MEMORY BIASES AND DEPRESSIVE REALISM

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by

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INTRODUCTION

Memory Biases and Depressive Realism

Contemporary cognitive theories of depression (Abramson, Metalsky, & Alloy, 1989; Abramson, Seligman, & Teasdale, 1978; Beck, 1967, 1987) posit that that depressed and nondepressed individuals process information differently from one another, and that these differences often have a profound influence on their mood. Despite this focus, basic questions about how best to characterize the thinking of depressed individuals, relative to nondepressed individuals, remain unanswered. For instance, Beck (1987, p. 9) characterized depressed individuals as possessing an "increasingly distorted cognitive processing". However, there is an extensive literature on the depressive realism phenomenon (see Alloy & Abramson, 1988, for a review) positing exactly the opposite position: that depressed persons are actually *better* judges of certain events in the world than their non-depressed counterparts. The current investigation will attempt to address this paradox and will focus specifically on the causal attributions that individuals assign to life events. Research into the influence of causal attributions on mood has characterized "attributional style" as a dispositional cognitive product, or outcome, of how an individual thinks about and processes a particular event (Hollon & Kriss, 1984). Attributional style has been shown to be important in whether or not an individual is at risk for, and subsequently will develop, depression (Alloy et al., 2000; Abramson et al., 1999; Alloy, Abramson et al., 1999). Despite the wealth of research on attributional style and depression, the question of whether or not those individuals suffering from depression are more or less accurate in how they assign causes to events remains an open one (Abramson et al., 2002; Ackermann & DeRubeis, 1991; Dobson & Franche, 1989; Haaga & Beck, 1995). Recently, Moore and Fresco

(2007) found that individuals suffering from, and at risk for, depression may be less accurate in the causes they assign to events. However, these findings were preliminary, have yet to be replicated, and left room for methodological improvements. Therefore, the first question that the current investigation will seek to address is: Are depressed individuals really "sadder but wiser" (see the title of the seminal Alloy & Abramson, 1979 article) in their attributional style or are they possessed of "depressive cognitive distortions" as others have characterized them (Beck, 1987)?

This question of differential accuracy, however, assumes that thoughts are static entities that do not change over time. What if, for example, the evaluations of an event of depressed individuals are relatively realistic (compared to nondepressed individuals) immediately after the event in question, but become negatively biased when this evaluation is recalled from memory? This proposed increase in cognitive bias over time may be due to dysfunctions in memory that are related to depressed mood. In fact, there is considerable research evidence attesting to the influence of mood on memory, and suggesting that depressed mood may have a negatively biasing effect on subsequent recall (see Blaney, 1986; Whitehouse, Turanski, & Murray, 2000; Williams, Watts, MacLeod, & Matthews, 1997 for reviews). It is therefore possible that the depressed individual is *both* wiser than the nondepressed individual *and* possessed of cognitive distortions, albeit at different points in the chain of information processing. The issue of bias as a potential property that emerges in the cognition of the depressed person over time is the second question that will be addressed in the current investigation. Both questions, however, are concerned with mood-related biases in information processing. Introduction to this more general topic will begin with a discussion of the depressive realism hypothesis. Research in support of depressive realism does not find any evidence of cognitive bias in depressed individuals. In fact, some depressive realism research has found a bias in *non*depressed individuals.

The Depressive Realism Hypothesis

The depressive realism hypothesis (Alloy & Abramson, 1979) posits not only that depressed individuals can make realistic inferences, but that they do so to a greater extent, as compared to nondepressed individuals. Evidence for this phenomenon comes in the form of studies utilizing what is called the "judgment of contingency task" where participants are asked to press a button, which results in the illumination of a green light a percentage of the time that is predetermined by the experimenter. Judgments of contingency refer to the degree of control that a participant's response has on a particular outcome. As such, there are two factors that the participant needs to attend to: the occurrence of the outcome in the presence of the response and the occurrence of the outcome in the absence of the response. Higher positive contingencies result when the outcome occurs at a higher rate in the presence of the response than in its absence (i.e. the response "causes" the outcome). Negative contingencies are also possible where the outcome is less likely to occur in the presence of the response than in its absence (i.e. the response suppresses the outcome). The dependent variable is the participant-rated contingency between pressing the button and the illumination of the light (e.g. the percentage of trials where a button push results in the light coming on or their degree of control over this outcome).

Consistent with the depressive realism effect, depressed individuals have been shown to more accurately make these kinds of judgments than nondepressed individuals (Alloy, Abramson, & Kossman, 1985; Alloy, Abramson, & Viscusi, 1981; Musson & Alloy, 1987; Vazquez, 1987). Nondepressed individuals experienced what has been referred to as an "illusion of control", where they consistently overestimated the degree of contingency between pressing the button and the illumination of the light, or their degree of control over the outcome. Depressed individuals experienced no such bias. In addition, these results were replicated over a variety of differing predetermined contingency conditions (Abramson, Alloy, & Rosoff, 1981; Alloy & Abramson,

1979; Dobson & Pusch, 1995; Ford & Neale, 1985; Martin, Abramson, & Alloy, 1984; Msetfi, Murphy, Simpson, & Kornbrot, 2005; Presson & Benassi, 2003; Vazquez, 1987). In these studies, depressed individuals were found to be better able to judge the experimenter-controlled contingency then nondepressed individuals in both low-contingency (25% contingency between pressing the button and illumination of the light), moderate-contingency (50%), as well as high- (75 and 100% contingency) and no-contingency (0%) conditions.

Other paradigms, referred to as the self-evaluation of task performance (Gotlib, 1983; Lobitz & Post, 1979; Rozensky et al., 1977) and recall of feedback paradigms (DeMonbreun & Craighead, 1977; Dennard & Hokanson, 1986; Nelson & Craighead, 1977) have also produced findings compatible with depressive realism. Studies examining the self-evaluation of task performance have participants engage in a task, then rate their performance on that task without the benefit of feedback. The participants' self-performance is then compared to their actual performance to determine how accurately it was perceived. In research examining the recall of feedback, ratings of the participants performance is given immediately after each subtask is completed, and the participants are then asked to rate their aggregate level of performance across the task as a whole. The participants' recall of the feedback they received is compared to the actual feedback to determine how accurate their recall was. In many studies (DeMonbreun & Craighead, 1977; Dennard & Hokanson, 1986; Gotlib, 1983; Lobitz & Post, 1979; Nelson & Craighead, 1977; Rozensky et al., 1977) depressed individuals were better able to evaluate or recall their performance than nondepressed individuals.

Studies comparing expectancies of success on various tasks with depressed and nondepressed individuals have replicated these findings as well (Alloy & Abramson, 1980; Alloy & Seligman, 1979; Golin, Terrell, & Johnson, 1977; Golin, Terrell, Weitz, & Drost, 1979). In many of these studies, the predictions of future success of depressed and nondepressed

individuals are compared on both chance-tasks as well as tasks designed to appear skill-determined (but are actually chance-determined), both prior to and immediately after reinforcement or punishment. Smaller changes in expectancies of success by nondepressed relative to depressed individuals have been found following reinforcement or punishment in the tasks designed to appear skill-based (Alloy & Abramson, 1980, Alloy & Seligman, 1979). Insofar as performance is expected to improve on skill-determined tasks, the findings that expectancies of the nondepressed participants do not change as much as the depressed participants is taken as evidence of perceptual bias in nondepressed participants. These differences between depressed and nondepressed participants have not been found using chance-determined tasks, where performance would not be expected to improve (Alloy & Abramson, 1980, Alloy & Seligman, 1979). Taken together, the aforementioned results have been interpreted by some as evidence that the depressed individuals more accurately perceive their performance on these tasks.

Despite the apparent wealth of findings in support of depressive realism, the results of numerous studies have not supported it. In addition, the methodology of some of the literature in support of a depressive realism effect has been cogently undermined. Studies assessing the accuracy of depressed and nondepressed persons' delayed recall of both task-performance (Craighead, Hickey, & DeMonbreun, 1979; DeMonbreun & Craighead, 1977) and ambiguous personality feedback (Dykman, Abramson, Alloy, & Hartlage, 1989; Gotlib, 1983; Vestre & Caulfield, 1986) have returned results largely showing both groups to be equally accurate. Depressed individuals have been shown to underestimate positive feedback that they receive and nondepressed individuals have been shown to overestimate it (Buchwald, 1977; Wener & Rehm, 1975), illustrating bias among both groups. A recent quantitative review of the depressive realism literature supports the view that studies within this literature have not produced consistent results (Moore & Fresco, 2009). This inconsistency holds when viewed across the varied methodologies

cited above (i.e. judgment of contingency, evaluation of performance and recall of feedback) to evaluate depressive realism with one exception. Research exploring attentional bias using the deployment of attention task (DOAT; Gotlib, McLachlan, & Katz, 1988) has uniformly support a depressive realism effect. In this task, two words are presented to participants that differ in emotional valence (depressed, manic, or neutral). The participants are told that one word will be replaced by a red-colored bar, one word by a green-colored bar, that one of these two bars will appear first, and that they are to judge which bar is presented first. However, in reality, the two colored bars are presented simultaneously. The bar which replaced the word that the participant was attending to would be perceived as appearing first. Therefore, a participant that consistently perceived the depressed words as appearing first would evidence a bias in attending to negatively-valenced words.

An additional critique of the depressive realism literature comes from Ackerman and DeRubeis (1991) who perceptively note that the aforementioned research on expectancies of success cannot be said to support depressive realism unequivocally, as no objective standard of reality exists for an expectation. They give the example of a plausible rival hypothesis in a nondepressed individual who may overestimate his/her chance of success with the expectation that practice will improve their future performance.

In addition to the evidence presented above that is not supportive of a depressive realism effect, Beck's cognitive theory (1967, 1987) of the etiology of depression posits anti-depressive realism effects and characterizes the depressed individual as deficient in their ability to accurately perceive reality. Predictions of this theory have been supported in research (Beck, Rush, Shaw, & Emery, 1979), and the theory serves as the basis for one of the most validated treatments of depression (DeRubeis & Crits-Cristoph, 1998), cognitive therapy of depression (Beck et al., 1979).

Beck's Theory

Beck's (1967, 1987) theory posits that depressed affect is heavily influenced by recurrent thoughts with negative content or automatic thoughts. These thoughts arise from deeply-held dysfunctional beliefs or schemas. Beck identified that schemas and automatic thoughts, and the depressed affect that results from them, tend to be self-perpetuating. The depressed person is thought to attend more to negative events in their lives. As a result, they are thought to interpret events in light of their own dysfunctional cognitions, both causing and perpetuating depressed mood. Beck (1987, p.14) characterizes the cognition of depressed individuals as "schema-driven", while that of nondepressed individuals as "data-driven", implying that depressed individuals' cognitions are systematically less informed by reality and hence, more irrational. For instance, a depressed person may experience a significant success (such as getting a good grade on a test) while in a depressed state but may minimize the importance of that event as due to chance because "I'm a failure, how can I do anything right"? Despite this significant role of cognitive bias in both Beck's theory and in cognitive therapy, however, there is only one study (Moore & Fresco, 2007) that has, so far, addressed this issue in the context of a cognitive theory of the etiology of depression. Replicating and extending this earlier work is the first aim of the present study, but first requires a discussion of the specific cognitive vulnerability-stress theory in question, the hopelessness theory of depression (Abramson, Metalsky, and Alloy, 1989).

Are Depressed Individuals Really "Sadder But Wiser"?

The hopelessness theory of depression (Abramson, Metalsky, and Alloy, 1989) has received considerable empirical support and follows from the reformulated learned helplessness theory (Abramson, Seligman, & Teasdale, 1978) of depression. Both of these theories emphasize

a cognitive vulnerability to depression that is based on the way that individuals assign causes to events in their lives. In the reformulated learned helplessness theory, the tendency to view negative events as due to internal ("It's all my fault"), stable ("The cause will exist to effect my life forever") and global ("The cause influences all aspects of my life") causes is thought to increase an individual's likelihood to develop depression when confronted with negative life events. In hopelessness theory, this cognitive vulnerability factor is retained, but the internality dimension is de-emphasized in favor of the stability and globality dimensions.

The strongest evidence in favor of hopelessness theory comes from the Cognitive Vulnerability to Depression Project (Alloy et al., 2000; Abramson et al., 1999; Alloy, Abramson et al., 1999), which utilized both a retrospective and prospective behavioral-high risk design. In this study, entire classes of college students were screened for their degree of cognitive vulnerability to depression. Currently-nondepressed individuals were identified as either "high-risk" or "low-risk" based on the degree to which they endorsed a depressogenic cognitive style as well as dysfunctional beliefs (Beck, 1967, 1987). They were then followed every six weeks for two and a half years and then followed every four moths for an additional three more years. Both the retrospective (Alloy et al., 2000) and the prospective (Abramson et al., 1999; Alloy, Abramson et al., 1999) portions of this design found higher rates of depression in the high-risk group as compared to the low-risk group.

Hopelessness theory is silent on the issue of how objective or realistic these attributions are (Abramson et al., 2002). Therefore, this degree of objectivity does not figure into the etiology of depression as defined by hopelessness theory, and the relationship between attributional style and realism remains unclear. These seemingly contradictory theories of how the depressed client should be viewed, as either more biased (according to Beck's widely-accepted theory and much clinical lore) or less biased in her or his perceptions (according to the literature in favor of

depressive realism), represents a paradox that has yet to be successfully resolved. The picture is further muddied by the aforementioned inconsistent results and serious methodological critique of the depressive realism literature.

One prior study has attempted to directly address the question of how realistic individuals both at risk for and currently suffering from depression are, in regards to their attributional style (Moore & Fresco, 2007). How objectively both dysphoric and nondysphoric participants attribute causes to events (i.e., to what extent their attributional style is objectively realistic) was recorded for 239 college undergraduates. Participants were first screened with a measure of attributional style (the Attributional Style Questionnaire; Peterson et al., 1982; Seligman et al., 1979) and contacted by telephone to invite them to participate in the study if they possessed either a depressogenic or nondepressogenic attributional style. Participants were assigned to attributional style groups (depressogenic or nondepressogenic) if the average of their attributional style scores were either in the upper (composing the depressogenic group) or lower quartiles (composing the nondepressogenic group) of the screening sample. They were also assigned to dysphoric or nondysphoric groups based on their scores on a measure of symptoms of depression (the Beck Depression Inventory - Second Edition; Beck et al., 1996), but were not sampled on this measure. Contrary to expectations derived from the depressive realism hypothesis, dysphoric individuals were found to possess less realistic attributions than nondysphoric individuals. Dysphoric participants were found to be pessimistically biased, while nondysphoric participants were found to be optimistically biased, although to a lesser extent. In addition, individuals with a depressogenic attributional style were also found to possess less realistic attributions then individuals with a nondepressogenic attributional style. Individuals with a depressogenic attributional style were found to be pessimistically biased and individuals with a nondepressogenic attributional style were found to be optimistically biased. However,

participants for this study were sampled based on their attributional style. Dysphoric and nondysphoric participants were sampled from this distribution of those both high and low on attributional style. This sampling strategy confounded dysphoria with risk for depression and may have unintentionally produced groups of dysphoric individuals composed only of individuals possessing a depressogenic attributional style (not sampling dysphoric individuals with a nondepressogenic attributional style). If dysphoric individuals with a nondepressogenic attributional style differ systematically from those with a depressogenic attributional style on how objectively they assign causes to events, excluding one of these dysphoric group subtypes from the dysphoric group in the aforementioned study seriously undermines the results. A sampling strategy that focused specifically on level of dysphoria may produce different results, and the current study sought to replicate and extend the previous findings of Moore and Fresco (2007) by utilizing such a strategy.

Do Biases in Memory Mediate the Effects of Depressive Realism?

The concept of mood-based biases in memory is not novel, by any means, and there is a large literature attesting to its existence (see Blaney, 1986; Whitehouse et al., 2000; Williams et al., 1997 for reviews). The literature on mood and memory will be briefly reviewed prior to a discussion of ways that specific memory processes will be examined in the current study.

Is Memory Influenced by Mood, and How?

The mechanism by which people process and retain information, and thereby learn and benefit from it, has been conceptualized as being composed of four distinct sub-processes: attention to stimuli, encoding of the information obtained from this stimuli (into memory), storage of this information (in memory), and its retrieval (from memory). There are consistent

findings which have illustrated that depressed participants attend to stimuli evenhandedly, while nondepressed participants show a preference for positively-valenced stimuli (Gotlib, McLachlan, & Katz, 1988; McCabe & Gotlib, 1995; McCabe, Gotlib, & Martin, 2000; McCabe & Toman, 2000). The current study will therefore focus on the three information processing sub-components related to memory (encoding, storage, and retrieval), and will attempt to localize the source of a possible memory bias there.

Initial studies of the influence of mood on memory have consistently found that depressed subjects show preferential recall for negatively-valenced material (Bradley & Matthews, 1983; Derry & Kuiper, 1981; McDowall, 1984; Dennard & Hokanson, 1986; Murray, Whitehouse, & Alloy, 1999). These findings have also been extended to include those at risk for depression, with risk being defined as possessing a depressogenic attributional style (Alloy, Abramson, Murray, Whitehouse, & Hogan, 1997). Negatively-valenced stimuli in these studies are typically represented by words that participants have to remember that are both depression- and personally-relevant, such as "blue", "inferior", or "clumsy". Research examining the process which might underlie these findings involves investigation of two key phenomena: mood congruent and state dependent memory. Mood congruence (see Blaney, 1986 for a review) assumes that information will be more likely to be stored and/or recalled when its content matches the mood an individual is in at the time of storage/retrieval. Research examining a mood congruent memory effect involves the assessment of mood at one time point, identification of depressed or dysphoric and nondepressed or nondysphoric individuals[1] based on this assessment, and an examination of differential recall of, at minimum, positively- versus negatively-valenced stimuli. Two methodological paradigms have largely been utilized to illustrate support for mood congruent memory. The first examines differential recall for success versus failure at a laboratory task (Buchwald, 1977; Craighead, Hickey, & DeMonbreun, 1979; Dobson & Shaw, 1981; Nelson

& Craighead, 1977; Werner & Rehm, 1975) and the second examines differential recall of positively- and negatively-valenced words (Bradley & Matthews, 1983; Dunbar & Lishman, 1984; Kuiper & Derry, 1982; Matthews & Bradley, 1983; Post, Lobitz, & Gasparikova-Krasnec, 1980).

State dependence, on the other hand, implies that information encoded into or recalled from memory in one mood is more likely to be recalled when the individual is in that same mood. There are two main differences between this and the mood congruence effect (Blaney, 1986). First, state dependence does not concern itself with the content of the stimuli. Second, mood congruence does not concern itself with matches between mood at encoding and retrieval. Therefore, unlike the literature on mood congruence, methodology examining state dependence involves, by necessity, the assessment of mood at two time points (both encoding and retrieval). Studies investigating state dependence effects typically involve participants organized into two or more groups at one time based on naturally-occurring or induced mood where they are exposed to to-be recalled stimuli, then tested for their recall of this information in either the same or a different mood some time later. Participants can therefore be classified into one of four groups: (1) Depression at encoding (we'll call the mood depression for simplicity, but state dependence is not theoretically-bound to this one mood)/Depression at retrieval, (2) Depression at encoding/Nondepression at retrieval, (3) Nondepression at encoding/Nondepression at retrieval, and (4) Nondepression at encoding/Depression at retrieval. State dependence effects would be said to occur if the recall for both Groups 1 and 3 exceeds that of both Groups 2 and 4. Unlike the research in mood congruence, however, the literature on state dependent memory is largely mixed (Bower & Mayer, 1985; Bower, Monteiro, & Gilligan, 1978; Leight & Ellis, 1981; Weingartner, Miller, & Murphy, 1977; Wetzler, 1985). While this cursory review of the literature may seem to indicate the superiority of a mood congruent effect, the picture is muddied by cogent

methodological critique of the research methodology used to evaluate these two competing theories (Blaney, 1986; Riskind, 1989; Whitehouse et al., 2000).

First, Blaney (1986) notes that mood congruent effects may actually represent state dependence effects. Insofar as the mood at encoding has not been assessed in past studies of mood congruence, the possibility that it is identical to the mood at retrieval, when the assessment is undertaken, cannot be ruled out. Which of these two factors, information content or mood match, is more salient cannot be ruled out if mood is only assessed at one time point, which is true for studies which have assessed for mood congruency. Secondly, research has illustrated that mood may not be the prime mediator of biases in memory (Matthews & Bradley, 1983; Pyszczynski, Hamilton, & Herring, 1989; Riskind, 1983; Riskind, 1989; Riskind, Rholes, & Eggers, 1982). These studies have shown that the increase in self-focus that has been shown to accompany depressed mood (Lyubomirsky & Nolen-Hoeksema, 1995; Lyubomirsky, Tucker, Caldwell, & Berg, 1999; Morrow & Nolen-Hoeksema, 1990), not the mood itself, is largely responsible for the preferential recall of negative material that has been consistently found.

Hertel (2000, 2004) posits that depressed individuals' lack of effort to perform controlled searches of memory underlies the characteristic negative biases in their memories. She hypothesizes that this deficit is mediated by the increase in self-focus and the presence of passive, negative, and perseverative or ruminative thinking. Rumination is more common in depressed than nondepressed individuals and has been shown to predict the onset of depressive episodes (Ingram, Lumry, Cruet, & Seiber, 1987; Lyubomirsky & Nolen-Hoeksema, 1995; Lyubomirsky, et al., 1999; Nolen-Hoeksema & Davis, 1999; Nolen-Hoeksema & Morrow, 1991). She uses four lines of reasoning to illustrate the point that rumination may disrupt effortful searches of memory, which result in the memory biases observed in depressed individuals:

1. Nondepressed participants performance on memory tasks, where the process of effortful memory search is experimentally disrupted, becomes worse after the disruption, whereas depressed participants performance remains relatively the same (Hertel & Knoedler, 1996)
2. No differences between depressed and nondepressed participants are found if the experimental task does not tap memory processes that require effortful search (Denny & Hunt, 1992; Hertel, 1994; Hertel & Hardin, 1990; Watkins, Matthews, Williamson, & Fuller, 1992)
3. The performance of depressed participants is worse than nondepressed participants when the memory task in question does require an effortful memory search (Hertel & Milan, 1994; Hertel & Rude, 1991a, 1991b; Rude & Hertel, 1987; Rude, Hertel, Jarrold, Covich, & Hedlund, 1999)
4. Differences in the memory of depressed and nondepressed participants disappear when depressed participants are experimentally induced to utilize effortful memory search strategies (Hertel, 1998; Hertel & Hardin, 1990; Hertel & Rude, 1991a, 1991b; Weingarter, Cohen, Murphy, Martello, & Gerdt, 1981).

This last point is particularly significant as it implies that, for depressed individuals, it is a lack of *effort*, not *ability*, which underlies the differences in memory found between this group and the nondepressed. It is possible that this lack of effort is a symptom of the lack of motivation and initiative that is characteristic of depression (Hertel, 2000, 2004) and that can be alleviated with behavioral treatments that have shown to be effective in targeting this symptom (Jacobson, Martell, & Dimidjian, 2001; Martell, Addis, & Jacobson, 2001).

The second aim of the current study is to determine if effects consistent with depressive realism disappear or reverse when responses involve delayed recall from memory as opposed to

perceptions of an event immediately after its occurrence. It is our hope that illustrating that dysphoric participants can be *both* more *and* less accurate in their perceptions of events, depending on when the judgments are made (immediately afterwards or after a delay, requiring retrieval of this information from memory) will help to resolve the paradox of whether depressed individuals are more accurate in their perceptions (as predicted by the depressive realism hypothesis) or less accurate (according to Beck and his colleagues) then nondepressed individuals. While the question of the objectivity of attributional style in dysphoric individuals was the focus of the first aim, memory bias will be assessed using a specific form of the judgment of contingency task that has reliably produced results consistent with depressive realism (Alloy & Abramson, 1979; Msetfi et al., 2005; Murphy, Vallee-Tourangeau, Msetfi, & Baker, 2005). Judgments of contingency were used as the main outcome variable in an attempt to merge the research on memory bias in depression with the depressive realism literature and make the former more directly relevant to the latter. The JOCT is the most widely used paradigm to examine depressive realism (Moore & Fresco, 2009). As a result, use of the JOCT would allow the current study to be readily integrated into the pre-existing depressive realism literature. Participants will be asked to recall the degree of control they felt pressing a button will have on the illumination of a light, both immediately after the task is completed and then 2 weeks afterwards. Judgments of control that decrease over time will be indicative of a negative memory bias and judgments that increase will be indicative of positive bias. We predict that nondysphoric participants will illustrate an "illusion of control" and overestimate the degree of contingency, while dysphoric participants will be relatively accurate in their ratings of control, consistent with prior research using the judgment of contingency task (JOCT; Alloy & Abramson, 1979; Msetfi et al., 2005; Murphy et al., 2005). However, we predict that dysphoric participants' Time 2 ratings will illustrate a negative bias, consistent with predictions from Beck's theory (1967, 1987), while

nondysphoric participants' ratings will remain largely unchanged. Memory bias is defined as changes in the participants' judgments of contingency from Time 1 to Time 2. Insofar as memory bias is defined as memory at one time relates to another time, our assessment of it cannot be said to be longitudinal in nature, as both time points constitute one variable.

In the first aim we predict that dysphoric participants will be less objective in making causal attributions than their nondysphoric counterparts. However, for our second aim, we predict a depressive realism effect. While these two predictions may seem contradictory, we posit that differences in the degree to which each task taps into memory for past events can explain this divergence in expectations. Determining the cause of events is a process that occurs daily, while the judgment of contingency task less closely approximates experiences outside of the lab. As a result, we would expect the assessment of causal attributions to more readily tap into long-term memory and therefore, to illustrate the negatively-valenced recall characteristic of depressed individuals. Making attributional style ratings for the statement "I failed a test today because I'm a failure as a person" is likely to provoke our own memories of similar experiences. Insofar as the judgment of contingency task is less likely to elicit such memories, we would predict it to be less influenced by this bias.

We predict that the expected negatively-biased recall in dysphoric participants will be mediated by both a tendency to ruminate in general, and, particularly, to specific ruminations about the participants' prior performance on the JOCT at Time 1. Participants will be administered a self-report questionnaire assessing their tendency to ruminate generally (the Response Styles Questionnaire; Nolen-Hoeksema & Morrow, 1991) at both Time 1 and Time 2. Participants' ratings from both time points will be averaged to obtain a more stable estimate of their tendency to ruminate, overall. In addition, participants will be administered a questionnaire developed specifically for the current investigation assessing the occurrence of repetitive thoughts

about the experiment in the two weeks between assessments (which will only be administered at Time 2). As shown by Hertel and others (see Hertel, 2000, 2004 for reviews), we predict that a general tendency to ruminate will disrupt effortful memory search processes and result in negatively-biased recall. We also predict that depressed participants' rehearsal of negative-focused thinking about their performance on the JOCT (i.e. ruminating specifically about their performance) will result in more elaborative processing of the negative aspects of their performance, and therefore, more negative recall at Time 2.

Hypotheses

Aim 1: Replication and Extention of Prior Work

1. Consistent with the results of Moore and Fresco (2007), the attributional style of dysphoric participants will be less realistic as compared to nondysphoric participants. Specifically, dysphoric participants' attributional style will be pessimistically biased whereas nondysphoric participants' attributional style will be optimistically biased.
2. Consistent with prior research (Alloy & Abramson, 1979; Msetfi et al., 2005; Murphy et al., 2005), participants' performance on the JOCT at Time 1 will evidence a depressive realism effect. Specifically, dysphoric participants' judgments of contingency will be closer than the judgments of nondysphoric participants to the degree of contingency pre-determined by the experimenter (either 0% or 75%). Nondysphoric participants' ratings will overestimate the degree of contingency, illustrating the "illusion of control" noted in this prior research.

Aim 2: Examination of the Relationship Between Biases in Memory and Depressive Realism

3. When asked to recall their prior performance on the JOCT two weeks later, dysphoric participants will underestimate the actual contingency to a greater degree than they did

initially at Time 1, illustrating negatively-biased memory recall. Nondysphoric participants' ratings will remain relatively unchanged during this two-week interval.

4. This differential underestimation of the contingency in the JOCT, which is predicted to affect dysphoric participants only, will be mediated by both a general tendency to ruminate as well as specific ruminative thoughts about performance on the JOCT during the two-week, inter-assessment interval.

METHOD

Participants and Procedure

Participants were recruited from Introduction to Psychology courses at a large Midwestern university and compensated with course credit and were screened with the Beck Depression Inventory - Second Edition (BDI-II; Beck, Steer, & Brown, 1996). This measure was administered during the university's mass testing procedure to an average of 361 potential participants per semester for each of four semesters. The average BDI-II score across the four semesters was 9.36 (*SD* = 9.27) and these scores ranged from 0 to 57. Participants with the highest BDI-II total scores in the screening sample for that semester, who also had a score of 14 or higher, were contacted to be included in the dysphoric group. Participants with the lowest BDI-II total scores, who also had scores of 13 or lower, were contacted to be included in the nondysphoric group. These criteria were used according to research which illustrated that they maximized both the sensitivity and specificity when compared to diagnoses of depression assessed via structured interview (Beck, et al., 1996). An average of 49 individuals per semester were determined to meet the eligibility criteria outlined above and asked to participate and 26 participants per semester consented to participate. The current study consisted of an initial assessment (Time 1: *n* = 105) and an assessment two weeks later (Time 2: *n* = 82). A two-week

delay was chosen in an attempt to build a certain degree of ambiguity into the recall task. First, it was thought that with a one-week delay, recall would be relatively easy, the task would therefore not be ambiguous to any significant degree, and would not significantly tax the memory capacity of the participants. Secondly, it was thought that with a three-week to month-long delay, so few participants would incidentally recall their performance that the task could not be said to be truly tapping memory performance at all. In addition, the use of a two- versus three-week interval would also serve to marginally decrease attrition from the Time 1 to Time 2 assessment. Therefore, it was decided that a two-week delay would best address these competing concerns. This project was approved by Kent State University's Institutional Review Board (#7-42).

Power Analysis

For Hypothesis 1, mean differences in the realism of the attributional style of dysphoric and nondysphoric participants were compared using a one-way ANOVA. A minimum of 128 participants were needed to have sufficient power (.80) to detect a medium effect (Cohen's [1988] $d = .50$). Examination of bias (pessimistic or optimistic) utilized 2 one-sample *t*-tests, one per group (dysphoric and nondysphoric). A sample of 100 participants would provide sufficient power to detect a medium effect, while a sample of 128 would provide sufficient power to detect a near-medium effect ($f = .22$). The current sample of 105 participants at Time 1 and 82 participants at Time 2 resulted in a slight lack of power. As a result, effect sizes were reported to provide a sample-size-free indication of the effects of our independent variables.

For Hypothesis 2, mean differences in the accuracy of dysphoric and nondysphoric participants' contingency judgments were compared using a one-way ANOVA. Similar to Hypothesis 1, a minimum of 128 participants was needed to have sufficient power to detect a medium effect. Examination of under- versus overestimation of contingency (an "illusion of

control") utilized 2 one-sample *t*-tests, one *t*-test per group (dysphoric and nondysphoric). As with Hypothesis 1, a sample of 100 participants provided sufficient power to detect a medium effect, while a sample of 128 provided sufficient power to detect a near-medium effect ($f = .22$). As with Hypothesis 1 above, we were slightly underpowered with Hypothesis 2. As a result, effect size statistics have been reported.

For Hypothesis 3, mean differences in memory recall between dysphoric and nondysphoric participants were compared using a one-way ANOVA. Two one-sample *t*-tests were used to examine the valence of bias (contingency judgments that become increasingly negative or positive over the 2-week interval) in both groups. As with Hypothesis 1 and 2 above, we were slightly underpowered with Hypothesis 3. As a result, effect size statistics have been reported.

For Hypothesis 4, a minimum of 68 participants was needed to have sufficient power to detect a medium effect ($f^2 = .15$) for the results of Sobel's (1982) test for mediation with 2 predictors.

Measures

The *Attributional Style Questionnaire* (ASQ; Peterson et al., 1982; Seligman et al., 1979) is a self-report inventory that assesses causal attributions for six hypothetical, positive and six hypothetical, negative events along the dimensions of stability and globality that are rated on a one to seven scale. Higher ratings represent more depressogenic responses and more stable and global causes while lower ratings represent more unstable and specific causes. A generality score is then computed by averaging the values of the twelve stability and globality items across negative events to produce a score that ranges from one to seven. An adequate degree of internal consistency was found for this measure in the current investigation at both the Time 1 ($\alpha = .78$) and Time 2 assessments ($\alpha = .76$).

The *Beck Depression Inventory - Second Edition* (BDI-II; Beck et al., 1996) is a 21-item self-report instrument that broadly assesses the symptoms of depression including the affective, cognitive, behavioral, somatic, and motivational components as well as suicidal wishes. Beck, Steer and Brown (1996) reported a high internal consistency in a university population (α = .93). Practically identical values to those obtained in Beck, Steer, and Brown (1996) were obtained in the current investigation at both the Time 1 (α = .94) and Time 2 assessments (α = .95). Beck, Steer, and Brown (1996) also found the BDI-II to possess adequate test-retest reliability and convergent validity with other measures of symptoms of depression.

The *Content Analysis of Verbatim Explanations* (CAVE; Peterson et al., 1992) is a procedure by which statements describing an actual event and its cause can be extracted from everyday speech and then independently rated on the dimensions of stability and globality (similar to the ASQ) by extensively-trained raters. This degree of training is particularly significant as it ensures that our raters were objective, allow us to state confidently whether a participant's perception of the CAVE stimuli is more or less objective, and limit the plausibility that rater bias may have accounted for the results obtained.

For the current study, pre-extracted and pre-rated cause-event statements were provided to participants, who were then asked to rate them without knowledge of the prior, independent ratings. Participants, therefore, made ratings on the same cause-event statements as the trained raters. Participants were presented six positive events and six negative events (12 total) and a generality score was computed by averaging the stable and global items for negative events, similar to that used with the ASQ. The causes were selected to represent a broad array of differing attributional styles, with depressogenic causes, neutral causes, and nondepressogenic causes being selected. The events were also sampled broadly and represented equal numbers of two

general content areas: events dealing with interpersonal matters ("I broke-up with my boyfriend/girlfriend") or achievement related matters ("I succeeded on a Math test").

The event-attribution statements were obtained from a daily diary study of attributional style conducted by the second author that asked participants to record the best and worst events of the day and the cause of those events (Fresco, Moore, & Craighead, 2006). It should be noted that the statements refer to real events, and the actual attributions to those events, that were generated by individuals similar to the current study participants (i.e. in age, level of education, race). By providing statements that the individual has no prior history with, the likelihood that the participant is responding to the statement itself, and not to memories of events closely associated to the cause or the event, is increased. Therefore, we see the use of experimenter-provided assessment materials as necessary to the internal validity of the study. CAVE raters were trained by first having them read Peterson's CAVE scoring manual (Peterson et al., 1992) and practice-rating 100 hypothetical event-attribution statements. Inter-rater agreement was also calculated for 3 raters, from a random sample of 86 extractions, which was taken from a pool of over 6,000 extractions collected as part of the aforementioned daily diary study. The extractions used in the current study were drawn from this pool and rated by the same 3 raters. An acceptable degree of inter-rater agreement was reported in a previous investigation ($\alpha = .82$, Moore & Fresco, 2007), as predicted. Insfoar as the same stimuli (cause-event statements) participants rated in Moore and Fresco (2007) was utilized in the current investigation, we did not have a new set of objective coders re-rate them. As a result, the inter-rater reliability value determined in Moore & Fresco (2007) is valid here. This finding is significant as a high degree of inter-rater agreement lends credibility to our claim that our raters were objective. Moore & Fresco (2007) also found that the CAVE materials possessed an adequate degree of internal consistency ($\alpha = .83$). While the values

for the current investigation are somewhat lower (Time 1: $\alpha = .71$; Time 2: $\alpha = .72$), they are still adequate for a scale used in research that is still under development (Hunsley & Mash, 2007).

In the current investigation, the participants' subjectively, self-rated CAVE materials for negative events (CAVE-Self) were compared to the objective ratings for negative events obtained previously by computing a difference score where the CAVE-Self ratings were subtracted from the objective ratings. In this manner a score of zero indicates perfect objectivity, increasingly positive scores (up to a maximum score of +7) indicate an optimistic (nondepressogenic) bias, and increasingly negative scores (up to a maximum of -6) indicate pessimistic (depressogenic) bias. This methodology was used in prior research (Moore & Fresco, 2007) examining the objectivity in the attributional style of both those at risk for depression and those currently suffering from dysphoria.

The *Judgment of Contingency Task* (JOCT; Alloy & Abramson, 1979) asks participants to press a button, which results in the illumination of a green light a predetermined percentage of the time. The dependent variable is the difference between the participant-rated and predetermined contingency between pressing the button and the illumination of the light. Using this difference score methodology, increasingly negative scores indicate participants' under-estimating the degree of contingency, while increasingly positive scores indicate participants overestimating this contingency, and illustrating an "illusion of control". A score of zero would indicate perfectly objective estimation of contingency/control. In these studies nondepressed individuals experienced an "illusion of control", where they consistently overestimated their degree of control over the outcome (Alloy, Abramson, & Kossman, 1985; Alloy, Abramson, & Viscusi, 1981; Musson & Alloy, 1987; Vazquez, 1987). Depressed individuals experienced no such bias. In the proposed study, the experimenter-determined contingency between a button press and the illumination of the light was varied, to determine the influence of this variable on

our results. Participants were randomly assigned to either a 0% or 75% contingency condition. The base rate of light onset in the 0% condition was relatively high (75%, regardless of whether a button is pressed), consistent with prior research that illustrated that this condition was most likely to produce a depressive realism effect (Msetfi, Murphy, Simpson, & Kornbrot, 2005; Murphy et al., 2005).

The following instructions were adapted from Msetfi, Murphy, Simpson, & Kornbrot (2005). The JOCT was programmed using REALbasic (Version 3) software and individually administered on Macintosh computers. Participants were seated in a 4' x 4' cubicle approximately 2' from the computer screen, the resolution of which was set at 800 x 600 pixels. Participants were first given instructions on the task requirements that were displayed on the computer screen. Specifically, they were instructed on the necessity of pressing the button on some occasions but not pressing the button on an approximately equal number of occasions. Each trial was constructed such that there was a 3 s. opportunity for the participants to make their response by pressing the space bar. This period was signaled by an on-screen message saying "You may press the button now!". This period was followed by a 2 s. period in which the lightbulb graphic either switched on or remained off. Each trial was followed by a 15 s. inter-trial interval, during which the unlit lightbulb remained on the screen. Judgments of contingency were made after 40 trials had been completed.

The *Recall of Performance Scale* was created to assess participants' recall of their performance on the JOCT after a two week delay. In addition to asking participants to re-rate the contingency between pressing the button and the illumination of a light they were asked the degree of certainty that they have in their recall and three questions assessing the accuracy of their memory of the room in which they were administered the JOCT at Time 1 ("Is this the same room where you were asked to perform the computerized task two weeks ago?", "Is this the same

cubicle where you filled out the questionnaires two weeks ago?", and "Was the research assistant the same, both then and now?").

The *Response Styles Questionnaire* (RSQ; Nolen-Hoeksema & Morrow, 1991) is a 71-item self-report questionnaire that assesses four different methods of coping with negative mood. Only the 25-item Ruminative Response Scale (RRS) will be utilized in the proposed investigation. Insofar as traditional scoring and use of the RRS has been shown to be confounded with the presence of depressive symptoms (Roberts, Gilboa, & Gotlib, 1998), two factor-analytically derived subscales ("Brooding" and "Pondering") will be utilized as an alternative scoring method. Both subscales have been shown to be uncontaminated by depressive symptom content, as well as possess discriminant validity. Brooding and pondering have been shown to differentially correlate with the presence of depressive symptoms, symptoms of GAD, and deficits in emotion regulation (Fresco, Armey, Turk, Mennin, & Heimberg, 2003; Fresco et al., 2004; Treynor, Gonzalez, & Nolen-Hoeksema, 2003). These factors have been shown to have adequate internal consistency (Brooding: α = .80 - .90, Pondering: α = .70 - .90; Fresco et al., 2004; Treynor et al., 2003), as well as adequate test-retest reliability with an undergraduate population (Fairchild & Fresco, 2006: r = .69; Treynor, et al., 2003: r = .67). The internal consistency values found in the current investigation for both Brooding (α = .88 for both the Time 1 and Time 2 assessments) and Pondering (α = .83 for both the Time 1 and Time 2 assessments) all fall within the ranges determined in these prior investigations.

Participants were also asked to rate 10 statements reflecting how often they thought about their performance on the JOCT between the Time 1 and Time 2 assessments ("Thought about how well I did on the computerized task") and aspects of the study, in general ("Thought about what the questionnaires might be evaluating"). These questions were asked on the same scale as

the RSQ and the instructions were only slightly modified to ensure the participant understood that the questions concerned their thoughts over the previous 2 weeks.

RESULTS

Analyses were undertaken to determine if the participant selection procedure based upon BDI-II total scores outlined above resulted in dysphoric and nondysphoric groups that were meaningfully different from one another. The results of two one-way ANOVA's indicated that these groups were significantly different from one another at both the Time 1 (F[1, 103] = 240.54, $p < .001$) and Time 2 assessments (F[1, 81] = 171.87, $p < .001$). At both assessments, the dysphoric group mean BDI-II total score (Time 1 = 25.47, Time 2 = 23.90) exceeded that of the nondysphoric group (Time 1 = 6.08, Time 2 = 4.76). In addition, both the Time 1 (Cohen's d = 3.06) and Time 2 results (d = 2.91) exceeded Cohen's (1988) convention for a large effect.

The dysphoric and nondysphoric groups were compared on a number of different demographic variables for descriptive purposes. Consistent with recent epidemiological data (Kessler et al., 2003), the female:male ratio in the dysphoric group was significantly higher (3.30:1 vs. 1.48:1; χ^2 [1, N = 105] = 3.33, p = .07), albeit only at a trend level. Both groups were predominantly Caucasian (dysphoric = 88.71%, nondysphoric = 88.37%), with little representation from other ethnic groups, and were roughly equivalent in this regard (χ^2 [4, N = 105] = 5.86, p = .21). The two groups did not differ in average age (dysphoric mean age = 18.91, nondysphoric mean age = 19.23, F[1, 101] = .30, p = .59) and both group were largely composed of individuals with high socio-economic status. The modal combined family income for both groups was $100,000 or more (comprising 20.00% of the dysphoric and 34.15% of the nondysphoric group) and the distribution of the various income categories did not differ between the two groups (χ^2 [10, N = 101] = 13.35, p = .21). Father's level of education (χ^2 [7, N = 105] =

13.37, $p = .06$), but not mother's (χ^2 [8, $N = 105$] = 10.37, $p = .24$), differentiated dysphoric from nondysphoric participants. A high school diploma represented the modal education level of the fathers of nondysphoric (representing 33.87% of the distribution), but not dysphoric participants, who fathers were most likely to have a four-year college degree (representing 27.91% of the distribution). With regards to mother's level of education, a high school diploma represented the modal value for nondysphoric participants (representing 38.71% of the distribution), whereas the distribution for dysphoric participants was more evenly distributed across all categories.

Replication of Moore & Fresco (2007)

Hypothesis #1 posited that dysphoric participants' attributional style would be pessimistically biased whereas the attributional style of nondysphoric participants would be optimistically biased. To evaluate this hypothesis, both dysphoric and nondysphoric participants' responses to the CAVE task were compared using a one-way ANOVA. Preliminary data analysis indicated that although the CAVE difference scores at Time 2 were approximately normally distributed (Shapiro-Wilk W [$df = 85$] = .98, $p = .42$), CAVE difference scores at Time 1 deviated significantly from normality (W [df = 83] = .97, $p = .02$). Insofar as normality is an assumption of the parametric statistical techniques we employed below (Howell, 2006), we attempted to normalize our data via identification and elimination of the outliers that we identified via preliminary data inspection. Outliers were identified based on an inspection of the distribution of the Time 1 CAVE difference scores. Three scores, representing: 1) a second modality that was also 2) more than 3 standard deviations ($SD = .71$) above the mean ($M = -.31$) and the top 2.86% of the distribution, were identified as outliers and removed from further analysis. This combined theory-driven and actuarial approach was utilized to prevent the removal of participants whose data meaningfully demonstrated the effect in question, albeit to an extreme extent. Also, we

expected the existence of a small, relatively homogenous subgroup of participants at the extremes that represented participants who did not respond to the content of the items (i.e. in a fixed manner or as a result of not attending properly to the stimulus).

After removal of outliers[2], both the Time 1 (W [df = 80] = .99, p = .52) and Time 2 CAVE difference scores (W [df = 80] = .98, p = .40) were approximately normally distributed. Insofar as the ANOVA assumes that the variances of all levels of any independent variables in question be approximately equal (Howell, 2006), this assumption was evaluated using Levene's Test (Levene, 1960). The result of this test found that the variances of both dysphoric and nondysphoric participants were approximately equal (Time 1: F[1, 100] = 7.51, p = .87; Time 2: F[1, 81] = .35, p = .56). Comparison of the degree to which dysphoric and nondysphoric participants' ratings of CAVE materials corresponded to trainer raters indicated that the ratings of nondysphoric participants differed at both Time 1 (F[1, 100] = 7.51, p = .007) and Time 2 (F[1, 81) = 8.30, p = .005; see Table 1 for a complete listing of descriptive statistics). The finding for Time 1 (d = .55) correspond to the convention for a medium effect while the Time 2 result (d = .64) fell between a medium and large effect. Consistent with Hypothesis #1, inspection of

Table 1
Comparison of Attributional Bias Between Dysphoric and Nondysphoric Participants

	Dysphoric *M* (*SD*)	Nondysphoric *M* (*SD*)
T1 CAVE Diff Score	-.56 (.61)	-.23 (.60)
T2 CAVE Diff Score	-.50 (.63)	-.05 (.72)

Note: T1 CAVE Diff Score = CAVE difference score at Time 1; T2 CAVE Diff Score = CAVE difference score at Time 2.

means for both groups indicated that the scores of nondysphoric participants were closer to zero and, therefore, more objective.

A series of four one-sample *t*-tests were run to determine if the Time 1 and 2 CAVE difference scores differed significantly from a score of zero (indicating perfect objectivity) for both dysphoric and nondysphoric participants. For nondysphoric participants, results indicated that their scores at both Time 1 ($t[61] = -1.40$, $p = .17$) and Time 2 ($t[50] = -.55$, $p = .59$) did not differ significantly from perfect objectivity. The finding for Time 1 fell between a small and medium effect ($d = .36$) and the finding for Time 2 corresponded to a small effect ($d = .16$). For dysphoric participants, however, results indicated that their scores at both Time 1 ($t[42] = -6.07$, $p < .001$) and Time 2 ($t[31] = -4.51$, $p < .001$) *did* differ significantly from perfect objectivity, with both of these findings exceeding the conventions for a large effect ($d = 1.87$ and 1.62, respectively). This latter finding supported our prediction that dysphoric participants would evidence a pessimistic attributional bias. However, the former finding, that nondysphoric participants evidenced no significant attributional bias, did not conform to our expectations and failed to replicate the results of a prior investigation (Moore & Fresco, 2007).

Replication of Msetfi, Murphy, Simpson, & Kornbrot (2005) and Murphy et al. (2005)

Hypothesis #2 posited that dysphoric participants' judgments of contingency would more closely approximate the contingency level predetermined by the experimenter on the JOCT (thereby producing a difference score close to zero) than nondysphoric individuals. Preliminary examination of the distribution of JOCT difference scores illustrated significant deviation from normality ($W[df = 105] = .96$, $p = .003$) due to the presence of outliers. A combined statistical and theory-driven, a priori approach was again employed to identify these outliers. Participants ($n = 18$) who fell into a second modality above (+76 or more) or below (-28 or less) 2 *SD*'s from the mean on the JOCT difference score were dropped from analysis. In addition, participants ($n = 36$) who fell either above (38 or more) or below (18 or less) one *SD* in number of button presses on

the JOCT were also dropped. A total of 43 participants were dropped (given the degree of overlap between difference score outliers versus button press outliers, this number is not a simple total). A more stringent criterion was used for this latter indicator of compliance with instructions, given: 1) the distribution of button presses demonstrated less variability and 2) participants were explicitly instructed to attempt to button press on approximately half (20) of the JOCT trials. Exploratory analyses were conducted to determine the degree of overlap between participants 2 or more *SD*'s from the JOCT difference score mean and participants deemed noncompliant. Considerable overlap was found between these two dichotomous categories (χ^2 [1, N = 105] = 6.94, p = .008) and 69.5% of participants were classified correctly as either non-outliers on both measures OR outliers on both measures. This degree of overlap indicates that, while the two categories are not identical, there is considerable overlap between them.

After removal of outlier/noncompliant participants[2], the distribution of JOCT difference scores approximated normality to the extent that the use of normal-theory parametric statistics was appropriate (W [df = 62] = .98, p = .24). The results of a Levene's Test (F[3, 58] = .38, p = .77) also indicated that the equality of variances assumption in ANOVA was met. Consistent with expectations, a main effect of level of contingency was found (F[1, 58] = 19.01, p < .001; see Table 2 below for a complete listing of descriptive statistics) indicating that participants were

Table 2
Comparison of Depressive Realism and Memory Bias Between Dysphoric and Nondysphoric Participants

	Total		Dysphoric			Nondysphoric		
Contingency	0%	75%	0%	75%	Total	0%	75%	Total
Dep Real	17.73 (18.25)	-7.38 (15.51)	17.80 (18.09)	-13.11 (15.80)	11.18 (21.62)	17.68 (18.71)	-1.64 (14.17)	14.27 (19.31)
Mem Bias	3.65 (11.63)	-10.25 (8.61)	4.50 (11.79)	-8.00 (11.69)	2.23 (12.50)	2.95 (11.73)	-12.50 (4.80)	.58 (12.28)

(T1)								
Mem Bias (Change)	4.27 (11.15)	-10.71 (9.20)	5.6 (10.22)	-8.33 (14.29)	3.42 (11.71)	3.24 (11.94)	-12.5 (4.80)	.72 (12.51)

Note: Dep Real = JOCT difference score; Mem Bias (T1) = Memory bias difference score comparing dysphoria groups defined via Time 1 BDI-II scores; Mem Bias (Change) = Memory bias difference score comparing dysphoria groups defined via stability between Time 1 and Time 2 BDI-II scores.

sensitive to whether the predetermined contingency on the JOCT was set at 0% or 75%. This finding (d = 1.15) exceeded Cohen's convention for a large effect, indicating that the degree of differentiation roughly corresponded to the large difference in degree of contingency between the 2 conditions. However, counter to the predictions of Hypothesis #2, a main effect for level of dysphoria (F[1, 58] = .97, p = .33) and a dysphoria by contingency condition interaction (F[1, 58] = 1.01, p = .32; see Table 2 for a complete listing of descriptive statistics) were not found. These findings indicated that dysphoric and nondysphoric participants were roughly equivalent in their ability to judge the predetermined contingency on the JOCT and that this result was found equally across both 0% and 75% contingency conditions. The findings for both the main effect and interaction corresponded to Cohen's convention for a small effect (both d = .26).

Differential Attrition

Prior to the evaluation of Hypothesis #3, which involved the incorporation of both Time 1 and Time 2 data, analyses were performed to attempt to identify differential attrition. Overall, 21.9% (n = 23) of Time 1 participants did not return 2 weeks later for their second assessment, leaving a final Time 2 sample of 82 participants. Comparison of attritors and completers on a variety of categorical variables of interest yielded no difference in rates of attrition between groups (Dysphoria: χ^2 [1, N = 105] = .03, p = .22; Contingency Condition: χ^2 [1, N = 105] = .24,

p = .62; Outlier Status: χ^2 [1, N = 105] = 1.48, p = .22; Compliance Status: χ^2 [1, N = 105] = .003, p = .96; Gender: χ^2 [1, N = 105] = .03, p = .87; Race: χ^2 [4, N = 105] = 4.93, p = .29; Income: χ^2 [10, N = 105] = 9.64, p = .47; Father's Level of Education: χ^2 [7, N = 105] = 9.92, p = .19; Mother's Level of Education: χ^2 [8, N = 105] = 13.55, p = .09; Use of Psychotropic Medication: χ^2 [1, N = 105] = 2.35, p = .13) using either a traditional p-value (.05), or with the more stringent cut-off suggested by Bonferroni correction (.005). Examination of continuous variables of interest also indicated no differences between attritors and completers (Age: F[1, 101] = .07, p = .80; CAVE Difference Score: F[1, 103] = .10, p = .76; JOCT Difference Score: F[1, 103] = .001, p = .97; Degree of Memory Bias: F[1, 80] = .04, p = .84; BDI: F[1, 103] = .42, p = .52; ASQ Generality: F[1, 103] = 1.18, p = .28; MASQ-GDA: F[1, 102] = .14, p = .71; MASQ-GDD: F[1, 102] = .16, p = .69; MASQ-AA: F[1, 102] = .30, p = .59; MASQ-AD: F[1, 102] = .72, p = .40; RSQ-Brooding: F[1, 103] = .003, p = .96; RSQ-Pondering: F[1, 103] = .16, p = .69), regardless of whether or not a Bonferroni correction was employed (p = .05 vs. .004). In addition, all of the aforementioned results fell at or below Cohen's convention for a small effect (d = .01 - .21). Taken together, these results indicate that it is extremely unlikely that differential attrition affected the results related to Hypothesis #3 to a significant extent.

Identification of Memory Bias Using the JOCT

Hypothesis #3 posited that, at Time 2, dysphoric participants would recall their Time 1 performance on the JOCT more negatively than the evaluation they made at Time 1 as part of the JOCT. To evaluate this hypothesis, participants' responses to the JOCT were compared to their responses to the ROPS, where the latter was subtracted from the former to form a recall bias difference score. Using this methodology, negative scores indicate a negative memory bias (where the perception of contingency is found to decrease over time), positive scores indicate a

positive bias, and scores approaching zero indicate a lack of memory bias. Similar to Hypothesis #2, outliers identified as being 2 or more *SD*'s from the mean on this memory bias difference score or as noncompliant with the instructions administered during the JOCT were removed from analyses. After removal of outlier/noncompliant participants[2], the distribution of memory bias difference scores approximated normality (W [df = 48] = .95, p = .05), however barely. The results of a Levene's Test (F[3, 44] = .61, p = .61) indicated that the equality of variances assumption in ANOVA was also met. Consistent with expectations, a main effect of level of contingency was found (F[1, 44] = 9.98, p = .003; see Table 2 for a complete listing of descriptive statistics) and this finding (d = .95) exceeded Cohen's convention for a large effect. However, counter to Hypothesis #3, participants identified as dysphoric and nondysphoric at Time 1 did not differ in how they recalled their Time 1 performance on the JOCT (F[1, 44] = .50, p = .50) and this relationship was exemplified equally in both contingency conditions (dysphoria x contingency: F[1, 44] = .11, p = .74) . Both of these findings (d = .21 & .10, respectively) fell below Cohen's convention for a small effect.

It is possible that the aforementioned findings resulted from individuals whose dysphoria status changed from Time 1 to Time 2, resulting in heterogeneous groups and increased error variance. To address this possibility, participants were reclassified into 4 groups: participants whose mood status was stable and dysphoric from Time 1 to Time 2 (n = 19), participants whose mood status was stable and nondysphoric (n = 25), participants whose mood changed from dysphoric to nondysphoric from Time 1 to Time 2 (n = 3), and participants whose mood changed from nondysphoric to dysphoric (n = 1). Given the small number of participants in the 2 unstable mood groups, these participants were dropped from analyses, leaving only participants whose mood was stable during the 2 weeks between Time 1 and Time 2. Once again, a main effect of level of contingency was found (F[1, 40] = 10.38, p = .003; see Table 2 for a complete listing of

descriptive statistics) and this finding (d = 1.02) exceeded Cohen's convention for a large effect. Despite these more homogenous groups, and counter to Hypothesis #3, dysphoria status did not differentiate how participants recalled their Time 1 performance on the JOCT (F[1, 40] = .51, p = .48) and this relationship was exemplified equally in both contingency conditions (dysphoria x contingency: F[1, 40] = .04, p = .85). Once again, both of these findings (d = .23 & .06, respectively) fell below Cohen's convention for a small effect.

The dichotomization of level of dysphoria from the dimensional BDI-II was undertaken to make our results comparable to the rest of the depressive realism literature (Moore & Fresco, 2009). However, it is possible that this artificial dichotomization created levels of our independent variable (dysphoria) that were heterogeneous as a result of this process. This heterogeneity could have resulted in the large error variances observed in the aforementioned findings. To evaluate this possibility, we attempted to address Hypothesis #3 by utilizing a dimensional measure of dysphoria (via the BDI-II) and a regression-based, quantitative methodology to detect a main effect of dysphoria on memory for Time 1 JOCT performance. This analysis was also chosen as it was necessary (Baron & Kenny, 1986) to evaluate to Hypothesis #4, which attempted to identify mediators of this main effect. BDI-II scores at both Time 1 and Time 2 were entered simultaneously because there was no a priori reason to suspect that either measure would contribute more variance to the prediction of memory bias. In addition, hierarchical entry would result in a residual change predictor score which did not accurately represent our construct of interest (level of dysphoria, as opposed to a difference between observed values and those predicted by Time 1 or Time 2 BDI-II scores). Results of this regression analysis indicated that these 2 predictors did not account for a significant amount of variance in the criterion (R^2 = .02, F[2, 79] = .63, p = .54) and neither the Time 1 BDI-II score (β = .22, t[80] = .94, p = .35), nor the Time 2 BDI-II score (β = -.12, t[80] = -.53, p = .60) served as

a significant predictor by itself. Both the results for the Time 1 (Cohen's [1988] $f^2 = .01$) and Time 2 BDI-II scores ($f^2 = .004$) fell below Cohen's convention for a small effect. Given the lack of support for Hypothesis #3 and the lack of a main effect to mediate, evaluation of Hypothesis #4 was abandoned.

DISCUSSION

The current investigation had several objectives. The first objective was to replicate the work of Moore & Fresco (2007). In support of this objective, the findings were consistent with Beck's conceptualization of cognition in depression such that dysphoric participants were negatively biased in their attributions whereas nondysphoric participants did not evidence any such bias. This later finding is not consistent with Moore and Fresco (2007), who found a significant positive bias in the attributions of nondysphoric participants.

The differences in results between the present investigation and that of Moore and Fresco (2007) address the distinction between relative versus absolute bias in depressive realism. Traditional theory on depressive realism (Alloy & Abramson, 1979) discusses bias in absolute terms: nondepressed individuals possess bias and depressed individuals do not. However, other researchers have posited a version of depressive realism that makes less strong claims regarding bias (Craighead, Hickey, & DeMonbreun, 1979; DeMonbreun & Craighead, 1977; Dykman, Abramson, Alloy, & Hartlage, 1989; Gotlib, 1983; Vestre & Caulfield, 1986). In this "weaker" depressive realism phenomena, both the perceptions of depressed and nondepressed individuals are biased, but it is the extent of this bias that is greater in nondepressed individuals. Both Moore and Fresco (2007), which found a greater degree of bias in depressed than nondepressed participants, and the current investigation are incompatible with both versions of depressive realism, albeit to differing extents. Moore and Fresco (2007) found support for the concept of

relative bias. However, the pattern of that bias was inconsistent with predictions from depressive realism. The current study, on the other hand, supports a more absolute view of bias.

The current investigation also attempted to replicate the work of Msetfi, Murphy, Simpson, & Kornbrot (2005) and Murphy et al. (2005), who demonstrated a depressive realism effect using a 0% contingency, high density version of the JOCT. Counter to our expectations, the accuracy of the contingency ratings of dysphoric and nondysphoric participants were not statistically different from one another. We also failed to demonstrate such an effect using a 75% contingency condition, illustrating that the lack of a depressive realism effect was not confined to a singular characteristic of our stimulus. These null findings did not appear to result from participant non-compliance as the findings did not change after dropping participants who were not compliant with the instructions for the JOCT. This preliminary data cleaning was undertaken to address the possibility that not all of the participants adequately read or understood the instructions of the JOCT.

Finally, the current investigation attempted to apply a well-established finding in the depression literature, that depressed individuals preferentially recall negatively-valenced material (Bradley & Matthews, 1983; Derry & Kuiper, 1981; McDowall, 1984; Dennard & Hokanson, 1986; Murray, Whitehouse, & Alloy, 1999), to the question of depressive realism. Counter to expectations, however, we found that the accuracy of the memories of dysphoric and nondysphoric participants were not statistically different (in regards to the recall of their performance on the JOCT two weeks prior). As with the depressive realism results mentioned above, these findings did not change once noncompliant participants were dropped from the analyses.

Limitations

Our inability to replicate prior JOCT findings and the well-established effect of negatively-biased recall in depressed individuals highlights the potential for flaws in our research design. Descriptive data for the Time 1 judgment of contingency scores indicate a large degree of variability in participant responses to the JOCT (0%: *M* = 22.48, *SD* = 22.09; 75%: *M* = 54.44, *SD* = 27.88) that is consistent across groups (see Table 2) and approximately twice the variability found in prior investigations using the JOCT (Msetfi, Murphy, Simpson, & Kornbrot, 2005 & Murphy et al., 2005). These data are consistent with, although certainly does not guarantee, that our participants were responding randomly to the JOCT. It does not appear that this random responding was a result of participants having too little information to make valid contingency judgments, as participants who either button-pressed too much or too little (and were noncompliant with the instructions of the JOCT) were excluded. The contingency between button press and light onset is a function of what happens when the button is pressed (does the light turn on?) and what happens when it is not pressed (does the light turn on despite the lack of a button press?). Without information on button presses and non-presses, contingency judgments are not valid. However, it is possible that most participants understood that they needed to button press on approximately 20 trials (as they were explicitly told to do so), but did not understand how doing so ensured a more valid judgment of contingency. If the participants did not understand that they needed to incorporate information from both button presses and non-presses, but pressed on approximately 20 trials, they would not be captured by our crude measure of validity. Unfortunately, we did not utilize a measure of the participants' understanding of the contingency judgment itself, which would have allowed this type of invalid response to be detected and its influence removed from further analyses. The possibility of random responding at the Time 1 JOCT would explain our inability to replicate a depressive realism effect at Time 1 and could explain the lack of recall bias findings, as well.

Research evidence suggests that the memories of depressed individuals are only negatively biased to the extent that rumination disrupts effortful recall strategies (see Hertel, 2000 & 2004 for reviews). In addition, research specifies that it may be the self-focus component of rumination that is most important in interrupting these effortful search strategies (Hertel & El-Messidi, 2006). One would expect that randomly-generated responses are less likely to be as personally relevant as responses involving forethought, deliberation, and effort. Given this lack of self-focused rumination and resultant lack of disruption of effortful search recall strategies, differences between dysphoric and nondysphoric participants would not be expected. Other aspects of the methods used could also have contributed to the largely null findings obtained. Using the JOCT, as opposed to naturally-occurring events, as the stimuli to assess for recall bias was preferable in its ability to provide relevance to the depressive realism literature. The JOCT provides its own objective standard of reality insofar as the experimenter can predetermine significant aspects of the to-be-perceived event. While naturally-occurring events would provide increased personal relevance, these events generally do not have an objective, "correct" interpretation, making identification of a depressive realism effect problematic. It is also possible that a two week delay was too long for an incidental recall task of this type and none of our participants could recall their Time 1 performance. Participants may have randomly responded because of the complete ambiguity of the memory task resulting in the large variability in memory responses we found (0% Contingency: M = 26.55, SD = 25.90; 75% Contingency: M = 56.81, SD = 22.39). The large variability in recall of JOCT responses, like the JOCT responses themselves, was consistent across groups (see Table 2).

Finally, diagnostic status was not assessed in the current investigation. The average degree of symptoms of depression in the dysphoric group placed them in the range of "mild depression" (Beck, et al., 1996). However, the dysphoric/nondysphoric distinctions in present sample can best

be thought of as analogs for depressed and nondepressed individuals. The use of an analog sample makes our sampling strategy compatible with the majority of research on depressive realism (Moore and Fresco, 2009). It also leaves the present study open to the critique that results obtained in an analog sample would not necessarily be obtained with a sample of individuals diagnosed with depression (Dobson & Franche, 1989; Haaga & Beck, 1995).

Clinical Implications

The current investigation represents the first replication of Moore and Fresco (2007). Taken together, both studies found that the ratings of attributions by dysphoric participants were more biased than those of nondysphoric participants and found that this bias was pessimistic in valence. These findings lend support to the traditional conceptualization of the depressed individual posited by Beck (1967, 1987), that the key deficit in depression is characterized by faulty information processing and misperception of daily events. In addition, they would be inconsistent with the view put forth by proponents of depressive realism (Alloy & Abramson, 1979), that the perceptions and information processing capabilities of the depressed are less biased than those of the nondepressed. The inability of certain individuals to realistically perceive the causes of events may help to explain the maintenance of mood disorders and their increasingly chronic and recurrent course in individuals lacking significant life stressors.

The current investigation may also have practical applications to the treatment of depression. Insofar as Beck (1987) characterizes depressed persons as lacking in the ability to objectively evaluate their environment, one of the primary goals of cognitive therapy of depression (Beck, Rush, Shaw, & Emery, 1979) is to teach depressed individuals to analytically monitor their own negative thoughts, independent of their current mood state, in such a way that they can identify and challenge the source of these thoughts. However, despite being one of the

most empirically supported treatments in the history of psychotherapy (Blackburn & Moorhead, 2001; DeRubeis & Crits-Cristoph, 1998) and one of the most efficacious treatments for depression (DeRubeis & Crits-Cristoph, 1998), it lacks a strong research base into the role increased realism may play in the success of treatment. Future study will involve using the novel method of assessing realism introduced here and evaluating if this tendency does indeed reliably covary with the alleviation of depressive symptoms in therapy, as predicted by Beck and his colleagues (1979).

Future Directions

In addition to direct investigation of the relevance of depressive realism to CBT treatment, the aforementioned limitations of the current study offer a wealth of future directions for research. Future research attempting to link the potential perceptual bias in depressive realism with the well-researched negative memory bias in depression (Bradley & Matthews, 1983; Derry & Kuiper, 1981; McDowall, 1984; Dennard & Hokanson, 1986; Murray, Whitehouse, & Alloy, 1999) would benefit from stimuli that are clearly personally-relevant. One paradigm that offers particular promise in this regard is the recall of feedback paradigm in depressive realism, where experimenter feedback on a task is compared with the participants' later recall of this feedback. Insofar as performance on the task in question is both salient and considered important for the population being sampled (e.g. an intelligence-related task for university students), this paradigm could provide both the objective standard of reality required to assess depressive realism and the personal relevance necessary to serve as a more accurate analog of cognitive performance outside of the laboratory. Participants' immediate perception of their performance could be compared to their recall of their performance at a later time in a manner similar to that of the current investigation. It would be important to ensure that this task was relatively simple with instructions

that were easy to understand to avoid the noncompliance problems that were also faced in the current study. A brief assessment of the participants understanding of these instructions would also ensure that, were comprehension problems to arise with the instructions, they could be more easily detected.

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FOOTNOTES

[1]Throughout the paper, the term "dysphoric" will be used to define individuals for whom symptoms of depression was assessed via self-report. Dysphoric is therefore differentiated from "depressed", which is a term reserved for individuals for whom a DSM-defined mood disorder was assessed via structured clinical interview.

[2]Identical results were obtained (with the exception of tests of normality) when outliers and noncompliant participants were included. These results are available from the first author upon request.

APPENDIX A

KENT STATE UNIVERSITY

DEPARTMENT OF PSYCHOLOGY
330-672-2166

Project Title: Memory and Mood Study

Project Investigator: Michael T. Moore

INTRODUCTION: You are being asked to participate as one of approximately 150 individuals in a research study examining how your emotions affect your memory performance. All participants are students of Kent State University at least 18 years of age, who represent a cross-section of students with respect to one's tendency to feel depressed.

YOUR PARTICIPATION: If you decide to take part in this study, one of the members of our research team will give you a packet of self-report questionnaires to answer in the lab. You will also be asked to complete a computerized task. Both the packet and the task will take you approximately 2 hours to complete. The measures in the packet inquire about how you might handle stressful life events, your memory about experiences in your past, and your current feelings. You are also eligible to return to the lab in two weeks and complete a similar battery of questionnaires that will also take approximately 2 hours.

BENEFITS: In return for your participation in this study, you will receive 4 points towards the fulfillment of course requirements in Introductory to Psychology. If you decide to participate in the session two weeks from now, you will receive an additional 4 points for a total of 8 points. Your participation in this study may also enable us to better understand how individuals deal with their stress, emotions, and unpleasant feelings.

RISKS: You may experience mild discomfort when asked to focus on topics that are potentially stressful or unpleasant, but our experience is that these effects are at most slight and last for a matter of minutes. If, however, you feel more than mild discomfort, we encourage you to contact the Kent State University Psychological Clinic (330-672-2372) or the University Psychological Services (330-672-2487). Alternatively, if you prefer, we will assist you with the referral. You are under no obligation to complete this study even if you sign this consent form. You may discontinue your participation at any time.

CONFIDENTIALITY: All data collected in this study remain strictly confidential within the limits of the law. For the purposes of the research, the information you provide to us will be identified only by a subject number and will be examined only by Michael Moore and qualified members of the research team. All data will remain in a locked file. After the study, data may be published in scientific journals, but data will never be published in any manner that can identify you.

VOLUNTARY PARTICIPATION: Your participation in this study is voluntary and you may decline to participate in it without any impact on your grade for the course. Should you choose to participate, you may voluntarily withdraw from it at any time, again without any penalty. This means that participating now does not in any way obligate you to participate in the session two weeks from now. By signing this form you are indicating that you have been informed about the research study in which you are agreeing to participate, and that you have had all of your questions satisfactorily answered. You will receive a copy of this form for your records.

QUESTIONS: If you have any questions now, during or following your participation regarding this study and its associated risks, please contact Michael Moore at (330) 672-7915. This project has been approved by Kent State University and the Psychology Department. If you have any questions about Kent State's rules for research, please call Dr. Peter C. Tandy, Acting Vice President for Research, Division of Research and Graduate Studies, telephone number (330) 672-3012.

CONTINUE ON BACK

SIGNATURE LINES: By signing this form I acknowledge that I have read and understand this form, and have had any questions regarding the risks and benefits of this study satisfactorily answered, and I am voluntarily consenting to participate in this study. Further, I realize that by signing this form I do not waive any of my legal rights.

Date: ___________ Participant Signature: ____

- -

INTRODUCTION: Frequently, our research lab has additional research opportunities for students and members of the community. Sometimes the research opportunity occurs in the same semester as your original participation. Other times, it might be in the future. We are inquiring as to whether you would be interested in being considered for this additional research opportunity.

YOUR PARTICIPATION: Saying yes now <u>does not obligate</u> you to participate in the future. Instead, you will be presented with a new consent form for that study and can make your decision at that point. Saying yes now simply means that <u>you give us permission to contact you for a future</u> research opportunity. Whether or not you sign in this section will have no bearing on your participation in the current project.

Date: ______________ Please Print Your Name: ___________________________________

Sign Here: ____

Local Address: ____

____

Permanent Address: ____

____

Local Telephone Number: _____________________ Permanent Telephone Number: ______________

Email Address: ______________________________

APPENDIX B

KENT STATE UNIVERSITY

DEPARTMENT OF PSYCHOLOGY
330-672-2166

Project Title: Memory and Mood Study

Project Investigator: Michael T. Moore

INTRODUCTION: You are being asked to participate as one of approximately 150 individuals in a research study examining how your emotions affect your memory performance. All participants are students of Kent State University at least 18 years of age, who represent a cross-section of students with respect to one's tendency to feel depressed.

YOUR PARTICIPATION: If you decide to take part in this study, one of the members of our research team will give you a packet of self-report questionnaires to answer in the lab. This packet will take you approximately 2 hours to complete. The measures in the packet inquire about how you might handle stressful life events, your memory about experiences in your past, and your current feelings.

BENEFITS: In return for your participation in this study, you will receive 4 points towards the fulfillment of course requirements in Introductory to Psychology. Your participation in this study may also enable us to better understand how individuals deal with their stress, emotions, and unpleasant feelings.

RISKS: You may experience mild discomfort when asked to focus on topics that are potentially stressful or unpleasant, but our experience is that these effects are at most slight and last for a matter of minutes. If, however, you feel more than mild discomfort, we encourage you to contact the Kent State University Psychological Clinic (330-672-2372) or the University Psychological Services (330-672-2487). Alternatively, if you prefer, we will assist you with the referral. You are under no obligation to complete this study even if you sign this consent form. You may discontinue your participation at any time.

CONFIDENTIALITY: All data collected in this study remain strictly confidential within the limits of the law. For the purposes of the research, the information you provide to us will be identified only by a subject number and will be examined only by Michael Moore and qualified members of the research team. All data will remain in a locked file. After the study, data may be published in scientific journals, but data will never be published in any manner that can identify you.

VOLUNTARY PARTICIPATION: Your participation in this study is voluntary and you may decline to participate in it without any impact on your grade for the course. Should you choose to participate, you may voluntarily withdraw from it at any time, again without any penalty. By signing this form you are indicating that you have been informed about the research study in which you are agreeing to participate, and that you have had all of your questions satisfactorily answered. You will receive a copy of this form for your records.

QUESTIONS: If you have any questions now, during or following your participation regarding this study and its associated risks, please contact Michael Moore at (330) 672-7915. This project has been approved by Kent State University and the Psychology Department. If you have any questions about Kent State's rules for research, please call Dr. Peter C. Tandy, Acting Vice President for Research, Division of Research and Graduate Studies, telephone number (330) 672-3012.

CONTINUE ON BACK

SIGNATURE LINES: By signing this form I acknowledge that I have read and understand this form, and have had any questions regarding the risks and benefits of this study satisfactorily answered, and I am voluntarily consenting to participate in this study. Further, I realize that by signing this form I do not waive any of my legal rights.

Date: ____________ Participant Signature: ____

- -

INTRODUCTION: Frequently, our research lab has additional research opportunities for students and members of the community. Sometimes the research opportunity occurs in the same semester as your original participation. Other times, it might be in the future. We are inquiring as to whether you would be interested in being considered for this additional research opportunity.

YOUR PARTICIPATION: Saying yes now <u>does not obligate</u> you to participate in the future. Instead, you will be presented with a new consent form for that study and can make your decision at that point. Saying yes now simply means that <u>you give us permission to contact you for a future</u> research opportunity. Whether or not you sign in this section will have no bearing on your participation in the current project.

Date: ______________ Please Print Your Name: ___________________________________

Sign Here: ____

Local Address: ____

____

Permanent Address: ____

____

Local Telephone Number: _____________________ Permanent Telephone Number: _____________

Email Address: ______________________________

APPENDIX C

Mood and Memory Phone Script

1. Hello, can I speak to *insert participant's name here*?

2. I'm calling about the experiment you participated in a few weeks ago. If you recall, we mentioned that you might be eligible to participate in another experiment and receive experimental points. Since you are eligible, would you be interested in participating and receiving up to 8 points?

3. *IF YES*: Great, let me tell you a little about the experiment then. We'll schedule a time for you to participate later, depending on when you're free. Basically, you'll complete some questionnaires that will ask you questions about how you think and your mood. Those questionnaires will take about 2 hours and you'll receive 4 experimental points for participating. You're then eligible to return 2 weeks later, complete more questionnaires and get 4 more points. Does that sound like something you'd be interested in doing?

4. *IF YES*: Record when they will be participating and the order of presentation of the questionnaires using the Random Number Generator (www.randomizer.org/form.htm) on the "**Mood and Memory Call List**". Collect their bubble sheets and place them in a manilla folder in the proper order and write their ID number on it.

5. *IF NO*: Record this on the "**Mood and Memory Call List**" under "Not Interested".

Frequently Asked Questions

How will my confidentiality be ensured? - As in mass testing, their names are separated from any sensitive information and only numbers are used. In addition, the experimenters are blind to any previous mass testing questionnaires they completed.

How the hell do I use the Random Number Generator? – The "Sets of Numbers" you want to generate is equal to the number of participants you've scheduled that day. It's easiest to wait until all of the participants are scheduled for the day, then generate numbers for all of them at once. If ten are scheduled that day and need numbers, generate ten sets.

FOR TIME 1: You always want "Numbers Per Set" = 7, "Number Range" = From 1 To 7, "Each Number in a Set Unique?" = "Yes", "Wish to Sort Your Outputted Numbers" = "No", and "Output Numbers" = "Place Markers Off". Then hit the "Randomize Now!" button. Let:

1=ASIS-R 2=ASQ 3=BDI-II 4=CAVE-Self 5=DAS 6=MASQ 7=RSQ-25

This way, for every person scheduled, you should have the order of presentation for their packet randomized. You then need to determine which of the AMT or computer task they'll receive first. To do that you'll use the 'ol randomizer again, but this time "Numbers Per Set"=1. This way, you should get a group number for each person as well. Let:

1=Computer Task first
2 = AMT first

FOR TIME 2: You always want "Numbers Per Set" = 9, "Number Range" = From 1 To 9, "Each Number in a Set Unique?" = "Yes", "Wish to Sort Your Outputted Numbers" = "No", and "Output Numbers" = "Place Markers Off". Then hit the "Randomize Now!" button. Let:

1=ASIS-R 2=ASQ 3=BDI-II 4=CAVE-Self 5=DAS 6=MASQ 7=RSQ-25
8=Recall of Performance Scale 9=RSQ-Specific

Remember: There is no computerized task at Time 2 and the AMT is given separately.

APPENDIX D

Please read the following to all participants, try to speak slowly and clearly:

Computer Task Directions:

"This part of the experiment asks you to complete a task on the computer and ask some questions about it. First I'll have you enter some information into the computer, then you'll practice doing the task, and finally we'll have you do the task itself. More detailed instructions will be provided on the computer monitor when we start the task. Do you have any questions for me before we begin?"

AMT Directions:

"I am interested in your memory for events that have happened in your life. I am going to read you some words. For each word I want you to think of an event that happened to you which the word reminds you of. The event could have happened at any point in your life beginning when you were small, but please do not include memories from last week. It might be an important event, or a trivial event. Just one more thing: the memory you recall should be a specific event – *an event that lasted less than a day, and occurred at a particular time and place*. So if I said the word "good" – it would not be OK to say, "I always enjoy a good party", because that does not mention a specific event. But it would be OK to say "I had a good time at Jane's party" (because that is a specific event). It is important to try to retrieve a different memory or event for each cue word. Let us try some words for practice."

If the participant does not give a specific response, use the following prompt *for the practice words only (DO NOT use any prompts for 18 the non-practice words)*:

"**Can you think of a specific time? One particular episode**"

Library
Grass
Chicken

After the 3 practice words, move on to the 18 test cues. *REMEMBER*: Allow only 60 sec. of writing per word and *DO NOT USE PROMPTS*.

Questionnaire Directions:

"In completing the following questionnaires, please be as honest as possible. Your confidentiality will be respected at all times and at no point will your name be associated with any of your answers. If you have any questions at all about any of the items on the questionnaires, please do not hesitate to ask. We would much rather that you do so as opposed to marking answers without properly understanding the questions or leaving questions blank. Please answer all of the questions and mark each item only once. If for any reason you are feeling fatigued, distracted, or otherwise unable to complete the questionnaires properly, let us know and we'd be more than happy to reschedule another time when you can complete them. Lastly, please make sure to read all of the directions in the packet carefully, as not doing so could seriously affect your responses."

REMEMBER: Check the individual's response to item #9 on the BDI-II before they leave, and if they marked 2 or 3, let me or another grad. student know before they leave so that we can assess for risk for suicide and refer them properly.

After the participant completes the Computer Task, AMT, and Questionnaires:

"Once again, thanks very much for your participation, we appreciate the time and effort it takes you to help us out."

ALSO: If a participant doesn't attend a scheduled session, attempt to reschedule it with them, but if that isn't possible record that they decided not to participate on the "**Mood and Memory Call List**"