JUDGMENT OF CONTINGENCY IN HOSPITALIZED DEPRESSIVES

DISSERTATION

Presented to the Graduate Council of the
University of North Texas in Partial
Fulfillment of the Requirements

For the Degree of

DOCTOR OF PHILOSOPHY

By

Juliana Soh-Chiew Ee, B.A., M.S.
Denton, Texas
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Numerous investigations with college students have found that mild depressives perceive environmental contingencies more accurately than do nondepressives. The present study explores this 'depressive realism' phenomenon in a hospitalized sample. One hundred and fifty subjects comprising patients with a diagnosis of major depression, mildly and non-depressed patients with a diagnosis of schizophrenia, and mildly and non-depressed medical/surgical patients were presented with five tasks of varying degrees of objective control. Subjects judged the degree of control their responses (pressing or not pressing a button) had over the task outcome (turning on a light). Monetary reinforcement was provided for light onset as well as for accurate judgments of control. Expectancy of control, expectancy of accuracy in control judgments, and immediate self-esteem measures were collected throughout the experiment. After each task, subjects were told whether their control estimates were accurate, and measures of esteem and praise/blame were taken following feedback.

The results did not support the 'depressive realism'

phenomenon. Subjects' judgment of control accuracy did not vary as a function of depression level. Subjects generally overestimated on low and underestimated on high objective control tasks. They appeared unresponsive to feedback and cash incentives and were not able to improve subsequent judgments of control. Regression analyses identified expectancy of control and frequency of light onset, but not actual control, as consistent predictors of judgment of control for each of the five tasks.

Contrary to speculations that relative accuracy in judging control derives from a lack of motivation to protect self-esteem, current results indicated that mildly depressed patients used overestimation of control to enhance self-esteem as did the nondepressed patients. Their pattern of attributing praise/blame after success/failure feedback also reflected self-esteem enhancement. For the clinical depressives, changes in self-esteem in response to over/underestimation of control and accuracy feedback, and patterns of assigning self-praise/blame indicated a tendency toward self-esteem protection/maintenance.

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CHAPTER I

INTRODUCTION

With U.S. prevalence figures of 1 to 2 percent for men and 3 to 5 percent for women (Myers et al., 1984), major depression is the most common psychiatric disorder seen by mental health professionals. Moreover, the percentage of Americans at risk for a depression of clinical severity during their lifespan has been estimated at 10 percent for men and over 20 percent for women (Boyd & Weissman, 1981). Depression is second only to schizophrenia in frequency of first and second admissions to mental hospitals, and it accounts for as much as one-third of the outpatient population (Woodruff, Clayton, & Guze, 1975). In addition, depression is a common by-product of various medical conditions and a secondary problem in many psychiatric syndromes.

As would be expected, depression incurs a variety of costs. Financially, costs include direct treatment expenses as well as losses through lowered productivity, job absenteeism, and withdrawal from the workforce. The costs of treatment and loss of time at work have been estimated at 10 billion dollars per year (Teuting, Koslow, & Hirschfeld, 1981). Other costs of depression include grief and pain; interpersonal, marital, and family conflict; physical

illness; injuries from careless behaviors; self-destructive acts; and death through suicide (Mendlewicz, 1989; Munoz, 1987).

Many explanations have been offered for the origin of depression. Freud (1957), for example, argued that depression results from anger turned inward following the experience of symbolic or real loss of a love object. Fenichel (1945) described depressed persons as love addicts, starving for attention and love. In behavioral terms, Lewinsohn (1974, 1975) theorized that depression results from reduced response-contingent positive reinforcement. In addition to these psychological factors, biochemical causes have also been implicated (e.g., Akiskal & McKinney, 1975).

Within psychology, cognitive models of the etiology, maintenance, and treatment of depression have gained increased attention over the last decade (e.g., Abramson, Seligman, & Teasdale, 1978; Alloy & Abramson, 1979; Derry & Kuiper, 1981; Kovacs, Rush, Beck, & Hollon, 1981; Peterson & Seligman, 1984). One reason for this burgeoning attention is the demonstrated efficacy of cognitive therapy. Rush, Beck, Kovacs, and Hollon (1977) showed that, with outpatients, cognitive therapy is a more effective treatment for depression than pharmacological approaches. Subsequent research has furnished additional support for the efficacy of cognitive therapy with depressives (Beck, Rush, Shaw, & Emery, 1979; Blackburn, Bishop, Glen, Whalley, & Christie,

1981; Kovacs et al., 1981; Murphy, Simons, Wetzel, & Lustman, 1984; Shaw, 1977; Taylor & Marshall, 1977).

Two major cognitive approaches to depression, Beck's (1967, 1976, 1987) theory and the reformulated learned helplessness theory (Abramson et al., 1978; Seligman, 1975), have been the impetus for much of the psychological research on depression.

Major Cognitive Theories of Depression

Beck's theory. The cognitive model proposed by Beck (1967, 1976, 1987) regards depression as resulting from the activation of negative and distorted thought patterns through which individuals view themselves, their world, and their future. These thought patterns precipitate the affective and motivational changes associated with the disorder. According to Beck, depressed patients have a systematic negative bias in their evaluations of self-worth. They see themselves as unworthy, ineffectual, and inadequate. They assume total responsibility for unpleasant events, tracing the cause of these events to personal unchangeable deficits. Preoccupation with these deficits leads to further self-derogation and demoralization.

Depressed individuals perceive their environment as presenting impassable obstacles to their attainment of the good things in life. They relentlessly misinterpret situations in a negative direction, disregarding the possibility that more positive interpretations are

available. Pessimism also colors their view of the future. For them, the future is bleak, filled with the never-ending problems of the present. Expecting defeat and failure in their struggle with the world, they become less motivated to exert effort in the task of living. Typical ways of coping with such negative expectations include seeking less demanding activities and withdrawal. Finally, suicidal wishes may predominate as possible escape routes to intolerable, unresolvable situations.

Beck has also included a role for cognitive schemata. Developed from early experiences, schemata are relatively rigid cognitive structures used to screen, evaluate, differentiate, and categorize experiences and stimuli. The schemas of depressed individuals express beliefs such as, "Since people don't like me, I am nothing" (Beck, 1987). Such a schema may remain dormant until activated by specific events. Once activated, the schema determines how the individual evaluates the present condition. Guided by schema-driven interpretations, the individual may initiate responses which further reinforce the schema. As depression deepens, the conditional phrase, "since people don't like me" is left out, leaving the individual with a core schema consisting of the absolute concept, 'I am nothing,' which powerfully shapes their interpretation of environmental events (Beck, 1987).

Finally, Beck's theory postulates the existence of

structural errors or errors of logic in the cognitions of depressed people. These errors are seen in depressed individuals' misinterpretation of events. They include arbitrary inference, selective abstraction, overgeneralization, magnification or minimization, personalization, and dichotomous thinking (Beck et al., 1979).

"Arbitrary inference" refers to drawing a conclusion from insufficient evidence. "Selective abstraction" is focusing on a detail taken out of context, while ignoring other important features of the situation and construing the whole event based on this detail. "Overgeneralization" refers to extending a concept beyond its range of applicability. "Magnification" and "minimization" consist of treating an event as more important or unimportant than it actually is. "Personalization" refers to relating external (negative) events to oneself without basis. "Dichotomous thinking" refers to evaluating experiences in simplistic, bipolar categories, with depressed people typically selecting highly negative category poles for conceptualizing their experiences. According to Beck, when faced with high stress, depressed people rely on these primitive, illogical ways of thinking.

In sum, the central feature of Beck's theory is that negative cognitive appraisals of life events, rather than the events themselves, lead to the onset and maintenance of

depressive symptoms. According to this view, depressed people, because of their consistent negative appraisals, are likely to underestimate the degree of control they exert over desired environmental outcomes.

Reformulated learned helplessness theory. The original helplessness model of depression was derived from laboratory studies with animals (Overmier & Seligman, 1967; Seligman & Maier, 1967). Researchers discovered that dogs given inescapable shock in a harness showed certain deficits hours later when placed in a shuttlebox where escape to avoid an aversive shock was possible. In contrast to dogs not subjected to shock, dogs given inescapable shock showed learning, motivational, and emotional deficits. Specifically, the dogs failed to repeat a previously successful escape response, they hardly initiated escape attempts, and they passively endured the shock without outward signs of emotional reaction. According to the investigators, these deficits were the result of learning by the pre-treated dogs that responses and outcomes were unrelated. The experience of response-outcome noncontingency led to expectation of future uncontrollability or helplessness. This expectation was then generalized to new situations resulting in observed deficits that paralleled depressive symptoms.

The learned helplessness model was subsequently applied to college students. In a typical helplessness induction

design, three groups of students are used. One group is exposed to controllable events such as shock or aversive noise which may be terminated by a behavioral response. A second group, yoked to the first, is subjected to similar environmental events, but termination of the aversive situation is independent of the subject's response. Serving as the control, the third group is exposed to neither controllable nor uncontrollable events. All three groups are subsequently asked to perform certain new tasks. Learned helplessness is inferred if the inescapable shock group performs poorly in comparison with the other two.

The original helplessness theory falls short of explaining many aspects of depression, such as its chronicity, relapse, and recurrence; subtypes of depression (e.g., endogenous-reactive); and its association with suicide. Following critics' suggestions to include the individual's causal explanation of the precipitating uncontrollable event, Abramson et al. (1978) reformulated the theory along attributional dimensions. According to the revised theory, the expectation of response-outcome noncontingency leads to motivational, cognitive, affective, and self-esteem deficits which characterize the symptoms of depression. The nature of these deficits depends on the causal attributions individuals make when they perceive that response and outcome are unrelated.

The reformulated model includes three attributional

dimensions: internal-external, stable-unstable, and global-specific. Within these three dimensions, there are eight possible combinations of attribution an individual can make about the cause of an undesired event. Individuals may explain the causes of negative events as located within the self (internal) or in the environment (external), as persisting over time (stable) or transient (unstable), and as having extensive effects (global) or limited to a circumscribed area of life (specific). Internality of causal beliefs affects self-esteem, stability is related to the chronicity of depression, and the globality of causal explanations influence pervasiveness of deficits following perception of helplessness. Furthermore, the depth of helplessness deficits depends on how certain the individual is concerning his or her expectation of uncontrollability. Affective and self-esteem deficits are also influenced by how important the outcome is to the individual.

Although the reformulated theory proposed that people exhibit consistent explanatory styles for perceived uncontrollable negative events, Metalsky and Abramson (1981) have argued that attributions are based upon a combination of stable, generalized beliefs held by the individual and information garnered from the occurring event. Thus, the relative contribution of individual and situational factors may vary depending on the situation.

In sum, the central feature of this cognitive model is

that expectation of future uncontrollability is sufficient to produce symptoms of helplessness and depression. The source of this expectation is the perception of prior negative events as uncontrollable. The nature of depressive deficits is mediated by the causal attributions made in response to a perception of noncontingency. Most pertinent to the judgment of control paradigm is the cognitive deficit, the inability to learn that response and outcome are contingent. Based on this theory, depressed people should underestimate their control over objectively controllable events and perceive themselves to have less control over contingent outcomes than do nondepressed individuals.

Hopelessness theory. Recently, Abramson, Metalsky, and Alloy (1989) revised the reformulated helplessness theory of depression and called their new model, hopelessness theory. According to this theory, hopelessness is a proximal sufficient cause of depression (particularly, hopelessness depression). Negative expectations about the occurrence of highly valued outcomes (negative outcome expectancy) along with expectations of helplessness about altering the likelihood of occurrence of these outcomes (helplessness expectation) constitute the core components of hopelessness. Faced with stress from negative life events, people are motivated to contemplate three questions in regard to their predicament: Why? What then? and What about me? In

response to these questions, if people make causal attributions that are stable and global, or if they conclude that negative consequences would follow the events, or if they deduce deprecatory characteristics about themselves given that the negative events have occured, then the probability of hopelessness is enhanced.

Because of recent empirical findings that depressives are often more accurate than nondepressives in their perceptions of contingency and other cognitive processes (e.g., Alloy & Abramson, 1979), hopelessness theory has formally dropped the hypothesis of a cognitive deficit (i.e., difficulty perceiving response-outcome relationships) as a symptom of hopelessness depression. Nevertheless, since the theory has retained a causal role for helplessness expectancies, as did the earlier reformulated helplessness model, the logic of hopelessness theory is still compatible with the prediction of a cognitive deficit.

Underlying commonality among theories. According to Beck, we would expect depressives to distort by underestimating their influence over favoured outcomes. Underestimating is consistent with having a negative bias against the self and a pessimistic view of both the world and the future. The reformulated learned helplessness theory emphasizes depressive negative bias in making inferences about personal control. Within this theory, those with helplessness-induced depression are thought to

hold a general expectation that they cannot control important life events. As a result, they should show a cognitive deficit, that is, their perception of response-outcome relationships would be distorted, leading them to consistently underestimate their personal control over events. Although hopelessness theory has removed distorted perception of response-outcome contingency as a symptom of depression, the central features of hopelessness (negative outcome expectancy and helplessness expectancy) still contain the logical bases for retaining the prediction that depressed people perceive they do not have control over objectively controllable environmental events.

Depressive Realism and Nondepressive Illusion

The cognitive distortion rooted in Beck's theory and the reformulated learned helplessness theory has prompted numerous studies in regard to the cognitive processes of depressed and nondepressed individuals. To evaluate the efficacy of these two models in predicting and portraying depressives' cognitions, studies in several areas are examined.

Expectancy for success. Langer (1975) reported that people are susceptible to an 'illusion of control,' in that they often overestimate their expectancies of success relative to the actual probability of success on chance-determined tasks. This occurs when features associated with skill-determined tasks, such as practice and personal

involvement, are introduced into these chance tasks.

Studies comparing the expectancies of depressives and nondepressives in a dice game found that depressives' expectancy of success accurately reflected the objective probability of success when they rolled the dice themselves, whereas nondepressives' expectancies exceeded the objective probabilities. However, when the experimenter rolled the dice, nondepressives were accurate, whereas depressives overestimated their expectancies. This finding was demonstrated with both college students and psychiatric inpatients (Golin, Terrell, & Johnson, 1977; Golin, Terrell, Weitz, & Drost, 1979).

Expectancy changes in response to performance outcome (success or failure) should be greater for skill-determined tasks (or tasks made to appear skill-determined) than chance tasks (Ruehlman, West, & Pasahow, 1985). This is because skill-determined outcomes vary with the individual's performance whereas chance-determined outcomes are dependent on factors unrelated to the individual's response.

According to the learned helplessness theory, depressed people, who may perceive noncontingency between response and outcome, are less likely than nondepresssed people to differentiate between skill and chance tasks (Ruehlman et al., 1985). These depressed individuals would perceive they have no control over outcomes regardless of whether the outcomes actually are related or unrelated to their

responses. As expected, studies examining changes in expectancies as a function of successful performance outcome over a series of trials reported that nondepressed individuals showed greater changes in expectancies than depressed individuals on 'skill' tasks (i.e., tasks made to appear skill-determined) but not on chance tasks (Miller & Seligman, 1973). Furthermore, Seligman and colleagues (Klein & Seligman, 1976; Miller & Seligman, 1976) found that subjects with prior exposure to controllable and no noise showed greater expectancy changes on 'skill' tasks than subjects exposed to uncontrollable noise. However, despite depressed subjects' lack of discrimination between 'skill' and chance tasks, their final expectancies of success in the 'skill' task condition were closer to the objective probability of success than were the expectancies of nondepressed subjects (Alloy & Abramson, 1988).

It thus appears that nondepressives are likely to succumb to an 'illusion of success' (Alloy & Abramson, 1988), whereas depressives tend to be more 'realistic' in rating their expectancies. However, it should be noted that depressed students showed smaller expectancy changes than did nondepressed students only when they performed the task themselves (Garber & Hollon, 1980) or when they were in a public setting (Sacco & Hokanson, 1978).

Self-evaluation. On laboratory tasks such as card-sorting, word recognition, and solving anagrams, depressed

people tended to evaluate their performance more harshly than do nondepressed people when there were no differences in actual performances (Loeb, Beck, & Diggory, 1971; Rozensky, Rehm, Pry, & Roth, 1977; Zarantonello, Johnson, & Petzel, 1979). These findings of differences in self-evaluation between depressives and nondepressives had been replicated in numerous other studies (e.g., Butler & Matthews, 1983; Ciminero & Steingarten, 1978; Lobitz & Post, 1979; Smolen, 1978; Wenzlaff & Grozier, 1988; Wollert & Buchwald, 1979; Zarantonello, Matthews, Slaymaker, Johnson, & Petzel, 1984). However, depressives' self-evaluations in the form of self-reinforcement and self-punishment were often less discrepant with their objective performance than were the self-evaluations of nondepressives (e.g., Nelson & Craighead, 1977; Gotlib, 1981). Studies have also indicated that depressives' tendency to self-reward less than nondepressives may be restricted to public measurement conditions (Sacco & Hokanson, 1982) and to rewarding themselves rather rating the amount of reward others should have (Lobitz & Post, 1979).

Studies have found depressives to be equally accepting of both positive and negative self-descriptive statements in their perceptions of self whereas nondepressives were biased toward positive evaluations and were more rejecting of negative self-statements and descriptions (Vazquez, 1987; Dennard & Hokanson, 1986; Vazquez & Alloy, 1987; Tabachnik,

Crocker, & Alloy, 1983; Crocker, Kayne, & Alloy, 1985). Lewinsohn, Mischel, Chaplain, and Barton (1980) found that depressed outpatients' ratings of their social skills more accurately reflected independent and 'blind' observers' ratings of them, whereas nondepressed outpatients and normal controls overestimated their social skills in comparison to observers' ratings. Interestingly, as the depression subsided over the duration of psychotherapy, depressed outpatients began to overrate their social skills as compared to observers' ratings.

Strack and Coyne (1983) found that depressed student targets correctly anticipated more rejection from a nondepressed subject with whom they interacted than did nondepressed targets. Thus, depressives' self-evaluations were in closer accord with others' ratings of them than were nondepressives' self-evaluations. Nondepressives evaluated themselves more positively than they were evaluated by others.

Attributions. In a meta-analytic review, Sweeney, Anderson, and Bailey (1986) found that depressed individuals, relative to nondepressed individuals, make internal, stable, and global attributions for negative events, and external, unstable, and specific attributions for positive events. Several other studies have demonstrated that whereas nondepressed individuals exhibit a consistent bias to make more internal, stable, and global

attributions for positive than for negative events, depressed individuals exhibit this bias to a lesser extent or provide relatively balanced attributions for positive and negative events (Alloy, 1982; Kuiper, 1978; Raps, Reinhard, Peterson, Abramson, & Seligman, 1982; Rizley, 1978; Sackheim & Wegner, 1986).

In studies using clinical samples, depressed psychiatric inpatients showed smaller self-serving attributional optimism, that is, more unbiased attributional styles than did nondepressed psychiatric controls or normal controls (Raps et al., 1982; Sackheim & Wegner, 1986). Hamilton and Abramson (1983) compared attributional styles of depressed inpatients to those of nondepressed psychiatric controls and nondepressed normal controls upon hospital admission and at the time of discharge. Upon admission, depressed patients showed less self-serving attributional bias than did nondepressed normals. However, when symptoms had remitted, the depressed group's attributional style had become more self-serving, while that of the other two groups remained unchanged.

Depressives' evenhanded attributional style seems specific to the self. Sweeney, Shaeffer, and Golin (1982) found that depressed students' attributions for positive and negative events were more evenhanded than those of nondepressed students for themselves but that the two groups' did not differ in attributions for others. Thus, it

appears that depressed individuals are more balanced in their attributional style, whereas nondepressed individuals are more likely to show self-serving attributional bias, making more internal, stable, and global attributions for positive events than for negative events.

Perception of control. Depressed individuals' subjective perception of control over task outcomes tends to closely mirror the objective contingency whereas nondepressed individuals tend to show an illusion of control, particularly when the tasks involve desirable outcomes or high frequency noncontingent positive outcomes (Alloy & Abramson, 1979). This finding has been demonstrated in numerous subsequent studies (e.g., Alloy & Abramson, 1982; Alloy, Abramson, & Kossman, 1985, Experiment 3; Alloy, Abramson, & Viscusi, 1981, neutral and no mood induction conditions; Benassi & Mahler, 1985, Experiments 1 & 2; Dresel, 1984; Martin, Abramson, & Alloy, 1984, self-condition; Vazquez, 1987, Experiments 1-3).

Summary. Support for the claims of cognitive distortion in Beck's theory and of cognitive deficit in the reformulated learned helplessness theory has been lacking or mixed. Contrary to predictions by both theories, depressed individuals' expectations of success, evaluation of own performance and personal characteristics, attributions for positive and negative events, and perception of personal control tend to reflect balance and accuracy when compared

to cognitions of nondepressed individuals.

Judgment of Contingency Studies: Further Examination

It was Alloy and Abramson's (1979) study of depressed-nondepressed differences in judging contingency that alerted depression researchers to the phenomenon of depressive realism (Alloy & Abramson, 1988). Alloy and Abramson (1979, experiments 1 through 4) presented depressed and nondepressed students with a series of contingency learning problems varying in the actual degree of contingency between students' responses (pressing or not pressing a button) and an experimental outcome (onset of a light) as well as in the frequency and desirability of the outcome. Contrary to predictions by the major cognitive models of depression, depressed students accurately judged the degree of control their responses had over the light onset in all conditions for both contingent and noncontingent response-outcome combinations. Although nondepressed students were accurate in judging their control over contingent response-outcome tasks, they tended to overestimate their degree of control over noncontingent tasks in which the outcome (light onset) occurred with high frequency and in which subjects won money each time the light appeared.

Following the investigation by Alloy and Abramson (1979), numerous studies have replicated their major findings (e.g., Alloy & Abramson, 1982; Alloy et al., 1985, Experiment 3; Alloy et al., 1981, neutral and no mood induction

conditions; Benassi & Mahler, 1985, Experiments 1 & 2; Dresel, 1984; Martin et al., 1984, self-condition; Vazquez, 1987, Experiments 1-3). (Although Bryson, Doan, and Pasquali (1984) initially claimed that they failed to replicate Alloy and Abramson's (1979) experimental results, further analyses by Benassi and Belli (1989) indicated otherwise.)

Alloy et al. (1981) temporarily induced depressed and elated mood states in naturally nondepressed and depressed females students. They found that depressed subjects who were temporarily elated showed an illusion of control over an objectively noncontingent outcome whereas nondepressed women made transiently depressed gave accurate judgments of control. Thus it appears that current affective state, whether natural or induced, has direct effects on judgments of contingency.

Using the learned helplessness research paradigm (e.g., Seligman, 1975), Alloy and Abramson (1982) exposed depressed and nondepressed students to controllable, uncontrollable, or no noise prior to having students engage in one of two noncontingent learning situations associated with success (winning money) or failure (losing money). According to the logic of learned helplessness theory, nondepressed individuals who had prior experience with uncontrollable events should fail to exhibit the illusion of control whereas depressed individuals exposed to controllable events

should subsequently overestimate control. Results indicated otherwise. Nondepressed subjects previously exposed to uncontrollable or no noise overestimated their control in the noncontingent success condition but were relatively accurate in the failure condition. Nondepressed subjects with prior exposure to controllable noise; however, gave accurate estimates of control in both conditions. Depressed subjects were accurate regardless of pre-treatment conditions or whether they won or lost money. The finding that nondepressives made helpless through exposure to an uncontrollable situation did not underestimate control over a subsequent task was supported by Ford and Neale (1985) who found that university students exposed to a helplessness induction accurately judged that they exerted a high degree of control over a subsequent controllable task rather than underestimating their control.

Abramson, Alloy, and Rosoff (1981) assigned depressed and nondepressed students to one of two conditions. In one condition, the subjects had to self-produce possible strategies to make the light appear and try out those methods (self-generated condition). In the other condition, potential methods of producing light onset, including the correct one (e.g., pressing the button early or late in the trial interval), were supplied by the experimenter to be tested by the subjects (experimenter-generated condition). Whereas nondepressed students were accurate in their

judgments of control in both conditions, depressed subjects underestimated control in the self-generated, but not in the experimenter-generated condition. Depressed students made fewer correct responses (a correct response consists of either pressing the button early or late in the trial interval) than did nondepressed students when they had to generate the hypotheses to produce task outcome, but not when the hypotheses were supplied for them. It was also found that depressed students' underestimation of control in the self-generated condition was related to their infrequency in performing the correct responses (pressing the button early or late). In other words, they failed to adequately try out the potential relationship between their responses and task outcome. Because the estimates of depressed students in the self-generated condition closely reflected the frequency at which they performed the correct controlling response, Alloy and Abramson (1988) suggested that depressives may rate contingency accurately if task exposure was prolonged, that is, the observed low estimation of response-outcome contingency would be unlikely if subjects were given more opportunities to perceive the contingency.

Somewhat similar to the above postulation is Dresel's (1984) study which found that depressed students exposed to a noncontingent problem with many response-outcome trials (48) were accurate in their control estimates, whereas those

given few trials (16) overestimated their control in a manner similar to nondepressives (Alloy & Abramson, 1988). Length of exposure to response-outcome contingency tended to have no effect on nondepressives' judgment of control. Further corroboration for this conclusion is the finding by Tang and Critelli (1990) that mildly depressed students became more accurate in judging control over a series of judgment of contingency tasks, and by the final task, these students were more accurate than nondepressives.

Suspecting that prior expectations may play a role in control accuracy, Koenig, Clements, and Alloy (1990) investigated how this variable might affect depressed and nondepressed individuals' judgment of control. The researchers found that among those who began the task with high expectations of control, depressed women were more accurate than nondepressed women, whereas depressed men were more inaccurate than nondepressed men. No differences were observed for groups with pretask low expectations. The investigators did not offer an explanation for the absence of illusion of control among those subjects with low initial control expectations.

Mikulincer, Gerber, and Weisenberg (1990) examined the role of self-esteem threat and self-focused attention in depressed and nondepressed students' judgment of control in both controllable and uncontrollable tasks. For the uncontrollable task, nondepressed students overestimated

control judgment more than did depressed students who were relatively accurate in their control estimates. As for the controllable task, depressives underestimated control whereas nondepressives were accurate, but only in the situation where a mirror was present. In the absence of a mirror, depressives were as accurate as nondepressives in judging control in this task condition. The researchers speculated that the presence of a mirror intensified depressives' tendency to focus on themselves making them more aware of their perceived negative characteristics and helplessness. As a result, they underestimated control. Furthermore, in the uncontrollable task condition, subjects who were led to believe that good performance on the task was important and desirable (high threat to self-esteem condition) overestimated their control compared to subjects told otherwise (low threat condition). However, this difference was present only when there was no mirror. This finding led the investigators to suggest that lack of control threatened self-esteem and so subjects overestimated control to protect self-esteem; however, the presence of a mirror decreased self-deception tendencies leading to more accurate appraisal.

Alloy and Abramson (1979) raised the issue of generalizing the findings of depressive realism to clinical depressives. Logically, in accord with Beck's (1967, 1976, 1987) theory which derived from work with clinically

depressed patients, depressives would be expected to exhibit a negative bias in estimating their control over desirable outcomes. That is, while nondepressives are optimistic in judging control and mild depressives show relatively unbiased estimates of control, clinical depressives would be expected to be more accurate than mild depressives in judging noncontingent tasks and to underestimate control on contingent tasks. Of course, underestimation of a noncontingent task is impossible.

Thus far, only two studies have applied the judgment of contingency paradigm to a clinical population (Cobbs, Critelli, & Tang, 1990; Lennox, Bedell, Abramson, Raps, & Foley, 1990). Cobbs et al. (1990) compared clinically depressed inpatients, nondepressed schizophrenic inpatient controls, and nondepressed hospital staff on a series of contingency learning problems. Five tasks of varying degrees of actual control (80-60: 20%, 80-20: 60%, 80-80: 0%, 80-0: 80%, and 80-40: 40%) were administered to the groups in a repeated measure design. For each of the five tasks, the first number represents the probability of light onset if the press response is chosen and the second number indicates the probability of light onset if the not-press response is selected. The absolute difference between the two probability values represents the degree of actual control subjects have over light onset (Alloy & Abramson, 1979). Results of the study did not replicate Alloy and

Abramson's (1979) findings. Subjects in all three groups overestimated on low control tasks and underestimated on high control tasks. Clinical depressives did not show a negative bias in underestimating control. Neither did they display more accurate control judgments than nondepressed controls on the noncontingent task (task 3: 80-80). According to Cobbs and his colleagues, control estimates more closely reflected reinforcement frequency rather than actual degree of control. This finding would be consistent with that of Jenkins and Ward (1965), who found that people's judgment of control was related to frequency of successful trials and bore no relationship to actual degree of contingency. The Cobbs et al. (1990) study used the Alloy and Abramson task instructions with a sample of hospitalized patients. These instructions were developed for a sample of university students, making it possible that the hospitalized psychiatric patients did not clearly understand the judgment of contingency task.

Lennox et al. (1990) exposed their subjects, comprising hospitalized patients with major depression, depressed and nondepressed schizophrenics, and nondepressed medical/surgical patients, to either a low control (75-50: 25%) or high control (75-0: 75%) task. Results revealed no differential accuracy in judgments of control among the four diagnostic groups. As with Cobbs et al. (1990), patients with mild depression were not included in this clinical

sample. Thus it remains to be seen whether mildly depressed patients would be more accurate in control judgments relative to clinically and non-depressed patients. It is possible that a curvilinear relationship may be observed in which both the clinical and non-depressives exhibit distortions with the mild depressives being relatively more accurate in their perceptions of environmental contingencies.

Limitations in Judgment of Contingency Findings

Recent studies on depressive realism have identified certain limitations in regard to depressives' accuracy and nondepressives' illusion of control. In particular, the depressive realism phenomenon may be specific to the self and to private measurement conditions. Martin et al. (1984) investigated the relationship between depression and the illusion of control for self versus others. Female nondepressives overestimated when they judged control both for themselves and for a confederate, while nondepressed males displayed this illusion only for the self. Depressed subjects were generally accurate in their judgments of control for self, but they overestimated the degree of control exhibited by the confederate, with the exception of depressed males judging a male confederate. Thus depressives' accuracy in judging control may be restricted to judging their own impact on environmental events as opposed to other people's impact.

Regarding public versus private dimensions, Benassi and Mahler (1985) demonstrated that the consistently accurate personal control estimates of mildly depressed college students on judgment of contingency tasks broke down when subjects completed those tasks in the presence of an observer and when outcomes were frequent but noncontingent. As opposed to their more accurate appraisals when alone, depressed subjects actually perceived themselves as having more control than nondepressed subjects when in the presence of an observer. Nondepressives, however, showed reduced illusion of control in the presence of an observer, reversing the findings of Alloy and Abramson (1979, experiment 2). The authors hypothesized that the scrutiny of an observer increased the demands for rationality on the nondepressed subjects, resulting in more accurate appraisals of control. No explanations were offered for the depressed subjects' illusion of control in the observer condition.

Vazquez (1987) found that, in noncontingent situation, depressed subjects overestimated personal control when outcomes were negative self-referent sentences but were accurate when the outcomes were positive self-referent statements. Depressives also gave higher control estimates than nondepressives when the outcomes were negative self-referent statements.

Newman and Benassi (1989) evaluated contrast effects in judgments of control across tasks. They found that subjects

initially presented with a high control task made lower control judgments on a subsequent moderate control task than did subjects given a noncontingent task prior to the moderate control task. These results offered evidence that the same response-outcome contingency may be judged differently in different contexts.

Finally, Mikulincer et al. (1990) also identified a restriction to the findings of Alloy and Abramson (1979, Experiment 1). Although both depressed and nondepressed subjects were accurate in judging control over a controllable task without a mirror present, depressed individuals underestimated control in the presence of a mirror. The investigators speculated that the presence of a mirror exacerbated the typical self-focused style of depressives leading to increased negative cognitive processes and easier activation of depressogenic self-schema and thus, the underestimation. Work by Musson and Alloy (1989, cited in Mikulincer et al., 1990), however, indicated that the presence of a mirror lowered a depressive's level of self-focus.

In sum, recent research has identified certain limitations in regard to depressive realism. Specifically, depressives' accuracy in judging control appears limited to self (Martin et al., 1984), to private rather than public situations (Benassi & Mahler, 1985), and to the absence of a mirror (Mikulincer et al., 1990).

Recent reviews of the depressive realism literature have further questioned the robustness of the phenomenon (Ackermann & DeRubeis, 1991; Dobson & Franche, 1989). Ackermann and DeRubeis (1991) concluded that, although there were numerous studies which confirmed that depressed people were relatively accurate in judging contingencies, an almost equivalent number of studies had disconfirming results. Specifically, depressed people tended to be more accurate than nondepressed people in experiments using the judgment of contingency paradigm and in making judgments about themselves relative to others. In contrast, nondepressed subjects tended to be more accurate than depressed subjects in their ability to recall performance feedback. Depressed subjects tended to be negatively biased in their recall.

A review by Dobson and Franche (1989) also concluded that the depressive realism phenomenon does exist, but appears largely confined to judgment of contingency investigations. For studies that examined recall of evaluative feedback and interpersonal judgments, depressives' relative accuracy was less apparent. They suggested that the depressive realism effect is likely to diminish as the experimental tasks become more meaningful and relevant to real-life situations.

Self-esteem

William James (1950), the earliest and most prominent self-theorist, regarded self-esteem as the degree to which an individual's achievement matched his or her aspirations

in an important area of life. Self-esteem has also been defined as the extent to which the individual believes that he or she is important, successful, worthwhile, and competent (Coopersmith, 1967).

Among the several reasons suggested by Alloy and Abramson (1988) to account for depressive realism and nondepressive illusion, a self-esteem hypothesis appears to be most viable. Most people are motivated to see themselves in a positive light and to enhance their self-esteem (Bradley, 1978; Darley & Goethals, 1980; Frankel & Snyder, 1978; Miller & Ross, 1975; Zuckerman, 1979). Taylor and Brown (1988) conclude, after a review of the literature, that mentally healthy people systematically distort reality to enhance self-esteem, maintain belief in personal control over environmental outcomes, and promote an optimistic view of the future. Thus, according to the self-esteem hypothesis, nondepressives' consistent overestimation of their control over desirable outcomes results from the motive to maintain and enhance self-esteem (Abramson & Alloy, 1981; Alloy & Abramson, 1979).

Nondepressives' optimistic biases and illusions may serve an adaptive function in protecting them from the demoralizing effects of unbiased perception. According to Alloy and Abramson (1982, 1988), depressives' relative accuracy in judging control may be due to a breakdown in their motivation to protect self-esteem. Two reasons were

suggested for how this breakdown may come about. First, depressives may be unable to generate self-enhancing strategies to defend against attacks to self-esteem. Second, their typically low levels of self-esteem may leave depressives with little that they are motivated to protect. One implication of the latter self-esteem explanation, in particular, is that the lower one's self-esteem, the more accurate and unbiased should be one's perception of reality.

Alloy and Abramson (1988) cite three related studies in support of the importance of self-esteem to depressive realism-nondepressive illusion. First, depressive realism and nondepressive illusion are observed only when subjects are making judgments of their own control over outcomes (Martin et al., 1984). Presumably, judging one's own degree of control over outcomes is more likely to affect one's self-esteem than judging another person's control. Second, nondepressives' illusion of control is seen when subjects are judging the relationship between their response and outcome as opposed to the relationship between an external stimulus and outcome (Abramson et al., 1985, Experiment 3). That is, nondepressive's optimistic bias is only evident when the subject's own behavior is involved in the task. Third, nondepressives with prior exposure to uncontrollable noise overestimated their control in a noncontingent task relative to nondepressives in the controllable noise condition (Alloy & Abramson, 1982). To account for their

findings, Alloy and Abramson suggested that nondepressives exposed to uncontrollable noise during the pretask period may have suffered a threat to their self-esteem. Subsequently, they overestimated their control on a noncontingent task to reduce potential loss in self-esteem.

One recent study appears not to support the self-esteem hypothesis. Mikulincer et al. (1990) used threat to self-esteem as a variable in a judgment of contingency task. Subjects in the high threat condition were led to believe that the contingency learning task was important and ability-related, so that outstanding performance would have implications for their self-esteem, while subjects in the low threat group were instructed otherwise. According to the self-esteem hypothesis, nondepressive illusion of control should be enhanced in the self-threat condition, whereas depressives should remain relatively accurate in either condition. However, no mood by self-threat interaction was reported.

Self-esteem: Protection Versus Enhancement

In the self-esteem hypothesis to explain depressive realism- nondepressive illusion, no attempts have been made to differentiate between self-esteem protection as opposed to enhancement. In a review of interpersonal self-esteem, Baumeister, Tice, and Hutton (1989) concluded that high self-esteem people are geared toward a self-enhancement orientation in regard to their self-esteem whereas low self-

esteem people are motivated to protect self-esteem.

According to these researchers, people with high self-esteem are ambitious, risk-takers who seek to portray themselves in a reputable and outstanding fashion. They attend to opportunities that will allow them to shine, claiming desirable attributes for themselves and personal impact on successful outcomes, even before the outcomes are known. In contrast, people with low self-esteem seek to avoid humiliation and embarrassment. Although they also prefer success and social acceptance (Jones, 1973; McFarlin & Blascovich, 1981; Shrauger, 1975; Swann, Griffin, Predmore, & Gaines, 1987) low self-esteem people are hesistant in bragging about their positive traits and accomplishments because failure to live up to their boastful claims may lead to embarrassment and make their failures more salient. Instead, these people devote their energies to rectifying their weaknesses and mistakes, thus, reducing opportunities to put them in a negative light.

Baumeister et al.'s (1989) postulation has been shared by numerous other researchers (Roth, Harris & Snyder, 1988; Roth, Snyder, & Pace, 1986; Wolfe, Lennox, & Cutler, 1986). In a study to investigate motives for self-handicapping, a strategy used by individuals to undermine their performance efficiency so as to manipulate external evaluations of their success or failure, Tice (1991) found that high self-esteem people engage in self-handicapping to enhance success while

low self-esteem subjects employ the same strategy to protect against threat of failure.

Depressed people have been found to score lower than nondepressed people on numerous measures of self-esteem (e.g., Altman & Wittenborn, 1980; Beck, 1974; Feather & Barber, 1983; Karoly & Ruehlman, 1983; Lewinsohn, Larson, & Munoz, 1982; Sacco & Hokanson, 1978). Baumeister et al. (1989) proposed that self-esteem scales measure self-presentational style rather than intrapsychic attitude. According to a review of studies, these researchers found that most people rate themselves relatively high on self-esteem scales with few people scoring below any scale's theoretical midpoint. In other words, people who score low on self-esteem scales are typically responding in a neutral manner to the scale items; their scores do not reflect a derogatory evaluation of self. However, the subjects in the studies which Baumeister and his colleagues reviewed came from the general population which is likely to comprise nondepressed and mildly depressed individuals. Thus, it can be assumed that the number of clinically depressed individuals in these studies is small. Based on Beck's (1967, 1976) work, we should expect clinically depressed individuals to score lower than mild depressives on self-esteem and to present themselves in a derogatory manner.

Self-esteem Model for Depressive Realism

As mentioned earlier, Alloy and Abramson (1982, 1988)

suggested that depressive realism as observed among mild depressives reflects a lack of motivation or ability to protect self-esteem. However, it could be argued that an equally logical explanation exists to explain the realistic perception of control found among depressives. It could be possible that depressives are not unmotivated or unable to protect self-esteem, as suggested by Alloy and Abramson, but that it is their motivation to protect whatever self-esteem they have that leads to their accurate appraisal of personal control. Based on the work of Baumeister and others, mild depressives, because of their lower self-esteem as compared to nondepressives, take a conservative approach in evaluating their abilities and, in this case, their control estimates. Overestimating control puts them at risk for embarrassment if found to be wrong and underestimating is inconsistent with their evenhanded perception of their abilities. Thus, they end up relatively accurate whether judging controllable or uncontrollable outcomes. Nondepressives, because of their high self-esteem, are willing to take risks to make themselves look even more favorable. Thus they overestimate control to increase their self-esteem.

In line with this reasoning and drawing upon the work of Beck, we should expect clinically depressed individuals to score lower than mild depressives on self-esteem. According to Beck (1967, 1976), clinical depressives tend to present

themselves in a derogatory manner, that is, they put themselves down by discounting their positive assets and focusing on their negative characteristics. This form of self-degradation may actually be construed as a self-protective strategy. Putting oneself down allows others to expect less from oneself. Because less is expected, occasions for failing are greatly diminished. Consequently, the individual encounters less chance of assault to an already fragile self-esteem and the opportunity for self-esteem loss is greatly reduced.

On the other hand, it is well-known that clinical depressives sometimes do actively attempt to hurt or punish themselves or engage in self-destructive behavior. Thus it would appear that clinical depressives may manifest both motives for self-protection and for self-harm, and that their self-derogatory style of self-presentation could derive from either of these motives. At this point, it is not clear how these two apparently contradictory motives might be combined, but with respect to judgments of control, they both suggest that clinical depressives would underestimate their control over desirable outcomes.

In sum, our self-esteem model for depressive realism differs from Alloy and Abramson's in this aspect: Whereas Alloy and Abramson stated that mild depressives are accurate in judging control because of their lack of motivation or ability to protect self-esteem, we believe that it is the

motivation to protect self-esteem that leads to their accurate judgment. Mild depressives protect by being conservative in their control estimates; thus, their accurate appraisal. Clinical depressives protect by underestimating control to reduce expectations of future performance and thus restrict the possibility of failure and further attacks on self-esteem. In addition, they may underestimate control because this is consistent with their view of themselves as worthless and inadequate. Nondepressives, because of their considerable reserves of self-esteem, are more motivated to enhance than to protect self-esteem. In other words, because they would not be as endangered by a simple failure as those with lower self-esteem, they can afford to take risks and seek enhancement.

Rationale of Present Study

As acknowledged by Alloy and Abramson (1988), questions have emerged concerning the generalization of their results to clinically depressed individuals. According to their interpretation of the role of self-esteem in control judgment, as depression becomes more severe, self-esteem should decrease, resulting in less of a motive to enhance esteem. Thus, severe depressives might be even more accurate perceivers than are mild depressives.

Another consideration regarding Alloy and Abramson's (1979; Abramson & Alloy, 1981) explanation of the depressive realism and nondepressive illusion of control effect deals

with methodology. In judgment of contingency studies, subjects typically indicate how much control they have over environmental outcomes on a scale ranging from 0% (no control) to 100% (complete control). Because of the theoretical linkage between judgment of contingency and learned helplessness, a disproportionate number of studies have employed noncontingent tasks, that is, tasks in which the actual level of control was zero (e.g., Alloy & Abramson, 1979, Experiments 2 & 3; Alloy et al., 1981; Alloy & Abramson, 1982; Alloy et al., 1985, Experiment 3; Alloy et al., 1981; Benassi & Mahler, 1985; Dresel, 1984; Koenig et al., 1990; Martin et al., 1984; Vazquez, 1987, Experiment 4).

Although several studies (Alloy & Abramson, 1979, Experiments 1 & 4; Vazquez, 1987, Experiment 1) have found that mildly depressed students did not underestimate control on contingent tasks, the disproportionate number of noncontingent tasks presents somewhat of a problem. When actual control is zero, it is impossible to detect underestimations of control, a response pattern that the cognitive models of depression predict to be characteristic of depressives. In this way, mild depressives' apparent accuracy in zero control may represent a design artifact rather than accurate perception. At the least, the existing research has not given subjects the same opportunities to display underestimation as overestimation effects.

It is not clear that the findings of laboratory-based studies on depressive realism generalize to the conditions of everyday life. For example, as indicated above, these studies have emphasized tasks with zero control rather than sampling representatively between 0 and 100 percent control. It would appear to be the case that many relevant tasks in everyday life, such as school or career performance and establishing personal relationships, are tasks with some level of actual control.

Several studies have found that depressives have lower expectancies of success than do nondepressives (e.g., Lobitz & Post, 1979; Loeb et al., 1971), suggesting they might also make lower expectancies of control on a contingency-learning task. If so, mild depressives' accuracy in judgment of control may result from an artifactual match between their relatively low expectancies and the relatively low levels of control actually employed in these studies. In other words, judgment of control may be more accurate in situations where expectancy is accurate, as is implied in learned helplessness theory (Abramson et al., 1978; Seligman, 1975). Thus, since depressives may hold lower expectancies of control than do nondepressives, tasks with low actual control may be relatively easier for depressives than for nondepressives. The present study determines whether this is true and rectifies this problem by representatively sampling from the range of possible levels of control. In

particular, it tests for an interaction between level of depression and level of actual control on judgment accuracy. It also determines whether such an interaction, if it exists, results from the effects of expectancy or from the direct perception of control unmediated by expectancy.

A final problem with research in this area is that laboratory tasks may not be perceived as situations where it is adaptive to respond accurately, rather than, for example, in terms of a self-enhancing bias. This may be particularly true for studies on judgment of contingency. Here, the major contingency learning task used with depressives and nondepressives was developed by Jenkins and Ward (1965) and popularized by Alloy and Abramson (1979). The task involves pressing or not pressing a button in order to turn on a light. Conditional probabilities for light onset given press or no-press responses are predetermined, and after a set number of trials subjects are asked to judged the control their responses had over light onset. Within this paradigm, there is no obvious payoff for accuracy. Many studies have used monetary reward, but the reward has been contingent on light onset.

Only two studies have provided a reward for making an accurate judgment of contingency and have given subjects accuracy feedback on their judgments (Cobbs et al., 1990; Tang & Critelli, 1990). In both studies, subjects were told at the end of each task whether their control estimates were

accurate and were given a cash award if their estimates were correct (within +/-10 percent of the objective contingency). Tang and Critelli (1990) found that mildly depressed students became more accurate with exposure to more tasks whereas nondepressed students continued to exhibit an illusion of control. In the Cobbs et al.'s (1990) study, the self-correction effect was not interpretable, as the greater accuracy demonstrated by subjects on the last task may have been due to differences in the difficulty level of those tasks. This would be the case if we assume that tasks with high frequency and low control are more difficult than those with both moderate frequency and control.

Thus, the payoff of self-satisfaction for making accurate judgments of control has generally been lacking in previous studies. In other words, previous research may not have created a context where it would be clearly advantageous to perceive contingencies in an accurate manner. Since judgment accuracy, rather than ability to turn on the light, has been the central concern in these studies, this may be an important consideration.

In the present study, each subject encountered five contingency learning tasks. Subjects were given a monetary incentive for turning on the light (to ensure that light onset is perceived as a desired outcome) and they received a monetary incentive for making accurate judgments of contingency. Since they were immediately told whether they

had earned this reward, they were also receiving feedback on the accuracy of their judgments of control. At the same time, as a direct implication of the repeated measures design, as subjects progressed through the experiment, their exposure to tasks requiring a judgment of contingency also increased. Dresel (1984) has found that exposure alone may increase accuracy for mildly depressed students on noncontingent tasks.

Thus, in the present study, we determine whether clinical depressives accurately perceive contingency, as do mildly depressed students, or whether they underestimate control as predicted by the major cognitive theories of depression. In addition, we determine whether inpatient groups representing nondepressed, mildly depressed, and clinically depressed individuals could respond adaptively and self-correct their judgments of control when it is clearly advantageous to do so, that is, when they are given the incentive, feedback, and task exposure to facilitate self-correction. The use of rewards for accurate performance, feedback on performance, and repeated attempts at task performance are all factors that could enhance the generalizability of these tasks to everyday life.

Use of the repeated measures format has a number of advantages. It allows subjects to experience several levels of actual control, which is important because it is hypothesized that level of depression will interact with

level of actual control on judgment of control accuracy. At the same time, sampling multiple levels of control in a between-subjects design with the five inpatient groups used in the present experiment (nondepressed and mildly depressed medical/surgical patients, nondepressed and mildly depressed schizophrenics, and clinical depressives) would require a prohibitive number of subjects. In addition, previous research (Tang & Critelli, 1990) suggests that the judgment of control tasks can be repeated in a one-hour session without fatigue effects. One possible drawback of the repeated measure design is that Newman and Benassi (1989) have found contrast effects with repeated judgment of contingency tasks. A medium control task presented after one of low control is judged to have a higher contingency than a medium tasks presented after a high control task. Since there is no evidence or theoretical rationale at the present time suggesting that level of depression would interact with contrast effects, the benefits of the repeated measures design appear to outweigh its potential drawbacks.

Hypotheses

(1) Nondepressives will overestimate their control over light onset, mild depressives will show a relatively accurate judgment of control, and clinical depressives will underestimate control. As mentioned earlier, the self-esteem of nondepressives, mild depressives, and clinical depressives may be positive, neutral (equally balanced), and

negative, respectively. Thus, one might expect nondepressives to overestimate to enhance self-esteem and mild depressives to show no systematic bias in estimation to protect self-esteem. Alloy and Abramson's self-esteem model suggests that those with low self-esteem would have little they need to protect and little motive to enhance self-esteem and thus, according to this model, clinical depressives should be as accurate or more accurate than mild depressives. The alternative model of self-esteem suggests that clinical depressives would underestimate control either (a) as a strategy of protecting self-esteem by severely restricting any expectations of optimal performance on the task or (b) as a way of maintaining consistency with a negative, derogatory view of self. This latter view is consistent with a prediction of underestimation based on current cognitive models of depression. We believe that, at this point, the argument for a prediction of underestimation by clinical depressives is stronger than the argument for accurate estimation.

(2) Using a college sample, Tang and Critelli (1990) found that mild depressives but not nondepressives responded adaptively to a contingency on accuracy and self-corrected, that is, they became increasingly accurate with longer exposure to tasks and feedback. Based on these findings, it is predicted that mildly depressed patients, but not clinically depressed or nondepressed patients will respond

adaptively to a contingency on accuracy and self-correct. Nondepressives will not self-correct because of their motivation to enhance self-esteem. Clinical depressives will fail to self-correct because their belief structures centering on themes of worthlessness and inadequacy will lead to systematic underestimation of control.

(3) With regard to specific tasks, nondepressives will show their greatest accuracy in high control, mild depressives in medium control, and clinical depressives in low control. Nondepressives will show their greatest overestimation in low control and clinical depressives will show their greatest underestimation in high control. Consistent with learned helplessness theory, the effect of level of depression and objective control on judgment of control will be, at least partially, a function of expectancy of control.

(4) In support of the presumed relationship between judgment of control and level of depression, judgment of control should vary with level of depression rather than with the patients' psychiatric/nonpsychiatric status. In other words, it is possible that in a hospitalized sample, judgment of control accuracy could be affected by whether or not individuals have received a psychiatric label rather than by their specific level of depression. For example, individuals who have been told that there is something wrong with their "minds" may assume that they have little control

over environmental events and systematically underestimate control. To reiterate, we believe that judgments of control will be more strongly related to level of depression than to psychiatric status.

(5) In accord with the self-esteem enhancement/ protection model developed above, overestimations in control judgments are expected to correlate positively with pre-post task increase in immediate self-esteem, with expectancy of control statistically controlled. This would support the view that overestimations are motivated by self-esteem enhancement.

(6) It is hypothesized that nondepressives overestimate contingencies to enhance self-esteem, mild depressives are relatively more accurate to protect self-esteem, and clinical depressives underestimate because of their extremely low levels of self-esteem. This will be tested by determining whether nondepressives overestimate and clinical depressives underestimate relative to mild depressives. In addition, to explore the relationship between judgment of control and self-esteem enhancement, nondepressives should show greater increases in immediate self-esteem after overestimation than do mild or clinical depressives.

(7) Sackheim and Wegner (1986) found that, consistent with Beck's theory, clinically depressed patients showed a self-derogatory pattern in giving themselves more blame for bad events than praise for good events. It seems credible

that accepting praise for success and taking blame for failure are two important ways that self-esteem is raised or lowered. It is predicted that praise given will correlate positively with increases in immediate self-esteem after accurate judgments. Blame taken is expected to correlate with decreases in immediate self-esteem after inaccurate judgments.

(8) It is predicted that nondepressives will give themselves more praise for accurate judgments than blame for inaccurate judgments, with expectancy of accuracy held constant. Mildly depressed patients, however, will show no difference between tendencies to give themselves praise or blame. Because of their self-derogatory pattern of self-perception, clinical depressives are expected to give themselves more blame for inaccurate judgments than praise for accurate judgments.

CHAPTER II

METHOD

Subjects

Subjects comprised five inpatient groups with 30 per group: clinical depressives, mildly depressed schizophrenics, mildly depressed general medical/surgical patients, nondepressed schizophrenics, and nondepressed general medical/surgical patients. These patients were from Terrell State Hospital, Terrell, Texas; Dallas Veteran's Administration Medical Center, Dallas, Texas; Denton Regional Medical Center, Denton, Texas; and John Peter Smith Hospital, Fort Worth, Texas.

Inclusion in the clinically depressed inpatient group was based on: (a) a primary psychiatric diagnosis, based on case files, of depressive disorder, (b) a score of 16 or greater on the Beck Depression Inventory (BDI), (c) a score of 20 or greater on the Mini-Mental State exam (MMS), (d) duration of current depressive episode of less than two years, (e) no evidence of psychosis or organic brain dysfunction, and (f) no evidence of bipolar disorder.

Inclusion in the mildly depressed schzophrenic group was based on: (a) a primary psychiatric diagnosis of schizophrenic disorder, (b) no accompanying or previous diagnosis of depressive disorder, (c) a score between 10 and

15 on the BDI, (d) an MMS score of 20 or greater, and (e) no evidence of organic brain dysfunction.

Inclusion in the mildly depressed general medical/surgical group was based on: (a) no evidence of psychiatric disorder present or past as indicated in case files, (b) a score between 10 and 15 on the BDI, (c) an MMS score of 20 or greater, and (d) no evidence of organic brain dysfunction.

Inclusion in the nondepressed schizophrenic group was based on: (a) a primary psychiatric diagnosis of schizophrenic disorder, (b) no accompanying or previous diagnosis of depressive disorder, (c) a score of less than 10 on the BDI, (d) an MMS score of 20 or greater, and (e) no evidence of organic brain dysfunction.

Inclusion in the nondepressed general medical/surgical group was based on: (a) no evidence of psychiatric disorder present or past as indicated in case files, (b) a score of less than 10 on the BDI, (c) an MMS score of 20 or greater, and (d) no evidence of organic brain dysfunction.

A total of 154 patients participated in the study; however, 4 research protocols were excluded from the data analyses. Three were excluded because of indications that the subjects did not understand the instructions. The fourth was not included because the combined Effort and Involvement scores fell below the predetermined cut-off total of 60. Mean age of the 150 subjects was 37.88 (SD =

9.23). Sixty-nine percent of the sample was Caucasian, 25 percent Black, and the remaining fell into the Hispanic, Asian, or Other categories. Thirty-one percent of the subjects were married, 33 percent single, 34 percent divorced or separated. In regard to religious affiliation, 73 percent of the participants considered themselves Protestant, 11 percent Catholic, with the remaining 15 percent falling into the Jewish, Atheist, or Other categories. Eighty-nine percent of this sample had at least a high school education and all of them, except 2 subjects, had attended at least 9 years of school. All subjects spoke and understood English. No subject had electroconvulsive therapy within the previous six months.

Measures

Mental state. The 'Mini-Mental State' (MMS; Folstein, Folstein, & McHugh, 1975) was used as a screening tool measuring cognitive mental status to help ensure that inpatient subjects were able to complete the judgment of control tasks. This exam consisted of eleven items and took about five to ten minutes to administer. Test-retest reliability over one day was $r = .89$. This exam has been used to accurately discriminate among patients with dementia, depression with cognitive impairment, depression without cognitive impairment, and normals. Concurrent validity is indicated by the scale's correlations of .78 with Wechsler Adult Intelligence Scale Verbal score and .66

with the Performance score. Because a score of 20 or less was found only in patients with dementia, delirium, and functional psychosis, diagnoses which indicate considerable cognitive impairment, this score was used as a cut-off point for the inclusion of patients in the experimental procedure.

Depression. Intensity of depression was measured by the Beck Depression Inventory (BDI; Beck et al., 1979), which consists of 21 groups of statement corresponding to the 21 symptoms and attitudes describing specific manifestations of depression assessed by the original BDI (Beck, Ward, Mendelson, Mock, & Erbaugh, 1961). Statements comprising each group were assigned values of 0, 1, 2, and 3 to indicate level of severity. The total depression score was obtained by summing the numerical value of the most severe statement endorsed in each of the 21 groups.

Coefficient alpha for the inventory ranged from .73 to .92 for nonpsychiatric samples and .76 to .95 in psychiatric groups (Beck, Steer, & Garbin, 1988). The validity and reliability of the instrument had been extensively documented (Beck & Steer, 1987; Beck et al., 1988; Bumberry, Oliver, & McClure, 1978), and this inventory has been used to assess depression status in nearly all studies of depressive realism.

Immediate state of depression. This variable was measured by the depression scale with filler items from the Multiple Affect Adjective Check List - Revised: Today Form

(MAACL-R, Zuckerman & Lubin, 1985). Developed to assess mood changes as a function of external conditions, the Today Form showed alpha reliabilities of .75 to .89 for the depression scale (D scale). Test-retest reliabilities for the same scale ranged from .19 over a two-day period to -.04 over a five-day interval, reflecting sensitivity to mood fluctuation. Convergent validity for the D scale had been demonstrated through significant correlations with self, peer, and observer ratings (Zuckerman & Lubin, 1985). This measure was used to verify that subject groups differ in immediate state of depression at the time of the judgment of contingency tasks.

Self-esteem. Global self-esteem was measured by the Rosenberg Self-Esteem scale (RSE; Rosenberg, 1965), a 10-item inventory. Subjects responded to each item along a four-point scale ranging from 1 (strongly agree) to 4 (strongly disagree). Items were scored in the positive direction with high scores denoting high global self-esteem. Although originally scored as a Guttman scale, many later researchers have used it as a Likert scale (Crandall, 1973; Wylie, 1989). Alpha coefficients reported ranged from .72 to .87 (Wylie, 1989), indicating good item consistency. Silber and Tippert (1965) reported a test-retest reliability of .85 over a two-week interval and correlations of .56 to .83 with other measures of self-concept and interviewers' ratings. Convergent validity was also demonstrated by the

scale's correlation of .60 with Coopersmith's (1981) Self-Esteem Inventory (Robinson, 1980) and .66 with the Self-Rating Scale (Fleming & Courtney, 1984; Fleming & Watts, 1980).

Immediate self-esteem. This variable was measured by the Wells (1988) self-esteem scale, a five-item scale designed to measure immediate feelings and attitudes of self-evaluation. This scale showed Cronbach alpha of .94 and test-retest reliability over a one-week period of .86. Concurrent validity of the scale was indicated by its correlation of .30 to .50 with two global measures of self-esteem: Rosenberg Self-Esteem scale (Rosenberg, 1965) and Bills, Vance, and McLean's (1951) index of adjustment and values. Moreover, when the Rosenberg Self-Esteem scale was modified and subjected to repeated administration with the Wells scale, correlation between the average scores of both scales was .72, providing evidence for the Wells scale as a valid measure of changes in self-perception and evaluation.

Expectancy of control. Subjects' expectancy of control for their responses (pressing or not pressing a button) over the experimental outcome (light onset) was measured on a 100 percentage point scale, with 0 percent representing 'no control' and 100 percent indicating 'complete control' (Alloy & Abramson, 1979).

Expectancy of accuracy. Subjects' expectancy that they would be able to make an accurate judgment of control on the

next task was measured on a 100 percentage point scale, with 0 percent representing 'completely inaccurate' and 100 percent indicating 'completely accurate'.

Judgment of control. Following each task, subjects judged the degree of control their responses exerted over light onset on a 100 percentage point scale, with 0 percent representing 'no control' and 100 percent indicating 'complete control' (Alloy & Abramson, 1979).

Praise/Blame. The amount of praise and blame subjects felt they deserved for their judgment of control accuracy was measured on separate 100 percentage point scales, with 0 percent representing 'no praise/blame' and 100 percent indicating 'total praise/blame'.

Post-experiment questionnaire. Subjects' impressions of the experiment was assessed by three open-ended questions: (1) 'What do you think are the purposes or hypotheses of this study?' (2) 'What responses do you feel the experimenter wanted you to make?' and (3) What are the factors affecting the flashing of the light? In addition, subjects indicated their effort and involvement in the tasks by responding to two 100 percentage point scales, with 0 percent representing 'no effort/no involvement' and 100 percent indicating 'total effort/complete involvement'.

Diagnosis. Case files of subjects in the psychiatric groups were consulted in regard to their diagnoses and medications received. All psychiatric patients, except for

four with a diagnosis of major depression, were receiving psychotropic medications. None of the medical/surgical patients received any form of psychotropic drugs.

Apparatus

The apparatus was similar to that described by Alloy and Abramson (1979). It consisted of two small lightbulbs (one red and the other green) and a push-button mounted on a 2′ by 1′ board. The push-button served as the response key for the subjects. One end of the board was sealed with a raised box-like cover, with an open end facing the experimenter. The buttons for manipulating the onset of the ready light (red) and the green light were located under this box-like structure, hidden from the view of the subject. Because two-room suites with one-way observation mirrors were not available in the present hospital settings, tasks and questionnaires were administered with the experimenter seated directly across from the subjects. A small Rolodex file was used to display subjects′ cash earnings throughout the experiment.

Procedure

Screening. All potential subjects were invited to participate in an individually-administered screening session. After completing the consent form (Appendix A), which provided information concerning the screening as well as the actual experimental procedures, each subject was given demographic items and the Beck Depression Inventory

(BDI) to complete. The Mini-Mental Status (MMS) exam was then administered. The screening session took approximately 20 minutes, for which the subject was paid $3.

Experimental subjects were chosen from the screening procedure on the basis of their BDI and MMS scores. Those who qualified were scheduled to participate the same day screening was completed. Those not selected were debriefed concerning the mood and cognition criteria that were used as a basis for selection of participants.

Experiment. Similar to the screening session, the actual experiment was carried out individually. Each subject was given the Rosenberg Self-Esteem scale and the Multiple Affect Adjective Checklist - Revised (MAACL-R) Today Form. Upon completion, instructions for the judgment of control tasks (Appendix B) were read to the subject while he or she read a printed copy of the instructions. The subject was introduced to the experimental apparatus and told that his or her job was to make a green light come on and to learn the degree of control his or her responses (pressing or not pressing the button) had over the light onset. Instructions informed subjects of the monetary contingencies, the number of trials per task, and the total number of tasks. Instructional procedures followed those of Alloy and Abramson (1979) with regard to content, but the language was simplified for a non-university population. In addition, subjects were also asked two questions to verify their

comprehension of the task. These were: (1) "If the green light comes on frequently when you press the button and it also comes on almost as frequently when you don't press the button, would you say that your control is low or high?" and (2) "If the green light comes on very frequently when you do not press the button and hardly at all when you do press, would you say that your control is low or high?" If a subject gave a wrong answer or appeared to guess the answer, the experimenter went through the instructions again and helped the subject arrive at the correct response.

Subjects were asked if they had any questions, which were then answered. They were reassured that all information was anonymous and confidential. The subject was then given ten or more trials to become familiar with the equipment. Testing did not begin until both subject and experimenter were confident that the subject felt comfortable with the equipment and understood what he or she was to do on the judgment of control tasks.

For each task, the order of measures was as follows: immediate self-esteem (Appendix D), expectancy of control (Appendix E), expectancy of accuracy (Appendix E), forty press/not press - light onset/offset trials, judgment of control (Appendix E), immediate self-esteem, accuracy feedback (subjects were given verbal feedback concerning the accuracy/inaccuracy of their judgment of control estimates), immediate self-esteem, and praise/blame (Appendix F). The

measures were administerd through paper-and-pencil. Each task took about eight minutes.

The procedure continued until all five tasks were completed. Subjects then retook the MAACL-R and RSE and completed the post-experimental questionnaire (Appendix G). Subjects were then thoroughly debriefed. Cash earnings were distributed and subjects were asked not to discuss the experiment with other patients. The experiment took about one and one half hours. All procedures were in accord with American Psychological Association guidelines.

Each subject completed five tasks, each consisting of 40 trials of pressing or not pressing a button to make a light come on. Below, the first number of each task indicates the percentage of light onset when subjects pressed the button and the second number represents onset percentage when subjects did not press the button (whether pressing or not pressing lead to the higher onset percentage was counterbalanced). The absolute difference between these two numbers adequately represents the degree of actual control on the task (Alloy & Abramson, 1979). For example, in the 60-40 task, the light came on 60% of the time when the subjects pressed the button and 40% of the time when they did not press. Thus, their actual control was 20%.

Three different tasks were employed, representing low (20%), medium (50%), and high (80%) levels of actual control, with the low and high control presented twice to

yield five tasks in all. Contingencies for the three tasks were, respectively, 60-40 (20% - low), 75-25 (50% - medium), and 90-10 (80% - high). The order of task presentation for all subjects was low, high, medium, high, low. Monetary contingencies were as follows: subjects were given five cents credit each time the light came on and one dollar credit for each accurate (within 10%) judgment of control. A visual display of the amount of money earned was available throughout the experiment, and cash earnings were distributed after all tasks and questions had been completed.

CHAPTER III

RESULTS

The 5 diagnostic groups refer to the following: Clinically depressed patients (CD), mildly depressed schizophrenic patients (MS), mildly depressed medical/surgical patients (MM), nondepressed schizophrenic patients (NS), and nondepressed medical/surgical patients (NM). In some analyses, the 5 diagnostic groups were re-combined into 3 mood groups, that is, the groups were categorized according to depression level. The 3 mood groups are: clinically depressed (CD), mildly depressed (MD; combination of MS and MM), and nondepressed (ND; combination of NS and NM) groups.

Descriptive Findings

A one-way ANOVA on age for the 5 diagnostic groups was not significant, $F(4, 145) = 1.03$, $p < .40$, indicating similar distributions of age among the 5 groups. Means and standard deviations were as follows: CD = 36.33 (10.02), MS = 37.30 (7.95), MM = 39.60 (9.26), NS = 36.37 (7.80), and NM = 39.80 (10.75). A one-way ANOVA on length of hospital stay for diagnostic groups was significant, $F(4, 145) = 2.84$, $p < .05$. Post-hoc Tukey-HSD tests revealed no significant differences between any 2 groups. Means and standard deviations for hospital stay (in days) were: CD = 8.73

(8.33), MS = 15.87 (21.83), MM = 9.73 (16.92), NS = 17.57 (17.59), and NM = 6.57 (7.46). Further analysis on the length of hospital stay for the sample grouped according to psychiatric/nonpsychiatric status revealed significant results, $F(2, 147) = 5.33$, $p < .006$. Post-hoc Tukey-HSD tests indicated that the schizophrenics were in the hospital significantly longer than the medical/surgical patients before being invited to participate in the experiment. This difference reflects the time required for psychotropic medication to have its effect in stabilizing newly admitted schizophrenics. Means and standard deviations for number of days in hospital were: Clinical depressives = 8.73 (8.33), Schizophrenics = 16.72 (19.67), and Medical/surgical patients = 8.15 (13.06). The comparison between schizophrenics and clinical depressives was not significant, presumably because of the smaller number of clinically depressed patients.

The Mini-Mental State (MMS) exam was administered during the screening session to exclude subjects with severe cognitive impairment. A one-way ANOVA on the MMS scores for the 5 diagnostic groups was significant, $F(4, 145) = 4.02$, $p < .005$. Post-hoc Tukey-HSD analysis indicated that the MMS score of nondepressed schizophrenics was significantly lower than that of the nondepressed medical patients. None of the subjects, however, obtained an MMS score below the cut-off score of 20. Means and standard deviations for MMS scores

were: CD = 28.03 (1.50), MS = 27.23 (2.14), MM = 27.87 (2.03), NS = 26.73 (1.89), and NM = 28.43 (1.57).

A one-way ANOVA on total Beck Depression Inventory (BDI) scores for the 5 diagnostic groups was significant, $F(4, 145) = 122.89$, $p < .0000$. As expected, post-hoc Tukey-HSD analysis indicated that the clinically depressed group was significantly more depressed than the 4 other diagnostic groups. In addition, BDI scores of the 2 mildly depressed groups were significantly higher than each of the 2 nondepressed groups. Means and standard deviations for BDI scores were: CD = 28.30 (9.66), MS = 12.70 (1.86), MM = 12.37 (2.01), NS = 4.13 (2.99), and NM = 4.70 (2.53).

The depression scale from the Multiple Affect Adjective Checklist - Revised (MAACL-R) was used to determine whether the diagnostic groups differed in their level of depressive mood at the time of the experiment. A one-way ANOVA on MAACL-R depression score for the 5 diagnostic groups produced significant results, $F(4, 145) = 22.52$, $p < .0000$. Post-hoc Tukey-HSD comparisons indicated that the depression score of the clinically depressed group was significantly higher than those of the other 4 diagnostic groups. The MAACL-R depression scores of mildly depressed and nondepressed schizophrenics were significantly different from that of nondepressed medical patients. Means and standard deviations for MAACL-R depression score were: CD = 6.03 (3.21), MS = 2.80 (3.21), MM = 1.43 (1.48), NS = 2.20

(2.54), and NM = 0.43 (0.90). The Pearson correlation between BDI and pre-experiment MAACL-R depression scores was .58, $p < .01$.

The depression scale from the MAACL-R was administered again at the conclusion of the experiment. A one-way ANOVA on post-experiment MAACL-R depression scores for the 5 diagnostic groups produced significant results, $F(4, 145) = 16.65$, $p < .0000$. Post-hoc Tukey-HSD tests indicated that the depression of the clinically depressed group was significantly higher than each of the 4 other diagnostic groups. Means and standard deviations for post-experiment MAACL-R depression score were: CD = 5.03 (3.36), MS = 2.37 (3.09), MM = 1.07 (1.39), NS = 1.03 (1.43), and NM = 0.67 (2.07).

A one-way ANOVA on pre-experiment Rosenberg Self-esteem (RSE) scores for the 5 diagnostic groups was significant, $F(4, 145) = 32.18$, $p < .0000$. Post-hoc Tukey-HSD comparisons indicated that the RSE scores for clinically depressed patients were significantly lower than each of the 4 other groups. The RSE scores for the 2 mildly depressed patient groups were also significantly lower than the scores of the 2 nondepressed patient groups. The correlation between BDI and RSE was - .72, $p < .01$. Means and standard deviations for pre-experiment RSE scores were: CD = 22.13 (5.23), MS = 29.57 (4.72), MM = 30.03 (4.07), NS = 33.30 (4.34), and NM = 34.00 (4.28).

The Rosenberg Self-esteem scale was also re-administered at the conclusion of the experiment. A one-way ANOVA on post-experiment RSE scores for the 5 diagnostic groups was significant, $F(4, 145) = 25.44$, $p < .0000$. Post-hoc Tukey-HSD tests indicated that the RSE scores of the clinical depressives were significantly lower than each of the 4 other diagnostic groups. The mildly depressed schizophrenics had significantly lower RSE scores than the 2 nondepressed patient groups and the mildly depressed medical/surgical patients' RSE scores were significantly lower than those of the nondepressed medical/surgical patients. Means and standard deviations for post-experiment RSE scores were: CD = 22.93 (6.27), MS = 28.97 (3.83), MM = 30.13 (3.97), NS = 32.67 (4.27), and NM = 34.00 (4.56).

A one-way ANOVA on the pre-experiment immediate self-esteem (ISE) scores for the 5 diagnostic groups was significant, $F(4, 145) = 22.30$, $p < .0001$. Post-hoc Tukey-HSD comparisons indicated that the ISE score of clinical depressives was significantly lower than each of the 4 other diagnostic groups. The ISE score of mildly depressed schizophrenics was significantly lower than those of nondepressed schizophrenics and medical patients. Furthermore, the mildly depressed medical patients scored significantly lower than the nondepressed medical patients on this measure. Means and standard deviations for pre-

experiment ISE score were: CD = 13.13 (8.23), MS = 22.50 (11.94), MM = 26.13 (8.03), NS = 29.40 (9.27), and NM = 34.57 (8.67). The correlation between pre-experiment RSE and ISE scores was .66, $p < .01$, which was within the range reported by Wells (1988).

A one-way ANOVA on the total frequency, summed across the five tasks, with which the press response was chosen for the 5 diagnostic groups was nonsignificant, $F(4, 145) = 1.30$, $p < .28$. Thus none of the groups differed on type of response. Means and standard deviations for the total press response were: CD = 108.93 (24.99), MS = 116.97 (29.07), MM = 110.13 (17.24), NS = 122.03 (29.48), and NM = 111.90 (28.07).

A one-way ANOVA for the total frequency of light onset across the 5 groups produced significant results, $F(4, 145) = 4.31$, $p < .003$. Post-hoc Tukey-HSD comparisons revealed that the clinically depressed patients produced significantly more light onset than the mildly depressed schizophrenic group. Means and standard deviations for total frequency of light onset were: CD = 133.53 (11.84), MS = 119.97 (14.19), MM = 127.23 (14.60), NS = 125.60 (12.51), and NM = 128.53 (11.30).

To determine whether the frequency of light onset differed for the low, moderate, and high (20%, 50%, 80%) control tasks, a 1 x 3 (Control) ANOVA with repeated measures on Control was conducted. The results indicated

significant differences in frequency of light onset among the three levels of control, $F(2, 298) = 80.55$, $p < .000$. Means and standard deviations were: Low = 23.06 (1.73), Moderate = 24.77 (4.76), and High = 28.04 (4.79). (Frequency of light onset for low and high control tasks was the average of light onset for Tasks 1 and 5, and Tasks 2 and 4, respectively.)

Because the diagnostic groups showed differences on length of hospitalization, MMS scores, pre-experiment MAACL-R depression scores, pre-experiment ISE scores, and total frequency of light onset, Pearson correlations between each of these variables and judgments of control averaged across the five tasks were computed. The correlation between average judgment of control and frequency of light onset was significant, $r = .41$, $p < .01$. The relationship between judgments of control and each of the remaining variables was nonsignificant. Table 1 (Appendix H) presents the Pearson correlations for these variables.

A one-way ANOVA on the expectancy of control scores averaged across the five tasks for the 5 diagnostic groups was marginally significant, $F(4, 145) = 2.33$, $p < .06$. Means and standard deviations for expectancy of control scores were: CD = 51.43 (14.93), MS =49.53 (23.85), MM = 47.93 (17.32), NS = 60.00 (18.55), and NM = 57.23 (16.75). A one-way ANOVA on the average expectancy of control scores for the 3 mood groups produced significant results, $F(2,$

147) = 4.49, $p < .02$. Post-hoc Tukey-HSD tests indicated that the mild depressives expected significantly lesser control over light onset than did the nondepressives. Means and standard deviations for average expectancy of control scores for the 3 mood groups were: CD = 51.43 (14.93), MD = 48.73 (20.68), and ND = 58.62 (17.58).

A one-way ANOVA on the expectancy of accuracy scores averaged across the five tasks for the 5 diagnostic groups was nonsignificant, $F(4, 145) = 1.75$, $p < .14$. Means and standard deviations for average expectancy of accuracy scores were: CD = 53.70 (16.62), MS = 54.17 (22.18), MM = 49.00 (16.15), NS = 60.33 (19.95), and NM = 58.47 (16.07).

A one-way ANOVA on the total number of tasks on which subjects provided accurate judgments of control for the 5 diagnostic groups was nonsignificant, $F(4, 124) = 1.30$, $p < .28$. Means and standard deviations for total number of tasks were: CD = 1.86 (0.76), MS = 1.52 (0.80), MM = 1.96 (0.79), NS = 1.67 (0.92), and NM = 1.64 (0.70).

A one-way ANOVA on the Effort scores for the 5 diagnostic groups produced nonsignificant results, $F(4, 145) = 1.11$, $p < .36$. Similarly, a one-way ANOVA on the Involvement scores for the 5 diagnostic groups was nonsignificant, $F(4, 145) = 1.49$, $p < .21$. This indicated that subjects in the 5 diagnostic groups exerted equivalent effort and were equally involved in the experimental procedures. In fact, none of the groups scored below a mean of 80 percent on the Effort

or Involvment scores. Means and standard deviations for Effort and Involvement scores, respectively, were: CD = 87.83 (14.42), 89.17 (12.25); MS = 83.17 (17.49), 84.67 (16.45); MM = 90.17 (13.74), 93.00 (10.31); NS = 83.00 (16.22), 85.33 (17.71); and NM = 85.83 (17.52), 87.67 (16.80).

Table 2 (Appendix H) presents the means and standard deviations for the above variables for the 5 diagnostic groups. Zero-order correlations between depression and self-esteem scores taken pre- and post-experiment are presented in Table 3 (Appendix H). Table 4 (Appendix H) displays the means and standard deviations for expectancy of control, expectancy of accuracy, judgment of control, accuracy score, and percentage of light onset for the 5 tasks by diagnostic groups.

Hypothesis Testing

Hypothesis 1: Nondepressives will overestimate their control over light onset, mild depressives will show a relatively accurate judgment of control, and clinical depressives will underestimate control. A one-way ANOVA of the average accuracy scores (judged control minus objective control) across the 5 tasks for the 3 mood groups produced nonsignificant results, $F(2, 146) = 2.28$, $p < .11$. Thus the clinical depressives did not underestimate control and the nondepressed patients did not overestimate control relative to the mildly depressed individuals. Means and standard

deviations for average accuracy scores were: CD = 8.20 (16.15), MD = 3.73 (19.61), and ND = 7.73 (15.16). Table 5 (Appendix H) presents the means and standard deviations for average accuracy scores for the 3 mood groups.

Hypothesis 2: Mildly depressed patients, but not clinically depressed or nondepressed patients will respond adaptively to a contingency on accuracy and self-correct, that is, they will become increasingly accurate with longer exposure to tasks and feedback. To test for this self-correction effect, the average accuracy scores for Tasks 1 and 2 (Trial 1), and Tasks 4 and 5 (Trial 2) were computed. A 5 (diagnostic group) x 2 (trial) ANOVA with repeated measures on Trial performed on the accuracy score produced a nonsignificant interaction, $F(4, 145) = 2.27$, $p < .07$. (The main effects for diagnostic group and trial were also nonsignificant.) Thus, contrary to the hypothesis, the 2 mildly depressed diagnostic groups did not show significant increases in accuracy of judged control with longer exposure to tasks and feedback. Means and standard deviations for average accuracy scores for Trial 1 and Trial 2, respectively, for the 5 diagnostic groups were: CD = 9.42 (19.64), 10.58 (21.29); MS = 2.08 (23.18), 2.33 (24.76); MM = 6.25 (15.71), 6.58 (27.46); NS = 16.17 (17.02), 3.83 (20.72); NM = 4.08 (20.12), 9.00 (18.34). Table 6 (Appendix H) presents the means and standard deviations for average accuracy scores for the diagnostic groups by trials.

Hypothesis 3: Nondepressives will be most accurate in judging contingency in high control, mild depressives in moderate control, and clinical depressives on low control tasks. To test this prediction, the accuracy scores for the two low (Tasks 1 and 5) and two high (Tasks 2 and 4) objective control tasks were collapsed to produce a total of 3 tasks of low, medium, and high (20%, 50%, and 80%) control. A 3 (Mood Group) x 3 (Task) ANOVA with repeated measures on Task performed on the judgment of control accuracy scores produced a nonsignificant Group x Task interaction, $F(4, 294) = 2.07$, $p < .09$. Therefore, the hypothesis was not suppported. The means and standard deviations for accuracy scores for the low, medium, high objective control tasks (in order) were: CD = 30.92 (20.21), 1.00 (26.21), -10.92 (20.23); MD = 29.67 (17.94), 1.42 (28.79), -21.04 (26.00); ND = 36.04 (18.24), 5.58 (24.32), -19.50 (19.95). The means and standard deviations are displayed in Table 7 (Appendix H).

This analysis indicated that subjects overestimated on the low control task, were relatively accurate on the moderate control task, and underestimated on the high control task. Means and standard deviations for average accuracy scores for the low, medium, and high control tasks were as follows (in order): 32.47 (18.64), 3.00 (26.47), and -18.40 (22.79). Separate paired t-tests conducted to determine if subjects' judgments of control differed from

the actual control produced significant results for the low [t(149) = 21.33, p < .001] and high [t(149) = -9.89, p < .001] control tasks, but not for the moderate [t(149) = 1.39, p < .18] control task.

Hypothesis 4: Judgments of control will vary with level of depression rather than with the patients' psychiatric/ nonpsychiatric status. A one-way ANOVA on the average accuracy scores across the 5 tasks for the patients grouped according to depression level (clinical, mild, and normal) was nonsignificant, F(2, 147) = 1.05, p < .35. Means and standard deviations for average accuracy scores for the 3 mood groups, CD, MD, and ND were 8.20 (16.15), 3.73 (19.61), and 7.73 (15.16), respectively. Similarly, a one-way ANOVA on the average accuracy scores across the 5 tasks for the patients grouped according psychiatric/nonpsychiatric status (clinically depressed, schizophrenics, and medical) was nonsignificant, F(2, 147) = 0.38, p < .68. Means and standard deviations for average accuracy scores for the patient groups were: clinically depressed = 8.20 (16.15), schizophrenics = 4.90 (19.13), and medical patients = 6.57 (15.98). Thus neither level of depression nor psychiatric diagnosis was associated with differential perceptions of control.

Since neither depression level nor psychiatric diagnosis was associated with differential judgments of control, further analyses were undertaken to determine the role of

other pertinent factors. Separate hierarchical regression analyses were conducted for each of the five tasks using expectancy of control, experienced actual control, and frequency of light onset to predict judgment of control. Since it is impossible to use an invariant value in a regression analysis, experienced actual control was used in place of actual control as a predictor variable for judgment of control. Recall that the accepted index of actual control in the judgment of contingency literature is the absolute difference between the conditional probability of light onset given the press response and the conditional probability of light onset given the no-press response (Alloy & Abramson, 1979). Experienced actual control refers to the actual degree of control experienced by each subject in the experimental session, which was the absolute value of the difference between frequency of light onset given press and frequency of light onset given no-press (Alloy & Abramson, 1979). Actual frequency of light onset experienced by each subject depends on randomized variables that in the long run would be the same for each subject, but that, on a particular task, would vary considerably from one subject to another. For example, on Task 1, the theoretical actual control was 20%, experienced actual control ranged from 0.00 to 63.16 with a mean of 15.91 and standard deviation of 11.94. Overall, the sample mean of subjects' experienced actual control deviated only slightly from the

theoretical actual control.

In the hierarchical regression analysis, expectancy of control was entered as the first predictor variable because of established findings that prior expectancies have an important role in current perception of a situation. Since subjects were told that their task was to judge control, experienced actual control was entered as a second predictor variable to determine the amount of additional variance in judgment of control scores accounted for by this variable over and above what has been accounted for by expectancy of control. Frequency of light onset (Jenkins & Ward, 1965) was the final predictor entered. For each of the five tasks, expectancy of control and frequency of light onset consistently emerged as significant predictors of judgment of control. R-squared values for expectancy of control, experienced actual control, and frequency of light onset (in order) for each task were: Task 1 = .24, .004, .03; Task 2 = .17, .002, .14; Task 3 = .24, .001, .07; Task 4 = .21, .003, .29; Task 5 = .33, .0003, .05. For example, for Task 1, expectancy of control accounted for 24% of the variation in judgment of control scores, experienced actual control added 0.4% to the variance accounted for by expectancy of control, and frequency of light onset helped explain an additional 3% of the variation in control judgments beyond what had already been accounted for by the first two predictor variables. Resulting multiple R-squared values

for Tasks 1 through 5 were .27, .31, .31, .50, and .38, respectively. Across the 5 tasks, expectancy of control accounted for an average of 24 percent of the variation in judgment of control. Frequency of light onset accounted for an average of 11 percent in addition to what had been accounted for by expectancy of control. It appears that subjects formulated their judgments of control based on their expectancies of control and observable frequency of light onset. Light onset can be considered a form of reinforcement, because subjects were instructed that one of their tasks was to turn on the light, for which they would receive a cash reward. Table 8 (Appendix H) presents the means and standard deviations for experienced actual control for each of the 5 tasks. Zero-order correlations for expectancy of control, frequency of light onset, experienced actual control, and judgment of control for the 5 tasks are displayed in Table 9 (Appendix H). Table 10 (Appendix H) shows the hierarchical regression results for each of the five tasks.

Hypothesis 5: Overestimation in judgments of control is motivated by a desire to enhance/protect self-esteem. To test this prediction, the average overestimation score (same as accuracy score) across those tasks in which a subject overestimated control was computed. The pre-post task change in immediate self-esteem was also averaged for those tasks on which overestimation occurred. The partial

correlation between overestimation in judgments of control and increase in self-esteem (post minus pre-task ISE), with expectancy of control held constant was significant, r = .38, $p < .001$. This hypothesis was supported. Expectancy of control was held constant because it was thought that overestimation could also result from having unrealistically high expectancies of control. Pearson correlation between overestimation of control judgments and self-esteem change without controlling for expectancy of control was .34, $p < .001$.

Further analysis was undertaken to determine the relationship between underestimation of control and ISE change. The partial correlation between underestimation in judgment of control and pre-post task change in ISE with expectancy of control held constant was marginally significant, r = .14, $p < .06$. Pearson correlation between underestimation and self-esteem change without controlling for expectancy of control was .11, $p < .12$. This indicates that there is a nonreliable tendency for underestimation to be associated with decrease in self-esteem.

The relationships between over/underestimation and self-esteem change scores were also examined for each of the mood groups separately. There was no relationship between extent of overestimation and self-esteem change for clinical depressives (r = -.07, $p < .38$), but extent of overestimation was significantly related to self-esteem

change for both the mildly depressed and nondepressed patient groups, *rs* = .57 and .41, *ps* < .001 and .002, respectively. This indicated that the mild depressives and nondepressives may have used overestimation of control to enhance their self-esteem, but such a strategy was not used by the clinical depressives.

The correlation between underestimation of control and self-esteem change was nonsignificant for both the clinically and mildly depressed subjects, *rs* = .17 and -.08, *ps* < .22 and .31, respectively, but significant for the nondepressed subjects, *r* = .29, *p* < .03. Thus for the nondepressives, underestimation of control was related to a decrease in self-esteem at post-task relative to pre-task. Table 11 (Appendix H) presents the correlations between over/underestimation of control and self-esteem change scores.

Hypothesis 6: Nondepressives will show greater increases in immediate self-esteem after overestimation than do mild or clinical depressives. A one-way ANOVA performed on the average immediate self-esteem change score (post-task ISE minus pre-task ISE) for tasks on which overestimation occurred for the 3 mood groups produced nonsignificant results, $F(2, 144) = 0.67$, $p < .52$. Thus for tasks on which overestimation occurred, the self-esteem change scores of nondepressives did not differ significantly from those of mild and clinical depressives. Means and standard

deviations for immediate self-esteem change scores of CD, MD, and ND were 0.39 (3.85), 0.91 (4.20), and 0.07 (3.90), respectively.

Further analysis was also performed for tasks on which underestimation occurred. A one-way ANOVA performed on the average ISE change score for tasks on which underestimation occured for the 3 mood groups produced nonsignificant results, $F(2, 120) = 0.49$, $p < .63$. Thus there was no difference in ISE change among the 3 mood groups when they underestimated control. Means and standard deviations for ISE change scores of CD, MD, and ND were 1.53 (4.75), 0.45 (5.50), and 0.50 (4.30), respectively. Table 12 (Appendix H) presents a summary of the means and standard deviations for average self-esteem change scores for over/underestimation.

Hypothesis 7: Subjects will show a positive correlation between praise deserved and increases in immediate self-esteem after accurate judgments. Subjects will also show a positive correlation between blame taken and decreases in self-esteem after inaccurate judgments. To test this hypothesis, average praise scores were computed for those tasks on which the participant was told he or she had given an accurate judgment of control. The average ISE change (post-feedback ISE minus pre-feedback ISE) was also computed for these tasks. The correlation between praise scores and self-esteem change scores was nonsignificant, $r = .14$, $p >$

.05. However, the correlation between blame scores and self-esteem change for tasks on which subjects were told their judged control was inaccurate was significant, $r = -.17$, $p < .05$. This indicated that greater self-blame was associated with a decrease in self-esteem.

Further analyses were undertaken to examine the relationship between praise and self-esteem change after accurate judgments and blame and self-esteem change after inaccurate judgments for each of the 3 mood groups. For tasks on which subjects were told they were accurate in their control judgments (success feedback), Pearson correlation between praise and self-esteem change scores was significant for the clinically depressed group ($r = .41$, $p < .05$) but not for the mildly depressed ($r = .11$, $p > .05$) or nondepressed ($r = .02$, $p > .05$) groups. For tasks on which subjects were told they were inaccurate (failure feedback), Pearson correlation between blame and self-esteem change scores was also significant for the clinical depressives ($r = -.40$, $p < .05$), but not for the mild depressives ($r = -.11$, $p > .05$) or nondepressives ($r = -.01$, $p > .05$). Pearson correlations between praise/blame and self-esteem change scores after accuracy feedback are displayed in Table 13 (Appendix H).

Hypothesis 8: Nondepressives will give themselves more praise for accurate judgments than blame for inaccurate judgments. Mild depressives will be evenhanded in claiming

praise or blame and clinical depressives will claim more blame for their failures than praise for their successes. To test this hypothesis, the average praise score across tasks on which the subject was accurate and average blame score across tasks on which the subject was inaccurate in their control estimates were computed. A one-way ANOVA was performed on the average praise minus average blame score for the 3 mood groups with expectancy of accuracy as covariate. The results were significant, $F(2, 125) = 7.60$, $p < .002$. Post-hoc Tukey-HSD comparisons indicated that the praise-blame difference scores for the nondepressed group differed significantly from those of the clinical and of the mild depressives. Means and standard deviations for praise-blame difference scores for CD, MD, and ND groups were -3.45 (41.23), 13.87 (34.54), 33.44 (36.60), respectively.

Separate t-tests were conducted to determine if each of these means differed from zero. The t-test for the clinically depressed group was nonsignificant, $t(27) = -0.44$, $p < .67$. T-tests for both the mildly depressed and nondepressed groups were significant, $t(51) = 2.90$, $p < .007$ and $t(48) = 6.40$, $p < .001$, respectively. This indicated that while the clinical depressives were evenhanded in giving themselves praise and blame, mildly depressed and nondepressed patients gave themselves significantly more praise for success than blame for failure.

Ancillary Analyses

Further analyses were completed to determine how subjects responded to success and failure feedback. For tasks on which subjects were told they were accurate (success feedback), a 1 x 3 (mood group) ANOVA was performed on the ISE change scores (post-feedback minus pre-feedback) averaged across these tasks for each subject. The ANOVA over the 3 mood groups was not significant, $F(2, 126) = 0.36$, $p < .71$. Means and standard deviations for self-esteem change scores were: CD = 1.71 (3.67), MD = 1.31 (3.00), and ND = 1.09 (2.74). Although the 3 groups did not differ in the extent of self-esteem change, there may have been a tendency for the clinical depressives to be more responsive than the mild depressives to success feedback who, in turn, may have been more responsive than the nondepressives.

For tasks on which subjects were told they were inaccurate (failure feedback), a one-way ANOVA was performed on the ISE change scores averaged across those tasks for each subject. This ANOVA across the 3 mood groups was also not significant, $F(2, 147) = 1.53$, $p < .23$. Means and standard deviations for self-esteem change scores were: CD = -2.09 (3.22), MD = -1.45 (3.39), and ND = -0.93 (2.44). Examination of the means revealed a similar trend, that is, for the clinical depressives to be more responsive than the mild depressives to failure feedback who, in turn, were more

responsive than the nondepressives. Taken together, these results suggested a trend, though nonsignificant, for responsiveness to feedback to be related to level of depression. Table 14 (Appendix H) presents the means and standard deviations for average self-esteem change scores after accuracy feedback.

For tasks on which subjects were accurate (success feedback), Pearson correlations between the two depression scores (BDI and pre-experiment MAACL-R depression) and average ISE change (post-feedback minus pre-feedback ISE) were nonsignificant, $r = .09$, $p > .05$ (BDI and ISE) and $r = .02$, $p > .05$ (MAACL-R depression and ISE). However, for tasks on which subjects were inaccurate (failure feedback), Pearson correlations between the two depression scores and ISE change were significant, $r = -.17$, $p < .05$ (BDI and ISE) and $r = -.20$, $p < .05$ (MAACL-R depression and ISE). This indicated that more depressed individuals were more likely to respond to failure feedback with a greater decrease in self-esteem. Pearson correlations between depression and self-esteem change scores after accuracy feedback are presented in Table 15 (Appendix H).

Further analyses were also conducted to determine how attribution of praise and blame for success and failure feedback varied among the 3 mood groups. For tasks on which subjects were accurate in their control estimates (success feedback), a 1 x 3 (mood group) ANOVA was performed on the

praise scores averaged across those tasks for each subject. The main effect for mood group was significant, $F(2, 126) = 4.11$, $p < .02$. Post-hoc Tukey-HSD analysis indicated that the nondepressives gave themselves significantly more praise than did the mild depressives after success feedback. Means and standard deviations for the praise scores were: CD = 50.77 (28.84), MD = 52.24 (29.40), and ND = 65.90 (24.06).

For tasks on which subjects were inaccurate, a 1 x 3 (mood group) ANOVA was performed on the blame scores averaged across those tasks for each subject. The main effect for mood group produced significant results, $F(2, 147) = 4.33$, $p < .02$. Post-hoc Tukey-HSD analysis revealed that the clinical depressives gave themselves significantly more blame than did the nondepressives after failure feedback. Means and standard deviations for blame scores were: CD = 52.28 (27.88), MD = 40.47 (28.57), and ND = 33.84 (27.54). Table 16 (Appendix H) presents the means and standard deviations for average praise/blame scores.

Separate paired t-tests were conducted for each of the 3 mood groups to determine pre- to post-experiment changes in depression and self-esteem. Although all 3 groups tended to be less depressed at the conclusion of the experiment, as measured by the depression scale of the MAACL-R, only the clinical depressives showed a significant decrease in immediate state of depression, $t(29) = 2.55$, $p < .02$. Means and standard deviations for pre- and post-experiment

depression scores, respectively, were: CD = 6.03 (3.21), 5.03 (3.36); MD = 2.12 (2.57), 1.72 (2.46); and ND = 1.32 (2.09), 0.85 (1.77). No significant changes were noted for the pre- and post-experiment global self-esteem scores for all 3 groups. Means and standard deviations for pre- and post-experiment self-esteem scores, respectively, were: CD = 22.13 (5.23), 22.93 (6.27); MD = 29.80 (4.38), 29.55 (3.91); and ND = 33.65 (4.29), 33.33 (4.43). Table 17 (Appendix H) presents the pre- and post-experiment depression and self-esteem scores.

CHAPTER IV

DISCUSSION

The primary objective of this study was to determine whether the depressive realism phenomenon would generalize to clinically depressed individuals or, as suggested by cognitive theories of depression, whether clinical depressives would underestimate their control over environmental events. A secondary objective was to examine whether the motivation for distorting judgments of control could be directly linked to attempts to protect or enhance self-esteem.

Depressive Realism-Nondepressive Illusion

Contrary to the predictions of both depressive realism and the major cognitive theories of depression, current results indicated that judgments of control did not vary as a function of level of depression or psychiatric/ nonpsychiatric status of patients. Nondepressives did not generally overestimate control and mild depressives were not relatively accurate in judging control. Clinical depressives were neither more accurate than mild depressives nor did they systematically underestimate control. With regard to specific tasks, nondepressives were neither most accurate on high control nor were clinical depressives most accurate on low control tasks. Judgment of control accuracy

did vary, however, in accord with level of objective control. Subjects overestimated on low control (20% control), were accurate on moderate control (50%), and underestimated on high control (80%) tasks.

Contrary to findings by Tang and Critelli (1990) with mildly depressed college students, mildly depressed hospitalized patients did not respond adaptively to accuracy feedback as the experiment progressed. None of the diagnostic groups appeared to utilize the feedback to provide more accurate judgments of control on later tasks, even though the last two tasks employed the same contingencies as the first two.

All diagnostic groups appeared to give relatively moderate control estimates across the five tasks despite the wide range of objective control they were allowed to experience. Across the five tasks, judgments of control ranged from means of 50 to 63 percent, whereas the actual objective control ranged from 20 to 80 percent.

Several factors may account for the lack of differences in judgment of control accuracy among the patient groups. First, it is possible that subjects failed to comprehend the notion of control pertinent to this experiment because they were cognitively impaired by virtue of their psychopathology or state of hospitalization. Impairment due to psychopathology seems unlikely because of the comparable performance of psychiatric and medical/surgical patients,

who had no history of psychiatric disorder. Moreover, subjects were asked two questions at the conclusion of pre-experiment instructions to ascertain their understanding of what 'control' meant in this study. Clarifications were provided on aspects of the instructions which were problematic for some patients. At the time of the experiment, subjects appeared to understand that frequent light onset in response to both their choices of press or no-press meant they had low control and more frequent light onset in response to one response choice over the other implied they had high control.

Participants were also screened with the Mini-Mental State Exam to rule out those with cognitive debilitation. No subject scored below 20 on this measure of cognitive functioning and 93 percent of the sample had a score of 25 or above. A score of 20 separates patients with cognitive disturbance from those without such impairment. As a basis for comparison, normal elderly hospitalized patients have shown a mean score of 27.6 on this test (Folstein et al., 1975). Subjects of current study obtained a mean of 27.7 ($\underline{SD}$ = 1.92).

Second, since laboratory tasks used in the present study were similar to those used in previous investigations, the discrepancy between current and previous findings in judgment of control accuracy could be due to differences in sample characteristics. Two distinguishing sample features

may be relevant: chronological age and hospitalized status. Evidence of the impact of these two factors on judgments of control may be seen in the range of control judgments made. Despite being exposed to a wide range of objective contingency (20% to 80%), subjects' judgments of control varied within an extremely limited range (50% to 63%). From the restrictive range of control judgments, it appeared that participants were highly conservative or cautious in estimating their control over light onset. Such caution may reflect a desire to avoid risk in making control estimates. At the risk of coming across as too grandiose, the subjects may have been reluctant to provide higher estimations when actual control was high. At the same time, they may have been unwilling to risk seeming incompetent by saying that they had little control when actual control was low. As a result, their control judgments fell within a narrow range, clustering around the mean of the one hundred-point scale.

Previous studies have typically used college students who are younger, usually in their early twenties, while the mean age for subjects of current sample was 38. Due to their older chronological age, subjects in this study brought a somewhat different set of life experiences to bear on the experimental procedure. As an older group that was somewhat removed from the college environment, these subjects may, perhaps, have been more intimidated by the cognitive tasks presented. Furthermore, the effects of hospitalization may

have increased feelings of vulnerability and helplessness for these subjects (Raps, Peterson, Jones, & Seligman, 1982). Such feelings could have contributed toward an overall conservative strategy in judging control.

Third, it is possible that the discordance between current and previous findings may represent another boundary condition for depressive realism. Recent studies have found that depressive realism is not a uniform phenomenon, but rather a tendency that occurs only under certain circumstances or only when using certain standards of comparison (Dobson & Franche, 1989; Ackermann & DeRubeis, 1991). In their review, Ackermann and DeRubeis (1991) found that there were as many studies that disconfirmed as there were studies which confirmed the hypothesis that depressed people are relatively accurate perceivers of environmental events. They concluded that many of the inconsistent findings were related to the type of tasks employed. Specifically, mild depressives were evenhanded or more accurate than nondepressives on contingency learning tasks where the subjects were evaluating themselves as opposed to others. However, in a majority of the studies that compared recall of evaluative feedback, nondepressives were more accurate than depressives. Dobson and Franche (1989), in their review of the depressive realism literature, concluded that depressive realism does exist; however, they cautioned against claiming that it is a robust phenomenon. According

to their evaluation, the strength of the phenomenon diminished as the ecological validity of studies increased. They suggested that depressive realism was confined mainly to contingency learning tasks. For investigations of interpersonal behavior, findings consistent with the phenomenon were more ambiguous or contradictory. It should further be noted that within the contingency learning paradigm whereby depressive realism hypothesis was usually confirmed, the relative accuracy of depressed subjects in judging control were largely restricted to studies using noncontingent, high reinforcement density tasks (Ackermann & DeRubeis, 1991).

Taking into consideration these recent reviews, the present findings point to a limited generalizability of depressive realism. Just as depressive realism has been found only in judgment of control for self (but not for other; Martin et al., 1984), in private (but not public; Benassi & Mahler, 1985), in high density reinforcement (but not low density reinforcement), noncontingent rather than contingent tasks (Alloy & Abramson, 1979), current findings suggest that the phenomenon may be restricted to a college (perhaps younger) sample as opposed to a hospitalized, clinical population. Support for this contention is available from the present study and the two previous ones which have used hospitalized, clinical samples (Cobbs et al., 1990; Lennox et al., 1990). Present findings were

similar to those by Cobbs et al. (1990), that is, subjects' judgments of control did not vary as a function of depression level. Using only contingent tasks, Lennox et al. (1990) found that inpatient groups including psychiatric patients with major depression, severely depressed and nondepressed schizophrenics, and nondepressed, nonpsychiatric medical/surgical patients also did not provide differential judgments of control.

Given the rather established finding that severely depressed people usually experience profound gloom and negativism, a question arises as to why clinical depressives in the present study provided judgments of control comparable to those of nondepressives. A recent study by Pelham (1991) may help explain this surprising result. Pelham found that the pessimism of depressives did not generalize to many areas of life. When depressed students in his study were asked to rate their most favorable belief about themselves, their ratings of this best aspect of themselves were no different from the nondepressives' ratings of their best self-view. He concluded that depressed students were capable of engaging in self-serving biases and that they may also strive to develop positive beliefs about themselves.

Applying these findings to the current study, it is possible that the clinical depressives were estimating their control as if they were rating their 'best talent,' or

perhaps a task that they perceived as being easily accomplished with success. At a superficial level, the button-pressing task with light onset as its goal may seem uncomplicated compared to 'real world' tasks such as finding a job or a desirable relationship partner. Moreover, feedback in the form of light onset and cash incentive was immediately available throughout the trials. Clinical depressives, given such a 'simple' task, might have felt a sense of success and confidence leading to control judgments that were comparable to those of nondepressives. Support for the clinical depressives' favorable feelings about the experimental tasks is available. Whereas, every diagnostic group showed a trend toward decrease in state depression at post-experiment, only the clinical depressives showed a significant decrease. It appeared that the experiment impacted the clinical depressives in a positive manner. The lack of difference in clinical depressives' average expectancies of control over tasks outcome compared to the expectancy scores of the other two mood groups further suggests they approached the experimental task with a sense of relative confidence and belief in attaining favorable outcomes or that they could perform the tasks as instructed.

Closer examination of the findings revealed that subjects' control estimates appeared to be influenced by their expectancies of control and by frequency of light onset or reinforcement rate, but not by level of actual

control. Note that light onset could be considered a form of reinforcement since subjects were instructed that one of their jobs was to turn on the light, and they were rewarded with cash for each light onset. Multiple regression analyses indicated that expectancies of control and reinforcement rate were each significantly predictive of judgments of control, producing task-averaged R-squared values of .24 and .11 respectively. Subjects' judgment of control was unrelated to experienced actual control. That is, subjects appeared to use a combination of prior expectancies and reinforcement rate rather than objective contingency to determine current judgments of control. The use of reinforcement rate to partly determine control judgments was not atypical. It has been documented elsewhere that people recall successes better than failures and are apt to bias information regarding contingency based on the frequency of a given outcome (Jenkins & Ward, 1965; Kahneman & Tversky, 1973).

Subjects' use of a combination of control expectations and reinforcement rate to determine control judgements can be understood from the work of Alloy and Tabachnik (1984). They proposed that judgments of the degree of covariation between two events are based on the joint effects of two pieces of information, situational and prior beliefs. Apparently, subjects in the present study erroneously focused on the situational variable of reinforcement rate

rather than the more task appropriate situational variable of objective contingency. Since no other studies have conducted similar regression analyses, it is presently unclear whether the judgments of control in previous findings also varied as a function of expectancy and frequency of light onset rather than actual contingency.

In sum, current findings did not support the depressive realism hypothesis. Judgment of control accuracy did not vary as a function of depression level. On the whole, subjects appeared to take a conservative approach to estimating control over experimental outcomes. This cautious style in judging control seemed to reflect the cognitive strategy of an older group of subjects in dealing with an unfamiliar cognitive task. Current findings may have identified another boundary condition for depressive realism. That is, the phenomenon may be restricted to a younger, nonhospitalized population with subclinical depression and only when noncontingent, high density reinforcement tasks are used. The examination of other factors that may have been influential in how subjects formulate their control estimates has identified pre-task control expactancies and frequency of light onset or reinforcement rate as consistent predictors of judgments of control. It appeared that subjects in the current study had based their control estimates on their beliefs about the extent of control they had over task outcome and the

frequency with which they were reinforced rather than the actual contingency. This finding is interesting because none of the previous studies has examined the relationship of these variables to judgments of control, creating the possibility that much of the depressive realism phenomenon for judgment of contingency may be spurious.

Implications for Major Cognitive Theories

The underlying commonality of major cognitive theories of depression is that depression is associated with negativistic information selection and distorted appraisal of environmental events. Beck's model contends that depressives interpret their experiences in a negative and distorted manner. This manner of processing information is produced by relatively rigid, well-organized schemata which consist of negativistic beliefs and assumptions about the self, the future, and the environment. Underestimation of one's effectiveness over environmental events would be the result of such a generalized pessimistic approach to life.

The reformulated learned helplessness theory 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, retains much of the same

logic as the reformulated learned helplessness theory, but it focuses on the core features of hopelessness (i.e., negative outcome expectancy combined with helplessness expectation) as the proximal cause of the hopelessness subtype of depression. This theory has dismissed the cognitive deficit as a formal symptom of hopelessness depression, largely because of the discovery of the depressive realism phenomenon. Nevertheless, since negative outcome expectancy and helplessness expectation have been retained as core components of hopelessness depression, a prediction of depressives' underestimation of control continues to be compatible with the logic of this theory. That is, if a person believes that what he or she wants is not likely to happen (negative outcome expectancy) and that he or she is powerless in changing the likelihood of occurrence of the much-desired outcome (helplessness expectation), this generalized sense of impotence would appear to imply generalized feelings of ineffectiveness in exerting control over desired environmental events.

In the area of outcome expectancies, results of the present study offer partial empirical support for the above theories. Significant differences in expectancies of control over experimental outcome were found between the mildly depressed and nondepressed groups, but not between the clinically depressed and other subjects. Compared to the mild depressives, nondepressives expected more control

over task outcome.

Clinical depressives in the current study did not underestimate control relative to the less depressed groups as would be predicted by cognitive models of depression. Their control judgments were no different from those of the mild depressives and nondepressives. To reconcile current findings with the predictions of cognitive theories, one might examine the nature of the experimental task. As is evident from Pelham's (1991) study, depressives' pessimism about personal ability may not generalize to every aspect of their lives. Beck's theorizing about depressives' cognitive functioning derives primarily from his work in a clinical setting. In such a setting, clients' weaknesses and problems would be emphasized. The current experimental task, with its simple goal of turning on a light, and with its continuous display of cash reward, may have created a generalized perception of success rather than failure. Directing subjects' attention to the simplicity and success associated with this task may have mitigated clinical depressives' tendencies to underestimate control.

One of the symptoms of depression, according to the reformulated learned helplessness theory, is that depressed people have difficulty perceiving response-outcome relationships. Current results indicated otherwise. Clinical depressives, in the present study, were as accurate in estimating control over task with moderate objective

control and were as inaccurate in judging control over tasks with high and low objective control as were mild depressives and nondepressives. They exhibited similar accuracies and distortions as the less depressed groups in their judgments of control. In this regard, Alloy and Abramson (1979) have argued that the deficits characteristic of depressed individuals may be more motivational than cognitive. They contended that depressives' poor performance on instrumental learning tasks in studies demonstrating the learned helplessness effect may have been due to their failure to act on the environment (motivational) rather than to their inability to perceive response-outcome relationships (cognitive). Current findings seemed to support this assertion. For the present experimental task, subjects were required to sample the two response alternatives to determine control. Such a task requirement would have removed the effect of motivation on behavior, that is, the subjects were "actively" involved in making response choices from trial to trial as they proceeded through the experiment. With the removal of motivational influences, depressives were capable of perceiving the relationship between their responses and task outcomes with the same degree of accuracy/inaccuracy as nondepressives.

Finally, it should be kept in mind that subjects in the current study were not selected to fulfil criteria for the hopelessness subtype of depression. Although the

desirability of such criteria has been affirmed, Abramson et al. (1989) have not yet developed the procedures which would allow screening for hopelessness depression, presumably because the theory is still in its infancy. Therefore, it may be inappropriate to say much about the hopelessness theory of depression based on current results.

Self-esteem Protection/Enhancement

Alloy, Abramson, and their colleagues (Alloy & Abramson, 1979; 1988; Alloy et al., 1985) asserted that mildly depressed college students were accurate in judging control because of a breakdown in their motivation to maintain self-esteem. This breakdown may occur because depressives' low self-esteem may render them incapable of using self-enhancing strategies to protect against attacks to self-esteem (Bibring, 1953), or their low levels of self-esteem may leave them with little to protect (Abramson & Alloy, 1981). In contrast, our self-esteem hypothesis asserts that both mild and clinical depressives retain the motivation to protect self-esteem, although this protection may be carried out through different cognitive-behavioral strategies than those employed by nondepressives.

Nondepressives, because of their high self-esteem and coping resources, were viewed as not requiring elaborate measures for self-esteem protection. At the same time, nondepressives were viewed as willing to take risks to enhance self-esteem. In the present context, risk-taking

would correspond to a readiness to overestimate one's control over environmental outcomes to enhance self-esteem. Clinical depressives were thought to employ extreme self-protection strategies, such as reducing expectations of future performance to minimize the chance of failure and thus guard against injury to self-esteem. Underestimating control protects self-esteem in two ways. It reduces the expectancy to perform at a high level of competence and it helps avoid the blame for not producing desired outcomes. Mild depressives were hypothesized to protect self-esteem by being moderate in their control estimates. Mildly depressed individuals are not generally feeling threatened enough to engage in an underestimation strategy. Neither are they self-confident enough to employ a risk-taking strategy of overestimation. Because they fall between these two extremes, mildly depressed individuals were thought to be relatively accurate in judging control.

Since current findings did not reveal differences in perceptions of control among the mood groups, this limits what can be concluded about the self-esteem hypotheses from results of the present study. The methodology of the present study did, however, include measures of immediate self-esteem taken pre-task, post-task, and post-feedback. These measures allowed a fine-grained examination of the roles played by self-esteem protection and enhancement when making judgments of contingency.

The present study found a positive relationship between extent of overestimation and immediate self-esteem change, which supports the general prediction that overestimating control is motivated by a desire to enhance self-esteem. Furthermore, the correlation between extent of underestimation and immediate self-esteem change was nonsignificant, suggesting that underestimation generally was not associated with a desire for self-denigration. The data also indicated that overestimation of control typically occurred on the low control task (40-60, 20% control). On this task, subjects' control judgments tended to fluctuate around the midpoint (50%) of the one-hundred percent scale, with reinforcement frequency (light onset) on this 40-60 task also only at about 50%. If, in making their judgments of control, subjects were merely responding to their success in turning on the light, then they should not have shown an increase in immediate self-esteem at post-task, since this task showed lower frequency of light onset than the high control task. However, the fact that their self-esteem did increase suggests that subjects may have been responding at some level to the actual contingency, even though at the time of self-esteem increase, subjects had not yet been given accuracy feedback (and moreover, such feedback was not directional). Having low control over outcomes is often threatening to one's sense of competence in managing environmental events, and this threat could, in turn, impact

evaluations of self-worth. As a result, subjects may have been motivated to overestimate control on the low control task, partly to enhance self-esteem, and partly to carry out the previously discussed conservative strategy in control estimation.

Extent of overestimation of control was positively correlated with immediate self-esteem increase for both mild depressives and nondepressives, but not for the clinical depressives. This suggests that both the mildly depressed and nondepressed patients may have been similarly motivated to use overestimation as a means to enhance self-esteem. Contrary to speculation by previous investigators (e.g., Alloy & Abramson, 1979; 1988) that mildly depressed college students were unable or unmotivated to protect/enhance self-esteem through overestimating control, current findings indicated that the mild depressives were as motivated and as capable as the nondepressives in using overestimation to enhance self-esteem. Although clinical depressives did not apparently use an overestimation strategy to make themselves feel better, they also did not show any systematic tendency to underestimate control to reduce self-esteem, even though this might have brought their behavior into accord with their negativistic views of self. Apart from the positive relationship between self-esteem increase and overestimation of control, nondepressives also showed a relationship, though weaker, between self-esteem decrease and

underestimation of control. Thus, although nondepressives showed a stronger tendency to increase self-esteem after overestimating than to decrease esteem after underestimation, their levels of immediate self-esteem appeared to be responsive to both over- and underestimation. In contrast, clinically and mildly depressed patients showed no systematic tendency to reduce feelings of self-worth after underestimating control. This may reflect defensive self-esteem protection on their part.

Current results also revealed depression-dependent relationships between praise/blame scores and self-esteem change after accuracy feedback. For tasks on which subjects were told they were accurate in their control estimates (success feedback), the relationship between praise and self-esteem change scores was generally nonsignificant. In contrast, for tasks on which subjects were told they were inaccurate (failure feedback), blame scores were related to self-esteem change. Subjects who engaged in greater self-blame were also likely to exhibit a simultaneous decrease in self-esteem. Thus it appeared that, for all subjects considered together, self-blame was more influential in lowering self-esteem than self-praise was in raising esteem. This provides an interesting contrast to the possible effects of overestimating, where in general, overestimation was more influential in raising self-esteem than underestimation was in lowering self-esteem. To reiterate,

on the present contingency judgment task, overestimating control appeared to be an effective strategy for raising self-esteem, while self-blame was associated with reductions in self-esteem.

In examining responses to success and failure feedback among the three mood groups, a positive relationship between praise and self-esteem increase after success feedback was found for the clinical depresives but not for the mild depressives or nondepressives. Similarly, the relationship between blame and self-esteem decrease after negative feedback was also confined to the clinical depressives. That relationships between praise/blame and self-esteem increase/decrease were only observed among the clinical depressives is noteworthy. Self-esteem reflects 'the evaluative feelings and attitudes people hold of themselves' (Wells, 1988, p. 662). It appears, then, that only the clinically depressed people were showing a consistency in matching self-reward/punishment with their more global perceptions of self. The lack of what would most likely be an expected relationship between praise/blame and self-esteem change for the mild and non-depressives may be indicative of a dissociation between self-reward/punishment and their general feelings of self-satisfaction. These subjects were, perhaps, more likely to distort on the praise/blame than on the self-esteem measure. If the less depressed groups were attempting to be kinder to themselves

by being more positively biased in their self-reward/ punishment tendencies, this would apparently reflect a desire for self-esteem enhancement which, again, contradicts speculation by previous investigators that mildly depressed students are unmotivated to protect self-esteem.

Self-esteem change after failure feedback, but not after success feedback, was also sensitive to level of depression. The more depressed subjects were, the more likely they were to react to failure feedback with a greater decrease in self-esteem. It appears that clinically depressed individuals were more vulnerable than other subjects to loss of self-esteem as a result of performance failure. This finding does not, in itself, indicate a lack of motivation to protect self-esteem, but it does indicate that clinically depressed subjects may be less capable than nondepressives of protecting themselves from the damaging effects of failure. For nondepressed and mildly depressed individuals, this ability to protect self-esteem after failure may be mediated by their lower levels of self-blame coupled with the observed lack of association, in these groups, between self-blame and self-esteem decrease.

In comparing subjects' praise and blame scores for positive and negative outcomes, respectively, nondepressives gave themselves more praise for success than blame for failure. Mildly depressed subjects showed a similar effect, although to a lesser degree. Clinical depressives were

evenhanded in self-praise and self-blame for good and bad outcomes. Thus similar to the lack of relationship between over/underestimation of control judgments and self-esteem change observed among clinical depressives, their pattern of self-reward/punishment in response to success/failure feedback revealed neither a tendency toward self-enhancement nor self-denigration.

Current findings in regard to clinical depressives' evenhanded manner of accepting praise for positive and blame for negative outcomes differed from the findings of Sackeim and Wegner (1986, Study 2). In this study, subjects were exposed to two tasks: story rating and number guessing. They were given a series of stories with positive or negative outcomes, asked to imagine themselves in the role of the main character, and then told to complete a self-rating for their level of responsibility (praise/blame) for the outcomes. The number guessing task required subjects to complete a number series. Feedback was provided throughout the trials as well as at the conclusion of the task concerning the accuracy of the subjects' performance, at which time a measure of praise/blame was also taken. For both tasks, depressed patients showed a negative bias in assuming greater blame for negative than praise for positive outcomes. The reverse was true for nondepressed schizophrenics and normal controls. Current findings did not reflect this self-punitive bias found among the

depressed patients of Sackeim and Wegner's study, although their patients were as severely depressed as the clinical depressives in the present study.

Examination of the nature of tasks used in both studies may help explain the differing results. One should note that the button-pressing task in the present study differed from the number-guessing task in the former's use of cash incentive for task outcome (light onset). The clinical depressives in the current study may have perceived themselves as doing well because of the money they were making. Moreover, their cash earnings were clearly displayed throughout the experiment which may have served as a continual reminder of their success. This may have reduced the amount of blame taken for inaccurate judgments of control as subjects in the present study generally did not feel that they were failing.

In general, suggestions by Alloy and Abramson (1979; 1988) that depressed college students were relatively accurate in perceiving environmental contingencies because of their lack of motivation or ability to protect self-esteem were not supported, particularly for mildly depressed patients. Mild depressives in the present study were as motivated and as capable of using an overestimation strategy to enhance self-esteem as were nondepressives. They also showed a pattern of giving themselves more praise for success than blame for failure, which was highly similar to

the self-serving behavior of nondepressives. Furthermore, their self-reward/punishment tendencies were positively biased and incongruent with changes in their general feelings of satisfaction with self. Compared to more depressed patients, they were less responsive to failure feedback. All these indicate not only a motivation to protect/maintain self-esteem, but also the ability to defend themselves from possible assaults to their esteem.

Although the clinical depressives did not use overestimation as a strategy to enhance self-esteem, they also did not reduce self-esteem after underestimation of control. This and their evenhanded approach to self-praise relative to self-blame imply that the clinical depressives were also motivated to protect or maintain self-esteem.

Limitations and Recommendations

Generalization of current results must take into account certain limitations. The physical set-up of the hospitals made it necessary to administer the experiment with the experimenter seated across from the participant. The experimenter, however, was not in the observer role identified in previous studies which examined judgments of control in private versus public settings (e.g., Benassi & Mahler, 1985). Instructions in such studies were constructed to cue subjects about the role of the observers present. Although, current subjects were not provided with instructions that would imply that observing and evaluating

the subject would be the primary task of the experimenter, some evaluation as reflected in awarding cash and providing feedback was still an inevitable part of the experimental procedure.

Continued exploration of the depressive realism phenomenon with psychiatric samples is needed. Replication using tasks on which the depressive realism-nondepressive illusion effect is most reliable should be carried out. Specifically, noncontingent tasks with high and low reinforcement density should be used with hospitalized patients since these were the tasks which originally provided consistent findings of the phenomenon. Thus far, only one study has included a noncontingent task in its research design. Cobbs et al. (1990) found that both depressives and nondepressives overestimated control on a high reinforcement density noncontingent task (80-80, 0% control), with these groups not differing in their control estimates. Further investigation is necessary to determine whether this finding is robust.

Investigation of judgments of control among individuals who display the hopelessness subtype of depression is also recommended. In theorizing about this particular subtype of depression, Abramson et al. (1989) have removed the difficulty in perceiving response-outcome relationships (cognitive deficit) as one of the symptoms of depression because of findings from a number of studies establishing

the depressive realism phenomenon (e.g., Martin et al., 1984; Vazquez, 1987). However, it remains to be tested whether individuals identified as suffering from hopelessness depression would underestimate environmental contingencies.

Although length of hospitalization was not a factor in control judgments in the present study, other effects of hospitalization (e.g., helplessness; Raps et al., 1982) on peoples' perception of control over environmental events cannot be ruled out. Therefore, replication with an outpatient sample would be warranted.

Since this is the first study that attempted to test the self-esteem hypothesis in regard to depressive realism, further investigations are necessary to broaden the empirical base. Ongoing measures of immediate self-esteem as subjects judge contingency on tasks of varying objective control would provide important information on the role of self-esteem in people's perception of their effectiveness over environnmental events.

Finally, it is recommended that future investigations use more personally relevant, real-life tasks to assess differential judgments of environmental events. For example, subjects could be asked to predict the likelihood of occurrence, over a predetermined passage of time, of events or behaviors in their personal and social world (e.g., getting a better paying job, meeting a mate,

participating in a physical exercise program). These subjects could subsequently be queried as to the actual occurrences of those events (Dunning & Story, 1991). Comparisons could then be made between predictions and actual occurrences to ascertain whether depressed people are generally more realistic in judging and predicting life events.

APPENDIX A

INFORMED CONSENT

APPENDIX A

Terrell State Hospital
Terrell, Texas

INFORMED CONSENT I

Participant's Name: _____

1. I hereby give consent to Juliana S. Ee to carry out the following investigational procedure:

Give questionnaires and conduct brief test of cognitive functioning. Participants' individual scores will be confidential and anonymous, and the results, as represented in group averages, will be reported in appropriate scientific journals. Experimental tasks involve no attendant discomforts or personal risks. Benefits to the participants include monetary payment of $3.00 for the initial screening session and, if the subject is selected, from $5.00 to $15.00 for the experimental session, depending on the participant's performance. Any questions about the experimental procedures will be answered. The participant is free to withdraw from the experiment at any time without prejudice. All experimental tasks and questionnaire items must be completed to receive monetary payment.

2. Having received the above information and satisfactory answers to the questions I have asked, I voluntarily consent to the procedures designated in paragraph 1.

Signed: _____ Date: _____
Participant

Signed: _____ Date: _____
Witness

Signed: _____ Date: _____
Witness

APPENDIX A -- CONTINUED

Terrell State Hospital
Terrell, Texas

INFORMED CONSENT II

I, _____, have received an explanation of the research investigating individual problem-solving skills which is being conducted at this hospital.

I understand that my participation will be limited to answering several brief questionnaires and completing five tasks, each consisting of forty trials of pressing or not pressing a button in order to make a green light come on. The total time involved will be approximately one hour. I will be paid approximately $5.00 to $15.00 for my participation, the exact amount depending upon my 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 investigator, Juliana S. Ee, and her immediate supervisor, Joseph W. Critelli, Ph.D.

I also understand that the information I provide will be used for scientific research purposes only, and that the combined results gained from the approximately 160 participants may be published in scientific or professional journals, with no references being made as to individual identities.

I understand that Juliana S. Ee 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 this hospital, diagnosis, 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 this 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 this 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 Institutional Review Board (IRB) of this facility at any time concerning my treatment and welfare, by calling the IRB chairperson (214-563-6452). I also understand that I may consult with a member of the Public Responsibility Committee at any time concerning my treatment and welfare by writing to: Public Responsibility Committee, Box 304, Terrell, TX 75160. 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: Judgment of Control and Problem-solving, being conducted by Juliana S. Ee. I understand that she will be available to answer additional questions at any time and can be contacted at 817-565-2671 or 817-898-0363 (mailing address: Box 13587, NT Station, Denton, TX 76203-3587). Having read and understood this information, I hereby agree to participate in this project.

I, _____, understand each of the
Name of Participant
above items relating to the research entitled: Judgment of Control and Problem-solving, being conducted by Juliana S. Ee. I hereby consent to my participation in the project.

_____ _____
Signature of Participant Date

I, _____, understand each of the
Name of Participant
above items relating to the research entitled: Judgment of Control and Problem-solving, being conducted by Juliana S. Ee. I choose not to participate in this project.

_____ _____
Signature of Participant Date

I have explained the above items to _____ and believe that he/she understands
Name of Participant
each of the items.

_____ _____
Investigator's Signature Date

We were present at the explanation of the above items to _____ and we believe
Name of Participant
that he/she understands each of the above items.

_____ _____
Witness Date

_____ _____
Witness Date

Having already begun my participation in the research entitled: Judgment of Control and Problem-solving, being conducted by Juliana S. Ee, I hereby choose to withdraw from further participation in this project. I understand that my withdrawal will have no effect whatsoever on my treatment or length of stay at this hospital, and that I will not be paid any cash.

_____ _____
Name of Participant Date

We are present and have witnessed that _____ have chosen to withdraw from
Name of Participant
participating in this research study, and we believe that he/she understands that his/her withdrawal will have no effect whatsoever on his/her treatment or length of stay at this hospital.

_____ _____
Witness Date

_____ _____
Witness Date

APPENDIX A -- CONTINUED

INFORMATION DOCUMENT

Veterans Administration Medical Center
Dallas, Texas

INFORMATION ABOUT: Judgment of Control and Problem-solving

INVESTIGATORS: Joseph W. Critelli, Juliana S. Ee

PURPOSE AND NATURE OF THIS RESEARCH PROJECT: This study will determine how mood affects a person's judgment of how much control he or she has over outcomes on a particular type of task. We expect to learn whether tests of current theories of mood obtained with nonhospitalized subjects will apply to hospitalized patients.

WHAT YOU WILL BE ASKED TO DO IF YOU PARTICIPATE IN THIS STUDY: You will be asked to fill out several brief questionnaires, taking about 15 minutes to complete. On the basis of these responses you may be invited to participate in the second part of this study, which will involve completing five problem-solving tasks. On these tasks you will either press or not press a button in order to turn on a light and to determine how much control you have over light onset. All procedures are experimental. There is no guarantee that you will benefit from this study. No x-rays or radioisotopes will be used.

DURATION OF YOUR PARTICIPATION IN THIS STUDY: For those completing only the questionnaires, the study should take less than twenty minutes. For other subjects, the study should take about an hour and ten minutes.

POSSIBLE RISKS OF YOUR PARTICIPATION IN THIS STUDY: There are no known physiological, psychological, or social risks from these procedures.

POSSIBLE BENEFITS OF PARTICIPATION IN THIS STUDY:
(a) To yourself: Benefits include monetary payment and participation in a potentially enjoyable task.

(b) To others: Benefits include increased scientific understanding of the relationship between mood and cognition in hospitalized patient.

PAYMENT FOR PARTICIPATION IN THIS STUDY: All subjects will receive $3 for completing the preliminary questionnaires. Those subjects completing part two will earn, in addition, from $5 to $15, depending on task performance. Payment will be made immediately after each subject completes his or her

participation in the study. Subjects must complete all of the tasks and questionnaires in order to claim any money.

YOUR RIGHTS CONCERNING THIS STUDY:

(a) You have a right not to participate in this study. If you choose not to participate, your medical care will not affected by this decision.

(b) You are free to withdraw from this study at any time without prejudice.

(c) Your privacy will be protected, but representatives of the VA Research Committee or its subcommittee may review the records of your participation in the study.

(d) If you have problems or questions relating to this study, you may call Dr. William Harford at the Dallas VA Medical Center (214) 376-5451, ext. 5742 or Dr. Joseph W. Critelli, University of North Texas, (817) 565-2671.

(e) If you have any questions about your rights as a participant in this study, you may contact the Chairperson, Subcommittee on Human Studies, at the Dallas VA Medical Center, (214) 376-5451, ext. 5755.

CERTIFICATION: I have read, or have been read, the information given above. I have been given a copy of this Information Document and VA Form 10-1086.

Subject's signature	Date
Witness' signature	Date
Signature of investigator or person obtaining consent on behalf of the investigator	Date

APPENDIX A -- CONTINUED

INFORMATION DOCUMENT

Denton Regional Medical Center
Denton, Texas

INFORMATION ABOUT: Judgment of Control and Problem-solving

INVESTIGATORS: Joseph Critelli, Ph.D.; Juliana Ee, M.S.

PURPOSE AND NATURE OF THIS RESEARCH PROJECT: This study will determine how mood affects a person's judgment of how much control he or she has over outcomes on a particular type of task. We expect to learn whether tests of current theories of mood obtained with nonhospitalized subjects will apply to hospitalized patients.

WHAT YOU WILL BE ASKED TO DO IF YOU PARTICIPATE IN THIS STUDY: You will be asked to fill out several brief questionnaires, taking about 20 minutes to complete. On the basis of these responses you may be invited to participate in the second part of this study, which will involve completing five problem-solving tasks. On these tasks you will either press or not press a button in order to turn on a light and to determine how much control you have over light onset. All procedures are experimental. There is no guarantee that you will benefit from this study. No x-rays or radioisotopes will be used.

DURATION OF YOUR PARTICIPATION IN THIS STUDY: For those completing only the questionnaires, the study should take less than twenty minutes. For other subjects, the study should take about an hour and thirty minutes.

POSSIBLE RISKS OF YOUR PARTICIPATION IN THIS STUDY: There are no known physiological, psychological, or social risks from these procedures.

POSSIBLE BENEFITS OF PARTICIPATION IN THIS STUDY:
(a) To yourself: Benefits include monetary payment and participation in a potentially enjoyable task.

(b) To others: Benefits include increased scientific understanding of the relationship between mood and cognition in hospitalized patient.

PAYMENT FOR PARTICIPATION IN THIS STUDY: All subjects will receive $3 for completing the preliminary questionnaires. Those subjects completing part two will earn, in addition, from $5 to $15, depending on task performance. Payment will be made immediately after each subject completes his or her

participation in the study. Subjects must complete all of the tasks and questionnaires in order to claim any money.

YOUR RIGHTS CONCERNING THIS STUDY:

(a) You have a right not to participate in this study. If you choose not to participate, your medical care will not be affected by this decision.

(b) You are free to withdraw from this study at any time without prejudice.

(c) Your privacy will be protected, but representatives of Denton Regional Medical Center Research Review Committee may review the records of your participation in the study.

(d) If you have problems or questions relating to this study, you may call Dr. Frank McGehee at Denton Regional Medical Center - Westgate Campus at (817) 566-4000 or Dr. Joseph Critelli, University of North Texas, (817) 565-2671.

(e) If you have any questions about your rights as a participant in this study, you may contact Dr. Frank McGehee at Denton Regional Medical Center - Westgate Campus.

CERTIFICATION: I have read, or have been read, the information given above. I have been given a copy of this Information Document.

_____	_____
Subject's signature	Date
_____	_____
Witness' signature	Date
_____	_____
Signature of investigator or person obtaining consent on behalf of the investigator	Date

APPENDIX A -- CONTINUED

INFORMATION DOCUMENT

John Peter Smith Hospital
Fort Worth, Texas

INFORMATION ABOUT: Judgment of Control and Problem-solving

INVESTIGATORS: Joseph Critelli, Ph.D.; Juliana Ee, M.S.

PURPOSE AND NATURE OF THIS RESEARCH PROJECT: This study will determine how mood affects a person's judgment of how much control he or she has over outcomes on a particular type of task. We expect to learn whether tests of current theories of mood obtained with nonhospitalized subjects will apply to hospitalized patients.

WHAT YOU WILL BE ASKED TO DO IF YOU PARTICIPATE IN THIS STUDY: You will be asked to fill out several brief questionnaires, taking about 20 minutes to complete. On the basis of these responses you may be invited to participate in the second part of this study, which will involve completing five problem-solving tasks. On these tasks you will either press or not press a button in order to turn on a light and to determine how much control you have over light onset. All procedures are experimental. There is no guarantee that you will benefit from this study. No x-rays or radioisotopes will be used.

DURATION OF YOUR PARTICIPATION IN THIS STUDY: For those completing only the questionnaires, the study should take less than twenty minutes. For other subjects, the study should take about an hour and thirty minutes.

POSSIBLE RISKS OF YOUR PARTICIPATION IN THIS STUDY: There are no known physiological, psychological, or social risks from these procedures.

POSSIBLE BENEFITS OF PARTICIPATION IN THIS STUDY:
(a) To yourself: Benefits include monetary payment and participation in a potentially enjoyable task.

(b) To others: Benefits include increased scientific understanding of the relationship between mood and cognition in hospitalized patient.

PAYMENT FOR PARTICIPATION IN THIS STUDY: All subjects will receive $3 for completing the preliminary questionnaires. Those subjects completing part two will earn, in addition, from $5 to $15, depending on task performance. Payment will be made immediately after each subject completes his or her

participation in the study. Subjects must complete all of the tasks and questionnaires in order to claim any money.

YOUR RIGHTS CONCERNING THIS STUDY:

(a) You have a right not to participate in this study. If you choose not to participate, your medical care will not be affected by this decision.

(b) You are free to withdraw from this study at any time without prejudice.

(c) Your privacy will be protected, but representatives of John Peter Smith Hospital Nursing Research Committee may review the records of your participation in the study.

(d) If you have problems or questions relating to this study, you may call Dr. Joseph Critelli or Juliana Ee at University of North Texas, (817) 565-2671. You may also contact Robbie Helmich, R.N., M.S.N., Chairperson, Nursing Research Committee at John Peter Smith Hospital, (817) 921-3431.

(e) If you have any questions about your rights as a participant in this study, you may contact Robbie Helmich, R.N., M.S.N., Chairperson, Nursing Research Committee at John Peter Smith Hospital, (817) 921-3431.

CONFIDENTIALITY AND ANONYMITY CONCERNING YOUR PARTICIPATION:

(a) To protect your identity, your responses will be referred to only by a code number in the data collected.

(b) The investigator, Juliana Ee, will be allowed access to your hospital chart 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 your length of stay at this hospital, diagnosis, treatment, and medications given. This information will be kept strictly confidential. You will be referred to only by a code number so that your name will not be revealed.

(c) The data gathered through this research will be stored in a locked research room at University of North Texas Psychology Department. Destruction of the research protocols will take place five years after the conclusion of this project.

(d) The information provided by you will be used for scientific purposes only. The combined results gained from the approximately 150 participants may be published in scientific or professional journals, with no reference being made to individual identities.

RESULTS OF RESEARCH:

(a) If you are interested in the final results of this research, you may contact Joseph Critelli, Ph.D. or Juliana Ee, M.S. at University of North Texas, Psychology Department, P. O. Box 13587, Denton, Texas 76203 (817-565-2671).

(b) If you have questions concerning this research, you may contact either Joseph Critelli or Juliana Ee at the above address or phone number.

CERTIFICATION: I have read, or have been read, the information given above. I have been given a copy of this Information Document.

Subject's signature	Date
Witness' signature	Date
Signature of investigator or person obtaining consent on behalf of the investigator	Date

APPENDIX B

INSTRUCTIONS FOR JUDGMENT OF CONTINGENCY TASKS

APPENDIX B

INSTRUCTIONS FOR JUDGMENT OF CONTINGENCY TASKS

For the next hour, you will be working on 5 similar tasks. These tasks involve learning how to turn on a green light and finding out the amount of control that you have over the light coming on. You will receive a cash reward for your participation, the exact amount will be determined by how well you do on the 5 tasks.

Each time the red light comes on, it indicates the start of a new trial, the chance to do something. During this time, you have the choice of either making a button press response or not making a button press response. A button press response consists of pressing this button with your finger once, right after the red light comes on. Not making a button press response consists of doing nothing when the red light comes on, that is, keeping your finger off the button.

If you decide to press the button on a given trial, you must press within three seconds after the red light comes on; otherwise the trial will be counted as a not press trial.

So, in this experiment there are only two things you can do on each of the trials: either press the button within three seconds after the red light comes on, or else, just keep your finger off the button or sit back and do nothing. Any questions so far?

You may find that the green light will come on, on some percentage of the trials on which you do make a button press response. You may also find that the light will come on, on some percentage of trials when you do not make a button press response. On the other hand, you may find that the green light will not come on, on some percentage of the trials on which you do make a button press response. And, you may find that the green light will not come on, on some percentage of the trials when you do not make a button press response.

So, there are four things that can possibly happen on any given trial:

You <u>press</u> the button and the green light <u>comes on</u>;
You <u>press</u> the button and the light does <u>not come on</u>;
You <u>don't press</u> the button and the light <u>comes on</u>; and
You <u>don't press</u> the button and the light does <u>not come on</u>.

You have two jobs: (a) to make money by turning the green light on, and (b) to make money by learning how much control you have over whether or not the light comes on.

You will earn 5 cents each time the green light comes on. You will also earn one dollar on each task if your judgment of how much control you had over the light was accurate for that task. You can earn about the same amount of money for turning the light on as for accurately judging your control. This display here will show the amount of money you have earned thus far as you proceed through the 40 trials for each of the 5 tasks. All the money you earn will be given to you after you have completed all 5 tasks and all the questionnaires. You must complete all procedures to claim your money.

Since one of your jobs is to learn how much control you have over whether the green light comes on as well as whether the light does not come on, it will help to press the button on some trials and to not press the button on other trials. This way, you will find out what happens when you don't press as well as what happens when you do press the button.

Even though it is important to try and make the light come on, because you will earn money for that, sometimes you may want to try and prevent the light from coming on, as this will help you decide how much control you have over the light. Any questions?

On each of the 5 tasks, after you take your 40 chances to turn the light on, you will be asked how much control you had over whether or not the light came on. You will do this by putting an "X" someplace on a scale that looks like this:

---

0 5 10 15 20 25 30 35 40 45 50 55 60 65 70 75 80 85 90 95100
no control — intermediate control — complete control

Put your "X" at 100 if you believe that you had complete control over whether or not the green light came on;
Put your "X" at 0 if you believe that you had no control at all over the light coming on; or
Put your "X" somewhere between these extremes if you believe that you had some, but not complete, control over whether or not the light came on.

Complete control means that whether or not the light came on was completely determined by your choice of pressing or not pressing the button.

No control means that your choices of pressing or not pressing the button did not influence the light in any way. Another way of looking at having no control is that whether or not the light came on was totally determined by things such as chance or luck rather than by your choices of pressing or not pressing the button.

Intermediate degrees of control mean that your choice of pressing or not pressing the button influenced, but did not completely determine, whether the light came on or not. In other words, your choices mattered to some extent, but not totally.

If one of your responses, either pressing or not pressing the button, makes the green light come on more often than does the other response, this would indicate that you do have some control over light onset. So, if the light comes on more often when you press the button than when you do not press, then you would have some degree of control over the light. At the same time, if the light comes on more often when you do not press the button than when you do press, you would also have some degree of control over the light.

On the other hand, if the light comes on whether or not you press the button, this would indicate that you did not have control. Similarly, if the light does not come on regardless of whether you press or do not press the button, this would also indicate that you did not have control over the light. Any questions?

Now I would like to ask you a couple of questions to be sure that you understand the instructions. Is that okay?

If the green light comes on frequently when you press the button and it also comes on almost as frequently when you don't press the button, would you say that your control is low or high?

If the green light comes on very frequently when you do not press the button and hardly at all when you do press, would you say that your control is low or high?

Before we start the actual experiment, we will have 10 practice trials to help you get used to the equipment. Are you ready?

Okay, this is how we will go about doing each of the 5 tasks. At the beginning of each task, you will rate how much control you expect to have over the green light coming on. At the end of each task, you will rate how much control

you actually had over the green light coming on. You will also respond to several other questions for each task.

There are 5 booklets of questionnaires in front of you, one for each task. You will complete page 1 of the booklet before the task begins. As soon as the task is over, you will complete the remaining pages in that booklet. Do not go back to previous booklets to check your answers. I will announce the beginning and the end of each task, and I will remind you to check your booklet. Any questions?

APPENDIX C

DATA SHEET

APPENDIX C

DATA SHEET I

CODE: _____

BDI SCORE: _____

MMS SCORE: _____

DIAGNOSIS: _____

LOCATION: _____

LENGTH OF HOSPITALIZATION: _____

MEDICATIONS: _____

_____

_____

PREVIOUS PSYCHIATRIC CARE (?): _____

_____

APPENDIX C -- CONTINUED

DATA SHEET II

CODE: _____ SEX: _____ AGE: _____

ETHNIC STATUS:

_____ White-American
_____ Black-American
_____ Hispanic-American
_____ American Indian
_____ Asian-American
_____ Other (please specify)

MARITAL STATUS:

_____ Single
_____ Married
_____ Separated
_____ Divorced
_____ Widowed
_____ Other (please specify)

RELIGIOUS PREFERENCE:

_____ Protestant
_____ Catholic
_____ Jewish
_____ Atheist
_____ Other (please specify)

HIGHEST LEVEL OF EDUCATION COMPLETED:

_____ Have doctoral degree
_____ Have masters degree
_____ Completed college
_____ Attended college (please indicate number of years)
_____ Completed high school
_____ Completed some schooling (please specify number of grades)
_____ No formal education

CURRENT OCCUPATION: _____

APPROXIMATE YEARLY INCOME (BEFORE HOSPITALIZATION):

_____ over $50,000
_____ $40,000 - $50,000
_____ $30,000 - $40,000
_____ $20,000 - $30,000
_____ $10,000 - $20,000
_____ less than $10,000

APPENDIX D

IMMEDIATE SELF-ESTEEM

APPENDIX D

Code: _____

Immediate Self-Esteem

Please rate the following by circling the appropriate number.

At this moment:

	not at all			some-what			quite			very
Are you feeling good about yourself?	0	1	2	3	4	5	6	7	8	9
Are you in control of the situation?	0	1	2	3	4	5	6	7	8	9
Would you say that for the most part you are living up to your expectations of self?	0	1	2	3	4	5	6	7	8	9
Would you say that for the most part you are living up to the expectations of others important to you?	0	1	2	3	4	5	6	7	8	9
Are you satisfied with how you are doing?	0	1	2	3	4	5	6	7	8	9

APPENDIX E

PRE- AND POST-TASK MEASURES

APPENDIX E

Code: _____

Please rate the following questions by putting an X on the line.

In a moment we will start Task #____.

(1) Before we start, please rate the degree of control you expect your responses (pressing or not pressing the button) will have over the green light coming on.

I expect to have

0 5 10 15 20 25 30 35 40 45 50 55 60 65 70 75 80 85 90 95100
no control — intermediate control — complete control

percent control in making the green light come on.

(2) At the end of the task you will rate the degree of control that you had over the green light coming on. Right now, please rate how accurate you expect your control rating to be.

I expect to be

0 5 10 15 20 25 30 35 40 45 50 55 60 65 70 75 80 85 90 95100
completely inaccurate — completely accurate

percent accurate in making a correct judgment of control rating at the end of this task.

==========

Now that you have completed Task #_____, please rate the degree of control that your responses (pressing or not pressing the button) had over the green light coming on.

I had

0 5 10 15 20 25 30 35 40 45 50 55 60 65 70 75 80 85 90 95100
no control — intermediate control — complete control

percent control in making the green light come on.

APPENDIX F

PRAISE/BLAME MEASURE

APPENDIX F

Code: _____

Please answer the following by putting an X on the line.

(1) Please rate the extent of praise that you deserve for your performance.

I feel I deserve

0 5 10 15 20 25 30 35 40 45 50 55 60 65 70 75 80 85 90 95100
no praise total praise

percent praise for my task performance.

(2) Please rate the extent of blame that you deserve for your performance.

I feel I deserve

0 5 10 15 20 25 30 35 40 45 50 55 60 65 70 75 80 85 90 95100
no blame total blame

percent blame for my task performance.

APPENDIX G

POST-EXPERIMENT QUESTIONNAIRE

APPENDIX G

Code: _____

I would like your impression of the experiment. Please work as quickly as you can through these questions. Remember, your first impression is the best answer.

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

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

3. What are the factors affecting the flashing of the green light?

4. Please rate the amount of effort you put into performing the tasks.

I put

0 5 10 15 20 25 30 35 40 45 50 55 60 65 70 75 80 85 90 95100
no effort — total effort

percent effort into performing the tasks.

5. Please rate how involved you were in performing the tasks.

I was

0 5 10 15 20 25 30 35 40 45 50 55 60 65 70 75 80 85 90 95100
no involvement — total involvement

percent involved in performing the tasks.

APPENDIX H

TABLES

APPENDIX H

Table 1

Pearson Correlations Between Average Judgment of Control and Selected Variables on Which Experimental Groups Differed

	JOC	LOH	MMS	ISE	DEP	FLO
Judgment of Control (JOC)	--	.14	-.06	.09	-.07	-.41**
Length of Hospitalization (LOH)		--	-.00	-.12	.14	.11
Mini-Mental State (MMS)			--	-.08	-.01	.12
Pre-Expt. Immediate Self-esteem (ISE)				--	-.53**	-.15
Pre-Expt. MAACL-R Depression (DEP)					--	.16
Frequency of Light Onset (FLO)						--

*p < .05, **p < .01

APPENDIX H -- CONTINUED

Table 2

Means and Standard Deviations for Descriptive Variables and Dependent Measures Averaged Across Tasks

		CD	MS	MM	NS	NM
Age	M	36.33	37.30	39.60	36.37	39.80
	SD	10.02	7.95	9.26	7.80	10.75
Length of Hosp. (days)	M	8.73	15.87	9.73	17.57	6.57
	SD	8.33	21.83	16.92	17.59	7.46
MMS	M	28.03	27.23	27.87	26.73	28.43
	SD	1.50	2.14	2.03	1.89	1.57
BDI	M	28.30	12.70	12.37	4.13	4.70
	SD	9.66	1.86	2.01	2.99	2.53
Pre-Expt. RSE	M	22.13	29.57	30.03	33.30	34.00
	SD	5.23	4.72	4.07	4.34	4.28
Post-Expt. RSE	M	22.93	28.97	30.13	32.67	34.00
	SD	6.27	3.83	3.97	4.27	4.56
Pre-Expt. MAACL-R Depression	M	6.03	2.80	1.43	2.20	0.43
	SD	3.21	3.21	1.48	2.54	0.90
Post-Expt. MAACL-R Depression	M	5.03	2.37	1.07	1.03	0.67
	SD	3.36	3.09	1.39	1.43	2.07
Total Freq. Press Response	M	108.93	116.97	110.13	122.03	111.90
	SD	24.99	29.07	17.24	29.48	28.07

APPENDIX H -- CONTINUED

Table 2 -- Continued

		CD	MS	MM	NS	NM
Total Freq. No-Press Response	M	91.07	83.03	89.87	77.97	88.10
	SD	24.99	29.07	17.24	29.48	28.07
Total Freq. Light Onset	M	133.53	119.97	127.23	125.60	128.53
	SD	11.84	14.19	14.60	12.51	11.30
Expect. of Control	M	51.43	49.53	47.93	60.00	57.23
	SD	14.93	23.85	17.32	18.55	16.75
Expect. of Accuracy	M	53.70	54.17	49.00	60.33	58.47
	SD	16.62	22.18	16.15	19.95	16.07
No. of Tasks Accurate	M	1.86	1.52	1.96	1.67	1.64
	SD	0.76	0.80	0.79	0.92	0.70
Effort	M	87.83	83.17	90.17	83.00	85.83
	SD	14.42	17.49	13.74	16.22	17.52
Involvement	M	89.17	84.67	93.00	85.33	87.67
	SD	12.25	16.45	10.31	17.71	16.80

Note. CD = Clinically depressed patients; MS = Mildly depressed schizophrenics; MM = Mildly depressed medical patients; NS = Nondepressed schizophrenics; NM = Nondepressed medical patients.

APPENDIX H -- CONTINUED

Table 3

Intercorrelations Between Depression and Self-esteem Measures

	BDI	PRED	POSD	PRER	POSR	ISE
BDI	--	.58**	.56**	-.72**	-.66**	-.57**
Pre-Expt. MAACL-R Depression (PRED)		--	.76**	-.53**	-.53**	-.53**
Post-Expt. MAACL-R Depression (POSD)			--	-.52**	-.52**	-.50**
Pre-Expt. Rosenberg Self-esteem (PRER)				--	.91**	.66**
Post-Expt. Rosenberg Self-esteem (POSR)					--	.66**
Pre-Expt. ISE						--

**p < .01

APPENDIX H -- CONTINUED

Table 4

Means and Standard Deviations for Repeated Measures

		CD	MS	MM	NS	NM
Task 1 - 20% control						
Expect. of Control	M	48.83	45.83	48.00	59.00	55.17
	SD	22.43	27.17	18.55	19.49	18.82
Expect. of Accuracy	M	51.67	48.00	50.83	55.00	60.50
	SD	21.55	24.06	18.80	20.97	15.28
Judgment of Control	M	54.50	51.00	51.83	61.50	56.50
	SD	23.90	23.47	14.65	21.02	21.42
Accuracy Score	M	34.50	31.00	31.83	41.50	36.50
	SD	23.90	23.47	14.65	21.02	21.42
Percent of Light Onset	M	60.25	57.58	57.92	59.42	57.50
	SD	8.05	6.38	6.95	6.97	5.91
Task 2 - 80% control						
Expect. of Control	M	49.00	40.33	43.17	58.33	52.17
	SD	20.15	26.97	19.19	23.57	23.73
Expect. of Accuracy	M	50.33	50.50	45.83	64.33	55.00
	SD	20.38	30.66	18.76	25.52	22.25
Judgment of Control	M	64.33	53.17	60.67	70.83	51.67
	SD	21.24	31.80	28.64	22.56	29.08

APPENDIX H -- CONTINUED

Table 4 -- Continued

		CD	MS	MM	NS	NM
Accuracy Score	M	-15.67	-26.83	-19.33	- 9.17	-28.33
	SD	21.24	31.80	28.64	22.56	29.08
Percent of Light Onset	M	73.33	69.42	66.83	73.50	70.42
	SD	11.30	16.18	14.66	14.88	14.42
Task 3 - 50% control						
Expect. of Control	M	53.50	50.33	47.33	65.50	53.67
	SD	21.66	30.17	23.44	24.51	23.27
Expect. of Accuracy	M	54.83	53.50	49.00	61.50	55.83
	SD	21.87	29.10	23.90	25.30	26.30
Judgment of Control	M	51.00	49.00	53.83	51.17	60.00
	SD	26.21	29.29	28.58	25.92	22.17
Accuracy Score	M	1.00	- 1.00	3.83	1.17	10.00
	SD	26.21	29.28	28.58	25.92	22.17
Percent of Light Onset	M	64.75	57.08	61.92	61.92	64.00
	SD	10.91	13.18	10.50	12.55	11.44
Task 4 - 80% control						
Expect. of Control	M	49.83	52.00	50.17	59.33	59.17
	SD	22.65	29.05	26.05	27.16	19.92
Expect. of Accuracy	M	52.33	57.67	50.50	58.83	56.67
	SD	24.13	26.64	24.58	30.25	21.87

APPENDIX H -- CONTINUED

Table 4 -- Continued

		CD	MS	MM	NS	NM
Judgment of Control	M	73.83	53.67	68.33	54.33	65.17
	SD	23.81	31.46	30.97	29.09	19.72
Accuracy Score	M	- 6.17	-26.33	-11.67	-25.67	-14.83
	SD	23.81	31.46	30.97	29.09	19.72
Percent of Light Onset	M	79.17	57.08	74.33	64.33	72.58
	SD	13.96	20.15	15.69	19.91	14.50
Task 5 - 20% control						
Expect. of Control	M	56.00	59.17	51.00	57.83	66.00
	SD	24.86	30.35	28.33	26.77	18.91
Expect. of Accuracy	M	59.33	61.17	48.83	62.00	64.00
	SD	24.56	28.58	24.20	29.47	21.57
Judgment of Control	M	47.33	51.00	44.83	53.33	52.83
	SD	25.15	24.26	29.11	27.11	22.08
Accuracy Score	M	27.33	31.00	24.83	33.33	32.83
	SD	25.12	24.26	29.11	27.11	22.08
Percent of Light Onset	M	56.33	78.75	57.08	54.83	56.83
	SD	7.59	5.86	5.46	7.93	6.19

Note. CD = Clinically depressed patients; MS = Mildly depressed schizophrenics; MM = Mildly depressed medical patients; NS = Nondepressed schizophrenics; NM = Nondepressed medical patients.

APPENDIX H -- CONTINUED

Table 5

Means and Standard Deviations for Average Accuracy Scores by Mood Groups

	n	M	SD
Clinical Depressives	30	8.20	16.15
Mild Depressives	60	3.73	19.61
Nondepressives	60	7.73	15.16

Note. Accuracy score = judged control minus objective control.

APPENDIX H -- CONTINUED

Table 6

Means and Standard Deviations for Average Accuracy Scores for Diagnostic Groups by Trials

		Trial 1 (Tasks 1 & 2)	Trial 2 (Tasks 4 & 5)
Clinical Depressives	M	9.42	10.58
	SD	19.64	21.29
Mildly Depressed Schizophrenics	M	2.08	2.33
	SD	23.18	24.76
Mildly Depressed Medical Patients	M	6.25	6.58
	SD	15.71	27.46
Nondepressed Schizophrenics	M	16.17	3.83
	SD	17.02	20.72
Nondepressed Medical Patients	M	4.08	9.00
	SD	20.12	18.34

Note. Accuracy scores = judged control minus actual control.

Trial 1 = average of Tasks 1 and 2.

Trial 2 = average of Tasks 4 and 5.

APPENDIX H -- CONTINUED

Table 7

Means and Standard Deviations for Average Accuracy Scores for Tasks of Varying Degrees of Control

		Low 20% control	Moderate 50% control	High 80% control
Clinical Depressives	M	30.92	1.00	-10.92
	SD	20.21	26.21	20.23
Mild Depressives	M	29.67	1.42	-21.04
	SD	17.94	28.79	26.00
Non Depressives	M	36.04	5.58	-19.50
	SD	18.24	24.32	19.95
Entire Sample	M	32.47	3.00	-18.40
	SD	18.64	26.47	22.79

Note. Accuracy score = judged control minus objective control.

Low (20% control) = average of Tasks 1 and 5.

High (80% control) = average of Tasks 2 and 4.

APPENDIX H -- CONTINUED

Table 8

Descriptive Statistics for Experienced Actual Control

	M	SD	Range
Task 1 (40-60) 20% control	15.91	11.94	0.00 -- 63.16
Task 2 (10-90) 80% control	78.09	11.34	42.00 -- 100.00
Task 3 (25-75) 50% control	50.72	15.29	4.40 -- 85.71
Task 4 (10-90) 80% control	77.97	12.69	7.69 -- 97.22
Task 5 (40-60) 20% control	23.16	17.35	0.00 -- 64.87

APPENDIX H -- CONTINUED

Table 9

Intercorrelations Between Judgment of Control, Expectancy of Control, Experienced Actual Control, and Frequency of Light Onset

	JOC	EOC	EAC	FLO
Task 1 - 20% control				
Judgment of Control (JOC)	--	.51**	.17*	.20*
Expect. of Control (EOC)		--	.21**	.07
Exper. Actual Control (EAC)			--	.08
Freq. of Light Onset (FLO)				--
Task 2 - 80% control				
Judgment of Control (JOC)	--	.41**	-.03	.44**
Expect. of Control (EOC)		--	.03	.17*
Exper. Actual Control (EAC)			--	.18*
Freq. of Light Onset (FLO)				--
Task 3 - 50% control				
Judgment of Control (JOC)	--	.49**	.03	.25**
Expect. of Control (EOC)		--	.01	.04
Exper. Actual Control (EAC)			--	.34**
Freq. of Light Onset (FLO)				--
Task 4 - 80% control				
Judgment of Control (JOC)	--	.46**	-.01	.52**
Expect. of Control (EOC)		--	-.14	-.02
Exper. Actual Control (EAC)			--	.03
Freq. of Light Onset (FLO)				--
Task 5 - 20% control				
Judgment of Control (JOC)	--	.57**	.04	.22**
Expect. of Control (EOC)		--	.04	-.01
Exper. Actual Control (EAC)			--	-.19*
Freq. of Light Onset (FLO)				--

*$p < .05$, **$p < .01$

APPENDIX H -- CONTINUED

Table 10

Hierarchical Regression Results

	Mult. R	R^2	R^2 chg	F chg	Sig. F change	F	p
Task 1 - 20%							
Expect. of Control	.490	.240	.240	46.43	.00	F(1,147)=46.43	.000
Exper. Act. Control	.494	.244	.004	0.78	.38	F(2,146)=23.57	.000
Freq. of Light	.519	.270	.026	5.07	.03	F(3,145)=17.84	.000
Task 2 - 80%							
Expect. of Control	.407	.166	.166	28.60	.00	F(1,144)=28.60	.000
Exper. Act. Control	.409	.167	.002	0.28	.60	F(2,143)=14.37	.000
Freq. of Light	.558	.311	.143	29.56	.00	F(3,142)=21.35	.000
Task 3 - 50%							
Expect. of Control	.486	.236	.236	44.15	.00	F(1,143)=44.15	.000
Exper. Act. Control	.486	.237	.001	0.12	.72	F(2,142)=22.00	.000
Freq. of Light	.551	.304	.067	13.60	.00	F(3.141)=20.50	.000
Task 4 - 80%							
Expect. of Control	.459	.211	.211	37.44	.00	F(1,140)=37.44	.000
Exper. Act. Control	.462	.214	.003	0.51	.48	F(2,139)=18.91	.000
Freq. of Light	.707	.500	.286	78.78	.00	F(3,138)=45.92	.000
Task 5 - 20%							
Expect. of Control	.573	.328	.328	72.38	.00	F(1,148)=72.38	.000
Exper. Act. Control	.573	.329	.001	0.07	.79	F(2,147)=36.00	.000
Freq. of Light	.619	.383	.054	12.77	.00	F(3,146)=30.17	.000

APPENDIX H -- CONTINUED

Table 11

Pearson Correlations Involving Extent of Over/ Underestimation of Control and Immediate Self-esteem Change

Corr. between and controlling for	Over/Under ISE	Over/Under Expcon	Expcon ISE	Over/Under ISE Expcon
When Overestimation Occurs				
Entire Sample	.34***	.33***	-.04	.38***
CD	-.11	.55***	-.11	-.07
MD	.53***	.32**	-.01	.57***
ND	.40***	.20	-.00	.41***
When Underestimation Occurs				
Entire Sample	.11	.29***	-.08	.14
CD	.19	-.12	-.26	.17
MD	-.10	.58***	-.06	-.08
ND	.28*	.10	-.03	.29*

*p < .05; **p < .01; ***p < .001

Note. CD = Clinical Depressives; MD = Mild Depressives; ND = Nondepressives.

Immediate Self-esteem = post-task ISE minus pre-task ISE.

Expcon = Expectancy of Control.

APPENDIX H -- CONTINUED

Table 12

Means and Standard Deviations for Average Immediate Self-esteem Change Scores for Tasks on which Over/Underestimation Occurred

		When Subjects Overestimated Control	When Subjects Underestimated Control
Clinical Depressives	M	0.39	1.53
	SD	3.85	4.75
Mild Depressives	M	0.91	0.45
	SD	4.20	5.50
Non Depressives	M	0.07	0.50
	SD	3.90	4.30

Note. Immediate self-esteem change scores = post-task ISE minus pre-task ISE.

APPENDIX H -- CONTINUED

Table 13

Pearson Correlations Between Praise/Blame and Immediate Self-esteem Change Scores After Feedback

	Correlation Between	
	Praise & ISE (Success Feedback)	Blame & ISE (Failure Feedback)
Entire Sample	.14	-.17*
Clinical Depressives	.41*	-.40*
Mild Depressives	.11	-.11
Nondepressives	.02	-.01

*$p < .05$

APPENDIX H -- CONTINUED

Table 14

Means and Standard Deviations for Average Immediate Self-esteem Change Scores After Feedback

		Success Feedback	Failure Feedback
Clinical Depressives	*M*	1.71	-2.09
	SD	3.67	3.22
Mild Depressives	*M*	1.31	-1.45
	SD	3.00	3.39
Non Depressives	*M*	1.09	-0.93
	SD	2.74	2.44

Note. Immediate self-esteem change scores = post-feedback minus pre-feedback ISE scores.

APPENDIX H -- CONTINUED

Table 15

Pearson Correlations Between Depression and Average Immediate Self-esteem Change Scores After Accuracy Feedback

Corr. Between and	BDI ISE	MAACL-R Depression ISE
Success Feedback	.09	.02
Failure Feedback	-.17*	-.20*

*$p < .05$

Note. ISE = immediate self-esteem

APPENDIX H -- CONTINUED

Table 16

Means and Standard Deviations for Average Praise/ Blame Scores After Accuracy Feedback

		Praise (Success Feedback)	Blame (Failure Feedback)
Clinical Depressives	M	50.77	52.28
	SD	28.84	27.88
Mild Depressives	M	52.24	40.47
	SD	29.40	28.57
Non Depressives	M	65.90	33.84
	SD	24.06	27.54

APPENDIX H -- CONTINUED

Table 17

Means and Standard Deviations for Pre- and Post-Experiment Depression and Global Self-esteem Scores

		Pre-Expt. MAACL-R Depression	Post-Expt. MAACL-R Depression	Pre-Expt. Self-esteem	Post-Expt. Self-esteem
CD	*M*	6.03	5.03	22.13	22.93
	SD	3.21	3.36	5.23	6.27
MD	*M*	2.12	1.72	29.80	29.55
	SD	2.57	2.46	4.38	3.91
ND	*M*	1.32	0.85	33.65	33.33
	SD	2.09	1.77	4.29	4.43

Note. CD = Clinical Depressives; MD = Mild Depressives; ND = Nondepressives.

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