

## Judgment of Contingency in Depressed and Nondepressed Students: Sadder but Wiser?

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### SUMMARY

How are humans' subjective judgments of contingencies related to objective contingencies? Work in social psychology and human contingency learning predicts that the greater the frequency of desired outcomes, the greater people's judgments of contingency will be. Second, the learned helplessness theory of depression provides both a strong and a weak prediction concerning the linkage between subjective and objective contingencies. According to the strong prediction, depressed individuals should underestimate the degree of contingency between their responses and outcomes relative to the objective degree of contingency. According to the weak prediction, depressed individuals merely should judge that there is a smaller degree of contingency between their responses and outcomes than nondepressed individuals should. In addition, the present investigation deduced a new strong prediction from the helplessness theory: Nondepressed individuals should overestimate the degree of contingency between their responses and outcomes relative to the objective degree of contingency.

In the experiments, depressed and nondepressed students were presented with one of a series of problems varying in the actual degree of contingency. In each problem, subjects estimated the degree of contingency between their responses (pressing or not pressing a button) and an environmental outcome (onset of a green light). Performance on a behavioral task and estimates of the conditional probability of green light onset associated with the two response alternatives provided additional measures for assessing beliefs about contingencies.

Depressed students' judgments of contingency were surprisingly accurate in all four experiments. Nondepressed students, on the other hand, overestimated the degree of contingency between their responses and outcomes when noncontingent outcomes were frequent and/or desired and underestimated the degree of contingency when contingent outcomes were undesired. Thus, predictions derived from social psychology concerning the linkage between subjective and objective contingencies were confirmed for nondepressed students but not for depressed students. Further, the predictions of helplessness theory received, at best, minimal support.

The learned helplessness and self-serving motivational bias hypotheses are evaluated as explanations of the results. In addition, parallels are drawn between the present results and phenomena in cognitive psychology, social psychology, and animal learning. Finally, implications for cognitive illusions in normal people, appetitive helplessness, judgment of contingency between stimuli, and learning theory are discussed.

*Development of the Concept of Contingency in Learning Theory*

An important part of animals' and humans' knowledge of the world is the knowledge of the relationship or contingency between events. The concept of contingency provides a cornerstone for a number of highly influential contemporary theories of learning (e.g., Bindra, 1972; Bolles, 1972; Mackintosh, 1975; Maier & Seligman, 1976; Rescorla, 1967; Rescorla & Wagner, 1972). Each of these theories posits an organism that is sensitive to relationships between stimuli in its environment and/or between its own responses and environmental outcomes. An important question emerges in an analysis of an organism's commerce with environmental contingencies: Do organisms form subjective representations (i.e., beliefs, expectations, or cognitive representations) of contingencies that mirror objective, real world contingencies? This article focuses on an empirical answer to this question.

Contemporary theorists distinguish between contingency and temporal contiguity or pairing views of learning (Premack, 1965; Prokasy, 1965; Rescorla, 1967). For example, Rescorla argued that contingency, rather than temporal contiguity, is the essential condition for "true" Pavlovian conditioning. The crucial distinction between the pairing and contingency views is that the former

centers on the relationship between the conditional stimulus (CS) and the unconditional stimulus (UCS), whereas the latter centers on both this relationship and the relationship between the absence of the CS ( $\overline{\text{CS}}$ ) and the UCS. A large body of evidence on animal learning supports the assertion that animals are, in fact, sensitive to both positive and negative contingencies (e.g., Rescorla, 1969c; Rescorla & Wagner, 1972; Wagner & Rescorla, 1972).

In addition, Rescorla (1967) has pointed out that in contrast to the contiguity view, the concept of contingency has an important advantage in that it allows one to distinguish between mere absence of excitatory conditioning and an active inhibitory process. According to the contingency view, inhibitory conditioning occurs whenever there is a negative relationship between CS and UCS (i.e., the UCS occurs more often in the *absence* of the CS than in its presence). This prediction has been strongly confirmed (Bull & Overmier, 1968; Dweck & Wagner, 1970; Hammond, 1966, 1967, 1968; Hammond & Daniel, 1970; Kimble & Ost, 1961; Moskowitz & Lolordo, 1968; Reiss & Wagner, 1972; Rescorla, 1967, 1969a, 1969b, 1969c, 1971; Rescorla & Lolordo, 1965; Siegel & Domjan, 1971; Weisman & Litner, 1969, 1972). Finally, a contingency account of conditioning suggests that the only appropriate baseline against which to assess the associative effects of both excitatory and inhibitory conditioning is a condition in which there is no contingency between CS and UCS. Indeed, Rescorla (1967) has argued for just such a "truly random control" procedure.

From the standpoint of the contingency learning framework, it is important to know what the organism learns when there is no objective contingency between stimulus and reinforcer. Does the organism merely learn nothing about the stimulus, or does it learn explicitly that stimulus and reinforcer are, in fact, uncorrelated? Mackintosh's (1973, 1975) investigations of the phenomenon of "learned irrelevance" have led him to support the latter position. Thus, Mackintosh (1973) has found that random presentations

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of a given CS and UCS specifically retard the subsequent formation of an association between the two. To account for this phenomenon, Mackintosh (1973, 1975) proposed an attentional theory that posits that organisms actively learn to ignore stimuli uncorrelated with reinforcement.

Paralleling its development within the Pavlovian tradition, the concept of contingency also has received increasing attention in theoretical accounts of instrumental learning (e.g., Bloomfield, 1972; Catania, 1971; Church, 1969; Gibbon, Berryman, & Thompson, 1974; Maier, Seligman, & Solomon, 1969; Premack, 1965; Seligman, Maier, & Solomon, 1971; Weiss, 1968). For example, Seligman et al. (1971) have argued that organisms are sensitive to conjoint variations in two response–outcome probabilities: the conditional probability of an outcome given the occurrence of a response,  $P(O/R)$ , and the conditional probability of that outcome given the nonoccurrence of that response,  $P(O/\bar{R})$ . According to Seligman et al., any account of instrumental learning that fails to incorporate both of these probabilities is incomplete.

Seligman and his colleagues (Maier & Seligman, 1976; Maier et al., 1969; Seligman et al., 1971), in their investigation of the “learned helplessness” phenomenon, have emphasized the theoretically interesting case in which the two response–outcome probabilities are equal. These investigators argue that organisms that have been exposed to aversive events that terminate independently of any instrumental responses actively learn that reinforcement is independent of their behavior and that these events are, in fact, uncontrollable. A great deal of evidence supports the contention that organisms are sensitive to response–outcome independence (for a review of these studies see Maier & Seligman, 1976, and Seligman, 1975b). Furthermore, according to the learned helplessness hypothesis, such learning results in motivational and cognitive deficits as well as in emotional disturbance. The motivational component of learned helplessness may be described as reduced incentive for initiating voluntary responses, and the cognitive

deficit is reflected in difficulty in learning future response–outcome contingencies. Finally, the learned helplessness hypothesis also states that organisms that have learned that outcomes are noncontingently related to responses become “depressed” (see Maier & Seligman, 1976, and Seligman, 1975b, for a review of the relevant evidence).

The debilitating consequences of exposure to situations in which responses and outcomes are unrelated have been observed across a wide variety of experimental situations and within a large number of species, including dogs (Overmier, 1968; Overmier & Seligman, 1967; Seligman & Groves, 1970; Seligman & Maier, 1967), mice (e.g., Braud, Wepman, & Russo, 1969), rats (e.g., Baker, 1976; Goodkin, 1976; Maier, Albin, & Testa, 1973; Maier & Testa, 1975; Seligman & Beagley, 1975; Seligman, Rosellini, & Kozak, 1975; Williams & Maier, 1977), fish (Padilla, Padilla, Ketterer, & Giacalone, 1970), cats (e.g., Seward & Humphrey, 1967), and humans (e.g., Fosco & Geer, 1971; Glass & Singer, 1972; Hiroto, 1974; Hiroto & Seligman, 1975; Klein, Fencil-Morse, & Seligman, 1976; Klein & Seligman, 1976; Miller & Seligman, 1975; Roth & Bootzin, 1974; Roth & Kubal, 1975; Thornton & Jacobs, 1971).

Of particular import to the present investigation are the human helplessness studies, because they provide evidence about the sensitivity of humans to the objective presence or absence of contingencies between their responses and environmental outcomes. Hiroto's (1974) experiment is representative of human helplessness studies. In a triadic design analogous to those employed in the animal helplessness studies, Hiroto compared three groups of college students: an “escapable” group that received experience with noises contingent on button-pressing responses, an “inescapable” group that received experience with noises noncontingently related to button pressing, and a “no noise” group. All groups were subsequently tested on a human shuttlebox for escape/avoidance from noise. The results were similar to those obtained from other species: Students who had received prior

exposure to noncontingent noise showed impaired performance of the requisite escape/avoidance response in the shuttle box test compared to students receiving prior exposure to contingent noise or no noise. These results are typical and provide evidence for the motivational and cognitive components predicted by the learned helplessness hypothesis.

The emotional component of learned helplessness has come under investigation. In two studies of the affective component, non-depressed college students exposed to uncontrollable noises became more depressed relative to either a group that received controllable noises (Gatchel, Paulus, & Maples, 1975) or to a group that received no noises (Miller & Seligman, 1975). Noting the similarities between the behaviors exhibited by animals and humans exposed to noncontingent aversive events in the laboratory and the behavioral symptoms of naturally occurring depression in humans, Seligman and his colleagues (Abramson, Seligman, & Teasdale, 1978; Miller, Rosellini, & Seligman, 1977; Seligman, 1975a, 1975b; Seligman, Klein, & Miller, 1976) have proposed a learned helplessness theory of depression. The helplessness theory claims that the expectation that important outcomes and responding are independent causes the major motivational, cognitive, and emotional symptoms of human depression.

### *Subjective Reality of Contingency*

This brief review of experimental evidence bearing on contingency learning does indeed suggest that both animals and humans are sensitive to the presence and absence of correlations between stimuli and/or responses and outcomes. More specifically, this evidence points to the role of objective contingencies as determinants of organisms' behavior in Pavlovian and instrumental learning situations. Although the experimental evidence underscores the need for a concept of contingency in theoretical accounts of learning, the role of subjective representations of contingencies in such learning is still very much at issue. Res-

corla and Wagner (1972), for example, in their model of Pavlovian conditioning, have developed a molecular theoretical formulation that does not invoke a concept of contingency with subjective reality for their experimental subjects. Maier and Seligman (1976), on the other hand, argue that objective contingencies are represented subjectively and that this representation directly influences the organism's behavior.

### *Methods for Assessing Humans' Representations of Contingency*

Although it is difficult to demonstrate convincingly that animals have cognitive representations of objective contingencies, methods developed in experiments on human learning may provide a more conclusive test of the subjective reality of concepts of contingency. Although the majority of human helplessness studies have examined performance decrements within the transfer of training paradigm, Seligman and his associates (Abramson, Garber, Edwards, & Seligman, 1978; Klein & Seligman, 1976; Miller & Seligman, 1973, 1975, 1976) have employed what appears to be a more direct method for assessing humans' cognitive representations of contingencies. These investigators have noted the similarity between the helplessness concept that outcomes and responding are independent and Rotter's (1966) concept of external control.

Rotter and his associates (James, 1957; James & Rotter, 1958; Phares, 1957; Rotter, Liverant, & Crowne, 1961) used tasks in which success appeared to be determined by either chance or skill. They demonstrated that verbalized expectancies for future success are affected by outcomes of previous trials. In particular, they found that outcomes of previous trials have a greater effect on expectancies for future success when the person believes outcomes are dependent on responses (skill determined) than when he or she believes outcomes are independent of responses (chance determined).

Employing this logic, Miller and Seligman (1976) and Klein and Seligman (1976)

examined verbalized expectancies of success on skill and chance tasks for college students given prior exposure to contingent, noncontingent, or no noises. In both studies, students exposed to prior uncontrollable noises showed less expectancy change in an ostensible skill task than students exposed to prior controllable noises or no noises, although the groups did not differ on a chance task. From these results, Miller and Seligman (1976) and Klein and Seligman (1976) inferred that helpless students had acquired a generalized expectancy of response–outcome independence.

However, a conceptual problem precludes acceptance of the chance–skill method as providing an assessment of humans' cognitive representations of response–outcome contingencies. Recent developments in attribution theory (Weiner et al., 1971; Weiner, Nierenberg, & Goldstein, 1976) suggest that changes in expectancy of success are a function of the perceived likelihood that factors that produced prior successes will be present again in the future rather than a function of subjects' perceptions of response–outcome contingencies. Therefore, although learned helplessness theory postulates that organisms acquire cognitive representations of objective contingencies, no method within the helplessness framework has yet proved adequate to assess the subjective reality of such concepts of contingency.

A more promising method for assessing humans' capacities for cognitive representation of contingencies was developed by Jenkins and Ward (1965) independently of work on helplessness. They presented subjects with a series of contingency problems in an instrumental learning situation. For each problem, subjects were given 60 trials on which a choice between two responses (Button 1 and Button 2) was followed by one of two possible outcomes (score or no score). All subjects received some problems in which responses and outcomes were contingently related and other problems in which responses and outcomes were noncontingently related. It is interesting that Jenkins and Ward used problems in which the desired outcome (score) occurred frequently

but noncontingently as well as problems in which it occurred rarely but noncontingently. At the end of each problem, subjects were asked to rate, on a 0 to 100 scale, the degree of control (contingency) that their response choices had exerted over the outcomes. The experimenters argued that a contingency between response and outcome exists (i.e., the outcome is controllable) when the probability of that outcome given the occurrence of one response differs from the probability of that outcome given the occurrence of another response. When, for all responses, there is no difference between these conditional probabilities, the outcome is said to be uncontrollable. Whereas Seligman et al. (1971) only distinguished between contingent and noncontingent cases, Jenkins and Ward extended the concept of control to include the *degree* of control (or contingency) defined as the magnitude of the difference between the two relevant conditional probabilities.

The results from Jenkins and Ward's (1965) studies indicate that humans' subjective representations of contingencies are not isomorphic with objective contingencies. Regardless of the actual degree of contingency, subjects' ratings of degree of control correlated highly only with the number of successful trials (the number of trials on which the score card appeared) and were completely unrelated to the actual degree of control.

Results similar to those reported by Jenkins and Ward have been obtained in studies of the effects of noncontingent reinforcement on performance in learning tasks. For example, Bruner and Revusky (1961) found that human subjects in an instrumental learning situation developed complex response patterns, which were irrelevant to the production of reinforcement, on nonfunctional telegraph keys. Similarly, Wright (1962) showed that subjects' response patterns were more orderly at high levels of noncontingent reward than at intermediate levels. Finally, in a stimulus prediction situation, Hake and Hyman (1953) found that subjects did not respond to a random series of binary digits as if it were random.

### *Factors Predicted to Affect the Judgment of Contingency*

A general theme runs through these contingency learning studies: People often treat noncontingent situations as if they were contingent. They act as though outcomes are dependent on responses when they are not and as though one event can be predicted from another when it cannot. Given these behavioral results, it is not unreasonable to expect that peoples' representations of contingencies also may differ systematically from the objective, real world contingencies. From these studies, one might surmise that a variable likely to systematically affect the relationship between subjective contingencies and objective contingencies is the frequency of the desired outcome. People may be more likely to believe that a contingency exists between their responses and desired environmental outcomes when those outcomes occur with relatively high frequency.

A second source of hypotheses concerning the linkage of objective contingencies to subjective contingencies is the clinical and experimental work on the psychopathology of depression. As noted earlier, the learned helplessness model of depression states that when people acquire the expectation that important outcomes and responses are independent, they exhibit the major motivational, cognitive, and affective symptoms of depression. Similar theories of depression, embodying the core concept of helplessness or hopelessness, have been proposed by clinicians (e.g., Arieti, 1970; Beck, 1967; Bibring, 1953; Lichtenberg, 1957; Melges & Bowlby, 1969).

A deduction from the learned helplessness model is that depressed individuals often will underestimate the degree of contingency between their responses and environmental outcomes (e.g., Miller & Seligman, 1973; Seligman, 1975b). Seligman and his associates recently have tested his prediction by employing the chance-skill method discussed earlier. Similar to nondepressed college students who received experience with uncontrollable events, naive depressed students exhibited smaller expectancy changes following success (Klein & Seligman, 1976; Miller

& Seligman, 1973, 1975) and failure (Klein & Seligman, 1976; Miller & Seligman, 1975, 1976) on a skill task than did nondepressed students who received experience with controllable events or naive nondepressed students. In addition, Abramson et al. (1978) reported that unipolar depressives also showed small expectancy changes in a skill task relative to hospitalized control subjects and schizophrenics. Although the expectancy change differences between depressed and nondepressed individuals are robust and often replicated findings, the interpretation of these findings with respect to depressives' cognitive distortions of contingencies is still unclear. As noted earlier, expectancy changes on chance and skill tasks may not be valid indices of people's beliefs about response-outcome contingencies.

Our summary of the work on human contingency learning and depression suggests that people's cognitive representations of response-outcome relationships may not directly mirror environmental contingencies. Studies of people's behavior in noncontingent situations provide a clue that one possible source of systematic error in transduction of environmental contingencies is the nature and frequency of the outcome itself. In addition, the learned helplessness model of depression suggests that depressed and nondepressed individuals also differ systematically in their subjective representations of response-outcome relationships. Both a strong and a weak prediction may be deduced from the helplessness model. According to the strong prediction, depressed individuals underestimate *absolutely* the degree of objective contingency between their responses and outcomes. According to the weak prediction, depressed individuals merely underestimate the degree of objective contingency *relative* to nondepressed individuals. Both views, therefore, predict a net difference in subjective judgments of contingencies between depressed and nondepressed people. However, the strong view further specifies that depressed individuals underestimate the degree of contingency relative to the objective contingency.

The present investigation tested these pre-

dictions by examining subjective judgments of contingency in depressed and nondepressed students using a modification of the method developed by Jenkins and Ward (1965). A number of measures, including behavioral indices as well as verbalized judgments, were used to assess students' representations of contingency. Students were asked to judge the degree of control their responses exerted over outcomes rather than the degree of contingency between responses and outcomes. Contingency is a general term and refers to the degree of relationship between any two events. In the Pavlovian conditioning situation, the events of interest are both stimuli, and the relationship between them is best construed as one of predictability. Alternatively, in the instrumental learning situation, the two events consist of the organism's responses and some outcome or reinforcer. The relation between such events is best construed as one of controllability—the response exerts either some or no degree of control over the outcome (see Seligman, 1975, for a discussion of the distinction between predictability and controllability). Thus, control is best defined as the dependence of an outcome on a response. A further reason for using the terminology of control when interacting with the subjects in the present experimental context is that it conveys the technical meaning of contingency in everyday language (Jenkins & Ward, 1965).

For the present series of experiments, it was necessary to construct an index of the actual degree of control or dependency between responses and outcomes. In statistics, the phi coefficient ( $\phi$ ) is commonly used to quantify the degree of relation between two events. However, in their study of judgments of contingency, Jenkins and Ward (1965) used the magnitude of the difference between the conditional probability of an outcome given the occurrence of one response versus the conditional probability of the outcome given the occurrence of another response as the index of degree of control or contingency. Mathematically, this index is a close approximation to  $\phi$  and, in fact, is highly correlated with  $\phi$ . We chose

to use this difference-in-probability metric as our index of objective degree of control or contingency because it is mathematically less cumbersome than  $\phi$  and because it facilitates comparison of prior studies on the judgment of contingency with the present study. An example clarifies the difference-in-probability metric used in the present investigation. According to this metric, if the probability of obtaining an A on an exam is .90 when you study and .10 when you do not study, the degree of contingency between studying and obtaining an A is .80. Alternatively, if the probability of obtaining an A is 1.0 whether you study or not, then studying and obtaining an A are noncontingently related.

In Experiment 1, students were presented with one of a series of problems in which there was some degree of contingency between their responses and outcomes. The problems differed in the degree of contingency between responses and outcomes. Experiments 2, 3, and 4 examined students' abilities to detect contingency and noncontingency between responses and outcomes under different conditions of outcome frequency and desirability.

### Experiment 1

In this study, we examined depressed and nondepressed college students' abilities to detect the degree of contingency between their responses and environment outcomes. Students were presented with one of three contingency problems differing in degree of contingency or control. The contingency problems consisted of a series of trials on which the subject made one of two possible responses (pressing a button or not pressing a button) and received one of two possible outcomes (a green light or no green light). At the end of the series of trials, the subject was asked to judge the degree of contingency that existed between button pressing and green light onset.

In Experiment 1, as well as in Experiments 2, 3, and 4, a modification of the method of Jenkins and Ward (1965) was used. Because Jenkins and Ward found that subjects' judgments of degree of control correlated highly with the frequency of rein-

forcement, it was important to ensure that frequency of reinforcement was not confounded with actual degree of contingency. The present experiment was designed so that frequency of reinforcement and actual degree of contingency were negatively correlated provided that the subject sampled the two response alternatives, pressing the button and not pressing the button, fairly regularly. This procedure ensured that if students relied on percentage or frequency of reinforcement as an indicant of degree of contingency, they would give incorrect judgments negatively correlated with actual degree of contingency. Thus, the prediction that subjective representations of contingency are more closely related to the frequency of the outcome than to objective contingency was examined.

Including both depressed and nondepressed students in Experiment 1 tested the learned helplessness theory of depression. According to the strong helplessness prediction, depressed students should underestimate the degree of contingency on each of the three contingency problems. According to the weak prediction, depressed students merely should judge that they have less control than nondepressed students on each of the three problems.

Making a judgment about the degree of contingency between one's responses and an environmental outcome is best conceived of as an instance of drawing an inference from raw data. If subjects do err in their judgments of contingency, it could be either because they have not collected the appropriate raw data or because they have drawn incorrect inferences from a set of appropriate raw data. For example, a subject may know the probability of green light onset associated with pressing and not pressing but be unable to organize these probabilities in the manner necessary for making a correct judgment of contingency. Alternatively, a subject could make an incorrect judgment of contingency because he or she simply did not know the conditional probabilities of green light onset. Therefore, in Experiment 1, as well as in Experiments 2, 3, and 4, we examined subjects' judgments of the probability of green

light onset when they pressed and did not press.

A further issue of interest is the subject's choice behavior in a contingency learning situation. Although we argued previously that subjects' instrumental behavior in a contingency learning situation is not a sufficient condition for inferring a cognitive representation of contingency, such behavioral data may provide converging evidence regarding such a representation. Consequently, after giving their series of judgments, subjects were presented with a behavioral task in which they could obtain money by producing green light onset.

### *Method*

*Subjects.* Ninety-six undergraduates, 48 males and 48 females, from the University of Pennsylvania served as paid volunteers. The data of one additional subject were discarded because she failed to follow the experimental instructions. Subjects were assigned to a depressed or nondepressed group on the basis of their Beck Depression Inventory (BDI) scores (Beck, 1967).<sup>1</sup> Subjects with BDI

<sup>1</sup> Four criteria were used for the selection of a depression inventory. The inventory had to be self-administered, relatively short, capable of providing quantitative information for assessing depth of depression, and relatively well validated. Although Zung's (1965) Self-Rating Depression Scale and the BDI both meet each of these selection criteria, the BDI was selected because its validity has been demonstrated by more detailed and comprehensive studies. Beck (1967) has reported reliability and validity studies involving nearly 1,000 subjects. In two studies, he found significant differences in mean BDI scores between groups of psychiatric inpatients and outpatients rated as exhibiting none, mild, moderate, or severe depression. Metcalfe and Goldman (1965) reported similar results in a cross-validation study. The correlations between BDI scores and clinically rated severity of depression in these three studies were .65, .67, and .61, respectively. Recently, Bumberry, Oliver, and McClure (1978) found that the BDI is a valid instrument for measuring depression in a college student population. In addition, studies examining predictions from the learned helplessness model of depression have found that the BDI is a very sensitive predictor of behavior. These studies consistently have found substantial correlations between BDI scores and a variety of deficits. Miller and Seligman (1976) reported the range to be between .45 and .53. Correlations between BDI scores and noise escape measures have been as high as .74 (Klein & Seligman, 1976).

Table 1

*Means and Standard Deviations of BDI and MAACL Scores by Problem, Mood, and Sex for Experiment 1*

Problem and test	Nondepressed				Depressed			
	Males		Females		Males		Females	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
75-50								
BDI	3.1	1.9	3.4	3.4	11.8	2.0	13.4	4.7
MAACL	13.1	5.1	13.8	7.8	17.9	6.5	21.0	7.6
75-25								
BDI	3.5	2.0	4.8	2.4	12.9	3.1	16.1	9.7
MAACL	16.0	4.3	14.6	3.9	17.6	5.5	14.6	9.8
75-0								
BDI	4.0	2.7	4.8	1.8	12.1	3.1	20.0	6.6
MAACL	10.9	5.5	11.4	5.8	17.1	7.1	17.8	9.9

*Note.* BDI = Beck Depression Inventory; MAACL = Multiple Affect Adjective Check List.

scores of 9 or above were assigned to the depressed group, and subjects scoring 8 or lower were assigned to the nondepressed group. The final sample consisted of 48 depressed students (24 males and 24 females) and 48 nondepressed students (24 males and 24 females). In addition, subjects completed the Multiple Affect Adjective Check List (MAACL) Today form (Zuckerman & Lubin, 1965) as a further index of depression although these scores were not used as a criterion for selecting depressed and nondepressed students. The correlation between the BDI and the MAACL depression scores was .51 ( $p < .001$ ). Table 1 presents the mean BDI and MAACL scores for all experimental groups. Subjects were randomly assigned to the three experimental conditions with the restriction that each condition contain equal numbers of males and females and depressed and nondepressed students. Half of the subjects in each condition were tested by Experimenter 1 and half by Experimenter 2.

**Experimental design.** The experiment was a 3 (Problem Type)  $\times$  2 (Mood—depressed, nondepressed)  $\times$  2 (Sex—males, females)  $\times$  2 (Response—press, no press) factorial design. The three problems differed in degree of control. In Problem 1, (75—50), subjects had 25% control; in Problem 2 (75—25), they had 50% control; and in Problem 3, (75—0), they had 75% control. The first number of each problem name denotes the percentage of trials on which the outcome of interest (green light onset) occurred when the subject pressed the button. The second number denotes the percentage of trials on which green light onset occurred when the subject did not press the button. As defined above, the degree of control (contingency) was determined by the difference between these two numbers.<sup>2</sup> Of the 96 subjects, 32 (8 depressed males, 8 depressed females, 8 nondepressed males, and 8

nondepressed females) were assigned to each problem.

Within each problem, subjects were counter-balanced for whether pressing or not pressing the button led to a higher percentage of green light onsets. For example, in Problem 1 the green light came on 75% of the time when the button was pressed and 50% of the time when the button was not pressed (75—50) for half of the subjects. For the other half of the subjects in Problem 1, the green light came on 50% of the time when the button was pressed and 75% of the time when it was not pressed (50—75). In addition to differing on degree of contingency, the three problems were also designed to differ on overall percentage of reinforcement (overall frequency of green light onsets). In particular, the percentage of reinforcement for the problems was negatively correlated with the degree of contingency provided that the subjects sampled the two responses (pressing and not pressing) fairly regularly. Table 2 summarizes the experimental design and presents the number of subjects in each cell of the design with the percentage of reinforcement received by these subjects. As can be seen in Table 2, the percentage of reinforcement did, in fact, correlate negatively with degree of contingency ( $r = -.59$ ,  $p < .001$ ).

**Dependent measures.** The major dependent measures were four judgment scales and performance on a behavioral task. On the Judgment of Control scale, subjects rated the degree of control their responses (pressing and not pressing) exerted over

<sup>2</sup> Inspection of individual subjects' protocols for all four experiments revealed that the actual degree of control experienced by each subject in the experimental session deviated only slightly or not at all from the nominal degree of control.

the experimental outcome (green light onset). On the second judgment scale, Judgment of Total Reinforcement, subjects estimated the overall percentage of trials on which green light onset occurred regardless of which response they made. The last two scales, Judgment of Reinforcement if Press and Judgment of Reinforcement if Not Press, were designed to assess whether subjects knew the raw data necessary to compute the conditional probabilities that were necessary for making an accurate judgment of control. On these two scales, subjects estimated the percentage of trials on which the green light came on when they pressed and when they did not press, respectively.

Because Ward and Jenkins (1965) found that subjects often used the invalid heuristics of percentage of successes and confirming cases in arriving at their verbalized judgments of control, these measures were also used in the present experiment. In line with Ward and Jenkins (1965), percentage of successes was defined as the percentage of trials on which the green light appeared when the subject pressed the button. Confirming cases was defined as the sum of the number of trials on which the subject pressed the button and the green light came on plus the number of trials on which the subject did not press the button and the green light did not come on.

A behavioral task was employed as a further measure of subjects' representations of contingencies. In this task, subjects were given 10 trials from the contingency problem they had just received and were paid to maximize green light onset.

Several additional dependent measures were included on a postexperimental questionnaire. First, subjects rated how certain they were of the accuracy of their judgments of degree of control. In addition, two open-ended questions allowed sub-

jects to describe the evidence that convinced them that they either had or did not have control as well as the evidence that would have convinced them of the opposite conclusion. Finally, subjects were asked whether they employed any complex hypotheses (e.g., sequential or time-dependent patterns) during the contingency learning problems, and if so, what these hypotheses were.

*Apparatus and materials.* The experiment was conducted in a two-room suite. Standard switching relay circuitry equipment for controlling stimulus presentation and for recording subjects' responses was housed in the observation room of the suite. Subjects were seated in the experimental room such that they could be observed through a one-way mirror by the experimenter in the observation room.

The stimulus presentation consisted of a black wooden stand-up platform (23 cm  $\times$  23 cm) on which a yellow and a green light were positioned 5 cm from the top of the platform facing the subject. The subject's response apparatus consisted of a 15.5 cm  $\times$  7.5 cm  $\times$  4 cm wooden box, also painted black, on which a spring-loaded button was mounted in the center.

The contingencies between subjects' responses (pressing the button and not pressing) and experimental outcomes (green light onset or no green light) were programmed with two punched tape readers and conventional switching circuitry. The outcomes were controlled by the subject's responses through relays and a programming device. If the subject pressed the button within 3 sec, Tape Reader 1, which controlled the schedule of green light onsets associated with pressing, was activated. If the subject failed to press the button within 3 sec, Tape Reader 2, which controlled the schedule of green light onsets associated with not pressing,

Table 2  
*Number of Subjects per Cell, Degree of Contingency, and Percent of Reinforcement for Experiment 1*

Problem	Response	Nondepressed						Depressed					
		Males			Females			Males			Females		
		<i>n</i>	C	R	<i>n</i>	C	R	<i>n</i>	C	R	<i>n</i>	C	R
75-50	75-50	4	25	62.2	4	25	63.5	4	25	63.0	4	25	68.5
	50-75	4	25	65.5	4	25	64.2	4	25	58.0	4	25	65.0
75-25	75-25	4	50	54.0	4	50	57.5	4	50	51.0	4	50	52.0
	25-75	4	50	48.0	4	50	47.2	4	50	48.0	4	50	47.5
75-0	75-0	4	75	49.5	4	75	41.2	4	75	47.5	4	75	53.5
	0-75	4	75	37.0	4	75	46.7	4	75	45.7	4	75	50.0

*Note.* Response refers to whether pressing or not pressing the button was associated with the higher percentage of reinforcement. *n* = number of subjects per cell, C = percent degree of contingency, R = percent of reinforcement.

was activated. (See the Experimental Design section for the actual programmed contingencies.)

The BDI (Beck, 1967) and the MAACL (Zuckerman & Lubin, 1965) depression inventories, four judgment scales, and a postexperimental questionnaire constituted the experimental materials. Each of the four judgment scales was marked off in units of five with extreme values of 0 and 100. For the Judgment of Control scale, the extreme values were labeled No Control and Complete Control and the 50% point was labeled Intermediate Control. The other three scales (Judgment of Total Reinforcement, Judgment of Reinforcement if Press, and Judgment of Reinforcement if Not Press) were simply labeled as percentages.

*Procedure.* When the subject entered the room, he or she was seated at a desk and was administered the BDI and MAACL. The experimenter was not present while the subject filled out the mood inventories. After completing the inventories, the subject was assigned to an experimental condition and led to a table on which the apparatus for the contingency learning problem was displayed. The actual procedure and instructions for each of the three contingency problem groups were identical. Each of the contingency problems consisted of 40 3-sec trials on which the subject had the option of either pressing a button or not pressing a button. Onset of a yellow light signaled the start of each trial. At the end of the 3-sec trial, a green light was either presented or not presented dependent on the subject's response and the contingency problem to which the subject had been assigned. The intertrial interval ranged from 10 to 25 sec with a mean of 14 sec. All subjects were given the following instructions:

Now, in this problem-solving experiment, it is your task to learn what degree of control *you* have over whether or not this green light comes on. Each time the yellow light comes on indicates the start of a new trial, the occasion to do something. For each trial, after the yellow light comes on, you have the option of either making a button press response or not making a button press response. A button press response consists of pressing this button *once and only once* immediately after the yellow light comes on. Not making a button press response consists, of course, of doing nothing when the yellow light comes on. If you do intend to press the button on a given trial, you must press within three seconds after the yellow light comes on; otherwise the trial will be counted as a not press trial. So, in this experiment there are only two possibilities as to what you can do on each of the trials: either press the button within three seconds after the yellow light comes on, or else, just sit back and do nothing. Any questions so far?

You may find that the green light will go on, on some percentage of the trials on which you do make a button press response. You may also find

that the green light will go on, on some percentage of the trials when you do not make a button press response. Alternatively, you may find that the green light will *not* go on, on some percentage of the trials on which you do make a button press response. And, you may find that the green light will *not* go on, on some percentage of the trials when you do *not* make a button press response. So, there are four possibilities as to what may happen on any given trial: 1) you press and the green light does come on; 2) you press and the green light does not come on; 3) you don't press and the green light does come on; 4) you don't press and the green light does not come on. Since it is your job to learn how much control *you* have over whether the green light comes on, as well as whether the green light does not come on, it is to your advantage to press on some trials and not on others, so you know what happens when you *don't* press as well as when you *do* press. Moreover, the knowledge that you gain from this problem will enable you to earn some money later on in the study. Any questions?

When it was clear that the subject understood the outline of the task, he or she was then shown the Judgment of Control scale and the concept of control was discussed briefly:

Forty trials will constitute the problem. After the problem, you will be asked to indicate your judgment of control by putting an "X" someplace on this scale: at 100 if you have complete control over the onset of the green light, at 0 if you have no control over the onset of the green light, and somewhere between these extremes if you have some but not complete control over the onset of the green light. Complete control means that the onset of the green light on any given trial is determined by your choice of responses, either pressing or not pressing. In other words, whether or not the green light goes on is totally determined by whether you choose to press or to just sit back and not press. No control means that you have found no way to make response choices so as to influence in any way the onset of the green light. In other words, the onset of green light has nothing to do with what you do or don't do. Another way to look at having no control is that whether or not the green light comes on, on any given trial, is totally determined by factors such as chance or luck, rather than by your choice of pressing or not pressing. Intermediate degrees of control means that your choice of responses, either pressing or not pressing, influences the onset of the green light even though it does not completely determine whether the green light goes on or not. In other words, what you do or don't do matters to some extent but not totally. Another way to look at having intermediate control is that one response, either pressing or not pressing, produces the green light onset more often than

does the other response. So, it may turn out that you will have no control, that is, your responses will not affect the onset of the green light, or it may turn out that you will have some degree of control, either complete or intermediate, that is, one response produces green light onset more often than does the other. Any questions before we begin?

The experimenter then left the room and the subject proceeded with the contingency learning problem. At the end of the 40 trials, the experimenter returned and reread the section of the instructions discussing the concept of control. The subject then completed each of the four judgment scales by placing an X on the scale corresponding to his or her estimate.

Following the judgment scales, the subject participated in the behavioral task. This task consisted of 10 trials from the contingency learning problem that the subject had just completed. On each trial in which the green light came on, the subject earned a quarter. The instructions for the behavioral task were as follows:

Now, in this last part of the study, you have a chance to earn money by demonstrating what you have just learned in the first part of the study. You will now receive 10 trials from the problem you just learned about. The relationship between your responses and the onset of the green light remains exactly the same. But this time your objective is to maximize the number of trials on which the green light goes on. On each trial on which the green light does go on, you will earn a quarter. Alternatively, on each trial on which the green light does not go on, you will not earn any money. At the end of the 10 trials you will get to keep all of the money you have earned. So, it is to your advantage to maximize the number of trials on which the green light goes on. Any questions?

The experimenter was absent during the behavioral task but returned at the end of the task and gave the subject the money he or she had earned. The subject was then administered the postexperimental questionnaire and was carefully debriefed. After the debriefing, all subjects were given additional money regardless of how much they had actually earned so that their total equalled \$2.50.

### Results<sup>3</sup>

Since the experiment was conducted by two different experimenters, it was necessary to determine whether there were any experimenter effects. Analysis of variance revealed no significant experimenter effects for any of the dependent measures. Therefore, this factor was not included in the subsequent statistical analyses.

*Judgment scales.* No main effects or interactions involving the factor of response

were obtained on any of the judgment scales. Subjects' judged control, judged reinforcement, and judged reinforcement if press and if not press scores were not affected by whether the higher probability of green light onset was associated with the response of pressing or not pressing. For this reason, all statistical analyses on the judgment scales employed a 3 (Problem Type)  $\times$  2 (Mood)  $\times$  2 (Sex) design with subjects' scores collapsed over the response factor.

Subjects' scores on the Judgment of Control scale indicated that they accurately judged the degree of contingency between their responses and outcomes in the three problems. A Problem  $\times$  Mood  $\times$  Sex analysis of variance on the Judgment of Control scores revealed a significant main effect for problem type,  $F(2, 84) = 23.54, p < .001$ . Post hoc Scheffé tests indicated that subjects believed they had significantly more control in the 75-0 problem than in the 75-25 problem and significantly more control in the 75-0 problem than in the 75-50 problem (both  $ps < .001$ ). Although subjects' ratings of control did not differ significantly between the 75-50 and the 75-25 problems, the difference was in the appropriate direction. Somewhat surprisingly, depressed and non-depressed subjects did not differ in their ratings of degree of control. The analysis of variance revealed no significant main effects for mood or sex and no significant interactions involving these factors. Table 3 presents the mean judgment of control scores for each experimental group.

In addition to judging degree of control for the contingency problem to which they were assigned, subjects also rated how certain they were of the accuracy of these judgments. A Problem  $\times$  Mood  $\times$  Sex analysis of variance of the certainty ratings yielded a significant main effect for problem,  $F(2, 84) = 4.83, p < .01$ . The more control subjects had, the more certain they were of the accuracy of their judged degree of control. A post hoc Scheffé test showed that subjects were significantly less certain in the 75-50 problem than in the 75-0 problem ( $p < .05$ ), with

<sup>3</sup> All statistical tests in Experiments 1-4 are two-tailed.

Table 3

*Means and Standard Deviations of Judged Control Scores by Problem, Mood, and Sex for Experiment 1*

Problem	Nondepressed				Depressed			
	Males		Females		Males		Females	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
75-50	32.6	27.6	38.1	20.0	36.2	21.3	33.8	29.2
75-25	38.1	28.6	54.2	16.4	56.5	16.3	37.6	31.0
75-0	82.8	17.3	65.6	16.5	71.1	16.1	71.9	21.5

the certainty in the 75-25 problem falling in between.

Although Jenkins and Ward (1965) found that subjects' judgments of control were unrelated to actual degree of control and highly correlated with the actual number of successes or reinforcements, the opposite results were obtained in the present experiment. The product-moment correlation of individual judgments of control with the actual degree of control, based on all subjects and all problems, was .56 ( $p < .001$ ). In contrast, the partial correlation between individual judgments of control and judged percentage of reinforcement (with actual percentage of reinforcement held constant) was not significantly different from zero ( $r = -.12$ , *ns*).<sup>4</sup> Further, subjects' judgments of control were also uncorrelated with the heuristics of percentage of successes ( $r = -.13$ , *ns*) and confirming cases ( $r = -.01$ , *ns*).

To summarize, subjects were accurate in judging degree of control and did not appear to use invalid heuristics such as percentage of reinforcement, percentage of successes, or confirming cases. As Figure 1 shows, judged control paralleled actual control for both depressed and nondepressed subjects. It should be noted, however, that subjects did show a slight tendency to overestimate degree of control in the 75-50 problem.

The fact that subjects accurately judged the degree of contingency between their responses and outcomes suggests that they knew the relevant conditional probabilities and organized them appropriately. A measure of the discrepancy between judged percentage of reinforcement if press and the

actual percentage of reinforcement if press was used to assess the accuracy of subjects' knowledge of the conditional probabilities. A similar discrepancy score was computed for the percentage of reinforcement if not press.

Figure 2 shows that both depressed and nondepressed subjects were quite accurate in judging the percentage of reinforcement when they pressed the button. The mean discrepancy scores (judged - actual) for the three problems were as follows: -9.50 for 75-50, +.54 for 75-25, and +2.22 for 75-0. A Problem  $\times$  Mood  $\times$  Sex analysis of variance on these discrepancy scores yielded a significant main effect for problem,  $F(2, 84) = 8.45$ ,  $p < .001$ , and no other significant main effects or interactions. Post hoc Scheffé tests revealed that subjects were significantly more accurate in judging the percentage of reinforcement in the 75-25 problem than in the 75-50 problem ( $p < .05$ ) and more accurate in the 75-0 problem than in the 75-50 problem ( $p < .05$ ).

Similarly, both depressed and nondepressed subjects were quite accurate in judging the percentage of reinforcement when they did not press. The mean discrepancy scores were as follows: -13.88 for 75-50, -3.84 for 75-25, and +2.03 for 75-0. A Problem  $\times$  Mood  $\times$  Sex analysis of variance of the discrepancy scores for not press

<sup>4</sup> In computing the correlation between judged control and judged percentage of reinforcement, actual percentage of reinforcement was partialled out, because the study was designed so that actual percentage of reinforcement correlated negatively with actual control.

yielded a significant main effect for problem,  $F(2, 84) = 9.02$ ,  $p < .001$ , and no other significant main effects or interactions. Post hoc Scheffé tests revealed that subjects were significantly more accurate in judging the 75-25 problem than the 75-50 problem ( $p < .05$ ) and more accurate in judging the 75-0 problem than the 75-50 problem ( $p < .05$ ).

In all three problems, one of the two responses was associated with 75% reinforcement. We wished to determine whether subjects were equally accurate in discerning this conditional probability in the three problems. For those subjects who had pressing associated with 75% reinforcement, the mean discrepancy scores (judged reinforcement if press - actual reinforcement if press) were -14.06 for 75-50, +1.00 for 75-25, and -.69 for 75-0. A Problem  $\times$  Mood  $\times$  Sex analysis of variance on these discrepancy scores revealed a significant main effect for problem,  $F(2, 36) = 6.74$ ,  $p < .003$ , and no other significant main effects or interactions. Post hoc Scheffé tests showed that subjects were more accurate in detecting the 75% reinforcement rate in the 75-25 problem than in the 75-50 problem

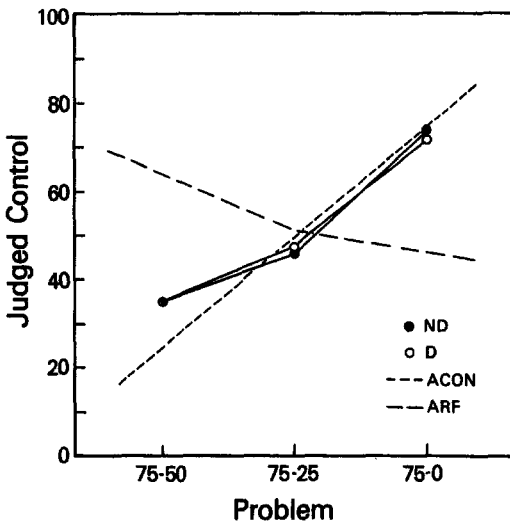


Figure 1. Judged control for depressed (D) and nondepressed (ND) students as a function of problem type in Experiment 1. (Actual degree of control [ACON] and actual percentage of reinforcement [ARF] are also shown as a function of problem type.)

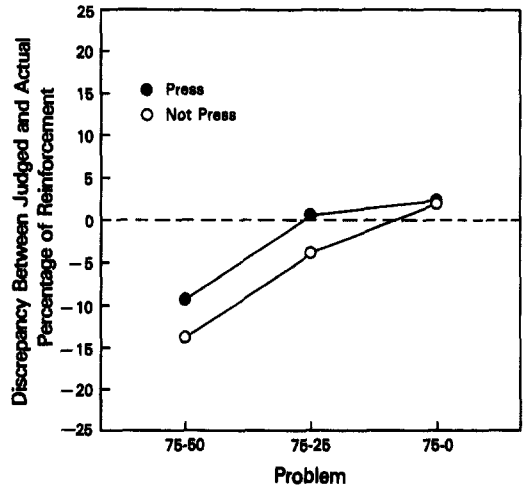


Figure 2. Discrepancy between judged and actual percentage of reinforcement for pressing and not pressing as a function of problem type in Experiment 1.

( $p < .05$ ) and more accurate in the 75-0 problem than in the 75-50 problem ( $p < .05$ ).

For those subjects who had not pressing associated with 75% reinforcement, the mean discrepancy scores (judged reinforcement if not press - actual reinforcement if not press) were -20.50 for 50-75, -9.06 for 25-75, and +1.44 for 0-75. A Problem  $\times$  Mood  $\times$  Sex analysis of variance of these discrepancy scores yielded a significant main effect for problem,  $F(2, 36) = 6.70$ ,  $p < .003$ , but no other significant main effects or interactions. A post hoc Scheffé test showed that subjects were significantly more accurate in the 0-75 problem than in the 50-75 problem ( $p < .01$ ). In summary, although subjects were in general quite accurate in judging the conditional probabilities associated with pressing and not pressing, it is clear that as the two conditional probabilities became more similar (i.e., as the degree of contingency decreased), the degree of accuracy decreased.

The final judgment scale that subjects completed measured their judgments of total percentage of reinforcement irrespective of their responses. Subjects were also highly accurate in making this judgment. The discrepancy between judged total reinforcement

and actual total reinforcement was very small for all three problems:  $-.22$  for 75-50,  $+4.16$  for 75-25, and  $+6.28$  for 75-0. A Problem  $\times$  Mood  $\times$  Sex analysis of variance of these discrepancy scores revealed no significant effects. Further, the product-moment correlation between judged total reinforcement and actual total reinforcement was  $.73$  ( $p < .001$ ).

**Behavioral task.** The results of the behavioral task provided converging evidence that subjects were sensitive to the degree of contingency between their responses and outcomes. On the behavioral task, subjects most frequently performed the response that had been associated with the higher probability of green light onsets in the contingency problems. For example, if not pressing had been associated with the higher percentage of green light onsets in the contingency learning problem, subjects tended to not press more often than press on the behavioral task.

Further, Figure 3 shows that subjects' tendency to perform the response that maximized green light onsets in the behavioral task varied directly with the degree of control in the three problems. That is, subjects were most likely to perform the response that maximized green light onsets in the 75-0 problem and least likely to perform this response in the 75-50 problem. A Problem  $\times$  Mood  $\times$  Sex  $\times$  Response analysis of variance on the number of correct responses in the behavioral task yielded a significant main effect for problem,  $F(2, 72) = 13.39$ ,  $p < .001$ , and no other significant main effects or interactions. Post hoc Scheffé tests showed that subjects maximized onset of the green light more frequently in the 75-0 and the 75-25 problems than in the 75-50 problem ( $ps < .01$ ).

**Postexperimental questionnaire.** Subjects' responses on the postexperimental questionnaire suggest that they relied on the relative efficacy of one response versus another as the crucial evidence for determination of degree of control. A typical subject assigned to the 25-75 problem, when asked what evidence convinced her that she had some degree of control, replied, "Because the green light went on more often when I sat

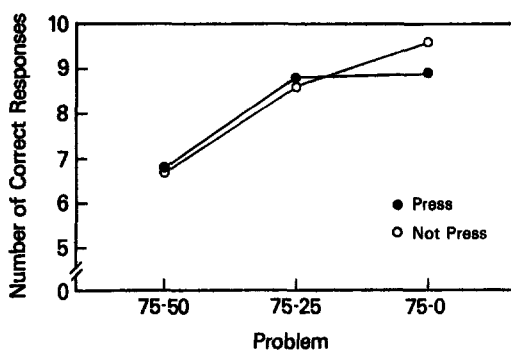


Figure 3. Number of correct responses in the behavioral task as a function of problem type in Experiment 1 for subjects who had pressing associated with the higher percentage of reinforcement (press) and for subjects who had not pressing associated with the higher percentage of reinforcement (not press).

back, and usually when I pressed the button it did not." The majority of subjects reported that they did not try any complex patterns of responding such as alternation or double alternation of pressing and not pressing during the contingency problem.

### Discussion

Taken together, the results of Experiment 1 suggest that at least under some circumstances, when a contingency between responses and outcomes exists, subjective representations of contingencies mirror objective contingencies across a wide range of the response-outcome contingency space (c.f. Seilgman et al., 1971; see also Ward & Jenkins, 1965, for another demonstration of isomorphism between subjective and objective contingencies). When presented with a contingency learning problem, students were not only able to perform the response that maximized reinforcement, but more importantly, they were able to judge accurately the degree of contingency between their responses and outcome. This result obtained regardless of whether the response associated with the higher frequency of the outcome was pressing or not pressing the button. The results also indicate that students knew the conditional probabilities relevant to a determination of degree of control. Therefore, not only were students sensitive to the relative

efficacy of one response versus the other, but they appeared to use this difference as the basis for their judgments of control.

It should be noted, however, that only a few students actually subtracted one probability from the other in judging degree of control. The majority of students did not subtract formally but appeared to rely on a more "holistic" or "gestalt" impression of the difference between probabilities. Additional support for the conclusion that students relied on the relative efficacy of one response versus the other as providing the critical evidence for degree of control was their verbalized logic on the postexperimental questionnaire.

That students were able to judge accurately the degree of contingency between their responses and outcomes and did not rely on an invalid heuristic such as percentage of reinforcement does not agree with the results reported by Jenkins and Ward (1965). The most probable explanation for the difference between the two studies involves the operationalization of the degree of contingency itself. In Jenkins and Ward's study, subjects were presented with two response buttons,  $R_1$  and  $R_2$ . Jenkins and Ward determined the "actual degree of control" in each of their contingency problems by comparing the probability of the outcome when  $R_1$  is made to the probability of the outcome when  $R_2$  is made. In their analysis of the actual degree of control, Jenkins and Ward did not take into account the probability of the outcome when no response (neither  $R_1$  nor  $R_2$ ) was made. The fact that trials were self-initiated in the Jenkins and Ward experiment increases the likelihood that subjects did, indeed, consider the probability of the outcome when no response (neither  $R_1$  nor  $R_2$ ) occurred. If the subjects in the experiment were "computing" this third conditional probability in addition to the two conditional probabilities associated with  $R_1$  and  $R_2$ , then degree of control would correlate with percentage of successes. It is possible that subjects in Jenkins and Ward's experiment arrived at their judgments of control by comparing the probability of the outcomes' occurrence

when they did nothing with some average of the probability of the outcomes' occurrence when they pressed  $R_1$  and  $R_2$ . Given that the probability of the outcomes' occurrence when neither  $R_1$  nor  $R_2$  was made was zero, this difference would, in fact, be very close to the actual percentage of successes the subject received. If the preceding analysis is correct, subjects in both Jenkins and Ward's experiment and the present experiment used the relative efficacy of responses as the basis for judgments of contingency.

Not only did the two experiments differ in the operationalization of the concept of contingency but they also differed in the number of contingency problems presented to each subject. Whereas subjects in Jenkins and Ward's study were presented with several contingency problems, subjects in the present experiment received only one problem. Work in progress in our laboratory (Alloy & Abramson, Note 1) suggests that experience with one type of contingency problem systematically affects judgments of control on other types of contingency problems. The above procedural differences may account for the discrepancy between the results of the present experiment and those of Jenkins and Ward.

Experiment 1 provided no evidence that depressed students distort response-outcome relations in the manner predicted by the learned helplessness theory of depression. Depressed and nondepressed students did not differ in their judgments of contingency, and both groups were accurate. This result is potentially important because it is based on a method for assessing cognitive representations of contingency in depressed and nondepressed people that is more direct than earlier methods (e.g., Klein & Seligman, 1976; Miller & Seligman, 1973, 1975). Because Experiments 2, 3, and 4 are also relevant to whether depressed and nondepressed individuals differ systematically in their representations of contingencies, further consideration of the failure to confirm the learned helplessness predictions in Experiment 1 is postponed until the General Discussion. In Experiment 2, we examined depressed and nondepressed students' judg-

ments of contingency in two problems in which responses and outcome were noncontingently related.

### Experiment 2

Although both depressed and nondepressed students in Experiment 1 were quite accurate in judging the degree of control when faced with problems in which there was some degree of contingency, it was possible that they would not be as accurate in detecting a lack of contingency between their responses and outcomes. Indeed, a number of investigators (Chapman & Chapman, 1967; Langer, 1975; Smedslund, 1963; Starr & Katkin, 1969; Wortman, 1975) have found that people often believe a relationship is contingent when it is noncontingent. In Experiment 2, students were presented with one of two problems in which responses and outcomes were noncontingently related but differed in the overall frequency of green light onset. If people use the invalid heuristic of percentage of reinforcement in the noncontingent case, then they will believe that they have more control in the problem in which green light onset occurs frequently than in the problem in which green light onset occurs infrequently.

Affective state was not predictive of judgments of control in Experiment 1; however, the learned helplessness model of depression again predicts that judgments of control will vary systematically with affect in Experiment 2. Although several proponents of the learned helplessness model (e.g., Klein & Seligman, 1976; Miller & Seligman, 1973) have argued that depressed and nondepressed people should not differ in their perception of noncontingent response–outcome relationships and that both groups will be accurate, we believe the model does not support this prediction. On the contrary, the learned helplessness model of depression makes both a strong and a weak prediction concerning people's representations of contingency in the noncontingent case. According to the strong prediction, nondepressed people should tend to overestimate the degree of contingency when responses and outcomes

are noncontingently related, whereas depressed people will be accurate. According to the weak prediction, there simply will be a net difference between depressed and nondepressed people's judgments of noncontingency. The weak prediction does not require that depressed people accurately perceive noncontingency, but merely that nondepressed people believe they have more control than depressed people believe they have.

Helplessness theory explains difficulty in discerning objective contingency by depressed people by proactive interference from an expectation of uncontrollability. Similarly, because the model regards nondepressed people as having generalized expectations of control, these expectations should proactively interfere with the detection of noncontingencies. Just as depressives' generalized expectation of response–outcome independence interferes with their ability to perceive that outcomes are now dependent on responses, nondepressives' generalized expectation that outcomes are dependent on responses should interfere with their ability to perceive that outcomes are independent of responses. Such reasoning has been used to explain the immunization phenomenon in helplessness (e.g., Maier & Seligman, 1976). Therefore, according to both the weak and strong predictions of learned helplessness theory, nondepressed students should tend to show an "illusion of control" (Langer, 1975) in Experiment 2.

### Method

*Subjects.* Sixty-four undergraduates, 32 males and 32 females, from the University of Pennsylvania served as paid volunteers. Using the same BDI criterion as in Experiment 1, subjects were assigned to a depressed or nondepressed group. As in Experiment 1, subjects completed the MAACL as a further index of depression. The correlation between the BDI and MAACL scores in Experiment 2 was .44 ( $p < .005$ ). The final sample consisted of 32 depressed students (16 males and 16 females) and 32 nondepressed students (16 males and 16 females). Table 4 presents the mean BDI and MAACL scores for all experimental groups. Subjects were randomly assigned to two experimental conditions, with the restriction that each condition contain equal numbers of males and females and depressed and nondepressed students.

Table 4

*Means and Standard Deviations of BDI and MAACL Scores by Problem, Mood, and Sex for Experiment 2*

Problem and test	Nondepressed				Depressed			
	Males		Females		Males		Females	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
25-25								
BDI	2.6	2.6	5.0	1.8	10.6	1.8	15.0	6.6
MAACL	14.0	5.3	12.5	3.8	15.6	5.2	19.8	7.0
75-75								
BDI	4.1	3.0	5.5	2.8	11.0	2.4	15.1	6.1
MAACL	8.5	4.2	14.4	5.2	13.9	2.4	21.0	5.3

*Note.* BDI = Beck Depression Inventory; MAACL = Multiple Affect Adjective Check List.

Half of the subjects in each condition were tested by Experimenter 1 and half by Experimenter 2.

**Experimental design.** The experiment was a 2 (Problem Type)  $\times$  2 (Mood—depressed, non-depressed)  $\times$  2 (Sex—male, female) factorial design. The two problems differed in the percentage of reinforcement but did not differ in degree of control. In Problem 1 (25-25), subjects had no control and were reinforced (green light onset) on 25% of the trials. In Problem 2 (75-75), subjects also had no control but were reinforced on 75% of the trials. Of the 64 subjects, 32 (8 depressed males, 8 depressed females, 8 nondepressed males, and 8 nondepressed females) were assigned to each problem.

**Dependent measures.** The dependent measures were identical to those employed in Experiment 1.

**Apparatus and procedure.** The apparatus and procedure were also identical to those employed in Experiment 1.

## Results

Because the experiment was conducted by two different experimenters, it was necessary to determine whether there were any experimenter effects. Analysis of variance revealed no significant experimenter effects for any of the dependent measures. Therefore, this factor was not used in the subsequent statistical analyses.

**Judgment scales.** Subjects' scores on the Judgment of Control scale revealed that non-depressed subjects showed an illusion of control in the 75-75 problem, but not in the 25-25 problem, whereas depressed subjects were relatively accurate in both problems. A Problem  $\times$  Mood  $\times$  Sex analysis of variance of the judgment of control scores yielded sig-

nificant main effects for problem,  $F(1, 56) = 7.18, p < .01$ , and mood,  $F(1, 56) = 4.16, p < .05$ ; a significant Problem  $\times$  Mood interaction,  $F(1, 56) = 5.54, p < .03$ ; and a significant Problem  $\times$  Mood  $\times$  Sex interaction,  $F(1, 56) = 4.40, p < .04$ .

Interpretation of the significant main effects and double interaction is difficult without analysis of the significant triple interaction. To analyze the triple interaction, Problem  $\times$  Mood analyses of variance were performed separately for both males and females. The Problem  $\times$  Mood analysis of variance conducted for females revealed a significant main effect for problem,  $F(1, 28) = 6.90, p < .02$ , and a significant Problem  $\times$  Mood interaction,  $F(1, 28) = 9.94, p < .004$ . A simple main effects test (Winer, 1962) revealed that nondepressed females judged that they had more control in the 75-75 problem than in the 25-25 problem,  $F(1, 28) = 16.71, p < .001$ , whereas the judgments of depressed females did not differ between the two problems. In addition, nondepressed females' judgments of control were significantly greater than depressed females' judgments of control in the 75-75 problem,  $F(1, 28) = 12.70, p < .005$ , but not in the 25-25 problem. Although the Problem  $\times$  Mood interaction was not significant for the males, their pattern of results was similar to that of the females. Table 5 presents the mean judged control scores for all of the experimental groups.

To summarize, nondepressed subjects

Table 5

*Means and Standard Deviations of Judged Control Scores by Problem, Mood, and Sex for Experiment 2*

Problem	Nondepressed				Depressed			
	Males		Females		Males		Females	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
25-25	20.0	26.7	7.5	14.5	13.8	12.7	17.1	23.7
75-75	30.3	12.8	51.4	29.8	21.2	28.5	13.1	13.6

overestimated the degree of control in the 75-75 noncontingency problem, but thought they had little control in the 25-25 noncontingency problem. In contrast, the depressed subjects made relatively accurate judgments about both problems. The presence of the triple interaction indicated that the effect of overestimating the degree of control in the 75-75 problem was more pronounced for nondepressed females than for nondepressed males. The triple interaction is presented graphically in Figure 4.

In addition to judging degree of control for one of the two problems, subjects also rated how certain they were of the accuracy of their judgment of control ratings. A Problem  $\times$  Mood  $\times$  Sex analysis of variance revealed no significant effects for these certainty ratings.

The finding that nondepressed subjects did not accurately judge the degree of contingency between their responses and green light onset suggests either that they did not know the relevant conditional probabilities associated with pressing and not pressing or that they knew the relevant probabilities but organized them inappropriately. Analysis of the scores measuring discrepancy between judged percentage of reinforcement and actual percentage of reinforcement associated with pressing provides evidence that both depressed and nondepressed subjects were relatively accurate in judging the conditional probabilities. The mean discrepancy scores (judged - actual) for percentage of reinforcement associated with pressing in the two problems were +1.62 for 25-25 and -6.12 for 75-75. A Problem  $\times$  Mood  $\times$  Sex analysis of variance of these discrepancy

scores yielded a significant problem effect,  $F(1, 56) = 4.32$ ,  $p < .05$ , and no other significant main effects or interactions. The mean discrepancy scores (judged - actual)

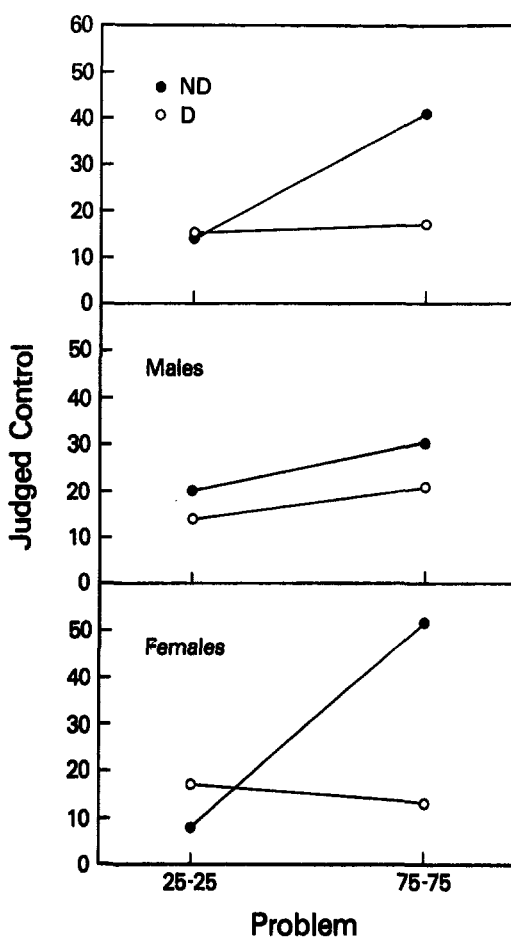


Figure 4. Judged control for depressed (D) and nondepressed (ND) students as a function of problem type in Experiment 2. (The Problem  $\times$  Mood  $\times$  Sex triple interaction is also presented.)

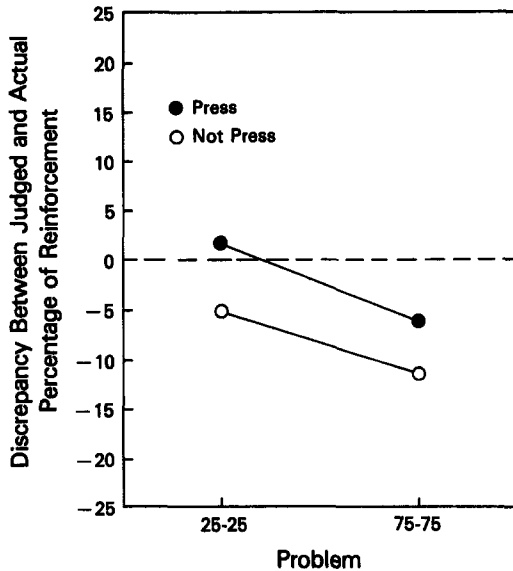


Figure 5. Discrepancy between judged and actual percentage of reinforcement for pressing and not pressing as a function of problem type in Experiment 2.

for percentage of reinforcement associated with not pressing in the two problems were  $-5.12$  for 25-25 and  $-11.44$  for 75-75. A Problem  $\times$  Mood  $\times$  Sex analysis of variance yielded no significant effects. Figure 5 shows the discrepancy scores for subjects' judgments of the percentage of reinforcement associated with pressing and not pressing.

It appears, then, that all subjects did have the appropriate data with which to make an accurate judgment of control. However, unlike the depressed subjects, the nondepressed subjects' actual judgments of control in the 75-75 problem were inaccurate. This suggests that the nondepressed subjects did not rely on the differential effectiveness of responses in organizing the probability data but rather may have relied on some invalid organizational rule. As in Experiment 1, a correlational analysis of the judgment of control data was performed in order to determine whether subjects were, in fact, using the invalid heuristics of percentage of reinforcement, percentage of successes, or confirming cases. Because the analysis of variance showed that depressed and nondepressed subjects differed systematically in

their judgments of control, separate correlational analyses were performed for depressed and nondepressed subjects. Nondepressed subjects' judgments of control correlated significantly with the invalid heuristics of judged percentage of reinforcement ( $r = .52$ ,  $p < .005$ ), percentage of successes ( $r = .49$ ,  $p < .005$ ), and confirming cases ( $r = .48$ ,  $p < .005$ ). Depressed subjects' judgments of control did not correlate significantly with judged percentage of reinforcement ( $r = -.02$ ), percentage of successes ( $r = -.01$ ), or confirming cases ( $r = .15$ ).

The final judgment scale to be reported is subjects' judgments of total reinforcement irrespective of their responses. The discrepancy between judged total reinforcement and actual total reinforcement was very small for both problems:  $-1.16$  for 25-25, and  $-3.33$  for 75-75. A Problem  $\times$  Mood  $\times$  Sex analysis of variance of these discrepancy scores yielded no significant effects or interactions. In addition, the correlations between judged total reinforcement and actual total reinforcement, based on all subjects, was significant ( $r = .94$ ,  $p < .0001$ ). Thus, subjects' judgments of total percentage of reinforcement were highly accurate.

**Behavioral task.** Although there is no "correct response" for maximizing green light onset in the behavioral task when outcomes are noncontingently related to responses, it is interesting to determine whether any systematic differences emerge in these data. A Problem  $\times$  Mood  $\times$  Sex analysis of variance of the number of presses in the behavioral task yielded no significant main effects or interactions. Neither mood nor problem type affected subjects' performance in the behavioral task. In general, the majority of subjects pressed the button an intermediate number of times, although a few subjects pressed rarely and others most of the time.

**Postexperimental questionnaire.** Unlike Experiment 1, a number of subjects reported that they tried complex patterns of responding while engaged in the problems of Experiment 2. A chi-square analysis revealed that the nondepressed subjects reported trying complex hypotheses more often than did the

depressed subjects,  $\chi^2(1) = 3.14$ ,  $p < .08$ . Seventeen of the nondepressed subjects and 10 of the depressed subjects reported trying complex hypotheses.

Subjects also answered open-ended questions about the kind of evidence that convinced them that they either did or did not have control. In the 25–25 problem, both depressed and nondepressed subjects tended to use appropriate logic in determining their judgments of control. For example, a depressed female in the 25–25 problem said, "I believe I had no control because I tried different sequences of pressing and nonpressing which didn't make any difference. I would have believed I had some control if the green light went on or off in accord with my actions—at least to some degree."

It is interesting that nondepressed subjects often used incorrect logic in arriving at their judgments of control in the 75–75 problem. The following examples illustrate the kinds of errors in reasoning that characterized the judgments of the nondepressed subjects. Some of the nondepressed subjects who showed an illusion of control seemed to use "intuition" rather than logic in judging that they had control: "I believe I had some degree of control. There was no objective evidence. I merely worked on intuition, whim, and so forth." Other nondepressed subjects' logic indicated that they were using the invalid heuristic of percentage of reinforcement: "I believed I had control because the green light came on a lot. I would have believed I had no control if the green light never came on." Finally, another subset of nondepressed subjects believed that very complex patterns of responses were responsible for the high frequency of green light onset: "Counting in intervals of five and judging how the light reacted at a certain time within that interval led me to believe I had control." Thus, the illusion of control shown by nondepressed subjects in the 75–75 problem was often associated with inappropriate reasoning.

### *Discussion*

Although the results of Experiment 1 strongly support the idea that humans' rep-

resentations of contingency mirror objective contingencies, the findings of Experiment 2 suggest that when responses and outcomes are noncontingently related, subjective representations of contingencies are not always isomorphic with objective contingencies. Both affective state and frequency of reinforcement interacted to produce systematic errors in judgment of noncontingency. Nondepressed individuals showed an illusion of control in a noncontingent, high-density reinforcement situation, but not in a noncontingent, low-density reinforcement situation. Depressed individuals, on the other hand, were not affected by the density of reinforcement in situations in which outcomes were noncontingently related to responses. They accurately detected lack of relation between responses and outcomes.

Both in the contingency problems of Experiment 1 and in the noncontingent, low-density reinforcement problem of Experiment 2, all subjects (including the nondepressives) tended to use appropriate logic in arriving at their judgments of control. In the noncontingent, high-density reinforcement problem, however, the logic of nondepressed students' judgments seemed to break down. Although nondepressed students' judgments of control correlated with actual degree of control in Experiment 1, in Experiment 2 they correlated with invalid heuristics such as how frequently the outcome occurred overall, how frequently the outcome occurred when they pressed the button, and how frequently the outcome occurred when they pressed plus how frequently it did not occur when they did not press.

Nondepressed students' use of invalid heuristics in arriving at their judgments of control was corroborated by their answers to open-ended questions that probed their reasoning. Some nondepressed students appeared to abandon all rational strategies in favor of intuitive strategies in arriving at judgments of control. Moreover, students' judgments of the conditional probabilities clearly indicate that it is generally the organization of the conditional probability data, rather than the perception of the data themselves, that is responsible for failure to detect noncontingency. Accordingly, Ward

and Jenkins (1965) found that when the conditional probability data were organized for the subjects in tabular form, rather than presented on a trial-by-trial basis, subjects' judgments of contingency improved markedly.

The results of Experiment 2 partially confirm the strong prediction of the helplessness model of depression. Although depressed students were relatively accurate in detecting a lack of contingency between their responses and outcomes, nondepressed students showed an illusion of control when the outcome occurred frequently, but not when the outcome occurred infrequently. In the 25-25 problem, 50% of both depressed and nondepressed students actually said that they had zero control. In the 75-75 problem, 50% of depressed students, but only 6% of nondepressed students, said that they had zero control.

What is the important psychological dimension that distinguishes between the 25-25 and 75-75 problems? Of course, by definition, the two problems differed on frequency of the outcome's occurrence. Students may, however, have construed high frequency of reinforcement as a "good" outcome and low frequency of reinforcement as a "bad" outcome. Informal conversations with the subjects during the debriefing revealed that they did, in fact, view "goodness" of outcome as being determined by the frequency of green light onset. Thus, valence of the outcome, rather than frequency per se, may have been the crucial psychological variable underlying nondepressed students' errors in the judgment of noncontingency.

It is interesting that the illusion of control was much more pronounced in nondepressed females than in nondepressed males. This sex difference is puzzling from the standpoint of learned helplessness because the model makes no differential predictions concerning sex. Langer's (1975) study on the illusion of control provides a clue for interpretation of the sex effect in Experiment 2. Langer noted that males, unlike females, did not show illusions of control in those studies in which demand for rationality was very high. This explanation pertains to the present study because rational judgments of contin-

gency were strongly emphasized in the experimental instructions. This reasoning leads to the prediction that nondepressed males would show a bigger illusion of control relative to depressed people if there were less focus on rationality in the experimental instructions.

The considerations of this discussion prompted us to perform a third experiment in which we further examined the illusion of control. First, obtaining an illusion or cognitive distortion in a "normal" group is a surprising finding and warrants replication in a different context. Second, valence of the outcome needs to be specifically manipulated while holding frequency of reinforcement constant because outcome valence may be crucial in accounting for differences in judged contingency between depressed and nondepressed people. Finally, a situation in which less demand for rationality is placed on the subject may be required in order to enhance the illusion of control.

### Experiment 3

Experiment 3 further examined the illusion of control in a situation that was less highly structured and more like the "real world." In Experiment 2, students' major task was to determine the degree of control their responses exerted over green light onset. In everyday life, however, detecting environmental contingencies occurs in the larger context of learning how to obtain desired outcomes and avoid aversive outcomes. Thus, Experiment 3 examined judgment of contingency in a situation in which outcomes were response independent and students were focused on obtaining monetary rewards. Students were informed from the outset that they would be asked to judge how much control they had at the end of the task.

Experiment 3 also investigated the importance of the valence of the outcome as a psychological determinant of the illusion of control. Depressed and nondepressed students were assigned to one of two problems similar to those of Experiment 2, except that green light onset was associated with either gain or loss of money. At the same

Table 6

*Means and Standard Deviations of BDI and MAACL Scores by Problem, Mood, and Sex for Experiment 3*

Problem and test	Nondepressed				Depressed			
	Males		Females		Males		Females	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Lose								
BDI	4.4	1.8	2.8	2.4	14.2	3.5	18.5	8.9
MAACL	9.1	4.5	8.0	3.5	20.0	4.5	23.0	7.0
Win								
BDI	3.2	1.8	3.1	2.9	14.5	4.6	13.0	5.4
MAACL	8.5	3.2	9.0	3.3	18.9	3.6	22.1	3.2

*Note.* BDI = Beck Depression Inventory; MAACL = Multiple Affect Adjective Check List.

time, frequency of green light onset was held constant across the two problems. Changes in mood were examined to determine whether this outcome valence manipulation was effective for both depressed and nondepressed students.

In Experiments 1 and 2, the Beck Depression Inventory, which measures enduring depression, was used to select depressed and nondepressed students. For most students, scores on the BDI correlated highly with initial depression scores on the Multiple Affect Adjective Check List, a measure of transient mood. A few subjects, however, scored high on one inventory and low on the other. To insure separation of depressed and nondepressed groups, the initial depression score of the MAACL was also used as a mood selection criterion in Experiment 3.

### Method

**Subjects.** Sixty-four undergraduates, 32 males and 32 females, from the University of Pennsylvania served as paid volunteers. The data of one subject were discarded because of an error in experimental procedure. Subjects were assigned to depressed or nondepressed groups on the basis of both BDI and MAACL scores. The BDI criterion was identical to that of Experiments 1 and 2. In addition, subjects had to score 14 or above, or 13 or below on the MAACL to qualify as depressed or nondepressed, respectively. Subjects who did not meet both criteria did not qualify for the experiment. Given that subjects were selected on the basis of both BDI and MAACL scores, it is not surprising that the correlation between scores was higher than in Experiments 1 and 2 ( $r = .84$ ,  $p <$

.001). Table 6 presents the mean BDI and MAACL scores for all experimental groups. The final sample of subjects consisted of 32 depressed students (16 males and 16 females) and 32 nondepressed students (16 males and 16 females). Subjects were randomly assigned to the two experimental conditions, with the restriction that each condition contain equal numbers of males and females and depressed and nondepressed students. Half of the students in each condition were tested by Experimenter 1 and half by Experimenter 2.

**Experimental design.** The experiment was a 2 (Problem Type)  $\times$  2 (Mood)  $\times$  2 (Sex) factorial design. The two problems differed in the valence of the outcome, but did not differ in either degree of control or frequency of the outcome. In Problem 1 (lose), subjects had no control and were reinforced (green light onset) on 50% of the trials. Subjects in this problem started out with \$5. Each time the green light did not come on, subjects lost a quarter; when the green light did come on, no money was lost. In Problem 2 (win), subjects had no control and were also reinforced (green light onset) on 50% of the trials. Subjects in this problem began with no money and gained a quarter each time the green light came on. On trials on which the green light did not come on, subjects did not gain any money. Thus, all subjects in the lose condition lost \$5, and all subjects in the win condition won \$5. Of the 64 students, 32 (8 depressed males, 8 depressed females, 8 nondepressed males, and 8 nondepressed females) were assigned to each condition.

**Dependent measures.** With the exception of the behavioral task, all dependent measures employed in Experiments 1 and 2 were also employed in Experiment 3. In addition, the MAACL was administered both before and after the problems to determine whether subjects' moods were affected by the outcome valence manipulation. The MAACL yielded three affect dependent measures: an anxiety change score, a depression change score, and a

hostility change score. An additional question was added to the postexperimental questionnaire to provide another measure of subjects' beliefs about control: "To what extent do you think that onset of the green light was due to factors other than your own responding?"

*Apparatus.* The apparatus for Experiment 3 was identical to that of Experiments 1 and 2, with one addition. A Kodak 650 H Carousel slide projector with a remote control panel presented slides depicting the amount of money subjects had at any point in time. The slides were projected onto a 75 cm  $\times$  60 cm screen positioned on the wall approximately 90 cm from the subject. The remote control panel was operated from the control room of the two-room suite.

*Procedure.* When the subject entered the room he or she was seated at a desk and was administered the BDI and MAACL according to the procedure of Experiment 1. Upon completion of the mood inventories, the subject was assigned to the appropriate group and led to a table on which the apparatus for the contingency learning problem was displayed. The procedure for the win and lose problems was identical to that of the problems in Experiment 1, with the addition that a new slide depicting the subject's current earnings was presented on each trial after the green light had either appeared or not appeared. The instructions for the win and lose problems were similar to those of Experiment 1. The major difference in the instructions was that in Experiment 1 subjects were told that their major task was to learn the degree of control their responses had over the onset of the green light whereas in Experiment 3 their ostensive major task was to learn how to turn on the green light. This difference was reflected in a change in two sentences of the original instructions. The first sentence was changed from "Now, in this problem-solving experiment, it is your task to learn what degree of control *you* have over whether or not this green light comes on" to "Now, in this problem-solving experiment, it is your task to learn how to turn *on* this green light." The sixth sentence of the second paragraph was changed from "Since it is your job to learn how much control *you* have over whether the green light comes on, as well as whether the green light does not come on, it is to your advantage . . ." to "Since it is your job to learn how to turn *on* the green light, it is to your advantage. . . ."

An additional paragraph concerning the gain or loss of money aspect of the experiment was inserted between paragraphs 2 and 3 of the original instructions, before the judgment of control instructions. The paragraph for the win problem was:

Moreover, how often the green light comes on in this problem will determine how much money you earn in the experiment. On each trial on which the green light does go on, you will earn a quarter. Alternatively, on each trial on which the green light does *not* go on, you will not

earn any money. At the end of the problem, you will get to keep all of the money you have earned up to a maximum of \$5. So, in general, the more successful you are in producing the green light, the more money you will take away with you at the end of the experiment. During the problem, you will be able to see how much money you currently own by watching the green screen. The slide projector to your left will keep track of your accumulation, if any, of quarters.

The paragraph for the lose problem was:

Moreover, how often the green light comes on in this problem will determine how much money you earn in the experiment. I will start you off with \$5. On each trial on which the green light does *not* go on, you will lose a quarter. Alternatively, on each trial on which the green light does go on, you will not lose any money. At the end of the problem, you will get to keep all of the money you have left. Of course, the maximum amount of money you can lose is \$5. I will not ask for any money out of your pocket. In general though, the more successful you are in producing the green light, the more money you will take away with you at the end of the experiment. During the problem, you will be able to see how much money you currently own by watching the green screen. The slide projector to your left will keep track of your loss, if any, of quarters.

After the judgment of control instructions were read, the experimenter left the room and the subject proceeded with the contingency learning problem. At the end of the 40 trials, the experimenter returned and paid the subject \$5 or removed \$5, depending on the problem to which the subject had been assigned. The control instructions were then reread, and the subject completed each of the four judgment scales. Following the judgment scales, the subject completed the MAACL for a second time, followed by the postexperimental questionnaire. The experimenter left the room while the subject completed these questionnaires. Finally, the subject was carefully debriefed and given \$5 for participating if he or she had been in the lose condition. Subjects in the win condition were allowed to keep the \$5 they had already received.

## Results

As in Experiments 1 and 2, analysis of variance showed no experimenter effects on any of the dependent measures, and this factor was dropped in subsequent statistical analyses.

*Judgment scales.* A Problem  $\times$  Mood  $\times$  Sex analysis of variance of the judgment of control scores yielded significant main effects for problem,  $F(1, 56) = 31.97$ ,  $p < .001$ ;

Table 7

*Means and Standard Deviations of Judged Control Scores by Problem, Mood, and Sex for Experiment 3*

Problem	Nondepressed				Depressed			
	Males		Females		Males		Females	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Lose	6.9	11.9	21.2	8.8	21.2	20.3	11.4	13.1
Win	49.4	29.3	64.4	9.8	17.2	16.5	36.0	28.1

mood,  $F(1, 56) = 8.88$ ,  $p < .004$ ; and sex,  $F(1, 56) = 4.14$ ,  $p < .05$ ; and a significant Problem  $\times$  Mood interaction,  $F(1, 56) = 11.96$ ,  $p < .001$ . The significant main effect for sex indicated that females thought that they had more control than males. To interpret the significant main effects for problem and mood, it is necessary to analyze the Problem  $\times$  Mood interaction. To analyze the interaction, a simple main effects test (Winer, 1962) was used. Nondepressed subjects judged that they had more control in the win problem than in the lose problem,  $F(1, 56) = 41.53$ ,  $p < .001$ , whereas depressed subject' judgments did not differ between the two problems. In addition, nondepressed subjects judged that they had more control than did depressed subjects in the win problem,  $F(1, 56) = 20.73$ ,  $p < .001$ , but not in the lose problem. Table 7 presents the means and standard deviations for the judgment of control ratings for all experimental groups. In addition, Figure 6 graphically portrays the Problem  $\times$  Mood interaction.

A Problem  $\times$  Mood  $\times$  Sex analysis of variance of the certainty ratings also revealed a significant Problem  $\times$  Mood interaction,  $F(1, 56) = 5.87$ ,  $p < .02$ . Depressed subjects were more certain of their judgments of control in the win problem than in the loss problem. Nondepressed subjects, on the other hand, were more certain of their judgments in the lose problem than in the win problem.

Figure 7 shows that subjects again were relatively accurate in judging the conditional probabilities of green light onset associated with pressing and not pressing. The mean

scores of discrepancy between judged percentage of reinforcement and actual percentage of reinforcement associated with pressing were  $-1.38$  for the win problem and  $-12.97$  for the lose problem. A Problem  $\times$  Mood  $\times$  Sex analysis of variance of these discrepancy scores revealed a significant problem main effect,  $F(1, 56) = 9.50$ ,  $p < .003$ , and no other significant main effects or interactions. The mean discrepancy scores (judged - actual) for percentage of reinforcement associated with not pressing were  $-6.50$  for the win problem and  $-8.69$  for the lose problem. No significant effects were obtained on a Problem  $\times$  Mood  $\times$  Sex analysis of variance of these scores.

In Experiment 2, nondepressed subjects

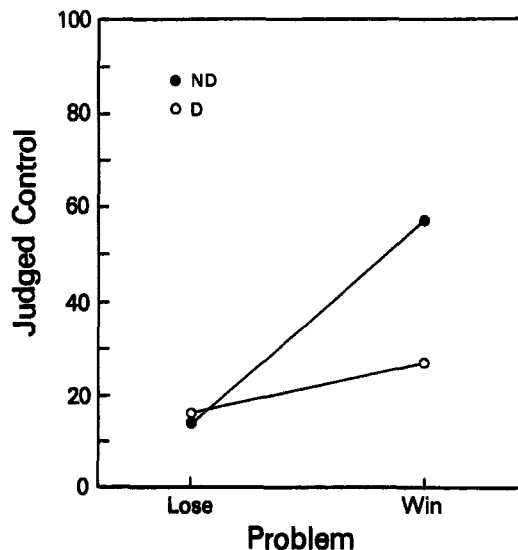


Figure 6. Judged control for depressed (D) and nondepressed (ND) students as a function of problem type in Experiment 3.

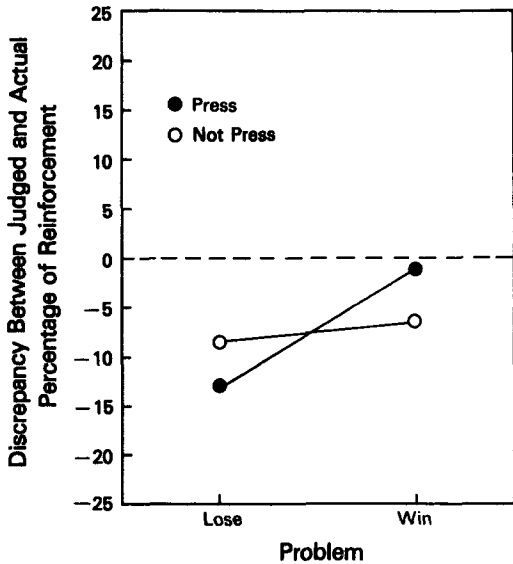


Figure 7. Discrepancy between judged and actual percentage of reinforcement for pressing and not pressing as a function of problem type in Experiment 3.

appeared to use the invalid heuristics of percentage of reinforcement, percentage of successes, and confirming cases whereas depressed subjects did not. These three heuristics are intercorrelated in that they all rely on the common element of the frequency of green light onset. If frequency of green light onset were the psychological dimension underlying nondepressed subjects' judgments of control in Experiment 3, then judgments of control again should have been correlated with these heuristics for nondepressed subjects. No such significant correlations were obtained, however, for either depressed or nondepressed subjects. If the nondepressed subjects had only been using these heuristics, they would have overestimated the degree of control in both the win and lose problems, since both problems had a high frequency of reinforcement (50%). The obtained result of a Problem  $\times$  Mood interaction on the judged control scores points to the psychological importance of the valence of the outcome for the nondepressed subjects over and above the frequency of the outcome.

Although subjects' judgments of total

reinforcement irrespective of their responses were highly accurate in Experiments 1 and 2, the outcome valence manipulation of the present experiment affected these judgments adversely. A Problem  $\times$  Mood  $\times$  Sex analysis of variance of the scores measuring the discrepancy between judgment of total reinforcement and actual total reinforcement yielded a significant main effect for problem,  $F(1, 56) = 10.07$ ,  $p < .002$ . In general, subjects accurately estimated the frequency of green light onset in the win problem (discrepancy score =  $-.62$ ), but underestimated green light frequency in the lose problem (discrepancy score =  $-8.66$ ). However, a marginally significant Problem  $\times$  Mood interaction,  $F(1, 56) = 3.76$ ,  $p < .06$ , shows that this effect was more pronounced in nondepressed than in depressed subjects.

*Affect change scores.* All subjects experienced mood changes as a function of outcome valence, suggesting that the outcome valence manipulation was effective. A Problem  $\times$  Mood  $\times$  Sex analysis of variance was performed on each of the three affect change scores (anxiety, depression, and hostility). For the anxiety change score, significant main effects for problem,  $F(1, 56) = 6.53$ ,  $p < .02$ , and mood,  $F(1, 56) = 11.80$ ,  $p < .001$ , were obtained, with no other significant main effects or interactions. The analysis of variance for change in depression also yielded significant main effects for problem,  $F(1, 56) = 10.60$ ,  $p < .002$ , and mood,  $F(1, 56) = 16.14$ ,  $p < .001$ . Finally, the analysis of variance for the hostility change scores revealed a significant main effect for problem,  $F(1, 56) = 4.62$ ,  $p < .04$ , and a marginally significant main effect for mood,  $F(1, 56) = 3.88$ ,  $p < .06$ . As can be seen in Figure 8, all subjects became more dysphoric in the lose problem than in the win problem. In addition, the main effect for mood is probably best interpreted as representing a constraint placed on subjects' affect change scores by their initial MAACL scores. That is, depressed subjects showed larger changes in the euphoric direction in the win problem, and nondepressed subjects showed larger

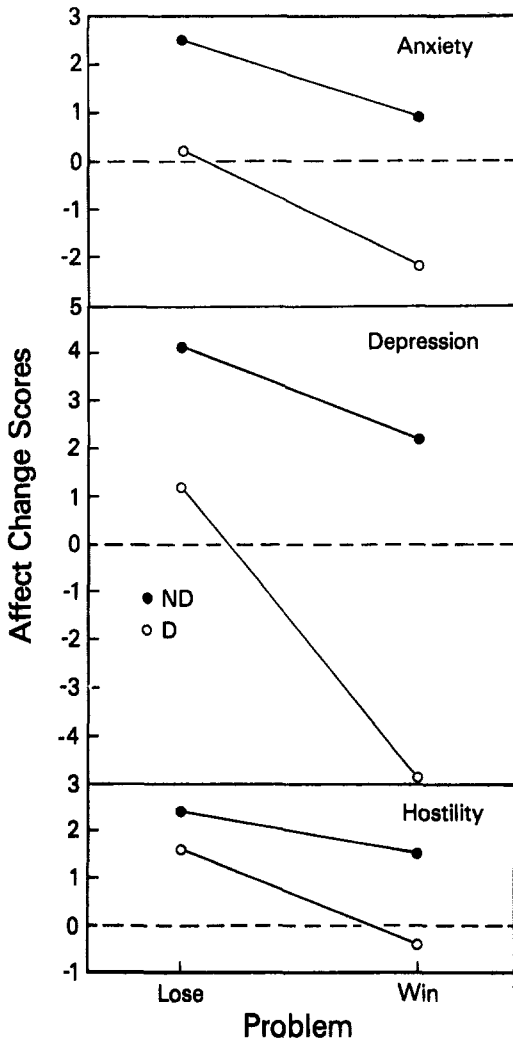


Figure 8. Anxiety, depression, and hostility change scores for depressed (D) and nondepressed (ND) students as a function of problem type in Experiment 3. (Positive values indicate affect changes in the dysphoric direction, and negative values indicate affect changes in the euphoric direction.)

changes in the dysphoric direction in the lost problem.<sup>5</sup>

*Postexperimental questionnaire.* In general, the analysis of the questionnaire data corroborated the findings already reported for judgments of control. A Problem  $\times$  Mood  $\times$  Sex analysis of variance on the question assessing the degree to which subjects believed that factors other than their own responding produced green light onset

revealed a significant problem effect,  $F(1, 56) = 13.14$ ,  $p < .025$ , and a significant Problem  $\times$  Mood interaction,  $F(1, 56) = 4.70$ ,  $p < .04$ . A simple main effects test showed that nondepressed subjects believed that factors other than their own responding were significantly more important in the lose problem than in the win problem,  $F(1, 56) = 10.12$ ,  $p < .005$ , whereas there was no difference between the problems for depressed subjects. In addition, depressed subjects believed that factors other than their own responding were significantly more important in the win problem than did the nondepressed subjects,  $F(1, 56) = 4.18$ ,  $p < .05$ . There was no difference between depressed and nondepressed subjects in the lose problem. Thus, subjects' answers on the "Factors" question were consistent with their answers on the Judgment of Control scale.

It is interesting that in the open-ended questions assessing students' logic in arriving at their judgments of control, nondepressed subjects often reported that winning the \$5 was sufficient evidence for concluding that they had control. Depressed subjects, on the other hand, did not use this type of reasoning. Finally, unlike Experiment 2, there was no difference between the number of depressed and nondepressed subjects who reported trying complex hypotheses. Only 9 of the total 64 subjects reported not trying complex patterns of responding.

### Discussion

Experiment 3 provided strong corroborative evidence for the hypothesis developed

<sup>5</sup> Although one might think that analysis of covariance should be used to equate for initial MAACL scores, we believe this statistical technique is inappropriate because the depressed and nondepressed groups were selected partially on the basis of their initial MAACL depression score. In addition, since the MAACL initial anxiety and hostility scores tended to correlate highly with the initial depression score, it was deemed inappropriate to use the analysis of covariance on these change dependent measures as well. By this same logic, analysis of covariance was not used in Experiment 4.

in Experiment 2: Depressed people accurately detect noncontingency between their responses and outcomes whereas nondepressed people show illusions of control. It is interesting that nondepressed people showed more pronounced illusions of control in situations in which they were attempting to obtain desired outcomes than in situations in which they were judging the degree of control over more neutral outcomes. In addition, recall that in Experiment 2, nondepressed males showed only a slight tendency to overestimate the degree of contingency in the noncontingent, high-density reinforcement situation whereas in Experiment 3 their illusion of control was considerably larger and, in fact, quite pronounced. This finding is in line with Langer's (1975) hypothesis that males are more susceptible to illusions of control in situations in which there is less emphasis on rationality.

The results of Experiment 3 demonstrated that overestimations of control in nondepressed people are a function of the valence of the outcome of interest. Nondepressed students believe that they have a high degree of control in situations in which they obtain desired outcomes noncontingently, but believe that they have very little control in situations in which they lose desired outcomes noncontingently.

The importance of the role of valence, rather than mere frequency, of the outcome in determining judgments of contingency in Experiment 3 is supported by the actual pattern of judged control scores for nondepressed students. If frequency of the desired outcome were the sole determinant of nondepressed students' judgments of control in situations of response-outcome independence, then they should have rated that they had about 50% control (i.e., show an illusion) in the lose problem as well as in the win problem. The obtained result was that they only showed an illusion in the win problem. Lack of correlation between invalid heuristics involving frequency of the desired outcome and judged control scores for nondepressed students provides further support for the role of outcome valence as a determinant of judgments of control.

Moreover, the finding that nondepressed students overestimated the degree of control only in the win problem and not in the lose problem provides partial, but not complete, support for the learned helplessness model of depression. The model makes no differential predictions concerning outcome valence. Everyday observation suggests that in the real world when bad outcomes happen to an individual he or she often has no control over them, and when good outcomes happen the individual often does have control. This observation is most likely due to the fact that when individuals actually have control, they do not allow bad outcomes to occur. Perhaps, then, past experience with having control over good outcomes but not bad outcomes acts in conjunction with the generalized expectancy of control predicted for nondepressives by the helplessness theory to enhance the illusion of control over good outcomes and to attenuate the illusion of control over bad outcomes (see Bem, 1972, for a related discussion of attributional style).

Perhaps one could explain the failure of depressed students to show an illusion of control in the win problem similar to nondepressed students by arguing that winning \$5 was not as "good" an outcome for depressed students as for nondepressed students. Indeed, Costello (1972) views reinforcer ineffectiveness as the *sine qua non* of depression. Such an explanation is disconfirmed, however, by the results of the affect change scores. On all three of the affect measures, depressed students showed at least as great an enhancement of mood in the win condition as the nondepressed students, if not greater. Thus, the outcome valence manipulation was equally as effective for depressed students as for nondepressed students.

Finally, in addition to judgments of control, judgments of reinforcement or outcome frequency also were adversely affected by the outcome valence manipulation of Experiment 3. All students believed that the desired outcome of green light onset was more frequent in the win problem than in the lose problem; however, this effect was more

pronounced in the nondepressed than in the depressed students. Thus, in hedonically charged situations in which outcomes are noncontingently related to responses, nondepressed individuals show two kinds of errors: distortions in judgments of contingency and distortions in judgments of outcome frequency.

### Experiment 4

The finding in Experiments 2 and 3 that nondepressed people overestimated the degree of control between their responses and desirable outcomes whereas depressed people were relatively accurate, supports the new prediction of the helplessness model. This finding provides a unique kind of support for helplessness theory, since previous tests of the theory only have looked at depressive deficits in learning contingent relationships. Failure to observe underestimation of degree of control by depressed students in Experiment 1 while at the same time observing overestimation of degree of control by nondepressed students is puzzling from the standpoint of helplessness theory. Because helplessness theory depends so heavily on absolute or relative depressive deficits in contingency learning, it is important to determine whether under some conditions depressive do, in fact, underestimate the degree of contingency between their responses and outcomes. Perhaps depressed students would underestimate the degree of control that their responses exerted over contingent outcomes if these outcomes were hedonically charged. The fact that the nondepressive illusion of control was more robust in Experiment 3 than in Experiment 2 supports this line of reasoning. Thus, in Experiment 4 students received one of two problems, both of which had a 50% degree of contingency between responses and green light onset, but which differed in outcome valence.

### Method

**Subjects.** Sixty-four undergraduates, 32 males and 32 females, from the University of Pennsylvania served as paid volunteers. Procedures for

subject selection were identical to those in Experiment 3. As in Experiment 3, the correlation between the BDI and the MAACL was quite high due to the selection procedure ( $r = .78$ ,  $p < .001$ ). Table 8 presents the mean BDI and MAACL scores for all experimental groups. The final sample of subjects consisted of 32 depressed students (16 males and 16 females) and 32 nondepressed students (16 males and 16 females). Subjects were randomly assigned to the two experimental conditions, with the restriction that each condition contain equal numbers of males and females and depressed and nondepressed students. One experimenter tested all subjects.<sup>6</sup>

**Experimental design.** The experiment was a 2 (Problem Type)  $\times$  2 (Mood)  $\times$  2 (Sex)  $\times$  2 (Response) factorial design. The two problems differed in the valence of the outcome, but did not differ in degree of control. In Problem 1 (lose), subjects had 50% control (75-25 or 25-75). Subjects in the lose problem started out with \$4. Each time the green light did not come on subjects lost 30 cents. On trials in which the green light did come on, no money was lost. In Problem 2 (win), subjects also had 50% control (75-25 or 25-75). Subjects in the win problem began with no money, and each time the green light came on they gained a dime. On trials in which the green light did not come on subjects did not gain any money. Thus, all subjects in the lose condition lost between \$3.50 and \$4, and all subjects in the win condition won between \$3.50 and \$4. Within each of the problems, subjects were counterbalanced for whether pressing or not pressing led to the higher percentage of green light onset (i.e., 75-25 or 25-75).

**Dependent measures.** All dependent measures were identical to those in Experiment 3.

**Apparatus and procedure.** The apparatus and procedure were identical to those in Experiment 3, with the exception of specific instructions concerning the amount of money that could be won or lost.

### Results

**Judgment scales.** A Problem  $\times$  Mood  $\times$  Sex  $\times$  Response analysis of variance of the judgment of control scores yielded significant main effects for problem,  $F(1, 48) = 13.45$ ,  $p < .001$ ; and response,  $F(1, 48) = 11.02$ ,  $p < .002$ ; and significant Problem  $\times$  Mood,  $F(1, 48) = 6.90$ ,  $p < .02$ ; and Mood  $\times$  Response,  $F(1, 48) = 3.87$ ,  $p < .055$ , interactions. To interpret the obtained main effects, it is necessary to analyze the interactions involving these factors. To analyze the Problem  $\times$  Mood interaction, a simple

<sup>6</sup> We would like to thank Linda Kanefield for conducting Experiment 4.

Table 8  
Means and Standard Deviations of BDI and MAACL Scores by Problem,  
Mood, Sex, and Response for Experiment 4

Problem	Response and test	Nondepressed				Depressed			
		Males		Females		Males		Females	
		<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Lose	75-25								
	BDI	4.5	1.7	5.8	2.6	11.5	3.1	11.2	1.0
	MAACL	9.8	4.5	8.5	3.5	21.0	4.4	19.2	3.4
	25-75								
Win	75-25								
	BDI	1.5	1.7	3.0	1.4	12.5	2.1	13.2	3.9
	MAACL	9.5	2.7	6.5	2.7	20.0	2.8	21.2	5.1
	25-75								
	BDI	4.2	2.5	2.8	1.7	11.5	2.1	12.0	2.7
	MAACL	10.5	2.1	11.2	1.1	15.5	2.1	25.0	3.3

Note. Response refers to whether pressing or not pressing the button was associated with the higher percentage of reinforcement. BDI = Beck Depression Inventory, MAACL = Multiple Affect Adjective Check List.

main effects test was used (Winer, 1962). Nondepressed subjects judged that they had less control in the lose problem than in the win problem,  $F(1, 48) = 19.60$ ,  $p < .001$ ,

whereas depressed subjects' judgments did not differ between the two problems. In addition, nondepressed subjects judged that they had less control in the lose problem than did depressed subjects,  $F(1, 48) = 8.46$ ,  $p < .01$ , but not in the win problem. Figure 9 portrays the Problem  $\times$  Mood interaction.

A simple main effects test on the Mood  $\times$  Response interaction showed that nondepressed subjects judged that they had less control when not pressing was associated with 75% reinforcement (25-75) than when pressing was associated with 75% reinforcement (75-25),  $F(1, 48) = 14.40$ ,  $p < .001$ . The judgments of depressed subjects were not affected by whether pressing or not pressing was associated with 75% reinforcement. In addition, nondepressed subjects judged that they had significantly less control when not pressing was associated with 75% reinforcement than did depressed subjects,  $F(1, 48) = 5.78$ ,  $p < .05$ , but not when pressing was associated with 75% reinforcement. Table 9 presents the means and standard deviations for the judgment of control ratings for all experimental groups.

A Problem  $\times$  Mood  $\times$  Sex  $\times$  Response analysis of variance on the certainty ratings

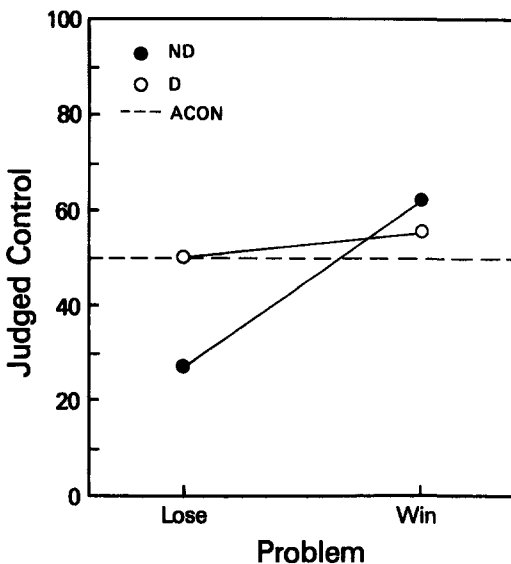


Figure 9. Judged control for depressed (D) and nondepressed (ND) students as a function of problem type in Experiment 4. (Actual degree of control [ACON] is also shown as a function of problem type.)

Table 9

*Means and Standard Deviations of Judged Control Scores by Problem, Mood, Sex, and Response for Experiment 4*

Problem	Response	Nondepressed				Depressed			
		Males		Females		Males		Females	
		<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Lose	75-25	53.8	36.4	42.5	16.6	54.5	15.4	52.5	36.9
	25-75	6.2	7.5	6.2	9.5	44.2	13.4	49.5	23.3
Win	75-25	75.0	12.0	67.0	15.5	64.5	12.6	56.0	18.6
	25-75	41.2	29.5	66.2	32.0	63.5	22.5	40.0	25.5

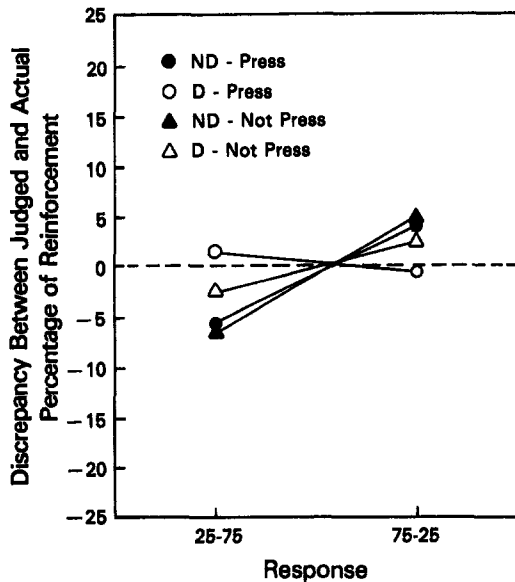
*Note.* Response refers to whether pressing or not pressing the button was associated with the higher percentage of reinforcement.

revealed a significant main effect for sex,  $F(1, 48) = 4.08, p < .05$ . Females were less certain of their judgments of control than males.

Depressed subjects were more accurate than nondepressed subjects in judging the conditional probability of green light onset associated with pressing the button. A Problem  $\times$  Mood  $\times$  Sex  $\times$  Response analysis of variance of the mean discrepancy (judged - actual) scores for percentage of reinforcement associated with pressing yielded a significant Mood  $\times$  Response interaction,  $F(1, 48) = 6.06, p < .02$ . For the nondepressives, the mean discrepancy scores associated with pressing were +4.12 for 75-25 and -5.75 for 25-75. For depressives, the mean discrepancy scores were -.50 for 75-25 and +1.44 for 25-75. A simple main effects test on these scores showed that nondepressives' discrepancy scores differed between 75-25 and 25-75,  $F(1, 48) = 8.46, p < .01$ , whereas depressives' scores did not differ. Within the 75-25 condition, the discrepancy scores of depressed and nondepressed subjects did not differ, whereas they did differ in the 25-75 condition,  $F(1, 48) = 4.48, p < .05$ . The mean discrepancy (judged-actual) scores for percentage of reinforcement associated with not pressing were -.38 for the win problem and +.44 for the lose problem. No significant main effects or interactions were obtained on a Problem  $\times$  Mood  $\times$  Sex  $\times$  Response analysis of variance of the mean discrepancy scores for percentage of rein-

forcement associated with not pressing. Figure 10 portrays these discrepancy scores.

By the same logic as in Experiment 3, use of the invalid heuristics of percentage of reinforcement, percentage of successes, and confirming cases cannot be invoked to explain nondepressives' different judgments of control in the win and lose problems.



*Figure 10.* Discrepancy between judged and actual percentage of reinforcement for pressing and not pressing and for depressed (D) and nondepressed (ND) students as a function of response in Experiment 4. (Response refers to whether pressing [75-25] or not pressing [25-75] was associated with the higher percentage of reinforcement.)

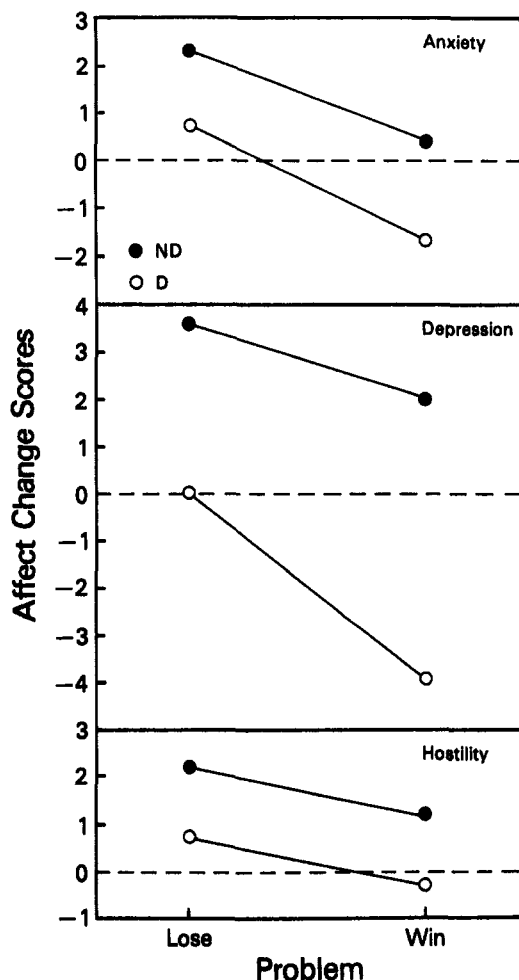


Figure 11. Anxiety, depression, and hostility change scores for depressed (D) and nondepressed (ND) students as a function of problem type in Experiment 4. (Positive values indicate affect changes in the dysphoric direction, and negative values indicate affect changes in the euphoric direction.)

These three heuristics are intercorrelated in that they all rely on the common element of the frequency of green light onset. Since the frequency of green light onset did not differ in the win and lose problems, use of any of these heuristics as the basis for judgments of control would lead to similar ratings of control in the two problems. Interestingly, nondepressed subjects' judgments of control did correlate with percentage of successes ( $r = .47, p < .01$ ) and confirming cases ( $r = .45, p < .01$ ). Depressed subjects' judgments

of control did not correlate significantly with these heuristics. The finding that judged control ratings correlated with percentage of successes and confirming cases for nondepressed but not depressed subjects is consistent with and may explain the Mood  $\times$  Response interaction for judgments of control.

A Problem  $\times$  Mood  $\times$  Sex  $\times$  Response analysis of variance of the scores measuring the discrepancy between judgment of total reinforcement and actual total reinforcement yielded a significant main effect for problem,  $F(1, 47) = 4.44, p < .05$ . In general, subjects accurately estimated the frequency of green light onset in the lose problem (discrepancy score =  $-.09$ ) but overestimated green light frequency in the win problem (discrepancy score =  $+6.12$ ).

Subjects experienced mood changes as a function of outcome valence. A Problem  $\times$  Mood  $\times$  Sex  $\times$  Response analysis of variance was performed on each of the three affect change scores (anxiety, depression, and hostility). For the anxiety change score, significant main effects for problem,  $F(1, 48) = 9.85, p < .003$ , and mood,  $F(1, 48) = 7.67, p < .008$ , were obtained, with no other significant main effects or interactions. The analysis of variance for change in depression also yielded significant main effects for problem,  $F(1, 48) = 7.87, p < .007$ , and mood,  $F(1, 48) = 23.26, p < .001$ .

The analysis of variance for change in hostility revealed no significant effects. Figure 11 shows that subjects tended to become more dysphoric in the lose problem than in the win problem. As in Experiment 3, the main effect for mood probably represents a constraint placed on subjects' affect change scores by their initial MAACL scores.

*Postexperimental questionnaire.* Subjects' responses on the postexperimental questionnaire provided converging evidence for nondepressives' errors in judgments of control. A Problem  $\times$  Mood  $\times$  Sex  $\times$  Response analysis of variance of the question assessing the degree to which subjects believed that factors other than their own responding produced green light onset revealed a significant Problem  $\times$  Mood

interaction,  $F(1, 48) = 4.68, p < .04$ , and a significant Mood  $\times$  Response interaction,  $F(1, 48) = 4.68, p < .04$ . The simple main effects test analyzing the Problem  $\times$  Mood interaction showed that nondepressed subjects believed that factors other than their own responding were significantly more important in the lose problem than in the win problem,  $F(1, 48) = 7.91, p < .01$ , whereas there was no such difference for depressed subjects. In addition, nondepressed subjects believed that factors other than their own responding were significantly more important in the lose problem than did the depressed subjects,  $F(1, 48) = 4.84, p < .05$ . There was no difference between depressed and nondepressed subjects in the win problem.

The simple main effects test analyzing the Mood  $\times$  Response interaction showed that nondepressed subjects believed that factors other than their own responding were significantly more important when not pressing was associated with 75% reinforcement (25-75) than when pressing was associated with 75% reinforcement (75-25),  $F(1, 48) = 7.91, p < .01$ . Depressed subjects did not rate factors other than their own responding as differentially important in the two conditions. In addition, nondepressed subjects believed that factors other than their own responding were significantly more important than did depressives when not pressing was associated with 75% reinforcement (25-75),  $F(1, 48) = 4.84, p < .05$ . Depressed and nondepressed subjects did not differ when pressing was associated with 75% reinforcement (75-25). Thus, subjects' answers on the "factors" question were consistent with their answers on the judgment of Control scale.

It is interesting that in the open-ended questions assessing subjects' logic in arriving at their judgments of control, nondepressives accurately reported that one response was more effective than the other in producing the green light in the win problem, but often inaccurately reported that the two responses were equally ineffective in the lose problem. Depressed subjects, on the other hand, accurately reported that pressing and not pressing were differentially effective regardless

of whether they were winning or losing money.

### *Discussion*

Contrary to the prediction of the learned helplessness hypothesis, depressed students did not underestimate the degree of contingency between their responses and outcomes in a hedonically charged situation. Outcome valence was not without an effect in Experiment 4, however. The nondepressed students underestimated the degree of control their responses exerted over outcomes. Nondepressives erroneously believed that their responses had very little control over the green light when they lost money, but not when they won money, even though the actual degree of control was 50% in each case. Thus, as in Experiment 3, outcome valence influenced the judgments of contingency of nondepressed, but not depressed, students.

In addition, nondepressed students' judgments of control also were affected by whether the active (pressing) or the passive (not pressing) response was associated with the higher frequency of reinforcement. Nondepressives underestimated the degree of contingency when the passive response led to the higher frequency of reinforcement. Depressed students, on the other hand, accurately judged the degree of contingency between their responses and outcomes regardless of whether the active or passive response was more effective.

Nondepressives underestimated the degree of control when not pressing was the more effective of the two responses. This finding is consistent with and might be deduced from the additional fact that nondepressives' judgments of control correlated with the invalid heuristics of percentage of successes and confirming cases. Recall that percentage of successes is defined as the percentage of trials on which the green light appears when the subject presses the button, and confirming cases is defined as the sum of the number of trials on which the subject presses the button and the green light comes on plus the number of trials on which the subject does not press the button and the green light

does not come on. Use of either of these heuristics would lead to a lower rating of control in the 25–75 condition (passive) than in the 75–25 condition (active). The effect of outcome valence (lose vs. win) cannot be explained by use of these heuristics, since for half of the students in both the lose and win problems, the active response was more effective, whereas for the other half, the passive response was more effective.

### General Discussion

The results of Experiments 1 through 4 suggest that contrary to previous assertions in the psychological literature (Jenkins & Ward, 1965; Smedslund, 1963), people do have a concept of contingency, that in the response–outcome case entails the relative efficacy of one response versus another in determining outcomes. However, in many instances, people do not use this concept in determining the degree of contingency between their responses and outcomes. Thus, people's subjective representations of contingencies often fail to mirror objective contingencies.

The most convincing demonstration of appropriate use of relative efficacy in determining degree of contingency is provided by the judged control data of Experiment 1. College students were able to quantify accurately the degree of relation between their responses and an environmental outcome across a wide range of the contingency space. The vast majority of past studies concluding that people do not rely on relative efficacy in judging contingency have centered on people's behavior and beliefs only in situations in which responses and outcomes are noncontingently related (Bruner & Revusky, 1961; Langer, 1975; Wortman, 1975; Wright, 1962). Although the noncontingent case is certainly interesting (see below), it is only one type of contingency and therefore is not appropriate as the sole base from which to draw broad inferences about people's general concept of contingency. Also, students' verbalized logic in Experiment 1 suggests that they relied on differential effectiveness of responses in judging the degree of contingency. It is not surpris-

ing that students also ably performed the response that maximized green light onset when green light onset was associated with monetary reward in the behavioral task of Experiment 1.

Although people's subjective representations of contingencies mirrored objective contingencies in Experiment 1, such isomorphism broke down in Experiments 2, 3, and 4. These errors in judgments of contingency were not random. Instead, they were systematically related to both emotional state and characteristics of the outcome. In brief, depressed students accurately judged the degree of contingency regardless of its actual magnitude. Nondepressed students overestimated degree of contingency when noncontingent outcomes were frequent and/or desired and underestimated degree of contingency when contingent outcomes were undesired. Moreover, nondepressed students were relatively accurate in judging the conditional probabilities of outcomes given responses. This suggests that the locus of their judgment errors is in the *organization* of the incoming response–outcome data and not in the perception of the data themselves.

An interesting asymmetry exists in nondepressed students' susceptibility to errors in judging contingency. Nondepressives were more prone to erroneous judgments when responses and outcomes were noncontingently related than when responses and outcomes were contingently related. Although manipulations of reinforcement frequency affected judgments of control adversely in the noncontingent case, such manipulations were not sufficient to produce errors in the contingent case. These results may imply that noncontingency is a psychologically more difficult relationship to perceive or understand than contingency.

Two pieces of evidence from Experiment 1 suggest that students do find lack of contingency more difficult to perceive than presence of contingency. First, students were least able to maximize reinforcements in the behavioral task for the problem with the least degree of contingency. Second, students also felt least certain about the accuracy of their judgments of control in the problem

with the least degree of contingency. Moreover, Piaget and Inhelder (1975) have argued that understanding independent relations comes at a developmentally later stage than understanding dependent relations. These authors contend that the notion of chance is absent in the young child and necessarily depends for its development on intuitions of causality or nonchance. Of course, this discussion need not imply that people will never err in judging degree of control in situations in which a contingency does exist (see Experiment 4), but rather that people are more susceptible to errors in judging noncontingency.

*Parallels in Cognitive Psychology,  
Social Psychology, and Animal Learning*

Similar to the present finding that subjective and objective contingencies are not always isomorphic, Kahneman and Tversky (Kahneman & Tversky, 1972, 1973; Tversky & Kahneman, 1971, 1974) have found that subjective and objective probabilities also do not always coincide. These cognitive psychologists have found that people often fail to use the mathematical rules of chance in making predictions. Instead, they rely on a limited number of heuristics such as representativeness and availability. People's use of representativeness leads them to make predictions based on assessments of similarity between evidence and a predicted outcome. The heuristic of availability leads them to predict on the basis of the ease with which relevant instances of the event to be predicted come to mind. Although use of these heuristics sometimes yields reasonable judgments, it often leads to severe and systematic errors. Thus, use of invalid heuristics is not limited to detection of contingencies between responses and outcomes but extends to other instances of subjective judgment as well.

Review of work in social psychology suggests that in addition to frequency and valence of outcome, several other psychological dimensions are relevant to people's cognitive representations of contingencies. For example, Langer (1975) has shown that when elements characteristic of skill situations (e.g., practice) are introduced into

situations in which outcomes and responses are noncontingently related, people often behave as if they have control. The factors of foreknowledge of the goal and personal involvement in the task also have been shown to induce illusions of control (Wortman, 1975). In addition, if successes occur early in a task as opposed to late in a task or randomly interspersed throughout the task, people often act as if they have more control (Langer & Roth, 1975). Finally, Chapman and Chapman (1967) have shown that the higher the associative connection between two events, the more likely it is that these events will be seen as correlated when in fact they are uncorrelated. In common with the present investigation, these studies found that when faced with objective lack of contingency, people may use a number of invalid heuristics that lead to erroneous judgments of control. An extrapolation from the results of the present study is that depressed individuals would be less likely to succumb to these various types of illusion of control. Indeed, Golin, Terrell, and Johnson (1977) found that depressed subjects did not show an illusion of control (Langer, 1975) in a chance task in which elements characteristic of a skill task had been introduced.

The majority of these social psychology studies also employed a control group that was not subjected to the various manipulations shown to induce the illusion of control. Subjects in these control groups were accurate in assessing noncontingency. Therefore, these studies corroborate the results of Experiment 1 that people do have a concept of control that entails differential effectiveness of responses, but in the context of certain powerful psychological factors (e.g., hedonically relevant outcomes, personal involvement, and practice) people do not use differential effectiveness of responses as the index of contingency and instead resort to more primitive heuristics or strategies. (Abramson & Alloy, in press, recently have suggested explanations about the kinds of psychological factors that induce distortions in subjective judgments of contingency.)

A number of parallels also appear to exist

between the present results and work on animal contingency learning. For example, Rescorla (1968) has reported that the greater the degree of contingency between CS and UCS in a Pavlovian fear conditioning paradigm, the greater the degree of fear shown by rats in a conditioned emotional response (CER) test. These results indicate that animals are behaviorally sensitive to degrees of contingency. The counterpart of this finding in the present investigation is represented by the ability of students in Experiment 1 to quantify accurately the degree of contingency between their responses and outcomes.

Similarly, those variables that determine illusions of control in humans appear to have counterparts producing analogous effects in animals. An animal analogue of the problems in which outcomes were noncontingent in the present investigation is the truly random control (TRC) procedure where CSs and UCS are presented randomly with respect to each other (Rescorla, 1967). To assess the degree of conditioning accruing to the CS, a CER test typically is employed (Annau & Kamin, 1961; Estes & Skinner, 1941). The general finding is that at asymptote, animals exposed to the TRC procedure show no CER suppression, indicating the absence of both excitatory and inhibitory conditioning (Rescorla, 1967, 1972). Preasymptotically, however, animals exposed to the TRC procedure often do show excitatory conditioning (Ayres, Benedict, & Witcher, 1975; Benedict & Ayres, 1972; Keller, Ayres, & Mahoney, 1977; Kremer, 1974; Kremer & Kamin, 1971; Quinsey, 1971; Rescorla, 1968, 1972).

It is interesting that a number of the variables that determine the magnitude and duration of this initial excitatory conditioning in the TRC procedure have parallels in the variables shown to produce overestimations of contingency by nondepressed students in the present study. For example, Rescorla (1972) has shown that the greater the overall probability of the UCS in a procedure where CSs and UCSs are uncorrelated, the greater the magnitude of preasymptotic conditioning. This finding is

analogous to the results of Experiment 2, in which nondepressed students overestimated the degree of contingency between responses and outcomes when the outcome of interest occurred with high probability, but not when it occurred with low probability. Second, Quinsey (1971) demonstrated that the greater the magnitude or intensity of the UCS in a TRC procedure, the greater the initial excitatory conditioning that is obtained. One can view UCS intensity as one of the many variables that would contribute to the valence of the outcome. In Experiment 3, of course, nondepressed students showed illusions of control when good outcomes occurred but not when bad outcomes occurred.

Another variable that has been demonstrated to affect preasymptotic conditioning in the TRC procedure is the number of initial CS-UCS pairings: the greater the number of initial pairings, the greater the excitatory conditioning (Benedict & Ayres, 1972). Analogously, Langer and Roth (1975) found that people are most likely to succumb to an illusion of control in an objectively chance situation when they receive a large number of initial successes. Finally, the effect of valence on subjective judgments of contingency has a parallel in the work on "superstitious" conditioning (Skinner, 1948) in operant situations in which reinforcement is noncontingent. The vast majority of demonstrations of superstitious behavior in animals have been in appetitive rather than aversive paradigms (for a review of the superstition literature, see Herrnstein, 1966, and Staddon & Simelhaag, 1971).

These striking parallels between animals and humans in contingency learning situations suggest that there may be certain fundamental processes underlying contingency learning across species. If there are such basic processes common to animals and humans, then other variables shown to affect the magnitude and duration of preasymptotic conditioning in the TRC procedure in animals may be predicted similarly to affect illusions of control in humans. For example, the finding that the longer an animal is exposed to the TRC procedure, the less

excitatory conditioning it shows (Keller et al., 1977; Rescorla, 1972) suggests that if nondepressed students in the present study were given greater numbers of trials to learn about noncontingency, the magnitude of their overestimations of degree of control might also diminish. Similarly, the other variables shown to affect preasymptotic conditioning in animals such as the salience of the CS (Kremer & Kamin, 1971), proportion of the session occupied by the CS (Kremer & Kamin, 1971), intertrial interval length (Kremer & Kamin, 1971; Quinsey, 1971), and number of unpaired UCSs (Keller et al., 1977; Rescorla, 1968) should also have counterparts in instrumental learning situations that affect the magnitude of the illusion of control.

It is important to remember that depressed students did not show illusions of control when the variables of outcome frequency or valence were manipulated in the present investigation. Accordingly, we would predict that depressed people also would be less affected by the other aforementioned variables and further speculate that "depressed" animals—animals that have been exposed to uncontrollable outcomes (Seligman, 1975b)—should also be less influenced by these variables in the TRC procedure. Alloy and Ehrman (Note 2) have found support for this prediction. Rats that receive uncontrollable shocks show less excitatory conditioning in a TRC procedure in which a tone and shock are noncontingently related than rats that receive controllable or no shocks.

### *Theoretical Alternatives*

We have demonstrated that both depressed and nondepressed people have a concept of contingency that entails relative efficacy of responses, but nondepressives fail to apply this concept under many circumstances. The following section of the article evaluates the learned helplessness theory as an account of the judgment of contingency findings and presents several alternative explanations.

The following two predictions were derived from the helplessness model of depression. First, depressed individuals will

underestimate the degree of control their responses exert over outcomes relative to either the objective degree of control or to nondepressed individuals. Second, nondepressed people will overestimate the degree of response–outcome relation relative to either the actual degree of relation or to depressed people. Clearly, the overall pattern of results in Experiments 1 through 4 does not support these predictions. Although depressed students were surprisingly accurate in judging degree of contingency, nondepressed students showed both illusions of control and illusions of no control depending on the particular experimental situation.

The present investigation's failure to confirm predictions made by the learned helplessness theory of depression is particularly important because this study provides the most direct assessment of people's perceptions of response–outcome contingencies to date. A number of theorists (Buchwald, Coyne, & Cole, 1978; Costello, 1978; Huesmann, 1978), have argued cogently that studies on learned helplessness and depression have failed to provide convincing evidence for the specific "associative deficit" postulated by helplessness theory: Depressives fail to perceive the relation between their responses and outcomes. In the main, previous studies on learned helplessness and depression have demonstrated that similar to nondepressed students made helpless, depressed students show impaired performance on a number of laboratory tasks relative to nondepressed students. Insofar as these studies show that depressed students perform similarly to helpless students, they provide some support for the learned helplessness theory of depression. The major problem with these studies, however, is that they fail to isolate the cognitive component from the motivational component of helplessness (Costello, 1978). The results of the present investigation provide no evidence for the hypothesized associative deficit in depressives; if anything, they suggest that *nondepressives* have difficulties in assessing response–outcome relationships. (See Alloy & Seligman, in press, for a comprehensive discussion of the associative

deficit in learned helplessness and depression.)

How can the present study's failure to obtain evidence for the associative deficit in depression be reconciled with the often replicated finding (e.g., Klein & Seligman, 1976; Miller & Seligman, 1975) that depressives do poorly on instrumental learning tasks? The learned helplessness hypothesis, of course, postulates that depressives' poor performance on instrumental tasks is, in part, mediated by the associative deficit. One way to explain these two seemingly disparate findings is to propose a revised helplessness theory of depression. The revised hypothesis maintains that depressives are characterized by a generalized expectation of no control, but postulates that this expectation only interferes with initiation of responses (the "motivational deficit" of helplessness) and not with the perception of response-outcome relationships (the "associative deficit" of helplessness).

In other words, the revised helplessness hypothesis suggests that depressed individuals often perform poorly on instrumental tasks because they fail to generate the response that increases the probability of the successful outcome, not because they have trouble discerning the effect their responses exert on these outcomes. The observation in Experiment 2 that depressed students were less likely to generate complex hypotheses than were nondepressed students provides some support for this revised helplessness model. Additional support for this hypothesis comes from projective testing in the clinic. Depressives generate fewer and less complex responses than nondepressives on the Rorschach test (Beck, 1945). Furthermore, experiments on animal helplessness have shown that rats exposed to uncontrollable shocks only show deficits on tests for helplessness if the response necessary to control reinforcement in the test task is a difficult one (Maier et al., 1973; Seligman & Beagley, 1975).

This revised hypothesis makes a testable prediction: Depressives should underestimate the degree of control a response exerts over an outcome in situations in which the

controlling response is complex (because depressives will not generate the appropriate response), but not in equally complex situations in which the controlling responses are generated for them. Abramson, Alloy, and Rosoff (Note 3) recently have confirmed this prediction. A major disadvantage of such a revised hypothesis is that it fails to explain the robust inaccuracies of nondepressives in judging contingencies.<sup>7</sup>

An alternative, though not necessarily incompatible, framework for examining the judgment of contingency results is a motivational account derived from social psychology. The finding that nondepressed students perceive a contingency between their responses and desirable outcomes but not between their responses and undesirable outcomes is reminiscent of a general phenomenon in social psychology (Cohen, 1964; Feather, 1969; Fitch, 1970; Johnson, Feigenbaum, & Weiby, 1964; Langer & Roth, 1975; Medow & Zander, 1965; Streufert & Streufert, 1969; Weiner et al., 1971; Wortman, Costanzo, & Witt, 1973). These investigators have examined the causal attributions of individuals who received false feedback that they had either succeeded or failed at a task. Subjects in these experiments generally have attributed causality to themselves when they succeeded and to factors in the environment or situation when they failed.

In explaining this general finding, some theorists (e.g., Fitch, 1970; Wortman et al., 1973) have relied on the notion that people are motivated to maintain or enhance self-esteem. The logic here is that taking credit for good outcomes maintains or enhances self-esteem and viewing bad outcomes as caused by factors outside the self is not damaging to self-esteem (see Bradley, 1978; Miller & Ross, 1975; and Miller, 1978, for a review of evidence bearing on self-serving attributions). Similar reasoning may be applicable to the judgment of contingency

<sup>7</sup> A further indication of the robustness of the nondepressive "illusion of control" is the performance of R.L.S., a learning theorist who was a pilot subject and showed a larger illusion than any other subject in the 75-75 problem.

results for nondepressed students in Experiments 2, 3, and 4. That is, nondepressed students may have shown illusions of control when good outcomes occurred in order to preserve or increase self-esteem but may have shown illusions of no control when bad outcomes occurred in order to prevent loss of self-esteem. It should be noted that lack of control can result either from internal factors such as personal incompetence or external factors such as the harshness of the environment (see Abramson et al., 1978, for a comprehensive discussion of the relation between perceived control and attributions). For such a motivational account to go through, nondepressives who believe they have no control over bad outcomes must attribute this lack of control to external rather than to internal factors.

If one assumes that depressed individuals are not motivated to maintain or enhance self-esteem, then their accuracy in detecting contingencies regardless of outcome valence follows. Indeed, the depressive often is characterized by low self-esteem (Beck, 1967, 1976; Bibring, 1953; Freud, 1917/1957), and moreover, Bibring (1953) has argued that depressives are *not* motivated to regain self-esteem precisely because the mechanism for self-deception in these individuals has broken down. Freud (1917/1957) appears to espouse a similar view:

When in his [the depressive's] heightened self-criticism he describes himself as petty, egoistic, dishonest, lacking in independence, one whose sole aim has been to hide the weakness of his own nature, it may be, so far as we know, that he has come pretty near to understanding himself; we only wonder why a man has to be ill before he can be accessible to a truth of this kind. (p. 246)

Thus, the present findings are compatible with a view that postulates differential motivation for maintaining self-esteem in depressed and nondepressed individuals. A deduction from this motivational hypothesis is that the attributional style observed in the social psychological experiments would be characteristic of nondepressed, but not depressed, people. In line with this prediction, a number of investigators have found that nondepressives show the self-enhancing attributional style, but depressives do not

(Klein, Fencil-Morse, & Seligman, 1976; Kuiper, 1978; Rizley, 1978; Seligman, Abramson, Semmel, & von Baeyer, 1979). Further work is needed to directly test the adequacy of a motivational account of differences between depressed and nondepressed people in judging contingencies.

### *General Implications*

A consistent and intriguing theme in our results is the tendency of nondepressed students to succumb to various "cognitive illusions." While cognitive accounts of depression emphasize depressives' errors in interpreting their environment and behavior (e.g., Beck, 1967, 1976), our findings demonstrate the existence of cognitive errors made by nondepressed people.

Interestingly, a number of recent studies document nondepressive illusions in areas other than the judgment of contingency. For example, in a study of the selective recall of positive and negative feedback, Nelson and Craighead (1977) found that depressed college students accurately recalled the frequency of negative feedback on a laboratory task, whereas nondepressed students underestimated the frequency of negative feedback (see also DeMonbreun & Craighead, 1977). Similarly, Rozensky, Rehm, Pry, and Roth (1977) reported that nondepressed control patients rewarded themselves to a greater degree than their objective performances would warrant. Although depressed patients also tended to over-reward themselves, they were more accurate in self-reward than the nondepressives. Finally, Lewinsohn, Mischel, Chaplin, and Barton (in press) found that depressed patients accurately assessed their social competence whereas nondepressed psychiatric and normal control subjects perceived themselves more positively than other people saw them. Interestingly, even after they had received therapy, the depressed patients were more realistic in assessing their competencies than were the control subjects.

Taken together, these studies suggest that at times depressed people are "sadder but wiser" than nondepressed people. Nondepressed people succumb to cognitive illu-

sions that enable them to see both themselves and their environment with a rosy glow. A crucial question is whether depression itself leads people to be "realistic" or whether realistic people are more vulnerable to depression than other people.

An obvious question emerges concerning the generality of the present results to clinical populations of depressives. Further studies are necessary to determine which subtypes of depressed inpatients (unipolar-bipolar, endogenous-reactive), if any, show similar accuracy in judging relations between their responses and outcomes relative to individuals with no psychopathology or other forms of psychopathology. A second clinical question concerns the relation between the illusion of control and mania. Perhaps individuals in manic states would show greater overestimations of noncontingent response-outcome relations than nondepressed individuals (see Langer, 1975, for a similar discussion).

It is worth pointing out that the illusion of control shown by nondepressed individuals when good outcomes occur noncontingently has important implications for the experimental paradigm of "appetitive helplessness" (Maier & Seligman, 1976). Appetitive helplessness refers to a procedure in which an organism is exposed to uncontrollable positive events and then is tested for deficits in instrumental responding. This paradigm is of particular theoretical interest for the learned helplessness model because the bulk of experimental evidence testing the theory comes from uncontrollable aversive paradigms whereas the theory states that belief in uncontrollability in either appetitive or aversive situations will lead to helplessness. The results of the present investigation suggest that empirically, appetitive helplessness may be difficult to obtain, since nondepressed subjects in these paradigms may believe erroneously that they have control when in fact they do not. Therefore, to insure a fair test of helplessness theory, investigators of appetitive helplessness need to determine whether their subjects are, in fact, concluding that they have no control over objectively uncontrollable positive outcomes.

In the experiments in this article, students judged the degree of contingency between responses and outcomes. Previous studies of behavior in stimulus prediction situations have demonstrated that people often believe that noncontingently related stimulus events also are correlated (e.g., Chapman & Chapman, 1967; Hake & Hyman, 1953; Starr & Katkin, 1969). Future research is necessary to determine whether depressed individuals' relative accuracy in judging the degree of control that their responses exert over outcomes is accompanied by similar accuracy in judging degree of contingency between two stimulus events in a prediction situation.

The starting point of the present investigation was the concept of contingency developed within the learning tradition; an appropriate ending point is the implications of this investigation for learning theory. Many contemporary learning theorists have proposed views of learning that require an organism capable of cognitively representing relations between stimuli and/or responses and outcomes (Bandura, 1977; Bolles, 1972; Estes, 1972; Mackintosh, 1973; Maier & Seligman, 1976). In these views, changes in behavior are mediated by these cognitive representations. The fact that humans' subjective representations of contingencies do not always mirror objective contingencies suggests that such learning theories are incomplete until they can specify the rules for relating the two.

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