JUDGMENT OF CONTINGENCY AND THE COGNITIVE FUNCTIONING OF CLINICAL DEPRESSIVES

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By

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Twenty-four psychiatric staff, 24 clinically depressed inpatients, and 24 nondepresssed schizophrenic inpatients at a state psychiatric facility completed five tasks under either reward or punishment conditions. Each task consisted of 30 trials of pressing or not pressing a button to make a light appear. Monetary reinforcement was contingent on light onset for the final ten trials of each task. Cash incentives for judgment of control accuracy were added for Tasks 3, 4, and 5. Cognitive functioning was evaluated on each task by measuring expectancy, judgment of control, evaluation of performance, and attribution. Mood and self-esteem were measured before and after the procedure.

No significant differences were observed across mood groups for expectancy of control or judgment of control accuracy. Subject groups also did not differ in the attributions they made or in how successful they judged their performances to be. They set realistic, attainable criteria for success which were consistent with relevant conditional probabilities.

Subjects in reward gave themselves more credit for task performance than subjects in punishment gave themselves blame

for comparable performances. Punishment subjects demonstrated more stable, external attributions than those in reward.

Across tasks, subjects overestimated when actual control was low and underestimated when actual control was high. Contrary to the "depressive realism" effect described by Alloy and Abramson (1979), clinical depressives did not display more accurate judgments of control than did nondepressives. All subjects appeared to base their control estimates on reinforcement frequency rather than actual control. Subjects showed a type of illusion of control for high frequency, low control tasks. Presumably, success in turning the light on led them to assume that their actions controlled light onset. Comparison to previous subclinical studies suggests a possible curvilinear relationship between judgment of control accuracy and level of psychopathology, with mild depressives displaying relatively greater accuracy than either nondepressives or clinical depressives.

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INTRODUCTION

Whether measured in terms of health care costs, reduced work force, or the effect on individuals' personal lives, depression poses a major threat to health. Recent surveys (American Psychiatric Association, 1980; National Institute of Mental Health, 1985; President's Commission on Mental Health, 1978) estimate that from eight to twenty million people in the United States suffer from affective disorders at any given time. Moreover, 25% of the individuals in the United States will at some time in their lives suffer from a depressive disorder, with nearly one-third of those cases requiring inpatient hospitalized care. In terms of hospital costs alone, the impact of depression is staggering, with over $10 billion spent annually on treatment and inpatient care (National Institute of Mental Health, 1985).

In addition to its debilitating effects on daily functioning, depression has been repeatedly linked to suicide. It is estimated that 80% of actively suicidal patients are clinically depressed. Indeed, depressed patients are approximately twenty-five times more likely to commit suicide than those in the normal population (Flood &

Seager, 1968; Robins & Guze, 1972; Rosenhan & Seligman, 1984). It is clear that studying the causes, characterstics, course, and treatment of this disorder is and should be a vital national concern.

Unfortunately, decades of research have failed to isolate a single central symptom of depression. Instead, the clinical picture identifies a varying constellation of features (Lewinsohn, 1975; Pehm, 1976). The psychological and medical literatures are filled with differing diagnostic systems for the major depressive disorders, resulting in a lack of uniformity as to diagnosis and treatment intervention. The most universally accepted classification system is the American Psychiatric Association's (1986) Diagnostic and Statistical Manual of Mental Disorders, revised Third Edition (DSM III-R). For a diagnosis of depressive disorder, the DSM-III requires at least four of the following specific symptoms to persist for a minimum of two weeks: significant weight or appetite changes, sleep disturbance (insomnia or hypersomnia), psychomotor agitation or retardation, loss of interest or pleasure in usual activities, energy loss or fatigue, feelings of worthlessness or self-reproach, diminished abilities to concentrate, and recurrent thoughts of death or suicide.

Subtypes of depression have frequently been distinguished diagnostically according to the presence or absence of environmental precipitators (Paykel, 1979a). This distinction of endogenous vs. reactive depression has been extensively investigated and supported, both through diagnostic techniques (Nelson & Charney, 1981; Spitzer, Endicott, & Robins, 1978; Willner, 1984) and physiological criteria (Carroll, 1982; Kupfer, Foster, Coble, McPartland, Ulrich, & Hyg, 1978). This same body of research has attempted to identify the differing etiologies and characteristics of psychotic vs. neurotic depression. However, despite possible differences in physiology, onset, course, and treatment, patterns of cognitive functioning appear remarkably similar across endogenous vs. reactive and psychotic vs. neurotic depressive subtypes (Nelson & Charney, 1981; Willner, 1984).

The majority of theories regarding the etiology of depression may be broadly classified into biological and psychosocial approaches. Within the biological approaches, a genetic predisposition to depression has garnered much research support. Results from twin (Allen, 1976; Perris, 1979) and adoption studies (Cadoret, 1978; Mendlewicz & Rainer, 1977) have provided the most convincing evidence. Another approach has identified the role of neurotransmitters as a possible causal agent in depression onset (Akiskal, 1979; Carson & Carson, 1984; Depue & Monroe, 1978). The successful clinical use of tricyclic antidepressants, monoamine oxidose inhibitors, and electroconvulsive therapy among depressed patients to alter neurotransmitter levels in the brain has lent substantial support to this position.

Psychosocial approaches to the etiology of depression have been extensively researched. Depression has been linked to: developmental issues (Abraham, 1957; Akiskal, 1979;Freud, 1917; Jacobson, 1971; Klein, 1935); precipitating social stressors (Brown, 1972; Brown, Harris & Copeland, 1977; Brown, Sklair, Harris & Birkley, 1973; Paykel, 1973; Paykel, Myers, Dienelt, Lindenthal, & Pepper, 1969); social skills deficits (Coyne, 1976; Hersen, Eister, Alford, & Agras, 1973; Lewinsohn & Graf, 1973; McLean, 1976; Youngren & Lewinsohn, 1978); reinforcement anomalies (Coyne, 1976; Lewinsohn, 1974; Lewinsohn, Biglan, & Zeiss, 1976; Lewinsohn, Weinstein, & Shaw, 1969); aversive control (Ferster, 1973, 1974; Lazarus, 1968; Suarez, Crowne, & Adams, 1978); loss of reinforcer effectiveness (Carson & Adams, 1980; Costello, 1972a, 1972b; Strickland, Hale, & Anderson, 1975); maladaptive cognitions (Beck, 1967, 1974, 1976, 1979; Ellis, 1962; Kovacs & Beck, 1978; Valins & Nisbett, 1971); and learned helplessness (Abramson, Seligman, & Teasdale, 1978; Seligman, 1975).

Among the psychosocial approaches, cognitive models of depression have received increased attention in recent years. A vast and growing literature presents a compelling case for the cognitive viewpoint (Coyne & Gotlib, 1983), identifying depression-related cognitions as causal factors in depressive symptoms (Shaw & Dobson, 1981). Cognitive therapies for depression have been shown to produce greater improvement, more complete remissions, and fewer dropouts than either drug treatment or behavior therapy alone (Beck, 1976; Coyne & Gotlib, 1983; Evans, Hollon, DeRubeis, Piasecki, Grove, Garvey, & Tuason, 1988; Hollon & Garber, 1988; Rush, Beck, Kovacs, & Holton, 1977). These studies suggest that nonendogenous subtypes of depression are more responsive to cognitive interventions, since they are thought to arise from cognitive distortion. A plethora of recent studies has investigated these models, with particular attention to those of Seligman and Beck.

Cognitive Models of Depression

Seligman's original learned helplessness model.

Seligman's original learned helplessness model (Seligman, 1975) derived from infrahuman laboratory studies which examined the debilitating effects of uncontrollable aversive events (Maier, Albin, & Testa, 1973; Masserman, 1971; Overmier & Seligman, 1967; Thomas & Dewald, 1977). Studies with humans (e.g., Hiroto, 1974, Hiroto & Seligman, 1975) gave strikingly similar results, which were used to explain debilitated human task performance and then offered as an explanatory model of depression. According to this initial formulation, when exposed to uncontrollable aversive stimuli, individuals experience three deficits: motivational, involving the retarded initiation of voluntary responses as a consequence of the expectation that outcomes are uncontrollable;

cognitive, since learning that an outcome is uncontrollable makes it difficult to later learn that certain responses can produce that outcome; and emotional, since depressed affect results from learning that outcomes are uncontrollable (Seligman, 1975).

A reformulation of the model (Abramson, Seligman, & Teasdale, 1978) included a stronger focus on attributional processes and the distinction between universal and personal helplessness. Universal helplessness occurs when an individual believes that an event is independent of both his own and others' responses. Conversely, personal helplessness occurs when an individual believes that only his own responses are unrelated to outcomes. Seligman and his colleagues argued that when a person finds that he is helpless, he asks *why*. The causal attributions one makes then determine the nature of one's deficits, as well as affecting future self-esteem (Abramson et al., 1978). Following an experience of helplessness, that uncontrollability is attributed to some cause which is either stable or unstable, global or specific, and internal or external. Those experiencing personal helplessness attribute the cause of negative outcomes to stable, global, and internal factors, while those experiencing universal helplessness attribute negative outcomes to specific, external causes. Therefore, while both personal and universal helplessness may result in depressed affect (based on the perception that actions are noncontingently related to desired outcomes), personal helplessness specifies that an individual will develop subsequent expectations for future noncontingency, since that noncontingency is perceived to generalize to a broad variety of future events. In other words, individuals displaying personal helplessness perceive that they "cannot solve solvable problems" (Abramson et al., 1978).

Abramson et al. (1978) described the theoretical sequence of events leading to depression as: objective noncontingency --> perception of present and past noncontingency --> attribution for present or past noncontingency --> expectation of future noncontingency --> symptoms of helplessness. The revised learned helplessness model thus views depressive symptoms as resulting directly from expectations of future negative outcomes which the individual is unable to control (Abramson et al., 1978).

More recently, the reformulated helplessness theory has been revised and renamed hopelessness theory (Abramson, Alloy, & Metalsky, 1988; Abramson, Metalsky, & Alloy, 1989; Alloy, Abramson, Metalsky, & Hartlage, 1988). In this latest revision, hopelessness, rather than helplessness, is now identified as the central explanatory metaphor in this subtype of depression. Individuals experiencing hopelessness depression perceive either that highly desired outcomes are unlikely to occur or that highly aversive outcomes are probable. Further, they feel unable to offer behaviors to alter the probability of those outcomes. While similar to the reformulated learned helplessness model in describing the depressed individual's self-perceived inability to effectively control environmental events, hopelessness theory differs by deemphasizing response-outcome noncontingency, instead focusing on the aversiveness of an event as producing the depressed individual's hopeless outlook.

Hopelessness theory also introduced the logical concepts of necessary, sufficient, and contributory causes of depressive symptoms. Necessary causes are defined as etiological factors which are required for depressive symptoms to be demonstrated. Sufficient causes are etiological factors that, if present, ensure the manifestation of depressive symptoms. Contributory causes are neither necessary nor sufficient, but their presence increases the probability that depressive symptoms will occur. Alloy, Abramson, and their colleagues also distinguished between distal and proximal causes in describing the etiological sequence of events leading to the manifestation of depressive symptoms. Distal causes are defined as those which occur early in the etiological sequence, even when there is little or no evidence of overt depressive symptomotology. Conversely, proximal causes occur late in the causal pathway, immediately prior to or concurrent with the onset of depression (Abramson et al., 1988; Abramson et al., 1989; Alloy et al., 1988).

By introducing these concepts, these authors offered a more comprehensive, systematic explanation of how personal and situational variables lead to the development of depressive symptoms. A significant alteration in their theory involves the admission that hopelessness depression (previously learned helplessness) is only one subtype in the broad, heterogeneous class of depressive disorders. In this subtype, the expectation of hopelessness is viewed as a proximal sufficient cause of depression. This theory represents an improvement in that it allows for more complex interactions of causes in the development of depressive symptoms. For example, the attribution of negative life events to stable, global causes, as well as the attachment of high importance to these events, are seen as proximal contributory causes of depression that increase the likelihood of developing hopelessness and thus depressive symptoms. Stated differently, Abramson et al. (1989) assert that certain individuals possess a depressive attributional style, attributing negative events to stable, global factors. When they are confronted with specific negative events, individuals who display a depressogenic attributional style should therefore be more likely to display depressive symptoms than individuals without this style. Conversely, in the presence of positive life events (or the absence of negative life events), people displaying a depressogenic attributional style should be no more likely to develop hopelessness (and therefore depression) than those not exhibiting this attributional style (Abramson et al., 1989).

By defining hopelessness depression as only one subtype of depression, these authors explicitly state that depression is a hetereogeneous disorder. This revision in their theory allows for the possibility that other factors, such as genetic vulnerability, norepinephrine depletion, or loss of interest in reinforcers, may be adequate to produce depressive symptoms (Abramson et al., 1988).

It is also important to note that the reformulated learned helplessness theory (Abramson et al., 1978) viewed the perception of an uncontrollable event as beginning the causal chain of depression. The logic of the newer hopelessness theory requires only the occurrence of a (perceived) negative event, rather than the occurrence of an uncontrollable event, to begin the series of cognitive distortions culminating in depression.

Although the newer theory deemphasizes helplessness in favor of hopelessness, hopelessness cannot be adequately conceptualized without fully acknowledging an individual's perception of uncontrollability over an event. Alloy and Abramson have moved away from a more direct focus on helplessness and noncontingency, eliminating the notion of the depressive's cognitive deficit. The cognitive deficit refers to the depressed individual's tendency to underestimate response-outcome contingency due to past experiences of (perceived) noncontingency. However, this perception of noncontingency remains as a logically implied variable which is central to the etiological chain of hopelessness depression. An individual cannot develop a hopeless outlook without first perceiving a lack of personal control over environmental events. In other words, although Alloy and Abramson have removed helplessness from its position as the theory's central metaphor, it remains as an unavoidable factor leading to the development of hopelessness. Further, the extreme narrowing of the newer theory appears to represent an admission that helplessness theory has failed to withstand empirical testing. By claiming now to describe only one, perhaps small, subset of depression, this theory has given up much of its clinical utility and explanatory power.

Beck's Cognitive model. Beck's (1967, 1974, 1976, 1979, 1987) theory states that the root of depression lies in the individual's negative cognitive set. His research links vulnerability to depression with aberrant cognitions which distort the depressed individual's perception of environmental experiences. The depressed affective state is secondary to these negative cognitions. Although depressive episodes may be precipitated by external events, Beck claims that it is the individual's perception and appraisal of those events which make them depression-inducing. Three specific areas of distorted cognition are identified: the cognitive triad, schemata, and faulty information processing.

<u>Cognitive triad</u> : The cognitive triad includes negativistic views of self, current experiences, and the future. Beck suggests that this triad manifests itself in the misperceptions and misinterpretations voiced by depressed people and lies relatively dormant until triggered by environmental stressors. Three areas of cognition in particular are distorted. First, depressives view themselves as inadequate, undesirable, deficient, defective, and deprived. Second, they inaccurately attribute negative or unpleasant experiences to personal inadequacies, assuming that their personal faults have caused their misfortunes. They interpret current, ongoing experiences as failures, even though more reasonable, positive explanations may be available. Thus, small obstacles are seen as impassable barriers. Third, the future is characterized by hopelessness, since ongoing negative events are seen as indefinite and unremitting.

<u>Schemas</u>: Schemas are cognitive structures which include a catalogue of past experiences for screening, differentiating, evaluating, and coding environmental information (Tang, 1987; Taylor & Crockett, 1981). They serve as a means for organizing and evaluating groups of similar past experiences. This concept can be used to

explain why a particular individual may react differently to objectively similar events, or may show the same type of response in apparently dissimilar situations (Kovacs & Beck, 1978). Although they may remain inactive for long periods of time, schemas can be instantly reactivated by certain environmental triggers. Schemas also provide hypotheses about incoming information, including plans for gathering and interpreting schema-related information. They may even provide a basis for activating behavior sequences. Although schemas may be modified by new or disconfirmatory experiences, they comprise a fairly stable knowledge structure (Taylor & Crockett, 1981). While schemas are efficient and automatic, errors do occur in information processing. Errors typically reflect distortions or misinterpretations of irrelevant or neutral information, or filling in missing information which is consistent with that schema. Errors therefore represent schema-driven attempts to make inconsistent information or events consistent with that schema, i.e., information is inappropriately interpreted to fit with previous experiences.

Beck employed the concept of schemata to explain why depressives continue to adopt self-defeating and painful attitudes despite contradictory evidence. Previous experiences of failure may predispose the depressive to become preoccupied with repetitious thoughts of failure. These thoughts are then easily triggered in future situations. As depression deepens, the logical connection between the actual stimuli and the negative thoughts diminishes, so that even seemingly irrelevant stimuli can cue depressive reactions.

Faulty information processing: Beck (1967, 1969) claimed that depressives often appraise environmental information in black and white terms. Events are evaluated in extreme, categorical, absolute, negative, and judgmental fashion.

These faulty styles of processing are grouped into paralogical, stylistic, and semantic errors. Three main types of paralogical errors are described. Arbitrary inference refers to drawing specific conclusions in the absence of confirming evidence or in the presence of opposing evidence. Selective abstraction involves focusing on details while ignoring other more salient information. Overgeneralization describes drawing a conclusion on the basis of isolated, inconsistent examples and applying that conclusion to unrelated situations. Stylistic errors include disproportionate magnification or minimization in appraising an event's significance, so that a minor error may be seen as catastrophic. Semantic errors are of two types. First, depressives tend to personalize external events, i.e., relate those events causally to themselves when no such relationship exists. Second, dichotomous thinking describes the depressive's tendency to place all experiences into two extreme categories, such as absolute success or absolute failure, and then assume that the extreme negative view must be true (Beck, 1967, 1969, 1974, 1979).

The proposed sequence of events leading to depression in Beck's original model is as follows: environmental stress activates the cognitive triad --> illogical matching of negative schemas and environmental stimuli --> negative distortions and systematic errors --> symptoms of depression (Carson & Carson, 1984). Beck assigns central significance to cognitive factors in the depressed individual's organization of incoming stimuli and stresses how this cognitive organization then affects emotion, motivation, and behavior (Coyne & Gotlib, 1983; Taylor & Fiske, 1983).

Alloy and her colleagues have slightly reorganized Beck's theory in much the same way that they revised the reformulated learned helplessness theory (now the hopelessness theory). These researchers assert that although Beck's model is much less explicit than the hopelessness theory, it contains similar structural components, incorporating the notions of necessary, sufficient, contributory, distal, and proximal causes of depression. Alloy et al. (1988) described Beck's negative cognitive triad as a proximal sufficient cause of depression. The development of this negative cognitive triad is made more likely by the occurrence of cognitive distortions/errors, which are viewed as proximal contributory causes of depression. These were previously described as the unrealistic or distorted perceptions made by depressed individuals. These distortions are in turn produced by negative, maladaptive cognitive schemata (contributory causes of depression), which are located more distally on the etiological chain of depression.

New research investigating the cognitive processing of depressives versus nondepressives has called into question Beck's notion that the schematic processing of depressives and nondepressives is qualitatively different (Dykman, Abramson, Alloy, and Hartlage, 1989). The findings of Dykman et al. (1989) strongly suggest that schematic processing operates similarly in depressed and nondepressed individuals, since both groups bias their encoding of ambiguous information in the direction of their respective schemas. Thus, while the processing of information is similar, the content likely differs. Additionally, Hollon and Garber (1988) found no significant differences in depressives' and nondepressives' information processing strategies. Cognitive differences were instead found to result from the content of their thoughts. In other words, the schematic processing of depressed individuals is not uniquely different in style or process from nondepressives. Instead, depressives appear to think by using the same inferential rules as nondepressives. Only the content or valence of their thoughts differs (Hollon and Garber, 1988). This distinction suggests that there is nothing uniquely different about the manner in which depressives think. Depressives' cognitions appear subject to the same inferential rules and biases that characterize human cognition in general (Dykman et al., 1989).

<u>Comparison of the hopelessness model and Beck's model of depression</u>. At first glance, these theories appear to represent radically different conceptions of depressive psychopathology. Beck's (1967, 1974, 1976, 1979) model states that depressives assume excessive responsibility for negative outcomes (self-blame), whereas the hopelessness model (Abramson et al., 1988; Abramson et al., 1989; Alloy et al., 1988) asserts that depressed individuals are hopeless due to self-perceived inability to control or avoid significant negative outcomes (Abramson & Sackeim, 1977; Blaney, 1977; Rizley, 1978). (Note: hopelessness theory and the reformulated learned helplessness theory will be combined as hopelessness theory for purposes of this discussion, unless their differing contents require that they be discussed separately.)

Several explanations have been offered to explain the apparent discrepency between these models. First, earlier researchers have reasoned that Beck's model and the learned helplessness model are complementary (Coyne & Gotlib, 1983; Fiske & Linville, 1980). Beck explains how depressed individuals arrange and organize incoming external stimuli, while the reformulated learned helplessness model focuses on how they attribute meaning to that information. In their review of cognitive theories of depression, Coyne and Gotlib (1983) suggested that although these two models apparently offer contrasting formulations, their differences mainly involve a matter of emphasis. Fiske and Linville (1980) noted that attributional and schematic formulations prove to be complementary in many instances. Beck's schematic analysis focuses particular attention on the organization of prior knowledge and how this organization determines the processing of current incoming stimuli (Coyne & Gotlib, 1983). Conversely, attributional models center on how incoming information is explained and how that interpretation influences subsequent cognitions, affect, and behavior.

Second, both theories claim that depressives are harsh and negativistic in evaluating their own behavior. While Beck more explicitly articulates self-blame in his theory, both reformulated learned helplessness and hopelessness theories likewise describe the negative outlook which arises from depressives' self-perceived inability to avoid aversive events.

Third, both Beck's theory and the reformulated learned helplessness model predict that depressed individuals underestimate the contingency between their responses and environmental outcomes. Beck's theory states that depressives view themselves as personally at fault for their failure to emit success-producing behaviors. Reformulated learned helplessness theory likewise predicts that depressives see themselves as unable to produce positive outcomes or avoid negative ones. Both theories therefore share the claim that depressives view themselves as unable to control significant outcomes.

In contrast, the newer hopelessness theory differs from its predecessor by dismissing the previously postulated cognitive deficit of the reformulated learned helplessness theory. While learned helplessness theory distinguished between personal and universal helplessness, hopelessness theory has deemphasized this distinction and the accompanying predictions regarding differences between efficacy and outcome expectancies in nondepressed individuals and those displaying personal helplessness. Reformulated learned helplessness theory claimed that individuals displaying personal helplessness mistakenly believe that their behaviors are noncontingently related to outcomes, based on previous perceptions of noncontingency (i.e., the cognitive deficit). Depressives believe that solutions exist, but are unavailable to them (high outcome expectancy, low efficacy expectancy). However, Abramson et al. (1989) cite the recent findings of "depressive realism," i.e., the relative accuracy of mildly depressed subjects' judgments of control as compared to nondepressed subjects, as leading to their decision not to include the cognitive deficit as part of hopelessness theory. The depressive realism research has not supported learned helplessness theory's claim that depressives underestimate control relative to objective contingencies.

Abramson et al.'s failure to include the cognitive deficit in hopelessness depression diminishes the logical consistency of the theory. The cognitive deficit of learned helplessness theory served a key role in explaining how depressives' self-perceived failure to control events around them is perpetuated across time. Since depressives view themselves as ineffective and helpless, they continue to assume that they have less control than is actually available to them. By dismissing this premise, hopelessness theory fails to adequately reconcile depressives' judgment of control accuracy with their negative outcome expectancies and their expectations of helplessness. Depressives' view of themselves as helpless and ineffective suggests that they should underestimate contingency relative to objective criteria, rather than display consistent accuracy, due to their perception that they cannot prevent aversive events. By removing these explicit claims, hoplessness theory loses its main explanatory mechanism for how negative outcome expectancies arise, i.e., how hopelessness is produced.

Hopelessness theory has not yet specified how an individual develops a negative outcome expectancy, nor has it fully articulated how the depressed individual's expectations of helplessness arise. It also remains unclear how the negative outcome expectancy described in hopelessness theory differs from reformulated learned helplessness theory's claim of high outcome expectancy and low efficacy expectancy among individuals displaying personal helplessness. The theory's failure to adequately develop these concepts leaves it incomplete and logically vulnerable. In fact, despite Abramson et al.'s (1989) dismissal of this deficit, Vazquez (1987) recently stated that the reformulated learned helplessness claim of a genuine cognitive deficit among depressives may still be valid. In short, hopelessness theory remains in a state of flux. It is clear that future revisions need to reconcile the findings of "depressive realism" with the logic of hopelessness theory.

Fourth, Abramson, Alloy, and their colleagues (Abramson et al., 1988; Abramson et al., 1989; Alloy et al., 1988) have recently integrated Beck's original model and the hopelessness model in a way that makes them more conceptually similar. These authors now define a specific subtype of depression, negative cognition depression, which both models describe. These researchers argue that the two major cognitive theories are best conceptualized as diathesis-stress models. That is, when confronted with equivalent stressors, people who are predisposed to depression should be more likely to experience a depressive reaction than those who do not display this predisposition. Conversely, in situations where stress is nonexistent (or in the presence of positive life events), individuals with this depressive predisposition are no more likely to develop depressive symptoms than those not possessing this risk factor (Abramson et al., 1988; Dykman et al., 1989). In other words, it is the interaction of predisposition and environmental stress that precipitates depressive symptoms.

Cognitive Functioning in Depressives

Since the mid 1970's, Beck's theory and the reformulated learned helplessness theory have served as springboards for a number of studies on the cognitive functioning of depressives. To evaluate the efficacy of these two models in predicting and characterizing depressives' cognitions, research investigating four broad areas is examined, as in Coyne and Gotlib (1983): expectancy of control, self-evaluation of performance, attributional processes, and judgment of contingency.

1. Expectancy of control. Expectancies may be understood as cognitive representations of response-outcome contingencies (Bolles, 1972). A commonly used experimental paradigm for investigating expectancies has involved evaluating expectancy over tasks where outcomes are ostensibly determined by skill or by chance. According to Rotter's (1966) social learning theory, expectancies derive from beliefs regarding the extent of personal control one exerts over response-outcome contingencies (Ruehlman, West, & Pasahow, 1985). Theoretically, expectancy shifts which follow success or failure on an ostensible skill task should

be greater than those following success or failure on a chance task. That is, performance is more informative in a "skill" than a chance condition, since skill-based outcomes are perceived as resulting from personal responses, while chance-based outcomes are seen as reflecting factors beyond personal control (Phares, 1973).

Seligman's (1975) original learned helplessness theory holds that depressives believe outcomes to be beyond their personal control, responding to "skill" tasks as if outcomes were governed by chance. Research by Seligman and his colleagues found that depressives' expectancies increased less after success and decreased less after failure than those of nondepressives in "skill" conditions, but not in chance conditions (Abramson, Garber, Edwards, & Seligman, 1978; Klein & Seligman, 1976; Miller & Seligman, 1973, 1976; Peterson & Seligman, 1984).

Coyne and Gotlib (1983) argued that while differences may exist between the expectancy shifts of depressed and nondepressed people, these differences are not as strong or consistent as originally hypothesized. In fact, the reformulated learned helplessness camp has conceded that "in the absence of knowledge about individual attributions, the reformulated helplessness hypothesis cannot make clear-cut predictions about expectancy changes and helplessness" (Abramson et al., 1978).

Aside from consistency across time and situations, expectancy can be evaluated in terms of accuracy, i.e., the degree of match between expectancies and actual outcomes. Both Beck's model (1974) and the reformulated learned helplessness model (Abramson et al., 1978) have predicted that depressives expect more failure and less success than is realistic, while nondepressives are accurate in their expectancies. Research results supporting these views are mixed.

Several studies compared expectancies of depressives and nondepressives in a dice game using college student (Golin, Terrell, & Johnson, 1977) and psychiatric samples (Golin, Terrell, Weitz & Drost, 1979; Lobitz & Post, 1979) in which either subjects rolled the dice themselves or the dice were thrown by the experimenter. Their findings indicated that depressives' expectancies of success accurately reflected the objective probability of success when they rolled the dice themselves, whereas nondepressives' expectancies exceeded the objective probabilities for success. This finding was demonstrated using both college student (Golin, Terrell, & Johnson, 1977) and psychiatric patient (Golin, Terrell, Weitz, & Drost, 1979) samples. In contrast, nondepressives' expectancies were accurate when the experimenter rolled the dice, but depressives' expectancies were unrealistically high. Accuracy of expectancy was therefore affected by self versus other control. Garber and Hollon (1980) and Sacco and

Hokanson (1978) also found that depressed students gave smaller changes in expectancies of success than nondepressives on "skill" tasks only when they performed the tasks themselves. No differences were observed between depressives and nondepressives when another individual completed the task. Depressives also reported smaller expectancies of success on "skill" tasks in a public setting, but not in a private one. In short, depressives appear less susceptible to "illusions of success" than nondepressives for themselves, but more susceptible to "illusions of success" for others (Alloy & Abramson, 1988).

Alloy and Ahrens (1987) investigated depressives' prediction of the likelihood of future positive and negative events both for themselves and for others. They found that depressed college students were more pessimistic than nondepressives regarding both themselves and others. Additionally, nondepressives demonstrated a self-enhancing bias, overestimating the probability of their own success and underestimating the probability of their failure relative to their forecast for others who were judged identical to themselves on predictor variables. In contrast, depressives' forecasts for self relative to others were unbiased. Crocker, Alloy, and Kayne (1988) reported that nondepressed students judged positive events to be more likely and negative events to be less likely to happen to themselves than to the "average" college student; depressives' estimates of the likelihood of future positive and negative events for self versus others were unbiased.

These studies, as well as other recent empirical results, suggest that the expectancies of mildly depressed individuals are in fact more accurate than those of nondepressed people. Lobitz and Post (1979) found that nondepressed subjects expected themselves to perform better than other subjects, while mildly depressed subjects expected to do only as well as the others. Actual performances supported the expectancies of the mildly depressed subjects. Several studies have reported that depressives and nondepressives demonstrate similar success expectancies in skill tasks (Golin et al., 1977; Miller & Seligman, 1973; Smolen, 1978), but depressives do seem less optimistic under chance-determined conditions (Golin et al., 1977). This finding suggests that depressives may be more resistent than nondepressives to developing an illusory belief of control (Golin et al., 1979).

Layne's (1983) review article asserted that, in contradiction to the current prominent cognitive models of depression, empirical evidence does not support the contention that the general cognitions of depressives are distorted. His review stated that depressed people suffer significantly less distortion than normals and psychiatric controls. Layne contended that the empirical evidence does support the notion that depressives are pessimistic

(Erickson, Post, & Paige, 1975; Krantz & Hammen, 1979). He added, however, that many researchers have incorrectly inferred that depressives are cognitively distorted as well, based on the unspoken assumption that nondepressives think realistically and any who think differently must be irrational. Layne claimed that cognitive distortion must be assessed by the external criteria of reality, not the (sometimes unrealistic) optimism of normal, nondepressed people.

This broad group of empirical findings, that mild depressives are more accurate than nondepressives in judging expectancy of control, strongly contradicts the claims of Beck and the learned helplessness/hopelessness models. Langer (1975) offered a complex explanation for the inaccuracy of nondepressives' expectancies. She described nondepressives' inordinately high and optimistic expectations of successful personal outcomes as reflecting an "illusion of control." This illusion of control involves people's drive to control their environment in a self-enhancing manner, by claiming a causal relationship between their actions and positive outcomes, even when such a relationship is weak or absent. This distorted illusion of control then leads to bolstered confidence for subsequent performances.

Little evidence is available to explain the relative accuracy of depressives' expectancies. Langer's (1975) account offered no explanation for this phenomenon; however, it can be inferred that depressives generally do not experience an illusion of control, and therefore have appropriate performance expectations.

In order to better predict people's expectancies, Bandura (1977) differentiated between efficacy expectations and outcome expectations. Efficacy expectancy describes the belief that one has the ability to produce a specified behavior. Outcome expectancy refers to the belief that a given behavior will produce a desired outcome. High efficacy expectancies have been found to positively influence behavior by fostering productive action, while low self efficacy leads to perceptions of uncontrollability (Bandura & Wood, 1989). According to Abramson et al. (1978), people experiencing personal helplessness would have low efficacy expectations but high outcome expectancies, i.e., they would believe that correct responses do exist but are unavailable to them due to their personal inadequacies. Conversely, the newer hopelessness theory (Abramson et al., 1989) has revised the claims of the 1978 reformulation by stating that a subset of depressives are hopeless due to their negative outcome expectancies. They view themselves as unable to alter the probabilities of avoiding aversive events. The newer hopelessness theory (Abramson et al., 1989) similarly asserts that both negative expectations about the occurrence of highly valued outcomes and expectations of helplessness about changing the likelihood of occurrence of those outcomes lead to the development of hopelessness depression. In other words, depressives hold lower outcome expectancies than nondepressives due to their hopeless outlook. Tang (1987), however, using a college student sample, found that mild depressives did not differ from nondepressives in efficacy or outcome expectancies. The predictions from the Beck and the reformulated learned helplessness models that depressives would view themselves as inadequate was supported only for depressives who scored low on a measure of defensiveness (Tang, 1987).

In sum, both Beck's model and the reformulated learned helplessness model have claimed that depressives expect more failure and less success than is realistic, while nondepressives are relatively accurate in their expectancies. The reformulated learned helplessness model in particular claims that depressives view themselves as unable to "solve solvable problems" (Abramson et al., 1978). The newer hopelessness model similarly claims that depressives view themselves as unable to alter the probabilities of avoiding aversive events. Empirical support for these claims has been rather limited. The majority of recent results has suggested that mild depressives are in fact more accurate than nondepressives in their expectancy of control estimates, casting considerable suspicion on the validity of those claims.

2. Evaluation of performance. Both Beck's model and the reformulated learned helplessness model predict that depressed individuals will underestimate success and overestimate failure in comparison to nondepressed individuals. (Hoplessness theory has to date made no specific claims in this area). Empirical results have lent mixed support for this view. Several studies have supported these predictions by finding that depressives evaluate their performances more harshly than do nondepressives in the absence of actual performance differences (Butler & Mathews, 1983; Lobitz & Post, 1979; Smolen, 1978; Wenzlaff & Grozier, 1988; Wollert & Buchwald, 1979; Zarantonello, Mathews, Slaymaker, Johnson, & Petzel, 1984). This has been demonstrated for social skills (Lewinsohn, Mischel, Chaplin, & Barton, 1980), for the Digit Symbol test (Ciminero & Steingarten, 1978), and for cognitively complex tasks such as solving six-letter anagrams (Zarantonello, Johnson, & Petzel, 1979).

Other studies have also found that depressives' cognitive deficits lie in the perceptions of their abilities rather than in the abilities themselves (Lobitz & Post, 1979). Loeb, Beck, and Diggory (1971) found that depressed patients rated the quality of their experimental performances lower than did nondepressed patients, when the actual performances of the two groups did not differ. Rehm and her colleagues found that depressed hospitalized patients were less likely than nondepressed hospitalized patients to reward themselves following responses to a word recognition task, even though both groups performed equally well (Rozensky, Rehm, Pry, & Roth, 1977). Similarly, Nelson and Craighead (1977) discovered that mildly depressed college students rewarded themselves less than a nondepressed control group on an experimental task, but their self-reward was closer to the actual reinforcement rate than was that of nondepressed subjects, who greatly overrewarded themselves. Finally, Lobitz and Post (1979) found that depressives were quite critical in evaluating their own personal performances, but not in evaluating the performances of others. However, despite the apparent negativism of depressives' ratings, their self-reinforcement and punishment appeared in closer agreement with objective criteria than that of nondepressives.

Sacco and Hokanson (1982) likewise found that depressives self-reward less than nondepressives, but only in public measurement conditions. Depressed students self-rewarded less than nondepressives in a public setting, but they self-rewarded more than nondepressives in a private setting. These findings parallel those of their earlier research results on expectancies of success (Sacco and Hokanson, 1978).

In another study examining the evaluation of personal performance versus that of others, Ahrens (1986) gave depressed and nondepressed students mixed information about their task performance relative to the performance of others. In evaluating their own performance, depressives ignored information about others who had failed the task, instead comparing themselves to successful others. This pattern was reversed for nondepressives, who ignored the performances of successful others and compared themselves to unsuccessful others (Ahrens, 1986).

Results supporting depressives' more pessimistic self evaluations of performance appear rather widespread and consistent. However, although negative self-evaluation has been shown to be characteristic of depression, it does not appear specific to this disorder. Barling and Fincham (1979) found that low scores on general measures of psychological adjustment have also been linked to negative self-evaluation. Additionally, Gotlib (1981, 1982, 1983) suggested that lowered levels of self-evaluation may not be specific to depression, for he found similarly low self-evaluations among both depressed and nondepressed psychiatric patients.

In addition to Gotlib's work, several studies have pointed to inaccurate self-evaluations by nondepressives, as well as depressives. In a manner similar to the self-enhancing bias in which individuals take credit for their perceived successes and deny responsibility for failures (Harvey & Weary, 1984; Kelly & Michela, 1980), nondepressives often rate themselves more favorably than they are rated by observers (Lewinsohn et al., 1980; Rizley, 1978).

Considering this contradictory group of results, current research support for depressives' negative evaluation of performance remains confusing. While the majority of evidence does suggest that depressives evidence some degree of negative self-evaluation, there also exists considerable support for inaccuracies among nondepressives. It is possible that the self-evaluations of depressives are negative in comparison to those of nondepressives, but not in comparison to objective criteria.

Several rationales are available for explaining why depressives' negative self-evaluative biases may not involve cognitive distortion. First, as Layne (1983) pointed out, depressives' generally pessimistic stance should not be confused with cognitive distortion. Some recent research suggests that depressed individuals may actually live more difficult lives than nondepressed, as children (Lloyd, 1980a) and as adults (Dohrenwald & Dohrenwald, 1974; Ilfeld, 1977; Lloyd, 1980b). In this way, depressives' pessimism may be as accurate a reflection of their past and as realistic a forecast of their future as is the optimism of normals (Layne, 1983).

Second, Langer (1975) indicated that in many instances, normals are inaccurate in their judgment of personal situational control. Most situations offer no clear indications as to the amount of actual control available to an individual. It may be adaptive to assume control and then act accordingly. Nonetheless, normals are often innaccurate in these judgments, maintaining an illusion of personal control. Langer (1975) and Layne (1983) both indicated that this illusion of control reflects some degree of cognitive distortion on the part of normals. Therefore, the hypothesized cognitive distortions of depressives must be evaluated against external criteria of reality, and not against the standard of nondepressives' (often distorted) perceptions. Differences between normals and depressives may well identify the inaccuracy of normals' evaluations rather than underestimations among depressives.

Third, Mischel (1973) argued that self-evaluation could be more closely related to initial expectancies than to actual performances. Previous similar experiences of failure may predispose depressives to an initial expectancy of failure. Similarly, Beck (1974, 1976, 1979) and Abramson et al. (1978) suggested that depressives view their personal inadequacy as causing perceived failure. As a result, they may assume that their performance is inferior to others'. In this way, depressives' negative self-evaluations could reflect attributions for their initial expectancies of failure as much as appraisals of their actual performance (Tang, 1987).

In short, support for the claims of Beck's model and the reformulated learned helplessness model, that depressives will underestimate success and overestimate failure, remains mixed. Depressives have been found to be more pessimistic in their self-evaluations, e.g., rewarding themselves less than nondepressives for comparable performances. However, this overly critical style does not appear specific to depressives, since it has been found in nondepressed psychiatric patients as well. Further, proof that this pessimism constitutes distortion is limited. Nondepressives have conversely been found to be overly favorable in their self-evaluations in comparison to the evaluations of observors. Research suggests that although self-evaluations of depressives may be negative in comparison to those of nondepressives, they may not differ significantly from objective criterion. Further research is needed to more fully address this issue.

3. Attributional processes. Several cognitive theories, including Beck's model and the hopelessness model, claim that depressed people make irrational causal attributions (Abramson et al., 1978, 1989; Alloy et al., 1988; Beck, 1967, 1974, 1976, 1979; Ellis, 1962; Rehm, 1977; Vestre, 1984). Attributions are viewed as supplying a critical link between negative experiences and subsequent depressive pathology. In particular, the learned helplessness camp (Abramson et al., 1978; Seligman, 1975) has

posited that depression-prone individuals attribute negative outcomes to internal, stable, and global causes. Further research (e.g. Seligman, Abramson, Semmel, & von Baeyer, 1979) added that these individuals attribute positive outcomes to external, unstable, and specific causes. Empirical support has proven strong for the former claim (Alloy, Abramson, Peterson, & Seligman, 1984; Blaney, Behar, & Head, 1980; Golin, Sweeney, & Schaeffer, 1981; Raps, Peterson, Reinhard, Abramson, & Seligman, 1982; Seligman et al., 1979; Zuroff, 1981), with mixed support for the latter (Abramson et al., 1978; Kuiper, 1978; Lewinsohn et al., 1981; Rizley, 1978).

Some researchers have suggested that depressives employ an "evenhanded" attributional response style, explaining both successes and failures in similar fashion. Rizley (1978) found nondepressed subjects to be more optimistic than mildly depressed subjects, attributing their experimental task successes to internal factors and their failures to external ones. Mildly depressed subjects, on the other hand, displayed neither an optimistic nor a pessimistic attributional style.

In a similar vein, Kuiper (1978) found that mildly depressed college students demonstrated similar attributions for success and failure outcomes along the internal-external dimension. Conversely, nondepressives who experienced success displayed more internal attributions than those who experienced failure. Kuiper (1978) concluded that nondepressives' internal attributions for success served a self-enhancing function, while their external attributions for failures served a self-protective one. In contrast, mildly depressed subjects evidenced neither a self-enhancing nor a self-protective style.

Findings from social psychology appear to support this phenomenon (Ruehlman et al., 1985). Studies have indicated that nondepressed individuals manifest more internal attributions for success than for failure (Fitch, 1970; Langer & Roth, 1975; Miller, 1976; Sicoly & Ross, 1977; Wortman, Costanzo, & Ross, 1977). In addition, a meta-analysis conducted by Alloy (1982) supports the existence of attributional optimism in the nondepressed, with unbiased attributional styles for the mildly depressed.

Similar to results of studies investigating expectancy of success, several attribution studies have suggested that depessives' attributional evenhandedness and nondepressives' self-serving bias may be specific to the self. Sweeney, Shaeffer, and Golin (1982) discovered greater attributional evenhandedness in depressives than nondepressives for the self; however, attributions for others did not differ between the two groups.

The notion that attributional evenhandedness may result from low self-esteem rather than from depression per se has received mixed empirical support. Tennen and Herzberger

(1987) found that when self-esteem was controlled, depression was only mildly related to attributional style. Conversely, when depression was controlled, self-esteem was more highly correlated. In a replication study, Crocker, Alloy and Kayne (1987) instead found depression to be a significantly better predictor of attributional evenhandedness than self-esteem.

Three basic methodologies for empirically evaluating attributional styles have included: examining attributions made following success or failure in experimental laboratory tasks; assessing attributions over hypothetical events; and investigating attributions for actual stressful life events. Among the laboratory studies which have used experimentally-controlled success and failure tasks, support for the hypothesis that depressed individuals are more internal than nondepressed in their attributions for failure has been consistent and widespread (e.g., Kuiper, 1978; Rizley, 1978). However, little support has been found for depressed-nondepressed differences in internal attributions for success (Zuroff, 1981), attributions to stability (Gotlib & Olson, 1983; Kuiper, 1978; Rizley, 1978), attributions to effort, luck, and task difficulty (Willis & Blaney, 1978), and ratings of perceived control (Abramson et al., 1978; Rizley, 1978; Willis & Blaney, 1978).

Other studies have evaluated attributions for experimentally-created positive and negative hypothetical events, e.g., imagining a failing grade on a mid-term examination. Only equivocal support for the learned helplessness model has been obtained. Results have generally been inconsistent, and expected group differences, when obtained, have been of small magnitude (Danker-Brown & Baucom, 1982; Lewinsohn, Steinmetz, Larsen, & Franklin, 1981; Manly, McMahon, Bradly, & Davidson, 1982; Peterson, Schwartz, & Seligman, 1981).

In studies of attributions for stressful life events, empirical results have also been mixed. The work of Hammen and her colleagues (Barthe & Hammen, 1981; Gong-Guy & Hammen, 1980; Hammen, 1980; Hammen & DeMayo, 1982) represents the majority of the research in this area. Their research has indicated no consistent or pervasive differences betweeen depressives and nondepressives in their attributions for stressful events. Generally, studies in this area have produced marginal evidence for depressives' attributions of stressful events to internal causes.

The weak empirical results in the area of attributions could in part be due to an over-reliance on the use of student or subclinical samples (Coyne & Gotlib, 1983). The issue of continuity from mild depression to clinical depression as it relates to specific cognitive variables such as attribution, remains unsettled (Raps et al., 1982). Attempts to generalize the results of studies using mildly depressed college students to clinical populations is risky, since qualitative and not just quantitative differences in functioning may exist (Arieti & Bemporad, 1978; Depue & Monroe, 1978; Miller, 1975; Seligman, 1978). Indeed, few studies examining the attributional styles of severely depressed individuals are available, and their results are confusing and contradictory.

Among the studies using clinical samples, Miller, Klee, and Norman (1982) evaluated the attributions of depressed and nondepressed psychiatric inpatients. Depressed subjects were defined as those patients having received a primary diagnosis of depression who achieved a score greater than 17 on the Beck Depression Inventory (BDI; Beck, Ward, Mendelson, Mock, & Erbaugh, 1961). Nondepressed subjects evidenced no symptoms of depression or schizophrenia and achieved a BDI score less than 12. While no significant differences were found between groups on attributions concerning experimentally-induced success/failure tasks or hypothetical positive/negative events, the depressed patients did manifest a greater depressive style in response to actual stressful events, i.e., they were more negativistic (Miller et al., 1982).

In the Raps et al. (1982) study, depressed male inpatients, as compared to nondepressed schizophrenics and medical/surgical patients, made more internal, stable, and global attributions for negative events. In addition, they displayed less stable and internal attributions for positive events than did the medical/surgical patients. Results also indicated greater evenhandedness among depressives than both other groups in their attributions for good versus bad events. Since these results were specific to the depressed group, the authors concluded that this response pattern is not a general characteristic of psychopathology.

Hamilton and Abramson (1983) compared cognitive response patterns of depressed inpatients to those of nondepressed psychiatric controls and nondepressed normal controls upon admission and then again at time of discharge. Depressed subjects had met diagnostic criteria for unipolar major depressive disorder of an episodic, and not chronic nature, and had scored 16 or greater on the BDI. Nondepressed psychiatric subjects were inpatients with diagnoses other than depression who scored 15 or less on the BDI. Nondepressed normals were community recruits free of diagnosed pathology who attained a BDI score of 8 or less. Hamilton and Abramson (1983) set a higher BDI cut-off criterion for nondepressed psychiatric subjects due to the finding that psychiatric patients, regardless of clinical diagnosis, tend to be slightly more depressed than normal samples (e.g., Gotlib, 1982). Although nondepressed psychiatric subjects' scores were higher than those of the nondepressed normals, their mean BDI score fell within Beck's (1961) nondepressed range ($M = 8.10$), thereby qualifying them as clinically nondepressed.

Upon admission, depressives displayed significantly less of a self-enhancing attributional style than nondepressed normals. Indeed, 30% of the depressed patients evidenced a self-derogatory style. However, when symptoms had remitted, the depressed group's attributions had become more self-serving, while those of the other two groups remained unchanged.

In sum, while much research on attributional processes has been generated by the learned helplessness model, results have been mixed. It appears that the only claim receiving widespread support has been that depressives more strongly attribute negative outcomes to internal causes. Support for the theory's other claims has not been consistent.

Alternate explanations. Beck's notion of the depressive cognitive style is somewhat broader than Seligman's in encompassing cognitions other than attributional style, such as excessive concern over others' judgments, and perfectionistic, unrealistic standards for personal performance (Hamilton & Abramson, 1983). The earlier research of Seligman and his colleagues apparently assumed a one-to-one relationship between attributions and depressive symptomatology. Alternate conceptualizations suggest that the variation found in the attributions of depressives may be due to uncontrolled intervening variables. Three such variables will be described briefly.

First, unexpectedness of outcome has been shown to influence attribution (Feather & Smith, 1971). Gotlib and Olson (1983) claimed that nondepressives' external attributions for failure and depressives' external attributions for success reflect their differing response styles to unexpected outcomes. For depressives, who more likely expect failure (Beck, 1874; Rehm, 1977), success is unexpected and is attributed to external factors. The opposite pattern is true for nondepressives. Thus, these differing causal attributions may derive from initial expectancies for success and failure.

Noting that previous studies in this area utilized experimentally (i.e. externally) controlled feedback, Gotlib and Olson (1983) completed a study wherein subjects were allowed to formulate their own judgments of success or failure. With the effects of unexpectedness removed, depressed and nondepressed groups did not differ in their attributions for success and failure. All subjects, regardless of mood, who rated their performance as a success attributed their outcome more to internal and less to external factors than did subjects rating their performance a failure. These results suggest that the relationship between causal attributions and depression may not be as direct as originally thought. Rather, initial expectancies of success or failure may mediate subsequent attributions.

Second, confirmation of expectancy and valence of outcome have been shown to affect outcome attributions. Investigating classroom written test performances, Chapman and Lawes (1984) gathered students' pre-test expectancies for passing or failing the exam, as well as their pre- and post-exam attributions. Those expecting to pass who actually failed showed increased attributions to external, unstable causes as compared to their pre-exam attributions. Stated differently, when outcomes are disconfirmed, people may shift from internal to external unstable causes, such as chance factors. Pre- and post-exam attributions were consistent in cases where expectations were confirmed for passing or failing the exam. Moreoever, those passing who intially expected to pass viewed internal factors as more important than external factors in their success. Results of other studies (Chapman & Lawes, 1984; Riskind, Rholes, Brannon, & Burdick, 1987) parallel those of Gotlib and Olson (1983) by demonstrating a relationship between attributions and expectancy.

Third, Rosenbaum, Jaffie, and Yoram (1983) found that individuals' expectations for self-efficacy and their general repertoire of self-control skills could be as important as their attributions of helplessness on experimental tasks. Those with higher levels of self-efficacy attributed personal success to internal rather than external causes.

To summarize the research on attributions, it appears that the relationship between attributions and depressive pathology is not as strong or as direct as predicted by Seligman's reformulated learned helplessness model (Coyne & Gotlib, 1983; Peterson & Seligman, 1984; Peterson, Villanova & Raps, 1985). Current findings fail to support a simple one-to-one relationship between causal attributions and depression. Rather, a more complex interaction of variables, including expectancy and outcome valence, appears to be involved. Additionally, according to Abramson, Alloy, and their colleagues (Abramson et al., 1988; Abramson et al., 1989; Alloy et al., 1988), many previous studies have failed to fully consider the heterogeneity of depressive disorders, inappropriately lumping all depressives together and examining their expectations, self-evaluations, attributions, etc., as if they represented a homogenous phenomenon (Abramson et al., 1988).

Judgment of Contingency

Contingency is a general term referring to the degree of relationship between any two elements. This relationship implies controllability, as when a response exerts some degree of control over an outcome (Seligman, 1975). Control is therefore defined as the dependence of an outcome on a response, conveying the meaning of contingency in a less technical manner (Jenkins & Ward, 1965).

The notion of contingency is foundational for many theories of learning (Bolles, 1972; Mackintosh, 1975; Maier & Seligman, 1976; Rescorla & Wagner, 1972). Each of these theories claims that organisms are sensitive to relationships among stimuli in the environment, as well as to relationships between their own responses and environmental outcomes (Alloy & Abramson, 1979). When there is no objective contingency between stimulus and reinforcer, however, does the organism learn nothing about the stimulus, or does it learn explicitly that stimulus and reinforcer are unrelated (Alloy & Abramson, 1979)? Mackintosh (1975) argued that organisms actively learn to ignore stimuli which they have determined to be uncorrelated with reinforcement, focusing instead on reinforcement-related stimuli.

Reformulated learned helplessness theory (Abramson et al., 1978; Maier & Seligman, 1976) has argued that organisms which have been exposed to aversive stimuli terminating independently of their responding learn that reinforcement is unrelated to their behavior. In this way, they perceive these events as uncontrollable. Emotional, motivational, and cognitive deficits result from this state of helplessness, as the individual becomes "depressed" (Maier & Seligman, 1976; Seligman, 1975).

More recently, the hopelessness revision (Alloy et al., 1988; Abramson et al., 1988) has come to view the negative valence of an event, rather than its uncontrollability, as the central factor leading to the development of hopelessness depression. This revision also suggested that self-esteem deficits result from the self-perception of helplessness, as well as the previously enumerated emotional and motivational deficits of depression.

Contingency research has placed central importance on the role of objective contingencies as determinants of organisms' behaviors. The role of subjective representations of contingencies, however, remains controversial. For example, Rescorla and Wagner's (1972) classical conditioning model is based entirely on the notion of objective contingency. Conversely, Maier and Seligman (1976) claimed that individuals form subjective representations of objective contingencies, and that these subjective representations directly influence behavior.

Several studies have attempted to assess humans' cognitive representations of contingency. A few have investigated the similarity between the learned helplessness notion of response-outcome independence and Rotter's (1966) external locus of control. Using tasks in which successes appeared to be determined by either skill or chance, Rotter and his coworkers (James, 1957; Phares, 1957; Rotter, Liverant, & Crowne, 1961) demonstrated that outcomes of previous trials have a greater effect on expectancies for future successes when the individual believes that outcomes are skill-determined (i.e., dependent on his or her responses) rather than chance-determined (i.e., independent of his or her responses). Learned helplessness researchers (Klein & Seligman, 1976; Miller & Seligman, 1976) employed this approach and found that students exposed to prior uncontrollable aversive noises exhibited less expectancy change in an ostensible skill task than students exposed to no noises or to prior controllable noises. They inferred that the helpless students had developed an expectancy of response-outcome independence, i.e., helplessness.

In work completed prior to the formulation of the learned helplessness model, Jenkins and Ward (1965) devised a format for evaluating cognitive representations of contingency. As opposed to Seligman, Maier, and Solomon (1971), who investigated the dichotomy of contingent and noncontingent cases, Jenkins and Ward broadened the construct of contingency to include an evaluation of degree of control, which they defined as the magnitude of the difference between two relevant conditional probabilities. Subjects were presented with a series of contingency tasks, each consisting of 60 trials, in which they were given a choice between two responses leading to one of two possible outcomes (reward or no reward). The procedure was designed so that responses and outcomes were contingently related on some problems, but not on others. Additionally, Jenkins and Ward included problems with a high frequency of reinforcement, but a noncontingent relationship between response and outcome, as well as noncontingent problems with a low frequency of reinforcement. Following each series of trials, subjects rated the degree of control their response exerted on outcomes, using a scale of 0 to 100. Jenkins and Ward (1965) claimed that a contingent relationship between a response and an outcome exists when the probability of that outcome given one response differs from the probability of the same outcome given an alternate response.

By evaluating degree of control, Jenkins and Ward were able to demonstrate that people's subjective cognitive representations of contingencies are not isomorphic with objective contingencies. Subjects' ratings of degree of control were correlated highly with the frequency of reinforcing trials, regardless of the actual degree of objective contingency. There was no apparent relationship between ratings of contingency and actual degree of control (Jenkins & Ward, 1965). Similar results from subsequent studies seem to support the contention that people's subjective representations of contingency are not accurate pictures of actual contingencies (Bruner & Revusky, 1961; Hake & Hyman, 1953; Wright, 1962). The nature of these inaccuracies has become a somewhat controversial theoretical and empirical topic in the recent literature.

Hopelessness model. The learned helplessness model (Abramson et al., 1978; Seligman, 1975), as well as Alloy and Abramson's (Abramson et al., 1988; Abramson et al., 1989;

Alloy et al., 1988) more recent hopelessness theory revision, claim that when individuals expect outcomes to be independent of their responses, they exhibit the motivational, affective, and self-esteem symptoms of depression. While hopelessness theory does not explicitly address depressives' perceptions of noncontingency responses in outcomes, the reformulated learned helplessness theory clearly predicts that depressives will underestimate environmental contingencies. A strong and a weak prediction have been deduced from the reformulated learned helplessness model. According to the strong prediction, depressives will underestimate absolutely the degree of objective contingency present between their responses and external outcomes. The weak prediction claims that depressives will merely underestimate the degree of objective contingency relative to nondepressives. Therefore, while both views predict that depressives' subjective appraisals of contingency are lower than those of nondepressives, only the strong view claims that depressives underestimate control relative to objective contingencies.

Empirical support from researchers outside the learned helplessness camp has been relatively sparse. The general theme running through these contingency learning studies (Alloy & Abramson, 1979, Experiment 2; Bruner & Revusky, 1961; Hake & Hyman, 1953; Jenkins & Ward, 1965) is that individuals often treat noncontingent situations as if they were contingent. That is, they behave as though outcomes are dependent upon their responses when they are not, and as though one event can be predicted from another when it cannot (Alloy & Abramson, 1979). These findings thus suggest that people's contingency representations differ systematically from actual, objective contingencies in noncontingent situations, which are psychologically more ambiguous, threatening, and difficult to judge than other contingencies. This is particularly evident in stituations involving high frequency of reinforcement, where it is more difficult to discriminate between reinforcement frequencies and actual control levels. In contrast to Seligman's predictions, these researchers have claimed that it is not mild depressives who are inaccurate by underestimating contingencies, but nondepressives who are more prone to distort their perceptions of contingencies in self-enhancing ways.

A recent review of the research investigating "depressive realism" (Alloy & Abramson, 1988) has identified four central concepts which are necessary in evaluating cognitive differences between depressives and nondepressives. These concepts describe the quality and nature of individuals' judgments, as well as the consequences of those judgments. First, error/distortion describes the inconsistency of an individual's conclusion with some commonly accepted measure of objective reality. This communicates the accuracy or realism of a conclusion/judgment. Second, bias refers more broadly to a tendency to make (inaccurate) judgments in a systematic, consistent way across situations. Third, irrationality considers the relative appropriateness of bias to an individual's typical environment. For example, if an individual's inferential bias accurately reflects the contingencies of his or her usual environment, it would not be irrational for that individual to utilize that bias in evaluating current situational information (Abramson et al., 1988). Fourth, maladaptiveness refers to the negative physical and psychological consequences which occur as a result of an individual's judgments. Regardless of an individual's distortion or biases, affective, behavioral, and physical consequences inevitably result from those judgments (e.g., inappropriate self-blame, depressed affect). Those which negatively affect health and well-being are therefore considered maladaptive (Alloy & Abramson, 1988).

Alloy and Abramson research. Alloy and Abramson (1979) performed a series of experiments to assess the possibly differing abilities of mild depressives and nondepressives to evaluate objective environmental contingencies. Subjects' task in each problem was to estimate the degree of contingency between their responses (pressing versus not pressing a spring-load button) and an environmental outcome (onset of a green light).

In experiment 1 (Alloy & Abramson, 1979), subjects were presented with a series of trials in which there was a 25%, 50%, or 75% degree of contingency between their responses and outcomes. Following each series, subjects were asked to judge the degree of contingent relationship between their button pressing and the green light onset. All subjects were successful at performing the responses that maximized reinforcement and at detecting the degree of contingency between response and outcome, suggesting that both depressives and nondepressives knew the relevant conditional probabilities and organized that information appropriately (Alloy & Abramson, 1979). Moreover, both groups were accurate in judging the percentage of reinforcement when they chose not to press, and not just on those trials where they chose to press the button. Subjects were more accurate in moderate than high or low control situations. Taken together, these results suggest that when an actual contingency between response and outcome exists, subjective representations of contingency mirror objective contingencies among both nondepressed and mildly depressed subjects. These results contradicted Beck's and Seligman's view that mildly depressed students distort response-outcome relationships by underestimating their degree of control.

While experiment 1 involved problems with some degree of actual contingency, experiment 2 placed subjects in one of two conditions where responses and outcomes were noncontingently related but differed in overall frequency of reinforcement. Nondepressed students exhibited an illusion of control in a noncontingent, high frequency of reinforcement condition by overestimating their degree of control over outcomes. However, in a noncontingent, low frequency of reinforcement situation, nondepressives were relatively accurate in evaluating degree of control. Depressed students, on the other hand, were unaffected by frequency of reinforcement, accurately detecting their lack of control in both situations. Therefore, both affective state and frequency of reinforcement interacted to produce systematic errors in judgment of noncontingency. Alloy and Abramson (1979) postulated that subjects here may have construed a high frequency of reinforcement as a "good" outcome and a low rate of reinforcement as a "bad" outcome, so that valence of outcome, rather than frequency per se, may have been the central variable underlying nondepressives' errors in judging noncontingency.

Experiment 3 explored valence of task outcome. Students were assigned to one of two problems with green light onset signaling either gain (reward) or loss (punishment) of money. Responses and outcomes were noncontingently related, and onset frequency was maintained at 50% across the two problems. Results corroborated those of experiment 2; depressed subjects accurately detected noncontingency across situations, while nondepressed subjects demonstrated an illusion of control, overestimating control in reward but not in punishment. Moreover, nondepressives displayed more pronounced illusions of control in this experiment, where they were attempting to gain cash rewards, than in experiment 2 where they were merely judging their degree of contol over more neutral outcomes. The results of this study demonstrated that nondepressives' overestimations of control are a function of outcome valence.

Experiment 4 sought to determine whether depressives might underestimate their degree of control over hedonically charged contingent outcomes. The fact that nondepressives' illusion of control was stronger in experiment 3 than in experiment 2 supports this reasoning. In experiment 4, subjects received one of two problems, each of which had a 50% degree of contingency between responses and light onset (e.g., the light appeared on 75% of the trials where subjects pressed the button and 25% of the trials where they did not). The two conditions differed in outcome valence (gain or loss of money). Contrary to the predictions of Seligman's and Beck's models of depression, depressed subjects were accurate in their estimations of contingency across situations--they did not underestimate the degree of contingency between their responses and outcomes in a positive situation (gain cash), nor did they underestimate their control in a negative situation (lose cash). However, nondepressed subjects erroneously believed their responses had very little control over light onset when they lost money, but they overestimated their actual control when they gained money. Additionally, only nondepressed subjects were affected by whether an active (pressing) or a passive (not pressing) response was related to higher frequencies of reinforcement.

Conclusions of Alloy and Abramson study. Alloy and Abramson's (1979) finding that depressed students' judgments of contingency were accurate led them to the novel claim that depressives were "sadder but wiser." Conversely, nondepressed students were more inaccurate: they overestimated the degree of contingency between their responses and outcomes when outcomes were desired and underestimated the degree of contingency when outcomes were undesired.

Certainly, Alloy and Abramson's (1979) claims of "depressive realism" (Mischel, 1979) caused quite a stir among cognitive psychologists, who had long assumed that depressives suffer from significant cognitive distortion across wide areas of functioning, including judgment of contingency. To have demonstrated that their estimations were consistently accurate, and that it was nondepressives who exhibited cognitive distortion, was a revolutionary concept in the literature.

To further substantiate their claims that depressives actually perceive contingency more accurately than do nondepressives, Alloy and Abramson undertook several projects to replicate their initial findings. These studies consistently demonstrated nondepressives' overestimation of control in positive outcomes, as compared to depressives' accurate judgments of degree of contingency (Abramson, Alloy, & Rosoff, 1981; Alloy, Abramson, & Viscusi, 1981; Martin, Abramson, & Alloy, 1984).

Alloy, Abramson, and Viscusi (1981) investigated whether current mood state, induced or naturally occurring, influences judgment of control estimates. Using a college student sample, they found that depressed subjects who were temporarily elated most resembled subjects in nondepressed neutral or no induction control groups, while nondepressives who were temporarily depressed manifested a response style most similar to depressed neutral or no induction subjects. This pattern suggests that current mood state, whether natural or induced, affects judgment of control. Further, positive affect appears related to overestimations of control, while mildly depressive affect seems related to more accurate estimations of control.

In general, studies by Alloy, Abramson, and their colleagues suggest that although both mildly depressed and nondepressed subjects are capable of offering accurate judgments of contingency (e.g., Alloy & Abramson, 1979, experiment 1), a fairly consistent pattern of responding differentiates mild depessives from nondepressives. Nondepressives tend to display positively biased responses, while mild depressives exhibit generally unbiased processing, resulting in greater accuracy of judgments. This notion of depressive realism is in direct conflict with the predictions of depressive distortion found in many current theories such as the Beck and learned helplessness models. These findings demolished the heretofore implicit assumption that nondepressed individuals may serve as a relatively unbiased comparison group. In response to the growing body of empirical support for depressive realism, the newly formulated hopelessness theory has eliminated learned helplessness theory's previous claim of a specific cognitive deficit identifying depressive underestimations of control. However, hopelessness theory only briefly alluded to this change, inadequately explaining how depressives might display unrealistic (and possibly distorted) expectations of control, yet maintain accurate judgments of control. In short, it remains unclear how hopelessness theory will convincingly reconcile the depressive realism phenomenon with its overall conceptual framework.

While mild depressives display more pessimism as compared to nondepressives (Layne, 1983), and may exhibit a negative self-evaluative style (Lobitz & Post, 1979; Loeb et al., 1971; Nelson & Craighead, 1977; Rehm et al., 1977) and a self-derogatory attributional style (Hamilton & Abramson, 1983), current evidence suggests that their inaccuracies are not consistently apparent across cognitive functioning in general. Further, among the Alloy and Abramson studies, mild depression appears associated with accurate judgments of control in contingent, as well as noncontingent, situations (Ruehlman et al., 1985). Depressives' accuracy in judging contingencies appears unaffected by frequency or desirability of outcome.

In contrast, positivistic distortions characterized nondepressives' pattern of responding. They relied on frequency and desirability of outcome when inappropriate, but to their advantage. That is, they believed that a contingency existed between their responses and outcomes when those outcomes were highly desired or occurred with relatively high frequency (Alloy & Abramson, 1979). These errors appear to reflect the use of positivistic evaluative schemata (Ruehlman et al., 1985).

In addition, other studies using different methodologies have obtained similar results. Langer (1975) and Golin and his colleagues (Golin et al., 1977; Golin et al., 1979) reported that normal and nondepressed psychiatric controls displayed an illusion of control in a chance situation into which apparent elements of skill were introduced. Depressed subjects, however, did not fall victim to this illusion.

In a variation of the Alloy and Abramson design, Tang (1987) investigated judgment of control in a college student sample. Subjects were divided into mildly depressed and nondepressed groups based on BDI scores and given four tasks under either reward or punishment conditions. Monetary reinforcement was made contingent on light onset for all tasks and on accuracy of judgment of control for the final three tasks. Frequency of reinforcement and degree of actual control were varied across tasks.

Results indicated that all subjects were more accurate in moderate than in low control conditions and in low than in moderate frequency conditions. Subjects in reward conditions were more accurate in perceiving reinforcing events, and they gave themselves more credit on task performance than punishment condition subjects gave themselves blame for similar performances. Regarding monetary contingencies for accuracy of control judgments, depressives first overestimated control, then gradually decreased their estimations until they were generally accurate on the fourth task. These findings partially supported Alloy and Abramson's (1979) position, since the mildly depressed subjects became increasingly accurate in judgment of control across tasks (Tang, 1987).

To account for nondepressives' illusion of control for positive outcomes, Alloy and Abramson (1979) noted that both depressed and nondepressed subjects were successful in judging contingencies in experiment 1, while the two groups varied on experiment 4, which involved cash earnings. Central to Alloy and Abramson's (1979) argument was the claim that since nondepressed subjects were able to evaluate contingent tasks accurately in neutral outcome situations, their illusion of control did not result from the faulty perception of incoming environmental data, but in the organization of that data. In other words, the presence of cash incentives exerted a distorting effect on the organization of the contingency data.

Alloy and Abramson (1979) posited that nondepressed individuals were motivated in these contingency learning situations to maintain or enhance their self-esteem. Several studies have indicated (Bradley, 1978; Frankel & Snyder, 1978; Miller, 1978; Miller & Ross, 1975) that assuming responsibility for positive outcomes preserves or increases self-esteem, while attributing negative or aversive consequences to external factors likewise maintains self-esteem. Stated differently, nondepressives assigned themselves more personal control on tasks where cash was gained in order to bolster their self-esteem, whereas they preserved self-esteem by blaming their loss of money on a lack of personal control over task outcomes.

Similarly, Alloy and Abramson (1979) claimed that the absence of this motive to enhance self-esteem characterized depressed subjects' consistently accurate judgments of control. Earlier studies unrelated to judgment of contingency indirectly support this notion by finding that depressives often manifest low self-esteem (Beck, 1964, 1967, 1976, 1979; Bibring, 1953). Since depressives were not motivated to bolster their low self-esteem and thereby distort estimations of personal control, their judgments of contingency betweeen responses and outcomes were consistently accurate.

Boundary conditions of depressive realism. The growing body of research on depressive realism contains more recent results which describe limits to depessives' accuracy in judgment of control. In their recent review, Alloy and Abramson (1988) cited studies which found nondepressives' inferences to be more accurate or less biased than depressives' under certain conditions, while the two groups' cognitions may be equally susceptible to bias or distortion in other situations. Both situational and personal factors were found to exert mitigating effects on depressive realism. Specifically, depressive realism and nondepressive illusion of control may be specific to the self and to private judgments.

Martin, Abramson, and Alloy (1984) investigated the relationship between depression and the illusion of control for self versus others. Depressed and nondepressed college students provided ratings of control in a noncontingent "win" problem, where they judged the amount of control they or a confederate exerted. Nondepressives consistently manifested an illusion of control regarding self-judgments. In addition, female nondepressives overestimated control both for self and for confederate, while nondepressed males displayed this illusion only for self. Depressed subjects, on the other hand, were generally accurate in their judgments of control for self, but they overestimated the degree of control exhibited by the confederate. Martin et al. (1984) suggested that mildly depressed individuals may utilize automatic or schema-based processing when making judgments about others but use specific situational cues in making self-evaluative judgments (Ruehlman et al., 1985). In any event, depressives' realism apparently may be confined to self-referent judgments, rather than to judgments of others' control.

In a more recent study, Vazquez (1987) generally replicated the findings of Alloy and Abramson (1979), but also identified a precise boundary to depressives' realism. In noncontingency situations, depressed subjects overestimated personal control when outcomes were negative self-referent sentences. Conversely, nondepressives overestimated control relative to depressives when outcomes were positive self-referent statements. In other words, depessives succumb to an illusion of control when their judgments of control are based on negative self-referent statements.

Regarding public versus private situations, one recent study (Benassi & Mahler, 1985) demonstrated that the consistently accurate personal control estimates of mildly depressed college students on Alloy and Abramson type tasks broke down when subjects completed those tasks in the presence of an observer and outcomes were frequent but noncontingent. In the "actor alone" condition, subjects completed the experimental tasks with no one else in the room. In the "actor plus observer" condition, each subject completed pre-task questionnaires at the same table with a subject observer. Throughout the experiment, the observer sat in a chair to the right and behind the subject. At the onset of the procedure, the observer was explicitly directed to pay close attention to the actor's responses and task outcomes because questions would be directed at the observer following the experiment.

As opposed to their more accurate appraisals when alone, depressed subjects actually perceived themselves as having more control over frequently-occurring response-independent outcomes than nondepressed subjects when in the presence of an observer. Surprisingly, in contrast to situations where they performed their tasks alone, nondepressives demonstrated a reduced illusion of control in the presence of an observer. These results suggest that the presence of an observer literally reverses the pattern of judgment of control found by Alloy and Abramson (1979, experiment 2). The authors hypothesized that the scrutiny of an observer increased the demands for rationality on the nondepressed subject, resulting in more accurate judgments of personal control. No explanation was offered for depressives' illusion of control in the observer condition. Thus, these researchers suggested that in order to gain an adequate understanding of the implications of contingency judgments on the cognitive functioning of depressives and nondepressives, interpersonal as well as intrapersonal variables must be examined.

Newman and Benassi (1989) evaluated contrast effects in judgments of control across tasks. They found that subjects who were initially presented with a high contingency task made lower judgments of control on a subsequent moderate contingency task than subjects who were given a noncontingent task followed by one with moderate contingency. These results offered evidence that the same response-outcome contingencies may be judged differently in different contexts, i.e., judgments of the same medium control trials were significantly different depending on the initial context. This conclusion strongly suggests that order effects could bear an impact on how multi-task judgment of control results are interpreted. However, review of Newman and Benassi's (1989) methodology reveals that they confounded level of contingency with frequency of reinforcement, since their high contingent, moderate contingent, and noncontingent tasks offered high (70-0), moderate (55-10), and low reinforcement rates (35-35), respectively. It is therefore difficult to determine whether subjects were responding to objective control or to frequency of reinforcement in their contrast effects. Indeed, a number of other studies may have inappropriately attributed results to differing control levels, when frequency of reinforcement actually may have influenced or even accounted for the results. Other studies which may have confounded control level with frequency of reinforcement include Alloy and Abramson's (1979) experiment 4 (75-25); Ford and Neale (1985) (75-25); and Vazquez's (1987) experiment 1 (50-25).

In sum, recent research has identified boundary conditions for the depressive realism phenomenon by describing specific situations in which mildly depressed individuals display overestimations of control. Depressives' accuracy appears specific to self (Martin et al., 1984) and to private, but not public situations (Benassi & Mahler, 1985). These results therefore suggest that mild depressives are not invulnerable to inaccuracy in their judgments of control. Further, by suggesting situational specificity for the accuracy of both depressives' and nondepressives' judgments of control, these studies do not support the claims of the Beck and learned helplessness/hopelessness models, which predict consistent negative distortion by depressives across situations.

<u>Weaknesses of Alloy and Abramson findings</u>. Several problems emerge upon reviewing Alloy and Abramson's arguments. The first regards their interpretation of depressives' low self-esteem. Individuals can behave consistently with their self-esteem, or they can seek to enhance it (Brockner, 1983; Maddi, 1980; Wortman & Brehm, 1975). Both of these models explain nondepressives' illusion of control but fail to

adequately explain why depressives prefer to maintain their low self-esteem rather than strive to enhance it. Further studies seem necessary to substantiate Alloy and Abramson's rationale for depressives' accuracy in judging contingencies.

Second, Alloy and Abramson did not employ an expectancy measure or explore the impact of expectancy on judgment of contingency estimates made by subjects. While they apparently assumed that subjects' judgment of control scores measured actual perception of control, these scores could actually have reflected the combined effects of subjects' pre-task expectancies of control in addition to their actual perceptions of control. In other words, Alloy and Abramson failed to separate out the factors contributing to the production of an accurate judgment of control. In this regard, two separate processes could lead to an accurate judgment of control: producing an expectancy of control estimate that accurately matches the actual degree of contingency, or actually perceiving the contingency accurately, independently of expectancy. Expectancy of control has been found to correlate highly with judgment of control (Tang, 1987). By failing to directly evaluate subjects' expectancy of control, Alloy and Abramson were unable to determine whether depressives were "sadder but wiser" in having more accurate expectancies or in perceiving contingencies more accurately. Sampling subjects' expectancy scores should additionally allow a more direct test of the hopelessness theory, since it maintains that depressives hold negative expectations about the occurrence of highly valued outcomes and their ability to change the likelihood of those outcomes.

The present investigation represents an improvement over the design of Alloy and Abramson by separately evaluating both expectancy and judgment of contingency in order to assess the influence of subjects' pre-task expectancies on post-task judgments of control estimates. While subjects will be offered cash incentives for light onset on Tasks 1 and 2, they also will be rewarded for accuracy of judgment of control on Tasks 3 through 5, to determine whether depressives and nondepressives respond differently in self-correcting their estimates of control when a monetary contingency is placed on accuracy. Further, the cognitive processes determining how subjects produce accurate or inaccurate judgments of control will be examined. A particular emphasis will be placed on discriminating whether subjects become more accurate in their judgments of control across tasks because of their accurate expectancies or because they more accurately perceive the actual levels of contingency, independently of expectancy.

Further, both mildly depressed and nondepressed college students have been found to produce initial expectancy of control estimates of near 50% (Tang, 1987). To adequately evaluate the relationship between expectancy scores and judgment of control scores within clinically depressed and nondepressed groups, levels of actual control will be varied from 0% to 80% across five tasks. In this way, the possible coincidental match between subjects' pre-task expectancies of control and their post-task judgments of control can be evaluated to determine whether subjects produce more accurate judgments of control when actual control is near 50% than on tasks with very high or very low levels of actual control.

Fourth, while several studies examining other cognitive variables have yielded findings which generally support Alloy and Abramson's position (e.g., Golin et al., 1977, 1979; Lewinsohn et al., 1980; Lobitz & Post, 1979), the results are by no means unanimous. A study by Bryson, Doan and Pasquali (1984) questioned Alloy and Abramson's conclusions that depressives are "sadder but wiser." These authors contended that depressives are not "wiser," since depressed subjects in their study overestimated control as much as nondepressed subjects did in both high and low frequency of reinforcement situations. In a replication of Alloy and Abramson's (1979) second experiment, Bryson et al. (1984) rejected Alloy and Abramson's interpretation that overestimations of control reflect a self-serving bias. Overestimates of control in the high frequency, zero control condition were interpreted as resulting from selective inattention to or poor recall of how often the green light appeared when no button press responses were made. However, comparable overestimates by both groups also occurred in the low frequency condition, and were not seen as reflecting a self-serving bias, since students in the low frequency condition were critical of their own performance.

The Bryson et al. (1984) study yielded no evidence that mood influences judgment of control in noncontingent learning tasks. Contrary to the findings of Alloy and Abramson (1979), these authors concluded that mild depression does little to eliminate people's natural biases toward overestimating control when responses and outcomes are noncontingent. However, these findings are at odds with the majority of studies evaluating the judgment of contingency phenomenon, as documented in Alloy and Abramson's (1988) recent literature review. Little empirical support is currently available to substantiate the Bryson et al. (1984) claims, while considerable support for Alloy and Abramson's (1979) claims has been gathered.

Fifth, perhaps the most critical caution pertaining to the findings of Alloy and Abramson concerns the issue of generalizability. Subjects utilized in each of their recent studies (Abramson et al., 1981; Alloy & Abramson, 1979, 1982; Alloy et al., 1981; Martin et al., 1984) were described as mild depressives and drawn from a population of college undergraduates. Indeed, all studies published to date which have investigated judgment of contingency have used student samples rather than clinically depressed individuals. Three implications emerge from this fact. First, the need to investigate the cognitive functioning of clinical depressives becomes increasingly important. Second, drawing inferences to the cognitive functioning of clinical depressives based upon the experimental behavior of college students must be regarded as questionable. Third, the implications of Alloy and Abramson's (1979) findings for the theories of Beck, learned helplessness, and hopelessness models remain unclear, or at least incomplete, without a study investigating judgment of control patterns among clinical depressives.

The assertion has frequently been made that mild depressive states in individuals drawn from a nonclinical population represent the low end of a continuum of severity, with clinical depression at the other pole. In this view, the two differ quantitatively, but not qualitatively (Coyne & Gotlib, 1983). Other researchers have strongly cautioned against such claims, warning that the two phenomena may differ in kind and not just degree (e.g., Arieti & Bemporad, 1978; Depue & Monroe, 1978; Miller, 1975; Seligman, 1978). Depue and Monroe (1978) reviewed several studies which suggested that although the mild depression found among generally normal subject samples may be manifested in the subjective mood and cognitive features similar to that of clinical depressives, mild depressives lack the overt behaviors, somaticized anxiety, and physical complaints of clinical patients. For example, Golin and Hartz (1979) failed to identify among mildly depressed college students the systematic somatic disturbance factor previously identified among psychiatric inpatients (Weckowitz, Muir, & Cropley, 1967).

At this point, the question of continuity from the subclinically, mildly depressed to the clinically depressed remains unresolved. According to Alloy and Abramson's theoretical viewpoint, as depression increases in severity, self-esteem should decrease, resulting in reduced motives to enhance self-esteem. Following this line of reasoning, severely depressed individuals should perceive contingencies even more accurately than mildly depressed. Obviously, no studies have demonstrated this trend, and from a clinical standpoint, it seems unlikely. This issue underscores the need for judgment of contingency studies using clinical psychiatric subjects.

Desirability of Control

Many theories include implicit or explicit references to the motive to control the events of one's life. Indeed, several researchers have positioned the concept of control as the centerpiece of their theories (Abramson et al., 1978; Beck, 1974, 1976, 1979; Brehm, 1966; Seligman, 1975; White, 1959; Wortman & Brehm, 1975). Burger and his colleagues (Burger, 1984, 1985, 1986, 1987; Burger & Arkin, 1980; Burger & Cooper, 1979; Burger & Schnerring, 1982) have extended the notion of personal control to examine desirablity of control.

Although the specific level of control motivation obviously varies across situations, these researchers have asserted that there exists a stable underlying motive for control which can be measured.

Burger differentiated between two types of individuals. High desire for control (DC) individuals are described as assertive, active, and decisive. They seek to influence others and manipulate events to produce desired outcomes. Low DC individuals are seen as more passive, less assertive, and indecisive. They prefer to forfeit decision-making to others and are less prone to attempt to influence others' behaviors or decisions.

At first sight, the desire for control construct appears quite similar to perceived locus of control (Levenson, 1974; Rotter; 1966). While locus of control describes the degree to which an individual believes he or she exerts control over events, the notion of desire for control examines how attractive such control is to the individual. Although similar, these traits have been demonstrated to be statistically independent (Burger & Cooper, 1979).

One area of research that bears an intriguing relationship to the desire for control is the notion of the "illusion of control," the tendency to assume personal control over positive outcomes that are actually chance-determined (Langer, 1975; Langer & Roth, 1975; Strickland, Lewicki, & Katz, 1966; Wortman, 1975). This illusion of control can be seen as a function of people's drive to control the environment in a self-enhancing manner, and their inflated, distorted perceptions of personal control lead to an enhanced sense of confidence for future performances (Langer, 1975).

Kelley (1971) has reasoned that behind the illusion of control lies the motivation to obtain control over situations. The strength or intensity of this desire for control should then interact differentially with situational variables to create the perception of control over outcomes which may be chance-determined.

Following this line of reasoning, Burger (Burger & Cooper, 1979) found that high DC individuals displayed the illusion of control phenomenon to a greater extent than those low in the desire for control in a dice throwing experimental task. Stated differently, high DC individuals are more susceptible to evidencing an illusion of control, in that they are so motivated to see themselves as in control that they are easily fooled into perceiving control (Burger, 1986). Those who display relatively low control motivation appear able to evaluate chance-determined tasks more realistically and accurately (Wolfgang, Zenker, & Viscusi, 1984).

In sum, if the difference between high and low DC individuals is a reflection of their motivation to control events, then the differences in the susceptibility of each of these groups to the illusion of control can be viewed as evidence that the illusion of control results from motivated distortion. That is, high DC individuals succumb to this illusion to satisfy their drive to perceive themselves as in control. The stronger the motive for control, the more likely the individual will distort perceptions to satisfy this motivation (Burger, 1986).

Burger and Schnerring (1982) found that high DC subjects displayed a greater illusion of control than those low in desire for control on an experimental gambling task, but only when winnings could be traded for extrinsic rewards (prizes). This finding corroborated Burger and Cooper's (1979) earlier finding that differences between high and low DC individuals will be most apparent when there are distinct advantages in controlling events.

In some respects, these findings parallel those of Alloy and Abramson (1979), who discovered that nondepressed individuals, like Burger's high DC subjects, displayed an illusion of control in experimental situations with chance-determined outcomes and monetary consequences. Burger's finding of motivated distortion among high DC individuals, where their drive to perceive themselves as being in control leads to an illusion of control, parallels Alloy and Abramson's discussion of a self-enhancing bias among nondepressives. This distortion is self-enhancing in that, on noncontingent experimental tasks, nondepressives assume credit or control for positive outcomes, yet deny responsibility for negative outcomes. In other words, subjects in both studies distorted their perceptions by assuming that they had personal control when little or none was actually present, presumably to satisfy their desire to view themselves in positive terms.

Desirability of control and depression. Desirability of control has also been related to vulnerability to depression. In a study of learned helplessness effects, Burger and Arkin (1980) exposed high and low DC subjects to helplessness training with aversive noise blasts. Subjects then examined a word list for two minutes and were asked to recall as many words as possible. While high and low DC subjects in the no-noise conditions did not differ in their recall performances, high DC individuals in the pre-task noise conditions committed more errors than low DC individuals. Burger and Arkin concluded that individuals demonstrating a high desire for control displayed a greater reaction to the aversive noise blasts than low DC subjects, since it prevented them from controlling the situational events. Apparently, those who desire little control are less likely than those who desire high control to suffer impaired performances when they cannot control aversive stimuli (Burger & Arkin, 1980; Burger & Cooper, 1979). These findings are consistent with Wortman and Brehm's (1975) suggestion that individuals react more strongly to a lack of perceived control when the importance of that control is high.

Previous research has investigated the potential relationship between locus of control and measures of depression. However, studies generally have identified only a small and somewhat inconsistent pattern of correlation between the two, with a tendency for depression to be correlated with an external locus of control (see Rehm & O'Hara, 1979, for a review). Since Rotter's (1966) concept of external locus of control has been shown in factor-analytic studies to measure more than one construct, it remains unclear whether an external score denotes a belief that chance or powerful others or a combination of both is responsible for the individual's outcome (Cherlin & Borque, 1974; Mirels, 1970). More recently, Burger (1984) found some support for the notion that individuals high in the desire to control events, coupled with a general external perceived locus of control, may be more vulnerable to depression due to their perception that the higher levels of control they desire lie outside their reach.

Again, it is of interest to note the relationship between the findings of Burger and his colleagues and those of Alloy and Abramson. According to Burger, high DC individuals appear more susceptible to developing an illusion of control (Burger, 1985, 1986; Burger & Cooper, 1979; Burger & Schnerring, 1982), learned helplesness effects (Burger, 1984; Burger & Arkin, 1980), and depressed affect (Burger, 1984; Burger & Arkin, 1980). Overall, they are seen as more attuned than low DC individuals to environmental cues which might suggest the opportunity to control situational outcomes. In success situations, especially when extrinsic rewards are involved, high DC individuals maintain their illusion of control. But when thwarted, their illusions are disconfirmed and their more highly reactive style leaves them vulnerable to learned helplessness effects and depressed affect.

More recent research (Burger, 1989) has indicated that there may even be conditions under which increases in perceived personal control result in a tendency to relinquish that control, experience negative affect, and evidence poor performance on subsequent tasks. These changes are thought to result from three basic sources. First, increased self-perceived control is accompanied by an increase in one's concern about evaluations by others, i.e., the person with the most influence in a situation is also most likely to be held responsible for the outcome. Taking on a leadership role places the leader in a position to accept responsibility for others' (good or bad) performances. Second, past experiences of success do not always lead to expectations of future success. Acknowledging personal ability limitations influences the decision to retain or relinquish control over future events. Third, Burger contends that changes in perceived predictability over future events may lead to anxiety and the decision to relinquish personal control. Burger contends that high DC individuals, due to their consistent attempts to maintain control, may be vulnerable to experiencing negative reactions to increases in perceived personal control (Burger, 1989).

Alloy and Abramson (1979) likewise asserted that nondepressives distort contingency data by overestimating personal control over situational outcomes. The illusion of control concept they utilized is similar to that described by Burger and his colleagues and derives from the same base in the literature (e.g., Langer, 1975; Langer & Roth, 1975; Wortman & Brehm, 1975). Additionally, nondepressives' desire to enhance personal self-esteem by overestimating control for successes and externalizing blame for failures resembles Burger's claim that high DC individuals overestimate successes to maximize their perceptions of personal control.

The relationship between these two variables invites empirical investigation of the possible interactions among mood and desire for control on perception of environmental contingencies. Among nondepressed individuals (who, according to Alloy and Abramson, display an illusion of control), high DC individuals should evidence greater illusion of control than low DC nondepressives. Among depressed individuals, high DC individuals should exhibit more severe depression than low DC depressives, since they more strongly desire personal control but, as indicated by their lowered expectations of control (Golin et al., 1977, 1979), perceive that control as unavailable to them. In other words, high DC individuals should display the more extreme characteristics among individuals comprising both the depressed and nondepressed groups.

Self-esteem

Self-esteem has been defined as an individuals's characteristic self-evaluation (Wylie, 1961), encompassing one's aspirations and values (James, 1890) and the reflected appraisals of others (Mead, 1934). It has been described as developing from adequate parental attention and concern (Rosenberg, 1965). Coopersmith (1967) offered four central determining factors. These include: the amount of respect, acceptance, and concern received from significant others; a history of success and status achieved by the individual; whether one achieves goals or aspirations in personal significant areas; and the individual's characteristic manner of responding to devaluation.

Self-esteem and depression: Alloy and Abramson (1979) have proposed that depressives lack the motive to enhance self-esteem, which accounts for their accuracy in judgment of control. Indeed, several studies have demonstrated a relationship between depression and low self-esteem (Abramson et al., 1978; Bebring, 1953; Beck, 1967, 1974, 1976; Zemore & Bretell, 1983). Feelings of personal dissatisfaction and inadequacy have frequently been attributed both to depressives and to individuals with low self-esteem. Each also typically endorses the similar attributional style of using excessive self-blame and blaming failure on internal causes (Beck, 1974, 1976, 1979; Shikanai, 1983). A causal relationship between low self-esteem and depression, however, is difficult to delineate. The question of whether feelings of inadequacy induce depression, or vice versa, remains unanswered. Another possibility is that depression and low self-esteem are different names for the same phenomenon, or names for the cognitive and emotive components of that phenomenon.

Self-esteem and task performance: The role of self-esteem in mediating performance following failure has been demonstrated by Brockner (1983). Students were presented a task subsequent to an experimentally-induced experience of brief failure, extended failure, or no failure. Individuals with low self-esteem performed marginally better than those with high self-esteem in the brief failure condition, but performed significantly worse than high self-esteem individuals in the extended failure condition. Brockner claimed that low self-esteem people's improved performance after a small amount of failure was due to their initially strong motivation to overcome that failure, while their debilitated performance following extended failure reflected learned helplessness (Wortman & Brehm, 1975). Conversely, the performance pattern of high self-esteem individuals supported Frankel and Snyder's (1978) egotism theory. Egotism theory suggests that a brief amount of failure is not sufficient to threaten high self-esteem individuals, resulting in a lack of increased motivation to improve subsequent task performance. Prolonged failure, however, motivates and mobilizes high self-esteem individuals to protect their self-esteem by enhancing task performance.

To summarize, studies on self-esteem suggest that the drive to enhance self-esteem can be a central determinant of task performance. A strong relationship between low self-esteem and depression has also been supported. Alloy and Abramson (1979) asserted that depressives do not display a strong motive for self-enhancement. That is, they do not demonstrate an illusion of control in success or excessive external blame in failure, and they are consistently accurate in their judgments. Finally, Brockner's (1983) finding that low self-esteem individuals perform better following brief, but not extended failure, appears somewhat discrepent with Alloy and Abramson's claim that depressives lack the motive to enhance self-esteem. While low self-esteem individuals may initially strive to improve their performances after brief failures, they may in general demonstrate relatively less motivation for self-esteem enhancement than nondepressives.

<u>Summary</u>

This review of the literature suggests that depressives do not suffer comprehensive cognitive distortion in the manner suggested by the two major cognitive theories of depression. While empirical evidence supports the presence of certain cognitive deficits among depressives (as compared to nondepressives), such as a negative self-evaluative style (Lobitz & Post, 1979; Loeb et al., 1971; Nelson & Craighead, 1977; Rehm et al., 1977) and a self-derogatory attributional style (Hamilton & Abramson, 1983), their functioning is in some ways more accurate than that of nondepressives. In particular, their judgments of contingency appear accurate across both contingent and noncontingent situations, while nondepressives exaggerate personal control over positive outcomes and minimize control over negative outcomes. Nondepressives' greater motive to enhance self-esteem has been hypothesized to explain the differential accuracy of the two groups. While recent studies have identified public versus private (Benassi & Mahler, 1985) and self versus other (Martin et al., 1984) boundary conditions for depressive realism, the broad body of research on judgment of contingency offers strong support for the accuracy of depressives relative to nondepressives.

Purposes of Present Study

This study utilizes an Alloy and Abramson (1979) type of task to examine the cognitions of clinically depresssed psychiatric inpatients. It represents the first empirical effort to investigate judgment of contingency in a clinical sample. This study will compare the response patterns of clinically depressed psychiatric patients to those of clinically nondepressed psychiatric inpatient controls and nondepressed normal controls.

The psychological literature has produced contradictory findings regarding the breadth and depth of cognitive distortion among the depressed. It is possible that depressed individuals may evidence distortions along certain dimensions of cognitive functioning, but maintain accurate functioning in other areas (e.g., judgment of contingency). This study seeks to empirically delineate specific areas of distortion by the administration of a comprehensive cognitive assessment with each task. The particular variables investigated are expectancy of control, judgment of control, evaluation of performance, and attribution. Alloy and Abramson's (1979) hypothesis that depressives lack the motive to enhance self-esteem will also be investigated.

An additional goal of the study involves the determination of whether nondepressives' illusion of control is maintained when a contingency is placed on accuracy. Outside of the experimental situation, adopting an "illusion of control" could prove adaptive unless tangible consequences exist for inaccuracy, since an increase in self-perceived personal agency may boost mood and self-esteem and lead to greater productivity. Finally, the relationship between desirability of control and accuracy in perceiving environmental contingencies will be examined to determine whether a high desire for control contributes to reduced accuracy in judging contingencies.

Hypotheses

(1) Depressives will set comparatively high outcome expectancies but demonstrate low efficacy expectancies in situations where they lose money for the light not appearing. Nondepressives, however, will show comparatively high efficacy expectancies, but low outcome expectancies, in this situation. Depressives typically view themselves as inadequate and worthless (low efficacy expectations) following negative outcomes (Abramson et al., 1978; Beck, 1974). They believe that responses leading to desired outcomes exist, but are not available in their behavioral repertoire (high outcome expectations). This expectancy pattern will be more pronounced for depressed subjects with a high desire for control. Conversely, nondepressives experiencing negative outcomes persist in viewing themselves as capable of producing the most effective responses available to them (high efficacy expectations), but attribute negative outcomes to factors unrelated to these responses (low outcome expectations). Nondepressives thereby protect self-esteem by blaming aversive experiences on external factors, assuming a lack of personal control (Bradley, 1978; Frankel & Snyder, 1978). High DC nondepressed normals will

display this pattern to a greater extent than low DC nondepressives.

(2) When there is a monetary contingency on light onset but not on accuracy of judgment of control, a higher proportion of depressives than nondepressives will demonstrate underestimations of control, while a higher proportion of nondepressives than depressives will display overestimations of control. Additionally, depressives will underestimate control in reward relative to the objective degree of contingency, while nondepressives will overestimate control when there is a monetary contingency on light onset but not on accuracy of judgment. No specific hypothesis is made regarding group differences in the punishment condition when there is no monetary contingency for accuracy. Similar to findings of previous studies which employed psychiatric controls (Derry & Kuiper, 1981; Gotlib, 1982; Hamilton & Abramson, 1983; Lewinsohn et al., 1980; Raps et al., 1982), clinically nondepressed psychiatric subjects will exhibit response patterns similar to those of nondepressed normals.

(3) When there is no monetary contingency for accuracy of judgment, high DC subjects will evidence greater illusions of control in reward conditions and greater underestimations of control in punishment conditions than low DC subjects. This effect will be strongest for high DC nondepressives and weakest for low DC depressives.

(4) Nondepressed normal subjects will self-correct their judgments of control, becoming more accurate across tasks on which they are rewarded for accuracy or punished for inaccuracy. That is, they will correct their illusion of control in positive outcomes and their underestimation of control in negative outcomes, in order to enhance self-esteem by achieving maximal gain and minimal loss. This increase in accuracy will be greater among high DC nondepressives than low DC nondepressives, since research has suggested that low DC individuals are less susceptible to developing the illusion of control effect in the first place (Burger, 1986). Finally, depressed subjects should demonstrate smaller increases in accuracy than both nondepressed groups. Previous studies indicate that depressives are sometimes unresponsive to environmental changes (Beck, 1967, 1974, 1976, 1979), and are therefore less affected by external reinforcement and punishment than nondepressives. Thus, their judgments of control should remain fairly constant at a low level across conditions (Ruehlman et al., 1985).

(5) Compared to the two nondepressed groups, depressed subjects will evidence greater stability and internality in their causal attributions when they lose money for the light not appearing. Beck's model and the reformulated learned helplessnes model claim that depressives tend to assume personal responsibility for negative outcomes and attribute those outcomes to factors involving personal inadequacies.

(6) Both a strong and a weak prediction are made regarding self-esteem. According to the strong prediction, as first suggested by Alloy and Abramson (1979), depressives should assume more blame when losing money for lack of light onset than they will take credit for gaining money for light onset. Such a pattern would suggest that depressives lack the motive to enhance self-esteem. According to the weak prediction, depressives should assume more blame when losing money for lack of light onset than will nondepressives, and they should claim less credit for gaining money for light onset than will nondepressives. In other words, depressives will demonstrate less motive to enhance self-esteem than will nondepressives.

(7) Depressives will set a higher criterion for successful performance in making the light appear than will the two nondepressed groups, and this criterion will be highest for high DC depressives and lowest for low DC nondepressed normals.

CHAPTER II

METHOD

Subjects

Twenty-four psychiatric staff, 24 depressed inpatients, and 24 nondepressed schizophrenic inpatients at Terrell State Hospital in Terrell, Texas, were selected to participate in the experiment. Inclusion in the clinically depressed group was based on the following criteria: (a) a primary psychiatric diagnosis, as indicated in case files, of depressive disorder; (b) duration of current depressive episode less than two (2) years; (c) a score of 16 or greater on the Beck Depression Inventory (BDI; Beck, Ward, Mendelson, Mock, & Erbaugh, 1961); (d) no evidence of organicity; (e) no evidence of bipolor disorder; (f) no electroconvulsive therapy within the past six (6) months.

Individuals comprising the nondepressed schizophrenic group were classified as follows: (a) a primary psychiatric diagnosis of schizophrenia, with no accompanying diagnosis or previous diagnosis of a depressive disorder; (b) a score of less than or equal to 13 on the BDI; (c) no evidence of organicity; (d) no electroconvulsive therapy within the previous six (6) months.

Thirty-three males and 39 females ranging in age from 19 to 64 years (*M* = 35.13, *SD* = 11.53, median = 33) were selected as subjects. The majority (64%) were Caucasian, (31%) were Black, and the rest (4%) were Mexican and Oriental. Equivalent racial ratios were maintained within each mood group: depressives (69% White, 25% Black, 6% Mexican and Oriental); schizophrenics (63% White, 33% Black, 4% Mexican and Oriental); nondepressed normals (61% White, 37% Black, 2% Mexican and Oriental). Equivalent sex ratios were also maintained across mood groups: depressives (58% women, 42% men); schizophrenics (65% women, 35% men); nondepressed normals (65% women, 35% men). All participants were literate and English-speaking. Subjects received cash earnings for their voluntary participation. Severity of psychopathology among inpatient subjects was controlled by matching the scores of depressed and nondepressed psychiatric inpatients on the Brief Psychiatric Rating Scale (BPRS; Overall & Gorham, 1962). All inpatient subjects were receiving psychotropic medications.

Nondepressed controls consisted of English-speaking psychiatric staff with no history of depression or schizophrenia, who achieved a BDI score of less than 9.

Instruments

Depression. Intensity of depression was evaluated by the Beck Depression Inventory (BDI; Beck, Ward, Mendelson, Mock, & Erbaugh, 1961). Each of the inventory's 21 items describes a specific behavioral manifestation of depressive pathology. Values from 0 to 3 are assigned to each statement to indicate level of severity. The composite score is obtained by summing the numerical values associated with each endorsed item.

Internal consistency of the BDI has been assessed by two methods. First, Kruskal-Wallis non-parametric analysis of variance by ranks indicates that each of the categories bears a significant relationship to the total scale (all *ps* $< .001$, except item S, $p < .01$). Second, split-half reliability has been calculated by a Pearson correlation coefficient between odd and even categories, producing corrected reliability coefficients of .93. Clinical ratings obtained from psychiatrists over a period of several weeks have been shown to parallel changes in BDI scores, attesting to the stability of the inventory.

Numerous studies (e.g., Bumberry, Oliver, & McClure, 1978; Davis, Burrows, & Poynton, 1975; Hammen, 1980) have evaluated the validity of the BDI and yielded satisfactory results. Correlations obtained from these studies between BDI scores and clinical ratings of depression severity have ranged from .60 to .77 (Beck, 1967; Bumberry et al., 1978; Metcalfe & Goldman, 1965). Moreover, the BDI has been described as a reliable discriminator between depression and anxiety (Beck, 1976). Roughly following the cut-off points used by Beck et al. (1961), depressed psychiatric patients scoring 16 or above were assigned to the depressed group, while psychiatric staff scoring below 9 were placed in the nondepressed normal control group. In order to prevent the nondepressed schizophrenic group from being comprised of individuals who are untypically free of depressive symptomatology, those schizophrenics who scored 13 or less on the BDI and met the other inclusion criteria were included in this group. Gotlib (1982) found that psychiatric patients, regardless of clinical diagnosis, tended to be somewhat more depressed than normal subjects, even though their group mean BDI score still lay within the nondepressed range (i.e., < 9). This effect was attributed primarily to hospitalization effects. Several studies (e.g., Hamilton & Abramson, 1983; MacDonald & Kuiper, 1984, 1985) have followed this reasoning by employing slightly higher BDI cut-off scores for nondepressed psychiatric controls than nondepressed normals. That is, requiring that nondepressed psychiatric subjects score as low on the BDI as nondepressed normals could inadvertently unmatch the two groups on resistance to depression. This higher BDI cut-off allows the nondepressed psychiatric controls to more closely represent the population from which they are drawn, while still producing a group mean within the nondepressed range (i.e., < 9). Therefore, while it is conceded that the schizophrenic controls used here may not be totally free from depressive pathology, the use of these inclusion criteria enables them to be considered clinically nondepressed.

Desirability of control: This construct was measured by the Desirability of Control Scale (DC Scale; Burger & Cooper, 1979), a 20-item scale utilizing a 7-point Likert format, anchored by "This statement doesn't apply to me at all" (1) and "This statement always applies to me" (7). Approximately one-third of the items were designed so that a response at the low end of the scale indicates a high desire for control.

The internal consistency of the scale using the Kuder-Richardson formula 20 has been shown to be .81. Correlations of individual items with the total DC Scale score have ranged from .31 to .66 (Burger & Cooper, 1979). Test-retest (six-week interval) reliability of the scale was found to be satisfactory (r = .75). Five factors were found to account for 50.4% of the original scale variation: a General Desire for Control factor; a Decisiveness factor; a Preparation-Prevention Control factor; an Avoidance of Dependence factor; and a Leadership factor.

To gauge the validity of the DC Scale, it was necessary to distinguish between the desire for control and other similar constructs. A weak negative relationship (r = -.19) was found with the Rotter Internal-External Locus of Control Scale (Rotter, 1966). This result suggests that while people who perceive events to be caused internally also display a slight tendency to desire control over events, the two scales appear to measure different personality constructs (Burger & Cooper, 1979).

To test for possible responding from a socially desirable set, DC Scale scores were correlated with the Marlowe-Crowne Social Desirability Scale (Crowne & Marlowe, 1960). The low positive correlation found (r = .11) suggests that those reporting a desire for control are not merely responding in a socially desirable manner (Burger & Cooper, 1979).

General level of psychopathology. General level of intensity of psychopathology was evaluated by the Brief Psychiatric Rating Scale (BPRS; Overall & Gorham, 1962). The scale, originally developed to provide a rapid assessment technique for psychiatric inpatients, includes an evaluation in a somewhat standardized 18 to 20 minute interview format, of sixteen specifically defined areas of patient functioning. These areas, which the clinician evaluates using 7-point category rating scales, range from somatic concerns and anxiety to unusual thought content and blunted affect. A "total pathology" score is then obtained by summing the ratings of the 16 scales.

Reliabilities of combined ratings of the 16 BPRS scales by two independent raters have been found to range from .56 for the Tension Scale to .87 for both the Guilt Feelings Scale and the Hallucinatory behavior Scale. Reliability scores for ten of the sixteen scales have been found to be .80 or greater (Overall & Gorham, 1962).

<u>Self-esteem</u>. Self-esteem was evaluated by the New York State Self-Esteem Scale, also referred to as the Rosenberg Self-Esteem Scale (RSE; Rosenberg, 1965, 1979). This 10-item scale employs a 7-point Likert format, ranging from 1 ('Strongly Agree') to 7 ('Strongly Disagree'). High scores denote high self-esteem. Response patterns are alternated irregularly to prevent possible response sets by placing high self-esteem categories on the right end of the continuum on some items and on the left side on others. The scale is based on "contrived items" (Stouffer, Borgatta, Hays, & Henry, 1952): Scale Item I is contrived from the combined responses of items 3, 7, and 9; Scale Item II from items 4 and 5; Scale Items III, IV, and V from individual items 1, 8, and 10, respectively; and Scale Item VI from items 2 and 6.

There is reasonable evidence that the RSE is unidimensional (Crandall, 1973; Rosenberg, 1965, 1979) and an accurate measure of global self-esteem. Coefficient alpha has been reported as .88, suggesting good item consistency. Test-retest reliability over a two-week interval has been shown to be .85 (Silber & Tippett, 1965). Convergent validity of the scale has been demonstrated by a correlation of .60 with the Coopersmith Self-Esteem Inventory (Coopersmith, 1967) and the Self-Rating Scale ($\underline{r}$ = .66; Fleming & Courtney, 1984; Fleming & Watts, 1980).

Additionally, self-esteem scores have been shown to be negatively related to depressive affect (r = -.30) and anxiety (r = -.48), further attesting to the instrument's construct validity. (Note: high self-esteem scores denote high self-esteem).

Mood changes. Comparison of mood before and after the experiment was completed using the Multiple Affect Adjective Check List Today Form (MAACL: Zuckerman & Lubin, 1965), a 132-item adjective checklist used to assess anxiety (21 items), hostility (28 items), and depression (40 items). Forty-three additional adjectives serve as unscored buffer items. The Today Form of the MAACL was created to assess changes in depression, anxiety, and hostility as a function of environmental factors. Reliabilities for odd-even and plus-minus items varied from .17 to .92 (median = .72) for different groups of subjects. Internal consistency estimates were satisfactory (.77, .79, and .84 for anxiety, depression, and hostility, respectively). Test-retest reliabilities over a one week interval ranged from .00 to .40, indicating sensitivity to mood fluctuation (Kelly, 1972).

The MAACL has been demonstrated to reveal meaningful changes in affect for groups of subjects experiencing differing types of stress and stress-alleviating manipulations (Kelly, 1972; Megargee, 1972; Polivy, 1981). Each of the MAACL Today Form subscales has been shown to bear small negative correlations with subscale K of the Minnesota

Multiphasic Personality Inventory, suggesting that subjects were willing to admit socially undesirable feelings as a transient state (Kelly, 1972). Both the anxiety and hostility subscales were significantly related to observer ratings of anxiety and hostility, respectively (Zuckerman, Lubin, & Robin, 1965).

Comprehensive cognitive assessment. To identify and evaluate possible distortions in specific areas of cognitive functioning, a comprehensive cognitive assessment was completed for each subject on each task. Variables evaluated included: expectancy, judgment of control, evaluation of performance, and attribution.

(1) Expectancy. Subjects' expectancy as to how much their responses (pressing or not pressing a button) would control the experimental outcome (onset of green light) was evaluated on a 0 to 100% scale (0% as "no control" and 100% as "complete control"; Alloy & Abramson, 1979). Outcome expectancy was measured by a similar scale which indicated the percentage of time subjects believed the green light would appear if they emitted an optimal sequence of responses. Efficacy expectancy, the likelihood of producing an optimal series of responses, was similarly represented by a percentage from 0 to 100% (0% as "no chance at all" and 100% as "total certainty"). All three scales were administered prior to each task.

(2) Judgment of control. Following each task, subjects estimated their degree of personal control over light onset on a scale ranging from 0 to 100% (0% as "no control" and 100% as "complete control"; Alloy & Abramson, 1979). Subjects also completed a scale to evaluate their estimated maximal control, i.e., the percentage of trials they judged that the light would appear if they made an optimal sequence of responses. Third, the likelihood of emitting the optimal sequence of responses was represented on a scale ranging from 0 to 100% (0% as "no chance at all" and 100% as "total certainty").

(3) Evaluation of performance. Subjects were asked to indicate the degree of control they felt they should obtain for a successful performance on a scale of 0 to 100% (0% as "no control" and 100% as "complete control"). Additionally, subjects in the reward condition estimated the amount of credit they felt they deserved on a scale of 0 to 100% (0% as "no credit given" and 100% as "complete credit given"). Subjects in the punishment condition completed a similar scale measuring amount of blame they deserved (0% as "no blame given" and 100% as "complete blame given").

(4) Attribution. Attributions were monitored by a self-report questionnaire patterned after Weiner, Nierenberg, and Goldstein (1976). Two forms were used (reward and punishment), with the only differences reflecting appropriate word changes. This questionnaire was composed of four 7-

point scales on causal stability (Weiner, 1974) and locus of control (Rotter, 1966). The first scale consisted of two internal attributes of differing stability: 1 as "Tried hard" (unstable) and 7 as "Always good" (stable); question 2 contained two unstable attributes varying along the locus of control dimension: 1 as "Tried hard" (internal) and 7 as "Always lucky" (external); the third question involved two external attributes differing in stability: 1 as "Always lucky" (unstable) and 7 as "Always easy" (stable); and, question 4 offered two stable attributes varying in locus of control: 1 as "Always good" (internal) and 7 as "Always easy" (external). Two separate composite scores were formed by summing scores on questions 1 and 3 for stability, and questions 2 and 4 for locus of control. High scores indicated stability and externality of attribution, respectively.

Post-experiment questionnaires. Following the completion of all experimental tasks, subjects were asked to complete three open-ended questions regarding their overall impressions of the experiment: 1) "What do you think are the purposes or hypotheses of this study?"; 2) "What responses did you feel the experimenter wanted you to make?"; and, 3) "What are the factors affecting light onset?" Finally, subjects were asked to describe the degree to which they were trying to offer accurate estimates of judgment of control, on a scale from 1 ("Did not try hard") to 7 ("Tried very hard").

Apparatus

The apparatus was patterned after that described by Alloy and Abramson (1979). The stimulus presentation device consisted of a black 15.5 cm X 9 cm X 5 cm stand-up platform with one red and one green light on the top of the platform. The response apparatus consisted of a 12.5 cm x 6 cm x 4 cm black box, with a spring-loaded button mounted in the center. The experimenter administered the tasks and recorded the subject's responses while seated out of view, behind the subject. A small Roll-o-dex file was used to display subjects' cash earnings during the study.

Experimental Design

This study was a 3 (Trait mood: Clinically depressed psychiatric inpatient, Clinically nondepressed schizophrenic inpatient, Nondepressed normal) X 2 (Condition: Reward, Punishment) factorial design. Every subject completed five similar tasks, each consisting of 30 trials of choosing to press or to not press a button to illuminate a light. Below, the first number describing each task represents the percentage of trials resulting in green light onset when the subject pressed the button, while the second number signifies the onset percentage when the subject chose not to press. The absolute difference between these two values denotes the subject's actual degree of control over task outcomes (Alloy & Abramson, 1979). For example, in the (80-20) task, the green light appeared on 80% of the trials in which the subject pressed the key, and 20% of the trials in which he or she produced a no-press response. Actual contingency on this task was 60%. Both level of control and overall frequency of light onset for the five tasks were experimentally manipulated. In Task 1 (80-60), subjects had 20% control; in Task 2 (80-20), subjects had 60% control; in Task 3 (80-80), subjects had 0% control; in Task 4 (80-0), subjects had 80% control; and in Task 5 (80-40), subjects had 40% control. Of the four tasks involving some degree of actual control, each subject received two tasks with higher frequency of green light onset for button-pressing and the other two tasks with higher frequency of light onset for the non-press responses.

For each of the five tasks, subjects were instructed that the first 20 trials represented a contingency learning problem in which they were to determine how much control they had over whether the light came on or did not come on. No monetary contingencies were employed for these first 20 trials. For the final 10 trials, subjects were told that while the relationship between their responses and light onset would remain exactly the same as in the first 20 trials, their objective for these 10 trials would be to maximize the number of trials on which the green light appeared, i.e., their job was to turn the green light on.

Reward and punishment comprised the two problem conditions. In the reward condition, subjects were awarded a dime credit for each of the final ten trials in which the green light appeared. Additionally, these subjects were paid $1.00 for producing accurate judgments of control over outcomes, beginning with the third task. Accuracy of judgment was operationally defined as lying within ten percentage points of actual control. Subjects in the reward condition generally earned about four dollars apiece. In the punishment condition, subjects began with ten dollars of credit. Ten cents were deducted from that total for each trial in which the green light did not come on, and $1.00 was deducted for each inaccurate judgment of control on tasks three through five. Subjects in the punishment condition generally lost about four dollars out of their ten dollars initial credit.

Procedure

Screening. Groups of psychiatric staff, and patients diagnosed as depressives or schizophrenics were given written questionnaires to complete. These included: The Beck Depression Inventory (BDI), the Desirability of Control Scale (DC Scale), and a demographic survey. This procedure required approximately one half-hour. Experimental subjects were selected from the screening groups on the basis of their BDI scores. Psychiatric staff scoring less than 9 on the BDI were selected as Nondepressed symptom-free control subjects. Of the depressed patients meeting the selection criteria, those scoring 16 or greater on the BDI were included in the Depressed group. Schizophrenics who met the selection criteria and scored 13 or less on the BDI were chosen as Nondepressed schizophrenic controls. Subjects comprising the two psychiatric groups were also matched on pathology severity, based on their scores on the Brief Psychiatric Rating Scale. Twenty-four depressed, 24 schizophrenic nondepressed, and 24 psychiatric staff nondepressed controls were selected, for a total of 72 subjects. Those selected were used as subjects the same day screening was completed. Individuals were each paid $1.00 for participating in the screening process.

Experiment. Each subject was run individually. After being welcomed by the experimenter and seated at a desk, a written informed consent form was completed and the subject was administered the Rosenberg Self-Esteem Scale (RSE) and the Multiple Affect Adjective Check List (MAACL) Today Form. He/she was then told that the study investigated individual problem-solving skills. The experimenter outlined the experimental procedure by reading written instructions in a booklet. Each subject was required to complete cognitive assessment questionnaires before and after all five tasks.

The experimental apparatus was next presented to the subject, who was instructed that he/she would complete five similar tasks. Two goals were described: learning how to turn the green light on, and learning the amount of control he/she had over whether the light appeared or did not appear.

It was explained that 0% control referred to no control whatsoever and 100% control referred to total control.

Each subject was instructed to produce the button-press response within three seconds after the red light went off, otherwise that trial would counted as a no button-press response. A total of only four possible outcomes for every trial was described: 1) he/she pressed the button and the green light appeared; 2) he/she pressed the button and the green light did not appear; 3) he/she did not press the button and the green light appeared; or 4) he/she did not press the button and the green light did not appear. Subjects were told that it would be to their advantage not to press the button on some trials, since it was vital that they know the results of no-press responses as well as press responses (Alloy & Abramson, 1979).

Subjects were placed randomly in either the reward or punishment condition. Those in the reward condition were given a dime credit every time the green light appeared during the last ten trials of all tasks. Also, subjects judged their degree of control over green light onset for all five tasks. After task 2, they were advised that an additional $1.00 credit could be gained for each accurate estimation of control following tasks 3, 4, and 5. Feedback involving subjects' success at earning $1.00 for accurate judgments was given before a new task was begun.

Ten dollars credit was given to subjects beginning the punishment condition. A dime was deducted from that total each time the green light did not appear during the last ten trials of each task, and an additional $1.00 was removed for each inaccurate judgment of control following tasks 3, 4, and 5.

Similar to Alloy and Abramson (1979), a Roll-o-dex card depicting the subject's current cash earnings was displayed in front of the subject. Following each trial, the card was changed to reflect an increase or decrease in cash earnings, based on whether the light appeared or did not appear. Additionally, the card reflected changes in earnings due to accurate or inaccurate control estimates following tasks 3, 4, and 5. Feedback was given to each subject following the completion of all experimental tasks and questionnaires. Cash earnings were not distributed until all five tasks and the post-experiment questionnaires were completed.

Before beginning the experimental procedures, the subject was asked if there were any questions, and he/she was then asked to sign a consent form for participation in the study. He/she was reassured that all information would be anonymous and confidential. After the form was signed, the experimenter left the subject's view and began the experiment.

From behind the subject, the experimenter illuminated the red light for one second to signify the start of each trial. Five practice trials were administered before beginning the tasks to familiarize each subject with the experimental apparatus. The experimenter then either flashed or did not flash the green light according to the scheduled sequence of green lights and the subject's responses of pressing or not pressing the button. The experimenter then recorded the responses of the subject on each trial.

The experimenter instructed the subject to complete the expectancy scale before each task, and the cognitive assessment following the completion of each task. The assessment questionnaries typically required five to ten minutes to complete.

This procedure was repeated for each of the five tasks. Following their completion, the experimenter administered the Self-Esteem Scale and the Multiple Affect Adjective Check List. Cash earnings were then distributed and the subject was debriefed and asked not to discuss the experiment with other patients. The subject was then thanked for participating and dismissed. The entire procedure required approximately one and one-half hours. All procedures abide by APA guidelines.

CHAPTER III

RESULTS

Results are reported under the subheadings of desirability of control, expectancy of control, judgment of control, evaluation of performance, attribution, mood changes, and self-esteem. The following sequence of statistical analyses was performed on each major dependent variable: (1) a 3 (Mood: Clinically Depressed Inpatient, Clinically Nondepressed Schizophrenic Inpatient, Nondepressed Normal) X 2 (Condition: Reward, Punishment) ANOVA for the five tasks combined; (2) a 3 (Mood: Clinically Depressed Inpatient, Clinically Nondepressed Schizophrenic Inpatient, Nondepressed Normal) X 2 (Condition: Reward, Punishment) X 3 (Task: 3, 4, 5) ANOVA with repeated measures on tasks; (3) a 3 (Mood: Clinically Depressed Inpatient, Clinically Nondepressed Schizophrenic Inpatient, Nondepressed Normal) X 2 (Condition: Reward, Punishment) X 5 (Control: 20, 60, 0, 80, 40) ANOVA with repeated measures on control; and (4) Pearson correlations with major variables.

Main effects for mood and condition were detailed in the first procedure, while self-correction effects due to contingencies on accuracy of judgment of control were examined in the second procedure. Task effects across the five experimental control levels were evaluated in the third procedure, as well as possible interactions between control, mood, and/or condition. Since main effects for mood and condition were detailed in the mood X condition ANOVAs, they will not be reiterated when reporting the control and task ANOVAs.

Neuman-Keuls Tests were performed for all significant main effects involving three or more treatment levels. Post hoc simple main effects analyses were performed for all significant interactions (Winer, 1971).

Tables Q-1 through Q-3 are included in Appendix Q and Figure 1 is included in Appendix P. Table Q-1 presents the means and standard deviations for all major dependent variables combined across the five tasks. Table Q-2 contains the means and standard deviations for the variables in differing degrees of task control. Table Q-3 reports the intercorrelations among major variables. Figure 1 depicts values for simple judgment of control, overall reinforcement, maximal reinforcement frequency and actual control, across all five experimental tasks.

Preliminary analyses were completed to confirm that depressed, schizophrenic, and nondepressed normal subjects differed statistically from each other appropriately. A one-way ANOVA on subjects' Beck Depression Inventory scores revealed a significant between groups effect, $F(2,71) = 84.89$ ($p < .0001$). Neuman-Keuls post hoc analyses indicated that all three mood groups were statistically different from each other (*M*s = 25.71, 8.58, 4.80). A one-way ANOVA on age suggested no significant variation across mood groups (*M*s = 37.39, 35.28, 36.06; $p > .05$). Results of a *t* test on depressives' and schizophrenics' Brief Psychiatric Rating Scale scores yielded nonsignificant differences (*M*s = 55.43, 57.96; $p > .05$), suggesting that these two groups displayed similar levels of pathology. (Nondepressed normals were not administered the Brief Psychiatric Rating Scale.) A one-way ANOVA on level of education produced a between groups effect, $F(2,71) = 5.64$ ($p < .05$). Post hoc analyses revealed that nondepressed normals had completed a signficantly higher number of years' schooling than depressed or schizophrenic subjects (*M*s = 14.64, 12.61, 12.96). All staff controls had at least a high school graduation, while 69% of depressives and 78% of schizophrenics had completed high school. Finally, separate *t* tests revealed that the use of a press versus a no-press response did not differentially affect judgment of control scores (*M*s = 55.03, 54.29; $p > .05$) or effectiveness in achieving light onset (*M*s = 64.93, 64.12; $p > .05$).

Desirability of Control

A 3 X 2 ANOVA on subjects' desirability of control scores yielded nonsignificant results ($p > .05$). Subjects' self-perceived need to control environmental events was not found to systematically vary among mood groups or treatment conditions.

Desirability of control was positively related to post-task self-esteem and deviation from objective criterion for success (rs = .41, .27; ps < .05). This variable was negatively correlated with self-correction scores for nondepressed subjects ($r = -.41$; $p < .05$).

Expectancy of Control

1. Level of expectancy

Expectancy of control was identified as subjects' pre-task estimations of how much control they would have on the next task. A 3 (Mood) X 2 (Condition) ANOVA was completed on these scores. No significant differences among mood groups or treatment conditions were observed ($p > .05$).

A 3 (Mood) X 2 (Condition) X 3 (Task) ANOVA with repeated measures on tasks yielded nonsignificant results ($p > .05$). A 3 (Mood) X 2 (Condition) X 5 (Control) ANOVA with repeated measures on control identified a main effect for control, $F(4, 264) = 2.89$ ($p < .05$). Post hoc analyses revealed that subjects reported lower expectancy of control on Task 1 relative to Tasks 3 and 4 (Ms = 48.10, 58.13, 57.63). Additionally, a 3 (Mood) X 2 (Condition) ANOVA on the difference scores of judgment minus expectancy of control identified nonsignificant differences among mood groups and treatment conditions ($p > .05$).

A Pearson product moment correlation evaluating the relationship between reinforcement frequency on Tasks 1 through 4 and subjects' expectancy estimates on Tasks 2 through 5 produced significant results ($r = .31$, $p < .05$).

Level of expectancy of control was positively correlated with: outcome expectancy, efficacy expectancy, accuracy of judgment of control, judgment of efficacy, evaluation of performance, self-correction, accuracy of maximal control, deviation from objective criterion for success, and frequency of reinforcement (rs = .45, .72, .83, .70, .47, .43, .33, .51, .29, respectively; ps < .05).

2. Outcome expectancy

Outcome expectancy identifies subjects' pre-task estimations of green light onset for optimal responses. A 3 (Mood) X 2 (Condition) ANOVA on this variable failed to identify significant differences between mood groups or conditions (ps > .05). Results from a 3 (Mood) X 2 (Condition) X 3 (Task) ANOVA with repeated measures on tasks produced nonsignificant results ($p > .05$).

A 3 (Mood) X 2 (Condition) X 5 (Control) ANOVA showed a 3-way interaction, $F(8, 264) = 2.82$ ($p < .01$); and a main effect for tasks, $F(4, 264) = 8.90$ ($p < .0001$). The main effect for tasks showed that all subjects held significantly lower outcome expectancies for Task 1 than for each of the other tasks (Ms = 56.51, 72.85, 71.11, 72.90, 69.58). Post hoc analyses of the 3-way interaction revealed two significant findings. First, depressives in the reward condition reported lower outcome expectancies on Task 1 than on all other tasks (*M*s = 43.33, 72.50, 85.00, 77.92, 83.33). Second, nondepressed normal subjects in punishment held lower outcome expectancies on Task 1 than on Tasks 2 and 4 (*M*s = 52.50, 69.12, 73.33). No other significant variations were observed.

Outcome expectancy was positively related to: level of expectancy, efficacy expectancy, accuracy of judgment of control, judgment of efficacy, evaluation of performance, accuracy of maximal control, and deviation from objective criterion for success (*r*s = .45, .60, .41, .53, .33, .77, .57, respectively; *p*s $< .05$).

3. Efficacy expectancy

Efficacy expectancy describes subjects' pre-task estimations of the possibility of making an optimal set of responses. Results from a 3 (Mood) X 2 (Condition) ANOVA on efficacy expectancy were nonsignificant ($p > .05$). A 3 (Mood) X 2 (Condition) X 3 (Task) ANOVA with repeated measures on tasks also yielded nonsignificant results ($p > .05$).

A 3 (Mood) X 2 (Condition) X 5 (Control) ANOVA with repeated measures on control produced a main effect for control, $F(4, 264) = 4.96$ ($p < .001$). Neuman-Keuls post hoc analyses indicated that all subjects had significantly lower efficacy expectancies on Task 1 than on all other experimental tasks (*Ms* = 46.96, 56.83, 55.94, 58.47, 54.29)

A 3 (Mood) X 2 (Condition) ANOVA on the difference scores of efficacy expectancy minus outcome expectancy revealed nonsignificant differences across mood groups or conditions ($p > .05$). Results of a *t* test suggested that this difference score was significantly different from zero for all subjects ($M = -14.09$, $t = -6.84$, $df = 71$, $p < .05$), with subjects reporting higher outcome than efficacy expectancies. A nonsignificant correlation was observed between this difference score and desirability of control ($r = .17$, $p > .05$).

Efficacy expectancy was found to be correlated with: level of expectancy, outcome expectancy, accuracy of judgment of control, judgment of efficacy, evaluation of performance, self-correction, accuracy of maximal control, and deviation from objective criterion for success (*rs* = .72, .60, .72, .82, .46, .44, .40, .59, respectively; *ps* < .01).

Judgment of Control

1. Accuracy of judgment of control without monetary reinforcement for accuracy

This variable was calculated by taking the average of the difference between subjects' judgments of control and actual control for the first two tasks. Small difference scores suggest accuracy of judgment of control, larger positive difference scores suggest overestimations, and larger negative difference scores suggest underestimations. The 3 (Mood) X 2 (Condition) ANOVA completed on this variable did not identify significant differences among mood groups or conditions ($p > .05$).

2. Accuracy of judgment of control with monetary reinforcement for accuracy

A 3 (Mood) X 2 (Condition) X 3 (Task) ANOVA on accuracy of judgment of control scores with repeated measures on tasks produced a main effect for tasks, $F(2, 232) = 474.63$ ($p < .0001$). Neuman-Keuls post hoc analyses of this main effect revealed that subjects' accuracy of judgment of control differed across Tasks 3, 4, and 5 (*Ms* = 63.61, -29.94, 11.86). Considering the possibility that experimentally determined levels of differing actual control across tasks might account for these findings, a 3 (Mood) X 2 (Condition) X 3 (Task) ANOVA was also completed on simple judgment of control scores. Results of this analysis likewise revealed a main effect for tasks, $F(2,132) = 4.78$ ($p < .01$) Neuman-Keuls post-hoc analyses of this main effect indicated that subjects reported greater control on Task 3 (80-80) than on Tasks 4 (80-0) and 5 (80-40) (*Ms* = 63.61, 50.06, 51.86).

3. Overall accuracy of judgment of control

Accuracy of judgment of control was also evaluated across all five tasks combined. 3 (Mood) X 2 (Condition) ANOVAs on simple judgment of control scores and on accuracy of judgment of control scores combined across tasks produced nonsignificant results ($ps > .05$).

A 3 (Mood) X 2 (Condition) X 5 (Control) ANOVA on accuracy of judgment of control, with repeated measures on control, produced a main effect for level of actual control, $F(4, 264) = 224.38$ ($p < .0001$). Results of Neuman-Keuls analyses indicated that subjects' accuracy of judgment of control differed across each of the five experimental tasks (Ms = 35.77, -8.47, 63.61, -29.94, 11.86). A 3 (Mood) X 2 (Condition) X 5 (Control) ANOVA with repeated measures on control was also completed on simple judgment of control scores to determine if subjects' judgments of control varied across tasks when experimentally determined levels of control were not considered. Results of this analysis revealed a main effect for tasks, $F(4, 264) = 5.01$ ($p < .001$). Neuman-Keuls post-hoc analyses suggested that subjects felt they had more control over task outcome on Task 3 (80-80) than on all other tasks (Ms = 55.74, 51.53, 63.61, 50.06, 51.86). Results of t tests confirmed that subjects' judgment of control scores on each of the five experimental tasks differed significantly from actual control levels, (ts = 10.07, -2.44, 19.10, -8.91, 3.71, $df = 71$, $ps < .05$). Subjects were consistently inaccurate in their judgments of control across tasks.

Separate 3 (Mood) X 2 (Condition) X 5 (Control) ANCOVAs on simple judgment of control scores were completed with expectancy scores and cash earned for light onset (frequency of reinforcement) as covariates. Results of both analyses yielded nonsignificant differences (*ps* > .05). Equivalent procedures completed on accuracy of judgment of control scores produced similar results (*ps* > .05). In other words, both the level of control effect on judgment of control accuracy and the task effect on simple judgment of control became nonsignificant when either expectancy of control or frequency of onset was covaried.

Light onset frequency (as measured by cash earned for light onset) was found to be significantly correlated with simple judgment of control scores for subjects in reward (r = .36, p < .05), while the parallel correlation for punishment subjects fell just outside the range of significance (r = .26, p = .09).

A Spearman correlation assessing the relationship between simple judgment of control scores and light onset frequency for each subject and then averaged across subjects revealed a sizable, but still nonsignificant relationship (r = .58, p > .05). Although sizable, this correlation is nonsignificant because of the extremely low degrees of freedom and the resulting .90 correlation required for the .05 statistical significance level. While this correlation may not be entirely reliable, it suggests that the relationship between reinforcement frequency and judgment of control is moderately strong and apparently three times stronger than that between actual control and simple judgment of control ($r = -.31$, $p > .05$). Furthermore, reinforcement frequency was positively correlated with simple judgment of control, while atual control was negatively correlated. Inspection of Figure 1 supports this negative correlation. By accounting for 34% of the variance between judgment of control and reinforcement frequency, the former correlation describes a meaningful relationship between these two variables and provides a secondary source of support for the above mentioned correlation between simple judgment of control scores and frequency of reinforcement for reward subjects.

Positive correlations were also observed between accuracy of judgment of control and: level of expectancy, outcome expectancy, efficacy expectancy, judgment of efficacy, evaluation of performance, self-correction, accuracy of maximal control, cash earned for light onset in reward, and deviation from objective criterion for success. (rs = .83, .41, .72, .74, .46, .52, .33, .36, and .54, respectively; ps < .05).

4. Self-correction

No monetary contingency for accuracy was present when subjects offered judgments of control for the first two tasks. Judgment of control for Tasks 3, 4, and 5 involved a monetary reinforcement for the accuracy of their estimations. Since monetary contingencies for accuracy were first present on Task 3, self-correction was defined as the improvement in the accuracy of subjects' judgments of control on Task 5 relative to Task 3. Self-correction scores were therefore calculated as the difference between the absolute value of accuracy of judgment of control for the third task minus the last task. Positive self-correction scores suggest improved accuracy, while negative scores suggest less accurate judgments on the last task.

A 3 (Mood) X 2 (Condition) ANOVA on the difference between subjects' accuracy of judgment of control scores on Task 3 versus Task 5 yielded nonsignificant results, indicating that self-correction did not vary as a function of mood or treatment condition ($p > .05$). However, the 3 (Mood) X 2 (Condition) X 5 (Control) ANOVA with repeated measures on control evaluating accuracy of judgment of control identified a main effect for control, $F(4, 264) = 224.38$ ($p < .0001$), with Neuman-Keuls post hoc analyses revealing that subjects' accuracy differed across each of the five tasks (Ms = 35.77, -8.47, 63.61, -29.94, 11.86). Similarly, the 3 (Mood) X 2 (Condition) X 3 (Task) ANOVA on accuracy of judgment of control with repeated measures on tasks produced a task effect, $F(2,23) = 474.63$ ($p < .0001$). Post hoc analyses of this task effect revealed greater accuracy on Task 5 than Task 3 (Ms = 11.86, 63.61). In sum, while no mood-related differences in self-correction were observed, subjects were found to be more accurate on Task 5 relative to Task 3.

Separate correlations were completed between desirability of control and accuracy of judgment of control on Tasks 3 and 5. Additionally, a correlation was performed to evaluate the relationship between DC and the differential accuracy of control on Task 5 versus Task 3. Each of these correlations produced nonsignificant results ($ps > .05$).

Self-correction was positively related to: level of expectancy, efficacy expectancy, accuracy of judgment of control, judgment of efficacy, deviation from subjective criterion for success, deviation from objective criterion for success, and pre- to post-task changes in hostility (rs = .43, .44, .52, .28, .29, .32, .25, respectively; $ps < .05$). This variable was negatively related to nondepressed subjects' desirability of control scores ($r = -.41$, $p < .05$).

5. Judgment of efficacy

Judgment of efficacy refers to subjects' post-task estimations of the possibility of their having made an optimal set of responses on a given task. A 3 (Mood) X 2 (Condition) ANOVA on judgment of efficacy showed no significant differences ($p > .05$).

A 3 (Mood) X 2 (Condition) X 3 (Task) ANOVA with repeated measures on tasks produced a main effect for tasks, $F(2, 132) = 5.52$ ($p < .01$). Post hoc results indicated that subjects offered higher estimations of the possibility of having made an optimal set of responses on Task 3 than on Tasks 4 and 5 (Ms = 61.85, 51.21, 53.61).

A 3 (Mood) X 2 (Condition) X 5 (Control) ANOVA with repeated measures on control showed a main effect for control, $F(4, 264) = 2.85$ ($p < .05$). Post hoc Neuman-Keuls analyses revealed that all subjects estimated a greater likelihood of making an optimal set of responses on Task 3 than they did on Tasks 2 and 4 (*M*s = 61.85, 53.96, 51.21, respectively).

Judgment of efficacy was correlated with: level of expectancy, outcome expectancy, efficacy expectancy, accuracy of judgment of control, evaluation of performance, self-correction, accuracy of maximal control, and deviation from objective criterion for success (*r*s = .70, .53, .82, .74, .54, .28, .46, .58, respectively; *p*s < .05).

6. Accuracy of maximal reinforcement frequency

Accuracy of maximal control refers to the difference between subjects' post-task estimations of light onset during optimal responses and actual maximal reinforcement frequency, which was fixed at 80 across all five tasks. In calculating accuracy of maximal control scores, small differences represent relative accuracy of estimations, while larger positive differences suggest overestimations, and larger negative differences suggest underestimations.

A 3 (Mood) X 2 (Condition) ANOVA yielded nonsignificant results ($p > .05$). A 3 (Mood) X 2 (Condition) X 3 (Task) ANOVA with repeated measures on tasks showed a main effect for tasks, $F(2, 132) = 3.15$ ($p < .05$). Post hoc analyses of these scores suggested significantly different accuracy on Task 5 versus Task 3. Subjects were relatively accurate on Task 3, while underestimating maximal reinforcement frequency on Task 5 (*M*s = 0.47, -7.67).

A 3 (Mood) X 2 (Condition) X 5 (Control) ANOVA with repeated measures on control produced a group X control two-way interaction, $F(8, 264) = 2.30$ ($p < .05$). Results of post hoc analyses revealed that schizophrenics were less accurate in estimating maximal reinforcement frequency on Task 1 relative to Tasks 3 and 4 (*M*s = -22.46, 3.33, -3.63), and they underestimated maximal reinforcement frequency on Task 1 relative to other mood groups (*M*s = -22.46, -0.62, -0.21).

Accuracy of maximal reinforcement frequency was positively related to: level of expectancy, outcome expectancy, efficacy expectancy, accuracy of judgment of control, judgment of efficacy, evaluation of performance, and deviation from objective criterion for success (*r*s = .33, .77, .40, .33, .46, .36, .50, respectively; *p*s < .01). This variable was negatively correlated with deviation from subjective criterion for success ($r = -.51$; $p < .001$).

Evaluation of Performance

1. Credit/blame deserved

At post-task, subjects indicated the amount of credit they deserved in reward or the amount of blame they deserved in punishment. Since the amount of money earned for light onset in reward was found to differ significantly from that earned in punishment, (Ms = \$3.29, \$5.22, $t = 5.02$, $df = 35$, $p < .01$), this variable was used as a covariate in the following analyses.

A 3 (Mood) X 2 (Condition) ANCOVA on credit/blame scores with cash earned for light onset as the covariate revealed a main effect for condition, $F(1, 65) = 12.14$ ($p < .001$). Subjects in reward gave themselves more credit than those in punishment gave themselves blame for comparable performances (Ms = 64.15, 36.49).

A 3 (Mood) X 2 (Condition) X 3 (Task) ANCOVA with repeated measures on tasks and money earned for light onset as the covariate showed a condition X task two-way interaction, $F(2, 132) = 3.31$ ($p < .05$). Post hoc analyses revealed that subjects in reward felt they deserved more credit for their performance on Task 3 relative to Task 4 (Ms = 71.11, 57.50). No other condition X task results were significant.

A 3 (Mood) X 2 (Condition) X 5 (Control) ANCOVA with repeated measures on control and money earned for light onset as the covariate produced a condition X control two-way interaction, $F(4, 264) = 3.19$ ($p < .05$). Results of post hoc analyses revealed that subjects in reward felt they deserved more credit for their performances on Tasks 1 and 3 relative to Tasks 2 and 4 (Ms = 70.33, 71.11, 56.56, 57.50, respectively).

Credit scores were found to be positively related to: level of expectancy, outcome expectancy, efficacy expectancy, accuracy of judgment of control, judment of efficacy, accuracy of maximal control, and deviation from objective criterion for success (rs = .69, .54, .69, .74, .72, .39, .54, respectively; ps < .05).

Blame scores were positively correlated with: judgment of efficacy, post-task depression scores, and post-task hostility scores (rs = .45, .33, .35, respectively; ps < .05).

2. Deviation from subjective criterion for success

This variable was defined as the difference between subjects' personally determined level of light onset judged necessary for a successful performance and their post-task estimation of light onset during an optimal series of responses, i.e., the difference between a personally satisfying response and the estimated best possible response. Positive difference scores suggest that subjects set a personal criterion for success higher than the amount of control possible. Conversely, negative difference scores indicate a personal criterion for success lower than the estimated maximal control.

The 3 (Mood) X 2 (Condition) ANOVA on this variable did not identify a significant difference between subjects' subjective criteria for success and their estimations of maximal control across mood groups or conditions, nor were their scores significantly different from zero ($ps > .05$). ANOVAs with repeated measures on tasks and control each failed to produce significant results ($ps > .05$). A nonsignificant trend suggested that schizophrenics reported a lower criterion for success relative to estimated maximal control than did depressives and nondepressed normals.

Deviation from subjective criterion for success was positively related to: self-correction and deviation from objective criterion for success ($rs = .28, .49$, respectively; $ps < .05$). Deviation from subjective criterion for success was negatively correlated with accuracy of maximal control ($r = -.51$; $p < .001$).

3. Deviation from objective criterion for success

Deviation from objective criterion for success was defined as the difference between subjects' criteria for success and actual maximal reinforcement frequency (80). Positive difference scores indicate subjects' criterion for success as higher than the actual reinforcement frequency possible, while negative difference scores indicate that their criterion for success was lower than the actual reinforcement frequency possible. A 3 (Mood) X 2 (Condition) ANOVA on this variable yielded nonsignificant differences ($p > .05$), and results of a t test revealed that subjects' scores were not significantly different from actual maximal reinforcement frequency ($ps > .05$). ANOVAs with repeated measures on tasks and control levels likewise did not produce significant differences (*p*s > .05).

Deviation from objective criterion for success was positively correlated with: level of expectancy, outcome expectancy, efficacy expectancy, accuracy of judgment of control, judgment of efficacy, evaluation of performance, self-correction, accuracy of maximal control, deviation from subjective criteria for success, and desirability of control (*r*s = .51, .57, .59, .54, .58, .31, .32, .50, .49, .27, respectively; *p*s < .01).

Attribution

1. Stability

High scores on this variable indicate stability of attribution. The 3 (Mood) X 2 (Condition) ANOVA on stability of attribution showed a main effect for condition, $F(1, 66) = 29.20$ ($p < .001$). Subjects in the punishment condition demonstrated greater stability in their attributions than subjects in reward (*M*s = 8.74, 6.89). ANOVAs evaluating task and control effects each produced nonsignificant results (*p*s > .05).

Stability of attribution was positively related to locus of control and post-task self-report depression scores (*r*s = .77, .27, respectively; *p*s < .05).

2. Locus of control

High scores on this variable suggest externality. A 3 (Mood) X 2 (Condition) ANOVA on locus of control showed a main effect for condition, $F(1, 66) = 10.03$ ($p < .005$). All subjects had a greater external locus of control in punishment than in reward ($Ms = 8.41, 7.27$).

The 3 (Mood) X 2 (Condition) X 3 (Task) ANOVA with repeated measures on tasks produced a main effect for mood group, $F(2, 132) = 3.57$ ($ps < .05$), and a main effect for condition $F(1,66) = 10.03$ ($p < .005$). Subjects in punishment reported a greater external locus of control than those in reward ($Ms = 8.53, 7.31$; Note: these means are based on three tasks rather than five, as above). Additionally, post hoc analyses on the main effect for mood revealed that schizophrenics displayed a more external locus of control than did nondepressed normal subjects ($Ms = 8.56, 7.24$).

A 3 (Mood) X 2 (Condition) X 5 (Control) ANOVA with repeated measures on control revealed a group X control two-way interaction, $F(8, 264 = 3.31$ ($p < .005$). Post hoc analyses revealed that schizophrenic subjects had a higher external locus of control on Tasks 3, 4, and 5 than on the initial task ($Ms = 8.79, 8.54, 8.33, 7.29$, respectively).

Locus of control was related to stability of attribution and pre- to post-task changes in self-esteem ($rs = .77, .25$, respectively; $ps < .05$).

Mood Changes

1. Post-task mood

3 (Mood) X 2 (Condition) ANOVAs were completed on subjects' post-task anxiety, depression, and hostility

scores. Main effects for mood group were found for anxiety and depression scores, $F(2, 66) = 5.34, 3.70$ ($ps < .05$). Post hoc analyses of post-task anxiety scores suggested lower anxiety levels for nondepressed normals than the other two mood groups ($Ms = 50.58, 60.13, 57.13$). Review of post-task depression scores revealed significantly higher self-reported depression for clinically depressed subjects than nondepressed normal subjects ($Ms = 64.04, 53.42$).

Post-task anxiety was positively related to Beck Depression Inventory scores, post-task depression scores, and post-task hostility scores ($rs = .44, .85, .78$, respectively; $ps < .001$). Post-task anxiety scores were negatively correlated with post-task self-esteem scores ($r = -.42$; $p < .001$).

Post-task depression scores were positively related to Beck Depression Inventory scores, post-task anxiety scores, post-task hostility scores, and stability of attribution ($rs = .43, .85, .82, .27$, respectively; $ps < .05$). They were negatively correlated with post-task self-esteem scores ($r = -.41$; $p < .001$).

2. Pre- to Post-task mood changes

This variable is defined as the difference between subjects' mood after and before the experimental procedure. Positive difference scores reveal greater mood intensity post-task. Negative difference scores suggest decreases in the intensity of mood post-task. Results of 3 (Mood) X 2

(Condition) ANOVAs on anxiety, depression, and hostility mood change scores showed main effects for mood group on all three scales, $\underline{F}(2, 66) = 3.87, 3.65, 6.17$ ($\underline{p}$s $< .05$). Post hoc analyses indicated that nondepressed normal subjects experienced increases in anxiety levels during the course of the experimental procedure, while depressed subjects and nondepressed schizophrenics experienced decreases in anxiety ($\underline{M}$s = 4.08, -3.67, -2.50). Nondepressed subjects also experienced an increase in the intensity of depression, while depressed subjects experienced a decrease in the intensity of their depression. Nondepressed schizophrenic subjects' depression levels remained relatively unchanged ($\underline{M}$s = 3.54, -4.42, -0.17). Nondepressed subjects also experienced an increase in hostility during the experiment, while the other two groups underwent decreases in hostility levels ($\underline{M}$s = 4.29, -3.62, -1.04).

Pre- to post-task anxiety change scores were correlated with changes in depression and hostility ($\underline{r}$s = .64, .69; $\underline{p}$s < .001). Depression change scores were related to anxiety and hostility change scores ($\underline{r}$ = .64, .67; $\underline{p}$s < .001). Depression change scores were negatively related to Beck Depression Inventory scores ($\underline{r} = -.30$; $\underline{p} < .05$).

Hostility change scores were positively related to self-correction scores, anxiety change scores, and depression change scores ($\underline{r}$s = .25, .69, .67, respectively; $\underline{p}$s < .05).

They were negatively related to Beck Depression Inventory scores ($r = -.35$; $p < .005$).

Self-esteem

1. Post-task self-esteem

A 3 (Mood) X 2 (Condition) ANOVA was completed to evaluate systematic differences in subjects' post-task self-esteem scores. Results identified a main effect for mood group, $F(2, 66) = 6.10$ ($p < .005$). Nondepressed normal subjects reported higher self-esteem levels than depressed and schizophrenic subjects (Ms = 55.46, 43.54, 48.08). Post-task self-esteem was related to desirability of control, judgment of efficacy, and post- to pre-task difference scores for depression (rs = .41, .25, .24, respectively; ps < .05). Post-task self-esteem was negatively correlated with Beck Depression Inventory scores and post-task anxiety, depression, and hostility (rs = -.47, -.42, -.42, -.39, respectively; ps < .001).

2. Pre- to post-task changes in self-esteem

Self-esteem change scores are identified as the difference between subjects' Rosenberg Self-Esteem scores following and before the experiment. Positive scores suggest increased self-esteem following the experimental procedure, while negative scores suggest decreased self-esteem. All results on a 3 (Mood) X 2 (Condition) ANOVA on self-esteem change scores were nonsignificant (ps > .05).

Changes in self-esteem were related to Beck Depression Inventory scores, post-task hostility, and locus of control (*r*s = .26, .27, .25, respectively; *p*s < .05).

Correlations of other variables

Table Q-3, (Appendix Q) reports the intercorrelations among all major experimental variables. Subjects' scores on the Beck Depression Inventory were correlated with: post-task anxiety, depression, and hostility scores, and pre- to post-task changes in self-esteem (*r*s = .44, .43, .31, .26, respectively; *p*s < .05). Beck scores were negatively related to: post-task self-esteem scores, and pre- to post-task change scores in depression and hostility (*r*s = -.47, -.30, -.35, respectively; *p*s < .05).

Results on specific hypotheses

(1) The first hypothesis was that depressives would demonstrate high outcome expectancy, but low efficacy expectancy in punishment, while nondepressives would show high efficacy expectancy, but low outcome expectancy. Separate ANOVAs were completed on efficacy expectancy, outcome expectancy, and the difference scores of efficacy expectancy minus outcome expectancy. This hypothesis was only partially supported. In contrast to the prediction that only depressives would set higher outcome expectancies than efficacy expectancies, all subjects reported higher outcome expectancies relative to efficacy expectancies across all

experimental tasks (*p* < .05). No significant mood or condition effects were observed among these analyses (*p* > .05). Post hoc analyses of main effects for control levels on these procedures indicated that all subjects reported lower efficacy and outcome expectancies on Task 1 relative to the remaining four tasks (*p* < .0001). Nonsignificant correlations (*ps* > .05) between desirability of control and difference scores between efficacy expectancy minus outcome expectancy did not support the predictions that high DC depressives would set higher outcome expectancies than efficacy expectancies relative to low DC deressives, or that high DC nondepressives would set higher efficacy expectancies than outcome expectancies relative to low DC nondepressives.

(2) The hypothesis that a higher proportion of depressives than nondepressives would underestimate control in reward, and a higher proportion of nondepressives than depressives would overestimate control when there was a monetary contingency for light onset but not for accuracy of judgment of control, was not supported. Chi square analyses of the proportions of overestimations and underestimations among mood groups yielded nonsignificant differences (*ps* > .05). Additionally, although *t* tests revealed that subjects' judgment of control estimates varied significantly from zero, all subjects overestimated on Task 1 and underestimated on Task 2 (*ps* < .05). No mood-related differences were observed. Therefore, the hypothesis that depressives would

underestimate control in reward relative to the objective degree of contingency on Tasks 1 and 2, while nondepressives would overestimate control, was not supported.

(3) The hypothesis that high DC subjects would evidence greater illusions of control than low DC subjects in reward conditions when no monetary contingency for accuracy of judgment were present, was not supported, nor was the hypothesis supported that high DC subjects would underestimate control in punishment. Correlations evaluating the degree of relationship between these variables yielded nonsignificant results ($ps > .05$).

(4) The hypothesis that the accuracy of nondepressed normals' judgments of control would be greater on Task 5 relative to Task 3 was generally supported. Self-correction of nondepressives' judgment of control scores was therefore demonstrated, as this concept was originally defined. Mood X condition X task and mood X condition X control ANOVAs produced task and control effects on accuracy, and post hoc procedures on both analyses identified greater accuracy on Task 5 relative to Task 3. These results suggest that all subjects, and not just nondepressives, improved the accuracy of their judgments of control on Task 5 versus Task 3. Therefore, the related predictions that depressives would display smaller increases in accuracy than other subjects, and that their judgments of control would remain consistently low across tasks, were not supported. Additionally, DC was

negatively correlated with nondepressives' self-correction scores ($p < .05$), suggesting that low DC nondepressives demonstrated greater self-correction than high DC nondepressives. Similar correlations performed on depressives' and schizophrenics' scores yielded nonsignificant results ($ps > .05$).

(5) The prediction that depressives would evidence greater stability and internality in their causal attributions than all other subjects in punishment was not confirmed ($ps > .05$). Overall, although subjects displayed more stable and external attributions in punishment than in reward, depressives were not found to have more stable or internal attributions than other subjects. Results of the mood X condition X task ANOVA on locus of control revealed that schizophrenics displayed a more prominent external locus of control relative to nondepressed normal subjects on Tasks 3, 4, and 5 ($p < .05$). However, this finding was not significant across all five tasks.

(6) Two related hypotheses investigating self-esteem were tested. The hypothesis that depressives would assume more blame for their performance in punishment than they would take credit in reward, was not supported ($p > .05$). Results suggested that these scores did not vary by mood group on this variable, with subjects in all three groups reporting that they deserved more credit in reward than they deserved blame in punishment for comparable performances.

The hypothesis that depressives would assume more blame than nondepressives in punishment, and take less credit than nondepressives in reward, was also not supported ($ps > .05$). This finding provides no evidence that clinical depressives lack the motive to enhance self-esteem.

(7) The final hypothesis stated that depressives would set a higher criterion for successful performance than would nondepressives. ANOVAs on Deviation from Subjective Criterion for Success and Deviation from Objective Criterion for Success yielded nonsignificant differences across mood groups or treatment conditions ($ps > .05$). However, desirability of control was positively correlated with Deviation from Objective Criterion for Success, suggesting that high DC subjects set more unrealistic goals for success than did low DC subjects.

CHAPTER IV

DISCUSSION

Results will be summarized and discussed under the subheadings of desirability of control, expectancy of control, judgment of control, evaluation of performance, attribution, mood and self-esteem, depression effect, condition effect, and task/control effect.

Desirability of Control. The present study did not demonstrate a consistent relationship between the desirability of control construct and judgment of control ($r = .10$, $p > .05$), nor did the ANOVA on desirability of control reveal significant differences across mood groups or treatment conditions ($p > .05$). However, among nondepressed subjects, desirability of control was negatively related to the ability to self-correct judgments of control when monetary contingencies were placed on accuracy ($r = -.41$, $p < .05$). Low DC nondepressives more effectively self-corrected their judgments of control across tasks than did high DC nondepressives. At least among nondepressives, a high desire for control apparently inhibited the accurate perception of objective contingencies when monetary incentives were set in place. This finding suggests that high DC individuals may display a more pervasively rigid or inflexible cognitive style which interferes with their ability to effectively monitor and reevaluate changing environmental information, while low DC individuals may be more receptive to changes in environmental contingencies when they are prompted by cash incentives.

Broader evidence suggests that the relationship between self-correction and desirability of control should be interpreted with caution. First, post hoc analyses of the control level effects on simple judgment of control scores revealed that subjects' control estimates on Task 3 differed from all other tasks, not just Task 5. It is therefore misleading to state that subjects were more accurate on Task 5 than on Task 3, when they were in fact more accurate on all other tasks relative to Task 3. Second, separate correlations between DC and accuracy of judgment of control scores for Task 3 and Task 5 were both nonsignificant, as was the correlation between DC and the difference scores on accuracy of judgment of control on Task 3 minus Task 5. These correlations reveal no appreciable changes in accuracy for either low DC or high DC subjects from Task 3 to Task 5. Taken together, these results strongly suggest that the obtained self-correction results were artifactual, more likely representing a control level effect due to the greater difficulty of Task 3 versus all other tasks.

Significant correlations between desirability of control and the difference scores between subjects' personal criteria for success and actual control, suggest that high DC subjects set more excessive personal performance standards than low DC subjects. This finding is consistent with Burger's (1984, 1985, 1986) notion that a high desire for control is related to the tendency to set unrealistic personal performance expectations.

Desirability of control was also positively related to subjects' pre-task and post-task self-esteem scores. High DC individuals apparently felt better about themselves than did those low in the desire for control, and their participation in this study was not found to alter this relationship. These findings suggest that the desire to control environmental events may be associated with positive self-regard, just as previous research has demonstrated a relationship between the illusion of control and self-esteem enhancement (Frankel & Snyder, 1978; Wortman & Brehm, 1984).

<u>Expectancy of control</u>. The present study did not identify significant differences in expectancy of control between nondepressed subjects and clinically depressed individuals. These findings are inconsistent with the basic tenets of the Beck and hopelessness/learned helplessness models, which contend that depressives' distorted thinking produces lower expectancies of control than nondepressives, based on their pessimistic outlook.

Examination of subjects' expectancy scores revealed only minimal variations in expectancy across tasks. Subjects generally held lower expectancies on Task 1 relative to Tasks 3 and 4. It appears that subjects' lower expectancy on Task 1 may have resulted from their initial caution upon beginning the experiment. The high density of reinforcement present on Task 3 may have contributed to the slight elevation in expectancy scores for Task 4, although it is unclear why subjects reported elevated expectancy at the beginning of Task 3.

Overall expectancy scores suggest that subjects were not particularly responsive to experimental manipulations of objective control. While objective control varied from 0% to 80% across the five tasks, average expectancy scores ranged only from 49% to 58%, suggesting that subjects did not greatly modify their expectancies based on the actual contingency of a previous task. Additionally, the failure of expectancy ANOVAs to identify level of control effects from task to task indicates that subjects' expectancy estimates did not appreciably fluctuate across tasks in response to changing control levels. The correlation between reinforcement frequency on Tasks 1 through 4 and expectancy scores on Tasks 2 through 5 however, indicates that their expectancy estimates were influenced by the amount of reinforcement they perceived on the preceding task.

The high correlations demonstrated among expectancy, simple judgment of control, accuracy of judgment of control, efficacy expectancy, and judgment of efficacy suggest that all four of these variables similarly reflect subjects' perceptions of their ability to personally influence light onset, regardless of actual control. Subjects apparently held a rather unbending view of their ability to control task outcomes which did not change appreciably from pre-task to post-task, suggesting that their exposure to varying levels of actual control did not greatly affect their personal estimates of task control. This accounts for the extremely high correlations among these variables. In sum, these results suggest that subjects did not base their expectancy estimates on actual control.

Results of the ANOVA evaluating subjects' difference scores between accuracy of judgment of control and expectancy within the same task revealed that reward subjects held higher pre-task expectancies than the post-task judgments of control, while punishment subjects' expectancies were lower than their post-task control judgments. Punishment condition subjects were apparently more pessimistic at the onset of each task, while reward subjects were more optimistic. No significant mood effects were observed.

Clinical depressives were also not found to differ from nondepressives in outcome expectancy or efficacy expectancy. Subjects generally demonstrated higher outcome expectancies than efficacy expectancies, indicating that they were somewhat skeptical pre-task of their ability to produce consistently effective or successful responses. Task effects on both variables suggested lower efficacy and outcome expectancies on Task 1 than on all other tasks. Additionally, depressives in reward had lower outcome expectancies on Task 1 than on other tasks, and nondepressives in punishment had lower outcome expectancies on Task 1 than on Tasks 2 and 4. These findings are similar to those evaluating level of expectancy, and may be explained by subjects' caution or skepticism at the onset of the experimental procedure. However, following the completion of Task 1, their scores rose to a fairly consistent level across the remaining tasks.

In short, current expectancy of control results are inconsistent with the claims of the Beck model and the hopelessness/learned helplessness models, which assert that depressives hold lower expectations of control than do nondepressives. Specifically, these results disconfirm the claim of the hopelessness theory (Abramson et al., 1989) that depressives demonstrate lower outcome expectancies relative to nondepressives. Depressive subjects in the current study did not demonstrate a negative bias or distortion in their expectancy estimates, and their scores were in fact quite similar to those of nondepressives. This is particularly significant, given that the current study used clinical depressives who demonstrated a much higher level of pathology than the mildly depressed college students used in previous studies. For example, depressed students in Alloy and Abramson's (1979) study had an average Beck Depression Inventory score of 14.38 (Task 1), while depressed subjects in the current study had an average Beck score of 25.71. The severity of subjects' depression was therefore not associated with lower expectancies of control as compared to nondepressed subjects.

Judgment of control: Alloy and Abramson's (1979) prototypical study has generated considerable empirical investigation of the phenomenon of judgment of contingency. In comparing depressives' and nondepressives' cognitions, they conducted four experiments which systematically investigated judgment of control under various conditions. In experiment 1, depressives and nondepressives did not differ from each other, with both being more accurate in estimating control under moderate (50%) than high (75%) or low (25%) control. Experiment 2 differed by offering zero percent actual control. In both high and low frequency conditions, depressives were generally accurate in their judgments of control. In contrast, nondepressed subjects demonstrated an illusion of control on high frequency of reinforcement tasks, but were relatively accurate in evaluating control in low frequency of reinforcement. Experiment 3 included monetary reward and punishment conditions with noncontingent, 50% reinforcement frequency. As in experiment 2, depressed subjects accurately detected noncontingency across situations, while nondepressives demonstrated an illusion of control, overestimating control in reward, but not in punishment. Further, nondepressives' illusions of control were more pronounced than in experiment 2, where they were judging their degree of control over more neutral outcomes. Experiment 4 utilized monetary reward and punishment conditions with moderate control (50%). Results replicated depressives' accuracy across situations, while nondepressives were relatively accurate in reward but underestimated control in punishment.

Later research has generally supported these findings (Dresel, 1984; Vazquez, 1987), and replicated them in situations involving mood induction (Alloy, et al., 1981; Ford & Neale, 1985), exposure to controllable/uncontrollable noises (Abramson & Alloy, 1982), in judgments of control for self (but not for another) (Martin et al., 1984), and in private (but not in public) conditions (Benassi & Mahler, 1985).

Results of the current study do not identify significant differences in the accuracy of nondepressives' and clinical depressives' judgments of control. In general, all subjects overestimated control by an average of fifteen percent relative to objective contingencies over the five tasks combined. However, this statistic is misleading when interpreted in isolation. Actual control levels varied from 0% to 80%, while judgment of control scores ranged only from 50% to 63%. In general, when actual control was low, subjects generally overestimated control, while they underestimated control when actual control was high. If a task sampling 100% control had been offered, it is likely that this 15% difference would have been erased. This pattern of findings is not indicative of a systematic illusion of control or motivated distortion among subjects to assume consistently greater control than is actually present.

On a broad interpretive level, several explanations could account for the current findings. First, it is possible that subjects in the present study failed to understand the basic concept of control as it was presented because they were cognitively debilitated. Stated differently, these subjects may have been unable to adequately negotiate the complexity of these tasks due to their current state of decompensation. Although initially appealing, this explanation fails to account for the relatively comparable performances of psychiatric inpatients and nondepressed normal staff controls (who had displayed no history of psychiatric disturbance). Additionally, psychiatric subjects' scores on other measures (e.g., setting reasonable goals for judging their performances as successful, taking credit for positive outcomes and externalizing responsibility for negative outcomes) suggest that they adequately understood the experimental procedure.

Secondly, all subjects demonstrated a considerable overestimation of control on Task 3, which offered a high level of reinforcement but zero percent actual control. As discussed above, while this single finding is consistent with the illusion of control phenomenon, closer evaluation of the overall results does not support a systematic illusion of control or a generalized, motivated distortion. Subjects overestimated control when actual control was low, while they underestimated control on high contingency tasks.

Third, subjects may have been overly influenced in formulating their judgments of control by the amount of reinforcement they received for light onset. Their relatively unchanging judgments of control across tasks suggests that they were not responding to actual control, but instead were influenced by the more obvious and more constant factor of reinforcement frequency. Subjects may have interpreted control as "getting what they wanted." As long as they were being successful in turning the light on (which may have resulted from the pre-set high reinforcement frequency), subjects may have assumed that their behavior controlled light onset. They uncritically accepted the conclusion that if they experienced success, they were in control over task outcomes. Through this assumption, all subjects adopted a distorted sense of personal control, since they failed to question the fact that they could receive high reinforcement but have little or no control over the outcome.

Although this response style suggests some degree of distortion, it differs from the systematic overestimations descriptive of the illusion of control phenomenon. Subjects in the current study did not appear motivated to overestimate their control; rather, they were more closely attuned to the reinforcement they received, such that their control estimates paralleled reinforcement rates instead of actual control levels. This finding is consistent with that of Jenkins and Ward (1965), who found that subjects' control estimates correlated highly with the number of successful trials and were unrelated to the actual degree of control.

Several sources of statistical evidence suggest that current subjects were more strongly influenced by reinforcement frequency than by actual control in formulating their judgments of control. First, all subjects felt they had more control over the outcome on Task 3 than on each of the other tasks. This is intriguing since Task 3 involved the highest density of reinforcement (80-80), but the lowest (0%) actual control. Subjects received a high percentage of reinforcement regardless of their responses, which apparently boosted their confidence levels and their resulting judgments of control. Both depressives and nondepressives evidenced this pattern of responses. In short, subjects were "tricked" by the high reinforcement on this task, suggesting that their high control estimates were fueled by their perceptions of success.

Figure 1 summarizes the relationship across tasks among subjects' simple judgment of control scores, the frequency of reinforcement they received, actual control levels, and maximal reinforcement frequency (which was held constant at 80% across tasks). This figure clearly illustrates how subjects' judgment of control estimates closely paralleled the amount of reinforcement they received on each task, while no such relationship is observed between judgment of control scores and actual control (refer to Figure 1).

Second, when reinforcement frequency was covaried in the judgment of contingency analysis, the level of control effect disappeared, indicating that reinforcement frequency at least partially contributed to subjects' control estimates. A similar pattern of results obtained when expectancy scores were covaried in the judgment of control analysis, likewise suggesting that subjects' expectancy estimates influenced their judgments of control at post-task.

Third, frequency of reinforcement was found to be positively correlated with simple judgment of control scores for reward subjects ($\underline{r}$ = .36, $\underline{p}$ < .05). While this correlation was nonsignificant for punishment subjects, it approximated significance ($\underline{r}$ = .29, $\underline{p}$ = .09). Additionally, the Spearman correlation evaluating the relationship between simple judgment of control scores and reinforcement frequency for each subject and then averaged across subjects was sizable ($\underline{r}$ = .58, $\underline{p}$ > .05). With this correlation, however, the .05 statistical significance level required a correlational value of .90 due to the limited number (5) of paired comparisons per correlation (i.e., the number of tasks in this study). Although potentially unreliable, by accounting for 34% of the variance between these two variables, this correlation describes a moderately strong relationship between judgment of control and reinforcement frequency and offers an additional source of support for the notion that subjects based their control estimates on reinforcement frequency rather than actual control. In short, in the virtual absence of evidence for the claim that subjects based their control estimates on actual control, these sources of support argue that they instead were more sensitive to the success they experienced turning on the green light.

An integration of current findings with previous results suggests that mildly depressed college students are not particularly susceptible to cognitive distortion in their judgments of contingency, while nondepressed normals, mildly depressed schizophrenic inpatients, and severely depressed inpatients may demonstrate consistent inaccuracies. One explanation for this pattern of findings suggests a curvilinear relationship between psychological defensiveness and level of pathology.

This type of relationship has previously been documented in other fields of psychology. For example, the Yerkes-Dodson Law (1908) describes an inverted-U shaped relationship between arousal and performance. This law states that as arousal increases, performance improves, but then decays with further increases. Individuals exposed to moderate levels of arousal have been found to perform more efficiently and productively than those in either high or low arousal conditions. Individuals experiencing low arousal are comparatively unmotivated and apathetic, while those exposed to high arousal become inefficient and unproductive.

A similar type of curvilinear relationship between psychological defensiveness and level of pathology could explain the current findings. Distortive defensiveness may serve two separate roles. First, nondepressed normals may distort in order to maintain positive mood and protect/enhance self-esteem. Conversely, previous research has suggested that mild depressives' cognitions are not distorted due to their lack of this motive (Alloy & Abramson, 1979; Frankel & Snyder, 1978; Wortman & Brehm, 1975). Second, inpatient schizophrenics and depressives, due to their more severe psychopathology, may display distorted cognitions as a self-protective mechanism, fending off further deterioration in functioning. Clinical inpatients may display cognitive distortion in an effort to elevate mood and rebuild self-esteem. Since this explanation is obviously speculative, and since no research prior to the current study is available to document depressive realism within a clinical population, further research using severely depressed subjects is indicated to evaluate this hypothesis.

Self-correction. Nondepressives did demonstrate self-correction in their judgments of control, as this variable was originally defined. Their control estimates were more accurate on Task 5 relative to Task 3. Additionally, only nondepressives' self-correction scores were negatively correlated with desirability of control, suggesting that low DC nondepressives were better able to improve the accuracy of their control judgments than high DC nondepressives. The correlation of these two variables indicates that low DC nondepressives more effectively modified their cognitions in the face of changing environmental contingencies.

It should be cautioned that interpreting these results in isolation may be misleading. First, depressed and schizophrenic subjects also demonstrated a pattern of self-correction similar to nondepressives. Only Task 3 (80-80, 0% control) simple judgment of control scores were found to differ from all other tasks (not just Task 5). This finding suggests a control level effect for Task 3, rather than true self-correction. Subjects were "tricked" by the high reinforcement and low control of Task 3, resulting in extreme overestimations of control on this task as compared to all other tasks. Second, separate correlations between DC and accuracy of judgment of control for Task 3 and Task 5 were nonsignificant, suggesting that there was no true differential pattern of accuracy among high DC or low DC individuals, further undermining the notion that subjects improved the accuracy of their judgment of control estimates across tasks. In short, support for a true self-correction phenomenon appears quite limited.

Evaluation of performance. Current results indicate that subjects in reward gave themselves more credit than those in punishment gave themselves blame for comparable performances. This finding is consistent with previous research which has described the tendency of individuals to enhance self-esteem in positive situations and lessen insults to esteem in negative situations (Alloy & Abramson, 1979; Tang, 1987). In other words, individuals took credit for success and denied responsibility for failure. However, contrary to Beck's model and the hopelessness/learned helplessness models, which predict that depressed individuals will underestimate success and overestimate failure relative to nondepressives, current findings did not suggest that depressives and nondepressives differed from each other on this variable.

Most subjects considered their performances to be successful in comparison to their own personally determined standards for success. This finding held true across mood groups and treatment conditions. These findings are inconsistent with previous research (e.g., Tang, 1987), which found that college student subjects set their criteria for successful performance higher than estimated maximal reinforcement frequency and higher than actual maximal reinforcement frequency. Perhaps the subjects in this study (psychiatric patients and staff) set lower personal expectations for themselves than did college students, who may have been higher functioning individuals with higher expectations for their performance.

Attribution. Current results strongly suggest that subjects in punishment believed that the causes of their outcomes were significantly more stable and enduring than did reward subjects. In general, subjects in punishment also had a more external locus of control than those in reward. Additionally, nondepressives showed a greater internal locus of control than did schizophrenics. Depressives did not display a clearly internal or external locus of control and were not found to differ statistically from the other mood groups.

These results are inconsistent with previous findings which suggested that depressives demonstrate more internal and stable attributions when confronted with failure or other aversive circumstances (Abramson et al., 1978; Alloy, Abramson, Peterson, & Seligman, 1984). Instead, depressives and nondepressives in the present study demonstrated attributions which enhanced their self-esteem in reward and protected it in punishment (Frankel & Snyder, 1975; Weiner, 1976). They assumed credit for their successes and externalized blame for their failures. In short, the current attribution findings are consistent with the similarity between depressives' and nondepressives' judgment of control scores.

Mood and self-esteem. Both at the beginning and at the completion of the experiment, depressives and schizophrenics had lower self-esteem and were more anxious than nondepressed subjects. Not unexpectedly, depressed subjects scored higher than nondepressives on a depressive measure post-task.

Both depressives and schizophrenics became less anxious and hostile at post-task, while nondepressed staff became more anxious and hostile. Additionally, depressed subjects reported feeling less depressed at post-task than at pre-task, while nondepressed staff reported an increase in depression. These results suggest that the experimental tasks had different emotional effects on depressives and schizophrenics than on nondepressives. Nondepressed normals apparently perceived the procedure as somewhat threatening and invasive, while the other subjects (psychiatric patients) apparently failed to attach negative connotations to their experience. Perhaps the psychiatric patient subjects viewed the experimental tasks as an opportunity for success or achievement, resulting in a mood and self-esteem rebuilding experience. Alternately, they may have viewed the procedures as some form of therapeutic intervention. Nondepressed normals, on the other hand, may have viewed the tasks as evaluative and invasive, and realized that their performances would be compared to those of the psychiatric patients, resulting in higher post-task levels of depression, anxiety, and hostility.

Depression effect. In general, these results do not replicate Alloy and Abramson's (1979) findings, since significant differences were not observed between depressives and nondepressives for expectancy, judgment of contingency, evaluation of performance, or attribution. This was true for all tasks combined, as well as for the initial two tasks which offered monetary reinforcement for light onset but not for accuracy of judgment. Nondepressed schizophrenic inpatients likewise failed to differ significantly from depressed and nondepressed subjects in their estimates of contingency. Overall, these results are not indicative of an illusion of control as represented by a systematic overestimation of their control over environmental events. Subjects overestimated on high frequency/low control tasks and underestimated on high frequency/high control tasks. Subjects did not consistently inflate self-perceived control over task outcomes; rather, they were apparently lulled into believing that they held a moderate amount of control when reinforcement levels were high, regardless of actual control.

<u>Condition effect</u>. Placing subjects in either the reward or punishment condition undoubtedly bore a significant impact on their cognitions. Compared to subjects in the punishment condition, those in reward gave themselves more credit than those in punishment gave themselves blame for comparable performances. Punishment condition subjects reported more stable and external attributions, consistent with previous research findings which describe a self-protective bias in negative or aversive situations (Frankel & Snyder, 1975).

These results therefore support the notion that individuals ascribe themselves more personal control in positive than in negative situations. In addition, individuals in reward gave themselves more credit for their performance, despite the fact that punishment condition subjects earned significantly more money. This supports Rotter's (1966) contention that people attribute positive outcomes to internal factors, but reduce stress by attributing negative occurrences (e.g., losing money) to external factors.

<u>Task/control effect</u>. Equivocal support was obtained for the notion that individuals' cognitions varied with degree of contingency. Level of control effects were observed in the analyses of several major variables, suggesting several factors which may have contributed to subjects' differing cognitive responses across tasks: expectancy estimates; self-correction effects due to experimenter feedback and cash

incentives for accuracy; increased task exposure; varying levels of actual control; or varying frequencies of reinforcement across tasks.

In evaluating the possible interaction of factors which may have influenced subjects' judgment of control estimates, degree of task difficulty may have affected the manner in which subjects perceived the contingency level on a given task. That is, certain tasks were more difficult than others with regard to accuracy of judgment of control. Additionally, some tasks may be particularly difficult for researchers to interpret clearly because reinforcement frequency is confounded with actual control. These two separate types of task difficulty bear a direct influence on how judgment of control estimates are interpreted.

First, some tasks are more difficult than others for subjects to accurately judge control. Tasks higher in reinforcement density are likely to be more difficult to judge, since the high stimulus value of reinforcement (i.e., success) makes it more difficult for individuals to disentangle the amount of control actually present from their experience of (increased) success. This is particularly true on high reinforcement noncontingent tasks, where subjects' experience of success can apparently override the lack of actual control.

For example, a task involving a low amount of control and relatively low reinforcement (40-20, 20% control) would be considered relatively easy, partly because the low control limits the possible range for underestimations of control. At the same time, subjects are also less likely to overestimate control on low control/low reinforcement tasks, since their predominant experience of failure in turning on the light makes the low amount of control more obvious. Additionally, a task involving high control and high reinforcement (80-0, 80% control) would also be considered easy, since the differential effectiveness of press versus no-press responses is clearly identifiable. If reinforcement frequency remains high and actual control is reduced, it becomes harder for a subject to be aware of how little control is present when he/she is still successful in producing light onset. Perhaps the most difficult is the task offering high reinforcement with little or no control (80-80, 0% control). This type of task exerts a strong stimulus pull for subjects to base their control estimates on reinforcement frequency. Indeed, this type of task has most typically produced the illusion of control effect among nondepressives in several previous studies (Alloy & Abramson, experiments 2 and 3, 1979; Dresel, 1984; Vazquez, experiment 2, 1987). Both depressives and nondepressives in the current study overestimated control on this task (80-0, 0% control), producing an apparent illusion of control effect. However, their pattern of responses does not appear to result from the motivated systematic exaggerations in judgments of control;

instead, they reflect an insensitivity to objective control and a selective focus on light onset frequency.

At another level, certain tasks may be relatively easy for subjects to offer apparently accurate judgments of control, yet pose a clear interpretive dilemma for researchers. This difficulty results from the confounding of reinforcement frequency with objective control. These tasks are comparatively easy for subjects, because they can respond to reinforcement frequency and still appear to be sensitive to the objective contingency. For example, on an 80-20 (60%) control task, it is difficult to determine whether subjects are more greatly influenced by their perception of reinforcement frequency or their accurate appraisal of the objective contingency. Therefore, although subjects from previous studies may have demonstrated seemingly accurate judgments of control, it is unclear whether their responses actually may have represented their sensitivity to the reinforcement they received. Previous studies which have confounded level of control with frequency of reinforcement include: Alloy and Abramson (1979); Ford and Neale (1985); Newman and Benassi (1989); and Vazquez (1987).

It should also be noted that the current study represents a distinct departure from the single task paradigm of previous studies, since all subjects completed five consecutive contingency tasks. Given that the current data are not entirely consistent with previous judgment of contingency results, it is difficult to know how previous results might have been altered had subjects completed multiple contingency tasks. It is possible that exposing subjects to consecutive tasks offering consistently high reinforcement frequency may have predisposed current subjects to develop a cognitive "set" across tasks, where their responses were consistently influenced by their experience of success. This may explain subjects' failure to offer more accurate control estimates on the relatively easy Task 4 (80-0, 80% control), since they had become conditioned during the previous three tasks to respond to reinforcement frequency.

Evaluating the current findings in light of Newman and Benassi's (1989) "contrasts effects" suggests that subjects in the current study did not significantly alter their judgment of control estimates based on actual control from a previous task, since post hoc analyses of control level effects on the simple judgment of control ANOVA did not identify a contrast effect pattern. Only the judgment of control scores on Task 3 were found to differ from those of other tasks. The current results therefore appear incompatible with the "contrast effects" hypothesis, i.e., that subjects initially exposed to high contingency would underestimate control on a subsequent task of moderate contingency. It should be noted, however, that Newman and Benassi confounded frequency of reinforcement with level of control, since their high contingency task was also high in reinforcement, the moderate contingency task offered moderate reinforcement, and the noncontingent task was low in reinforcement (70-0, 55-10, 35-35). These authors assumed that their subjects were responding to actual control rather than to frequency of reinforcement, a questionable assumption given the currently obtained findings.

Summary of current findings. The present study offered five tasks where reinforcement was held consistently high across tasks while actual control levels were systematically manipulated. Monetary contingencies were placed on subjects' responses in both reward and punishment conditions. Results across all tasks did not identify differential response styles among clinical depressives, nondepressed schizophrenics, and nondepressed normals. Further, subjects in general did not appear responsive to experimental manipulations of objective control. Their judgments of control remained relatively consistent across tasks, more closely paralleling frequency of reinforcement rates rather than objective control levels.

Limitations

In summarizing these results, it should be acknowledged that the methodology of the current study may have been affected by limitations of the physical plant of the state hospital where this research was conducted. Since this project was conducted in the field rather than in the laboratory, less complete control was available over variables such as extraneous noise levels, conditions of the experimental room, etc. All subjects were tested under similar conditions in a group therapy room on the treatment unit. Unfortunately, no rooms were available which allowed the experimenter to conduct the experiment from a remote location. As a result, this required that the experimenter be present in the room with the subject while the study was conducted. Although the experimenter was always seated behind the subject and out of his/her field of vision, it is possible that subjects perceived themselves to be in the presence of an observer, thereby influencing their judgment of control estimates.

Several methodological differences between the present study and Benassi and Mahler's (1985) research suggest, however, that the current experimenter was not in an obvious observer role. The observer in the Benassi and Mahler study was clearly identified as such, while the experimenter in the current study was present only to conduct the tasks. First, the Benassi and Mahler methodology explicitly stated that instructions be offered clearly to both the participating subject and the observer. Second, the observer was asked whether he/she understood the procedures or had specific questions. Third, the observer was told, in the presence of the participating subject, that he/she would be asked questions regarding task outcomes and the participating subject's performance. In constrast, the experimenter's apparent role in the current study was merely to present tasks. No instructions were given which implied that the current experimenter would be observing or evaluating the subject's performance.

Applications to the Beck and Hopelessness Models of Depression

Beck's (1979) cognitive model contends that depressives are negative in their views toward the self, current experiences, and the future. They assume excessive self-blame for negative events, and this distortion causes them to underestimate control over environmental events. The reformulated learned helplessness model (Abramson et al., 1978) claims that individuals displaying personal helplessness attribute negative events to internal, global, and stable causes, perceiving themselves as unable to emit success-producing behaviors. They perceive their behaviors as being noncontingently related to outcomes, causing them to underestimate control over external events. The newer hopelessness theory (Abramson et al., 1989) retains much of the same logic as the reformulated learned helplessness theory, but is has deemphasized attribution and focused on the core feature of hopelessness as the causal agent in this particular subtype of depression. Although this theory technically has dismissed the cognitive deficit which was endorsed by its predecessors, it remains necessary for explaining depressives' underestimations of control.

These cognitive theories predict that depressives display consistently inaccurate cognitive functioning in that they deemphasize personal agency in positive situations and focus on negative aspects of their environment. Beck originally explained these patterns as resulting from depressives' history of negative experiences, negative schemas, and faulty information processing. Results of the present study offer little empirical support for the above constructs. Although depressives did report higher symptoms of anxiety and depression than nondepressives, as well as lower self-esteem, their performance on cognitive tasks did not systematically vary from that of nondepressives.

Both the newer hopelessness theory (Abramson et al., 1988; Alloy et al., 1988) and the recent revision of Beck's theory (Beck, 1987; Dykman et al., 1989) assert that depressives negatively evaluate their own performance. Beck's theory suggests that depressives view themselves as inadequate, worthless, and unable to produce necessary responses. Additionally, they claim less credit than nondepressives in positive outcomes and assume more blame in negative outcomes. Hopelessness theory states that depressives hold perceptions of low personal control and negative expectations about the future. Results of this study give little support for these ideas. Depressives and nondepressives were not found to differ in their efficacy and outcome expectancies or in their attributional styles. All experimental subjects claimed more credit for their successes than they accepted blame for comparable performances in punishment.

Secondly, while the newer hopelessness theory is still unclear in its position on depressive underestimations of control, both the Beck and reformulated learned helplessness models predict that depressives underestimate control over environmental contingencies. Beck's theory states that depressed individuals incorrectly see themselves as incapable of producing effective responses. Similarly, the reformulated learned helplessness theory (Abramson et al., 1978) states that depressives develop perceptions of noncontingency between their responses and environmental outcomes due to previous perceptions of uncontrollability. Again, results of the current study are not supportive of these contentions. Both in the presence and absence of monetary contingency for accuracy of control, depressed and nondepressed subjects did not differ from each other in their judgment of control estimates, nor did their expectancy of control estimates vary by mood.

Thirdly, Beck contends that depressives set unrealistically high criteria for personal success. The present results instead suggest that both depressives and nondepressives set relatively realistic personal criteria for success which were in keeping with both objective control levels and their own personally determined criteria for a successful performance. This finding implies that depressives were able to accurately and appropriately evaluate their potential for successful behavior relative to both objective and personal standards. Depressed subjects therefore did not view themselves as incapable of producing effective responses on this task, as these theories have asserted.

Finally, it should be acknowledged that subjects in the current study were not selected to fulfill criteria for the hopelessness subtype of depression. The current subjects were required only to demonstrate a current diagnosis of non-bipolar affective disorder. No specific measure was administered specifically to assess the hopelessness subtype. As a result, it is inappropriate to use the current results as an empirical test of hopelessness theory. Abramson et al. (1989) have argued that it is inappropriate to simply lump together all depressive subjects and examine their levels of hopelessness to test the theory. Therefore, a direct test of the hopelessness theory lies beyond the scope of the present study. Indirectly, however, the striking similarity of depressives' and nondepressives' responses in the current study does not support the premise that clinical depressives as a broad diagnostic group display a hopeless outlook. It is suggested that future studies examining judgment of contingency select clinically depressed subjects who fulfill the diagnostic criteria for the hopelessness subtype of depression, to more accurately determine the direct relationships of hopelessness and judgment of contingency.

Applications to previous research on depressive realism

Alloy and Abramson's (1979) research suggested that depressed individuals are "sadder but wiser," based on the contention that their predictions of environmental contingencies are more consistently accurate than those of nondepressives in a wide variety of experimental situations. They found that nondepressives, on the other hand, overestimated actual control in positive situations, and sometimes underestimated control in negative situations. Alloy and Abramson further stated that depressives' accuracy derived from their lack of a motive to enhance self-esteem.

The current results do not support these contentions. Subjects in all mood groups offered judgment of control estimates which were statistically different from actual control on each of the five experimental tasks, overestimating on low control tasks and underestimating on high control tasks. No significant differences were observed across mood groups or reward/punishment conditions. As noted earlier, although depressives and nondepressed schizophrenics reported a greater degree of anxiety, depression, and hostility than nondepressed individuals, as well as lower self-esteem, they did not differ across the majority of variables in this study, such as expectancy, judgment of control, attribution, or personally determined criteria for success. In short, this study does not provide support for Alloy and Abramson's broad claim that depressives are "sadder but wiser."

More specifically, these results clearly indicate that all subjects, regardless of mood or treatment condition, were unresponsive to experimenter manipulations of actual control across tasks. Subjects' judgment of control scores did not vary as a function of actual control, nor were there systematic variations among mood groups or treatment conditions. Instead, subjects' judgment of control estimates consistently reflected an overreliance on the level of reinforcement they received on each experimental task. The current findings do not describe an illusion of control among nondepressives, nor do they identify greater "realism" among depressives. Subjects' responses are not descriptive of a motivated, systematic inflation of control; rather, all subjects were inaccurate by overestimating on low control tasks and underestimating on high control tasks.

Stated differently, these findings indicate that all subjects were somewhat sensitive to changes in reinforcement frequency across tasks, since the fluctuations in their judgment of control scores roughly paralleled the overall frequency of reinforcement (refer to Figure 1). In this way, subjects utilized an inappropriate strategy for gauging personal control. Although not consistent with the findings of Alloy and Abramson (1979), these results are quite consistent with those of Jenkins and Ward (1965), the originators of the press no-press light onset task, who found that humans' subjective representations of contingencies are not isomorphic with objective contingencies. These authors found that subjects' ratings of control correlated highly only with the number of successful trials, regardless of the objective contingency present. In other words, the control estimates of both Jenkins and Ward's (1965) subjects and the current subjects were unrelated to the actual degree of control. More attuned to the (high) frequency of success they experienced, these subjects neglected to adequately assess the degree of control their responses actually exerted over outcomes. While not as blatantly self-serving as the illusion of control distortion, where individuals systematically believe themselves to have more control than is actually present, this distortion is nonetheless fueled by the perception of personal success. Subjects took credit for controlling outcomes as long as they experienced success.

Given that the current study utilized the same basic instructions and task format as previous judgment of contingency studies, it is intriguing that the current results are strikingly dissimilar to certain previous findings (Alloy & Abramson, experiment 4, 1979; Ford 7 Neale, 1985; Newman & Benassi, 1989; Vazquez, 1987). Since the current study offered a more comprehensive sampling of actual control levels, these results call into question whether previous studies correctly interpreted their results based on a limited sampling of actual control. While the current study evaluated control esimates across a wide range of contingencies, nearly all previous studies sampled only one level of contingency and therefore may have inaccurately interpreted their results. For example, on contingency tasks where the reinforcement frequency and actual control levels were similar, subjects in fact may have been responding to frequency instead of actual control. Examples include Newman and Benassi's (1989) study which confounded high reinforcement frequency with high actual control (70-0), moderate reinforcement frequency with moderate control (55-10), and low reinforcement frequency with low actual control (35-35). Other examples include Alloy and Abramson's (1979) experiment 4 (75-25), Ford and Neale (1985; 75-25), and Vazquez's (1987) experiment 1 (50-25).

In more fully elaborating this notion, it must first be acknowledged that previous research appears to offer widespread support for the phenomena of depressive realism and nondepressive illusion of control. Following Alloy and Abramson's (1979) landmark study, numerous investigations have supported mild depressives' greater accuracy compared to nondepressives in evaluating environmental contingencies in a wide variety of situations. On tasks involving either noncontingency or a high frequency of reinforcement, nondepressives consistently have been found to demonstrate an illusion of control, while depressives were relatively accurate (Alloy & Abramson, 1979, experiments 2 and 3; Alloy, Abramson & Kossman, 1985; Vazquez, 1987, experiment 2). Studies evaluating judgments of control following mood induction or exposure to previous controllable/uncontrollable noises produced a similar pattern of findings (Alloy & Abramson, 1982; Alloy, Abramson, and Viscusi, 1981). Dresel (1984) likewise observed depressive accuracy and nondepressive distortion when subjects were exposed to a noncontingent outcome for many (48) trials. Other studies have replicated these phenomena regarding judgment of control for the self, but not others (Martin et al., 1984) and in private, but not public situations (Benassi & Mahler, 1985). In short, depressive realism and nondepressive illusions of control appear to be robust phenomena under usual conditions, as elaborated in Alloy and Abramson's (1988) recent review of the "depressive realism" literature.

However, it is important to note that in each of the above mentioned studies, researchers offered tasks with either a high density of reinforcement <u>or</u> cash incentives for light onset. Previous results suggest that the presence of either of these factors was sufficient to induce inaccuracies among nondepressives, while depressives maintained accuracy.

Vazquez (1987) has speculated that nondepressives may display a lower "threshold of biasing," relative to mild depressives, in formulating their judgments of control. That is, they may be more easily enticed to base their responses on frequency of reinforcement, rather than accurately discerning objective contingencies (Alloy & Abramson, 1979).

It should be emphasized that in the studies reporting depressive accuracy/nondepressive distortion, these mood effects were limited to noncontingent tasks in which outcomes occurred frequently or subjects won money. On most all contingent tasks, depressed and nondepressed college students were equally accurate in their control estimates, and both groups demonstrated mild overestimations of control on high reinforcement tasks where contingency levels approached zero (Alloy & Abramson, experiment 1, 1979; Ford & Neale, 1985; Vazquez, experiment 1, 1987). Further, earlier studies have concurred that people tend to overestimate control as response-outcome contingencies approach zero (Jenkins & Ward, 1965). Bryson and his colleagues claimed that by asking subjects to evaluate their degree of control over task outcomes, those subjects were led to believe that some degree of control would be present. Bryson et al. (1984) speculated that this contributed to subjects' overestimations on noncontingent tasks. In sum, although previous results suggest that depressives and nondepressives are differentially vulnerable to demonstrating inaccuracies in their judgments of control, it appears that both groups can be enticed into basing their control estimates on the successes they experience. The presence of either high reinforcement or cash incentives has been identified previously as a possible causative factor in nondepressives' inaccurate judgments of control. Perhaps the combined effects of these two variables are sufficient to seduce even depressives into basing their control estimates on reinforcement frequency.

Only one previous study has incorporated both high frequency of reinforcement and a cash incentive in its experimental design. Alloy and Abramson's (1979) experiment 4 assessed subjects' judgment of control estimates in a 75-25 contingency task with a cash incentive for light onset. On this task, both depressives and nondepressives offered relatively accurate judgments of control which ranged between fifty and fifty-five percent. The current study obtained strikingly similar results on its 80-20 task, which closely paralleled Alloy and Abramson's 75-25 design. (Current subjects also estimated approximately fifty-five percent control on this task). However, the current study additionally sampled four other objective control levels while maintaining a generally high level of reinforcement across tasks. Results of the current multi-task sampling of control suggest that even when contingency levels changed, judgments of control remained relatively consistent, closely paralleling reinforcement rates. These results therefore suggest that the combined effects of high reinforcement density and cash incentive for light onset may lead individuals (regardless of mood) to respond to the higher saliency of reinforcement, rather than attending to objective control levels. In this way, Alloy and Abramson's results on this task may actually reflect subjects' sensitivity to reinforcement frequency, rather than to judgment of control, as those authors claimed.

Further, these results suggest that repeated exposure to high frequency of reinforcement and cash incentives for light onset may create a cognitive "set," where subjects are seduced by their high success rate into assuming consistently moderate to high control, regardless of objective contingencies. This also suggests that subjects' failure to correctly appraise the control level on the relatively easy Task 4 (80-0) may have been due to their previously established pattern of overreliance on reinforcement frequency on the more difficult initial three tasks. (This task offered the widest, most easily discernable differential in reinforcement rate between press and non-press responses, making it the least difficult among the five tasks).

Since prior studies have not utilized a multi-task design, it previously has been impossible to evaluate the extent to which individuals based their judgments of control on reinforcement frequency. Stated differently, due to inadequate sampling of control levels, previous studies may have overlooked the prominent effect of reinforcement frequency on subjects' judgment of control estimates in situations involving both cash incentive for light onset and high reinforcement density. This suggests that certain previous depressive realism studies may have obtained artifactual results, where subjects' apparent accuracy actually identified their greater sensitivity to reinforcement frequency than to objective control. Additional research using a multi-task design is needed to evaluate subjects' differential accuracy in judgments of control for tasks involving differing combinations of control and reinforcement frequency. This would allow for a more comprehensive, systematic assessment of the possible interaction between reinforcement frequency and actual control on judgment of control estimates.

Conclusion

In sum, while the previous literature offers support both for depressive realism and nondepressive illusion of control, current results are not supportive of either of these phenomena in the current clinical sample. Despite the implementation of instructions and test format which were patterned directly after Alloy and Abramson (1979), the current subjects' judgment of control estimates were more greatly influenced by reinforcement frequency than by objective control. Consideration of the cognitive difficulty of the current tasks suggests that the high level of success these subjects experienced, coupled with the cash incentives they received for light onset, may have diminished their sensitivity to the objective contingencies. The high stimulus saliency of frequent reinforcement made these tasks more difficult than those of some previous studies which involved low contingency and low control. In accounting for the lack of differences in depressives' versus nondepressives' judgments of control, it appears that the combined effects of these two factors may have been sufficient to cause depressives as well as nondepressives to disattend to objective control and instead focus on the high frequency of reinforcement present.

Additionally, an integration of these results with previous studies which have documented judgment of control accuracy among mild depressives suggests that the current findings could identify a curvilinear relationship between mood and the accurate appraisal of environmental contingencies. The current literature offers strong support for both depressive realism and nondepressive illusion of control using subclinical samples in a wide variety of contingency settings. Since the current results offer the first set of empirical data investigating these phenomena within the clinical population, it is particularly noteworthy that no consistent differences were observed between mood groups. This discrepancy may perhaps identify qualitative differences in the cognitions of clinical depressives (or psychiatric patients in general) and mildly depressed college students.

Previous studies have suggested that nondepressives experience cognitive distortion in order to enhance or protect self-esteem (Wortman & Brehm, 1975), while mildly depressed college students are not motivated to maintain or enhance self-esteem (Alloy & Abramson, 1979). Since the current sample utilizing clinically depressed psychiatric inpatients failed to demonstrate a consistent pattern of accuracy, it is hypothesized that clinical depressives may experience similar cognitive distortion as nondepressives, but perhaps for different reasons. While nondepressives may distort (i.e., overrely on reinforcement frequency) to protect or enhance self-esteem, clinical depressives may display similar inaccuracies in an attempt to regain a sense of personal control and to rebuild self-esteem. This may serve as a self-protective mechanism, fending off further deterioration in functioning by allowing them to focus on their successes (i.e., high frequency of light onset) rather than objective control levels. This increased focus on the experience of success may describe the process by which their coping resources are restored during recovery. Further investigation is necessary to more fully document this relationship.

Recommendations

This study represents the first empirical investigation of judgment of contingency using a clinical sample. Additionally, it provides a comprehensive evaluation of depressives' cognitive functioning. However, there are obviously several limitation which must be considered when attempting to generalize these results. The following recommendations are suggested to overcome such limitations in future studies.

1. Due to the obvious lack of empirical studies on judgment of contingency with clinical depressives, continued exploration of this phenomenon is needed to broaden the empirical base. Possible future studies could include comparisons of the cognitive functions of both mildly and severely depressed individuals to determine if differences exist between these two groups. Exploration of these variables among individuals displaying the hopelessness subtype of depression is also suggested.

2. In exploring the cognitive functioning of clinical depressives, nondepressed schizophrenic inpatients were selected as a psychiatric control group to determine if general effects of psychopathology or hospitalization influence cognitive functioning. In future studies, consideration might be given to utilizing psychiatric controls with other diagnoses, since selecting exclusively schizophrenics limits the ability to compare depressives to the psychiatric population in general. Additionally, it is recommended that future studies include nondepressed medical patients, so that the effects of hospitalization apart from psychopathology might better be documented.

3. Another difficulty in directly comparing the cognitive functioning of depressives and schizophrenics relates to severity of pathology. The Brief Psychiatric Rating Scale was utilized in this study as a crude means of selecting relatively equally impaired individuals with qualitatively different disorders. Unfortunately, this measure is statistically unsophisticated and is likely insufficient in selecting individuals with similar levels of psychopathology. Although time consuming, it is suggested that a formalized clinical interview such as the Schedule for Affective Disorders and Schizophrenia Scale (SADS; Endicott & Spitzer, 1978), be incorporated into future studies to more adequately define inclusion criteria and determine severity of pathology.

4. It is suggested that the basic paradigm of press-no press be modified. Most subjects in the current study had difficulty grasping the notion that a "no press" response could be equally as effective or more effective than a "press" response. In real life, individuals are repeatedly taught that the initiation of behavior is required to obtain an outcome. This notion is at odds with the experimental design of this study, where a "press" or "no press" response might be equally effective. Even though no systematic differences in responding were found for press versus no-press, subjects continually demonstrated difficulty with this basic concept. It is suggested that future studies involve a

modification whereby a subject may push one of two different colored buttons in their pursuit of light onset.

APPENDIX A

PRE-EXPERIMENT INSTRUCTIONS

Appendix A

Instructions to subjects before completion of pre-experiment questionnaires

"Thank you for participating in our research project. Our purpose is to investigate individual problem-solving skills. For the next hour, you will be given five similar tasks. These involve learning how to turn on a green light and determining the amount of control that you have over the green light onset. You will receive a cash reward for your participation, the amount of which will be determined by your performance on the five tasks. Before the tasks, we would like you to complete some questionnaires. When filling out the forms, please work as quickly as you can. Do not spend too much time thinking about the items or checking over your answers. Your first impression is most important."

APPENDIX B

BIOGRAPHICAL INFORMATION SHEET

Appendix B

BIOGRAPHICAL INFORMATION SHEET

Code Number:____________ Sex:________ Age:_____________

Ethnic Status:

______________ White-American
______________ Black-American
______________ Mexican-American
______________ Oriental-American
______________ Other (please specify)

Marital Status:

______________ Single
______________ Married
______________ Divorced
______________ Widowed

Highest level of education completed:

______________ Doctoral Degree
______________ Master's Degree
______________ College graduate
______________ Attended College (please specify number of years)
______________ High School graduate
______________ School grades completed (please specify number of grades)
______________ No formal education

Current Occupation: ______________________________

Approximate Yearly Income:

______________ Over $50,000
______________ $40,000 - $50,000
______________ $30,000 - $40,000
______________ $20,000 - $30,000
______________ $10,000 - $20,000
______________ Less than $10,000

Religious Belief:

______________ Protestant
______________ Catholic
______________ Jewish
______________ Other (please specify)
______________ None

APPENDIX C

PRE-TASK INSTRUCTIONS: FORM A (REWARD CONDITION)

Appendix C

Pre-task instructions: Form A (Reward Condition)

"Thank you for participating in our research project. Our purpose is to investigate individual problem-solving skills. For the next hour, you will be given five similar tasks. These involve learning how to turn on a green light and determining the amount of control that you have over green light onset. You will receive a cash reward for your participation, the amount of which will be determined by how well you do on the five tasks.

Now, please look at these two boxes. This smaller black wooden box here is the box on which you are going to make your responses by either pressing or not pressing the red button. Now, in this problem-solving experiment, it is your task to turn the green light on and to learn the degree of control you have over whether or not the green light comes on.

Each time the red light comes on,it indicates the start of a new trial, the occasion to do something. After the red light comes on, you have the option of either making a button-press response or not making a button-press response. A button-press resonse consists of pressing this button with your left thumb once and only once immediately after the red light goes off. Not making a button-press response consists, of course, of doing nothing when the red light goes off. Please keep your left thumb off the red button when you are not making a button-press response. If you intend to press the button on a given trial, you must press it within three seconds after the red light goes off, otherwise the trial will be counted as a no-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 red light goes off, or else, just sit back and do not press the button. Any questions so far?

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 comes on; and 4) you don't press and the green light does not come on. Since you also have to know what happens when you do not press the button, it is to your advantage not to press the button on some trials. Any questions?

You are required to do five similar tasks like this, Tasks 1, 2, 3, 4, and 5, with each task consisting of 30 trials. On all 30 trials of each task, your job is to determine how much control you have over whether the light comes on or does not come on. In addition, on the final ten trials of each task, you can earn a dime credit every time the green light comes on. On each trial in which the green light does not come on, you will not earn anything. So, it is to your advantage to maximize the number of trials in which the green light comes on. A slide showing your current cash earnings will be projected onto a screen to remind you of your earnings after each trial. Any questions?

At the beginning and at the end of each task, you will be asked to indicate your judgment of control by selecting a number from 0 to 100: 100 if you have complete control over the onset of the green light, 0 is 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 response, either pressing or not pressing. In other words, whether or not the green light comes 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 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 do not do. Intermediate degrees of control means that your choice of response, either pressing or not pressing, influences the onset of the green light even though it does not completely determine whether the green light comes on or not.

The money you have earned will be distributed after you have completed all five tasks and all the questionnaires. You have to complete all five tasks and questionnaries to claim your money. Should you decide to stop at any time during the experiment, you are allowed to do so, but you cannot claim any money from the experiment. The experimenter will leave the area when you are ready and he/she will monitor the experiment from a nearby area.

In order to participate in this research, we ask you to sign the consent form here. Your identity and any information from you will remain anonymous. There are five booklets of questionnaries in front of you, each marked Task 1, Task 2, Task 3, Task 4, and Task 5. You will complete page 1 in the corresponding booklet before you start each task, and complete the rest after you finish the task. Check carefully the label of the booklet corresponding to the number of the task you have just done. That is, complete page 1 on booklet labeled Task 1 before your start task 1, and finish the rest of the questionnaires on the same booklet immediately after you have complete task 1. The order of the tasks is always 1, 2, 3, 4, and 5. Do not go back to previous booklets to check for answers. The experimenter will announce the beginning and the end of each task and will remind you to check your booklet. Do you have any questions?

Now, please answer page 1 of the booklet lableled Task 1. Let me know when you are ready to begin the experiment.

APPENDIX D

PRE-TASK INSTRUCTIONS: FORM B (PUNISHMENT CONDITION)

Appendix D

Pretask instructions: Form B (Punishment Condition)

"Thank you for participating in our research project. Our purpose is to investigate individual problem-solving skills. For the next hour, you will be given five similar tasks. These involve learning how to turn on a green light and determining the amount of control that you have over green light onset. You will receive a cash reward for your participation, the amount of which will be determined by how well you do on the five tasks.

Now, please look at these two boxes. This black box has a green light and a red light. This smaller black wooden box here is the box on which you are going to make your responses by either pressing or not pressing the red button. Now, in this problem-solving experiment, it is your task to turn the green light on and to learn the degree of control you have over whether or not the green light comes on.

Each time the red light comes on, it indicates the start of a new trial, the occasion to do something. After the red 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 blue button with your left thumb once and only once immediately after the red light goes off. Not making a button-press response consists, of course, of doing nothing when the red light goes off. Please keep your left thumb off the red button when you are not making a button-press response. If you intend to press the button on a given trial, you must press it within three seconds after the red light goes off, otherwise the trial will be counted as a no-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 red light goes off, or else, just sit back and do not press the button. Any questions so far?

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 comes on; 4) you don't press and the green light does not come on. Since you also have to know what happens when you do not press the button, it is to your advantage not to press the button on some trials. Any questions?

You are required to do five similar tasks like this, Tasks 1, 2, 3, 4, and 5, each task consisting of 30 trials. On all 30 trials of each task, your job it to determine how much control you have over whether the light comes on or does not come on. In addition, on the final ten trials of each task, you will lose a dime credit every time the green light does not come on. On each trials in which the green light does come on, you will not lose anyting. So, it is to your advantage to maximize the number of trials in which the green light comes on. A slide showing your current cash earnings will be projected onto a screen to remind you of your earnings after each trial. Any questions?

At the beginning and at the end of each task, you will be asked to indicate your judgment of control by selecting a number from 0 to 100: 100 if you have complete control over the onset of the green light, 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 response, either pressing or not pressing. In other words, whether or not the green light comes 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 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 do not do. Intermediate degrees of control means that your choice of response, either pressing or not pressing, influences the onset of the green light even though it does not completely determine whether the green light comes on or not.

The money in your credit, after all the deductions are made, will be distributed after you have completed all your tasks and the questionnaries. You have to complete all five tasks and all questionnaires to claim your money. Should you decide to stop at any time during the experiment, you are allowed to do so, but you cannot claim any money from the experiment. The experimenter will leave the area when you are ready and he/she will monitor the experiment from a nearby area.

In order to participate in this research, we ask you to sign the consent form here. Your identity and any information from you will remain anonymous. There are five booklets of questionnaires in front of you, each marked Task 1, Task 2, Task 3, Task 4, and Task 5. You will complete page 1 in the corresponding booklet before you start each task, and complete the rest after you finish the task. Check carefully the label of the booklet corresponding to the number of the task you have just done. That is, complete page 1 on the booklet lableled Task 1 before your start task 1, and finish the rest of the questionnaires on the same booklet immediately after you have completed task 1. The order of the tasks is always 1, 2, 3, 4, and 5. Do not go back to previous booklets to check for answer.

The experimenter will announce the beginning and the end of each task and will remind you to check your booklet. Do you have any questions?

Now, please answer page 1 of the booklet lableled Task 1. Let me know when you are ready to begin the experiment."

APPENDIX E

CLIENT INFORMED CONSENT FORM

Appendix E

CLIENT INFORMED CONSENT FORM

I, ____, have received an explanation of the research investigating individual problem-solving skills which is being conducted at Terrell State Hospital.

I understand that my participation will be limited to answering several brief questionnaires and completing five tasks, each consisting of thirty trials of pressing or not pressing a button in order to make a green light appear. The total time involved will be approximately one hour. I will be paid approximately five dollars for my participation, the exact amount depending upon my task performance.

I understand that all my responses will be kept confidential and that I will be referred to only by code in the data collected, so that my name will not be disclosed. I also understand that any information I give will not be shared with anyone other than the principal investigator, David Cobbs, M.A., and his immediate supervisor, Joseph Critelli, Ph.D.

However, I understand that the information I provide will be used for scientific research purposes only, and that the combined results gained from the approximately sixty participants may be published in scientific or professional journals, with no references being made as to individual identities.

I understand that Mr. Cobbs will be allowed access to my patient file under the strict supervision of a member of the hospital staff, to gather only that information which is necessary for the completion of this research. This information will include such data as my length of stay at Terrell State Hospital, and diagnoses, treatment, and medications given. I also understand that this information will be kept strictly confidential and that I will be referred to only by code, so that my name will not be revealed.

I understand that my participation in this project will have no effect on my treatment or length of stay at Terrell State Hospital, and that this research will not place my health at risk in any way. I further understand that there are no physiological, psychological, or social risks involved in this study.

I understand that my participation in this project is strictly voluntary and that I may withdraw my consent to participate at any time with no effects whatsoever on my treatment or length of stay at Terrell State Hospital. I also understand that I will not be paid my cash earnings until the conclusion of the procedures presented to me, and that I must complete all of the tasks and questionnaires in order to claim any money.

I understand that I may consult with a member of the Terrell State Hospital Institutional Review Board (IRB) at any time concerning my treatment and welfare, by calling the IRB chairman. I also understand that I may consult with a member of the public responsibility committee at any time concerning my treatment and welfare. The public responsibility committee is a group of volunteers who work to protect the rights and interests of clients.

I understand all of the above statements relating to my participation in the research entitled A Comparison of Judgment of Contingency and Cognitive Functioning (in other words, a comparison of problem-solving skills), being conducted by David Cobbs, M.A. I understand that he will be available to answer additional questions at any time by contacting him at (214) 490-4044 or (817) 565-2682, or by writing to him at Box 13587, NT Station, Denton, Texas 76203. Having read and understood this information, I hereby agree to participate in this project.

I understand each of the above items relating to the participation of ________________________ in the research of
Name of Subject

A Comparison of Judgment of Contingency and Cognitive

Functioning (in other words, a comparison of problem-solving skills), under the care of David Cobbs, M.A., and I hereby consent to my participation in the research project.

______________________________ ______________________

NAME OF SUBJECT DATE

I understand each of the above items relating to the participation of __________________________________ in the
Name of Subject

research of A Comparison of Judgment of Contingency and Cognitive Functioning (in other words, a comparison of problem-solving skills) being conducted by David Cobbs, M.A. I choose not to participate in this research project.

______________________________ ______________________

NAME OF SUBJECT DATE

I have explained the above items to

_____________________________________ and believe that
Name of Subject Giving Consent

_______________ understands each of the items.
he/she

______________________________ ______________________

INVESTIGATOR'S SIGNATURE DATE

We were present at the explanation of the above items to

____ and we believe
Name of Subject Giving Consent

that _________________ understands each of the above items.
he/she

____
WITNESS DATE

____
WITNESS DATE

§§

Having already begun my participation in the research entitled A Comparison of Judgment of Contingency and Cognitive Functioning (in other words, a comparison of problem-solving skills), being conducted by David Cobbs, M.A., I hereby choose to withdraw from further participation in this research project. I understand that my withdrawing will have no effect whatsoever on my treatment or length of stay at Terrell State Hospital, and that I will not be paid any cash.

__________________________ __________________
NAME OF SUBJECT DATE

We were present and have witnessed that

______________________________ has chosen to withdraw
Name of Subject Withdrawing Consent

from participating in this research study, and we believe

that ____________ understands that ____________
he/she his/her

withdrawal will have no effect whatsoever on

______________ treatment or length of stay at Terrell
his/her

State Hospital.

____

WITNESS DATE

____

WITNESS DATE

APPENDIX F

HOSPITAL STAFF INFORMED CONSENT FORM

Appendix F

HOSPITAL STAFF INFORMED CONSENT FORM

I, ____, have received an explanation of the research investigating individual problem-solving skills which is being conducted at Terrell State Hospital.

I understand that my participation will be limited to answering several brief questionnaires and completing five tasks, each consisting of thirty trials of pressing or not pressing a button in order to make a green light appear. The total time involved will be approximately one hour. I will be paid approximately five dollars for my participation, the exact amount depending upon my task performance.

I understand that all my responses will be kept confidential and that I will be referred to only by code in the data collected, so that my name will not be disclosed. I also understand that any information I give <u>will not</u> be shared with anyone other than the principal investigator, David Cobbs, M.A., and his immediate supervisor, Joseph Critelli, Ph.D.

However, I understand that the information I provide will be used for scientific research purposes only, and that the combined results gained from the approximately sixty participants may be published in scientific or professional journals, with no references being made as to individual identities.

I understand that my participation in this project will have no effect on my employment position at Terrell State Hospital, and that this research will not place my health at risk in any way. I further understand that there are no physiological, psychological, or social risks involved in this study.

I understand that my participation in this project is strictly voluntary and that I may withdraw at any time with no effects whatsoever on my employment position at Terrell State Hospital. I also understand that I will not be paid my cash earnings until the conclusion of the procedures presented to me, and that I must complete all of the tasks and questionnaires in order to claim any money.

I also understand that if I so choose, I will be given verbal feedback from Mr. Cobbs on the questionnaires I fill out as part of this research project. This feedback would be for the purpose of my own personal benefit and information, and would not be shared with anyone else unless I choose so myself. This information is strictly confidential and will in no way be shared with anyone else without my consent, nor will it place my employment position at Terrell State Hospital at risk in any way. Additionally, I understand that I may consult with a member of the Terrell State Hospital

Institutional Review Board (IRB) at any time concerning my participation in this study, by calling the IRB chairman.

I understand all of the above statements relating to my participation in the research entitled A Comparison of Judgment of Contingency and Cognitive Functioning (in other words, a comparison of problem-solving skills), being conducted by David Cobbs, M.A. I understand that he will be available to answer additional questions at any time by contacting him at (214) 490-4044 or (817) 565-2682, or by writing to him at Box 13587, NT Station, Denton, Texas 76203. Having read and understood this information, I hereby agree to participate in this project.

I understand each of the above items relating to the participation of

______________________ in the research of A Comparison
Name of Subject

of Judgment of Contingency and Cognitive Functioning (in other words, a comparison of problem-solving skills), under the care of David Cobbs, M.A., and I hereby consent to my participation in the research project.

______________________ ______________________
NAME OF SUBJECT DATE

I understand each of the above items relating to the participation of ______________________ in the
Name of Subject

research of A Comparison of Judgment of Contingency and Cognitive Functioning (in other words, a comparison of problem-solving skills) being conducted by David Cobbs, M.A. I choose not to participate in this research project.

NAME OF SUBJECT ______________________ DATE ______________________

I have explained the above items to ______________________ (Name of Subject Giving Consent) and believe that ____________ (he/she) understands each of the items.

INVESTIGATOR'S SIGNATURE ______________________ DATE ______________________

We were present at the explanation of the above items to ______________________ (Name of Subject Giving Consent) and we believe that ____________ (he/she) understands each of the above items.

WITNESS ______________________ DATE ______________________

WITNESS ______________________ DATE ______________________

§§

Having already begun my participation in the research entitled A Comparison of Judgment of Contingency and Cognitive Functioning (in other words, a comparison of problem-solving skills), being conducted by David Cobbs, M.A., I hereby choose to withdraw from further participation in this research project. I understand that my withdrawing will have no effect whatsoever on my employment position at Terrell State Hospital, and that I will not be paid any cash.

_______________________________ _______________________
NAME OF SUBJECT DATE

We were present and have witnessed that

___________________________________ has chosen to withdraw
Name of Subject Withdrawing Consent

from participating in this research study, and we believe

that _____________ understands that _____________
he/she his/her

withdrawal will have no effect whatsoever on

________________ employment position at Terrell State
his/her

Hospital.

_______________________________ _______________________
WITNESS DATE

_______________________________ _______________________
WITNESS DATE

APPENDIX G

PRE-EXPERIMENT QUESTIONNAIRE

Appendix G

Pre-experiment Questionnaire

Instruction: Please fill out the following questions as accurately as possible and answer all questions.

1. Please rate the degree of control you expect your responses (pressing and not pressing) will have over the green light onset.

   Use a scale of 0 to 100%. Remember 0% means no control, 100% means complete control, and percentages between 0 and 100 indicate differing amounts of partial control.

   ____________________%

2. If you press the button at the right times on this task, what % of the time do you believe the green light will come on?

   ____________________%

3. What do you feel is the likelihood of your being able to press the button at the right times on this task?

   Use a scale of 0 to 100%. Remember 0% indicates no possibility at all, 100% means a total certainty, and percentages between 0 and 100 indicate differing amounts of likelihood.

   ____________________%

APPENDIX H

JUDGMENT SCALES: FORM A (REWARD CONDITION)

Appendix H

Judgment Scales: Form A (Reward Condition)

Instructions: Please fill out the following questions as accurately as possible and answer all questions. Remember all questions refer to the task you have just completed.

1. Please rate the degree of control your responses (pressing and not pressing) had over the onset of the green light.

   Use a scale of 0 to 100%. Remember 0% means no control, 100% means complete control, and percentages between 0 and 100 indicate differing amounts of partial control.

   ____________________%

2. What degree of control do you feel you should have attained to make a successful performance?

   Use a scale of 0 to 100%.

   ____________________%

3. If you had pressed the button at the right times on this task, what % of the time do you think the light would have come on?

   ____________________%

4. What was the likelihood that you pressed the button at the right times?

   Use a scale of 0 to 100%. Remember 0% indicates no possibility at all, 100% means a total certainty, and percentages between 0 and 100 indicate differing amounts of likelihood.

   ____________________%

5. Based on your performance on this task, how much credit do you feel you deserve?

   Use a scale of 0 to 100%. 0% means no credit given, 100% means complete credit given, and percentages between 0 and 100 indicate differing amounts of credit deserved.

   ____________________%

APPENDIX I

JUDGMENT SCALES: FORM B (PUNISHMENT CONDITION)

Appendix I

Judgment Scales: Form B (Punishment Condition)

Instructions: Please fill out the following questions as accurately as possible and answer all questions. Remember all questions refer to the task you have just completed.

1. Please rate the degree of control your responses (pressing and not pressing) had over the onset of the green light.

   Use a scale of 0 to 100%. Remember 0% means no control, 100% means complete control, and percentages between 0 and 100 indicate differing amounts of partial control.

   ____________________%

2. What degree of control do you feel you should have attained to make a successful performance?

   Use a scale of 0 to 100%.

   ____________________%

3. If you had pressed the button at the right times on this task, what % of the time do you think the light would have come on?

   ____________________%

4. What was the likelihood that you pressed the button at the right times on this task?

   Use a scale of 0 to 100%. Remember 0% indicates no possibility at all, 100% means a total certainty, and percentages between 0 and 100 indicate differing amounts of likelihood.

   ____________________%

5. Based on your performance on this task, how much blame do you feel you deserve?

   Use a scale of 0 to 100%. 0% means no blame given, 100% means complete blame given, and percentages between 0 and 100 indicate differing amounts of blame deserved.

   ____________________%

APPENDIX J

ATTRIBUTION SCALES: FORM A (REWARD CONDITION)

Appendix J

Attribution Scales: Form A (Reward Condition)

Instructions: Answer the following questions using the scales provided. If you feel one end of the scale best describes your impression, circle the number corresponding to this end. Remember as you move towards the center, it means your impression becomes more neutral. Remember all questions refer to the task you have just completed.

1. Did you gain money because you tried especially hard or because you are always good at these kinds of tasks?

   1 2 3 4 5 6 7
   Tried hard ... Always good

2. Did you gain money because you tried especially hard or because you are always lucky at these kinds of tasks?

   1 2 3 4 5 6 7
   Tried hard ... Always lucky

3. Did you gain money because you are always lucky at these tasks or because these tasks are always easy?

   1 2 3 4 5 6 7
   Always lucky ... Always easy

4. Did you gain money because you are always good at these kinds of tasks or because these tasks are always easy?

   1 2 3 4 5 6 7
   Always good ... Always easy

APPENDIX K

ATTRIBUTION SCALES: FORM B (PUNISHMENT CONDITION)

Appendix K

Attribution Scales: Form B (Punishment Condition)

Instructions: Answer the following questions using the scales provided. If you feel one end of the scale best describes your impression, circle the number corresponding to this end. Remember as you move towards the center, it means your impression becomes more neutral. Remember all questions refer to the task you have just completed.

1. Did you lose money because you did not try especially hard or because you are always bad at these kinds of tasks?

1	2	3	4	5	6	7
Did not try hard						Always bad

2. Did you lose money because you did not try especially hard or because you are always unlucky at these kinds of tasks?

1	2	3	4	5	6	7
Did not try hard						Always unlucky

3. Did you lose money because you are always unlucky at these tasks or because these tasks are always difficult?

1	2	3	4	5	6	7
Always unlucky						Always difficult

4. Did you lose money because you are always bad at these kinds of tasks or because these tasks are always difficult?

1	2	3	4	5	6	7
Always bad						Always difficult

APPENDIX L

POST-TASK INSTRUCTIONS: FORM A (REWARD CONDITION)

Appendix L

Post-task instructions: Form A (Reward Condition)

"In order to encourage accuracy in your judgment of control, you can earn an additional $1.00 if you are accurate in your end-of-task judgment of control for each of the remaining tasks. We define accuracy as being within ten percentage points of the actual control. In other words, if you judge the degree of control accurately at the end of task 3, you earn an extra $1.00, another $1.00 for task 4, and another $1.00 for task 5. However, if you are inaccurate in your judgment of control on these tasks, you will not earn the extra money. Please note that your judgment of control will not affect the money you have earned every time the green light comes on. To summarize, you can earn money in two ways: to make the green light come on and to judge your control accurately. The experimenter will tell you whether you do or do not earn this extra money by showing your current cash earnings on the screen in front of you after you complete the questionnaires for the current task and before the beginning of the next task. Any questions? If no, please proceed to page 1 of Task 3 and let me know when you have finished."

APPENDIX M

POST-TASK INSTRUCTIONS: FORM B (PUNISHMENT CONDITION)

Appendix M

Post-task instructions: Form B (Punishment Condition)

"In order to encourage accuracy in your judgment of control, you will lose an additional $1.00 if you are inaccurate in your end-of-task judgment of control for each of the remaining tasks. We define accuracy as being within ten percentage points of the actual control. In other words, if you judge the degree of control inaccurately at the end of task 3, you lose an extra $1.00, another $1.00 for task 4, and another $1.00 for task 5. However, if you are accurate in your judgment of control on these tasks, you will not lose the extra money. Please note that your judgment of control will not affect the money you have lost every time the green light did not come on. To summarize, you can lose money in two ways: when the green light does not come on and when your judgment of control is inaccurate. The experimenter will tell you whether you do or do not lose this extra money by showing your current cash earnings on the screen in front of you after you complete the questionnaires for the current task and before the beginning of the next task. Any questions? If no, please proceed to page 1 of Task 3 and let me know when you have finished."

APPENDIX N

POST-EXPERIMENT INSTRUCTIONS

Appendix N

Instructions to subjects after completion of all tasks:

"We would like to get your impression of the experiment. Please work as quickly as you can through these questionnaires. Remember your first impression is the best answer. Do not spend too much time thinking about the item or checking over your answer."

****

Instructions: Please answer all questions as accurately as possible.

1. What do you think are the purposes or hypotheses of this study?

2. What responses did you feel the experimenter wanted you to make?

3. What are the factors affecting the green light onset?

4. To what extent were you trying to be accurate in your judgment of control over green light onset?

1	2	3	4	5	6	7
Did not try hard						Tried very hard

APPENDIX O

NAMES AND DEFINITIONS OF MAJOR VARIABLES

Appendix O

Names and Definitions of Major Variables

Variable	Definition
Expectancy of Control	Pre-task estimation of control
Outcome Expectancy	Pre-task estimation of light onset for optimal responses
Efficacy Expectancy	Pre-task estimation of the possibility of making an optimal set of responses
Accuracy of Judgment of Control without Reinf for accuracy	Post-task judgment of control - actual control average for the first 2 tasks
Simple Judgment of Control	Post-task judgment of control
Accuracy of Judgment of Control	Post-task judgment of control minus actual control
Self-correction	Absolute value of accuracy of judgment of control for Task 3 minus absolute value of accuracy of judgment of control for Task 5
Judgment of Efficacy	Post-task estimation of possibility of having made an optimal set of responses
Accuracy of Maximal Reinf	Post-task estimation of light onset during optimal responses minus actual maximal

Variable	Definition
	reinforcement frequency (80)
Evaluation of Peformance (credit/blame)	Post-task estimation of credit deserved (reward) or blame deserved (punishment)
Deviation from Subj Criterion for Success	Personally determined level of light onset judged necessary for a successful performance minus personal estimation of best response possible
Deviation from Obj Criterion for Success	Personally determined level of light onset judged necessary for a successful performance minus actual maximal reinforcement frequency (80)
Mood Changes	Post-task mood minus Pre-task mood
Self-esteem Changes	Post-task self-esteem minus Pre-task self-esteem

APPENDIX P

PREDICTIONS OF MAJOR COGNITIVE VARIABLES

Appendix P

Predictions of Major Cognitive Models

Variable	Beck	Reformulated Learned Helplessness	Hopelessness
Level of expectancy	Dep underestimate relative to Nondep	Dep underestimate relative to Nondep (softened claim from original '75 model)	Dep underestimate relative to Nondep
Efficacy/ Outcome expectancy	No specific prediction	Dep: high outcome, low efficacy Nondep: low outcome, high efficacy	Efficacy: no specific prediction Outcome: Dep lower than Nondep
Judgment of control	Dep underestimate relative to Nondep and to objective control	Weak hypoth: Dep underestimate relative to Nondep Strong hypoth: Dep underestimate relative to objective criterion	No specific prediction (cognitive deficit removed from theory)
Self-evaluation	Dep negatively evaluate performance relative to Nondep and to objective criterion	Dep negatively evaluate performance relative to Nondep and to objective criterion	No specific position to date
Attributions	Dep attrib negative events to self ("self-blame")	Dep attribute negative events to internal, stable, global	Dep attrib negative events to stable, global causes (theory deemphasizes attribution)

Note. Dep = Depressives
Nondep = Nondepressives

APPENDIX Q

TABLES

Appendix Q

Table Q-1

Means and Standard Deviations Across Tasks

Clinical Depressives				
Variable	Reward		Punishment	
N = 12	M	SD	M	SD
Level of Expectancy	61.75	26.39	54.48	22.70
Outcome Expectancy	72.42	24.81	64.17	29.73
Efficacy Expectancy	56.33	27.24	48.88	23.08
Acc Judg Cont w/o rein acc	17.71	4.27	16.17	3.93
Overall Judg of Control	59.00	29.75	52.97	27.41
Overall Acc Judg Cont	19.00	29.75	12.97	27.41
Self-correction	52.50	19.73	28.75	14.28
Judgment of Efficacy	57.33	29.35	52.88	21.82
Acc Mxml Reinf freq	1.73	27.01	-8.50	30.45
Credit(rew)/Blame (pun)	62.38	32.20	41.72	34.42
Subj Criterion for Succ	80.92	26.50	69.08	24.42
Dev Subj Criterion Succ	-0.82	38.03	-2.42	21.96
Dev Obj Criterion Succ	0.92	26.43	-10.92	21.38
Stability	7.10	2.57	8.85	1.41
Locus of Control	7.30	2.28	8.58	1.13
Post Anxiety	59.83	22.76	60.42	28.11

Appendix Q--Continued

Variable N = 12	Reward		Punishment	
	M	SD	M	SD
Post Depression	64.42	29.18	63.67	28.84
Post Hostility	59.83	27.49	58.00	27.31
Change Anxiety	-2.83	2.69	-2.17	2.48
Change Depression	-2.25	2.47	-6.58	4.03
Change Hostility	-1.25	2.07	-6.00	4.21
Post Self-esteem	45.25	16.28	41.83	14.91
Change Self-esteem	0.42	2.26	4.17	8.02

Nondepressed Normals				
Variable	Reward		Punishment	
N = 12	M	SD	M	SD
Level of Expectancy	59.17	15.55	50.58	34.25
Outcome Expectancy	78.33	22.60	63.00	25.61
Efficacy Expectancy	59.33	17.30	52.92	21.57
Acc Judg Cont w/o rein acc	11.25	17.21	13.96	19.61
Overall Judg of Control	55.25	20.13	54.17	30.99
Overall Acc Judg Cont	15.25	20.13	14.17	30.99
Self-correction	43.75	34.81	30.83	31.07
Judgment of Efficacy	59.75	17.56	53.25	27.24
Acc Mxml Reinf freq	3.00	22.40	70.88	28.17
Credit(rew)/Blame (pun)	66.37	19.08	34.92	32.38
Subj Criterion for Succ	75.97	21.58	79.85	18.32
Dev Subj Criterion Succ	-7.03	22.81	8.97	28.84
Dev Obj Criterion Succ	-4.03	21.58	-0.15	18.32
Stability	6.18	1.86	8.32	1.34
Locus of Control	6.77	1.69	8.03	1.20
Post Anxiety	47.67	19.49	53.50	20.81
Post Depression	48.58	20.06	58.25	27.49
Post Hostility	48.42	19.98	56.17	27.13
Change Anxiety	2.00	2.39	6.17	4.73

Variable _N_ = 12	Reward		Punishment	
	_M_	_SD_	_M_	_SD_
Change Depression	1.75	2.16	5.33	3.68
Change Hostility	2.83	2.91	5.75	3.67
Post Self-esteem	57.58	8.92	53.33	8.69
Change Self-esteem	-0.58	2.41	2.42	5.06

Nondepressed Schizophrenics				
Variable	Reward		Punishment	
N = 12	M	SD	M	SD
Level of Expectancy	57.08	30.52	54.48	22.70
Outcome Expectancy	67.47	29.76	66.17	27.32
Efficacy Expectancy	58.48	29.20	51.07	24.00
Acc Judg Cont w/o rein acc	16.46	22.09	6.25	11.64
Overall Judg of Control	54.27	34.57	51.70	29.58
Overall Acc Judg Cont	14.27	34.57	11.70	29.58
Self-correction	28.08	29.10	44.92	26.31
Judgment of Efficacy	54.77	29.24	54.63	25.16
Acc Mxml Reinf freq	-7.95	29.90	72.15	27.06
Credit(rew)/Blame (pun)	63.70	31.32	32.83	33.66
Subj Criterion for Succ	66.17	31.77	64.25	27.96
Dev Subj Criterion Succ	-5.88	29.16	-7.90	25.19
Dev Obj Criterion Succ	-13.83	31.77	-15.75	27.96
Stability	7.40	2.56	9.07	1.51
Locus of Control	7.75	2.66	8.62	1.35
Post Anxiety	57.33	21.83	56.92	20.98
Post Depression	59.00	28.43	62.25	30.02
Post Hostility	59.00	28.43	58.67	27.74
Change Anxiety	-4.58	3.21	-2.75	2.61

Variable $\underline{N}$ = 12	Reward		Punishment	
	$\underline{M}$	$\underline{SD}$	$\underline{M}$	$\underline{SD}$
Change Depression	-1.42	2.02	1.08	1.99
Change Hostility	-2.08	2.65	0.00	.94
Post Self-esteem	48.25	9.21	47.92	9.41
Change Self-esteem	-2.00	3.81	-0.58	2.41

Table Q-2

Means and Standard Deviations for Differing Degrees of Task Control

Task 1 (80-60, 20% Control) - Clinical Depressives				
Variable	Reward		Punishment	
N = 24	*M*	*SD*	*M*	*SD*
Level of Expectancy	50.00	19.19	58.33	16.97
Outcome Expectancy	43.33	24.34	61.67	24.89
Efficacy Expectancy	42.08	27.26	38.75	17.07
Simple Judg of Control	60.42	30.93	52.33	26.92
Acc of Judg of Cont	40.42	30.93	32.33	26.92
Judgment of Efficacy	64.17	28.75	49.42	23.75
Acc Maximal Reinf freq	2.50	25.18	-3.75	24.41
Credit(rew)/Blame (pun)	71.67	29.41	48.33	35.12
Subj Criterion for Succ	79.17	25.92	72.50	19.13
Dev Subj Criterion Succ	-3.33	40.81	-3.75	26.21
Dev Obj Criterion Succ	-0.83	25.92	-7.50	19.13
Stability	7.17	2.04	9.08	1.51
Locus of Control	7.00	2.49	8.75	1.60

Task 2 (80-20, 60% Control) - Clinical Depressives				
Variable	Reward		Punishment	
_N_ = 24	_M_	_SD_	_M_	_SD_
Level of Expectancy	66.50	29.12	54.50	22.31
Outcome Expectancy	72.50	29.81	72.08	29.65
Efficacy Expectancy	57.92	30.71	44.83	24.70
Simple Judg of Control	55.00	36.68	60.00	26.02
Acc of Judg of Cont	-5.00	36.68	0.00	26.02
Judgment of Efficacy	50.00	31.62	53.33	19.23
Acc Maximal Reinf freq	79.17	29.53	73.33	29.34
Credit(rew)/Blame (pun)	51.08	38.70	38.75	33.31
Subj Criterion for Succ	83.75	24.60	71.67	17.36
Dev Subj Criterion Succ	4.58	40.65	-1.67	27.33
Dev Obj Criterion Succ	3.75	24.60	-8.33	17.36
Stability	7.17	2.41	9.25	1.48
Locus of Control	7.50	2.24	8.33	0.89

Task 3 (80-80, 0% Control) - Clinical Depressives

Variable	Reward		Punishment	
N = 24	M	SD	M	SD
Level of Expectancy	63.75	27.06	55.83	21.09
Outcome Expectancy	85.00	20.34	62.92	29.73
Efficacy Expectancy	56.67	24.89	52.50	22.31
Simple Judg of Control	73.33	26.74	54.17	27.37
Acc of Judg of Cont	73.33	26.74	54.17	27.37
Judgment of Efficacy	65.83	28.19	62.92	25.80
Acc Maximal Reinf freq	1.17	31.85	-3.75	27.89
Credit(rew)/Blame (pun)	74.58	27.34	42.50	34.94
Subj Criterion for Succ	83.33	26.66	69.58	25.45
Dev Subj Criterion Succ	2.17	42.01	-6.67	24.25
Dev Obj Criterion Succ	3.33	26.66	-10.42	25.45
Stability	7.33	2.53	8.75	1.76
Locus of Control	7.50	2.11	8.67	1.37

Task 4 (80-0, 80% Control) - Clinical Depressives				
Variable	Reward		Punishment	
N = 24	M	SD	M	SD
Level of Expectancy	67.50	27.34	55.00	24.68
Outcome Expectancy	77.92	27.67	61.67	33.60
Efficacy Expectancy	62.08	26.75	59.17	27.04
Simple Judg of Control	55.42	31.66	49.58	27.67
Acc of Judg of Cont	-24.58	31.66	-30.42	27.67
Judgment of Efficacy	55.42	32.78	45.00	19.77
Acc Maximal Reinf freq	7.08	20.05	-16.25	35.75
Credit(rew)/Blame (pun)	53.75	37.18	39.17	32.04
Subj Criterion for Succ	81.67	27.16	71.67	28.23
Dev Subj Criterion Succ	-5.42	27.83	7.92	18.02
Dev Obj Criterion Succ	1.67	27.16	-8.33	28.23
Stability	6.92	2.94	8.75	1.29
Locus of Control	7.25	2.30	8.50	0.90

Task 5 (80-40, 40% Control) - Clinical Depressives				
Variable	Reward		Punishment	
N = 24	M	SD	M	SD
Level of Expectancy	60.00	29.23	48.75	28.45
Outcome Expectancy	83.33	21.88	62.50	30.79
Efficacy Expectancy	62.92	26.58	49.17	24.29
Simple Judg of Control	50.83	22.75	48.75	29.09
Acc of Judg of Cont	10.83	22.75	8.75	29.09
Judgment of Efficacy	51.25	25.42	53.75	20.57
Acc Maximal Reinf freq	-1.25	28.45	-12.08	34.87
Credit(rew)/Blame (pun)	60.83	28.35	39.83	36.68
Subj Criterion for Succ	76.67	28.15	60.00	31.91
Dev Subj Criterion Succ	-2.08	38.82	-7.92	14.05
Dev Obj Criterion Succ	-3.33	28.15	-20.00	31.91
Stability	6.92	2.94	8.42	1.00
Locus of Control	7.25	2.26	8.67	0.89

Task 1 (80-60, 20% Control) - Nondepressed Normals				
Variable	Reward		Punishment	
$\underline{N}$ = 24	$\underline{M}$	$\underline{SD}$	$\underline{M}$	$\underline{SD}$
Level of Expectancy	53.75	12.45	39.58	30.93
Outcome Expectancy	73.75	24.04	52.50	24.36
Efficacy Expectancy	50.00	20.56	54.58	20.17
Simple Judg of Control	52.92	22.10	53.75	36.13
Acc of Judg of Cont	32.92	22.10	33.75	36.13
Judgment of Efficacy	64.17	19.17	55.00	29.92
Acc Maximal Reinf freq	8.33	18.01	-8.75	29.78
Credit(rew)/Blame (pun)	66.00	17.84	32.50	39.40
Subj Criterion for Succ	76.50	18.51	74.83	29.85
Dev Subj Criterion Succ	-11.83	18.12	3.58	44.43
Dev Obj Criterion Succ	-3.50	18.51	-5.17	29.85
Stability	6.67	2.15	8.42	0.90
Locus of Control	6.67	1.50	7.92	1.08

Task 2 (80-20, 60% Control) - Nondepressed Normals				
Variable	Reward		Punishment	
$\underline{N}$ = 24	$\underline{M}$	$\underline{SD}$	$\underline{M}$	$\underline{SD}$
Level of Expectancy	57.50	11.58	45.83	40.10
Outcome Expectancy	82.50	21.37	69.17	27.21
Efficacy Expectancy	62.50	15.30	53.33	20.60
Simple Judg of Control	49.58	20.28	54.17	27.46
Acc of Judg of Cont	-10.42	20.28	-5.83	27.46
Judgment of Efficacy	60.00	14.62	55.42	21.47
Acc Maximal Reinf freq	6.67	23.87	76.25	27.06
Credit(rew)/Blame (pun)	67.08	21.26	42.08	26.75
Subj Criterion for Succ	80.42	21.05	81.67	13.71
Dev Subj Criterion Succ	-6.25	24.87	5.42	21.79
Dev Obj Criterion Succ	0.42	21.05	1.67	13.71
Stability	5.67	2.15	8.42	1.16
Locus of Control	6.83	1.64	8.17	0.72

Task 3 (80-80, 0% Control) - Nondepressed Normals				
Variable	Reward		Punishment	
_N_ = 24	_M_	_SD_	_M_	_SD_
Level of Expectancy	63.75	12.81	53.33	36.70
Outcome Expectancy	88.33	15.86	57.92	24.63
Efficacy Expectancy	62.92	16.30	55.42	18.40
Simple Judg of Control	68.75	13.51	62.08	30.41
Acc of Judg of Cont	68.75	13.51	62.08	30.41
Judgment of Efficacy	62.92	9.64	54.17	31.03
Acc Maximal Reinf freq	10.83	17.30	-12.09	31.51
Credit(rew)/Blame (pun)	73.75	15.54	28.75	26.47
Subj Criterion for Succ	81.25	18.60	82.92	11.77
Dev Subj Criterion Succ	-9.58	21.37	15.00	34.90
Dev Obj Criterion Succ	1.25	18.60	2.92	11.77
Stability	6.08	2.02	7.92	1.88
Locus of Control	6.75	1.22	8.17	1.34

Task 4 (80-0, 80% Control) - Nondepressed Normals				
Variable	Reward		Punishment	
N = 24	M	SD	M	SD
Level of Expectancy	64.58	17.25	59.17	29.06
Outcome Expectancy	74.58	24.81	73.33	25.88
Efficacy Expectancy	64.58	16.71	51.67	24.53
Simple Judg of Control	53.33	17.62	42.08	31.08
Acc of Judg of Cont	-26.67	17.62	-37.92	31.08
Judgment of Efficacy	53.75	26.21	52.92	24.44
Acc Maximal Reinf freq	-6.25	25.15	-10.83	26.70
Credit(rew)/Blame (pun)	58.75	23.56	38.33	32.78
Subj Criterion for Succ	67.50	28.64	78.17	19.55
Dev Subj Criterion Succ	-6.25	20.35	9.00	23.82
Dev Obj Criterion Succ	-12.50	28.64	-1.83	19.55
Stability	6.58	1.62	8.17	1.53
Locus of Control	6.25	2.01	7.92	1.31

Task 5 (80-40, 40% Control) - Nondepressed Normals				
Variable	Reward		Punishment	
N = 24	M	SD	M	SD
Level of Expectancy	56.25	23.66	55.00	34.44
Outcome Expectancy	72.50	26.93	62.08	25.98
Efficacy Expectancy	56.67	17.62	49.58	24.16
Simple Judg of Control	51.67	27.16	58.75	29.86
Acc of Judg of Cont	11.67	27.16	18.75	29.86
Judgment of Efficacy	57.92	18.15	48.75	29.32
Acc Maximal Reinf freq	-4.58	27.67	-10.17	25.79
Credit(rew)/Blame (pun)	66.25	17.21	32.92	36.52
Subj Criterion for Succ	74.17	21.09	81.67	16.70
Dev Subj Criterion Succ	-1.25	29.32	11.83	19.27
Dev Obj Criterion Succ	-5.83	21.09	1.67	16.70
Stability	5.92	1.38	8.67	1.23
Locus of Control	6.33	2.06	8.00	1.54

Task 1 (80-60, 20% Control) - Nondepressed Schizophrenics				
Variable	Reward		Punishment	
N = 24	M	SD	M	SD
Level of Expectancy	54.58	30.86	38.33	29.18
Outcome Expectancy	53.67	26.94	54.17	25.75
Efficacy Expectancy	49.25	31.03	47.08	27.59
Simple Judg of Control	63.33	31.50	51.67	35.31
Acc of Judg of Cont	43.33	31.50	31.67	35.31
Judgment of Efficacy	49.50	31.22	57.08	30.03
Acc Maximal Reinf freq	-17.83	37.72	-27.08	32.01
Credit(rew)/Blame (pun)	73.33	27.08	30.83	39.93
Subj Criterion for Succ	61.67	31.21	54.58	29.88
Dev Subj Criterion Succ	-0.50	23.11	1.67	16.70
Dev Obj Criterion Succ	-18.33	31.21	-25.42	29.88
Stability	8.25	2.77	9.25	1.42
Locus of Control	6.67	2.50	7.92	1.38

Task 2 (80-20, 60% Control) - Nondepressed Schizophrenics				
Variable	Reward		Punishment	
N = 24	M	SD	M	SD
Level of Expectancy	57.83	32.28	41.67	31.07
Outcome Expectancy	75.83	29.91	65.00	28.20
Efficacy Expectancy	68.25	26.58	54.17	24.85
Simple Judg of Control	49.58	36.46	40.83	29.06
Acc of Judg of Cont	-10.42	36.46	-19.17	29.06
Judgment of Efficacy	57.92	29.20	47.08	21.58
Acc Maximal Reinf freq	-2.17	24.55	-13.42	34.58
Credit(rew)/Blame (pun)	51.50	28.96	37.50	39.11
Subj Criterion for Succ	56.67	31.93	60.83	33.36
Dev Subj Criterion Succ	-21.17	31.30	-5.75	38.97
Dev Obj Criterion Succ	-23.33	31.93	-19.17	33.36
Stability	7.50	2.07	9.08	1.56
Locus of Control	7.58	2.54	8.33	1.30

Task 3 (80-80, 0% Control) - Nondepressed Schizophrenics				
Variable	Reward		Punishment	
N = 24	*M*	*SD*	*M*	*SD*
Level of Expectancy	69.58	22.51	42.50	29.04
Outcome Expectancy	63.33	32.78	69.17	28.83
Efficacy Expectancy	56.08	29.55	52.08	22.00
Simple Judg of Control	59.33	34.77	64.00	33.32
Acc of Judg of Cont	59.33	34.77	64.00	33.32
Judgment of Efficacy	61.50	30.90	63.75	24.13
Acc Maximal Reinf freq	-2.50	24.26	9.17	15.79
Credit(rew)/Blame (pun)	65.00	37.60	27.92	27.09
Subj Criterion for Succ	73.75	32.27	68.33	24.34
Dev Subj Criterion Succ	-3.75	38.44	-20.83	22.04
Dev Obj Criterion Succ	-6.25	32.27	-11.67	24.34
Stability	7.25	1.76	8.83	1.34
Locus of Control	8.08	2.84	9.50	1.62

Task 4 (80-0, 80% Control) - Nondepressed Schizophrenics				
Variable	Reward		Punishment	
N = 24	M	SD	M	SD
Level of Expectancy	51.17	33.34	48.33	29.87
Outcome Expectancy	74.08	23.67	75.83	25.03
Efficacy Expectancy	59.33	21.04	54.08	23.25
Simple Judg of Control	46.17	37.01	53.75	26.21
Acc of Judg of Cont	-33.83	37.01	-26.25	26.21
Judgment of Efficacy	56.58	25.64	43.58	25.51
Acc Maximal Reinf freq	-5.17	30.97	-2.08	26.92
Credit(rew)/Blame (pun)	60.00	33.98	32.92	31.87
Subj Criterion for Succ	64.17	35.02	72.50	21.27
Dev Subj Criterion Succ	-10.67	17.42	-5.42	21.05
Dev Obj Criterion Succ	-15.83	35.02	-7.50	21.27
Stability	6.67	2.31	9.25	2.01
Locus of Control	8.17	2.48	8.92	1.44

Task 5 (80-40, 40% Control) - Nondepressed Schizophrenics

Variable N = 24	Reward M	Reward SD	Punishment M	Punishment SD
Level of Expectancy	52.25	33.61	43.17	27.39
Outcome Expectancy	70.42	35.51	66.67	28.79
Efficacy Expectancy	59.50	37.77	47.92	22.31
Simple Judg of Control	52.92	33.13	48.25	24.00
Acc of Judg of Cont	12.92	33.13	8.25	24.00
Judgment of Efficacy	48.33	29.26	61.67	24.53
Acc Maximal Reinf freq	-12.08	32.01	-15.83	26.01
Credit(rew)/Blame (pun)	68.67	28.97	35.00	30.30
Subj Criterion for Succ	74.58	28.40	65.00	30.53
Dev Subj Criterion Succ	6.67	35.51	-9.17	27.21
Dev Obj Criterion Succ	-5.42	28.40	-15.00	30.53
Stability	7.33	1.56	8.92	1.24
Locus of Control	8.25	2.93	8.42	1.00

Table Q-3

Intercorrelations Among Major Variables

	Expectancy and Attribution				
Variable	Lvl of Expect	Outcome Expect	Effic Expect	Stabil	Loc of Cont
Age	NS	NS	NS	NS	-.28*
BDI	NS	NS	NS	NS	NS
BPRS	NS	-.29*	NS	NS	NS
Desir Control	NS	NS	NS	NS	NS
Level Expect	1.00****	.45****	.72****	NS	NS
Outcome Expect	.45****	1.00****	.60****	NS	NS
Effic Expect	.72****	.60****	1.00****	NS	NS
Acc Judg Cont	.83****	.41****	.72****	NS	NS
Self-correct	.47****	NS	.31**	NS	NS
Judg Efficacy	.70****	.53****	.82****	NS	NS
Acc Mx Rein freq	.33***	.77****	.40****	NS	NS
Reinf Freq	.29*	.23*	.25*	-.50****	-.33***
Credit given	.69****	.54****	.69****	NS	NS
Blame given	NS	NS	NS	NS	NS
Dev Subj Succ	NS	NS	NS	NS	NS
Dev Obj Succ	.51****	.57****	.59****	NS	NS
Stability	NS	NS	NS	1.00****	.77****
Locus Control	NS	NS	NS	.77****	1.00****

Variable	Lvl of Expect	Outcome Expect	Effic Expect	Stabil	Loc of Cont
Post Anxiety	NS	NS	NS	NS	NS
Post Depr	NS	NS	NS	.27*	NS
Post Hostil	NS	NS	NS	NS	NS
Change Anx	NS	NS	NS	NS	NS
Change Depr	NS	NS	NS	NS	NS
Change Hostil	NS	NS	NS	NS	NS
Post Self-est	NS	NS	NS	NS	NS
Change Self-est	NS	NS	NS	NS	.25*

* = $p < .05$; ** = $p < .01$; *** = $p < .005$;
**** = $p < .001$; NS = Nonsignificant

Judgment of Control and Reinforcement Frequency					
Variable	Acc of Control	Self-Correct	Acc Mxml Rein	Judg of Effic	Rein Freq
Age	NS	NS	NS	NS	NS
BDI	NS	NS	NS	NS	NS
BPRS	NS	NS	NS	NS	NS
Desir Control	NS	NS	NS	NS	NS
Level Expect	.83****	.47****	.33***	.70****	.29*
Outcome Expect	.41****	NS	.77****	.53****	.23*
Effic Expect	.72****	.31***	.40****	.82****	.25*
Acc Judg Cont	1.00****	.66****	.33***	.74****	NS
Self-correct	.66****	1.00****	NS	.29*	NS
Judg Efficacy	.74****	.29*	.46****	1.00****	NS
Acc Mx Rein Freq	.33***	NS	1.00****	.46****	NS
Reinf Freq	NS	NS	NS	NS	1.00****
Credit given	.74****	.45**	.39*	.72****	NS
Blame given	NS	NS	NS	.45**	NS
Dev Subj Succ	NS	NS	-.51****	NS	NS
Dev Obj Succ	.54****	.30**	.50****	.58****	NS
Stability	NS	NS	NS	NS	-.50****
Locus Control	NS	NS	NS	NS	-.33***
Post Anxiety	NS	NS	NS	NS	NS
Post Depr	NS	NS	NS	NS	NS

Variable	Acc of Control	Self-Correct	Acc Mxml Rein	Judg of Effic	Rein Freq
Post Hostil	NS	NS	NS	NS	NS
Change Anx	NS	NS	NS	NS	NS
Change Depr	NS	NS	NS	NS	NS
Change Hostil	NS	NS	NS	NS	NS
Post Self-est	NS	NS	NS	.25*	NS
Change Self-est	NS	NS	NS	NS	NS

* = $p < .05$; ** = $p < .01$; *** = $p < .005$;
**** = $p < .001$; NS = Nonsignificant

Evaluation of Performance and Desirability of Control					
Variable	Credit	Blame	Dev Subj	Dev Obj	Desir of Control
Age	-.38*	NS	NS	NS	NS
BDI	NS	NS	NS	NS	NS
BPRS	NS	NS	NS	NS	NS
Desir Control	NS	NS	NS	.27*	1.00****
Level Expect	.69****	NS	.74****	NS	NS
Outcome Expect	.54***	NS	NS	.57****	NS
Effic Expect	.69****	NS	NS	.59****	NS
Acc Judg Cont	.74****	NS	NS	.54****	NS
Self-correct	.45**	NS	NS	.30**	NS
Judg Efficacy	.72****	.45***	NS	.58****	NS
Acc Mx Rein Freq	.39*	NS	-.51****	.50****	NS
Reinf Freq	NS	NS	NS	NS	NS
Credit given	1.00****	NS	NS	.54****	NS
Blame given	NS	1.00****	NS	NS	NS
Dev Subj Succ	NS	NS	1.00****	.49****	NS
Dev Obj Succ	.54****	NS	.49****	1.00****	.27*
Stability	NS	NS	NS	NS	NS
Locus Control	NS	NS	NS	NS	NS
Post Anxiety	NS	NS	NS	NS	NS
Post Depr	NS	.33*	NS	NS	NS

Variable	Credit	Blame	Dev Subj	Dev Obj	Desir of Control
Post Hostil	NS	.35*	NS	NS	NS
Change Anx	NS	NS	NS	NS	NS
Change Depr	NS	NS	NS	NS	NS
Change Hostil	NS	NS	NS	NS	NS
Post Self-est	NS	NS	NS	NS	.41****
Change Self-est	NS	NS	NS	NS	NS

* = $p < .05$; ** = $p < .01$; *** = $p < .005$;
**** = $p < .001$; NS = Nonsignificant

BDI, BPRS, and Post-Mood					
Variable	BDI	BPRS	Post Anx	Post Depr	Post Hostility
Age	NS	NS	NS	NS	NS
BDI	1.00****	NS	NS	-.30*	-.35**
BPRS	NS	1.00****	NS	NS	NS
Desir Control	NS	NS	NS	NS	NS
Level Expect	NS	NS	NS	NS	NS
Outcome Expect	NS	-.29*	NS	NS	NS
Effic Expect	NS	NS	NS	NS	NS
Acc Judg Cont	NS	NS	NS	NS	NS
Self-correct	NS	NS	NS	NS	NS
Judg Efficacy	NS	NS	NS	NS	NS
Acc Mx Reinf Freq	NS	NS	NS	NS	NS
Reinf Freq	NS	.32*	NS	NS	NS
Credit given	NS	NS	NS	NS	NS
Blame given	NS	NS	NS	NS	NS
Dev Subj Succ	NS	NS	NS	NS	NS
Dev Obj Succ	NS	NS	NS	NS	NS
Stability	NS	NS	NS	NS	NS
Locus Control	NS	NS	NS	NS	NS
Post Anxiety	.44****	NS	NS	NS	NS
Post Depr	.43****	NS	NS	NS	NS

Variable	BDI	BPRS	Post Anx	Post Depr	Post Hostility
Post Hostil	.31**	NS	NS	NS	NS
Change Anx	NS	NS	1.00****	.64****	.69****
Change Depr	-.30*	NS	.64****	1.00****	.67****
Change Hostil	-.35***	NS	.69****	.67****	1.00****
Post Self-est	-.47****	NS	NS	.24*	NS
Change Self-est	.26*	NS	NS	NS	NS

* = p < .05; ** = p < .01; *** = p < .005;
**** = p < .001; NS = Nonsignificant

Mood Change and Self-Esteem					
Variable	Change Anxiety	Change Depr	Change Hostil	Post Slf-Est	Change Slf-Est
Age	NS	NS	NS	NS	NS
BDI	NS	-.30*	-.35**	-.47****	.26*
BPRS	NS	NS	NS	NS	NS
Desir Control	NS	NS	NS	NS	NS
Level Expect	NS	NS	NS	NS	NS
Outcome Expect	NS	NS	NS	NS	NS
Effic Expect	NS	NS	NS	NS	NS
Acc Judg Cont	NS	NS	NS	NS	NS
Self-correct	NS	NS	NS	NS	NS
Judg Efficacy	NS	NS	NS	.25*	NS
Acc Mx Reinf Freq	NS	NS	NS	NS	NS
Reinf Freq	NS	NS	NS	NS	NS
Credit given	NS	NS	NS	NS	NS
Blame given	NS	NS	NS	NS	NS
Dev Subj Succ	NS	NS	NS	NS	NS
Dev Obj Succ	NS	NS	NS	NS	NS
Stability	NS	NS	NS	NS	NS
Locus Control	NS	NS	NS	NS	.25*
Post Anxiety	NS	NS	NS	-.42****	NS
Post Depr	NS	NS	NS	-.41****	NS

Variable	Change Anxiety	Change Depr	Change Hostil	Post Slf-Est	Change Slf-Est
Post Hostil	NS	NS	NS	-.39****	.27*
Change Anx	1.00****	.64****	.69****	NS	NS
Change Depr	.64****	1.00****	.67****	.24*	NS
Change Hostil	.69****	.67****	1.00****	NS	NS
Post Self-est	NS	.24*	NS	1.00****	NS
Change Self-est	NS	NS	NS	NS	1.00****

* = p < .05; ** = p < .01; *** = p < .005;
**** = p < .001; NS = Nonsignificant

APPENDIX R

FIGURE 1

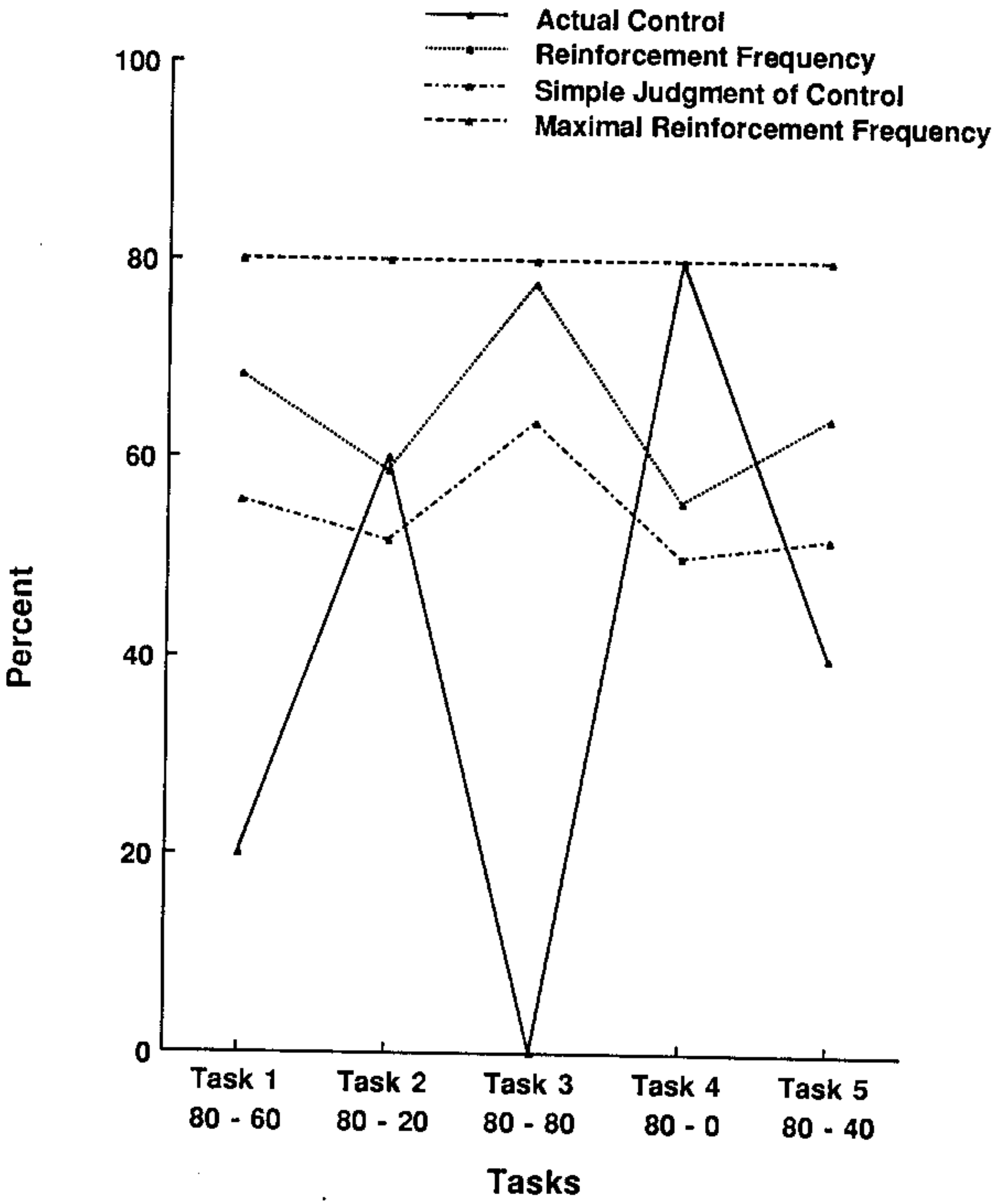

Figure 1. Relationships across tasks among simple judgment of control, reinforcement frequency, maximal reinforcement frequency, and actual control.

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