because of the construction of tenements. But the rise in dwelling density typically did not cut down the overcrowding. Total population increased greatly instead, with overcrowding superimposed on the high dwělling densities.

APPENDIX C

Postsession Questionnaire

1. How unpleasant did you find the noise?

_____I found the noise extremely unpleasant.
_____I found the noise unpleasant.
_____I found the noise slightly unpleasant.
_____I found the noise neither pleasant nor unpleasant.

2. To what extent did you feel that you and your partner were treated equally in this experiment?

_____I feel I was treated much better than my partner.
_____I feel I was treated better than my partner.
_____I feel I was treated slightly better than my partner.
_____I feel my partner and I were treated equally.
_____I feel I was treated slightly worse than my partner.
_____I feel I was treated worse than my partner.
_____I feel I was treated much worse than my partner.

3. To what extent did you feel that your performance on the first task (the puzzles) influenced your performance on the second task (the proofreading)?

_____I feel my performance on the first task greatly influenced my performance on the second task.
_____I feel my performance on the first task influenced my performance on the second task.
_____I feel my performance on the first task influenced my performance on the second task to a slight degree.
_____I feel my performance on the first task did not influence my performance on the second task.

4. How many noise bursts did you receive?

_____I received very few noise bursts.
_____I received few noise bursts.
_____I received a moderate number of noise bursts.
_____I received many noise bursts.
_____I received very many noise bursts.

5. How many noise bursts did your partner receive?

_____My partner received very few noise bursts.
_____My partner received few noise bursts.
_____My partner received a moderate number of noise bursts.
_____My partner received many noise bursts.
_____My partner received very many noise bursts.

6. Which of the following best expresses your estimation of the difficulty level of the puzzles?

_____I felt that all of the puzzles were impossible (i.e., mathematically insoluble).
_____I felt that all of the puzzles were very hard and a couple were impossible.
_____I felt that the puzzles were very hard.
_____I felt that the puzzles were of moderate difficulty.
_____I felt that the puzzles were easy.
_____I felt that the puzzles were extremely easy.

7. Which of the following best expresses the degree to which you felt that the noise in the first part of the experiment was dependent on your performance on the puzzles?

_____I felt the noise bursts were completely independent of my performance on the puzzles.
_____I felt the noise bursts were dependent on my performance on the puzzles to a lsight degree.
_____I felt the noise bursts were dependent on my performance on the puzzles.
_____I felt the noise bursts were completely dependent on my performance on the puzzles.

8. Which of the following best expresses your personal feelings about your partner?

_____I feel that I would probably like this person very much.
_____I feel that I would probably like this person.
_____I feel that I would probably like this person to a slight degree.
_____I feel that I would neither particularly like nor particularly dislike this person.
_____I feel that I would probably dislike this person to a slight degree.
_____I feel that I would probably dislike this person.
_____I feel that I would probably dislike this person very much.

9. Which of the following best expresses your feelings about working together in another experiment with your partner?

_____I believe I would very much dislike working with this person in an experiment.
_____I believe I would dislike working with this person in an experiment.
_____I believe I would dislike working with this person in an experiment to a slight degree.
_____I believe I would neither particularly like nor particularly dislike working with this person in an experiment.
_____I believe I would enjoy working with this person in an experiment to a slight degree.
_____I believe I would enjoy working with this person in an experiment.
_____I believe I would very much enjoy working with this person in an experiment.

10. Which of the following best expresses your personal feelings about the experimenter?

_____I feel that I would probably like this person very much.
_____I feel that I would probably like this person.
_____I feel that I would probably like this person to a slight degree.
_____I feel that I would probably neither particularly like nor particularly dislike this person.
_____I feel that I would probably dislike this person to a slight degree.
_____I feel that I would probably dislike this person.
_____I feel that I would probably dislike this person very much.

11. Which of the following best expresses your feelings about participating in another study conducted by this experimenter?

_____I believe I would very much dislike working with this person in an experiment.
_____I believe I would dislike working with this person in an experiment.
_____I believe I would dislike working with this person in an experiment to a slight degree.
_____I believe I would enjoy working with this person in an experiment.
_____I believe I would very much enjoyr working with this person in an experiment.

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