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An Examination of Executive Control Biases and Rumination in Currently, Remitted and Never Depressed Individuals

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An Examination of Executive Control Biases and Rumination in Currently, Remitted and Never Depressed Individuals

by

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Abstract

Cognitive theories of depression propose that biases in executive control may be involved in the development, maintenance, and/or recurrence of depression. One suggested mechanism is that executive control biases may contribute to depressive rumination. The primary purpose of this study was to comprehensively evaluate depression-related biases in three components of executive control, namely inhibition, working memory updating, and set shifting. The secondary purpose was to evaluate relationships among biases in executive control and rumination. Currently depressed (n = 53), remitted depressed (n = 55), and non-clinical control (n = 51) participants were tested on separate computer-based paradigms designed to measure inhibition, working memory updating, and set shifting, respectively, involving emotional stimuli. As hypothesized, currently depressed participants exhibited biases in each of the executive control components. Specifically, currently depressed participants showed a reduced ability to inhibit the processing of negative distracting stimuli and to update working memory with emotional information, relative to control participants. Currently depressed participants also had greater difficulty shifting away from an emotion-relevant task set than from an emotion-irrelevant task set, whereas control participants did not show this bias. Remitted depressed participants did not demonstrate similar biases to currently depressed participants. Hypotheses regarding the relationships among executive control biases and rumination were largely unsupported; possible explanations for these findings are considered. This study is the most comprehensive evaluation of executive control biases in clinical depression to date, and provides novel insight into the nature of depression-related biases. The theoretical and clinical relevance of the findings, the limitations of the study, and directions for future research are discussed.

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Chapter 1: Introduction

1.1 Major Depressive Disorder

Major depressive disorder (MDD) is defined as a period of decline in mood and functioning, during which an individual experiences persistent low mood and/or decreased interest in or enjoyment of activities for a minimum of two weeks (American Psychiatric Association, 2013). During this period, the individual also experiences at least four additional depressive symptoms on a near constant basis, which may include a significant change in appetite or weight, insomnia or hypersomnia, psychomotor agitation or slowing, low energy, feelings of worthlessness or excessive guilt, impaired thinking, concentration, or decision-making and suicidal ideation or behaviour. These symptoms are experienced by the individual at a level that causes significant distress and/or interferes with functioning in various life domains.

MDD is a highly prevalent psychiatric disorder. Epidemiological research suggests that approximately 12 - 18% of individuals will meet criteria for MDD in their lifetimes (Kessler et al., 2003; Hasin, Goodwin, Stinson, & Grant, 2005; Patten et al., 2006; Williams et al., 2007). In Canada, it has been found that the 12-month prevalence of MDD is 4.8% (Patten et al., 2006), and that about 3% of the population experiences a first onset of depression each year (Wang et al., 2010). The high prevalence of MDD is paralleled by its high burden. The majority of individuals with MDD report severe to very severe role impairment (Kessler et al., 2003; Williams et al., 2007). Further, depression was the leading cause of non-fatal burden of disease worldwide in the Global Burden of Disease Study 2010 (Whiteford et al., 2013). The annual cost of depression in Canada was estimated 16 years ago at $14.4 billion in treatment, lost productivity, and premature death (Health Canada, 2001). More recent economic data estimated the national annual cost of depression in the United States to be $210 billion dollars in 2010

(Greenberg, Fournier, Sisitsky, Pike, & Kessler, 2015). This figure is comprised of 45 – 47% direct costs related to MDD treatment, 5% costs related to suicide, and 48 – 50% costs related to the workplace, including reduced productivity and absenteeism. The impact of depression is exacerbated by the fact that it is often a recurrent disorder, with an estimated relapse rate as high as 75% within two years of recovery (Gotlib & Hammen, 2002). These data highlight the need for researchers to identify vulnerability factors for depression, to improve the prevention and treatment of this disorder.

1.2 Depression and Executive Control

Leading models of depression posit a key role for cognitive factors in the onset, maintenance, and recurrence of depression (Beck, Rush, Shaw, & Emery, 1979). Nearly 40 years ago, Beck and colleagues (1979) proposed that depression is the result of a diathesis-stress process, whereby underlying cognitive vulnerability is activated by life stress. Cognitive vulnerability to depression includes both negative cognitive content (i.e., thinking) and biased information processing that prioritizes negative material. Beck (1976) proposed that people develop internal cognitive structures, termed schemas, based on their life experiences. In general, the purpose of schemas is to efficiently filter, interpret, and organize incoming information. For individuals who are vulnerable to depression, however, schemas centre on negative themes such as loss, rejection, failure, and worthlessness, and incoming information is selectively filtered and processed in a way that is consistent with those themes. Depression vulnerability is thus characterized by biases in attention, interpretation, and memory that favour the processing of negative information (Clark, Beck, & Alford, 1999). Depressive schemas, and their effect on information processing, give rise to negative thoughts in response to various situations and environmental cues. These thoughts typically focus on negative and pessimistic views about

oneself, the world, and the future – collectively termed the "cognitive triad" (Beck et al., 1979). When depressive schemas are activated by adverse life events and/or stressors, vulnerable individuals are likely to experience a detrimental cycle of increasingly negative thoughts, information processing, and mood, ultimately leading to clinical depression. An extensive body of research has supported the primary tenets of Beck's cognitive model of depression. Numerous studies have found that depressed individuals exhibit negative thoughts, beliefs, and attributions, as well as biases in attention, interpretation, and memory that favour the processing of negative information (Clark et al., 1999; Gotlib & Joormann, 2010).

More recently, there has been increased focus on the executive control processes that may underlie negative thinking and information processing in depression (Gotlib & Joormann, 2010). Executive control is defined as "general purpose control mechanisms that modulate the operation of various cognitive subprocesses and thereby regulate the dynamics of human cognition" (Miyake et al., 2000, p. 50). Executive control has been conceptualized to involve three related but separable functions: 1) inhibition; 2) monitoring and updating of information in working memory; and 3) mental set shifting (Miyake et al., 2000). Although inhibition is a multifunction construct (Friedman & Miyake, 2004), it has often been defined, and will be defined here, as the ability to prevent the entry and processing of irrelevant information in working memory. Monitoring and updating of information ("updating") in working memory encompasses evaluation of incoming information for its relevance to current tasks, and the removal of irrelevant information from working memory (Morris & Jones, 1990). Mental set shifting ("shifting") is the ability to switch among multiple tasks, operations, or mental sets (Monsell, 2003). Working memory is a limited capacity system for the temporary storage and manipulation of information and is an essential link between perception and higher-order cognitive processes

and behaviours (Baddeley, 2003). Inhibition, updating, and shifting are crucial abilities that permit selective and flexible processing of information in working memory.

It is important to distinguish between depression-related *deficits* and *biases* in executive control. Deficits in executive control refer to broad impairments in inhibition, updating, and shifting regardless of the content or affective valence of the information that is being processed. In contrast, depression-related biases in executive control refer to a pattern of inhibition, updating, and shifting that prioritizes the processing of negative information. Executive control deficits and biases in depression have been studied relatively independent of one another, with little integration of the findings from these lines of research (Gotlib & Joormann, 2010). In general, however, research has found limited evidence that depression is associated with broad deficits in executive control when processing neutral information. Employing a variety of executive functioning measures, several studies have demonstrated that severely depressed patients show reduced performance in inhibition, updating, and set shifting processes relative to non-depressed individuals (e.g., Harvey et al., 2004; Meiran, Diamond, Toder, & Nemets, 2011; Reppermund, Ising, Lucae, & Zihl, 2009; Stordal et al., 2004). However, depressed individuals often do not demonstrate consistent impairment across all executive control tasks (e.g., Harvey et al., 2004; Purcell, Maruff, Kyrios, & Pantelis, 1997), and other research suggests that mild to moderate depression may not be associated with appreciable executive control impairments in otherwise healthy young adults (Grant, Thase, & Sweeney, 2001; Harvey et al., 2005; Joormann, Nee, Berman, Jonides, & Gotlib, 2010). Grant et al. (2001) concluded that executive control deficits may be more characteristic of severely depressed inpatients and elderly depressed individuals. Consistent with this conclusion, a meta-analysis found that increased severity of depression is significantly associated with greater executive function impairment (McDermott &

Ebmeier, 2009); thus, individuals with less severe depression may have largely intact executive functioning when processing neutral information.

1.3 Depression-Related Executive Control Biases

In contrast to the uncertain association between depression and general executive control deficits, which may be moderated by presentation, severity, and age (Grant et al., 2001; McDermott & Ebmeier, 2009), a more reliable association has been observed among depression and executive control impairments for emotional information specifically. Such an association is consistent with the prediction from the cognitive model that depression-relevant schemas bias information processing in a way that favours congruent information (Beck et al., 1979; Clark et al., 1999). Although overall there is evidence for biased executive control in depression, research has focused more on biases in certain executive control processes (e.g., inhibition) than others (e.g., shifting). Thus, there is actually little evidence that depressed individuals exhibit biases across the various executive control processes. In addition, it is unclear whether biases in executive control are an epiphenomenon of an active depressive episode or whether they reflect a stable vulnerability factor that may persist beyond the active phase of depression and maintain risk for future episodes of depression. If the former, then it would be expected that executive control biases would be observed in individuals who are currently depressed, but not in those who have recovered from an episode of depression. However, if the latter, then remitted depressed individuals (i.e., individuals who are not currently depressed but who have experienced at least one MDE in the past) should show biases in executive control for emotional material similar to currently depressed individuals. In this section, the evidence for executive control biases in both current and remitted depression is reviewed.

Of the three executive control processes, inhibition is by far the most frequently studied in the context of depression. Using several paradigms, studies have demonstrated that current depression is characterized by impaired inhibition of negative stimuli (Gotlib & Joormann, 2010). The Stroop task (Stroop, 1935) is a commonly used measure of inhibition (Miyake et al., 2000), in which individuals are required to name the ink or font colour of a presented item while ignoring the item itself. Response times on the Stroop task are typically longer when the item is a word compared to a meaningless non-word, and when the item is a colour word that is incongruent with its font colour (e.g., the word "red" presented in blue font) compared to when the item is a colour word that is congruent with its font colour (e.g., the word "red" presented in red font), representing variants of the Stroop effect (MacLeod, 1991). To the extent that the individual has difficulty inhibiting the semantic processing of the word, the Stroop effect will be larger. The emotional Stroop task is a version of the task in which the presented items are words that vary in affective valence. The relative performance of clinical and non-clinical samples on the emotional Stroop task is a demonstration of the effect of psychopathology on inhibition of emotional material. A meta-analysis of studies of the Stroop task in depression found that clinically depressed participants demonstrated modest but significant slowing when naming the colour of negative words compared to both neutral and positive words, whereas control participants showed no differences in response times across negative, positive, and neutral words (Epp, Dobson, Dozois, & Frewen, 2012). Thus, Stroop task evidence suggests that depressed individuals have particular difficulty inhibiting the semantic processing of negative material.

A second paradigm that has been used to study inhibition biases in depression is the negative affective priming (NAP) task (Wentura, 1999). In the NAP task, participants are required to identify a target stimulus as positive or negative while ignoring a simultaneously

presented distracter stimulus. The affective valence of the target stimulus in the subsequent trial may either match or not match the valence of the to-be-ignored distracter stimulus. Response times to a target stimulus are typically slower if its valence matches that of the distracter in the previous trial, a finding known as the NAP effect (Wentura, 1999). The NAP effect is interpreted as the time required to overcome inhibition of the emotional representation of the previous distracter in order to respond to a target of the same valence. Reduced inhibition of the distracter therefore is reflected in a smaller NAP effect. Joormann (2004) originally found that dysphoric participants showed a reduced NAP effect compared to non-dysphoric participants for negative target words, but not positive target words, consistent with her hypothesis that dysphoric participants would be impaired in their ability to inhibit processing of negative task-irrelevant information. Reduced inhibition of negative material as assessed by the NAP task has also been demonstrated in clinically depressed participants relative to control participants using both face stimuli (Goeleven, De Raedt, Baert, & Koster, 2006) and word stimuli (Joormann & Gotlib, 2010). Again, reduced inhibition was specific to negative stimuli; depressed participants exhibited intact negative priming for positive stimuli (Goeleven et al., 2006; Joormann & Gotlib, 2010).

Finally, depression-related biases in inhibition have been studied using the emotional flanker task (EFT) in a few studies (Zetsche & Joormann, 2011; Zetsche, D'Avanzato, & Joormann, 2012). In the EFT, participants identify whether a target stimulus is positive or negative while ignoring simultaneously presented "flanking" stimuli that vary in affective valence. In the version of the task used by Zetsche and colleagues, each trial of the EFT consisted of four words arranged in a 2 × 2 matrix, with one target word presented in green letters and three identical distractor words presented in red letters. A measure of interference was

calculated which was the difference in response times between trials in which the distractor words were incongruent with the target in valence (i.e., positive distractors when the target was negative and negative distractors when the target was positive) and trials in which the distractor words were neutral, for targets of the same valence. In contrast to the NAP task in which inhibition of a distractor stimulus in one trial is inferred by the speed of responding to a target stimulus of the same valence on a subsequent trial, inhibition of distractor stimuli is inferred in each trial of the EFT by the speed of responding to a target stimulus in the presence of the distractors. Reduced inhibition of emotional stimuli is indicated by slower responding to incongruent versus neutral distractor trials. In a non-clinical sample, interference scores on the EFT were unrelated to depressive symptoms (Zetsche & Joormann, 2011). However, in a clinical sample, Zetsche et al. (2012) found that currently depressed participants had marginally greater interference scores for negative distractor words relative to control participants.

Evidence for impaired inhibition of negative material in remitted depression is limited. A few studies employing the emotional Stroop task have not found differential responding to negative words relative to positive and neutral words in remitted depressed participants (Gotlib & Cane, 1987; Hedlund & Rude, 1995; Merens, Booij, & Van Der Does, 2008). Joormann (2004; Experiment 3) found a marginally significant difference between remitted depressed and control participants on the NAP task for trials involving negative target stimuli, such that remitted depressed participants showed a somewhat reduced NAP effect compared to control participants. However, Joormann and Gotlib (2010) found that remitted depressed individuals did not differ from control individuals in their inhibition of negative words on the NAP task. In sum then, there is consistent evidence that current depression is associated with a deficit in the inhibition of negative task-irrelevant information. Research has generally not found a similar

association in remitted depression (see Joormann, 2004 for an exception), but too few studies have been conducted to conclude that this association does not exist.

A few studies have investigated biases in monitoring and updating of working memory in relation to depression. For instance, Joormann and Gotlib (2008) found that depressed individuals took longer to remove no-longer-relevant negative information from working memory than controls. In this study, participants completed a modified Sternberg task that required them to memorize two lists of emotional words and then to subsequently ignore one of the lists based on a cue presented following the lists. A probe word that was either novel or from the relevant (i.e., non-ignored) or irrelevant (i.e., to-be-ignored) word lists was then presented and participants identified whether the probe belonged to the relevant word list. An intrusion effect was calculated that reflected the difference in response times to probe words from the irrelevant list and novel probe words of the same valence. Currently depressed participants had significantly greater intrusion effects than control participants for negative probes but not positive probes (Joormann & Gotlib, 2008). Joormann et al. (2010; Experiment 1) also observed that depressed participants had significantly greater intrusion effects for negative probes than control participants, but no group difference was found for intrusion effects for positive probes. In a second experiment using a variant of the task that used neutral stimuli (i.e., letters) instead of affective stimuli (i.e., emotional words), there was no group difference in the magnitude of intrusion effect (Joormann et al., 2010; Experiment 2). Thus, the results of these two studies suggest that currently depressed individuals have a specific difficulty in removing negative information from working memory.

In contrast, Yoon, LeMoult, and Joormann (2014) found a non-valence-specific impairment among participants with depression in removing irrelevant emotional information

from working memory compared to participants with social anxiety disorder and control participants. Depressed participants made significantly more errors when responding to irrelevant probes than the other participant groups, but this effect did not vary across negative and positive probes. In addition, Zetsche et al. (2012) did not find any differences between depressed and control participants on intrusion effects for either positive or negative probe words. However, Zetsche and colleagues noted that in their version of the task, following the presentation of the two word lists, the irrelevant word list was presented again while participants attempted to suppress those words, whereas a cue was used in the tasks of Joormann and Gotlib (2008) and Joormann et al. (2010) to indicate which words were to be ignored without presenting those words again. They suggested that the second presentation of the irrelevant word list may have facilitated performance on the task, thus minimizing group differences in performance (Zetsche et al., 2012).

The task employed in the reviewed studies (Joormann et al., 2010; Joormann & Gotlib, 2008; Yoon et al., 2014; Zetsche et al., 2012) required participants to remove information from working memory, but did not involve continuous updating of working memory with new information. Only one study has examined updating of working memory in currently depressed individuals involving both the removal and addition of emotional information (Levens & Gotlib, 2010). Levens and Gotlib (2010) used an affective n-back task (ANB) which required participants to determine whether the emotional expression of a presented face image matched the emotional expression of a face image presented two trials previously. Thus, each trial of the ANB required participants to discard an old emotional expression (from three trials ago) from working memory, add a new emotional expression (from the current trial) to working memory, evaluate the current expression and compare it to the appropriate emotional expression held in

working memory (from two trials ago), and make a response. Levens and Gotlib (2010) found that depressed participants were faster to integrate sad faces into working memory, and slower to disengage from sad faces held in working memory, relative to control participants. Thus, depressed participants appear to have biases in updating that function to keep negative information active in working memory.

In the only study to evaluate the stability of updating biases outside of an active episode of depression, Levens and Gotlib (2015) compared remitted depressed and control participants on the ANB. The results indicated that remitted depressed participants were slower to disengage from sad faces than happy faces. Control participants showed the opposite pattern, and were slower to disengage from happy faces than neutral and sad faces. Thus, this study suggests that individuals who have recovered from depression may continue to demonstrate biases that maintain the activation of negative over positive information in working memory.

To summarize, the research on depression-related updating biases is somewhat mixed. Whereas three studies have found that currently depressed individuals show difficulty in discarding irrelevant negative material from working memory (Joormann et al., 2010; Joormann & Gotlib, 2008; Levens & Gotlib, 2010), one study found that this difficulty applied to irrelevant material in general as opposed to negative material specifically (Yoon et al., 2014), and one study found no group differences in updating (Zetsche et al., 2012). However, only the task of Levens and Gotlib (2010) required participants to both add and remove stimuli, which may be a more inclusive measure of working memory monitoring and updating. On the ANB, depressed participants showed facilitated integration of and delayed disengagement from negative stimuli in working memory. Moreover, only one study has investigated updating biases among remitted

depressed individuals, and found that biases for negative material may persist following remission (Levens & Gotlib, 2015).

A small number of studies have employed an internal shifting paradigm to assess the ability of depressed individuals to update and shift between internal working memory representations. The internal shifting paradigm requires participants to keep a mental count of the number of presented words or images in prescribed categories (Lo & Allen, 2011; De Lissnyder, Koster, & De Raedt, 2012a). This paradigm assumes that in trials where the presented stimulus belongs to the same category as the stimulus in the previous trial, participants need to update their mental count in a category that is already activated in working memory, whereas if the presented stimulus belongs to a different category as the previous stimulus, participants are required to shift their internal focus to the other category and then update their mental count for that category. In one version of the internal shifting paradigm, participants sequentially complete a neutral condition in which they are presented with food or household object words one at a time, and an affective condition in which they are presented with positive and negative adjectives one at a time (Lo & Allen, 2011). In both conditions, participants are required to keep a mental count of the number of words presented in the two categories, responding by key press once they have updated their mental count, and reporting the total number of presented words in both categories at the end of each block of trials. Lo and Allen (2011) observed that depressed youth (ages 16 to 24) had larger switch costs (difference in response times between trials that require an internal shift between categories and trials that do not require a shift) than control youth for the affective task only. There was no group difference in switch costs on the neutral task.

In a second version of the internal shifting paradigm, the same stimuli set (images of neutral and angry faces) is used in both the neutral and affective conditions, but participants are

required to keep a mental count of the number of presented male and female faces in the neutral (i.e., emotion-irrelevant) condition and a mental count of the number of presented neutral and angry faces in the affective (i.e., emotion-relevant) condition. In a sample of dysphoric and non-dysphoric participants, De Lissnyder et al. (2012a) found no group differences in switch costs for both the neutral and affective conditions. In a separate study using a clinical sample, De Lissnyder and colleagues (2012b) observed that currently depressed participants had larger switch costs than control participants, but this effect did not differ across the neutral and affective conditions. De Lissnyder et al. (2012b) also compared groups on response times when switching from an angry face to a neutral face and from a neutral face to an angry face in the affective condition. While they predicted that currently depressed participants would have longer response times when shifting from an angry face versus a neutral face, no interaction was observed between group and previous facial expression on response times. Thus, the few studies that have compared dysphoric and depressed individuals to control individuals on variants of the internal shifting paradigm have yielded inconsistent results. No studies have evaluated the performance of remitted depressed individuals on this task.

A conceptual issue regarding the internal shifting paradigm is that it is unclear which executive control processes it assesses. The task has been variously referred to as a measure of internal attention shifting or switching by some researchers (e.g., Lo & Allen, 2008; Lo, Lau, Cheung, & Allen, 2012) and as a measure of cognitive or interference control by others (e.g., De Lissnyder et al., 2012a; 2012b; Demeyer, De Lissnyder, Koster, & De Raedt, 2012). Although the task requires shifting of internal focus between two categories, it does not require shifting between multiple task sets or operations, which is how set shifting has been defined (Miyake et al., 2000; Monsell, 2003). Each trial within a condition requires participants to complete the

same task set – that is, to categorize the presented stimulus according to a particular attribute (e.g., gender). Participants complete different task sets across the conditions (e.g., categorizing faces according to gender in the neutral condition and categorizing faces according to emotional expression in the affective condition), but are not required to switch back and forth between these two task sets as the conditions are completed sequentially. Thus, the internal shifting paradigm does not measure the shifting component of executive control according to accepted definitions (Miyake et al., 2000; Monsell, 2003). Instead, the internal shifting paradigm might be better conceptualized as a measure of working memory monitoring and updating as it requires participants to monitor, shift between, and update internal representations in working memory. If this conceptualization is accurate, then the findings from studies that have employed the internal shifting paradigm should be considered in conjunction with the other findings from studies on depression-related updating biases.

Importantly, if the internal shifting paradigm should not be considered a measure of set shifting, then there exist no studies on biases in set shifting specifically in the context of either current or remitted depression. A couple studies have used an affective shift task to measure inhibition and set shifting in response to emotional stimuli in non-clinical samples (De Lissnyder, Koster, Derakshan, & De Raedt, 2010; Everaert, Grahek, & Koster, 2016b). The affective shift task is an odd-one-out search task. In each trial, participants are presented with four emotional faces presented in a 2 × 2 matrix, which differ on the dimensions of emotion (angry or happy), gender (male or female), and colour (dark grey or light grey). Prior to the presentation of the faces, a cue word is presented that indicates the relevant stimulus dimension for that trial (emotion, gender, or colour). Participants are required to identify the face that differs from the other faces on the basis of the cued stimulus dimension. Switching is indexed by

comparing response times to trials that require a shift to a different stimulus dimension (e.g., emotion-gender) with response times to trials that do not require a shift (e.g., emotion-emotion). De Lissnyder et al. (2010) found that dysphoric and non-dysphoric undergraduate students did not differ on switch costs when shifting from the emotion to gender cue or vice versa. However, when analyses were restricted to a subset of participants at the extremes on depression symptoms, dysphoric participants had a greater switch cost when shifting from the emotion to gender cue than non-dysphoric participants, reflecting an impairment in shifting away from an emotion-relevant task set. This impairment was not valence-specific; no group differences were observed on response times on shift trials from the emotion cue when the previous trial was an angry or happy face. In a non-clinical sample of undergraduate students, Everaert et al. (2016b) found no significant correlation between set shifting on the affective shift task and depressive symptoms. Taken together, the results of these studies do not strongly support a relationship between depression and set shifting biases, but the use of non-clinical samples may have led to an underestimation of this relationship.

The reviewed literature suggests that depression may be associated with biases in the various functions of executive control when processing negative information. In particular, there is reliable evidence that currently depressed individuals have difficulty inhibiting the processing of negative task-irrelevant material. However, there are a number of significant gaps in the literature on depression-related executive control biases. First, few studies have examined emotional biases in the monitoring and updating of working memory in depression, and these have yielded some inconsistent findings. Moreover, only one study has employed a task that required both the addition and removal of stimuli from working memory (Levens & Gotlib, 2010). Second, there is a lack of research on set shifting biases in depression. Although a few

studies have evaluated the performance of currently depressed participants on the internal shifting paradigm relative to control participants, this task does not assess the ability to shift between multiple task sets and therefore may be better conceptualized as a measure of working memory updating. No studies to date have evaluated emotional biases specific to shifting between task sets in the context of clinical depression. Third, little to no data exist regarding the association between remitted depression and biases across the three executive control processes. In particular, only one study has examined updating biases (Levens & Gotlib, 2015) and none have examined shifting biases among remitted depressed individuals.

The current study was designed to address the identified gaps in the literature. Currently depressed, remitted depressed, and non-clinical control participants were compared on executive control tasks involving emotional stimuli. The primary purpose of the study was to determine whether currently and remitted depressed participants demonstrate biases in inhibition, monitoring and updating of working memory, and/or mental set shifting when processing negative material relative to control participants, using separate tasks intended to specifically measure each of the respective executive control functions.

1.4 Executive Control Biases and Rumination

Another source of uncertainty in the literature is the mechanism(s) through which executive control biases may be related to depression. It is conceivable that biases in inhibition, updating, and shifting that increase the availability of negative material in working memory would lead to more negative thinking and mood, which may in turn maintain or worsen depressive states. It has also been suggested that executive control biases may underlie depressive rumination (Joormann & D'Avanzato, 2010; Koster, De Lissnyder, Derakshan, & De Raedt, 2011). Nolen-Hoeksema (1991) described rumination as a response style to negative

affect characterized by repetitive thinking about one's mood as well as the causes and implications of the negative mood. Across a number of studies, Nolen-Hoeksema and her colleagues demonstrated that rumination exacerbates and prolongs negative mood, is associated with higher levels of depressive symptoms, and predicts the onset of future depressive episodes (see Nolen-Hoeksema, Wisco, & Lyubomirsky, 2008 for a review).

For instance, several studies have shown that inducing rumination in the laboratory increases negative mood in dysphoric participants (Lyubomirsky, Caldwell, Nolen-Hoeksema, 1998; Lyubomirsky & Nolen-Hoeksema, 1993, 1995; Nolen-Hoeksema & Morrow, 1993). In a naturalistic study of university students who experienced the Loma Prieta earthquake in San Francisco, a ruminative response style reported prior to the earthquake predicted greater depressive symptoms following the earthquake, controlling for pre-earthquake levels of depression (Nolen-Hoeksema & Morrow, 1991). In addition, students who had more ruminative thoughts about the earthquake in the immediate days that followed its occurrence experienced greater depressive symptoms six weeks later. In another prospective study, participants who reported more rumination at one month following the death of a family member by terminal illness were more depressed up to 18 months following the loss (Nolen-Hoeksema & Larson, 1999; Nolen-Hoeksema, Parker, & Larson, 1994). Nolen-Hoeksema (2000) examined whether rumination would prospectively predict diagnoses of depression in a community sample of over 1000 adults. The results of this study showed that baseline rumination predicted diagnoses of MDD, including new onsets of the disorder, one year later.

Two types of rumination have been distinguished (Treynor, Gonzalez, & Nolen-Hoeksema, 2003). The first is reflection, which is considered the adaptive form of rumination and involves cognitive problem solving to alleviate one's negative mood or situation. The second

is brooding, which is considered the maladaptive form of rumination and involves passively focusing on one's negative mood or situation. The brooding type of rumination appears to be most closely linked to depression. Treynor et al. (2003) found that brooding was correlated with depressive symptoms concurrently and predicted symptoms 1 year later in a community sample. In contrast, although there was a small correlation between reflection and depressive symptoms concurrently, reflection was associated with lower depression over time. Treynor et al. (2003) suggested that reflection may be prompted by negative mood and/or produce transient increases in negative mood, but over time lead to effective problem solving and reduced depression. Although research has supported the distinction between adaptive and maladaptive forms of rumination in non-clinical samples, it has been proposed that this distinction may be blurred in currently depressed populations (Joormann, Dkane, & Gotlib, 2006; Whitmer & Gotlib, 2011). Reflective self-analysis in the context of the negative and distorted thinking of depressed individuals may not be adaptive, and reflection and brooding may perpetuate one another such that the two forms of rumination become indistinguishable over time. In support of this notion, factor analytic results replicated the distinct reflection and brooding factors in non-clinical and remitted depressed samples, but did not show the same clear two-factor solution in a currently depressed sample (Whitmer & Gotlib, 2011). Still, even among depressed individuals, brooding appears to be more strongly correlated with depressive symptoms than reflective, intentional forms of rumination (Joormann et al., 2006; Whitmer & Gotlib, 2011).

Researchers have hypothesized that executive control deficits or biases may contribute to the tendency to ruminate (Joormann & D'Avanzato, 2010; Koster et al., 2011). For instance, Koster and colleagues (2011) proposed the impaired disengagement hypothesis of rumination. According to this model, rumination is initiated when an internal (e.g., negative mood or

memory) or external stressor (e.g., negative event) elicits thoughts about the causes of the stressor. Such thoughts tend to be self-critical and analytical, and typically lead to negative affect as the individual considers his or her contribution to the stressor. For many individuals, this process of self-reflection and evaluation is time limited. Typically, people will eventually either generate a solution to the problem/stressor or disengage attention from thoughts about the stressor, leading to mood repair. People with impairments in executive control, however, may have difficulty disengaging attention from negative thoughts and instead engage in prolonged rumination and maintain a critical inward focus. Prolonged rumination in turn interferes with effective problem-solving and emotion regulation. Such individuals thus become stuck in a cycle of ruminative thinking and negative mood, which ultimately may precipitate or exacerbate a depressive episode (Joormann, Yoon, & Zetsche, 2007; Koster et al., 2011).

Based on this hypothesis, studies have begun to test whether executive control biases are associated with rumination. There is preliminary evidence for a link between biased inhibition and rumination, although findings are inconsistent. De Lissnyder et al. (2010) found the predicted relationship between impaired inhibition of negative material and brooding in a non-clinical sample. In another non-clinical sample, Joormann (2006) found that overall rumination predicted reduced inhibition on the NAP task after controlling for depression symptoms. This relationship was not specific to inhibition of negative material, however, as rumination was similarly related to decreased inhibition of negative and positive words. Joormann and Gotlib (2010) found a specific relationship between inhibition of negative words on the NAP and rumination among currently depressed participants only. For the remitted depressed and control participants in the study, inhibition of emotional words was unrelated to rumination. In contrast to these findings, using a pictorial version of the NAP, Goeleven et al. (2006) observed no

correlation between inhibition of sad or happy face images and rumination among currently, remitted, and never depressed participants. The study by Zetsche and Joormann (2011) produced yet another distinct pattern of results. Cross-sectional data indicated that inhibition of emotional material on both verbal and pictorial versions of the NAP was not correlated with rumination, and *greater* inhibition of negative distractor words on the EFT was related to rumination and brooding, which was opposite to the predicted direction of relationship. However, reduced inhibition of sad faces on the pictorial NAP at baseline predicted increases in rumination at a 6-month follow-up, whereas baseline inhibition of negative words on the verbal NAP and EFT tasks did not significantly predict future rumination. Thus, there is some evidence that reduced inhibition of emotional material may be related to greater rumination cross-sectionally and over time, and that this relationship may be most pronounced for depressed individuals. However, there are several inconsistencies across the different study findings, and as such, whether there is a reliable association between biased inhibition and rumination remains unclear.

Most of the studies on depression-related updating biases also assessed rumination and demonstrated a relationship between updating and rumination. In their study employing the modified Sternberg task that required participants to memorize and then subsequently ignore a list of emotional words, Joormann and Gotlib (2008) found a specific association between updating biases and rumination for currently depressed participants only. For currently depressed participants, there was a significant correlation between intrusion effects for negative probes and rumination, including both reflection and brooding subtypes. This relationship remained after controlling for depression symptoms. There was no correlation between intrusion effects for either negative or positive probes and rumination in the control group. Joormann et al. (2010) found that intrusion effects for negative probes were significantly correlated with rumination in

their full sample of depressed and control participants, whereas this correlation was not found for intrusion effects for positive probes. However, these analyses did not control for individual differences in depression severity, and thus a unique relationship between intrusion effects and rumination was not demonstrated. Findings from the Yoon et al. (2014) study indicated that reduced accuracy on irrelevant probe trials (averaged across negative and positive probes) relative to neutral probe trials significantly predicted brooding among depressed participants, controlling for depression scores. This relationship was not observed among socially anxious or control participants. Finally, Zetsche et al. (2012) found that the intrusion effect for negative probes significantly predicted rumination scores in the full sample, controlling for diagnostic status (currently depressed versus control group). In contrast, inhibition of negative distractors on the EFT was not associated with rumination. These results suggest that rumination may be more strongly related to difficulty in removing negative information from working memory than to difficulty in inhibiting the access of negative information to working memory (Zetsche et al., 2012).

As discussed above, the task used in all four of these studies required participants to remove no-longer-relevant material from working memory, but not to update working memory with new material. The only studies to evaluate updating involving both the addition and removal of emotional stimuli from working memory in the context of current and remitted depression did not assess associations with rumination (Levens & Gotlib, 2010; 2015). Pe, Raes, and Kuppens (2013b) conducted a laboratory study (Experiment 1) and a daily life study (Experiment 2) to evaluate the associations between working memory updating ability, emotion regulation, and emotion, in non-clinical undergraduate samples. In both of these studies, there was no direct correlation between updating ability as assessed by accuracy rates on the ANB and

rumination. Thus, the available research indicates that an impaired ability to discard negative information from working memory is related to a greater tendency to ruminate, and that this relationship may be strongest for currently depressed individuals. However, the extent to which rumination is related to updating involving both the addition and removal of emotional material among clinical samples is unknown.

Performance on the internal shifting paradigm has also been linked to rumination in a few studies, although findings are mixed. Recall that in the study by De Lissnyder et al. (2012a), dysphoric and non-dysphoric participants did not differ on switch costs in either the affective or neutral conditions. However, when the participants were re-grouped into low and high ruminator groups based on self-reported trait rumination, high ruminators had significantly greater switch costs than low ruminators in the affective condition but not the neutral condition. This effect appeared to be largely the result of small switch costs for low ruminators rather than large switch costs for high ruminators though, as within-group analyses showed no difference between the switch costs for the affective and neutral conditions among high ruminators, but greater switch costs for the neutral versus affective condition among low ruminators. Furthermore, regression analyses did not reveal significant associations between switch costs in the affective condition and either the reflection or brooding subtypes of rumination. In their sample of currently depressed and control participants, De Lissnyder et al. (2012b) found that general switch costs (averaged across affective and neutral conditions) were significantly correlated with rumination scores. However, this correlation did not control for depression symptoms, and when the brooding and reflection subscales were examined separately, no significant associations with switch costs were found.

Demeyer et al. (2012) conducted a prospective study that showed that switch costs in the affective condition predicted depressive symptoms in remitted depressed individuals one year later, and that rumination fully mediated this relationship. A similar mediation model was not found with switch costs in the neutral condition as the initial variable. There were no significant baseline correlations between switch costs in either the affective or neutral conditions and rumination. De Lissynder and colleagues (2012c) also found that shift costs in the affective condition were associated with increased brooding in response to stress in a prospective longitudinal design with a non-clinical sample. Specifically, there was a stronger association between stress and brooding when participants had larger baseline switch costs in the affective condition. Switch costs in the neutral condition did not moderate the association between stress and brooding. Thus, findings from studies that employed the internal shifting paradigm indicate that impairments in switching between emotional representations in working memory are related to a tendency to engage in rumination, and depressive brooding specifically, although the cross-sectional results are inconsistent. Longitudinal data indicate that such impairments may interact with stress to lead to maladaptive forms of rumination, which in turn may increase depressive symptoms. Because the internal shifting paradigm does not assess shifting between emotional and non-emotional task sets, these findings may be more aptly considered as corroborating evidence for the link between emotional biases in working memory updating and rumination. In their study employing the affective shift task, De Lissnyder et al. (2010) found that brooding significantly predicted a general shifting impairment among non-clinical participants, whereas depressive symptoms and reflection were non-significant predictors. However, it is unknown whether shifting biases or deficits are related to rumination in clinical samples.

As a whole, the reviewed research points, albeit hesitantly, to a relationship between executive control biases and rumination. These results are consistent with theories that propose that individuals who have difficulty inhibiting, updating, and shifting from emotional material may be less able to disengage from negative thinking in response to stressors and instead engage in prolonged rumination (Koster et al., 2011). This process may interfere with effective emotion regulation and contribute to depressed mood. The relationship between executive control biases and rumination may be strongest for biases for negative information and the maladaptive form of rumination, depressive brooding. In addition, this relationship may be most operative, and therefore more consistently observed, among currently depressed individuals than individuals without clinical levels of depressive symptomatology. There are also several inconsistencies and gaps in the literature. An association with rumination is more consistently found for impairments in removing negative material from working memory than for impairments in inhibiting processing of negative material. Also, research has not employed tasks that assess updating of working memory involving both the addition and removal of emotional material, or shifting between multiple task sets, to evaluate relationships with rumination among currently or remitted depressed individuals. Thus, associations of depression-related updating and shifting biases with rumination have yet to be clearly demonstrated.

Therefore, the secondary purpose of the current study was to evaluate the relationship between executive control biases and rumination, including reflection and brooding subtypes. This association was examined for each of the three executive control functions, which were measured by separate tasks designed specifically to assess those functions. The design of the study permitted the investigation of whether relationships between executive control biases and rumination were stronger for currently depressed participants relative to remitted depressed and

control participants. The relationships were first examined within the full sample of participants, and then within the separate participant groups.

1.5 The Present Study

The present study investigated two specific research questions and addressed several of the limitations of previous work. First, the study examined whether currently and remitted depressed individuals would demonstrate biases in inhibition, monitoring and updating of information in working memory, and/or mental set shifting when processing emotional material relative to non-clinical control individuals. Second, the study tested whether rumination, and brooding in particular, would be related to biases in these components of executive control.

To evaluate these questions, participants were tested on separate tasks designed to measure the respective executive control functions. Inhibition was assessed using the EFT, based on a variant of the task used by Fenske and Eastwood (2003). The EFT measures individual differences in the ability to inhibit the processing of task-irrelevant emotional material. In the present version of the EFT, participants were required to categorize a target face image as happy or sad while ignoring face images simultaneously presented to the left and right of the target. The flanking face images were identical to each other but varied in valence across trial types. A measure of interference from incompatible flankers was computed, which reflected the difference in response times to trials in which the flankers were incongruent in affect to the target (i.e., sad flanking faces when the target is a happy face and vice versa) and trials in which the target was presented alone or with flankers that were the same affect as the target. The various trial types included in the EFT permitted evaluation of the extent to which peripheral stimuli in general, as well as specific types of peripheral emotional stimuli (sad and happy faces), would interfere with responding.

Monitoring and updating of working memory was assessed using the ANB developed by Levens and Gotlib (2010). The ANB measures individual differences in the ability to continually update emotional representations held in working memory. Participants indicated whether a currently presented face image displayed the same emotional expression (happy, neutral, or sad) as the face image presented two trials earlier. Thus, throughout the task participants were required to hold multiple emotional expressions in working memory and compare the current expression to the appropriate expression in working memory. On each trial of the ANB, participants updated working memory with the new emotional expression and discarded the no-longer-relevant emotional expression from three trials ago. Levens and Gotlib (2010; 2015) suggested that the various trial types of the ANB, which differ in terms of the valence of material entering and leaving working memory, permit the investigation of participants' ability to integrate and discard different types of emotional stimuli.

Set shifting was assessed using the colour-emotion task (CET). The CET was developed for the present study to measure individual differences in the ability to shift between emotion-relevant and emotion-irrelevant task sets. The CET also permitted the investigation of valence-specific shifting ability – that is, the ability to shift to a new task set when the previous stimulus processed was negative (sad face) compared to when the previous stimulus processed was positive (happy face). The emotion-relevant task set in the CET involved categorizing target face images according to the emotional expression of the face (happy or sad). The emotion-irrelevant task set involved categorizing target face images according to the colour of the frame around the image (blue or red). The target stimulus in each trial consisted of a face image with a coloured frame, and thus included both emotion-relevant and emotion-irrelevant stimulus attributes. Participants switched between the emotion-relevant and emotion-irrelevant task sets according to

the location of the computer screen in which the target image was presented. The difference in response times between trials in which there was a switch between task sets and trials in which there was no switch (i.e., the same task set as the previous trial remained active) provided a measure of the time required to reconfigure between task sets.

Face images were selected as stimuli for each of the executive control tasks based on the reasoning that facial emotion may be particularly effective for eliciting biases in executive control among depressed participants. Facial emotion provides important information about the emotional states of others, and therefore plays an instrumental role in interpersonal functioning (Bistricky, Ingram, & Atchley, 2011). It has been argued that facial affect may be a more salient and effective means of communicating emotion than words (Bistricky et al., 2011). Facial affect may be particularly salient to depression-vulnerable individuals due to their heightened concern of interpersonal rejection and resulting vigilance for signs of rejection (Joiner & Coyne, 1999). Based on similar reasoning, researchers have argued that cognitive biases among depressed individuals should be stronger for emotional faces than verbal stimuli (Gotlib, Krasnoperova, Yue, & Joormann, 2004). To the extent that this argument is valid, face images may be reliable stimuli for probing executive control biases among currently depressed, and potentially remitted depressed, individuals.

The primary hypothesis of the study was: 1) currently depressed participants would show biases in inhibition, updating, and set shifting when processing emotional material relative to non-clinical control participants. It was further hypothesized that such biases would be specific to negative material, resulting in the following sub-hypotheses: 1a) compared to control participants, currently depressed participants would have greater difficulty inhibiting the processing of negative task-irrelevant stimuli on the EFT; 1b) compared to control participants,

currently depressed participants would show greater ease in integrating negative stimuli into working memory and greater difficulty discarding negative stimuli from working memory on the ANB; and 1c) compared to control participants, currently depressed participants would have greater difficulty shifting away from a task set that requires the processing of the emotional attributes of a stimulus, and particularly when shifting is required after processing a negative stimulus, on the CET. The hypothesized biases in executive control among currently depressed participants would thus function to increase the availability and activation of negative information in working memory.

The secondary hypothesis of the study was: 2) biases in inhibition, updating, and set shifting of emotional material would be related to trait rumination, and depressive brooding in particular, when controlling for depression symptoms. Further, it was hypothesized that associations with trait rumination, and brooding especially, would be observed for negative biases in executive control. Specifically, it was hypothesized that: 2a) greater difficulty inhibiting the processing of negative task-irrelevant stimuli on the EFT would be correlated with rumination and brooding, controlling for depression; 2b) greater ease in integrating negative stimuli into working memory and greater difficulty discarding negative stimuli from working memory on the ANB would be correlated with rumination and brooding, controlling for depression; and 2c) greater difficulty shifting away from a task set that requires the processing of the emotional attributes of a stimulus, and particularly when shifting is required after processing a negative stimulus, on the CET would be correlated with rumination and brooding, controlling for depression. Thus, it was hypothesized that biases that would allow negative information increased access to and processing in working memory would be related to a greater tendency to engage in ruminative thinking, particularly in the form of depressive brooding. It was expected

that these relationships would be strongest, and therefore most likely to observed, among currently depressed participants. Given that rumination is one mechanism by which executive control biases are proposed to contribute to depression (Koster et al., 2011), relationships between rumination and executive control should be most consistently observed in a depressive state.

Given the limited prior research on executive control biases in remitted depression, no specific hypotheses were made concerning the inhibition, updating, and set shifting abilities of remitted depressed individuals when processing emotional information. If remitted depressed participants demonstrate similar biases for negative material as hypothesized for currently depressed participants, then it would suggest that executive control biases for negative material are stable vulnerability factors for depression that continue to operate outside of an active episode of depression. On the other hand, if remitted depressed participants do not show similar biases in inhibition, updating, and set shifting as hypothesized for currently depressed participants, then it would suggest that executive control biases may remit along with other depressive symptoms when an individual recovers from an active episode of depression. It should be noted that the absence of executive control biases in remitted depressed individuals would provide evidence that such biases do not actively maintain risk for future episodes of depression, but would not preclude the possibility that such biases maintain, exacerbate, or prolong ongoing episodes of depression.

The Wisconsin Card Sorting Task (WCST; Heaton, 1981; Heaton, Chelune, Talley, Kay, & Curtiss, 1993) was included in the protocol as a measure of executive control involving non-affective stimuli. The WCST consists of four key cards and 128 response cards with geometric figures that vary along the stimulus dimensions of colour, number, and shape. Participants are

required to match each of the response cards according to one of the stimulus dimensions. To match the response cards correctly, participants must determine the matching rule based on experimenter feedback about correct and incorrect responses. After a predetermined number of successive correct matches, the matching rule changes without warning, and participants must determine the new rule based on feedback. Researchers have suggested that successful performance on the WCST is dependent on higher-order executive functions such as abstract reasoning and the ability to flexibly respond to changing environmental contingencies and shift cognitive strategies (Heaton et al., 1993; Miyake et al., 2000). The WCST has been used to evaluate cognitive deficits resulting from frontal lobe damage, as well as to measure executive functioning in various populations, including depressed individuals (Channon, 1996; Fossati, Amar, Raoux, Ergis, & Allilaire, 1999; Fossati, Ergis, & Allilaire, 2001; Merriam, Thase, Haas, Keshavan, & Sweeney, 1999; Moritz et al., 2002; Must et al., 2006). The WCST was used in the present study to provide a measure of general executive functioning or control in the absence of emotional stimuli. The results of the WCST were compared to those from the measures of executive control involving emotional stimuli to aid interpretation of the study findings.

Chapter 2: Method

2.1 Participants

The sample consisted of three participant groups: 1) a currently depressed group; 2) a remitted depressed group; and 3) a non-clinical control group. Inclusion criteria for all groups were an age between 18 and 65 years inclusive and specific eligibility criteria for one of the three participant groups. Exclusion criteria for all groups were a current or past diagnosis of Bipolar Disorder or Psychotic Disorder or current Alcohol Dependence or Substance Dependence.

Participant groups were defined according to the *Diagnostic and Statistical Manual of Mental Disorders, 4th Edition* (DSM-IV; American Psychiatric Association, 2000) diagnostic criteria. However, consistent with the *Diagnostic and Statistical Manual of Mental Disorders, 5th Edition* (DSM-5; American Psychiatric Association, 2013), the bereavement exclusion for a diagnosis of MDD was omitted. Diagnosis and eligibility were determined through administration of the Mini International Neuropsychiatric Interview, Version 6.0.0 (MINI; Sheehan et al., 1998). Eligibility criteria for the participants groups were are follows. Participants in the currently depressed group met diagnostic criteria for a current Major Depressive Episode (MDE) over the past two weeks. Participants in the remitted depressed group met diagnostic criteria for a past, but not current MDE, and had a score of less than 20 on the Beck Depression Inventory-II (BDI-II; Beck, Steer, & Brown, 1996b). The non-clinical control group had no current psychiatric diagnoses and no lifetime history of MDD. Social Anxiety Disorder, Panic Disorder, Agoraphobia, Generalized Anxiety Disorder, Posttraumatic Stress Disorder, and Obsessive-Compulsive Disorder were assessed for the purpose of sample description, but were not exclusionary criteria for either the currently or remitted depressed groups.

Participants were recruited from the community primarily through public advertisements using various media. The majority of participants responded to advertisements that were delivered via social media (Facebook). Online classified ads were also posted on the Kijiji Calgary website. Recruitment posters (Appendix A) were displayed at hospitals, clinics, and public locations (e.g., grocery stores, coffee shops) in Calgary. Participants were also recruited through a pool of volunteers who had previously participated in a study related to depression in another research laboratory in the Psychology Department at the University of Calgary and consented to be contacted about future studies.

The study was completed during two in-person laboratory sessions. In the first session, the primary investigator administered the MINI to assess for inclusion and exclusion criteria and establish diagnoses. A total of 227 individuals participated in the first study session. Based on the MINI, 49 participants were excluded. Twenty-seven of those participants were excluded because they were categorized as remitted depressed based on the MINI but had a score above 20 on the BDI-II. Two participants met criteria for a current MDE in partial remission, but did not meet the full diagnostic criteria. Two participants reported depressive symptoms on the phone screen interview, but did not meet diagnostic criteria for a current or past MDE on the MINI. Fourteen participants were excluded because they met a diagnosis of Psychotic Disorder (n = 6), Bipolar Disorder (n = 4), or Alcohol or Substance Dependence (n = 4). One participant was excluded on the basis of exceeding the age limit of the study. Finally, three participants were excluded because they displayed cognitive or interpersonal deficits that interfered with completion of the MINI interview or the computer tasks. The remaining 178 participants were invited to take part in the second laboratory session. Of these, 19 participants either were unable to be scheduled after repeated attempts or declined to take part in the second session. The final

sample consisted of 53 currently depressed, 55 remitted depressed, and 51 control participants (N = 159) who completed both of the study sessions.

2.2 Measures

MINI.

The MINI was administered to participants to assess for inclusion and exclusion diagnoses, as well as to gather relevant clinical information such as co-morbid diagnoses. The MINI is a brief, structured clinical interview that assesses 16 DSM-IV Axis-I diagnoses, one Axis-II diagnosis (Antisocial Personality Disorder), and suicidality. For the purpose of the present study, the Anorexia Nervosa, Bulimia Nervosa, and Antisocial Personality Disorder modules were not administered, as these diagnoses were not exclusionary criteria and were not relevant to the current study purposes. For participants who endorsed current or past depression, the MDE module was supplemented with oral questions to gather additional information about depression course and chronicity, including age of onset, duration of longest depressive episode, duration of current depressive episode (for currently depressed participants), and duration since most recent depressive episode (for remitted depressed participants).

The MINI is the most widely used structured diagnostic interview worldwide (Medical Outcome Systems, 2014). It was developed as a brief but equally valid alternative to more lengthy structured diagnostic interviews, such as the Structured Clinical Interview for DSM-IV Axis I Disorders (SCID-I; First, Spitzer, Gibbon, & Williams, 1995) and the Composite International Diagnostic Interview (CIDI; WHO, 1990). Whereas the SCID-I and CIDI typically require 1 - 2 hours to administer, the mean duration of the MINI is approximately 16 – 21 minutes (Lecrubier et al., 1997; Pinninti, Madison, Musser, & Rissmiller, 2003; Sheehan et al., 1997). The validity of the original version of the MINI was evaluated in relation to the SCID-I

for the third edition of DSM (Spitzer, First, Gibbon, & Williams, 1990) and the CIDI. The MINI demonstrated good correspondence with the SCID-I and CIDI for the majority of diagnoses (Lecrubier et al., 1997; Sheehan et al., 1997). In particular, the kappa coefficient for the diagnosis of MDD was .84 in relation to the SCID-I and .73 in relation to the CIDI. Sheehan et al. (1998) also reported good correspondence between MINI diagnoses generated by general practitioners and expert diagnoses generated by psychiatrists in a large sample of primary care patients (K = .68 for the diagnosis of MDD). In addition, the MINI has demonstrated excellent interrater reliability, with observed kappa values of .90 or higher for the majority of diagnoses and of 1.00 for MDD (Sheehan et al., 1997). Good test-retest reliability estimates have been reported for most diagnoses (K= .83 – .87 for the diagnosis of MDD; Lecrubier et al., 1997; Sheehan et al., 1997). The MINI has been found to have good acceptance by patients (Pinninti et al., 2003).

The MINI modules were administered in order. Each diagnostic module (except for the Psychotic Disorders modules) begins with one or two screening questions that indicate the absence of the diagnosis if answered negatively. If the screening questions are answered positively, then follow-up questions corresponding to the DSM-IV diagnostic criteria for each diagnosis are administered. Each question is rated “yes” or “no” based on the interviewee’s response and the clinical judgment of the interviewer. At the end of each module the interviewer indicates whether the diagnostic criteria are met. Due to the brevity of the MINI, the interview focuses mainly on current diagnoses. Lifetime diagnoses are assessed only if they have relevance for current diagnoses; for instance, a past diagnosis of Mania or Hypomania is assessed in order to determine a current diagnosis of Bipolar Disorder. The DSM-IV criterion that symptoms must not be attributable to the physiological effects of a substance or medical condition is not

systematically assessed for each diagnostic module in the MINI. In the current study, this criterion was explored for the diagnosis of MDD only.

All MINI interviews were conducted by the principle investigator. The reliability of the diagnoses based on these interviews was evaluated by having an experienced diagnostician (Keith S. Dobson) review and score a sample of 30 audio recorded interviews (approximately equal numbers across participant groups and selected randomly within each group) without knowledge of the diagnoses provided by the principle investigator. Agreement between the primary interviewer and the second rater (K.S.D.) for inclusion and exclusion diagnoses was assessed. There was perfect correspondence for the diagnoses of current and past MDD (K = 1.00). There was also high agreement for all exclusionary diagnoses, with only one discrepancy between the raters on a diagnosis of Bipolar Disorder. While both raters noted that the participant reported manic-type symptoms, one rater judged these symptoms as failing to meet the criteria for a manic or hypomanic episode whereas the other rater made a diagnosis of "borderline" Bipolar Disorder. The discrepancy was resolved quickly through discussion; both raters agreed that the manic-type symptoms were more likely attributable to anxiety than Bipolar Disorder.

BDI-II.

The BDI-II was administered to assess participants' self-reported depressive symptoms. The BDI-II has 21 items that assess the presence and severity of various symptoms of depression over the past two weeks. Items are rated on a 4-point scale, with total scores ranging from 0 – 63. Higher scores indicate greater depressive symptomatology. Total scores are categorized as minimal depression (0 – 13), mild depression (14 – 19), moderate depression (20 – 28), and severe depression (29 – 63) (Beck et al., 1996b). Factor analyses of the BDI-II items have revealed a correlated two-factor solution, reflecting Somatic-Affective and Cognitive symptom

dimensions that comprise a higher-order factor of general depression (Beck et al., 1996b; Steer, Ball, Ranieri, & Beck, 1999).

The psychometric properties of the BDI-II are strongly supported. Internal consistency (Cronbach's alpha) estimates of .91 - .92 have been reported for outpatient samples (Beck et al., 1996b; Beck, Steer, Ball, & Renieri, 1996a). In support of its construct validity, the BDI-II shows expected convergent associations with other measures of depression, and these are larger than associations with measures of anxiety. Beck et al. (1996b) reported BDI-II correlations of r = .71 with the revised Hamilton Rating Scale for Depression (Riskind, Beck, Brown, & Steer, 1987) and r = .47 with the revised Hamilton Rating Scale for Anxiety (Riskind et al., 1987). Likewise, Steer, Ball, Ranieri, and Beck (1997) found that the BDI-II correlated more strongly with the depression subscale (r = .89) than with the anxiety subscale (r = .71) of the SCL-90-R (Derogatis, 1983). The internal consistency for the BDI-II in the current sample was α = .97.

Ruminative Responses Scale.

The Ruminative Responses Scale (RRS; Nolen-Hoeksema & Morrow, 1991) assesses the tendency to ruminate in response to a negative mood. The RRS is a 22-item self-report questionnaire that assesses rumination focused on the self, on symptoms, and on possible consequences and causes of symptoms. Items are rated on a 4-point scale (1 = almost never and 4 = almost always), with total scores ranging from 22 – 88. Higher scores indicate greater ruminative response style. In addition to the total score, the RRS items can yield two five-item subscale scores representing "Reflection" and "Brooding" (Treynor et al., 2003). The Reflection and Brooding subscales have been shown to be moderately correlated (r = .37) and demonstrate differential associations with other constructs, including depression symptoms.

The RRS demonstrates good internal consistency, with reported Cronbach's alpha values of .89 in a university student sample (Nolen-Hoeksema & Morrow, 1991) and .90 in community samples (Nolen-Hoeksema, 2000; Nolen-Hoeksema et al., 1994). The Reflection and Brooding subscales have lower internal consistencies than the total scale as expected given the five-item length of each subscale, with reported alpha coefficients of .72 and .77 respectively (Treynor et al., 2003). Test-retest correlations of .80 over five months (Nolen-Hoeksema et al., 1994) and .67 over one year (Nolen-Hoeksema, 2000) have been observed for the full scale, indicating adequate stability of RRS total scores over time and supporting the conceptualization of rumination as a trait phenomenon. Evidence for the construct validity of the RRS was reported by Nolen-Hoeksema, Morrow, and Fredrickson (1990, as cited in Nolen-Hoeksema & Morrow, 1991), who found that initial RRS scores were significantly correlated (r = .62) with reported use of rumination in a daily diary study. A recent study employing experience sampling methodology (ESM) found that initial Brooding scores were uniquely associated with ESM-measured rumination, but not with ESM-measured worry, supporting the convergent and discriminant validity of this subscale (Kircanski, Thompson, Sorensen, Sherdell, & Gotlib, 2015). The internal consistency of the total RRS scale was α = .96 in the current sample. The Reflection subscale had an internal consistency of α = .85 and the Brooding subscale had an internal consistency of α = .89, and the subscales were significantly correlated, r = .64, p < .001.

Demographic questionnaire.

A demographic questionnaire was administered to collect information on age, gender, ethnicity, and marital status (Appendix B). Questions regarding medication and therapy status and history were also asked in an interview format prior to administration of the MINI

(Appendix C). Demographic information was collected for the purpose of sample description and to test whether any relevant demographic variables differed across the participant groups.

2.3 Affective Executive Control Tasks

Equipment and stimuli.

The computer tasks were displayed on an ASUS® 27” monitor with 1920 × 1080 resolution and widescreen (16:9) aspect ratio. Participants sat approximately 46 cm away from the computer screen and used one of two different keyboards with labelled response keys. On one of the keyboards, the / key was labelled with a schematic happy face with a blue frame and the Z key was labelled with a schematic sad face with a red frame. On the other keyboard, the / key was labelled with a schematic sad face with a red frame and the Z key was labelled with a schematic happy face with a blue frame. Participants used the keyboard that corresponded to the stimulus-response mapping condition to which they were assigned, as described in the individual task descriptions. On both keyboards, the 1 key on the number pad was labelled “SAME” and the 2 key on the number pad was labelled “DIFF”; these keys were used in the ANB as described in a following section.

All of the affective executive control tasks were programmed and administered using E-Prime® 2.0 software. Happy, neutral, and sad faces from the Karolinska Emotional Directed Faces database (KDEF; Lundqvist, Flykt, & Öhman, 1998) were used as stimuli in each task. A total of 56 happy, 56 neutral, and 56 sad faces were selected for the stimuli pool, with an equal number of male and female faces per category. Within each task, stimuli were selected randomly from the stimuli pool by gender and affect. The KDEF images were cropped to display only the face, removing the hair, ears, and neck. The KDEF database has been validated by an independent research team (Goeleven, De Raedt, Leyman, & Verschuere, 2008).

Inhibition: EFT.

The EFT (Fenske & Eastwood, 2003) was used to assess inhibition of irrelevant emotional stimuli. In each trial of the EFT, a target face image expressing happy or sad affect was presented in the centre of the computer screen. Participants were instructed to indicate whether the target image was a happy or sad face by a corresponding key press. The EFT consisted of four flanker conditions: no-flanker, compatible flanker, incompatible-affect flanker, and incompatible-neutral flanker (Appendix D). For no-flanker trials, the target face image was the only image presented on the screen. For the compatible flanker, incompatible-affect flanker, and incompatible-neutral flanker trials, two identical flanking face images were presented to the left (midpoint of left image was at 25% of the x-axis of the display) and right (midpoint of the right image was at 75% of the x-axis of the display) of the target face image. The flanking face images displayed the same affect as the target face image in compatible flanker trials, incongruent affect to the target face image in incompatible-affect flanker trials (i.e., sad flanker faces when the target face image was happy and vice versa), and neutral affect in incompatible-neutral flanker trials.

Participants were told that one image would be presented in the centre of the computer screen in each trial and their task was to indicate whether the centre image was a happy facial expression or a sad facial expression. Participants were informed that on some trials, only one image would be presented in the centre of the computer screen, and on other trials, there would be two identical images of facial expressions presented on either side of the centre image. They were instructed to attend only to the centre image and identify the expression as quickly and as accurately as possible, ignoring the images presented on either side of the centre image. Half of the participants pressed the / key to indicate a happy face and the Z key to indicate a sad face,

and half of the participants used the opposite stimulus-response mapping. In each trial, a fixation cross first appeared in the centre of the computer screen for 1000 ms. Once the fixation cross disappeared, the image display appeared on the computer screen (one centre image for no-flanker trials and one centre image flanked by two identical distracter images for flanker trials). Once the participant responded to the trial by pressing a computer key, the trial ended and the images disappeared from the screen. An inter-trial interval of 1000 ms preceded the next trial. Participants' reaction times and accuracy of responses were recorded for each trial.

Participants completed 192 test trials in four blocks of 48 trials each. All face images were presented at the same size (45% of the display in height [486 pixels], and 22% of the display in width [422 pixels]) on a white background. Each block consisted of 12 no-flanker trials, 12 compatible flanker trials, 12 incompatible-affect flanker trials, and 12 incompatible-neutral flanker trials with an equal number of happy target faces and sad target faces for each of the trial types. Half of the trials consisted of male face images and half of the trials consisted of female face images. The order of trials was randomized within blocks. Prior to completing the test trials, participants completed a set of eight practice trials consisting of one of each trial type for both happy and sad target face images (randomized and consisting of an equal number of male and female face images). Participants were permitted to take a rest break after each block of trials, and pressed the space bar to begin the next block.

Dependent variables.

The EFT allows the calculation of two measures of primary interest (Fenske & Eastwood, 2003). The first is the *flanker compatibility effect*, which is the difference in response times between compatible and incompatible flanker trials. The flanker compatibility effect is an index of interference from incompatible flankers compared to performance with compatible flankers.

The inclusion of two types of incompatible flanker trials permits comparison of two flanker compatibility effect scores, one reflecting the difference in response times between compatible and incompatible-affect flanker trials and one reflecting the difference in response times between compatible and incompatible-neutral flanker trials. The flanker compatibility effect involving incompatible-neutral flankers is interpreted as a measure of the extent to which attention is attracted to the target affective face (Fenske & Eastwood, 2003). Since the same incompatible flankers (i.e., neutral faces) are used for both happy and sad target faces, any difference in flanker compatibility effects for happy and sad target faces is accounted for by the difference in affect displayed by the target faces. A smaller flanker compatibility effect indicates that attention is more strongly drawn to the target face and less influenced by the presence of peripheral stimuli. In contrast, the incompatible-affect flanker trials involve affective target faces (e.g., happy faces) and affective flanking faces (e.g., sad faces), which can both differ in their ability to attract attention. Comparison of the flanker compatibility effect involving incompatible-neutral flankers and the flanker compatibility effect involving incompatible-affect flankers for the same target faces allows investigation of the extent to which affective flankers interfere with target responding. Since the target faces are the same across both flanker compatibility effects, any difference in the effects for the incompatible-affect and incompatible-neutral flankers can be attributed to the difference in affect displayed by the flanking faces. A larger flanker compatibility effect for incompatible-affect flankers than incompatible-neutral flankers for the same target face would suggest that attention is being drawn away from the target face and allocated to the processing of the peripheral affective stimuli.

The second dependent measure generated by the EFT is the *flanker/no-flanker effect*, which is the difference in response times between no-flanker and incompatible flanker trials

(Fenske & Eastwood, 2003). The flanker/no-flanker effect is an index of interference from incompatible flankers compared to performance when peripheral stimuli are absent. Responses to trials with flankers are typically longer than responses to no-flanker trials, as peripheral information must be filtered in order to attend and respond to the target (Eriksen & St. James, 1986, as cited in Fenske & Eastwood, 2003). As with the flanker incompatibility effect, two flanker/no-flanker effect scores may be calculated, one reflecting the difference in response times between no-flanker and incompatible-neutral flanker trials and one for the difference in response times between no-flanker and incompatible-affect flanker trials. The flanker/no-flanker effect for incompatible-neutral flanker trials was used as a measure of the extent to which attention is held by the target affective face. The flanker/no-flanker effect of incompatible-affect flanker trials, relative to the flanker/no-flanker effect for incompatible-neutral flanker trials, reflected the extent to which attention was allocated to the affective flanker stimuli.

Based on the broader hypothesis that currently depressed participants would show reduced ability to inhibit the processing of negative irrelevant information, it was expected that currently depressed participants would have larger flanker incompatibility and flanker/no-flanker effects for happy target faces with incompatible-affect (i.e., sad) flankers than non-clinical control participants. This group effect was not expected to be observed for happy target faces with incompatible-neutral flankers, nor for sad target faces with incompatible-affect (i.e., happy) or incompatible-neutral flankers.

Updating: ANB.

The ANB (Levens & Gotlib, 2010) was used to assess participants' ability to update emotional material in working memory. The ANB involved a 2-back task, in which participants determined whether a presented facial expression was the same or different from the facial

expression presented two trials earlier. Participants viewed a series of happy, neutral, and sad faces presented one at a time in the centre of the computer screen. Participants were instructed to indicate as quickly as possible by key press whether the current expression matched or did not match the expression from two trials back. Participants pressed the "1" key on the number key pad (labelled "SAME") for same responses and the "2" key on the number key pad (labelled "DIFF") for different responses. Each face image was presented for 2000 ms and there was an inter-trial interval of 2000 ms. Participants' reaction times and accuracy of responses were recorded for each trial.

The ANB consists of four trial types: match-set trials, break-set trials, perseveration-set trials, and no-set trials (Levens & Gotlib, 2010; Appendix E). Match-set trials are those in which the facial expression matches the facial expression presented two trials earlier and require a *same* response. Match-set trials assess participants' ability to identify an emotional face and link the face to stimuli held in working memory that belong to the same emotional category. The remaining three trial types are those in which the facial expression does not match the facial expression presented two trials earlier and require a *different* response. A break-set trial is a trial that follows a match-set trial and requires participants to break the set that was formed on the previous trial. The facial expression presented three trials ago must be removed from working memory and replaced with the current facial expression. To respond correctly on break-set trials, participants must disconnect the matched pair held in working memory and discard the facial expression presented three trials ago from working memory, in order to add the current facial expression. Thus, break-set trials assess the ability to disengage from a previously matched facial expression. Perseveration-set trials also follow a match-set trial, but the current facial expression, which must be added to working memory, belongs to the same emotional category as the

previous matched set. As with break-set trials, participants must disconnect the matched pair held in working memory, discard the facial expression presented three trials ago, and add the current facial expression. Because the current facial expression belongs to the same emotional category as the facial expression presented three trials earlier (but not the facial expression presented two trials earlier), participants are required to not perseverate on the previous matched pair. A no-set trial is a trial that does not follow a match-set trial. Thus, participants are just required to determine that the current facial expression does not match the relevant facial expression held in working memory from two trials ago and make a response. No-set trials assess participants' ability to update working memory with a new stimulus and evaluate its relation to the other stimuli currently held in working memory.

As in Levens and Gotlib (2010), break-set trials were categorized according to the emotional expression of the set that the participant was required to break rather than of the current trial. This decision rule is because break set trials measure participants' ability to disconnect and remove affective stimuli from working memory, and thus it is the affective valence of the stimuli to be removed rather than the affective valence of the current facial expression that is of relevance to this measure. Match-set, perseveration-set, and no-set trials were categorized based on the emotional expression of the current trial.

The ANB consisted of six blocks of 55 trials each[1]. For each block, participants were instructed to view the first two trials without responding and begin the task on the third trial. Thus, 53 trials from each block were included in the data analysis. Following Levens and Gotlib (2010), participants were told that they may lose track of the sequence of trials due to the difficulty of the task, and if this occurred during the task, to re-start the task by viewing the

[1] Nineteen participants (10 currently depressed and 9 remitted depressed participants) completed an earlier version of the ANB that consisted of six blocks of 46 trials each.

following two trials and resuming responding on the third trial. Three of the blocks consisted of only male faces and three of the blocks consisted of only female faces. The order of the blocks was randomized across participants. Participants completed two sets of 18 practice trials prior to the test trials[2]. One of the practice trial sets consisted of all male faces and one consisted of all female faces, and the order of the two practice trial sets was randomized. Feedback ("correct" vs. "incorrect") was displayed on the screen after each practice trial. Feedback was not provided for the ANB test trials.

0-back task.

Prior to completing the 2-back portion of the ANB, participants completed a 0-back task (Appendix F). The 0-back task was used to assess participants' ability to perceive and categorize facial expressions and to provide a comparison for 2-back performance. The 0-back task was similar to the 2-back task, except that participants were required to indicate whether a presented facial expression was the same or different from a target emotion expression.

The 0-back task consisted of three blocks of 36 trials each. At the beginning of each block, a target facial expression label and image were presented (e.g., the label "happy" with an image of a happy face). Each block had a different target emotional expression (happy, neutral, or sad). After the presentation of the target facial expression label and image, participants completed the test trials. Participants were instructed to indicate as quickly and as accurately as possible by key press whether the emotional expression of the presented face stimulus was the same or different from the target expression. There were two blocks of male faces and one block of female faces or two blocks of female faces and one block of male faces (randomly determined across participants). The pairing of target expression (happy, neutral, or sad) and stimulus sex

[2] Nineteen participants (10 currently depressed and 9 remitted depressed participants) completed an earlier version of the ANB that included only one set of 10 practice trials, as well as a reduced number of test trials as noted.

(male or female) of the blocks was random. In each block, there were an equal number of happy, neutral, and sad faces presented in random order. Prior to beginning the 0-back test trials, participants completed six practice trials. For the practice trials, the target facial expression (happy, neutral, or sad) and stimulus sex (male or female) was randomly determined for each participant. Feedback ("correct" vs. "incorrect") was displayed on the screen after each practice trial. Feedback was not provided for 0-back test trials.

Dependent variables.

Accuracy rates and response times for each trial type (match-set, break-set, perseveration-set, and no-set trials) and affect (happy, neutral, and sad) were examined as dependent variables. For the response time variables, participants' mean response times on the 0-back task trials were subtracted from their response times for each trial type and affect. Whereas 0-back and 2-back trials both required participants to perceive and identify emotional faces, the 2-back trials also required participants to update working memory and compare a current emotional expression to an expression held in working memory. The difference in response times therefore reflected the processing time attributable to working memory monitoring and updating.[3]

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[3] Other researchers have used different procedures and indices to analyze data from the ANB. For instance, Levens and Gotlib (2010; 2015) converted response time means to *z*-scores by subtracting each participant's overall response time mean from his or her trial type response time means, and then dividing by the participant's overall response time standard deviation (calculated from the participant's trial type response time means). Everaert et al. (2016b) categorized trials according to the valence of the emotional expression entering working memory (current trial *n*) and the valence of the emotional expression to be discarded from working memory (*n*-3) on the trial. They then computed indices representing response times for adding and discarding negative stimuli to working memory relative to response times for trials for which the entering and exiting stimuli were both neutral (i.e., *WM discard negative material* index = RT to enter-neutral/discard-negative trials – RT to enter-neutral/discard-neutral trials and *WM enter negative material* index = RT to enter-negative/discard-neutral trials – RT to enter-neutral/discard-neutral trials. For completeness, and to permit comparison of the results across studies, the data were also prepared and analyzed according to the methods of Everaert et al. (2016b) and Levens and Gotlib (2010; 2015).

Based on the broader hypothesis that currently depressed participants would demonstrate biases in the updating of working memory that maintain the processing of negative information, it was hypothesized that currently depressed participants would be faster to integrate sad faces into working memory, as reflected by faster response times for sad match-set and no-set trials than happy and neutral match-set and no-set trials. It was also hypothesized that currently depressed participants would be slower to disengage from sad faces in working memory, as reflected by slower response times for sad break-set and perseveration-set trials compared to happy and neutral break-set and perseveration-set trials. No specific hypotheses were made regarding the accuracy rates. Whereas some studies have used accuracy rates on the ANB as the primary dependent variable representing individual differences in working memory updating ability (e.g., Pe et al., 2013b), others have not found depression-related differences on accuracy rates and have used response times as the primary dependent variable (e.g., Levens and Gotlib, 2010; 2015).

Shifting: CET.

The CET was used to assess participants' ability to shift between task sets, and in particular, to shift between a task in which the affective valence of the stimuli is relevant (emotion-relevant task) and a task in which the affective valence of the stimuli is irrelevant (emotion-irrelevant task). The CET was developed for the present study, adapted from a non-affective measure of task switching used by Rogers and Monsell (1995). In each trial of the CET, a single stimulus was presented that consisted of a face image with a coloured frame. Participants switched between categorizing the affective expression of the face as happy or sad (emotion task) and categorizing the colour of the frame around the image as red or blue (colour task). The stimuli were presented in each quadrant of the computer screen in a clockwise order, beginning

in the top-right quadrant of the screen (Appendix G). Stimuli in the top quadrants of the screen were presented at 25% of the y-axis of the display (measured from the top) and stimuli in the bottom quadrants of the screen were presented at 75% of the y-axis of the display. Stimuli presented in the right quadrants of the screen were presented at 68% of the x-axis (measured from the left) of the display and stimuli presented in the left quadrants of the screen were presented at 32% of the x-axis of the display. Half of the participants were instructed to indicate whether the image was a happy or sad face by a corresponding key press when the images were presented in the top-half (top-left and top-right quadrants) of the screen, and to indicate whether the colour of the frame was red or blue by a corresponding key press when the images were presented in the bottom-half (bottom-left and bottom-right quadrants) of the screen. The other half of the participants had the opposite stimulus location-task mapping and were instructed to indicate whether the image was a happy or sad face when the images were presented in the bottom-half of the screen and to indicate whether the colour of the frame was red or blue when the images were presented in the top-half of the screen. Thus, the sequence of trials was AABB and participants were required to make a predictable switch between the emotion task and the colour task on every second trial.

The attribute to which the participant was asked to respond for each task is considered the relevant attribute. The affect displayed by the face image was therefore the relevant attribute in the emotion task and the colour of the frame around the image was the relevant attribute in the colour task. The other attribute (i.e., the affect of the face image in the colour task and the colour of the frame in the emotion task) is considered the irrelevant attribute. The relevant attributes in both tasks were mapped to the same response set. Participants pressed one key if the affect of the face image was happy (in the emotion task) or if the colour of the frame was blue (in the colour

task) and another key if the affect of the face image was sad (in the emotion task) or if the colour of the frame was red (in the colour task). As in the Rogers and Monsell (1995) task, there were two conditions – the no-crosstalk condition and the crosstalk condition (Appendix G). Participants were not explicitly informed that there were two conditions or of the differences between the two conditions. In the no-crosstalk condition, the irrelevant attribute was not associated with any response, and thus was considered neutral. In the emotion task, the colour of all of the frames around the images was green, and in the colour task, the affect displayed by all of the face images was neutral. Because the irrelevant attribute was not associated with any response in either task, each stimulus could only be categorized according to the relevant attribute. In the crosstalk condition, the irrelevant attribute was neutral in one-third of the trials. In the other two-thirds of trials, the irrelevant attribute was associated with a response in the currently inappropriate task. In one-third of the trials, the relevant and irrelevant attribute were associated with the same response in both tasks (i.e., a happy face image with a blue frame or a sad face image with a red frame), and these were thus considered congruent trials. In the remaining third of the trials, the irrelevant attribute was associated with the opposite response in the currently inappropriate task as the relevant attribute in the appropriate task (i.e., a happy face image with a red frame or a sad face image with a blue frame). These trials were thus considered incongruent trials. Responses in the crosstalk condition are typically longer than responses in the no-crosstalk condition, because the presence of an irrelevant attribute that is associated with a response in the currently inappropriate task makes that task harder to inhibit (Rogers & Monsell, 1995). Although all switch tasks require the inhibition of the currently inappropriate task set, performance in the crosstalk condition may be particularly affected by participants' ability to suppress the inappropriate task set and corresponding responses. The no-crosstalk condition may

therefore be considered a “purer” measure of switching ability, whereas performance in the crosstalk condition may require a greater degree of inhibition ability in addition to switching.

Participants were told that they would be presented with images in each of the four quadrants of the computer screen, one at a time and in clockwise order. Half of the participants were instructed to press the / key if the facial expression was happy in the emotion task or if the colour of the frame was blue in the colour task, and to press the Z key if the facial expression was sad in the emotion task or if the colour of the frame was red in the colour task. The other half of the participants received the opposite stimulus-response mapping instructions (i.e., / key for sad face or red frame and Z key for happy face or blue frame, in the appropriate tasks). Participants were instructed to respond as quickly and as accurately as possible. In each trial of the CET, the stimulus was presented for 5000 ms or until the participant responded by key press. There was then an inter-trial interval of 500 ms before the next stimulus was presented. Participants’ reaction times and accuracy of responses were recorded for each trial.

The no-crosstalk and crosstalk blocks consisted of 120 trials each. In the no-cross talk block, the emotion task consisted of an equal number of trials with happy and sad faces and all of the image frames were green. The colour task consisted of an equal number of trials with blue and red image frames and all of the faces displayed neutral affect. The order of trials within the emotion task and within the colour task was randomized, and the trial types were equally divided across stimulus location (right or left quadrant). In the crosstalk block, the emotion task consisted of an equal number of trials with each combination of the following stimulus variables: face image affect (happy or sad) and image frame colour (blue, red, or green). The colour task consisted of an equal number of trials with each combination of the following stimulus variables: image frame colour (blue or red) and face image affect (happy, sad, or neutral). Again, the order

of trials within the emotion task and within the colour task was randomized, and the trial types were equally divided across stimulus location (right or left quadrant). Participants completed the no-crosstalk and crosstalk blocks in random order. One of the blocks involved all female face stimuli and one of the blocks involved all male face stimuli, and the pairing of the face image sex (female or male) with the block condition (no-crosstalk or crosstalk) was randomized across participants. Participants were permitted a rest break between the blocks of trials, and pressed the space bar to begin the next block. Participants completed 12 practice trials from either the no-crosstalk or crosstalk condition (randomly determined) prior to beginning the test trials.

Dependent variables.

The primary dependent variable generated by the CET is the switch cost, which is the difference in response times between trials that require a switch between task sets (i.e., AB, BA) and trials that are otherwise identical but do not require a switch between task sets (i.e., BB, AA). Response times are typically slower for switch trials compared to no-switch trials because participants must reconfigure between task sets and suppress the now inappropriate task set to make a correct response in the currently appropriate task (Monsell, 2003). To the extent that it takes participants longer to suppress the inappropriate task set and/or shift to the appropriate task set, switch costs will be greater.

Separate switch costs were calculated for the emotion task and the colour task. The switch cost for the emotion task reflects the difference in response times when performing the emotion task that is due to shifting *away* from the colour task set. Likewise, the switch cost for the colour task reflects the difference in response times when performing the colour task that is due to shifting *away* from the emotion task. Larger switch costs for the colour task compared to the emotion task would reflect greater difficulty shifting away from an emotion-relevant task to

an emotion-irrelevant task. Switch costs were also compared for shifting away from positive versus negative stimuli. Separate switch costs were calculated for trials in which the previous stimulus involved a happy face and for trials in which the previous stimulus involved a sad face, for both the emotion task and the colour task. Note that these switch costs could only be calculated for the crosstalk condition, since the previous target was always a neutral face on switch trials in the emotion task and on no-switch trials in the colour task.

It was hypothesized that currently depressed participants would have more difficulty switching from the emotion-relevant task to the emotion-irrelevant task compared to control participants, which would be indicated by greater switch costs for the colour task. In addition, it was hypothesized that in the crosstalk condition currently depressed participants would have particular difficulty switching from the emotion-relevant task to the emotion-irrelevant task when the previous target face was sad.

Pre-experimental training: No-switch blocks.

Prior to the no-crosstalk and crosstalk blocks, participants completed one block of each task (colour task and emotion task) to allow sufficient practice of the stimulus-response mappings for each task, as recommended by Rogers and Monsell (1995). Each of the no-switch blocks consisted of 60 trials. In each block, the images were presented either only on the top-half of the screen or on the bottom-half of the screen, alternating between the right and left quadrants. Thus, participants completed only one task in each of the blocks and were not required to switch between task sets. Half of the participants completed the emotion task when the stimuli were presented on the top-half of the screen and the colour task when the stimuli were presented on the bottom-half of the screen, and half of the participants had the opposite stimulus location-task mapping. The stimulus location-task mapping for each participant was the same as in the

subsequent switch blocks. The order of completion of the emotion and colour tasks was random. For each participant, one of the no-switch blocks consisted of only male face images and one of the no-switch blocks consisted of only female face images. The pairing of the face image sex (female or male) with the task set (emotion or colour task) was randomly determined across participants. Participants were permitted to take a rest break between the blocks of trials, and pressed the space bar to begin the next block. Participants completed six practice trials prior to beginning the test trials for each no-switch block.

Trial types within the no-switch blocks were the same as in the crosstalk block. That is, the emotion task consisted of an equal number of trials with each combination of the following stimulus variables: face image affect (happy or sad) and image frame colour (blue, red, or green), and the colour task consisted of an equal number of trials with each combination of the following stimulus variables: image frame colour (blue or red) and face image affect (happy, sad, or neutral). The order of trials within each task was randomized, and the trial types were equally divided across stimulus location (right or left quadrant). The no-switch blocks were employed to increase participants' familiarity with the stimulus-response mappings for each task, and thus responses on the no-switch blocks were not analyzed as dependent variables.

2.4 Non-Affective Executive Functioning Task: WCST

A computerized version of the WCST (Heaton, 1981; Heaton et al., 1993) was administered as a measure of executive functioning involving non-affective stimuli. In the computerized version of the task, four key cards are presented in a row at the top of the computer screen, displaying one red triangle, two green stars, three yellow crosses, and four blue circles, respectively. In each trial, a response card is presented on the screen below the key cards with figures that vary along the following dimensions: colour (red, green, yellow, or blue), number

(one, two, three, or four), and shape (triangle, star, cross, or circle). Each response card can be matched to the key cards according to one, two, or all three dimensions. Participants were instructed to match each of the response cards to one of the four key cards by pressing a corresponding key. Participants received computer feedback after each trial about whether the response was correct or incorrect.

The task of the participant in the WCST is to determine what rule, or category, should be used to match the response cards to the key cards. The first category according to which participants were to match the cards was colour. Thus, participants received "right" feedback when they correctly matched response cards to the same-coloured key card and "wrong" feedback when they attempted to match response cards to key cards based on any other dimension. After the participant correctly matched 10 successive cards, the matching rule changed to shape, although participants were not informed of this change. Participants then received "right" feedback when they correctly matched response cards to the same-shape key card and "wrong" feedback when they attempted to match response cards to key cards based on any dimension other than shape. After 10 successive correct matches, the matching rule changed to number. This pattern was repeated until the participant successfully completed six matching categories (colour, shape, number, colour, shape, number) or until the participant responded to 128 response cards, whichever occurred first.

Prior to completing the WCST, instructions were provided to participants verbally by the experimenter. The instructions were adapted from the WCST manual to be appropriate for the computerized version of the task. Although participants were aware that the task was to match the response cards to one of the key cards, they were not informed about the nature of the matching rules or that the rule would change throughout the task. The Wisconsin Card Sorting

Test® Computer Version 4 (WCST:CV4™) – Research Edition software by PAR Psychological Assessment Resources, Inc. was used for administration and scoring of the WCST.

Dependent variables.

Several scores on the WCST were examined as dependent variables. *Number of Trials* was the total number of trials completed by the participant during the task. The minimum number of trials in which the WCST can be completed is 60, although this outcome would require the participant to correctly match every response card, including correctly anticipating matching rule changes. The maximum number of trials on the WCST is 128, which may include successful completion of any number of matching categories from 0 to 6. A smaller number of trials indicates that the participant required fewer trials to complete all six matching categories, and thus reflects better performance on the task. *Number of Perseverative Errors* was the total number of incorrect responses that were perseverative in nature. Perseverative errors occur when a participant attempts to match a response card on the basis of a stimulus characteristic that has been established as incorrect, and reflect a failure to inhibit an old matching principle. *Number of Non-Perseverative Errors* was the total number of incorrect responses that were not perseverative in nature. A greater number of perseverative and non-perseverative errors reflect worse performance on the task. *Failure to Maintain Set* was the number of times a participant made an error before completing a category after making five or more consecutive correct matches during that category. A failure to maintain set indicates that a participant correctly identified the matching rule but then switched to an incorrect rule, and may reflect distractibility. A higher failure to maintain set reflects worse performance on the task.

2.5 Procedure

The majority of individuals self-referred to the study based on the study advertisements. Trained research assistants conducted phone screen interviews with prospective participants to provide information about the study and assess for inclusion and exclusion criteria. A small subset of participants were recruited through a pool of volunteers who had previously participated in a study related to depression in another research laboratory in the Psychology Department at the University of Calgary and consented to be contacted about future studies. These individuals did not complete a phone screen as diagnostic information was available from the previous study. Instead, an e-mail was sent to these individuals to provide them with information about the study and invite them to participate. Individuals who were eligible for the study based on the phone screen and who were willing to participate, as well as those who responded to the e-mail invitation, were scheduled to participate in the study at the Depression Research Laboratory at the University of Calgary.

The study was conducted over two individual sessions that were approximately 60 to 90 minutes each in duration. At the beginning of each session, participants read a study information sheet that included a brief description of the study purpose and protocols, as well as information about the risks and benefits of participation and the collection and storage of data, and then provided consent to participate (Appendix H). In the first study session, participants were administered questions about therapy and medication status and history and then the MINI diagnostic interview to assess for inclusion and exclusion criteria, establish group status (i.e., currently depressed, remitted depressed, or control), and evaluate additional diagnoses. Following completion of the MINI, participants completed a battery of self-report questionnaires via Qualtrics© survey software, which included the BDI-II, RRS, and demographic

questionnaire, as well as additional questionnaires that assessed anxiety symptoms, avoidance, and emotion regulation strategies but were not relevant to the present hypotheses. The demographic questionnaire was always presented first, with the remaining questionnaires presented in random order. Finally, participants completed the computerized WCST. Participants who were eligible based on study inclusion and exclusion criteria following the first session, and who were willing to continue their participation, were scheduled for the second study session within two weeks. The two-week timeframe was selected to correspond with the duration criteria for a diagnosis of MDD and for remission (Frank et al., 1991). The majority of the sample (84%) completed the second study session within two weeks of the first (M = 1.44 weeks; SD = 0.77 weeks; range = 0.28 – 4.84 weeks).

In the second study session, participants first completed a questionnaire that assessed mood state but was not used to evaluate the present study hypotheses. Next, participants completed the three affective executive functioning tasks in the following order: EFT, CET, ANB. Participants were assigned to one of four counterbalance conditions that corresponded to stimulus-response and stimulus location-task mappings in the affective executive functioning tasks. The CET involved two stimulus-response mappings (/ key for happy face or blue frame and Z key for sad face or red frame; or / key for sad face or red frame and Z key for happy face or blue frame, in the appropriate tasks) and two stimulus location-task mappings (emotion task when images were presented in the top-half of the screen and colour task when images were presented in the bottom-half of the screen; or colour task when images were presented in the top-half of the screen and emotion task when images were presented in the bottom-half of the screen). Thus, the four counterbalance conditions corresponded to each combination of the stimulus-response and stimulus location-task mappings. The EFT also involved two stimulus-

response mappings (/ key for happy face and Z key for sad face; or / key for sad face and Z key for happy face). Participants always used the same stimulus-response mapping for both the EFT and the CET to prevent interference from incongruent stimulus-response mappings across the tasks. Written task instructions were presented on the computer screen at the beginning of each task, and the experimenter was also available to clarify the instructions and answer any questions. Due to the increased difficulty level of the ANB, in addition to the written instructions the experimenter reiterated the instructions verbally and confirmed participants' understanding for this task.

Participants were fully debriefed and provided the opportunity to ask questions at the end of each study session. In addition to the debriefing letter, information regarding online and community resources for depression information and treatment were provided to participants (Appendix I). Participants received a $25 gift card for each study session they attended. The primary investigator conducted all of the first study sessions, and trained research assistants conducted the second study sessions.

Chapter 3: Results

All a-priori study hypotheses were tested at a significance level of $p < .05$. A significance level of $p < .10$ was used to define marginally significant effects. Full test statistics are reported for all significant or marginally significant effects. For brevity, all non-significant effects are noted as non-significant (i.e., $p > .10$) without reporting the full test statistics.

3.1 Demographic and Treatment Information

The sample consisted of 53 currently depressed, 55 remitted depressed, and 51 non-clinical control participants ($N = 159$). The sample size was based on a priori statistical power analyses to provide a power level of approximately .80 to detect a difference between the currently depressed and control groups on the executive control tasks of the magnitude generally observed in prior research (e.g., $d = \sim.50 – 1.00$; Levens & Gotlib, 2010; Zetsche et al., 2012). Demographic information for the full sample and the separate participant groups is presented in Table 1. One participant in the control group was missing a value for age, and thus is not included in the descriptive or between-groups analyses of this variable. Univariate analysis of variance (ANOVA) or a chi-square test, as appropriate, was applied to each demographic variable to test for differences across participant groups. The currently depressed, remitted depressed, and control participants did not differ significantly in terms of age, gender, ethnicity, or level of education, all *p*s > .10 (see Table 1). The chi-square test comparing groups on marital status was significant, $\chi^2 (4) = 16.59$, $p = .002$. Inspection of the standardized residuals indicated that a greater proportion of currently depressed participants were divorced, separated, or widowed ($z = 2.8$) and a smaller proportion of remitted depressed participants were divorced, separated, or widowed ($z = -2.3$), than would be expected under the null hypothesis of independence between participant group and marital status. Given that there was no theoretical

Table 1

Demographic information across participant groups and full sample.

Variable	Currently Depressed	Remitted Depressed	Control	Full Sample	F / $\chi 2$
Age, *M (SD)*	38.87 (13.65)	34.91 (14.90)	32.66 (14.68)	35.53 (14.55)	2.46
Gender, *n* (%)					
Female	45 (84.9)	48 (87.3)	41 (80.4)	134 (84.3)	.97
Male	8 (15.1)	7 (12.7)	10 (19.6)	25 (15.7)	
Marital Status, *n* (%)					
Never married	20 (37.7)	26 (47.3)	27 (52.9)	73 (45.9)	16.59*
Married/ Common law	19 (35.8)	28 (50.9)	19 (37.3)	66 (41.5)	
Divorced/ Separated/ Widowed	14 (26.4)	1 (1.8)	5 (9.8)	20 (12.6)	
Ethnicity, *n* (%)					
Caucasian	36 (67.9)	41 (74.5)	29 (56.9)	106 (66.7)	24.06
Asian	3 (5.7)	3 (5.5)	9 (17.6)	15 (9.4)	
South Asian	1 (1.9)	7 (12.7)	6 (11.8)	14 (8.8)	
Latin American	3 (5.7)	0	3 (5.9)	6 (3.8)	
West Indian	3 (5.7)	2 (3.6)	0	5 (3.1)	
Filipino	3 (5.7)	1 (1.8)	0	4 (2.5)	
First Nations	1 (1.9)	0	2 (3.9)	3 (1.9)	
Black	1 (1.9)	0	1 (2.0)	2 (1.3)	
Other	2 (3.8)	1 (1.8)	1 (2.0)	4 (2.5)	
Education, *n* (%)					
High school or less	9 (17.0)	6 (10.9)	10 (19.6)	25 (15.7)	7.08
Some community/ technical college or university	17 (32.1)	22 (40.0)	12 (23.5)	51 (32.1)	
Completed community/ technical college or Bachelor's degree	23 (43.4)	21 (38.2)	19 (37.3)	63 (39.6)	
Master's or professional degree	4 (7.5)	6 (10.9)	10 (19.6)	20 (12.6)	

Note. * $p < .05$.

or prior empirical basis to expect marital status to be related to executive functioning beyond its shared variance with depression, and that cell sizes were not sufficient for ANOVA in all cases (i.e., ≤ 5 for remitted depressed participants who were divorced, separated, or widowed and control participants who were divorced, separated, or widowed), marital status was not included as a factor in the primary analyses of dependent variables.

Clinical characteristics and treatment status and history across the participant groups were collected for descriptive purposes and are presented in Table 2. Five participants in the remitted depressed group were missing data on age of first onset of MDD, length of longest MDE, and weeks since last MDE as these questions were implemented into the protocol after data collection had begun. The currently depressed participants had a mean BDI-II score in the "severe" range, whereas both remitted depressed and control participants reported "minimal" depression (Beck et al., 1996b). As expected, a univariate ANOVA determined that BDI-II scores differed significantly across participant groups, F (2, 156) = 254.07, $p < .001$, $\eta_p^2 = .77$. Follow-up Tukey tests confirmed that currently depressed participants had greater depressive symptoms than remitted depressed participants, mean difference = 23.06, SE = 1.34, $p < .001$, who in turn had greater depressed symptoms than control participants, mean difference = 5.97, SE = 1.36, $p < .001$. Total rumination as measured by the RRS also differed significantly across participant groups, F (2, 156) = 95.03, $p < .001$, $\eta_p^2 = .55$. Tukey tests indicated that currently depressed participants reported greater rumination than remitted depressed participants, mean difference = 8.54, SE = 2.31, $p = .001$, who in turn reported greater rumination than control participants, mean difference = 22.85, SE = 2.33, $p < .001$. A similar group effect was observed for both reflection, F (2, 156) = 27.57, $p < .001$, $\eta_p^2 = .26$, and brooding, F (2, 156) = 63.05, $p < .001$, $\eta_p^2 = .45$. Currently depressed participants reported both greater reflection, mean difference

Table 2

Clinical characteristics across participant groups.

Variable	Currently Depressed	Remitted Depressed	Control
Depression symptoms (BDI-II), *M (SD)*	31.70 (9.22)	8.64 (6.76)	2.67 (3.71)
Rumination (RRS), *M (SD)*	62.94 (11.30)	54.40 (15.17)	31.55 (8.17)
Reflection, *M (SD)*	13.13 (3.41)	11.25 (4.26)	7.82 (3.28)
Brooding, *M (SD)*	13.92 (3.66)	12.25 (3.90)	6.94 (1.94)
Age of onset of first MDE, *M (SD)*	17.96 (9.97)	22.22 (10.29)	-
# Previous MDEs, *Median (Interquartile Range)*	3 (1 – 8)	3 (1 – 5)	-
Length of current MDE in weeks, *Median (Interquartile Range)*	63 (11– 322.75)	-	-
Chronic MDD (> 2 years)			
Yes, *n* (%)	24 (45.3)	-	-
No, *n* (%)	29 (54.7)		
Length of longest MDE in weeks, *Median (Interquartile Range)*	156 (41 – 520)	34.5 (17.75 – 82.25)	-
Weeks since last MDE, *Median (Interquartile Range)*	-	42 (16.75 – 96.25)	-
Current therapy+			
Yes, *n* (%)	18 (34.0)	13 (23.6)	0 (0.0)
No, *n* (%)	35 (66.0)	42 (76.4)	51 (100.0)
Therapy ever+			
Yes, *n* (%)	48 (90.6)	46 (83.6)	17 (33.3)
No, *n* (%)	5 (9.4)	9 (16.4)	34 (66.7)

Variable	Currently Depressed	Remitted Depressed	Control
Current antidepressant use			
Yes, *n* (%)	33 (62.3)	27 (49.1)	0 (0.0)
No, *n* (%)	20 (37.7)	28 (50.9)	51 (100.0)
Antidepressant use ever			
Yes, *n* (%)	44 (83.0)	40 (72.7)	0 (0.0)
No, *n* (%)	9 (17.0)	15 (27.3)	51 (100.0)

Note. MDE = Major Depressive Episode. +Therapy refers to professional therapy or counselling for any reason.

= 1.88, *SE* = .71, *p* = .024, and brooding, mean difference = 1.67, *SE* = .64, *p* = .026, than remitted depressed participants. In turn, remitted depressed participants reported both greater reflection, mean difference = 3.43, *SE* = .72, *p* < .001, and brooding, mean difference = 5.31, *SE* = .64, *p* < .001, than control participants.

There was considerable variability among the currently and remitted depressed groups on number of previous MDEs, length of current MDE, length of longest MDE, and weeks since last MDE. Due to the skewed nature of these variables, medians and interquartile ranges are presented in Table 2 instead of means and standard deviations. Of note, slightly less than half of the sample of currently depressed participants (45.3%) reported a current MDE lasting longer than 2 years, or chronic MDD. The median length of current MDE among currently depressed participants was 63 weeks. Among remitted depressed participants, the median duration of time since most recent MDE was 42 weeks. About one-third of currently depressed participants (34.0%) and one-quarter of remitted depressed participants (23.6%) were currently in therapy, whereas no control participants reported current therapy. The large majority of currently (90.6%) and remitted depressed (83.6%) participants reported that they had received therapy at any point in their lives, whereas about one-third of control participants (33.3%) reported that they had ever received therapy. Almost two-thirds of currently depressed participants (62.3%) and about one-half of remitted depressed participants (49.1%) were currently taking antidepressant medication, and the majority of currently (83.0%) and remitted depressed (72.7%) participants reported taking antidepressant medication at some point in their lives. No control participants reported any current or past antidepressant medication use.

Nearly two-thirds of currently depressed participants (66.0%) reported at least one comorbid diagnosis, and 37.7% reported two or more comorbid diagnoses (Table 3). Of the

Table 3

Comorbid diagnoses across participant groups.

Diagnosis	Currently Depressed	Remitted Depressed	Control
Panic Disorder, *n* (%)	8 (15.1)	0 (0.0)	0 (0)
Agoraphobia, *n* (%)	12 (22.6)	3 (5.5)	0 (0.0)
Social Anxiety Disorder, *n* (%)	13 (24.5)	3 (5.5)	0 (0.0)
Obsessive-Compulsive Disorder, *n* (%)	6 (11.3)	2 (3.6)	0 (0.0)
Posttraumatic Stress Disorder, *n* (%)	11 (20.8)	0 (0.0)	0 (0.0)
Generalized Anxiety Disorder, *n* (%)	20 (37.7)	7 (12.7)	0 (0.0)
Any comorbid diagnosis, *n* (%)	35 (66.0)	13 (23.6)	0 (0.0)
2+ comorbid diagnoses, *n* (%)	20 (37.7)	2 (3.6)	0 (0.0)

remitted depressed group, almost one-quarter (23.6%) reported at least one comorbid diagnosis and only two participants reported two or more comorbid diagnoses. Generalized anxiety disorder, social anxiety disorder, and agoraphobia were the most common comorbid diagnoses in both the currently depressed and remitted depressed groups. By definition, the non-clinical control group did not report any psychiatric diagnoses.

3.2 Inhibition (EFT)

Statistical analysis.

One participant had missing data on the EFT because of a computer malfunction. Three participants were excluded from data analysis involving the EFT because they had mean error rates that exceeded the sample mean by more than 3 SDs. The mean error rate for the remaining participants was 1.5%. Response times were analyzed for correct trials only. Prior to calculating mean response times, trials with response times that were greater than 3 SDs away from the participant's own mean for all trials were identified as trial outliers and removed from the data (2.0% of all trials). Mean response times for each flanker condition and target affect were then computed (see Table 4 for mean response times across participant groups).

As described in the Methods section, separate flanker compatibility effect scores for happy target faces and sad target faces were generated for each participant by subtracting the mean response time for compatible flanker trials from the mean response times for the incompatible-affect and incompatible-neutral flanker trials involving the same target affect. Separate flanker/no-flanker effect scores for happy target faces and sad target faces were also computed for each participant by subtracting the mean response time for the no-flanker trials from the mean response time for the incompatible-affect and incompatible-neutral flanker trials involving the same target affect. Thus, four flanker compatibility effect scores and four

Table 4

EFT mean response times (in ms) in each flanker condition across participant groups.

	Currently depressed	Remitted depressed	Control
		Happy target	
No flanker	795.86 (227.49)	730.83 (152.97)	673.91 (156.85)
Compatible	815.10 (227.34)	747.65 (162.10)	705.32 (182.21)
Incompatible-neutral	827.11 (233.18)	755.36 (161.37)	702.22 (176.97)
Incompatible-affect	841.07 (250.61)	757.48 (169.40)	702.95 (187.43)
		Sad target	
No flanker	817.44 (258.76)	716.04 (135.37)	695.39 (164.79)
Compatible	837.96 (259.82)	768.55 (164.91)	737.87 (213.44)
Incompatible-neutral	845.22 (259.25)	744.99 (137.61)	709.85 (170.43)
Incompatible-affect	838.59 (263.40)	773.33 (171.83)	722.52 (195.58)

Note. Standard deviations are presented in parentheses. Participants who were outliers on error rates are excluded (n = 3).

flanker/no-flanker effect scores were computed, consisting of every combination of flanker type (incompatible-affect, incompatible-neutral) and target affect (happy, sad). Participant outliers were defined as participants who had flanker compatibility effect scores or flanker/no-flanker effect scores that were greater than 3 SDs away from the mean of the participant group to which they belonged, for each target valence. Based on this criterion, seven participants were identified as outliers on the flanker compatibility effect scores and six participants were identified as outliers on the flanker/no-flanker effect scores; these participants were removed from the analyses involving the respective dependent variables.

Separate 3 (Group: Currently Depressed vs. Remitted Depressed vs. Control) × 2 (Target Affect: Happy vs. Sad) × 2 (Flanker Type: Incompatible-Affect vs. Incompatible-Neutral) ANOVAs were conducted on the flanker compatibility and flanker/no-flanker effect scores. Main effects were followed by pairwise comparisons with corrections for family-wise error rate and interactions were explored further using appropriate tests of simple effects. The assumption of homogeneity of variance was evaluated for the flanker compatibility and flanker/no-flanker effect scores by examining the ratio of the largest cell variance to the smallest cell variance (F_{max}) for each variable. An F_{max} of 10 or less indicates adequate homogeneity of variance across participant groups when cell sizes are approximately equal (Tabachnick & Fidell, 2007), as was the case in the present study. For each of the flanker compatibility and flanker/no-flanker effect scores, F_{max} was less than 6, indicating no violation of the assumption of homogeneity of variance. The results regarding the EFT did not differ across the two stimulus-response mappings used in the task; inclusion of counterbalance as a between-subjects factor in the ANOVAs did not change any of the effects.

Flanker compatibility effect across participant groups.

The ANOVA conducted on the flanker compatibility effect scores revealed a marginally significant main effect of group, F (2, 145) = 2.93, p = .057, η_p^2 = .04, and a significant main effect of target affect, F (1, 145) = 11.53, p = .001, η_p^2 = .07, which were qualified by a significant three-way interaction between target affect, flanker type, and group, F (2, 145) = 4.02, p = .020, η_p^2 = .05.

To facilitate interpretation of the three-way interaction, separate 3 (Group) × 2 (Target Affect) ANOVAs were conducted on the flanker compatibility effect scores for each flanker type. For incompatible-affect flankers, there was a main effect of target affect, F (1, 145) = 7.28, p = .008, η_p^2 = .05, such that the flanker compatibility effect was smaller for sad target faces (M = -5.05 ms) than for happy target faces (M = 12.17 ms). There was also a marginally significant Group × Target Affect interaction, F (2, 145) = 2.61, p = .077, η_p^2 = .04, which was followed up with separate one-way ANOVAs with group as the independent variable for happy and sad target faces. For happy target faces with incompatible-affect (i.e., sad) flankers, there was a significant main effect of group, F (2, 145) = 3.82, p = .024, η_p^2 = .05. Follow-up Tukey tests revealed that currently depressed participants had a mean flanker compatibility effect that was significantly larger than that of control participants, mean difference = 32.35 ms, SE = 12.52 ms, p = .029, and marginally larger than that of remitted depressed participants, mean difference = 25.94 ms, SE = 12.08 ms, p = .084. Remitted depressed and control participants did not significantly differ on their flanker compatibility effect for happy target faces with incompatible-affect flankers, p > .10. For sad target faces with incompatible-affect (i.e., happy) flankers, the effect of group was non-significant, p > .10.

The 3 (Group) × 2 (Target Affect) ANOVA for incompatible-neutral flankers revealed a significant main effect of target affect, F (1, 145) = 7.73, p = .006, η_p^2 = .05, which again reflected a smaller overall flanker compatibility effect for sad faces (M = -11.47 ms) than happy faces (M = 7.28 ms). The main effect of group and the Group × Target Affect interaction were non-significant, ps > .10. The flanker compatibility effect scores for each target affect and flanker condition by participant group are displayed graphically in Figure 1.

Flanker/no-flanker effect across participant groups.

The ANOVA conducted on the flanker/no-flanker effect scores revealed a marginally significant Group × Target Affect × Flanker Type interaction, F (2, 146) = 2.54, p = .083, η_p^2 = .03. No other main or interaction effects were significant in the model, ps > .10. To facilitate interpretation of the interaction, separate 3 (Group) × 2 (Target Affect) ANOVAs were conducted on the flanker/no-flanker effect scores for each flanker type. For incompatible-affect flankers, there was a marginally significant main effect of Group, F (2, 146) = 2.74, p = .068, η_p^2 = .04, which was qualified by a significant Group × Target Affect interaction, F (2, 146) = 4.73, p = .010, η_p^2 = .06. Separate one-way ANOVAs for happy and sad target faces with group as the independent variable revealed a significant effect of group for happy target faces, F (2, 146) = 3.39, p = .036, η_p^2 = .04, and for sad target faces, F (2, 146) = 4.20, p = .017, η_p^2 = .05. Follow-up Tukey tests showed that currently depressed participants had flanker/no-flanker effects for happy target faces with incompatible-affect (i.e., sad) flankers that were significantly larger than control participants, mean difference = 30.39 ms, SE = 12.61 ms, p = .045. The flanker/no-flanker effect for happy target faces did not differ significantly between currently and remitted depressed participants or between remitted depressed and control participants, ps > .10. For sad target faces with incompatible-affect (i.e., happy) flankers, remitted depressed participants had

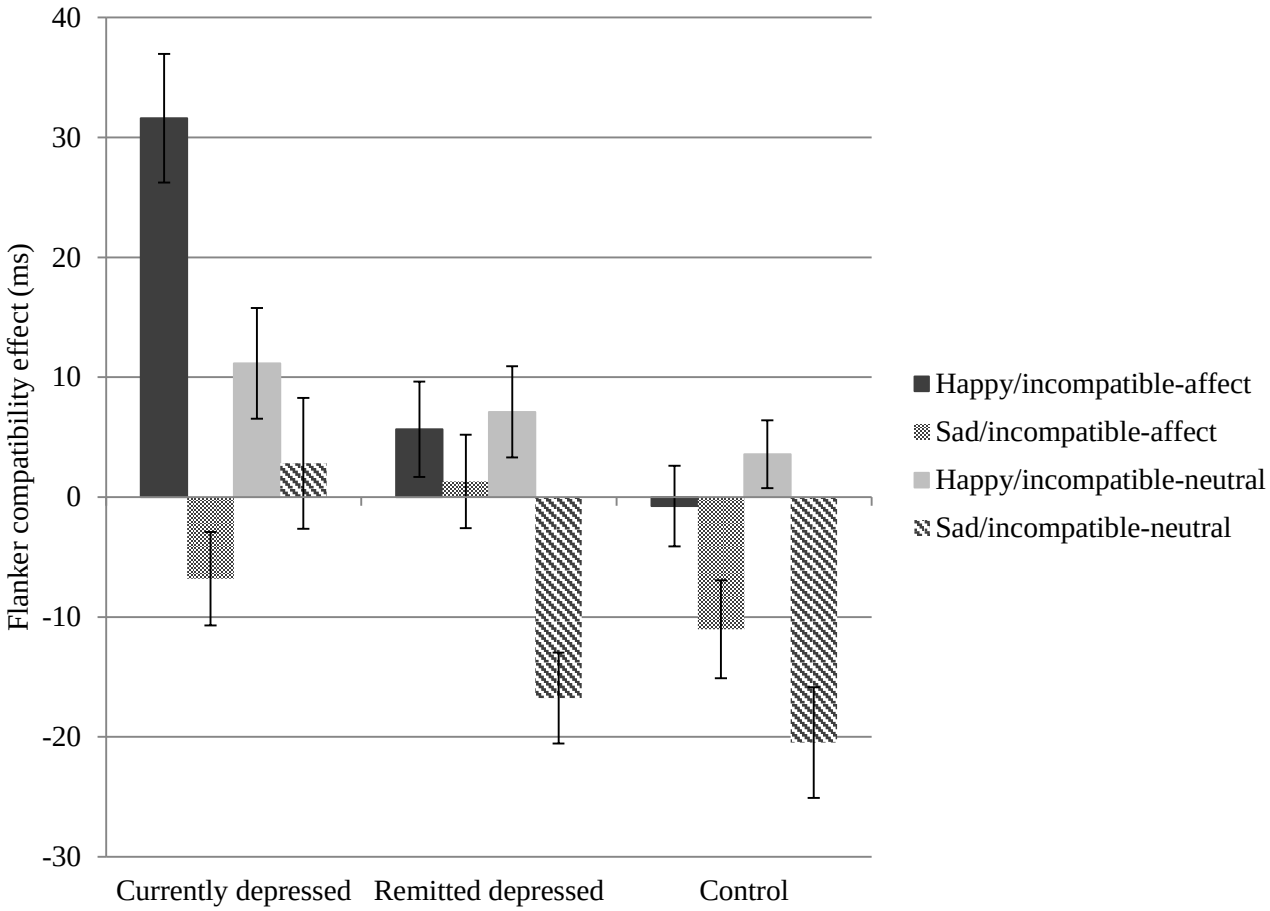

Figure 1. EFT flanker compatibility effect scores for each of the trial types across participant groups. Errors bars represent one standard error. Participants who were outliers on error rates (*n* = 3) or flanker compatibility effect scores (*n* = 7) are excluded.

flanker/no-flanker effects that were significantly larger than control participants, mean difference = 29.19 ms, *SE* = 11.28 ms, *p* = .028, and marginally larger than currently depressed participants, mean difference = 26.23 ms, *SE* = 11.09 ms, *p* = .050, whereas control and currently depressed participants did not differ from one another, *p* > .10. The ANOVA conducted on the incompatible-neutral flankers revealed no significant main or interaction effects, *p*s > .10. The flanker/no-flanker effect scores for each target affect and flanker condition by participant group are displayed graphically in Figure 2.

In sum, the EFT results indicate that the response times of currently depressed participants to happy target faces are particularly slowed by the presence of sad distracting faces relative to control participants, but not by the presence of neutral distracting faces. In contrast, remitted depressed participants' response times to sad target faces are particularly influenced by positive but not neutral distracting faces, compared to currently depressed and control participants.

3.3 Monitoring and Updating of Working Memory (ANB)

Statistical analysis.

One participant had missing data on the ANB because of a computer malfunction, and two participants had considerable difficulty with the task and aborted the ANB. Data from these three participants are not included in the ANB analyses. For each participant, mean accuracy rates and response times for each trial type were calculated and examined as dependent variables. For the accuracy rates, participant outliers were defined as participants who had mean accuracy rates that were greater than 3 SD away from the mean of the participant group to which they belonged, for each trial type. Seven participants were identified as outliers and removed from data analyses involving the ANB accuracy rates. Mean response times were calculated based on

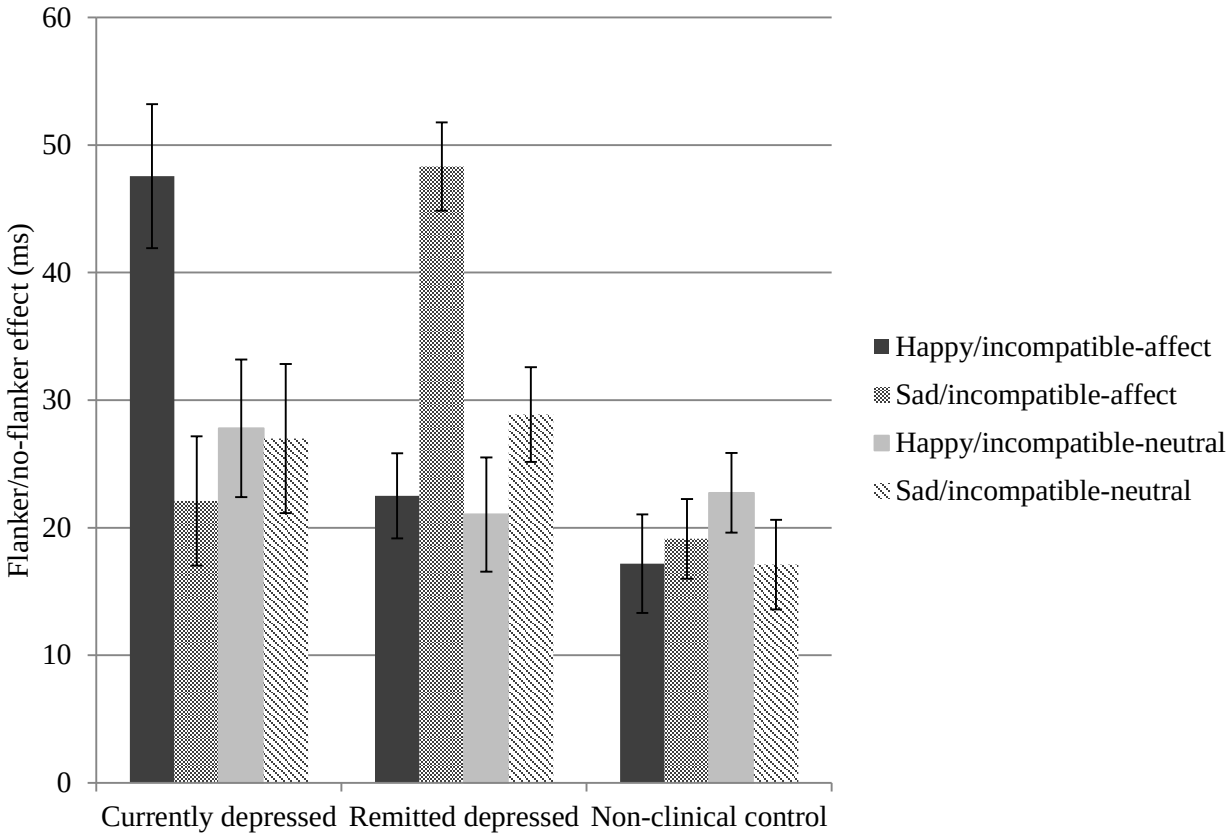

Figure 2. EFT flanker/ no-flanker effect scores for each of the trial types across participant groups. Errors bars represent one standard error. Participants who were outliers on error rates (*n* = 3) or flanker/ no-flanker effect scores (*n* = 6) are excluded.

correct trials only. Prior to calculating mean response times, trial outliers with response times that were greater than 3 SDs away from the participant's own mean for all trials were identified and removed from the data (1.6% of all trials in the 0-back task and 0.7% of all trials in the 2-back task). Participants with a mean accuracy rate of less than 50% (corresponding to worse than chance performance; $n = 13$) were excluded from analyses involving the response times due to the unreliability of mean response times based on a small number of correct trials. As described in the Methods, participants' mean response times on the 0-back trials were subtracted from their mean response times for each trial type on the 2-back trials to control for individual differences in response times related to perceiving, identifying, and responding to emotional stimuli. Outliers were defined as participants who had response times that were greater than 3 SD away from the mean of the participant group to which they belonged, for each trial type. Based on this criterion, six participants were identified as outliers and removed from data analyses involving the ANB response times.

Accuracy rates and response times were examined using separate 3 (Group) × 4 (Trial Type: Match-Set vs. Break-Set vs. Perseveration-Set vs. No-Set) × 3 (Trial Affect: Happy vs. Neutral vs. Sad) ANOVAs. Main effects were followed by pairwise comparisons with corrections for family-wise error rate and interactions were explored further using appropriate tests of simple effects. As noted in the Methods section, 10 currently depressed and 9 remitted depressed participants completed an earlier version of the ANB that had a reduced number of practice trials (one set of 10 trials rather than two sets of 18 trials) and test trials (six sets of 46 trials rather than six sets of 55 trials). Analyses are reported with these 19 participants included in the sample; effects in the omnibus ANOVAs were unchanged when these participants were excluded. The assumption of homogeneity of variance across participant groups was upheld for

accuracy rates and response times for all trial types, as F_{max} was less than 4 in all cases. All within-subjects effects that violated the assumption of sphericity were adjusted using the Greenhouse Geisser correction (adjusted degrees of freedom are noted as adj. *df*). Mean accuracy rates and response times for each of the trial types are displayed in Table 5.

Accuracy rates across participant groups.

The ANOVA conducted on the ANB accuracy rates yielded significant main effects of group, F (2, 146) = 8.49, $p < .001$, $\eta_p^2 = .10$, trial type, F (2.60, 379.49, adj. *df*) = 33.62, $p < .001$, $\eta_p^2 = .19$, and trial affect, F (1.87, 273.10, adj. *df*) = 73.52, $p < .001$, $\eta_p^2 = .34$, as well as a significant Trial Type × Trial Affect interaction, F (4.70, 686.07, adj. *df*) = 13.42, $p < .001$, $\eta_p^2 = .08$. Follow-up Tukey tests indicated that currently depressed participants were significantly less accurate on the ANB (70.39%) than control participants (82.15%), mean difference = -.12, *SE* = .03, $p < .001$, but did not differ significantly from remitted depressed participants (76.15%), $p > .10$. Remitted depressed participants were marginally less accurate on the ANB than control participants, mean difference = -.06, *SE* = .03, $p = .097$.

To further explore the Trial Type × Trial Affect interaction, separate repeated measures ANOVAs with trial affect as the within-subjects factor were conducted on the accuracy rates for each of the ANB trial types. The ANOVA conducted on the match-set trial accuracy rates yielded a significant main effect of trial affect, F (2, 296) = 55.17, $p < .001$, $\eta_p^2 = .27$. Paired samples *t*-tests with a Bonferroni correction applied ($p < .017$) showed that accuracy rates were significantly higher for happy match-set trials than both sad match-set trials, t (148) = 11.17, $p < .001$, and neutral match-set trials, t (148) = 3.72, $p < .001$. Accuracy rates were also significantly higher for neutral match-set trials than sad match-set trials, t (148) = 6.38, $p < .001$. The ANOVA conducted on the break-set trial accuracy rates yielded a marginally significant effect of

Table 5

Mean response times and accuracy rates for ANB trial types across participant groups.

	Currently depressed			Remitted depressed			Control		
	RT (ms)	RT-Raw (ms)	Acc %	RT (ms)	RT-Raw (ms)	Acc %	RT (ms)	RT-Raw (ms)	Acc %
Match set									
Happy	73.78	947.14	73.03	117.93	962.81	80.26	90.22	975.97	86.80
	(156.37)	(169.68)	(21.99)	(201.12)	(224.97)	(11.03)	(158.25)	(185.06)	(12.66)
Neutral	223.46	1096.82	71.11	242.24	1087.12	76.81	244.09	1129.84	80.01
	(160.48)	(163.53)	(19.24)	(185.68)	(204.19)	(13.49)	(158.94)	(183.68)	(15.02)
Sad	250.47	1123.84	65.00	290.09	1134.97	68.58	296.85	1182.60	74.41
	(150.45)	(164.87)	(21.38)	(196.40)	(213.62)	(14.14)	(136.87)	(159.47)	(15.34)
Break set									
Happy	197.06	1070.43	75.13	252.53	1097.41	81.88	231.06	1116.81	87.37
	(166.75)	(191.70)	(21.22)	(253.21)	(267.32)	(13.55)	(172.50)	(187.09)	(10.55)
Neutral	195.02	1068.38	76.71	235.79	1080.67	81.64	249.00	1134.75	86.93
	(175.54)	(178.13)	(22.08)	(222.20)	(234.85)	(13.13)	(167.31)	(177.82)	(12.04)
Sad	230.31	1103.67	74.38	271.06	1115.94	79.25	261.00	1146.76	84.61
	(163.22)	(187.47)	(22.92)	(202.46)	(231.68)	(12.88)	(150.14)	(155.75)	(14.65)
Perseveration set									
Happy	252.90	1126.26	71.92	294.06	1138.94	77.43	283.50	1169.25	83.28
	(168.27)	(172.76)	(24.94)	(224.33)	(252.23)	(17.37)	(165.07)	(178.32)	(14.77)
Neutral	323.07	1196.43	61.63	356.07	1200.95	67.33	353.39	1239.14	78.72
	(228.88)	(233.49)	(27.09)	(226.85)	(247.95)	(19.77)	(185.05)	(192.16)	(18.04)
Sad	340.10	1213.46	63.29	395.73	1240.61	72.53	390.99	1276.74	80.55
	(193.59)	(223.58)	(25.75)	(235.26)	(248.29)	(13.98)	(190.72)	(220.69)	(11.41)
No set									
Happy	261.90	1135.26	76.87	274.51	1119.39	82.12	293.96	1179.71	83.92
	(162.68)	(167.14)	(18.28)	(198.89)	(221.10)	(11.34)	(147.82)	(161.68)	(12.17)
Neutral	339.22	1212.59	65.95	341.64	1186.52	71.37	367.31	1253.06	78.46
	(165.72)	(177.57)	(23.22)	(214.83)	(226.99)	(16.05)	(159.28)	(175.25)	(12.56)
Sad	259.39	1132.75	69.72	304.98	1149.86	74.56	335.89	1221.64	80.74
	(171.70)	(175.22)	(19.20)	(221.74)	(238.18)	(11.42)	(163.01)	(175.54)	(11.83)

Note. Standard deviations are presented in parentheses. RT = raw response time – mean response time on the 0-back task; RT-raw = raw response time; Acc = accuracy rate. Participant outliers on each of the dependent variables are excluded as described in text.

trial affect, F (1.88, 277.98, adj. df) = 2.62, p = .078, η_p^2 = .02. None of the pairwise comparisons between the different affect types were significant at the Bonferroni-corrected significance level of $p < .017$, despite a trend for lower accuracy rates for sad break-set trials compared to happy break-set trials, t (148) = -2.03, p = .045, and neutral break-set trials, t (148) = -2.16, p = .032. The ANOVA conducted on the perseveration-set trial accuracy rates yielded a significant main effect of trial affect, F (1.92, 283.58, adj. df) = 19.12, $p < .001$, η_p^2 = .11. Bonferroni-corrected ($p < .017$) paired samples t-tests showed that accuracy rates were significantly higher for happy perseveration-set trials than both neutral perseveration-set trials, t (148) = 5.55, $p < .001$, and sad perseveration-set trials, t (148) = 4.33, $p < .001$. Accuracy rates were also higher for sad perseveration-set trials than neutral perseveration-set trials, although this difference did not reach the corrected level of significance, t (148) = 2.15, p = .033. Finally, the ANOVA conducted on the no-set trial accuracy rates yielded a significant main effect of trial affect, F (1.91, 281.91, adj. df) = 45.37, $p < .001$, η_p^2 = .24. Paired samples t-tests showed that accuracy rates were significantly higher for happy no-set trials than both neutral no-set trials, t (148) = 8.50, $p < .001$, and sad no-set trials, t (148) = 6.37, $p < .001$. Accuracy rates were also significantly higher for sad no-set trials than neutral no-set trials, t (148) = 3.47, p = .001.

To evaluate the possibility that the lower accuracy of currently depressed participants relative to control participants on the ANB was due to difficulty in perceiving and identifying emotional faces rather than difficulty in updating working memory with emotional faces, a 3 (Group) × 2 (Trial Type: Match vs. No-Match) × 3 (Trial Affect) ANOVA was conducted on the accuracy rates for 0-back trials[4] (see Table 6).

[4] This analysis excluded 16 participants who were identified as outliers on the basis of having 0-back trial accuracy rates that were more than 3 SD lower than the mean of the participant group to which they belonged.

Table 6

Accuracy rates (%) for 0-back trial types across participant groups.

	Currently depressed	Remitted depressed	Control
Match trials			
Happy	96.81 (6.63)	98.91 (2.84)	97.70 (4.50)
Neutral	92.91 (11.12)	95.29 (6.95)	96.10 (5.72)
Sad	93.09 (10.03)	91.30 (9.93)	92.02 (10.56)
No-match trials			
Happy	98.05 (3.46)	97.10 (3.82)	97.78 (5.58)
Neutral	86.52 (16.51)	93.93 (5.25)	95.39 (5.58)
Sad	93.97 (7.31)	96.20 (4.38)	96.19 (4.24)

Note. Standard deviations are presented in parentheses. Acc = accuracy rate. Participants who were outliers on 0-back accuracy rates are excluded (n = 16).

The results revealed significant main effects of group, F (2, 137) = 5.22, p = .007, η_p^2 = .07, and trial affect, F (1.76, 241.30, adj. df) = 31.71, p < .001, η_p^2 = .19, a Group × Trial Affect interaction, F (3.52, 241.30, adj. df) = 5.41, p = .001, η_p^2 = .07, and a Trial Type × Trial Affect interaction, F (2, 274) = 12.53, p < .001, η_p^2 = .08, which were all qualified by a three-way interaction between group, trial type, and trial affect, F (4, 274) = 2.44, p = .048, η_p^2 = .03. To further explore the three-way interaction, separate 3 (Group) × 3 (Trial Affect) ANOVAs were conducted on the 0-back accuracy rates for match and no-match trials. For match trials, there was a significant main effect of trial affect, F (1.77, 242.31, adj. df) = 19.05, p < .001, η_p^2 = .12, and no other significant effects in the model. Bonferroni-corrected (p < .017) paired samples t-tests indicated that participants were significantly more accurate for match trials involving happy faces than match trials involving both neutral, t (139) = 4.09, p < .001, and sad, t (139) = 5.89, p < .001, faces. Accuracy rates for match trials were also higher for neutral faces than sad faces, t (139) = 2.52, p = .013. For no-match trials, there were significant main effects of group, F (2, 137) = 7.86, p = .001, η_p^2 = .10, and trial affect, F (1.64, 224.57, adj. df) = 25.80, p < .001, η_p^2 = .16, which were qualified by a significant Group × Trial Affect interaction, F (3.28, 224.57, adj. df) = 7.09, p < .001, η_p^2 = .09.

Three one-way ANOVAs with group as the independent variable were conducted on the accuracy rates for no-match trials involving happy, neutral, and sad faces. For happy faces, the effect of group was non-significant, p > .10. The effect of group was significant for neutral faces, F (2, 137) = 9.55, p < .001, η_p^2 = .12, and marginally significant for sad faces, F (2, 137) = 2.54, p = .082, η_p^2 = .04. Follow-up Tukey tests showed that currently depressed participants had significantly lower accuracy rates for no-match trials involving neutral faces on the 0-back task than both remitted depressed, mean difference = -.07, SE = .02, p = .003, and control

participants, mean difference = -.09, *SE* = .02, $p < .001$. Remitted depressed and control participants did not differ from each other, $p > .10$. Despite the marginal effect of group in the one-way ANOVA, none of the Tukey pairwise comparisons were significant for the accuracy rates for no-match trials involving sad faces, *p*s > .10.

Thus, currently depressed participants displayed difficulty in distinguishing neutral faces from happy and sad target faces, relative to remitted depressed and control participants. Because this difficulty was specific to neutral faces it is unlikely that the lower accuracy of currently depressed participants on the ANB (2-back trials) was due to a general impairment in the perception and identification of facial emotion. Rather, these results suggest that currently depressed participants have both a specific impairment in recognizing neutral faces and a general impairment in updating working memory with emotional material.

Response times across participant groups.

The ANOVA conducted on the ANB response times yielded significant main effects of trial type, F (2.65, 355.05, adj. *df*) = 117.57, $p < .001$, $\eta_p^2 = .47$, and trial affect, F (2, 268) = 131.14, $p < .001$, $\eta_p^2 = .50$, which were qualified by a Trial Type × Trial Affect interaction, F (5.26, 705.01) = 27.78, $p < .001$, $\eta_p^2 = .17$. There was no main effect of group, and group did not interact with any of the effects in the model, *p*s > .10.

To further explore the interaction between trial type and trial affect, separate repeated measures ANOVAs with trial affect as the within-subjects factor were conducted on the response times for each of the ANB trial types. For match-set trials, the effect of trial affect was significant, F (2, 272) = 180.04, $p < .001$, $\eta_p^2 = .57$. Bonferroni-corrected ($p < .017$) paired samples *t*-tests indicated that response times for happy match-set trials were significantly faster than response times for both neutral match-set trials, t (136) = -12.89, $p < .001$, and sad match-

set trials, t (136) = -18.94, $p < .001$. Response times for neutral match-set trials were also significantly faster than response times for sad match-set trials, t (136) = -4.43, $p < .001$. For break-set trials, the effect of trial affect was also significant, F (2, 272) = 3.31, $p = .038$, $\eta_p^2 = .02$. None of the pairwise comparisons between the different affect types were significant at the Bonferroni-corrected significance level of $p < .017$, despite a trend for slower response times for sad break set-trials than both happy break-set trials, t (136) = 2.12, $p = .036$, and neutral break-set trials, t (136) = 2.41, $p = .018$. The ANOVA conducted on the perseveration-set response times yielded a significant effect of trial affect, F (2, 272) = 28.05, $p < .001$, $\eta_p^2 = .17$. Bonferroni-corrected ($p < .017$) paired samples t-tests indicated that response times for happy perseveration-set trials were significantly faster than response times for both neutral perseveration-set trials, t (136) = -4.56, $p < .001$, and sad perseveration-set trials, t (136) = -7.83, $p < .001$. Response times for neutral perseveration-set trials were also significantly faster than response times for sad perseveration-set trials, t (136) = -2.46, $p = .015$. Finally, the ANOVA conducted on the response times for no set trials also yielded a significant effect of trial affect, F (2, 272) = 35.87, $p < .001$, $\eta_p^2 = .21$. Bonferroni-corrected ($p < .017$) paired samples t-tests indicated that response times for happy no-set trials were significantly faster than response times for both neutral no-set trials, t (136) = -8.07, $p < .001$, and sad no-set trials, t (136) = -2.94, $p = .004$. Response times for sad no-set trials were also significantly faster than response times for neutral no-set trials, t (136) = -5.49, $p < .001$.[5]

[5] As noted in the Methods (Chapter 2.3), the response time data were also prepared and analyzed according to the methods of Everaert et al. (2016) and Levens and Gotlib (2010; 2015). Results were similar across the various methods of analysis. Importantly, there were no main or interaction effects involving group in any of the analyses.

3.4 Shifting (CET)

Statistical analysis.

Five participants were excluded from data analysis involving the Colour-Emotion Task (CET) because they had mean error rates that exceeded the sample mean by more than 3 SDs. The mean error rate for the remaining participants was 3.0%. Response times were analyzed for correct trials only. Trials with response times that were greater than 3 SDs away from the participant's own mean for all trials were identified as trial outliers and excluded from the data (1.8% of all trials in the switch blocks). Mean response times for each condition, task, and trial type were then computed. Table 7 displays mean response times for switch and no-switch trials for each task across the participant groups, for the no-crosstalk and crosstalk conditions.

To evaluate shifting ability, switch costs were calculated for the colour and emotion tasks by subtracting mean response times for no-switch trials from mean response times for switch trials. Switch costs were generated for the no-crosstalk and crosstalk conditions separately. Thus, four switch costs were computed, consisting of every combination of condition (no-crosstalk, crosstalk) and trial task (colour, emotion). Participant outliers who had switch costs that were greater than 3 SD away from the mean of the participant group to which they belonged, for each condition and task, were identified and removed from the analyses (n = 3).

A 3 (Group) × 2 (Condition: No-Crosstalk vs. Crosstalk) × 2 (Trial Task: Colour vs. Emotion) ANOVA was conducted on the mean switch costs. Main effects were followed by pairwise comparisons with corrections for family-wise error rate and interactions were explored further using appropriate tests of simple effects. The assumption of homogeneity of variance was evaluated for the switch costs for each condition and trial task. F_{max} values were below 2 in all cases, indicating adequate homogeneity of variance across the participant groups. The reported

Table 7.

CET mean response times (in ms) by condition and trial task across groups.

	Currently Depressed	Remitted Depressed	Control
No-Crosstalk Condition			
Colour Task			
Switch trials	1214.33 (295.51)	1043.62 (235.94)	1062.28 (284.04)
No-switch trials	776.88 (216.08)	699.30 (170.17)	686.18 (151.24)
Emotion Task			
Switch trials	1235.37 (285.68)	1134.55 (277.21)	1125.74 (303.41)
No-switch trials	883.01 (214.89)	776.43 (162.20)	777.39 (215.81)
Crosstalk Condition			
Colour Task			
Switch trials	1295.00 (351.80)	1124.88 (270.33)	1157.13 (280.83)
No-switch trials	897.47 (279.97)	756.35 (191.60)	769.01 (211.75)
Emotion Task			
Switch trials	1409.84 (339.14)	1250.28 (307.82)	1230.40 (340.40)
No-switch trials	973.79 (243.46)	896.15 (192.92)	905.17 (219.29)

Note. Standard deviations are presented in parentheses. Participants who were outliers on error rates are excluded (n = 5).

results did not differ across the stimulus-response and stimulus location-task mappings on the CET; inclusion of counterbalance as a between-subjects factor in the ANOVAs did not change any of the effects.

Switch costs across participant groups.

The ANOVA conducted on the mean switch costs yielded a marginally significant effect of trial task, F (1, 148) = 3.05, p = .083, η_p^2 = .02, which was subsumed under a significant Group × Condition × Trial Task interaction, F (2, 148) = 6.21, p = .003, η_p^2 = .08. To further explore the three-way interaction, separate 3 (Group) × 2 (Trial Task) ANOVAs were conducted on the mean switch costs in the no-crosstalk and crosstalk conditions. For the no-crosstalk condition, there was a marginally significant main effect of trial task, F (1, 148) = 3.36, p = .069, η_p^2 = .02, which was qualified by a marginally significant Trial Task × Group interaction, F (2, 148) = 2.56, p = .081, η_p^2 = .03. For the crosstalk condition, there were no significant effects in the model, all ps > .10 (Figure 3). The marginally significant interaction in the no-crosstalk condition was followed up with separate one-way ANOVAs on the mean switch costs for the colour and emotion tasks with group as the independent variable. For the colour task (i.e., switching *from* the emotion task), the effect of group was marginally significant, F (2, 148) = 2.95, p = .056, η_p^2 = .04. Follow-up Tukey tests showed that currently depressed participants had switch costs that were marginally larger than remitted depressed participants, mean difference = 82.94 ms, SE = 35.64 ms, p = .055, whereas the mean difference between currently depressed and control participants (64.73 ms) was not statistically significant, p > .10. There was no significant difference in mean switch cost on the colour task between remitted depressed and control participants, p > .10. For the emotion task (i.e., switching *from* the colour task), the effect of group was non-significant, p > .10 (Figure 4).

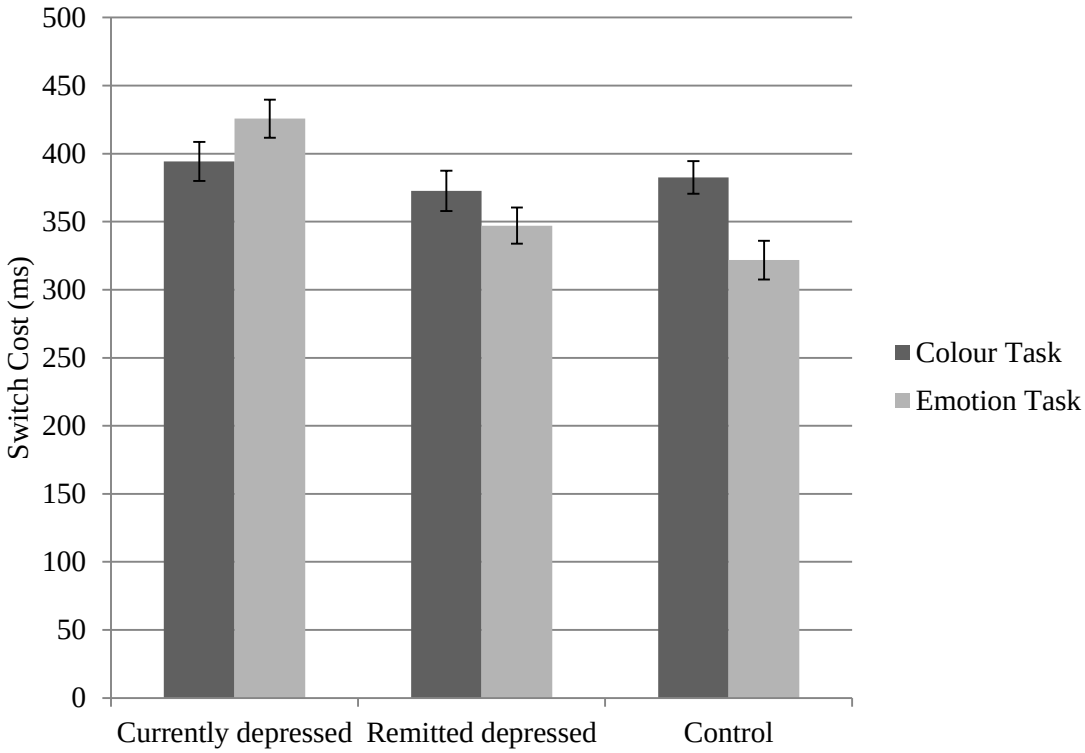

Figure 3. CET switch costs for the colour and emotion tasks in the crosstalk condition, across participant groups. Errors bars represent one standard error. Participants who were outliers on error rates (n = 5) or switch costs (n = 3) are excluded.

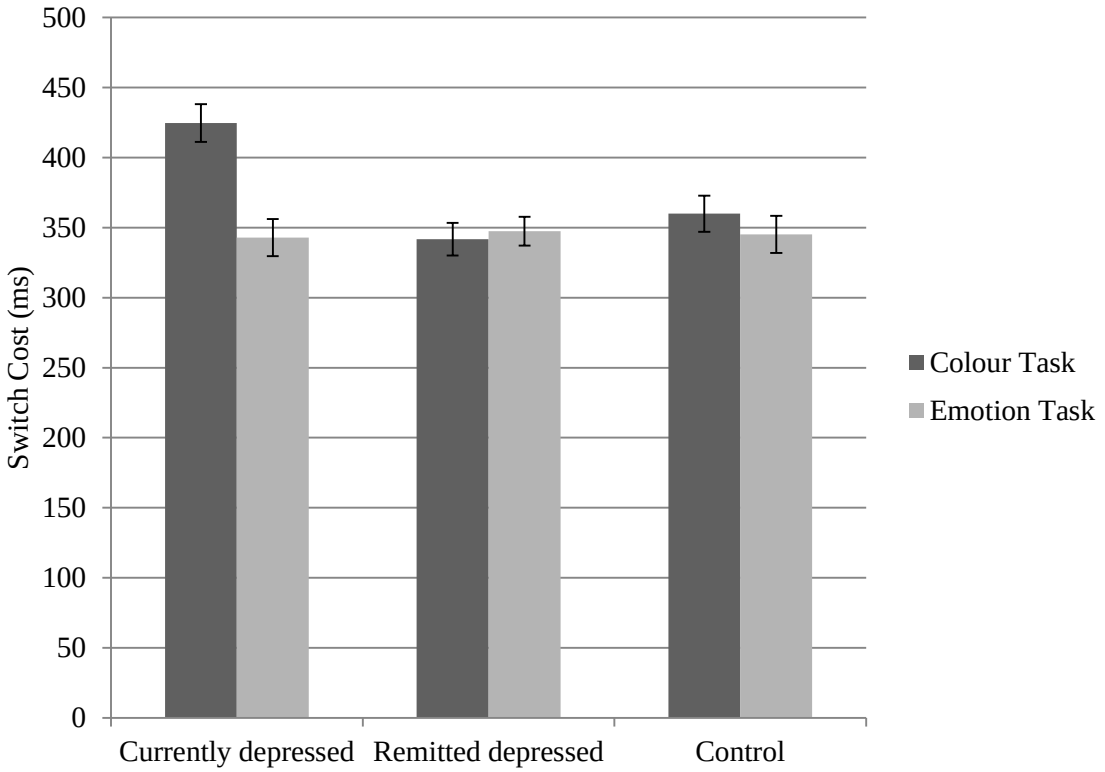

Figure 4. CET switch costs for the colour and emotion tasks in the no-crosstalk condition, across participant groups. Errors bars represent one standard error. Participants who were outliers on error rates (n = 5) or switch costs (n = 3) are excluded.

Paired samples t-tests were also conducted to compare switch costs in the no-crosstalk condition for the emotion and colour tasks within each participant group. Currently depressed participants had significantly larger switch costs on the colour task than on the emotion task, $t(48) = 2.27$, $p = .028$. There were no significant differences between switch costs on the colour task and the emotion task for remitted depressed and control participants, ps > .10 (Figure 4). Thus, currently depressed participants had significantly greater switch costs when switching from the emotion task to the colour task than when switching from the colour task to the emotion task, in the no-crosstalk condition specifically.

Analyses were then conducted to determine whether the affect of the previous target face influenced the speed with which participants were able to switch task sets. Within the crosstalk condition, separate switch costs were calculated for each participant by subtracting the mean response time for no switch trials from the mean response times for switch trials, for each task and previous target affect. That is, four switch costs were calculated reflecting the switch cost on the colour task when shifting away from happy faces, the switch cost on the colour task when shifting away from sad faces, the switch cost on the emotion task when shifting away from happy faces, and the switch cost on the emotion task when shifting away from sad faces (see Table 8). A 3 (Group) × 2 (Task) × 2 (Previous Target Affect: Happy vs. Sad) mixed ANOVA was conducted on the mean switch costs. The results revealed no significant main or interaction effects in the model, including the hypothesized interaction between group and previous target emotion, all ps > .10.[6]

[6] Separate switch costs for each task and previous target affect could not be computed for the no-crosstalk condition, as the previous target affect was always neutral for no-switch trials in the colour task and switch trials in the emotion task. However, to further inform whether previous target affect influenced the speed with which participants were able to switch task sets, a 3 (Group) × 2 (Previous Target Affect: Happy vs. Sad) ANOVA was conducted on the

Table 8.

CET switch costs in the crosstalk condition by trial task and previous target affect across groups.

	Currently Depressed	Remitted Depressed	Control
Colour Task			
Switch cost from happy	404.42 (279.87)	403.32 (261.50)	401.85 (250.61)
Switch cost from sad	398.89 (225.61)	360.93 (256.59)	383.83 (235.48)
Emotion Task			
Switch cost from happy	453.33 (292.37)	374.33 (232.95)	329.82 (298.38)
Switch cost from sad	446.42 (232.28)	378.38 (210.34)	359.35 (262.01)

Note. Standard deviations are presented in parentheses. Participants who were outliers on error rates (n = 5) or switch costs (n = 3) are excluded.

response times for switch trials in the colour task in the no-crosstalk condition. There were no significant main or interaction effects involving previous target affect, ps > .10.

3.5 Non-Affective Executive Functioning (Wisconsin Card Sort Task)

Statistical analysis.

Four participants were missing data on the Wisconsin Card Sort Task (WCST) due to experimenter error (i.e., failure to save the scores properly before exiting the program) or participant time constraint. Participant outliers were defined as participants who had scores on the WCST dependent variables that were greater than 3 SDs away from the mean of the participant group to which they belonged. Based on this criterion, one participant was identified as an outlier on number of trials, three participants were outliers on number of perseverative errors, four participant were outliers on number of non-perseverative errors, and three participants were outliers on failure to maintain set. These participants were excluded from the analyses involving the WCST dependent variables on which they were outliers. Mean scores on each of the WCST dependent variables across participant groups are displayed in Table 9.

Separate one-way ANOVAs were conducted on each of the dependent variables of the WCST with participant group as the independent variable. Main effects were followed by pairwise comparisons with corrections for family-wise error rate. To control for correlations between the WCST variables, a multivariate analysis of variance (MANOVA) was also conducted on all of the dependent variables, with participant group as the independent variable. For each of the WCST dependent variables, F_{max} was less than 6, indicating adequate homogeneity of variance across participant groups.

WCST performance across participant groups.

The effect of group was marginally significant in the ANOVA conducted on the number of trials, F (2, 151) = 2.47, p = .088, η_p^2 = .03. However, none of the pairwise comparisons in the follow-up Tukey tests were significant, ps > .10. The effect of group was non-significant for

Table 9

Mean scores on the WCST dependent variables across participant groups.

Dependent Variable	Currently Depressed	Remitted Depressed	Control
Trials	91.22 (19.38)	91.04 (18.80)	84.62 (11.57)
Perseverative errors	10.22 (8.41)	9.55 (5.29)	7.90 (3.56)
Non-perseverative errors	9.71 (7.88)	8.70 (6.43)	6.98 (3.87)
Failure to maintain set	0.59 (0.76)	0.60 (0.86)	0.32 (0.65)

Note. Standard deviations are displayed in parentheses. Participant outliers on each of the dependent variables are excluded as described in text.

number of perseverative errors, $p > .10$. For number of non-perseverative errors, there was a marginally significant effect of group, F (2, 148) = 2.39, $p = .095$, $\eta_p^2 = .03$. This marginal effect reflected greater non-perseverative errors in the currently depressed group relative to the control group, mean difference = 2.73, $SE = 1.27$, $p = .082$, whereas currently and remitted depressed groups and remitted depressed and control groups did not differ significantly, ps > .10. The ANOVA conducted on failure to maintain set did not yield a significant effect of group, $p > .10$. In the MANOVA conducted on all of the WCST dependent variables, the effect of group was non-significant, $p > .10$. Thus, overall reliable differences were not observed among the participant groups on WCST performance as measured by total number of trials, number of perseverative errors, number of non-perseverative errors, and failure to maintain set.

3.6 Associations between Rumination and Affective Executive Control

Statistical analysis.

To evaluate the hypothesis that biases in executive control that favour the processing of negative information may be related to rumination, bivariate correlations were conducted between total rumination, reflection, and brooding and the following executive control bias measures: 1) EFT flanker compatibility and flanker/no-flanker effects for happy faces with incompatible-affect (i.e., sad) flankers; 2) ANB response times for sad match-set, sad break-set, sad perseveration-set, and sad no-set trials (raw mean response time for each trial type minus mean response time in the 0-back task); and 3) CET switch costs for the colour task (i.e., shifting *from* the emotion task) in the no-crosstalk and crosstalk conditions, and switch costs from sad faces for the colour and emotions tasks in the crosstalk condition. These correlations were first conducted in the full sample and then within the participant groups.

Participants who were identified as outliers on error rates on the EFT and CET as described in the previous sections were removed from the correlational analyses involving the respective tasks. Likewise, participants who had accuracy rates of less than 50% on the ANB as identified previously were removed from the correlational analyses involving the ANB variables. For the correlations conducted in the full sample, participants with means on the variables involved in the correlation analysis that were greater than 3 SD away from the sample mean were excluded from the analysis. For the correlations conducted within the participant groups, participants with means on the variables involved in the correlation analysis that were greater than 3 SD away from the mean of the participant group to which they belonged were excluded from the analysis. Outliers removed in each analysis are reported in the relevant sections below. To test the possibility that associations between executive control biases and rumination may be due to shared variance with depression severity, significant correlations were re-evaluated using partial correlations controlling for BDI-II scores.

EFT measures.

No participants were identified as outliers on total rumination, reflection, and brooding in the full sample. Three participants were outliers in the full sample on the flanker compatibility effect for happy faces with incompatible-affect flankers, and two participants were outliers for the flanker/no-flanker effect for happy faces with incompatible-affect flankers. These participants were excluded from the full-sample correlational analyses involving the variables on which they were outliers. Within the participant groups, one control participant was an outlier on total rumination, two control participants were outliers on reflection, and one control participant was an outlier on brooding. In addition, three participants were outliers on the flanker/no-flanker compatibility effect scores for happy faces with incompatible-affect flankers (one currently

depressed, one remitted depressed, and one control participant). These participants were excluded from the within-group correlational analyses involving the variables on which they were outliers.

Bivariate correlations for total rumination, reflection, and brooding with the flanker compatibility and flanker/no-flanker effects for happy faces with incompatible-affect flankers are presented in Table 10. As indicated, none of the correlations were significant in the full sample. Among currently depressed participants, there was a significant negative correlation between reflection and the flanker/no-flanker effect for happy faces with incompatible-affect flankers, r = -.34, p = .016. This correlation remained significant after controlling for BDI-II scores, r = -.33, p = .021. In the remitted depressed and control groups, none of the rumination measures were significantly correlated with either the flanker compatibility or flanker/no-flanker effects for happy faces with incompatible-affect flankers. Thus, inhibition was largely unrelated to rumination in the full sample and within the participant groups. As the one exception, currently depressed participants who reported greater reflection demonstrated less interference from sad flankers when responding to happy target faces, controlling for individual differences in depression severity.

ANB measures.

No participants were identified as outliers on total rumination, reflection, and brooding in the full sample. One participant was an outlier on response times for sad match-set trials, one participant was an outlier on response times for sad break-set trials, and one participant was an outlier on response times for sad no-set trials, in the full sample. These participants were excluded from the full-sample correlational analyses involving the variables on which they were outliers. Within the participant groups, one control participant was an outlier on total rumination,

Table 10.

Bivariate correlations for total rumination, reflection, and brooding with the EFT flanker compatibility and flanker/no-flanker effects for happy faces with incompatible-affect flankers, in the full sample and within participant groups.

	Total Rumination	Reflection	Brooding
Full Sample			
Compatibility effect	.06	.02	.06
Flanker/no-flanker effect	.01	-.04	-.01
Currently Depressed			
Compatibility effect	-.11	-.12	-.06
Flanker/no-flanker effect	-.26	-.34*	-.14
Remitted Depressed			
Compatibility effect	-.08	-.13	.00
Flanker/no-flanker effect	.01	-.05	-.07
Control			
Compatibility effect	-.01	-.05	.03
Flanker/no-flanker effect	-.04	.22	-.15

Note. * $p < .05$. Full-sample and within-group outliers on each of the variables are excluded as described in text.

two control participants were outliers on reflection, and one control participant was an outlier on brooding. In addition, one currently depressed participant was an outlier on response times for sad match-set trials, one control participant was an outlier on response times for sad break-set trials, and one currently depressed participant was an outlier on response times for sad perseveration-set trials. These participants were excluded from the within-group correlational analyses involving the variables on which they were outliers.

Bivariate correlations for total rumination, reflection, and brooding with the response times for sad match-set, break-set, perseveration-set, and no-set trials are presented in Table 11. In the full sample, there was a marginally significant negative correlation between response times for sad match-set trials and reflection, $r = -.15$, $p = .069$. When controlling for BDI-II scores, however, this correlation was non-significant, $r = -.12$, $p > .10$. There was also a marginally significant negative correlation between response times for sad no-set trials and reflection, $r = -.15$, $p = .084$. This correlation was reduced to non-significance after controlling for BDI-II scores, $r = -.10$, $p > .10$. None of the other bivariate correlations were significant or marginally significant within the full sample, ps $> .10$.

Among currently depressed participants, response times for sad perseveration-set trials were marginally correlated with total rumination, $r = .27$, $p = .087$. However, this correlation was reduced to non-significance after controlling for BDI-II scores, $r = .21$, $p > .10$. Within the remitted depressed group, there was a marginally significant negative correlation between response times for sad match-set trials and reflection, $r = -.27$, $p = .055$. This correlation remained marginally significant after controlling for BDI-II scores, $r = -.27$, $p = .058$. None of the other bivariate correlations were significant or marginally significant within the participant groups, ps $> .10$.

Table 11.

Bivariate correlations for total rumination, reflection, and brooding with the response times for sad match-set, break-set, perseveration-set and no-set trials and mean accuracy rates on the ANB, in the full sample and within participant groups.

	Total Rumination	Reflection	Brooding
Full Sample			
Match set	-.12	-.15†	-.10
Break set	-.05	-.07	-.08
Perseveration set	-.05	-.13	-.05
No set	-.12	-.15†	-.12
Mean accuracy	-.15†	-.19*	-.12
Currently Depressed			
Match set	.15	.05	.13
Break set	.10	-.01	.04
Perseveration set	.27†	.00	.19
No set	.13	-.11	.08
Mean accuracy	.03	-.15	.11
Remitted Depressed			
Match set	-.16	-.27†	-.17
Break set	-.07	-.10	-.20
Perseveration set	-.05	-.09	-.16
No set	-.08	-.17	-.17
Mean accuracy	.09	.05	.01

	Total Rumination	Reflection	Brooding
Control			
Match set	-.03	-.21	-.02
Break set	-.07	-.13	.10
Perseveration set	-.10	-.23	.15
No set	.07	-.14	.13
Mean accuracy	-.09	-.11	.02

Note. † $p < .10$; * $p < .05$. Full-sample and within-group outliers on each of the variables are excluded as described in text.

Given that depression status was found to be related to ANB accuracy rates in the between-groups analyses, bivariate correlations were also conducted between total rumination, reflection, and brooding and mean accuracy rates within the full sample and then within the participant groups (Table 11). In the full sample, there was a marginally significant negative correlation between total rumination and accuracy rates, r = -.15, p = .057, and a significant negative correlation between reflection and accuracy rates, r = -.19, p = .016. However, both of these correlations were reduced to non-significance after controlling for BDI-II scores, r = -.04, p > .10 for total rumination, and r = -.13, p > .10 for reflection. Within the participant groups, there were no significant correlations between rumination, reflection, and brooding, and accuracy rates, all ps > .10. Thus, updating of working memory was largely unrelated to rumination when controlling for depression severity, both within the full sample and within the participant groups. As the one exception, remitted depressed participants who reported greater reflection had faster response times for sad match-set trials, although this association was marginal.

CET measures.

No participants were identified as outliers on total rumination, reflection, and brooding in the full sample. Two participants were outliers in the full sample on the switch cost for the colour task in the no-crosstalk condition, and three participants were outliers on the switch cost away from sad faces for the emotion task in the crosstalk condition. These participants were excluded from the full-sample correlational analyses involving the variables on which they were outliers. Within the participant groups, one control participant was an outlier on total rumination, two control participants were outliers on reflection, and one control participant was an outlier on brooding. In addition, two participants (one currently depressed and one control) were outliers on the switch cost for the colour task in the no-crosstalk condition, and two participants (one

currently depressed and one control) were outliers on the switch cost away from sad faces for the emotion task in the crosstalk condition. These participants were excluded from the within-group correlational analyses involving the variables on which they were outliers.

Bivariate correlations for total rumination, reflection, and brooding with switch costs for the colour task in the no-crosstalk and crosstalk conditions and switch costs from sad faces for the colour and emotions tasks in the crosstalk condition are presented in Table 12. As indicated, there were no significant correlations in the full sample. In the currently depressed group, switch costs for the colour task in the no-crosstalk condition were marginally significantly correlated with reflection scores, $r = .25$, $p = .080$. After controlling for BDI-II scores, the correlation remained marginally significant, $r = .27$, $p = .065$. Switch costs for the colour task in the crosstalk condition were also marginally correlated with reflection in the currently depressed group, $r = .26$, $p = .067$. This correlation remained marginally significant when BDI-II scores were controlled, $r = .26$, $p = .068$. There were no significant or marginally significant correlations between the rumination measures and shift costs among remitted depressed or control participants, all ps $> .10$. Thus, among currently depressed participants, shifting from an emotion-relevant task set was related, albeit modestly, to reflective rumination. Shifting away from negative material specifically was unrelated to rumination.

Table 12.

Bivariate correlations for total rumination, reflection, and brooding with CET switch costs for the colour task in the no-crosstalk and crosstalk conditions, and switch costs from sad faces on the colour and emotions tasks in the crosstalk condition, in the full sample and within participant groups.

	Total Rumination	Reflection	Brooding
Full Sample			
No-crosstalk condition: Colour task switch cost	.04	.11	.01
Crosstalk condition: Colour task switch cost	-.01	.10	-.04
Crosstalk condition: Colour task switch cost from sad faces	-.01	.05	-.02
Crosstalk condition: Emotion task switch cost from sad faces	.15	.09	.10
Currently Depressed			
No-crosstalk condition: Colour task switch cost	-.01	.25†	-.12
Crosstalk condition: Colour task switch cost	.05	.26†	-.07
Crosstalk condition: Colour task switch cost from sad faces	.03	.15	-.01
Crosstalk condition: Emotion task switch cost from sad faces	-.10	-.04	-.09
Remitted Depressed			
No-crosstalk condition: Colour task switch cost	.01	.06	-.02
Crosstalk condition: Colour task switch cost	-.04	.11	-.07

	Total Rumination	Reflection	Brooding
Crosstalk condition: Colour task switch cost from sad faces	.01	.07	.01
Crosstalk condition: Emotion task switch cost from sad faces	.13	.09	.00
Control			
No-crosstalk condition: Colour task switch cost	-.18	-.16	-.09
Crosstalk condition: Colour task switch cost	-.08	-.03	-.01
Crosstalk condition: Colour task switch cost from sad faces	-.10	.06	-.10
Crosstalk condition: Emotion task switch cost from sad faces	.06	.13	.01

Note. † $p < .10$. Full-sample and within-group outliers on each of the variables are excluded as described in text.

Chapter 4: Discussion

The present study evaluated two sets of hypotheses. First, this study tested the hypotheses that currently depressed participants would show biases in inhibition, monitoring and updating of working memory, and set shifting when processing emotional material, relative to control participants. Although no specific hypotheses were made regarding biases in these components of executive control among remitted depressed participants, it was also of interest to examine whether remitted depressed participants would demonstrate similar biases as currently depressed participants. Second, this study tested the hypotheses that biases in inhibition, monitoring and updating of working memory, and set shifting when processing emotional material would be related to rumination, and brooding in particular, and that this relationship would remain after controlling for individual differences in depression symptoms. It was also predicted that the relationship between executive control biases and rumination/brooding would be strongest, and therefore most likely to be observed, among currently depressed participants.

The hypotheses regarding the existence of executive control biases among currently depressed participants were largely supported by the data. Compared to control participants, currently depressed participants demonstrated a reduced ability to inhibit interference from negative task-irrelevant material. In contrast, no group differences were observed in the ability to inhibit interference from neutral or positive material, which suggests that the depression-related bias in inhibition is specific to material that is negative in valence. Currently depressed participants also had an impaired ability to update working memory with emotional material relative to control participants. Contrary to hypothesis, however, there was no evidence in the present study that updating biases among currently depressed participants were valence-specific. Currently depressed participants had lower performance across trial types on the updating task,

rather than showing the hypothesized pattern of facilitated integration of negative stimuli into working memory and impaired disengagement from negative stimuli in working memory. Finally, currently depressed participants showed a bias in shifting, as they were slower to shift from an emotion-relevant task set to an emotion-irrelevant task set than the other way around, whereas control participants did not show this bias. Although this bias was emotion-specific, the results did not support a valence-specific shifting bias. That is, currently depressed participants in the present study did not show greater difficulty shifting between task sets after processing a negative stimulus versus a positive stimulus. The present results provide stronger support for the existence of depression-related biases in executive control than general deficits. In addition to the finding of a valence-specific bias in inhibition and an emotion-specific bias in shifting among currently depressed participants, it was found that currently depressed participants did not show a reliable deficit in general executive functioning involving the processing of non-emotional information. Remitted depressed participants did not demonstrate similar biases in executive control as currently depressed participants.

The hypotheses that executive control biases would be associated with rumination and brooding received little support in this study. Within the full sample of participants, there were no significant relationships between negative biases in inhibition, updating, or set shifting and rumination after controlling for depression symptoms. When these relationships were tested within the separate participant groups, there were some inconsistent associations between executive control biases and rumination. Opposite to the hypothesized directional relationship, *greater* inhibition of negative task-irrelevant stimuli was associated with a greater tendency to engage in the reflective form of rumination among currently depressed participants, controlling for levels of depression. Currently depressed participants who had more difficulty shifting away

from an emotion-relevant task set also reported greater reflection. For remitted depressed participants, quicker integration of sad stimuli into working memory was related to greater reflection. Thus, the hypothesis that relationships between executive control biases and rumination would be strongest among depressed participants received some support. However, even among depressed participants, most of the predicted relationships were not observed.

4.1 Inhibition

Inhibition of emotional stimuli was assessed in the present study using the EFT (Fenske & Eastwood, 2003). In the EFT, participants identified the affective valence of a centrally presented image of a facial expression. Response times on trials in which the central image was presented alone, or with two flanking images displaying compatible facial affect, were compared to response times on trials in which the central image was presented with two flanking images displaying neutral or incompatible facial affect. The flanker compatibility effect represented the difference in response times between incompatible and compatible flanker trials, with separate effect scores generated for incompatible-neutral flankers and incompatible-affect flankers. The flanker/no-flanker effect represented the difference in response times between incompatible flanker and no-flanker trials, again with separate effect scores generated for incompatible-neutral flankers and incompatible-affect flankers.

The hypothesis that currently depressed participants would demonstrate reduced inhibition of negative peripheral information was supported by the data. Currently depressed participants had larger flanker compatibility and flanker/no-flanker effects for trials involving happy target faces with incompatible-affect (i.e., sad) flanking faces, relative to control participants. In contrast, currently depressed and control participants did not differ significantly in their flanker compatibility and flanker/no-flanker effects for trials involving happy target faces

with neutral flanking faces, or for trials involving sad target faces with either neutral or incompatible-affect (i.e., happy) flanking faces. This pattern of findings negates the possibility that currently depressed individuals have a reduced ability to inhibit distracting information generally, regardless of valence, or have a reduced ability to inhibit distracting information when the target stimulus is positive. In particular, because currently depressed participants had larger flanker compatibility and flanker/no-flanker effects than control participants for trials involving happy target faces with sad flankers, but not for trials involving happy target faces with neutral flankers, it can be reasonably concluded that the sad flanking faces accounted for the reduced inhibition. Thus, the results suggest that currently depressed individuals have a specific impairment in the inhibition of negative task-irrelevant stimuli.

The present findings are consistent with studies that have found reduced inhibition of negative material among depressed individuals using various paradigms, including the emotional Stroop task (Epp et al., 2012) and the NAP task (Goeleven et al., 2006; Joormann, 2004; Joormann & Gotlib, 2010). Two previous studies have used a variant of the EFT to evaluate depression-related inhibition biases (Zetsche & Joormann, 2011; Zetsche et al., 2012). Whereas Zetsche and Joormann (2011) found that interference scores on the EFT were unrelated to depressive symptoms in a non-clinical sample, Zetsche et al. (2012) found that currently depressed participants had marginally greater interference scores for negative distractor words relative to control participants. The present study used a version of the EFT based on Fenske and Eastwood (2003), which differed somewhat from the version used by Zetsche and colleagues. In particular, the primary dependent variables differed across the studies, which may have contributed to the mixed findings. In the present study (and in Fenske & Eastwood, 2003), the primary measure of emotional interference reflected the difference in response times on

incompatible-affect flanker trials and compatible (flanker compatibility effect) or no-flanker (flanker/no-flanker effect) trials. Thus, the comparison trial conditions involved no interference (no-flanker trials) or minimal interference (compatible flanker trials) from distracting stimuli. In the studies by Zetsche and colleagues, the measure of interference reflected the difference in response times on trials in which the distractors were opposite in valence to the target and trials in which the distractors were neutral. Despite being neutral in valence, neutral distractors were still incongruent with the target stimulus and would have thus interfered with responding. The difference in response times between trials with incompatible-affect distractors and neutral distractors may be a more conservative measure of affect-related interference then, and reduced variance on this measure could have decreased or obscured associations with depression in the studies by Zetsche and Joormann (2011) and Zetsche et al. (2012).

No specific hypotheses were made about the performance of remitted depressed participants on the EFT given the limited prior research on inhibition in remitted depression. It was found that remitted depressed individuals did not show similar reduced inhibition of negative task-irrelevant information as currently depressed individuals. The flanker compatibility and flanker/no-flanker effects for trials involving happy target faces with sad flanking faces were marginally larger for currently depressed participants than remitted depressed participants, whereas remitted depressed and control participants did not differ significantly from each other. A few studies employing the emotional Stroop task (Gotlib & Cane, 1987; Hedlund & Rude, 1995; Merens et al., 2008) and the NAP task (Joormann & Gotlib, 2010) have also found that remitted depressed participants did not differ from control participants in their inhibition of negative information (but see Joormann, 2004 for an exception). Reduced inhibition of negative

information may therefore be characteristic of the symptomatic phase of depression but improve upon remission.

It is possible that reduced inhibition of negative material only emerges for remitted depressed individuals under periods of stress or negative affect through a diathesis-stress process (Scher, Ingram, & Segal, 2005). If so, it may be necessary to use a negative mood induction or other priming strategy to observe these biases in remitted depressed participants. Some prior cognitive bias research has demonstrated that following a mood induction, remitted depressed individuals demonstrate negative biases in memory, interpretation, and attention that were absent prior to the induction (see Scher et al., 2005 for a review). However, other studies have found biases in these processes in remitted depression without the use of mood inductions or priming (Fritzsche et al., 2010; Joormann & Gotlib, 2007; Soltani, Newman, Quigley, Fernandez, Dobson, & Sears, 2015). Moreover, there is some evidence that even following negative mood induction or priming, previously depressed individuals do not show inhibition biases on the emotional Stroop task (Gilboa & Gotlib, 1997; Hedlund & Rude, 1995). Additional research employing mood induction procedures is needed to examine remitted depressed individuals' inhibition of negative material in and out of a negative mood state. Unexpectedly, remitted depressed participants showed a reduced ability to inhibit the processing of positive distracting information in the present study. Remitted depressed participants had larger flanker/no-flanker effects than currently depressed and control participants for trials involving sad target faces with happy flanking faces, but not sad target faces with neutral flanking faces. Although this result was not predicted, it is consistent with the finding of Joormann and Gotlib (2010) that remitted depressed individuals had reduced inhibition of positive words, but not negative words, on the NAP task. It is possible that reduced inhibition of positive material reflects a protective bias that

precedes or maintains the remission of depression among vulnerable individuals. Given the novelty of this finding, and that remitted depressed participants' reduced inhibition of positive flankers was found only for the flanker/no-flanker effect measure and not for the flanker compatibility effect measure in the present study, this interpretation is put forth with caution. Research investigating this possibility is warranted.

4.2 Updating

Updating of working memory was assessed in the present study using the ANB (Levens & Gotlib, 2010). Participants were presented with a series of happy, sad, and neutral faces and identified whether the presented facial expression matched the facial expression from two trials earlier. Participants also completed a 0-back task in which they identified whether presented faces matched a target facial expression. The 0-back task provided a measurement of participants' ability to perceive and categorize emotional expressions. In contrast, the 2-back portion of the ANB assessed participants' ability to perceive and update the contents of working memory with an emotional expression, and compare the current emotional expression to a previous expression held in working memory. The ANB consists of several trial types that vary in the valence of the incoming and outgoing emotional expressions and in the cognitive processes that are involved. Thus, participants can be compared on the speed with which they integrate and disengage from different types of emotional stimuli in working memory. The ANB requires both the addition of new stimuli to working memory, and the removal of no-longer-relevant stimuli from working memory, and thus is an inclusive measure of the updating function of executive control.

Contrary to hypothesis, the participant groups did not differ in valence-specific response times for any of the trial types on the ANB. However, currently depressed participants had lower

accuracy rates for all trial types than control participants. This group difference was not moderated by the valence of the emotional expression to be integrated or discarded from working memory. On the 0-back task, there was a significant group difference only for accuracy rates for no-match trials involving neutral faces. Currently depressed participants had lower accuracy rates than both remitted depressed and control participants in categorizing neutral faces when the target expression was happy or sad. This result suggests that currently depressed individuals may have difficulty distinguishing neutral faces from other emotional expressions. Prior research has also found that depressed individuals show biases in the recognition of neutral faces (Gur et al., 1992; Leppänen, Milders, Bell, Terriere, & Hietanen, 2004). For instance, Leppänen et al. (2004) compared depressed and control participants on a forced-choice task that required participants to identify presented facial expressions as happy, neutral, or sad. Although depressed participants did not differ significantly from control participants in their recognition of happy and sad faces, they were less accurate in their recognition of neutral faces. Leppänen et al. proposed that depressed individuals may not readily perceive neutral faces as affectively neutral. In contrast to these findings, Levens and Gotlib (2010) did not observe a difference between currently depressed and control participants in accuracy rates for any emotional expression on the 0-back task. However, their data showed that accuracy rates were lower overall for neutral faces than both happy and sad faces. Inspection of the accuracy rates for neutral faces on the 0-back task indicates that control participants in Levens and Gotlib (2010) were much less accurate than control participants in the current study (76% versus 96% for match trials and 83% versus 95% for no-match trials, respectively), which could be due to the use of different stimulus sets across the two studies. The low accuracy rate for neutral faces in both depressed and control samples in Levens and Gotlib (2010) may have precluded detection of a depression-specific impairment.

Given that currently depressed participants were impaired in their recognition of neutral faces specifically on the 0-back task, and did not differ from the other participant groups in their recognition of happy and sad faces, it is unlikely that their lower performance on the ANB was due solely to impaired recognition of emotional expressions. Rather, in addition to a specific deficit in the recognition of neutral expressions, currently depressed individuals appear to have difficulty updating working memory with emotional stimuli. This finding is consistent with previous evidence that depression is associated with emotional biases in working memory updating (Joormann & Gotlib, 2008; Joormann et al., 2010; Levens & Gotlib, 2010; Yoon et al., 2014; Zetsche et al., 2012).

Only one previous study used the ANB to compare currently depressed and control participants on updating biases (Levens & Gotlib, 2010). Two differences between the results of that and the current study warrant discussion. First, in the current study group differences emerged on 2-back accuracy rates, but not response times, whereas Levens and Gotlib (2010) observed group differences on 2-back response times, but not accuracy rates. Cognitive researchers have distinguished between performance *effectiveness* (measured by response accuracy) and performance *efficiency* (measured by response times) on cognitive experimental tasks in which participants are required to make a response to a stimulus (Eysenck, Derakshan, Santos, & Calvo, 2007). Eysenck et al. (2007) proposed that negative emotion has the potential to influence both performance effectiveness and efficiency. If sufficient cognitive resources are available, individuals may be able to preserve performance effectiveness on a task through increased effort and the allocation of more processing resources to the task, but at the cost of reduced efficiency (i.e., greater response times). If resources are unavailable or insufficient, however, performance effectiveness will be reduced. Thus, an effect on performance efficiency

(i.e., greater response times) in one study and an effect on performance effectiveness (i.e., lower response accuracy) in another may not represent truly discrepant findings. Rather, in both studies a detrimental effect of depression on working memory updating involving emotional material was demonstrated. Participants in the current study completed the ANB at the end of the second study session following the two other executive control tasks. It is possible that their cognitive resources were depleted, which would explain why group differences emerged on performance effectiveness (Schmeichel, 2007). In Levens and Gotlib (2010), currently depressed participants may have been able to preserve effectiveness through increased utilization of available processing resources, which would lead to the observed reduction in efficiency. Other studies involving the ANB in non-clinical samples have also used accuracy rates as the primary dependent variable, arguing that response times are only a reliable measure of ability when accuracy rates are high (e.g., > .80; Pe et al., 2013b).

Second, in the present study currently depressed participants had reduced accuracy relative to control participants on the ANB overall, across trial types and emotional expressions. In contrast, Levens and Gotlib (2010) found group differences in response times for certain trial types and emotional expressions. Specifically, currently depressed participants had slower response times for match set trials for all emotional expressions and for break set trials for sad faces, and faster response times for break set trials for happy faces and for no set trials for sad faces, relative to control participants. Thus, currently depressed individuals had valence-specific biases in updating, such that they integrated sad stimuli more quickly into working memory and disengaged from happy and sad stimuli more quickly and slowly, respectively. Although these findings are consistent with the perspective that depression is associated with biases that favour the maintenance of negative material in working memory (Beck et al., 1979; Gotlib & Joormann,

2010), it is not clear that valence-specific biases should necessarily be observed on the ANB. In each trial of the ANB, multiple emotional expressions are held and manipulated in working memory. For instance, on a sad break-set trial, participants must disconnect a matched pair of sad faces held in working memory (from one trial ago and three trials ago) and discard the sad face from three trials ago. However, there is also the face image from two trials ago, which must be shuffled down the order of stimuli held in working memory, and the current face image, which must be integrated into working memory, and these may be of other emotional expressions. Thus, any of the emotional expressions being manipulated on a given trial of the ANB may influence responding. Furthermore, new content being integrated into working memory may interact with and modify existing content (Levens & Gotlib, 2015). For example, the integration of sad content into working memory may enhance the intensity or salience of other sad content, negatively bias neutral content, and/or weaken the intensity or salience of positive content in working memory (Levens & Gotlib, 2015). The manipulation and interaction of multiple emotional expressions on each trial of the ANB may explain why currently depressed participants showed impaired performance overall in the present study, rather than specific impairments for certain trial types and emotional expressions.

The overall impaired performance on the ANB among currently depressed participants, rather than trial- and valence-specific impairments, leads to an issue in its interpretation. It is unclear whether the lower performance of currently depressed participants was due to difficulty in updating working memory with emotional stimuli or to difficulty in updating working memory generally. As noted before, there is an important distinction between *deficits* and *biases* in executive control, and prior research more strongly supports the existence of biases than broad deficits in depression (Grant et al., 2001; Joormann & Gotlib, 2010). Studies that have examined

performance on versions of the n-back task involving non-affective stimuli have generally not found any differences between currently depressed and control participants on accuracy rates or response times (Barch, Sheline, Csernansky, & Snyder, 2003; Harvey et al., 2005; Matsuo et al., 2007; Rose, Simonotto, & Ebmeier, 2006, but see Harvey et al., 2004 for an exception). Thus, it appears likely that the reduced accuracy of currently depressed participants on the ANB was due to difficulty in updating working memory with emotional stimuli, rather than a general deficit. The present data cannot disconfirm a general deficit, however, as a non-affective version of the n-back task was not administered.

Due to limited prior research, no specific hypotheses were made concerning working memory updating in remitted depressed participants. The remitted depressed participants did not differ from control participants in response times on any of the ANB trial types, but were marginally less accurate overall. Thus, there is some evidence for the persistence of emotional biases in working memory updating in remitted depression, although these do not appear to be as strong as biases in current depression. Levens and Gotlib (2015) found that remitted depressed participants exhibited updating biases characterized by slower disengagement from sad stimuli and slower integration of neutral stimuli into working memory. Again, it is possible that impairments in working memory updating involving emotional material may be more reliably observed in remitted depressed individuals when activated by a negative mood state, and future research should investigate this possibility.

4.3 Shifting

Shifting was assessed using the Colour-Emotion Task (CET). Developed for the purpose of the present study, the CET measures the speed with which participants are able to shift from an emotion-relevant task set (i.e., identifying the affect displayed in an image of a facial

expression) to an emotion-irrelevant task set (i.e., identifying the colour of a frame around an image of a facial expression) and vice versa. Larger switch costs when shifting from an emotion-relevant task set to an emotion-irrelevant task set versus when shifting from an emotion-irrelevant task set to an emotion-relevant task set would indicate a specific difficulty in shifting away from the processing of emotional information. The CET consists of two conditions: a no-crosstalk condition and a crosstalk condition. The no-crosstalk condition does not require participants to inhibit a response set from the currently inappropriate task and thus may be considered a "purer" measure of shifting ability, whereas the crosstalk condition involves a greater degree of response inhibition in addition to set shifting. Within the crosstalk condition, however, it is possible to compare switch costs on trials in which the prior target image was a sad face and switch costs on trials in which the prior target image was a happy face. Larger switch costs when shifting task sets when the prior target image was sad versus happy would indicate a particular difficulty in shifting away from the processing of negative information.

As hypothesized, currently depressed participants had larger switch costs when switching away from the emotion task (i.e., emotion-relevant) to the colour task (i.e., emotion-irrelevant) than when switching away from the colour task to the emotion task, whereas remitted depressed and control participants did not show this bias. Thus, it appears that currently depressed individuals have particular difficulty switching away from task sets that require the processing of emotional information. This is the first study to examine biases in shifting in a clinically depressed sample using a paradigm that requires switching back and forth between task sets in a single block of trials. Thus, these data provide the first clear demonstration that depression is associated with emotion-specific biases in set shifting. In a non-clinical sample, De Lissnyder et al. (2010) also found that dysphoric participants had greater shift costs when shifting from an

emotion-relevant task than when shifting from an emotion-irrelevant task, but only when they restricted their dysphoric sample to individuals who scored above the clinical cut-off on the BDI-II. Thus, previous research in non-clinical samples may have underestimated the association between depression and emotional set-shifting biases (e.g., De Lissnyder et al., 2010; Everaert et al., 2016b).

The larger switch costs for currently depressed participants when shifting from the emotion task to the colour task was observed in the no-crosstalk condition only. This pattern of findings may be accounted for by the fact that the no-crosstalk condition provides a more specific measure of shifting than the crosstalk condition. In the no-crosstalk condition, the irrelevant attribute of the target image (i.e., the facial expression in the colour task and the colour of the frame in the emotion task) is not associated with any response, and is thus considered neutral. In the crosstalk condition, on one-third of trials the irrelevant attribute is neutral, but in the other two-thirds of trials, it is associated with either a congruent or incongruent response, with the correct response dictated by the relevant attribute of the target image. The presence of an irrelevant attribute that is associated with a response in the currently inappropriate task makes that task harder to inhibit (Rogers & Monsell, 1995). Thus, although all shifting tasks require inhibition of the currently inappropriate task set, the crosstalk condition also requires inhibition of a currently inappropriate response set on most trials. The added inhibition demands of the crosstalk task trials may have obscured differences in switch costs across participant groups. Researchers using similar switch tasks to assess shifting biases in future studies may therefore wish to use only the no-crosstalk condition.

The advantage of the crosstalk condition is the ability to calculate separate switch costs for trials in which the prior target image was negative and trials in which the prior target image

was positive. In the present study, no group differences were observed between switch costs for prior sad faces and switch costs for prior happy faces, in either the colour or emotion task. This was not particularly surprising, given that there were no group differences in overall switch costs for either the colour or emotion task in the crosstalk condition, as discussed above. As previously noted, separate switch costs for trials in which the prior target image was negative and trials in which the prior target image was positive could not be computed in the no-crosstalk condition (because the prior target image was always a neutral face for no-switch trials in the colour task and switch trials in the emotion task). However, examination of response times on switch trials for the colour task in the no-crosstalk condition indicated that currently depressed participants did not differ in their response times when switching away from happy versus sad faces. Thus, it is unlikely that the larger switch costs for currently depressed participants when shifting from the emotion task to the colour task in the no-crosstalk condition were due to difficulty disengaging attention from a negative stimulus. Rather, the larger switch costs for currently depressed participants appear to be related to difficulty shifting away from a task set that requires emotional processing. Other studies have similarly found that depressed participants do not demonstrate larger switch costs specifically when shifting from negative stimuli (De Lissnyder et al., 2010; 2012b). However, these studies either did not involve a clinical sample (De Lissnyder et al., 2010) or did not employ a paradigm that requires switching back and forth between task sets in a single block of trials (De Lissnyder et al., 2012b). Additional research on task set shifting in clinical depression is therefore needed to corroborate the present results.

No hypotheses were made regarding the performance of remitted depressed participants on the CET given the lack of prior research on shifting biases in remitted depression. Remitted depressed participants did not show similar impairments in shifting away from an emotion-

relevant task set as currently depressed participants. Thus, shifting biases appear to be specific to the symptomatic phase of depression. There exist no studies with which to directly compare these results, but the lack of a bias in set shifting is consistent with the general lack of biases observed among remitted depressed participants on the other executive functioning tasks in the present study. As discussed in the previous sections, it is also possible that shifting biases are a latent vulnerability among remitted depressed individuals that must be activated by stress or negative mood to be observed. Future research should test the diathesis-stress hypothesis by examining set shifting biases in remitted depressed individuals in and out of a negative mood state.

4.4 Non-Affective Executive Functioning

Non-affective executive functioning was assessed using the WCST (Heaton, 1981; Heaton et al., 1983). The WCST consisted of four key cards and 128 response cards with figures that varied along the following dimensions: colour (red, green, yellow, or blue), number (one, two, three, or four), and shape (triangle, star, cross, or circle). The task of participants was to correctly match each of the response cards to one of the four key cards according to one of the stimulus dimensions, which constituted the current matching rule. Participants were not told the matching rule or that the matching rule would change throughout the task, which required participants to determine the correct matching rule based on feedback and to flexibly change strategies in response to the changing contingencies. The task continued until participants completed six full matching categories or responded to 128 cards, whichever occurred first. Several scores on the WCST were used to evaluate performance. Number of trials was the total number of trials completed by the participant during the task, where a smaller number of trials indicates that the participant required fewer trials to complete all six matching categories, and

thus reflects better performance. Number of perseverative errors was the number of incorrect responses that were due to an attempt to match a response card on the basis of a matching rule that was established as incorrect. Number of non-perseverative errors was the number of incorrect responses that were not due to perseverating on an incorrect matching rule. A greater number of perseverative and non-perseverative errors reflect worse performance on the task. Finally, failure to maintain set was the number of times a participant made an error before completing a category after making five or more consecutive correct matches during that category. A higher failure to maintain set reflects worse performance on the task.

Overall, the results from the WCST indicated no reliable difference in performance across the participant groups. Currently depressed participants had a marginally greater number of non-perseverative errors than the control group. However, participant groups did not differ significantly on any of the other WCST scores, and there was no significant effect of group when the dependent variables were considered together in a multivariate analysis. Thus, currently depressed participants did not demonstrate an appreciable impairment in executive functioning relative to control participants when processing non-affective stimuli. In contrast to this finding, previous research has found that dysphoric and depressed individuals perform worse on the WCST than controls (Channon, 1996; Merriam et al., 1999; Moritz et al., 2002; Must et al., 2006). However, a few other studies have not found differences between depressed and control participants on the WCST (Fossati et al., 1999; 2001; Martin, Oren, & Boone, 1991). It may be that depression severity is related to performance on the WCST (Martin et al., 1991; Merriam et al., 1999), and that impairments may be more likely to be observed among samples with a greater severity of symptoms, such as inpatient samples.

Research involving other executive control tasks to measure inhibition, updating, and shifting abilities when processing non-affective stimuli has yielded similar mixed findings. Although depression has been associated with executive control deficits in some studies, this association has not been consistently observed across different tasks and studies (e.g., Grant et al., 2001; Harvey et al., 2004; 2005; Joormann et al., 2010; Purcell et al., 1997), and the presence of deficits may be related to indicators of severity, such as hospitalization and comorbidity. Currently depressed participants in the present study were non-treatment seeking individuals recruited from the community, which may explain why they did not exhibit appreciable deficits in general executive functioning. Taken together, this evidence corroborates the notion that depression is more reliably associated with biases in executive control that prioritize the processing of emotional material – and especially, negative material – than with general deficits in executive functioning. Findings of valence-specific and emotion-specific impairments in inhibition and set shifting in the present study also provide support for this notion.

4.5 Executive Control Biases and Rumination

Rumination was assessed using the RRS, which is a 22-item self-report measure of the tendency to engage in rumination in response to a negative mood (Nolen-Hoeksema & Morrow, 1991). In addition to a total rumination score, the RRS provides scores on reflection and brooding subscales (Treynor et al., 2003). Reflection is the tendency to engage in cognitive problem solving aimed at improving one's negative mood or situation, and is considered the more adaptive form of rumination. Brooding is the tendency to passively focus on one's negative mood or situation, and is considered the maladaptive form of rumination. Items from the full RRS that appeared to overlap with depressive symptoms were removed from the reflection and

brooding subscales, and thus these subscales are conceptualized to be relatively independent of depression symptoms (Treynor et al., 2003).

It was hypothesized that executive control biases for negative material would be related to rumination, and in particular, to depressive brooding as a maladaptive form of rumination. It was further hypothesized that these relationships would be strongest among currently depressed participants, and thus most likely to be observed in this group. Little support was found for these hypotheses. To test the association between negative biases in inhibition and rumination/brooding, correlations were computed between the flanker compatibility and flanker/no-flanker effects for happy faces with incompatible-affect (i.e., sad) flankers on the EFT and rumination, reflection, and brooding. Within the full sample, there were no significant correlations. When these correlations were examined within the separate participant groups, the only significant association that emerged was within the currently depressed group. For currently depressed participants, the flanker/no-flanker effect for happy faces with sad flankers was significantly negatively correlated with reflection scores. This correlation remained after controlling for individual differences in depression symptoms. Thus, currently depressed individuals who demonstrated less response interference from negative task-irrelevant stimuli reported a greater tendency to engage in the reflective form of rumination. There were no significant associations between negative biases in inhibition and rumination among remitted depressed and control participants.

The finding that a better ability to inhibit negative material was associated with greater reflection among currently depressed participants was unexpected, and may be open to alternative interpretations. On the one hand, if reflection is considered to be an adaptive form of rumination, then it may be theoretically consistent that depressed individuals who are better able

to resist interference from negative stimuli would tend to engage in more adaptive forms of ruminative thinking. These individuals may have a better ability to prevent self-reflection from devolving into more maladaptive forms of rumination, such as passive brooding. On the other hand, adaptive and maladaptive forms of rumination have not been found to be clearly distinguishable among currently depressed individuals (Whitmer & Gotlib, 2011). Whitmer and Gotlib (2011) conducted a factor analysis of the RRS in samples of currently depressed, remitted depressed, and control participants. Among remitted depressed and control participants, the results supported a two-factor solution representing the reflection and brooding subtypes. Among currently depressed participants, however, items did not clearly load on reflection and brooding factors. Whitmer and Gotlib suggested that in a depressed state, reflective self-analysis may elicit brooding, as well as become maladaptive itself through distorted thought processes. If reflection is maladaptive in the context of current depression, then the finding of a correlation between greater inhibition of negative material and reflection is inconsistent both with theory (e.g., Koster et al., 2011) and with prior research that has found a link between reduced inhibition of negative material and rumination (Joormann & Gotlib, 2010). It should be noted that several other studies examining the association between inhibition biases and rumination have yielded inconsistent or null results (e.g., Goeleven et al., 2006; Zetsche & Joorman, 2011; Zetsche et al., 2012). In fact, Zetsche and Joormann (2011) also unexpectedly found that less response interference from negative stimuli on the EFT was related to greater rumination in a non-clinical sample. Additional research is needed to resolve these discrepant findings.

The association between negative biases in working memory updating and rumination/brooding was tested by computing correlations between response times for sad match-set, sad break-set, sad perseveration-set, and sad no-set trials on the ANB and rumination,

reflection, and brooding. No significant correlations were observed in the full sample after controlling for depression. Within-group analyses indicated that there was a marginally significant negative correlation between response times on sad match-set trials and reflection for remitted depressed participants, controlling for depression symptoms. Thus, remitted depressed participants who integrated sad stimuli into working memory more quickly tended to engage in greater reflective rumination. The interpretation of this finding is again dependent on the adaptive versus maladaptive nature of reflection. Whether there is truly an adaptive form of rumination has been debated in the literature (Nolen-Hoeksema et al., 2008), although Whitmer and Gotlib (2011) did find clearly distinguishable reflection and brooding factors on the RRS among a remitted depressed sample. If reflection is adaptive among remitted depressed individuals, then it is not clear why faster integration of sad stimuli in working memory would be associated with greater reflection. Given its marginal significance, and the unclear nature of reflective rumination, future studies are needed to determine the reliability of this finding.

Among currently depressed participants, none of the associations between the response times for the various trial types and rumination or brooding were significant after controlling for depression. This finding is inconsistent with prior research that has found that difficulty in removing negative information from working memory is related to greater rumination among currently depressed individuals (e.g., Joormann et al., 2010; Joormann & Gotlib, 2008; Zetsche et al., 2012), and with theory that proposes that difficulty in disengaging from negative material in working memory may underlie depressive rumination (Koster et al., 2011). Given the association between ANB accuracy rates and depression status in the between-group analyses, correlations were also conducted between mean accuracy rates and rumination, reflection, and

brooding. No significant correlations were observed between accuracy rates and the rumination measures either in the full sample or within the participant groups.

To test the association between emotion- and valence-specific biases in set shifting and rumination/brooding, correlations were computed between CET switch costs when shifting away from the emotion task, as well as switch costs when shifting task sets following a sad face specifically, and rumination, reflection, and brooding. No significant correlations were observed in the full sample. Within-group analyses indicated that there were marginally significant correlations between switch costs when shifting away from the emotion task in both the no-crosstalk and crosstalk conditions and reflection for currently depressed participants. After controlling for individual differences in depression symptoms, these correlations remained marginally significant. Switch costs for shifting task sets following a sad face specifically were not related to any of the rumination measures. Thus, greater reflection appears to be related to difficulty shifting away from an emotion-relevant task set, rather than difficulty shifting attention from negative material specifically, among currently depressed individuals. For remitted depressed and control participants, none of the associations between switch costs and rumination were significant.

Given that rumination involves repetitive processing of the emotional aspects of situations, it is theoretically consistent that difficulty shifting away from a task set that involves emotional processing would be related to rumination. This finding is also consistent with prior research that has found that difficulty switching between task sets and emotional representations in working memory is related to rumination, despite some inconsistencies across study findings (De Lissnyder et al., 2010; 2012a; 2012b; 2012c; Demeyer et al., 2012). In a non-clinical sample, De Lissnyder et al. (2010) found that brooding significantly predicted impaired shifting on the

affective shift task, controlling for depression. The finding that impaired shifting was associated with brooding in a non-clinical sample, and with reflection in the current clinically depressed sample, may reflect the varying distinctiveness of reflection among different samples. If reflection is not distinct from maladaptive forms of rumination among currently depressed populations (Whitmer & Gotlib, 2011), then this may explain the differential associations observed across studies. Even so, the present finding that difficulty shifting from an emotion-relevant task set is associated with reflection among currently depressed participants requires replication, particularly given the marginal significance of the finding.

In sum then, biases in inhibition, working memory updating, and shifting were largely unrelated to rumination in the present study. The hypothesis that the relationships between executive control biases and rumination would be strongest in the context of depression was somewhat supported, given that significant correlations were observed only within the currently depressed and remitted depressed groups. However, even among depressed individuals, the observed patterns of correlations were mostly inconsistent with the theorized relationships. The hypothesis that executive control biases would be most strongly related to the presumed maladaptive form of rumination – brooding – was entirely unsupported, as none of the bias measures were significantly related to brooding scores. As discussed, this may reflect the fact that relationships between biased executive control and rumination were observed for depressed individuals only, and there is no clear distinction between adaptive and maladaptive forms of rumination in the context of current depression (Whitmer & Gotlib, 2011).

It is possible that there are important moderators and/or mediators of the relationship between executive control biases and rumination that may contribute to the inconsistent findings presently and in the literature broadly. Two recent papers that were part of the same larger study

found that negative biases in inhibition, updating, and shifting were associated with attention and interpretation biases (Everaert et al., 2016b), and that attention and interpretation biases were associated with depressive symptoms via brooding (Everaert et al., 2016a). As these two studies were published separately, it is unknown whether executive control biases were directly associated with rumination or brooding in the data. Nevertheless, these studies suggest that biases in higher-order cognitive processes such as attention and interpretation may be mediators of the relationships between executive control biases, rumination, and depression. Other studies have found that biases or deficits in executive control may moderate the impact of rumination on emotional experience. In a daily life study, Pe et al. (2013a) observed no direct association between the ability to discard previously relevant negative information from working memory and rumination. However, an impaired ability to discard negative information was significantly associated with a greater increase in negative emotion after engaging in rumination. Pe et al. (2013b) also examined the relationship between working memory updating, rumination, and emotion, using accuracy rates on the ANB as the index of updating, in a laboratory study and a daily life study. In both studies, the relationship between rumination and negative emotion was greater for participants with low versus high updating ability, whereas no direct associations between updating and rumination were observed. These two studies suggest that executive control biases may not be related to the use of rumination per se, but to the emotional impact of rumination. Studies examining more complex relationships between executive control and rumination are a recent development in the literature and have not yet extended to clinical populations. Additional studies, including those in the context of clinical depression, will help to advance understanding of these relationships.

4.6 Theoretical and Clinical Implications

The present results hold important implications for theories of depression vulnerability. It was found that depression is associated with biases for emotional information across the three main components of executive control. Previous studies had reliably found biases in inhibition among depressed individuals (e.g., Epp et al., 2012; Goeleven et al., 2006; Joormann, 2004; Joormann & Gotlib, 2010; Zetsche & Joormann, 2011; Zetsche et al., 2012), and the current evidence corroborates these earlier findings. However, there was limited prior research on biases in working memory updating in depression, particularly when examining continuous updating involving both the addition and removal of stimuli. In addition, no previous studies had examined biases in shifting between task sets among depressed individuals using a task designed specifically to assess that function. Thus, the present results are a significant and novel contribution to the literature on executive control biases in depression. This is the first study to evaluate and demonstrate depression-related biases in all three components of executive control.

Biases in executive control may contribute to depression in various ways. Impairments in the abilities to inhibit emotional stimuli from entering working memory, update working memory with emotional stimuli, and shift away from emotional stimuli in working memory would increase the availability and processing of such material. Increased processing of emotional, and especially negative, material may directly lead to negative mood states. In turn, negative mood may further bias executive control and influence depressive symptomatology. Biases in executive control may also contribute to depression through their influence on negative thinking. It is well-documented that depression is characterized by negative thoughts, attributions, and beliefs (Clark et al., 1999). Biases that increase the availability of negative and other emotionally laden information in working memory may reinforce negative thinking. An

overly negative internal cognitive environment may serve as evidence to depressed individuals of the validity of their negative thoughts and attributions. In addition, current research proposes that biases in executive control may underlie biases in higher-order cognitive processes, such as attention and interpretation, which in turn may lead to depression (Everaert et al., 2016b).

As outlined in theoretical accounts, executive control biases may underlie rumination and lead to ineffective emotion regulation (Koster et al., 2011), although these relationships were generally not observed in the present data. Impairments in the abilities to inhibit, update, and shift away from emotionally charged information may make it more difficult for depressed individuals to disengage from ruminative thinking and engage in more adaptive forms of emotion regulation such as reappraisal (Joormann & D'Avanzato, 2010). Rumination may also interfere with effective problem solving and thus exacerbate ongoing stressors (Nolen-Hoeksema et al., 2008). Finally, executive control biases may contribute to depression via interpersonal functioning impairments. In the present study, biases in inhibition, updating, and shifting were found among currently depressed individuals for emotional face images. Facial emotion provides important information about the emotional states of others, and therefore unbiased processing of facial emotion is likely to be crucial to healthy social functioning. Biases that enhance the processing of negative social cues may contribute to the documented interpersonal risk factors for depression (Dobson, Quigley, & Dozois, 2014; Joiner & Coyne, 1999).

The results of this study also indicate that the biases in executive control associated with depression are not found in euthymic remitted depressed individuals. Across the three executive control tasks, remitted depressed individuals demonstrated similar performance to controls (with the exception of ANB accuracy rates, on which a marginal group difference was found). This finding suggests that executive control biases may be part of an active episode of depression, but

improve along with the remission of other symptoms as an individual recovers from a depressive episode. This finding does not preclude the possibility that remitted depressed individuals demonstrate emotional biases in executive control when under stress or in a negative mood state. The cognitive model of depression proposes that underlying cognitive vulnerability is activated by negative life events and stressors, and it is the interaction of vulnerability and stress that typically leads to the onset and persistence of depression (Beck et al., 1979; Clark et al., 1999). In a depressed state, underlying vulnerabilities are activated and thus biases in executive control are readily observed. However, once individuals have recovered from a depressive episode and are in a euthymic state, executive control biases – if they remain as a latent cognitive vulnerability – may not be observed unless activated. It is possible that biases in executive control in combination with negative mood states may increase risk for relapse among remitted depressed individuals. Indeed, the high rate of relapse in depression strongly suggests that risk factors remain after an individual recovers from an active depressive episode (Gotlib & Hammen, 2002). Future research must evaluate whether remitted depressed individuals demonstrate executive control biases similar to currently depressed individuals when in a state of stress or negative mood.

The implications of the present results for theories of the relationship between biased executive control and rumination are limited, given the largely null and theoretically inconsistent correlations among these variables. However, among currently depressed participants, impaired shifting from an emotion-relevant task set was marginally related to the reflective form of rumination, controlling for depressive symptoms. This finding thus provides some support for the notion that rumination is due to difficulty shifting away from the processing of emotional material (Koster et al., 2011). The observed results also indicate that associations between

executive control biases and rumination may not be restricted to the putative maladaptive form of rumination – brooding – which is consistent with the proposition that adaptive and maladaptive forms of depressive rumination are not distinct (Whitmer & Gotlib, 2011). Thus, it might be expected that executive control biases may be related to either of the forms of rumination, or overall rumination, in studies with clinical samples.

The results of the present study also have clinical relevance. Given that executive control biases may contribute to depression through various mechanisms, targeting these biases as part of depression treatment may improve outcomes. Biases in executive control may be targeted via a top-down or bottom-up approach. A top-down approach would involve working with depressed individuals to change their negative thinking and beliefs, in order to produce subsequent positive changes in depressive schemas and executive control biases. Indeed, the modification of maladaptive cognition is the aim of the gold-standard psychological treatment for depression, cognitive-behavioural therapy (CBT; Beck et al., 1979; Beck & Dozois, 2011, Cuijpers et al., 2013). To the extent that CBT and other evidence-based psychotherapies for depression produce changes in depressive schemas, corresponding improvements in executive control biases may result. In turn, improvements in executive control biases may produce positive change in mood, thinking, emotional regulation, and social functioning. However, there is little support as of yet for executive control biases as a mechanism of change in CBT. Recent studies provide evidence that modifying interpretations can produce corresponding biases in memory (Joormann, Waugh, & Gotlib, 2015; Tran, Hertel, & Joormann, 2011). In both depressed and non-clinical samples, training participants to interpret ambiguous scenarios in either a positive or negative manner resulted in more memory intrusions that were congruent in valence with the training condition. These studies provide some preliminary support for the notion that teaching depressed

individuals to engage in positive reappraisal as part of CBT may produce corresponding improvements in cognitive biases. Future research is needed to evaluate whether CBT and other psychotherapies produce changes in executive functioning biases specifically.

A bottom-up approach would involve modifying executive control biases directly in order to produce subsequent improvements in mood and depressive symptoms. This approach belongs to the category of approaches termed cognitive bias modification (CBM). CBM paradigms were initially developed to permit experimental manipulation of cognitive biases in order to test causal hypotheses about the effect of biases on mood and stress reactivity (MacLeod, Rutherford, Campbell, Ebsworthy, & Holker, 2002). Researchers have since become interested in whether CBM procedures might have efficacy for the treatment of anxiety and depression symptoms. Although studies of CBM in the context of depression are relatively recent, the data thus far have been somewhat underwhelming. Hallion and Ruscio (2011) conducted a meta-analysis of the efficacy of CBM procedures for the reduction of anxiety and depression symptoms. They found that CBM had a small effect on symptoms at post-test, but this effect was reduced to non-significance when publication bias was considered. In studies that measured symptoms after exposure to a stressor, CBM had a small effect on symptoms that remained significant after controlling for publication bias. When depression outcomes were considered separately from anxiety, however, the effect of CBM was non-significant in studies that measured depression symptoms both at post-test and following exposure to a stressor. A more recent meta-analysis of CBM in anxiety and depression yielded even less encouraging results (Cristea, Kok, & Cuijpers, 2015). After removing outliers and adjusting for publication bias, fifteen randomized controlled trials of CBM for depression symptoms produced a non-significant combined effect size of g = .09. Hallion and Ruscio (2011) suggested that the

negative findings may be because CBM does not produce sufficient change in biases to influence clinical symptoms substantively, or CBM could have a more gradual effect on symptoms that only becomes apparent over time.

Studies of CBM in depression have typically attempted to modify biases in attention or interpretation (Cristea et al., 2015; Hallion & Ruscio, 2011). No studies have examined whether depression-related executive control biases can be manipulated, or whether manipulation of executive control biases produces corresponding changes in depression symptoms. Schweizer, Hampshire, and Dalgleish (2011) tested the effect of working memory training involving either emotional or non-emotional material on subsequent executive control in a non-clinical sample. They found that participants in either working memory training condition demonstrated post-training improvements on another measure of working memory and a measure of fluid intelligence, relative to participants in a control training condition. However, only participants who received working memory training involving emotional material showed post-training improvement on a version of the emotional Stroop task. Thus, there is initial evidence that executive control involving emotional material can be improved in non-clinical samples. Future research is required to determine whether these findings generalize to clinical samples, and whether CBM targeting executive control biases produces a reduction in depression symptoms.

Executive control may also be directly influenced by mindfulness training. Mindfulness has been described as “paying attention in a particular way: on purpose, in the present moment, and nonjudgmentally” (Kabat-Zinn, 1994, p. 4). Several psychological interventions have been developed that are based on or incorporate the principles of mindfulness (e.g., Kabat-Zinn, 1982; Segal, Williams, & Teasdale, 2002). These interventions involve mindfulness meditation exercises that train nonjudgmental awareness of thoughts, emotions, and bodily sensations.

Although research is limited, mindfulness based therapies have been shown to be effective for reducing depression symptoms among clinically depressed individuals (Hofmann, Sawyer, Witt, & Oh, 2010). It has been proposed that mindfulness based therapies may have a beneficial effect on depression vulnerability through their influence on attentional or executive control capacity (Baer, 2003; Teasdale, Segal, & Williams, 1995). Preliminary evidence supports this proposition. Chambers, Lo, and Allen (2008) evaluated the effect of mindfulness training on attentional control and working memory in a sample of 20 non-clinical individuals who completed an intensive 10-day mindfulness meditation retreat. The results indicated that individuals who completed the training demonstrated improvements in working memory from pre-test to post-test, whereas the control group did not. Mindfulness training also produced reductions in depressive symptoms not seen in the control group. Thus, this study provides initial evidence that mindfulness training may produce positive change in working memory and sustained attention ability, as well as depressive symptomatology. Additional research is needed to determine whether mindfulness produces similar benefits in working memory and cognitive control in depressed samples, and to establish the reliability of these effects.

The effect of mindfulness training on working memory and affect was tested in military service members during a high-stress period prior to deployment (Jha, Stanley, Kiyonaga, Wong, & Gelfand, 2010). Military members who received training and reported high practice time showed improvement in working memory capacity from pre-test to post-test, whereas those who reported low practice time showed a decline in working memory capacity from pre-test to post-test. At post-test, greater practice time was correlated with lower negative affect, and improvement in working memory capacity mediated this relationship. Thus, mindfulness practice

may protect against the negative effects of stress on mood via improved working memory capacity; research is needed to evaluate these mechanisms in the context of clinical depression.

4.7 Study Strengths and Limitations

The present study is the most comprehensive investigation of executive control biases in clinical depression to date. As the only study to evaluate biases in inhibition, updating, and shifting among clinically depressed individuals using separate tasks designed to specifically measure each component of executive control, this study provides novel insight into the existence of executive control biases in depression, and particularly biases in updating and shifting, for which prior evidence was limited. A number of methodological features strengthen the conclusions that can be drawn from the study. First, the study employed clinical and control samples recruited from the community and diagnosed via administration of a structured clinical interview. This design feature enhances the generalizability and clinical relevance of the findings compared to research involving non-clinical student samples. Moreover, the inter-rater reliability of inclusion and exclusion diagnoses was assessed and found to be excellent. Second, the inclusion of a remitted depressed sample in addition to a currently depressed sample allowed for the testing of the state versus trait nature of depression-related executive control biases. This enhanced the contribution of the study findings to theories of the nature of executive control biases in and out of an active depressive episode. Third, the sample size of the study was determined based on a priori power analyses and was considerably larger than most other studies of executive control biases in clinical samples. As such, the results may be considered more reliable and likely to be replicated than results from underpowered studies.

Finally, the experimental paradigms used to assess executive control biases were selected and designed with careful consideration of the limitations of prior research. In particular, an

effort was made to ensure that the paradigms provided a comprehensive measure of the intended executive control component as defined in the literature (e.g., Miyake et al., 2000). For instance, the ANB was selected to measure working memory updating as it involved the continuous addition and removal of stimuli from working memory. In addition, the CET was designed to involve shifting between emotion-relevant and emotion-irrelevant task sets as most prior shifting bias research had been conducted using a task that did not require participants to shift between task sets within a block of trials. As the CET was adapted from a non-emotional shifting paradigm for the purpose of the current study, this is the first demonstration of the utility of the CET for assessing emotional biases in set shifting. In addition to the depression-related bias, the standard shift cost (i.e., greater response times for shift versus no-shift trials), as well as the expected increase in response times for the crosstalk versus no-crosstalk condition and for the emotion versus colour task, were demonstrated, thus providing preliminary evidence for the validity of the CET as a measure of set shifting. Future studies on emotional biases in set shifting may benefit from the existence of this novel paradigm. In all three tasks, stimulus features (e.g., gender of the face image) and stimulus-response mappings were counterbalanced to prevent methodological features from confounding the results.

One limitation of the study was that participants completed only affective versions of the inhibition, updating, and shifting tasks. It was recognized that if any of the participant groups demonstrated impaired performance on the tasks for specific types of emotional stimuli (e.g., negative stimuli), then it could be concluded that such participants exhibited valence-specific biases in the respective executive control functions. However, if any of the participant groups demonstrated impaired performance on the tasks for all stimuli regardless of valence, then it would be unclear whether the impaired performance was due to the inclusion of emotional

stimuli and reflected emotion-specific biases in executive control, or whether impaired performance was due to general deficits in executive control. It was found that currently depressed participants demonstrated a valence-specific bias in inhibition and an emotion-specific bias in set shifting, and thus this limitation does not apply to these results. However, this limitation is relevant to the results concerning the updating task as currently depressed participants had lower accuracy than control participants for all trial types regardless of the target facial affect (see also Chapter 4.2).

The aforementioned limitation was identified in the study design process. In an ideal study design, participants would have completed the executive control tasks with both affective and non-affective stimuli in order to differentiate between valence-specific biases, emotion-specific biases, and general deficits in executive control. However, due to constraints on participant and study resources, it was not feasible to have participants complete both affective and non-affective versions of the inhibition, updating, and shifting tasks. To mitigate this limitation, the WCST was included as a non-affective measure of executive functioning. Its inclusion in the study provided a measure of general executive functioning in the absence of emotional stimuli, which could be compared to the measures of executive control involving emotional stimuli to aid interpretation of the study findings. The lack of group differences in performance on the WCST is consistent with the notion that the observed impairment in working memory updating in the currently depressed participants was related to the emotional nature of the stimuli. The WCST was selected due to its wide use as a measure of general executive functioning in various populations, including clinically depressed populations (Channon, 1996; Fossati et al., 1999; 2001; Merriam et al., 1999; Moritz et al., 2002; Must et al., 2006). However, discrepancies between the results of the inhibition, updating, and shifting tasks and the WCST

may be due to differences between the tasks other than the affective versus non-affective nature of the stimuli, including the level of difficulty of the tasks and the cognitive functions involved in task performance. Indeed, researchers have argued that a limitation of the WCST is that performance on the task may involve several complex cognitive abilities (Miyake et al., 2000). In addition to considering the results from the WCST, the results for the updating task were interpreted in the context of existing literature on executive control functions in depression, which does not strongly support the existence of broad cognitive impairments in depression outside of severe depression with psychotic features or age-related cognitive decline (e.g., Grant et al., 2001; Harvey et al., 2004; 2005; Joormann et al., 2010; Purcell et al., 1997). Despite these attempts to mitigate the identified limitation in study design, the present results must be interpreted with this limitation in mind.

Although the inclusion of a remitted depressed sample was noted as a strength of the study, another limitation concerns the interpretation of the results for remitted depressed participants. A reliable finding across the executive control tasks in the current study was that remitted depressed individuals did not demonstrate biases similar to currently depressed individuals. Thus, executive control biases appear to improve when an individual recovers from an active episode of depression, and may not be observable in a euthymic state. However, cognitive models propose that latent cognitive vulnerability produces risk for depression in interaction with negative life events or stressors (Beck et al., 1979). Thus, executive control biases may be observed for remitted depressed individuals only when in a state of negative mood or stress. Executive control biases that remain dormant in remitted depression until activated by stress may represent one of the risk factors for depressive relapse. The present study did not include a mood induction and was thus unable to test this hypothesized diathesis-stress process.

The results of the study are limited by the fact that several of the observed effects were small and only marginally significant. Although hypotheses were evaluated at the conventional significance level of $p < .05$, the decision was made to probe and interpret with caution effects that were marginally significant at $p < .10$. A review of the literature on executive control biases suggests that this is not an uncommon practice in this area of research (e.g., Joormann, 2004; Zetsche et al., 2012). The sample size of the present study was determined based on effect sizes from prior studies for the contrast between currently depressed and control groups on the executive control bias of interest. Thus, while the study was adequately powered to detect medium to large differences between the groups on the executive functioning bias measures, the power of the more complex ANOVAs involving multiple factors (both within-subjects and between-subjects) was not known. Although some of the intermediary interactions were marginally significant, the final hypothesized contrasts between currently depressed and control participants were statistically significant at the conventional level. Moreover, these contrasts were conducted using the Tukey test to correct the family-wise error rate. As such, it was deemed defensible to explore and cautiously interpret marginal effects in the context of a prior hypotheses and prior empirical findings. Indeed, categorical rejection of an alternative hypothesis when an effect is consistent with that hypothesis and previous research but does not meet a conventional level of significance may lead to Type II errors and introduce confusion into the literature.

Finally, the participant sample was comprised primarily of women (84%). This in part reflects the fact that rates of depression in females are approximately twice that of males (e.g., Kessler et al. 2003), but also suggests some bias in the reach of and/or response to the

recruitment methods. Additional studies involving samples with a higher proportion of males are needed to determine whether the present results generalize to males.

4.8 Future Research Directions and Conclusion

The findings and limitations of the present study point to two primary directions for future research. First, there is a need for research on the mechanisms by which executive control biases may contribute to the onset and persistence of depression. It was noted in Chapter 4.6 that biases in executive control may contribute to depression through effects on mood, negative thinking, emotion regulation/rumination, and/or interpersonal functioning. Studies are required to evaluate the associations between biases and each of these hypothesized mechanisms. It will be important for future research to move beyond the establishment of cross-sectional associations to the testing of directional and causal hypotheses in appropriately designed studies. For instance, prospective studies are needed to evaluate whether executive control biases at baseline predict changes in the hypothesized mechanisms and depression symptoms at later time points. Such research has already begun (e.g., De Lissnyder et al., 2012c; Demeyer et al., 2012; Zetsche & Joormann, 2011), but several mechanisms remain to be empirically examined. Researchers may also consider the interaction of executive control biases and stress in predicting changes in hypothesized cognitive, emotional, and social mechanisms and depressive symptoms, particularly in samples not currently in an active depressive state (De Lissnyder et al., 2012c; Quinn & Joormann, 2015). To test causal hypotheses about the role of executive control biases in depression, research is first needed to ascertain whether such biases are modifiable. If it is possible to experimentally induce and ameliorate biases, then the effect of their manipulation on hypothesized mediators and depression symptoms can be evaluated.

Second, future research should test the diathesis-stress model of executive control biases in remitted depression. Such studies could examine biases in each of the components of executive control in remitted depressed individuals in and out of a state of negative mood or stress. If remitted depressed individuals exhibit biases similar to currently depressed individuals following a mood induction, then it may be inferred that biases remain as a latent vulnerability following remission and may be activated under stress. Prospective studies should also be carried out to investigate whether baseline executive control biases interact with stress to predict future increases in depression symptoms or relapse to depression among remitted depressed individuals. It may be of value to examine the efficacy of CBM procedures targeting executive control biases for reducing risk of relapse, either alone or in combination with other psychological interventions such as CBT or mindfulness based interventions. Again, it would be of interest to evaluate affect, thinking, emotion regulation, rumination, and social factors as mediators of relapse risk in prospective studies and as mediators of treatment outcome in CBM research.

Although these two directions have been identified as particularly important for future research, additional ideas have been proposed throughout this chapter. Finally, recent data has highlighted the problem of replication failures in psychological research (Open Science Collaboration, 2015). Replication of existing findings, including those of the present study, should be a priority for future research to determine the robustness of the effects.

To conclude, cognitive models posit that biases in executive control may contribute to the development and persistence of depression (Beck et al., 1979; Gotlib & Joormann, 2010). In support of this notion, currently depressed individuals in the current study exhibited emotional biases in each of the main components of executive control. Compared to control participants, currently depressed participants were impaired in their ability to inhibit the processing of

negative task-irrelevant material, update working memory with emotional material, and shift away from an emotion-relevant task set. Remitted depressed individuals did not demonstrate similar biases, indicating that executive control biases are not exhibited by depression-vulnerable individuals in a euthymic mood state. Participant groups did not differ significantly in performance on an executive functioning task involving non-affective stimuli. This pattern of results is consistent with the proposal that the effect of depression on executive control is most pronounced when processing mood-congruent information. This study provides novel evidence that depression is associated with emotional biases in shifting between task sets, and is the first to demonstrate depression-related biases in inhibition, working memory updating, and set shifting in a single design.

The results of this study did not provide reliable support for the hypothesized relationships between executive control biases and rumination. Possible explanations for these results were considered. The relationship between biases and rumination may be more complex than a direct relationship, and involve moderators and mediators not considered in the present study. Future research should focus on testing directional and causal hypotheses about the role of executive control biases in depression and their mechanisms of action, as well as diathesis-stress models of executive control biases in remitted depression. Pursuit of these research directions will lead to a better understanding of how executive control biases are related to depression vulnerability, which in turn may aid efforts to treat and prevent depression.

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APPENDIX A: RECRUITMENT POSTER

UNIVERSITY OF CALGARY

University of Calgary
Depression Research

University of Calgary researchers in the Depression Research Laboratory are currently seeking participants (18 – 65 years of age) for a study on how depression affects how people process emotional information.

We are seeking different types of participants. You may be eligible if:

- **You are currently depressed**
  OR
- **You have been depressed in the past, but are not currently depressed**

People who are eligible for the study (as determined by a 15-minute confidential phone interview) will be asked to complete two study sessions that involve:

- An in-person interview about symptoms of depression, anxiety, drug/alcohol use, and unusual experiences (interviews will be audio-recorded)
- Self-report questionnaires about demographic information, symptoms, mood, and emotion regulation
- Computer tasks that require you to view images and make decisions about them

Participants will receive a $25 gift card for each study session in which they participate (up to $50 value total for two sessions). Each study session will take approximately 1 – 1.5 hours.

Confidential inquiries can be made through the Depression Research Laboratory
E-mail: depression.research.lab@gmail.com

This research project has been reviewed by, and received ethics clearance through, the University of Calgary Conjoint Faculties Research Ethics Board

APPENDIX B: DEMOGRAPHIC QUESTIONNAIRE

Demographic Information

Please answer the following questions about yourself.

1. Date of birth (mm/dd/yyyy): _____

2. Age: _____

3. Sex:
   - Male
   - Female
   - Other

4. What is your marital status?
   - Married or living with someone as if married
   - Widowed
   - Divorced or annulled
   - Separated
   - Never been married

5. What is the highest level of education that you have completed?
   - Less than high school
   - Completed high school
   - Some technical/ community college
   - Completed technical/ community college
   - Some university
   - Bachelor's degree
   - Master's degree or Professional degree

6. Were you born in Canada?
   - Yes
   - No

   If you were born outside of Canada, in what country were you born? _____

7. How would you describe your cultural/ethnic background?
   - Asian (e.g., Chinese, Japanese, Korean)
   - South Asian (e.g., East Indian, Pakistani)
   - Southeast Asian (e.g., Cambodian, Indonesian, Vietnamese, Loatian)
   - West Indian (e.g., Afghan, Iranian)
   - Arab
   - Black
   - Filipino
   - First Nations

- Latin American
- White/European
- Other _____

8. What language(s) did you first learn at home or in childhood and still understand and use?
   - English
   - French
   - Other _____

9. Do you have normal or corrected-to-normal vision?
   - Yes
   - No

10. In your estimation, how many episodes of depression have you experienced in your life?
    - None
    - One
    - 2 – 5
    - 5 or more

11. If you have experienced depression, how long ago was your last depressive episode?
    - N/A
    - Currently experiencing a depressive episode
    - Less than a month ago
    - Less than six months ago
    - Less than a year ago
    - More than a year ago

12. Have you ever been diagnosed with depression by a mental health professional?
    - Yes
    - No

13. Are you presently receiving therapy or counseling for depression?
    - Yes
    - No

14. Have you had therapy or counseling for depression in the past?
    - Yes
    - No

15. If you received counseling from a therapist, for how long did you go?
    - N/A
    - 1 – 6 months
    - 7 – 12 months
    - 13 – 18 months

- 19 – 24 months
- More than 24 months

16. Are you taking medication for depression right now?
    - Yes
    - No

17. Have you taken medication for depression in the past?
    - Yes
    - No

18. If you have used antidepressant medications, for how long did you use them?
    - N/A
    - 1 – 6 months
    - 7 – 12 months
    - 13 – 18 months
    - 19 – 24 months
    - More than 24 months

19. Have you ever been diagnosed with anxiety by a mental health professional?
    - Yes
    - No

20. Are you presently undergoing therapy or counseling for anxiety?
    - Yes
    - No

21. Have you had therapy or counseling for anxiety in the past?
    - Yes
    - No

22. Are you taking medication for anxiety right now?
    - Yes
    - No

23. Have you taken medication for anxiety in the past?
    - Yes
    - No

APPENDIX C: TREATMENT AND MEDICATION HISTORY

Treatment and Medication History

Are you currently receiving counselling or therapy? Yes No

If Yes:

Reason(s) for therapy:

_____
_____
_____

Treatment description(s):

_____
_____
_____

Duration of treatment:

_____

Date of treatment:

_____

Have you ever received counselling or therapy in the past? Yes No

If Yes:

Reason(s) for therapy:

_____
_____
_____

Treatment description(s):

_____
_____
_____

Duration of treatment:

_____

Date of treatment:

_____

Are you currently on any prescribed medication?

If YES: Name of medication(s) (if known):
Name: _____ Condition: _____ Dosage: _____
Name: _____ Condition: _____ Dosage: _____
Name: _____ Condition: _____ Dosage: _____
Name: _____ Condition: _____ Dosage: _____
Name: _____ Condition: _____ Dosage: _____

Has the dosage of your medication(s) been changed over the last six weeks? (If yes, detail changes):
_____
_____
_____
_____
_____

Other than the medications listed, have you ever taken medication for a mental health problem like depression or anxiety in the past? Yes No

If yes: Name of medication(s) (if known):

Name: _____ Condition: _____
Name: _____Condition: _____
Name: _____Condition: _____
Name: _____Condition: _____
Name: _____Condition: _____
Name: _____Condition: _____

APPENDIX D: EMOTIONAL FLANKER TASK

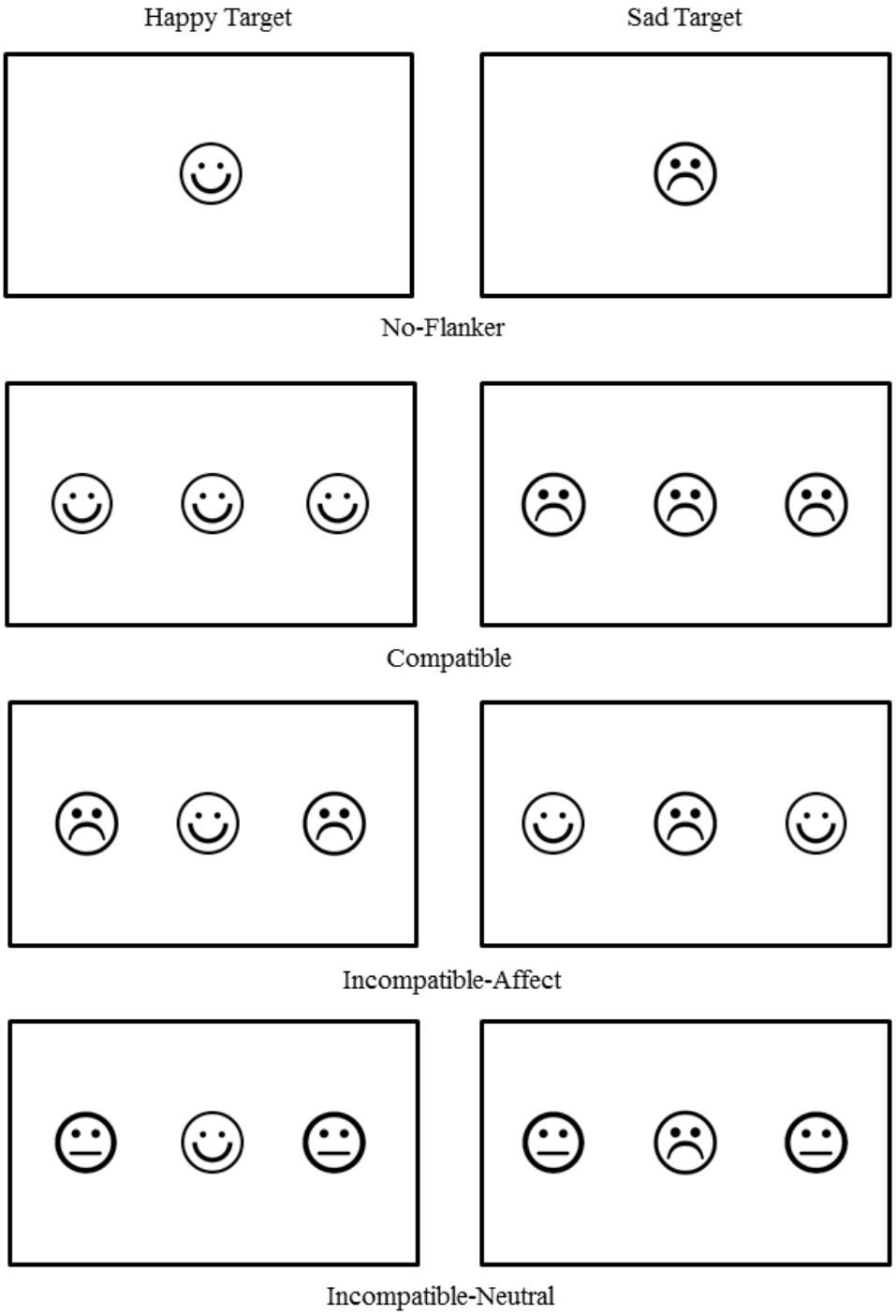

Figure 5. Examples of stimulus displays for the EFT trial types. Face images replaced with schematic stimuli due to copyright restrictions. Image adapted from Fenske & Eastwood (2003).

APPENDIX E: AFFECTIVE N-BACK TASK

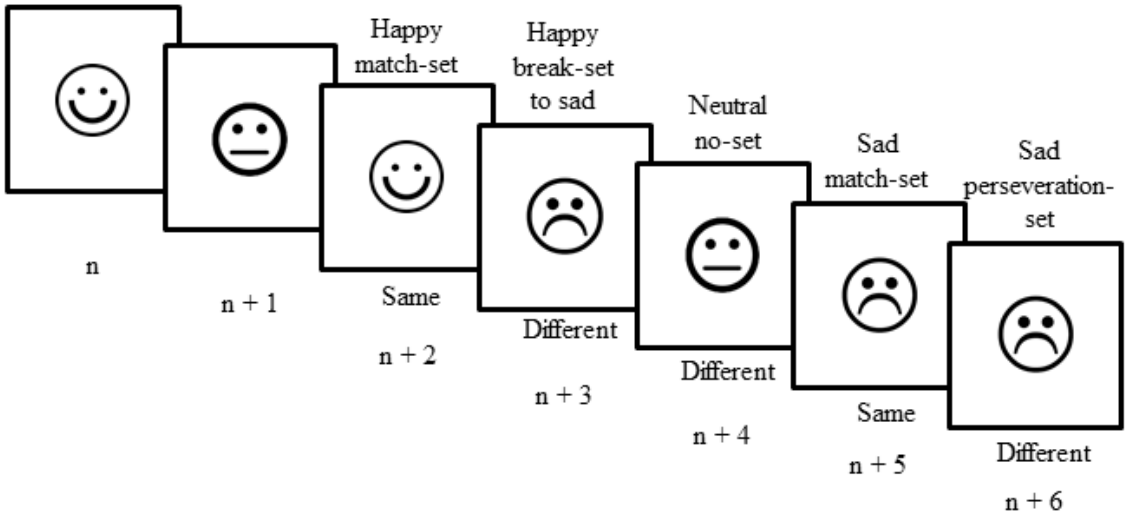

Figure 6. Examples of stimulus displays for ANB trial types (2-back trials). Face images replaced with schematic stimuli due to copyright restrictions. Image adapted from Levens & Gotlib (2010).

APPENDIX F: 0-BACK TASK

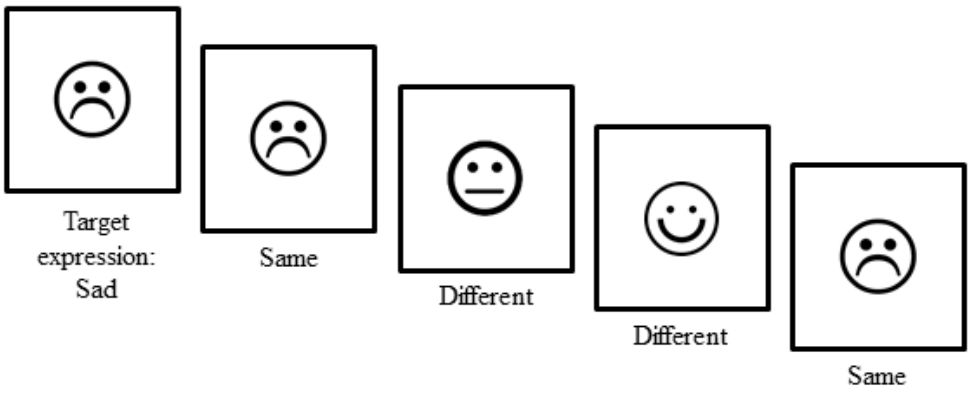

Figure 7. Examples of stimulus displays for the 0-back task. Face images replaced with schematic stimuli due to copyright restrictions. Image adapted from Levens & Gotlib (2010).

APPENDIX G: COLOUR-EMOTION TASK

No-Crosstalk Condition

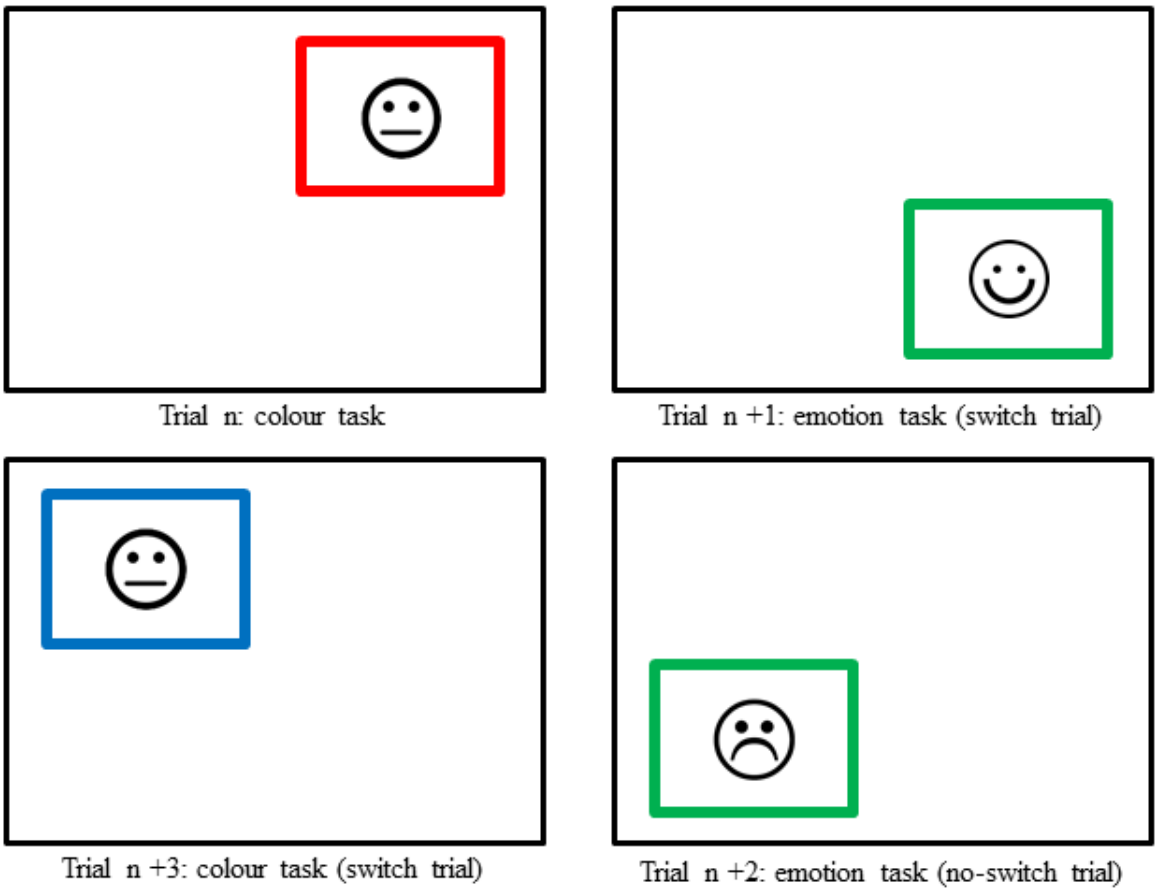

Figure 8. Examples of stimulus displays for CET trial types in the no-crosstalk condition. Stimulus location-task mapping in these examples is the colour task when the stimulus is displayed in the top-half of the screen and the emotion task when the stimulus is displayed in the bottom-half of the screen. Face images replaced with schematic stimuli due to copyright restrictions.

Crosstalk Condition

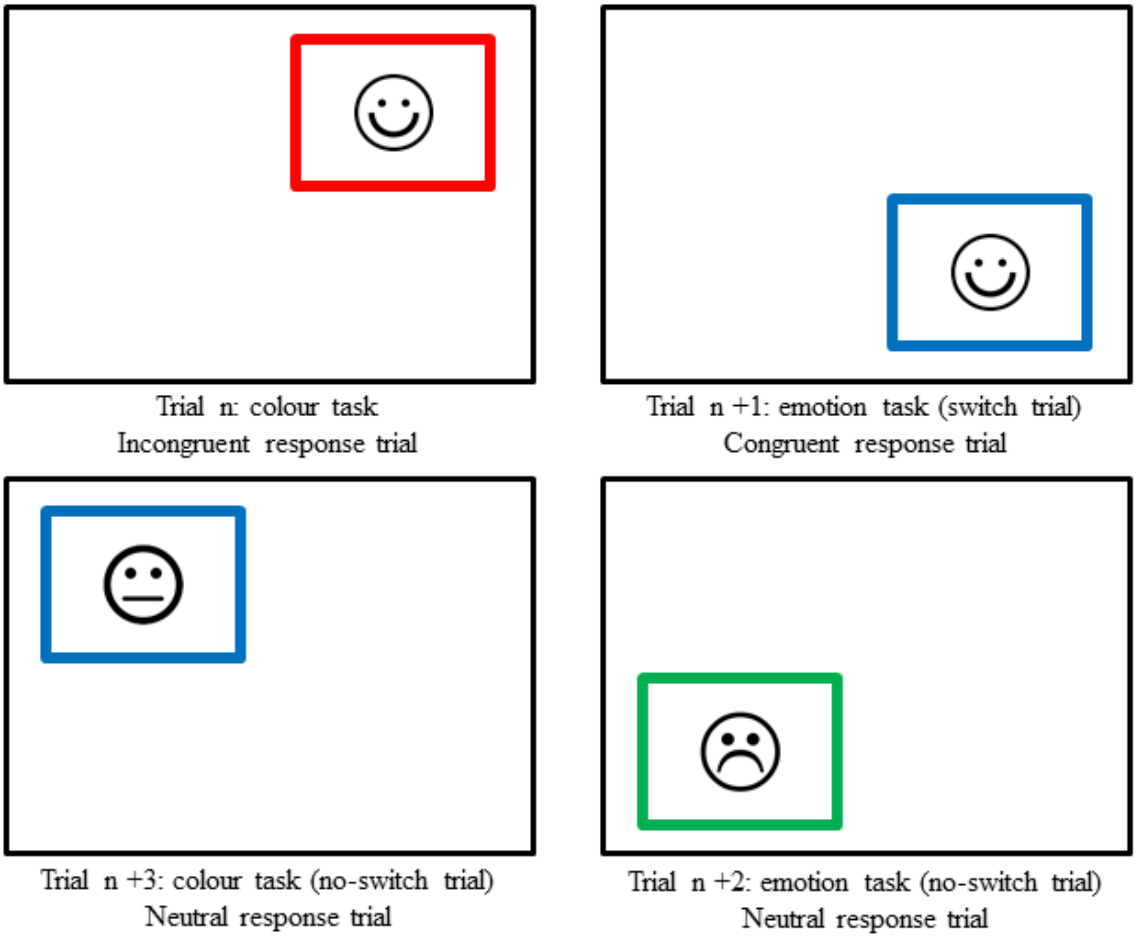

Figure 9. Examples of stimulus displays for CET trial types in the crosstalk condition. Stimulus location-task mapping in these examples is the colour task when the stimulus is displayed in the top-half of the screen and the emotion task when the stimulus is displayed in the bottom-half of the screen. Face images replaced with schematic stimuli due to copyright restrictions.

APPENDIX H: INFORMED CONSENT FORMS

UNIVERSITY OF CALGARY

Name of Researcher, Faculty, Department, Telephone & Email:

Leanne Quigley, Department of Psychology, Phone: 403-220-3697, E-mail: lquigley@ucalgary.ca

Supervisor:

Dr. Keith Dobson, Department of Psychology

Title of Project:

Examining the Relationship between Depression and Emotion-Related Biases in Executive Functioning: Part One

Sponsor:

URGC Seed Grant
Vanier Canada Graduate Scholarship, Social Sciences and Humanities Research Council of Canada

This consent form, a copy of which has been given to you, is only part of the process of informed consent. If you want more details about something mentioned here, or information not included here, you should feel free to ask. Please take the time to read this carefully and to understand any accompanying information.

The University of Calgary Conjoint Faculties Research Ethics Board has approved this research study.

Purpose of the Study

We are studying how depression affects how people process emotional information. We are interested in whether individuals who are currently depressed or who have experienced depression in the past process emotional information differently from individuals who have never been depressed. If individuals with current or past depression process emotional information differently than individuals who have never been depressed, this may help us understand the cognitive processes that contribute to the development and recurrence of depression.

What Will I Be Asked To Do?

This study takes place over two study sessions. In the first study session, you will be asked to complete a private and confidential interview, which will take approximately 45 minutes. In the interview, you will be asked whether you are currently experiencing, or have experienced in the past, a number of different symptoms and problems, such as depression, anxiety, and substance use.

Please note that there are some exceptions to confidentiality of which you must be aware and in which I would be legally and/or professionally obligated to break confidentiality. These exceptions are: 1) situations where I believe a child or vulnerable individual may be at risk of abuse and/or neglect; 2)

situations where I believe a person may be at risk of harming his/herself or another individual; and 3) situations where my files are subpoenaed by a court of law.

We will audio-record the interviews. The purpose of audio-recording the interviews is to ensure that the quality of our interviews meets scientific standards. A random subsample of the interviews will be reviewed by Dr. Keith Dobson, who is the faculty supervisor of this research and an experienced clinician. The audio-recordings will only be used for the purpose of this quality assurance review, and will not be used in any presentation or publication of the study results.

After the interview, you will complete a computer task that requires you to categorize cards. You will receive instructions prior to completing the task. The computer task will take approximately 15 – 20 minutes to complete. You will also complete a number of questionnaires that assess demographic characteristics (e.g., age, marital status, ethnicity), symptoms of depression and anxiety, and emotion regulation strategies. It will take you approximately 15 minutes to complete these questionnaires.

Your participation today will require approximately 90 minutes of your time and you will receive a gift card in the amount of $25 in appreciation of your participation. At the end of the study session, the researcher will schedule you for a second study session within the next two weeks at a time that is convenient for you, provided you are eligible for the second study session. Eligibility for the second study session is based on your responses to the interview. The second study session will consist of a number of computer tasks where you will be required to view images of faces and make decisions about them. The second study session will require approximately 60 minutes of your time, and you will receive another $25 gift card in appreciation of your participation in the second session.

Your participation in this study is completely voluntary. You may refuse to participate in any part of the study, and may decline to answer any and all questions. You may withdraw from the study at any time without penalty. Should you decide to withdraw from the study, your data will be permanently deleted. However, there is one exception to this. If at any point during the study you indicate that you intend to harm yourself or another person, it will be necessary to retain your data for a period of five years following the completion of the study, even if you choose to withdraw from the study.

What Type of Personal Information Will Be Collected?

Should you agree to participate, you will be asked to provide your age, gender, marital status, ethnicity, education level, employment status, and treatment status and history (e.g., medication or therapy for psychological problems). You are free to choose which questions to answer. You will also be asked during the interview about whether you are currently experiencing, or have experienced in the past, a number of different symptoms and problems, such as depression, anxiety, and substance use. Again, you may refuse to answer any questions. Only the researcher and the supervisor associated with this study will have access to the audio-recorded interviews for the quality assurance purpose described above. The audio-recordings will never be used in any publication or presentation.

I grant permission for my interview to be audio-recorded: Yes _____ No_____

Are there Risks or Benefits if I Participate?

You will be asked about different problems and symptoms (e.g., depression, anxiety, substance use) that you may be currently experiencing or have experienced in the past. You may refuse to answer any question you wish. Some people may experience distress when asked to think or talk about problems they have or have had in the past. If this is the case for you, the researcher will encourage you to access support in the community. You will be provided with a list of available resources for the treatment of psychological disorders in the community.

As a reminder, all of the information you provide will be kept confidential. However, there are legal exceptions to this confidentiality, including any information you may provide regarding 1) harm to yourself or others; or 2) the current and ongoing abuse of a child.

As a result of your participation in this study, you will learn of available resources for the treatment of psychological disorders within the community. You will also have the opportunity to be provided with the study results upon the completion of the study. You may also indirectly benefit from the potential that this study holds in efforts to enhance the understanding, treatment, and prevention of depression. As mentioned, you will receive a gift card in the amount of $25 for each study session in which you participate (for a maximum total of $50 value in gift cards).

What Happens to the Information I Provide?

All information you provide will be kept confidential. Your name will not be associated with any of the data you provide. Upon entry to the study, each participant will be assigned a number code, and all of your data will be identified only by this code to ensure confidentiality. A master coding sheet containing participant ID codes and contact information (i.e., name, telephone number, e-mail address) will be kept as a separate file, and will be encrypted and stored on a password protected computer. Only the researchers and supervisor of this study will have access to the master coding sheet. All electronic data, including the audio-recorded interviews, will be stored on a password protected computer. The paper data (i.e., consent forms) will be securely stored in a locked filing cabinet in a secure room in the Depression Research Lab (Administration 059) to which only researchers associated with this study have access. The audio-recorded interviews will be permanently deleted following data analysis. The master coding sheet (containing participant contact information) will be permanently deleted and the consent forms will be securely shredded after a period of five years following the completion of the study. The remaining anonymous study data will be archived indefinitely and may be used for future research purposes. Only group information will be summarized for any presentation or publication of results, and therefore, your individual data will not be identifiable in any presentation or publication of results.

Signatures

Your signature on this form indicates that 1) you understand to your satisfaction the information provided to you about your participation in this research project, and 2) you agree to participate in the research project.

In no way does this waive your legal rights nor release the investigators, sponsors, or involved institutions from their legal and professional responsibilities. You are free to withdraw from this research project at any time. You should feel free to ask for clarification or new information throughout your participation.

Participant's Name: (please print) _____

Participant's Signature: _____ Date: _____

Researcher's Name: (please print) _____

Researcher's Signature: _____ Date: _____

Questions/Concerns

If you have any further questions or want clarification regarding this research and/or your participation, please contact:

Leanne Quigley
Department of Psychology, Faculty of Arts
Phone: 403-220-3697, E-mail: lquigley@ucalgary.ca

Dr. Keith Dobson
Department of Psychology, Faculty of Arts
Phone: 403-220-5096, E-mail: ksdobson@ucalgary.ca

If you have any concerns about the way you've been treated as a participant, please contact an Ethics Resource Officer, Research Services Office, University of Calgary at (403) 210-9863; email cfreb@ucalgary.ca.

A copy of this consent form has been given to you to keep for your records and reference. The investigator has kept a copy of the consent form.

UNIVERSITY OF CALGARY

Name of Researcher, Faculty, Department, Telephone & Email:

Leanne Quigley, Department of Psychology, Phone: 403-220-3697, E-mail: lquigley@ucalgary.ca

Supervisor:

Dr. Keith Dobson, Department of Psychology

Title of Project:

Examining the Relationship between Depression and Emotion-Related Biases in Executive Functioning: Part Two

Sponsor:

URGC Seed Grant
Vanier Canada Graduate Scholarship, Social Sciences and Humanities Research Council of Canada

This consent form, a copy of which has been given to you, is only part of the process of informed consent. If you want more details about something mentioned here, or information not included here, you should feel free to ask. Please take the time to read this carefully and to understand any accompanying information.

The University of Calgary Conjoint Faculties Research Ethics Board has approved this research study.

Purpose of the Study

We are studying how depression affects how people process emotional information. We are interested in whether individuals who are currently depressed or who have experienced depression in the past process emotional information differently from individuals who have never been depressed. If individuals with current or past depression process emotional information differently than individuals who have never been depressed, this may help us understand the cognitive processes that contribute to the development and recurrence of depression.

What Will I Be Asked To Do?

This study takes place over two study sessions. You have already completed the first study session. In the second session, you will be asked to complete three computer tasks. The computer tasks will require you to view images of facial expressions and make decisions about them. For example, in one of the tasks you will be asked to alternate between indicating whether a face has a blue or red frame around it and whether a facial expression is happy or sad. In another task, you will be asked to indicate whether the currently presented facial expression matches or does not match the facial expression presented two trials ago. The trials in the computer tasks will be divided into blocks and you will be given the opportunity to rest between the blocks of trials. You will also be given the opportunity to rest between

the separate computer tasks. You will be given more information about the computer tasks, both verbally and in written form, prior to completing the tasks.

Your participation today will require approximately 60 - 90 minutes of your time and you will receive a gift card in the amount of $25 in appreciation of your participation. Your participation in this study is completely voluntary. You may refuse to participate in any part of the study. You may also withdraw from the study at any time without penalty. Should you decide to withdraw from the study, your data will be permanently deleted. However, there is one exception to this. If at any point during the study you indicate that you intend to harm yourself or another person, it will be necessary to retain your data for a period of five years following the completion of the study, even if you choose to withdraw from the study.

What Type of Personal Information Will Be Collected?

You already consented to providing personal information (e.g., age, gender, marital status, ethnicity, education level, employment status, treatment status and history, current and past mental health problems) in the first part of this study. You received an information sheet/consent form at the beginning of your first study session that detailed how the confidentiality of your information will be protected and who will have access to the data. No additional personal information will be collected in this second study session.

Are there Risks or Benefits if I Participate?

The computer tasks require sustained attention and concentration over numerous repeated trials. Some people may experience some mental fatigue either during or after the completion of the computer tasks. You will have the opportunity to rest between blocks of trials and between the computer tasks. You may also refuse to complete any part of the computer tasks, and may withdraw from the study at any time without penalty.

As a result of your participation in this study, you will have the opportunity to learn about cognitive psychology computer tasks that are used to study information processing in humans. You will also have the opportunity to be provided with the study results upon the completion of the study. You may also indirectly benefit from the potential that this study holds in information efforts to enhance the understanding, treatment, and prevention of depression. As mentioned, you will receive a gift card in the amount of $25 for each study session in which you participate (for a maximum total of $50 value in gift cards).

What Happens to the Information I Provide?

All information you provide will be kept confidential. Your name will not be associated with any of the data you provide. Upon entry to the study, each participant will be assigned a number code, and all of your data will be identified only by this code to ensure confidentiality. A master coding sheet containing participant ID codes and contact information (i.e., name, telephone number, e-mail address) will be kept as a separate file, and will be encrypted and stored on a password protected computer. Only the researchers and supervisor of this study will have access to the master coding sheet. All electronic data will be stored on a password protected computer. The paper data (i.e., consent forms) will be securely stored in a locked filing cabinet in a secure room in the Depression Research Lab (Administration 059)

to which only researchers associated with this study have access. The audio-recorded interviews will be permanently deleted following data analysis. The master coding sheet (containing participant contact information) will be permanently deleted and the consent forms will be securely shredded after a period of five years following the completion of the study. The remaining anonymous study data will be archived indefinitely and may be used for future research purposes. Only group information will be summarized for any presentation or publication of results, and therefore, your individual data will not be identifiable in any presentation or publication of results.

Signatures

Your signature on this form indicates that 1) you understand to your satisfaction the information provided to you about your participation in this research project, and 2) you agree to participate in the research project.

In no way does this waive your legal rights nor release the investigators, sponsors, or involved institutions from their legal and professional responsibilities. You are free to withdraw from this research project at any time. You should feel free to ask for clarification or new information throughout your participation.

Participant's Name: (please print) _____

Participant's Signature: _____ Date: _____

Researcher's Name: (please print) _____

Researcher's Signature: _____ Date: _____

Questions/Concerns

If you have any further questions or want clarification regarding this research and/or your participation, please contact:

Leanne Quigley
Department of Psychology, Faculty of Arts
Phone: 403-220-3697, E-mail: lquigley@ucalgary.ca

Dr. Keith Dobson
Department of Psychology, Faculty of Arts
Phone: 403-220-5096, E-mail: ksdobson@ucalgary.ca

If you have any concerns about the way you've been treated as a participant, please contact an Ethics Resource Officer, Research Services Office, University of Calgary at (403) 210-9863; email cfreb@ucalgary.ca.

A copy of this consent form has been given to you to keep for your records and reference. The investigator has kept a copy of the consent form.

APPENDIX I: DEBRIEFING FORMS

Date: March 26, 2014

Debriefing Letter

Title of Project: Examining the Relationship between Depression and Emotion-Related Biases in Executive Functioning: Part One

Student Investigator: Leanne Quigley
Department of Psychology, Faculty of Arts
Phone: 403-220-3697, E-mail: lquigley@ucalgary.ca

Faculty Supervisor: Dr. Keith Dobson
Department of Psychology, Faculty of Arts
Phone: 403-220-5096, E-mail: ksdobson@ucalgary.ca

Thank you for participating in this study in the Depression Research Lab. As a reminder, the purpose of this study is to examine how depression affects how people process emotional information. We are interested in whether individuals who are currently depressed or who have experienced depression in the past process emotional information differently from individuals who have never been depressed. The findings of this study may help us understand the cognitive processes that contribute to the development and recurrence of depression.

Please remember that any data pertaining to you as an individual participant will be kept confidential. Once all the data are collected and analyzed for this project, I plan on sharing this information with the research community through seminars, conferences, presentations, and journal articles. All electronic data will be stored on a password protected computer. The paper data will be securely stored in a locked filing cabinet in a secure room in the Depression Research Lab (Administration 059) to which only researchers associated with this study have access. The master coding sheet (containing participant contact information) and audio-recorded interviews will be permanently deleted and the consent forms will be securely shredded after a period of five years following the completion of the study. The remaining anonymous data will be retained indefinitely, and may be used for future research purposes. If you are interested in receiving more information regarding the results of this study, or if you have any questions or concerns, please contact me at either the phone number or email address listed at the bottom of the page.

If participation in this study has raised any personal concerns for you that you would like to discuss, you may call the Depression Research Lab at 403-220-3697 for further resources. Information about mental health resources in the community can also be found on the back of this form.

The University of Calgary Conjoint Faculties Research Ethics Board has approved this study.

Leanne Quigley
Department of Psychology, Faculty of Arts
Phone: 403-220-3697, E-mail: lquigley@ucalgary.ca

Date: March 26, 2014

Mental Health Resources in the Community:

- If you think you might be depressed, see your doctor immediately

- The Psychologists' Association of Alberta provides a free service to help you located qualified psychologists in your community.
  Online referral service: www.psychologistsassocation.ab.ca
  Telephone referral service: 403-246-8255

- Other community mental health resources:

  U of C Health Clinic: 210-9355 (psychiatrists on staff)
  Calgary Distress Centre: 266-1605 (24 hours, speak to a trained counsellor)
  Alberta Mental Health Line: 1-877-303-2642
  Calgary Mental Health Crisis: 266-1605 (mobile response)
  South Calgary Health Centre: 943-9300 (walk-in therapy and urgent care)
  Sheldon Chumir Health Centre: 955-6200 (urgent mental health services)
  Calgary Communities against Sexual Abuse: 237-6905 (24 hours)

  Foothills Hospital: 944-1315 (psychiatric emergency)
  Peter Lougheed Hospital: 943-4904 (psychiatric emergency)
  Rockyview Hospital: 541-3537 (psychiatric emergency)

- Learn more about depression, anxiety and stress by visiting online at:
  www.amhb.ab.ca
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  www.informalberta.ca
  www.cmha.ca
  www.suicideinfo.ca
  www.beyondblue.org.au
  www.moodgym.anu.edu.ca
  www.feelingblue.com

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Debriefing Letter

Title of Project: Examining the Relationship between Depression and Emotion-Related Biases in Executive Functioning: Part Two

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