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Development of a self-distancing task and initial validation of responses

Kathrine A. Shepherd, M.A.
Department of Psychological Sciences, Kent State University, Kent, Ohio, USA

Karin G. Coifman, Ph.D.
Department of Psychological Sciences, Kent State University, Kent, Ohio, USA

Lindsey M. Matt, M.A.
Department of Psychological Sciences, Kent State University, Kent, Ohio, USA

David M. Fresco, Ph.D.
Department of Psychological Sciences, Kent State University, Kent, Ohio, USA

Abstract

Mindfulness-based interventions are believed to counteract cognitive biases that exacerbate cognitive and physiological reactivity to emotional experiences and that contribute to the development and persistence of psychopathology. One process by which mindful practices may produce such salutary effects, is by enhancing the capacity to "decenter"—or to adopt a self-distanced, non-judgmental perspective on conscious experiences (e.g., thoughts, memories, and feelings). Findings consistently indicate that decentering, assessed via self-report, represents an important aspect of mental health and well-being; however, numerous researchers have called for more objective measures of skills associated with mindfulness and decentering to further evaluate the mechanisms and benefits of mindfulness-based practices. Thus, in the current investigation, we developed a behavioral task that requires mental manipulation of negative emotional (and neutral) material away from the self (self-distancing), as a means to assess the skills associated with mindfulness and decentering that likely underlie healthy emotional processing. In two non-meditating, university samples, we found that higher levels of self-reported mindfulness and higher levels of one facet of decentering (the capacity to adopt a distanced perspective on experiences) predicted behavioral indicators of self-distancing. Results suggest that the self-distancing task shows considerable promise for capturing skills associated with mindfulness and at least one element of decentering.

Keywords

decentering; mindfulness; self-distancing; distanced perspective; mental manipulation

Correspondence concerning this article should be addressed to Kathrine Shepherd, Department of Psychological Sciences, Kent State University, Kent, Ohio 44124. Contact: kshephe9@kent.edu.

Introduction

Background and significance

In recent years, there has been a rapidly growing movement to integrate mindfulness-based practices—that cultivate conscious awareness of present moment experiences—into psychotherapeutic interventions. Indeed many prevalent third-wave therapies, including Mindfulness-Based Stress Reduction (MBSR; Kabat-Zinn & Hanh, 2009), Mindfulness-Based Cognitive Therapy (MBCT; Segal, Williams, & Teasdale, 2002), Dialectical Behavior Therapy (DBT; Linehan, 1987), and Acceptance and Commitment Therapy (ACT; Hayes, Strosahl, & Wilson, 1999), contain elements of mindfulness as part of their treatment approaches for a wide range of psychological disorders (Baer, 2003). The prevalence of mindfulness-based interventions stems from theory and evidence from randomized control trials, which suggest that mindfulness practices can help to counteract biases in perceptions, cognitions, and emotions that contribute to the development and maintenance of a range of psychological disorders, and can promote healthier ways of relating to self-relevant, negative emotional information (Baer, 2003; Hölzel et al., 2011; Vago & Silbersweig, 2012).

Healthy, adaptive responding to self-relevant, negative emotional information (e.g., threats) entails allocating attentional and cognitive resources to the source of the information to coordinate a contextually appropriate response (Phillips, Drevets, Rauch, & Lane, 2003). In a high-threat context, for example, an appropriate response can be reflexive and involve little or no higher cognitive intervention (e.g., avoiding a snake in one’s path without stopping to consider whether it is poisonous) (Öhman & Mineka, 2001). However, if the threat becomes irrelevant (e.g., recognizing the snake is innocuous), an adaptive response is to flexibly disengage attention and redirect focus to other aspects of current moment experience, such as the pursuit of goals or rewards (Bonanno & Burton, 2013).

By contrast, cognitive and attentional biases that are characteristic of many forms of psychopathology increase the likelihood that individuals will orient attention to negative, self-relevant emotional information, and have difficulties disengaging attention from that material once it has entered the field of awareness (Goeleven, De Raedt, Baert, & Koster, 2006; Gotlib & Joormann, 2010; Joormann, 2004; Joormann & Gotlib, 2006). These difficulties can prolong and intensify cognitive and physiological responses to negative events (i.e., as in depression: Joormann, 2010; Joormann & Gotlib, 2008), and lead to perseverative, negative self-referential thinking processes such as brooding rumination and worry, that are known to increase risk for the development of most mood and anxiety disorders (Mennin & Fresco, 2013; Nolen-Hoeksema, Wisco, & Lyubomirsky, 2008).

One way that mindfulness practices aim to counter biased processing is through promoting and training non-judgmental observation of experiences (for review, see Desbordes et al., 2014). In mindfulness practices, individuals monitor conscious experiences as they arise in awareness and practice shifting to an observing, “decentered” perspective on them with an attitude of curiosity and non-judgment (Desbordes et al., 2014; Teasdale et al., 2002). This metacognitive shift in perspective—frequently termed “decentering”—is thought to foster a broad contextual awareness of mental events as subjective, transient events, rather than accurate reflections of reality, or “me” (Bernstein et al., 2015; Safran & Segal, 1990;

Teasdale, 1999). For example, the thought, "I am a failure," might instead be viewed as, "I am having a thought that I am a failure; my thoughts aren't facts."

Reflecting on negative experiences from a decentered perspective is thought to decrease the likelihood that those experiences will lead to destructive forms of self-referential processing such as rumination and worry (e.g., Mennin & Fresco, 2013; Teasdale, 1999). Indeed, a series of experimental studies have shown that adopting a self-distanced ("fly-on-the-wall") perspective when recalling distressing autobiographical memories reduced depressogenic, ruminative thinking and physiological reactivity, and facilitated adaptive self-reflection, when compared to adopting a self-immersed, first-person perspective (Ayduk & Kross, 2008; Kross & Ayduk, 2008; Kross & Ayduk, 2009; Kross, Ayduk, & Mischel, 2005; Wisco & Nolen-Hoeksema, 2011).

Decentering is a naturally occurring characteristic of individuals that can be trained using traditional cognitive therapy methods (Beck, Rush, Shaw, & Emery, 1979; Fresco, Moore, et al., 2007; Fresco, Segal, Buis, & Kennedy, 2007; Mennin & Fresco, 2013). However it has been suggested that the capacity to decenter may be optimally cultivated via mindfulness meditation training, through the repeated practice of observing mental phenomena (e.g., thoughts, emotions, sensations) objectively, as transient mental events, with an attitude of acceptance (Segal et al., 2002; Teasdale, 1999; Teasdale et al., 2002).

Assessment of mindfulness and decentering

Despite the rapidly growing enthusiasm for mindfulness-based approaches in clinical treatments, investigators are still working to operationalize mindfulness and decentering as psychological constructs, and to develop psychometrically sound measures that can be used to study their roles in treatment (Baer, Walsh, & Lykins, 2009). In fact, there are numerous descriptions of mindfulness and decentering in the clinical literature, but there are still many challenges in defining and measuring these complex and elusive constructs, including the challenge of how to differentiate them from one another and from other related constructs (Baer et al., 2009; Bernstein et al., 2015; Desbordes et al., 2014; Grossman & Van Dam, 2011).

Self-report questionnaires are currently the primary means of assessing gains in mindfulness and decentering. In recent years, at least eight scales have been developed to operationalize dispositional mindfulness based on different conceptualizations of skills cultivated with mindfulness practices (for review, see Baer et al., 2009; Sauer et al., 2013). Of these instruments, the Five Facet Mindfulness Questionnaire (FFMQ) has advantages over other measures, given that it was developed using factor-analytic integration of five previously developed mindfulness scales. The FFMQ includes five distinct facets reflecting skills associated with mindfulness: *observing*, *describing*, *acting with awareness*, *nonjudging of inner experience*, and *nonreactivity to inner experience* (Baer, Smith, Hopkins, Krietemeyer, & Toney, 2006); it has been widely used to assess a trait-like capacity to be mindful in everyday activities, and its facets have consistently been associated with level of meditation training and symptomatic improvements (Baer, 2011).

The most widely used self-report measure of decentering is the Experiences Questionnaire (EQ; Fresco, Moore, et al., 2007). Fresco and colleagues (2007) showed that relevant items load onto a single factor of *decentering*, which demonstrated good concurrent and discriminant validity. EQ *decentering* has been positively associated with self-reported mindfulness (Carmody, Baer, LB Lykins, & Olendzki, 2009; Tanay, Lotan, & Bernstein, 2012) and negatively associated with depression symptoms, depressive rumination, and experiential avoidance (Fresco, Moore, et al., 2007). EQ *decentering* has also been predictive of longer time to relapse in individuals with major depressive disorder following acute treatment with cognitive therapy (Fresco, Segal, et al., 2007) and prophylactic treatment with MBCT (Bieling et al., 2012). Additionally, generalized anxiety disorder (GAD) patients treated with MBSR (Hoge et al., 2015) or emotion regulation therapy (ERT; Mennin, Fresco, Ritter, & Heimberg, 2015) evidenced gains in EQ *decentering*, which were associated with temporal mediation of treatment gains with respect to GAD symptoms, worry, depression symptoms, functional impairment, and quality of life (Mennin, Fresco, Heimberg, & O'Toole, Under review).

Despite their advantages and wide use, self-report measures of mindfulness and decentering have numerous methodological and conceptual limitations (Grossman, 2011). In general, self-report measures are not ideal for evaluating treatment outcomes because they are prone to demand characteristics and expectations about improvements (Lilienfeld, Ritschel, Lynn, Cautin, & Latzman, 2014; Carmody et al., 2009; Grossman, 2011). Furthermore, individuals may have difficulty reporting on their use of skills, resulting in discrepancies between self-report and skill use in everyday life (Veenman, Prins, & Verheij, 2003; Veenman & Spaans, 2005). Self-report measures of mindfulness, in particular, have frequently been criticized for a lack of coherence in how they define the construct of mindfulness (i.e., which facets or elements of mindfulness they emphasize), as reflected by findings that different measures of mindfulness are often only modestly correlated with each other. Additionally, mindfulness measures have been criticized for missing critical aspects of the theoretical and experiential nature of the construct, which was originally derived from Buddhist traditions (Desbordes et al., 2014; Grossman, 2008; Grossman & Van Dam, 2011).

Furthermore, when using self-report measures of mindfulness, it is important to consider that the functioning of items and scales may vary depending on the level of meditation experience of the respondents (Baer et al., 2008; Lilja, Lundh, Josefsson, & Falkenström, 2013). For example, in experienced meditators, the FFMQ was positively associated with psychological adjustment; but in non-meditating samples, the FFMQ *observing* subscale was shown to be associated with maladaptive cognitive styles (e.g., thought suppression and absent-mindedness) (Baer et al., 2008). Accordingly, there is some evidence to suggest that the process of observing internal experiences, in the absence of other qualities of mindful attention (e.g., non-judging and non-reactivity) might be detrimental (see e.g., Eisenlohr-Moul et al., 2012; Derosiers, Vine, Curtiss & Klemanski, 2014).

Self-report measures of decentering also appear to suffer from conceptual issues. For example, decentering is often measured as a single dimension despite that definitions portray a multi-faceted construct (Fresco, Moore, et al., 2007; Gecht et al., 2014). Indeed, the EQ items were originally developed to capture three facets of decentering: the capacity to view one's self as not synonymous with one's thoughts, the ability not to habitually react to one's negative experiences, and the capacity for self-compassion (Fresco, Moore, et al., 2007). However, a recent factor-analysis using a German translation of the EQ, suggested that a two-factor solution—differentiating the capacity to adopt a *distanced perspective* and to have an *accepting self perception*—provided a better fit to the available data as compared to the single-factor solution (Gecht et al., 2014).

In light of the methodological and conceptual limitations of self-report measures of mindfulness and decentering, there is a pressing need for objective measures—which may be used in conjunction with self-report methods—to advance knowledge of processes that underlie the effects of therapeutic interventions.

Current investigation

In the current investigation, we sought to begin addressing the need for objective measures of skills associated with mindfulness and decentering. Specifically, we aimed to develop a behavioral task that required mental manipulation of negative emotional (and neutral) material away from the self (self-distancing), to capture skills that likely underlie healthy emotional processing and which are thought to be cultivated in mindfulness-based interventions (Bieling, Hawley, Bloch, Corcoran et al., 2012; Lau, Bishop, Segal, Buis et al., 2006; Carmody, Baer, Lykins & Olendzki, 2009; Teasdale et al., 1999; Vago & Silbersweig, 2012). The current investigation represents a first attempt to validate responses to this "self-distancing task" in relation to self-report measures of mindfulness and decentering.

Self-distancing task design and rationale

In the self-distancing task, participants viewed images of objects with negative and neutral valences. Negative objects were selected for having content that is considered threatening or unpleasant (e.g., spider, razor blade), whereas neutral objects had no affective valence (e.g., paperclip, picture frame). Participants were asked to evaluate the size of each object relative to their own hand or relative to a standard shoebox (e.g., would this spider fit in your hand or not?). Participants were told that to accurately evaluate the size of the objects, they should visualize the objects in their own hand or in the shoebox, and they were asked to respond as quickly and accurately as possible. Self-reported negative affect was collected following negative and neutral trials to confirm that negative object stimuli elicited more negative affect than neutral stimuli.

Critically, the ability to accurately evaluate whether an object would fit in one's own hand or in a shoebox was presumed to require mental manipulation of objects *toward*- or *away* from the self. Specifically, in the "hand" context, participants had to mentally *pull* the objects into their hands to evaluate the relative size of the objects, whereas in the "shoebox" context, participants had to mentally *push* the objects into the shoebox to evaluate their relative size (i.e., create distance from the objects). The mental act of bringing negative objects toward the self—into one's own hand—was intended to cause distress because the act is inconsistent with fundamental motivations to increase distance from threatening or unpleasant stimuli (Chen & Bargh, 1999; Rinck & Becker, 2007). By contrast, pushing negative objects away from the self— into a shoebox—was intended to be less distressing because the act is consistent with natural motivations to increase distance from threatening or unpleasant stimuli (Chen & Bargh, 1999; Rinck & Becker, 2007). Accordingly, we conceptualized the “hand” condition as an imagined “high-threat” context and the “shoebox” context as an imagined “low-threat” context.

Evolutionary theories propose that automatic defensive reactions to threatening stimuli can be difficult- or impossible to cognitively over-ride, and higher-order cognitive operations (e.g., mental manipulation) should be impeded in high-threat contexts. By contrast, automatic reactions to threatening or unpleasant stimuli can be more easily countered via higher-order cognitive intervention in low-threat contexts (Öhman & Mineka, 2001). Thus, for the average person, a task involving mental manipulation of emotional stimuli should be more difficult in a high-threat scenario (i.e., the “hand” context) versus a low-threat scenario (i.e., the “shoebox” context). Critically however, individuals who have difficulty self-distancing or detaching from emotional material might struggle with mental manipulation of emotional material even in a low-threat scenario (e.g. depression and cognitive and attentional biases; c.f., Joorman et al., 2011), whereas their more adept counterparts would have less difficulty.

In the self-distancing paradigm, we assessed the ease/difficulty with which individuals were able to perform the size evaluation task in the hand/high-threat and the shoebox/low-threat contexts, using two indicators of performance: response times (RTs) and accuracy (i.e., ability to evaluate whether objects would fit in the hand/shoebox).

RTs are frequently used as outcome measures in experimental research to evaluate the complexity of mental operations, with longer RTs indicating more complex operations (Sternberg, 1969). Participants typically take longer to respond to negative stimuli versus neutral stimuli, a phenomenon that is thought to reflect increased allocation of attention to motivationally salient stimuli (Öhman & Mineka, 2001; Ortner, Kilner, & Zelazo, 2007). In the current investigation, smaller differences in RTs to negative versus neutral stimuli were taken to indicate better mental manipulation and evaluation of negative material (i.e., a smaller effect of emotional valence on mental manipulation and evaluation abilities), and vice versa.

Accuracy is typically treated as an extraneous variable in experimental studies; incorrect trials are removed during analyses to prevent their negative impact on the results (e.g., Prinzmetal, McCool, & Park, 2005). Indeed, removing inaccurate trials in the self-distancing task would likely enhance power to detect and measure the effects of our task manipulations on RTs. However, in the current investigation, accuracy was a dependent variable of interest: the ability to accurately judge the relative size of objects was thought to depend on mental manipulation abilities. Thus, we analyzed all trials and sought to examine the moderating role of accuracy on RTs. In addition, as with RTs, we interpreted smaller differences in accuracy to negative versus neutral stimuli to indicate better mental manipulation of negative material (i.e., a smaller effect of emotional valence on mental manipulation abilities), and vice versa.

The self-distancing task design presented a challenge with regard to evaluating the accuracy of responses. Specifically, trials varied in whether the size-evaluation was clear-cut. For example, while it is relatively easy to determine whether a spider would fit in one's hand, it is less obvious as to whether a snake would fit in one's hand. This "ambiguity problem" had two important consequences. First, it necessitated generating response norms to statistically determine the "correct" response for each trial before we were able to determine the accuracy of individual responses. These norms had to be generated in a sample of psychologically healthy individuals to ensure that "normal" responses were not affected by symptoms of psychopathology. Thus, our measure of accuracy—termed "correctness"—is a construct rather than a concrete measurement: it refers to the number of participant responses that corresponded to the responses of healthy individuals, and it serves as an index of general mental manipulation and evaluation abilities. A second consequence of the "ambiguity problem" was that it represented a potential confound that could complicate interpretation of RTs. More ambiguous trials could produce longer RTs. Thus, if participants were more confused about whether objects would fit in their hands versus in the shoebox, we would be unable to conclude that RT effects were due to our variable of interest rather than response ambiguity. As a result, it was critical to ensure that levels of response ambiguity were similar across conditions, prior to testing more specific hypotheses regarding RT in the current investigation.

To begin validating responses to the self-distancing task, we conducted two studies (Study 1 and Study 2). In Study 1, we recruited a small group of healthy individuals in which to generate response norms that could be later used to compute the correctness of responses in Study 2, as well as to ensure that levels of ambiguity were similar across conditions. Participants in Study 1 completed a structured clinical interview and a self-report measure of depression symptoms to rule out any current or lifetime history of psychopathology.

In Study 2, we recruited a larger sample of individuals who were not pre-screened for mental health to examine task performance in a naturalistic sample that included a range of functioning. Participants in Study 2 completed self-report measures of mindfulness (FFMQ; Baer et al., 2006), decentering (EQ; Fresco, Moore, et al., 2007), and depression (QIDS-SR_{16}; Rush et al., 2003). We evaluated depression symptoms, because depression has been consistently associated with general executive impairments linked to difficulties with mental manipulation of emotional material, and thus it represents a potentially important covariate of task performance (Gotlib & Joormann, 2010; Joormann, Levens, & Gotlib, 2011; Mathews & MacLeod, 2005). For parsimony in predicting experimental variables, we created a composite score of FFMQ mindfulness by summing the four scales that were previously shown to load on to an overarching mindfulness factor in non-meditating samples. We did not include the *observing* subscale in this composite score, because as mentioned previously, *observing* has been associated with maladaptive characteristics in similar participant populations (Baer et al., 2008). With regard to the EQ, we calculated the single factor *decentering* scale (Fresco, Moore, et al., 2007), as well as the *distanced perspective* scale from the factor analysis of Gecht and colleagues (2014), which specifically assesses the capacity to manipulate emotional material away from the self (i.e., self-distancing).

In Study 2, we validated responses to the self-distancing task in two stages. In stage one, we verified that our manipulations were successful, and in stage two, we validated the task against self-report measures of mindfulness and decentering. We generated the following hypotheses, which follow from the self-distancing task design and rationale.

Manipulation check hypotheses

Ambiguity (Studies 1 and 2)—Levels of ambiguity (i.e., uncertainty with regard to whether a stimulus object would or would not fit in the hand or shoebox) will not vary as a function of valence (negative, neutral) or context (hand/high-threat, shoebox/low-threat) conditions.

Negative affect (Study 2)—Negative object stimuli will elicit more negative affect than neutral stimuli, as reflected by higher self-reported negative affect following negative versus neutral trials.

Response time (RT) and correctness (Study 2)—Overall, mental manipulation of negative stimuli should be more difficult than mental manipulation of neutral stimuli. Thus, RTs to negative stimuli will be longer than RTs to neutral stimuli (i.e., a main effect of emotional valence on RTs), and "error rates" to negative stimuli will be higher than "error rates" to neutral stimuli (i.e., a main effect of emotional valence on correctness). Critically however, mental manipulation should be more difficult in the hand/high-threat context compared with the shoebox/low-threat context (i.e., the effects of valence and context on RTs and accuracy will interact). Accordingly, the difference in RTs to negative versus neutral stimuli will be smaller in the shoebox/low-threat context compared with the hand/high-threat context; and the difference in "error rates" to negative versus neutral stimuli will be smaller in the shoebox/low-threat context compared with the hand/high-threat context. In addition, the effects of the context and valence manipulations will be stronger in individuals who demonstrate high levels of correctness, as it should be easier to detect and measure manipulation effects in individuals who demonstrate higher levels of accuracy.

Depression (Study 2)—Higher levels of self-reported depression will be associated with lower levels of overall correctness.

Validation hypotheses

Individual differences (Study 2)—After controlling for factors associated with general mental manipulation abilities (i.e., correctness and depression symptoms), higher (versus lower) levels of mindfulness and decentering/distancing will be associated with smaller differences in RTs to negative versus neutral objects in the shoebox/low-threat condition. There will not be a relationship between mindfulness and decentering/distancing scales to RTs in the hand/high-threat condition.

Method: Self-distancing Task Design

Stimuli

Stimuli consisted of 144 color images of objects, previously rated and standardized for negative and neutral emotional valence[1]. Negative stimuli were objects with unpleasant or threatening content (e.g., spider, razor blade), whereas neutral stimuli were objects without any obvious emotional salience (e.g., roll of tape, picture frame). Object images had a resolution of 256 × 256 pixels and were presented on a white background.

Apparatus

Stimulus presentation and response collection were performed using Psychtoolbox (http://www.psychtoolbox.org) running on MATLAB (http://www.mathworks.com), controlled by a Dell desktop computer with screen resolution of 1280 × 800. Response times (RTs) were collected using a standard keyboard.

Design

We used a 2 (valence: neutral, negative) × 2 (context: hand, shoebox) within-subjects, blocked design (Fig.1). Participants saw four blocks of images of objects (2 negative, 2 neutral). Before each block of images, they were given one of two instructions to follow for all subsequent object images in the block: 1) decide whether each object would be small enough to fit inside the palm of your hand, or 2) decide whether each object would be small enough to fit inside a standard shoebox. An exemplar of a standard shoebox was provided for reference. Participants viewed each image sequentially and each image remained onscreen until they indicate a response by keypress ("yes", "no", "don't know"), or until ten seconds elapsed. The time from stimulus onset to the keypress response was recorded for each trial. Participants were instructed to "respond as quickly and accurately as possible to each image." They were told, "your performance on the task will improve if you imagine each object sitting in the palm of your own hand, or sitting in the shoebox."

For each participant, negative or neutral images were randomly ordered and assigned to the hand or the shoebox context. Thus, a particular stimulus image could have appeared either in the hand context or the shoebox context for a given participant, depending on its random assignment. No participant saw the same image twice. Participants completed eight practice trials prior to the start of the experiment (four neutral/hand trials, four neutral/shoebox trials). At baseline, and after each block, participants were prompted to rate their current level of negative affect using a modified version of the Positive and Negative Affectivity Schedule (PANAS; Watson, Clark, & Tellegen, 1988) described in Study 2 measures. Levels of self-reported negative affect following negative versus neutral stimulus blocks were used to evaluate the overall effect of stimulus valence on affect.

[1]Stimulus selection and valence normalization procedures are described in detail by Shenhav, Barrett, and Bar (2013). Stimulus images were viewed by independent raters who provided valence ratings on a 7-point Likert scale ranging from very unpleasant to very pleasant (centered on neutral): negative ($M = 2.18$, $SD = 0.39$), neutral ($M = 4.16$, $SD = 0.45$).

Method: Study 1

Participants

Study 1 participants were 24 healthy adults (66% female, 67% Caucasian; mean age=20.33, SD = 3.17, age range: 18–30 years) recruited from the university community at Kent State University. Participants were screened for symptoms of depression and anxiety using two commonly used screening measures: the Center for Epidemiological Studies Depression (CES-D; Radloff, 1977), and the Generalized Anxiety Disorder Questionnaire for DSM-IV (GAD-Q-IV; Newman et al., 2002). We recruited individuals with scores below 16 on the CES-D (Lewinsohn, Seeley, Roberts, & Allen, 1997), and scores below 7.67 on the GAD-Q-IV (Moore, Anderson, Barnes, Haigh, & Fresco, 2014), and who reported no history of psychological or neurological disorder. All participants were able to understand, read, and speak in English, and had normal, or corrected to normal vision and hearing.

Procedure

Upon arrival to the laboratory, participants were seated in a quiet room, where they provided written, informed consent, completed a structured clinical interview and self-report measures of depression to rule out symptoms of psychopathology, and filled-out computerized questionnaires assessing demographic information (Qualtrics software; http://www.qualtrics.com). Participants then completed the self-distancing task, were compensated, and debriefed.

Symptom measures

Structured Clinical Interview for DSM-IV-TR, Research Version (SCID; First, Spitzer, Gibbon, & Williams, 1995)—The SCID is a widely used semi-structured interview that allows for current and lifetime diagnoses of Axis I disorders. The SCID interviewer was a graduate student therapist, who had formal supervision and training in conducting DSM-IV diagnostic interviews and in differential diagnosis through observing- and being observed by experienced interviewers. The student demonstrated competence in diagnostic accuracy prior to conducting interviews independently. Five randomly selected interviews were reviewed for diagnostic accuracy by a senior, doctoral-level clinician; a consensus of "no diagnosis" was achieved in each case (i.e., no participant had any current- or lifetime history of DSM-IV Axis I disorders).

The Quick Inventory of Depressive Symptomatology – Self Report ($QIDS-SR_{16}$; Rush et al., 2003). The $QIDS-SR_{16}$ is a 16-item self-report measure that assesses depressive symptoms experienced in the past week. These items are based on diagnostic criteria for a major depressive episode. The response options for each item range from 0 to 3, where 0 indicates the absence of that symptom. The mean level of self-reported depression symptoms (M = 3.67, SD = 2.41, range 1-8) was in the normal range of non-depressed individuals (Rush et al., 2003)[2]. The $QIDS-SR_{16}$ demonstrated questionable internal consistency in this sample according to Cronbach's alpha (α = .62).

[2]Four out of twenty-four participants in Study 1 had a score of 8 on the $QIDS-SR_{16}$, which falls on the cusp of normal (7 or less) to mild depression symptoms (8-12) as indicated by published cut-off scores (Rush et al., 2003). We experimented with removing those

Response norming

To normalize responses to stimuli, we conducted an item analysis to statistically determine the “correct” response for each trial (i.e., “yes,” “no,” “I don’t know”). Specifically, we examined the distribution of responses to each stimulus as a function of the condition in which the stimulus appeared (i.e., hand or shoebox). We calculated the modal response for each trial. The modal response was taken to indicate the “correct” response for that trial. These norms were later used to determine “correctness” of responses in Study 2.

Results: Study 1

Ambiguity hypothesis

Levels of ambiguity (i.e., uncertainty with regard to whether a stimulus object would or would not fit in the hand or shoebox) will not vary as a function of valence or context conditions.

Test of ambiguity hypothesis

We calculated an ambiguity score for each stimulus depending on the condition in which it appeared (i.e., hand or shoebox). This score represented the percentage of total responses that were inconsistent with the modal, “correct” response. Higher scores indicated higher levels of ambiguity. To verify that levels of ambiguity were statistically similar across conditions, ambiguity scores were subjected to repeated-measures analyses of variance (rmANOVA) with context (hand vs. shoebox) and valence (negative vs. neutral) as within-subject factors. Effect sizes were calculated as Partial Eta-squared (η^2). The 2 × 2 rmANOVA revealed no main effects of valence, $F(1,71) = .77$, $\eta^2 = .011$, or context, $F(1,71) = .06$, $\eta^2 = .001$, and no interaction of valence × context on ambiguity scores, $F(1,72) = 2.72$, $\eta^2 = .037$, indicating that levels of ambiguity were not associated with the manipulations of interest.

Method: Study 2

Participants

Study 2 participants were 94 adults (66% female 73.4% Caucasian; mean age = 20.82, SD = 4.66, age range: 18–48 years) recruited from the university community at Kent State University. All participants were able to understand, read, and speak in English, and had normal, or corrected to normal vision and hearing.

Measures

Depression symptoms—Depression symptoms were assessed using the *Quick Inventory of Depressive Symptomatology* – Self Report (QIDS-SR_{16}; Rush et al., 2003), as described in Study 1. The QIDS-SR_{16} demonstrated questionable internal consistency in this sample (α = .68).

participants from the dataset to ensure that the presence of depression symptoms did not impact the stimulus normative responses (i.e., the modal responses). The removal of these participants did not affect the modal responses for any trial.

Negative Affect

State negative affect (NA) was measured using items selected from the Positive and Negative Affectivity Schedule (PANAS; Watson et al., 1988). The NA scale of the PANAS consists of 10-items. Participants rate the extent to which they are currently experiencing a range of negative emotions from “very slightly or not at all” (1) to “very much” (5). For brevity in the current study, we selected six items from the PANAS based on having the highest factor loadings onto NA (Crawford & Henry, 2004) (i.e., upset, guilty, hostile, irritable, jittery, scared). Participants responded using a sliding scale from “not at all”(1) to “extremely”(5). Items were summed to create composite NA scores. The baseline NA scale demonstrated questionable internal consistency in this sample ($\alpha = .64$), whereas the NA scale following stimulus blocks ranged from good to excellent (neutral trials: $\alpha = .89$, negative trials $\alpha = .91$).

Mindfulness

Trait mindfulness was assessed using the *Five Facet Mindfulness Questionnaire* (FFMQ; Baer et al., 2006). The FFMQ is a 39-item questionnaire, assessing five facets: *observing*, *describing*, *acting with awareness*, *non-judging of internal experience*, and *non-reactivity to internal experience*. Participants rate their agreement with statements on a 5-point Likert-type scale. For parsimony of predictive models in the current investigation, we created a composite score of FFMQ-*mindfulness* by summing the four scales that were previously shown to load on to an overarching mindfulness factor in non-meditating samples (Baer et al., 2008). We did not include the *observing* subscale in this composite score, because it was positively correlated with maladaptive constructs, including dissociation, absent-mindedness, thought suppression, and symptoms of psychological disorders in similar participant populations (Baer et al., 2006; Baer et al., 2008). Accordingly, in the current study, the *observing* subscale was marginally positively associated with depression symptoms, $r(83) = .21$, $p = .053$, and was uncorrelated with three of the four remaining FFMQ subscales (subscale inter-correlations can be found in Table 2). The resulting FFMQ-*mindfulness* composite subscale demonstrated good internal consistency in this sample ($\alpha = .88$).

Decentering

Decentering was measured using the *Experiences Questionnaire, decentering subscale* (EQ; Fresco, Moore, et al., 2007). The EQ-*decentering* subscale consists of 11 items. Participants rate the frequency with which they have various experiences on a 5-point Likert-type scale ranging from “never” (1) to “all the time” (5). A recent re-examination of the factor structure of EQ-*decentering*, suggested a 2-factor structure: *distanced perspective* (4 items) and *accepting self-perception* (4 items) (Gecht et al., 2014). The *distanced perspective* factor is said to assess the capacity to view experiences objectively, with healthy psychological distance (e.g., “I can separate myself from my thoughts and feelings”), and the *accepting self-perception* factor is thought to reflect aspects of self-awareness, acceptance and compassion (e.g., “I can treat myself kindly”) (Gecht et al., 2014). In the current study, we examined the single-factor *decentering* scale, and the *distanced perspective* subscale, according to the hypotheses outlined above. Unfortunately, during data collection in the current investigation, a computer-questionnaire coding error resulted in failure to encode questionnaire data for the first 22 study participants (71% female 81% Caucasian; mean age = 21.35, SD = 5.48). Individuals with missing EQ data were not found to be systematically different from the sample in terms of relevant characteristics such as depression or mindfulness. In the remaining participants, the 11-item *decentering* subscale demonstrated acceptable internal consistency (α = .74), and the *distanced perspective* (α = .58) subscale demonstrated poor internal consistency.

Results: Study 2

Initial treatment of data

We calculated an overall “correctness” score for each participant, which represented the number of total responses that matched the response norms of psychologically healthy individuals that were generated in Study 1[3]. Higher correctness scores indicated that responses were more consistent with those of healthy normal individuals and vice versa. We identified five participants who had less than 50% “correct” responses and who were statistical outliers in terms of correctness (i.e., less than two standard deviations below the mean correctness) (n = 5). These participants likely did not comply with- or understand task instructions, and thus were removed from the sample to minimize their negative impact on the results. To remove bias from our estimates of RT and to increase the power of our statistical tests, we removed participants with extreme RT scores. Specifically, we removed participants if their mean RT was greater or less than two standard deviations of the overall mean RT for all participants (n = 4) (e.g., Ratcliff, 1993). The excluded individuals (n = 9) were not found to be systematically different from the sample in terms of relevant characteristics such as depression, levels of FFMQ-*mindfulness* or EQ-*decentering*. The final sample (n = 85) was 79% Caucasian, 68% female, mean age = 20.82, SD = 4.83, age range: 18–48 years, with a mean correctness of 85.36%.

Tests of manipulation check hypotheses

Ambiguity hypothesis—Ambiguity (i.e., uncertainty with regard to whether a stimulus object would or would not fit in the hand or shoebox) will not vary as a function of valence or context conditions.

Test of ambiguity hypothesis—We calculated and analyzed ambiguity scores according to the exact procedures described in Study 1. Ambiguity scores were subjected to an rmANOVA, with context (hand, shoebox) and valence (negative, neutral) as within-subject factors. The 2 × 2 rmANOVA revealed no main effects of valence, $F(1,71) = 3.08$, $\eta^2 = .042$, or context, $F(1,71) = .24$, $\eta^2 = .003$, and no interaction of valence × context on ambiguity scores, $F(1,72) = .42$, $\eta^2 = .006$, confirming that levels of ambiguity were not significantly associated with the manipulations of interest in Study 2. To further rule out

[3]If men have larger hands on average than women, they might be more likely to indicate that objects would fit in their hands; similarly, women might be more likely to say that objects would not fit in their hands. Independent samples t-tests revealed that men and women did not differ in a) the frequency with which they reported that objects would fit in their own hands, or b) the frequency with which they reported that objects would not fit in their own hands. Thus, gender did not appear to be associated with a systematic response bias.

potentially problematic effects of ambiguity, we examined the effects of removing trials for which responses were deemed highly ambiguous[4].

Negative affect hypothesis—Participants will report more negative affect in response to negative versus neutral trials.

Test of negative affect hypothesis—Negative affect was subjected to an rmANOVA, with context (hand, shoebox) and valence (negative, neutral) as within-subject factors. The 2 × 2 rmANOVA revealed a main effect of valence on negative affect, confirming that negative objects elicited more negative affect than neutral objects, $F(1,84) = 16.559$, $p < .001$, $\eta^2 = .17$. There was no main effect of context- and no interaction of context × valence on negative affect. Means are presented in Table 1.

Correctness hypotheses—Correctness to negative stimuli will be higher than correctness to neutral stimuli (i.e., a main effect of emotional valence on correctness). Critically however, the effect of valence will be stronger in the hand/high-threat context compared with the shoebox/low-threat context (i.e., the effects of valence and context on correctness will interact).

Tests of correctness hypotheses—We calculated correctness scores for each of the four combinations of valence and context (negative/hand, negative/shoebox, neutral/hand, neutral/shoebox). Correctness scores reflected the number of responses in each condition that matched the response norms of psychologically healthy individuals (generated in Study 1). Higher correctness scores indicated that responses were more consistent with those of healthy normal individuals and vice versa. Correctness scores were subjected to rmANOVA, with context (hand, shoebox) and valence (negative, neutral) as within-subject factors. Effect sizes were calculated as η^2 for ANOVA and Cohen's d for post-hoc t-tests. As expected, the 2 × 2 rmANOVA revealed a significant interaction of valence × context on correctness, $F(1,84) = 9.19$, $p < .01$, $\eta^2 = .099$. Paired-samples t-tests revealed that the effect of valence was significant in the hand context $t(84) = 2.72$, $p < .01$, $d = -.31$: participants had fewer correct responses in the negative/hand context compared with the neutral/hand context; the effect of valence was not significant in the shoebox context, $t(84) = -.51$. Means are presented in Table 1.

Response time (RT) hypotheses—RTs to negative stimuli will be longer than RTs to neutral stimuli (i.e., a main effect of emotional valence on RTs). Critically however, the effect of valence will be stronger in the hand/high-threat context compared with the shoebox/low-threat context (i.e., the effects of valence and context on RT will interact). In addition, the effects of the context and valence manipulations will be stronger in individuals who demonstrate high levels of correctness.

[4]We identified highly ambiguous trials using the following procedure. First, we identified stimuli for which greater than- or equal to 30% of responses were inconsistent with the modal response in Study 1; then in Study 2, we identified stimuli for which greater than or equal to 45% of responses were inconsistent with the modal response. We used a more conservative threshold in the experimental sample due to the fact that it consisted of a greater number of participants relative to the normative sample, and thus more reliable ambiguity scores. If a trial was ambiguous in both samples, it was removed. This procedure resulted in removal of 10 trials (one trial from the negative/hand context and three trials in each of the other conditions). Removing these trials only slightly improved the strength of interactions. Thus, we retained the ambiguous trials in further analyses.

Tests of response time (RTs) hypotheses—We calculated median RTs to complete the size evaluation for each of the four combinations of valence and context (negative/hand, negative/shoebox, neutral/hand, neutral/shoebox). We used median RTs because they are less sensitive to outliers than arithmetic means (e.g. Heuer, Rinck, & Becker, 2007; Rinck & Becker, 2007) and thus would be less susceptible to potential impacts of response ambiguity. Median RTs were subjected to rmANCOVA, with context (hand, shoebox) and valence (negative, neutral) as within-subject factors, with total correctness as a covariate. The 2 × 2 ANCOVA revealed a significant three-way interaction of valence × context × correctness, $F(1,83) = 5.72$, $p < .05$, $\eta 2 = .064$, a significant 2-way interaction of valence × context, $F(1,83) = 5.42$, $p < .05$, $\eta 2 = .061$, and a significant 2-way interaction of valence × correctness, $F(1,83) = 8.94$, $p < .01$, $\eta 2 = .097$.

The significant three-way interaction indicated that the interaction of valence and context on RT varied as a function of correctness. Thus, to further explore three-way interaction, we visually examined the interaction of valence and context on RT at high- and low levels of correctness. As expected, this examination suggested that the interaction of valence and context was more pronounced at higher levels of correctness: at high levels of correctness, the discrepancy in the effect of valence on RT in the hand context versus the shoebox context appeared larger.

To further examine the relationship of the valence × context interaction to correctness using a more sensitive statistical technique, we used Pearson Bivariate correlation to compute the association of correctness to the components of the interaction. First, we calculated the difference in RTs to negative versus neutral trials for the hand context and the shoebox context separately (i.e., negative minus neutral “difference scores”). Correctness was negatively associated with the difference score in the shoebox, $r(83)= -.39$, $p <.001$, but not the hand context $r(83)= -.01$, $p >.05$. Specifically, at higher levels of correctness, the difference between negative and neutral objects in the shoebox context was smaller. Second, we calculated the difference in RTs to the hand versus the shoebox context for negative versus neutral trials separately (i.e., hand minus shoebox “difference scores”). Correctness was positively associated with the difference score for negative trials, $r(83)= .21$, $p <.05$, but not for neutral trials, $r(83)= -.16$, $p >.05$. In other words, at higher levels of correctness the difference in RTs to the hand versus shoebox context was bigger for negative trials. Taken together, this pattern of results indicated that the effects of the valence and context manipulations were stronger at higher levels of correctness, as expected.

Depression hypothesis—Higher levels of self-reported depression will be associated with lower levels of overall correctness.

Test of depression hypothesis—Bivariate Pearson correlations revealed that depression was marginally negatively associated with total correctness, $r(83)= -.20$, $p = .063$, confirming that individuals with higher levels of depression symptoms produced fewer correct responses than individuals with lower levels of depression.

Exploratory correlation analyses—Total correctness was not significantly associated with FFMQ-*mindfulness*, $r(83) = .18$, EQ-*decentering*, $r(63) = .02$, EQ-*distanced perspective*, $r(63) = .10$, age $r(83) = -.18$, gender $r(83) = .08$, or handedness $r(83) = .14$.

Tests of validation hypotheses

Individual differences hypotheses—After controlling for factors associated with general mental manipulation abilities (i.e., correctness and depression symptoms), higher (versus lower) levels of mindfulness and decentering/distancing will be associated with smaller differences in RTs to negative versus neutral objects in the shoebox/low-threat condition. There will not be a relationship between mindfulness and decentering/distancing scales to RTs in the hand/high-threat condition.

Tests of individual differences hypotheses—We conducted separate regression analyses for FFMQ-*mindfulness*, EQ-*decentering*, and EQ-*distanced perspective*, and ran two regression analyses for each scale: one for the hand context, and one for the shoebox context. RT difference scores served as the dependent measures in all regressions. RT difference scores were calculated for the hand and shoebox contexts separately, by subtracting median RTs for neutral trials from median RTs for negative trials. In these analyses, we used a conservative, family-wise Bonferroni multiple-comparison correction (i.e., three tests per hypothesis, $p < .017$; Dunn, 1961) when interpreting the significance of our results. Difference score means and zero-order correlations with other measures are presented in Table 2.

In each regression analysis, we controlled for factors associated with general mental manipulation and evaluation abilities (i.e., correctness and depression symptoms). To be as specific as possible when controlling for the effects of correctness on RT difference scores, we used "correctness difference scores" as covariates. Correctness difference scores were calculated in the same manner used to calculate RT difference scores; specifically, correctness difference scores were calculated for the hand and shoebox contexts separately, by subtracting correctness for neutral trials from correctness for negative trials. We entered correctness difference scores and depression symptoms in the first step of each model. The second step in each model included the individual difference variable of interest (FFMQ-*mindfulness*, EQ-*decentering*, EQ-*distanced perspective*). Means and zero-order correlations of measures of mindfulness and decentering scales, and correctness scores are presented in Table 2. The results of regression analyses are presented in Tables 3-5.

Hierarchical Regression Analyses: Mindfulness predicting RT difference scores (Table 3)

Hand context (high-threat)—The initial step combining correctness and depression symptoms was not significant, accounting for 3% of the variance in difference scores for the hand context. Adding FFMQ-*mindfulness* did not explain additional variance in difference scores.

Shoebox context (low-threat)—The initial step was significant, accounting for 11% of the variance in difference scores for the shoebox context. Adding FFMQ-*mindfulness* explained an additional 8% of the variance in difference scores, and significantly increased the overall R^2 of the equation to .19, $F(1,81) = 7.61$, $p < .01$, Cohen's $f^2 = .233$.

Summary—After controlling for depression and the specific effects of correctness, individuals who reported higher levels of FFMQ-*mindfulness* did not differ from individuals with lower levels in the effect of valence in the high-threat condition; however higher levels of FFMQ-*mindfulness* were associated with smaller effects of valence in the low-threat condition relative to lower levels (i.e., smaller differences in RTs to negative versus neutral stimuli).

Hierarchical Regression Analyses: Decentering predicting RT difference scores (Table 4)

Hand context (high-threat)—The initial step combining correctness and depression symptoms was not significant, accounting for 2% of the variance in difference scores for the hand context. Adding EQ-*decentering* did not explain additional variance in difference scores.

Shoebox context (low-threat)—The initial step was not significant, accounting for 8% of the variance in the difference scores for the shoebox context. Adding EQ-*decentering* explained an additional 4% of the variance in difference scores, but did not significantly increase the overall R^2 of the equation, $F(1,59) = 1.72$, $f^2 = .120$.

Summary—The full EQ-*decentering* scale was not significantly associated with the effects of valence on RTs in either the high- or low-threat condition; however, the observed small to medium effect size suggests a potentially meaningful relationship.

Hierarchical Regression Analyses: Distanced Perspective predicting RT difference scores (Table 5)

Hand context (high-threat)—The initial step combining correctness and depression symptoms was not significant, accounting for 2% of the variance in difference scores for the hand context. Adding EQ-*distanced perspective* did not explain additional variance in difference scores.

Shoebox context (low-threat)—The initial step was significant, accounting for 8% of the variance in difference scores for the shoebox context. Adding EQ-*distanced perspective* explained an additional 13% of the variance in difference scores, and significantly increased the overall R^2 of the equation to .21, $F(1,59) = 9.21$, $p < .01$, $f^2 = .258$.

Summary—After controlling for depression and the specific effects of correctness, individuals who reported higher levels of EQ-*distanced perspective* did not differ from individuals with lower levels in the effect of valence in the high-threat condition; however higher levels of EQ-*distanced perspective* were associated with smaller effects of valence in the low-threat condition relative to lower levels (i.e., smaller differences in RTs to negative versus neutral stimuli).

Discussion

Numerous researchers have called for the development of valid, objective measures of mindfulness and decentering, which may be used to evaluate the benefits of many common interventions (e.g., MBCT, MBSR, ERT, DBT, ACT) (e.g., Baer et al., 2009). In the current investigation we sought to begin addressing this gap. Specifically, we developed a task that required individuals to mentally manipulate negative emotional (and neutral) material away from the self, in order to capture self-distancing skills that likely underlie healthy emotional processing, and which are thought to be cultivated in many cognitive therapy interventions. Our findings provide preliminary support that this task holds promise for objectively evaluating general mental manipulation and evaluation abilities, as well as the capacity to self-distance from emotional material.

Results suggested that our manipulations were successful. We found that RTs were longer for negative compared with neutral objects, but the effect of object valence on RTs varied as a function of systematically manipulating the threat context (i.e., hand vs. shoebox). Specifically, the difference in RTs to negative versus neutral objects was smaller in the low-threat context versus the high-threat context. The same pattern of results was found with respect to error rates (i.e., correctness): error rates were higher for negative compared with neutral objects, and the effect of valence on error rates was smaller in the low-threat context. Furthermore, in terms of RTs, the discrepancy between the high- and low-threat contexts was bigger in individuals who had higher levels of overall correctness (i.e., whose size evaluation responses were more consistent with the responses of psychologically healthy individuals). In other words, at higher levels of correctness, the interaction of object valence and context was more pronounced. This finding is consistent with our expectation that it would be easier to detect and measure manipulation effects in individuals who had higher levels of accuracy.

Taken together, these findings were consistent with our hypothesis that individuals would have greater difficulty manipulating and evaluating the size of negative objects in the high-threat relative to the low-threat context: the high-threat context was expected to impede mental manipulation and evaluation to a greater extent than the low-threat context (Öhman & Mineka, 2001). Indeed, the act of bringing negative objects closer to the body was thought to be inconsistent with fundamental motivations to avoid harm, and to be more disruptive to higher-level cognitive processes such as mental manipulation; whereas pushing objects away from the self was consistent with natural distancing motivations and thus was expected to be less disruptive (Heuer et al., 2007; Rinck & Becker, 2007).

In addition to validating our manipulations at the group-level, a primary, overarching hypothesis of the current investigation was that individuals with higher levels of mindfulness and decentering/distancing would demonstrate a greater capacity to manipulate emotional material away from the self; that is, they would show smaller differences in RTs to negative versus neutral objects in the low-threat context, relative to individuals with lower levels of mindfulness and decentering/distancing. Results largely supported these predictions. After controlling for individual differences in factors affecting general mental manipulation and evaluation abilities (i.e., correctness and depression), mindfulness predicted smaller effects of object valence in the low-threat, but not the high-threat context. Furthermore, we observed the same pattern of results for the *distanced perspective* facet of the EQ.

However, contrary to expectations, the full-scale decentering measure was not a statistically significant predictor of task performance, even though the effect size approached conventions for a medium effect. This null result might reflect a weaker association of the *accepting self-perception* factor items to task performance. Accordingly, performance on the self-distancing task might depend more heavily on the capacity to adopt a distanced perspective, rather than on aspects of self-perception. These findings may also lend credence to the multifaceted definition of decentering offered by Bernstein and colleagues (2015). However more research is needed to further understand these relationships, particularly in light of ongoing efforts to resolve- and validate the definition and measurement of decentering with instruments such as the EQ (e.g., Fresco, Moore, et al., 2007; Gecht et al., 2014).

Our findings are consistent with theory and research suggesting that mindfulness and self-distancing are associated with better abilities to mentally manipulate, or *work with* emotional material in conscious awareness, without becoming stuck in recursive patterns of self-referential thinking (e.g., rumination and worry; Hölzel et al., 2011; Teasdale, 1999; Vago & Silbersweig, 2012). Indeed, higher levels of mindfulness have been associated with numerous cognitive advantages, including better attentional orienting (Jha, Krompinger, & Baime, 2007), flexible disengagement of attention from emotionally evocative stimuli (Ortner et al., 2007), and working memory capacities (Chambers, Lo, & Allen, 2008; Jha, Stanley, Kiyonaga, Wong, & Gelfand, 2010; van Vugt & Jha, 2011). However, a priority of subsequent validation studies will be to examine whether the self-distancing task is sensitive to the effects of mindfulness meditation training, in addition to self-reported mindfulness.

One of the goals of mindfulness-based practices is to reduce biased processing of negative stimuli through the cultivation of decentering and “equanimity” (Vago & Silbersweig, 2012). Equanimity refers to “an even-minded mental state or dispositional tendency toward all experiences or objects, regardless of their affective valence (pleasant, unpleasant or neutral) or source” that is thought to depend on decentering capacity (for review see, Desbordes et al., 2014). Thus, more experienced meditators, who have developed a greater capacity for decentering and equanimity, might be expected to show minimal differences in RTs to negative and neutral stimuli, at least in the low-threat context. However, it is an open question as to whether experience in meditation would predict RT effects in the high-threat context as well; it is possible that experienced meditators would demonstrate better mental manipulation and evaluation abilities even in the high-threat context, potentially related to more efficient disengagement from emotional material (Desbordes et al., 2012; Ortner et al., 2007; Vago & Nakamura, 2011).

A secondary finding in the current study was that individuals who reported higher levels of depression symptoms made more errors in judging the size of objects overall. This finding may reflect general difficulties with mental manipulation and evaluation related to depression. Indeed, depressed individuals have demonstrated difficulties manipulating both negative and neutral material in working memory, compared with their non-depressed counterparts; and these difficulties manipulating negative material in particular, have been linked to higher levels of depressive rumination (Joormann et al., 2011). It is important to note, however, that a normative, non-clinical sample was examined in the current study. Thus further studies using clinically depressed individuals are needed to better elucidate the relationship of depression symptoms to task performance. Additionally, future work could explore whether, a) self-distancing task performance is associated with other disorders of biased emotional processing, such as generalized anxiety disorder (Mennin & Fresco, 2013), and b) whether the self-distancing task is sensitive to mindfulness-based interventions and other psychotherapies that aim to enhance the capacity to mentally manipulate emotional material, (e.g. MBCT, MBSR, ERT, ACT, DBT).

Future work is also needed to examine associations of self-distancing task performance to other theoretically related mental phenomena. The capacity to mentally manipulate emotional material away from the self may be related to a range of cognitive processes including basic metacognitive capacities to monitor (i.e., observe) and control cognition (Teasdale et al., 2002), as well as individual differences in cognitive flexibility and the capacity to adapt responses to contextual contingencies (Davis & Nolen-Hoeksema, 2000; Joormann et al., 2011). Furthermore, as the capacity to self-distance is thought to serve an emotion regulation function, future work should examine the relationship of self-distancing task performance to other known forms of cognitive emotion regulation, including re-appraisal or re-construal of experiences (Ayduk & Kross, 2008, 2010).

Our study had several limitations. First, the generalizability of our results to more representative populations is limited by our exclusive use of university community samples. In the future, it will be important to extend this work to more representative samples, as well as meditating- and clinical samples. Relatedly, our future studies will work to establish more comprehensive trial response norms. The current study used a small sample of healthy individuals on which to establish the “correctness” of responses in the larger Study 2 sample. More comprehensive norms that are based on a larger group of healthy individuals would increase the likelihood that the response norms accurately represent the responses of the typical, healthy person.

Other limitations pertain to the use of self-report measures to validate our manipulations. Self-report measures of mindfulness and decentering arguably suffer from conceptual and methodological issues, which complicate their interpretation. For example, in the current investigation, we used a composite measure of mindfulness, which excluded the *observing* subscale due to findings suggesting that *observing*, in the absence of other qualities of mindful attention (e.g., *non-judging*), captures a maladaptive process in non-meditating university community samples (Baer et al., 2008; Eisenlohr-Moul et al., 2012; Derosiers et al., 2014). Thus our measure of mindfulness did not represent the full range of skills associated with FFMQ mindfulness. Second, the EQ-*distanced perspective* scale demonstrated low internal consistency, which suggests that the factor structure of this scale may be unreliable, at least with regard to the English version of the EQ used herein. These two limitations are related to larger issues in the self-report measurement of mindfulness and decentering, reviewed above, and represent important issues for further study. Third, in Studies 1 and 2, the measure of depression symptoms we used demonstrated somewhat lower internal consistency than what has been observed in published studies of the measure in clinical samples (e.g., Rush et al., 2003). We suspect that this discrepancy reflects limitations related to the use of measures developed based on clinical syndromes to assess symptoms in nonclinical populations (e.g., Dohrenwend, Shrout, Egri, & Mendelsohn, 1980); however there appears to be a lack of internal consistency data for this measure in nonclinical samples on which to compare our estimates. Despite that this measure was not a focus of the current studies (i.e., it was used as a secondary screening measure in Study 1 and as a covariate in Study 2), it should be noted that its low internal consistency could negatively impact the reliability of the symptom estimates obtained.

An additional limitation of the current study pertains to the presumptions underlying the context manipulations. Specifically, to accurately perform the size judgment task, we presumed that participants had to imagine bringing the stimulus objects into their own hands (i.e., pulling the objects toward the self) or putting the objects into a shoebox (i.e., pushing the objects away from the self). These types of presumptions are common to cognitive tasks that involve an imagery component. For example, in the typical behavioral approach-avoidance paradigm, a picture on a computer screen shrinks when participants push a joystick away from themselves; or when a joystick is pulled, the picture grows until it fits the screen (e.g., Rinck & Becker, 2007). In a similar paradigm, participants are prompted to imagine emotionally laden stimuli growing in size or shrinking in size on a computer screen (Davis, Gross, & Ochsner, 2011). This visual zooming effect creates the illusion that the pictures are approaching or receding in physical space, and we are to presume that participants perceive the images moving toward or away from their bodies. Nonetheless, these types of presumptions can hinder interpretation of the behavioral findings. While, the current investigation did not allow us to test these presumptions directly, future studies could interview participants about their strategy use as a first step toward elucidating the mental processes involved in the size evaluation task.

Finally, our study is limited by its exclusive focus on negative and neutral stimuli. Future iterations of this task could include a positive condition, which would provide an additional level of contrast with negative and neutral conditions.

Despite limitations of the current investigation, initial results suggest that the self-distancing task holds promise for objectively evaluating the capacity to manipulate emotional material away from the self, as well as more general mental manipulation and evaluation abilities. The self-distancing task has several advantages. For example, its non-verbal nature, combined with short administration time (less than 10 minutes), may make it amenable to use with children and non-English speaking populations. Additionally, the design of the self-distancing task minimizes the effects of demand characteristics and participant expectations on the results, as the size evaluation task is ostensibly unrelated to the contextual manipulations of interest. This latter advantage might make the self-distancing task a good candidate for assessing treatment-related changes. Future work will continue to validate this task in meditating- and clinical populations and against other mental phenomena associated with mental health and well-being.

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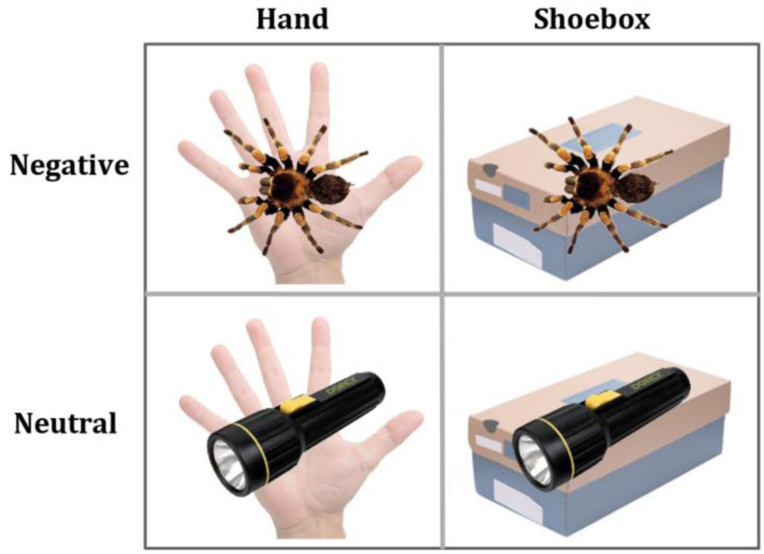

Figure 1. Self-distancing Task design

Note. Figure depicts a blocked, 2 (valence: neutral, negative) × 2 (context: hand, shoebox), counterbalanced, factorial design. Note that images were randomly assigned to condition between participants and no participant saw the same image twice.

Table 1

Group means of dependent measures by condition

Measure	Valence	Context	
		Hand M (SD/SE)	Shoebox M (SD/SE)
Negative Affect (NA)	Negative	.10 (.11)	.10(.12)
	Neutral	.06(.11)	.07(.10)
Correctness	Negative	31.47(4.49)	30.58(3.23)
	Neutral	30.14(4.07)	30.79(2.95)
Response Time (RT)	Negative	1011.97(30.94)	991.49(28.83)
	Neutral	932.13(25.00)	938.73(24.71)

N = 85. NA was assessed using a modified, short-version of the PANAS NA scale. Correctness means represent the number of responses that were consistent with normative responses. RTs are in milliseconds, and RT means are adjusted for correctness with standard errors in parentheses.

Table 2

Group means and zero-order correlations of measures

Measure	M (SD)	1	2	3	3a	4	5	6	7
1. Mindfulness	96.87(13.75)	--							
2. Depression	6.16(3.59)	−.66***	--						
3. Decentering	46.97(2.18)	.57***	−.42**	--					
3a. Distanced perspective	15.90(2.18)	.50***	−.28*	.69***	--				
4. Hand (Correct:Neg-Neu)	1.33(4.51)	.01	−.09	.01	.05	--			
5. Shoebox (Correct:Neg-Neu)	−.20(3.64)	.01	−.14	−.09	−.12	.36**	--		
6. Correctness (Overall)	126.00(10.62)	.18	−.20	.02	.10	.17	.24*	--	
7. Hand (RT:Neg-Neu)	88.00(255.75)	−.07	.10	−.01	−.10	−.14	−.13	−.01	--
8. Shoebox (RT:Neg-Neu)	39.00(261.60)	−.25*	.11	−.15	−.32*	.03	−.33**	−.39**	−.17

Note. 1. Mindfulness is a composite scale derived from the sum of four FFMQ subscales (describing, acting with awareness, nonjudging, non-reacting). 2) Depression was assessed using the QIDS-SR$_{16}$. 3-3a) Decentering and distanced perspective were assessed using the EQ. 4) Mean correctness to negative trials minus mean correctness to neutral trials in the “hand” condition. 5) Mean correctness to negative trials minus mean correctness to neutral trials in the “shoebox” condition. 6) Mean total correctness. 7) Median RT to negative trials minus median RT to neutral trials in the “hand” condition. 8) Median RT to negative trials minus median RT to neutral trials in the “shoebox” condition. RT “Correct” = correctness scores. N= 85 for all measures except *N* = 63 for EQ scales (decentering and distanced perspective).

* $p < .05$.

** $p < .01$.

*** $p < .001$.

Table 3

Hierarchical Regression Analyses: Mindfulness predicting RT difference scores

Variable	Beta	Partial Correlation
Model 1: Hand context		
Step 1: $R^2 = .03$, $\Delta F(2,82) = 1.11$, $f^2 = .026$		
Correctness	−.13	−.13
Depression	.08	.09
Step 2: $R^2 = .03$, $\Delta F(1,81) = .04$, $f^2 = .028$		
Correctness	−.13	−.13
Depression	.06	.05
Mindfulness	−.03	−.02
Model 2: Shoebox context		
Step *1:* $R^2 = .11$, $\Delta F(2,82) = 5.19^{**}$, $f^2 = .126$		
Correctness	−.32**	−.32
Depression	.06	.06
Step 2: $R^2 = .19$, $\Delta F(1,81) = 7.61^{**}$, $f^2 = .233$		
Correctness	−.35**	−.36
Depression	−.19	−.15
Mindfulness	−.37**	−.29

Note: $N = 85$. The dependent variable is the difference in RTs between negative and neutral trials calculated and entered separately for the hand and shoebox contexts; higher scores indicated a greater effect of stimulus valence on RTs. “Correctness” is the difference in correctness between negative and neutral trials, calculated and entered separately for hand and shoebox contexts; higher scores indicated a greater effect of stimulus valence on correctness. Depression was assessed using the QIDS-SR_{16}. Mindfulness is a composite scale derived from the sum of four FFMQ subscales (describing, acting with awareness, nonjudging, non-reacting).

*$p < .05$,

**$p < .01$.

Table 4

Hierarchical Regression Analysis: Decentering predicting RT difference scores

Variable	Beta	Partial Correlation
Model 1: Hand context		
Step *1:* $R^2 = .02$, $\Delta F(2,60) = .47$, $f^2 = .015$		
Correctness	−.07	−.07
Depression	.10	.10
Step 2: $R^2 = .02$, $\Delta F(1,59) = .08$, $f^2 = .017$		
Correctness	−.07	−.07
Depression	.12	.11
Decentering	.04	.04
Model 2: Shoebox context		
Step 1: $R^2 = .08$, $\Delta F(2,60) = 2.65$, $f^2 = .088$		
Correctness	−.27*	−.27
Depression	.07	.07
Step 2: $R^2 = .12$, $\Delta F(1,59) = 1.72$, $f^2 = .120$		
Correctness	−.29*	−.29
Depression	−.01	−.01
Decentering	−.18	−.17

Note: $N = 63$. The dependent variable is the difference in RTs between negative and neutral trials calculated and entered separately for the hand and shoebox contexts; higher scores indicated a greater effect of stimulus valence on RTs. "Correctness" is the difference in correctness between negative and neutral trials, calculated and entered separately for hand and shoebox contexts; higher scores indicated a greater effect of stimulus valence on correctness. Depression was assessed using the QIDS-SR_{16}. Decentering was assessed using the EQ.

* $p < .05$.

Table 5

Hierarchical Regression Analysis: Distanced Perspective predicting RT difference scores (N = 63)

Variable	Beta	Partial Correlation
Model 1: Hand context		
Step 1: R^2 = .02, ΔF(2,60) = .47, f^2 = .015		
Correctness	−.07	−.07
Depression	.10	.10
Step 2: R^2 = .02, ΔF(1,59) = .34, f^2 = .021		
Correctness	−.07	−.07
Depression	.08	.08
Distanced Perspective	−.08	−.08
Model 2: Shoebox context		
Step 1: R^2 = .08, ΔF(2,60) = 2.65, f^2 = .088		
Correctness	−.27	−.27
Depression	.07	.07
Step 2: R^2 = .21, ΔF(1,59) = 9.21**, f^2 = .258		
Correctness	−.32*	−.34
Depression	−.04	−.04
Distanced Perspective	−.37**	−.37

Note: N = 63. The dependent variable is the difference in RTs between negative and neutral trials calculated and entered separately for the hand and shoebox contexts; higher scores indicated a greater effect of stimulus valence on RTs. “Correctness” is the difference in correctness between negative and neutral trials, calculated and entered separately for hand and shoebox contexts; higher scores indicated a greater effect of stimulus valence on correctness. Depression was assessed using the QIDS-SR_{16}. Distanced perspective was assessed using the EQ.

* $p < .05$,

** $p < .01$.