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Training Affective Flexibility: Effects of an Affective-Control Training Task on Emotion Regulation

Ashley M. Malooly
University of Miami, amalooly@gmail.com

UNIVERSITY OF MIAMI

TRAINING AFFECTIVE FLEXIBILITY: EFFECTS OF AN AFFECTIVE-CONTROL TRAINING TASK ON EMOTION REGULATION

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Ashley Marie Malooly

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Ashley Marie Malooly

Approved:

________________	________________
Charles S. Carver, Ph.D. Professor of Psychology	Matthias Siemer, Ph.D. Psychology Research Scientist/Scholar Yale University
________________	________________
Kiara R. Timpano, Ph.D. Associate Professor of Psychology	Heather Henderson, Ph.D. Associate Professor of Psychology University of Waterloo
________________	________________
Bonnie Levin, Ph.D. Professor of Neurology	Guillermo Prado, Ph.D. Dean of the Graduate School

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Studies have shown that cognitive reappraisal is an adaptive emotion regulation strategy. However, individuals differ in how effectively they use reappraisal to regulate negative emotions such as sadness. Cognitive processes, such as those involved in task-switching, inhibition, and attention, may influence how well an individual can utilize cognitive reappraisal. This study sought to investigate whether a cognitive process associated with reappraisal, affective flexibility (AF), could be trained and could improve an individual's ability to effectively down-regulate sadness. Also examined were potential effects of AF training on symptoms of depression and anxiety, and transfer effects to emotional working memory. Healthy participants with no more than minimal depression were randomly assigned to either an active AF training or control training condition. Results indicated that the training versus control manipulation was ineffective. Participants across both groups exhibited reduced anxiety and improved emotional working memory. No effects of training on down-regulation of sadness using cognitive reappraisal were observed. Results may indicate that AF training has little effect on maladaptive emotion regulation in healthy controls. However, further examination of AF training within the context of anxiety disorders may be warranted.

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Chapter 1

Introduction

People regulate their own emotions on a daily basis – to help them reach their goals, interact more effectively with others, or just feel good (Koole, 2009). There are many conscious strategies that people can use to change their emotions, and no other strategy has received as much empirical attention as cognitive reappraisal (Gross, 1998). As its name suggests, cognitive reappraisal involves a change in perspective regarding an emotion-triggering situation, with the goal of changing the way one feels about the situation (Gross, 1999).

Many studies have shown that cognitive reappraisal is an effective mechanism for reducing both subjective and objective indicators of sadness (e.g. Gross, 1998). Compared with participants instructed to suppress their feelings, participants instructed to reappraise have been found to report less negative emotion and to exhibit lower physiological activity, comparable to participants assigned to a non-regulation condition (Gross, 1998).

Cognitive reappraisal has also been shown to facilitate social interactions (Gross & John, 2003). Participants instructed to suppress their emotions tend to be rated by others as less socially skilled than individuals instructed to reappraise. On the other hand, individuals who reappraise report experiencing more positive emotions, sharing their emotions more often with others, and having closer social relationships than individuals who suppress (Gross & John, 2003). Furthermore, individuals who use reappraisal are better able to attend to and remember the details of emotion-triggering events compared with those instructed to suppress their emotions or use distraction, as individuals who suppress or distract themselves must attend to bodily sensations or thoughts outside of present awareness, respectively, in order for these strategies to work (Richards & Gross, 2006).

Finally, cognitive reappraisal is associated with reduced risk of developing depression and anxiety disorders, particularly in the face of stress (Gross & John, 2003). One study has even indicated that cognitive reappraisal may moderate the relationship between stress and depression, such that among individuals with high stress, good reappraisal ability may serve as a buffer against developing depression (Troy, Wilhelm, Shallcross, & Mauss, 2010).

The above findings beg the question – why doesn't everyone use cognitive reappraisal to regulate their emotions, given the unequivocal findings that reappraisal is beneficial in so many domains of functioning? The answer may lie in individual differences that affect an individual's ability to select and effectively implement this emotion regulation strategy. In this dissertation, individual differences in the cognitive processing of affective material were examined as one such factor which may impact how well an individual can cognitively reappraise negative emotions. Next, a study is described which was designed to test the idea that these individual differences may, in fact, be amenable to change through training, in particular, training aimed at increasing participant's affective flexibility. It was predicted that this training may have positive effects on an individual's ability to reappraise, on symptoms of depression and anxiety, and may generalize to another measure of emotional control, emotional working memory.

Individual Differences in Emotion Regulation

Human emotions have evolutionary value – anger evolved as a signal that something is wrong and needs to be fixed, happiness to signal an opportunity for play and exploration, and sadness to alert others of distress and to rally support (Tooby & Cosmides, 2008). However, sometimes our emotions may be inappropriate and we may desire to change components of our emotional response. For example, if one thinks about the funny show he was watching last night while he was in a business meeting, he may experience amusement and may signal that amusement with laughter. However, laughing at this time may be viewed as out of place, unprofessional, and may even get the employee in trouble. The employee has several options for avoiding this faux pas. He may try to change his response by *suppressing* his laughter, holding his face still and physically trying to contain his laughter. She may *modify the situation* by excusing herself to the restroom temporarily. He may attempt to *deploy his attention* elsewhere to avoid laughing, such as by looking at the stern face of his boss. Or she may try to *reappraise* her thoughts by remembering that it was only a television show and reminding herself that now is not an appropriate time to think about this, she can laugh about it later after the meeting.

The above example demonstrates the complexity of emotion regulation and the many stages of emotion generation at which emotion regulation may intervene in order to change one's emotions (Gross & Thompson, 2007). In order for an emotion to occur, a person must be in an emotion-triggering situation, pay attention to the emotion-triggering event, and must appraise the event as meaningful and relevant. Once these 3 criteria have been met, an emotional response (e.g. laughter) may be generated.

Emotion regulation may act on any of these 4 steps in the emotion-generation process. A person may select or modify situations such that they avoid particular emotions, as in the case with social anxiety, wherein social situation are avoided so as to avoid anxiety. The individual may also direct his or her attention elsewhere in order to change the feelings, such as by thinking “happy thoughts” while watching a sad movie (*distraction*) or by paying attention to other aspects of the film, such as the lighting and camera angles (*attentional deployment*). The person may try to control the physical aspects of the emotional reaction, for example, by going to the gym to “blow off steam.” Alternatively, people may change the way that they think about the situation in order to change the way that they feel about it, or reappraise.

Reappraisal is believed to be a particularly effective emotion regulation strategy because it intervenes early in the emotion generation process. For example, it may entail changing distorted thoughts to prevent, stop, or reduce rising intensity of negative emotions (Gross & Thompson, 2007). Because of its effects early in the emotion generation process, it has also been found to be among the most effective strategies at reducing the subjective and behavioral aspects of emotional responding (Gross, 1998; Hajcak, Moser, & Simons, 2006; Ray, Wilhelm, & Gross, 2008; Urry, 2010).

Furthermore, whereas most emotion regulation strategies work by either taking attention away from the situation at hand or by focusing attention on something else (e.g. one’s bodily reactions), reappraisal works through a shift in mental set which causes the *same* emotion-triggering information to be processed in a *different* way. Individuals who use reappraisal, therefore, continue to process the emotion-triggering event but do so in a way that is less emotionally evocative. This helps to explain findings that reappraisal of an event is less likely to contribute to poor recall of the event when compared with distraction or suppression, for example (Richards & Gross, 2006). It may also contribute to findings suggesting that individuals who are instructed to reappraise are rated as more socially skilled than individuals who suppress (Butler, et al., 2003).

Group and individual variations appear to exist in the ability to use cognitive reappraisal effectively. Gender differences are particularly important to examine in the context of emotion regulation. Women may experience negative emotions more intensely than men, as evidenced by gender differences in self-reported emotional intensity (Gohm 2003) and increased N1 and N2 event-related potential amplitudes in response to negative stimuli (Gardener, Carr, MacGregor, & Felmingham, 2013). Differences in appraisals of negative events have been offered as a mechanism which may be driving this gender difference (Hyde, Mezulis, Abramson, 2008).

Women also report that they use almost all emotion regulation strategies more often than men, including cognitive reappraisal (Nolen-Hoeksema, 2012). There is even some evidence to suggest that men and women may have different neural correlates of cognitive reappraisal. A study examining fMRI data of 13 women and 12 men using cognitive reappraisal indicated that compared with women, men exhibited lesser activation in prefrontal regions, greater reductions in amygdala activation, and lesser activation of ventral-striatal areas while reappraising (McRae, Ochsner, Mauss, Gabrieli, & Gross, 2008). The authors suggested that the men in their study may have expended less effort and cognitively reappraised more automatically than the women, and/or the women may have used positive emotions to reappraise moreso than the men. Taken together, the literature does seem to suggest that gender differences may exist in the experience and regulation of negative emotions, such that women may experience more intense negative emotions than men and may regulate their emotions with more effort.

Individual differences in cognitive reappraisal ability are also important to examine, as they have implications for psychopathology. One study has shown that reappraisal ability may moderate the relationship between stress and depression in a community sample (Troy et al., 2010). The authors measured reappraisal ability based on self-reported sadness following a reappraisal challenge. They also included an objective measure of sympathetic activation, skin conductance response, which has been shown to exhibit a negative relationship with sadness. The authors found that among participants with a high number of stressful live events, high reappraisal ability was associated with fewer symptoms of depression. This relationship was non-significant for those with low life-stress. The authors concluded that high cognitive reappraisal ability may create a buffering effect against depression among individuals with high life stress.

Brain imaging studies have also been used to examine reappraisal's corresponding neural circuitry and to differentiate it from other emotion regulation strategies (e.g. Goldin, McRae, Ramel, & Gross, 2008). One study found that down-regulation of negative emotions elicited by a film via reappraisal was associated with early increased activation of the medial prefrontal cortex and the left orbitofrontal cortex, both of which are associated with cognitive control. This was coupled with later reduced activation of the left amygdala and insula, both of which are associated with emotional reactivity. Such a relationship suggests that during reappraisal, early prefrontal activation helps to deactivate limbic brain regions, resulting in reduced negative affect. Suppression of negative emotions, on the other hand, was associated with increased right amygdala and insula activity with no changes in the left, suggesting enhanced emotional responding during suppression. Furthermore, prefrontal areas implicated in cognitive control, such as the ventrolateral and dorsolateral prefrontal cortex, were not activated until late in the film. These findings suggest that differences between reappraisal and suppression, in terms of both their implementation and their outcomes, are rooted in differences in dynamic brain activity. Furthermore, results indicate that early cognitive control contributes to the effective regulation of negative emotion observed with cognitive reappraisal.

It is also important to note that due to the prefrontal cortical involvement in reappraisal, this emotion regulation strategy follows a developmental trajectory whereby the ability to down-regulate negative emotions with reappraisal improves over time (McRae, et al., 2012). One study by McRae, Gross, and colleagues (2012) examined reappraisal ability in children, adolescents, and emerging adults (ages 18-23). Emerging adulthood in particular is an interesting age to study with regard to reappraisal, as significant changes in frontal lobe connectivity occur due to a surge in dendritic pruning and myelination, which are associated with decreased impulsivity and increased planning (e.g. Johnson, Blum, & Giedd, 2009). McRae et al. found that, compared with children and adolescents, emerging adults reported greater decreases in negative emotion using cognitive reappraisal. A linear relationship between age and reappraisal success was found, indicating that the ability to reappraise increases with age. These results were supported with imaging data as well – linear increases were noted in activation of the left ventrolateral prefrontal cortex and left inferior frontal gyrus with increasing age, suggesting that these frontal areas associated with cognitive control are more easily recruited by older adolescents and emerging adults compared with children. Interestingly, a quadratic effect was also found. While reappraising, adolescents exhibited more activation than either children or emerging adults in the posterior cingulate, which is involved with mental state attributions. This finding suggests that developmental status may affect the way in which reappraisal is used, making the transitional period around 18 years of age, the typical age of incoming college undergraduates, a fascinating time to study reappraisal.

Cognitive Control and Emotion Regulation

As one may gather from the findings above, individual differences in cognitive control have been inextricably linked with cognitive emotion regulation strategies such as reappraisal (e.g. Ochsner & Gross, 2005). Cognitive control is subsumed under the category of abilities referred to as executive functions, specifically, a set of higher-order processes located in the prefrontal cortex which are involved with self-regulation and problem solving (e.g. Zelazo & Cunningham, 2007). Executive functions are used to develop mental representations, create a plan, maintain the plan and representation in mind, execute the plan, evaluate the outcome of one's behavior, and make adjustments if needed. The complexity of behaviors subsumed by cognitive control necessitates that different cognitive processes are recruited at different times and for different sorts of tasks. For example, attempting to remember a phone number to dial using verbal rehearsal requires one to develop and repeat a representation of the numbers (e.g. create a mental representation and maintain it using working memory), create a plan to get to a phone quickly, and dial the numbers. Once dialed, errors can be monitored for and corrected if needed. All the while, distractors must be inhibited such that the phone number is not dislodged from memory. All of these different processes are subsumed under the umbrella of working memory.

Importantly, this example also demonstrates the dynamic interactions between bottom-up and top-down processing involved in cognitive control (Zelazo & Cunningham, 2007). Bottom-up processing is data-driven, meaning that it is invoked by external environmental demands and involves the movement of information from simple to complex processing. Top-down processing, on the other hand, is more cognitively driven and implementational in nature, moving from higher order processing areas to lower-level areas involved with execution.

For example, encountering and attempting to remember a phone number is bottom-up because this task originates from an environmental demand and requires the recruitment of some higher-order cognitive resources such as working memory. Strategies used to remember the phone number, such as grouping the numbers into chunks or verbally rehearsing the numbers, represent top-down processing, in that a plan created in the brain to improve memory is superimposed on the data and used to maintain the data in mind. Bottom-up processing will again come into play when monitoring the outcome of the strategy – one may notice that saying the numbers in mind is not working as well as expected. This would again necessitate the recruitment of top-down processing to modify the strategy, such as saying the numbers out loud, to improve retention.

Cognitive emotion regulation strategies such as reappraisal operate in a similarly dynamic fashion by recruiting cognitive processes in the prefrontal cortex which are involved in general self-control and problem-solving. Indeed, studies have shown that, compared with passive viewing, reappraisal is associated with increased activation in the left dorsolateral and ventrolateral prefrontal cortex (Ochsner, Bunge, Gross, & Gabrieli, 2002), both of which are activated when responding to conditional rules (Bunge & Zelazo, 2006). Furthermore, as mentioned previously, studies indicate that early recruitment of prefrontal regions appears to drive later decreases in areas implicated with emotional experience, such as the amygdala (Goldin, McRae, Ramel, & Gross, 2008).

Patient populations lend further evidence to support the link between reappraisal and cognitive control. For example, patients with Parkinson's disease have been shown to exhibit deficits in executive functions related to loss of dopamine in frontal-striatal circuitry. This frontal-subcortical dopamine deficiency seems to affect both emotional and non-emotional planning and organization. One study linked deficits in patients' performance on the Tower of London task, a measure of planning and organization, with their degree of self-reported emotional instability (Volpato, Signorini, Meneghello, & Semenza, 2009). A similar relationship exists for patients who have undergone a traumatic brain injury (TBI) involving the frontal lobes and their circuitry. Deficits in a task assessing working memory were found to predict lower use of adaptive coping strategies such as planful problem solving and increased use of maladaptive strategies such as avoidance, again suggesting a tight coupling of cognitive control and emotion regulation (Krpan, Levine, Stuss, & Dawson, 2007).

Affective control processes. Recently, there has been a shift away from assessing cognitive control simply with "cold" cognitive stimuli (e.g. numbers) and toward integrating tasks using emotional stimuli (Joormann & D'Avanzato, 2010). Research suggests that cognitive control processes related to affective information may be qualitatively different from the processing of emotionally neutral information. For example, one study found that an emotion regulation task, but not a cognitive task, disrupted the maintenance of emotional material in working memory, suggesting that a separate domain of working memory may exist for the emotional material (Mikels, Reuter-Lorenz, Beyer, & Fredrickson, 2008).

Furthermore, research indicates that tasks assessing cognitive processing of affective stimuli may actually be better predictors of vulnerability to emotion dysregulation and psychopathology than tasks using neutral stimuli (Joormann & D'Avanzato, 2010). For example, Joormann and Gotlib (2010) have found that among depressed participants, reduced inhibition of specifically negative emotional material is associated with reduced use of beneficial emotion regulation strategies such as reappraisal. The same lab has also found evidence that depression and rumination, a maladaptive coping strategy characterized by non-productive thought recycling, are linked to poorer manipulation of negative emotional material in working memory (Joormann, Levens, & Gotlib, 2011).

Other researchers have also found evidence for this relationship between affective control processes and emotion regulation. For example, attentional control capacity for emotion (ACCE) involves the ability to deploy visual attention to different aspects of an emotional stimulus in order to make judgments based on rules (Johnson, 2009a). Studies have found that ACCE is separate from general cognitive control (Johnson, 2009a) and that ACCE was uniquely associated with spontaneous down-regulation of frustration (Johnson, 2009b). Another study found that individuals who exhibited difficulty in shifting attention from an emotional to a neutral task set experienced higher trait anxiety and more worries than those who were able to shift efficiently (Johnson, 2009b).

Our lab has also found evidence for an association of an affective process and emotion regulation. *Affective flexibility* refers to the ability to efficiently shift between processing the affective and non-affective aspects of an emotional stimulus in order to make decisions (Malooly, Genet, & Siemer, 2013). This process is measured using a task-switching paradigm during which participants categorize emotional pictures according to different rules, one based on the valence of the image (positive or negative) and another based on a neutral component (the number of human beings depicted). Trials in such a paradigm may represent a repetition of the previous sorting rule or a switch to another sorting rule. Switch trials require several aspects of cognitive control: the participant must disengage from and inhibit the previous sorting rule, quickly shift attention to the new sorting rule, and make a response based on the new mental set.

Findings from this task have indicated that more flexibility in switching toward processing the emotional components of negative stimuli is uniquely associated with emotion regulation outcomes (Malooly, Genet, & Siemer, 2013). On these trials, participants must disengage from an emotional task set and switch toward a neutral task set in order to categorize a negative image. Quicker reaction times on these trials have been associated with less self-reported rumination in daily life (Genet, Malooly, & Siemer, 2013), better reappraisal efficacy (Malooly, Genet, & Siemer, 2013), and lower symptoms of depression (Malooly & Siemer, 2012), suggesting that flexibility on these trials is adaptive. On the other hand, it was also seen that increased flexibility on trials involving an attentional shift away from processing the emotional content of a positive stimulus has been associated with increased rumination, suggesting that affective flexibility is adaptive only within particular contexts (Genet, Malooly, & Siemer, 2013).

The field of affective control processes is still emerging, and studies to date have treated affective processing as a trait measure. However, given the relationships among affective control processes, emotion regulation, depression, and anxiety, it seems important to gain more understanding into how these processes work, and whether gender differences may play a role. In particular, it remains to be seen whether affective flexibility functions is a static trait, or may be amenable to change through training.

Training Cognitive Processes

The prospect of completing computerized tasks in order to train cognitive processes such as attention, memory, and concentration has proliferated in both psychology research and the media (e.g. "Lumosity," in Hardy & Scanlon, 2009; cognitive control therapy for depression in Siegle, Ghinassi, & Thase, 2007). The idea is simple: participants complete tasks on a computer designed to "exercise" their brain and should see improvements in their daily functioning. The methodology of such training paradigms varies: some studies have included multiple tasks aimed at training various aspects of cognitive control including attention, perceptual speed, working memory, and executive function (e.g. Hardy, Drescher, Sarkar, Kellett, & Scanlon, 2011; Schmiedek, Bauer, Lövdén, Brose, & Lindenberger, 2010; Siegle, Ghinassi, & Thase, 2007) whereas others have focused on one particular component of cognitive control such as attention (e.g. MacLeod, Rutherford, Campbell, Ebsworthy, & Holker, 2002). Across methodologies, there is at least some evidence to suggest that these training interventions may work and may generalize to non-trained skills (e.g. Hardy, Drescher, Sarkar, Kellett, & Scanlon, 2011; Schmiedek, Bauer, Lövdén, Brose, & Lindenberger, 2010; Siegle, Ghinassi, & Thase, 2007). However, results are mixed. A meta-analysis of working memory training studies cautions that transfer to other measures has not been adequately measured, and gains in other areas (i.e., intelligence) are not consistently linked to changes in working memory capacity (Shipstead, Redick, & Engle, 2012).

Cognitive Control Training

The recent explosion of research on cognitive control training has largely stemmed from some seminal research by Jonides and colleagues, in which the authors propose that cognitive training on a working memory task can improve fluid intelligence, the ability to use logical reasoning to solve problems (e.g., Jaeggi, Buschkuehl, Jonides, & Perrig, 2008). That lab has run several studies in which participants have been trained on a dual n-back task. During this task, participants are visually presented with a box in a spatial location and orally presented with a letter and are instructed to indicate whether each of the stimuli matches the one presented n-back. The task is adaptive, with *n* increasing as participants showed mastery and decreasing if participants exhibited difficulty. The authors argue that this design promotes general cognitive control, as participants have to improve “general strategies” rather than “task-specific strategies,” as well as used divided attention.

In a series of studies, the authors reported that 25-minutes of daily training resulted in improvement in working memory capacity, which also transferred to improvements on tests of fluid intelligence such as Raven’s Progressive Matrices. Furthermore, the authors varied the number of training sessions from 8-19 sessions and found evidence for a “dosage-dependent” relationship, meaning that participants who completed more training sessions exhibited more improvement in their working memory and fluid intelligence.

Another example of broad cognitive training which has become part of popular culture, Lumosity, was developed by the Lumos Labs to provide generalist cognitive training to a broad audience via the internet (Hardy & Scanlon, 2009). Participants complete games which claim to train attention, working memory, processing speed, mental flexibility, and problem solving, and can do so at their own convenience, on their computer or smart phone. For example, in one game, the participant must keep track of several swimming fish in order to feed each fish only once per round. This task is said to train both visual divided attention and working memory – participants must focus on the task of feeding each fish only once and keep in minds which fish have been fed and which fish have not.

Another task is similar to the Wisconsin Card Sorting task (Grattan, & Eslinger, 1989) and requires participants to sort cards according to one of 5 hidden rules (the shape on the card, the color of the shape, the number of shapes, the orientation of lines on the card, or the type of border surrounding the card, Hardy & Scanlon, 2009). This task is intended to train cognitive flexibility and working memory, in that participants must infer the current sorting rule and be able to update their hypothesis should they encounter evidence that the sorting rule has changed. The games are also adaptive, meaning that as the participant's performance improves, the difficulty of the game increases. In the fish feeding game, for example, distractors are introduced and increase as participants gain proficiency with feeding every fish one time. Thus, participants are consistently challenged at an appropriate level, in order to promote learning and refinement of the skill.

Several studies have now been conducted examining the Lumosity training package among different populations. For example, one study examined whether a subset of the training program could have a positive impact on childhood cancer survivors ranging in age from 7-19, as children with a history of cancer are at a high risk of cognitive impairment (Kesler, Lacayo, & Jo, 2011). Children completed one 20-minute attention, working memory, and cognitive flexibility training session per day, 5 days per week over the course of 8 weeks for a total of 40 20-minute training sessions. The results of the study indicated that compared with their own baseline, the trained children improved their processing speed, cognitive flexibility, and memory, as well as increased their own prefrontal cortical activation. Furthermore, the intervention was well tolerated, with 83% of participants completing the intervention as intended.

Similar training effects of the Lumosity intervention on cognitive functioning have been found when examining samples of healthy adults (Hardy, et al., 2011) and adults with mild cognitive impairment (Finn & McDonald, 2011). There was also evidence that among the healthy adults receiving the Lumosity intervention, the gains in visual attention and working memory were not simply task-specific and transferred to untrained tasks assessing these same constructs (Hardy, et al., 2011). Thus, there is some initial evidence that some groups may experience improved cognitive abilities following broad cognitive training. However, it is important to note that the studies of children and healthy adults did not report any gains in outcomes actually associated with daily functioning (e.g. self-reported memory functioning and mood) and that the study investigating older adults indicated that participants had *no change* in their everyday memory functioning or mood (Finn & McDonald, 2011). Thus, although some evidence suggests that the Lumosity intervention may improve aspects of cognitive functioning, there is so far no evidence that this translates into meaningful differences in participants' lives.

Another broad-based cognitive training intervention was used in a community sample of young (ages 20-31) and older (65-80) adults, which intended to train perceptual speed, working memory, verbal memory, and attention (the COGITO study, Schmiedek, Bauer, Lövdén, Brose, & Lindenberger, 2010). The tasks were completed through an online database on the participant's home computer and took about one to one and a half hours to complete per session. Example tasks included quickly categorizing numbers as odd or even (e.g., perceptual speed), memorization of word-number pairs and upon receiving a word cue, entering the appropriate number (episodic memory), and determining if the position of dots within a matrix matched the position of the dots presented 3 trials back (working memory).

Similar to the Lumosity studies, the results of this study found that participants in both age groups exhibited some improvements to their own cognitive processing by completing the training paradigm most days of the week over the course of 6 months. Additionally, results of this study suggested that gains made during the COGITO study generalized to daily functioning, including improved sense of general well-being and life satisfaction. Although these findings sound exciting, the training intervention required a significant commitment on the part of study participants – the intervention required at least one hour of computerized tasks per day, 4-6 days per week, with an average of 101 hours of training. This is quite time-consuming, and naturally, younger participants became bored with the intervention over time. Furthermore, improvements in well-being and life satisfaction were not explicitly linked with improvements in performance, which introduces the possibility that some other variable (e.g., participant motivation, belief in the intervention, placebo effect) may actually be driving this improvement.

Broad cognitive training has also been used as an intervention with psychiatric patients, particularly in the cases of psychosis (e.g., Lindenmayer, et al., 2008) and mood disorders (e.g., Siegle, Ghinassi, & Thase, 2007), both which are associated with deficits in cognitive processing. For example, one study provided COGPACK – a broad cognitive training intervention targeting attention, concentration, psychomotor speed, learning/memory, and executive function – to patients with severe mental illnesses including bipolar, schizophrenia, and schizoaffective disorders (Lindenmayer, et al., 2008). Participants completed 2 hours of training per week, typically spread out over 2-3 days, over the course of 12 weeks. They also participated in a skills group discussion to help patients apply the trained cognitive skills to their daily activities. Compared with a control group matched for computer exposure, but who received no training and no skills group discussion, the patients who completed the COGPACK training exhibited greater improvements in neuropsychological variables (e.g., attention, psychomotor speed) at post-treament, as well as worked significantly more weeks than controls at a 12-month follow-up. However, cognitive symptoms were not re-assessed post-intervention, so it is unclear if these gains were maintained. Furthermore, it is important to note that participants assigned to the training group were, essentially, exposed to a broad neuropsychological battery during training sessions. It is unclear if gains were a result of actual training or more simply a practice effect. It is also noteworthy that neuropsychological and work-related outcomes were not explicitly linked to training-related gains, and again, could be due to some third variable, including the skills discussion group. Furthermore, the COGPACK group did not differ from the control group in terms of symptom improvement, with both groups improving similarly over the course of the study.

In the case of depression, Siegle and colleagues provided depressed participants already receiving treatment with training on the Wells (2000) Attention Training task and the adaptive Paced Auditory Serial Addition Task (PASAT, Gronwall, 1977) to examine whether a broad executive control training task may augment the benefits of psychotherapy (Siegle, Ghinassi, & Thase, 2007). The Wells task requires participants to focus on one sound at a time among distractors and to count the sounds, as well as continuing to count while being asked to switch among the sounds. It requires selective attention and cognitive control. The PASAT, on the other hand, requires participants to add a presented digit to the previously presented digit, but not to keep a running sum. The task also increases in difficulty over time such that numbers are presented faster as the participant's performance improves. Thus, this task requires working memory, inhibition, psychomotor speed, selective attention, and cognitive control, and has been shown in previous studies to be associated with activation of the prefrontal cortex. Depressed participants completed both tasks in a 35-minute session, 6 times over the course of 2 weeks. The results indicated that improvements in PASAT performance (indicating training) were linked with reduced symptoms of depression, decreased frequency of rumination, and more regular activity in the amygdala and dorsolateral prefrontal cortex.

Thus, the above studies indicate that broad-based cognitive interventions have the potential to generalize to daily functioning (Lindenmayer, et al., 2008) and may result in symptom and brain regulation improvements (Siegle, Ghinassi, & Thase, 2007) among individuals with psychopathology. However, it is important to note that brain imaging results in Siegle and colleagues (2007) study were based on fMRIs of only 6 participants, and should thus be interpreted with great caution.

It is also important to note some concerns regarding studies of working memory training, which seem applicable to studies of cognitive control training as well (Shipstead, Redick, & Engle, 2012). These concerns include the use of inadequate or no-contact control groups, a tendency toward defining "change" on a cognitive construct using one single measure, and using subjective reports as outcome variables. Furthermore, as indicated previously, several studies have been unable to link gains on transfer measures (i.e., intelligence) to training-specific effects – in this case, increased working memory capacity. As such, caution is recommended in interpreting results of working memory training interventions. Additional research is needed using multiple measures of cognitive abilities and examination of mechanisms which are actually leading to changes.

Affective control training. Taking the cognitive control training area a step further, one lab has adapted a working-memory n-back training task into an emotional working memory n-back training task by integrating emotional images and words into the task (e.g., Schweizer, Grahn, Hampshire, Mobbs, & Dalgleish, 2013). After completing 20 days of 20-minute emotional working memory n-back training with either exclusively negative or exclusively neutral emotional stimuli, community participants who completed the training with negative stimuli, but not those who trained with neutral stimuli, showed transfers of gains to another task assessing capacity for affective control – the emotional stroop task (Schweizer, Hampshire, & Dalgleish, 2011).

In another study, college-student participants completed 18-20 days of n-back training (or a control matching task) which integrated both negative and neutral emotional stimuli (Schweizer, et al., 2013). The results indicated that in the training group, participants made gains in the emotional n-back task and that these gains translated into reduced frontal activity during a 3-back task as well as improved emotion regulation efficiency from baseline to post-training.

These studies suggest that affective control training with an emotional n-back task may be effective when emotionally evocative stimuli are used. Furthermore, they provide initial evidence that the transfer effects of working memory training may be far-reaching enough to affect emotion regulation. However, it is important to note that in these studies, the authors did not report on relevant variables such as symptoms of depression or anxiety, which may impact performance on such tasks.

Attentional Bias Modification Training

Another sort of cognitive training using emotional stimuli, attentional bias modification training, has received extensive research attention (MacLeod, Rutherford, Campbell, Ebsworthy, & Holker, 2002). The idea behind this sort of training is that individuals may have pre-existing biases toward attending to particular kinds of emotional information based on clinical diagnoses, individual differences, etc. In the case of anxiety, research has shown that individuals with various anxiety disorders have a predisposition toward attending to negative emotional material (e.g., Mathews & MacLeod, 2002). The evidence is mixed with depression, but some studies indicate that depressed participants exhibit a selective bias for dysphoric information which is associated with severity of symptoms (Wells & Beevers, 2010). Furthermore, evidence suggests among healthy control participants, biases toward negative emotional stimuli or away from positive emotional stimuli are individual difference variables that may represent anxiety vulnerability (Taylor, Bomyea, & Amir, 2011).

Researchers have used a modified dot-probe task in an attempt to retrain habitual visual attention away from negative or threatening material (MacLeod, et al., 2002). Participants are told that they will see a probe, such as a dot, and that they are to identify the exact probe (e.g., one or two dots) by pressing a key. For a given trial, participants would be presented with two emotional stimuli, for example, one positive and one neutral word, which would appear one on top of the other on the computer screen. Following the presentation of these two words, a probe stimulus would appear in the position of one of the two words. The participant then responds to the probe.

In order to manipulate attentional biases, the dot probe is fixed to follow one sort of stimulus or another for the majority of trials. For example, in a task training attention away from negative stimuli, the dot probe may follow the location of the neutral word 80% of the time. There are several variations of this task, with some labs using emotional images rather than words, others using different areas of affective space (e.g., positive and negative), and others using letter probes (e.g., “E” or “F”) instead of dot probes. However, the idea remains the same. Over time, participants become trained to anticipate that the dot will appear in a particular location and, in theory, develop a pattern of attending to these types of stimuli on future trials.

In one study, MacLeod and colleagues (2002) trained healthy undergraduate participants on the attentional bias modification task, with attention trained either toward neutral words or toward negative words, to examine the effect of training on later emotional vulnerability. To assess emotional vulnerability, all participants completed a stressful anagram task during which their performance was recorded "for training purposes," and they were given false feedback stating that their accuracy was "unusually low." The results indicated that participants trained with a negative attentional bias experienced more negative emotions as a result of the stressor than those who were trained with a neutral bias. This study was among the first to suggest that attentional biases may be modifiable in healthy participants, and that modifying attentional bias, in turn, affected later emotional reactions.

Since then, many other researchers have investigated the efficacy of the attentional bias modification tasks. Among another group of nonclinical participants, positive attentional bias modification training was provided to examine the effect of a positive bias on later stress reactivity (Taylor, Bomyea, & Amir, 2011). Participants were trained using a paradigm similar to the one described above, using positive and neutral emotional words and a letter discrimination probe. Following the training, participants were required to give a 5-minute, video-taped, impromptu speech focusing on either a technical (e.g., nuclear power) or controversial (e.g., abortion) topic. The results indicated that effectiveness of the bias modification task varied among individuals, and that the degree of positive bias modification was associated with the degree of stress reactivity and decay in positive mood following the speech stressor. However, trait levels of social anxiety moderated this relationship, such that those individuals high on social anxiety exhibited less positive attentional bias following the training. This study lends additional evidence to substantiate the relationship between attentional bias modification and emotional vulnerability, and provides some evidence that training effects may generalize to positive stimuli. Furthermore, it also indicates the importance of measuring trait anxiety as a control variable which may affect capacity for attentional training.

Among clinical populations, there is some evidence to indicate that attentional bias modification paradigms may have an impact on anxiety-related behaviors and symptoms (e.g., Amir, Weber, Beard, Bomyea, & Taylor, 2008). For example, Amir and colleagues (2008) found that using a one-session paradigm to train socially anxious participants' attention away from disgust faces and toward neutral faces resulted in reduced anxiety following a public speaking challenge. Furthermore, those individuals were rated as giving better speeches by blind raters than those in a control condition.

In another study of social anxiety, the same lab provided participants diagnosed with generalized social phobia with eight 20-minute sessions of the same attentional bias modification training described above over the course of 4 weeks (Amir, et al., 2009). The results indicated that the training was effective, and that participants improved in both self-reported and clinician reported symptoms of social phobia. Furthermore, participants maintained their gains at a 4-month follow-up, indicating that the effects of the attention bias modification training persisted over time.

A meta-analysis lends additional support to the efficacy of cognitive bias modification training for anxiety (Hallion & Ruscio, 2011). The meta-analysis indicated that across studies, a medium effect of attention bias modification has been found on biases themselves, and that induced biases have a small but significant effect on anxiety.

Furthermore, there was a trend toward a benefit of multiple training sessions, as opposed to single session training studies.

As mentioned before, the literature regarding depression is more mixed. Results of a meta-analysis examining the effects of attentional bias modification training on depression concluded that there was no true effect (Hallion & Ruscio, 2011). However, there has been some promising research in this area. For example, one study used emotional images and faces to train mildly to moderately depressed participants' attention away from negative images and toward neutral ones (Wells & Beevers, 2010). Participants completed 4 training sessions over the course of two weeks and attended a follow-up session 2 weeks after the completion of training. The results indicated that compared with a control group, participants who completed the attentional bias modification intervention experienced greater decreases in depression at follow-up. Furthermore, the difference between groups was mediated by change in attentional bias, such that individuals who showed greater evidence of modified attentional bias also showed greater reductions in depressive symptomatology.

In conclusion, research on cognitive control training interventions, including broad-based interventions and attentional-bias modification paradigms, is expanding quickly and providing some evidence to suggest that such interventions may have beneficial effects on healthy participants and patients with psychopathology. Regarding broad-based interventions, research indicates that such training may improve measures of general cognitive control (Hardy, et al., 2011; Schmiedek, et al., 2010), although evidence of generalizability to daily functioning is currently lacking. Broad cognitive control training interventions may also improve symptoms of depression among those with moderate to severe symptoms (Siegle, Ghinassi, & Thase, 2007). However, the applicability of such interventions to emotional functioning in general, and particularly among relatively healthy participants, is currently limited.

The attentional bias literature has some more evidence to suggest that such interventions may improve symptoms of anxiety (e.g., Amir, et al., 2009) and vulnerability to negative emotions (Taylor, Bomyea, & Amir, 2011). However, the specificity of such interventions makes it unclear as to how well such training would generalize to non-anxious control participants, as well as how it might generalize to daily life. Furthermore, the evidence remains mixed regarding the efficacy of such interventions with depressed participants. Given the high comorbidity of anxiety and depression, and the presence of these symptoms among relatively healthy persons in the community, it appears imperative to develop cognitive training interventions which may work effectively with both mood and anxiety symptomatology.

Current Study

Although there is evidence linking affective control processes and emotion regulation (e.g., Malooly, Genet, & Siemer, 2013), few studies have examined the trainability of affective control processes (e.g., Schweizer, et al., 2013). These results are encouraging but require replication. More training studies have focused on broad cognitive control interventions (e.g., Hardy, et al., 2011) or attentional bias modification using the dot probe task (e.g., Amir, et al., 2009). The idea of transferability to other cognitive abilities, such as fluid intelligence, as a result of cognitive control training is exciting (e.g., Jaeggi, et al., 2008). However, the evidence remains mixed at best

(Shipstead, Redick, & Engle, 2012), and becomes even more uncertain when considering emotional outcomes.

Some studies of broad cognitive control interventions indicate that there may be promise for the treatment of depression (Siegle, Ghinassi, & Thase, 2007); however, there has been less focus on general emotion regulation in this literature and it is unclear if such tasks could generalize to individuals with subclinical depressive symptomatology, mixed depression and anxiety symptomatology, or emotion dysregulation outside of depression or anxiety.

Furthermore, these broad-based cognitive interventions have used affectively neutral training stimuli. Given the finding that tasks assessing *affective*-control processes with *emotionally-evocative* stimuli may be more closely linked with vulnerability to negative emotions than tasks assessing “cold” cognitive processing using neutral stimuli (Joormann & D’Avanzato, 2010), it makes sense that training tasks using emotional stimuli may also be more appropriate than those using neutral stimuli in addressing emotional dysfunction.

Existing studies on attention bias modification training present evidence that such tasks may be useful in the treatment of anxiety disorders (e.g., Amir, et al., 2009) and may also be applicable to those with subclinical anxiety (e.g., Taylor, Bomyea, & Amir, 2011). However, these studies have been confined to the regulation of anxiety and frustration, making it is unclear if such training improves emotional functioning overall (e.g., in the case of sadness) or only in the specific case of anxiety. Furthermore, despite a moderate effect of the ABM paradigm on attention bias, the effect of ABM training on anxiety remains small (Hallion & Ruscio, 2011).

Results of the ABM paradigm with depression have been mixed, with some studies finding an effect (e.g., Wells & Beevers, 2010) and a meta-analysis finding no effect across studies (Hallion & Ruscio, 2011). It may be that the relationship between attention and depression is different from the relationship between attention and anxiety, or that a cognitive control component of training is needed to address the specific cognitive deficits associated with depression (Joormann & D'Avanzato, 2010). Thus, a broader training intervention incorporating affective stimuli may be more appropriate for individuals vulnerable to depression than an ABM training task alone.

Creating a training intervention based on the affective flexibility task described previously may be a nice compromise between a broad, cognitive control intervention and an attentional bias modification paradigm. First, the affective flexibility task was modeled from a cognitive flexibility task and requires inhibition of one rule set, shifting to a new rule set, and working memory to keep the rule set in mind, making it an excellent measure of cognitive control (Malooly, Genet, & Siemer, 2013). The use of affective stimuli is also a benefit, as this makes the task a specific measure of cognitive control of affective material, or affective control. Second, the task may be broken down into particular kinds of switch trials based on the trial type and the valence of the target stimulus. Previous studies have indicated that 2 particular switches – those requiring redirection of attention toward a non-emotional mental set (number of human beings) when the target image depicts negative emotional content, and those requiring redirection of attention toward an emotional mental set in order to categorize a positive image – are both associated with emotion-regulation outcomes (Genet, Malooly, & Siemer, 2013; Malooly, Genet, & Siemer, 2013). Thus, breaking the task down into specific kinds of switches allows for sensitivity to attentional biases in addition to the more general cognitive control component. As such, the affective flexibility task appears to be a good candidate for assessing both affective control and attentional biases.

Furthermore, previous research indicates that the affective flexibility task is associated with constructs of interest such as better performance on a task assessing reappraisal efficacy (Malooly, Genet, & Siemer, 2013), self-reported rumination in daily life (Genet, Malooly, & Siemer, 2013), and symptoms of depression (Malooly & Siemer, 2012) within healthy samples. This indicates that the task is broadly applicable to emotion regulation and dysregulation in healthy and sub-clinically depressed participants.

Although this task has yet to be studied in the context of anxiety, some preliminary evidence suggests that the affective flexibility task correlates with Neuroticism (e.g., "Worries a lot," John, Donahue, & Kentle, 1991) which represents emotional reactivity and vulnerability to negative emotion (Malooly, 2012). Therefore, it seems reasonable to assume that this task may also be linked with trait anxiety. Given its applicability to trait Neuroticism, depressive symptoms, reappraisal, and rumination, it seems logical that this task may be appropriate to adapt into a training intervention, targeting emotional functioning, which could be used with healthy controls, subclinical participants, and possibly diagnosed participants.

Based on the state of the current literature and the associations among the affective flexibility task and emotion regulation outcomes, the goal of the current study was to adapt the affective flexibility task into a training task to determine if affective flexibility training may improve reappraisal efficacy. To determine if affective flexibility training and improvements in emotion regulation may also generalize to symptoms of psychopathology, the effect of training on symptoms of depression and anxiety was also examined. Furthermore, to assess if the results may generalize to affective processing in general, transfer tasks were used to assess gains in other domains of affective processing. Gender differences were included throughout analyses to examine whether differences in experience and regulation of negative emotions may play a role in results. I hypothesized that (1) training on the AF task would selectively change specific switching costs, such that participants would perform more efficiently on trial types which have been trained, (2) following AF training, participants would show improvements in emotion regulation in general and be better able to down-regulate negative emotions using cognitive reappraisal, (3) AF training would improve symptoms of depression and anxiety, both of which entail emotion dysregulation, in a healthy sample, and (4) the effects of training on the AF task would generalize to other measures of affective-control processing.

Chapter 2

Method

Participants

The sample was drawn from students enrolled in Introductory Psychology courses at the University of Miami. Participants were recruited using an internet-based research pool, wherein students sign up to complete experiments as part of a course requirement. All participants were required to be fluent in English and able to commit to completing experimental and training sessions for two weeks. The experiment aimed to recruit 40 undergraduate participants who reflected the ethnic diversity of the area. This *n* was based on an a-priori sample size calculation for a repeated measures ANOVA with between subjects factors, using a medium effect size and power=.80 with G*Power 3 software (Faul, Erdfelder, Lang, & Buchner, 2007).

Screening for study inclusion occurred during a mass pretesting session. To ensure that the sample consisted of healthy participants, participants completed the Beck Depression Inventory-II (BDI-II; Beck, Steer, & Brown, 1996) to assess symptoms of clinical depression and suicide risk. Participants who either (1) met criteria indicating symptoms of moderate-severe depression (BDI-II > 19) or (2) endorsed items indicating risk for suicide were ineligible to participate in this study. Participants were also excluded if they reported taking psychiatric medication (e.g., an antidepressant) or had a history of head injury (e.g., unconsciousness).

In total, 55 participants began the study protocol. Five participants chose not to complete the follow-up session, and another six participants completed fewer than four training or control sessions. This resulted in a final *n* of 44 participants, with a mean age of 18.84 (SD = 1.98). Almost one third of the sample was male (31 females, 13 males) and the sample was racially diverse (47% non-Hispanic White, 30% Hispanic, 11% Black, 9% Asian, 3% other). It is notable that for some of the following analyses, the sample size was reduced by a recording error which affected 11 participants' baseline data, and by inaccurate performance on particular tasks. This results in different *n*s for each analysis, with a range of 28-44. Individual *n*s are reported for each specific analysis.

Based on results of the CES-D completed at the baseline session, 27% ($n = 12$) of study completers endorsed symptoms of mild depression as evidenced by a total CES-D score of 16 or more ($m = 11$, SD = 7.36). Study non-completers did not differ from study completers on age, $t(53) = -.49$, $p = .63$, gender, $\chi^2(1) = .19$, $p = .66$, or race, $\chi^2(4) = 3.42$, $p = .49$. Study non-completers did; however, endorse lower symptoms of depression, $t(22.63) = 2.03$, $p = .05$ than study completers. Non-completers also exhibited quicker sorting of sad words on the working memory manipulation measure, $t(41) = 2.41$, $p < .05$, and quicker perseveration-set reaction times on the n-back task, $t(8.73) = 2.$, $p < .05$, compared with study completers. Demographic characteristics of study completers and study non-completers, and means/standard deviations on study dependent variables, are summarized in Tables 1 and 2, respectively.

Also notable, a recording error affected collection of baseline measures of affective flexibility, working memory manipulation performance, and n-back performance for some participants. Thus, n differed across tasks. As a result, specific *n*s are reported for individual analyses.

Overview and Measures

At the Baseline session, participants completed a demographics questionnaire and documented relevant health information (i.e., current medications). They also completed questionnaires assessing symptoms of depression and anxiety. Next, participants completed a series of computerized tasks assessing affective flexibility, reappraisal ability, and affective control processing (Emotional Working-Memory and Emotional N-Back) to establish baseline performance. At the end of the Baseline session, participants were randomly assigned to either the Training or the Control condition (see below for details).

Next, participants completed a minimum of four, 15-minute training or control sessions over the course of two weeks wherein they completed either the affective flexibility training task or a control task, depending on the participant's assignment. Training sessions were completed in the laboratory and participants could schedule training sessions according to their availability. Participants were only allowed to attend one training session per day. Participants were provided with a $20 incentive if they completed five training sessions and all other study requirements.

After participants completed four or five training or control sessions, participants attended a separate Follow-up session to assess for any changes in study variables as a result of training. The Follow-up session occurred a minimum of one day after the date of last training session (mean time to follow-up = 3.25 days, median = 2 days, SD = 3.13). The procedure was similar to that outlined for the Baseline session. Participants completed questionnaire measures assessing symptoms of anxiety and depression. They also repeated the affective flexibility task, the reappraisal task, and the measures of affective processing (Emotional Working-Memory and Emotional N-Back) to assess gains in each domain. Means and standard deviations for study dependent variables are presented in Table 3. Inter-correlations between dependent variables at the baseline session, follow-up session, and across sessions, are also presented in Tables 4 and 5. The details of the measures are outlined below.

Self-report measures.

Demographics. Participants reported on basic demographic information such as age, gender, and ethnicity. Relevant health information (e.g., medications) was also documented. Items assessing participants' current fatigue, confusion, and disorientation were also included.

Symptoms of Depression. The Center for Epidemiological Studies Depression Scale (CES-D; Radloff, 1977) was used to assess participants' depressive symptomatology. Participants indicated their agreement with various statements (e.g., "I felt depressed") on a 4-point scale ranging from 0 = "Rarely or none of the time (less than 1 day)" to 3 = "Most or all of the time (5-7 days)." Studies have found the CES-D to be psychometrically sound in college undergraduates (Santor, Zuroff, Ramsay, Cervantes, & Palacios, 1995).

Trait Anxiety. Participants' trait-level anxiety was assessed using the State-Trait Anxiety Inventory (STAI; Spielberger, Gorsuch, Lushene, Vagg, & Jacobs, 1983). Participants rated statements (e.g., "I worry too much over something that really doesn't matter") indicative of anxiety on a 4-point scale anchored with 1 = "almost never" and 4 = "almost always." The STAI has been found to be psychometrically sound in various samples (Spielberger, Gorsuch, Lushene, Vagg, & Jacobs, 1983; Spielberger, 1989).

Emotional experience. Participants' momentary emotional experiences were assessed immediately following the demographics questionnaires, following a neutral film clip, and following a sad film clip (see below) using items from the PANAS. Following the demographics questionnaires, participants completed a questionnaire listing emotional words (e.g., "happy," "sad," "angry") rated based on a 5-point scale anchored by 1 = "not at all" and 5 = "extremely." Following the neutral and sad film clips, participants were presented with emotional words representative of sad mood (e.g., "sad," "down," "depressed") and asked to rate their momentary emotional state on a scale of 1 to 5 by pressing a key on a computer keyboard.

Emotion regulation follow-up questionnaire. Following the sad film clip, participants were asked to describe how they down-regulated their emotions in their own words. Responses were coded to reflect whether the participant endorsed using cognitive reappraisal to regulate emotions, used some other strategy (e.g., distraction), or did not regulate emotions. Open-ended responses were coded by this author.

Affective flexibility task. Flexible affective processing was assessed using the affective flexibility switching task described in Malooly, Genet, and Siemer (2013). The task required participants to categorize affective images according to different rules. This task began with two 10-trial practice blocks intended to orient participants to the task, and to introduce them to the different sorting rules. Each block required participants to sort pictures of positive and negative valence according to one rule at a time. The practice blocks were accuracy-gated, that is, participants were not able to advance to the experimental trials until they achieved sufficient accuracy (80%). The practice blocks repeated until the participant's performance improved.

Next, participants completed 320 experimental trials in which they sorted affective images using both rules, which changed according to a pseudorandom sequence. The trials were grouped into 2 blocks of 160 trials, with a break in between in order to reduce effects of participant fatigue.

During a given trial, participants viewed a fixation cross in the center of the screen for 250ms, followed by a blank screen for 250ms, then an emotional picture surrounded by a colored cuing frame. Pictures were drawn from the International Affective Picture Set (IAPS; Lang et al., 2008). Each trial cue was presented in the left and right sides of the colored frame surrounding the image ("+" and "-" for positive and negative, or "≤1" and "≥2" for one or fewer human beings and two or more human beings). Participants categorized each image by pressing one of two adjacent keys on a computer keyboard using the pointer and middle fingers of their dominant hand. The cue and image remained on the screen until a response was logged. They were also instructed to make a selection as quickly and accurately as possible. After logging a response, participants were presented with a fixation cross, followed by a blank screen, then a new cue and stimulus pair.

The instructions for the task were presented in a 20-point, Courier New font, with white text on a black background. The cues were presented in a 65-point, Courier New font, with either black text on a white background or black text on a grey background. The larger font size was intended to increase the salience of the cues, as they were presented on both sides of the screen, rather than in the center. The task was counterbalanced across participants such that the mapping of the cues onto keys for the emotional sorting rule differed across participants (e.g., "+" will appear on the left and "-

"will appear on the right for half of participants, and vice versa). The key mapping for the non-emotional rule stayed the same across participants. This resulted in 2 possible combinations of key mappings.

The trial rule and the type of image presented on a given trial followed a pseudorandom sequence. In order to complete the task successfully, participants needed to flexibly switch between an affective and a non-affective task set while evaluating emotionally evocative images. A given trial during this task could have involved a change in rule from the previous trial (switch trial) or a repetition of the rule from the previous trial (repetition trial). The additional time required for trials in which the rule switches compared with when the rule stays the same, referred to as switching costs, were calculated by subtracting the average reaction time for repetition trials from the average reaction time for switch trials. This difference reflected the "cost" associated with switching mental set. Means and standard deviations for different types of switches, repetitions, and switch costs are presented in Table 6.

Reappraisal challenge task. Participants began by watching a neutral film clip (Coral Sea Dreaming; Hannon, 1999) in order to establish a baseline neutral mood. They were instructed to watch the film without attempts to regulate their emotions. Following the baseline film clip, participants rated their current mood using PANAS items presented on the computer screen.

Next, all participants were instructed to down-regulate their emotions using reappraisal during the next film clip. An experimenter verbally described the strategy and provided some examples of how one might reappraise, such as "by viewing the film from a detached, technical perspective, by reminding themselves that the characters are actors and that the film isn't real, or by imagining that the situation depicted will eventually get better." The experimenter checked that each participant understood the directions by asking for an example of how she or he could have reappraised during the previous film-clip and by eliciting any questions the participant may have. Once the participant demonstrated good understanding of the instructions, the participant reappraised while watching one of two film clips used to elicit sadness in the laboratory, either from *The Champ* (Zeffirelli, 1979) or *Return to Me* (Tugend & Hunt, 2000).

Participants were randomized to view one clip or the other, such that each participant viewed a different clip at the baseline session than at the post-training session. At the conclusion of the film clip, participants were again asked to rate their current mood using PANAS items presented on the computer. They also answered an open-ended question on the computer inquiring about how they down-regulated sadness during the clip.

Affective flexibility training task. To train affective flexibility, participants were administered a modified version of the affective flexibility task. Evidence from previous studies suggested that lower (quicker) switching costs in two particular switches were associated with self-report of emotion regulation in the laboratory and in daily life: switches toward processing the neutral aspects of negative images (Malooly, Genet, & Siemer, 2013; Genet, Malooly, & Siemer, 2013), and switches toward processing the emotional aspects of positive images (Genet, Malooly, & Siemer, 2013). Thus, these switches were over-represented at an 80:20 ratio in the pseudorandom sequence used to determine trial type and stimulus.

Participants assigned to the active training condition completed 200 trials in which they sorted affective images according to the same two rules used for the affective flexibility task. Like the affective flexibility task, the training task presented participants with a fixation cross for 250ms, followed by a blank screen for 250ms, then an emotional picture drawn from the IAPS (Lang et al., 2008) surrounded by a frame indicating the rule for the current trial ("+" and "-" for positive and negative, or "≤1" and "≥2" for one or fewer human beings and two or more human beings). Participants categorized the image using the pointer or middle finger of their dominant hand by pressing one of two adjacent keys on a computer keyboard. The image and cue remained on the screen until a response was logged. Participants' key mapping for the AF training task was matched to be the same as the key mapping used during their baseline and follow-up visits. Participants were reminded to complete each trial both as quickly and accurately as possible.

Following a logged response, participants were presented with a fixation cross, a blank screen, then a new image and cuing frame. Task instructions were presented with 20-point, Courier New font, with white text on a black background. The cues within the cuing frames used a 65-point, Courier New font, with either black text on a white background or black text on a grey background. The cue and picture changed according to a pseudorandom sequence; however, the contingencies for specific trials were set such that about 40% of switching trials involved a switch toward the non-affective (human) rule with a negative target image and about 40% required a switch toward the affective rule with a positive target. The other 20% of switch trials were non-training switches. Trials were grouped into blocks of 20 with feedback on accuracy presented at the end of each block, in order to keep participants engaged in the task. Blocks with accuracy greater than or equal to 90% resulted in encouraging feedback, whereas blocks with accuracy below 90% resulted in corrective feedback.

Control Training Task. Participants assigned to the control condition were also administered a modified version of the training task. Switches which had not been associated with reappraisal ability or symptoms of depression, according to previous studies (Genet, Malooly, & Siemer, 2013; Malooly, Genet, & Siemer, 2013; Malooly & Siemer, 2012) were overrepresented in this task. Specifically, this included switches toward processing the emotional aspects of negative images and switches toward processing the neutral aspects of positive images. As with the active training condition, these switches were overrepresented at a ratio of 80:20 in the pseudorandom trial sequence such that participants had more exposure to these switch trials compared with others.

As in the AF training condition, participants in the control condition completed 200 trials in which they sorted affective images according to different rules. Again, participants were presented with a fixation cross for 250ms, a blank screen for 250ms, and then an emotional image within a cuing frame. Participants used their index and middle fingers of their dominant hand to categorize images using one of two adjacent keys on a keyboard. They were reminded to complete the task both as quickly and accurately as possible. The affective image and cuing frame remained on the screen indefinitely until a response was made. Following a logged response, participants were presented with a fixation cross, then a new image and cuing frame. Again, key mapping remained consistent for the participant on this task, and the AF task at baseline and at follow-up. Task instructions were presented with 20-point, Courier New font, with white text on a black background. The cues within the cuing frames used a 65-point, Courier New font, with either black text on a white background or black text on a grey background.

The cue and picture appeared according to a pseudorandom sequence with the contingencies for specific trials set. However, the control condition comprised at least 40% of switching trials with a switch toward the non-affective (human) rule with a positive target image, and at least 40% of switches toward the affective rule with a negative target. The other 20% of switch trials included the switches of interest. Similar to the training task, trials were grouped into blocks of 20 with feedback on accuracy presented at the end of each block in order to keep participants engaged in the task. Blocks with accuracy greater than or equal to 90% resulted in encouraging feedback, whereas blocks with accuracy below 90% resulted in corrective feedback.

Transfer Tasks.

Emotional working-memory manipulation task. Participants completed an emotional working-memory manipulation task which required them to both maintain and manipulate emotional words in working memory (Joormann, Levens, & Gotlib, 2011). During each trial, participants viewed three emotional words of the same valence (positive, negative, or neutral). Each trial began with a fixation cross for 750ms, and then a word was presented for 1000ms. This was repeated twice to present the other two words. Once all three words were presented, participants viewed a fixation cross for 750ms, and then saw a cue (either “forward” or “backward”) presented for 750ms. Following the cue, participants viewed a blank screen for 3000ms, and then a probe word. The word remained on the screen until a response was logged. . Participants identified whether the probe word was the first, second, or third word in the sequence by pressing a key using their pointer, middle, or ring finger of their dominant hand.

The task started with practice trials to acquaint participants with the procedure. Participants needed to successfully complete at least one “forward” trial and one “backward” trial in order to proceed to the experimental blocks. The experimental blocks comprised 72 trials total – 36 trials grouped into two blocks. All instructions, word stimuli, and cues were presented in 26-point, Courier New font, with white text against a black background. Sorting costs, the additional response latency time expected in backward compared with forward trials, were the construct of interest. This construct reflects the additional time needed to manipulate emotional information in working memory, while controlling for maintenance and recall.

Emotional n-back task. Finally, participants completed an emotional n-back task which involved disengaging from and updating emotional material in working memory (Levens, & Gotlib, 2010). Participants were presented with a series of emotional faces one at a time which exhibited a positive, negative, or neutral facial expression. Participants were asked to indicate whether the emotion of the facial expression in the current trial matched the emotion of the face presented two trials back by pressing a key on the keyboard.

To familiarize participants with the n-back task, participants began by completing at least five practice trials, in which they decided if the emotion of the current face matched the emotion of the face two trials back. The five-trial practice block repeated until participants were able to complete the block accurately. Participants then advanced to the experimental trials.

The experimental 2-back task consisted of 220 trials divided into four blocks of 55 trials each. Each block began with the presentation of two faces, during which participants were instructed not to make a response, and then the probe faces, which resulted in 53 usable trials per block. Participants used the index and middle fingers of their dominant hand to respond as quickly and accurately as possible, using the "n" and "m" keys on a keyboard. Each trial began with a 500ms fixation cross, then a face presented for 2000ms, followed by a blank screen for 2000ms. The task continued to advance regardless of participant response. Accuracy, response rates, and reaction times were collected to examine the updating of emotional information in working memory. Task instructions were presented in 26-point, Courier New font, with white text on a black background.

In order to be successful on this task, participants needed to process the facial expression on the current face, encode it into working memory, determine if the current emotional expression matched the one presented 2 trials earlier, and make a response. Thus, this task assessed several aspects of emotional working memory capacity, maintenance, and manipulation.

Reaction times were used to calculate several different types of trials. "No-set" trials were those in which neither the previous trial nor the current trial involved a matched set. The participant simply checked for similarity between the current and stored stimuli and responded accordingly. "Match-set" trials, on the other hand, were those trials in which the current face matched the face presented 2 trials ago (Levens & Gotlib, 2010). There were also 2 types of trials which follow a match-set, in which the current face did not match the face presented 2 trials ago. “Break-set” trials were those trials immediately following a “match” trial in which the participant broke apart the stimulus pair matched in the previous trial, removed the first face in the pair (3 trials back) from working memory and added a new face to working memory.

“Perseveration-set” trials also immediately followed a match trial, but they involved presentation of a stimulus which matched the previous set, meaning that stimulus n, n-1, and n-3 are all the same. To complete such a trial correctly, participants had to avoid perseverating and break the previous set, while adding an incoming stimulus to working memory which matched the valence of the stimulus which was removed. The ”break-set” and “perseveration-set” trials were of particular interest as they require additional affective control over stimuli in working memory, compared with “no-set” and “match-set” trials.

Emotional stimuli.

Emotional images. Pictures from the IAPS were selected for inclusion in the affective flexibility task and the training/control tasks based on valence ratings (Lang et al., 2008) as well as ease of categorization. Positive pictures had a valence rating between 8 and 6 on a scale ranging from 1 = most negative to 9 = most positive. Negative pictures had a mean valence rating between 2 and 4 based on the same 9-point scale. These areas of affective space were selected such that the images were emotionally evocative yet the emotional content should not have negatively affect task performance. IAPS images were also chosen based on accuracy according to pilot data (Malooly, Genet, & Siemer, 2013).

The training and control tasks utilized a subset of those images used in the affective flexibility task, so as to limit practice effects with particular pictures. This was done, rather than selecting new images, to ensure that the images maintained the same ease of categorization as the non-training task.

Emotional words. Words used in the working-memory manipulation task were drawn from the Affective Norms of English Words list (ANEW; Bradley & Lang, 1999). The ANEW list described the valence ratings for each word based on a 9 point-scale, with 1 = most negative to 9 = most positive. Words were selected for task inclusion based on valence ratings. Positive words had ratings ranging from 7-9 and negative words had ratings from 1-3.

Emotional faces. Images of emotional faces to be used in the n-back task were drawn from the NimStim Face Set (Tottenham et al., 2009). Images were 138 full color photos of faces including 46 sad faces, 46 happy faces, and 46 neutral faces from 23 models. Half of the photos depicted facial expressions with an open mouth and half with a closed mouth. Further, half of the models were male and the other half were female. The gender of the faces was controlled across blocks such that participants only categorized faces from one gender per block. The order of the blocks was also counterbalanced across participants, such that half of participants viewed the female block first and the other half viewed the male block first.

Film clips. During the reappraisal-ability challenge task, participants were shown a short film clip from one of two commercially available movies: *The Champ* (Zeffirelli, 1979) or *Return to Me* (Tugend & Hunt, 2000). The film clip from *The Champ* showed a young boy crying as his father, a boxer, lay on a table dying. The clip from *Return to Me* showed a man grieving the recent loss of his wife. Both film clips have been shown to reliably elicit sadness in previous studies (e.g., Gross & Levenson, 1995). Prior to viewing a negative film clip, participants were shown a neutral film clip from *Coral Sea Dreaming* (Hannon, 1999) with instructions to view the film clip without regulation of emotions in order to establish baseline mood.

Procedure

Baseline visit. Participants completed the baseline protocol one at a time. Upon arrival at the lab, participants signed an informed consent form, completed demographics forms, and completed measures of depression and anxiety symptomatology. After completing the questionnaires, participants were seated inside a soundproof room with a computer. The experiment began with the participant completing the affective flexibility task, then the emotional regulation task. Each participant completed the above tasks in the same order; however, versions of each task were counterbalanced across participants.

Next, participants completed the two additional measures of affective processing (Emotional Working-Memory and Emotional N-Back). The order of these tasks was counterbalanced across participants. The self-report questionnaires were administered on paper, whereas the affective flexibility, emotion regulation, and affective processing tasks were presented on a 19” computer monitor running E-Prime 2.0 software. Participants were seated approximately one foot from the monitor. At the end of the session, participants were assigned a participant number to use during training sessions over the next two weeks.

Training sessions. Over the course of the two weeks following the baseline session, participants completed four or five training or control sessions. Half of participants were randomized to the active training condition, in which participants were expected to become more efficient with the switches that had been associated with improved affect regulation. The other half were assigned to the control condition, in which participants were trained to become more efficient in the switches which had appeared unrelated to emotional functioning. Participants were blind to assignment to either condition.

Training sessions began with a baseline mood rating using PANAS items presented on a computer screen. Participants were also asked to report on relevant variables such as sleepiness and confusion. Next, participants completed their assigned version of the affective flexibility training task (active training or control). Participants completed the training task in the laboratory during times scheduled with a research assistant. The tasks were presented on a 19” computer monitor running E-Prime 2.0 software. Participants were seated approximately 1.5 feet from the monitor.

Post-training session. As soon as one day after completing the last training or control session, participants completed the post-training assessment (mean time to follow-up = 3.25 days, median = 2 days, SD = 3.13). As in the baseline session, participants were seated inside of a soundproof room and completed a series of tasks on a computer. First, participants completed current CES-D and STAI measures to assess changes in depressed and anxious symptomatology. Next, participants completed the affective flexibility task and the reappraisal challenge task. Following completion of these tasks, participants completed the two transfer measures of emotional processing (Emotional Working-Memory and Emotional N-Back). The order of the tasks was again counterbalanced across participants. As before, the self-report questionnaires were administered on paper and the remaining tasks were presented on a 19” computer monitor running E-Prime 2.0 software. Again, participants were seated approximately 1 foot from the monitor.

Chapter 3

Results

Preliminary Analyses

Data Preparation and Screening. Items within each self-report measure were summed to create scale scores. The scale scores were then examined to assess for the presence of outliers. No participants were identified with outlying scores on either the CES-D (Baseline: M = 10.22, SD = 6.91; Follow-up: M = 10.20, SD = 6.42) or STAI (Baseline: M = 36.24, SD = 8.94; Follow-up: M = 34.30, SD = 8.00).

Participants' data from the affective flexibility task were examined in terms of accuracy and reaction time outliers. Only data from the experimental blocks of the tasks were examined. No participants performed worse than chance (accuracy < 50%) on the baseline or follow-up affective flexibility task. Reaction times (RTs) for inaccurate trials on the affective flexibility task were replaced with a missing value such that calculation of switching costs was based only on those trials in which the participant answered correctly.

RT data were also examined and cleaned to reduce the influence of outliers. The mean and standard deviation of reaction time was calculated for each participant for each task. These values were used to set individual RT windows for each task. RT values for a participant which were more than 2.5 standard deviations above that participant's mean were replaced with the upper limit of the RT window. This approach has been used in similar studies involving reaction time data (e.g., Greenwald, Nosek, & Banaji, 2003). RTs less than 100ms were excluded from analyses (Luce, 1986).

Data cleaning and preparation for the transfer tasks, Emotional Working-Memory Manipulation and Emotional N-Back tasks, resembled the procedures outlined above. Data were first screened for accuracy. Reaction time data for trials in which an incorrect response was logged were removed from analyses by replacement with a missing value. Reaction time windows were also calculated for each participant such that trials in a given task in which the RT was 2.5 standard deviations above the participant's mean were replaced with the upper limit of the RT window. Again, RTs less than 100ms were excluded from analyses (Luce, 1986).

One participant performed worse than chance (accuracy < 50%) on the Emotional N-Back task at baseline and at follow-up, two others performed worse than chance on the Emotional N-Back at follow-up only, and another participant performed worse than chance (accuracy < 33.33%) on the Working-Memory Manipulation task at follow-up. These data were eliminated from analyses. All other participants performed at chance level or better.

Participant's mood rating data immediately following the neutral film clip and the reappraisal task were examined to create a sadness scale. Scores on the four mood items "sad," "down," "depressed," and, "upset" were summed to a factor representing sadness. The items created a reliable scale, with Cronbach's alpha = .89 at follow-up and Cronbach's alpha = .90 at baseline. A difference score was also calculated by subtracting baseline sad mood following the neutral film clip from sad mood at the end of the reappraisal task. The purpose of the difference score was to control for sad affect present prior to the reappraisal manipulation. Participants' sad mood scale summed scores and difference scores were also examined to look for outliers. One outlier with a high sadness score was flagged in analyses; however, the inclusion or exclusion of this individual did not materially affect results. Thus, this individual's data were retained in analyses.

Data from the Emotion Regulation Follow-Up Questionnaire were also examined to confirm that participants indeed used reappraisal as instructed. Open ended responses to a question regarding strategies used during the task were coded by the author. Two participants whose open-ended responses indicated that they did not regulate their emotions during the emotion regulation task at baseline were flagged in analyses. The inclusion or exclusion of these participants did not materially change results, so the data were retained in analyses. All participants' responses indicated use of reappraisal at follow-up.

Calculation of Affective Flexibility Switch Costs.

General switch costs. Following data screening, each participant's average reaction time for switch trials and repetition trials on the affective flexibility task were computed. The reaction times of all switch trials during which a correct response was logged were averaged to create a mean switch RT. The above procedure was repeated for repetition trials, such that all reaction times for correct repetition trials were averaged to create a mean repetition RT. To establish the presence of switch costs, a paired-samples *t*-test was conducted to compare mean reaction times on switch trials with mean reaction times on repetition trials. Results of the *t*-test indeed showed that RTs for switching trials were longer than RTs for repetition trials on the affective flexibility task at baseline, $t(40) = 9.91$, $p < .001$, and at follow-up, $t(48) = 6.98$, $p < .001$. Once switching costs were established, they were calculated by subtracting the mean RT for repetition trials from the mean RT for switching trials.

Specific switch costs. Specific switching costs, taking into account the rule being switched to as well as the valence of the target image, were also established. Similar to the procedure described above, the RTs of repetition trials in which the rule stays the same were subtracted from the RTs of switching trials in which the rule changes toward a particular rule. For example, to calculate the specific switching costs associated with redirecting attention toward a non-affective task set with a negative target stimulus, the RTs for all trials in which the participant must switch from the affective rule to the non-affective rule while categorizing a negative image were averaged. To determine the appropriate corresponding repetition trial RT, the RTs for all trials in which the non-affective rule is repeated and a negative image is categorized were averaged.

A total of four different types of specific switches were examined. Attentional switches toward a non-affective task set with a negative target were already described and were a trial type of interest in the affective flexibility training task. The other trial type trained was those switch trials in which attention must be redirected toward processing the emotional valence of a positive image. These trials were compared with trials in which the affective rule is repeated and the current target image was positive. The two other switch costs were trained on the control task and have not been associated with emotion regulation in prior studies. The first type of switch involves the redirecting of attention toward the affective task set with a negative target image, which was compared with trials in which the affective rule is repeated with a negative target image. The final type was those switch trials in which the participant must switch toward processing a positive image using a non-affective task set, which was contrasted with trials in which the participant must categorize a positive image while repeating the non-affective rule.

Paired samples *t*-tests were used to establish the presence of these switch costs. For the baseline affective flexibility task, all of the t-tests were significant, $p < .001$, with the exception of trials involving switching toward the human rule with a negative image, $t(39) = 1.26$, $p = .22$. On the follow-up affective flexibility task, every *t*-test was significant, $p < .001$. Switch costs themselves were then calculated by subtracting the corresponding mean repetition RT from the mean switch RT.

Speed-accuracy trade-off. It is possible that switch costs on the affective flexibility task could be due to something other than set shifting, namely, a speed-accuracy trade-off. This would occur if participants exhibited slower RTs on switch trials compared with repetition trials (shown above), and were more accurate on switch trials compared with repetition trials, suggesting that as speed decreases, accuracy increases, and vice versa. To rule out this possibility, paired samples *t*-tests were conducted comparing participants' average accuracy during switch trials compared with repetition trials. Participants' baseline (mean difference = -.02, $t(40) = -4.40$, $p < .001$) and follow-up (mean difference = -.02, $t(48) = -4.27$, $p < .001$) affective flexibility task data showed that participants' accuracy on switch trials was actually worse than their performance on repetition trials.

Transfer Tasks.

Emotional working-memory manipulation. For this task, *sorting costs* were calculated, which represent the additional time required for sorting emotional material in working memory outside of pure maintenance. These costs were calculated by subtracting response latencies on forward trials within one emotional valence from their corresponding backward trials. For example, to calculate sorting costs for positive words, RTs for trials in which the participant must name the location of a positive word forward were subtracted from those trials in which the participant must identify the location of a positive word backwards. The same procedure was repeated to calculate sorting costs for negative words.

Emotional n-back. Four different kinds of trials were calculated for the 2-back task based on RTs. As described before, trials could have been No-set trials, in which there was no match in the previous trial or in the current trial. Match-set trials were those trials when the current face matches the face presented two trials ago. The next two trials followed a match-set trial. Break-set trials required participants to break a previously matched pair, to remove the face 3 trials back from working memory and to update working memory with a new face. Perseveration-set trials occurred when the current trial matched a previously identified set. They required the participant to break the previous set and to avoid perseverating, all while adding a new stimulus to working memory which matched the one being removed. Reaction times for all correct trials within these four categories were averaged to create mean RTs for each trial type.

Main Analyses

Hypothesis 1: Training on the AF task will selectively change specific switching costs, such that participants in the training group will perform more efficiently on trials which have been trained. To examine this hypothesis, a two (gender) by two (group) by two (time) by two (rule) by two (picture valence) mixed ANOVA was run to compare participants' switch costs on the four different kinds of trials, before and after training. I expected to find a significant group x time x rule interaction, indicating that participants in the two training groups showed improvements on their respectively trained switches from baseline to follow-up. The sample size for this analysis included only 28 participants with complete data. This *n* resulted from elimination of 11 participants whose baseline affective flexibility data was lost due to a recording error, one other participant whose affective flexibility task data at follow-up was missing, and four other participants who did not have data on a specific switching cost due to inaccurate performance.

Results of the five-way ANOVA indicated no significant main effect of gender, $p = .91$. Although the interaction of gender and rule approached significance, $p = .07$, there were no significant interactions of gender with any study variables, *p*-values > .10. (Post-hoc power analyses for gender and interactions with gender indicated low power for these analyses.) There was also no significant main effect of group, $p = .34$, nor any significant interactions of group with other study variables, all *p*-values > .25. Observed power for these analyses was also low. Refer to Table 8 for full ANOVA results. Results did, however, indicate significant main effects of time, $F(1, 24) = 9.96$, $p < .01$, and rule, $F(1, 24) = 19.15$, $p = .001$, as well as significant interactions between time and rule, $F(1, 24) = 13.13$, $p = .001$, and between rule and valence, $F(1, 24) = 24.73$, $p < .001$. However, the ANOVA results are best understood by examining the three-way interaction among time, rule and valence, $F(1, 24) = 4.23$, $p = .05$.

Results of follow-up time (2) by valence (2) ANOVAs for the two different sorting rules indicated a significant time by valence interaction for the emotion sorting rule, $F(1, 27) = 9.84$, $p < .01$, partial $\eta^2 = .27$, observed power = .86. This interaction was followed-up with paired samples t-tests examining changes in switch costs from baseline to follow-up for both positive and negative images sorted using the emotional rule. Results revealed that although participants improved on emotion-rule switch costs for both positive, $t(29) = 3.30$, $p < .01$, and negative images, $t(30) = 4.18$, $p < .001$, the degree of improvement was significantly greater for trials involving a negative target image (mean difference = 180.88, SD = 214.12, 95% CI = 92.44 – 269.32) compared with trials involving a positive target image (mean difference = 102.55, SD = 185.44, 95% CI = 33.09 – 171.80). This interaction is depicted in Figure 1.

There was no significant main effect of time, $F(1, 31) = .60$, $p = .44$, partial $\eta^2 = .02$, observed power = .12, nor of image valence, $F(1, 31) = 1.46$, $p = .24$, partial $\eta^2 = .05$, observed power = .22 for the human sorting rule. There was also no significant interaction between time and image valence, $F(1, 31) < .01$, $p = .97$, partial $\eta^2 < .01$, observed power = .05 for the human sorting rule.

Due to the large amount of missing baseline AF task data, a two (gender) by two (group) by two (sorting rule) by two (image valence) repeated measures ANOVA was also run examining the follow-up data exclusively. I anticipated that if AF training were successful, I would find a significant group by switch type interaction, indicating that the training group would exhibit lower switch costs than the control group on the two switch types trained (emotion rule, positive image; non-emotional rule, negative image). The *n* for this analysis comprised 41 participants. This *n* reflected removal of one participant whose affective flexibility task data at follow-up were missing, and two other participants who did not have data on a specific switching cost due to inaccurate performance.

Results indicated no significant main effects of gender, $F(1, 37) = .46$, $p = .50$, group, $F(1, 37) = .01$, $p = .94$, sorting rule, $F(1, 37) = 2.52$, $p = .12$, nor image valence, $F(1, 37) = .09$, $p = .76$. There was a significant interaction between sorting rule and valence, $F(1, 37) = 5.08$, $p = .03$, which is depicted in Figure 2. This interaction was followed up with two paired-samples *t*-tests examining differences in switch costs for image valence at each sorting rule. Results indicated no significant differences between switch costs for positive (m = 76.52, SD = 148.15) and negative (m = 44.74, SD = 161.68) images when images were sorted using the human rule, $t(42) = .86$, $p = .40$. For the emotion rule, on the other hand, there was a near significant difference between switch costs for positive (m = 93.16, SD = 180.24) and negative (m = 150.03, SD = 156.09) images, $t\ (40) = -1.83$, $p = .08$.

Hypothesis 2: Following AF training, participants in the training group will be better able to down-regulate negative emotions using cognitive reappraisal. This hypothesis was examined using a two (gender) by two (group) by two(time) repeated measures ANOVA on self-reported sadness. I expected to see a significant interaction such that both groups' sadness ratings looked similar at the baseline session, but the trained participants exhibited lower self-reported sadness at follow-up compared with the control group. Only those participants whose memory for the film clip was greater than chance, as assessed by 3 "yes" or "no" questions about the film, were included in the analyses. This criterion eliminated 6 participants at the baseline session and 3 participants at the follow-up session; one additional participant was removed due to missing emotion regulation task data at follow-up, leaving a total of 34 participants.

An analysis examining sad mood following the sad film clip as the dependent variable found no significant main effects of gender, $F(1, 30) = 2.00$, $p = .17$, partial $\eta^2 = .06$, observed power = .28, time, $F(1, 30) = .04$, $p = .84$, partial $\eta^2 < .01$, observed power = .05, or training group $F(1, 30) = .20$, $p = .66$, partial $\eta^2 = .01$, observed power = .07. There were also no significant two-way interactions of group by time $F(1, 30) = .10$, $p = .75$, partial $\eta^2 < .01$, observed power = .06, of gender by time, $F(1, 30) = .94$, $p = .34$, partial $\eta^2 = .03$, observed power = .16, nor of group by gender, $F(1, 30) = 1.20$, $p = .28$, partial $\eta^2 = .04$, observed power = .19. The three way interaction of gender, group, and time was also non-significant, $F(1, 30) = .03$, $p = .86$, partial $\eta^2 < .01$, observed power = .05. Means and standard errors for sad mood broken down by gender, group, and time are presented in Table 9.

Additionally, an analysis examining sad mood, controlling for baseline sad mood, similarly had no significant main effects of gender, $F(1, 30) = 1.58$, $p = .22$, partial $\eta^2 = .05$, observed power = ..23, training group $F(1, 30) = .62$, $p = .44$, partial $\eta^2 = .02$, observed power = .12, or time, $F(1, 30) = .44$, $p = .51$, partial $\eta^2 = .01$, observed power = .10. There were also no significant interactions between gender and group, $F(1, 30) = 1.87$, $p = .18$, partial $\eta^2 = .06$, observed power = .26, between time and gender, $F(1, 30) = .14$, $p = .71$, partial $\eta^2 = .01$, observed power = .07, or between time and group, $F(1, 30) = .02$, $p = .67$, partial $\eta^2 = .01$, observed power = .07. No significant three-way interaction among gender, group, and time was observed, $F(1, 30) = .18$, $p = .67$, partial $\eta^2 = .01$, observed power = .07. Means and standard errors for sad mood difference scores are presented in Table 10.

I was interested in whether the degree of improvement as a result of training may have had an effect on improvement in emotion regulation. This was investigated with a mediated regression analysis with dummy-coded group (training or control) entered as the independent variable, AF switch cost change scores (baseline – follow-up) entered as the mediator, and emotion regulation improvement (self-reported sadness ratings at baseline – follow-up) entered as the dependent variable. Of the 34 participants included in the above ANOVA, an additional seven participants were missing data for one or more of the AF task switch costs. This resulted in a final *n* of 27 participants.

I anticipated a significant positive association between group and emotion regulation efficacy, such that those in the training group would exhibit a greater change in self-reported sadness from baseline to follow-up compared with the control group. I also expected a significant positive association between training group and AF change scores such that the training group would exhibit greater gains in the trained switches (toward switches toward the human rule involving negative images and switches toward the affective rule involving positive images) than those in the control group. Furthermore, I anticipated that those who improved the most on the AF training intervention were also the most improved in terms of reappraisal efficacy. Finally, I expected that the indirect pathway from training group to reappraisal efficacy via changes in beneficial AF would at least partially explain the relationship between training group status and reappraisal efficacy.

Results did not provide support for these predictions. The regression of sad mood change scores onto dummy-coded training group was not significant (p = .90). When switch costs toward the affective rule for negative images were regressed onto dummy-coded training group, the effect was not significant, $F(1, 23) = 2.35$, $p = .14$, and no regressions of switch costs onto training group were significant ($p > .40$). Furthermore, there were no significant regressions of sad mood change scores onto switch costs ($p > .54$). As such, the remainder of the mediation analysis was aborted.

Correlations were also run to examine relationships among sad mood following the emotion regulation paradigm and switching costs from the affective flexibility task (see table 7). It is notable that there were no significant correlations between switch costs and sadness measures at baseline; however, there were some significant correlations at follow-up. This lack of correlations at the baseline session is different from what has been observed in previous research (i.e., Malooly, Genet, & Siemer, 2013) and may offer some explanation as to why none of the above results were significant.

Hypothesis 3: AF training will reduce symptoms of depression and anxiety in a healthy sample. To investigate whether AF training had a beneficial effect on symptoms of depression and anxiety, two separate two (gender) by two (group) by two (time) repeated measure ANOVAs were conducted – one with CES-D total score as the dependent variable, and another with STAI total score as the dependent variable. I expected to see a significant group x time interaction such that, in the training group, total scores on the CES-D and STAI decreased from baseline to follow-up, whereas the control group exhibited no change. The *n* for the analysis examining change in the CES-D included 42 total participants. This *n* was the result of removal of one participant who had missing data on the CES-D at baseline, and another with missing follow-up CES-D data. The total *n* for the analysis examining change in STAI score from baseline to follow-up was 44 participants.

Results of the ANOVA examining the CES-D were all non-significant, contrary to study hypotheses. See Table 11 for detailed results from this analysis. Results of the second ANOVA, which examined changes in the STAI from baseline to follow-up between groups, were more consistent with study hypotheses. Although there were no significant main effects of gender or group, nor any significant interactions, there was a significant main effect of time, $F(1, 40) = 18.36$, $p < .001$. Results indicate that participants' scores on the STAI were reduced from baseline (m = 36.95 SE = 1.67) to follow-up (m = 33.60, SE = 1.46). Refer to Table 12 for full ANOVA results.

Hypothesis 4: The effects of training on the AF task will generalize to other measures of affective-control processing. To examine if the effects of AF training would transfer to other tasks (emotional working memory manipulation, emotional n-back), a series of repeated measures ANOVAs were run examining changes in study variables from baseline to follow-up. For the emotional working-memory manipulation task, a two (gender) by two (group) by two (time) by two (word valence) ANOVA was run comparing sorting costs for positive and negative words at baseline and at follow up between the training and control groups. I expected that sorting costs for both positive and negative words would decrease in the training group, but would remain the same in the control group. The final *n* for analyses examining the emotional working memory task was 34. This *n* was due to exclusion of one participant whose follow-up n-back data were missing, and nine participants whose baseline working memory data were missing due to a recording error.

Results of the repeated measures ANOVA (see Table 13 for full results) indicated a significant main effect of time, $F(1, 30) = 6.52$, $p < .05$, partial $\eta^2 = .18$, observed power = .70. Sorting costs for both positive and negative words were significantly faster at follow-up (positive words: *M* = 234.51, *SEM* = 40.54; negative words: *M* = 256.08, *SEM* = 49.31) compared with baseline (positive words: *M* = 366.82, *SEM* = 75.12; negative words: *M* = 378.13, *SEM* = 47.00).

There was also a marginally significant gender x group x time interaction, $F(1, 30) = 3.29$, $p = .08$, partial $\eta^2 = .10$, observed power = .42. Follow-up two (group) by two (time) by two (word valence) way ANOVAs were run separately for each training group, to further examine this marginally significant interaction. Results of these ANOVAs found a marginally significant main effect of time in the control condition, $F(1, 14) = 3.05$, $p = .10$, partial $\eta^2 = .18$, observed power = .37, and a significant time by gender interaction in the training condition, $F(1, 16) = 6.47$, $p < .05$, partial $\eta^2 = .29$, observed power = .42. This interaction is depicted in Figure 3. The time x gender interaction was followed up with paired samples *t*-tests examining sorting costs at baseline and follow-up in the training group for males vs. females. Results indicated that for males in the training group, a significant reduction in sorting costs took place from the baseline to the follow-up session, (mean difference = 241.80, *sd* = 221.30, $t(8) = 3.28$, $p < .05$). For females in the training group, no significant change in sorting costs occurred, (mean difference = -37.37, *sd* = 243.78, $t(8) = -.46$, $p = .66$).

To examine changes on break-set and perseveration-set trials in the emotional n-back task, a two (gender) by two (group) by two (time) by two (trial type) way ANOVA was run. I anticipated that the training group would exhibit a significant decrease in both types of trials from baseline to follow-up, but that the control group would not show a change in scores. The *n* for these analyses was 31, which reflected elimination of one participant whose data from the follow-up session task were missing, and 12 participants whose data from the baseline session were lost due to a recording error.

Full results of the repeated measures ANOVA are presented in Table 14. Results indicated a significant main effect of time, $F(1, 27) = 4.29$, $p = .05$, partial $\eta^2 = .14$, observed power = .51, indicating that across trial types, reaction times decreased from the baseline visit (M = 1249.86, SE = 43.04) to the follow-up visit (M = 1171.70, SE = 50.26). There was also a significant main effect of trial type, $F(1, 27) = 12.86$, $p = .001$, partial $\eta^2 = .32$, observed power = .93, showing that reaction times for perseveration-set trials (M = 1264.89, SE = 48.89) were longer than reaction times for break-set trials (M = 1156.66, SE = 41.59). Although some interactions were trending toward significance (time by gender: $F(1, 27) = 2.57$, $p = .12$, partial $\eta^2 = .09$, observed power = .34; time by training group, $F(1, 27) = 2.81$, $p = .11$, partial $\eta^2 = .09$, observed power = .37), no significant interactions were found.

Chapter 4

Discussion

The primary goal of the current study was to examine the efficacy of a new affective-control training paradigm, the affective flexibility training task, in order to determine if affective flexibility may be trained. This was accomplished by creating an emotional sorting paradigm with different trial contingencies. For the training group, trials associated in the past with improved regulation of sadness and reduced symptoms of depression were over-represented (Malooly & Siemer, 2012; Malooly, Genet, & Siemer, 2013). A control group was used in which the sorting task over-represented trials not associated with emotion regulation or depression in previous studies. I anticipated that the training group would show improvements in switches associated with emotion regulation and depression, whereas the control group would not.

Additional study aims included examination of the effect of this task on regulation of sadness, and symptoms of depression and anxiety. I hypothesized that being assigned to the active training group would be associated with improved regulation of sadness, and reduced symptoms of depression and anxiety at study completion; whereas the control group would show no changes on these measures.

The study also sought to examine if effects of training on the affective flexibility task may generalize to other measures of emotional processing. I anticipated that participants assigned to the training group would exhibit improved performance on measures of emotional working memory at follow-up, whereas participants assigned to the control group may show changes to a lesser degree, or show no changes. Finally, based on findings that women experience their emotions more intensely and regulate them more effortfully than men, the role of gender was examined related to all study hypotheses.

Results of the current study indicated no differences between the active affective-control training group and the control group. Although surprising, such findings are consistent with some recent research finding reductions in symptoms across time, but no significant group x time interactions for active vs. control ABM training for depression (e.g., Beevers, Clasen, Enock, & Schnyer, 2015) and anxiety (e.g., Carlbring, Apelstrand, Sehlin, Amir, Rousseau, Hofmann, & Andersson, 2012). Beevers and colleagues (2015) proposed that both negative attentional bias and deficits in general attentional control maintain depression, which may explain why the control group and active training groups in their study improved similarly on symptoms of depression.

Similarly, Enock and colleagues assigned socially anxious participants to either an active or placebo ABM training paradigm and found that both groups improved to a statistically similar degree (Enock, Hofman, & McNally, 2014). These results indicated that the ABM training, rather than the specific contingency training in the active ABM paradigm, was likely the active ingredient leading to improvements in social anxiety. Indeed, it seems to be the case that regardless of specific switches trained, both groups in the current study exhibited similar gains in switch costs involving switches toward the affective rule when categorizing positive and negative pictures, suggesting some improved affective flexibility.

It may be the case that pre-existing differences among participants may have contributed to efficacy of training. Britton and colleagues (2015) examined healthy adults with social anxiety symptoms who received either active or placebo ABM. Results of the study indicated that regardless of group assignment, participants who came into the study with greater left amygdala activation in response to threat exhibited greater reduction in social anxiety symptoms. However, once baseline amygdala activation was controlled for, a significant difference did emerge, indicating greater improvement in the active ABM group compared with the control group.

Another group examined how emotional reactivity affected the efficacy of cognitive control training for depression (Vanderhasselt, De Raedt, Namur, Valiengo, Lotufo, Bensenor, et al., 2016). Results indicated that pre-existing differences in negative emotional reactivity predicted response to the cognitive control training intervention, such that participants with higher negative emotional reactivity exhibited greater reductions in symptoms of depression as a result of the intervention.

Therefore, it is important to consider the possibility that individual differences in emotional reactivity, affective recognition, and anxiety sensitivity, for example, may have similarly influenced the effects of training on the affective flexibility training task. Further analyses could attempt to control for individual differences in anxiety sensitivity by using STAI scores as a covariate in analyses. A future study may also begin to untangle this relationship by including a baseline measure of emotional reactivity, such as a negative film clip with instructions to view without attempts to regulate emotions. It may also be useful to include a measure of facial and affective recognition to control for individual differences in affect recognition. Further examination with participants experiencing a range of emotional reactivity and anxiety sensitivity would also be needed to examine this idea, as this study purposefully focused on healthy controls who were experiencing relatively normative levels of depression and anxiety symptomatology.

Furthermore, the possible influence of effort on study results should also be considered. A recent study has found that physiological indicators of effort were predictive of response to cognitive control training (Siegle, Price, Jones, Ghinassi, Painter, & Thase, 2014). Those authors examined depressed participants receiving intensive outpatient level-of-care, who were assigned to receive either CCT or treatment as usual. The results indicated that among the participants assigned to the CCT paradigm, those who exhibited increased pupil dilation during the first session, indicating that they were more engaged in the intervention, exhibited a greater reduction in rumination in response to the intervention. Furthermore, those participants who completed CCT and exhibited physiological indicators of task engagement were also less likely to return to intensive outpatient level-of-care during the following year.

Although the current study did not specifically include measures which may have indexed level of effort, this may have been useful given that 11 study completers met criteria for mild depression, and effort was a concern on the emotion regulation task based on accuracy in responding to simple “yes/no” questions regarding a film clip. Future studies could include physiological measures such as pupil dilation, or use eye tracking to better assess level of effort.

It was also surprising that of the switches which were associated with reappraisal efficacy, switches toward the human rule in the presence of a negative image (Malooly, Genet, & Siemer, 2013), and switches toward the affect rule while categorizing a positive image (Genet, Malooly, & Siemer, 2013), only switches toward the affect rule with a positive image improved as a result of training. Furthermore, it was surprising that switches toward the affect rule while categorizing a negative image also improved. Based on descriptive statistics, it is also interesting that this specific switch cost was significantly higher than the other switch costs to begin with, and improved to a level more comparable with the other switch costs at follow up.

The data indicate that across time, switch trials involving a negative image take longer, and are presumably more challenging, than switch trials involving a positive image. It may be the case that the negative image itself is evoking an emotional response, which makes the rule switch, regardless of the rule being switched toward, more difficult.

The data also indicate that for repetition trials, trials involving repetitions of the human rule are slower and again presumably more difficult than trials involving repetitions of the affect rule. This may make sense, as we may be more accustomed to categorizing emotional stimuli by valence, and less so by non-emotional rules. As such, trials involving repetition of the human rule should not be as quick as repetitions of the affect rule, since participants are using a more novel mental set. The combination of a longer reaction time for categorizing negative images, and a quicker reaction time for categorizing images based on the emotion rule, combine to create the largest switch costs for trials involving the switch toward the emotional rule when categorizing a negative image. Since this switch cost is the largest, it also makes sense that it has the most room for improvement with training.

It was also surprising to find that self-report of emotion regulation efficacy did not significantly improve as a result of training. This may have been a result of the self-report nature of the measure used in this study. A recent meta-analysis found that while attention-bias modification training resulted in significant improvement on clinician-rated anxiety, improvements were not significant for patient-rated symptoms (Linetzky, Pergamin-Hight, Pine, & Bar-Haim, 2015). Thus, any improvement in emotion-regulation efficacy may have been undetectable by study participants.

It may also be the case that training on the AF task was insufficient to substantially impact emotion regulation efficacy. Indeed, while there is no consensus on the appropriate dose of CCT or ABM needed for observable effects, many CCT interventions use 10 - 12 days of cognitive training (e.g., Hoorelbeke, Koster, Demeyer, Loeys, & Vanderhasselt, 2016; Siegle, Ghinassi, & Thase, 2007) whereas ABM training studies range from one session (e.g., Amir et al., 2008) to twice weekly over four weeks (e.g., Britton, et al., 2015) or more. An intervention using training on an emotional n-back task required participants to complete 18-20, 20-minute sessions (Schweizer, et al., 2013). Thus, it is unclear if the dose of AF training used here was sufficient to create training effects, and indeed, the lack of group differences and improvement on only one switch cost from baseline to follow-up suggests that participants may not have received enough training to reap benefits.

Furthermore, the majority of the cited studies used clinically anxious or depressed samples (e.g. Amir, et al., 2008; Siegle, Ghinassi, & Thase, 2007), healthy samples with high anxiety (e.g. Britton, et al., 2015), or unrestricted community samples (e.g. Schweizer, et al., 2013). These types of participants may have greater room for improvement with training than a healthy sample with no more than mild psychopathology, as used in this study.

It is also important to note that while AF task performance and emotion regulation task performance independently appeared normal at the baseline session, these tasks did not correlate as expected based on prior studies (e.g. Genet, Malooly, & Siemer, 2013;

Malooly, Genet, & Siemer, 2013). The tasks did; however, correlate in a more expected way when examining the follow-up data. It is unclear why AF switch costs and sadness scores on the emotion regulation task did not correlate at baseline. It is possible that limiting the study participants to healthy controls with no more than minimal depressive symptoms may have limited correlations among these measures. In fact, in previous studies, participants with a wider range of depressive symptoms were allowed to be included in the studies (Genet, Malooly, & Siemer, 2013; Malooly, Genet, & Siemer, 2013). Anecdotally, participants in this study also seemed well-versed in cognitive reappraisal as an emotion regulation strategy, with no participants being excluded for using a different strategy. It may be the case that these healthy controls were already quite proficient in their use of cognitive reappraisal and, as such, this may have created a ceiling effect with regards to training effects.

Furthermore, although some studies of healthy controls have found evidence of specific training effects for ABM on emotional responding (e.g. MacLeod et al., 2002), it may be the case that the effects are short-lasting for healthy controls, as participants in MacLeod and colleagues' study and in others completed both ABM and an emotion regulation task within one session. In this study, on the other hand, participants were unable to complete their follow-up visit on the same day as their last training session and waited an average of 2 days before returning to the laboratory to complete the assessment.

Another important consideration is that some studies have found that training itself did not affect emotion regulation ability; but rather, changes as a result of training affected ability to regulate emotions (e.g. Taylor, Bomyea, & Amir, 2011). This is an important point, as participants improved in one switch associated with emotion regulation efficacy, but not on the other. In addition, a recent study of cognitive control training with healthy controls found that although the intervention reduced use of rumination in “low positive affect states,” the intervention did not have an effect on adaptive emotion regulation (Hoorelbeke, Koster, Demeyer, Loeys, & Vanderhasselt, 2016). The authors concluded that cognitive interventions may have a limited role in improving emotional regulation in healthy controls. Finally, since this intervention was only somewhat effective in modifying switch costs associated with emotion regulation, it may make sense that in turn, emotion regulation was not significantly improved.

Results involving changes in symptoms of anxiety as a result of AF training were somewhat more in line with study hypotheses. Again, no group differences were found with regards to changes in symptoms based on inclusion in the training or control group, similar to results found by laboratories using ABM paradigms (e.g. Beevers, Clasen, Enock, & Schnyer, 2015; Carlbring, et al., 2012). Results for anxiety indicated a small, but statistically significant decrease in symptoms of anxiety from baseline to follow-up. These results are fairly consistent with the literature, suggesting a small effect of training on anxiety symptoms (e.g. Amir, et al., 2009; Hallion & Ruscio, 2011). Furthermore, similar to some other studies, it appears that the contingency training manipulation was ineffective despite improvements across groups (e.g. Enock, Hofman, & McNally, 2014). This reduction in self-reported anxiety is unlikely clinically significant, but is promising given the brevity of the intervention. Although a small decrease in anxiety over the course of two weeks could be viewed as an artifact rather than as a result of training, this possibility is unlikely given that participants completed this task at various points throughout the academic year, including beginning or ending the study around midterms, finals, holiday breaks, and graduation. Evidence of a small decrease is thus unlikely solely due to the effects of time or situation. Future studies may wish to examine if a training effect would remain or even become more prominent in a clinically anxious sample compared with the reduction found with healthy controls.

In contrast to anxiety, there was no significant relationship between study involvement and symptoms of depression. It is unclear why completing the study was associated with reduced symptoms of anxiety, but not with reduced symptoms of depression. It may be the case that the AF task is more closely linked to symptoms involving high arousal, which is characteristic of anxiety, rather than more cognitive and low-arousal symptoms of depression. Although a previous study found that performance on certain AF task switch costs was associated with depression (Malooly & Siemer, 2012), it is again important to note that these were not the switch costs which significantly improved as a result of training. As such, there may not have been a high enough “dose” of training provided to modify these switch costs. Furthermore, the above study did not restrict participants based on level of depression, as was done in the present study, which may have created more range in depression to establish correlations.

There is also inconsistency in the literature as to whether computerized interventions are helpful in the case of depression. Some studies have found positive effects of CCT on symptoms of depression (e.g. Siegle, Ghinassi, & Thase, 2007; Siegle, Price, Jones, Ghinassi, Painter, & Thase, 2014), and rumination (Hoorelbeke, Koster, Vanderhasselt, Callewaert, & Demeyer, 2015), but research is limited in this area and positive effects have not been substantiated for more than a few months. Results regarding effects of ABM training on depression have been inconsistent (Beevers, Clasen, Enock, & Schnyer, 2015; Hallion & Ruscio, 2011; Wells & Beevers, 2010). Again, these studies specifically selected participants who were either experiencing moderate-severe depression (e.g. Beevers, Clasen, Enock, & Schnyer, 2015; Siegle, Ghinassi, & Thase, 2007; Siegle, Price, Jones, Ghinassi, Painter, & Thase, 2014) or high-trait ruminators (Hoorelbeke, Koster, Vanderhasselt, Callewaert, & Demeyer, 2015), who would have more room to improve as a result of training interventions. Since participants in this study were healthy participants with no more than mild symptoms of depression, there may have been little room for improvement in these participants' symptoms. Future studies should examine if AF training may have any effect on symptoms of depression in a sample with moderate or severe levels of depression, as used in other studies.

Findings regarding transfer of affective flexibility training to other measures of affective control were also promising. Similar to studies of cognitive control training finding transfer effects to measures of working memory (Hoorelbeke, Koster, Vanderhasselt, Callewaert, & Demeyer, 2015), attention (Iacoviello, Wu. Alvarez, Huryk, Collins, Murrough, et al., 2014), and global executive control (Preiss, Shatil, Čermáková, Cimermanová, & Ram, 2013), the current study also found improvements in both the training and control groups on measures of emotional working memory following study completion. Although a practice effect could be argued, further analysis of the emotional working memory task data revealed that reduction in sorting costs was due to decreased RTs for backward trials only, while RTs for forward trials stayed consistent from baseline to follow-up. If results were due to a practice effect only, one might expect reductions in both backward and forward trials from baseline to follow-up.

That improvements were specific to backward trials may indicate specificity of increased flexibility as a result of the intervention, rather than simple increase in speed. Interestingly, improvements in sorting of emotional words were present for both positive and negative words, indicating global improvement in emotional working memory rather than improvement specific to processing of negative words.

Results regarding the emotional n-back task, on the other hand, may be due at least in part to a practice effect. Although both break-set and perseveration-set trials showed improvement in RTs from baseline to follow-up, similar improvements in reaction time were found for no-set and match-set trials, which require less demand on emotional working memory. However, given this possibility, it remains unlikely that improvements on this task were due to practice effects alone. Even the “simpler” trials on this task proved quite difficult for participants, as such, improvement across trials types may indeed reflect improved affective flexibility. Additionally, analyses of accuracy did not show significant improvements in accuracy from baseline to follow-up. I would anticipate if a practice effect was driving this relationship, there may be a similar improvement in accuracy as well as RT.

Despite several interesting findings noted above, several limitations of the present study deserve mention. First, the sample size used for this study, particularly when accounting for participants with missing data, was very small and was inadequate to detect more modest training effects. Indeed, the majority of post-hoc power analyses indicated that the study was greatly under-powered. As such, it is best to view the present study as a pilot study of the affective flexibility training task, which requires additional examination with larger samples.

The amount of training received by participants, four sessions over the course of two weeks, may also be a study limitation. As there is no consensus regarding appropriate "dose" of ABM training or CCT, the number of sessions was chosen based on an estimation of the amount of training needed to create change, and feasibility with a healthy college sample. Future studies may wish to increase number of training sessions to determine if the amount of sessions in the current study was sufficient to warrant change in emotion regulation. Furthermore, increasing number of training sessions in future studies would allow for examination of dose-dependent effects of AF training on outcome measures.

An additional study limitation could be the non-adaptive nature of the affective flexibility training task, which may have become too easy for participants over time. Indeed, a recent study from Amir's laboratory has investigated an adaptive ABM training task, in which socially anxious participants were explicitly oriented to the goal of the training (to change attentional bias) and received regular feedback regarding progress toward this goal in terms of advancement to new levels (Amir, Kuckertz, & Strege, 2016). Results of the study indicated that the additional interventions resulted in changed attentional bias, and that the change in attentional bias (rather than number of trials) was associated with reduced anxiety symptoms. Although participants in the current study did receive some feedback at training sessions regarding their accuracy, the feedback was vague (either over or under 90% accurate) and the task did not respond to increasing accuracy by increasing difficulty. Furthermore, participants were not explicitly informed that the goal of the training sessions was to improve flexibility in processing emotional material. Future studies may wish to examine the possibility that participants' knowledge of study goals, and an adaptive training task, may prove more effective in reducing symptoms of anxiety or indicators of emotional responding.

The use of self-reported emotional reporting as a basis for emotion regulation efficacy may have proven to be an additional study limitation. Although this method has been successful in past studies in the laboratory (e.g. Malooly, Genet, & Siemer, 2013), it may not have been sensitive enough to detect changes in emotional responding over the course of two weeks. Future studies may wish to incorporate physiological measures such as heart rate variability, skin conductance, late positive potentials, or imaging measures to obtain a more objective rating of emotional responding at baseline and follow-up. Recording of participants' emotional responding and clinician-rated emotional reactivity may also be useful data. Finally, it could be informative to obtain participants' daily emotional experiences through experience sampling methods, to determine if training may translate to daily emotional experiences rather than laboratory derived measures of emotional responding.

Another study limitation is the lack of a measure assessing participants' level of effort and engagement in the study procedures. This may prove particularly important as some recent research has shown that attentional engagement significantly impacts the outcome of cognitive control training (Siegle, Price, Jones, Ghinassi, Painter, & Thase, 2014). Indeed, although research assistants or the author were present at study sessions, the completion of baseline and follow-up visits inside a sound-proof booth presented difficulty in fully monitoring participants' task engagement. The group format at training sessions also created challenges in successfully monitoring participants' level of engagement. Future studies may wish to incorporate measures of attention and engagement such as pupil dilation to rule out possibility that participant inattention affected the efficacy of the study intervention.

Despite these study limitations, the results of the present study remain an important contribution to the literature regarding the training of cognitive and affective-control processes. This study was the first to examine the efficacy of a new affective control training paradigm – the affective flexibility training task. Furthermore, the study included measures of regulation of negative emotions, depression, and anxiety all within one study, permitting comprehensive examination of the possible effects of this training paradigm. Surprisingly, results indicated that the only switch costs trained on this task were switches involving the affective rule. Furthermore, although training on the affective flexibility task did not affect regulation of negative emotions in healthy participants, the task did have a small, positive impact on a measure of anxiety and other measures of affective control. Furthermore, the contingency of specific switches did not appear to matter with regards to training – rather, the process of sorting the emotional images in general appeared to be the active ingredient associated with effects on anxiety and affective control.

Future studies may want to investigate if increasing the dosage of affective flexibility training, or if changing the proportion of training trials vs. non-training per session, may bring about a difference between the training and control conditions. Furthermore, it could be useful to examine if affective flexibility training may have immediate effects by completing follow-up assessment on the same day, rather than waiting to complete this on another day. Given the association between affective flexibility task training and trait anxiety, future studies may wish to examine the efficacy of the affective flexibility task as an adjunctive treatment for patients with anxiety disorders. It may also be informative to examine the affective flexibility training task with a moderate-severely depressed sample to further investigate any relationship between flexibility training and symptoms. Furthermore, since the task did not appear to affect regulation of negative emotions for healthy controls, it may also be useful to examine if the AF training task may have a beneficial effect on positive emotion regulation, such as up-regulation or savoring of pleasant emotions.

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Table 1. *Demographic characteristics of study completers and study non-completers.*

	Study Completers, n = 44	Non-Completers, n = 11
Age	18.55 (.52)	18.84 (1.98)
Gender		
Females	31 (70.45%)	7 (63.64%)
Males	13 (29.55%)	4 (36.36%)
Race		
White	21 (47.72%)	4 (36.36%)
Hispanic	13 (29.55%)	4 (36.36%)
Black	5 (11.36%)	0
Asian	4 (9.09%)	2 (18.18%)
Other	1 (2.23%)	1 (9.09%)

Characteristics for age presented as mean(standard deviation). Characteristics for gender and race presented as n(percentage).

Table 2. *Means and standard deviations for study variables at baseline session, for study completers vs. non-completers.*

	Study Completers, n = 44	Non-Completers, n = 11
CES-D†	11.00 (7.36)	7.18 (3.49)
STAI-T	37.16 (9.26)	32.55 (6.64)
Reappraisal Sadness	6.60 (2.99)	7.00 (2.35)
AF Task Accuracy	88.30% (11.06)	81.83% (13.12)
AF Mean RT – Switch	1560.69 (318.07)	1557.42 (275.84)
AF Mean RT - Repetition	1403.24 (256.79)	1350.00 (259.83)
AF Overall SC	157.46 (106.79)	207.42 (107.51)
AF SC – Emotion/Pos	165.56 (192.48)	190.82 (212.67)
AF SC – Emotion/Neg	345.47 (234.64)	389.51 (202.58)
AF SC – Human/Pos	104.20 (190.46)	88.90 (163.84)
AF SC – Human/Neg	34.30 (180.59)	31.27 (77.36)
WM Happy Sorting Cost	390.05 (383.05)	320.69 (193.41)
WM Sad Sorting Cost*	407.13 (264.76)	96.49 (448.59)
N-Back Break Set	1202.93 (214.23)	1083.18 (86.02)
N-Back Perseveration Set*	1291.16 (231.05)	1085.91168.39)

* $p < .05$, † $p = .05$

Table 3. *Means and standard deviations for study variables at baseline session and follow-up sessions, for active training vs. control group participants.*

	Training Group		Control Group	
	Baseline	Follow-up	Baseline	Follow-up
CES-D	12.13 (6.95)	11.26 (6.50)	9.70 (7.79)	8.80 (6.34)
STAI-T	38.04 (7.58)	35.04 (7.31)	36.19 (10.92)	33.14 (9.05)
Reappraisal Sadness	6.31 (2.21)	6.56 (3.39)	6.60 (2.74)	4.50 (1.10)
AF SC – Emotion/Pos	216.64 (227.34)	90.24 (128.56)	111.07 (133.59)	99.33 (228.23)
AF SC – Emotion/Neg	378.26 (158.50)	122.56 (145.48)	251.51 (184.59)	164.38 (179.20)
AF SC – Human/Pos	114.11 (224.33)	76.09 (166.02)	77.20 (129.49)	77.01 (128.84)
AF SC – Human/Neg	58.44 (139.36)	46.04 (141.77)	63.13 (162.26)	43.23 (185.77)
WM Happy Sorting Cost	384.71 (238.31)	237.03 (209.32)	395.14 (519.36)	262.50 (213.88)
WM Sad Sorting Cost	371.47 (162.29)	294.46 (269.96)	414.96 (314.62)	245.07 (240.05)
N-Back Break Set	1225.91 (222.10)	1097.60 (190.92)	1152.25 (205.61)	1063.13 (270.44)
N-Back Perseveration Set	1296.42 (256.63)	1157.89 (236.84)	1274.19 (218.14)	1218.77 (327.59)

Results presented as mean(standard deviation)

Table 4. *Inter-task correlations collapsed across group.*

	CES-D	STAI-T	Sadness	AF E/P	AF E/N	AF H/P	AF H/N	WM Hap	WM Sad	NB Break	NB Pers
CES-D	---	.78**	.24	-.05	-.14	.46**	.12	-.07	-.08	-.06	.13
STAI-T	.65**	---	.13	-.13	-.06	.40*	.07	.07	-.02	-.08	.12
Sadness	.13	.09	---	.06	-.07	.36†	.15	-.18	.18	-.31	-.29
AF E/P	-.05	-.09	.22	---	.50**	.05	.20	.20	.26	.30	.30
AF E/N	-.05	-.06	.38*	.30†	---	.01	.23	.42*	.22	.52**	.36
AF H/P	-.19	.20	.17	.42**	-.06	---	.38*	-.24	-.27	.05	.15
AF H/N	.14	.18	-.29	.57**	-.17	-.23	---	.16	.11	.39*	.21
WM Hap	.20	.21	.05	.04	-.08	-.04	-.10	---	.731**	.19	.18
WM Sad	.52**	.34*	.20	-.05	.03	-.02	-.03	.66***	---	.08	-.02
NB Break	.19	.17	-.20	-.05	-.46**	-.12	.02	.25	.32*	---	.69**
NB Pers	.09	.05	.02	.04	-.11	-.12	-.17	.15	.25	.66***	---

Correlations among variables at baseline session appear above the diagonal, correlations among follow-up variables appear below the diagonal. † $p = .05$. * $p < .05$. ** $p < .01$. *** $p < .001$

Table 5. *Inter-task correlations among study variables at baseline and follow-up sessions, collapsed across groups.*

	Follow-up										
Baseline	CES-D	STAI-T	Sadness	AF E/P	AF E/N	AF H/P	AF H/N	WM Hap	WM Sad	NB Break	NB Pers
CES-D	.72***	.70**	.02	-.01	-.19	.26	.16	.15	.38*	.11	.09
STAI-T	.51***	.88***	.14	-.07	-.10	.17	.18	.11	.27	.11	.12
Sadness	.27	.08	.59***	.37*	.38*	.37*	-.17	-.05	.09	-.33*	-.08
AF E/P	.04	-.04	-.18	.38*	-.05	.06	-.04	.04	.12	.33	.32
AF E/N	.02	.04	-.06	-.23	.12	-.03	.28	.21	.39*	.11	.03
AF H/P	.37*	.44**	.14	.33	-.12	.47**	.05	-.04	-.09	.17	.15
AF H/N	.11	.16	-.12	.25	-.04	.50**	-.22	.26	.00	.12	-.03
WM Hap	.36*	.20	.02	-.23	-.00	.03	.06	.38*	.47**	.27	.15
WM Sad	.24	.09	.03	-.08	.16	.01	.02	.24	.47**	.17	.20
NB Break	.08	-.02	-.08	.09	-.24	.10	-.33	.33	.41*	.61***	.45*
NB Pers	.04	.14	-.13	.22	-.28	.07	-.11	.42*	.33	.67***	.59***

† p = .05, * $p < .05$, ** $p < .01$, *** $p < .001$

Table 6. *Means and standard deviations for reaction times on specific affective flexibility task trial types and specific switch costs at baseline and at follow-up, across groups*

.

Trial Type	Baseline	Follow-up
Repetition Trials		
Emotion rule, positive image	1295.71 (228.20)	943.33 (223.52)
Emotion rule, negative image	1346.81 (233.03)	970.45 (281.46)
Human rule, positive image	1366.57 (273.85)	964.49 (236.46)
Human rule, negative image	1630.59 (349.44)	1089.44 (354.08)
Switch Trials		
Emotion rule, positive image	1461.26 (320.48)	1035.42 (337.41
Emotion rule, negative image	1664.83 (361.44)	1112.92 (300.50)
Human rule, positive image	1479.91 (282.53)	1041.01 (294.30)
Human rule, negative image	1713.60 (383.97)	1134.18 (268.40)
Switch Costs		
Emotion rule, positive image	165.56 (192.48)_	92.09 (178.16)
Emotion rule, negative image	314.89 (181.08)	142.47 (161.77)
Human rule, positive image	96.21 (182.68)	76.52 (148.15)
Human rule, negative image	60.71 (148.52)	44.74 (161.68)

Results presented as mean(standard deviation). All differences were significant, p < .001

Table 7. *Correlations among affective flexibility (AF) switch costs and sad mood following the emotion regulation (ER) task, at baseline and at follow-up.*

AF Switch Costs	ER task sadness – Baseline	ER task sadness – Follow-up
Baseline		
Emotion/Positive	.07	-.14
Emotion/Negative	-.12	-.07
Human/Positive	.35*	.21
Human/Negative	.18	-.08
Follow-up		
Emotion/Positive	.36*	.30†
Emotion/Negative	.37*	.39*
Human/Positive	.37*	.23
Human/Negative	-.19	-.35*

* $p < .05$, † $p = .05$

Table 8. *Hypothesis 1, five-way ANOVA (gender x group x time x rule x valence) results.*

	SS	Df	MS	F	p	Partial η^2	Power
Gender	586.22	1	586.22	.01	.91	< .01	.05
Group	76955.40	1	76955.40	1.67	.21	.07	.24
Gender x Group	95697.09	1	95697.09	2.07	.16	.08	.28
Error	1109567.48	24	46231.98				
Time	222473.75	1	222473.75	9.96	< .01	.29	.86
Time x Group	30287.38	1	30287.38	1.36	.26	.05	.20
Time x Gender	341.62	1	341.62	.02	.90	<.01	.05
Time x Group x Gender	5912.08	1	5912.08	.27	.61	.01	.08
Error (time)	536256.47	24	22344.02				
Rule	556545.48	1	556545.48	19.15	< .001	.44	.99
Rule x Group	3515.49	1	3515.49	.12	.73	<.01	.06
Rule x Gender	102304.61	1	102304.61	3.5	.07	.13	.44
Rule x Group x Gender	26649.84	1	26649.84	.92	.35	.04	.15
Error (rule)	697507.86	24	29062.83				

(table continues)

	SS	Df	MS	F	p	Partial η^2	Power
Valence	47891.76	1	47891.76	1.22	.28	.05	.19
Valence x Group	2392.65	1	2392.65	.06	.81	<.01	.06
Valence x Gender	2559.78	1	2559.78	.07	.80	<.01	.06
Valence x Group x Gender	73153.14	1	73153.14	1.86	.19	.07	.26
Error (valence)	942669.66	24	39277.90				
Time x Rule	248113.85	1	248113.85	13.13	.001	.35	.94
Time x Rule x Group	10165.63	1	10165.63	.54	.47	.02	.11
Time x Rule x Gender	13947.23	1	13947.23	.74	.40	.03	.13
Time x Rule x Group x Gender	42.71	1	42.71	<.01	.96	<.01	.05
Error (time x rule)	453652.25	24	18902.18				
Time x Valence	27671.14	1	27671.14	1.32	.26	.05	.20
Time x Valence x Group	6207.75	1	6207.75	.30	.59	.01	.08
Time x Valence x Gender	34553.41	1	34553.41	1.64	.21	.06	.23
Time x Valence x Group x Gender	28034.70	1	28034.70	1.33	.26	.05	.20
Error (time x valence)	504484.04	24	21020.17				

(table continues)

	SS	Df	MS	F	p	Partial η^2	Power
Rule x Valence	285718.48	1	285718.48	24.73	<.001	.51	>.99
Rule x Valence x Group	4689.27	1	4689.27	.41	.53	.02	.09
Rule x Valence x Gender	846.11	1	846.11	.07	.79	<.01	.06
Rule x Valence x Group x Gender	5390.43	1	5390.43	.47	.50	.02	.10
Error (rule x valence)	277334.39	24	11555.60				
Time x Rule x Valence	46514.93	1	46514.93	4.23	.05	.15	.51
Time x Rule x Valence x Group	536.78	1	536.78	.05	.83	<.01	.06
Time x Rule x Valence x Gender	2229.80	1	2229.80	.20	.66	.01	.07
Time x Rule x Valence x Group x Gender	683.66	1	686.66	.06	.81	<.01	.06
Error (time x rule x valence)	263905.20	24	10996.05				

Table 9. *Means and standard errors of the sad mood scale, assessed following the reappraisal challenge task, by gender, training group, and time.*

	Baseline	Follow-up
Training Group		
Male	5.86 (1.07)	6.00 (.74)
Female	6.54 (.79)	5.85 (.54)
Control Group		
Male	4.33 (1.64)	5.00 (1.13)
Female	7.00 (.85)	6.46 (.59)

Scale was based on responses to four items representing sad mood (sad, down, depressed, upset) rated on a Likert scale from 1-5. Scores could range from 4 – 20.

Table 10. *Means and standard errors of the sad mood difference scale by gender, training group, and time.*

	Baseline	Follow-up
Training Group		
Male	1.86 (1.03)	1.86 (.79)
Female	2.23 (.76)	1.31 (.58)
Control Group		
Male	.33 (1.57)	< .01 (1.21)
Female	2.36 (.82)	2.09 (.63)

Table 11. *Hypothesis 3, three-way ANOVA (gender x group x time) results for CES-D.*

	SS	Df	MS	F	p	Partial η^2	Power
Gender	87.86	1	87.86	1.08	.31	.03	.17
Group	86.20	1	86.20	1.06	.31	.03	.17
Gender x Group	18.80	1	18.80	.23	.63	.01	.08
Error	3098.08	38	81.53				
Time	28.46	1	28.46	2.00	.17	.05	.28
Time x Group	5.01	1	5.01	.35	.56	.01	.09
Time x Gender	.03	1	.03	<.01	.96	<.01	.05
Time x Group x Gender	2.56	1	2.56	.18	.67	.01	.07
Error	541.42	38	14.25				

Table 12. *Hypothesis 3, three-way ANOVA (gender x group x time) results for STAI.*

	SS	Df	MS	F	p	Partial η^2	Power
Gender	25.92	1	25.92	.17	.68	<.01	.07
Group	93.70	1	93.70	.62	.44	.02	.12
Gender x Group	6.73	1	6.73	.05	.83	<.01	.06
Error	6024.56	40	150.61				
Time	183.03	1	183.03	18.36	<.001	.32	.99
Time x Group	.39	1	.39	.04	.85	<.01	.05
Time x Gender	11.15	1	11.15	1.11	.30	.03	.18
Time x Group x Gender	.12	1	.12	.01	.91	<.01	.05
Error	398.78	40	9.97				

Table 13. *Hypothesis 4, four-way ANOVA (gender x group x time x word valence) results for emotional working memory task sorting costs.*

	SS	Df	MS	F	p	Partial η^2	Power
Gender	95359.98	1	95359.98	.44	.51	.02	.10
Group	37748.78	1	37748.78	.18	.68	.01	.07
Gender x Group	205854.22	1	205854.22	.96	.34	.03	.16
Error	6440642.46	30	214688.08				
Time	465839.60	1	465839.60	6.52	.02	.18	.70
Time x Gender	69725.51	1	69725.51	.98	.33	.03	.16
Time x Group	17953.38	1	17953.38	.25	.62	.01	.08
Time x Group x Gender	235250.82	1	235250.82	3.29	.08	.10	.42
Error (Time)	2143495.48	30	71449.85				
Word Valence	7783.74	1	7783.74	.26	.62	.01	.08
World Valence x Gender	13868.44	1	13868.44	.46	.50	.02	.10
Word Valence x Group	2824.95	1	2824.95	.09	.76	<.01	.06
Word Valence x Gender x Group	6300.11	1	6300.11	.21	.65	.01	.07
Error (Word Valence)	904970.81	30	30165.69				

(table continues)

	SS	Df	MS	F	p	Partial η^2	Power
Time x Word Valence	757.87	1	757.87	.03	.87	<.01	.05
Time x Word Valence x Gender	8374.33	1	8374.33	.31	.58	.01	.08
Time x Word Valence x Group	34197.35	1	34197.35	1.26	.27	.04	.19
Time x Word Valence x Gender x Group	30246.14	1	30246.14	1.11	.30	.04	.18
Error (Time x Word Valence)	815608.38	30	27186.95				

Table 14. *Hypothesis 4, four-way ANOVA (gender x group x time x trial type) results for emotional n-back reaction times.*

	SS	Df	MS	F	p	Partial η^2	Power
Gender	366639.96	1	366639.96	2.06	.16	.07	.28
Group	50740.46	1	50740.46	.29	.60	.01	.08
Gender x Group	679.40	1	679.40	<.01	.95	<.01	.05
Error	4795475.60	27	177610.21				
Time	148015.63	1	148015.63	4.29	.05	.14	.51
Time x Gender	88887.20	1	88887.20	2.57	.12	.09	.34
Time x Group	96890.52	1	96890.52	2.81	.11	.09	.37
Time x Group x Gender	22366.68	1	22366.68	.65	.43	.02	.12
Error (Time)	932737.87	27	34545.85				
Trial Type	283845.16	1	283845.16	12.86	.001	.32	.93
Trial Type x Gender	9632.65	1	9632.65	.44	.51	.02	.10
Trial Type x Group	28027.59	1	28027.59	1.27	.27	.05	.19
Trial Type x Gender x Group	11330.03	1	11330.03	.51	.48	.02	.11
Error (Trial Type)	595924.13	27	22071.26				

(table continues)

	SS	Df	MS	F	p	Partial η^2	Power
Time x Trial Type	.13	1	.13	<.01	>.99	<.01	.05
Time x Trial Type x Gender	4162.67	1	4162.67	.25	.62	.01	.08
Time x Trial Type x Group	855.20	1	855.20	.05	.82	<.01	.06
Time x Trial Type x Gender x Group	5838.23	1	5838.23	.35	.56	.01	.09
Error (Time x Trial Type)	448447.93	27	16609.18				

Figure 1. *Interaction of time and image valence for affective flexibility task switch costs involving the emotional sorting rule.*

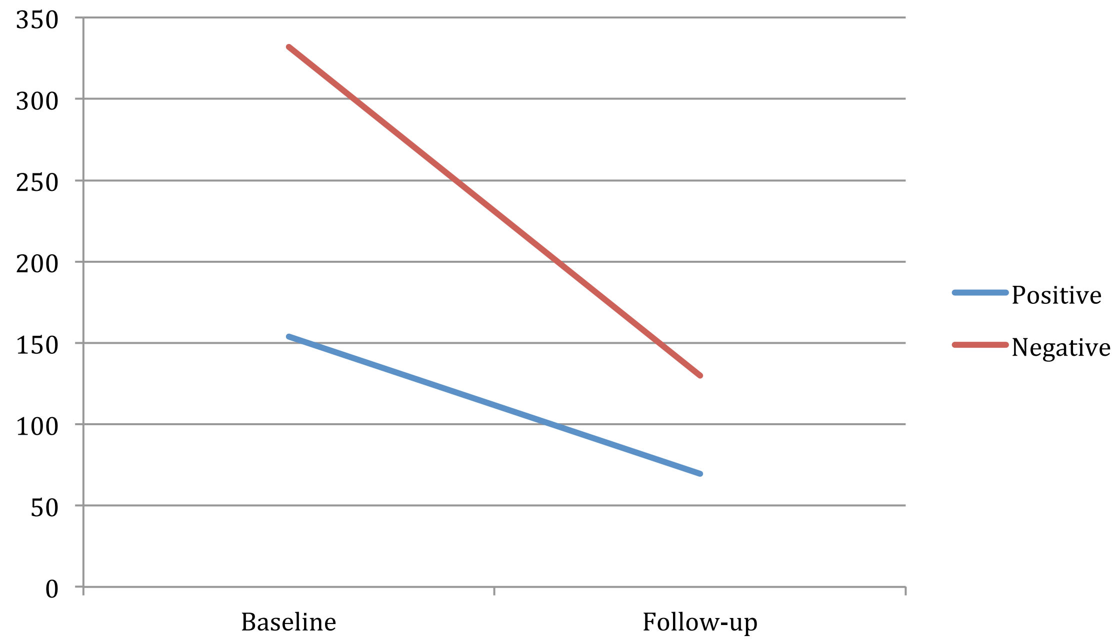

Figure 2. *Interaction between sorting rule and image valence on affective flexibility task switch costs at follow-up.*

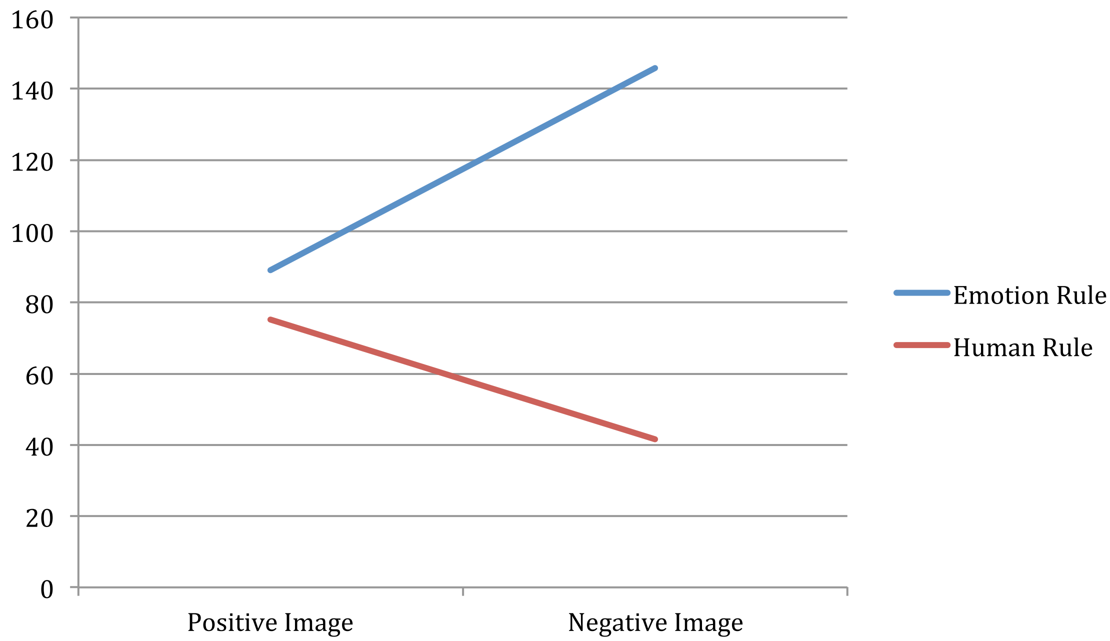

Figure 3. *Working memory manipulation task results, interaction of time and gender in the training condition.*