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THEORETICAL/REVIEW

Investigating mindfulness influences on cognitive function: On the promise and potential of converging research strategies

Yanli Lin[1] · Rongxiang Tang[1] · Todd S. Braver[1]

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Abstract
Research investigating the effects and underlying mechanisms of mindfulness on cognitive functioning has accelerated exponentially over the past two decades. Despite the rapid growth of the literature and its influential role in garnering public interest in mindfulness, inconsistent methods in defining and measuring mindfulness have yielded variable findings, which contribute to the overall dearth of clear generalizable conclusions. The focus of this article is to address the lack of cohesion in the collective methodologies used in this domain by providing a new perspective grounded in classic cognitive and experimental psychology principles. We leverage the concept of converging operations to demonstrate how seemingly disparate research strategies can be integrated towards a more unified and systematic approach. An organizing taxonomic framework is described to provide useful structure in how mindfulness can be operationalized, measured, and investigated. We illustrate the rationale and core organizing principles of the framework through a selective review of studies on mindfulness and cognitive control. We then demonstrate the utility of the approach by showing how it can be applied to synthesize extant methodologies and guide the development of future research. Specific suggestions and examples pertaining to experimental design and statistical analysis are provided.

Keywords Mindfulness · Meditation · Cognition · Cognitive control

Introduction

Mindfulness is surging in popularity among various communities around the world. Although originating from Buddhist contemplative traditions, mindfulness has undergone a clear evolution over the last 40 years in modern and globalized secular contexts, which have led it to become a rapidly increasing focus of scientific inquiry (McMahan, 2012; McMahan& Braun, 2017). Yet this intense research interest has also revealed the notorious difficulty in operationalizing a precise definition for what is meant by the term mindfulness itself (Baer, 2011; Gethin, 2015; Van Dam et al., 2018). Nevertheless, within Western scientific contexts, there is reasonable consensus from most scholars that mindfulness reflects a specific psychological state of attention or awareness to the present moment (Brown et al., 2007; Kabat-Zinn, 1990; Langer, 1990), accompanied by attitudinal qualities of curiosity, nonjudgment, and acceptance (Baer, 2019; Bishop et al., 2004; Kabat-Zinn, 1990, 2003).

Reflecting the training orientation of its early Buddhist roots, much of the research interest in mindfulness has been oriented towards demonstrating that mindfulness is a state or skill that can be strengthened. A variety of approaches to mindfulness training have been investigated, ranging from its intentional adoption in everyday life (Langer, 1990), to engagement in a variety of formal meditation practices (Hölzel et al., 2011; Lutz et al., 2008; Vago & Silbersweig, 2012), and extending to participation in a number of structured mindfulness-based interventions (MBIs; Baer, 2003; Creswell, 2017; Kabat-Zinn, 2003). Indeed, a consequence of the meteoric rise of scientific research on mindfulness has been the emergence of the distinct field of contemplative science (Wallace, 2009), marked by research centers, professional societies, scientific conferences, and dedicated journals.

A rapidly growing area within contemplative science has been the investigation of mindfulness effects on cognitive functioning. Perhaps influenced by the historical precedent for contemplative traditions to emphasize the training of concentration and perceptual clarity, scientific investigations of mindfulness have been linked to cognition since their

✉ Yanli Lin
lyanli@wustl.edu

[1] Department of Psychological and Brain Sciences, Washington University in St. Louis, St. Louis, MO 63130, USA

inception. For example, nearly all theoretical models of mindfulness reference the construct of attention (Bishop et al., 2004; Grabovac et al., 2011; Hölzel et al., 2011; Lindsay & Creswell, 2017; Lutz et al., 2008; Lutz et al., 2015; Shapiro et al., 2006; Vago & Silbersweig, 2012). Likewise, much of the earlier basic and clinical research efforts were aimed at exploring the intersection of mindfulness and attentional abilities (Cahn & Polich, 2006; Teasdale et al., 1995; Valentine & Sweet, 1999). As the field evolved, collective interest grew to encompass more varied and specific cognitive functions (e.g., creativity and problem solving; Berkovich-Ohana et al., 2017; Colzato et al., 2017), with hopes of developing a more precise and nuanced understanding of the neurocognitive underpinnings of mindfulness and its seemingly diverse array of psychological benefits.

Consequently, the past 15 years have witnessed an influx of research predicated on two general aims: (1) elucidating how mindfulness and cognitive functions are related, including identifying overlapping neurocognitive processes and their respective boundary conditions; and (2) evaluating the extent to which various forms of mindfulness training can modulate or enhance cognitive ability. Despite the substantial growth of the empirical literature, neither aim appears particularly close to being achieved, with studies reporting inconsistent methods and varied findings. Indeed, meta-analytic and narrative reviews of mindfulness and cognitive function have yielded mixed findings, with modest overall support for the salutary effect of mindfulness (Cásedas et al., 2020; Chiesa et al., 2011; Gallant, 2016; Gill et al., 2020; Im et al., 2021; Lao et al., 2016; Leyland et al., 2019; Prakash et al., 2020; Yakobi, Smilek, & Danckert, 2021). Moreover, these reviews have ubiquitously concluded that methodological limitations preclude the drawing of clear generalizable conclusions; as such, they have contributed to the growing calls for the field to increase its methodological rigor and standardization.

The goal of the current paper is to respond to these calls by providing a new perspective for how to advance the collective methodology of mindfulness research, particularly in terms of investigating its potential influence on cognitive functioning. Toward this end, we present a conceptual framework from which to both collate and capitalize on the existing methodological variation present in the mindfulness cognition literature (see Fig. 1). For tractability and demonstrative purposes, we circumscribe our analysis of cognition to the construct of cognitive control, a fundamental feature of human cognition that enables goal-directed action, adaptive behavior, and self-regulation more generally (Egner, 2017). The standard definition of cognitive control is the ability to encode, maintain, and update goal representations, while applying a variety of subordinate cognitive functions to meet task demands (Botvinick & Braver, 2015).

Importantly, cognitive control is a broad construct that has been operationalized and investigated from cognitive, neuroscience, and computational perspectives (Egner, 2017). Moreover, the construct of cognitive control has also been well-studied in terms of its relationship to emotion regulation, metacognition, memory and other constructs that are closely related to mindfulness (Banich et al., 2009; Engle, 2010; Ochsner et al., 2012; Shea et al., 2014). Cognitive control is uniformly viewed to be essential for successful navigation of the modern world; conversely, its disruption is reliably implicated in states of dysregulation and pathology, rendering it a relevant construct to understand the clinical implications of mindfulness, as well as its basic components (Joormann & Tanovic, 2015; McTeague et al., 2016; Wylie et al., 2010).

Consequently, by critically reviewing the prevailing research strategies and methods utilized in studies of mindfulness and cognitive control as a generalizable example, we hope to accelerate the collective development of a more cohesive, systematic, and synthetic research approach within the broader expanse of mindfulness science more generally. In contrast to other reviews which center around the effect of mindfulness on cognitive outcomes, our focus here is to gear discussion toward methodology—namely, how convergent utilization of seemingly disparate research strategies might advance scientific progress and conceptual understanding. Indeed, the unique contribution of our approach lies in integrating classic principles from cognitive and experimental psychology, such as the use of converging operations and factorial designs, with multimodal measures to enable greater control and precision in manipulating and measuring mindfulness. Together, we hope to demonstrate how this approach might translate into a more generalizable, flexible, and ultimately productive research strategy. More specifically, we believe that the approach described here can help resolve longstanding challenges (e.g., variable findings, low replicability, lack of mechanistic rigor and specificity) and enable rapid knowledge advancement within this domain.

Toward this end, we begin by briefly outlining how four different operationalizations of mindfulness have emerged in response to the problem of construct heterogeneity. We then introduce the concept of converging operations before reviewing how these common operationalizations of mindfulness have been used in the context of investigating mindfulness and cognitive control, highlighting their respective strengths, limitations, and prevalence within extant study designs. Next, we leverage the core principles of converging operations to propose a modular framework that can be harnessed to organize and guide research, replete with study design examples and analytic suggestions. Finally, we conclude with a discussion on how deriving better methods to study how mindfulness influences cognitive control can inform our understanding of cognitive control as well.

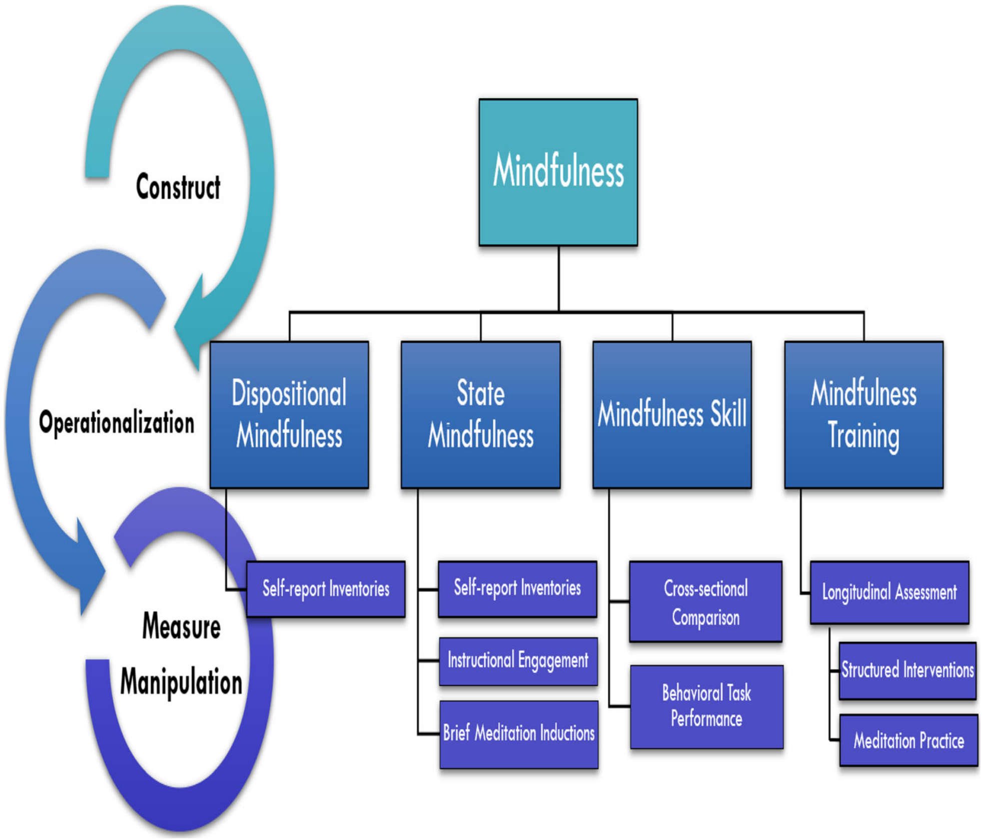

Fig. 1 Proposed taxonomy of mindfulness construct separated across operationalization and measure/manipulation

Mindfulness: A heterogenous construct

As mentioned earlier, defining mindfulness has been an enduring challenge. Inherent in the problem is the fact that mindfulness is not a unitary construct. Instead, mindfulness is an umbrella term that can refer to a state of mind, a characterological trait, a form of contemplative practice, and a type of clinical intervention (Davidson & Kaszniak, 2015; Lutz et al., 2015; Vago & Silbersweig, 2012; Van Dam et al., 2018). Moreover, the same term has also been referred to as a broad modern sociocultural movement (Farb, 2014) and to a religious path of liberation (i.e., a soteriological process; Kuan, 2008). Such construct heterogeneity introduces substantial challenges to scientific investigation, insofar that meaningful progress is predicated on the ability to clearly define and operationalize mindfulness within the context of a given research question. In response to this problem, researchers have adopted the practice of separating the broad polylithic construct of mindfulness into constituent measurable components. Although there are many conceivable ways to partition mindfulness, one organizing heuristic within contemporary psychological science is to operationalize mindfulness in four distinct ways: as a dispositional trait, a psychological state, a skill (or level of expertise) acquired through long-term practice and experience, and as the prospective outcome of training. As depicted in Fig. 1, these distinctions highlight a crucial point—that how mindfulness is operationalized inextricably shapes how it is measured (which will be reviewed in the sections to follow) and ultimately understood through the lens of scientific investigation.

It is worthwhile to briefly consider the distinctions among the four different ways that mindfulness has been operationalized. Dispositional trait mindfulness refers to an individual's natural propensity to be mindful. Consequently, trait mindfulness has been considered to be a stable personality characteristic that varies across people. State mindfulness pertains to the degree that an individual is mindful at a singular time point; as such, it is subject to temporal and situational variation. Mindfulness skill refers to the set of skills and level of expertise that an individual has acquired as a function of sustained and long-term mindfulness practice. Lastly, mindfulness training involves prospective practice, during which contemplative techniques are deliberately applied to cultivate the attitudinal qualities of mindfulness. Mindfulness training can span formal meditation practice (e.g., meditation retreats) to MBIs (e.g., mindfulness-based cognitive therapy; MBCT), which encompass a series of instructor-led classes involving didactic and experiential exercises aimed at developing and applying mindfulness skills toward therapeutic goals (e.g., mood regulation), and within various population cohorts (e.g., depressed patients). From a practical perspective, these operationalizations are by no means mutually exclusive entities, but are rather interrelated, nested, and interactive in real-world contexts. For example, prolonged engagement of state mindfulness can be construed as mindfulness training, which is widely thought to enhance trait mindfulness. Greater trait mindfulness may in turn be conceptualized as the propensity to which individuals are inclined to engage state mindfulness and utilize mindfulness skills. However, from a scientific perspective, these four different aspects of mindfulness are easily conflated, which may obscure accurate measurement, and moreover, impede research efforts to delineate precise effects and mechanisms. Consequently, the chief incentive for separating mindfulness into constituent operationalizations is to render it more amenable for scientific investigation.

Despite the many strengths and contributions of this approach, differential operationalization has led to the proliferation of separate and increasingly balkanized lines of research. Potentially even more problematically, this approach can foster an ethos of insularity that compartmentalizes progress within the constructed operational boundaries. Although much knowledge has been advanced *within* each operational facet of mindfulness, there is far less known about what can be gleaned by investigations that cut *across* the facets. This is plainly evident when considering how cognitive mindfulness studies are synthesized within the review literature. Using the aforementioned reviews as an example, we observe that Cásedas et al. (2020), Chiesa et al. (2011), Gallant (2016), Im et al. (2021), Lao et al. (2016), Yakobi et al. (2021), and Prakash et al. (2020) only included studies involving multisession MBIs, whereas both Leyland et al. (2019) and Gill et al. (2020) exclusively examined brief mindfulness induction studies. Moreover, reviews from other areas have centered around studies of dispositional mindfulness (e.g., Tomlinson et al., 2018) and mindfulness skill (e.g., Luders & Kurth, 2019), to the exclusion of other operationalizations. In short, how mindfulness is operationalized shapes not only how research is conducted, but also how it is collated and consumed. It appears that an unintended consequence of differential operationalization is that it has rendered an increasingly narrow lens through which to understand the relationship between mindfulness and cognitive function.

With that said, assuming that constituent operationalizations of mindfulness are extensions of the same construct, then it appears reasonable and potentially valuable to examine the intersection or common effects observed across operational divides. This is integral to the cognitive psychology concept of converging operations, simply defined as the use of multiple approaches to address a common research question.[1] Belying this simplicity, however, is the critical caveat that although converging operations are typically understood to refer to the use of multiple methods to produce a more balanced and nuanced research strategy, the actual utility of the approach is predicated on careful evaluation and assimilation of the data and findings obtained across diverse methods and operationalizations. Without intentional assessment of the interconnection between methods, different approaches can quickly divide and devolve into parochialism. Consequently, our main contention here is that mindfulness research could stand to benefit from a systematic review of the extent of complementarity, consistency, or *convergence* among findings as a function of operationalization. Below, we begin to concretize this perspective by reviewing the research strategies most commonly associated with the four common operationalizations of mindfulness mentioned above, including the strengths and limitations of prevalent experimental manipulations and assessment measures (see Fig. 1 for a roadmap of the review). For illustrative purposes, we focus on the Stroop and flanker task, two classic task paradigms that have been widely used to study cognitive control and are readily amenable to behavioral, neural, and multimodal biobehavioral methodologies.

At the end of every section, we briefly collate and summarize studies to demonstrate how assessment of convergence can be used to synthesize the extant mindfulness cognitive control literature (see Table 1). The primary aim of this review is to highlight how differential operationalization has influenced the prevailing research strategies of the field. Therefore, description and analysis of study findings, which can be found elsewhere (e.g., Cásedas et al., 2020), will be kept to a minimum. To further maintain focus and tractability, we limit the scope of the review to studies involving adult samples. Lastly, although many other tasks could be included here, the selection of a standardized task battery (i.e., Stroop and flankers) is critical for demonstrative purposes, as it fixes the dependent variables of interest and enables the most straightforward evaluation of convergence. The key idea is that in a converging operations approach, the outcome variable and its measurement modality are controlled (but not necessarily always fixed), while operationalization of mindfulness is free to vary, thus enabling the parsing of outcome variability as a specific function of how mindfulness is operationalized and measured.

Before beginning this brief review, it is important to acknowledge that the Stroop and flanker task are not static paradigms. Indeed, many different behavioral metrics can be computed from the two tasks; consequential differences between the task parameters being utilized—both between the Stroop and flanker and also within each task—are common across studies (e.g., Tillman & Wiens, 2011; and see Rouder & Haaf, 2019 for a review). Moreover, understanding how cognitive control emerges within these paradigms is continually evolving, and remains an active area of research in and of itself (e.g., Bugg et al., 2008; Bugg & Gonthier, 2020). In

[1] Note that this definition is a broader expansion of the original definition by Garner et al. (1956), in which converging operations were defined as "any set of two or more experimental operations which allow the selection or elimination of alternative hypotheses or concepts which could explain an experimental result" (p. 150–151). However, as we will describe further, even this narrower original definition is still appropriate for the research strategy that we advocate for testing specific hypotheses involving mindfulness and cognitive function.

Table 1 Possible research designs involving different combinations of mindfulness operationalizations, catalogued by reviewed studies utilizing the flanker or Stroop task as dependent measures of interest

Possible Designs	Operationalization				Representative Studies (Used Measure/Manipulation)	Significant Findings
	Dispositional Mindfulness	*State Mindfulness*	*Mindfulness Skill*	*Mindfulness Training*		
1	yes	no	no	no	**Anicha et al.,2012**; Di Francesco et al., 2017; **Jaiswal et al.,2018**; **Lin et al.,2019** (all self-report)	3/4 (75%)
2	no	yes	no	no	**Keng et al.,2017** (instructional engagement); Larson et al., 2013(induction);**Norris et al.,2018** (induction)	2/3 (67%)
3	no	no	yes	no	**Chan & Woollacott,2007**; **Jo et al.,2017**; **Kozasa et al.,2012**; **Van den Hurk et al.,2010** (all cross-sectional)	4/4 (100%)
4	no	no	no	yes	**Allen et al.,2012**(meditation);**Becerra et al.,2017**(meditation);**Braboszcz et al.,2013**(meditation); Bueno et al., 2015(MBI);**Elliott et al.,2014**(meditation);**Fan et al.,2014**(meditation);**Kwak et al.,2020**(meditation);**Tang et al.,2007**(meditation);**Verhoeven et al.,2014**(MBI);**Zylowska et al.,2008** (MBI)	9/10 (90%)
5	yes	yes	no	no	Bing-Canar et al., 2016(induction);**Keng et al.,2013** (instructional engagement); Lin et al., 2019 (induction)	1/3 (33%)
6	yes	no	yes	no	**Andreu et al.,2017**; **Teper & Inzlicht,2012**; **Moore & Malinowski,2009**; Bailey et al., 2019 (all cross-sectional)	3/4 (75%)
7	yes	no	no	yes	**Ainsworth et al.,2013**(meditation); Anderson et al., 2007(MBI); Esch et al., 2017(meditation);**Huang et al.,2019**(MBI); Jensen et al., 2012(MBI); Josefsson et al., 2014(MBI);**Oken et al.,2010**(MBI); Oken et al., 2017(meditation);**Rodriguez Vega et al.,2014**(MBI);**Shields et al.,2020**(meditation); Tang et al., 2020 (MBI); Zhu et al., 2019 (MBI)	5/12 (42%)
8	no	yes	yes	no	No data	
9	no	yes	no	yes	No data	
10	no	no	yes	yes	Jha et al., 2007* (**cross-sectional**) (MBI)(meditation)	1/3 (33%)
11	yes	yes	yes	no	No data	
12	yes	yes	no	yes	No data	
13	yes	no	yes	yes	No data	
14	no	yes	yes	yes	No data	
15	yes	yes	yes	yes	No data	

Note. Studies reporting significant improvement in behavioral flanker/Stroop outcome as a function of the primary mindfulness manipulation (as denoted in parentheses) are bolded and aggregated relative to the total number of studies.* This study employed a hybrid cross-sectional longitudinal design involving MBI and meditation retreat participants. Only the baseline cross-sectional analyses comparing experienced meditators to novices yielded significant findings.

other words, methodological variability is not limited to operationalization of mindfulness but extends to how cognitive control is measured as well. Therefore, assessment of convergence is essentially bidirectional and may require careful consideration for how both constructs are operationalized. Nevertheless, for our purposes below, we simplify measurement of cognitive control to the Stroop and flanker task and to the standard metrics of assessment in order to spotlight the role of converging operations as it pertains to mindfulness research.

After completing the review, we will draw upon the evaluated research strategies to demonstrate how converging operations can be applied to systematically guide future research. There, we show how modular integration of measures and manipulations across separate operationalizations of mindfulness is achieved via the use of factorial designs, and illustrate how this strategy may lead to a more cohesive and programmatic research approach. We conclude by discussing the key caveats of the framework, including the unique complexity of cognitive control, how studies of mindfulness can advance understanding of cognitive control, and the general challenge of studying two multifaceted constructs in tandem.

Dispositional mindfulness

Self-report inventories

As scientific interest in mindfulness accelerated at the advent of the 21st century, substantial efforts were directed toward developing and refining individual difference measures of dispositional mindfulness. Naturally influenced by personality psychology, these endeavors manifested in the now-common practice of assessing trait mindfulness via self-report questionnaires. Unsurprisingly, the definitional and nomological complexity of mindfulness engendered different approaches toward scale construction, culminating in the creation of a multiplicity of measurement inventories (see Rau & Williams, 2016, for a review). Our focus here is not to evaluate differences between measures (cf. Baer, 2019; Park et al., 2013; Rau & Williams, 2016), but rather to assess the collective utility of self-report measures of dispositional mindfulness for evaluation of cognitive effects.

Dispositional mindfulness scales have been instrumental in advancing understanding of the salutary properties of mindfulness on psychological functioning. Indeed, an impressive and continuously expanding body of research has linked dispositional mindfulness to a wide range of adaptive functioning in the cognitive domain (Noone et al., 2016; Riggs et al., 2015), as well as to other domains, such as emotional (Tomlinson et al., 2018) and social functioning (Donald et al., 2019). In addition to illuminating the relationship among mindfulness and various facets of psychological well-being, dispositional mindfulness measures exhibit sensitivity to mindfulness training and have been used to assess the efficacy and underlying mechanisms of MBIs (Gu et al., 2015; Khoury, Lecomte, Fortin, et al., 2013a; Quaglia et al., 2016). From a logistical perspective, these measures are quick and cost effective, minimizing burden to experimental design by requiring only the addition of one or more questionnaire measures to cognitive assessment.

Despite these contributions and advantages, measuring dispositional mindfulness via self-report is not without its caveats or limitations. In fact, the approach has been subject to incisive criticism since its inception, with scholars highlighting problems involving semantic ambiguity, measurement inequivalence, susceptibility to demand characteristics, and intermeasure heterogeneity (Bergomi et al., 2013; Christopher et al., 2009; Grossman, 2008, 2011; Van Dam et al., 2009; Van Dam et al., 2010). At the crux of these issues is whether individuals can accurately understand and reliably respond to scale items in concordance with researchers' working definition of mindfulness; and the extent to which familiarity with meditation or susceptibility to social desirability factors might bias responding (e.g., see Van Dam et al., 2009). Such concerns are supported by meta-analytic findings showing that although MBIs generally increase self-reported mindfulness, comparison with active controls shows little to no specific advantage for MBIs (Visted et al., 2015).

With respect to cognitive control, studies utilizing the Stroop or flanker have generally linked higher dispositional mindfulness with better control across measures of Stroop accuracy, (Jaiswal et al., 2018), Stroop interference cost (Anicha et al., 2012), and brain and behavior measures of flanker interference (Lin et al., 2019). With that said, Di Francesco et al. (2017) found that higher dispositional mindfulness was associated with slower overall response times (RTs) across flanker trials during a tone variant of the Attention Network Task, challenging the general notion that trait mindfulness is related to enhanced attention efficiency. Such discrepancy may speak in part to the longstanding issues in self-report assessment, insofar that scales do not always yield consistent findings when applied across different samples and task variants (Bergomi et al., 2013; Grossman, 2019).

State mindfulness

Self-report inventories

State mindfulness is likewise measurable via self-report. Example measures include the State-MAAS(Brown & Ryan, 2003), Toronto Mindfulness Scale (TMS; Lau et al., 2006), and State Mindfulness Scale (SMS; Tanay & Bernstein, 2013). Despite their differences (see Tanay & Bernstein, 2013), construction of these scales is predicated on a mutual recognition that mindfulness can refer to a dynamic psychological state, and as such is not limited to assessment of stable traits or static patterns of behavior. State mindfulness scales have been used to examine popular but untested theoretical assumptions about the nomological network of mindfulness. For example, Kiken et al. (2015) found empirical support for the widespread assumption that increases in state mindfulness during meditation practice would lead to greater trait mindfulness and psychological well-being over time. State mindfulness inventories have also been employed as quality control metrics for evaluating novel mindfulness-based clinical applications (Luberto & McLeish, 2018), as well as online mindfulness training modalities (Mahmood et al., 2016). Collectively, these studies demonstrate the utility in operationalizing state mindfulness as a unique component of mindfulness, showing that state mindfulness is not only measurable but is inextricably linked to mindfulness training and functional outcomes. With that said, self-report measures of state mindfulness share many of the same constraints and limitations as dispositional trait measures. Again, many problems have been raised around the central issue of whether an individual, particularly those inexperienced with mindfulness, can accurately report the nature and quality of their own state of mindfulness.

These problems may be a primary reason why state mindfulness measures have not frequently been utilized in studies of mindfulness and cognitive function. We were unable to identify any studies that examined the flanker or Stroop task in relation to self-reported state mindfulness. The disparity in the sparse utilization of state, relative to trait mindfulness measures in cognitive studies may reflect the broader issue that neither the theoretical distinction nor expected convergence between state and trait mindfulness is clearly demonstrable via self-report measures (Bravo, Pearson, Wilson, & Witkiewitz, 2018). This finding has tended to drive investigators to standardly select one measure over the other. Although it is important and relatively easy to differentiate state and trait mindfulness conceptually, distinguishing them empirically relies on proper use of psychometrically sound measures with sufficient discriminative validity. It remains an open question whether self-report assessment is well suited to capture meaningful variability between trait and state mindfulness. Nonetheless, developing the ability to accurately assess situational, training-related, and general temporal fluctuations in state mindfulness (and ideally in ways separable from trait mindfulness) appears vital for advancing the field.

Instructional engagement

Toward this end, self-report assessment is not the only viable method to investigate state mindfulness. State mindfulness can also be subject to experimental manipulation, during which participants are explicitly instructed to engage in a mindful state as they complete a task. Task outcomes are then compared between the state mindfulness condition and a control condition. Although infrequently used in the cognitive literature, instructional engagement of state mindfulness has featured more prominently in eating behavior and affective picture viewing studies aimed at understanding the self and emotion regulatory properties of mindfulness. For example, several studies have shown that adoption of state mindfulness during food viewing and tasting (i.e., mindful eating) leads to reduced caloric intake and healthier food preferences (Allirot et al., 2018; Hendrickson & Rasmussen, 2017; Mantzios et al., 2019; Papies et al., 2012). In the domain of emotion regulation, Uusberg et al. (2016) found that when participants were instructed to view negative pictures mindfully, EEG measures of emotional reactivity were found to be attenuated, and this pattern occurred over and above the effects of habituation and distraction. Indeed, a key advantage of the approach is that it is particularly amenable to investigation of concurrent effects of state mindfulness through the use of third-person neurobiological assessment modalities, such as EEG and fMRI.

As mentioned before, however, this approach appears sparsely used in the cognitive mindfulness literature. Nevertheless, we identified two relevant studies that utilized the Stroop task—although, in these designs, state mindfulness was induced during emotion regulation and reward exposure conditions *prior to*, rather than during, actual Stroop task performance. The studies compared the cognitive costs of mindfulness and cognitive reappraisal as emotion regulation strategies, finding that instructions to engage in state mindfulness in response to a negative mood induction produced less Stroop interference in mildly depressed patients (Keng et al., 2013) and healthy individuals (Keng et al., 2017). Importantly, this paradigm demonstrates that modulation of state mindfulness can exert meaningful effects on outcomes. As such, in a point we discuss in detail later, this work highlights the importance for future research to examine the role of state mindfulness in cognitive control task performance, which may hold promise in facilitating the decomposition of state and trait mindfulness into dissociable components of variation.

Brief meditation inductions

Another way to manipulate and assess state mindfulness involves the use of brief guided mindfulness meditation, commonly referred to in the literature as mindfulness inductions (see Gill et al., 2020; Leyland et al., 2019, for two recent meta-analyses). Importantly, the distinction between a mindfulness induction and the instructional engagement described in the prior section is that, here, participants undergo a brief guided practice, lasting typically from 5 to 20 minutes, to help bring them into a state of mindfulness prior to a task, whereas in the former, participants are only given explicit instructions to adopt a mindful state *during* task completion, without the addition of a guided practice.

The steady rise of mindfulness induction studies speaks in part to the unique advantages of the approach. In addition to the logistical and pragmatic benefits associated with conducting mindfulness investigations within a single laboratory session (which include the adoption of within-subject designs), mindfulness inductions offer exceptional control, in that guidance is given in a specific meditation technique to examine its acute after-effects. Indeed, one of the core distinguishing features of the mindfulness induction is the level of specificity with regard to the cultivation of a singular and specific state, as opposed to an aggregation of multiple mindfulness techniques or training modalities. The utility of this distinction becomes apparent when considering the fact that there is technical variation among mindfulness practices. For example, focused attention (FA), open monitoring (OM), and loving-kindness(LK) meditation are three empirically distinguishable meditative practices, yet they are often subsumed together under the umbrella term "mindfulness meditation" (Fox et al., 2016; Lutz et al., 2008; Manna et al., 2010). Therefore, induction designs enable more targeted investigation of specific states associated with different meditation techniques. A final advantage is that the inherent brevity of mindfulness inductions minimizes placebo effects and demand characteristics related to repeated training, group participation, or prolonged exposure to implicit beliefs/social attitudes towards mindfulness. Consequently, mindfulness induction studies can be conducted with novice nonmeditators, thus affording a unique opportunity to examine how individuals respond to a "first exposure" of mindfulness. As such, this approach is naturally positioned to advance efforts toward predicting responsivity to longer-term meditation practice or MBIs.

Paradoxically, the strength of mindfulness inductions is also its limitation: A single controlled session of guided meditation is not a valid substitute for more extended mindfulness training and is unlikely to occur in a vacuum naturalistically (hence, why we categorized mindfulness inductions within the state mindfulness operationalization as opposed to mindfulness training). With that said, we identified several induction studies that include the Stroop or flanker task, most of which employed EEG paradigms to assess brain activity during task performance immediately following a mindfulness induction. Interestingly, these studies have consistently reported null effects on behavioral performance (Bing-Canar et al., 2016; Larson et al., 2013; Lin et al., 2019; but see Norris et al., 2018, for an example involving moderation by personality factors), while at the same time observing modulation in performance-related EEG activity (Bing-Canar et al., 2016; Larson et al., 2013; Lin et al., 2019). This pattern indeed suggests that a single brief induction may be insufficient to alter behavioral performance, but can nonetheless affect neural processing, with the implication that sustained training may be necessary before mindfulness-induced plasticity translates to behavioral change.

Mindfulness skill

Cross-sectional comparison

One way to estimate the effects occurring from longer-term development of mindfulness skills on psychological, cognitive, and behavioral functioning is to conduct cross-sectional comparisons between experienced meditators and novice controls. With the well-known caveat that such designs are ill-equipped to draw causal inferences, cross-sectional studies are quite popular, and have generally shown that experienced meditators exhibit superior cognitive and emotion regulatory abilities as well as functional and structural neuroanatomical differences relative to novices (Andreu et al., 2017; Brefczynski-Lewis et al., 2007; Luders & Kurth, 2019; Sobolewski et al., 2011; Taylor et al., 2011). Despite shedding light on the possible effects of long-term practice, there are two notable limitations to this approach. First, the amount of training experience that constitutes "expertise" or "experienced" is arbitrary and can vary dramatically across studies. Second, the *type* of meditation experience can be highly variable given the inherent diversity represented in most contemplative traditions. Unless stringent selection criteria are applied, such heterogeneity can impede the ability to accurately link observed effects to specific skills or training modalities.

Interestingly, cross-sectional studies support the aforementioned suggestion that single session inductions may be too brief to modulate behavioral performance of cognitive control. Several studies involving the Stroop and flanker have reported better accuracy (Andreu et al., 2017; Jo et al., 2017; Teper & Inzlicht, 2012; Van den Hurk et al., 2010) and reduced congruency interference (Chan & Woollacott, 2007; Jha et al., 2007; Kozasa et al., 2012; Moore & Malinowski, 2009) in experienced meditators relative to controls. Importantly, three of the aforementioned studies recorded EEG, reporting that meditators exhibited larger error-related negativity (ERN) amplitudes following errors (Andreu et al., 2017; Teper & Inzlicht, 2012), and that task performance was accompanied by enhanced theta phase synchrony between the medial prefrontal cortex and motor cortex (Jo et al., 2017). Moreover, an fMRI study found that experienced meditators exhibited no accuracy decrements between incongruent and congruent Stroop trials and displayed less activation in attention control brain regions during conflict processing relative to novice controls (Kozasa et al., 2012). Collectively, these studies suggest that meditators possess superior conflict monitoring abilities. Although convincing, it is important to note that a more recent study, which used novel whole-scalp EEG analysis, reported no behavioral or neural differences between meditators and controls (Bailey et al., 2019). A final note is that although mindfulness skill has primarily been estimated cross-sectionally, and typically based on self-reported hours of practice, efforts are underway to derive performance-based measures of mindfulness skills with adequate psychometric properties, such that these can be utilized as alternative means of skill assessment in experimental designs (Hadash & Bernstein, 2019; Levinson et al., 2014).

Mindfulness training

Longitudinal assessment of structured interventions

Studies of mindfulness training typically involve longitudinal repeated assessment (i.e., pre-interventnion vs. post-intervention) to measure the effects of MBIs. Belying this simplicity, however, is the considerable ambiguity in defining what constitutes a MBI (Cullen, 2011). Since the inception of Jon Kabat-Zinn's seminal Mindfulness-Based Stress Reduction (MBSR), there has been a proliferation of interventions that contain mindfulness as part of their programming. However, significant differences in the teaching, training, and application of mindfulness make it challenging to obtain a precise standard for which to apply to MBIs. One popular criterion is that MBIs must emphasize mindfulness meditation as the core interventional component, with personal practice as an essential part of the curriculum (Goldberg et al., 2018; Klingbeil et al., 2017; Strauss et al., 2014; Vago et al., 2019). Under these qualifying circumstances, several well-established psychotherapeutic interventions that otherwise emphasize mindfulness, such as acceptance and commitment therapy (Hayes et al., 1999) and dialectical behavior therapy (Linehan et al., 1993), are not considered MBIs. Although this criterion appears to be increasingly adopted, defining the parameters of an MBI is a far from settled issue, and continues to remain quite variable in the literature (e.g., Godfrey et al., 2015; Khoury, Lecomte, Gaudiano, & Paquin, 2013b; Virgili, 2015). Given the centrality of meditation practice in the mindfulness and contemplative science literature, the following discussion is based on defining MBIs as structured interventions that focus on mindfulness meditation.

Although MBIs have spurred the growth of mindfulness in science, public health, and many other societal domains, they have done so in part through the rapid development of novel mindfulness-based applications, including mobile and online delivery modalities. Such proliferation can contribute to definitional erosion and undermine standardization, resulting in a sprawling and increasingly heterogenous literature. These issues are compounded by the fact that MBIs inherently contain a multiplicity of meditation practices, didactic exercises, and social activities that challenge the elucidation of specific treatment factors (Britton et al., 2018). Moreover, the other three operationalizations of mindfulness (dispositional mindfulness, state mindfulness, mindfulness skill) can be, and often are subsumed within MBIs; as such, they cannot be easily dismantled. Together, the expanding array of treatment targets and assortment of therapeutic factors associated with MBIs represent a considerable challenge against mechanistically focused investigations.

It is therefore perhaps unsurprising that research examining the effect of MBIs on cognitive control have yielded mixed results. Several studies reported no change in behavioral performance on the Stroop task after MBI training relative to active (Jensen et al., 2012; Josefsson et al., 2014) and waitlist controls (Anderson et al., 2007; Zhu et al., 2019). Similar null findings have been reported for flanker performance on the ANT—neither MBSR nor a 14-session college mindfulness seminar improved performance relative to controls (Jha et al., 2007; Tang et al., 2021). Yet other studies focusing on specialized cohorts have found MBI-related cognitive control benefits, including improved Stroop performance in training psychotherapists (Rodriguez Vega et al., 2014), dementia caregivers (Oken et al., 2010), formerly depressed patients (Verhoeven et al., 2014), and bereaved individuals (Huang et al., 2019). Lastly, Zylowska et al. (2008) reported improved flanker performance on the ANT after an 8-week MBI adapted for individuals with ADHD; whereas Bueno et al. (2015)—using the same intervention and task—observed nonspecific improvement across both MBI and control participants, while noting that performance was not initially impaired in the ADHD group at baseline. Taken together, this caveat is in line with the emerging picture that regarding change in cognitive control measures, individuals with preexisting stress or clinical vulnerabilities may exhibit higher responsivity to MBIs than that observed in healthy young adults.

Longitudinal assessment of meditation practice

Investigations into the effects of mindfulness meditation training do not have to be circumscribed to either one-session inductions or embedded within broader MBIs, but can also involve longer-term training of a "standalone" meditation practice. Importantly, longitudinal meditation studies have the potential to generate inferences beyond an "initial exposure" to mindfulness practice and are better equipped to test more nuanced theories associated with specific meditative techniques, relative to standard MBI studies. For example, recent work by Yoshida et al. (2020) showed that 8 weeks of FA meditation practice, relative to relaxation training, uniquely enhanced EEG signatures of attention across both meditation and performance of an auditory oddball task. Exemplifying the advantages of this approach, the authors were able to link modulation of top-down, but not bottom-up attentional processing, to the specific training of FA. Moreover, in a direct comparison of FA and OM meditation, Ainsworth et al. (2013) reported that both practices reduced flanker interference on the ANT after 3 training sessions. Although a promising start, studies involving longer training durations and higher practice intensity may be needed to thoroughly parse the effects of specific meditative practices.

Toward this end, retreat studies, during which participants complete repeat assessments while undergoing daily intensive meditation training at a residential retreat center, are particularly well suited (King et al., 2019). As opposed to relying on cross-sectional designs involving experienced meditators, retreats afford rare access to prospectively investigate the upper bounds of practice duration and intensity. Broadly speaking, retreat investigations have mostly yielded findings of enhanced attentional ability (Braboszcz et al., 2013; Elliott et al., 2014; MacLean et al., 2010; Slagter et al., 2007; Zanesco et al., 2013), though there are emerging efforts to examine emotional processes as well (Blanco et al., 2020). Although retreat designs have produced strong evidence in support of training-related neuroplasticity (e.g., reduced duration of attentional blink; Slagter et al., 2007), questions remain on how best to obtain adequate controls and whether effects are generalizable outside the retreat setting.

Longitudinal meditation investigations involving the Stroop and flanker have yielded mostly consistent results, with several studies reporting meditation related improvement across Stroop (Fan et al., 2014) and flanker performance (Becerra et al., 2017; Tang et al., 2007). An fMRI study obtained similar results, reporting that 6 weeks of mindfulness meditation training reduced Stroop interference and enhanced prefrontal brain activation during executive processing relative to active controls (Allen et al., 2012). In contrast to these findings however, five 1.5-hour training sessions of mixed mindfulness meditation exercises did not improve flanker performance on the ANT (Esch et al., 2017); and 6 weeks of mindfulness meditation training in older adults did not alter Stroop or flanker performance (Oken et al., 2017).

Retreat studies involving intensive meditation practice have also produced relatively consistent findings, reporting that retreat participation improved Stroop (Braboszcz et al., 2013) and flanker performance (Elliott et al., 2014; Shields et al., 2020). Complementing these findings, a recent fMRI study found that retreat participants exhibited reduced flanker interference and increased ACC and DLPFC activation during conflict resolution after 4 days of intensive meditation relative to controls (Kwak et al., 2020). In contrast to these findings however, flanker performance on the ANT did not differ among individuals who completed a 1-month retreat, MBSR, or non-intervention control in the previously mentioned study by Jha et al. (2007).

Although longitudinal meditation studies appear to produce relatively more consistent findings, the approach is not without its unique caveats and limitations. As evident from the research reviewed above, the duration and intensity of meditation training can vary dramatically across studies and there does not appear to be a common "standard" training interval as with MBIs (e.g., 8 weeks). Furthermore, despite being inherently more amenable to the investigation of specific forms of meditation, longitudinal meditation studies often include representation from a wide range of contemplative traditions. Even though these approaches are ostensibly unified by the common goal of cultivating mindfulness, they can contain consequential discrepancies in the type (e.g., FA vs. OM) and modality (e.g., walking vs. sitting) of meditation training. Therefore, future efforts to increase standardization within this promising research strategy may further amplify and strengthen its unique advantages.

Converging operations as a guide to research

The studies reviewed above show that although there is some converging evidence supporting a salutary relationship between mindfulness and cognitive control, the exact nature of this relationship is unclear. In particular, even when the experimental tasks of interest were constrained to the Stroop and flanker, disparate findings were observed both across and within different operationalizations of mindfulness. Although a clever adaptation to the problems posed by construct heterogeneity, two unintended consequence of differential operationalization is the proliferation of methodological variability and the increasingly disparate lines of research accompanying it. Consequently, it is unclear how findings obtained from one operationalization relate to, or generalize to, discoveries from another. This raises significant questions about whether inferences generated at the operational level speak more about the broader construct of mindfulness per se, or instead to the specific methods that undergird their measurement. As it stands, it is difficult to envision how the field can advance toward a more holistic understanding of mindfulness without reconsidering its collective approach to research.

Toward this end, we have synthesized the aforementioned research strategies associated with each operationalization of mindfulness to create a taxonomic framework that can be used to guide the development and execution of a more integrated and standardized investigative approach (see Fig. 1). The driving principle behind the application of this framework is *modularity*, insofar that each operationalization can serve as *separate* yet *additive* components of investigation. Applying the logic of factorial design, methods from one operationalization can be either directly combined with methods from other operationalizations within a single study or leveraged successively across a series of studies—the key is that both approaches purposively account and assess for convergence across operations by treating them as experimental factors. Here, it is important to point out that although the literature already includes single studies containing multiple operationalizations, this source of methodological variability has yet to be explicitly recognized and appropriately organized within a structuring framework predicated on factorial designs.

Consequently, we have charted different study designs involving various combinations of operationalizations (i.e., factors) based on our taxonomic framework. From this framework, we can categorize each of the studies reviewed above accordingly (see Table 1). By tabulating the extant literature in this way, we begin to see that the strength of the relationship between mindfulness and cognitive control is likely to vary as a function of how and in whom mindfulness is measured (e.g., cross-sectional studies of mindfulness skill comparing experienced meditators to novice controls yield relatively higher rates of significant outcomes). As can also be observed, single or two factor designs (often involving measurement of dispositional mindfulness to control for group differences in experimental studies) are common, but many combinations that include three or four factors in the design have yet to be used. The main point is that by embracing the modular nature of the proposed taxonomy, researchers can flexibly combine different operationalizations to answer questions in a systematic and potentially novel, analytically incisive way. Importantly, the approach does not inherently value any specific operationalization(s) above others but instead embraces factorial logic to treat them as experimental factors that can be effectively manipulated to test different hypotheses. With that said, however, it is prudent to acknowledge that some operationalizations may be more relevant or better suited to address certain kinds of research questions. Indeed, the strategy for determining how best to prioritize and organize different operationalizations of mindfulness is a critical one that warrants further consideration. Nevertheless, a starting recommendation that we make is that researchers investigating mindfulness effects on cognition think about potential research designs from within this taxonomic framework, so that they can make an informed decision about how best to categorize and position their work.

Barring any theoretical or logistical motivations that necessitate a particular preference or order, our key recommendation is to treat state mindfulness as the foundational operationalization from which to build and expand inquiry. Conceptually, state mindfulness is the component of mindfulness that is most directly phenomenological and therefore can serve as the experiential basis that grounds all other operationalizations. Methodologically, manipulation of state mindfulness via direct experimental control can likewise serve as a regularizing force that standardizes variability related to other operationalizations of mindfulness. For example, beginning a laboratory experiment with a brief mindfulness induction, followed by instructional engagement to sustain the target mindful state into task performance, standardizes the extent to which participants are actively (and properly) adopting mindfulness during the task, while also affording enhanced experimental control over the type of mindfulness technique employed (e.g., FA vs. OM).

Importantly, variability in the propensity to be mindful and the specific technique of mindfulness adopted during actual task performance represent subtle, but potentially confounding experimental factors that could meaningfully account for outcome variability. Moreover, these factors are likely to differ systematically across people with varying levels of dispositional mindfulness, mindfulness skill, and mindfulness training. Therefore, another reason to manipulate state mindfulness as a baseline experimental procedure is to account for this potential covariation. In particular, both main effects and potential interactions between factors can be explored with respect to the distinct operationalizations of mindfulness. A final reason to center state mindfulness manipulations followed by experimental assessment of cognitive effects is that state mindfulness can be independently assessed, either via self-report indices or through other passive physiological or neurophysiological monitoring (e.g., pupillary oscillations, heart-rate variability, EEG, fMRI, etc.). These measures of state mindfulness can then be directly linked with any observed cognitive effects, which provides additional leverage over variability (i.e., individual differences in state mindfulness quality could meaningfully account for variability in subsequent cognitive effects).

Indeed, it is straightforward to expand the investigation to encompass multiple operationalizations by simply adding factors, using the appropriate measures or manipulations, to the research design. To better illustrate this, we created two example studies that incorporate additional factors to the state mindfulness manipulation described above. Because we did not identify any extant studies that contained more than two factors in our review (see Table 1), we present three-factor designs in order to highlight the utility and flexibility of the approach. The first study involves assessment of state mindfulness, mindfulness skill, and dispositional mindfulness in the context of a standard flanker paradigm (see Fig. 2). We use this study to briefly outline the basic design features of the approach and offer an intuitive statistical approach for modeling the data from which to build upon. The second study involves examining the interplay among state mindfulness, mindfulness training, and dispositional mindfulness using a more complex multisession, multimodal assessment battery (see Fig. 4). There, we expand discussion to highlight the role of cognitive neuroscience methods, review multimodal analytic possibilities, including the implications associated with different possible findings, and evaluate the potential impact of the approach in terms of its explanatory power for addressing outstanding challenges in mindfulness research and MBI studies in particular.

Example Study 1

For the first study example, we leverage a cross-sectional experimental approach, manipulating mindfulness skill by randomizing expert and novice meditators to either a brief FA induction or control condition, followed by instructions to maintain the FA or control state into performance of a hypothetical flanker task. The session concludes with all participants completing a trait mindfulness measure to assess dispositional mindfulness. The chief advantage of this design is that it enables the estimation and parsing of both the main effects as well as interactions among the three factors.

Although there are many viable statistical methods to model data from this design, we recommend a regression-based approach due to its analytic flexibility in estimating variance associated with specific predictors and interaction terms. For example, a linear mixed-effects regression could model trial-level task data as within-subject predictors (e.g., incongruent vs. congruent trial type), mindfulness skill (expert vs. novice), and state mindfulness (FA vs. control) as between-subject categorial predictors, and dispositional mindfulness (trait mindfulness scores) as a continuous predictor. Using this approach, one possible way to explicitly test the degree of convergence is to assess the amount of unique variance accounted for by each operationalization. For example, "full convergence" may be evidenced statistically if each of the three factors can independently predict flanker performance (after controlling for the other two factors). However, it is not always necessary or reasonable to expect full convergence, since the operationalizations are conceptually interrelated. Therefore,

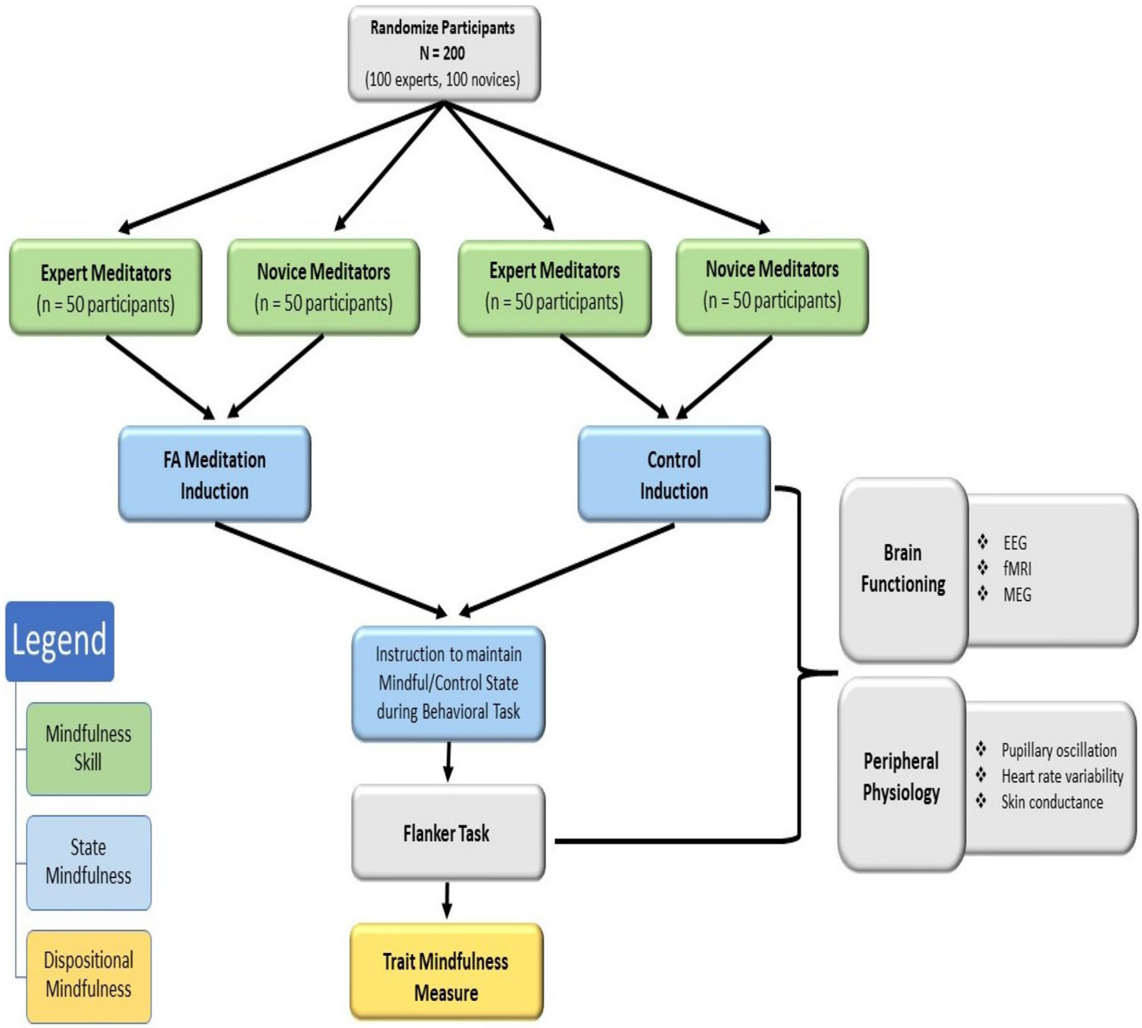

Fig. 2 Example study design involving three operationalizations of mindfulness: mindfulness skill (green), state mindfulness (blue), dispositional mindfulness (yellow). Neurobiological and psychophysiological measures can be applied during the induction and behavioral task phase

the data may be better fit by explicitly modeling the interactions between the factors. To illustrate these possibilities more clearly, we present a bar graph of hypothetical results depicting plausible main and interactive effects from our example study (see Fig. 3).

Speaking to the versatility of the approach, it is worth mentioning that the FA induction from the example study could be easily replaced with another induction (e.g., OM), or additional inductions could be added to the existing two conditions for further comparison. The key point is that experimental manipulation of state mindfulness prior to task performance allows researchers to flexibly compare and control for different mindfulness/meditative techniques of interest. Furthermore, the basic design is naturally amenable to measurement across multiple levels of analysis and is particularly compatible with neural or psychophysiological measurement. For example, EEG, fMRI, or peripheral physiology measures could be recorded during the state inductions and into flanker task performance—enabling investigation of the neurophysiological mechanisms linking specific mindfulness states with cognitive control.

Example Study 2

Likewise, different operationalizations can be substituted, added, or subtracted as factors to fit the needs of the research question. The second example study (Fig. 4) in particular, demonstrates how the approach can be leveraged to accommodate longitudinal assessment of mindfulness training. Although more resource intensive, this can be accomplished by embedding the state mindfulness inductions into the assessment phases (e.g., pre, post, follow-up) of a standard RCT design. Insofar that the core training component of MBIs is meditation practice, the brief inductions naturally serve as longitudinal data samples of meditation training across the intervention, highlighting the analytic advantages

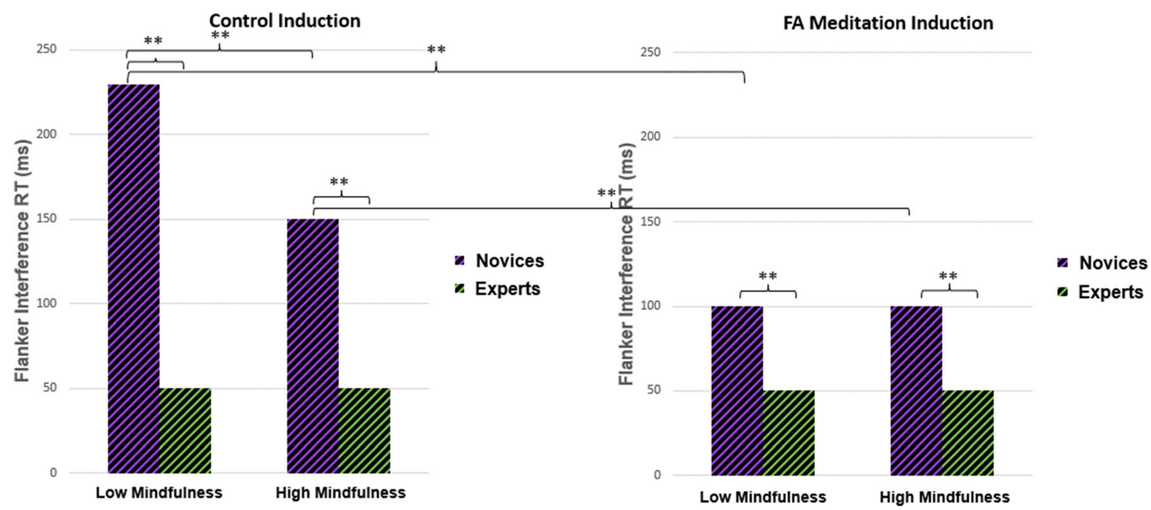

Fig. 3 Hypothetical results from example study depicted in Fig. 2. Statistically significant contrasts are denoted by asterisked brackets. The bar graph illustrates a main effect of Mindfulness Skill (experts exhibit reduced interference effect relative to novices), a two-way Mindfulness Skill × State Mindfulness interaction (novices but not experts exhibit reduced interference effect after FA induction), and a three-way Mindfulness Skill × State Mindfulness × Dispositional Mindfulness interaction (novices but not experts with high levels of dispositional mindfulness exhibit a reduced interference effect relative to low dispositional mindful novices, but this is only observable in the control condition).

of the framework in differentiating state mindfulness from mindfulness training. Moreover, in light of emerging evidence that challenge the treatment specificity of mindfulness training in producing efficacious outcomes in MBI studies (Canby et al., 2021; Rosenkranz et al., 2019; Shallcross et al., 2015; Williams et al., 2014), multifactor designs incorporating both the operationalizations of mindfulness training and state mindfulness hold significant promise towards developing a more targeted and rigorous approach toward evaluating putative MBI mechanisms. In contrast to self-report outcome measures, which do not necessarily solicit engagement of mindfulness during their completion, the state mindfulness manipulations facilitate participant adoption (i.e., via guided induction) and "use" (i.e., via instructional engagement) of mindfulness states during actual task performance across multiple behavioral measures of cognitive control (i.e., flanker and Stroop task). Put more formally, this approach combines the unique strengths of brief inductions and longitudinal training designs while mitigating their respective disadvantages in service of maximizing *substantive validity*, defined as the extent to which the theoretical process (es) associated with a construct is actually engaged and utilized during its assessment (Messick, 1995).

Another key advantage of the design is that it enables "mindfulness" to be properly parsed and investigated as a pluralistic intervention mechanism, comprised of distinct psychological states that are cultivated across multiple meditation practices. For example, as mentioned above, MBIs such as MBCT have been dismantled into FA and OM training components (Britton et al., 2018). By explicitly manipulating state mindfulness to differentiate FA and OM as part of the pre–post assessment battery, it may be possible to parse how the specific development of FA and OM *within the broader context of MBI training* might differentially influence cognitive control. Furthermore, as described above, by recording EEG or other noninvasive peripheral measures (e.g., heart-rate variability) during the state inductions, or self-report indices immediately after the induction, the analysis can be expanded to include both first-person and third-person measures with minimal burden or adjustment to the overall study design.

Here, it is worth elaborating how the experimental design strategy that we are advocating would be strengthened with the integration of advanced third-person cognitive neuroscience/neurobiological methods. The key lies in the ability to obtain real-time objective measures of psychological states, including neural activity occurring *during* the actual process of different meditation practices as well as instructional engagement of mindful states across task performance. For example, spectral power EEG or network-based fMRI approaches can be applied to inductions to derive neural measures of meditation (Kakumanu et al., 2018; Miyoshi et al., 2020). These metrics can then be analyzed in relation to task outcomes, providing valuable insight into the relationship between neural processing occurring during meditation practice and its ostensible "off-the-cushion" effects on subsequent task performance. In addition to conducting analyses between induction and task performance, it is also possible to compare data *across* different inductions. For example, multivariate methods such as representational similarity analysis (RSA; Kriegeskorte et al., 2008) and graph theoretic approaches (Bullmore & Sporns, 2009; Rubinov & Sporns, 2010) can be used to model and estimate neural pattern characteristics

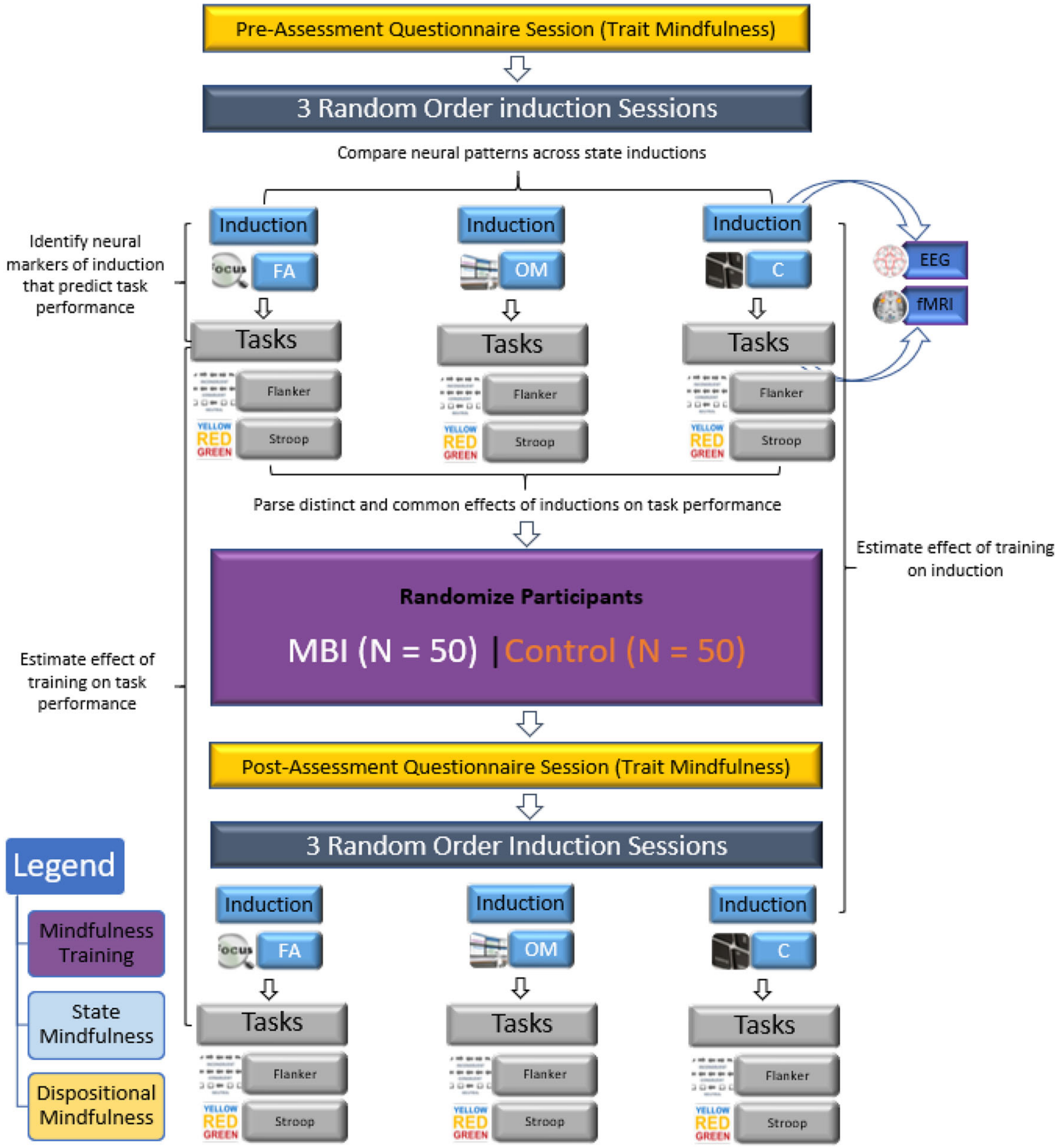

Fig. 4 Example study design involving three operationalizations of mindfulness: mindfulness training (purple), state mindfulness (blue), dispositional mindfulness (yellow). Each induction session is comprised of brief guided meditation (FA = focused attention; OM = open monitoring; C = control), followed by instructional engagement to maintain the induction state across task performance. Neurobiological and psychophysiological measures can be applied during the induction and behavioral task phase

across inductions, providing a crucial means to objectively distinguish theoretically different mindfulness states and meditative practices.

Critically, longitudinal changes in neural meditation metrics can be modeled as mediators of observed improvements in task performance, providing a rigorous examination of the neural mechanisms underlying the functional effects associated with the training of specific MBI meditation practices. Furthermore, by conducting pre–post comparisons of meditative neural activity and task performance across inductions, it is possible to parse the extent to which MBI training modulates the technical and functional distinctions/similarities between meditation practices. Assuming sufficient duration and intensity of training, repeated assessment using this kind of protocol could lead to the mapping of unique training trajectories, linking phenomenological and/or neurophysiological changes associated with different intervention components (e.g., styles of meditation practice) with change in functional outcome.

From an analytic perspective, the factorial structure of the research design is again naturally conducive to mixed linear modeling approaches that can model the effects of state mindfulness (i.e., induction: FA vs. OM vs. C), mindfulness training (i.e., group: MBI vs. control intervention), dispositional mindfulness (i.e., trait mindfulness score), and time (pre vs. post). For example, aggregated trial-level task data (i.e., combined across induction sessions) can be modeled by submitting induction and time as within-subject categorical predictors, trait mindfulness score as a continuous predictor, and group as a between-subject categorical predictor. Furthermore, because it is plausible that induction effects could vary unsystematically across individuals (particularly during the pre-intervention period), induction could be modeled as both a fixed effect and random effect nested within subject. Critically, this approach enables the two distinct but easily conflated operationalizations of mindfulness—as a situational psychological state induced during brief meditation and task performance (induction effects), and a type of longitudinal intervention training (Group × Time interactions)—to be parsed both separately and interactively while controlling for individual differences in dispositional mindfulness.

Consequently, the resulting output is relatively straightforward to interpret, and as such holds substantial promise for addressing operationalization issues in relation in the MBI literature. For example, if a Group × Time interaction emerges such that the MBI training resulted in superior pre–post task performance relative to controls, then there may be evidence in support of the possibility that the effects of MBI training are relatively homogenous and undifferentiable with respect to the development and application of theoretically different meditation practices (FA & OM). On the other hand, if only a main effect of induction emerges (i.e., certain inductions produce better performance than others irrespective of MBI training), then it may be reasonable to infer that MBI training does not meaningfully influence outcomes, but rather task performance is modulated by the brief adoption of particular mindfulness states. Perhaps most interestingly, an Induction × Group × Time interaction could elucidate the nature of functional divergence associated with the training of different meditation practices across the span of MBI participation (e.g., session effects on performance change both as a function of MBI training and how well this training aligns with the pre-task meditation state that was induced). Interactions involving dispositional mindfulness would further signal that induction/training effects are contingent upon trait levels of mindfulness.

Null findings may be equally informative. For example, if no effects are observed or that only a main effect of time emerges (e.g., practice effects), then there is compelling evidence that mindfulness broadly construed does not meaningfully influence task outcomes. In this scenario, it may be fruitful to examine if null effects are also present in pre–post comparisons of self-report trait mindfulness scores—this would provide a basis from which to evaluate the criterion validity of self-report mindfulness measures (e.g., that although trait mindfulness scores increase across the MBI intervention, they do not actually account for meaningful variability in task performance). In general, comparison of experimental/task-based and self-report measures of mindfulness appears particularly critical in light of the methodological issues raised earlier. Lastly, an unexpected Induction × Time interaction, with no further effect of group (i.e., that the effect of induction sessions on task performance differs across time but is nonspecific to group), could introduce the concerning but nonetheless informative possibility that the effects of brief inductions may themselves be unreliable or susceptible to practice effects that supersede any effect of group. Related to this point, because all participants complete the same task battery across time points, a "test–retest" assessment of pre-intervention induction effects is effectively built into the design for the control group. Therefore, it may be instructive to check if pre-intervention findings are replicated in the control group at post-assessment (irrespective of what happens in the MBI group).

Similar models can be applied to fit neural data. It may be particularly fruitful, for instance, to examine how prototypic measures of meditative neural activity (e.g., EEG alpha or theta synchronization) might differ across induction sessions, and are modulated by MBI training. Examining the relationship between spectral power indices of meditation and task outcomes as a function of MBI training may provide a powerful means to test prevailing theories on the functional significance of meditative neural oscillatory activity (see Lee et al., 2018; Lomas et al., 2015, for reviews). Importantly, neural meditation metrics shown to be sensitive to MBI training can be subsequently entered as predictors of task performance, enabling a systematic data-driven approach to discern "neuromarkers" of treatment (Gabrieli et al., 2015).

Practical considerations and caveats

The prior section illustrates the converging operations research strategy through example experimental designs described as standalone multifactorial studies. However, the modular nature of the framework we describe is also amenable to alternative implementations. In particular, one conservative and cost-effective approach is to conduct an incremental series of smaller-scale studies. In this situation, it may nonetheless be fruitful to start with state mindfulness and brief induction paradigms before progressing onto more complex study designs involving longer-term training. Similar to the rationale conveyed above, this establishes a "baseline" effect of state mindfulness on task performance at the outset and can also serve as an initial test of whether different mindfulness techniques produce discrepant effects on a standardized set of cognitive control measures. Later studies could build off this approach by grounding methodological decisions in previous work (e.g., selection of cognitive control tasks and data processing parameters), explicitly testing how mindfulness expertise, prolonged meditation training, or participation in an MBI might compare with the effects observed from brief state mindfulness manipulations.

An alternative reasonable approach is to start with dispositional mindfulness. Because dispositional mindfulness is itself considered to be multifaceted, employing a multidimensional measure, like the FFMQ, confers a higher degree of specificity that can inspire more targeted follow up research questions. Importantly, measures of cognitive control that demonstrate reliable relationships with trait mindfulness can be prioritized for hypothesis generation and testing during later studies involving other operationalizations. In practice, the low cost and relative ease of measuring dispositional mindfulness allows it to be easily combined with state mindfulness manipulations, forming a solid two-factor design from which to begin an investigation. Critically, the driving rationale behind these suggestions is to anchor investigation around an empirically derived set of initial findings, tasks, and measures, thereby promoting standardization by constraining the degrees of freedom around the theoretical and methodological underpinnings of subsequent, presumably more complex and resource intensive studies.

If conducting a laboratory study is too costly or infeasible, another promising possibility involves synthesizing the extant literature by applying the framework toward meta-analysis. Specifically, the operational factors can be coded across collated studies (keeping in mind that some studies will have multiple factors and respective effect sizes) and entered as a categorical random effect moderator (to accommodate the assumption that effect sizes vary across operational factors; see Borenstein et al., 2010). Assuming that the outcome variable is clearly and reasonably defined (e.g., separation of cognition into constituent functional domains; a la Cásedas et al., 2020; Chiesa et al., 2011) and that appropriate study inclusion/exclusion criteria is applied, such moderation analysis offers a comprehensive way to investigate the central claim here—that differences in how mindfulness is operationalized contribute to variability in study findings. Moreover, between-study variability within each factor can be quantified and compared, shedding light on the extent to which particular operationalizations yield more variable findings than others. It goes without saying that systematically parsing the extent to which inconsistent findings are attributable to methodological variance in operationalizing mindfulness is an important yet understated step in understanding the nature of the relationship between mindfulness and cognitive functioning. In deciding whether and how to perform the analysis, it will be imperative to ensure that there are enough studies within each factor to ensure sufficient statistical power (Hedges & Pigott, 2004). Toward this end, it will likely be necessary to expand investigation beyond cognitive control to include other domains of cognition.

With all that said, it behooves us to clarify a few key points. First, although our general recommendation is to consider state mindfulness as the foundational operationalization from which to build a study, the inclusion and ordering of operationalizations is fundamentally malleable. Indeed, the primary advantage of the framework is that it is possible to start with or include any number of factors or operationalizations, so long as it is appropriately tethered to conceptual and logistical considerations. Second, we are not advocating for extreme positivism, in that any cognitive control task/measure found to relate to a particular measure of mindfulness or exhibit sensitivity to mindfulness training must necessarily be interpreted as reflecting a "true" relationship between the constructs. Relatedly, our framework reflects a methodological synthesis of different research strategies and should not be mistaken as a theoretical model of mindfulness (i.e., that mindfulness is divisible in nature and comprised of distinct separable subcomponents). Third, sound use of converging operations rests on active consideration of the theoretical foundations of the research question; it should not be conflated with rote convergence or reproducibility. In other words, it is perfectly reasonable *not* to expect convergence in the absolute sense. For example, one might expect that state mindfulness and dispositional mindfulness independently influence cognitive control in low skill non-meditators, whereas this distinction may be absent in high skill meditators, for whom the demarcation of mindfulness as a state and trait is known to be more porous and less distinguishable (Brewer et al., 2011). In sum, the framework proposed here builds off the core principles of converging operations and factorial design to systemize methodological variability, providing a potential solution to the problem of construct heterogeneity by unifying separate operationalizations of mindfulness into a cohesive research strategy.

Implications from (and for) cognitive control research

It is important to acknowledge that appropriate development and sound use of cognitive control measures is itself a highly complex topic that warrants its own consideration. For illustrative and tractability purposes, we adopted a simplifying assumption that cognitive control is stable, easily measurable, and well understood. However, research on cognitive control naturally contains its own methodological challenges and theoretical complications. In particular, although we simplified measurement of cognitive control to the Stroop and flanker task, assessment of cognitive control can theoretically include any number of psychometrically sound tasks. Furthermore, despite the relative ease in evaluating convergence across studies with a fixed number of standardized tasks, it may not always be advantageous or even reasonable to utilize a prototypic assessment battery comprised of "classic" task paradigms. In fact, retreat studies have successfully utilized a thresholding procedure to tailor attention tasks to match the perceptual discrimination ability of each practitioner (MacLean et al., 2010; Sahdra et al., 2011; Zanesco et al., 2013, 2018; Zanesco et al., 2019). On the side of cognitive control, our own work has centered around the use of a customized task battery, comprised of modified variants of classic cognitive control tasks designed to test different theoretical modes of cognitive control (Braver et al., 2021; Etzel et al., 2021). Indeed, "task optimization" procedures spanning the mindfulness and cognitive control literature offer a promising alternative to a standard "one-size-fits-all" approach to task selection.

Although the use of custom tasks inherently introduces methodological variance and can compromise generalizability, there is significant potential in enhancing construct and ecological validity by calibrating tasks to meet the specific demands of the research question, characteristics of the study cohort, as well as the design of the study itself. For instance, in conducting longitudinal MBI research like that of Example Study 2, it may be fruitful to consider implementing thresholding procedures to match task difficulty to the abilities of the individual at baseline. This may afford a more robust way to capture individual variability associated with the influence of state mindfulness and mindfulness training by minimizing task-relatedceiling/floor effects (which statistical procedures such as random intercepts modeling cannot fully address). On the other hand, it may be less sensible to utilize this approach when comparing experts versus novices (as in Example Study 1) given that thresholding to individual ability may reduce the sensitivity to detect between-group differences in mindfulness skill. Nonetheless, it remains imperative to ensure that selected tasks are sufficiently developed to capture meaningful variability across the intended analyses—this point is particularly relevant for standard versions of the flanker and Stroop task, for which accuracy is typically high in normative healthy populations.

Another measurement related issue involves assessing and controlling for individual differences in motivation and effort. Notably, Jensen et al. (2012) demonstrated that incentivized non-active controls performed similarly to, or even outperformed MBSR participants on several RT-based attention tasks. Indeed, the inclusion of incentivized and non-incentivized control conditions into a standard longitudinal RCT design enables rigorous experimental control and may be the gold standard approach in accounting for the influence motivational factors. Although incorporation of an incentivized control condition is certainly compatible with our approach (both Example Study 1 and Example Study 2 can be extended this way), it carries the cost of reducing statistical power and by extension, could increase the logistical and financial burden associated with recruiting additional participants. Consequently, an alternative approach may be to include multimodal measures of motivation and cognitive effort into the assessment battery. Toward this end, self-report indices, such as the Need for Cognition Scale (Cacioppo & Petty, 1982), could be used to assess trait levels of cognitive engagement, whereas behavioral measures, such as the cognitive effort discounting task (Westbrook et al., 2013; Westbrook et al., 2019), can be used to quantify individual differences in the willingness to expend effort. Analytically, these metrics can be entered as covariates to statistically control for motivationally related confounds. Furthermore, exploratory analyses could be conducted to formally test the extent to which mindfulness related improvements in cognitive functioning may be mediated or moderated by cognitive effort (which itself may be influenced by mindfulness manipulations).

A final issue worth acknowledging is the growing concern regarding the psychometric properties of performance-based cognitive tasks. As clearly outlined by Hedge et al. (2018), many classic cognitive control measures such as the Stroop and flanker task were designed to maximize experimental robustness (i.e., replicable within-subject differences between manipulated task conditions) at the expense of low between-subject variability. Consequently, behavioral measures have been shown to be weakly correlated with self-report measures of the same purported cognitive construct (which are typically reliable in the classical sense and exhibit high between-subject variance; see Dang et al., 2020). Contextualized more succinctly, participants with strong performance on the flanker task may not necessarily rate themselves higher on self-report measures of cognitive control relative to poor performers. Moreover, from the perspective of ecological validity, better flanker performance does not likewise necessarily translate into better cognitive control ability in real world situations.

Despite these salient limitations, "classic" behavioral paradigms may still prove useful for investigating the influence of mindfulness on cognitive control. For example, given that each individual serves as their own control, utilization of the flanker and Stroop task as part of a pre–post assessment battery (a la Example Study 1) remains a reasonable way to estimate the effect of mindfulness training on cognitive control (assuming sufficient number of trials and difficulty to minimize ceiling/floor effects as discussed above). The psychometric properties of the tasks can be further bolstered analytically by shifting away from computing average and subtraction-based summary scores, which tend to inflate measurement error and reduce between subject-variability(Hedge et al., 2018), toward trial-level modeling approaches that account for trial-to-trial variation (Rouder & Haaf, 2019). With that said, these solutions do not obviate the need for deriving behavioral based measures of cognitive control that are both experimentally robust *and* sensitive to individual differences.

In the domain of mindfulness and cognition, such tasks may still need to be developed and specifically benchmarked to existing mindfulness measures with strong inter-individual psychometric properties. Even newer behavioral measures such as the breath counting task mentioned above are only weakly correlated with individual difference measures of trait mindfulness and effects of mindfulness skill (Levinson et al., 2014).

On the theoretical end, it is important to remember that cognitive control is itself a diverse construct that includes different modes of operation and a multiplicity of subordinate functions (Egner, 2017). For example, we point to the tripartite framework of Miyake et al. (2000), and efforts to highlight and differentiate key cognitive control processes within translational work aimed at measuring clinical intervention and outcome effects (Barch et al., 2009). Likewise, work in our own group has been aimed at differentiating proactive and reactive control as distinct modes and individual difference dimensions of cognitive control (Braver, 2012; Braver et al., 2021), which have also attracted the interest of mindfulness researchers (Aguerre et al., 2021; Chang et al., 2018; Incagli et al., 2020; Li et al., 2018).

Given the strong conceptual and practical overlap between mindfulness and cognitive control, it seems likely that adoption of our recommended framework may also advance understanding of cognitive control. First, if the voluntary engagement of mindfulness constitutes a goal-directed behavior subject to capacity constraints and motivational influence (Cohen, 2017), then the very act of being mindful is likely to involve some degree of cognitive control. This close relationship has been the basis for several theoretical models of mindfulness, which highlight the potential impact of mindfulness training on subordinate control functions such as selective attention, conflict monitoring, attentional shifting, and response inhibition (Hölzel et al., 2011; Lutz et al., 2008; Shapiro et al., 2006; Tang, Holzel, & Posner, 2015; Vago & Silbersweig, 2012). Despite the prevailing sentiment that mindfulness training may enhance cognitive control (Chang et al., 2018; Quaglia et al., 2019; Teper et al., 2013), further investigation aimed at addressing construct heterogeneity and the methodological limitations that pervade both domains is needed to thoroughly evaluate the claim. Critically, if clear converging evidence is obtained to support the notion that mindfulness can modulate cognitive control, then understanding of cognitive control would be substantively impacted by at least four plausible inferences: (1) cognitive control is indeed malleable and amenable to training; (2) modulation of cognitive control (at least insofar that it is measured behaviorally) is susceptible to "far transfer" effects; (3) control performance can be influenced by training that prioritizes engagement of *endogenous* attentional processes; (4) the proximal mechanism of mindfulness effects on cognitive and psychological functioning might occur via enhancement of cognitive control processes.

Toward this end, perhaps the most promising feature of our design is the ability to test whether *specific* elements of mindfulness can modulate theoretically relevant cognitive control processes across multiple operationalizations and levels of analysis. For example, FA practice shares considerable theoretical overlap with proactive control (Braver, 2012), such that it reflects a mode of sustained behavior that is maintained by an active predefined goal representation (i.e., attend to breath/target and redirect attention when mind wanders). Consequently, one plausible hypothesis to test is that FA training enhances proactive control. Here, the unique strengths of the converging operations framework are demonstrable insofar that the approach enables the parsing of FA as both an inducible psychological state, and a specific component of mindfulness training. Therefore, by examining the main and interactive effects of state FA and longer-term FA-based meditation training (e.g., via MBI) on proactive control metrics (e.g., using the AX-CPT task), it should be possible to delineate the boundary conditions by which FA influences proactive control. Furthermore, application of cognitive neuroscience methods extends investigation across multiple levels of analysis, enabling promising dissociation analyses in which the neural correlates of FA are compared with that of proactive control and contrasted with other less theoretically related modes of cognitive control (e.g., reactive control). This would provide compelling data to further refine the intricacies of the nomological relationship between mindfulness and cognitive control. As illustrated here, developing a thorough understanding of mindfulness and cognitive control—and certainly cognitive functioning more broadly—will necessitate a more balanced, granular, and critical approach that addresses *both* of these constructs, as well as the various operationalizations and subordinate functions encompassed within them.

Conclusion

Research intersecting mindfulness and cognitive functioning has exerted a clear impact on modern society. It has led to the development of many novel interventions and inspired people to take interest in the training of their own minds towards the enhancement of cognitive functioning. Through its evolution, the field, like any maturing scientific discipline, must face and surmount the prevailing challenges of the times. As we have tried to convey here, much of the today's outstanding difficulties involve defining and measuring the intrinsic complexities of mindfulness. Accelerating investigative interest from widespread corners of academia and medicine has compounded the problem, culminating in a sprawling literature rife with variability and a dearth of generalizable conclusions.

In trying to understand and address these issues, we analyzed how mindfulness has been studied in relation to cognitive control from an experimental cognitive perspective—showing that in addition to its many contributions, differential operationalizations of mindfulness have unwittingly promoted a balkanized approach toward mindfulness research. Ultimately, careful consideration of converging operations may help accelerate progress in filling the aforementioned gaps in knowledge and addressing formidable methodological challenges—collectively leading to a more versatile, integrative, and systematic approach from which to study cognitive mindfulness effects. As we have suggested earlier, the utility of converging operations is perhaps optimized when extended to all relevant constructs within the purview of a particular research question and implemented with the logic of factorial design. Although admittedly a sizeable undertaking, we hope that this discussion has stimulated interest and inspired new directions for interested investigators to apply the proposed framework to their own work.

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