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EXPLORING THE EFFECTS OF BRIEF MINDFULNESS AND REAPPRAISAL TRAINING
ON EXECUTIVE CONTROL AND AFFECT

by

LEE WARD SCHAEFER

Under the Direction of Akihiko Masuda, Ph.D.

ABSTRACT

Despite the efficacy of mindfulness-based interventions for a wide range of psychological problems, their mechanisms remain unknown. Analogue studies of key treatment components can help distinguish these approaches. One such component, focused-breathing meditation, has rarely been studied in direct comparison to another active and theoretically distinct technique, cognitive reappraisal. The present study examined the effects of mindful breathing and cognitive reappraisal instructions on negative affect and executive control, two potential mechanisms of mindfulness, in a laboratory setting. Non-clinical college undergraduates ($N = 136$) were randomly assigned to a 10-minute mindfulness, reappraisal, or mind-wandering control condition. Contrary to hypotheses, no between-group differences were found in sadness ratings, state mindfulness, or the inhibitory control dimension of executive control following the intervention. The mindfulness condition showed lower inattention compared to the mind-wandering condition. Implications of these results are discussed in terms of specific theoretical mechanisms of mindfulness- and cognitive-reappraisal-based interventions.

INDEX WORDS: Mindfulness, Reappraisal, Meditation, Attention, Executive functions,

Emotion regulation

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LEE WARD SCHAEFER

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Master of Arts

in the College of Arts and Sciences

Georgia State University

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Lee Ward Schaefer
2016

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TRAINING ON EXECUTIVE CONTROL AND AFFECT

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LEE WARD SCHAEFER

Committee Chair: Akihiko Masuda

Committee: Robert Latzman

Jessica Turner

Electronic Version Approved:

Office of Graduate Studies

College of Arts and Sciences

Georgia State University

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DEDICATION

This thesis is dedicated to my wife, Emily Bird, whose unwavering support and common sense always keeps me in mind of what really matters.

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1 INTRODUCTION

Recent years have seen a proliferation of psychotherapies that incorporate, either explicitly or implicitly, elements of mindfulness, a concept with roots in Buddhist philosophy and practice (Brown, Ryan, & Creswell, 2007). These therapies include Mindfulness-Based Stress Reduction (MBSR; Kabat-Zinn, Massion, Kristeller, & Peterson., 1992), Mindfulness-Based Cognitive Therapy (MBCT; Segal, Williams, & Teasdale, 2002), and Acceptance and Commitment Therapy (ACT; Hayes, Strosahl, & Wilson, 1999). Mindfulness-based therapies have demonstrated effectiveness for a wide variety of psychological problems, including depression and anxiety (Baer, 2003; Hofmann, Sawyer, Witt, & Oh, 2010). Mindfulness-based therapies are theorized to yield their clinical effectiveness (e.g., symptoms reduction) by promoting clients' dispositional mindfulness or their application of mindfulness skills to regulate emotion and behavior more adaptively (Hayes, Follette, & Linehan, 2004; Hayes, Villatte, Levin, & Hildebrandt, 2011).

There is considerable evidence that multi-week mindfulness-based interventions produce increases in trait, or dispositional, mindfulness, and increasing evidence that these increases predict and even mediate post-program symptom reduction or increased well-being (Carmody & Baer, 2008; Keng, Smoski, & Robins, 2011). While these findings bolster a theoretical argument that increases in mindfulness mediate treatment outcome for mindfulness-based therapies, they do not elucidate the mechanism by which mindfulness itself produces these beneficial outcomes. More research on the mechanisms of change in psychosocial interventions is essential, both for psychotherapies in general (Kazdin, 2005) and for mindfulness-based treatments in particular (Baer, 2003; Hayes & Wilson, 2003; Shapiro, Carlson, Astin, & Freedman, 2006). Mechanisms research investigates “how” or “why” psychological treatments have their salutary effects.

Laboratory analogue experiments, such as the proposed study, are well suited to examining such questions (Kazdin, 2005; Levin, Hildebrandt, Lillis, & Hayes, 2012). An analogue study can isolate components of a larger treatment package, control with greater precision the precise “dosage” of each component that participants receive, and assess potential mediators and outcomes more immediately, in a controlled setting.

The present analogue study explored the immediate effects of a brief mindfulness intervention on affect and executive control. The study had two chief aims: First, it sought to replicate findings (Keng, Robins, Smoski, Dagenbach, & Leary, 2013; Broderick, 2005; Hufziger & Kuehner, 2009; Arch & Craske, 2006; Erisman & Roemer, 2010) on the short-term effects of brief mindfulness training on negative affect and to examine whether those effects were accompanied by improvements in executive control. Second, it sought to compare brief mindfulness training to an active control, cognitive reappraisal instructions, to clarify points of similarity and difference in their immediate effects.

1.1 Defining Mindfulness

Among prevailing definitions of mindfulness, perhaps the most widely-cited are those by MBSR developer Jon Kabat-Zinn, who has described mindfulness as “an open-hearted, moment-to-moment, non-judgmental awareness” (Kabat-Zinn, 2005, p. 24) and “paying attention in a particular way: on purpose, in the present moment, and non-judgmentally” (Kabat-Zinn, 1994, p. 4).¹ These descriptions are evocative and consistent with how mindfulness is commonly taught in Western meditation training programs. Yet Kabat-Zinn’s descriptions are intended primarily for a lay audience and for clinical applications, and as such, they leave ambiguous certain key

¹ Langer (1989, 1997) has developed a research program based on a distinct definition of mindfulness that emphasizes its relationship to creativity, problem-solving, and the ability to adopt multiple perspectives. Langer’s model of mindfulness does not assume a relationship between mindfulness meditation practice and the cultivation of mindfulness as a trait or state, however. It thus has less relevance to the clinical applications of mindfulness training of interest in this review and will not be discussed.

issues—whether mindfulness is unidimensional, for instance, or how “open-heartedness” or “non-judgment” might be operationalized. Hayes and Wilson (2003), among others, have critiqued popular definitions like Kabat-Zinn’s for leaving unclear whether mindfulness is a psychological process with particular downstream outcomes, a method for achieving certain outcomes, or an outcome itself (Hayes & Wilson, 2003). For the purposes of empirical research, mindfulness is still a poorly-defined psychological construct.

The absence of a consensus definition has led to a proliferation of various, competing definitions of mindfulness (e.g. Chiesa, 2013; Bergomi, Tschacher, & Kupper, 2013; Bishop et al., 2004). Mindfulness has been defined both as a stable trait (e.g., Brown & Ryan, 2003; Baer et al., 2006) and as a transient psychological state (Lau et al., 2006). Most self-report scales of mindfulness purport to assess it as a trait-level construct, with only two widely used scales assessing state mindfulness (Toronto Mindfulness Scale; Lau et al., 2006; Mindful Awareness and Attention Scale – State Version; Brown & Ryan, 2003). Even among trait mindfulness scales, however, there is considerable variability in definitions of the construct. The Mindful Awareness and Attention Scale (MAAS; Brown & Ryan, 2003) measures mindfulness as a unitary construct reflecting present-moment awareness, or the absence of “mindlessness.” By contrast, the Southampton Mindfulness Questionnaire (SMQ; Chadwick et al., 2008) and Cognitive and Affective Mindfulness Scale-Revised (CAMS-R; Feldman et al., 2007) both conceptualize mindfulness as a particular way of responding to distress, affect or cognitions. The Five Facet Mindfulness Questionnaire (FFMQ; Baer et al., 2006) presents yet another conceptualization of mindfulness, this time derived from an exploratory factor analysis of other mindfulness scales, as a multifaceted construct that includes attentional and behavioral components, as well as an ability to describe or label internal experience.

Because each conceptual definition of mindfulness implicates specific potential mechanisms for its salutary effects on psychological health, the variation in definitions across self-report scales contributes to confusion about how mindfulness “works.” To address this confusion, Bishop and colleagues (2004) attempted to develop a consensus definition of mindfulness with specific claims about its potential mechanisms. Bishop et al. (2004) stated that mindfulness consists of two components: 1) “self-regulation of attention so that it is maintained on immediate experience,” and 2) “an orientation [toward one’s inner experience] that is characterized by curiosity, openness, and acceptance” (Bishop et al., 2004, p. 232). This model conceptualizes mindfulness as a “state-like phenomenon” (p. 237), arising from the practice of formal meditation, and also “similar to a skill that can be developed with practice.” In Hayes and Wilson’s (2003) formulation, then, Bishop et al. define mindfulness as a “process” arising specifically from the “method” of meditation or mindfulness training.

The same research team also developed the Toronto Mindfulness Scale (TMS; Lau et al., 2006) to assess mindfulness immediately following a meditation practice period. Interestingly, factor analysis of the final TMS items failed to replicate the two-dimensional Bishop et al. (2004) structure, instead yielding two factors (Curiosity and Decentering) most closely associated with the only the second, attitudinal facet of mindfulness (Lau et al., 2006). A trait measure, the Philadelphia Mindfulness Scale (PHLMS; Cardaciotto, Herbert, Forman, Moitra, & Farrow, 2008) was also developed explicitly based on the Bishop et al. (2004) consensus definition and has a confirmed two-factor structure directly corresponding to the model’s attention and acceptance facets. Feldman and colleagues also based the CAMS-R on this model, and while the final CAMS-R has a four-factor structure, these facets closely parallel the Bishop et al. model as well. Other trait mindfulness scales, such as the FFMQ or MAAS, however, do

not contradict this definition so much as they differ in their points of emphasis, positing that mindfulness can be measured outside of formal meditation practice or that it can consist of a broader or narrower range of psychological processes. Thus the Bishop et al. model has not definitively resolved issues in measuring mindfulness.

A key advantage of Bishop and colleagues' definition, however, is that it implicates specific, measurable attentional processes as correlates or outcomes of mindfulness (Bishop et al., 2004; Lutz, Slagter, Dunne, & Davidson, 2008). Importantly, not only are these processes plausibly enhanced in individuals with high dispositional mindfulness, they are also plausible correlates of a mindful *state* achieved during or immediately following meditation practice. In most forms of meditation, the practitioner attempts to *sustain* her attention on a particular object of focus. Upon noticing that her mind has wandered from this object to other sensations or thoughts, she *switches* her attention back to the original meditative object. She also attempts to let go of evaluative or elaborative patterns of thought, *inhibiting* these more discursive modes of cognition. Thus, Bishop and colleagues argue, dispositional mindfulness and meditation practice should be associated with improvements in sustained attention, attentional flexibility or set switching, and response inhibition (Bishop et al., 2004).

This study adhered to the Bishop et al. (2004) definition of mindfulness in regarding mindfulness as consisting of self-regulated attention to present experience, conducted with a nonjudgmental and accepting orientation. Drawing on Bishop and colleagues' proposed mechanisms, the study explored the contributions of mindfulness to two potential correlates: enhanced sustained attention and inhibitory control. While these executive processes represent candidate mechanisms of mindfulness, though, improved sustained attention or inhibitory control alone is not an adequate account of how mindfulness leads to psychological well-being. To

explain mindfulness' salutary effects on psychological health, one must also address mindfulness' relationship to negative affect or distress, the primary target of most clinical applications of mindfulness (Khoury et al., 2013). Recent theoretical and empirical work has filled this gap by placing mindfulness, as defined by Bishop et al. (2004), within the framework of emotion regulation (e.g., Chambers, Gullone, & Allen, 2009; Hayes & Feldman, 2004; Farb, Anderson, Irving, & Segal, 2012).

1.2 Mindfulness and Emotion Regulation

As presented by Gross (2007), emotion regulation refers to the process of modulating one's emotional experience or responses to emotion-eliciting stimuli. Emotion regulation has been called "the processes by which we influence which emotions we have, when we have them, and how we experience and express them" (Gross, 2002, p. 282). Maladaptive emotion regulation, or emotional dysregulation, is presumed to be a common feature across psychopathologies (Gratz & Roemer, 2004; Werner & Gross, 2004). Many psychological interventions in turn purport to target emotion regulation, by teaching more adaptive emotion regulation strategies, usually with the aim of reducing distress by altering the frequency, intensity or duration of positive or negative emotional experiences (e.g., Mennin, 2004; Liverant, Brown, Barlow & Roemer, 2008).

Gross' process model of emotion (1998) identifies emotion regulation strategies that theoretically operate at different stages of the emotion generation process. These strategies include distraction, suppression, reappraisal and acceptance, among others. Distraction and reappraisal have been described as "antecedent-focused" strategies, as they theoretically operate early in the emotion generation process, affecting attentional deployment toward and initial cognitive appraisals of a stimulus (Gross, 1998). Research has identified certain strategies (e.g.,

reappraisal) as generally adaptive, and others as generally maladaptive (e.g., rumination, suppression), in terms of their association with psychopathology and/or effects on subsequent affect, cognition or behavior (Aldao & Nolen-Hoeksema, 2010). For example, expressive suppression, an effortful attempt not to express the behavioral signs of a particular emotion, entails the evaluation that a particular emotion is bad or unwanted and can paradoxically increase the experience of that emotion (Gross & John, 2003).

Although mindfulness is not part of Gross' canonical set of emotion regulation strategies, some researchers have conceptualized mindfulness as a technique or strategy for handling difficult emotions (e.g., Hayes & Feldman, 2004; Chambers, Gullone, & Allen, 2009; Farb et al., 2013). Conceptualizing mindfulness as a form of emotion regulation is particularly appropriate when considering clinical applications of meditation and mindfulness training, rather than the mere phenomenology of mindfulness. Although these accounts of mindful emotion regulation differ in some respects, they all recognize that mindfulness' emphasis on nonjudgment is antithetical to some maladaptive emotion regulation strategies, such as expressive suppression.

In addition, most attempts to integrate mindfulness with the emotion regulation literature note that mindfulness could act as what Farb and colleagues (2013) term a "meta-strategy" (p. 551). By enhancing awareness of one's habitual responses to emotional stimuli, and reducing these automatic response tendencies (e.g., Wenk-Sormaz, 2005; Kang, Gruber, & Gray, 2013), mindfulness could facilitate the flexible, context-specific application of other emotion regulation strategies. Increasingly, flexibility in the application or use of regulation strategies is regarded as a key individual difference indicator of psychological health, rather than strict reliance on so-called "adaptive" strategies (e.g., Aldao, 2013; Aldao & Nolen-Hoeksema, 2010; Bonanno & Burton, 2013).

Studies of state mindfulness are necessarily more limited in their ability to assess emotion regulation flexibility, however, and thus most laboratory studies of brief mindfulness training have examined self-reported affective responses to a stressor. Interestingly, though, the experimental literature on emotion regulation, when it has addressed mindfulness at all, has typically treated it as a variant of distraction (e.g., Webb, Miles, & Sheeran, 2012) or acceptance (e.g., Kohl, Rief, & Glombiewski, 2012) strategies. Mindfulness, as defined by Bishop et al. (2004), certainly shares features with distraction, as both strategies require attentional control. Distraction entails the effortful deployment of attention to an object other than an aversive emotional stimulus, however. By contrast, mindful attention to the present moment does not preclude attending to the emotional experience and would likely involve some awareness of an aversive stimulus in the present moment. It is true that in the early stages of formal meditation training, mindfulness might resemble distraction more closely, as novice meditators are typically instructed to focus their attention on a specific meditational object, such as the physical sensations of the breath (Lutz et al., 2008). Nevertheless, even in the context of novice meditators, mindfulness does not fit easily into a traditional taxonomy of emotion regulation strategies because of its simultaneous stipulation of an accepting, nonjudgmental stance toward emotions. Noting that distraction can be characterized by an almost opposite stance of withdrawal from aversive emotion, Farb et al. (2013) describe mindfulness as a distinct form of emotion regulation in that it represents “approach-oriented attention deployment” (p. 550).

1.2.1 Brief mindfulness interventions in emotion regulation.

Experimental research also supports the possibility that brief mindfulness training can enhance emotion regulation. These studies typically use a short (10-15 minute) meditation exercise and assess emotion regulation via self-reported mood ratings following a mood

induction or emotional challenge. Laboratory-based mindfulness training interventions, such as brief focused-breathing meditation practice, have been shown to reduce negative affect, relative to distraction and rumination conditions, when the exercise follows an experimental sad mood induction (Broderick, 2005; Huffziger & Kuehner, 2009; Kuehner, Huffziger, & Liebsch, 2009). Mindfulness and acceptance manipulations have also demonstrated a buffering effect in lab studies, reducing negative affective response to a subsequent emotional stressor relative to rumination, distraction and worry (Arch & Craske, 2006; Erisman & Roemer, 2010; Keng et al., 2013; Singer & Dobson, 2007). Some conflicting findings in this literature warrant closer examination, however.

In two studies with nearly identical designs, Huffziger, Kuehner and colleagues (Huffziger & Kuehner, 2009; Kuehner et al., 2009) examined the effects of 8-minute mindfulness instructions on experimentally-induced negative mood in comparison to rumination and distraction instructions. These studies employed the same mood induction procedure, in which participants recalled three negative autobiographical events and then incubated this mood for several minutes, and then received instructions in mindfulness or a comparison strategy, either rumination or distraction. In a sample of previously depressed adults (the majority remitted, but including some with residual or full depressive symptoms), mindfulness training and distraction both reduced negative affect and increased positive affect as compared to rumination (Huffziger & Kuehner, 2009). By contrast, in a non-clinical student sample, distraction evidenced the same improvements in mood with respect to rumination, but mindfulness training did not significantly differ from either rumination or distraction in its effects on positive or negative affect (Kuehner, Huffziger, & Liebsch, 2009). Given that the mood induction produced significantly deleterious effects on affect in both samples, these

discrepant findings may be indicative of differences in the responsiveness of clinical and healthy populations to mindfulness or reappraisal instructions. Indeed, another analogue study with a remitted depressed population found that training in acceptance, using a script based on MBCT, was superior to rumination or no training, and comparable to distraction, in attenuating the negative emotional response to a subsequent mood induction (Singer & Dobson, 2007).

Erisman and Roemer (2010) obtained similarly mixed results from an experimental comparison of mindfulness and neutral educational control instructions with an analogue clinical sample. Students endorsing elevated difficulties in emotion regulation viewed affectively-laden film clips before and after brief mindfulness training, delivered via audio recording, or a control (educational) condition. The brief mindfulness training was 10 minutes long and relatively extensive when compared to other such brief instructions. In addition to providing a description of mindfulness and brief instructions in mindful awareness of breathing instructions, and an opportunity to practice mindfulness of breath, the mindfulness intervention also included an explanation of applying mindfulness to emotional experiences, and a mindfulness of emotions exercise focused on noticing responses to a poem. Participants in the mindfulness condition reported greater decentering on the TMS, indicating that the training was successful at inducing an aspect of state mindfulness. Mindfulness participants also reported greater positive affect in response to a positive emotional film clip and less negative affect in response to a clip with mixed affective content. There were no significant differences in self-reported affect between conditions, however. This finding was especially notable for negative affect in response to distressing film clips, as these were arguably the emotional stimuli most similar to the negative mood inductions used in the studies discussed above. With only 15 participants in each condition, Erisman and Roemer's (2010) study may have lacked statistical power to detect these

between-condition effects. It is also possible, though, that mindfulness and distraction act similarly or with similar efficacy on emotions elicited through film clips. Watching emotional film clips involving third parties may evoke different emotions than negative autobiographical recall, and distraction and mindfulness may be equivalent strategies in regulating such empathic emotions.

Yet other studies of brief mindfulness training with healthy samples have found benefits of mindfulness over distraction, as well as other putatively maladaptive (e.g., rumination) and neutral comparison conditions. Broderick (2005) administered a negative mood induction to healthy, meditation-naïve students, then provided instructions in either rumination distraction or mindfulness. Participants who received the 8-minute audio-recorded mindfulness training reported significantly lower post-training levels of negative affect than those receiving distraction or rumination instructions, with participants in the distraction condition reporting levels of negative affect between those in the mindfulness and rumination conditions. As mindfulness was the only experimental condition involving audio instructions, methodological differences between conditions might partially account for Broderick's findings. However, Arch and Craske (2006) obtained similar results from a healthy student sample using audio-recorded instructions in all three of their experimental conditions. Arch and Craske (2006) compared focused-breathing meditation instructions to worry and unfocused attention conditions, examining training-related changes in participants self-reported affect in response to viewing affectively-valenced images. Participants in the focused-breathing group reported less negative affect in response to negative slides presented after training than did those in the worry group. A similar but non-significant trend was present between the focused-breathing and unfocused

attention conditions. With 20 participants in each condition, Arch and Craske's (2006) study also may have been underpowered to detect such a difference.

Using similar, 10-minute focused-breathing meditation instructions with a healthy student sample, Ortner and Zelazo (2014) failed to find meditation-specific increases in either of the TMS state mindfulness subscales, when compared to a guided-imagery distraction exercise. Distraction and mindfulness also produced equivalent reductions in negative affect and anger on an anger-provoking writing task. However, both TMS-Decentering and TMS-Curiosity interacted with condition to predict negative affect and anger, such that decentering and curiosity only predicted improvements in both affective outcomes for participants receiving mindfulness instructions.

Taken together, these findings suggest that brief mindfulness training can produce salutary effects on negative mood in healthy samples, even in comparison to other neutral or potentially adaptive strategies like distraction. Emerging findings also suggest that such brief training can induce decentering—a component of state mindfulness as assessed by the TMS—in healthy (Feldman, Greeson, & Senville, 2010) or analogue clinical populations (Erisman & Roemer, 2010) and that state mindfulness may ameliorate subsequent negative affective responding (Ortner & Zelazo, 2014). Still, it remains unclear whether meditation-specific increases in state mindfulness coincide with these reductions in negative affect and by what mechanism mindfulness diminishes this response in experimental settings.

1.3 Executive Processes in Mindful Emotion Regulation

Almost all definitions of mindfulness explicitly treat mindfulness, whether trait or state, as entailing a particular quality of attention. From a neuroscientific perspective, mindfulness training is believed to enhance emotion regulation via improvements in attention and executive

functioning more broadly. Specifically mindfulness may enhance adaptive emotion regulation in mood disorders by reducing habitual modes of cognitive elaboration in favor of sustained attention and monitoring associated with the anterior cingulate cortex (ACC), among other regions (Farb, Anderson, & Segal, 2012).

A growing body of literature supports the view that mindfulness is associated with, and may even improve, executive functions. Executive function encompasses a variety of cognitive processes theoretically linked to frontal lobe activity and more complex, planned, controlled or flexible behavior than is typically associated with more basic functions like attention (Suchy, 2009). Although definitions of these processes vary somewhat, commonly recognized executive functions include selective and sustained attention, shifting mental sets, updating and monitoring working memory, and inhibiting prepotent responses (Miyake et al., 2000; Banich, 2009). Individuals with greater meditation experience perform better than controls on measures of sustained attention, change blindness, visual perspective shifting and visual selective attention (Valentine & Sweet, 1999; Moore & Malinowski, 2009; Hodgins & Adair, 2010).

These associations are also borne out in studies of meditation training programs, suggesting that multi-day or multi-week mindfulness interventions may enhance executive functioning (Jha, Krompinger, & Baime, 2007; Chambers, Lo, & Allen, 2008; Tang et al., 2007). Some evidence also supports the efficacy of even briefer mindfulness interventions in enhancing some aspects of executive functioning. In an early study involving three 20-minute training sessions over the three days, Wenk-Sormaz (2005) found evidence that meditation training decreased healthy participants' interference on the Stroop task, a measure of response inhibition, as compared to educational and rest control conditions. Two more recent studies suggest that this immediate effect on Stroop performance can be achieved with even briefer periods of meditation

training. Lee and Orsillo (2014) administered an emotional Stroop task to participants with elevated GAD symptoms after they received 20-minute training in mindfulness, relaxation, or a mind-wandering control exercise intended to exacerbate GAD-related worry. Participants in the mindfulness condition demonstrated reduced Stroop interference as compared to mind-wandering but not relaxation, suggesting that a brief mindfulness induction can improve inhibitory control with respect to an analogue pathological condition but perhaps not in comparison to another analogue clinical intervention. However, Keng and colleagues (2013) found that a mindfulness condition significantly reduced depressed participants' interference on the classic Stroop task as compared to cognitive reappraisal, despite employing an even briefer, 10-minute mindfulness induction period. Methodological differences in populations (healthy vs. GAD vs. depression) or the use of a mood induction before testing Stroop performance might account for some of the variability in these findings.

Nevertheless, these experimental studies suggest that brief mindfulness training can produce immediate effects on inhibitory control in analogue clinical populations that are similar to those found after extended, multi-week meditation training programs. More research is needed to elucidate how these short-term effects relate to the general benefits of mindfulness for psychological well-being. The other potential executive processes affected by longer-term mindfulness training, such as sustained attention, have not been studied in the context of these brief, experimental settings. This experimental research has also largely been conducted with analogue clinical samples (with disorder-specific study designs) and employed only one measure of inhibitory control, the Stroop task. Finally, aside from one study (Keng et al., 2013), no experimental investigations of brief mindfulness inductions have examined mindfulness' putative effects on executive function in the context of the emotion regulatory benefits of

mindfulness that have been well established in the experimental literature. Such examination of concurrent executive and affective processes in brief mindfulness training is not only a gap in the literature, it is also increasingly indicated by emerging research in the mechanisms of mindfulness.

1.3.1 Executive control as a mechanism of mindful emotion regulation.

Recently, studies have used the enhanced temporal specificity of electroencephalography (EEG) to corroborate the potential significance of enhanced executive functioning in the emotion regulatory effects of mindfulness. Teper and Inzlicht (2013) found that experienced meditators exhibit a heightened error-related negativity, an event-related potential (ERP) associated with an ACC-driven negative emotional reaction, to their own errors on the Stroop task, as compared to non-meditators. They propose that, by enhancing conflict-monitoring and executive control abilities, mindfulness allows people to regulate their negative emotional response to failure feedback more rapidly, thus facilitating adaptive responding (Teper, Segal, & Inzlicht, 2013). This theory is consistent with prior findings that mindfulness enhances flexibility in behavioral paradigms that include reinforcement, facilitating extinction and reducing stimulus over-selectivity (McHugh, Simpson, & Reed, 2010; McHugh et al., 2012; McHugh & Wood, 2013).

Other EEG studies indicate that trait mindfulness is associated with altered responding to emotional stimuli at the level of attentional deployment. Individuals higher in dispositional mindfulness have demonstrated attenuated immediate emotional responses to high arousal unpleasant images (Brown, Goodman, & Inzlicht, 2013) and less neural differentiation of rewarding and neutral feedback (Teper & Inzlicht, 2014). Cross-sectionally, trait mindfulness is positively associated with both self-reported emotion regulation ability and behavioral traits linked to the prefrontal systems presumed to underlie executive functions (Lyvers et al., 2013).

Neuroscience findings further complicate the relationship between trait mindfulness and the role of executive processes in mindful emotion regulation. Using functional neuroimaging, Taylor and colleagues (2011) found functional neuroimaging evidence that meditation experience is associated with distinct mechanisms of state mindfulness. For both experienced and novice meditators, a state mindfulness induction reduced emotional intensity in response to affective pictures. For experienced meditators, state mindfulness was associated with deactivation in default mode network regions (medial prefrontal and posterior cingulate cortices) and not with changes in amygdala activity. By contrast, though, novice meditators practicing mindfulness showed increased activity in the medial frontal gyrus and decreased amygdala activity. Taylor et al. (2011) suggest that, in beginners, mindfulness practice involves active down-regulation of emotional responding (similar to reappraisal), while for experienced meditators, mindfulness involves acceptance and reductions in elaboration and reappraisal. Reviewing neuroimaging studies of mindfulness and emotion regulation, Chiesa, Serretti, and Jakobsen (2013) concluded that the extant evidence does indicate distinct mechanisms at differing levels of meditation experience. For novice meditators, they argue, mindfulness training appears to involve “top-down” emotion regulation, recruiting prefrontal regions to down-regulate limbic activity, while for experienced meditators, mindfulness practice is associated with a more “bottom-up” emotion regulation strategy characterized by decreased activity in default mode network areas. Thus, for novices, meditation practice may be especially likely to involve increased executive control, including sustained attention and inhibition, similar to reappraisal (Chiesa et al., 2013).

Limited research has been conducted on the role of executive processes in laboratory-based studies of mindful emotion regulation. Fergus, Wheless, and Wright (2014) compared

mindful-breathing—paired with progressive muscle relaxation (PMR)—to the attention training technique (ATT), a component of Wells’ (2009) metacognitive therapy. The ATT is structured to target the self-focused attention characteristic of worry and depressive rumination by training selective attention, divided attention and attentional switching in response to auditory stimuli (Wells, 2009). Fergus and colleagues compared the effects of these two techniques on changes in nonclinical undergraduates’ self-reported state anxiety and attentional focus (internal vs. external, rated dimensionally by a single-item 7-point scale). Both ATT and mindfulness-plus-PMR reduced self-reported anxiety, but the techniques had differential effects on attention, with ATT producing reductions in self-focused attention and mindfulness training increasing self-focused attention. In their experimental comparison of mindfulness and reappraisal, Keng et al. (2013) found that both active interventions reduced negative affect following the mood induction, with respect to no training, but that participants receiving mindfulness made fewer errors on the subsequent Stroop task. The authors interpreted this result as indicating that both interventions, when applied as “online” emotion regulation strategies, depleted participants’ cognitive resources, but that mindfulness incurred relatively fewer cognitive costs, perhaps because it involves less voluntary control over cognitive content. This cognitive-depletion interpretation is compelling, but differences between the two intervention scripts could account for the increased cognitive “cost” of reappraisal in this particular study without indicating a fundamental difference between the two strategies. For example, the reappraisal script that Keng et al. (2013) used asked participants to imagine a hypothetical scenario (an ambiguous social situation), which they then reinterpreted. The mindfulness script offered a potential imaginal image as guidance for participants but did not require participants to imagine a specific scene in detail and thus presumably required less effort. More importantly, the cognitive-depletion

account of Keng and colleagues' findings ignores a significant aspect of the theoretical distinction between reappraisal and mindfulness: although mindfulness involves less cognitive elaboration than reappraisal, it also involves, at least in the early stages of training, a distinct form of attention.

1.4 Experimental Comparisons of Mindfulness and Reappraisal

Direct comparisons of mindfulness and reappraisal are fraught because the nature of relationship between mindfulness and cognitive reappraisal remains controversial. Proponents of the “third wave” or contextual approach to CBT typically claim that mindfulness-based therapies are part of this more recent response to traditional CBT (Hayes, 2004; Hayes et al., 2011). Hayes' and colleagues' argue that, despite their diverse origins and target disorders, contextual behavior therapies are alike in that they “target the context and function of psychological events such as thoughts, sensations, or emotions, rather than primarily targeting the content, validity, intensity, or frequency of such events” (Hayes et al., 2011, pp. 157-158). Proponents of the contextual approach argue that traditional CBT takes this latter approach, directly targeting cognitive content and attempting to change it through techniques like cognitive restructuring or reappraisal. This distinction is controversial, however, as many argue that traditional CBT and contextual approaches may operate through the same mechanisms (Arch & Craske, 2008; Herbert & Forman, 2013). Empirical findings regarding the specificity of mechanisms to contextual or traditional CBTs have been equivocal thus far (Arch et al., 2012; Niles et al., 2014; Forman et al., 2007), and extant studies have generally compared traditional CBT to acceptance-based therapies, rather than those that include explicit and formal mindfulness training.

Moreover, despite the insistence by some researchers that mindfulness represents a strategy for responding to emotions that is counter to the traditional CBT technique of

reappraisal, others suggest that mindfulness may even overlap with reappraisal. In cross-sectional research, decentering has been shown to mediate the relationship between social anxiety and—separately—both reappraisal and mindfulness (Hayes-Skelton & Graham, 2013), suggesting at the very least that these constructs may share a mechanism. In a series of studies, Garland and colleagues have found that positive reappraisal is associated with dispositional mindfulness (as measured both by the FFMQ full scale and subscales) and years of meditation experience (Hanley & Garland, 2014; Hanley, Garland, & Black, 2014), self-reported state mindfulness on the TMS prospectively predicts use of positive reappraisal (Garland, Hanley, Farb, & Froeliger, 2013), and increases in self-reported reappraisal mediate the relationship between increased dispositional mindfulness and stress reduction (Garland, Gaylord, & Fredrickson, 2011).

Because reappraisal is both considered a signature technique of traditional CBT (Hofmann, Heering, Sawyer, & Asnaani, 2009; Beck, 1979) and a potential correlate of mindfulness (Hanley & Garland, 2014; Garland et al., 2013), however, it provides a particularly apt point of comparison when exploring the short-term effects of mindfulness training. Yet, aside from Keng et al. (2013), very few studies have been conducted to date directly comparing mindfulness and reappraisal instructions in a lab setting. Some studies comparing reappraisal to acceptance instructions have found minor advantages for reappraisal in down-regulating negative affect (Hofmann et al., 2009; Szasz, Szentagotai, & Hofmann, 2011). Generally, however, such acceptance instructions have not included the explicit attentional training component of true mindfulness interventions.

In one of the few extant comparisons, Shikatani and colleagues (2014) employed a 40-minute mindfulness intervention modeled after existing experimental and treatment

implementations of mindfulness training, including a therapeutic rationale and psychoeducation (Roemer & Orsillo, 2009; Erisman & Roemer, 2010). Participants with social phobia completed a stressful public speaking task intended to elicit post-event processing (PEP), or negative retrospective review of one's performance. Then they received either mindfulness, cognitive restructuring, or control instructions. Participants rated their level of PEP, state anxiety and state affect before and after the interventions and at follow-up one day later. While both mindfulness and restructuring reduced PEP and improved affect as compared to control, no differences were found in these measures between the active conditions (Shikatani, Antony, Kuo, & Cassin, 2014). Findings from the few direct comparisons of analogue reappraisal and mindfulness interventions thus suggest parity between these two techniques in their effect on negative affect.

Notably, though, Shikatani et al. (2014) and Keng et al. (2013) used meditation instructions that emphasized the acceptance component of mindfulness. Although Keng et al. (2013) employed a relatively shorter intervention and some attentional training, their mindfulness script also included instructions in the acceptance of emotions. Emotional acceptance, while a core component of all definitions of mindfulness, is typically addressed in later sessions of long-term mindfulness interventions (e.g., Segal, Williams, & Teasdale, 2002). In the earlier stages of formal mindfulness training, practitioners typically spend significant time on focused-attention, or concentrative meditation, which emphasizes the self-regulation of attention and enhancing awareness of present-moment physical experience (Lutz et al., 2008). By including both focused-attention and emotional acceptance instructions, brief, laboratory-based mindfulness interventions can perhaps better capture the complexity of mindfulness as a construct. Yet these broader instructions, especially when given to novices, arguably sacrifice

some ecological validity by asking participants to practice, in a short period of time, a range of meditative techniques that would be taught more gradually in a formal training program.

This distinction between early and later formal meditation training is crucial in an experimental comparison of reappraisal and mindfulness. Mindfulness as it is practiced by experienced meditators might differ from reappraisal primarily because it deemphasizes cognitive elaboration and evaluation in favor of acceptance. By contrast, mindfulness for novice meditators might be distinct from reappraisal more in its stipulation of a sustained attentional focus on affectively neutral stimuli like the bodily sensations of breathing. Thus it is essential for such comparative work to assess participants' trait-level mindfulness and/or meditation experience as a potential covariate of a state mindfulness induction.

1.5 Study Goals and Objectives

The present study examined the immediate effects of brief reappraisal and mindfulness interventions on two potential mechanisms of traditional cognitive-behavioral and mindfulness-based therapies: executive control, as assessed by sustained attention and inhibitory control on the Go/No-Go Task, and emotion regulation, operationalized as reduction in self-reported negative affect. As mentioned above, few studies have directly compared these two techniques in a laboratory setting or examined both affective and executive functioning effects within the same study, despite evidence that implicates attention and inhibition in mindful emotion regulation. Keng et al. (2013) conducted the one extant comparison that assessed both cognitive and affective effects, but the proposed study departed from and expanded on their findings in several crucial ways. First, the present study assessed mindfulness' effects on sustained attention, which had not previously been studied in such brief interventions. Second, the study also measured state mindfulness following interventions, to assess the specificity of focused-breathing

meditation in enhancing this understudied construct. Finally, Keng et al. (2013) and others have generally used broader instructions in mindfulness, incorporating acceptance and guided imagery, that depart from the relatively narrow form of focused-breathing meditation typically taught to novices (Lutz et al., 2008). The present study investigated a focused-breathing mindfulness intervention, and compared its effects on sustained attention, inhibitory control and negative affect to those of cognitive reappraisal instructions.

As a study of analogue clinical interventions, the study design generally prioritized internal validity at the expense of external validity. Because previous findings of between-group differences in state decentering and curiosity (as measured by the TMS) have generally yielded small- to medium-size effects (Feldman et al., 2010; Alberts & Thewissen, 2011), efforts were made to minimize other potential sources of between-participant variability. Although no theory or extant evidence suggests systematic sex differences in responsiveness to mindfulness interventions, some evidence suggests that women respond differently—both more severely and with a longer duration—to laboratory mood inductions than men (Albersnagel, 1988; Goodwin & Williams, 1982; Butler & Nolen-Hoeksema, 1994). Recruitment was thus limited to female student participants.

I hypothesized that the focused-breathing mindfulness and reappraisal would produce equivalent reductions in negative affect, as compared to mind-wandering control condition. Given findings that brief meditation can enhance inhibitory control in laboratory settings and longer-term mindfulness training improves sustained attention (e.g., Lee & Orsillo, 2014; Chambers et al., 2008), it was hypothesized that participants receiving mindfulness instructions would demonstrate greater sustained attention and inhibitory control, as compared to both reappraisal and no instructions.

2 METHOD

2.1 Participants

Participants were 143 female undergraduate psychology students recruited from Georgia State University's SONA psychology participant database (see Table 1 for full sample characteristics). To meet inclusion criteria, participants had to be female, 18 years or older, and able to read and write English. Of those, 136 completed all study procedures. Participants were generally of traditional college age ($M = 19.87$, $SD = 5.82$). The sample was racially diverse. Participants were permitted to identify themselves using more than one racial/ethnic category: 56% identified as Black/African-American, 24.8% identified as Asian/Pacific Islander, 13.8% identified as White, 12.8% identified as Hispanic/Latino, 1.8% as Native American/American Indian, and 1.8% as Other. Racial/ethnic demographic data were also recoded as a single categorical variable, with participants reporting more than one racial/ethnic identity coded as "multiracial." Based on this recoding, the sample was 9.2% Hispanic/Latino, 22.9% Asian/Pacific Islander, 6.4% White, 48.6% Black/African-American, 0.9% other, and 10.1% multiracial, with 2 participants (1.8%) not reporting any ethnic or racial identity. Participants were asked whether they currently meditate; 18.7% reported practicing some form of meditation. Of the total sample, 5.1% endorsed meditating on a monthly basis, 11.8% reported meditating weekly, and 1.5% reported meditating daily.

Table 1. Sample Characteristics and Means and Standard Deviations for Baseline Potential Covariates.

	Full Sample (<i>N</i> = 136)	All analyzed (<i>n</i> = 109)	Mindfulness (<i>n</i> = 36)	Reappraisal (<i>n</i> = 36)	Control (<i>n</i> = 37)
Age	19.87 (5.82)	19.32 (3.07)	19.69 (4.83)	19.23 (2.04)	19.05 (1.27)
Race/Ethnicity					
Native American	2.2%	1.8%	2.8%	0%	2.7%
Latino/Latina	11%	12.8%	16.7%	11.1%	10.8%
Asian	34%	24.8%	19.4%	27.8%	27%
White	12.5%	13.8%	25%	2.8%	13.5%
Black/African-American	57.4%	56%	50%	55.6%	62.2%
Other	1.5%	1.8%	2.8%	2.8%	0%
Currently Meditate	18.4%	15.6%	16.7%	16.7%	13.5%
Daily	5.1%	0.9%	0%	0%	2.7%
Weekly	9.6%	9.2%	5.6%	11.1%	10.8%
Monthly	3.7%	5.5%	11.1%	5.6%	0%
GHQ-12	11.75 (5.21)	11.23 (4.92)	10.14 (3.45)	12.33 (5.85)	11.20 (5.05)
ACS	53.38 (7.40)	53.51 (7.01)	54.10 (7.52)	51.22 (7.20)	55.15 (5.79)
PHLMS-Ac	25.72 (7.82)	26.22 (7.51)	26.88 (7.49)	26.89 (7.69)	24.92 (7.38)
PHLMS-Aw	38.00 (5.83)	37.83 (5.84)	38.44 (5.20)	36.28 (5.66)	38.73 (6.44)

Note: GHQ-12 = General Health Questionnaire; ACS = Attentional Control Scale; PHLMS-Ac = Philadelphia Mindfulness Scale – Acceptance subscale; PHLMS-Aw = Philadelphia Mindfulness Scale – Awareness subscale.

2.2 Materials and Design

The study employed an experimental analogue design, with all procedures administered via computer (see Figure 1 for a graphical depiction of the study design). Participants (*N*=136) were randomly assigned to one of three conditions: focused-breathing mindfulness (*n* = 43), cognitive reappraisal (*n* = 47), and mind-wandering control (*n* = 46). After baseline assessments and mood rating, participants completed a negative mood induction, followed by a second mood rating. Next, in the intervention phase, participants listened to audio-recorded training and

practice instructions in their assigned emotion regulation (or control) strategy. An assessment of state mindfulness and a third mood rating followed the intervention. Finally, participants completed the Go/No-Go task, a behavioral measure of sustained attention and inhibitory control, as well as demographics measures.

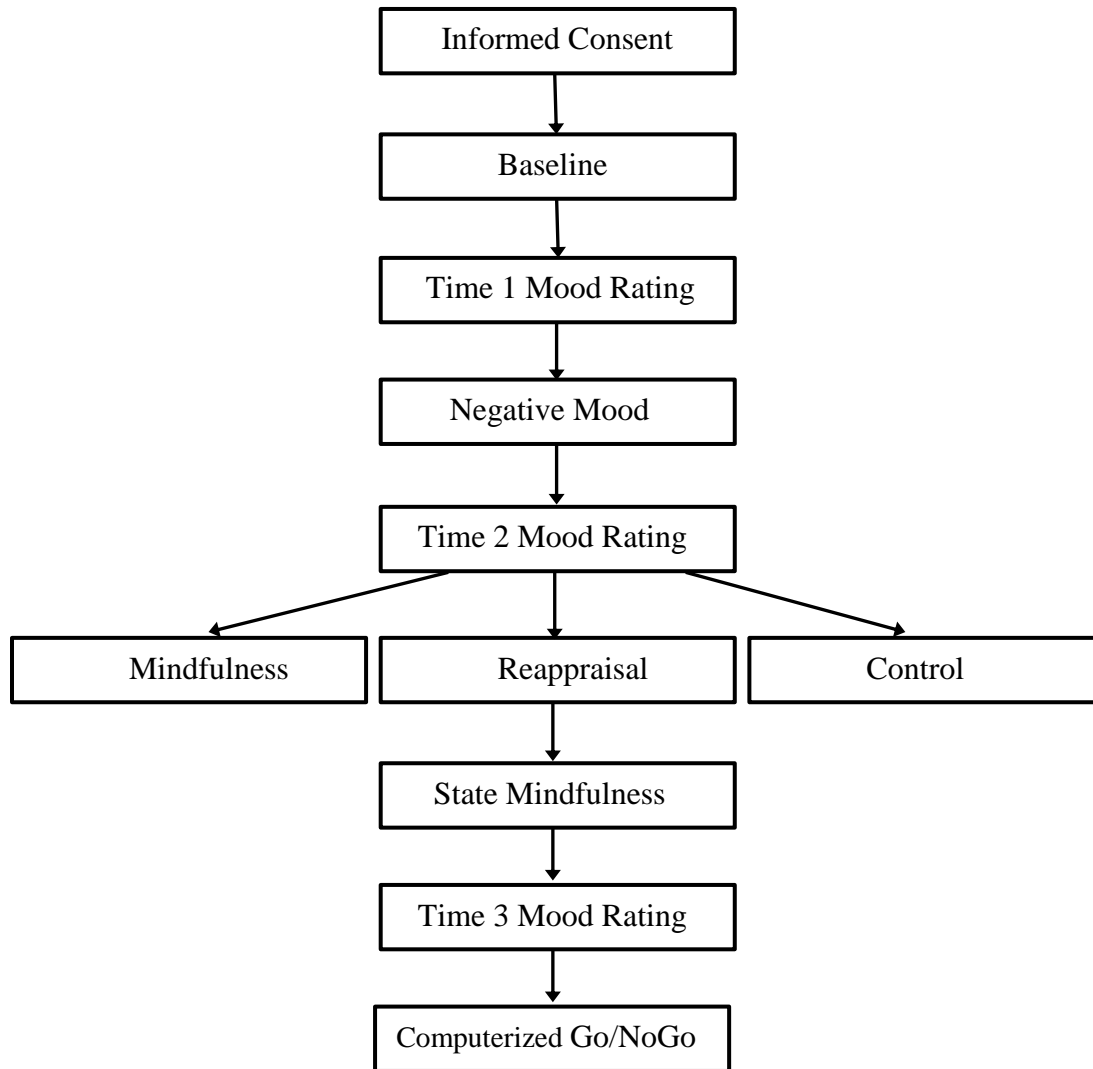


Figure 1. Schematic diagram of experimental design.

2.3 Measures

2.3.1 *Trait mindfulness.*

The Philadelphia Mindfulness Scale (PHLMS; Cardaciotto et al., 2008) was used to examine participants' dispositional, or trait mindfulness. The PHLMS is comprised of 20 items based on the Bishop et al. (2004) and Kabat-Zinn (1994) definitions of mindfulness. Consistent with the Bishop model, the PHLMS has two 10-item subscales assessing awareness (e.g., "When I am startled, I notice what is going on inside my body") and acceptance (e.g., "If there is something I don't want to think about, I'll try many things to get it out of my mind"). Respondents rate items on a 5-point, Likert-type scale (1 = "never," 5 = "very often") based on how often they experienced each in the past week. Awareness items are summed to obtain a subscale score, with higher scores indicating greater awareness, while acceptance items are reverse-scored before summing, with higher scores reflecting greater acceptance. Both the acceptance and awareness subscales have good internal consistency in non-clinical (α ranging from .75 to .86) and clinical ($\alpha = .75$ for both subscales) samples (Cardaciotto et al., 2008; Butryn et al., 2011). In a student sample, both acceptance and awareness subscales were significantly correlated with scores on the MAAS, while only awareness scores were correlated with the MAAS in a general psychiatric sample (Cardaciotto et al., 2008). In a non-clinical sample, acceptance scores were negatively correlated with thought suppression, rumination and depressive symptoms (Cardaciotto et al., 2008), while awareness scores were correlated with response-time variability on a continuous performance task (Ruocco & Direkoglu, 2013). Both total scores and subscale scores from the PHLMS have been used previously (Ruocco & Direkoglu, 2013). For the present study, the acceptance (PHLMS-Ac) scale demonstrated good

internal consistency ($\alpha = .87$), while the awareness scale (PHLMS-Aw) demonstrated acceptable internal consistency as well ($\alpha = .78$).

2.3.2 *State mindfulness.*

The Toronto Mindfulness Scale (TMS; Lau et al., 2006) was used both as a manipulation check, to verify that the meditation group demonstrated greater state mindfulness relative to the comparison conditions at post-intervention. The TMS is a self-report measure intended to assess state mindfulness retrospectively, with respect to a preceding period of actively practicing mindfulness. Explicitly developed to fit the Bishop et al. (2004) model of mindfulness, the TMS consists of 13 statements, rated on a four point scale (0 = “not at all, 4 = “very much”), describing what the respondent “just experienced.” Exploratory and confirmatory factor analyses supported a two-factor structure. The 7-item Decentering subscale assesses “awareness of one’s experience with distance and disidentification rather than being carried away by one’s thoughts and feelings,” while the 6-item Curiosity subscale reflects “awareness of present moment experience with a quality of curiosity” (Lau et al., 2006, p. 1452). Sample decentering items include “I was open to taking notice of anything that might come up,” and “I experienced myself as separate from my changing thoughts and feelings.” Sample curiosity items include “I was curious about my reactions to things” and “I was curious about each of the thoughts and feelings I was having.” Lau and colleagues (2006) report internal consistencies (Cronbach’s α) of .84 for the Decentering scale and .86 for the Curiosity scale in an adult sample with meditation experience ranging from zero to 17 years. In a recent study of 10-minute mindfulness instructions with a nonclinical student sample, internal consistencies were .52 for Decentering, .85 for Curiosity, and .76 for the total TMS score (Ortner & Zelazo, 2014). Other studies with non-meditating students have obtained Cronbach’s α of .83 to .90 for Curiosity and .63 to .69 for

Decentering (Quickel, Johnson, & David, 2014; Erisman & Roemer, 2010; Frewen, Lundberg, MacKinley, & Wrath, 2011). For the present sample, the Curiosity subscale had a Cronbach's α of .87 and the Decentering subscale had a Cronbach's α of .74.

2.3.3 Psychological distress.

Total scores on the General Health Questionnaire (GHQ-12; Goldberg, 1978) were examined as a potential covariate to control for baseline between-condition differences in psychological distress. The GHQ-12 is a self-report assessment of general psychological health. Participants rate the frequency with which they experience various behavioral and psychological stressors on a 4-point, Likert-type scale, ranging from 0 (*not at all*) to 3 (*much more than usual*). Scores range from 0 to 36, with higher scores indicating greater psychological distress. In other studies with the proposed study population, the GHQ-12 has demonstrated adequate internal consistencies of .87 and .88 (Masuda, Price, Anderson, Schmertz, & Calamaras, 2009; Masuda & Wendell, 2010). In the present study, the GHQ-12 also demonstrated adequate internal consistency ($\alpha = .83$).

2.3.4 Trait attentional control.

Baseline levels of attentional control, measured by the Attentional Control Scale (ACS; Derryberry & Reed, 2002), were examined as another potential covariate. The ACS is a 20-item, self-report measure of trait-level attentional control. Items assessing the ability to focus (e.g., "When trying to focus my attention on something, I have difficulty blocking out distracting thoughts") and shift attention (e.g., "I have trouble carrying on two conversations at once") are rated on a 4-point, Likert-type scale (1 = "almost never," 4 = "always"). Exploratory and confirmatory factor analyses support a two factor model, composed of focusing and shifting factors (Olafsson et al., 2011). In a recent study with a healthy student sample internal

consistency was adequate ($\alpha = .77$), and total scores on the ACS were moderately correlated with trait mindfulness as measured by both the FFMQ and MAAS (Brown, Goodman, & Inzlicht, 2013). For the present study, internal consistency was also adequate ($\alpha = .80$).

2.3.5 Negative affect.

At three time points, participants used a computer-displayed Visual Analogue Scale (VAS) to indicate their current level of sadness. A horizontal line, with numbered and descriptive anchors on opposite ends (0 = *Not at all sad*, 100 = *Extremely sad*) was displayed and participants were prompted to move a sliding indicator to the position that best reflected their current mood. The VAS is a widely used method of rapidly assessing within-subject changes in negative and positive affect and has shown sensitivity to affective changes following brief mindfulness, reappraisal, worry and rumination inductions (e.g., Arch & Craske, 2006; Keng et al., 2013; Singer & Dobson, 2007). Visual analog scales assessing changes in state anxiety have demonstrated good test-retest reliability and sensitivity to a variety of laboratory stressors, as well as concurrent validity with multiple-item self-report scales (for a review, see Rossi & Pourtois, 2012). Psychometric data on VAS assessing mood are more limited; Ahearn and Carroll (1996) found evidence for good test-retest reliability over 1 hour in a clinical sample.

2.3.6 Sustained attention and inhibitory control.

Total omission and commission errors on the Go/No-Go Task (Mueller & Piper, 2014) were used to assess participants' sustained attention and inhibitory control, respectively. The Go/No-Go Task is a free, open-source implementation in the Psychology Experiment Building Language (PEBL; Mueller & Piper, 2014) of a computerized continuous performance task assessing sustained attention and inhibitory control (see Figure 2 for a screen shot of the computer program). Participants are required to respond to target letters by pressing a keyboard

button while refraining from responding to non-targets. Targets and non-targets are presented sequentially, in a pseudorandom order, with letters appearing in one of four quadrants of a square in the center of the screen. Each stimulus presentation lasts 500 milliseconds, with an inter-stimulus interval of 1500 milliseconds. The PEBL version has two conditions: in the first condition, the target letters are *Ps*, and non-targets are *Rs*.” In the second condition, target and non-target rules are reversed, but the ratio of *Ps* to *Rs* remains the same (80:20, *P:R*), so that the first condition assesses performance with high frequency targets and the second with low-frequency targets. Each condition consists of 160 trials (presentation of a single stimulus), for a total of 320 trials and approximate task duration of 8 minutes. A brief training phase precedes each condition, and on-screen instructions inform participants that they are permitted to take a brief break between the first and second conditions.

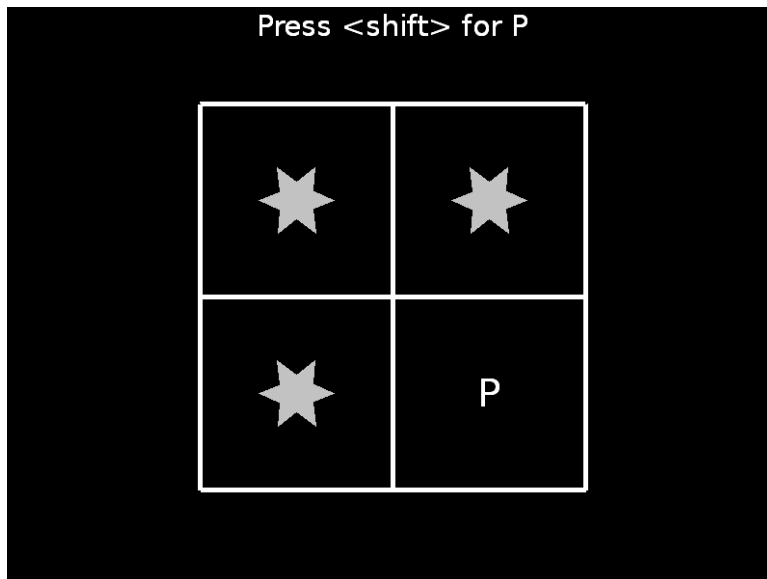


Figure 2. Screen shot of Go/No-Go Task (Mueller & Piper, 2014).

Omission errors, or failures to respond to a target, are believed to index inattention; thus fewer omission errors are indicative of greater sustained attention. Commission

errors, or responses to non-targets, are considered an index of impulsivity or failure of inhibitory control (Barkley, 1991).

Parameters for the PEBL Go/No-Go are identical to those used by Bezdjian et al. (2009) in a study of impulsivity and inattention in children. That study found that commission errors were significantly inversely related to hyperactivity/impulsivity symptom ratings and hit reaction times, suggesting that commission errors are a valid index of impulsive responding. Omission errors have also been examined, in both Bezdjian et al.'s version of the Go/No-Go and other continuous performance tasks, as an index of sustained attention (Barkley, 1991).

2.4 Mood Induction

After completing a battery of self-report measures including the potential covariates listed above, participants completed a modified, negative autobiographical recall exercise based on a model by Brewer, Doughtie, and Lubin (1980). Participants were prompted to think and then write about three negative life events that made them feel “sad,” “defeated,” “lonely,” “rejected,” or “hurt.” As the addition of music has been found to enhance the effectiveness of the mood induction procedures (e.g., Clark, 1983; Martin, 1990; Westermann, Spies, Stahl & Hesse, 1996), participants also listened to a 10-minute audio recording of mood-suggestive music (“Adagio in G-minor,” by Albinoni) while completing the mood induction procedures. Such a combined procedure has previously been effective at inducing negative mood in depressed (Huffziger & Kuehner, 2009), sub-clinically depressed (Keng et al., 2013), and healthy undergraduate participants (Hussey & Barnes-Holmes, 2012).

2.5 Experimental Manipulations

Instructions for the mindfulness condition (see Appendix E) were adapted from scripts used by Kiken and Shook (2011) and Keng et al. (2013), which were themselves adapted from

frequently used instructions by Singer and Dobson (2007) and Arch and Craske (2006). To align the mindfulness intervention with the focused-attention meditation instruction typically provided to novices (Lutz et al., 2008; Segal et al., 2002), these instructions emphasized the self-regulation of attention and awareness of present-moment experience, with less explicit emphasis on the nonjudgmental acceptance of that experience. The combined instructions directed participants to focus their attention on the present-moment sensations of their breath (e.g., “focus your awareness on the physical sensations of your breathing”). The instructions also explained that unpleasant thoughts or feelings may arise (“You may notice... that your mind has wandered... is instead lost in thought or preoccupied with a feeling. These thoughts may be pleasurable, or they may be unpleasant.”). The instructions normalized this process and encouraged participants to accept these thoughts or feelings (“This is normal, and there’s no need to judge it.”). The instructions then stipulated that participants should “gently” refocus their attention on their breath.

The reappraisal instructions (see Appendix F) were adapted from those used by Keng et al. (2013). Participants were directed to try thinking of positive aspects of one of the negative life events they recalled previously or to imagine what an external observer would think of the event. In the mind-wandering control condition (see Appendix G), participants received instructions, based on those used by McHugh et al. (2012), to sit quietly, allow their thoughts to roam freely and wait for additional instructions after a brief period. To control for demand characteristics unrelated to the distinguishing features of the two active interventions (mindfulness and reappraisal), instructions in all conditions began with a brief rationale for the relevant approach to regulating emotions and included reminder statements interspersed with periods of silence. In

addition, instructions were matched for duration (10 minutes) and approximate word count (608 words for mindfulness, 613 words for reappraisal, 426 words for mind wandering).

2.6 Procedures

All experimental procedures were conducted in Dr. Masuda's laboratory space and administered via computer. Participants meeting inclusion criteria were assigned (via quasi-random assignment) to one of three conditions: mindfulness, reappraisal or control. To determine the order in which participants were assigned to conditions, a random sequence of numbers (integers ranging from 1 to 3, each corresponding to an experimental condition) was generated using an online tool. This sequence was matched to a list of participant numbers. Self-report measures, mood induction, interventions and the cognitive flexibility task were all administered by computer. On-screen prompts directed participants to notify the experimenter when assistance was needed (e.g., when opening the computer program to administer the Go/No-Go Task). Participants completed baseline questionnaires (PHLMS, GHQ, and ACS) and a baseline VAS mood rating (Time 1), and then were prompted to put on headphones and listen to a series of audio exercises. First, all participants received 10-minute mood induction instructions prompting rumination about a negative life event. After completing a second mood rating scale (Time 2), participants heard 10-minute audio instructions specific to their assigned intervention. Following the intervention, participants rated their mood a third time, completed the state mindfulness assessment (TMS), and then completed the Go/No-Go Task.

2.7 Data Analysis

All preliminary and main statistical analyses were conducted using IBM SPSS Statistics 21 for Windows.

2.7.1 Potential covariates.

Demographic variables (race, age, meditation practice) and baseline self-report measures (trait mindfulness, psychological distress, and attentional control) were examined as potential covariates in the main analyses. First, between-condition differences were assessed for each potential covariate. Categorical variables were evaluated with chi-square tests. Continuous variables were evaluated with ANOVAs if they met assumptions of linearity, normality, and homoscedasticity. Significant omnibus ANOVAs were further explored with post hoc tests. For continuous variables not meeting ANOVA assumptions, non-parametric tests were used. Any potential covariate with significant between-group differences was then examined further to assess the presence of a significant relationship to the outcome variables in the main planned analyses. In the case of categorical variables, ANOVAs were used to assess significant differences in outcome variables based on the potential covariate, with a significant ANOVA indicating inclusion of that covariate in analyses of the relevant outcome variable. For continuous variables, zero-order correlations between the potential covariate and outcome variable were examined. In the event of a significant correlation, the continuous covariate was included in all analyses involving the outcome variable with which it was significantly associated.

2.7.2 Data transformations and manipulation check.

Self-report outcomes (VAS sadness ratings at all three time points, TMS scale scores) were examined for normality and homoscedasticity, using skewness and kurtosis statistics, as well as normality plots and histograms. Variables for which skewness or kurtosis values or normality plots indicated significant concerns regarding non-normality or heteroscedasticity were transformed, using a square-root transformation. For variables with a high number of zero scores (e.g., VAS sadness ratings), the square-root transformation was performed after first adding a constant (i.e., $x_1 = \sqrt{(x + 1)}$). The transformed variables were then examined for the same assumptions.

2.7.3 Between-group differences in sadness.

A 3 (Condition [mindfulness, reappraisal, mind-wandering control]) \times 3 (Time) repeated-measures ANOVA was conducted to evaluate the effect of condition on VAS sadness ratings before mood induction, after mood induction, and after the intervention phase. Planned contrasts (weights: 1, 1, and -2, for mindfulness, reappraisal, and control, respectively) were employed to examine significant between-group effects, while pairwise comparisons were used to examine significant within-group effects. If the assumption of sphericity was not met, either Greenhouse-Geisser or Huynh-Feldt corrected degrees of freedom and p values were used. A power analysis was conducted using G*Power (Faul, Erdfelder, Lang, & Buchner, 2007) to determine an adequate sample size for detecting a small effect size ($f = .15$, $\alpha = .05$) for a 3×3 repeated-measures ANOVA. The power analysis indicated that a sample size of 93 was necessary to detect a small effect with a power of .80.

2.7.4 Between-group differences in sustained attention and inhibitory control.

For Go/No-Go task outcome measures (Omission or Commission errors) meeting assumptions of normality, one-way ANOVAs with post-hoc comparisons were conducted to examine the effect of condition on sustained attention and/or inhibitory control.

3 RESULTS

3.1 Mood induction manipulation check.

Of the 136 participants who completed all study procedures, 27 (19.9%) reported a reduction or no change in sadness from the Time 1 to Time 2 VAS mood ratings and were excluded from subsequent analyses. There were no significant differences between conditions in the number of participants excluded for failing to meet the mood induction criterion, $\chi^2(2, N = 136) = 0.72, p = .70$.

3.2 Missing data.

Item-level missing data analysis was conducted for all self-report measures and indicated that 14 participants (12.84%) had one or more missing item values. The total number of missing values was 17, which represented 0.24% of the total number of values, and no variable had more than 1.5% missing values. The maximum number of missing values for a single participant was 3. Using estimation-maximization (EM) methods, missing values were imputed was used to impute missing values (Schafer & Graham, 2002).

Due to administrative error, Go/No-Go task data were incomplete for one additional participant. This participant's data were excluded from analyses of the main Go/No-Go outcomes (omission and commission errors; indices of sustained attention and inhibitory

control), leaving a sample size of $N = 108$. However the participant's data were included in analyses of self-report outcomes (VAS sadness ratings and TMS scores).

3.3 Preliminary analyses

Table 1 displays demographic characteristics for the 109 participants meeting mood induction criteria. A one-way ANOVA revealed no significant differences between conditions in participant age. Using racial identity recoded as a single variable, a chi-square analysis revealed no significant differences in the distribution of participant race between conditions. Similarly, chi-square analyses indicated no significant between-condition differences in the number of participants endorsing current meditation practice or in the number of participants reporting daily, weekly, or monthly meditation practice (all $ps > .05$).

Table 2 displays descriptive statistics for baseline measures and primary outcome measures. Baseline levels of psychological distress (i.e., GHQ), trait mindfulness (i.e., PHLMS) and attentional control (i.e., ACS) were examined for between-group differences that might influence response to the experimental manipulation. Visual inspection of box plots and Levene's test for equality of variances indicated significant concerns regarding heteroscedasticity in GHQ scores across conditions. Normal P-P and Q-Q plots and normality tests were also indicative of significant concerns regarding the assumption of normality for PHLMS scores in the mindfulness and control conditions. Thus separate nonparametric Kruskal-Wallis tests were conducted for PHLMS and GHQ scores across all three conditions. The Kruskal-Wallis test indicated that distributions for GHQ across conditions were not significantly different ($p = .302$) and that PHLMS scores were distributed similarly across conditions as well ($p = .582$), suggesting no significant baseline differences between groups in psychological distress or trait mindfulness.

Table 2. Means and Standard Deviations for Primary Outcome Measures, for Overall Sample and by Condition.

	<u>Overall</u> (<i>N</i> = 109)	<u>Mindfulness</u> (<i>n</i> = 36)	<u>Reappraisal</u> (<i>n</i> = 36)	<u>Control</u> (<i>n</i> = 37)
VAS 1	12.72 (18.13)	11.47 (13.48)	17.61 (21.98)	9.19 (17.36)
VAS 2	39.01 (26.70)	39.06 (24.87)	39.81 (26.31)	38.19 (29.38)
VAS 3	18.74 (19.88)	18.08 (19.19)	22.53 (20.40)	15.68 (19.96)
Mood Induction	26.28 (20.54)	27.58 (16.96)	22.19 (18.73)	29.00 (24.87)
Intervention	-20.28 (17.34)	-20.97 (13.16)	-17.28 (15.1)	-22.51 (22.30)
TMS-Curiosity	21.02 (4.72)	20.33 (5.29)	20.72 (3.62)	21.97 (5.03)
TMS-Decentering	22.49 (4.44)	22.14 (4.30)	21.64 (3.59)	23.67 (5.13)
Go/No-Go Comissions	9.20 (5.347)	8.83 (4.99)	8.69 (5.60)	10.30 (5.29)
Go/No-Go Omissions	2.47 (14.76)	0.64 (1.52)	0.80 (1.11)	1.76 (3.77)

Note: Statistics for Go/No-Go Omission and Commission errors in Reappraisal condition

exclude one participant for whom data were incomplete due to administrative error (*n* = 35).

VAS = Visual Analog Scale sadness rating; Mood Induction = change score in Visual Analog Scale sadness from Time 1 to Time 2; Intervention = Visual Analog Scale sadness change score from Time 2 to Time 3; TMS = Toronto Mindfulness Scale.

Based on visual inspection of normality plots and tests for normality and homoscedasticity, ACS, PHLMS-Aw, and PHLMS-Ac scores met assumptions required for ANOVA. A MANOVA was conducted on PHLMS-Aw and PHLMS-Ac scores. Using Pillai's trace, there was no significant effect of condition on acceptance or awareness scores, $V = .05$, $F(4, 212) = 1.23$, $p = .28$. Results from a one-way ANOVA on ACS scores indicated a significant omnibus difference in mean ACS scores across all three conditions, $F(2, 106) = .318$, $p < .05$. Post-hoc comparisons using a Bonferroni correction revealed that baseline attentional control was significantly greater in the mind wandering control than in the reappraisal condition ($p < .05$).

Table 3 displays bivariate correlations between all baseline measures and all primary outcome measures. Regarding potential covariates, ACS scores were significantly related to sadness VAS ratings at Time 1 and Time 3, such that greater attentional control was associated with lower sadness ($p < .01$). Because of the baseline difference in attentional control between the control and reappraisal conditions, ACS scores were included as a covariate in subsequent between-groups analyses of VAS sadness ratings. Because no potential covariates were significantly related to Go/No-Go outcomes (Omission or Commission errors; indices of executive control), none were included in analyses.

Table 3. Bivariate Correlations among all Baseline and Outcome Measures.

	1	2	3	4	5	6	7	8	9	10	11	12
1. VAS1	1											
2. VAS2	.64**	1										
3. VAS3	.68**	.76**	1									
4. Commis	.01	.12	.05	1								
5. Omiss	-.08	-.01	-.07	-.14	1							
6. GHQ	.61**	.37**	.48**	-.06	.05	1						
7. ACS	-.37**	-.17	.26**	-.02	.07	-.45**	1					
8. PHLMS-Ac	-.32**	-.22*	.21*	-.01	.06	-.43**	.26**	1				
9. PHLMS-Aw	.03	.07	.09	-.11	.06	.00	.29**	-.20**	1			
10. PHLMS	-.26**	-.24*	-.24*	-.09	.09	-.38**	.42**	.74**	.51**	1		
11. TMS-C	.19*	.18	.13	-.03	.02	.13	-.05	-.17	.14	-.06	1	
12. TMS-D	.10	.10	.13	-.05	.08	-.05	.12	-.14	.23**	.03	.61**	1
13. TMS	.16	.16	.01	-.04	.06	.05	.04	-.17	.20**	-.02	.90**	.89**

Note: $N = 109$. * $p < .05$, ** $p < .01$. Commis = Go/No-Go Total Commission Errors; Omiss = Go/No-Go Total Omission Errors; TMS-C = Toronto Mindfulness Scale – Curiosity Subscale; TMS-D = Toronto Mindfulness Scale – Decentering Subscale

3.4 Mindfulness manipulation check

As a manipulation check to assess the effectiveness of the mindfulness intervention in increasing state mindfulness, a MANCOVA was conducted with TMS-Decentering and TMS-Curiosity scores as the dependent variables, group as the between-subjects variable, and ACS scores entered as a covariate. No significant omnibus effects were obtained, indicating that there were no differences between conditions in state decentering or curiosity following the intervention phase. Implications of this null finding are explored further in the Discussion.

3.5 Hypothesis 1: Equivalent reductions in negative affect across mindfulness and reappraisal, as compared to control

Hypothesis 1 was evaluated using a 3 (Condition) \times 3 (Time) repeated-measures ANCOVA, with VAS mood ratings as the dependent variable, and ACS scores included as a covariate. Mauchly's test indicated that the assumption of sphericity had been violated, $\chi^2(2) = 12.68, p = .002$, therefore degrees of freedom were corrected using Huynh-Feldt estimates of sphericity ($\epsilon = .93$). Results indicated that, when controlling for baseline attentional control, sadness ratings did not significantly differ as a function of time, $F(1.91, 196.61) = 1.47, p = .23, \eta^2_{\text{partial}} = .014$. Because the main effect of time was not significant, interactions and planned contrasts were not interpreted. Figure 3 displays the marginal means for VAS sadness ratings by condition, controlling for baseline attentional control. Visual inspection of the marginal means suggested that, across conditions, mean sadness ratings with the mood induction (Time 1 to Time 2) and then decreased with the intervention (Time 2 to Time 3). Indeed, across conditions, the change in mean sadness from Time 1 ($M = 12.72, SD = 18.13$) to Time 2 ($M = 39.01, SD = 26.70$), represented a large effect size ($d = 1.09$), as did the change in mean sadness from Time 2 to Time 3 ($M = 18.74, SD = 19.88; d = -0.812$).

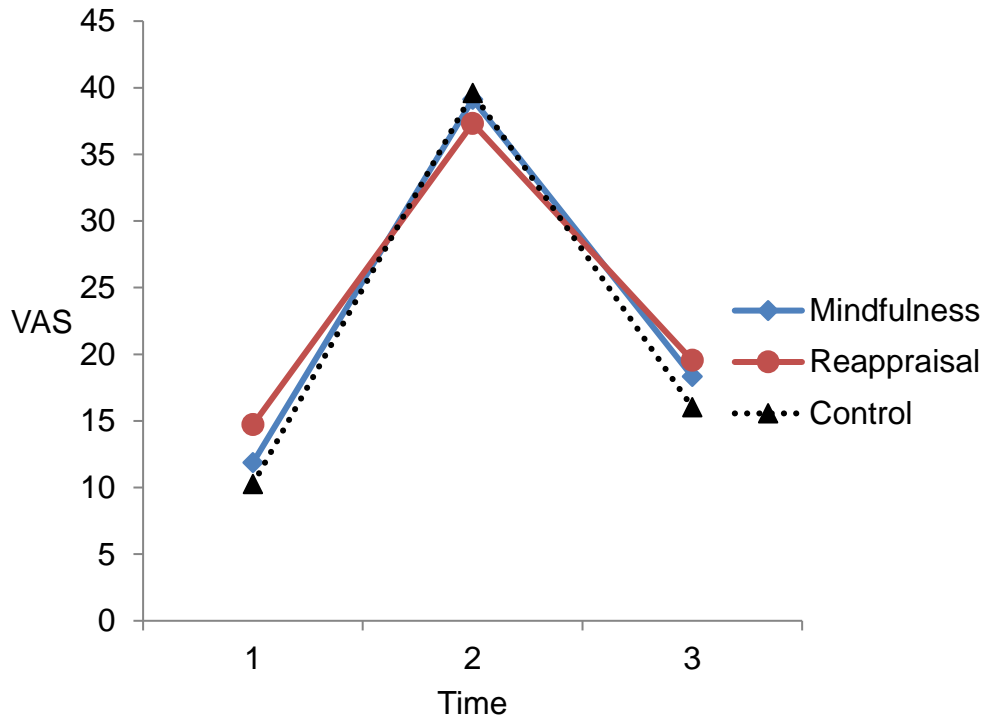


Figure 3. Estimated marginal mean sadness VAS ratings by condition, controlling for baseline attentional control.

3.6 Hypothesis 2: Mindfulness increases sustained attention and inhibitory control over reappraisal and control

Based on inspection of normality plots and skewness and kurtosis statistics, the assumption of normality was justified for Go/No-Go Commission errors (i.e., the index of impulsivity or failures of inhibitory control). Levene's test suggested that the assumption of homoscedasticity was also justified, $F(2, 105) = 0.31, p = .74$. Thus, a univariate ANOVA was conducted to evaluate the effect of condition (mindfulness vs. reappraisal vs. control) on Go/No-Go Commission errors. There were no significant differences between condition in mean number of Commission errors was not significant, suggesting that the groups did not differ in inhibitory control following the intervention phase, $F(2, 105) = 1.03, p = .36$.

Normality plots and tests indicated that the distribution of Omission errors (i.e., the index of sustained attention) was significantly non-normal in all three conditions, with skewness values of 3.06 ($SE = .39$) in the mindfulness condition, 1.94 ($SE = .40$) in the reappraisal condition, and 3.5 ($SE = .40$) in the control condition. Levene's test indicated significant concerns with heteroscedasticity, $F(2, 105) = 4.00, p < .05$. With similar sample sizes across groups, ANOVA can be considered robust to violations of normality and heteroscedasticity (Schmider, Ziegler, Danay, & Buhner, 2010; Rogan & Kesselman, 1977). Given the similar sample sizes in each condition, a univariate ANOVA was conducted on Go/No-Go Omission errors. The omnibus effect of condition on Omission errors was not significant $F(2, 105) = 2.22, p = .11, \eta^2_{\text{partial}} = .04$. Despite the nonsignificant omnibus finding, the small-to-medium effect size for condition suggests some relationship between condition and sustained attention on the Go/No-Go task.

Despite the reputation of ANOVA as robust to violations of the normality assumption, other analytic methods may be more appropriate to the analysis of such positively skewed data. In particular, low-frequency count data such as Go/No-Go omission errors may not be appropriately analyzed with ANOVA methods because they are bounded at zero. Transforming a skewed dependent variable using a log or square-root transformation and then using ordinary least squares (OLS) regression is an option as well. However, this method has been found inadequate for count data with large numbers of zero scores, as found in the current sample. Models based on the Poisson distribution have been found to be appropriate for such frequency data (Swartout, Thompson, Koss, & Su, 2015). Negative binomial regression in particular is appropriate for *over-dispersed* count data, for which the conditional variance exceeds the conditional mean at most levels of a predictor.

Negative binomial regression was performed on total omission errors with participant condition as the single categorical predictor. Model results are displayed in Table 4. Maximum likelihood estimation was used for parameter estimates. The overall model was significant, $\chi^2(2) = 6.45, p < .05$. The dispersion parameter was estimated at 2.24, 95% CI [1.35, 3.71]. The confidence interval for this parameter indicated that Go/No-Go Omission errors were likely over-dispersed and that the negative binomial distribution is thus a better fitting approximation of the sample data than the Poisson distribution. As compared to the control condition, assignment to the mindfulness condition was significantly negatively associated with omission errors. Model parameters in a negative binomial model can be exponentiated and interpreted as incident rate ratios (IRR), indicated the degree to which the incidence of the outcome count variable changes with respect to a change in the predictor. For the categorical predictor of participant condition in the present model, the IRR for a particular condition represents the change in incidence of Go/No-Go omission errors from the reference category (control). The mindfulness intervention was associated with a nearly two-thirds lower rate of omission errors, as compared to control, IRR = 0.36, 95% CI [.158, .838]. Assignment to the reappraisal condition, as compared to control, was not significantly associated with a change in the incidence of omission errors, although this relationship approached significance, $p = .06$, IRR = 0.46, 95% CI [0.20, 1.04]. When the model was re-run with reappraisal coded as the reference group, mindfulness was not associated with a significant change in omission error rate, $p = 0.62$, IRR = 0.8, 95% CI [0.33, 1.93]. Mind wandering was marginally significantly associated with increased omission errors, $p = .06$, IRR = 2.2, 95% CI [0.97, 4.99]. Thus, based on results of the negative binomial regression model, assignment to the mindfulness condition was associated with significant

improvement in sustained attention, as indexed by Go/No-Go task omission errors, as compared to the control condition.

Table 4. Negative Binomial Regression Models Predicting Total Go/No-Go Omission errors.

Variable	<i>b</i>	<i>S.E.</i>	<i>p</i>	95% CI	<i>IRR</i>	95% CI
Model 1						
Condition = Mindfulness	-1.01	0.43	0.02	[-1.85, -0.18]	0.36	[0.15, 0.84]
Condition = Reappraisal	-0.79	0.42	0.06	[-1.61, 0.03]	0.45	[0.2, 1.04]
Condition = Control	0					
Model 2						
Condition = Control	0.79	0.42	0.06	[-0.03, 1.61]	2.2	[0.97, 4.99]
Condition = Mindfulness	-0.23	0.45	0.62	[-1.11, .66]	0.8	[0.33, 1.93]
Condition = Reappraisal	0					

Note: $N = 108$; $\chi^2(2) = 6.45$, $p < .05$.

4 DISCUSSION

The current study sought to compare the effects of brief, laboratory-based mindfulness and reappraisal training on experimentally-induced sad mood, sustained attention and inhibitory control. Given the experimental literature supporting the effectiveness of both mindfulness and reappraisal (e.g., Keng et al., 2013; Wolgast, Lundh, & Viborg, 2011; Arch & Craske, 2006; Huffziger & Kuehner, 2009), I hypothesized that both strategies would reduce sadness as compared to a mind-wandering control condition, with no difference in sadness reduction between the two active conditions. Results did not support this hypothesis. Based on findings from previous analogue experiments (Wenk-Sormaz, 2005; Keng et al., 2013; Lee & Orsillo, 2014), I also hypothesized that mindfulness training would increase sustained attention and inhibitory control as compared to either reappraisal or mind-wandering. Results partially supported this hypothesis, regarding the effect of mindfulness training on sustained attention as compared to the mind-wandering control condition.

4.1 Group Differences in Sadness Ratings

Contrary to my hypothesis, no significant main effects or interactions of time or condition on sadness rating were found, when controlling for baseline attentional control. Equivalent levels of post-intervention sadness in the mindfulness and reappraisal conditions were expected, and this finding is consistent with the few other extant comparisons of these techniques in a laboratory setting (Keng et al., 2013; Shikatani, et al., 2014). However, the absence of any benefit to mindfulness or reappraisal with respect to mind-wandering is unexpected. Mind-wandering or no-instructions control conditions have often been found to be inferior to mindfulness in reducing negative affect (Keng et al., 2013; Arch & Craske, 2006; Singer in

Dobson, 2007). Moreover, mind-wandering has independently been associated with negative affect (Killingsworth & Gilbert, 2010).

Several alternative accounts might help explain the null finding. One possibility is that the mind-wandering condition was in fact as effective as the mindfulness and reappraisal conditions. Studies in both clinical and nonclinical samples have suggested that 10-minute instructions to engage in mood-incongruent distraction—without a specific object of focus—can reduce experimentally-induced sadness, in some cases as effectively as mindfulness or reappraisal (Singer & Dobson, 2007; Broderick, 2005). Rather than engaging in sadness-congruent rumination following the sad-mood induction, participants in the mind-wandering condition may have instead been distracted by positive or affectively neutral thoughts that improved their affect (Lyubomirsky, Caldwell, & Nolen-Hoeksema, 1989).

In addition to mind-wandering being more efficacious, it is also possible that the two active conditions were less efficacious in reducing the experimentally-induced negative mood than anticipated. More specifically, it is possible to speculate that the active interventions were too brief in length or “dosage” to alter the process that they were intended to influence (e.g., state mindfulness), at least for the present sample. Although significant effects of brief mindfulness training on negative affect have been found in comparison to mind-wandering in previous analogue experimental studies (Arch & Craske, 2006; Lee & Orsillo, 2014), there is variability in findings between novice and experienced meditator samples. Differential effects between mindfulness and control conditions may not reliably emerge at the level of such brief interventions or such a low “dosage.” As will be discussed in greater detail below, the absence of between-condition differences in state mindfulness raises the possibility that the interventions

were inadequate to target and selectively increase the state mindfulness construct specified by the state mindfulness scale (i.e., the Toronto Mindfulness Scale) used for the present study.

Another possibility is that nonspecific features of the control instructions accounted for its parity with the active conditions. Unlike many other experimental comparisons of mindfulness to mind-wandering, the present study matched the mind-wandering instructions to the active conditions on certain nonspecific factors. Instructions for mind-wandering presented it as “a strategy for dealing with negative emotions,” asked participants to sit in a comfortable and upright position, and offered a putative clinical rationale for the strategy (“Sometimes we don’t have time to let our minds wander or think through everything with all that goes on. ... Sometimes we want some time just to think”). The instructions were roughly matched with the two active conditions for word count and included brief reminder prompts throughout the 10-minute intervention phase. Instructions for all three conditions were also read by the same voice in a relaxing manner. A significant, if controversial, body of literature (Rosenzweig, 1936; Wampold et al., 1997; Wampold, 2007; Marcus, O’Connell, Norris & Sawaqdeh, 2014) supports the efficacy of nonspecific factors in psychotherapy, such as clinical rationale and therapist warmth. It is possible that the apparent benefits of brief mind-wandering for sadness found in this study reflect an exceptionally “robust” or active control condition.

4.1.1 Manipulation check and state mindfulness.

The absence of a significant main effect of condition on sadness ratings should also be understood within the context of the state mindfulness manipulation check. No differences were found between conditions in either decentering or curiosity following the intervention phase. Because the TMS was not administered prior to intervention, it is impossible to determine whether state mindfulness changed as a result of the interventions. Although the PHLMS is a

measure of trait, and not state, mindfulness, the lack of baseline differences in either PHLMS subscale suggests that participants entered with equivalent propensities to experience states of mindful awareness and acceptance. Thus it is possible that the training instructions did not successfully or specifically target the independent variable of interest: state mindfulness, as measured by the TMS subscales of curiosity and decentering. In particular, the present study's mindfulness instructions prioritized the first element of the Bishop et al. (2004) definition, self-regulation of attention, in asking participants to maintain their focus on their breath, notice when their mind wandered, and redirect their attention. The attitudinal component of the Bishop et al. (2004) definition—a curious and nonjudgmental orientation to experience—was expressed in the intervention as well, through instructions not to “fight” upsetting thoughts, not to “judge” instances of mind-wandering and to redirect attention “gently.” These aspects may have been underemphasized, though, as compared to the structure of the TMS subscales, which primarily capture the attitudinal aspect of mindfulness (Lau et al., 2006).

Although use of the TMS as a manipulation check in laboratory settings has been limited, a number of studies have found higher TMS total (Garland et al., 2013; Albers & Thewissen, 2011) or decentering scores (Feldman et al., 2010; Erisman & Roemer, 2010) following a brief mindfulness intervention, as compared to mind-wandering, thought suppression, no instructions, education control, relaxation, or compassion meditation. Mindfulness instructions in the present study were similar to those used in some of these same studies. For example, mindfulness instructions in all of the studies finding intervention-specific effects on state mindfulness included an emphasis on attending to the physical sensations of breathing, noticing and accepting instances of mind-wandering, and gently redirecting attention back to the breath. In some cases, instructions also included descriptions of applying mindfulness to accept difficult emotions

(Erisman and Roemer, 2010) or imagery to facilitate mindful awareness of thoughts (“like clouds passing in a clear blue sky”; Garland et al, 2013). It is unclear, however, whether these more extensive instructions contributed to the higher TMS scores. In fact, Feldman et al. (2010) found post-mindfulness intervention levels of decentering comparable those in the current study using a mindful breathing instructions that excluded any techniques thought to induce decentering through demand effects (e.g., labeling thoughts, visualizing thoughts as images, explicit descriptions of decentering). Using similarly narrow mindfulness instructions, Frewen et al. (2011) also found mean levels of post-mindfulness decentering and curiosity comparable to those in all three conditions of the current study. This suggests that the mindfulness audio intervention, as delivered, may have been adequate to induce state mindfulness.

Aside from Feldman et al. (2010), however, the other studies finding higher TMS scores following mindfulness practice employed neutral (e.g., no instructions, education) or analog-pathological (e.g., suppression) comparison conditions. None compared mindfulness to reappraisal. Although not hypothesized in this study, finding equal levels of decentering following both mindfulness and reappraisal is consistent with theory underlying mindfulness-based cognitive therapy, which holds that decentering is a common mechanism of mindfulness and traditional CBT (Segal, Williams, & Teasdale, 2002). Successful cognitive reappraisal requires one to view thoughts as mutable and not fixed aspects of one’s experience; successful reappraisal may require a disidentified perspective on thought analogous to that achieved through mindfulness practice. Indeed, some cross-sectional evidence suggests that decentering mediates the effects of both mindfulness and reappraisal on social anxiety (Hayes-Skelton & Graham, 2013).

The finding that decentering was equivalent after both mindfulness and mind-wandering, however, runs counter to theory that considers mindfulness and mind-wandering to be antithetical (Brown & Ryan, 2003; Mrazek, Smallwood, & Schooler, 2012). As described by Bishop and colleagues (2004), the state of mindfulness involves noticing one's thoughts and stopping mind-wandering—even if temporarily—by returning the attention to an original object of focus. They argue that it is precisely this activity of cutting off mind-wandering that allows for a decentered perspective on cognition to develop (Bishop et al., 2004).

Interestingly, though, experimental support for the superiority of brief mindfulness over mind-wandering in inducing decentering is limited. Garland et al. (2013) found differences between mindfulness and mind-wandering instructions in their effects on overall TMS scores but did not report finding differences at the subscale level. Erisman and Roemer (2010) found decentering increased following mindfulness instructions, as compared to a neutral education audio control condition. Ortner and Zelazo (2014) found no significant advantage in decentering following mindfulness as compared to no instructions and found that decentering was significantly higher following distraction (guided imagery) than either other condition. Thus the present study contributes to an equivocal body of findings on the effectiveness of brief mindfulness instructions at increasing decentering. Future research is needed to clarify the conditions under such increases can be found, with special attention paid to the quality of comparison conditions.

Yet another consideration regarding the absence of the between-group differences in state mindfulness is potential variability in participant compliance with specific intervention instructions. Participants assigned to one condition may have attempted to regulate their experimentally-induced sadness using another technique. Random assignment might have

somewhat mitigated this threat. It is plausible, however, that participants assigned to the mindfulness and mind-wandering conditions might have been more likely to engage in non-assigned emotion regulatory strategies. Given that mindfulness was novel to the majority of participants, those assigned to the mindfulness condition may have been more likely to revert to more familiar methods of downregulating sadness. Mind-wandering, because it demanded less active attentional engagement from participants, may have been particularly conducive to participants' use of non-assigned regulatory techniques. As will be discussed in greater detail later, a key limitation of the present study's design is the failure to assess participants' adherence to their assigned instructions.

4.1.2 Duration of mood induction effects.

As noted above, it is also possible that the reduction in sadness at Time 3 indicated by effect size and visual inspection was simply the result of a naturalistic return to baseline. Especially in nonclinical student samples, laboratory-based mood induction procedures have been found to have a relatively brief duration. Data are lacking regarding the duration of negative moods induced through the combined music and autobiographical recall procedures used in the present study. The 10-20 minute Velten procedure bears many similarities to the combined procedure, though, and has been studied more extensively. Among healthy students, negative affect increases immediately following a Velten depression induction procedure but quickly decreases again (Frost & Green, 1982), returning to pre-induction levels as soon as 6-12 minutes into a post-induction waiting period (Chartier & Ranieri, 1989). This pattern is also evident when a Velten or music mood induction is followed by a cognitive task: self-reported negative affect diminishes quickly, nearly reaching pre-induction levels after 10 minutes (Albersnagel, 1988; Eich et al., 2007). Thus it is possible that, rather than reflecting the

equivalent efficacy of mindfulness, reappraisal, and mind-wandering at reducing sadness, these findings instead merely demonstrate that healthy people tend to recover from induced mood rather rapidly.

4.2 Differences in Sustained Attention and Inhibitory Control

Results partially supported the hypothesis that mindfulness would increase sustained attention and inhibitory control over reappraisal and mind-wandering. Mindfulness was associated with a nearly two-thirds reduction in the rate of omission errors on the Go/No-Go task as compared to control, suggesting that mindfulness improved sustained attention as compared to mind-wandering. However, no such reduction in omission errors was evident for mindfulness when compared to reappraisal. In addition, no significant differences were found between conditions in the mean number of commission errors, indicating that the interventions had equivalent effects on inhibitory control.

4.2.1 *Sustained attention.*

The substantial improvements in sustained attention for mindfulness as compared to mind-wandering are consistent with theory that enhanced attentional vigilance is a key mechanism of mindfulness (e.g., Brown & Ryan, 2003; Bishop et al., 2004). This finding contributes to a limited body of empirical literature regarding the effects of mindfulness on sustained attention (Chiesa, Calati, & Serretti, 2011). Only one laboratory-based study (Mrazek, Smallwood, & Schooler, 2012) has examined sustained attention performance following a brief mindfulness intervention. Using the Sustained Attention to Response Task (SART), a version of a Go/No-Go test, Mrazek and colleagues (2012) found that 8-minute mindful breathing practice reduced both SART errors—analogue to Go/No-Go omissions—and reaction time variability, as compared to minimal-instructions reading and passive relaxation comparison conditions.

They interpret this as evidence that mindfulness reduces mind-wandering. Morrison et al. (2014) found a similar improvement in SART omission errors by healthy students following a 7-week mindfulness training program, as compared to waitlist control. A relationship between increased mindfulness and sustained attention has also been found with cross-sectional research, using the Continuous Performance Test, a measure of vigilance and inhibition with similar duration and structure to the Go/No-Go task (Schmertz et al., 2009). Such a benefit was not found in a randomized clinical trial of MBSR and wait-list control, however (Anderson et al., 2007).

The apparent improvements of mindfulness over control should be interpreted with caution, in light of the absence of a significant difference in sustained attention between mindfulness and reappraisal. Common features of the two active conditions, and their distinction from control, might also account for the higher rate of attentional lapses in the control condition. The mindfulness and reappraisal intervention scripts both provided participants with a more specific object of attention (sensations of breathing or upsetting thoughts and evidence for/against them) than in the control condition. Both mindfulness and reappraisal instructions also provided a relatively structured sequence of mental steps for participants to follow. Mindfulness and reappraisal were more likely to be novel techniques for participants and perhaps thus more likely to continue to elicit attentional engagement from participants. It is possible, too, that the observed difference in omission errors only between mindfulness and control reflects the detrimental impact of mind-wandering on sustained attention (for a review, see Smallwood & Schooler, 2006).

However, the parity between reappraisal and mindfulness conditions in sustained attention performance is consistent with the view that, in novice meditators, mindfulness functions as a top-down emotion regulation strategy similar to cognitive reappraisal (Chiesa et

al., 2013). As the majority of participants reported no current meditation practice, those in the reappraisal and mindfulness conditions may have both engaged in effortful control of attention to downregulate their negative affect.

4.2.2 Inhibitory control.

Mindfulness did not differ from the comparison conditions in inhibition performance, as indexed by Go/No-Go commissions. Although laboratory-based studies have found post-intervention differences in inhibitory control following relatively brief mindfulness training (Keng et al., 2013; Lee & Orsillo, 2014; Wenk-Sormaz, 2005), these findings have been limited to Stroop task errors. As the Stroop task requires verbal responses, these previous findings might be taken to indicate the effects of short-term mindfulness training in enhancing inhibition of *verbal* prepotent responses. By contrast, commission errors on the Go/No-Go task used in the present study indicate failure to inhibit a prepotent *motor* response. Thus the failure to find a significant between-group difference in commission errors might only indicate that brief mindfulness training is insufficient to improve inhibitory control on motor tasks.

4.3 Limitations.

The present study had several notable limitations, some of which have been addressed previously. Many aspects of the study design privileged internal over external validity, yet there were also numerous threats to internal validity. Chief among these is uncertainty regarding the success of the experimental manipulation. Because no differences in state mindfulness measured by the TMS were found, it cannot be definitively established that the intervention selectively increased that form of state mindfulness in the present mindfulness condition. This limitation could have been addressed by use of either a repeated-measures design (with the TMS administered prior to mood induction) or manipulation check questions assessing participants'

use of different emotion regulation strategies during the intervention phase (Aldao, 2013). A manipulation check assessing the use of a variety of assigned and non-assigned strategies would also have served to assess for potential demand effects.

Although theoretically distinct from mindfulness, the mind-wandering control instructions had some areas of overlap with the two active conditions (e.g., clinical rationale) that were not theoretically specified. Because no significant between-group differences were found in sadness or state mindfulness, it is unclear whether the overlapping or distinct aspects of the different instructions account for the equivalent results across condition. Including a fourth condition with minimal mind-wandering instructions would have helped to clarify this ambiguity.

While helpful in limiting participant burden and facilitating the assessment of presumably transient effects, the use of a single-item measure of sadness and a single post-intervention measurement point also severely limits the generalizability of these findings. The internal consistency of single-item measures cannot be assessed. In addition, negative mood inductions often increase a variety of moods (e.g., anxiety as well sadness; Martin, 1990), and assessing a broader range of moods would have permitted the assessment of between-condition differences in the initial negative mood induced or the intervention response. Data from the single post-intervention sadness rating indicate that groups did not differ in sadness immediately following the intervention phase, but the lack of subsequent ratings leaves ambiguous whether groups followed the same trajectory in recovering from the sad mood.

Theories of mindfulness-based interventions generally do not specify distinct processes in clinical and non-clinical populations (Baer, 2003). Nevertheless, extrapolation of these findings to clinical interventions is severely limited by the present study's use of a nonclinical, student

sample. Caution must also be applied when extrapolating from the present study's all-female sample to mixed-gender populations.

In addition, the heterogeneity of current meditation practice in the study sample must be considered when extrapolating these findings to either all-novice or experienced-meditator populations. Although the majority of participants reported no current meditation practice, and only a small proportion (1.5%) reported practicing on a daily basis, past meditation experience was not explicitly assessed. Given that mechanisms of mindfulness training may differ by level of experience (Chiesa et al., 2013), it is possible that varying levels of experience—not captured by the PHLMS—contributed to the null findings or significant between-group difference in sustained attention.

4.4 Implications and directions for future research.

Overall, findings from the present study contribute to the variable literature on laboratory-based brief mindfulness interventions. In particular, the absence of an effect of condition on changes in sadness underscores the importance of selecting theoretically distinct comparison conditions. Theoretically distinct active comparisons, such as reappraisal or relaxation, can clarify important areas of overlap or divergence in mechanisms, but it is also essential to include an “inactive” control condition. The present study failed to establish a comparison between active conditions as well as those between active and non-active conditions.

The use of manipulation checks is also essential. Laboratory studies should assess state mindfulness before and after training, as well as assessing for adherence to assigned training instructions. One promising option for strengthening the internal validity of mindfulness manipulations is behavioral assessment of breath-counting (Levinson et al., 2014). Breath-counting, which typically includes instructions to sustain attention on the breath as well as to

notice and gently shift away from mind-wandering, is a common beginning method for teaching mindfulness (Lutz et al., 2008) and could be implemented as a behavioral manipulation check on mindfulness instructions or even as a brief mindfulness intervention itself. Levinson and colleagues (2014) have developed a computerized breath-counting task with preliminary construct validity evidence: task performance correlated with self-reported trait mindfulness, differentiated experienced meditators from controls, and was inversely associated with attentional capture by positively-valenced stimuli. Such a task with a clear sustained attention component might be especially appropriate for assessing adherence and response to focused-breathing meditation instructions like those used in the present study. Multi-modal assessment of state mindfulness, using behavioral and self-report data, could further clarify the relationships between mindfulness, reappraisal and aspects of state mindfulness such as decentering.

Further exploration of the immediate effects of brief mindfulness training on attentional control is also warranted. A significant effect of mindfulness, as compared to mind-wandering, was found for sustained attention, but no effect was found for inhibitory control. As stated above, differences in task demands may account for the discrepancy between this finding and other experimental studies finding an effect of mindfulness on inhibition. Studies using other inhibition tasks may help bolster or further qualify previous findings regarding the benefits of brief mindfulness training on inhibition. Using a Stroop task modified to require a motor response would be particularly instructive, as most studies finding such a benefit for mindfulness used the verbal Stroop task (Wenk-Sormaz, 2005; Keng et al., 2013; Lee & Orsillo, 2014), unlike the present study's motor Go/No-Go task.

Differences in the content and context of mindfulness interventions could also account for this discrepancy. Of the laboratory studies finding effects of mindfulness on inhibition, two

(Wenk-Sormaz, 2005; Lee & Orsillo, 2014) used narrowly-tailored breath meditation instructions similar to those in the present study. Neither of those studies employed the mindfulness intervention in the context of an experimentally-induced sad mood, however, while Keng et al. (2013) did but also employed more elaborate mindfulness instructions with visual metaphors and greater emphasis on acceptance. It is conceivable that, in the presence of an induced sad mood, extended mindfulness instructions with a more explicit acceptance component are necessary to produce the greater inhibitory control observed by Keng and colleagues (2013).

Electrophysiological evidence, particularly the study of event-related potentials (ERPs), offers a way of further contextualizing performance on cognitive tasks. A number of studies have examined the relationship of mindfulness to ERP components during tasks like the Stroop (Cahn & Polich, 2006). Preliminary ERP evidence suggests that trait mindfulness is associated with differences in momentary affective responding to performance feedback on these tasks (Teper & Inzlicht, 2014). Thus it is possible that participants receiving a brief mindfulness intervention might perform equally well as those practicing reappraisal or mind-wandering on a test of attentional control and yet demonstrate different, diminished affective responses to their errors.

4.5 Conclusion

This study examined the short-term effects of brief mindfulness instructions on experimentally-induced sadness, sustained attention, and inhibitory control. Extant research has suggested that mindfulness can reduce negative affect and enhance attentional control in laboratory settings, with respect to control (e.g., mind-wandering, education) or maladaptive (e.g., rumination, worry) comparison conditions. Results from this study partially support these

prior findings. Mindfulness did not alter sadness, as compared to either reappraisal or mind-wandering. Participants in the mindfulness condition demonstrated better sustained attention, but not inhibitory control, than mind-wandering (but not reappraisal) on a computer-based performance task. The pattern of between-condition differences is complicated by the absence of any between-group differences in state mindfulness following the intervention phase. Overall, these findings support the value of further investigations into attentional mechanisms of mindfulness-based interventions, while also underlining the importance of carefully selecting comparison conditions and manipulation checks with the potential to distinguish between-condition differences.

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APPENDICES

Appendix A Mindfulness Training Instructions

Mindfulness Training Instructions
(608 words; adapted from Kiken & Shook, 2011; Keng et al., 2013)

In this next phase of the study, you will learn how to use a strategy for dealing with negative emotions. We call this technique “mindfulness.”

Start by finding a comfortable position in your chair with your feet flat on the floor and your back straight but not stiff or straining. Resting your hands on the top of your thighs. Finding a position that is alert while relaxed. Closing your eyes if that feels comfortable.

What mindfulness involves is simply paying attention to our experience in the present moment, in the “here and now,” without judgment.

Typically, when we experience upsetting thoughts or emotions, we tend to think about them over and over again: We judge them as good or bad, or we try to push them away so that we don’t have to deal with them. The technique of mindfulness is not to struggle with upsetting thoughts or emotions, but just to be aware of them and let them be, as we pay attention to what we are experiencing moment-to-moment.

Now, I’m going to ask you to gently bring your attention to the sensations of your own breathing. We’re going to use your breathing like an anchor, to help you pay attention to your experience in the present moment.

As best you can, focus your awareness on the physical sensations of your breathing. You might notice the air moving in and out through your nose, or the feeling of your belly rising and falling, as you inhale and exhale. See if you can follow those sensations all the way, for the full length of your in-breath... and the full length of your out-breath.

It may seem hard at first to shift your focus from the upsetting thoughts and emotions you were experiencing earlier. Don’t try to fight them off, just try to pay attention to your in-breath, following it all the way in, and following your out-breath all the way out. Notice the physical sensations in your body as you breathe: the expansion in your chest, your abdomen, or your back. Notice any tension, or any feelings of relaxation, in your body. Notice how those sensations change as you breathe in and out.

You may notice, at some point, that your mind has wandered, that your awareness is no longer with your breathing, but is instead lost in thought or preoccupied with a feeling. These thoughts or feelings may be pleasurable, or they may be unpleasant. This is normal, and there’s no need to judge it. If you notice that your awareness *has* shifted from your breath, simply acknowledge the thoughts or feelings and let them go. Just gently redirect your awareness back to your breath.

This technique is all you need to do during this exercise. If you happen to think this is silly or boring, let those momentary thoughts be and then, gently, return to the process of noticing each breath in each moment.

Now, you will be given some quiet time to continue with this exercise. Every now and then during this quiet time, you will hear some reminders. Please continue to attend to the feelings of each breath in and out, until you hear instructions to stop.

Gently maintain attention on your breathing, being with each breath in for its full duration and each breath out for its full duration.

If your mind wanders, acknowledge that it has wandered to reconnect to the present, and gently shift your attention back to noticing the feeling of each breath.

[End]

This exercise is now over. When you feel ready, you may open your eyes if they were closed. Then, ask the experimenter for the next directions.

Appendix B Reappraisal Training Instructions

Reappraisal Training Instructions (613 words; adapted from Keng et al., 2013)

In this next phase of the study, you will learn how to use a strategy for dealing with negative emotions. We call this technique “reappraisal”.

Start by finding a comfortable position in your chair with your feet flat on the floor and your back straight but not stiff or straining. Resting your hands on the top of your thighs. Finding a position that is alert while relaxed. Closing your eyes if that feels comfortable.

What reappraisal involves is changing the way we think about events that upset us.

Typically, when we experience upsetting thoughts and emotions, we tend to have negative interpretations about the event that triggers them: We tell ourselves that whatever happened was bad, that it means something bad about us or about the world or the future. The technique of reappraisal is to change the way we interpret the event, so that we don’t feel as negatively about it. Reappraisal is trying to develop a more positive interpretation about a situation that—at first glance—may appear very negative to us.

There are many ways we can change our interpretations of a situation. We can start, for example, by asking, “Is this the only way I can think about the event or the situation?” “Is there a more positive way of thinking about the event so that I feel less negatively about it?” “What could be another way to look at what happened?”

We can also challenge our negative thoughts or interpretations of an event by questioning those thoughts, by asking ourselves, “What evidence do I have that says this thought or this interpretation is true?” “Is this thought consistent with my experience in other situations?” “Do I have evidence that this thought isn’t true?”

Imagine that you have a friend who always smiles or says “Hi” to you whenever you run into each other. Now, imagine that one time, you run into this friend, but instead of smiling or saying hello, they just walk past you without smiling. How might you feel if this happened? One interpretation of this situation would be, “My friend ignored me on purpose.” If you interpreted the situation this way, you might even think that you did something to offend your friend or make them angry. You might feel angry or insulted, or perhaps sad or even lonely.

But with a different interpretation of that event, you might not feel this way. If you had the interpretation, “My friend must not have seen me,” or “My friend was probably distracted or in a hurry,” then you might instead think about what was happening in your friend’s life. You might feel concerned for your friend, or you might simply not get as upset.

Now, let's practice the technique of reappraisal. Think back to the previous exercise, when you recalled three sad events in your life. Pick one of those events, and try to recall the thoughts and feelings that came up for you while remembering that sad experience.

How did you interpret that sad event?

Are there other ways, more positive ways, that you could interpret that experience?

If you can, try to imagine how another person, someone with a different point of view, might interpret that experience.

Consider the evidence for your interpretation. Do you have any other evidence about this event, evidence that would support a more positive view of it?

Spend the next few minutes practicing this technique of reappraisal. You can try to reappraise the other two sad events from the previous exercise, too.

[End]

This exercise is now over. When you feel ready, you may open your eyes if they were closed. Then, ask the experimenter for the next directions.

Appendix C Mind-Wandering (Control) Conditions

Mind-wandering (Control) Instructions

(426 words; adapted from Kiken & Shook, 2011, and McHugh et al., 2012)

In this next phase of the study, you will learn how to use a strategy for dealing with negative emotions. The strategy is to let your mind wander freely.

Start by finding a comfortable position in your chair with your feet flat on the floor and your back straight but not stiff or straining. Rest your hands on the top of your thighs. Find a position that is alert while relaxed. Close your eyes if that feels comfortable.

We're going to ask you to think about whatever comes to mind, without having to focus on anything in particular. Take this time to follow your thoughts and feelings – whatever you want to think about – as you do when you have time to think things through thoroughly. For example, sometimes we think about ideas for later in the day or week to organize our plans. Or, sometimes we think about something that happened earlier in our day. You may have a lot to think about, maybe important things, or your mind might just wander to anything. Either way, take time to think about whatever you want. Just let your mind think and wander freely.

We are simply giving you time to let your mind wander freely through all your thoughts. Sometimes we don't have time to let our minds wander or think through everything with all that goes on. Yet everybody has their own interests, concerns and ideas that occupy their thoughts when they have time. Sometimes we want some time just to think. So, during this time, you can let your mind go wherever it wants as time passes. Continue to let yourself think about whatever you want to.

That is all you need to do during this exercise. It's that simple. Use the time to let your mind wander and think freely without needing to focus hard on anything in particular. Even if you zone out a bit, that's okay. Now, you will be given some quiet time to continue with this exercise. Every now and then during this quiet time, you will hear some reminders. Please continue to let your mind wander and think freely during this time.

Remember: this is time for your mind to wander freely.

You don't have to think about one thing; think about as many different things as you want.

Don't focus too hard on anything. Think freely.

[End]

This exercise is now over. Slowly open your eyes if they are closed, and take a moment if you need one. Then, turn the computer monitor back on and click continue.