THE RELATIONSHIP OF MIND WANDERING
TO
WORKING MEMORY AND MINDFULNESS

By

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Abstract

Mind wandering, or thought unrelated to the task at hand, predicts mood in the moment more than five times better than what activity a person is doing and is implicated in clinical disorders such as depression. In addition to emotion, mind wandering predicts errors in reading comprehension and tasks requiring sustained attention. Given mind wandering's prevalence in 50% of daily life and its relevance to well-being and cognitive performance, it is worth better understanding how to regulate it.

Therefore, we investigated mind wandering's correlative, causal, and mechanistic relationship to two of its potential regulators, working memory and mindfulness. In chapter 2, we investigated whether working memory solely inhibits mind wandering or instead facilitates mind wandering in a context-dependent fashion. We accomplished this by assessing whether those with greater working memory capacity reported more mind wandering specifically during undemanding tasks. In chapter 3, we explored whether mindfulness was related to decreased mind wandering by developing a behavioral measure of mindfulness - breath counting - and assessing whether it was negatively correlated with mind wandering. In chapter 4, we investigated the causal relation of mind wandering with working memory and mindfulness by assessing whether working memory training would increase working memory and thus increase mind wandering, and whether breath counting training would increase mindfulness and thus decrease mind wandering. Finally, in chapter 5, we examined whether working memory capacity reduced mind wandering by restricting it from awareness. This mechanism was investigated by assessing whether awareness of mind wandering increased when working memory was loaded and thus no longer available to restrict mind wandering from awareness.

Findings suggest that working memory may facilitate mind wandering in undemanding contexts. In contrast, mindfulness may result in decreased mind wandering in the moments when it is exercised, and repeatedly exercising mindfulness through breath counting training may increase mindfulness.

Chapter 1

Introduction

Mind Wandering into Unhappiness

Giving undivided attention to the person or task in front of us is a gratifying but rare ability. For most, the mind wanders 30-50% of daily life and predicts affect in the moment more than five times better than what activity a person is doing (Killingsworth & Gilbert, 2010; Klinger & Cox, 1987). The majority of mind wandering, or task–unrelated thought (TUT), is about neutral and negative topics, and it is these thoughts that account for the correlation between negative affect and mind wandering (Killingsworth & Gilbert, 2010).[1]

Repetitive focus on negative task-unrelated thoughts, or rumination (Morrow & Nolen-Hoeksema, 1990), is a risk factor for developing depression (Nolen-Hoeksema, Wisco, & Lyubomirsky, 2008), a leading cause of disability worldwide (Vos et al., 2012). Moreover, for those currently depressed, rumination is associated with increased negative affect and duration of the depressive episode (Nolen-Hoeksema, 1991). Rumination in healthy controls and depressed participants alike correlates with high functional connectivity in "default" brain areas (Berman et al., 2011) that have been repeatedly implicated in mind wandering (Christoff, Gordon, Smallwood, Smith, & Schooler, 2009; Mason et al., 2007; Stawarczyk, Majerus, Maquet, & D'Argembeau, 2011). This evidence of a common neural substrate supports the view that

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[1] Despite the tendency of mind wandering on average to be associated with negative affect, this does not preclude that specific timings or contents of mind wandering may carry benefits (Watkins, 2008). For example, engaging in an undemanding task permissive to mind wandering following a creative impasse improves later creativity (Baird et al., 2012). However, these distinctions within mind wandering are beyond the scope of the present work and will not be addressed further.

rumination is part of the mind wandering continuum, underlining the relevance of mind wandering to well-being in both the general and clinical populations. It is no wonder mind wandering and its regulation by working memory and mindfulness are matters of active debate in the literature. Accordingly, they are the focus of the present research.

The Role of Working Memory in Mind Wandering

Mind wandering may come not only at an emotional cost but also a cognitive cost. Depressed individuals induced to ruminate (vs. not) perform more poorly on subsequent tests relying on working memory (Watkins & Brown, 2002), the limited-capacity cognitive resource that maintains and manipulates information not present in the external environment (Baddeley & Hitch, 1974). Likewise, Attention Deficit Hyperactivity Disorder (ADHD) is marked by both high levels of mind wandering (Shaw & Giambra, 1993) and poor performance on working memory tasks (Barkley, 1997).

Evidence supporting the theory that mind wandering consumes working memory resources needed for tasks is also found in healthy individuals. Mind wandering lessens when working memory resources are reduced, such as in old age (Krawietz, Tamplin, & Radvansky, 2012) or in activities that demand working memory resources (Teasdale et al., 1995). Additionally, when mind wandering does happen in activities dependent on working memory, performance is impaired (Allan Cheyne, Solman, Carriere, & Smilek, 2009b; Smallwood et al., 2004) and activity increases in brain areas implicated in working memory (Christoff et al., 2009; Stawarczyk, Majerus, Maquet, et al., 2011). Further, during tasks with minimal working memory demands, future-oriented mind wandering increases (Smallwood, Nind, & O’Connor, 2009), especially for individuals with greater working memory capacity (Baird, Smallwood, & Schooler, 2011), and such future mind wandering may be particularly goal directed as it increases when personal priorities are primed (Stawarczyk, Majerus, Maj, Van der Linden, & D'Argembeau, 2011). All these findings are in line with theory that while fragments of TUT can occur spontaneously and automatically, the elaboration and maintenance of such fragments as trains of TUT requires working memory resources (Smallwood & Schooler, 2006a; Teasdale, Proctor, Lloyd, & Baddeley, 1993a).

However, the theory that TUT uses working memory is controversial. An alternative theory suggests that mind wandering does not consume working memory resources but rather that mind wandering decreases during demanding tasks because good task performance requires working memory to restrict mind wandering from awareness (McVay & Kane, 2009a). This view claims working memory inhibits instead of facilitates mind wandering, citing as evidence the negative correlations found between mind wandering and working memory capacity during working-memory-demanding tasks (Kane et al., 2007a; McVay & Kane, 2009a; Unsworth, McMillan, Brewer, & Spillers, 2012).

Whereas previous state manipulation of working memory has failed to adjudicate between these two theories, a resolution could be reached by using trait working memory capacity differences to ask whether individuals who possess greater working memory resources mind wander more when their working memory is minimally occupied by an undemanding task. Such a finding would show that while greater working memory capacity may allow better restriction of mind wandering during working-memory-demanding tasks, during undemanding tasks greater working memory capacity may facilitate mind wandering.

The Role of Mindfulness in Mind Wandering

Just as mind wandering rate depends on working memory, it may also depend on mindfulness, or the ability to rest attention in present moment experience. Specifically, during moments when an individual is mindful, mind wandering is theorized to decrease. In line with this view, self-reported trait mindfulness has been found inversely related to both behavioral and self-report assessments of mind wandering (Allan Cheyne et al., 2009b; Mrazek, Smallwood, & Schooler, 2012). In addition, during mindfulness practice, long-term meditators (vs. novices) report less mind wandering and demonstrate less activity in the brain's default mode network (Brewer et al., 2011), which is typically more active in moments of mind wandering (Christoff et al., 2009) and in those who tend to mind wander most (Mason et al., 2007). Moreover, three months of mindfulness training reduces reaction time variability (Lutz et al., 2009), an index that is high in those who mind wander more (Allan Cheyne et al., 2009b) or have a diagnosis of ADHD (Johnson et al., 2008). Finally, in perhaps the most well-controlled study on the topic to date, meditation-naive participants receiving 8 minutes of guided mindfulness (vs. reading or rest) demonstrate better performance on a subsequent behavioral measure sensitive to mind wandering, the go/no-go Sustained Attention to Response Task (SART; Mrazek et al., 2012).

A shortcoming of the aforementioned studies is lack of a behavioral measure of mindfulness to confirm that the results of decreased mind wandering are not due to report-bias, self-selecting groups, or a more trivial outcome of mindfulness interventions (e.g. relaxation) as opposed to mindfulness per se. The development of a face-valid behavioral measure of mindfulness such as breath counting, tested for construct validity, would enable a rigorous assessment of the relation between mindfulness and mind wandering. If such a behavioral measure were inversely related to mind wandering as well as the errors and negative affect thought to accompany mind wandering, this would provide more solid evidence of a true inverse relation of mind wandering and mindfulness.

Attention Training and Mind Wandering

Despite the correlational evidence linking mind wandering rates to working memory and mindfulness, causal evidence is still lacking. Identifying experimental manipulations that increase working memory or mindfulness would provide a means to test for causal relationships.

In recent years, attention training research has shown promise that capacities such as working memory can be increased through repeated practice of tasks that measure working memory such as the n-back (e.g. (Dahlin, Nyberg, Bäckman, & Neely, 2008; Jaeggi, Buschkuehl, Jonides, & Perrig, 2008a; Klingberg et al., 2005; McNab et al., 2009a). For example, Jaeggi et al. (2008) administered a working memory n-back task for 20 consecutive weekdays and found improvements in working memory following training. Importantly, improvements from working memory training have been found to generalize beyond performance in the trained task, increasing working memory as measured by Backward Digit Span as well as enhancing theoretically related capacities such as general fluid intelligence (Jaeggi et al., 2008a) and cortical dopamine levels (McNab et al., 2009a).

In the same way that working memory may be improved by repeated practice of working memory tasks, mindfulness may be improved by repeated practice of a behavioral measure of mindfulness. Using these methods to experimentally increase working memory or mindfulness, mind wandering rates could be tested after training to assess whether mind wandering is increased with working memory training and decreased with mindfulness training.

Mechanisms for Modulating Mind Wandering

Given the evidence for working memory's ability to facilitate or reduce mind wandering in a context-dependent fashion, a question remains as to how this influence occurs. One theory of how working memory reduces mind wandering is by restricting it from awareness, where working memory capacity serves to create a kind of "spotlight" of awareness for on-task thoughts (McVay & Kane, 2009a). This theory is line with previous research on external distractors, where individuals with greater working memory capacity show less processing of flanking distractors as indexed by faster identification of central targets in a visual search (Ahmed & de Fockert, 2012). Moreover, when working memory is consumed by a secondary task, awareness of distractors increases (de Fockert & Bremner, 2011) and incongruent distractors slow target identification more (Lavie, Hirst, de Fockert, & Viding, 2004a) as if working memory were no longer available to restrict processing of peripheral distractors.

A research strategy similar to that used with external distraction could also be used to explore working memory's mechanism of reducing the internal distraction of mind wandering. For example, working memory could be loaded by a secondary task, e.g. of remembering numbers. If this manipulation increased awareness of mind wandering just as it increased awareness of external distractors, this would be evidence that working memory reduces mind wandering by restricting it from awareness.

However, working memory load is known to reduce the *quantity* of mind wandering itself, in line with theory that working memory resources are required to maintain and elaborate TUT (Levinson, Smallwood, & Davidson, 2012a; Teasdale et al., 1993a). Therefore, any observed change in *awareness* of mind wandering under working memory load could simply be due to a change in the amount of mind wandering of which to be aware. To remedy this ambiguity, perceptual load could be crossed with working memory load in a within-participant design in order to decrease mind wandering without affecting working memory (Forster & Lavie, 2007, 2009a; Lavie et al., 2004a; Lavie, 2005a). If mind wandering were equally decreased by working memory load and perceptual load but awareness of mind wandering were selectively increased by working memory load, this would provide clearer evidence that working memory can be used to restrict mind wandering from awareness. In addition, if those with greater working memory capacity reported less awareness of mind wandering, this would offer even stronger evidence.

The aforementioned avenues of research promise a better understanding of mind wandering and its regulation. With this knowledge, healthy and clinically diagnosed individuals alike may be better positioned to regulate a pervasive and influential dimension of mental life to their benefit.

Chapter 2

The Role of Working Memory in Mind Wandering

The average mind wanders during half of daily life, often thinking quite spontaneously about personal priorities unrelated to the task at hand (Giambra, 1995; Killingsworth & Gilbert, 2010; Klinger & Cox, 1987). Such task- unrelated thought (TUT) presents a paradox: Although the spontaneous nature of TUT suggests that it is a resource-free process, its priority-driven nature suggests that it is a resource- intensive process (Smallwood & Schooler, 2006a). Indeed, priority-driven attention that maintains and manipulates information not present is classically considered to require working memory (WM) resources (Baddeley & Hitch, 1974), and WM-related brain areas are active during TUT (Christoff et al., 2009; Stawarczyk, Majerus, Maquet, et al., 2011). The tension between the spontaneous and goal-directed features of TUT has stimulated debate on whether mind wandering consumes WM resources.

One perspective suggests that TUT requires WM resources in order to persist. According to this view, a fragment of TUT can occur spontaneously, but elaboration of such a fragment into a train of TUT requires WM resources (Smallwood & Schooler, 2006a; Teasdale et al., 1993a). This theory rests on findings that TUT increases when WM resources are available, such as during tasks that place few demands on WM resources (Mason et al., 2007; Teasdale et al., 1993a). Conversely, TUT decreases when WM resources are scarce, such as during tasks that do place a high demand on WM resources (Teasdale et al., 1995). Further, when TUT does occur during tasks relying on WM, performance can decline (Allan Cheyne et al., 2009b; Smallwood et al., 2004), which suggests that maintaining TUT may divert cognitive resources needed for tasks (Smallwood, Beach, Schooler, & Handy, 2008a; Teasdale et al., 1995).

An alternative perspective suggests that TUT does not require WM resources. According to this view, TUT occurs spontaneously and persists in a resource-free manner, entering awareness only when WM fails to restrict attention to a task (McVay & Kane, 2009a, 2010). This theory can likewise explain why TUT decreases during WM-demanding tasks, as good task performance requires WM to unfailingly restrict attention from TUT. Perhaps the strongest evidence for this theory comes from studies of individual differences in WM capacity (WMC). A recent laboratory study indicated that individuals who possess greater WM resources report less TUT during a go/no-go task commonly used to study mind wandering (the Sustained Attention to Response Task, or SART) (McVay & Kane, 2009a).

This evidence that WM may inhibit mind wandering seemingly contradicts any role for WM in maintaining TUT. However, to conclude that WM solely inhibits TUT is premature. The low frequency of trials that refresh task goals in the SART (11% no-go trials in McVay & Kane, 2009) encourages using WM resources to proactively maintain no-go-relevant task goals in order to overcome the habitual go response reinforced by the large majority of trials. Thus, the SART places demands on WM resources that otherwise might have facilitated TUT.

In contrast, tasks with a high frequency of trials that refresh task goals (50% or 100%) relieve WM from proactively maintaining these goals (Kane & Engle, 2003). Such contexts are well suited for exploring whether greater WM resources, when free, support greater TUT. Therefore, to evaluate the two competing models of TUT, we gave participants with a range of WMCs tasks that place low demands on WM and are permissive to mind wandering. In

Experiment 1, we used a visual search task (50% incongruent targets); in Experiment 2, we asked participants to press a key in time with their normal breathing (100% targets). We then examined whether participants with greater WMC mind-wandered more in these contexts placing low demands on WM, as predicted only by the theory that WM can support TUT.

Experiment 1

Method

Ninety-three members of the University of Wisconsin–Madison community received $10 per hour to complete a 30-min visual search task followed by a WMC assessment, the Automated Operation Span (OSPAN) task. The OSPAN task is known to correlate well with established WMC assessments and to predict general fluid intelligence.

As is standard, participants were excluded for scoring below 85% on OSPAN's secondary math task (*n* = 9) and for performing at chance in visual search (*n* = 10). These exclusions left 74 participants (28 males, 46 females; age range = 18–61 years, *M* = 24.7, *SD* = 8.9), with OSPAN scores from 9 through 73 (*M* = 58.9, *SD* = 13). OSPAN scores were squared to yield a more normal distribution (skew = −0.81, kurtosis = −0.46).

Visual search task. For a detailed description of this task, see Experiment 4 of Forster and Lavie (Forster & Lavie, 2009a). In brief, on each trial of the visual search task, a central ring of six letters containing the target—either *X* or *N*—was presented for 100 ms with a peripheral distractor—either *X* or *N*—to the left or right. Participants pressed a key to indicate the target's identity as quickly and accurately as possible.

Trials varied on two dimensions, perceptual load (low, high) and distractor identity (congruent, incongruent). Each block of 48 trials presented a single load condition. In low- load blocks, nontarget letters in the central ring were small *O*s, which allowed the target to be easily distinguished. In contrast, in high-load blocks, nontarget letters (*H, K, M, V, W*, and *Z*) were angular and target sized, which made the target more difficult to perceive. There were eight blocks of each load condition, ordered ABBAABBAABBAABBA. Within each block, trials varied in distractor identity (50% incongruent). Target identity and the position of targets and distractors were counterbalanced across trials.

At the end of each block came the thought probe: "What were you thinking just now?" Participants pressed "0" if they had been thinking task-related thoughts, that is, thoughts "about the task you are doing at that exact moment" (example given in the instructions: "Where's the *X*? Oh, there it is."). Conversely, participants pressed "1" for TUTs (examples given in the instructions: "I must stop by the supermarket on the way home," "I made lots of mistakes at the beginning of the experiment"). The TUT score equaled the percentage of probes on which a participant reported TUT.

OSPAN task. The OSPAN task consisted of 15 trials. On each trial, the display alternated three to seven times between single letters, which were to be memorized and reported at the end of the trial, and math equations (e.g., 1 + (3/3) = ?), which were to be verified before a response deadline. Deadlines were customized on the basis of the participant's latencies (*M* + 2.5 *SD*) on 15 math-only practice items. A participant's OSPAN score equaled the total number of letters recalled in correct sequence across the 15 trials (Unsworth, Heitz, Schrock, & Engle, 2005a).

Statistical analyses. TUT was analyzed using a general linear model with a within-participants categorical factor of perceptual load, WMC (a continuous quantitative factor of mean-centered OSPAN^2), and their interaction. When necessary, we included the following covariates in analyses of the low-load condition: response competition (RC) from distractors (i.e., the percentage increase in RT in incongruent relative to congruent trials, as in Forster & Lavie, 2009) in trials responded to correctly, error rate, and reaction time (RT) in trials responded to correctly.

Results

To test the theory that WM resources are necessary for mind wandering, we examined whether higher WMC predicted more TUT during low-perceptual-load visual search, a context permissive to mind wandering (Forster & Lavie, 2009a). A true absence of a positive correlation between WMC and TUT would contradict the theory, but in fact, higher-WMC individuals reported greater TUT during low load, $r(72) = .28$, $p = .01$ (see Fig. 1). This pattern did not result from a general bias of higher-WMC individuals to report greater TUT. TUT did not depend on WMC during high-perceptual-load blocks, $r(72) = -.03$, $p = .83$, a result consistent with the significant Load × WMC interaction, $F(1, 72) = 9.3$, $p < .01$, η 2 = .11, and with theory that high load induces low TUT regardless of an individual's WM resources by exhausting limited perceptual capacity in task-relevant processing (Forster & Lavie, 2007, 2009a; Lavie et al., 2004a; Lavie, 2005a). In sum, participants with more WM resources reported more mind wandering in an undemanding context.

Although we propose that higher-WMC participants mind-wandered more because they had more WM resources to mind-wander with, an alternative explanation is that higher- WMC participants found the low-load task easier. Indeed, task ease is known to increase TUT (McKiernan, D'Angelo, Kaufman, & Binder, 2006a), and under certain conditions, WMC may facilitate visual search performance (Kane, Poole, Tuholski, & Engle, 2006; Poole & Kane, 2009; Sobel, Gerrie, Poole, & Kane, 2007). To explore this alternative explanation, we conducted planned comparisons of the correlation between WMC and performance in the low-load condition. These analyses revealed that WMC was not correlated with either error rate or RT, *p*s > .1, but higher WMC was associated with less RC due to distractors, $r(72) = .29$, $p = .01$. None of these performance measures correlated with TUT, however, *p*s > .2. Nonetheless, we reanalyzed the correlation between WMC and TUT while controlling for the three performance measures. Higher WMC still significantly predicted greater TUT, semipartial $r(69) = .25$, $p = .04$. This analysis suggests that our finding that higher-WMC participants mind-wandered more was not simply due to differences in task difficulty.

Experiment 2

In an attempt to replicate the positive association between WMC and TUT and to further rule out task difficulty as an explanation of our findings, we designed a breath-awareness task that placed minimal demands on WM and produced no detectable WMC-related performance differences. We then tested whether participants with greater WMC mind-wandered more while performing this task.

Method

Forty-five members of the University of Wisconsin–Madison community received $10 per hour to complete this experiment, which consisted of a 6-min resting baseline, a 20-min breath-counting task, a 9-min breath-awareness task, questionnaires, and the OSPAN. Only breath-awareness and OSPAN data are discussed here.

As is standard, participants were excluded for scoring below 85% on OSPAN's secondary math task ($n = 3$). This left 42 participants (17 males, 25 females; age range: 18–65 years, $M = 26.5$, $SD = 10$), with OSPAN scores from 16 through 75 ($M = 57.7$, $SD = 13.4$). OSPAN scores were squared to yield a more normal distribution (skew = −0.28, kurtosis = −0.81).

For the breath-awareness task, participants were instructed, "Be aware . . . of the movement of breath . . . breathe normally . . . [with] each exhale, press the letter *L*." Participants were also instructed to catch themselves mind wandering: "If you suddenly realize that your attention was completely off task, that's ok. Press the CONTROL button, and gently bring the attention back to your breath." For each participant, we calculated the self-caught TUT score as the number of times the participant pressed the control button.

Approximately every 90 s (range = 60–120 s), two probes appeared in succession: "Just now where was your attention?" and "How aware were you of where your attention was?" Only data for the first probe are reported here. Participants responded to this probe using a 6-point Likert scale ranging from *completely on task* to *completely off task*. The probe- caught TUT score equaled the percentage of these six probes that a participant rated 4 or higher. The instructions included the same examples of TUT that were given to participants in Experiment 1.

A subset of participants wore a respiration belt (Model MP150CE, BIOPAC, Goleta, CA), depending on its availability, as it was shared by multiple research studies.

Results

Compliance with the motor instructions was confirmed in the subset of participants who wore the respiration belt. Their mean key-press rate tracked their mean breath rate, $r(9) = .99$, $p < .01$. Across the entire sample, the mean key-press rate did not depend on WMC, $r(40) = .02$, $p = .91$.

Results were in line with the theory that WM resources are necessary for mind wandering: Higher WMC predicted more probe-caught TUT during breath awareness, $r(40) = .33$, $p = .03$ (see Fig. 2). Self-caught TUT did not correlate with WMC, $r(40) = -.05$, $p = .76$.

Discussion

This study establishes that TUT increases with increasing WMC when tasks make few demands on WM resources. These findings support the view that WM enables TUT to persist in situations permissive to mind wandering (Smallwood & Schooler, 2006a; Teasdale et al., 1993a). Conversely, these findings challenge claims that the sole influence of WM on TUT is inhibitory, restricting attention to the task at hand (McVay & Kane, 2009a, 2010). Such a theory cannot easily explain why participants with more WM resources, though better at restricting attention, nonetheless mind-wandered more during the low-perceptual-load and breath-awareness tasks.

In light of existing data, we propose that WM enables a context-dependent moderation of TUT. In contexts that place few demands on WM and in which restricting attention to the task at hand is not prioritized, WM resources are free to maintain personal priorities and facilitate TUT. However, in WM-demanding contexts, if restricting attention to the task at hand is prioritized, WM resources can help maintain the goal to stay on task and inhibit TUT. Such a dual role of WM resources could explain both the positive correlation between WMC and TUT observed in the current study and the negative correlation between WMC and TUT observed in McVay and Kane's (2009) study of the SART. We propose, though, that even in the SART, when TUT does occur, WM resources are required to help it persist. This view is consistent with findings of increased activity in brain areas associated with WM during mind wandering in the SART (Christoff et al., 2009; Stawarczyk, Majerus, Maquet, et al., 2011).

The context-dependent effect of WM on mind wandering is likely to generalize beyond the laboratory. Experience sampling in daily life indicates that higher-WMC participants mind-wander more than low-WMC participants on tasks on which they report not trying to concentrate, but mind-wander less than low-WMC participants when they are concentrating (Kane et al., 2007b). As our data provided concurrent measurement of TUT and behavior, we were able to verify that the positive WMC-TUT association was not simply a side effect of WMC-related differences in task performance, a finding with potential ecological validity given that TUT in the laboratory can predict TUT in life (McVay, Kane, & Kwapil, 2009).

Our data suggest that in circumstances conducive to mind wandering, WM resources can help maintain TUT. An unanswered question is how they do this. We suggest that if WM resources are maintaining personal priorities, then they will prime the elaboration of TUT fragments into coherent trains of TUT on the basis of those priorities. Supporting this view, research has shown that TUT forms more connected sequences when a context makes WM resources available than when WM resources are consumed by a task (Teasdale et al., 1993a). TUT also becomes more future oriented in contexts that place minimal demands on WM (Smallwood et al., 2009), especially for individuals with greater WMC (Baird et al., 2011)—results in line with findings that future-oriented TUT increases when personal priorities are primed (Stawarczyk, Majerus, Maj, et al., 2011).

However, free WM resources are not obligated to support TUT. As in the high-load condition of Experiment 1, task- relevant perceptual processing may cut off TUT—presumably at early perceptual processing stages (Lavie, 2005a)—rendering it unavailable for elaboration by WM. Or task-unrelated personal priorities may be recognized as unhelpful and released from WM maintenance. Training in these and other strategies holds promise for reducing mind wandering associated with unhappiness (Killingsworth & Gilbert, 2010).

The relationship between WM and TUT merits more research. Given that half of daily life is typically spent mind wandering, opportunity abounds for exploring the way in which WM shapes and perpetuates people's internal worlds.

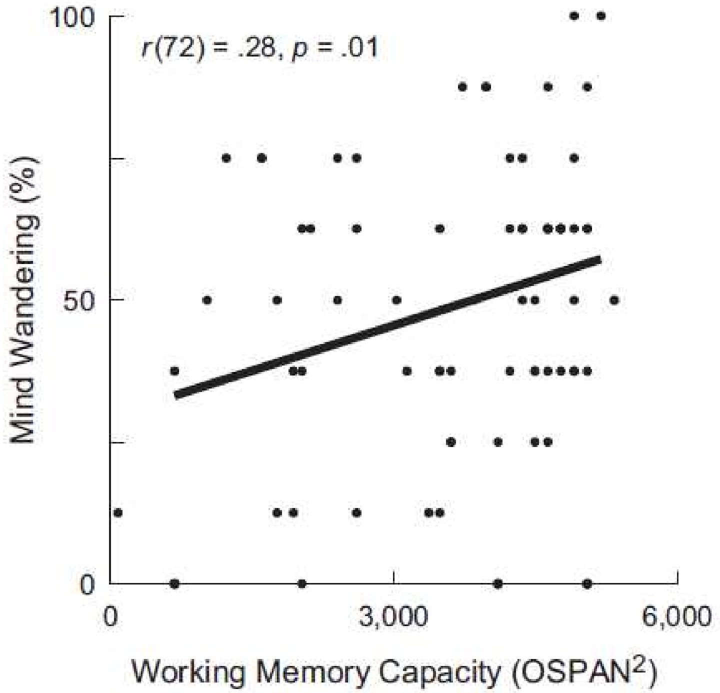

Fig. 1. Results from Experiment 1: scatter plot (with best-fitting regression line) showing the relation between working memory capacity and mind wandering during a low-perceptual-load visual search task that placed minimal demands on working memory. Mind wandering was indexed as the percentage of task blocks that ended with participants engaged in task-unrelated thought. Working memory capacity was indexed by how many letters participants could remember in sequence while simultaneously solving math problems in the Automated Operation Span (OSPAN) task.

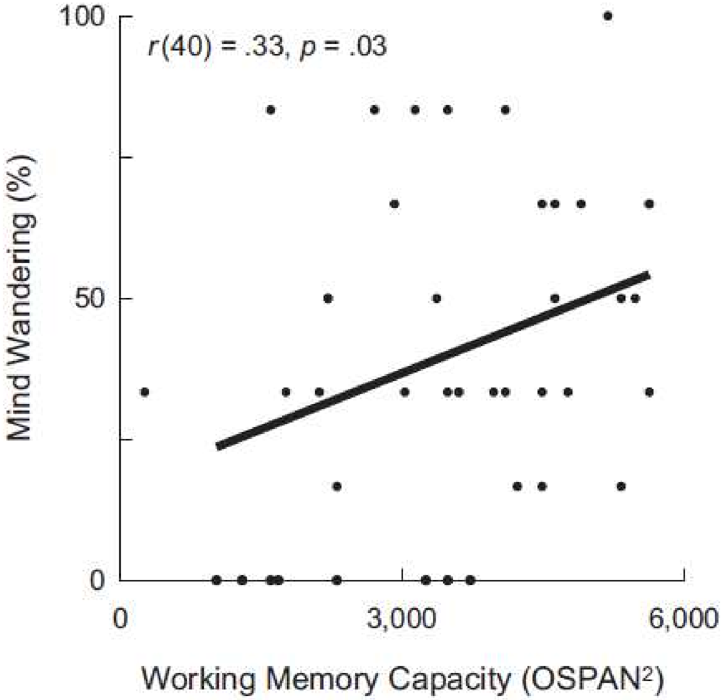

Fig. 2. Results from Experiment 2: scatter plot (with best-fitting regression line) showing the relation between working memory capacity and mind wandering during a breath-awareness task that placed minimal demands on working memory. Mind wandering was indexed as the percentage of probes to which participants responded that their thoughts were off task. Working memory capacity was indexed by how many letters participants could remember in sequence while simultaneously solving math problems in the Automated Operation Span (OSPAN) task.

Chapter 3

Measuring Mindfulness Behaviorally to Assess its Role in Mind Wandering

In 1890, founder of American Psychology William James wrote, “the faculty of voluntarily bringing back a wandering attention over and over again is the very root of judgment, character, and will. … An education which should improve this faculty would be *the* education *par excellence*” (James, 1890). In the 1960s and more recently, others have productively followed James’s interest in wandering attention – under the overlapping terms of mind wandering, task-unrelated-thought (TUT), and stimulus-independent thought – to document that it occurs 30-50% of daily life (Killingsworth & Gilbert, 2010; Klinger & Cox, 1987), and is associated with cognitive task errors (Antrobus, 1968) and worse mood (Killingsworth & Gilbert, 2010).

In contrast, research on the education of voluntarily bringing back a wandering mind has evoked both promise and controversy. Regarding its promise, the practice of returning attention to the present, which is core to mindfulness, has been associated with reduced pain (Zeidan et al., 2011), improved attention (Brefczynski-Lewis, Lutz, Schaefer, Levinson, & Davidson, 2007), and enhanced well-being (Brown & Ryan, 2003) among other benefits (Hölzel et al., 2011).

Nonetheless, mindfulness measurements are controversial. For example, self-reported mindfulness on the Mindful Attention Awareness Scale is not always increased by mindfulness training (Brown & Ryan, 2003). In addition, mindfulness trainings and monetary incentives equally increase certain cognitive test scores, suggesting that the demand characteristics inherent in mindfulness training studies may result in training studies measuring effects of nonspecific factors such as motivation as opposed to, or at least in addition to, mindfulness (Jensen, Vangkilde, Frokjaer, & Hasselbalch, 2012). Therefore, it is unclear the extent to which mindfulness per se is captured by self-report or responsible for improvements following putative mindfulness trainings.

It is therefore critical for the field to establish a behavioral and thus less biased measure of mindfulness. Unlike questionnaires, which suffer from retrospective distortions and susceptibility to implicit demand characteristics (e.g. pressure on meditators to report being mindful) behavioral measures prevent "faking good" as ability must be demonstrated instead of simply averred. A behavioral measure could also avoid the confounding, nonspecific training effects introduced in mindfulness training studies and provide a more efficient assessment. However, to our knowledge, no behavioral measure of mindfulness exists for scientific use. To address this gap, we present the first validation of such a measure.

Defining and Operationalizing Mindfulness.

We chose *present moment awareness* as a definition of mindfulness to operationalize. Grounded in traditional descriptions of mindfulness (*SI Introduction*), it is a commonality in the diversity of modern scientific definitions (e.g. (Baer, Smith, Hopkins, Krietemeyer, & Toney, 2006; Bishop et al., 2004; Brown & Ryan, 2003; Schooler et al., 2011)) and meditation styles, which variably emphasize nonattachment, nonjudgment, or other facets as well.

Mindfulness of breathing can be indexed by breath counting, which lends itself to objective behavioral study and draws face validity from its longstanding use in mindfulness practice (recorded c. 430 AD, (Buddhaghoṣa, 2010)). Prima facie, accurately counting breaths operationalizes mindfulness because it depends on 1) directly perceiving the experience of breathing in the present and 2) awareness that experience (such as mind wandering) is happening, which enables a return of attention to the breath whenever attention drifts. Therefore, although counting is not necessary for mindfulness, we propose mindfulness contributes to accurate breath counting.

Evaluating the Construct Validity of Breath Counting as an Index of Mindfulness

To test the proposition that breath counting measures mindfulness, we followed the recommendations of Cronbach and Meehl (Cronbach & Meehl, 1955) for establishing such construct validity. We reasoned that if breath counting measures mindfulness, then those skilled in breath counting should exhibit all the theorized consequences of mindfulness, including more meta-awareness, less mind wandering, better mood, and greater nonattachment. The theory behind each of these links in mindfulness's nomological network is briefly reviewed.

Evaluating Convergent Validity

Mindfulness is not the absence of stimulus-independent thought. Rather, both can coexist according to traditional mindfulness styles with instructions to be aware of the present moment experience of stimulus-independent thoughts arising and passing: "[one skilled in mindfulness] knows a distracted mind to be 'distracted'... a lustful mind to be 'lustful'... an angry mind to be 'angry'" (Anālayo, 2003). Mindfulness, then, should associate with greater meta-awareness (Fox et al., 2012), particularly of mind wandering and emotions, where meta-awareness is defined as the explicit recognition of the current contents of consciousness (Schooler et al., 2011). Therefore, we assessed the convergent validity of breath counting with meta-awareness in Study 1.

Although mindfulness is the presence of present moment awareness rather than the absence of stimulus-independent thought, in certain contexts mindfulness should result in lessened stimulus-independent thought. For example, when one intends to fully attend to an activity involving minimal discursive thought – e.g. mindfulness of breathing – then meta-awareness of task-unrelated thoughts and their causes (e.g. certain emotions) may lead to their decrease (see *Discussion*; (Schooler et al., 2011)). In a similar fashion, mindfulness should likewise attenuate task-unrelated thoughts that purportedly lower mood (>50% of mind wandering, (Killingsworth & Gilbert, 2010)) if they are understood as unnecessary. In support of these theories, previous research has shown self-reported mindfulness is inversely correlated with mind wandering as indexed by the Sustained Attention to Response Task (SART; (Allan Cheyne, Solman, Carriere, & Smilek, 2009c)) and positively correlated with well-being (Brown & Ryan, 2003). Therefore, if breath counting accuracy measures mindfulness it should associate with less mind wandering during breath counting and overall, as well as with better mood. We assessed breath counting's convergent validity with mood in Study 2, and with mind wandering in all studies.

Just as the increased meta-awareness of mindfulness may help lessen stimulus-independent thought, it may also attenuate the influence of certain emotions (Creswell, Way, Eisenberger, & Lieberman, 2007), such as wanting (Berridge, Robinson, & Aldridge, 2009). Indeed, awareness decreases the power of erotica to capture attention (Jiang, Costello, Fang, Huang, & He, 2006) and lessens the emotion-induced influences of weather on life satisfaction (Schwarz & Clore, 1983). Therefore, accurate breath counting should associate with nonattachment as demonstrated by a decreased influence of wanting. This prediction is in line with nonattachment's positive association with mindfulness in traditional theory (Anālayo, 2003) and self-report research (Sahdra, Shaver, & Brown, 2010). We tested the convergent validity of breath counting with nonattachment in Study 3.

Evaluating Discriminant Validity

In parallel to its convergent validity, we assessed breath counting for discriminant validity by examining its empirical alignment with the theoretical distinctions between mindfulness and established attention constructs such as sustained attention and working memory capacity. Mindfulness practice emphasizes the direct perception of present moment experience, which is a continuously present and changing process (e.g. the felt experience of breathing). In contrast, sustained attention tasks such as the SART emphasize the conceptual detection of infrequent and discrete target content (e.g. detecting a “3” present < 5% of total task time). In further contrast, working memory tasks such as the automated operation span task (OSPAN) emphasize the priority-driven maintenance and manipulation of information not present in the current environment (e.g. a string of letters). While each of the three tasks measure an attentional trait by assessing how well a person can maintain a certain attentional set (e.g. holding 7 letters in memory while doing math), breath counting should not be highly correlated with the OSPAN or SART, a prediction we assessed in Studies 1 and 2, respectively. Although the SART as an index of mind wandering would ideally be somewhat inversely correlated with breath counting, breath counting’s predicted correlates (e.g. history of meditation practice) should nonetheless remain significant correlates after controlling for the SART, a claim we tested in Study 3. Furthermore, although breath counting ability should be stable over time in the absence of intervention (assessed in Study 2), it should be selectively increased by a mindfulness intervention but unchanged by an intervention aimed to increase working memory (assessed in Study 4).

Evaluating Criterion and Incremental Validity

Following Cronbach and Meehl, we also assessed breath counting's criterion validity. As they noted, two indices that measure a similar construct should correlate. Therefore, we evaluated whether individuals reporting greater mindfulness on existing mindfulness questionnaires counted breaths more accurately as well (Study 1). We additionally tested for expected group differences by assessing whether long-term meditators counted breaths more accurately than controls (Study 3). Finally, we assessed breath counting's incremental validity relative to extant criteria by testing whether breath counting could explain individual differences in meta-awareness, mind wandering, and nonattachment beyond what could be explained by mindfulness surveys (Studies 1 and 3).

Results

Study 1

In Study 1 we explored the convergent, discriminant, criterion, and incremental validity of breath counting by assessing its correlation with meta-awareness, mind wandering, working memory, and trait mindfulness. We instructed 120 participants to "be aware… of the movement of breath" and count their breaths from one to nine repeatedly. With breaths 1-8 they pressed one button, and on breath 9 they pressed another, measuring counting accuracy. Every ~90 sec (60-120 sec range) experience sampling probed state mind wandering and meta-awareness, respectively, with two 6-point Likert scales, "just now where was your attention? [completely on-task / off-task]" and "how aware were you of where your attention was? [completely aware / unaware]." Participants were then probed for their count.

Accurate breath tracking was physiologically confirmed in a subset of 52 participants, with mean keypress rate tracking mean breath rate, $r = .99$. In addition, in the total sample mean keypress rate did not explain counting performance, $r = -.04$, $P = .67$, which showed an average error rate of 22% (*SD* 15%) with a mean of 29% of errors being self-caught.

Guided by theory that those with greater mindfulness experience greater meta-awareness, total task counting accuracy and state meta-awareness during breath counting were correlated across participants. In line with theory, skill in breath counting associated with greater meta-awareness, $r = .42$, $P < .001$ (Fig. 1*A*). Breath counting accuracy also associated with less state mind wandering across participants, $r = -.38$, $P < .001$, as predicted for a valid measure of mindfulness.

To examine these relationships at a finer timescale *within participants*, we investigated whether increased meta-awareness and diminished mind wandering were occurring in the very moments when mindfulness was present. We compared average meta-awareness ratings from correct vs. incorrect count probes within participants and found that moments of accurate counting (vs. miscounting) associated with increased meta-awareness, $t_{(101)} = 2.51$, $P = .01$. Mind wandering also decreased during moments of mindfulness indexed by accurate counting, $t_{(101)} = 4.02$, $P < .001$ (Fig. 1*B*). To confirm findings were not due to probe order, we replicated them in a separate block of 44 participants collected part way through Study 1 who received their count probes preceding TUT probes in an otherwise identical task (*SI Results*; Fig 1*A* and 1*B* insets).

When we changed the probe order, we also expanded our experiment battery to end with collecting from participants ($n = 93$) a measure of working memory, the OSPAN (described in (Unsworth, Heitz, Schrock, & Engle, 2005b); *SI Methods*), and two questionnaire measures of trait mindfulness, the Mindful Attention and Awareness Scale (MAAS; (Brown & Ryan, 2003); Table S1) and the Five Factors of Mindfulness Questionnaire (FFMQ; (Baer et al., 2006); Table S1). Supporting discriminant validity, we found breath counting accuracy uncorrelated with working memory capacity as measured by OSPAN, $r = .04$, $P = .71$. Supporting criterion validity, we found breath counting accuracy positively correlated with trait mindfulness as reported on the MAAS $r = .20$, $P = .05$ and FFMQ $r = .21$, $P = .05$. Regarding incremental validity, when the MAAS and FFMQ were entered with breath counting into a simultaneous regression for explaining state meta-awareness, counting accuracy still significantly and uniquely explained variance in meta-awareness, $rs = .45$, $P < .001$. The same was true for mind wandering, $rs = .46$, $P < .001$.

Study 2

Study 2 investigated breath counting's convergent validity with mood and overall mind wandering, its discriminant validity relative to sustained attention, and its test-retest reliability, as measures of attentional traits are expected to be stable over time. A new sample of 137 participants completed the state Positive and Negative Affect Scale (PANAS; (Watson, Clark, & Tellegen, 1988); 7 PANAS scores lost to technical malfunction) followed in counterbalanced order by the go/no-go SART ((Robertson, Manly, Andrade, Baddeley, & Yiend, 1997); *SI Methods*; Fig. S1) and a breath counting task. Trait mind wandering scores from the Imaginal Process Inventory (IPI; (Singer & Antrobus, 1972); Table S1) were available in a pre-existing survey database for 85 of them. Of those participants who performed breath counting as their first task, 54 did an identical breath counting task one week later to assess test-retest reliability.

Accurate breath tracking was again physiologically confirmed in a subset of 69 participants, where mean keypress rate tracked mean breath rate, r =.99. Average error rate was 16% (*SD* 15%) with a mean of 35% of errors being self-caught. Unlike in Study 1, in Study 2 those with slower keypress rates miscounted more, $r = -.19$, $P = .03$. All findings below remain significant when controlling for keypress rate unless otherwise stated.

Consistent with mindfulness theory, breath counting accuracy associated with better mood, where mood was indexed by a composite of negative minus positive affect, $r = -.22$, $P = .01$ (Fig. 2*A*). When positive and negative affect were simultaneously regressed on breath counting accuracy, accuracy independently correlated with both more positive affect, $rs = .17$, $P = .05$, and less negative affect, $rs = -.17$, $P = .05$. After controlling for keypress rate, however, the correlation with positive affect became non-significant, $rs = .15$, $P = .07$.

Further in line with mindfulness theory, breath counting accuracy associated with less overall mind wandering. This was true regardless of whether mind wandering was measured with the IPI, $r = -.27$, $P = .01$, or SART indices validated as indirect measures of mind wandering (Allan Cheyne et al., 2009c), namely errors of commission, $r = -.19$, $P = .03$, and RT variability, $r = -.32$, $P < .001$. Importantly, SART indices were far from perfectly correlated with breath counting, supporting its discriminant validity.

Breath counting demonstrated 1 week test-retest reliability of $ICC = .60$ (Fig. 3*A*; *SI Results*).

Study 3

Mindfulness is thought to associate with nonattachment, typified in part by a decreased influence of wanting. Wanting, defined as an incentive motivation to approach, can be irrationally incongruent with cognitive goals, e.g. as occurs in addiction (Berridge et al., 2009). When approach contradicts cognitive goals and is unhelpful, it indexes wanting with particular clarity. Therefore, to measure the influence of wanting we assessed how much individuals were slowed by attending to a distractor formerly paired with reward despite their cognitive goal of completing a visual search as quickly as possible. This validated measure of attention capture (B. A. Anderson, Laurent, & Yantis, 2011) parallels the paradigm of operant extinction used to measure wanting in animals (e.g. (Wyvell & Berridge, 2000)), and so we predicted it would correlate inversely with breath counting accuracy – supporting convergent validity – and do so beyond what could be explained by self-reported mindfulness –supporting incremental validity.

As described in detail elsewhere ((B. A. Anderson et al., 2011); *SI Methods* and Fig. S2), for the training portion of the attention capture task participants were monetarily rewarded when they accurately identified targets highlighted by colors. Later, during the testing portion, participants were told to ignore color as irrelevant and no rewards were given. Targets were instead highlighted by distinct shapes among distractors, and participants identified targets by keypress “as quickly as possible while minimizing errors.” On half of the trials one distractor was a color previously associated with reward. Attention capture scores were calculated by subtracting the average RT in trials with reward-associated color distraction from the average RT in trials without such distraction (as in (B. A. Anderson, Laurent, & Yantis, 2013)).

Replicating previous research, the presence of a formerly rewarded distractor successfully captured attention as demonstrated by significantly slower RTs on trials with stimuli previously paired with reward (vs. not), $t_{(38)} = 2.99$, $P < .01$. To assess breath counting’s convergent validity with nonattachment as exemplified by a decreased influence of wanting, we correlated counting accuracy with individual differences in the extent of attentional capture. We found that greater accuracy was associated with less capture, $r = -.31$, $P = .05$ (Fig. 2*B*), suggesting that breath counting ability is related to a reduced influence of wanting, as expected for a measure of mindfulness. In addition, in support of its incremental validity, breath counting accuracy remained a significant predictor of attention capture when entered in a simultaneous regression with the FFMQ, $rs = .38$, $P = .02$.

The participants we recruited for Study 3 were both long-term and novice meditators, as this population allowed us to simultaneously address a second aim of evaluating expected group differences in breath counting. We also took the opportunity to more deeply probe the discriminant validity of breath counting by evaluating whether its predicted covariation with meditation history could be explained merely by individual differences in sustained attention. We found that long-term meditators, purportedly skilled in mindfulness, displayed greater counting accuracy, $t_{(36)} = 2.23$, $P = .03$ (Fig. 3*B*), and less mind wandering, $t_{(36)} = 2.11$, P

= .04, than age-matched novice meditators. Importantly, the group difference in counting accuracy remained significant after controlling for SART commission errors, $t_{(35)} = 2.01$, $P = .05$, suggesting that breath counting measures skill in mindfulness beyond that accounted for by sustained attention.

Study 4

Study 4 further tested breath counting for discriminant validity by assessing its selective sensitivity to mindfulness training interventions. We reasoned that if breath counting measures mindfulness, than an individual's counting accuracy should increase following training in mindfulness but not training in working memory, a construct found uncorrelated with breath counting accuracy in Study 1.

We drew training methodology from a growing literature that mental capacities such as working memory can be improved with practice, as evidenced by neural plasticity and better performance on working memory measures following repeated practice of working memory tasks such as the spatial n-back task ((Jaeggi, Buschkuehl, Jonides, & Perrig, 2008b; McNab et al., 2009b; Davidson & McEwen, 2012) but see (Redick et al., 2013)). In the same way that working memory may be improved by repeated practice of working memory tasks, mindfulness may be improved by repeated practice of breath counting if it is indeed a mindfulness task. Therefore, in a randomized controlled trial we tested whether breath counting training – but not n-back training or no training – could increase counting accuracy and self-reported mindfulness as well as decrease mind wandering.

Participants were randomized into 3 training groups, a breath counting training, a spatial n-back training control, and a no-training control. Attrition rates were 27%, 33%, and 15%, making final sample sizes of 22, 20, and 29, respectively (see *SI Methods* and Fig. S3 and S4 for training protocols and retention details at each study phase; online breath counting training can be viewed at webtasks.keck.waisman.wisc.edu/b/demo). For 4 weeks, breath counting and n-back trainees completed 2 25 min trainings each weekday which ended with a mind wandering thought probe “just now where was your attention? [completely on-task / off-task].” When comparing the last 2 weeks to the first 2 weeks of training, both active training groups improved in training performance (Fig. S6*A*), but only breath counting participants decreased in mind wandering, group X time interaction $F_{(2, 40)} = 7.02$, $P = .01$, simple main effect of time for breath counting participants $F_{(1, 40)} = 25.18$, $P < .001$, simple main effect of time for n-back participants $F_{(1, 40)} = 1.26$, $P = .27$ (Fig. S6*B* and *SI Methods*), as expected for a mindfulness training.

Before and after the 4 week training period, all 3 groups completed testing including the FFMQ, a breath counting task with mind wandering probes, and a verbal 3-back task. In line with the hypothesis that repeated breath counting trains mindfulness, we found a group X time interaction in FFMQ scores, $F_{(2, 68)} = 4.83$, $P = .01$, such that although the two control groups did not significantly differ from each other in their pre - post change in trait mindfulness, $F_{(1, 68)} = 0.02$, $P = .88$, the breath counting group increased in trait mindfulness relative to the two control groups, $F_{(1, 68)} = 9.63$, $P < .01$.

As evidence that breath counting accuracy measures mindfulness, we also found a group X time interaction trend in counting accuracy, $F_{(2, 68)} = 2.97$, $P = .06$, and an interaction in mind wandering, $F_{(2, 68)} = 5.09$, $P < .01$, during the breath counting task. Specifically, planned comparisons revealed that the two control groups did not significantly differ from each other in their pre - post change in counting accuracy, $F_{(1, 68)} = 0.24$, $P = .63$ (Fig. 3*C*), or mind wandering, $F_{(1, 68)} = 3.75$, $P = .06$. However, the breath counting group demonstrated decreased mind wandering, $F_{(1, 68)} = 5.40$, $P = .02$, and improved counting accuracy, $F_{(1, 68)} = 5.89$, $P = .02$, relative to the two control groups (Fig. 3*C*). Moreover, within the breath counting group, those who increased most in counting accuracy as a result of training were also the ones who increased most in FFMQ trait mindfulness, $r = .44$, $P = .04$.

Discussion

While self-report measures have provided a helpful beginning for assessing mindfulness, to date a behavioral measure immune to biases inherent in self-report is still lacking. Here we validate breath counting as a behavioral measure of mindfulness. We found that breath counting accuracy tracked with naturally occurring variations in self-reported mindfulness, distinguished well-practiced meditators from novices, and increased following a mindfulness training.

Convergent Validity: Evidencing Both Theory and Methods

We also provided the first evidence that skill in mindfulness rigorously measured through behavior is related to more meta-awareness, less mind wandering, better mood, and greater nonattachment, in line with theoretical claims that underlie explanations of mindfulness's educational and health benefits. Our novel assessment of mindfulness's relation to nonattachment using attention capture especially highlights nonattachment as a mechanism by which mindfulness may ease addiction (Bowen et al., 2014), a disorder in which reward-associated attention capture is elevated (B. A. Anderson, Faulkner, Rilee, Yantis, & Marvel, 2013). Such convergence of breath counting and mindfulness theory helps substantiate both per Cronbach and Meehl: “we do not first ‘prove’ the theory, and then validate the test, nor conversely. ... Actually the evidence is significant for all parts” (Cronbach & Meehl, 1955).

One hypothesis to be explored for the convergence of mindfulness with these constructs is awareness-dependent learning and memory. As a specific example, present moment awareness, which would support meta-awareness of task-unrelated worry and the non-occurrence of a worrisome event, may form a memory that the worry was unnecessary. In the future, awareness of worry may retrieve that memory, reducing the priority of worrying and thus the working memory resources maintaining it (Levinson, Smallwood, & Davidson, 2012b). Reducing such mind wandering would reduce the negative emotions it triggers (Killingsworth & Gilbert, 2010), improving mood. And with fewer negative emotions to fuel it (Horowitz & Becker, 1973), mind wandering would reduce further. The same would apply to wanting, resulting in nonattachment.

Discriminant Validity: Clarifying Mindfulness

To establish the validity of a new construct, it must be distinguished from existing constructs. Our data suggest that mindfulness as indexed by breath counting is not reducible to mind wandering’s absence, working memory, or sustained attention, as is evident from the variance in breath counting accuracy unexplained by these measures. How then is mindfulness unique?

Mindfulness encourages awareness that task-unrelated thoughts are happening as present moment experiences. As a result, mindfulness and task-unrelated thoughts may coexist. At the same time, since mindfulness encourages direct perception of present experience, we suggest mindfulness may simultaneously reduce task-unrelated thought as a natural byproduct of more fully saturating perceptual resources (*SI Discussion*; (Forster & Lavie, 2009b; Levinson et al., 2012b)). This perspective can account for the inverse relation we found between counting accuracy and mind wandering without defining mindfulness by the absence of task-unrelated thought.

Such reduction of mind wandering is putatively independent of working memory (Forster & Lavie, 2009b; Lavie, Hirst, de Fockert, & Viding, 2004b), further distinguishing mindfulness from working memory tasks and the SART which depend on working memory to block task-unrelated thoughts from awareness (McVay & Kane, 2009b). Therefore, this perspective can also explain why mindfulness and working memory capacity are uncorrelated (*SI Discussion*), why n-back training did not improve breath counting accuracy, and why breath counting significantly differentiated long-term meditators and novices even after controlling for individual differences in sustained attention indexed by SART errors. Mindfulness also differs from sustained attention in that it changes one's relationship with emotions, as only breath counting accuracy (not SART errors, $r = .11$, $P = .51$) predicted less reward-associated distraction.

Criterion and Incremental Validity

Cronbach and Meehl (Cronbach & Meehl, 1955) observed that construct validity evolves by bootstrapping, wherein a new test is initially validated with existing imperfect tests (e.g. self-report) yet may be ultimately judged to have greater construct validity. For example, the thermometer received initial validation from self-reports of felt temperature, but ultimately outperformed self-reported temperature in predicting the pressure of a heated gas. We too validated breath counting using existing methods – mindfulness training and self report – and so it is important to discuss how we navigated their limitations and how breath counting compares with them in predicting the theoretical correlates of mindfulness.

In theory, training effects result from increasing a targeted quality (e.g. mindfulness). Yet in practice training effects can result from untargeted, nonspecific factors such as trainees' group interactions or motivation, as has been found in group mindfulness trainings (e.g. (Jensen et al., 2012)). Our methods minimized such factors. Evidencing this, we found verbal 3-back performance improved most following n-back training but did not differ following breath counting vs. no training (*SI Results*), suggesting the effects of our mindfulness training were not simply due to non-specific factors such as motivation that should have improved performance non-selectively on any task, including the verbal 3-back.

As mentioned, self-report is vulnerable to confounds such as retrospective bias and demand characteristics. We protected against spurious correlations between self-report and breath counting via replication with bias-resistant methods, as illustrated in our mind wandering data. For example, to decrease retrospective bias, participants reported on mind wandering occurring in the moment instead of the past using experience sampling methods that demonstrate convergent validity with neural measures (Christoff et al., 2009; Smallwood, Beach, Schooler, & Handy, 2008b). To decrease demand characteristics, we collected mind wandering reports with the IPI weeks before participants realized they might be in a breath counting experiment and experience any demands. And to sidestep self-report biases altogether, we administered the SART which has been validated as an indirect measure of mind wandering (Smallwood, Beach, et al., 2008b). In all cases, even after self-report bias was reduced, the association between breath counting and mind wandering replicated.

As construct validity progresses, one expects newer measures to display variance that can better predict the theoretical correlates of the construct (*SI Discussion*). In line with this view, using multiple regression we found that breath counting, over and beyond the FFMQ, predicted nonattachment as indexed by decreased attentional capture by reward-associated distractions. Breath counting also significantly explained an individual's meta-awareness and mind wandering beyond what was possible with the MAAS and FFMQ alone. These data demonstrate breath counting's incremental validity over existing measures for inferring skill in mindfulness.

Future Directions

The validation of breath counting is a first step in behaviorally measuring mindfulness that opens many avenues for research. As exemplified here, breath counting can now behaviorally evaluate trainings for their impact on mindfulness per se and identify which individual differences accompany mindfulness. It can also start a behavioral investigation on the extent to which mindfulness is a domain-general capacity. To take working memory research as an example, the development of behavioral measures with verbal vs. spatial content has clarified that working memory of words vs. spatial location is similar but distinct. While the difference suggests working memory may partly rely on content-specific abilities, the similarity points to a domain-general working memory capacity used to complete both types of tests (Kane et al., 2004). In the same way, breath counting may depend on both breath-specific factors and domain-general mindfulness. Future research correlating breath counting with behavioral measures of mindfulness of diverse content, including emotion, should elucidate the domain-generality and content-specificity of the structure of mindfulness.

Our initial findings suggest breath counting may be useful not only scientifically as a measurement tool but also clinically as a mindfulness training. As a training that simultaneously measures change in skill, it allows evidence-based tailoring of training on an individual basis. In theory, it could determine the guidance that most improves skill for an individual and insert it in the very moment his or her mindfulness lapses. Since the counting errors signaling these lapses occur with greater frequency than trainees notice on their own, such feedback may increase opportunities to practice voluntarily bringing back a wandering attention, a skill William James recognized as fundamental.

Conclusion

For over a thousand years mindfulness trainees have used breath counting for training in mindfulness. Its present adaptation for scientific purposes enables going forward in a rigorous behavioral investigation of the promise mindfulness shows in education (Diamond & Lee, 2011), physical health (Barrett et al., 2012), and well-being (Brown & Ryan, 2003).

Methods

Study 1

Usable data were collected from 164 participants (62% male; age: mean 22.5, ranging 17-65; 19 excluded: see *SI Methods* for details) for course credit. Participants gave informed consent and the University of Wisconsin-Madison Institutional Review Board approved procedures. Following a 6 min resting baseline, participants counted their breaths for 18 min. If they lost count, participants were instructed to press a button reserved for indicating self-caught miscounting and begin again at one with the next breath.

Experience sampling during breath counting yielded a set of 12 TUT ratings and a set of 12 meta-awareness ratings. Each set was averaged to index state mind wandering and state meta-awareness, respectively. For probe reaction time (RT) analyses, each of the 12 probes were binned as "on-task" (TUT Likert rating of 4-6) or "off-task" (TUT Likert rating of 1-3), and participants without data in both bins (e.g. never "off-task") were excluded from that analysis (n = 42). Counting accuracy was calculated as the number of correct count sets divided by the total number of count sets , i.e. 100% - [# of incorrect ongoing 9-counts + # of incorrect count probe responses + # of self-caught miscounts] / [# of ongoing 9-counts + # of count probe responses + # of self-caught miscounts]. For analyses of correct vs. incorrect count probes, participants without data in both bins (e.g. never off count at probe) were excluded from that analysis (n = 31).

Throughout Study 1, during breath counting a subset of participants (n = 52) wore a respiration belt (Model MP150CE, BIOPAC, Goleta, CA). Mean breath rate was computed as the average time between inhale peaks in the respiration signal.

Study 2

For course credit, a new set of 137 participants with usable data (38% male; age: mean 18.8, ranging 18-26; 11 excluded: see *SI Methods* for details) completed the PANAS and, in counterbalanced order, the SART (*SI Methods*) as well as a 15 min breath counting task without experience sampling. Counting accuracy was calculated as 100% - [# of incorrect ongoing 9-counts + # of self-caught miscounts] / [# of ongoing 9-counts + # of self-caught miscounts]. A subset of participants (*n* = 69) wore a respiration belt. Of those participants who performed breath counting as their first task, 54 with usable data (2 excluded) returned to lab one week later to breath count again. Additionally, we measured 85 participants' trait mind wandering by including the IPI in a larger mass survey they completed weeks before deciding to enroll in Study 2 (see *SI Methods* for items from the IPI and other questionnaires in Studies 1-4).

Study 3

We recruited a group of 14 long-term meditators (57% male, age: mean 53.6, ranging 29-67) from local Buddhist meditation groups and matched them in age to a group of 25 novice meditators (36% male, age: mean 53.7, ranging 29-68). For the purpose of the present study, a long-term meditator was defined as having practiced meditation formally for at least 30 min a day, 5 days a week for the past 3 years, and possessing a total of 750+ lifetime practice hours. Total practice hours in long-term meditators ranged 850-16700 (median 4288).

Participants were paid $10/hr plus in-task earnings to complete an attention capture training (*SI Methods*), the FFMQ, and the SART (1 novice SART lost to experimenter error). Participants then returned 3 weeks later for a final visit in which they completed a refresher attention capture task training followed by a breath counting task similar to that described in Study 1, save that it lasted 30 min, had 10 experience samplings each separated by ~3 min (1-5 min range), and did not include meta-awareness probes. Finally, participants performed an attention capture testing (*SI Methods*).

Study 4

Of the 113 participants recruited by offering $300 for completing an "attention training study," 94 completed a pre-test battery (*SI Methods*), including a verbal 3-back task, an 18 min breath counting task without meta-awareness probes or self-caught miscounting (counting accuracy calculated as 100% - [# of incorrect ongoing 9-counts + # of incorrect count probe responses] / [# of ongoing 9-counts + # of count probe responses]), and an FFMQ modified to query experience "in the last 2 weeks" so that the measure would be sensitive to changes that occurred during the 4 weeks of training. Participants were then randomized to breath counting, spatial n-back, or no training (*SI Methods*; http://webtasks.keck.waisman.wisc.edu/b/demo) and returned 4 weeks later to complete an identical post-test battery.

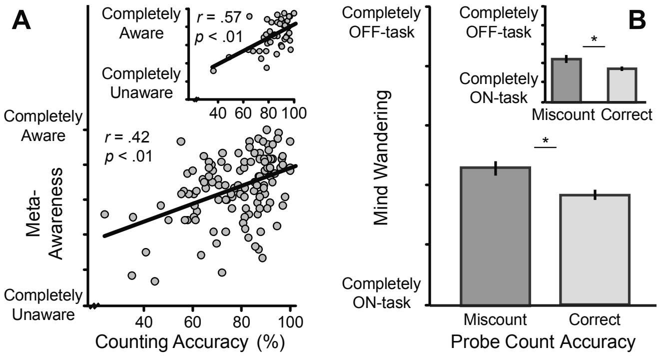

Fig. 1. Cognitive correlates of breath counting, and their replication (*Insets*). (1A) The relation across participants between state meta-awareness and counting accuracy. State meta-awareness was indexed as the average of 12 probe ratings during breath counting on a 6-point Likert scale ranging from “completely aware” to “completely unaware.” Counting accuracy was indexed as the percent of total task count sets correct. (1B) The relation within participants between momentary mind wandering and counting accuracy. During breath counting participants were randomly probed 12 times for their current count and mind wandering status on a 6-point Likert scale ranging from “completely on-task” to “completely off-task.” For each participant, mind wandering scores were averaged separately for moments when on-count vs. off-count, and then entered into group-level “correct” and “miscount” means displayed by bar graph (+/- 1 SE).

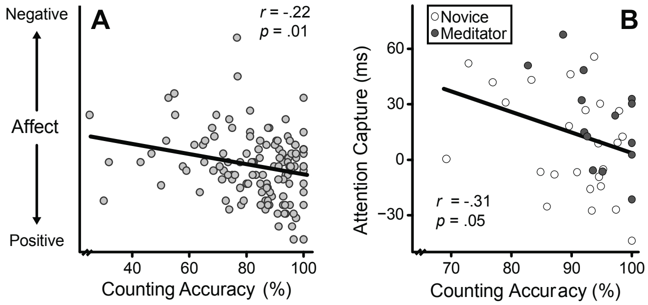

Fig. 2. Affective correlates of breath counting, indexed as the percent of total task count sets correct. (2A) The relation across participants between state affect (negative - positive) from the Positive And Negative Affect Scale and counting accuracy. (2B) The relation across all participants between attention capture (defined as response time when reward-associated distractors were present minus response time when they were absent) and counting accuracy.

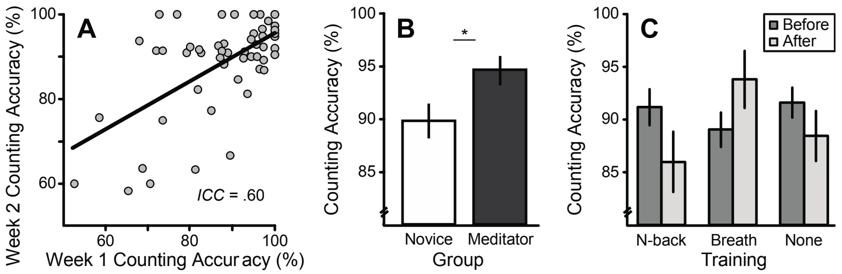

Fig. 3. Reliability and validity of breath counting accuracy, indexed as the percent of total task count sets correct. (3A) 1 week test-retest reliability. (3B) Long-term vs. novice meditators' counting accuracy (+/- 1 SE); the group difference remained significant after controlling for individual differences in sustained attention indexed by SART commission errors. (3C) Counting accuracy for each training group before and after the training period (+/- 1 SE).

Chapter 3 Supplemental Information

Present moment awareness encompasses 1) nondistraction from the present (e.g. not being lost in mind wandering) and 2) awareness that experience is happening in the present (including awareness of the experience of mind wandering). Using present moment awareness as a definition of mindfulness has basis in traditional descriptions of mindfulness. For example, commentary on the *Abhidhammattha Sangaha* from the 12th century defines mindfulness or *sati* as "attentiveness to the present" that 1) is characterized by "not floating away from the object" and 2) is manifest as "confronting an objective field" (of present moment experience) in "gaurdianship" (e.g. from becoming lost in mind wandering) (Bodhi, 1993).

With this definition in mind, we operationalized mindfulness practice in the context of "mindfulness of breathing", a meditation style requiring present moment awareness. Its main instruction mirrors the two aforementioned components of mindfulness: 1) to be aware of the experience of breathing (in the present, as opposed to lost in thought) and 2) to be aware when attention has wandered in order to then return attention to the breath. Having said this, we acknowledge that mindfulness is not specific to a particular experience (e.g. the breath) and that mindfulness practice may importantly include facets (e.g. nonjudgment, nonattachment) that may not be as emphasized in mindfulness of breathing as in other mindfulness styles.

Methods

Study 1

Participants. Of the 19 excluded participants, 11 misunderstood instructions, 5 fell asleep, and there were 3 technical malfunctions.

Operation Span Task. A full description of the task can be found elsewhere (Unsworth et al., 2005b). In brief, the task consisted of 15 trials. For each trial, the display switched three to seven times between letters for memorization and math equations (e.g. 1 + (3/3) = ?) for verification under response deadline. Each participant's latencies for 15 math-only practice questions were used to create individualized response deadlines (*M* + 2.5 *SD*s). A participant's score on the OSPAN was calculated as the sum of the letters recalled in accurate sequence from all 15 trials.

The Cognitive Failures Questionnaire was also administered as an exploratory measure (results listed in Table S2).

Study 2

Participants. Of the 11 excluded participants, 5 misunderstood instructions and 6 fell asleep. Of those participants who performed breath counting as their first task, 2 were excluded for misunderstanding instructions.

Sustained Attention to Response Task. The Sustained Attention to Response Task (SART) (Robertson et al., 1997) is a go/no-go task with single digits (1-9) presented every 1150 ms (digit presented for 250 ms followed by a 900 ms mask of an O with an X through it; Fig. S1). Performance was quantified by errors of commission during the 25 no-go trials and by the RT coefficient of variability (RT CV, defined as *SD* of RT / mean RT) during the 200 go trials. Errors of commission are theorized to index gross mind wandering, and RT CV is theorized to be sensitive to mind wandering of the type that occurs in parallel with successful but automated task performance (Allan Cheyne, Solman, Carriere, & Smilek, 2009a).

The Anger Rumination Scale and Adult ADHD Self Report Scale were also administered as exploratory measures (results listed in Table S2), with 9 and 11 scores lost to technical malfunction, respectively.

Study 3

Attention Capture Task. For attention capture task training and testing (described in detail by (B. A. Anderson et al., 2011); Fig. S2), participants completed trials in which a 400-600 ms fixation was followed by a ring of 6 different colored shapes and then a feedback display. During training, the shapes were all circles, and the target shape was either red or green. Participants indicated by keypress "as quickly as possible while minimizing errors" and with a 1000 ms response deadline whether the target contained a vertical or horizontal line; nontargets contained diagonal lines. Feedback displayed rewards earned for accurately identifying target line orientation on that trial as well as total rewards earned so far. During testing, participants were informed that shape color was now irrelevant to the task and should be ignored. Targets were now either a diamond among 5 distractor circles or a circle among 5 distractor diamonds, and participants indicated by keypress within 1500 ms whether targets contained a vertical or horizontal line. On 25% of the trials, one of the distractors was red, on 25% one was green, and on 50% no previously rewarded color – neither red nor green – was present. No rewards were given at testing; feedback only communicated accuracy from the trial just completed. During training and testing, red and green never appeared on the same trial.

Training consisted of 240 trials at visit 1 and 60 trials at the beginning of visit 2. Participants subsequently received 240 trials of testing at the end of visit 2. The locations and colors of targets and distractors were crossed and counterbalanced. Half of participants were assigned red for their high-reward target, half were assigned green. During training, accurate responses to high-reward targets were rewarded with high-reward feedback (10 cents) on 80% of trials and low-reward feedback (2 cents) on 20% of trials. The reverse was true for low-reward targets. As in past research (Anderson, Laurent, & Yantis, 2013), data were collapsed across high and low reward, and an attention capture score was calculated by subtracting the average RT in testing trials on which a red or green distractor was present from the average RT in testing trials without a red or green distractor. RTs more than 2.5 *SD*s above and below the mean RT for each participant were excluded from calculations.

The Barratt Impulsivity Scale was also administered as an exploratory measure (results listed in Table S2).

Study 4

The study recruited 113 participants with access to internet from the University of Wisconsin-Madison and surrounding community. Email and online advertisements described the duration and compensation ($300) for completing an “attention training study.” Of those who responded to the ads, 94 participants completed pre-testing and were then randomized using a random-number generator into 1 of 3 training groups. A breath counting training was targeted to increase mindfulness with 30 participants. An n–back training was targeted to increase working memory but not mindfulness with 30 participants. For a non-active control, a no-training group simply completed the test battery that all groups completed before and after the 4-week training period with 34 participants. Following attrition, final sample sizes were 22, 20, and 29 for the breath counting training, n-back training, and no-training groups respectively. These sample sizes and attrition rates are typical for attention training studies (e.g. (Harrison et al., 2013)).

On arriving for their first visit, participants completed a testing session including a breath counting task and a verbal 3-back task. Additionally, participants completed questionnaires online from home, including the Five Factors of Mindfulness Questionnaire in which participants were asked to rate themselves based on their experience in the past 2 weeks so that the measure would be sensitive to changes that occurred during the 4 weeks of training.

Participants who completed pre-testing were randomized to the 3 training conditions. Both active training groups trained from their personal computers online for 4 weeks, 5 days/week, for 25 minutes once in the morning and once in the evening each day. Two research assistants enrolled participants, randomized them to groups, and tracked their home practice. The assistants discontinued participation of anyone failing to satisfactorily complete at least 75% of training sessions (5 breath counting trainees, 8 n-back trainees). There were no significant deviations from the trial protocol.

After 4 weeks, all groups returned to lab for a post-testing with identical measures to pre-testing. Participants were not told the research hypotheses. Researchers collecting post-test data were not masked to intervention assignment.

Outcomes of interest administered pre- and post-training were analyzed using an ANOVA with training group (Mindfulness vs. N-back vs. No-training) as a between participant factor and time (Pre vs. Post training) as a within participant factor. Planned contrasts of active training group vs. controls (active control and no-training) on Pre vs. Post training scores were used to follow up on group X time interactions. Mind wandering during trainings sessions was analyzed using an ANOVA with active training group (Mindfulness vs. N-back) as a between participant factor and time (1st half vs. 2nd half of training) as a within participant factor. Training performance was analyzed within each active training group using paired t-tests (1st half vs. 2nd half of training).

Breath counting training. Participants counted their breaths from one to nine, again and again, for ~25 minutes. With breaths 1-8 they pressed the down arrow, and on breath 9 they pressed the right arrow to end the count set so that counting accuracy could be assessed for each set. Guided mindfulness audio instruction began each session, and feedback was delivered at the end of each count set according to participants' counting accuracy (e.g. see http://webtasks.keck.waisman.wisc.edu/b/demo and https://webtasks.keck.waisman.wisc.edu/b/8minSilent for abbreviated versions of the first and last training sessions, respecitvely). Sessions ended with participants answering the question "Just now where was your attention?" on a 6-point Likert scale ranging from "completely on-task" to "completely off-task".

N-back training. Participants performed a spatial adaptive single n-back task for ~25 minutes modified from Jaeggi et al. (2008). Just as dual n-back training, single n-back training has been found effective in improving scores on cognitive tasks (Jaeggi, Buschkuehl, Shah, & Jonides, 2014). In n-back training a square appeared every three seconds at a location on the screen. Participants pressed “A” if the square appeared at the same location it appeared n (e.g., 3) appearances before, or “L” if the square appeared at a different location (Fig. S4). At the end of ~75 sec, participants received feedback on their accuracy. They advanced to n+1-back training if accuracy was above 90%, or regressed to n-1-back training if accuracy was below 75%. Sessions ended with participants answering the question “Just now where was your attention?” on a 6-point Likert scale ranging from “completely on-task” to “completely off-task”.

Breath Counting Task. As in Study 1, but without meta-awareness probes or self-caught miscounting.

Verbal 3-back. A single letter appeared every three seconds, and participants pressed “A” if the letter was the same as the letter that appeared 3 letters before, or “L” if the letter was different (Fig. S5). Each task block lasted 75 seconds and blocks continued for 15 minutes without feedback.

Exploratory measures were also administered in the pre/post test battery, including the Operation Span task, Backward Digit Span task, Reading the Mind in the Eyes Task, Imaginal Process Inventory (Daydreaming subscale), Beck Depression Inventory, Psychological Wellbeing Scale, Positive and Negative Affect Scale, Spielberger Trait Anxiety Inventory, Acceptance and Action Questionnaire, Penn State Worry Questionnaire, Life Satisfaction Scale, the Self-Compassion Scale, and daily life experience sampling by text messages 8x/day for 7 days asking "How do you feel?" on a scale of (1)=bad through (9)=good and "Are you thinking of something other than what you are doing?" in a yes/no format (results listed in Table S2).

Results

Study 1

Inspection of probe RTs from the original probe order of Study 1 revealed that participants took longer to report off-task vs. on-task attention during TUT ratings, $t_{(76)} = 2.43$, $P = .02$. Since participants reported their count only after finishing the TUT rating, they needed to remember their count longer when reporting off-task attention. As a result, it could be argued that breath counting's association with mind wandering was simply a result of forgetting accurate counts due to taking longer to report mind wandering.

To further clarify breath counting's association with mind wandering and attempt replication, a separate block of 44 participants collected part way through Study 1 received the count probe first before TUT probes in an otherwise identical task. Findings replicated, with greater meta-awareness and less mind wandering in those skilled in breath counting, $r = .57$, $P < .001$ (Fig. 1*A* inset) and $r = -.62$, $P < .001$, respectively. Greater meta-awareness and less mind wandering were also found during moments of accurate counting within participants' performance, $t_{(28)} = 2.93$, $P < .01$ and $t_{(28)} = 2.06$, $P = .05$ (Fig. 1*B* inset), respectively. Comparing data collected using the different probe orders, count probe errors increased when count was probed last instead of first, $t_{(139)} = 5.51$, $P < .01$, indicating that intervening TUT probes interfered with remembering accurate counts. However, the replication even after interference was removed suggests the original findings were not the mere result of RT differences.

Study 2

Regarding test-retest, no significant practice effects were detected at the second test administration, $t_{(53)} = .73$, $P = .47$.

Study 4

We took steps to protect key findings from nonspecific training influences. We decreased the number of potential active ingredients in our mindfulness training: in contrast to typical mindfulness trainings that are in-person, group format, and use heterogeneous trainings that often include yoga and compassion meditation, we delivered a relatively process-pure training in breath counting done individually and online. In addition, we randomized participants to control groups including spatial n-back training, matched to breath counting in hours of practice and motivation as indexed by attrition.

We then interrogated the data to see whether our methodological precautions were effective. To check whether the improvements in mindfulness found with breath counting training were simply due to non-specific factors such as motivation that should improve performance non-selectively on any task, we tested all groups before and after the training period in a verbal 3-back task. If breath counting training specifically improved mindfulness, then verbal 3-back performance should improve only with spatial n-back training and not with breath counting training. In line with this prediction, we found a group X time interaction, $F_{(2, 68)} =$

3.72, $P = .03$, in which the breath counting group and no-training control group did not significantly differ from each other in their pre - post change in verbal 3-back accuracy, $F_{(1,\,68)} = 0.32$, $P = .58$, while the n-back training group improved relative to the breath counting group and no-training control group, $F_{(1,\,68)} = 6.86$, $P = .01$. These data suggest that breath counting training enhanced mindfulness as opposed to simply affecting non-specific factors such as motivation.

Unsurprisingly, n-back training in the first 10 days resulted in a numerically (but not significantly) lower mean rate of mind wandering than the task of breath counting (Fig. S6*B*), in line with previous findings that demanding tasks that place high loads on working memory – such as the 3-back – suppress mind wandering more than tasks without high working memory loads – such as simple counting (McKiernan, D'Angelo, Kaufman, & Binder, 2006b).

Discussion

In moments that mindfulness is present, it may decrease stimulus-independent thought through a number of mechanisms, one of which is outlined here. Since mindfulness encourages direct perception of present experience, we suggest mindfulness may reduce task-unrelated thought (TUT) as a natural byproduct of more fully saturating perceptual resources (Forster & Lavie, 2009b; Levinson et al., 2012b). According to Load Theory (Lavie, 2005b), when a task engages the limited pool of perceptual resources, there are fewer left to automatically spill over and perceive task-irrelevant stimuli. As a result, TUT would be supplanted in early stage perceptual processing by direct perception of present experience, and thus be unable to advance to late stage processing where it might otherwise be maintained and elaborated by working memory into trains of TUT (Levinson et al., 2012b; Smallwood & Schooler, 2006b; Teasdale, Proctor, Lloyd, & Baddeley, 1993b).

A second but not mutually exclusive interpretation of the lack of correlation between skill in mindfulness practice and working memory capacity is that of statistical suppression. Working memory may aid the initiation of mindfulness – i.e. remembering the intention to replace a wandering attention on the breath – and at the same time aid the maintenance of mind wandering about information not present (Levinson et al., 2012b) at the cost of meta-awareness (Schooler et al., 2011). As a result, working memory's opposing influences on breath counting performance may cancel.

Mindfulness questionnaires' vulnerability to retrospective bias and other self-report biases may also help explain why the correlations between breath counting accuracy and the FFMQ and MAAS did not show more overlapping variance. Indeed, modest correlations might have been expected, as they are not uncommon between self-report and behavioral measures of attentional constructs (e.g. (V. A. Anderson, Anderson, Northam, Jacobs, & Mikiewicz, 2002)).

Table S1. Questionnaires used in Studies 1 and 2 with example items.

Questionnaire	**Sample Item**
Mindful Attention and Awareness Scale (MAAS)	"I rush through activities without being really attentive to them."
Five Factors of Mindfulness Questionnaire (FFMQ)	"When I have distressing thoughts or images, I just notice them and let them go".
Imaginal Process Inventory (IPI)	"I dream at work (or school)."

Table S2. Exploratory measures in Studies 1-4.

Measure	**Statistic**
Study 1	*Counting accuracy correlation*
CFQ	$r = -.22$, $P = .03$
Study 2	*Counting accuracy correlation*
ARS	$r = -.13$, $P = .14$
ASRS	$r = -.17$, $P = .05$
Study 3	*Counting accuracy correlation*
BIS	$r = -.07$, $P = .67$
Study 4	*Group X Time interaction*
OSPAN	$F_{(2, 68)} = 0.74$, $P = .48$
BDS	$F_{(2, 68)} = 0.21$, $P = .81$
RMET	$F_{(2, 68)} = 0.29$, $P = .75$
IPI	$F_{(2, 68)} = 1.93$, $P = .15$
BIS	$F_{(2, 68)} = 0.55$, $P = .58$
BDI	$F_{(2, 68)} = 1.20$, $P = .31$
PWB	$F_{(2, 68)} = 0.65$, $P = .53$
PANASn	$F_{(2, 68)} = 0.69$, $P = .51$
PANASp	$F_{(2, 68)} = 1.56$, $P = .22$
STAIX	$F_{(2, 68)} = 1.45$, $P = .24$

AAQ	$F_{(2, 68)} = 0.74$, $P = .48$
ARS	$F_{(2, 68)} = 0.02$, $P = .98$
ASRS	$F_{(2, 68)} = 1.97$, $P = .15$
PSWQ	$F_{(2, 68)} = 1.85$, $P = .17$
LS	$F_{(2, 68)} = 1.15$, $P = .32$
Mood ES	$F_{(2, 68)} = 0.03$, $P = .97$
TUT ES	$F_{(2, 68)} = 1.14$, $P = .33$
SCS	$F_{(2, 68)} = 3.87$, $P = .03$*

*Although the n-back and no training control groups did not significantly differ from each other in their pre - post change in self compassion, $F_{(1, 68)} = 2.54$, $P = .12$, the breath counting group increased in self compassion relative to the two control groups, $F_{(1, 68)} = 5.92$, $P = .02$.

CFQ = Cognitive Failure Questionnaire; ARS = Anger Rumination Scale; ASRS = Adult ADHD Self Report Scale; BIS = Barratt Impulsivity Scale; OSPAN = Operation Span; BDS = Backward Digit Span; RMET = Reading the Mind in the Eyes Task; IPI = Imaginal Process Inventory (Daydreaming subscale); BDI = Beck Depression Inventory; PWB = Psychological Wellbeing; PANASn = Positive and Negative Affect Scale, negative affect only; PANASp = Positive and Negative Affect Scale, positive affect only; STAIX = Spielberger Trait Anxiety Inventory X-2; AAQ = Acceptance and Action Questionnaire; PSWQ = Penn State Worry Questionnaire; LS = Life Satisfaction; Mood ES and TUT ES = average response to text messages 8x/day for 7 days asking “How do you feel?” on a scale of (1)=bad through (9)=good and “Are you thinking of something other than what you are doing?” in a yes/no format, respectively; SCS = Self-Compassion Scale.

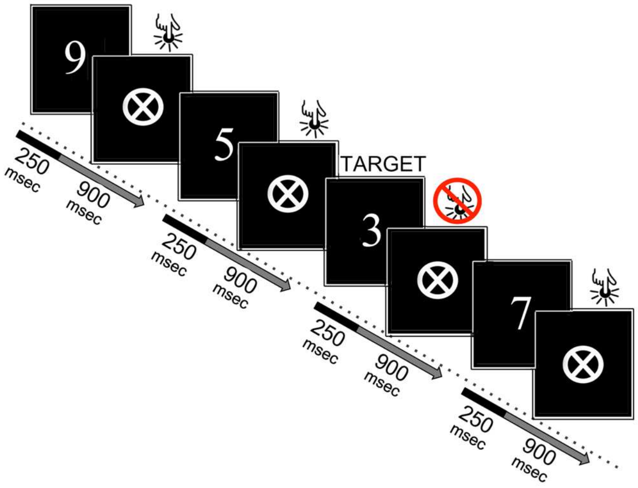

Fig. S1. Sustained Attention to Response Task (SART) schematic, adapted from (Grahn & Manly, 2012).

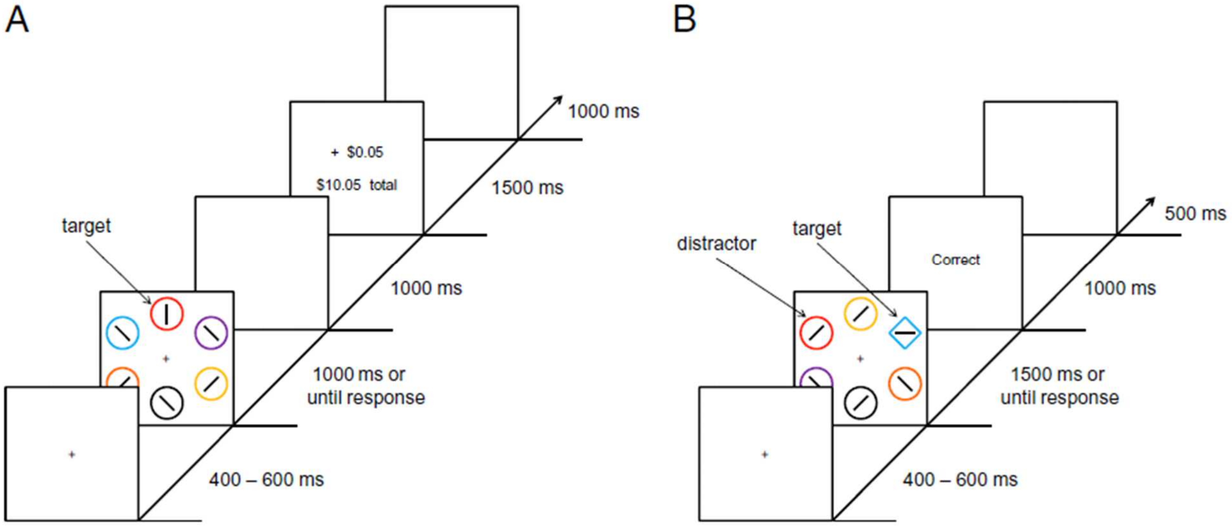

Fig. S2. Attention capture training (S3*A*) and testing (S3*B*) schematic, adapted with permission from (B. A. Anderson et al., 2011).

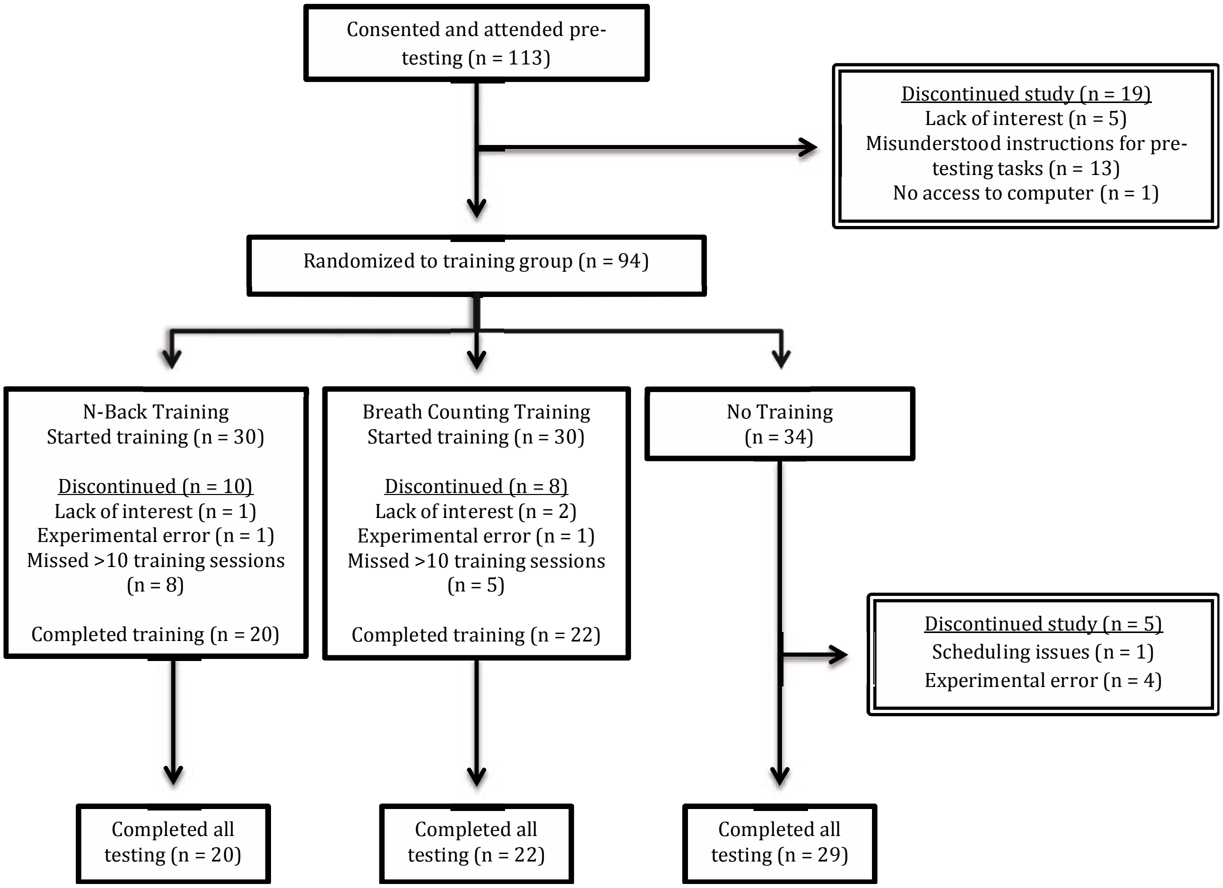

Fig. S3. Diagram detailing retention rates by Study 4 phase and reasons for dropouts.

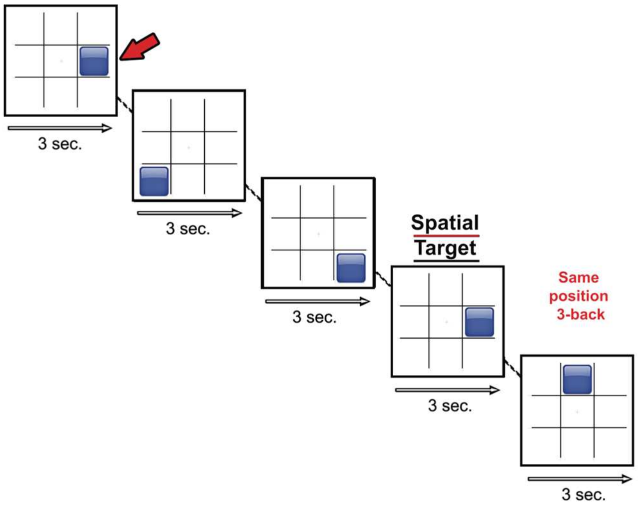

Fig. S4. N-back training schematic with a 3-back level exemplified.

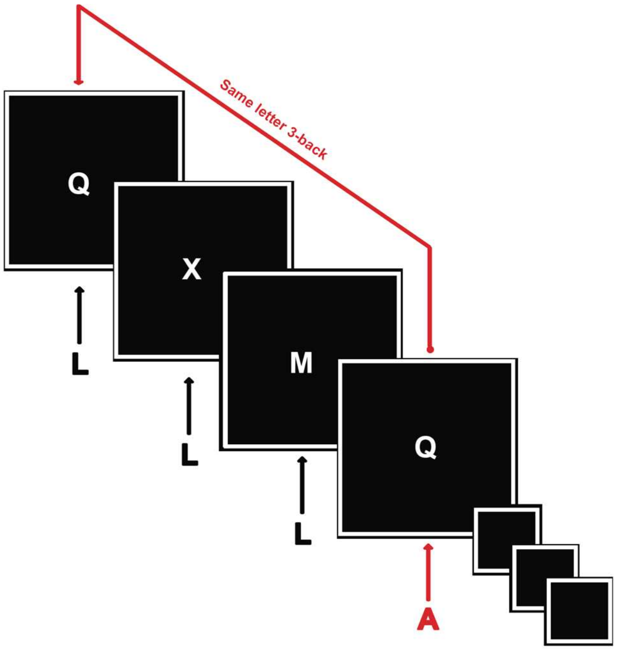

Fig. S5. Verbal 3-back schematic.

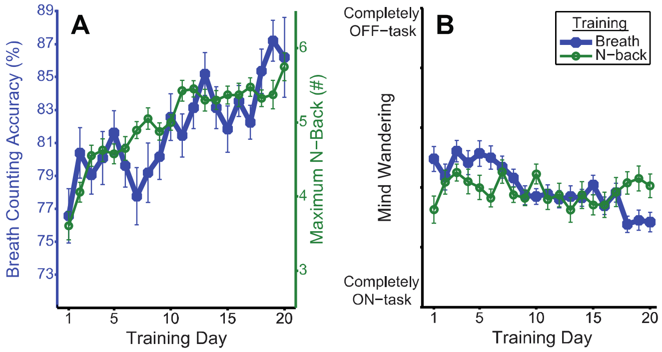

Fig. S6. Change in performance and mind wandering over the course of 20 consecutive weekdays of training. (6A) Performance during daily training was calculated from the average of 2 25 minute training sessions (AM and PM). Performance in breath counting was measured by a single rating at the end of each session that answered the question “Where was your attention just now?” on a 6-point likert scale ranging from “completely on-task” to “completely off-task.” Error bars represent within participants' +/- 1 SE. (6B) Mind wandering during daily training was calculated from the average of 2 25 minute training sessions (AM and PM). Mind wandering was measured by a single rating at the end of each session that answered the question “Where was your attention just now?” on a 6-point likert scale ranging from “completely on-task” to “completely off-task.” Error bars represent within participants' +/- 1 SE.

Chapter 4

Working Memory Training and Mind Wandering

Despite the correlational evidence linking mind wandering rates to working memory and mindfulness discussed in Chapters 2 and 3, establishing causal evidence requires further examination. Experimental manipulations such as attention trainings that might increase working memory or mindfulness would provide an opportunity to check for any resulting effects on mind wandering and thereby test for causal relationships. As Chapter 3 has already covered the results of our attention training research with an emphasis on the effects of mindfulness training, this supplemental chapter will address the results with an emphasis on working memory training.

In recent years, attention training research has shown promise that capacities such as working memory can be increased through repeated practice of tasks that measure working memory such as the n-back (e.g. (Dahlin et al., 2008; Jaeggi et al., 2008a; Klingberg et al., 2005; McNab et al., 2009a). For example, Jaeggi et al. (2008) administered a working memory n-back task for 20 consecutive weekdays and found improvements in working memory following training. Importantly, improvements from working memory training have been found to generalize beyond performance in the trained task, increasing working memory as measured by Backward Digit Span as well as enhancing theoretically related capacities such as general fluid intelligence (Jaeggi et al., 2008a) and cortical dopamine levels (McNab et al., 2009a).

As noted in Chapter 3, in the same way that working memory may be improved by repeated practice of working memory tasks such as the n-back task, mindfulness may be improved by repeated practice of a behavioral measure of mindfulness, breath counting. Using these methods with the aim of experimentally increasing working memory or mindfulness, we sought to test mind wandering rates after training to assess whether mind wandering would be increased with n-back training and decreased with breath counting training.

In addition to providing a needed causal test of the relation of mind wandering with both working memory and mindfulness, our dual training approach addressed methodological limitations in the attention training literature reviewed by Shipstead and colleagues (Shipstead, Redick, & Engle, 2012). They noted that many working memory training studies have implemented designs vulnerable to false positives, using no-contact control groups and single measures of the constructs targeted by training. In line with their advice, our n-back training group and breath counting training group served as active control groups for each other. Moreover, we included multiple measures of constructs of interest, allowing for a more rigorous approach to attention training research.

Method

In addition to the methods for training, pre- and post-testing, and statistical analyses reported in Chapter 3, participants received pre- and post-testing on the following measures relevant to our hypotheses regarding working memory training.

OSPAN. As in Chapter 2.

Backward digit span. Participants heard 14 strings of numbers progressing in length from three to nine digits, with two strings of each length. After each string participants reported the string's digits in reverse order (see Fig. 1 for illustration of task flow).

Daily life experience sampling. Participants received texts on their cell phones for seven consecutive days eight times a day (75-minute mean time between texts) starting at 11am. Participants were given a $25 bonus if they responded within 5 minutes to 90% of the texts. Each text contained three questions (A-C):

A) How do you feel? (1)=bad through (9)=good.

B) Are you thinking of something other than what you are doing? (0)=no, (1)=yes, something pleasant, (2)=yes, something neutral, or (3)=yes, something unpleasant.

C) Are you trying to concentrate on what you are doing? (1)=none through (9)=a lot.

Results

As discussed in Chapter 3, both active training groups improved in training performance. However, the gains in n-back training performance did not generalize far in other measures of working memory. We did find a near-transfer of spatial n-back training gains to verbal 3-back gains selective to the n-back training group, as mentioned in Chapter 3. However, we found no evidence for any other transfer from n-back training despite including two other measures of working memory, backward digit span (group X time interaction, F(2, 68) = 0.21, p = .81) and OSPAN (group X time interaction, F(2, 68) = 0.74, p = .48).

Given the lack of any consistent evidence that working memory had improved from n-back training, it is was unsurprising that mind wandering did not significantly increase following n-back training: when we sampled all groups before and after the training period on their mind wandering in daily life, we found no evidence of a group X time interaction, F(2, 68) = 1.14, p = .33. Moreover, although we found a significant group X time interaction regarding mind wandering during breath counting in lab, F(2, 68) = 5.09, p < .01, there was no evidence that n-back-training participants significantly differed from no-training participants in their pre - post change in mind wandering, F(1, 68) = 3.75, p = .06; if anything, the trend was for no-training participants to increase more in mind wandering than n-back-training participants.

Discussion

Attention training has long been viewed as a challenging endeavor. Although improvements in training tasks have repeatedly been found, these improvements are usually quite specific (Healy, Wohldmann, Sutton, & Bourne, 2006). For example, training in perception of a visual motion fails to improve perception of stimuli moving in untrained directions or in untrained retinal locations (Fahle & Poggio, 2002). It is notoriously difficult to achieve training gains that stem from increasing a domain general capacity and therefore generalize to untrained tasks.

Training that aims to increase working memory has recently shown promise of breaking that mold (Jaeggi et al., 2008a), in line with theory that working memory is a domain general capacity involved in the performance of tasks ranging from verbal reasoning to manipulating abstract spatial relationships (Kane et al., 2004). However, follow up studies have been less positive, yielding mixed results and failed replications (Harrison et al., 2013; Redick et al., 2012; Shipstead et al., 2012). We too did not find strong evidence that working memory training works beyond near transfer.

The dearth of significant transfer effects we observed may have resulted from the increased rigor of our methodology compared to earlier research. Instead of using only a no-contact control group or a single measure of working memory, we included an active control breath counting group and three measures of working memory. With these more sensitive methods, both we and Redick and colleagues (2012) were able to replicate a steady increase in n-back performance over the course of n-back training, but found no evidence that the improvements transferred to generally improve working memory, as indexed by multiple measures. Taken together, these two non-replications suggest that each other's findings are not due to poor experimentation, but rather more rigorous methodology.

Alternatively, the paucity of transfer we observed may have been due to differences in participants' motivation to train. Whereas we and Redick and colleagues (2012) paid participants to train, Jaeggi and colleagues (2008) did not. Jaeggi's participants may have been more motivated and intrinsically so, potentially accounting for why they observed transfer where others have not (Jaeggi et al., 2014). There is certainly precedent for specific attention training protocols coupled with motivated training environments successfully achieving transfer, for example through action video game training (Green & Bavelier, 2003).

Regardless of the cause, successful working memory training is clearly not as easily reached as initial findings first suggested. Nonetheless, the present results clarify important methods for testing whether additional training components such as increased motivation can indeed improve working memory in a way that transfers to other contexts.

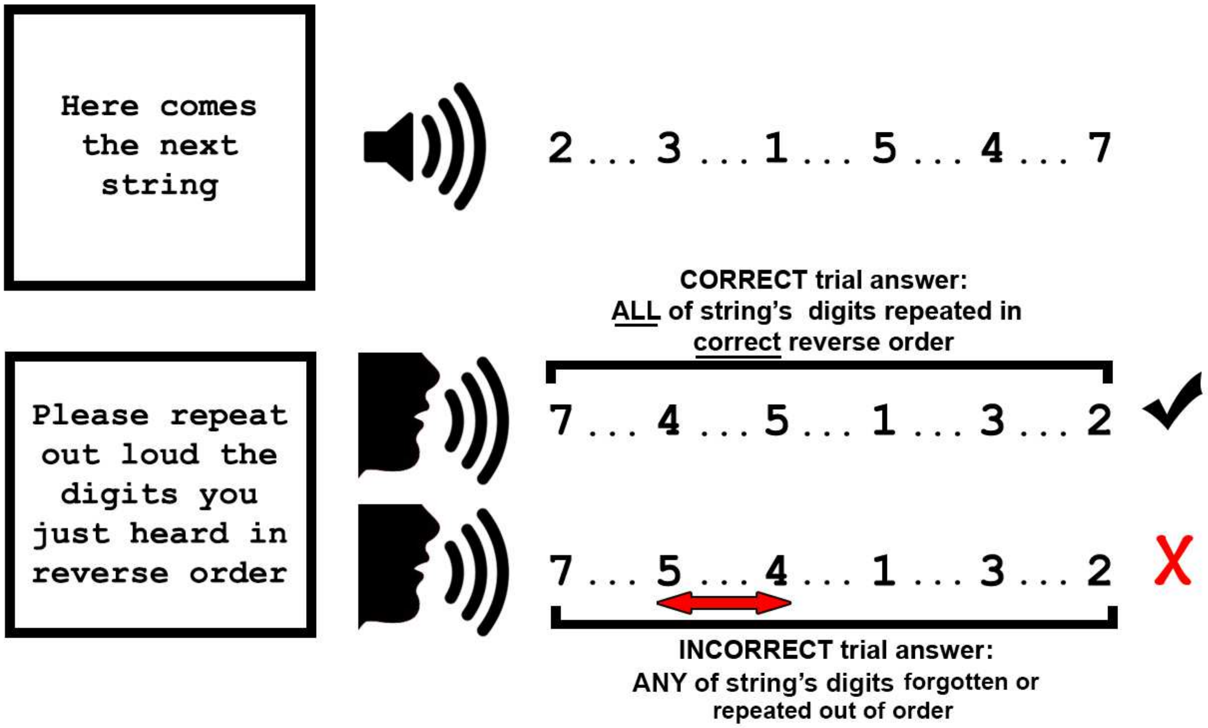

Fig. 1. Backward digit span (BDS) schematic.

Chapter 5

Mechanisms for Modulating Mind Wandering

Working memory, or the ability to maintain and manipulate information not present in the current environment, has received increasing attention since it's proposal by Baddeley in 1974 (Baddeley & Hitch, 1974). As its influence has been documented in an ever-expanding range of meaningful processes - including attenuating external distractions and regulating internal distractions such as mind wandering - the question of how working memory exerts its influence has become more salient.

Regarding internal distractions, some have proposed that working memory decreases mind wandering by restricting it from awareness, creating a kind of "spotlight" of awareness for on-task thoughts (McVay & Kane, 2009a). This view is consistent with data about external distraction, where those individuals with greater working memory capacity are less slowed by peripheral distractors during visual search (Ahmed & de Fockert, 2012). Furthermore, consuming working memory in a secondary task has been found to increase awareness of distractors (de Fockert & Bremner, 2011) and the extent to which incongruent distractors can slow a visual search (Lavie et al., 2004a). In these studies, it is as if occupying working memory in a secondary task renders it no longer able to restrict processing of peripheral distractors.

Although the theory that working memory restricts mind wandering from awareness has been forwarded for a number of years, it has not yet been directly tested. To test it, we sought to employ a research strategy similar to that used with external distraction. Specifically, we loaded working memory with a secondary task and asked if this increased awareness of mind wandering just as it had increased awareness of external distractors.

However, working memory load is known to decrease the amount of mind wandering itself, in line with theory that working memory resources are required to maintain and elaborate TUT (Teasdale et al., 1993a). Therefore, any change in awareness of mind wandering under working memory load could simply be due to a change in the amount of mind wandering of which to be aware. To disambiguate our findings, we crossed perceptual load with working memory load in a within-participant design in order to decrease mind wandering without affecting working memory (Forster & Lavie, 2009a; Lavie et al., 2004a). We predicted that mind wandering would be equally decreased by working memory load and perceptual load but awareness of mind wandering would be selectively increased by working memory load, substantiating theory that working memory normally restricts mind wandering from awareness when unoccupied by a secondary task.

In addition, as a parallel test of working memory's mechanism for regulating mind wandering, we measured individuals' working memory capacity and assessed whether those with greater working memory capacity reported less awareness of mind wandering.

Methods

All 83 participants with usable data were students recruited from the University of Wisconsin-Madison for research credit (age range 18-21, mean 18.7; 28% male). Participants were tested in groups of 22 or less on a 30-min visual search task with a working memory load, followed by the Operation Span measure of working memory capacity.

Visual search task with working memory load. (Fig. 1) The task was an adaptation of that of Lavie et al. (2004, Experiment 3). In brief, on each trial of the visual search task, a central ring of six letters containing the target—either X or N—was presented for 100 ms with target identity and position counterbalanced across trials. Participants pressed a key to indicate the target's identity as quickly and accurately as possible.

Trials were presented in blocks of 48. Blocks varied on two dimensions, perceptual load (low, high) and working memory load (low, high), creating four block types in all: high working memory load and high perceptual load (H/H), high working memory load and low perceptual load (H/L), low working memory load and high perceptual load (L/H), and low working memory load and low perceptual load (L/L). In blocks with low perceptual load, nontarget letters in the central ring were small Os, which allowed the target to be easily distinguished. In contrast, in blocks with high perceptual load, nontarget letters (H, K, M, V, W, and Z) were angular and target sized, which made the target more difficult to perceive. Perceptual accuracy was calculated as the percent of trials with accurately identified targets. Participants (n = 3) were excluded for < 55% accuracy on high perceptual load blocks.

For each block, either a single digit or six digits were presented at the start of the block for 2000 ms and queried at the end of the block after the thought probe. A single digit was presented in blocks with low working memory load; in contrast, six digits were presented in blocks with high working memory load. Working memory accuracy was calculated as the percent of blocks when numbers were correctly recalled. Participants (n = 6) were excluded for < 55% working memory accuracy.

Each participant received 16 blocks of trials. The first four blocks alternated between the four block types in one of the following orders: H/H, H/L, L/H, L/L; H/L, H/H, L/L, L/H; L/H, L/L, H/H, H/L; or L/L, L/H, H/L, H/H. A participant's subsequent blocks were run in the same order as the first four blocks. Presentation order was counterbalanced between participants so that each order was equally represented.

At the end of each block came the thought probe: "What were you thinking just now?" Participants pressed "A" if they had been thinking task-related thoughts, that is, thoughts "about the task you are doing at that exact moment" (example given in the instructions: "Where's the X? Oh, there it is."). Conversely, participants pressed "W" for task-unrelated thought (TUT; examples given in the instructions: "I must stop by the supermarket on the way home,") of which they were aware, and "Z" for TUTs of which they were unaware (see Figure 7 for a task schematic). The TUT score equaled the percentage of probes on which a participant reported TUT ([# of responses W or Z] / [# of responses W + # of responses Z + # of responses A]), reflecting quantity of mind wandering. In contrast, the TUT percent awareness score equaled the percentage of TUTs of which a participant reported being aware ([# of responses W] / [# of responses W + # of responses Z]), reflecting *awareness* of mind wandering regardless of quantity. To avoid undefined data points, participants reporting no TUT for any one of the four block types were excluded from analyses (n = 24); it was impossible to determine how aware they were of TUT because there was no TUT of which to be aware.

Operation span task. A full description of the task can be found elsewhere (Unsworth et al., 2005a). In brief, the task consisted of 15 trials. For each trial, the display switched three to seven times between letters for memorization and math equations (e.g. 1 + (3/3) = ?) for verification under response deadline. Each participant's latencies for 15 math-only practice questions were used to create individualized response deadlines (*M* + 2.5 *SD*s). A participant's score on the OSPAN was calculated as the sum of the letters recalled in accurate sequence from all 15 trials. Participants (n = 2) were excluded for < 85% accuracy on math items, as in past research (Unsworth et al., 2005a).

Statistical analyses. A within-participant 2x2 ANOVA (load: low, high; domain: working memory, perceptual) was used to investigate TUT scores. Likewise, a within-participant 2x2 ANOVA (load: low, high; domain: working memory, perceptual) was used to examine TUT percent awareness scores. Finally, regression was used to investigate any correlation between OSPAN scores and TUT percent awareness.

Results

Manipulation checks confirmed that mind wandering was reduced by both working memory load - main effect of working memory load, F(1, 82) = 6.37 p = .01 - and perceptual load - main effect of perceptual load, F(1, 82) = 6.12, p = .02. In addition, the extent of reduction in mind wandering did not differ when load was placed on working memory vs. perceptual resources - interaction of load X domain, F(1, 82) = 0.14, p = .71 (Fig. 2). These data replicate previous findings that consuming either working memory or perceptual resources in an external task decreases mind wandering (Forster & Lavie, 2009a; Teasdale et al., 1993a). In addition, these data suggest that our methods for imposing working memory and perceptual load were both effective and well matched in the extent to which they decreased mind wandering.

To test the hypothesis that loading working memory increases awareness of mind wandering - as would be expected if working memory normally restricts mind wandering from awareness - we tested for a main of effect of working memory load on TUT percent awareness. We found no significant effect, F(1, 82) = 0.09, p = .77. It is unlikely that the lack of effect was due to insensitivity of our experience sampling methods, because they successfully detected a main effect of perceptual load on TUT percent awareness, F(1, 82) = 5.54, p = .02, in line with previous research that perceptual load can decrease awareness of stimuli (Lavie). However, the effect was moderate and did not lead to a significant load X domain interaction, F(1, 82) = 0.85, p = .36 (Fig. 3).

As a second test of the theory that working memory is used to restrict TUT from awareness, we evaluated whether those with greater working memory capacity demonstrated less awareness of TUT across all conditions. Consistent with the lack of working memory effect mentioned above, there was no significant correlation of working memory capacity and awareness of TUT, r(81) = .04, p = .73, suggesting that working memory may not exert its effects on mind wandering through a mechanism of restricting mind wandering from awareness.

Discussion

We predicted that loading working memory would obstruct its theorized role of restricting mind wandering from awareness and result in increased awareness of mind wandering. In addition, we predicted those with greater working memory capacity would be better able to restrict mind wandering from awareness. However, we found evidence for neither.

It is unlikely the lack of predicted effects we observed resulted from insensitive methodology. Our working memory and perceptual load inductions were well matched and effective in reducing mind wandering. In addition, our sample size and experience sampling methods allowed us to detect changes in awareness of mind wandering that were consistent with research on perceptual load. Lavie's Load Theory claims that consuming limited perceptual resources in a perceptual load task should decrease surplus perceptual resources necessary for becoming aware of task-irrelevant stimuli (Lavie, 2005a). In line with this theory, our perceptual load induction decreased *awareness* of task-irrelevant thoughts, even after accounting for the decreased *quantity* of task-irrelevant of which to be aware. This extends findings that perceptual load decreases motion aftereffects and fMRI activation in motion-sensitive cortical area V5 in response to motion (Rees, Frith, & Lavie, 1997) by suggesting that TUT is yet another domain where perceptual load decreases awareness.

More research is needed to determine working memory's role in regulating mind wandering. It may be that working memory reduces mind wandering not by reducing awareness of mind wandering, but by influencing processing of mind wandering after it enters awareness. This view is in line with Lavie's two stage theory of stimulus processing (Lavie, 2005a), in which early stage perceptual resources perceive a stimulus so that it enters awareness, whereas late stage working memory resources govern processing of perceived stimuli (e.g. to inhibit responses to distracting stimuli). This view can also accommodate findings suggesting that once TUT is perceived, working memory capacity can either respond by elaborating it into a chain of TUT - thus increasing TUT (Levinson et al., 2012a) - or by inhibiting any prepotent elaborative response - thus decreasing TUT (McVay & Kane, 2009a).

Forefather of American Psychology William James famously stated "My experience is what I agree to attend to. Only those items which I notice shape my mind" (James, 1890). Working memory has been repeatedly associated with shaping mind wandering (Levinson et al., 2012a; McVay & Kane, 2009a), and mind wandering has been found to influence mood and attention (Killingsworth & Gilbert, 2010; Mrazek, Smallwood, Franklin, et al., 2012). Continuing research on the mechanism by which working memory shapes mind wandering promises to yield insights relevant to emotional and cognitive well-being.

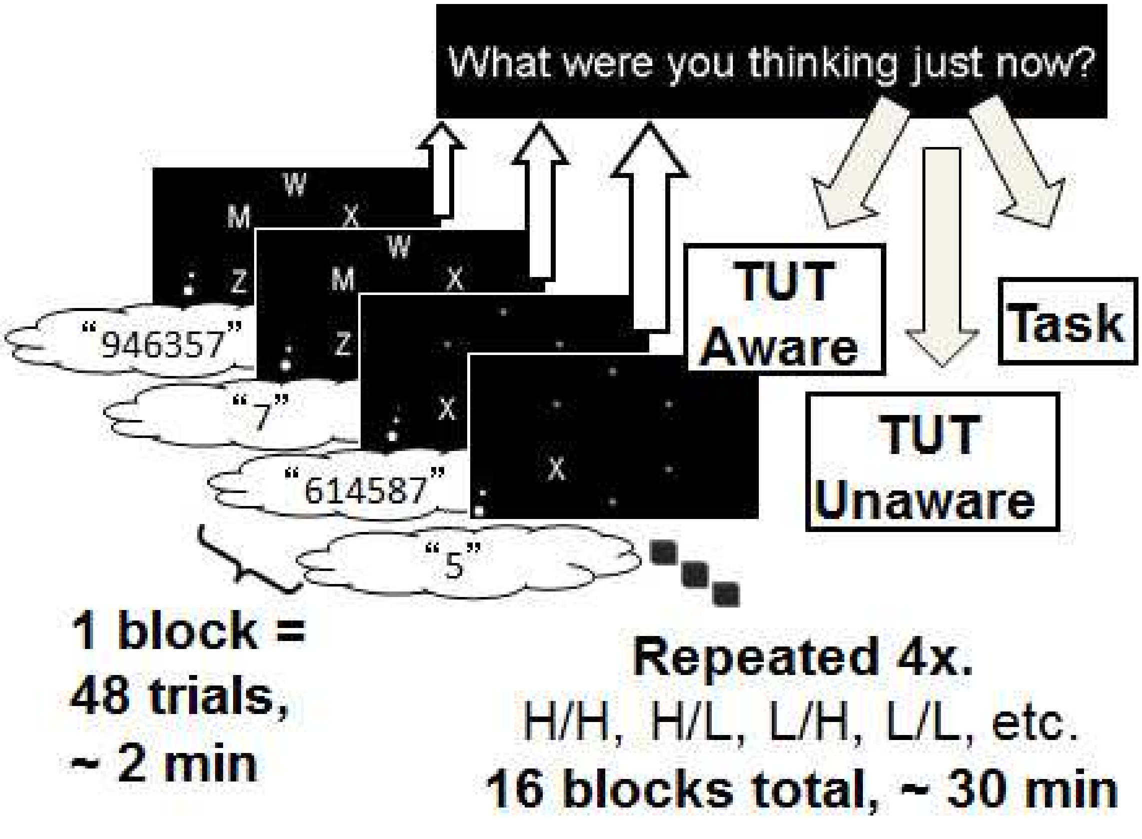

Fig. 1. Visual search task schematic

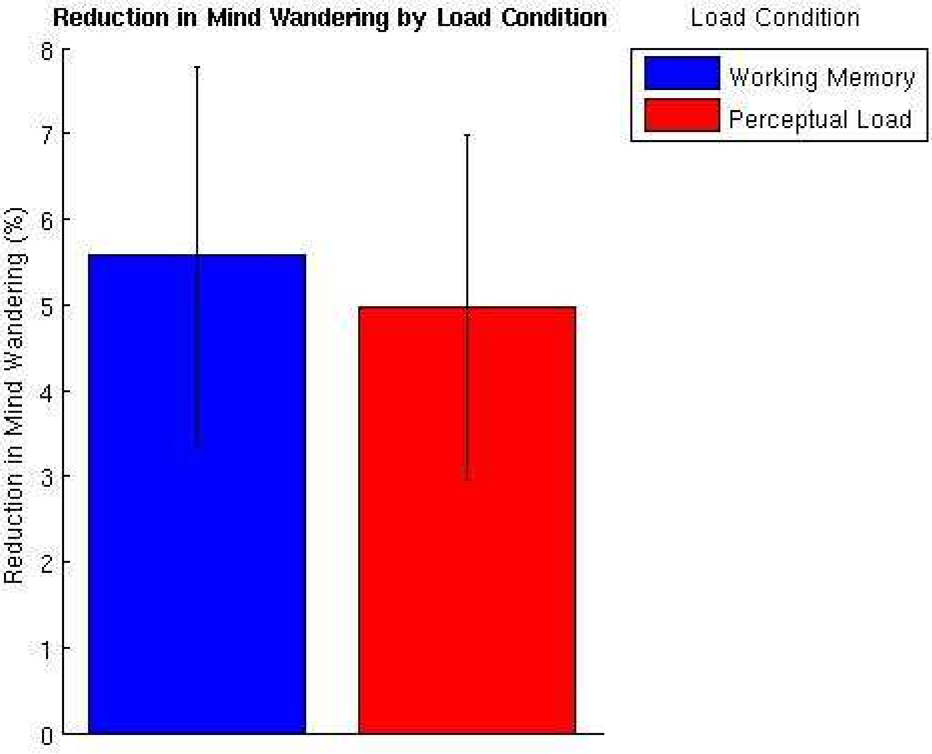

Fig. 2. Reduction of mind wandering as a result of loading working memory (blue) or perceptual resources (red).

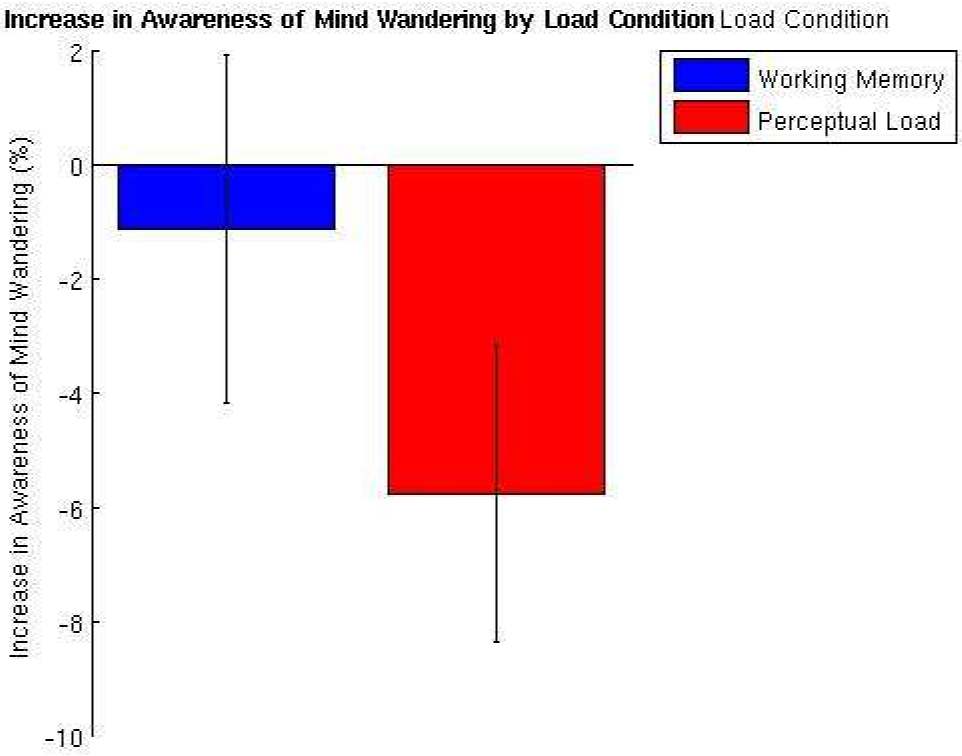

Fig. 3. Increase in awareness of mind wandering as a result of loading working memory (blue) or perceptual resources (red).

Chapter 6

Conclusion

In the preceding chapters we investigated what facilitates and inhibits mind wandering through both correlative and causal tests. Regarding the facilitation and inhibition of mind wandering, in Chapter 2 we assessed whether working memory can facilitate mind wandering in non-demanding contexts. We found working memory capacity was positively correlated with mind wandering when the context did not demand working memory resources or perceptual resources be devoted to an external task. As a result, we concluded that while greater working memory capacity may allow better inhibition of mind wandering during working-memory-demanding tasks, during undemanding tasks greater working memory capacity may facilitate mind wandering. In Chapter 3, we examined whether mindfulness can reduce mind wandering. We found behaviorally measured mindfulness was negatively correlated with mind wandering across participants, and that participants reported mind wandering more in the very moments mindfulness was lacking.

In light of these correlative findings, we proceeded to investigate whether there was a causal link between mind wandering and working memory or mindfulness. In Chapters 3 and 4 we employed two different attention training regimens aimed to increase working memory or mindfulness, respectively. We found the mindfulness training indeed increased mindfulness and decreased mind wandering during mindfulness practice. Unfortunately, the working memory training did not consistently increase working memory in a domain general way, preventing a causal test of whether increasing working memory facilitated mind wandering.

Finally, we sought to investigate the mechanism by which working memory inhibits mind wandering. Specifically, we aimed to test the theory that working memory reduces mind wandering by restricting it from awareness. Towards this end, in Chapter 5 we investigated whether consuming working memory with a secondary task would increase awareness of mind wandering. Although consuming working memory reduced the overall rate of mind wandering, it did not affect awareness of mind wandering, suggesting working memory's influence on mind wandering rates does not work through a mechanism involving awareness.

This research sheds new light on the mind wandering which pervades daily life. As mind wandering typically accompanies at least a third of nearly every daily life activity (Killingsworth & Gilbert, 2010), deeper understanding of how it is regulated has implications for the workplace, conversations with friends and family, and reading periods at school.

In each of these diverse settings, better understanding of how mind wandering is regulated has both cognitive and emotional implications. Our research suggests mind wandering consumes working memory resources when it is upregulated, leaving fewer resources available for the task at hand (Levinson et al., 2012a). This explains more conclusively than past research why mind wandering has been associated with cognitive errors (Allan Cheyne et al., 2009b) and poorer reading comprehension (Smallwood, McSpadden, & Schooler, 2008). Better understanding of mind wandering additionally has import for emotional well-being, as moments of mind wandering have been associated with worsened affect in everyday life (Killingsworth & Gilbert, 2010). Indeed, we found that mindfulness was not only related to reduced mind wandering, but better mood.

Our findings are relevant to well-being not only in the general population but also in clinical populations. For example, the inverse relation of breath counting with task-unrelated thoughts bolsters claims that clinical mindfulness interventions reduce depressive relapse by reducing rumination, the repetitive, negative task-unrelated thoughts known to increase incidence of depression (Spasojević & Alloy, 2001). Furthermore, these claims which suppose such clinical interventions increase mindfulness per se can be tested by assessing whether the clinical interventions increase breath counting accuracy.

Future research should further explore the mechanisms supporting the regulation of mind wandering. For example, mindfulness may reduce mind wandering in part by enhancing continuity of direct sensory perception (e.g. of the breath) and loading perceptual resources. This could be substantiated by neuroimaging that found increased somatosensory and insula activity during mindfulness of breathing correlating with better breath counting accuracy and decreased mind wandering. Regarding the theory that working memory facilitates mind wandering by elaborating chains of task-unrelated thought, rTMS and experience sampling in a undemanding context could be used to assess whether inactivating dorsolateral prefrontal regions decreased rates of mind wandering and the probability that instances of mind wandering were part of longer chains of thought.

Mind wandering pervades much of life. It provides a means of mental time travel and of simulating possible scenarios. However, it may not come free. The present research suggests that mind wandering represents an investment of cognitive resources, and offers insight into methods for regulating this investment to create the emotional and mental life that a person values.

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