# THE UNIVERSITY OF CALGARY

Depressive Realism in Clinically Depressed, Remitted and Nondepressed Female Subjects

by

Dennis Pusch

## A THESIS

SUBMITTED TO THE FACULTY OF GRADUATE STUDIES IN PARTIAL FULFILLMENT OF THE REQUIREMENTS FOR THE DEGREE OF MASTER OF SCIENCE DEPARTMENT OF PSYCHOLOGY

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# THE UNIVERSITY OF CALGARY FACULTY OF GRADUATE STUDIES

The undersigned certify that they have read, and recommend to the Faculty of Graduate Studies for acceptance, a thesis entitled "Depressive Realism in Clinically Depressed, Remitted and Nondepressed Female Subjects" submitted by Dennis Pusch in partial fulfillment of the requirements for the degree of Master of Science.

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#### ABSTRACT

The major cognitive theories of depression share the view that depressed individuals possess a systematic cognitive bias which causes them to interpret the world around them in a negatively distorted manner. However, in recent years a number of studies have emerged which challenge this traditional view of depression, suggesting instead that depressed individuals tend to exhibit realistic thinking. Alloy and Abramson (1979) sparked interest in the "depressive realism" phenomenon by demonstrating that dysphoric college students exposed to a judgment of contingency task were more accurate than nondysphoric students in judging the amount of control they had over the experimental situation. This finding has since been replicated in а series of methodologically similar studies. is still unclear, It though, whether or not depressive realism is operative in clinically depressed subjects. is also unclear if It realistic thinking serves as a vulnerability factor for depression, as opposed to being a mood-state dependent phenomenon.

The present study included 15 clinically depressed females, 15 remitted depressed females, and 15 females who had never met criteria for clinical depression. In the first part of the experiment, all subjects were exposed to a computerized version of the Alloy and Abramson (1979; Experiment 2) judgment of noncontingency task. It was hypothesized that the

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currently depressed subjects would demonstrate realistic judgments of control, while the remitted and nondepressed subjects would demonstrate an optimistic bias. In the second part of the experiment, the remitted and nondepressed subjects were exposed to a depressive mood induction and subsequently repeated the judgment of noncontingency task. It was hypothesized that these subjects would provide more realistic judgments after the second exposure to the task as a function of their lowered mood. A control group of 15 females was also exposed to the noncontingency task twice, without undergoing a mood induction, to demonstrate that judgments of control remain stable across time in the absence of a change in mood.

The results provide limited support for the experimental hypotheses. A one-way ANOVA failed to reveal any significant differences between the depressed, remitted and nondepressed subjects at Time 1. The depressed subjects proved to be unrealistic, overestimating their actual degree of control by almost 43%, as opposed to 33% for the remitted subjects and 43% for the nondepressed subjects. However, the remitted and nondepressed groups both became significantly more realistic following a depressive mood induction ( $\underline{F}(1,28) = 7.02$ ,  $\underline{p} < .05$ ), indicating that realistic thinking is at least partially related to current mood state. The nature and implications of these conflicting results are discussed in light of the broader depressive realism literature. Suggestions for future research are also provided.

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# DEDICATION

To my wife, Jackie. Without your love, patience, and support, none of this would have been possible. Thank you.

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### Introduction

Depression is the most common of all psychological disorders (Rosenhan & Seligman, 1989). This debilitating disorder is characterized by symptoms such as sadness, loss of interest and pleasure, low self-esteem, pessimism, difficulty making decisions, loss of appetite, and sleep disturbance (American Psychiatric Association, 1987). Estimates suggest that about one out of twenty Americans is currently severely depressed (Myers, Weismann, Tischler, Holzer, Leaf, Orvaschel, Anthony, Boyd, Burke, Kramer, & Stolzman, 1984). A given individual has a one in ten chance of developing a clinical level of depression at least once in their lifetime. However, the risk seems to be comparatively greater for individuals born after 1960, as evidenced by the Epidemiological Catchment Area study, which reported a roughly tenfold increase in risk for depression across two generations (Robins, Helzer, Weismann, Orvaschel, Gruenberg, Burke, & Regier, 1984). Neither race nor social class appear to be strongly related to the incidence of depression. However, gender does seem to be an important variable, with women generally being at twice the risk for depression as men (Nolen-Hoeksema, 1988).

When one considers the pervasiveness of depression in North American society, it hardly seems surprising that vast research efforts have been expended in an attempt to uncover the etiology and course of this illness. Various theoretical models have been put forward to explain depression (e.g., the psychodynamic model, the biological model, and the cognitive model; Rosenhan & Seligman, 1989), and these models have often spawned clinical/therapeutic interventions. Each of these models has made valid contributions to our understanding of depression, and no one model can claim to hold a corner on truth. With this caveat in mind, we turn our focus to the cognitive aspects of depression, as it is the investigation of cognitive factors which forms the basis for the present study.

The major cognitive theories of depression share the view that depressed individuals possess a systematic cognitive bias which causes them to look at and interpret the world around them in a negatively distorted manner (eg. Beck, 1976; Ellis, Abramson, Metalsky, & Alloy, 1962; Rehm, 1977; 1989). Teasdale (1983) reviewed the existing empirical evidence designed to test the negative cognition models of depression, and concluded that the association between depression and negative thinking appears to be irrefutable. Teasdale reported a series of studies which specifically indicated that certain negatively biased cognitions can produce and maintain the state of depression at both clinical and subclinical levels. Similar links between depression and depressive attributional biases (or dysfunctional attitudes, or negative automatic thoughts) have since been reported by a number of authors (eq., Eaves & Rush, 1984; Miranda & Persons, 1988; Wenzlaff & Grozier, 1988; Zimmerman, Coryell, Corenthal, & Wilson, 1986).

In contrast to the view that depressed individuals are negatively biased in their perceptions, a considerable amount of empirical evidence has emerged in the past decade which indicates that perhaps depressed individuals are the ones who are able to judge reality accurately, while nondepressed individuals may possess a self-serving optimistic bias,

resulting in unrealistically positive perceptions of their ability to bring about positive outcomes while avoiding undesirable ones (Sherman, 1980; Taylor & Brown, 1988). This "depressive realism" (Mischel, 1979) phenomenon was first reported by Alloy and Abramson in their "Sadder but wiser" paper, which featured a series of judgment of contingency tasks (Alloy & Abramson, 1979). This initial finding sparked considerable interest in the depression research community, mainly because the findings appeared to be counter-intuitive, and also because they contradicted the established cognitive theories of depression (Coyne & Gotlib, 1983).

The present study was designed to help provide answers to several important questions which have been raised in the depressive realism literature. The questions revolve mainly around the issue of the generalizability of the findings to realism and whether depressive serves date, as а vulnerabiliity factor for depression. In order to provide a framework for these questions, the relevant literature is briefly reviewed below. The rationale and hypotheses for the present study are then presented.

# Overview of Depressive Realism Literature

A number of experimental paradigms have been introduced in an effort to delineate the boundary conditions under which depressive realism operates. Foremost among these paradigms are the judgment of contingency studies mentioned above. Alloy and Abramson (1979, Experiments 1 through 4), building on a procedure initially used by Jenkins and Ward (1965), presented dysphoric and nondysphoric college students with one of 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 (the onset or not of a green light), as well as in the frequency and valence of the outcome. The actual degree of contingency in these experiments depended on the relationship between a subject's responses and the stimulus onset. For example, in a 75-25 situation, where the green light appeared 75% of the times that a subject pushed the button, but only appeared on 25% of the trials during which the subject did not push the button, the actual degree of contingency would be regarded as 50% (i.e., the absolute difference between the two probabilities). A noncontingent situation was one in which the green light appeared with the same frequency regardless of whether or not the button was pressed. For instance, if the green light appeared on 50% of the trials when the button was pushed, and also appeared on 50% of the trials when the button was not pushed (i.e., a 50-50 situation), the outcome was

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judged to be objectively noncontingent.

Contrary to the predictions of the learned helplessness model, the results showed that in both contingent and noncontingent conditions dysphoric students accurately judged the amount of control they had over the onset of the green light, while the nondysphoric students systematically erred in judging their control as greater than it objectively was. However, the effect was found to be strongest a) when there was no actual relationship between responses and outcomes, b) when the noncontingent outcomes occurred frequently, and c) when the outcomes were associated with success (i.e., winning money).

These initial findings were interpreted by Alloy and Abramson (1979) as being a function of the different levels of self-esteem between the two groups. It was suggested that nondepressed people's higher levels of self-esteem (Beck, 1976) could be accounted for by their tendency to perceive the environment in a distorted manner, resulting in a selfenhancing feeling of control. Naturally, depressed people would not be inclined to distort reality in a similar manner, since there would be little to be gained by protecting selfesteem which was already operating at a low level.

The original Alloy and Abramson (1979) results have since been replicated in a number of methodologically similar studies (eg., Mikulincer, Gerber, & Weisenberg, 1990; but see also Bryson, Doan, & Pasquali, 1984 for a failure to

replicate). In 1981, Alloy, Abramson, and Viscusi induced depressed mood in nondepressed college students and elated mood in naturally dysphoric students in order to assess the impact of these temporary mood states on the students' susceptibility to the illusion of control. The results indicated that mood state does seem to be an influential variable, as the dysphoric students temporarily made elated exhibited an illusion of control over an objectively uncontrollable event, while the nondepressed students . temporarily made depressed accurately judged the degree of control they had in the experimental situation. Of course, the finding that mood state may influence individuals' judgments of control in no way rules out the possibility that failure to succumb to the illusion may also serve as an invulnerability factor, lessening the likelihood of a given individual developing depressive mood states.

Alloy and Clements (in press) have recently tried to shed some light on whether the illusion of control is simply mood state dependent, or whether it may actually serve as an invulnerability factor for depression. These researchers measured subjects' degree of realism by exposing them to the Alloy and Abramson (1979, Experiment 2) noncontingent-win judgment of control task. The subjects were subsequently exposed to a lab stressor (failure on unsolvable block designs), and were also followed up one month later to assess their reactions to naturally occurring life stressors. It was

found that those subjects who showed a greater illusion of control were less likely to experience negative mood reactions following both the laboratory stressor and their own negative life events. When combined with the findings of Alloy et al. (1981), the results seem to suggest that the relationship between depression and the illusion of control may actually be reciprocal (Taylor & Brown, 1988). In other words, people who have a self-enhancing bias may be at lower risk for developing depression following stressful life events, but at the same time, positive affect may enhance a person's susceptibility to the illusion of control. Alloy and Clements (in press) conclude that "optimistic illusions, positive affect, and subjective and physical well-being may operate as a mutually interdependent, self-perpetuating adaptive system."

In 1982, Alloy and Abramson tested the hypothesis that the illusion of control was not so much related to current mood as it was to learned helplessness. According to the learned helplessness model, individuals who experience learned helplessness, regardless of mood state, will tend to provide realistic judgments of control. Alloy and Abramson (1982) tested this model by exposing both depressed and nondepressed subjects to either controllable or uncontrollable noise. They predicted that the subjects who were in the controllable noise condition would subsequently succumb to the illusion of control in a contingency task, and that the subjects exposed to uncontrollable noise would subsequently be accurate in

their judgments of control. Contrary to these predictions, however, depressed individuals were found to judge accurately that they had little control, regardless of their previous noise experience, in both win and lose situations. Giving depressed subjects exposure to controllable events was clearly ineffective in changing depressive's judgments of contingency.

As the judgment of (non)contingency literature grows, it becomes increasingly clear that some boundary conditions of depressive realism do exist. For instance, Vazquez (1987) found that when outcomes are self-referent and negative in a noncontingent situation, depressives show an illusion of control. Evidence has also been produced which indicates that depressed subjects will show an illusion of control if they are being closely observed in the experimental setting, and that nondepressed subjects will become more realistic in their judgments if similarly observed (Benassi & Mahler, 1985). А general criticism which has been levelled against the research to date is that experimenters have relied almost exclusively upon dysphoric college students as subjects (Dobson & Franche, 1989; Vazquez, 1987). Golin, Terrell, Weitz and Drost (1979), in an extension of an earlier study (Golin, Terrell, & Johnson, 1977), provided one of the few investigations of the illusion of control among depressed clinical patients. Subjects were asked to rate their expectancies of success in a chance-determined dice game. The nondepressed control group was found to exhibit the illusion of control, while the

depressed subjects' results were similar to those of their. dysphoric counterparts in other studies. In spite of these results, the Golin et al. (1979) study cannot be taken as solid evidence for the existence of depressive realism in clinically depressed populations for several reasons. First, the clinical subject group included patients with diagnoses of manic-depressive psychosis, psychotic depressive reaction, and schizo-affective disorder. Considering the impaired ability of psychotic patients to test reality accurately, any effects of mood on realism in this study must be seen as hopelessly confounded with the type and degree of pathology experienced by each subject. Second, the methodology used in this study (i.e., a chance-determined dice game) is not readily amenable to comparison with the existing body of judgment of contingency studies, which involve a completely different task. Finally, subjects were only asked to rate their expectancies for success, not their judgments of control. Since an objectively right or wrong expectancy for success does not exist, it would be false to conclude that one group was more "realistic" than the other.

Only one study to date has looked at the illusion of control among depressed clinical patients using the judgment of contingency paradigm. Lennox, Bedell, Abramson, and Raps (1990) replicated Alloy and Abramson's (1979; Experiment 1) methodology, using four groups of males who had been hospitalized with either major depression, schizophrenia with

depression, schizophrenia without depression, or nonpsychological medical/surgical problems. Consistent with the 1979 findings, all four groups were found to perceive control accurately when a) a contingent relationship existed between response and outcome, and b) the outcome was not affect-laden. Taken at face value, these findings suggest that the major differences in psychological functioning associated with major depression and schizophrenia do not appear to have a significant biasing effect on judgments. Surprisingly, it was also found that the groups were only accurate when an active response was required to produce the outcome (i.e., the button had to be pushed before the light came on). This result had previously been found in dysphoric groups, and not demonstrates that legitimate differences in judgments of control in contingent situations may exist between dysphoric college students and depressed clinical populations. It remains to be seen if this difference will be observed in objectively noncontingent situations (i.e., situations in which the probability of the desired outcome is the same for both active and inactive responses).

#### The Present Study

In the first stage of the present study, a clinically depressed group was contrasted with a remitted depressed group, a never-depressed group, and a control group in order to see if realistic thinking is operative in a clinically depressed population. The task utilized in this study was a

computerized version of the original Alloy and Abramson (1979; Experiment 2) judgment of noncontingency (75-75) task. This task was chosen for several reasons. First, it was considered desirable to choose a task which required active involvement on the part of subjects, as it has been demonstrated that such a task favours the onset of the illusion of control phenomenon (Langer, 1975). Second, the strongest evidence for depressive realism has been found using this specific paradigm. Finally, this methodology was chosen in response to the criticism that a number of the depressive realism studies to date have not allowed for an objective determination of subjects' accuracy (Ackermann & DeRubeis, 1991; Dobson & Franche, 1989). The judgment of noncontingency task is not a target of this criticism because it does provide knowledge of objective outcomes against which to compare subjects' judgments.

It is likely that depressive realism is a state dependent phenomenon (Alloy et al., 1981). State dependency would imply that individuals who are experiencing a depressed mood are likely to produce realistic judgments of control as a function of that mood, and that once the depression lifts the individual will revert to a positively biased pattern of thinking. A competing hypothesis is that some individuals possess a bias toward realism regardless of mood state, and that it is these individuals who are vulnerable to becoming depressed in the future. This position would harmonize with the view of a number of researchers who suggest that optimistic, self-enhancing biases may be an invulnerability factor which prevents individuals from becoming depressed when confronted with stressful life events (Carver & Gaines, 1987; Metalsky, Halberstadt, & Abramson, 1987). As mentioned earlier, the possibility of a reciprocal relationship also exists (Taylor & Brown, 1988). In the second stage of the present study, the remitted and nondepressed groups were both retested, after having undergone a mood induction procedure designed to create a temporarily saddened mood (Velten, 1968). This aspect of the study was designed to indicate whether depressive realism is dependent upon mood state, whether it serves as a vulnerability factor, or whether a combination of these two factors best describes the data.

Since both the remitted and nondepressed groups were tested twice on essentially the same task, it could be argued that any changes in judgments of contingency across testing situations could just as easily be attributed to practice effects as to changes in levels of depressive affect. A control group was employed to counter this possibility. Each subject in the control group was exposed to the noncontingency task twice, but these subjects did not receive the depressive mood induction. In fact, as described below, steps were taken to ensure that the control subjects' mood remained stable across the two tasks.

### Rationale for Subject Selection

The subjects selected for participation in this study

were all females. Two considerations guided the decision to exclude male subjects from the study. First, realism has already been shown to be operative in a group of clinically depressed males (Lennox et al., 1990). Replication of these findings among depressed females is considered desirable, rather than simply generalizing results from one gender to the other. The evidence that twice as many females as males receive treatment for depression (Nolen-Hoeksema, 1988) should alert us to the possibility that different vulnerability processes may be operative between men and women.

A second consideration revolved around the availability of subjects. An overview of hospital charts indicated that an additional six months would probably be needed in order to find an equal number of men and women to serve as subjects. Alternatively, the groups could have included a majority of females and just a few males. However, the small number of males in such a study would likely have been insufficient to allow for an adequately powerful test of gender differences. Rather than leaving room for the potential contaminating influence of a few male subjects, the decision was made to restrict the study to female subjects.

#### Rationale for Mood Induction

The growth of cognitive therapies over the past two decades is testament to a growing awareness on the part of researchers, theorists and clinicians that emotions and thought are strongly interactive in human beings. While

thought and emotion have historically tended to be studied separately (undeniably leading to considerable advances in our understanding of each area), the interactive relationship between these two areas has currently become one of the most active foci of research in psychology (Martin, 1990). Concomitant with the desire to study feelings and cognitions jointly has been the need to develop suitable methodologies for investigating emotional states in the laboratory. It was for this purpose that researchers have developed and employed a number of techniques for the induction of various moods.

The advent of these mood induction techniques has had a considerable impact on depression research. Depression is generally considered to consist of cognitive, behavioural and somatic components, of which all tend to covary. Theoretically, a change in any one of these three components may affect a person's vulnerability to depression, or even precipitate or maintain a depressive episode. The mood induction procedures developed to date offer a method of studying the covariation of the different components of depression. By creating a "mini-depression" under controlled conditions, researchers are able to learn more about people's vulnerability to each component separately, or discover something about the sequencing of the components in upward or downward mood swings. Considering the promising nature of these methodologies, it is not surprising that mood induction procedures have frequently been used in depression research (e.g., Alloy, Abramson, & Viscusi, 1981; Carson & Adams, 1980; Miranda & Persons, 1988; Mitchell & Madigan, 1984; Mukherji, Abramson & Martin, 1982; Natale, 1978; Nelson & Stern, 1988; Teasdale & Fogarty, 1979; Teasdale & Taylor, 1981).

The first depressive mood induction procedure to appear was devised by Velten (1968). His procedure required subjects to read and "try to feel the mood suggested" by 60 selfreferent mood statements. Examples of the depressed statements are "Every now and then I feel so tired and gloomy that I'd rather sit than do anything" and "I have too many bad things happen in my life." Velten compared a depressioninduction group with elation-induction and neutral-induction groups on seven measures of mood-relevant behaviour, and found significant differences in the predicted directions on five of the seven measures. These results led Velten to conclude that his procedure was a valid method for creating temporary mood states in the laboratory (Velten, 1968). Other researchers appear to have agreed with Velten, as evidenced by over 40 studies which have used his procedure, or minor variants of it (Kenealy, 1986).

The Velten procedure was chosen for use in the present study in spite of the reported successes of other induction methods, such as the musical induction procedure (Sutherland, Newman, & Rachman, 1982) or autobiographical recall (Brewer, Doughtie, & Lubin, 1980). The decision to proceed with the Velten method was made only after careful consideration of a number of criticisms and issues which have been raised concerning the Velten induction procedure (VIP) specifically, as well as mood induction procedures in general. These criticisms and issues will be briefly discussed in the remainder of this section.

The primary issue of concern is whether or not the VIP is able to create a mood state sufficiently similar to clinical depression to allow for the generalization of results from mood-induced populations to clinically depressed populations. Clark (1983) addressed this concern by reviewing all the studies which had used the VIP prior to 1983. Clark reported that subjects exposed to the VIP had been shown to have higher levels of self-reported depressed mood, slower count times, slower writing speed, slower word association speed, worse performance on the Digit Symbol Test, slower lever pulling speed, decreases in ratings of pleasure, slower recall of past disturbed appetite, experiences, loss of incentive, indecisiveness, and increased levels of corrugator muscle activity. These factors had all previously been shown to be reliable indices of clinical depression, causing Clark to conclude that "the Velten depression induction produces a state which is a good analogue of mild, naturally occurring retarded depression (Clark, 1983, p. 45).

A second issue concerns the number of subjects affected by the VIP. It has been reported that as many as a third to a half of all subjects show little or no mood change in response to the VIP (Polivy & Doyle, 1980; Sutherland et al., These findings are problematic for researchers 1982). interested in using the VIP because a) the subjects who do respond to the VIP cannot be seen as an entirely random sample of the population, and b) researchers may be forced to work their way through many more subjects than planned before they can meet the demands of various statistical procedures. However, Pusch and Hillson (1991) recently compared the same version of the VIP used in the present study with its main counterpart, the musical induction procedure (MIP; Sutherland et al., 1982) and found that 87% of the VIP subjects showed shifts of greater than 10% in the predicted direction on two separate measures of mood, while only 73% of the MIP subjects showed similar shifts. These authors also reported that the MIP subjects' mood change scores were significantly predicted by the subjects' age and their scores on the Irrational Beliefs Test (Jones, 1968), but that these variables did not predict VIP subjects' change scores. These results, though in need of replication, were compelling enough to warrant the use of the VIP in the present study.

Another question which must be addressed concerns the appropriateness of using a depressive mood induction with subjects who were actually clinically depressed at some point. It could be argued that remitted subjects, by virtue of having already demonstrated a proneness to depression, should not be exposed to a procedure which could precipitate a relapse.

This argument can be countered in three ways. First, a depressive mood induction has already been used with remitted depressed subjects, with no reports of deleterious effects (Miranda & Persons, 1988). Second, the VIP was framed for subjects in such a way that they were reminded that they were in complete control of the situation, as opposed to the time of the onset of their clinical depression, which undoubtedly Subjects were also involved feelings of lack of control. reminded that they were free to terminate their participation at any point. Finally, Frost and Green (1982) reported that the effects of the VIP only last for ten to twenty minutes, and that any residual negative affect can be effectively removed through the use of an elation induction procedure. Subjects in the current study were therefore offered the Velten elation induction procedure upon completion of the experimental tasks.

A final consideration concerning the VIP centres on the issue of demand characteristics. Several researchers have argued that because subjects are told exactly which mood they are to try to achieve, the resultant changes on mood measures may be a function of the subjects' response to the experimental situation (i.e., they may try to "look like" a depressed person because that is what is expected of them, not because they have experienced a genuine shift in mood) (Buchwald, Strack, & Coyne, 1981; Polivy & Doyle, 1980). Velten (1968) anticipated this argument, and included a group of subjects who were told to respond the same way that they thought a depressed person would respond. These subjects' profile of scores on a variety of mood measures did not closely approximate the profile of scores of the mood-induced subjects, suggesting that the VIP subjects were not simply trying to "look" depressed. The work of Clark (1983) cited above also indicates that a number of Velten-induced reactions, such as increased facial muscle activity, are probably too subtle to have been faked (Berkowitz & Troccoli, 1986).

In sum, the VIP has been repeatedly shown to induce mood states which approximate clinical depression in ways which cannot be accounted for by demand characteristics alone. It has also been demonstrated that the VIP can be used in conformity with ethical guidelines with both nondepressed and remitted populations, and that there is a reasonable expectation that the majority of subjects exposed to the procedure will be able to achieve the desired mood. The VIP was therefore identified as the mood induction procedure of choice for this experiment.

#### Experimental Hypotheses

On the basis of the depressive realism literature reviewed above, the following experimental hypotheses were generated:

1) It was predicted that the depressed subjects would be significantly more accurate than the remitted and nondepressed

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subjects in judging the amount of control they had over the onset of a computer graphic (by either pressing or not pressing a button). This prediction was made on the basis of similar results which have been reported in dysphoric populations (Alloy & Abramson, 1979; Vazquez, 1987), as well as in a clinical population in a contingent situation (Lennox et al., 1990). Support for this prediction would indicate that the depressive realism phenomenon is probably generalizable to the clinically depressed.

It was predicted that prior to a depressive mood induction, the remitted, nondepressed, and control subjects would all exhibit a self-enhancing bias in judging how much control they had over the onset of the computer graphic. The fact that the remitted subjects were predicted to respond like the nondepressed subjects as opposed to the depressed subjects was based on the presupposition that depressive realism is at least partially mood-state dependent (Alloy et al, 1981).

2) It was also hypothesized that inducing a temporary and mild level of depression in the remitted and nondepressed subjects would cause them to become more accurate in their judgments of control than they had been at the time of the initial testing. Further, it was believed that the postinduction ratings of control of the remitted and nondepressed subjects would approximate the ratings of the clinically depressed subjects. The remitted and nondepressed subjects' judgments of control after the second exposure to the noncontingency task were also predicted to be more accurate than the second exposure judgments of the control group subjects, who had not received the depressive mood induction. It was also predicted that the control subjects' judgments would remain stable across the two exposures to the noncontingency task. This pattern of results would again underscore relationship the between current mood and depressive realism.

It should be borne in mind that a competing hypothesis to 2) above would predict a different set of results for the remitted and normal subjects. If depressive realism is a vulnerability factor for depression (Alloy & Clements, in press), the remitted subjects would be found to be accurate in their judgments of control both before and after the mood induction procedure, while the nondepressed would exhibit the self-enhancing bias regardless of their mood state. If, on the other hand, there is a reciprocal relationship between depression and the illusion of control (Taylor & Brown, 1988), it would be reasonable to expect the nondepressives to show illusion of control prior to induction than more the remitteds, who in turn would be more biased than the currently depressed subjects (i.e., a linear relationship). If this prediction is valid, then both the remitted and the nondepressed subjects should become more accurate postinduction. However, the remitteds' results should look like those of the depressed subjects, while the nondepressed, in

spite of having become more accurate, should still show more illusion of control than the other two groups.

#### Method

#### Subjects and Assessment

Four groups of adult females recruited were for participation in this study. The first group consisted of 15 patients who were currently receiving either inpatient or outpatient treatment for major unipolar depression at the Holy Cross Hospital, Calgary, Alberta. Potential subjects were initially approached by either their therapist or primary nurse, who explained the general nature of the study and invited interested patients to volunteer their participation. Willing participants were then introduced to the experimenter, who conducted a clinical assessment of each potential subject to establish that the stringent inclusion criteria described below were met.

The second experimental group was comprised of 15 females who had at some point previously experienced an episode of major unipolar depression, but who no longer displayed any significant depressive symptomatology. Fourteen potential subjects were identified through the files of the outpatient unit at the Holy Cross Hospital. In order to maintain confidentiality, these women were initially contacted by the therapist who had handled their case. Volunteers' names were then passed on to the experimenter, who telephoned the

subjects to arrange a suitable time for assessment and experimental testing. Two subjects were excluded from participation because their Beck Depression Inventory scores (discussed further below) were sufficiently elevated (i.e., 11 and 14) to indicate that their depression may not have been fully remitted (Shaw, Vallis, & McCabe, 1985). The remaining three subjects in the group were garnered from respondents to an advertisement in а local newspaper. While the advertisement specifically asked for volunteers who had never been clinically depressed, several people who had previously been treated for depression called to offer their participation. A brief telephone interview indicated that three of these women probably did meet the inclusion criteria. Subsequent assessment with the BDI and the SADS (described below) revealed that these women did indeed fit into the remitted category.

The third group of subjects consisted of 15 females who had never experienced an episode of major unipolar depression. These subjects were recruited via an advertisement in a local newspaper, which offered \$15 to females, ages 18-55, who had no history of clinical depression and who would agree to participate in a research project. Fifteen subjects were nonrandomly chosen for participation from among the advertisement respondents who reported no previous episodes of depression. These subjects were chosen on the basis of their age, since it was considered desirable to match the ages of the nondepressed subjects with the ages of the subjects in the first two groups as closely as possible.

Two measures were obtained from subjects in the above three groups prior to the experimental tasks in order to assess the suitability of each subject for the study. The Beck Depression Inventory (BDI; Beck, Ward, Mendelson, Mock, & Erbaugh, 1961) is a self-report inventory which provides quantitative information for assessing a subject's current depth of depression. The 21 items on the BDI were selected to represent depressive symptoms, with each item consisting of four statements listed in order of symptom severity. Symptoms represented by the items include mood, pessimism, crying spells, guilt, self-hate and accusations, irritability, social withdrawal, work inhibition, sleep and appetite disturbance, and loss of libido (Beck, 1972). The BDI has been shown to be valid and reliable in a number of studies (see Beck, Steer, & Garbin, 1988 for a review).

Subjects in the depressed group were required to achieve a score of at least 16 on the BDI to be included in this study. Actual scores ranged from 16 to 44, with a mean of 28.13, which is considered indicative of a moderate to severe level of depression (Beck et al., 1988; Shaw et al., 1985). Subjects in the remitted and nondepressed groups were only included if their BDI score was 8 or lower. Means for the remitted and normal groups were 4.87 and 2.27 respectively, both of which are considered to be in the normal, nondepressed
range (Shaw et al., 1985).

The Schedule for Affective Disorders and Schizophrenia (SADS; Endicott & Spitzer, 1978) was also administered to subjects in the above three groups. The SADS is a semistructured interview which allows the examiner to evaluate symptom levels and overall functioning in terms of the preceeding week, during the worst period of the most recent episode of a given mental illness, and historically throughout a patient's life. The content of the instrument covérs symptoms related to both mood and thought disorders, and the data gathered during the interview allows the examiner to arrive at a Research Diagnostic Criteria (RDC) categorization (Spitzer, Endicott, & Robins, 1978). In this case, however, the data were compared to the criteria for depression outlined in the Diagnostic and Statistical Manual of Mental Disorders, 3rd ed., revised (DSM-III-R; American Psychiatric Association, It should be noted, though, that the criteria for a 1987). diagnosis of major depression according to RDC guidelines were incorporated into DSM-III (American Psychiatric Association, 1980), and were later carried on in DSM-III-R.

Subjects were only included in this study if they a) met the DSM-III-R criteria for a current episode of major depression, b) had previously warranted a DSM-III-R diagnosis of major depression but no longer did, or c) had never met the DSM-III-R criteria for major depression. It was also determined that none of the subjects currently or previously met the DSM-III-R criteria for dysthymia, bipolar disorder, schizophrenia, thought disorder, or alcohol/substance abuse.

The fourth group of subjects consisted of 15 female undergraduates who volunteered their participation in this study as part of an assignment for an introductory psychology course at the University of Calgary. As the purpose of this group was simply to serve as a methodological control, it was not deemed necessary to conduct a diagnostic interview with these subjects. No BDI cut-off score was set as an inclusion criterion for the control group subjects because it was predicted that the subjects' judgments of control would remain stable across time if their mood remained stable, regardless of their specific affective state at the outset. The mean BDI score of the control group was 5.4 (s.d. = 4.4), with individual scores ranging from 1 to 14.

#### <u>Measures</u>

The major dependent measure used in this study involved subjects' ratings of judgments of control. After exposure to the judgment of non-contingency task, subjects were presented with a judgment of control (JOC) scale on which they were asked to rate the degree of control their responses (either pressing or not pressing the key) had over the onset of the light bulb graphic. The scale was marked off in units of 10, with extreme values of 0 and 100 (see Appendix A). The extreme values were labelled "no control" and "complete control" respectively, with the 50% point being labelled "intermediate control." Subjects were also asked to rate their confidence in their judgments of control by writing an "X" on a 10-point scale ranging from "not sure at all" to "totally convinced" (CON; see Appendix B).

Two level of affect measures were administered to all subjects. The first measure consisted of a visual analog scale (VAS), which asked subjects to respond to the question "At this particular moment, how happy/sad do you feel?" by placing an "X" along a 10-point scale. The lower end of the VAS was labelled "completely sad," and the upper end was labelled "completely happy" (see Appendix C). The second affect measure involved a speed-writing task (SP); in which subjects were asked to write numbers backwards from 100 as quickly as possible for one minute (see Appendix D). A number of researchers have reported that subjects who undergo a depressive mood induction procedure write fewer numbers in one minute than controls (Alloy, Abramson, & Viscusi, 1981; Hale & Strickland, 1976; Natale, 1978; Velten, 1968), and that this measure therefore serves as an index of psychomotor retardation.

### Apparatus

The judgment of noncontingency task and the Velten mood induction procedure were both designed to be administered on an IBM PS/2 microcomputer with an IBM PS/2 colour monitor (27.5 cm x 20.5 cm).

Two separate versions of the judgment of noncontingency

task were created to counter the possibility that subjects' responses to the second administration of the task would be influenced by the perception that the second task was exactly the same as the first. In the first version, the subject was warned that a trial was about to start when the words "HERE IT COMES" were displayed on the lower left-hand section of the monitor for one second. A graphic of a round yellow light bulb (6 cm x 6 cm) would then appear in the middle of the left hand side of the screen for three seconds. During this three second interval the subject had the option of either pressing or not pressing the space bar on the keyboard. The program was designed to record how often each subject pressed the bar. After three seconds the yellow light bulb would disappear. A triangular green light bulb graphic (6 cm x 6 cm) would then appear in the middle of the right hand side of the screen randomly on 75% of trials. The fact that the graphic appeared randomly on 75% of trials, regardless of the subject's choice either to press or not press the space bar, indicates that subjects had no objective control over the task (i.e., a noncontingent situation). The green light bulb was displayed for two seconds if it did appear, and the program recorded how often this graphic appeared during each subject's 40 trials. If the green light bulb did not appear during a given trial, the screen would remain blank for two seconds. Following the two second interval, the warning message would again appear for one second, signalling the start of the next trial.

The second version of the judgment of noncontingency task was designed to operate on the same noncontingency schedule as the first version. However, the colours and shapes of the graphics and the response required of the subjects were altered slightly to change the appearance of the task. The graphic which appeared on the left side of the screen for three seconds was a green star (6 cm x 6 cm). Subjects were told to press the Enter key on the keyboard during the three second interval, and the graphic which appeared on the right side of the screen for two seconds on 75% of trials took the shape of a red diamond (6 cm x 6 cm). The same warning message was used in both versions of the task to indicate the start of the next trial.

The administration of the two versions of the judgment of noncontingency task were counterbalanced across the remitted, nondepressed and control subjects to eliminate any potential order effects. Thus, one subject would be presented first with the first version of the task and later the second version, and the next subject would be presented the two tasks in reverse order.

The computerized Velten mood induction procedure was designed to be presented in two parts. The first part consisted of a series of nine instruction messages which appeared on the monitor one at a time (see Appendix E). Subjects were free to read these instructions at their own pace, moving from one instruction message to the next simply

by pressing the space bar once. After subjects read the ninth instruction, a final press of the space bar initiated the display of the 59 Velten statements (one of the original 60 Velten statements was omitted because it was specifically directed at students; see Appendix F). Each statement was displayed in the middle of the monitor for twelve seconds. Subjects were not required to press any keys to move to the next Velten statement. After all the statements had been displayed, a brief message (i.e., "Session Over") appeared on the monitor to indicate that the session was over.

A second version of the Velten procedure, designed to induce happiness, was also created for the IBM PS/2. The 60 statements contained in this program were originally used by Velten (1968). This program did not include a set of instruction messages because any subjects exposed to this procedure would already have undergone the VIP designed to induce depressive affect. The 60 statements in the "happiness" program were designed to be displayed one at a time for twelve seconds each.

# Procedure

Depressed, remitted, and nondepressed subjects arrived at the experimental room, were briefly acquainted with the nature of the experiment, and were asked to sign a consent form (subjects received one of three versions of the consent form, depending on their group membership; see Appendix G). Subjects then filled out the BDI and were subsequently administered the SADS, which allowed for a determination of each subject's suitability for inclusion in the study (in accordance with the criteria outlined above).

The subjects deemed suitable for the study were then seated in front of a microcomputer and were told that they were about to participate in a task which would measure their ability to solve a certain type of problem (see Appendix H for a copy of the complete set of the verbal instructions). The procedure for completing the judgment of noncontingency task was explained, and an opportunity was given for questions to be asked. Once the subject indicated that she understood the instructions, she was given a set of 10 practice trials to familiarize her with the sequencing of the computer graphics and the appropriate time to push the button. The practice set was included in the study after pilot testing revealed that a number of subjects (particularly the depressed ones) seemed to have difficulty understanding the instructions vis a vis the concept of control in the task. It was not considered desirable to display a noncontingent situation in the practice set because such a situation would be unlikely to enhance a subject's understanding of the idea of control, and also because the trials would then be exactly the same as the experimental task, effectively giving subjects 50 trials rather than 40 in which to determine the relationship between pushing the button and the onset of the light. Instead, the 10 trials involved a 100% contingent situation. The type of

response required for the onset of the computer graphic (i.e., either active or passive) was randomized between subjects. Thus, one subject would receive 10 practice trials wherein pressing the computer key would always result in the appearance of the graphic, while not pressing would never result in the appearance of the graphic (i.e., 100-0 contingency), and the next subject would receive 10 trials wherein pressing the key would never produce the graphic, but not pressing the key would always result in graphic onset (i.e., 0-100 contingency). Subjects were allowed to proceed with the actual experimental task only after they indicated an understanding of the 100% relationship between key-pressing (or non-pressing) and the onset of the graphic in the practice trials. It is interesting that four of the depressed subjects had to be exposed to the practice set a second time because they initially failed to discern the direct connection between their actions and what was occurring on the monitor.

After completing the experimental task, subjects were read a set of instructions (see Appendix I) which asked them to use the 10-point JOC scale to indicate the degree of control they believed they had over whether or not the computer graphic appeared, as well as their level of confidence in their judgment of control. At this point the depressed subjects were asked to indicate their current mood state on a VAS and they also completed a one minute speed writing task, followed by three experimental tasks unrelated

to the present study. These subjects were then debriefed, paid, thanked, and dismissed.

The remitted and nondepressed subjects also filled out the judgment of control and confidence ratings after the experimental task, as well as the subjective mood measure, the speed writing task, and the three tasks which had no bearing on this study. The subjects were then told that they were about to undergo a mood induction procedure designed to create temporarily saddened affect, purportedly in an effort to discover how much voluntary control people have over their own moods. Verbal instructions for the VIP were given (see Appendix J), followed by a set of nine computerized instruction messages which were read at the subject's own pace.

Upon the subject's completion of the computerized instructions, the experimenter moved into the hallway, leaving the door slightly ajar, to afford the subject some privacy during the VIP. Once the twelve minutes required for the VIP had passed, the experimenter re-entered the room and administered the two level of affect measures again.

The remitted and nondepressed subjects were subsequently asked to complete the judgment of noncontingency task a second time. The instructions at this point indicated that there was a similarity between the upcoming task and the task completed earlier, but that the two tasks were not necessarily the same (see Appendix K for complete instructions). Each subject was presented the version of the task to which they had not been exposed at the time of initial testing. Subjects were not given another set of practice trials because they had previously demonstrated an adequate understanding of the task. Following the judgment of noncontingency task, subjects again provided their judgments of control and confidence level ratings.

After completion of the experimental tasks, all subjects were debriefed, thanked, and paid fifteen dollars. Subjects were also assessed on the VAS again just before leaving to ascertain that their mood had returned to its pre-induction Two subjects' VAS ratings indicated that they were level. still experiencing some sadness, likely due to the influence These subjects were encouraged to undergo the of the VIP. version of the VIP designed to induce happiness. Both subjects agreed, and after the procedure was completed, both subjects' VAS ratings indicated that they were in a happier mood than they had been in prior to the depressive mood induction.

The procedure for the control subjects followed that of the remitted and nondepressed subjects quite closely. The only differences were that the control subjects were not administered the SADS, the VIP, or the other three tasks unrelated to this study. The control subjects were asked to read a brief, neutral magazine article (relating to current research findings in developmental psychology) following the

first administration of the judgment of noncontingency task to ensure that the amount of time between the two administrations of the JOC task for these subjects was approximately the same as that of the remitted and nondepressed subjects. The article used was chosen after pilot testing revealed that reading this article did not seem to have any detectable effect on most subjects' mood. The control subjects were not paid for their participation, primarily because their time of involvement was minimal compared to the other three groups.

#### Results

## Subject Characteristics

The currently depressed group was comprised of 15 females. Their mean age was 37.3 years (s.d. = 7.7), with a range of 27 to 58 years. The remitted group consisted of 15 females whose mean age was 36.2 years (s.d. = 9.0), with a range of 24 to 53 years. The nondepressed group included 15 females who ranged in age from 20 to 55 years, with a mean age of 33.3 years (s.d. = 9.1). A one-way ANOVA revealed no significant differences in age between the three groups.

The subjects' level of education was also analyzed between groups. The depressed subjects had a mean of 12.7 (s.d. = 2.7) years of education. The remitted subjects had a mean of 14.1 (s.d. = 2.1) years of education. The nondepressed subjects had also received 14.1 (s.d. = 1.8) years of education on average. A one-way ANOVA revealed no significant differences in level of education among the three groups.

A Chi-squared analysis also indicated that there was no significant difference in marital status among the three groups. Eleven of the depressed subjects were currently married (73%), compared to ten of the nondepressed subjects (67%), and nine of the remitted subjects (60%).

### Correlations Among the Dependent Variables

The correlations among the experimental dependent measures (i.e., the subjects' judgments of contingency and their confidence ratings) and the two mood measures (the VAS and writing speed) are presented in two separate tables. Table 1 shows the correlations among the four measures at Time 1 for all of the depressed, remitted, and nondepressed subjects combined. None of these correlations reached a .05 level of significance.

Table 2 presents the correlations among the four measures at Time 1 and Time 2 for the remitted and nondepressed subjects only (because the depressed subjects were only tested once on each measure). The remitted and nondepressed subjects' judgments of contingency at Time 1 correlated .53 with their judgments of contingency at Time 2 (p < .01). Their confidence ratings at Time 1 correlated .57 with their confidence ratings following the mood induction procedure (p < .01). The second set of confidence ratings also correlated -.53 (p < .01) with the second set of judgment of contingency

# Table 1.

Correlations Between Pretest Judgments of Contingency (JOC1), Confidence Ratings (CON1), Visual Analog Scale Scores (VAS1) and Writing Speed (SP1) for Depressed, Remitted and Nondepressed Subjects Combined

	JOC1	CON1	VAS1	SP1	
JOC1	-	•			· · ·
CON1	-0.24	-			
VAS1	0.03	0.02	-		
SP1	-0.06	0.05	0.13		

Table 2.

Correlations Between Time 1 and Time 2 Judgments of Contingency, Conficence Ratings, Visual Analog Scale Scores and Writing Speed for Remitted and Nondepressed Subjects Combined

	JOC1	JOC2	CON1	CON2	VAS1
 JOC1		<u> </u>	<u></u>		
JOC2	0.52**	-			
CON1	-0.18	-0.15	-		
CON2	-0.29	-0.53**	0.57**	-	
VAS1	-0.11	-0.06	-0.11	0.23	-
VAS2	-0.15	0.10	-0.04	0.11	0.17
SP1	0.08	0.11	-0.12	-0.27	-0.34*
SP2	0.06	0.09	0.08	-0.09	-0.31*

\* <u>p</u> < .05

\*\* <u>p</u> < .01

	VAS2	SP1	SP2	
VAS2			<u></u>	
SP1	-0.01	-		
SP2	0.11	0.73***	-	

\*\*\* <u>p</u> < .001

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scores, suggesting that subjects who became more realistic post-induction were also more confident that their answer was correct than those subjects who became less accurate in judging their degree of control after the VIP.

Scores on the Time 1 VAS were correlated -.34 (<u>p</u> < .05) with Time 1 speed writing scores, and also correlated -.31with the subjects' performance on the speed writing task following the mood induction (<u>p</u> < .05). Speed writing scores at Time 1 were also significantly correlated with the postinduction speed writing scores (<u>r</u> = .73, <u>p</u> < .001).

The scores of the control group subjects were not included in the construction of either of the correlation matrices. This group was only included in the study to provide support for the hypothesis that judgments of contingency would remain stable across time in the absence of a mood induction procedure. These subjects were not selected on the basis of mood state or age, but rather, purely on the basis of gender and availability. Because a thorough assessment of these subjects' psychological state was not undertaken, it was desirable to separate their scores on the four dependent measures from the scores of the three experimental groups.

## Pretest Level of Affect

The selection of subjects for this study was guided by the assumption that the depressed subjects would report significantly more depressive affect and symptomatology than

the remitted and nondepressed subjects, who would not differ significantly in their pretest level of affect. The SADS interview was primary vehicle for ensuring the this difference. The data collected during the SADS interview were used to determine that all of the depressed subjects warranted a diagnosis of major depression according to DSM-III-R criteria, but that none of the remitted or nondepressed subjects met the criteria for the same diagnosis. In addition, it was expected that the depressed group would demonstrate significant pretest differences from the other two groups on the BDI, the level of affect VAS, and the speed writing task. These predictions were tested in a series of ANOVA and subsequent t-test analyses. The means and standard deviations of the three groups for the BDI, the VAS, and the speed writing task are presented in Table 3.

A one-way ANOVA revealed a significant difference between the three groups on the BDI ( $\underline{F}(2,42) = 99.90$ ,  $\underline{p} < .001$ ). Onetailed t-tests indicated that the depressed group scored significantly higher on the BDI than both the remitted subjects ( $\underline{t}(28) = 9.62$ ,  $\underline{p} < .001$ ) and the nondepressed subjects ( $\underline{t}(28) = 10.89$ ,  $\underline{p} < .001$ ). A two-tailed t-test revealed that the nondepressed group also scored significantly lower than the remitted group on the BDI ( $\underline{t}(28) = -3.13$ ,  $\underline{p} <$ .01). This significant difference in BDI scores is largely a function of the small standard deviations in the remitted and nondepressed groups' BDI scores rather than the magnitude of

# Table 3.

Mean Scores and Standard Deviations for the Beck Depression Inventory (BDI), Visual Analog Scale (VAS1, VAS2), and Writing Speed (SP1, SP2)

<u></u>				
			Mean	SD
BDI				
•	Depressed	(n = 15)	28.1	9.0
	Remitted	(n = 15)	4.9	2.6
	Nondepressed	(n = 15)	2.3	1.9
VAS1				
	Depressed		4.5	2.2
	Remitted		7.4	1.4
	Nondepressed		8.2	1.2
VAS2				
	Remitted		3.3	1.4
	Nondepressed		4.2	1.4
SP1				
	Depressed		52.2	9.9
	Remitted		59.9	9.6
	Nondepressed		59.2	8.3
SP2				
	Remitted		56.3	9.2
	Nondepressed		55.3	11.8

the difference between the group means (4.9 and 2.3, respectively). While a statistically significant difference between the remitted and nondepressed subjects' BDI scores does exist, the difference in their mean scores is not considered clinically significant, as both scores fall well within the "currently nondepressed" range (Shaw et al., 1985).

The VAS scores were significantly different between the three groups ( $\underline{F}(2,42) = 20.83$ ,  $\underline{p} < .001$ ), with the depressed group reporting more subjective feelings of sadness than the remitted ( $\underline{t}(28) = -4.36$ ,  $\underline{p} < .001$ ) and nondepressed subjects ( $\underline{t}(28) = -5.69$ ,  $\underline{p} < .001$ ). The difference in pretest VAS scores between the remitted and nondepressed subjects was not significant.

The three groups' speed writing scores were initially analyzed with a one-way ANOVA, which revealed that the difference in writing speed between the groups bordered on statistical significance ( $\underline{F}(2,42) = 3.15$ ,  $\underline{p} = .054$ ). Subsequent t-tests were conducted because specific group contrasts had been planned <u>a priori</u>. One-tailed t-tests showed that the depressed group wrote significantly fewer numbers in one minute than the remitted ( $\underline{t}(28) = -2.20$ ,  $\underline{p} <$ .05) and the nondepressed subjects ( $\underline{t}(28=2.10, \underline{p} < .05)$ ). A two-tailed t-test failed to find a significant difference between the remitted and nondepressed subjects' writing speeds.

# Mood Manipulation

1) Visual Analog Scale

The effectiveness of the VIP in inducing saddened affect in the remitted and nondepressed subjects was measured, in part, by subjects' change in subjective level of affect, as indicated on the VAS. It had been expected that subjects exposed to the VIP would have significantly lower scores on a 10-point happiness/sadness scale than they had prior to the mood induction. It was predicted that the Time 2 VAS (VAS2) scores of the remitted and nondepressed subjects would approximate the original VAS scores of the depressed subjects. It was also expected that the control subjects, who did not receive the VIP, would not exhibit any significant change in mood on this measure.

These predictions were supported. A one-way ANOVA failed to show any significant differences between the remitted and nondepressed subjects' VAS2 scores and the depressed subjects' VAS1 scores. There was, however, a significant difference between the control, remitted, and nondepressed subjects' VAS2 scores  $(\underline{\mathbf{F}})$ = 27.79, p < .001), with the remitted and nondepressed groups both expressing more saddened affect than All of the remitted and nondepressed the control group. subjects scores shifted at least 10% in the predicted direction following the VIP. Follow-up pretest-posttest analyses revealed that the control group's VAS scores did not change significantly from Time 1 to Time 2, but that the

remitted and nondepressed groups' VAS scores combined did become significantly lower from Time 1 to Time 2 ( $\underline{t}(29) =$ 12.37,  $\underline{p} < .001$ ). There was no significant difference between the remitted and nondepressed subjects' VAS2 scores.

2) Speed Writing Task

Although writing speed is a less robust measure of mood change than the VAS (Clark, 1983), a similar pattern of results to that described above was expected. It was predicted that the control group's writing speed at Time 2 (SP2) would be the same as their speed at Time 1 (SP1), but that the remitted and nondepressed subjects would become significantly slower in their writing speed following the mood induction, leaving no difference between their SP2 scores and the depressed subjects' writing speed (SP1) scores.

The predictions for changes in writing speed were supported. While there was no difference between the control, remitted and nondepressed groups in SP1 scores, a significant difference between the three groups appeared when the SP2 scores were analyzed ( $\underline{F} = 3.24$ ,  $\underline{p} < .05$ ). Subsequent analyses showed that the control subjects' SP scores had not changed over time, but that the remitted and nondepressed subjects' combined writing speed had become significantly slower between SP1 and SP2 ( $\underline{t}(29) = 3.05$ ,  $\underline{p} < .01$ ). There was no significant difference between the remitted and nondepressed subjects' SP2 scores. An ANOVA also showed that the writing speed of the remitted and nondepressed subjects after the mood induction

procedure was no longer significantly different than the writing speed of the depressed subjects.

### Frequency of Active Response

Each subject participating in the judgment of noncontingency task had the option of pressing a button on the keyboard during any given trial in order to try to cause a computer graphic to appear on the monitor. The frequency with which the subjects chose active responses was analyzed to determine if group differences exist. The means and standard deviations of the depressed, remitted, and nondepressed groups' frequency of responding are presented in Table 4.

There was no significant difference between groups in the number of times the button was pushed at Time 1. However, a one-way ANOVA revealed a significant difference when the depressed group's frequency of active responding at Time 1 was compared with the active responding of the remitted and nondepressed groups at Time 2 ( $\underline{F}(2,42) = 6.35$ ,  $\underline{p} < .01$ ). Follow-up t-tests showed no difference between the depressed and nondepressed subjects, but a significant difference between the depressed and remitted subjects ( $\underline{t}(28) = 2.96$ ,  $\underline{p} < .01$ ) and between the nondepressed and remitted subjects ( $\underline{t}(28) = 3.04$ ,  $\underline{p} < .01$ ). In other words, the remitted subjects chose an active response at Time 2 significantly less often than the nondepressed subjects at Time 2 and the depressed subjects at Time 1.

Table 4.

Means and Standard Deviations of Depressed, Remitted and Nondepressed Subjects' Frequency of Active Responding at Time 1 (FAR1) and Time 2 (FAR2)

		Mean	SD
FAR1	· · · · · · · · · · · · · · · · · · ·		
	Depressed .	25.60	3.83
	Remitted	21.13	6.97
	Nondepressed	22.13	4.37
FAR2			
	Remitted	20.80	4.97
	Nondepressed	26.27	4.86

### Tests of Main Experimental Hypotheses

The first experimental hypothesis predicted that clinically depressed subjects would provide more accurate assessments of their degree of control over the noncontingent task than either the remitted or nondepressed subjects. Contrary to this prediction, a one-way ANOVA revealed no significant differences between the three groups  $(\underline{F}(2, 42) =$ .82,  $\underline{p} = .449$ ) on the judgment of noncontingency task at Time All three groups were substantially over-optimistic in 1. their judgments of control, with the remitted group being the most realistic of the three. Table 5 presents the means, standard deviations and ranges of all four groups on the judgment of noncontingency task.

It had also been predicted that the remitted and nondepressed subjects would not differ from the control group subjects in their judgments of noncontingency at Time 1, but that the remitted and nondepressed subjects would become more realistic following the VIP, while the control group's accuracy would not change from Time 1 to Time 2. These predictions were supported. The three groups' control ratings were not significantly different after initial testing, but a  $2 \times 2$ (Group by Time) repeated measures ANOVA for the remitted and nondepressed subjects revealed that these subjects did become more realistic following the depressive mood induction. A main effect for Time was found  $(\underline{F}(1,28) =$ 7.02, p < .05), but there was no main effect for Group, nor a

Table 5.

Means, Standard Deviations and Ranges of All

Groups' Judgment of Control Ratings at

Time 1 (JOC1) and Time 2 (JOC2)

		Mean	SD	Range	
JOC1		····			
	Depressed	42.93	23.28	0 - 80	
	Remitted	32.33	30.05	0 - 100	
	Nondepressed	43.33	26.34	0 - 98	
	Control	36.47	37.13	0 - 95	
JOC2					
	Remitted	22.00	23.36	0 - 65	
	Nondepressed	28.80	22.38	0 - 60	
	Control	32.00	33.05	0 - 90	

significant Group by Time interaction (see Appendix L). A pretest-posttest analysis of the control subjects' judgments of control revealed that they became neither more nor less accurate from Time 1 to Time 2.

A one-way ANOVA was used to test the prediction that the remitted and nondepressed subjects' judgment of control (JOC) ratings following the VIP would be the same as the depressed group's initial JOC ratings. Surprisingly, a significant difference was found between the three groups ( $\underline{F}(2,42) = 3.23$ ,  $\underline{p} = .05$ ). Subsequent two-tailed t-tests revealed no significant JOC differences between the depressed and nondepressed groups, or between the nondepressed and remitted groups. However, a significant difference between the depressed and remitted subjects' JOC ratings was found ( $\underline{t}(28) = 2.46$ ,  $\underline{p} = .02$ ). The remitted subjects proved to be more realistic than their depressed counterparts.

One theory of depressive realism (discussed earlier) suggests that realism may serve as a vulnerability factor for depression. The vulnerability theory would be supported by the finding that remitted subjects are more realistic than nondepressed subjects regardless of mood state. In fact, ttests showed that neither of the two groups was more accurate than the other at any time. Across time, subjects in both groups averaged a 16% increase in the accuracy of their judgments of control. The scores of subjects who were already accurate at Time 1 were excluded from the computation of average increases in accuracy to control for ceiling effects, since these subjects had no possibility of becoming more accurate following the VIP.

# Confidence Ratings

Confidence ratings reflect the degree to which subjects perceived that their JOC ratings were accurate. The means and standard deviations of the depressed, nondepressed and remitted subjects' confidence ratings are presented in Table 6. A one-way ANOVA showed no difference in the levels of confidence between the groups at Time 1. A second one-way ANOVA revealed no differences between the depressed subjects' Time 1 confidence ratings and the ratings of the remitted and nondepressed subjects at Time 2. Pretest-posttest t-tests for the remitted and nondepressed subjects showed that neither group changed significantly in their level of confidence in their judgments of control across time.

## Discussion

The present study tested two experimental hypotheses. The first hypothesis was that realistic judgments of control would be provided by depressed subjects in a noncontingent situation, while nondepressed subjects in the same situation would demonstrate a self-enhancing bias by overestimating their control over a noncontingent task. The second hypothesis predicted that depressive realism would be found to be mood state dependent, with both remitted and nondepressed Table 6.

Means and Standard Deviations of Confidence Ratings of the Depressed, Remitted and Nondepressed Subjects at Time 1 (CON1) and Time 2 (CON2)

<u> </u>			
		Mean	SD
CON1			
-	Depressed	55.13	26.44
	Remitted	64.33	26.78
	Nondepressed	63.00	22.02
CON2	· .		
	Remitted	62.67	30.58
	Nondepressed	65.00	25.00

groups providing more realistic judgments of control after a depressive mood induction. Following a brief consideration of group affect differences and the efficacy of the VIP in inducing depressive sýmptomatology, the accuracy of each experimental hypothesis is discussed in some detail. Possible interpretations of the results are presented, as are limitations of the study and suggestions for future research. Validation of Subject Selection

The placement of subjects into experimental groups on the basis of the information gathered during the SADS interview and the administration of the BDI was supported by the subjects' scores on two separate level of affect measures. The depressed group scored significantly lower than the remitted and nondepressed groups on a Time 1 visual analog scale designed measure subjective levels of to The depressed group was also slower than happiness/sadness. the other two groups on a speed writing task at Time 1. These results clearly highlight the substantial converging difference in the current level of depression experienced by the depressed subjects and the remitted/nondepressed subjects. As predicted, there was no difference between the remitted and nondepressed subjects on either the VAS or the speed writing task at Time 1.

### Validation of Mood Manipulation

The results also suggest that the mood induction procedure was successful in creating a temporary state of

lowered affect in the remitted and nondepressed subjects. The exposure of these subjects to the VIP resulted in a change in their responses to the level of affect measures. Both groups indicated increased feelings of subjective sadness on the VAS, and both groups also had slower writing speeds following the VIP. While the scores of the remitted and nondepressed subjects were significantly different than those of the depressed subjects prior to the depressive mood induction, those differences disappeared once the VIP had been A control group, which did not receive the administered. Velten procedure, did not demonstrate a shift on either level of affect measure across time.

### Support for Experimental Hypotheses

The main experimental hypothesis stated that depressed subjects' judgments of control over an objectively. noncontingent situation would be realistic, compared to nondepressed subjects, who were expected to show a selfenhancing bias in their judgments of control. This hypothesis was not supported in this study. Rather than demonstrating realistic judgments of control, the depressed subjects overestimated their degree of control by 43%, and were no more realistic than either the remitted or nondepressed subjects at Time 1. Taken at face value, the results suggest that the depressive realism phenomenon previously found in dysphoric student samples is not generalizable to a population of clinically depressed females.

While the results of the depressed subjects may indicate that depressive realism does not exist in clinically depressed subjects, the results of the remitted and nondepressed subjects following mood induction must also be taken into consideration. The remitted and nondepressed subjects both became more realistic after they were exposed to the procedure designed to make them temporarily "depressed." The control group did not experience a similar shift toward realism after completing the judgment of noncontingency task a second time. The most obvious interpretation of this pattern of results is that depressive realism does exist at some level, and that it is a mood state dependent phenomenon. This interpretation is consistent with the conclusion reached in previous studies using dysphoric subjects (e.g., Alloy et al., 1981).

Overall, the results of this study seem to be somewhat contradictory. On the one hand, currently depressed subjects showed no more realism than remitted and nondepressed subjects. On the other hand, both remitted and nondepressed subjects became more realistic after they underwent a depressive mood induction. While no theory to date can completely account for this pattern of results, several explanations are worthy of consideration. These explanations are presented below, along with an evaluation of their plausibility.

The first explanation for the obtained observations is that the results are accurate. Depressive realism may operate

according to a U-shaped function, with severity of depressive symptomatology being the pivotal variable. If such a function were truly the best descriptor of the phenomenon, subjects in a nondepressed state would be expected to show an optimistic bias, subjects experiencing mild depression (either in the form of dysphoria or due to a depressive mood induction) would be expected to provide realistic judgments of control, and severely depressed subjects would again be expected to show an optimistic bias. Such an explanation definitely coincides with the results of this study, but is unattractive for two principle reasons. First, the explanation would have to be gender specific, since Alloy and Clements (in press) found that clinically depressed males were realistic. Second, this explanation lacks theoretical appeal. None of the existing theories of depression provides a basis for arguing that severely depressed subjects should exhibit a self-enhancing bias.

Another explanation for the discrepant results in this study could involve the test-taking attitudes/behaviours of the subjects. Intuition would suggest that severely depressed subjects in an evaluative situation might be unwilling to make a judgment of control which required placing an "X" at the extreme end of a 10-point scale, even if they believed the "X" should be there, and choose instead to remain in the relatively "safe" middle area of the scale. Subjects responding in this manner would likely indicate a lack of confidence in their judgments of control relative to subjects who legitimately believed that they had objective control over the task. However, the subjects' confidence ratings were not different between groups, suggesting that the depressed, remitted and nondepressed subjects all thought that they were accurate in their judgments to the same degree.

The only detected difference in the test-taking behaviour of the three groups was in the frequency of choosing active responses over passive responses across the 40 trials. There was no difference between the groups at Time 1, but a significant difference emerged at Time 2. The remitted group was found to press the button more often than either of the other two groups. If this variable were related to subjects' judgments of noncontingency, a difference between the remitted and nondepressed group's control ratings at Time 2 could be expected to emerge. In fact, the results fail to show any difference in the ratings of the two groups. Therefore, frequency of active responding is probably unrelated to depressive realism.

A third explanation for the observed pattern of results is that the presence of the experimenter in the experimental room during the noncontingency task may have had an influence on the depressed subjects' judgments of control. Table 7 presents the depressed and nondepressed subjects' judgments of control, as well as the position of the experimenter, across the high frequency outcome (i.e., 75/75 or 50/50) judgment of Table 7.

Depressed and Nondepressed Subjects' Ratings of Control and Position of Experimenter Across Judgment of Noncontingency Studies

Study Number*	Subjects' Gender	Nondepressed JOC <sup>**</sup>	Depressed JOC	Experimenter Location
1	Female	51.4%	13.1%	observation room
2	Female '	43.3%	30.4%	observation room
3	Female	44%	16.7%	observation room
4	Female	• 61%	30%	observation room
5	Female	58.8%	12.5%	behind screen
6	Female	48%	22%	observation room
7	Female	20%	45.3%	present with subject
8	Male & Female	29.4%	43.5%	present with subject
current study	Female	43.4%	42.9%	present with subject

*	study	1	Ξ	Alloy & Abramson (1979; Experiment 2)	
	study	2	=	Bryson, Doan, & Pasquali (1984)	
	study	3	=	Benassi & Mahler (1985; Experiment 1)	)
	study	4	=	Alloy, Abramson, & Viscusi (1981)	
	study	5	=	Vazquez (1987; Experiment 2)	
	study	6	=	Alloy & Abramson (1982)	
	study	7	=	Benassi & Mahler (1985; Experiment 1)	)
	study	8	=	Benassi & Mahler (1985; Experiment 2)	)
**	JOC =	J	ud	gments of Control	

noncontingency studies to date. Benassi & Mahler (1985) have demonstrated that when dysphoric subjects undergo the noncontingency task with no one else present they replicate the results of Alloy & Abramson (1979; Experiment 2), but when they undergo the task in the presence of an observer, depressed students perceive themselves to have more control than nondepressed students. Benassi & Mahler's methodology required the experimenter to be in the room, as well as a second subject whose task was to monitor closely the performance of the first subject on the noncontingency task. In the current study the experimenter was the only other person in the room during the experimental task, and he was seated in an unobtrusive position on the far side of the room while the subject was seated in front of the microcomputer. However, given the striking similarity between the depressed subjects' responses in the present study and the responses of the dysphoric subjects in the only other two studies which employed an observer in the experimental room, it is possible that the presence of the experimenter alone was enough to alter the perceptions of control of the depressed subjects.

A fourth explanation for the unexpected performance of the depressed subjects is that they may have been insulated against realistic thinking by exposure to a success experience prior to the noncontingency task. All subjects received a set of ten practice trials, during which they learned that they had 100% control over the experimental outcome. These trials, which were not provided in previous studies, were included after it was made clear during pilot testing that some subjects did not understand the concept of control in the task. Previous researchers have found that giving depressed subjects "therapy" for helplessness, consisting of exposure to controllable events or solvable problems, reverses performance and perceptual deficits associated with helplessness and depression (Klein & Seligman, 1976; Teasdale, 1978). Alloy and Abramson (1982) exposed dysphoric subjects to controllable noise in an effort to induce an illusion of control, but found the controllable noise to be largely ineffective in altering the subjects' judgments of control. The present study, however, involved exposure to a solvable problem which was very similar to the experimental task. The similarity between the tasks may have been strong enough to cause a reversal of the depressive realism phenomenon.

A final possible explanation for the study's results is that realistic thinking does operate in clinically depressed subjects, and that the results of this study simply represent an anomalous finding. This explanation is appealing because it accounts for the difference between this study and the other studies to date. It is unlikely that the anomaly would have occurred as a result of contaminated subject selection, because more stringent selection criteria were employed in this study than in most comparable studies. However, evaluating results according to statistical probabilities
guarantees that Type II errors will occasionally occur. Perhaps the "optimistic" judgments of the depressed group represent such an error. On the other hand, appropriate alpha levels were consistently used in this study as a safeguard against the possible occurrence of Type II errors, thus rendering this explanation for the observed results unlikely. Also, the differences in the judgments of control of the three groups at Time 1 did not even approach significance ( $\underline{F}(2,42)$ = .82,  $\underline{p}$  = .45). The failure of the depressed subjects to show even a modest trend in the direction of the main experimental hypothesis would therefore argue against a purely statistical explanation of the results. However, as is the case in all studies which produce unexpected results, the only way to determine adequately whether the findings are truly anomalous would be to replicate the study.

#### Limitations and Directions for Future Research

As with most clinical investigations, this study is confronted with problems of generalizability. These problems take two forms. First, because only female subjects were used in this study, the legitimacy of generalizing the results to male subject populations is questionable. Alloy and Clements' finding that clinically depressed males are (in press) realistic in their judgments of control adequately demonstrates that gender may play an important role in mediating clinically depressed subjects' judgments of control. A test of depressive realism using a noncontingent task with clinical subjects needs to be undertaken, with enough males and females included to allow for an adequately powerful test of gender differences.

second issue of generalizability concerns The the relationship between laboratory task results and real life situations. While most studies looking at dysphoric students' judgments of control in noncontingent situations have found evidence of depressive realism (for exceptions, see Benassi & Mahler, 1985; Bryson et al., 1984), similar findings have been less commonplace in studies employing other methodologies in the investigation of depressive realism. Other major areas of research investigating the depressive realism phenomenon include performance feedback studies, which differentiate depressed and nondepressed students' memory for, and reaction to, spurious negative feedback following a performance task, and interpersonal judgment and feedback studies, which contrast depressed and nondepressed subjects' perceptions either against some form of interpersonal feedback or against some other criterion group's interpersonal judgments (see Alloy & Abramson, 1988, for a review). Results from these studies have tended to be somewhat contradictory, leading to the suspicion that while depressive realism may be relatively easy to identify in a laboratory setting with little ecological validity, as researchers move into domains that have greater personal relevance to subjects the phenomenon becomes increasingly elusive (Dobson & Franche, 1989). For

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example, Dunning and Story (1991) recently asked depressed and nondepressed subjects to make predictions about future actions and outcomes that might occur in their personal academic and social worlds. It was found that depressed students proved to be more overconfident than the nondepressed students in the forecasts they rendered.

Coyne and Gotlib (1983) have argued that this weakening of the depressive realism phenomenon can be explained by the fact that the laboratory tasks with which subjects are confronted in judgment of (non)contingency studies involve stimuli which are ambiguous and unfamiliar. Clearly, subjects are not used to pushing buttons to try to determine their degree of control over experimental outcomes, and it is unclear how their behaviour in these artificial situations relates to their day-to-day behaviour, which usually occurs in relatively familiar and unambiguous contexts.

A second limitation of this study is that too few subjects may have been included to allow significant differences between the remitted and nondepressed subjects' judgments of control to be revealed. The failure of the remitted group to exhibit realistic thinking at Time 1 argues against realism serving as a pure vulnerability factor for depression. However, Taylor and Brown's (1988) theory of a reciprocal relationship between depression and illusion of control cannot yet be ruled out. The reciprocal relationship theory predicts that the remitted subjects would show an illusion of control at Time 1, but not as much as the nondepressed group, and that while both groups would become more accurate following depressive mood induction, the remitted group would be the more realistic of the two. This prediction was not supported in the current statistical analyses of the data. However, as illustrated in Figure 1, in spite of the fact that both groups became more accurate in their judgments as mood was lowered, the remitted subjects were more realistic than the nondepressed subjects at both Time 1 and Time 2. It is possible that replicating this study with a larger number of subjects would reveal these group differences to be significant, providing support for the reciprocal relationship theory.

In summary, the results of the current study pose an interpretive conundrum, due to the existence of plausible competing explanations. It is impossible to provide a definitive explanation for the depressed subjects' unrealistic judgments of control (especially in light of the realism observed in the remitted and nondepressed subjects following mood induction) without further research being conducted. A simple replication of this study would reveal if the findings reported here are anomalous. A somewhat more sophisticated design would involve manipulating experimenter presence and exposure to practice trials between and within groups, thus providing specific answers regarding the ability of these variables to influence subjects' judgments of control. Future



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studies would also benefit from a revision of the instructions for the JOC task. Clearer instructions would likely remove the need for practice trials altogether.

#### <u>Conclusion</u>

It can be argued that breaking new ground in our understanding of the cognitive correlates of depression requires research involving laboratory settings and analog tasks so that control over potential confounding variables can be maintained. Tight experimental control has been difficult to achieve in the depressive realism research to date which has employed innovative methodologies with increased face validity. A common complaint has been that many of the studies which involve "real life" situations do not allow for objective assessments of control in a given task, making it impossible to evaluate the relative accuracy of subjects' responses (Ackermann & DeRubeis, 1991). At the same time, though, building accurate models of depression requires that theoretical constructs have some connection to the everyday lives of individuals. The heuristic value of any model which can account for behaviour in the laboratory but not in real life must be seen as limited at best. What is urgently needed in the field of depressive realism research is an experimental methodology which has real life applicability but still allows for objective knowledge of contingencies. Unfortunately, no such methodology is currently available.

The advent of advanced methodologies among depressive

realism researchers would also provide impetus for a broadening of our focus from questions of mental illness and pathology to questions of mental health. Seligman (1991) has recently pointed out that the depressive realism findings may have more striking implications for our understanding of nondepressed people than depressed people. Seligman suggests that the apparently natural tendency of some nondepressed people to interpret life optimistically can be taught to individuals who tend to have a relatively more pessimisstic outlook, enabling them to stave off depression even in the face of negative life events. In a similar vein, Taylor and Brown (1988) reviewed the social cognition literature and concluded that "the mentally healthy person appears to have the inevitable capacity to distort reality in a direction that enhances self-esteem, maintains beliefs in personal efficacy, and promotes an optimistic view of the future" (p. 204). While misinterpretations of one's self and the environment may seem to be maladaptive, positive misinterpretations may in fact be adaptive, in that they create a sense of mastery and self-confidence which may allow the individual to strive forward in ways that might be avoided if the full meaning of negative events were accurately perceived. It is conceivable that some day the capacity to develop and maintain positive illusions will be generally thought of as a valuable human resource to be nurtured and promoted, rather than a defective processing system in need of correction. Such advances,

though, await the empirical support which methodologically sound and conceptually relevant depressive realism studies would provide.

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magnification of failure. Journal of Abnormal Psychology, 97(1), 90-93.

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## JUDGMENT OF CONTROL



Appendix B - Rating of Confidence Scale

# HOW CONVINCED ARE YOU THAT YOUR LAST ANSWER IS CORRECT?



80

Appendix C - Visual Analog Scale

AT THIS PARTICULAR MOMENT,

HOW HAPPY / SAD DO YOU FEEL?



COMPLETELY SAD

NEUTRAL

COMPLETELY HAPPY Appendix D - Speed Writing Task

## WRITE NUMBERS, GOING FROM 100 - 1,

### AS QUICKLY AS YOU CAN

#### Appendix E - Computerized Velten Instructions

Please read each of the following statements to yourself, then read each of them out loud. Concentrate on each of the statements as they appear before you, and make an effort to continue to do so until the next statements shows up on the monitor. Following these statements, there will be a problem solving task to perform, and you will have an opportunity to talk about your feelings.

In the first part of this experiment, I will be reading a series of statements. These statements represent a certain mood. My success will be largely a question of my willingness to be receptive and responsive to the idea in each statement, and to allow each idea to act upon me without interference. These ideas are called suggestions.

First, as each statement appears before me, I will simply read it to myself, and then I will read it once out loud in a manner appropriate to its intended seriousness. Then I'll go over each statement again and again in my head with the determination and willingness to really believe it. I will experience each idea. I will concentrate my full attention on it, and I will exclude other ideas which are unrelated to the mood.

I will attempt to respond to the feeling suggested by each item. I will then try to think of myself with as much clarity and realism as possible as definitely being and moving into that mood state. I am letting myself be receptive to these feelings. Different people move into moods in different ways. Whatever induces the mood in me fastest and most deeply is the best way for me. Some people simply repeat the statements over and over again to themselves with the intention of experiencing them.

Some people find it natural and easy to visualize a scene in which they had or would have had such a feeling or thought. Or, perhaps some easy combination of repeating the statements and imagining scenes will come to me. Very likely, I will begin to feel the way I do when I'm in that mood. I will continue to concentrate my full consciousness on experiencing and retaining the mood as each suggestion is presented. A certain amount of time will be devoted to each suggestion. I will continue to discipline and train myself in inducing a mood in myself by concentrating my full attention on the mood statements during any time interval.

To sum up: the whole purpose of this experiment is to see whether a person can talk him/herself into a mood. Some of these mood- statements may have no relation to anything I have ever thought, said, or done. Yet, exactly in the manner of

#### Appendix E (continued)

hypnosis, I will find it quite easy to accept and feel these emotions. I will be concentrating on doing so, rather than comparing each single statement to my life experience and then deciding whether it applies to me. I will let and strive to let them apply to me. I can do this.

I experience each statement as if it were written especially for me. At first I may experience the impulse to compare a single mood-statement to my life experience, or to resist statements which seem to be or are contradictory to what I feel myself to be. But, most people feel this at first. It will become apparent to me that if I am able to talk myself into a mood, then obviously I know how to talk myself out of one. If I find that I can do these things, then I have learned something about myself; I can control my moods to an extent.

If I feel the urge to laugh, it will probably be because humour is a good way to counteract unwanted feelings . . . or, it may be because I am surprised that I really am going into the mood. I will try to avoid these reactions, however, by keeping in mind that I have a chance of acquiring extremely useful information about myself and how to keep myself out of undesirable moods that occur in everyday life. IF FOR ANY REASON I FEEL I CANNOT CONTINUE, I WILL SO INDICATE.

The next screen will begin the series of statements. I will read each to myself first, then I will read it out loud. Then I will try to experience the mood as well as I can and continue to do so as the statements continue to be presented and I move further into the mood. After these cards there will be a problem solving task to perform. After that I will have an opportunity to talk about my feelings.

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#### Appendix F - Velten Mood Induction Statements

Today is neither better nor worse than any other day.

However, I feel a little blue today.

I feel rather sluggish now.

Sometimes I wonder whether life is all that worthwhile.

Every now and then I feel so tired and gloomy that I'd rather sit than do anything.

I can remember times when everybody but me seemed full of energy.

Too often I have found myself staring listlessly into the distance, my mind a blank, when I definitely should have been working.

It has occurred to me more than once that studying is basically useless, because you forget almost everything you learn anyway.

People annoy me; I wish I could be by myself.

I've had important decisions to make in the past, and I've sometimes made the wrong ones.

I do feel somewhat discouraged and drowsy ---- maybe I'll need a nap when I get home.

I'm afraid the trouble in the Middle East may get a lot worse.

There have been days when I felt weak and confused, and everything went miserably wrong.

I just don't seem to be able to get going as fast as I used to.

I've had daydreams in which my mistakes kept occurring to me. Sometimes I wish I could start over again.

Just a little bit of effort tires me out.

I'm ashamed that I've caused my parents needless worry.

#### Appendix F (continued)

I feel terribly tired and indifferent to things today.

I'm getting tired out. I can feel my body getting exhausted and heavy.

Just to stand up would take a great effort.

At times I've been so tired and discouraged that I went to sleep rather than face important problems.

I'm beginning to feel sleepy. My thoughts are drifting.

I couldn't remember things well right now if I had to.

My life is so tiresome ---- the same old thing day after day depresses me.

I want to go to sleep ---- I feel like just closing my eyes and going to sleep right here.

I just can't make up my mind; it's so hard to make simple decisions.

I've doubted that I'm a worthwhile person.

I'm not very alert; I feel listless and vaguely sad.

It often seems that no matter how hard I try, things still go wrong.

I feel worn out. My health may not be as good as it's supposed to be.

I'm uncertain about my future.

I've noticed that no one seems to really understand or care when I complain or feel unhappy.

I've laid awake at night worrying so long that I hated myself.

I'm discouraged and unhappy about myself.

The way I feel now, the future looks boring and hopeless.

Things are worse now than when I was younger.

Some very important decisions are almost impossible for me to make.

My parents never really tried to understand me.

#### Appendix F (continued)

I feel horribly guilty about how I've treated my parents at times.

I feel tired and depressed; I don't feel like working on the things I know I must get done.

Things are easier and better for other people than for me. I feel like there's no use in trying again.

I have the feeling that I just can't reach people.

It takes too much effort to convince people of anything. There's no point in trying.

Often people make me very upset. I don't like to be around them.

It's so discouraging the way people don't really listen to me.

I fail in communicating with people about my problems.

Sometimes I've wished I could die.

I've felt so alone before, that I could have cried.

My thoughts are so slow and downcast. I don't want to think or talk.

I just don't care about anything. Life just isn't any fun.

I'm so tired.

Life seems too much for me anyhow - my efforts are wasted.

I have too many bad things in my life.

I don't concentrate or move. I just want to forget about everything.

I feel dizzy and faint. I need to put my head down and not move.

Everything seems utterly futile and empty.

All of the unhappiness of my past life is taking possession of me.

I don't want to do anything.

I want to go to sleep and never wake up.

#### Appendix G

#### CONSENT FORM (for depressed subjects)

#### VULNERABILITY FACTORS IN CLINICAL DEPRESSION Keith S. Dobson, Ph.D. - Principal investigator

I hereby consent to participate in a study being conducted by Dr. Keith Dobson and Mr. Dennis Pusch of the Department of Psychology, University of Calgary. I understand that the study is investigating the role of certain types of thinking, and their potential relationship to clinical depression. The research will include a personal interview, the completion of several psychological questionnaires, and the completion of three tasks that will be administered by a microcomputer. I understand that the entire time that the study will take will be approximately one-and-a-half to two hours.

I further understand that the results of my participation in this study will be held in strict confidence. All documents will be held in a locked storage area, and the results of my participation will not be known except to the Principal Investigator and the members of the research team. I understand that identifying information about me will never be released, and that research publications that may result from the study will include only group data, and never my personal test results. All information regarding me will be destroyed five years after publication of the research.

By signing this consent form, I am indicating that I am participating in this study of my own free will. I am under no obligation to complete the study, and understand that if I decide to stop my participation no negative consequences will occur. Should I now or in the future desire further information about the research study, I am free to contact the Principal Investigator:

Dr. K.S. Dobson Department of Psychology University of Calgary Calgary, AB T2N-1N4 Phone: (403) 220-5096

Signature

Witness (Investigator)

Name (printed)

Name (printed)

Date

Date

#### Appendix G (continued)

#### CONSENT FORM (for remitted/nondepressed subjects)

#### VULNERABILITY FACTORS IN CLINICAL DEPRESSION Keith S. Dobson, Ph.D. - Principal investigator

I hereby consent to participate in a study being conducted by Dr. Keith Dobson and Mr. Dennis Pusch of the Department of Psychology, University of Calgary. I understand that the study is investigating the role of certain types of thinking, and their potential relationship to clinical depression. The research will include a personal interview, the completion of several psychological questionnaires, and the completion of three tasks that will be administered by a microcomputer. I also understand that prior to one of these tasks I will take part in a procedure designed to create a temporarily lowered mood, and that after the final task I may undergo another procedure designed to reverse any possible negative effects. I understand that the entire time that the study will take will be approximately one-and-a-half to two hours.

I further understand that the results of my participation in this study will be held in strict confidence. All documents will be held in a locked storage area, and the results of my participation will not be known except to the Principal Investigator and the members of the research team. I understand that identifying information about me will never be released, and that research publications that may result from the study will include only group data, and never my personal test results. All information regarding me will be destroyed five years after publication of the research.

By signing this consent form, I am indicating that I am participating in this study of my own free will. I am under no obligation to complete the study, and understand that if I decide to stop my participation no negative consequences will occur. Should I now or in the future desire further information about the research study, I am free to contact the Principal Investigator:

Dr. K.S. Dobson Department of Psychology University of Calgary Calgary, AB T2N-1N4 Phone: (403) 220-5096

Signature

Witness (Investigator)

Name (printed)

Name (printed)

Date

Date

#### Appendix G (continued)

#### CONSENT FORM (for control subjects)

#### VULNERABILITY FACTORS IN CLINICAL DEPRESSION Keith S. Dobson, Ph.D. - Principal investigator

I hereby consent to participate in a study being conducted by Dr. Keith Dobson and Mr. Dennis Pusch of the Department of Psychology, University of Calgary. I understand that the study is investigating the role of certain types of thinking, and their potential relationship to clinical depression. The research will include the completion of several psychological questionnaires, and the completion of two tasks that will be administered by a microcomputer. I understand that the entire time the study will take will be approximately one hour.

I further understand that the results of my participation in this study will be held in strict confidence. All documents will be held in a locked storage area, and the results of my participation will not be known except to the Principal Investigator and the members of the research team. I understand that identifying information about me will never be released, and that research publications that may result from the study will include only group data, and never my personal test results. All information regarding me will be destroyed five years after publication of the research.

By signing this consent form, I am indicating that I am participating in this study of my own free will. I am under no obligation to complete the study, and understand that if I decide to stop my participation no negative consequences will occur. Should I now or in the future desire further information about the research study, I am free to contact the Principal Investigator:

Dr. K.S. Dobson Department of Psychology University of Calgary Calgary, AB T2N-1N4 Phone: (403) 220-5096

Signature

Witness (Investigator)

Name (printed)

Name (printed)

Date

Date

#### Appendix H

#### JUDGMENT OF CONTINGENCY TASK

#### Instructions for first administration

This task is a measure of your ability to solve a certain type of problem.

The task consists of 40 trials, and will involve the use of the computer. For each trial, there are two types of pictures which may show up on the monitor. First, there will always be a YELLOW LIGHT BULB <GREEN STAR> which flashes on the left side of the monitor for three seconds at the start of every trial. While this light is on, you have the choice of either pressing or not pressing the SPACE BAR <ENTER KEY>. After those three seconds are up, a GREEN LIGHT BULB <RED DIAMOND> may or may not appear on the right side of the monitor for a couple of seconds. Your job will be to figure out how much control you have over whether or not the GREEN LIGHT BULB <RED DIAMOND> appears. Each time the YELLOW LIGHT BULB <GREEN STAR> comes on, you will make a choice about what to do on that trial, and either press or not press the BAR <KEY>. Ιf you decide to press, you can only press once, and it has to be during the three seconds that the light bulb is showing. If you decide not to press, then don't touch the SPACE BAR <ENTER KEY> during that three second interval. After the three seconds are up, the GREEN LIGHT BULB <RED DIAMOND> will either come on, or it won't come on.

So, let's run through the whole sequence. The YELLOW LIGHT <GREEN STAR> comes on - this is your signal - you decide whether to press the key or not press the key - and finally, you watch and see whether or not the GREEN LIGHT <RED DIAMOND> comes on. Remember, on each trial there are only two things you can try to do to control the GREEN LIGHT BULB <RED DIAMOND>: you either press the SPACE BAR <ENTER KEY> within three seconds after the YELLOW LIGHT <GREEN STAR> appears, or else just sit back and watch.

Since it is your job to learn how much control <u>you</u> have over whether or not the GREEN LIGHT <RED DIAMOND> comes on, it is to your advantage to press the SPACE BAR <ENTER KEY> on some trials and not on others, so you know what happens when you don't press as well as when you do press.

Any questions? Okay, just to give you the feel of it, let's go through a few practice trials before we do the real thing. (Allow subject 10 practice trials at 100% contingency).

#### Appendix I

#### Instructions for Judgment of Contingency Ratings

Right now I would like you to let me know how much control you believe you had over whether or not the GREEN LIGHT BULB <RED DIAMOND> appeared. I would like you to do this by putting an "X" somewhere along this line. If you put the "X" at the 100% end, it means that you believe you had total control over whether or not the picture came on. If you put an "X" at the 0% end, it means that you believe that you had no control over the light at all. Putting the "X" in the middle would mean that you think you had control over the LIGHT <DIAMOND> about half of the time. You are allowed to put the "X" anywhere along the line that you like. Any questions?

#### Instructions for Confidence Ratings

Now I would like you to let me know how confident you are that the answer you just gave me is right. If you are 100% sure that you put the "X" at the right place along the line, make a mark down here at the "totally convinced" end of the line. If you have no idea if you are even remotely close to the right answer, put a mark down here at the "not sure at all" end. Putting a mark in the middle would mean that you're not certain that you were exactly right, but you have a hunch that you were in the right neighbourhood. Again, you may place the mark anywhere along this line that you'd like. Any questions?

#### Appendix J - Verbal instructions for subjects undergoing the Velten Mood Induction Procedure

This is a study involving mood induction. Essentially, we want to know if people can talk themselves into a specific mood or feeling. Your success in talking yourself into the mood will depend to a great extent on your cooperation and willingness to participate in the experiment. This is not intended to be harmful or frightening, but is only an effort to find out if people can induce moods in themselves.

The main part of the experiment will involve the use of the computer. Once we start the computer program, there will be 68 separate messages or statements for you to work your way through. The first nine messages contain some instructions for the rest of the task, and you are free to go through these instructions at your own pace. When a message appears, please read it once quietly to yourself, and then once out loud. Hit this green button <SPACE BAR> when you finish reading, and the next instruction message will automatically appear.

The remaining 59 messages contain mood statements, which, when read, will suggest a certain feeling, and help you to create that feeling in yourself. Again, please read each of these statements once to yourself, and then once out loud. Once you have read it twice, continue to look at and concentrate on the statement until the next mood statement flashes onto the screen. You will never have to hit the button to move to the next mood statement. Your job is to concentrate on the statements - the computer will take care of the rest.

In summary then, you will first be going through nine instruction messages at your own pace, and then through 59 mood statements at the computer's pace.

Shortly after you begin, I will be moving into the hallway so that you can have some privacy. Just continue on with the task, and the computer will tell you when you are finished. That's when I'll come back into the room. But if you find yourself having some difficulty during the task, please don't hesitate to let me know. Any questions before we begin?

#### Appendix K

#### JUDGMENT OF CONTINGENCY TASK

#### Instructions for second administration

This last task is quite similar to the one we did earlier where you were trying to find out how much you could control whether or not the GREEN LIGHT BULB <RED DIAMOND> came on.

This task will again consist of 40 trials. But this time, the colours and shapes of the lights that show up on the screen will be different. Instead of a YELLOW LIGHT <GREEN STAR> and a GREEN LIGHT <RED DIAMOND>, this time you will see a GREEN STAR <YELLOW LIGHT BULB> and a RED DIAMOND <GREEN LIGHT BULB>. However, the basic principle is the same. The light on the left side of the screen will flash on for three seconds, and that will be the signal for you either to press or not press the button. However, this time, rather than using the SPACE BAR <ENTER KEY>, you will be using the ENTER KEY <SPACE BAR> Once again, it is your job to figure out how much you here. can control whether or not the picture on the right side of the screen appears by either pressing or not pressing the KEY <BAR>. The amount of control that you have over this task will not necessarily be the same as before, so it will be important for you to pay attention, and it will still be to your advantage to press the button on some trials, and not press it on others.

Since you have already had some experience with this type of task, we won't be doing any practice trials this time. Any questions before we begin?

#### Appendix L

#### SUMMARY TABLE

2 x 2 (Group by Time) Repeated Measures ANOVA on Judgments of Control for Remitted and Nondepressed Subjects

Source	SS	df	MS	F
Group	1188.15	1	1188.15	1.19
Subject/Group	27764.53.	28	991.59	
Time	2318.82	1	2318.82	7.02*
Group x Time	66.15	1	66.15	.20
Subj./G x T	9242.53	28	330.09	

.

\* <u>p</u> < .05

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