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RESEARCH ARTICLE

Parsing state mindfulness effects on neurobehavioral markers of cognitive control: A within-subject comparison of focused attention and open monitoring

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Abstract

Over the past two decades, scientific interest in understanding the relationship between mindfulness and cognition has accelerated. However, despite considerable investigative efforts, pervasive methodological inconsistencies within the literature preclude a thorough understanding of whether or how mindfulness influences core cognitive functions. The purpose of the current study is to provide an initial "proof-of-concept" demonstration of a new research strategy and methodological approach designed to address previous limitations. Specifically, we implemented a novel fully within-subject state induction protocol to elucidate the neurobehavioral influence of discrete mindfulness states—focused attention (FA) and open monitoring (OM), compared against an active control—on well-established behavioral and ERP indices of executive attention and error monitoring assessed during the Eriksen flanker task. Bayesian mixed modeling was used to test preregistered hypotheses pertaining to FA and OM effects on flanker interference, the stimulus-locked P3, and the response-locked ERN and Pe. Results yielded strong but unexpected evidence that OM selectively produced a more cautious and intentional response style, characterized by higher accuracy, slower RTs, and reduced P3 amplitude. Follow-up exploratory analyses revealed that trait mindfulness moderated the influence of OM, such that individuals with greater trait mindfulness responded more cautiously and exhibited higher trial accuracy and smaller P3s. Neither FA nor OM modulated the ERN or Pe. Taken together, our findings support the promise of our approach, demonstrating that theoretically distinct mindfulness states are functionally dissociable among mindfulness-naive participants and that interactive variability associated with different operational facets of mindfulness (i.e., state vs. trait) can be modeled directly.

Keywords Mindfulness · Cognition · EEG

Introduction

Mindfulness, broadly defined as the intentional awareness and nonjudgmental acceptance of present moment experience (Bishop et al., 2004; Kabat-Zinn, 1990), has been subject to considerable cognitive neuroscience investigation. Indeed, mindfulness is now a widely popular topic within the broader discourse on brain health, cognitive enhancement, and overall lifestyle satisfaction (Bristow, 2019; Clarke & Stussman, 2018; Congleton et al., 2015). A major impetus driving both scientific and public enthusiasm for mindfulness is the shared and persistent interest in understanding how mindfulness influences cognition (Chiesa et al., 2011). The past two decades in particular have witnessed an explosion of investigative scholarship aimed at discerning mindfulness effects on cognitive functioning, including at least five meta-analyses released on the topic in the past 3 years alone (Cásedas et al., 2020; Gill et al., 2020; Im, 2021; Whitfield et al., 2022; Yakobi et al., 2021), in addition to several recent narrative reviews (Gallant, 2016; Lao et al., 2016; Leyland et al., 2018; Prakash, 2021). Moreover, our own group recently contributed a prescriptive review (Lin, Tang et al., 2022), proposing a novel taxonomic framework and accompanying research strategy to accelerate progress within this domain.

Importantly, a synthesis of these reviews reveals a consistent cautionary message—namely, that studies within the mindfulness cognition literature to date have yielded inconsistent findings, relative to not only each other but also in relation to established theoretical models of mindfulness.

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For example, in the most recent meta-analysis, Whitfield and colleagues (2022) reported a small but reliable effect favoring mindfulness effects on executive function; however, this effect was solely driven by enhancement of working memory, whereas other subdomains of the construct exhibited considerable heterogeneity. Furthermore, there was a null effect for all domains of attention. This null finding elicits clear concern from a theoretical perspective, because attention has been a core and enduring component of most, if not all, influential models of mindfulness (Grabovac et al., 2011; Hölzel et al., 2011; Lutz et al., 2008, 2015; Shapiro et al., 2006; Y.-Y. Tang et al., 2015; Vago & Silbersweig, 2012). The primary point is to briefly re-emphasize what we and others have outlined previously, which is that the inconsistencies and limitations that pervade the field reflect a need for a more rigorous and systematic methodological approach, wherein persistent difficulties in conceptualizing and measuring mindfulness are explicitly addressed in tandem with the judicious use of psychometrically valid tasks and best-practice analytic techniques.

Toward this end, the primary goal of the current paper is to provide an initial empirical demonstration of a core component of our converging operations research strategy (Lin, Tang et al., 2022). Specifically, we demonstrate how a fully within-subject state induction design can be used to elucidate (and differentiate) the influence of discrete mindfulness states on cognitive functioning across the levels of brain and behavior. To develop the intuition and rationale behind our approach, we begin by succinctly reviewing the key principles of our strategic framework, detailed fully in Lin, Tang et al. (2022). We then describe the experimental design and analytical procedures of the current study, referring to a set of preregistered hypotheses to explain how methodological considerations were made in relation to the specific limitations and knowledge gaps that we sought to address. Finally, it should be mentioned that although the focus is to probe state mindfulness effects on cognition, it is the intent of our group to continue highlighting the other features and applications of our approach. Consequently, future work will build off the current paper to investigate other topical domains of mindfulness outside of its influence on cognitive functioning, including the use of more advanced research designs that incrementally extend the foundation covered here.

Dismantling mindfulness using state inductions

We recently developed a taxonomic framework that organizes methodological variability within the mindfulness cognition literature in terms of how mindfulness can be variously operationalized as a trait, state, skillset, and form of training (Lin, Tang et al., 2022). Furthermore, we showed that most studies to date have disproportionately utilized self-report trait mindfulness scales or longitudinal pre-post assessment of mindfulness-based interventions (MBIs). Consequently, oversaturation of these methods pigeonholes investigation of mindfulness to that of trait mindfulness and mindfulness training, insulating investigation of mindfulness effects on cognition to select designs that fall within those operational boundaries.

To address the relative lack of methodological diversity, we built upon our organizing framework to advance a flexible and mechanistically oriented research strategy that enables modular integration of different operationalizations. A focal point of our approach is to assess the actual *development* and *utilization* of specific mindfulness skills in service of enhancing substantive validity, defined as the extent to which a theoretical process(es) associated with a construct is actually engaged and utilized *during* its assessment (Messick, 1995). A crucial point is that investigation is rebalanced away from trait mindfulness toward state mindfulness—the operationalization of mindfulness that is most directly experiential and phenomenologically tethered and therefore serves as an optimal conduit from which to construct study designs that maximize substantive validity. Critically, this is precisely because state mindfulness, defined as a transient psychological state subject to voluntary engagement and situational variance, is itself a fundamental aspect (conceptually and practically) of both the cultivation and utilization of mindfulness.

With this rationale in mind, we report the results of a proof-of-concept study that investigates the extent to which state mindfulness is amenable to experimental manipulation using a novel, fully within-subject multi-session neurobehavioral assessment protocol. In particular, the design required participants to complete three electroencephalography (EEG) recorded testing sessions involving focused attention (FA), open monitoring (OM), or active control (C) conditions, with each FA/OM session beginning with a guided audio induction to first bring participants into the desired mindfulness state, followed by explicit instructions to maintain the state during performance of a behavioral task battery (a manipulation henceforth referred to as instructional engagement). Briefly, FA or the practice of sustaining attention to a target object (e.g., the breath), and OM, referring to nonjudgmental monitoring of arising momentary experience, are two distinct mindfulness practices that vary across a number of cognitive and experiential dimensions (Britton et al., 2018; Lutz et al., 2008, 2015). Although FA and OM can be characterized as distinct states with putatively distinguishable neural correlates and functional influences (Fox et al., 2016; Lee et al., 2018; Lippelt et al., 2014; Manna et al., 2010), they remain poorly differentiated within the mindfulness cognition literature.

Consequently, a primary advantage of our approach is that it enables mindfulness to be properly investigated as a *pluralistic* mechanism, comprised of distinct psychological states that are cultivated across categorically different meditation practices. The behavioral task battery, which includes the Eriksen flanker task as a classic measure of cognitive control, is held constant across testing sessions while the induction varies, thereby enabling a carefully controlled comparison of how qualitatively different mindfulness states influence a common set of neurobehavioral outcome measures. Furthermore, compared with traditional self-report measures that do not solicit engagement of mindfulness during their completion, instructional engagement prompts participants to maintain engagement of mindfulness states during actual task performance, mirroring a core pedagogical element of contemporary MBIs, insofar that participants are taught to apply the specific psychological states cultivated during formal mindfulness training toward daily activities (i.e., informal practice; Birtwell et al., 2019).

Critically, it is these unique features that differentiate the current design from previous mindfulness induction studies and that enable a number of outstanding conceptual and methodological limitations to be addressed. Specifically, a survey of Gill and colleague's (2020) meta-analysis reveals that the vast majority of mindfulness induction studies: 1) investigate only a singular type of mindfulness practice, with FA being significantly overrepresented relative to OM; 2) rely on between-group designs to derive comparative inferences; 3) do not regularize or account for engagement of mindfulness states during actual task performance. In other words, direct comparison of FA and OM is lacking, and among the select few studies that offer this comparison, the contrast is performed using between-group designs (Sharpe et al., 2021). Furthermore, it is important to include an active control state induction to identify common versus distinct effects of a given mindfulness practice, such as FA and OM.

Along similar lines, recent randomized controlled trials (RCTs) involving prospective mindfulness training have likewise utilized pre-post between-group designs to distinguish the influence of FA and OM (Britton et al., 2018; Brown et al., 2022; Canby et al., 2021; Lohani et al., 2020). Although a between-group approach is a reasonable avenue to begin parsing FA and OM effects, we contend that a fully within-subject design represents a closer proxy to the empirical question bearing the most conceptual and practical consequence—that is, to what extent are FA and OM states differentiable *within a given person?* Taken together, very little is currently known about how adoption and utilization of distinct mindfulness states influence cognition, despite accelerating interest in disentangling this dimension of heterogeneity within the broader construct of mindfulness.

Study goals and hypotheses

The current study sought to fill this gap in knowledge by leveraging the aforementioned design to investigate FA/OM influences on behavioral and event-related potential (ERP) indices of executive attention and error monitoring, two critical subfunctions of cognitive control that are frequently measured from the flanker task. In particular, we assessed executive attention by examining flanker congruency interference effects on trial-level performance accuracy and response times (RT); in addition to the stimulus-locked P3 (Polich, 2007), a central-parietal positive deflection thought to broadly index stimulus-related attention processing (Clayson & Larson, 2011; Frühholz et al., 2011; Groom & Cragg, 2015; Verleger et al., 2005). We also investigated early and late stages of error monitoring by measuring the response-locked error-related negativity (ERN; Falkenstein et al., 1991; Gehring et al., 2012; Gehring et al., 2018), a frontal-central negative deflection that has been functionally linked to the detection of conflicting response representations (Yeung et al., 2004) and/or predicted performance outcomes (Holroyd & Coles, 2002); and the error positivity (Pe; Overbeek et al., 2005), a central-parietal positive deflection that is generally considered to reflect conscious error recognition (O'Connell et al., 2007; Ullsperger et al., 2010; Wessel, 2012).

Collectively, these highly established metrics, which are both firmly grounded in basic cognitive neuroscience and studied previously in relation to mindfulness, are exceptionally well-suited to parse whether FA and OM states are functionally distinguishable with respect to cognitive control. With that said, however, it must be noted that previous mindfulness induction studies involving these measures have yielded very mixed findings (Andreu et al., 2017; Bailey, Freedman, et al., 2019; Bailey, Raj et al., 2019; Bing-Canar et al., 2016; Larson et al., 2013; Lin, Eckerle et al., 2019; Lin, Fisher et al., 2019a, 2019b; Norris et al., 2018; Quaglia et al., 2019; Saunders et al., 2016; Teper & Inzlicht, 2013), leading to the speculation that uncontrolled variation in mindfulness states and practices (particularly the FA vs. OM dichotomy), in addition to differences in trait mindfulness and mindfulness training experience may explain a significant proportion of outcome variability (see Lin, Eckerle, et al., 2019 for an extended discussion). Consequently, a primary goal of the current study was to follow-up on this possibility by systematically accounting for *all* these sources of variance, rigorously comparing FA and OM state effects on task performance via direct within-subject experimental manipulation, while assessing and controlling for variability in trait mindfulness in a targeted sample of novice participants.

Given the mixed nature of the prior literature combined with the novelty of the current design, we preregistered our hypotheses and analytic approach to minimize researcher's degree of freedom and post-hoc reinterpretation (https://osf.io/2zk3s). The preregistration also included hypotheses that pertained to testing FA and OM effects on emotional reactivity by using an affective picture viewing paradigm, as well as exploratory analyses designed to discriminate neural signatures of FA and OM state quality during the induction period. To maintain full transparency, these analyses are ongoing and will be reported in separate manuscripts later. We elected to split these papers based on the clear natural topical division, in addition to the need to balance thorough and careful analysis with reasonably expedient dissemination of study findings. We therefore circumscribe the following summary of the preregistered hypotheses and analytic methods to only that of the flanker task (but the entire preregistration is nevertheless available for full viewing online).

To our knowledge, the current study is the first to directly compare FA versus OM state induction effects on neurobehavioral indices of executive attention and error monitoring by using a fully within-subject design. Building on the foundation that FA and OM represent distinctive psychological states that can be reliably evoked and maintained via standardized instruction, we posited that brief but careful experimental manipulation of these states could produce differential effects on cognitive task performance. This premise is analogous to the expansive mood induction literature, which makes similar use of brief inductions to investigate how acute manipulations of mood influence cognition and other key domains of psychological functioning (Lench et al., 2011; Westermann et al., 1996). We further reasoned that such state-related differences may be particularly salient among a homogenous novice sample, wherein within-subject contrasts involving ostensibly novel mindfulness states could be more pronounced and detectable (especially compared to an active control, nonmindful state). In light of these considerations, along with accumulating evidence that FA and OM are indeed empirically dissociable (Brown et al., 2022; Fox et al., 2016; Lohani et al., 2020; Manna et al., 2010), we hypothesized that FA and OM states would be broadly functionally distinguishable with respect to cognitive control as well. More specifically, because the P3, ERN, and Pe are demonstrably separable components that occur across distinct temporal phases of flanker task performance, we anticipated that the pattern of FA and OM effects would differ depending on the specific ERP metric of interest.

Consequently, based on the theoretical assumption that FA will enhance selective attention to the target stimulus, we predicted that the FA induction would reduce behavioral and P3 flanker interference effects while increasing ERN amplitudes. Together, this would provide compelling evidence to demonstrate that FA fosters superior executive attention and heightened conflict monitoring by enhancing the prepotency of the target response representation (Lin, Fisher et al., 2019; Lin, Eckerle et al., 2019; Yeung et al., 2004). Conversely, we expected OM to selectively enhance Pe amplitudes, consistent with the possibility that OM promotes greater interoceptive awareness of error commission (Lin, Eckerle et al., 2019). To preview, our results did not align with these preregistered hypotheses but instead suggested an important role for OM in modulating behavioral performance and P3 effects.

Equally important, these hypotheses were statistically evaluated by using Bayesian mixed-effects modeling. Although Bayesian approaches have become more prevalent in recent years (van de Schoot et al., 2017), they remain seldomly used within the domain of mindfulness research. In our view, this is a significant missed opportunity, precisely because there have been repeated calls to reconcile mixed findings, strengthen analytic rigor, and advance replication efforts within the field (Davidson & Kaszniak, 2015). Toward this end, Bayesian techniques offer the remarkable advantage of sequential updating using informed priors, effectively allowing for the incorporation of previous effect size estimates during data modeling (Lin, Brough et al., 2022; Ly et al., 2019; Wagenmakers et al., 2018). Critically, this fosters a more accretive and naturally incremental approach toward evidence evaluation, enabling direct estimation of how the accumulation of new data influences prior effects and the magnitude to which they converge or differ.

Moreover, the sequential nature of the Bayesian framework is particularly synergistic with our converging operations research strategy insofar that different operationalizations of mindfulness can be modularly integrated across successive studies of increasing scope and complexity. Therefore, effect estimates associated with a specific operationalization (e.g., the effect of trait mindfulness on cognitive task performance) from a previous study can be entered into subsequent studies to improve both replication and extension efforts. Finally, we leveraged trial-level mixed modeling with random intercepts to fully capture trial-by-trial performance variability, enhance reliability of categorical contrast estimates (by circumventing the need to compute summary difference scores), and appropriately account for non-independence and subject-level variance.

Methods

Participants

Thirty native or fluent English-speaking participants with no previous mindfulness experience were recruited using community flyers, email, or word-of-mouth advertisement. As noted in the preregistration, the target sample size was determined before the start of data collection and exceeds the recommended number of 28 participants needed to detect the hypothesized within-subject interactions with 0.80 power (assuming medium effect size of $d = .50$). The assumption of a medium effect was derived on the basis of previous mindfulness EEG studies reporting medium (although inconsistent) effect sizes across similar outcome measures (Andreu et al., 2017; Bailey, Freedman et al., 2019; Bailey, Raj et al., 2019; Bing-Canar et al., 2016; Larson et al., 2013; Norris et al., 2018; Saunders et al., 2016; Teper & Inzlicht, 2013), in conjunction with the speculation that within-subject state induction effects would be more robust in novice samples due to the novel saliency between mindful and active control conditions. Of this sample, one participant failed to comply with task instructions (e.g., was observed falling asleep during inductions and randomly clicking during flanker performance) and was excluded from the analyses. The final sample therefore consisted of 29 participants (aged 18–35 years, M = 20.72, SD = 4.04; 17 females, 12 males), two of whom had one session of data removed due to low accuracy (performance accuracy fell 3 standard deviations [SDs] below the grand mean, as specified in the preregistration criteria for outlier exclusion). Two participants did not complete the manipulation check measure during one session because of experimenter negligence (forgetting to administer the survey) and technical difficulty (data collection server was down). Participants were compensated a total of $80 for full completion of the study. The study protocol (202012148) was approved by Washington University in St. Louis' institutional review board.

Design and procedures

The study protocol consisted of three randomized-order, 2-hr EEG testing sessions, each occurring on separate days and involving FA, OM, or active control (C) inductions. Before the start of the first testing session, participants completed a brief self-report battery containing demographic information, trait mindfulness, and self-compassion questionnaires. After completion of EEG setup, the mindfulness sessions began with a 10-min audio-guided FA/OM practice, followed by explicit instructions to maintain the FA/OM state during completion of the flanker task or affective picture viewing task (task order randomized). Participants then repeated the guided practice and were again reminded to maintain the target state before the start of the second task (i.e., each participant completed the induction twice per session). The active control session was identical in structure but involved listening to an educational TED talk, followed by nonspecific instructions for how to approach the tasks. Each audio induction was delivered twice per session and at the beginning of each task (with explicit reminders to maintain the target state during task performance) to ensure successful adoption, maintenance, and regularization of the state of interest. At the end of each session, participants completed a series of manipulation check measures assessing engagement and responsivity to the induction and tasks, as well as state mindfulness items to probe the subjective quality of the guided practice.

Tasks

Audio inductions

All audio inductions were duration matched to exactly 10 min in length. Briefly, the FA induction instructed participants to sustain attention to the breath and to redirect attention whenever mind wandering was detected. The OM induction instructed participants to direct open nonjudgmental awareness to arising thoughts, feelings, and physical sensations. Both inductions were recorded by a certified MBSR instructor and implemented successfully in previous work from our group (Tang & Braver, 2020). The active control induction was an abridged audio recording of a TED talk by the linguist Chris Lonsdale, who taught participants how to quickly acquire second-language proficiency, and was likewise used in previous studies (Lin, Eckerle et al., 2019; Lin et al., 2016). Because of previous evidence that the mindfulness inductions may selectively induce sleepiness (Lin et al., 2020), all participants were instructed to keep their eyes open during the audio inductions.

Flanker

Participants completed an arrow version of the flanker task (Eriksen & Eriksen, 1974). As standard, participants were instructed to respond as quickly and accurately as possible to the center arrow of a five-arrow string in which the target was either congruent (e.g., <<<<< or >>>>>) or incongruent (e.g., <<><< or >><>>) in relation to the flanking arrows. All characters were presented in a standard white font on a black background. There were a total of 512 trials, divided evenly into 8 blocks of 64 trials, with half of the trials in each block being congruent and the other half incongruent. During each trial, arrows were presented for 200 ms, and participants were given 950 ms to respond using their right index or middle finger to press the respective left and right mouse buttons that corresponded with the direction of the target center arrow. The intertrial interval varied between 600–1000 ms, during which a fixation cross (+) was presented at the center of the screen. The task was programmed and administered using E-Prime software (Psychology Software Tools Inc, Sharpsburg, PA).

Self-report measures

Trait mindfulness

Trait mindfulness was measured using the 39-item Five Facet Mindfulness Questionnaire (FFMQ; Baer et al., 2006), and the 15-item Mindful Attention Awareness Scale (MAAS; Brown & Ryan, 2003). The five facets of the FFMQ are: 1) observing; defined as perception of internal and external experiences; 2) describing, defined as verbalization of internal experiences; 3) acting with awareness, defined as attention to present moment experiences; 4) non-judging, defined as non-evaluation of inner experiences; 5) non-reactivity, defined as non-attachment or nonelaboration of internal experiences. Each item on the FFMQ is rated using a 5-point Likert scale ranging from 1 (*never* or *very rarely true*) to 5 (*very often* or *always true*), whereas the MAAS items are rated using a 6-point Likert scale ranging from 1 (*almost always*) to 6 (*almost never*).

Manipulation check

A manipulation check questionnaire adapted from previous work (Lin et al., 2020) was administered at the end of each session to assess for differences in engagement and reception to the audio inductions and tasks. For the audio inductions, participants were asked to rate the extent to which they found the audio engaging, interesting, and arousing (*1 = not at all, 7 = very*). Participants also indicated their emotional reaction (*1 = very negative, 4 = neutral, 7 = very positive*), comprehension level (*1 = did not understand, 7 = completely understand*), whether they learned anything (*1 = very little, 7 = very much*), and their physical comfort (*1 = not comfortable, 7 = very comfortable*). Similarly, for the flanker task, participants reported their engagement, interest, and arousal (*1 = not at all, 7 = very*), emotional reactivity (*1 = very negative, 4 = neutral, 7 = very positive*), physical comfort (*1 = not comfortable, 7 = very comfortable*), as well as task difficulty (*1 = not difficult, 7 = very difficult*). Finally, participants rated their sleepiness level (*1 = feeling active, vital, alert, or wide awake*, *8 = I fell asleep*), during both the audio induction and flanker task using the Stanford Sleepiness Scale (SSS; Hoddes et al., 1972).

Psychophysiological recording and data reduction

Participants were fitted with a 64-channel Lycra EEG cap, and continuous electroencephalographic activity was recorded by using a Brain Vision actiCHamp Plus system (Brain Vision LLC, Morrisville, NC). Recordings were taken from 32 Ag-AgCl electrodes placed in accordance with the 10/20 system with a sampling rate of 500 Hz. Horizontal and vertical electrooculogram activity were recorded by using three electrodes placed around the eyes, two lateral to the outer canthi of each eye and one directly under the right pupil and below electrode site Fp2.

Offline analyses were conducted by using BrainVision Analyzer 2 (BrainProducts, Gilching, Germany). All data were re-referenced to the average of all scalp electrodes (i.e., common average reference) and band-pass filtered between 0.1 and 30 Hz (12 dB/oct roll-off). Ocular artifacts were corrected by using the regression approach developed by Gratton et al. (1983). Artifact rejection was completed by using a computer algorithm that detected and removed trials that met the following criteria: a maximum voltage step of more than 50 μV between sample points; a voltage difference of more than 300 μV within 200-ms intervals; voltage exceeding ±200 μV or a maximum voltage difference <0.5 μV within 100-ms intervals.

ERPs were locked to stimuli onset (i.e., flanker arrows) or response press with a 200 ms pretrial baseline correction and computed by using the collapsed localizer method (Luck & Gaspelin, 2017). As standard, only correct responses were included to compute the P3 (Clayson & Larson, 2011; Frühholz et al., 2011; Rietdijk et al., 2014), which was quantified as the average activity occurring between 450 and 650 ms post-stimuli onset at the central-parietal recording site Pz where the amplitude was maximal. Similarly, the ERN and Pe were respectively quantified as the average activity occurring between 0–100 ms and 200–400 ms post-response on error versus correct trials at maximal frontal site FC1 and central-parietal site Pz.

Statistical analyses and predictions

The *brms* package in the R statistical software environment was used to conduct Bayesian mixed effects regression (Bürkner, 2017). Categorical variables (e.g., induction condition) were effect coded to enable proper estimation of main effects. To aid interpretation when appropriate, induction condition also was dummy coded, with C most often used as the referent condition to demonstrate how FA and OM differed from the active control condition. Trial-level behavioral data were submitted to logistic regression to model induction effects on performance accuracy. As standard, RTs were filtered to include only correct trials and modeled using a shifted lognormal distribution, which has been shown to better fit RTs than other distributions (Haines et al., 2020). The ERPs were modeled using linear regression, with trial congruency/response type included as categorical predictors to obviate the use of difference scores. Trait mindfulness was computed as a normalized composite score by z-scoring the average of FFMQ and MAAS scores (which were each z-scored first from the raw score).

As described in the preregistration, trial congruency (congruent, incongruent), response type (correct vs. error), and induction condition (FA, OM, C) were categorical predictors in the models, while trait mindfulness score and session number were entered as continuous covariates. We also included task order as a covariate, which was inadvertently overlooked during the drafting of our preregistration but nonetheless constitutes an important experimental variable over which to explicitly control. Subject-level variability was modeled by entering a random intercept nested within subject. Given the novelty of the design in conjunction with the mixed nature of previous study outcomes and lack of parameter-level data, uniform priors were conservatively selected to estimate fixed effects across all models, and random effects were weakly informative based on *brms* defaults. Below, we briefly summarize how our primary hypotheses were modeled, including the predicted output that would serve as grounds for falsification.

To test the hypothesis that FA would improve executive attention, as indexed via reduced behavioral and ERP indices of flanker congruency interference, we ran the following models: *Accuracy/RT/P3 ~ 1 + Trial Congruency * Induction Condition + Trait Mindfulness + Session Number + Task Order + (1 | Subject)*. Accuracy was modeled as the log-likelihood of committing a correct response relative to an incorrect response across flanker trials, whereas RT was modeled by using lognormal regression on correct trial RTs. The P3 was modeled by using linear regression on the average amplitude elicited across correct trials. Specifically, we expected two-way Trial Congruency x Induction Condition interactions, such that the FA induction would increase the likelihood of correct responses, reduce RTs, and reduce P3 amplitudes on incongruent trials relative to OM and C. Failure to detect any of these interactions would constitute strong multimodal evidence against the hypothesis that FA selectively enhances executive attention.

Similarly, the hypothesis that FA and OM will exert differential effects on early and late components of error monitoring was tested by using the following models: *ERN/Pe ~ 1 + Response Type * Induction Condition + Trait Mindfulness + Session Number + Task Order + (1 | Subject)*. Both the ERN and Pe were modeled by using linear regression on average amplitudes elicited across correct and error trials. We expected Response Type x Induction Condition interactions, such that: 1) the FA induction would increase ERN amplitudes on error trials relative to OM and C, signifying heightened conflict monitoring via greater selective attention to the target stimulus; and 2) the OM induction would selectively enhance the Pe on error trials, reflecting greater interoceptive awareness of error commission. Once again, failure to detect either of these two-way interactions would fail to support the hypothesis that FA and OM confer distinct functional influences on error monitoring.

Models predicting performance accuracy and ERPs were run using 4 Monte Carlo chains, each containing 2,000 iterations with 1,000 warm-up iterations, which were discarded. The RT model also used 4 Monte Carlo chains, but with 4,000 iterations each and 2,000 discarded warm-up iterations, to ensure sufficient effective sample sizes (ESS). We report the mean, standard deviation (SD), 95% credible interval (CI), R-hat, and ESS values for all parameter estimates. Importantly, we also compute the evidence ratio (ER) to directly weigh evidence in favor or against each of the predicted interactions described above. The ER is essentially a Bayes factor, quantifying the proportion of the posterior distribution that a specific parameter estimate falls above or below an expected threshold value relative to the null. For example, when testing the hypothetical hypothesis that FA will improve performance accuracy, the ER is the ratio of the posterior probability that FA > 0 (alternative hypothesis) relative to FA ≤ 0 (null hypothesis). ERs greater than one indicate that the evidence is in favor of the tested hypothesis, whereas ERs less than one indicate evidence against the hypothesis. Per Lee & Wagenmakers (2014), ER values between 1–3 (1–1/3) are suggestive of anecdotal evidence, 3–10 (1/3–1/10) moderate evidence, 10–30 (1/10–1/30) strong evidence, and >30 (<1/30) very strong evidence. All data, materials, and analysis code, including data wrangling scripts and supplementary analyses using conventional statistical approaches (e.g., rANOVAs involving aggregated summary scores) and exploratory moderators (e.g., session order), are openly available at (https://osf.io/uv9yn/).

Results

Demographic characteristics of the sample are provided in Table 1, and descriptive statistics of the trait mindfulness measures and manipulation check items are provided in Tables 2 and 3, respectively. We also provide traditional summary statistics characterizing flanker behavioral performance in Table 4. To streamline reporting in the sections to follow, we describe only induction related effects that were relevant to the testing of our core hypotheses specified above; however, full-model summaries containing all parameter estimates are presented in Tables 5, 6, 7, 8, 9 and 10. Notably, all expected ERP and session-order effects (i.e., larger P3, ERN, and Pe amplitudes on incongruent vs. congruent/error vs. correct; higher accuracy and faster RT with more practice) were confirmed in the model output and will not be described further below.

Manipulation check

One-way repeated measures ANOVAs were conducted to assess for session differences on the manipulation check measures. Briefly, the rANOVAs revealed significant differences in engagement, interest, emotional reaction, arousal, understanding, learning, and physical comfort (*F*s > 4.27, *p*s < 0.02), but not sleepiness ($F = 2.67$, $p = 0.08$) during

Table 1 Demographic information of participants

Variable	*N*
Gender	
Male	12
Female	17
Ethnicity	
Not Hispanic or Latino	27
Hispanic or Latino	2
Race	
White	13
Black or African American	0
Asian	15
Native Hawaiian or other Pacific Islander	1
American Indian or Alaskan Native	0
More than one race	0

Table 2 Descriptive statistics of mindfulness measures

	N = 29		
Variable	*Range*	*M*	*SD*
FFMQ total	78–177	123.86	17.85
FFMQ observe	18–37	25.72	4.96
FFMQ describe	17–35	27.69	5.27
FFMQ acting with awareness	11–37	23.69	5.40
FFMQ nonjudgment	12–40	25.45	6.51
FFMQ nonreactivity	10–31	21.31	4.66
MAAS	2.33–5.47	3.56	0.75

FFMQ = Five Facet Mindfulness Questionnaire, MAAS = Mindful Attention Awareness Scale. Correlation between FFMQ Total and MAAS: $r = 0.53$

the audio inductions. Follow-up Bonferroni-corrected *t*-tests were applied to elucidate the specific pattern of differences, revealing that participants rated the active control induction as more engaging, interesting, and physically comfortable than both the FA ($ts > 2.69$, $ps < 0.04$) and OM inductions ($ts > 2.70$, $ps < 0.04$). Participants also reacted more positively to the control induction than the FA induction ($t_{(28)} = 2.63$, $p = 0.04$) and reported higher levels of arousal during the control relative to the OM induction ($t_{(26)} = 2.66$, $p = 0.04$). Lastly, participants endorsed higher levels of understanding during FA compared with both the control audio ($t_{(28)} = 2.82$, $p = 0.03$) and OM ($t_{(26)} = 3.35$, $p = 0.01$). This pattern of findings is largely consistent with our previous work, showing that participants generally endorse more favorable ratings for the control TED talk than guided meditation (Lin et al., 2016; 2020). There was an unexpected difference in interest during flanker task performance ($F = 3.82$, $p = 0.03$), such that participants reported higher levels of interest during OM than the active control session ($t_{(27)} = 2.93$, $p = 0.02$). There were no other differences in task responsivity ($Fs < 1.43$, $ps > 0.25$).

Behavioral performance

Contrary to our predictions, the FA induction did not selectively reduce flanker interference; we found no evidence to support higher log-odds of correct responses ($b = 0.01$, SD = 0.04, 95% CI = [−0.07, 0.10], ER = 1.38), nor faster RTs ($b = 0.001$, SD = 0.001, 95% CI = [−0.001, 0.004], ER = 0.19)[1] on incongruent trials. Instead, there was an unexpected main effect of OM, such that the OM induction was decisively associated with higher overall accuracy ($b = 0.11$, SD = 0.04, 95% CI = [0.02, 0.20], ER = 234.29) and slower RTs ($b = 0.003$, SD = 0.001, 95% CI = [0.001, 0.006], ER = 306.69) collapsing across trial congruency. Follow-up dummy coded contrasts revealed that OM increased log-odds of trial accuracy and lengthened RTs relative to both the active control (accuracy: $b = 0.15$, SD = 0.74, 95% CI = [0.01, 0.30], ER = 53.05; RT: $b = 0.006$, SD = 0.002, 95% CI = [0.002, 0.010], ER = 249) and FA induction (accuracy: $b = 0.18$, SD = 0.08, 95% CI = [0.03, 0.34], ER = 120.21; RT: $b = 0.005$, SD = 0.002, 95% CI = [0.001, 0.009], ER = 48.08).

Given the plausible likelihood that OM may have promoted a more cautious but accurate style of responding (i.e., speed-accuracy tradeoff), we elected to formally test this possibility by modeling the influence of RT on accuracy. Consequently, we updated the preregistered model to include within-subject centered RT as a fully interactive moderator of induction and trial type while controlling for between-subject variance in overall RT[2]. In brief (see Table 7 for the full model output), there were expected main and interactive effects associated with RT and trial congruency, such that slower RTs (both between- and within-subject) were associated with higher accuracy (between-sub: $b = 5.64$, SD = 1.22, 95% CI = [3.28, 8.03], ER = Inf[3]; within-sub: $b = 6.20$, SD = 0.20, 95% CI = [5.82, 6.59], ER = Inf), incongruent trials were associated with lower accuracy ($b = -1.10$, SD = 0.04, 95% CI = [−1.18, −1.02], ER = Inf), and that slower RTs on incongruent trials were predictive of higher accuracy relative to congruent trials ($b = 3.35$, SD = 0.20, 95% CI = [2.96, 3.75], ER = Inf).

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[1] RT-related parameter estimates yielded small values because of fitting the data to a lognormal distribution. We therefore report estimates to the third decimal place.

[2] *Accuracy ~ 1 + Trial Congruency * Induction Condition * Within-Sub RT + Trait Mindfulness + Session Number + Task Order + Between-Sub RT + (1 | Subject)*

[3] An ER approaching infinity indicates that the entire posterior distribution of the parameter estimate exceeded the test value in the specified direction.

Table 3 Descriptive statistics of manipulation check responses

	C $N = 29$			FA $N = 29$			OM $N = 27$		
Variable	*Range*	*M*	*SD*	*Range*	*M*	*SD*	*Range*	*M*	*SD*
Audio engagement*	2–7	4.48	1.60	1–6	3.41	1.38	1–6	3.39	1.34
Audio interest*	2–7	4.90	1.52	1–5	3.10	1.05	1–6	3.14	1.38
Audio emotional reaction*	3–7	4.93	1.13	1–6	4.17	1.17	3–7	4.39	0.99
Audio arousal*	1–6	3.38	1.78	1–5	2.45	1.21	1–5	2.56	1.25
Audio understanding*	3–7	5.97	1.09	5–7	6.48	0.69	2–7	5.63	1.28
Audio learning*	2–7	4.93	1.49	1–6	3.66	1.29	1–7	4.41	1.45
Audio physical comfort*	3–7	5.17	1.20	2–6	4.38	1.15	3–6	4.56	1.12
Audio sleepiness	1–6	3.52	1.60	1–6	4.14	1.53	1–7	4.11	1.62
Flanker engagement	1–7	3.55	1.60	1–7	3.72	1.83	1–7	3.82	2.09
Flanker interest*	1–5	2.38	1.18	1–7	2.72	1.62	1–6	3.07	1.54
Flanker emotional reaction	2–6	3.62	0.90	3–6	3.93	0.70	2–6	3.82	0.91
Flanker arousal	1–7	2.69	1.78	1–7	3.03	1.61	1–7	3.04	1.85
Flanker difficulty	1–6	3.41	1.40	1–7	3.45	1.66	1–7	3.44	1.50
Flanker physical comfort	2–7	4.59	1.38	2–7	4.38	1.27	3–6	4.63	1.01
Flanker sleepiness	1–6	3.17	1.71	1–7	3.35	1.78	1–7	3.46	1.99

*Significant difference by condition (Fs > 3.18, ps < 0.03)

Critically, the RT by trial congruency interaction was further qualified by a three-way interaction involving induction condition, such that slower RTs on incongruent trials during only the OM induction were associated with higher trial accuracy ($b = 1.37$, SD = 0.29, 95% CI = [0.80, 1.92], ER = Inf), whereas slower RTs *reduced* accuracy during the FA induction ($b = -0.85$, SD = 0.30, 95% CI = [−1.43, −0.25], ER = 570.43), and marginally during the control induction ($b = -0.53$, SD = 0.26, 95% CI = [−1.05, 0.01], ER = 37.10). Dummy coded contrasts decisively confirmed that slowing down on incongruent trials during only the OM induction produced higher trial accuracy relative to both the FA ($b = 2.24$, SD = 0.50, 95% CI = [1.23, 3.22], ER = Inf) and active control condition ($b = 1.89$, SD = 0.47, 95% CI = [0.94, 2.80], ER = Inf), convincingly demonstrating that the enhancing influence of OM on performance accuracy is primarily driven by the promotion/adoption of a slower more cautious response style, which disproportionately benefits accuracy on the harder incongruent flanker trials.

P3

Inconsistent with our predictions, the FA induction did not reduce the P3 response on incongruent trials ($b = 0.01$, SD = 0.14, 95% CI = [−0.25, 0.29], ER = 0.86), nor was there a main effect of FA collapsing across trial congruency ($b = 0.13$, SD = 0.14, 95% CI = [−0.15, 0.40], ER = 0.23). Instead, there was very strong evidence of a main effect of OM, such that the OM induction was associated with reduced P3 amplitudes across both trial types ($b = -0.30$, SD = 0.14, 95% CI = [−0.57, −0.02], ER = 58.70; Fig. 1). Dummy coded contrasts demonstrated that OM reduced the P3 relative to active controls ($b = -0.48$, SD = 0.24, 95% CI = [−0.94, −0.02], ER = 45.51) but was only marginally smaller when compared to FA *(*$b = -0.43$, SD = 0.25, 95% CI = [−0.91, 0.06], ER = 22.53). Notably, the OM effect was not selective to trial congruency (i.e., no 2-way interaction; $b = 0.01$, SD = 0.14, 95% CI = [−0.25, 0.29], ER = 1.19), mirroring the pattern observed in the performance accuracy model.

ERN and Pe

Once again, neither of our predictions involving error monitoring were supported. Specifically, there was no evidence that FA enhanced the ERN ($b = 0.08$, SD = 0.21, 95% CI = [−0.34, 0.50], ER = 0.56), nor was there a selective effect of OM increasing the Pe ($b = 0.04$, SD = 0.24, 95% CI = [−0.42, 0.52], ER = 1.28). In fact, the state inductions did not modulate any response-locked ERP component (i.e., no main or interactive effects involving induction; bs < $|0.23|$, all CIs contain 0).

Exploratory analyses

Given the unexpected pattern of results, we conducted a set of exploratory analyses to extend and develop a more thorough understanding of our findings. In particular, we followed up on our own previous recommendation to explicitly model the interplay between different operational facets of

Table 4 Descriptive statistics of flanker behavioral performance aggregated across induction

Variable	Overall *N* = 85			C *N* = 29			FA *N* = 27			OM *N* = 29		
	Range	*M*	*SD*	*Range*	*M*	*SD*	*Range*	*M*	*SD*	*Range*	*M*	*SD*
RT (ms)	3–1149	401.67	104.75	3–1139	400.68	104.56	132–1149	400.95	108.04	164–1147	403.34	101.76
Incongruent RT (ms)	3–1149	430.70	94.58	3–1139	429.37	106.43	132–1149	430.37	110.70	164–1147	432.34	102.23
Congruent RT (ms)	130–1146	372.73	106.41	130–1109	372.02	94.33	141–1135	371.66	96.83	179–1146	374.43	92.67
Error RT (ms)	20–1147	332.90	116.94	20–1139	325.63	107.29	132–1142	333.43	115.24	164–1147	340.96	128.73
Correct RT (ms)	3–1149	405.86	102.48	3–1136	405.56	102.50	141–1149	405.23	106.14	187–1146	406.74	98.97
Incongruent error RT (ms)	20–1147	329.51	111.06	20–1139	321.99	99.18	132–1142	333.37	113.43	164–1147	334.30	121.09
Incongruent correct RT (ms)	3–1149	441.85	99.79	3–1136	442.07	99.90	205–1149	441.48	104.81	187–1146	441.99	94.87
Congruent error RT (ms)	130–1095	354.42	147.32	130–1071	349.06	147.78	186–876	333.82	126.37	179–1095	383.82	164.32
Congruent correct RT (ms)	141–1146	373.02	93.48	146–1109	372.41	93.15	141–1135	372.30	96.14	192–1146	374.30	91.27
Overall accuracy	0.80–1.00	0.93	0.05	0.81–1.00	0.93	0.05	0.82–1.00	0.93	0.05	0.79–1.00	0.94	0.05
Incongruent accuracy	0.73–0.99	0.89	0.08	0.72–1.00	0.89	0.09	0.69–0.99	0.89	0.08	0.69–1.00	0.90	0.08
Congruent accuracy	0.86–1.00	0.98	0.03	0.89–1.00	0.98	0.03	0.90–1.00	0.98	0.03	0.84–1.00	0.98	0.03

RT = response time

mindfulness (Lin, Tang et al., 2022), by examining whether trait mindfulness moderated the effect of the state mindfulness inductions. It is indeed conceptually plausible and perhaps even sensible that the ability to cultivate and make use of different mindfulness states may vary depending on the extent to which participants are dispositionally mindful, particularly among novice non-meditators. Consequently, to test this idea, we entered trait mindfulness as a fully interactive predictor alongside induction condition and trial/response type across our models (while retaining all other model parameters)[4] and reran the analyses. Although we did not have any specific predictions, we held the general expectation that greater trait mindfulness would strengthen the influence of the mindfulness inductions in ways that would broadly enhance task performance.

Interestingly, we found significant interactions involving trait mindfulness in both behavioral performance models. For accuracy, greater trait mindfulness was marginally associated with higher overall trial accuracy during the OM induction (b = 0.10, SD = 0.05, 95% CI = [−0.01, 0.20], ER = 27.78), but not the FA induction (b = 0.05, SD = 0.05, 95% CI = [−0.05, 0.14], ER = 5.04). Surprisingly, greater trait mindfulness was associated with lower trial accuracy during the active control condition (b = −0.14, SD = 0.05, 95% CI = [−0.24, −0.05], ER = 399). Dummy coded contrasts confirmed that individuals with greater trait mindfulness exhibited higher trial accuracy during both the OM (b = 0.24, SD = 0.09, 95% CI = [0.07, 0.41], ER = 199), and FA induction (b = 0.19, SD = 0.08, 95% CI = [0.03, 0.35], ER = 74.47), relative to the active control condition. Moreover, this effect was further qualified by a three-way interaction involving trial type, such that greater trait mindfulness was associated with lower log-odds of correct responses on incongruent trials relative to congruent trials during the FA induction (b = −0.10, SD = 0.05, 95% CI = [−0.19, −0.01], ER = 53.05), but not the OM induction (b = 0.05, SD = 0.05, 95% CI = [−0.05, 0.15], ER = 0.18) or active control condition (b = 0.05, SD = 0.05, 95% CI = [−0.04, 0.14], ER = 0.21), indicating elevated flanker error interference during FA for individuals with higher trait mindfulness.

For RTs, greater trait mindfulness was decisively associated with slower overall RTs during the OM induction (b = 0.007, SD = 0.001, 95% CI = [0.004, 0.009], ER = Inf), but faster RTs during the active control induction (b = −0.006, SD = 0.001, 95% CI = [−0.009, −0.004], ER = Inf). There was no influence of trait mindfulness on overall RTs during the FA induction (b = −0.001, SD = 0.001, 95% CI =

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[4] *Accuracy/RT/P3 ~ 1 + Trial Congruency * Induction Condition * Trait Mindfulness + Session Number + Task Order + (1 | Subject). ERN/Pe ~ 1 + Response Type * Induction Condition * Trait Mindfulness + Session Number + Task Order + (1 | Subject).*

Table 5 Model output for trial accuracy logistic regression analysis

Model	Fixed effects	Estimate (SD)	95% CI	R-hat	Bulk ESS	Tail ESS
Accuracy	Intercept	3.79 (0.57)	[2.69, 4.95]*	1.00	1057	1546
	Trial type	−1.00 (0.03)	[−1.06, −0.94]*	1.00	3440	2179
	Induction FA	−0.07 (0.04)	[−0.16, 0.01]	1.00	2382	2642
	Induction OM	0.11 (0.04)	[0.02, 0.20]*	1.00	2502	2671
	Induction C	−0.04 (0.04)	[−0.12, 0.05]	1.00	3402	3082
	Trait mindfulness	0.19 (0.19)	[−0.19, 0.57]	1.00	1476	2021
	Task number	−0.51 (0.37)	[−1.26, 0.22]	1.00	1051	1296
	Session number	0.22 (0.03)	[0.16, 0.27]*	1.00	3508	2549
	Trial type:induction FA	0.01 (0.04)	[−0.07, 0.09]	1.00	2716	2926
	Trial type:induction OM	0.00 (0.04)	[−0.08, 0.09]	1.00	2444	2913
	Trial type:induction C	−0.01 (0.04)	[−0.09, 0.07]	1.00	3179	2692

Trial type = incongruent, FA = focused attention, OM = open monitoring, C = control

*95% confidence interval does not contain 0

Table 6 Model output for trial response time (RT) lognormal regression analysis

Model	Fixed effects	Estimate (SD)	95% CI	R-hat	Bulk ESS	Tail ESS
RT	Intercept	6.048 (0.079)	[5.889, 6.204]*	1.000	2011	2492
	Trial type	0.085 (0.001)	[0.083, 0.086]*	1.000	8829	4972
	Induction FA	−0.001 (0.001)	[−0.004, 0.001]	1.000	8396	5924
	Induction OM	0.003 (0.001)	[0.001, 0.006]*	1.000	8485	5596
	Induction C	−0.002 (0.001)	[−0.005, 0.001]	1.000	8447	5824
	Trait mindfulness	−0.001 (0.026)	[−0.051, 0.050]	1.000	2695	3040
	Task number	−0.014 (0.052)	[−0.117, 0.093]	1.000	2032	2661
	Session number	−0.022 (0.001)	[−0.025, -0.020]*	1.000	8091	5148
	Trial type:induction FA	0.001 (0.001)	[−0.001, 0.004]	1.000	8355	5752
	Trial type:induction OM	−0.002 (0.001)	[−0.004, 0.001]	1.000	8521	5912
	Trial type:induction C	0.001 (0.001)	[−0.002, 0.003]	1.000	8523	6036

Trial type = incongruent, FA = focused attention, OM = open monitoring, C = control

*95% CI does not contain 0

[−0.003, 0.002], ER = 1.86). Interestingly, this effect was once again qualified by a three-way interaction involving trial type, such that greater trait mindfulness was associated with slower RTs on incongruent trials relative to congruent trials during the OM induction (*b* = 0.004, SD = 0.001, 95% CI = [0.001, 0.007], ER = 1332.33), but marginally *faster* RTs on incongruent trials during the FA induction (*b* = −0.002, SD = 0.001, 95% CI = [−0.005, 0.001], ER = 30.01) and active control condition (*b* = −0.002, SD = 0.001, 95% CI = [−0.004, 0.001], ER = 8.38). Dummy coded contrasts decisively confirmed that OM slowed both overall RTs and incongruent trial RTs, particularly for high trait mindfulness individuals, relative to both FA and active control conditions (*b*s > 0.006, all CIs exceed 0, ERs > 125.98).

Taken together, this constellation of findings is suggestive of the possibility that trait mindfulness may amplify the influence of the OM induction, insofar that individuals with greater trait mindfulness exhibited enhanced trial accuracy and slower RTs (which mirrored the main effects of the OM induction). Moreover, OM related improvements to trial accuracy for high trait mindful individuals may be driven by selective slowing during incongruent trials, which further mirrored the results of the RT moderation analyses (i.e., slowing down on incongruent trials during only OM uniquely enhanced trial accuracy), collectively suggesting that participants with greater trait mindfulness may exercise more caution during task performance when engaging the OM state, leading to higher trial accuracy at the expense of speed. Conversely, individuals with greater trait mindfulness displayed *relatively less* caution during the active control condition, where lower overall trial accuracy was coupled with faster overall RTs, and similarly during the FA

Table 7 Model output for accuracy moderated by trial response time (RT) logistic regression analysis

Model	Fixed effects	Estimate (SD)	95% CI	R-hat	Bulk ESS	Tail ESS
Accuracy by RT	Intercept	3.71 (0.46)	[2.79, 4.57]*	1.01	1410	2240
	Between-subject RT	5.64 (1.22)	[3.28, 8.03]*	1.00	2063	2462
	Within-subject RT	6.20 (0.20)	[5.82, 6.59]*	1.00	3478	2902
	Induction FA	0.12 (0.06)	[−0.00, 0.25]	1.00	2793	2848
	Induction OM	−0.09 (0.06)	[-0.20, 0.02]	1.00	2982	3194
	Induction C	−0.03 (0.06)	[−0.14, 0.08]	1.00	3273	3018
	Trial type	−1.10 (0.04)	[−1.18, -1.02]*	1.00	3583	3447
	Trait mindfulness	0.17 (0.15)	[−0.14, 0.47]	1.00	2102	2101
	Task number	−0.36 (0.29)	[−0.91, 0.24]	1.01	1408	2433
	Session number	0.32 (0.03)	[0.26, 0.38]*	1.00	4961	2766
	Within-subject RT:induction FA	1.42 (0.30)	[0.84, 2.01]*	1.00	3007	3217
	Within-Subject RT:induction OM	−1.30 (0.28)	[−1.83, −0.74]*	1.00	3291	2922
	Within-subject RT:induction C	−0.11 (0.26)	[−0.63, 0.40]	1.00	3287	3267
	Within-subject RT:trial type	3.35 (0.20)	[2.96, 3,75]*	1.00	3564	3326
	Induction FA:trial type	−0.13 (0.06)	[−0.25, −0.00]*	1.00	2857	2740
	Induction OM:trial type	0.14 (0.05)	[0.03, 0.25]*	1.00	3105	3309
	Induction C:trial type	−0.02 (0.05)	[−0.12, 0.09]	1.00	3177	3064
	Within-subject RT:induction FA:trial type	−0.85 (0.30)	[−1.43, −0.25]*	1.00	2911	3218
	Within-subject RT:induction OM:trial type	1.37 (0.29)	[0.80, 1.92]*	1.00	3573	3363
	Within-subject RT:induction C:trial type	−0.53 (0.26)	[−1.05, 0.00]	1.00	3051	2873

Trial type = incongruent, FA = focused attention, OM = open monitoring, C = control, RT = response time

*95% CI does not contain 0

Table 8 Model output for P3 linear regression analysis

Model	Fixed effects	Estimate (SD)	95% CI	R-hat	Bulk ESS	Tail ESS
P3	Intercept	3.73 (1.21)	[1.27, 6.12]*	1.00	606	1248
	Trial type	0.70 (0.10)	[0.50, 0.89]*	1.00	3760	3035
	Induction FA	0.13 (0.14)	[−0.15, 0.40]	1.00	2893	2830
	Induction OM	−0.30 (0.14)	[−0.57, −0.02]*	1.00	3028	3048
	Induction C	0.17 (0.14)	[−0.10, 0.44]	1.00	2350	2876
	Trait mindfulness	−0.14 (0.38)	[−0.87, 0.63]	1.00	895	1460
	Task number	0.15 (0.78)	[−1.38, 1.72]	1.00	491	1134
	Session number	−0.56 (0.12)	[−0.79, −0.32]*	1.00	3836	3193
	Trial type:induction FA	0.01 (0.14)	[−0.25, 0.29]	1.00	2705	2868
	Trial type:induction OM	0.01 (0.14)	[−0.27, 0.28]	1.00	2854	2940
	Trial type:induction C	−0.02 (0.14)	[−0.29, 0.26]	1.00	2814	3003

Trial type = incongruent, FA = focused attention, OM = open monitoring, C = control. *95% CI does not contain 0

induction, where lower accuracy on incongruent trials was coupled with faster incongruent trial RTs.

Interestingly, trait mindfulness was significantly associated with larger global P3 amplitudes (i.e., collapsed across trial congruency) during the active control condition (*b* = 0.31, SD = 0.14, 95% CI = [0.05, 0.58], ER = 67.97) but marginally smaller P3s during the OM induction (*b* = −0.18, SD = 0.14, 95% CI = [−0.45, 0.09], ER = 8.98) To aid interpretability, dummy-coded contrasts revealed that greater trait mindfulness was associated with smaller P3s during OM (*b* = −0.49, SD = 0.25, 95% CI = [−0.99, −0.01], ER = 44.98) and to a lesser extent FA (*b* = −0.43, SD = 0.24, 95% CI = [−0.91, 0.03], ER = 27.57), compared with active control, suggesting that more mindful individuals may exhibit more salient reductions in attentional processing to flanker stimuli when engaging mindful verses active control states during task performance.

Table 9 Model output for error-related negativity (ERN) linear regression analysis

Model	Fixed effects	Estimate (SD)	95% CI	R-hat	Bulk ESS	Tail ESS
ERN	Intercept	−1.67 (0.92)	[−3.45, 0.15]	1.00	2766	3028
	Response	−2.34 (0.15)	[−2.63, −2.04]*	1.00	6146	2785
	Induction FA	0.23 (0.22)	[−0.20, 0.65]	1.00	5312	3245
	Induction OM	−0.10 (0.21)	[−0.52, 0.31]	1.00	5651	3393
	Induction C	−0.13 (0.21)	[−0.54, 0.29]	1.00	5551	3199
	Trait mindfulness	−0.11 (0.32)	[−0.74, 0.54]	1.00	2744	2216
	Task number	−0.26 (0.54)	[−1.31, 0.80]	1.00	2492	2405
	Session number	0.38 (0.18)	[0.04, 0.73]*	1.00	5693	3118
	Response:induction FA	0.08 (0.21)	[−0.34, 0.50]	1.00	5835	3318
	Response:induction OM	0.14 (0.22)	[−0.28, 0.54]	1.00	5667	3581
	Response:induction C	−0.22 (0.21)	[−0.62, 0.20]	1.00	5266	3038

Response = error, FA = focused attention, OM = open monitoring, C = control. *95% CI does not contain 0

Table 10 Model output for error positivity (Pe) linear regression analysis

Model	Fixed effects	Estimate (SD)	95% CI	R-hat	Bulk ESS	Tail ESS
Pe	Intercept	3.97 (1.83)	[0.32, 7.54]*	1.00	920	1472
	Response	3.01 (0.17)	[2.68, 3.35]*	1.00	5360	2965
	Induction FA	−0.20 (0.24)	[−0.69, 0.27]	1.00	3543	3196
	Induction OM	−0.02 (0.25)	[−0.50, 0.47]	1.00	3538	2991
	Induction C	0.22 (0.24)	[−0.26, 0.70]	1.00	3654	3225
	Trait mindfulness	−0.78 (0.68)	[−2.08, 0.59]	1.00	1009	1770
	Task number	−1.08 (1.14)	[−3.29, 1.26]	1.00	943	1279
	Session number	−0.08 (0.21)	[−0.48, 0.32]	1.00	4602	2862
	Response:induction FA	0.04 (0.24)	[−0.43, 0.51]	1.00	3857	3260
	Response:induction OM	0.04 (0.24)	[−0.42, 0.52]	1.00	3936	3167
	Response:induction C	−0.08 (0.24)	[−0.55, 0.39]	1.00	3084	2902

Response = error, FA = Focused Attention, OM = open monitoring, C = control. *95% CI does not contain 0

Trait mindfulness did not moderate any induction effects on the ERN or Pe (*b*s < |0.36|, all CIs contain 0) but was unexpectedly associated with smaller ERN (i.e., trait mindfulness x response type interaction: $b = 0.51$, SD = 0.18, 95% CI = [0.16, 0.85], ER = 443.44) and larger Pe responses ($b = 0.40$, SD = 0.20, 95% CI = [0.01, 0.80], ER = 42.96), suggesting that dispositional mindfulness may be linked to reduced early conflict monitoring and enhanced error awareness after accounting for variability related to state mindfulness effects. Although this finding was unpredicted and peripheral to the primary goal of the exploratory analyses, it nevertheless highlights a core advantage of our converging operations research strategy—namely, that the approach enables direct estimation of the unique, shared, and interactive variance associated with different operationalizations of mindfulness. For brevity, we expand on this important methodological implication in the discussion section to follow.

Importantly, all previous lower order induction effects reported in the main preregistered analyses held across these exploratory models, demonstrating that inclusion of trait mindfulness as an individual difference moderator of the induction manipulation did not necessarily change the directionality or general pattern of influence of the inductions themselves. Full summaries of the model output described above is presented in Tables 11, 12, 13, 14 and 15, along with predicted probability plots of the key interactions involving accuracy and RT in Figs. 2 and 3, respectively.

Discussion

The present study was designed to distinguish the functional influence of FA and OM mindfulness states on cognitive control using a novel fully within-subject state induction design among a select sample of novice (i.e., mindfulness-naïve) participants. In particular, we leveraged several core methodological innovations from our converging operations framework (Lin, Tang et al. 2022) to directly manipulate

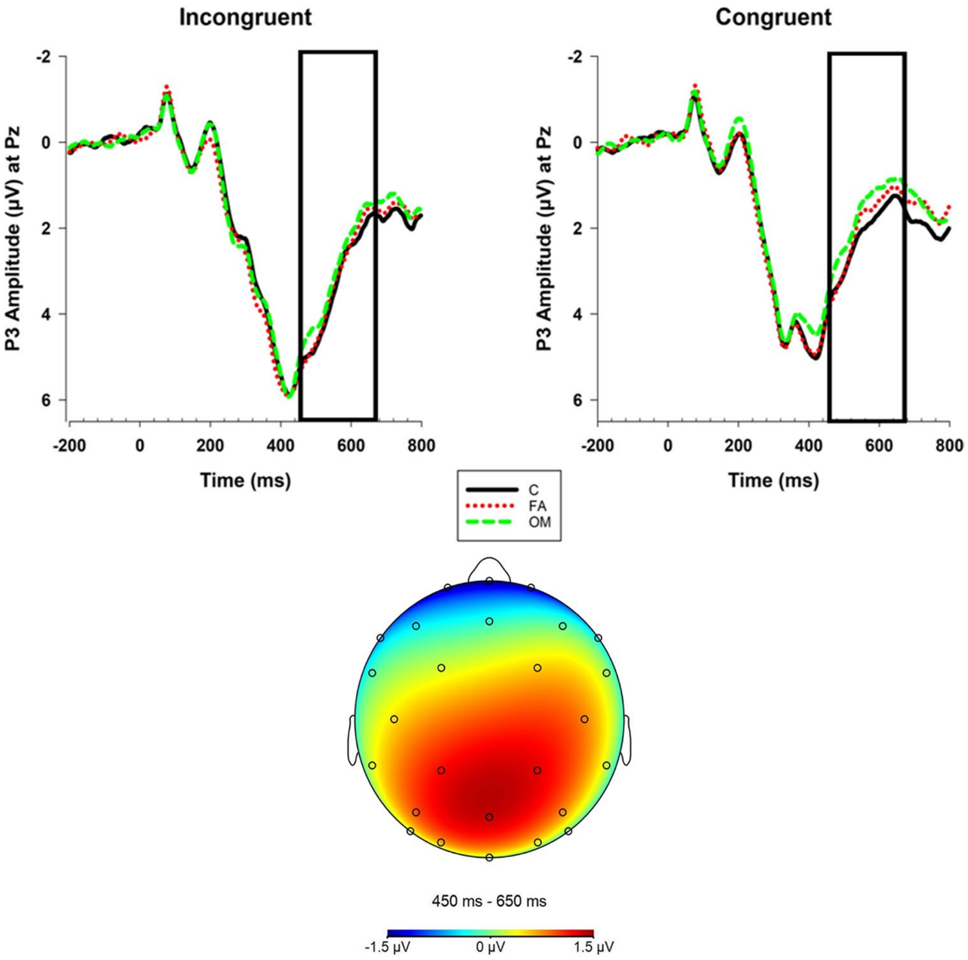

Fig. 1 Stimulus-locked, grand average waveforms depicting incongruent (left) and congruent (right) P3 amplitudes at electrode Pz, quantified as the average activity between 450 and 650 ms, separated by induction condition. Time 0 is flanker stimulus onset. Head map (bottom center) depicting scalp topography of the difference in response amplitude between incongruent and congruent trials across the 450–650-ms time window, collapsed across induction condition

state mindfulness across three separate testing sessions, each involving selective cultivation (via guided audio inductions) and on-task engagement of FA or OM states in addition to a stringent active control condition. Using an EEG flanker task paradigm, we sought to investigate the extent to which FA versus OM would differentially modulate neurobehavioral measures of executive attention and error monitoring, two core subfunctions of cognitive control. Toward this end, we leveraged Bayesian mixed effects modeling to test a series of preregistered hypotheses involving induction effects on flanker behavioral accuracy and RT, the stimulus-locked P3, and the response-locked ERN and Pe.

Interestingly, none of our preregistered hypotheses were supported. Contrary to predictions, the FA induction did not modulate flanker interference effects across accuracy, RT, or P3 amplitudes, clearly demonstrating that FA had no influence on executive attention indices. Furthermore, the FA and OM inductions did not respectively enhance the ERN and Pe response as predicted, but in fact neither state mindfulness induction had any discernible impact on error monitoring ERPs at all. In contrast, and perhaps surprisingly, we found strong evidence that OM selectively produced higher overall accuracy, slower RTs, and smaller P3 amplitudes. Moreover, our follow-up analyses revealed that OM related improvements on performance accuracy were selectively driven from slowing down on incongruent trials and that individuals with greater trait mindfulness responded slower during OM to the benefit of higher trial accuracy. Together, these findings suggest that OM induced a more cautious and possibly intentional response style, and that both the proclivity and effectiveness of engaging in this state were moderated by trait mindfulness.

Table 11 Model output for exploratory trial accuracy moderated by trait mindfulness logistic regression analysis

Model	Fixed effects	Estimate (SD)	95% CI	R-hat	Bulk ESS	Tail ESS
Accuracy	Intercept	3.79 (0.58)	[2.66, 4.90]*	1.00	1131	1705
	Trial type	−0.99 (0.03)	[−1.06, −0.93]*	1.00	5301	2733
	Induction FA	−0.07 (0.05)	[−0.16, 0.02]	1.00	3264	2982
	Induction OM	0.12 (0.05)	[0.03, 0.21]*	1.00	3309	3204
	Induction C	−0.05 (0.04)	[−0.14, 0.03]	1.00	3318	2921
	Trait mindfulness	0.14 (0.20)	[−0.25, 0.55]	1.00	1670	1844
	Session number	0.23 (0.03)	[0.18, 0.29]*	1.00	4905	2574
	Task number	−0.53 (0.38)	[−1.29, 0.22]	1.00	1071	1569
	Trial type:induction FA	0.00 (0.04)	[−0.09, 0.09]	1.00	3174	2867
	Trial type:induction OM	0.01 (0.04)	[−0.08, 0.10]	1.00	3402	3087
	Trial type:induction C	−0.01 (0.04)	[−0.09, 0.07]	1.00	3307	3141
	Trial type:trait mindfulness	0.08 (0.04)	[0.01, 0.15]*	1.00	4823	2751
	Induction FA:trait mindfulness	0.05 (0.05)	[−0.05, 0.15]	1.00	3391	2873
	Induction OM:trait mindfulness	0.10 (0.05)	[−0.01, 0.20]	1.00	3461	2949
	Induction C:trait mindfulness	−0.14 (0.05)	[−0.24, −0.05]*	1.00	3425	3249
	Trial type:induction FA:trait mindfulness	−0.10 (0.05)	[−0.19, −0.01]*	1.00	3485	3071
	Trial type:induction OM:trait mindfulness	0.05 (0.05)	[−0.05, 0.15]	1.00	3310	2921
	Trial type:induction C:trait mindfulness	0.05 (0.05)	[−0.04, 0.14]	1.00	3439	3953

Trial type = incongruent, FA = focused attention, OM = open monitoring, C = control. *95% CI does not contain 0

Table 12 Model output for exploratory trial response time (RT) moderated by trait mindfulness lognormal regression analysis

Model	Fixed effects	Estimate (SD)	95% CI	R-hat	Bulk ESS	Tail ESS
RT	Intercept	6.036 (0.078)	[5.881, 6.193]*	1.003	1922	2826
	Trial type	0.072 (0.001)	[0.070, 0.074]*	1.001	9791	4915
	Induction FA	−0.002 (0.001)	[−0.005, 0.001]	1.001	8924	5627
	Induction OM	0.006 (0.001)	[0.004, 0.009]*	1.000	8696	6050
	Induction C	−0.004 (0.001)	[−0.006, −0.001]*	1.000	9879	6366
	Trait mindfulness	−0.000 (0.026)	[−0.050, 0.051]	1.001	2327	3140
	Session number	−0.017 (0.001)	[−0.020, −0.015]*	1.000	10365	5751
	Task number	−0.023 (0.051)	[−0.125, 0.080]	1.004	1959	2986
	Trial type:induction FA	0.001 (0.001)	[−0.001, 0.004]	1.000	9290	6006
	Trial type:induction OM	−0.000 (0.001)	[−0.002, 0.002]	1.000	9421	6517
	Trial type:induction C	−0.001 (0.001)	[−0.004, 0.001]	1.000	9795	6700
	Trial type:trait mindfulness	0.003 (0.001)	[0.001, 0.005]*	1.001	10359	4944
	Induction FA:trait mindfulness	−0.001 (0.001)	[−0.003, 0.002]	1.001	9279	6194
	Induction OM:trait mindfulness	0.007 (0.001)	[0.004, 0.009]*	1.000	9596	6177
	Induction C:trait mindfulness	−0.006 (0.001)	[−0.009, −0.004]*	1.000	8993	5908
	Trial type:induction FA:trait mindfulness	−0.002 (0.001)	[−0.005, 0.001]	1.001	9029	5962
	Trial type:induction OM:trait mindfulness	0.004 (0.001)	[0.001, 0.007]*	1.000	9285	6541
	Trial type:induction C:trait mindfulness	−0.002 (0.001)	[−0.004, 0.001]	1.000	9991	6414

Trial type = incongruent, FA = focused attention, OM = open monitoring, C = control. *95% CI does not contain 0

Conceptual and methodological implications of null FA findings

We first consider the null results with regard to the FA induction because they are most directly proximal to our *a priori* hypotheses. Specifically, the FA induction did not reduce flanker interference or enhance the ERN as predicted. There have been long-standing theoretical frameworks pointing to a key role for FA to involve (and potentially strengthen) the ability to direct and sustain selective

Table 13 Model output for exploratory P3 moderated by trait mindfulness

Model	Fixed effects	Estimate (SD)	95% CI	R-hat	Bulk ESS	Tail ESS
P3	Intercept	3.85 (1.15)	[1.52, 6.09]*	1.00	761	1413
	Trial type	0.70 (0.10)	[0.51, 0.89]*	1.00	4157	3343
	Induction FA	0.12 (0.14)	[−0.15, 0.40]	1.00	2767	2914
	Induction OM	−0.30 (0.14)	[−0.57, -0.02]*	1.00	2673	2652
	Induction C	0.17 (0.14)	[−0.10, 0.44]	1.00	4044	3105
	Trait mindfulness	−0.17 (0.36)	[−0.86, 0.57]	1.00	766	1435
	Task number	0.15 (0.73)	[−1.25, 1.61]	1.00	712	1425
	Session number	−0.61 (0.12)	[−0.84, -0.37]*	1.00	3105	2920
	Trial type:induction FA	0.02 (0.14)	[−0.26, 0.29]	1.00	3097	2758
	Trial type:induction OM	0.00 (0.14)	[−0.26, 0.27]	1.00	2933	2789
	Trial type:induction C	−0.02 (0.14)	[−0.30, 0.25]	1.00	3875	2763
	Trial type:trait mindfulness	0.03 (0.10)	[−0.16, 0.22]	1.00	4439	3347
	Induction FA:trait mindfulness	−0.13 (0.14)	[−0.41, 0.14]	1.00	2728	2981
	Induction OM:trait mindfulness	−0.18 (0.14)	[−0.45, 0.09]	1.00	2545	2751
	Induction C:trait mindfulness	0.31 (0.14)	[0.05, 0.58]*	1.00	3267	2923
	Trial type:induction FA:trait mindfulness	0.02 (0.14)	[−0.26, 0.29]	1.00	3083	2750
	Trial type:induction OM:trait mindfulness	0.06 (0.14)	[−0.21, 0.34]	1.00	3255	2690
	Trial type:induction C:trait mindfulness	−0.08 (0.14)	[−0.36, 0.19]	1.00	3386	3111

Trial type = incongruent, FA = focused attention, OM = open monitoring, C = control. *95% CI does not contain 0

Table 14 Model output for exploratory ERN moderated by trait mindfulness

Model	Fixed effects	Estimate (SD)	95% CI	R-hat	Bulk ESS	Tail ESS
ERN	Intercept	3.79 (1.74)	[0.57, 7.42]*	1.00	3318	2598
	Response	−2.30 (0.15)	[−2.58, −2.01]*	1.00	8203	2680
	Induction FA	0.23 (0.22)	[−0.18, 0.66]	1.00	6270	3415
	Induction OM	−0.11 (0.21)	[−0.53, 0.30]	1.00	5681	3294
	Induction C	−0.12 (0.21)	[−0.52, 0.31]	1.00	5323	2704
	Trait mindfulness	−0.14 (0.32)	[−0.78, 0.49]	1.00	3088	2768
	Task number	−0.27 (0.52)	[−1.33, 0.74]	1.00	2982	2596
	Session number	0.35 (0.18)	[0.01, 0.70]*	1.00	7510	3080
	Response:Induction FA	0.10 (0.21)	[−0.30, 0.50]	1.00	5720	3180
	Response:Induction OM	0.15 (0.21)	[−0.25, 0.55]	1.00	6241	3302
	Response:Induction C	−0.24 (0.21)	[−0.65, 0.16]	1.00	5808	3404
	Response:trait mindfulness	0.51 (0.18)	[0.16, 0.85]*	1.00	7994	3105
	Induction FA:trait mindfulness	0.08 (0.26)	[−0.42, 0.60]	1.00	5107	3053
	Induction OM:trait mindfulness	−0.25 (0.29)	[−0.81, 0.31]	1.00	4859	3077
	Induction C:trait mindfulness	0.17 (0.25)	[−0.34, 0.68]	1.00	5693	3099
	Response:induction FA:trait mindfulness	0.03 (0.25)	[−0.46, 0.52]	1.00	5505	3423
	Response:induction OM:trait mindfulness	0.07 (0.26)	[−0.46, 0.52]	1.00	5685	2998
	Response:induction C:trait mindfulness	−0.10 (0.25)	[−0.45, 0.58]	1.00	5387	3394

Response = error, FA = focused attention, OM = open monitoring, C = control. *95% CI does not contain 0

attention (Ganesan et al., 2022; Lutz et al., 2008). Given that such skill is likewise paramount to cognitive control tasks that demand recruitment of executive attention and conflict monitoring (e.g., flanker, Stroop, Go/NoGo paradigms), we reasoned that adoption of the FA state would confer distinct advantages during flanker performance, including modulation of ERP components (i.e., P3 and ERN) that are known to be sensitive to selective attention processing (Polich, 2007; Yeung et al., 2004). Similarly, the null finding that OM did not enhance the Pe contradicts our hypothesis that

Table 15 Model output for exploratory Pe moderated by trait mindfulness

Model	Fixed effects	Estimate (SD)	95% CI	R-hat	Bulk ESS	Tail ESS
Pe	Intercept	3.99 (1.82)	[0.39, 7.64]*	1.00	1116	1703
	Response	3.05 (0.17)	[2.71, 3.37]*	1.00	4755	2860
	Induction FA	−0.21 (0.24)	[−0.70, 0.26]	1.00	3390	2628
	Induction OM	−0.03 (0.25)	[−0.52, 0.47]	1.00	3128	2513
	Induction C	0.23 (0.24)	[−0.24, 0.69]	1.00	3927	3248
	Trait mindfulness	−0.78 (0.68)	[−2.16, 0.53]	1.00	1232	1833
	Task number	−1.09 (1.13)	[−3.31, 1.12]	1.00	1078	1793
	Session number	−0.10 (0.21)	[−0.50, 0.31]	1.00	4161	3191
	Response:induction FA	0.05 (0.24)	[−0.42, 0.51]	1.00	3700	3187
	Response:induction OM	0.05 (0.23)	[−0.41, 0.49]	1.00	3399	3272
	Response:induction C	−0.10 (0.24)	[−0.56, 0.38]	1.00	4130	3183
	Response:trait mindfulness	0.40 (0.20)	[0.01, 0.80]*	1.00	5082	3378
	Induction FA:trait mindfulness	−0.12 (0.30)	[−0.71, 0.45]	1.00	2986	3093
	Induction OM:trait mindfulness	−0.24 (0.32)	[−0.55, 0.40]	1.00	3258	2915
	Induction C:trait mindfulness	0.36 (0.29)	[−0.20, 0.92]	1.00	3516	3076
	Response:induction FA:trait mindfulness	0.01 (0.29)	[−0.55, 0.58]	1.00	3714	2871
	Response:induction OM:trait mindfulness	−0.03 (0.30)	[−0.62, 0.56]	1.00	3352	2313
	Response:induction C:trait mindfulness	0.02 (0.28)	[−0.53, 0.59]	1.00	3525	3106

Response = error, FA = focused attention, OM = open monitoring, C = control. *95% CI does not contain 0

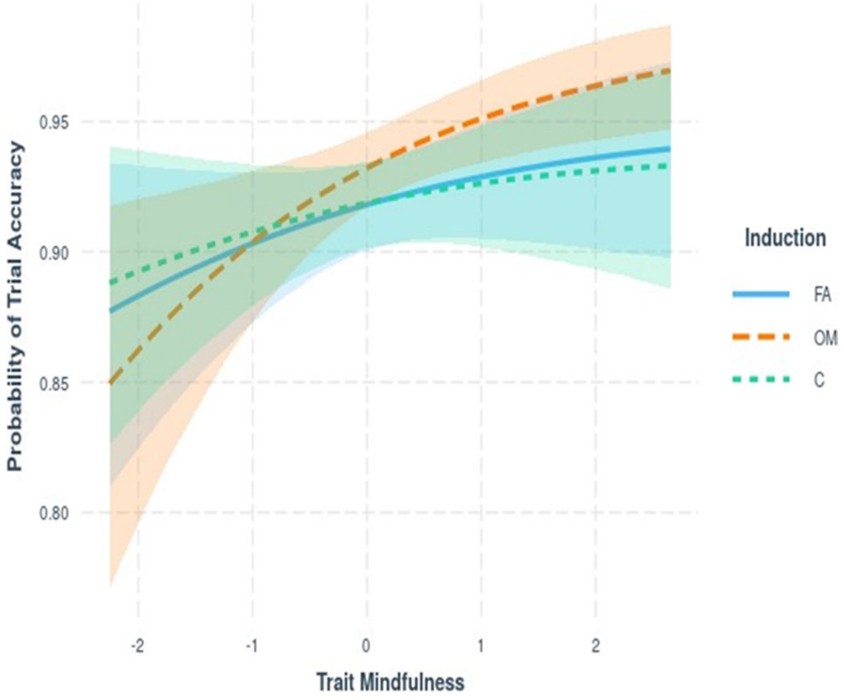

Fig. 2 Predicted probability plot of trial accuracy plotted as a function of trait mindfulness and induction condition

the increased attentional aperture and interoceptive properties of OM (Lutz et al., 2008; 2015) may foster greater bodily awareness of error commission (Lin, Eckerle et al., 2019; Ullsperger et al., 2010, 2014). Taken together, the fact that we were unable to obtain any supporting evidence for our *a priori* hypotheses indicates converging grounds for falsification and suggests that our underlying assumptions and rationale need revision.

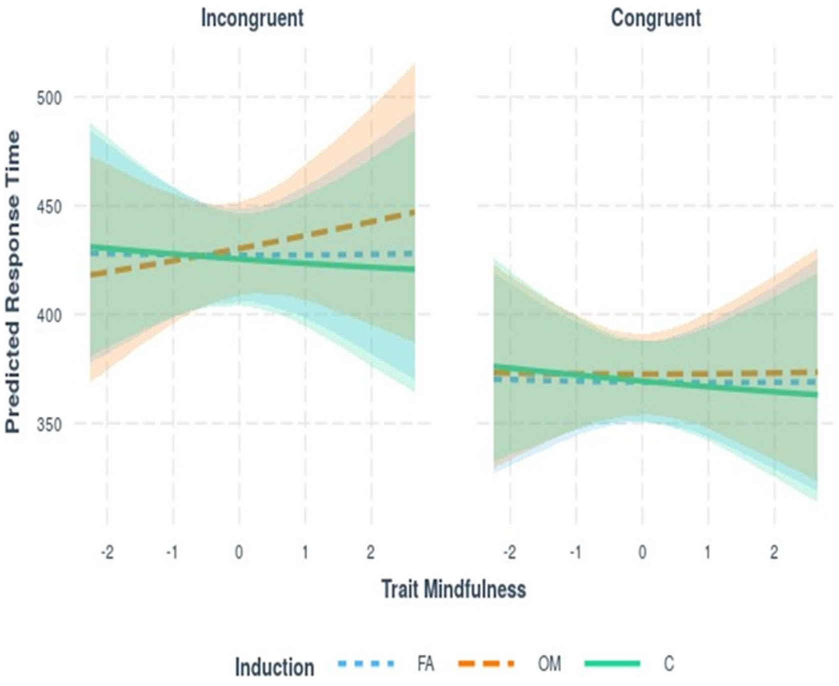

Fig. 3 Predicted plot of trial response time plotted as a function of trait mindfulness and induction condition separated across incongruent and congruent trials

Toward this end, perhaps the most parsimonious possibility to consider is that brief state mindfulness inductions (both FA and OM) simply do not modulate executive attention and error monitoring among novice non-meditators. Although a strong claim, this conclusion is supported by the novel features and strengths of our study design and analytic approach. In particular, our ability to directly manipulate FA, OM, and active control states both before and during actual task performance enables highly rigorous comparisons to delineate the unique and shared cognitive influence of distinct mindfulness states. Furthermore, our statistical models were stringent in controlling for individual differences in trait mindfulness and baseline performance ability, in addition to several key experimental confounds (e.g., session and task-order effects). Relatedly, the use of Bayesian estimation methods permitted us to quantify the strength of evidence in favor or against each hypothesis, with the posterior distributions of the predicted interaction estimates revealing null but as of yet inconclusive effects.

In addition to contextualizing our results within the specific methodological features of the current study, it is likewise important to weigh the implications of our null findings against the state of the broader literature. As mentioned earlier, previous mindfulness induction studies involving similar cognitive control tasks have yielded variable outcomes. For example, several studies have reported null effects on behavioral performance (Bing-Canar et al., 2016; Larson et al., 2013; Lin, Eckerle et al., 2019; Rodeback et al., 2020), whereas others have observed improved (Gorman & Green, 2016; Keng et al., 2013) or mixed (Saunders et al., 2016) performance. Likewise, findings pertaining to error monitoring ERPs have been similarly mixed; some studies reported that mindfulness inductions produced smaller Pe responses but no change in the ERN (Larson et al., 2013), larger Pe but no change in the ERN (Lin, Eckerle, et al., 2019; Rodeback et al., 2020), no change in the Pe but larger ERN (Saunders et al., 2016), or no modulation of neither Pe nor ERN (Bing-Canar et al., 2016).

Reflecting upon the inconsistency of the prior literature, a critical methodological caveat is that nearly all previous studies utilized between-group designs that involve markedly different experimental manipulations, with minimal specification and standardization for the *type* of mindfulness induction or control conditions compared across studies. For example, previous work ranged widely from comparing mindfulness versus stress inductions in Rodeback et al. (2020) to mindfulness of thoughts versus mindfulness of emotions in Saunders et al. (2016). Findings involving mindfulness manipulations are inherently shaped by the specific conditions being compared; thus, it is vital to construct study designs that include clear and conceptually meaningful comparisons that are pragmatically amenable to replication and extension. Consequently, implementation of tightly controlled comparisons between FA and OM, two widely recognized and well-defined mindfulness states, both benchmarked against an active control condition, represents a significant advance in remediating these previous limitations.

As alluded to in the introduction, research designs aimed at parsing the cognitive influence of mindfulness states are most sensibly, although often implicitly, referring to state change effects that presumably occur within- as opposed to between-individuals. Consequently, a fully within-subject experimental approach may represent a more "ecological" and empirically valid means to test the core research question at hand. From this perspective, our findings may help to clarify some of the extant ambiguity by providing new evidence that within-subject adoption of FA states do not produce discernable effects, relative to non-mindfulness control states, on well-characterized markers of executive attention and error monitoring.

Conceptual and methodological implications of unpredicted OM findings

With all that said, however, it was not the case that only null effects of the state induction paradigm were observed. Indeed, the surprising finding that the OM induction increased overall performance accuracy and RT, while reducing the P3 across both trial types, is suggestive that OM may have selectively fostered a more deliberate and intentional responding style where accuracy was prioritized over speed. Indeed, our follow-up analyses largely confirmed this possibility, revealing that slowing down on incongruent trials during only the OM induction produced higher accuracy. Moreover, the smaller P3 response may be indicative of a broadband reduction in attention allocation to the flanker stimuli (Clayson & Larson, 2011; Polich, 2007), possibly reflecting the OM-specific instruction to direct non-judgmental awareness and curiosity across various facets of arising experience during task performance (e.g., thoughts, feelings, physical sensations). Although highly speculative, this interpretation receives tangential support from the unexpected finding that participants rated the flanker task as more interesting during the OM session.

From a cognitive control perspective, this pattern of results is in line with the intriguing, but likewise speculative possibility that OM may promote cognitive efficiency, insofar that higher performance accuracy was observed in tandem with evidence of P3 modulation, a key neural marker of attentional updating. Interestingly, this notion is echoed by a very recent study showing that a brief, although unspecified, mindfulness induction similarly improved overall flanker accuracy while reducing global P3 amplitude using a pre-post between-groups design (Aly et al., 2023). Relatedly, another study involving cigarette smokers also reported reduced P3 amplitudes on a smoking themed Go/NoGo task following an unspecified mindfulness induction (Andreu et al., 2018). Although far from a replication given the substantial sample and task differences, the similar pattern of P3 modulation is notable given the paucity of induction studies that have measured the P3 during cognitive task performance.

Critically, our findings complement and extend these prior studies, suggesting that these effects may be *specific* to the cultivation and adoption of the OM state as opposed to FA. It should be noted that although both previous studies did not specify the type of mindfulness induction used, the descriptions provided are similar to that of OM (e.g., "…accept feelings, sensations, or thoughts in a mindful, non-judgmental way"; Andreu et al., 2018, p. 4). Along these lines, it may be valuable to consider why FA did *not* improve flanker performance, despite considerable conceptual reasons to suggest otherwise. One possibility pertains to the fact that FA involves adopting a relatively narrow scope of attention, where focus is continuously directed and sustained on a singular target object. Consequently, for novices in particular, FA may impose greater constraints and demands on the attentional control system, which may not be conducive for optimal task performance (see Esterman & Rothlein, 2019; Huang et al., 2023 for reviews). Another related possibility is that transference/maintenance of the FA state from induction to task performance may be more cognitively demanding insofar that attention is continuously being manipulated and sustained, whereas the task-related influence of OM could be driven from attitudinal, and less cognitively effortful, shifts related to the emphasis on cultivating non-judgment and non-reactivity. Taken together, it is therefore possible that with prolonged training, FA may produce similar or greater improvement in task performance than OM, but this idea remains in need of testing. This is a question that our approach is well-equipped to answer and one that our own group is preparing to address. We detail this plan, among other promising future directions, in the final section to follow.

Lastly, our exploratory analyses shed light on the interplay between trait and state mindfulness, providing complementary support to advance a more nuanced understanding of our primary findings. Most notably, our results suggest that trait mindfulness amplified the effects of the OM induction. Specifically, for individuals with greater trait mindfulness, the OM induction enhanced trial accuracy while slowing RTs. Moreover, greater trait mindfulness was associated with larger reductions in P3 amplitude during the OM induction relative to the active control condition, suggesting that for higher trait mindful individuals, cultivation and on-task engagement of the OM state may produce stronger attenuations in flanker attention processing. Together, these patterns represent a clear and highly compelling demonstration that trait mindfulness *uniquely* moderates the influence of the OM state in ways that are remarkably consistent with the "standalone" main effects of the induction manipulations across both brain and behavior. In other words, these findings provide support for the intuitive (and nomologically consistent) but relatively untested notion that the effects of state mindfulness manipulations may be more robust among more dispositionally mindful individuals.

It is worth noting that OM has been conceptualized by some as a more advanced mindfulness state relative to FA (Laukkonen & Slagter, 2021; Lutz et al., 2008); therefore, it could be consistent that stronger OM effects are observed in more “naturally” mindful individuals. Although intriguing, we caution against this line of reasoning insofar that our state inductions are by no means a proxy for dedicated meditation practice and do not involve the important contextual factors that inform and contribute to the understanding of OM as an advanced or later-stage meditation practice. It remains an open (and very interesting) empirical question whether initialization of FA training can bolster the practice and utilization of OM states. Sequential training designs, including potential use of state inductions, intermixing the order and amount of FA versus OM could offer a promising avenue toward addressing that question.

From a methodological perspective, the findings exemplify one of the core advantages of our converging operations research strategy, such that variability associated with different operational facets of mindfulness, including their interaction(s), can be modeled directly. It is worth mentioning that by explicitly modeling the interaction between trait and state mindfulness, we were able to clearly show that distinct operationalizations of mindfulness may indeed confer differential, and possibly interactive, effects on outcomes, depending upon what is being measured. Specifically, our findings demonstrate that behavioral performance and the P3 were modulated by state mindfulness (i.e., the OM induction) and further moderated by trait mindfulness level. Conversely, the ERN and Pe were selectively related to trait mindfulness but not state mindfulness, introducing the intriguing possibility that state mindfulness influences on error monitoring may require longer periods of training (particularly for novice nonmeditators), where repeated practice is needed to transform effortful engagement of potentially new and unfamiliar psychological states into a more habitual trait like quality. Indeed, the ability to partition variance associated with distinct facets of the mindfulness construct enables not only a more granular and precise understanding of how mindfulness impacts cognition but also may lead to the generation of new promising data-driven hypotheses that may be used to guide the design of future studies.

Limitations and future directions

The current study employed a novel within-subject state induction design combined with Bayesian mixed modeling to demonstrate that acute cultivation and engagement of FA states do not modulate neurobehavioral indices of executive attention or error monitoring in novice non-meditators. Moreover, we obtained strong, but unexpected evidence linking OM to a more intentional, and possibly self-aware, response style characterized by higher accuracy, slower RTs, and reduced P3 amplitudes. Finally, our exploratory analyses revealed that trait mindfulness amplified the effects of the OM induction, such that individuals with greater trait mindfulness exhibited higher trial accuracy, slower RTs, and smaller P3s. Capitalizing on the methodological innovations of our converging operations framework, and in particular the strategic use of state inductions as a foundational building block for future study designs, these findings represent an important contribution to help clarify and advance the mindfulness cognition literature.

Despite these notable strengths, the current study has a number of limitations that are important to acknowledge. First, it goes without saying that the results reported require replication and extension. Indeed, the generalizability of the conclusions offered are intrinsically limited by, and likely contingent upon, the specific task parameters, analytic features, and sample characteristics of the study. Although we were able to obtain nearly 90 sessions worth of data while using trial-level modeling procedures to increase the reliability of our estimates, our analyses would nevertheless be strengthened from having a larger sample with more data.

There is high value in conducting careful follow-up studies to incrementally validate and expand upon the current findings. In considering how to approach this, it is worth noting that in lieu of the null effects reported here, it may be prudent to conservatively assume small effect sizes when planning future state induction studies (as opposed to the prospective assumption of a medium effect size for the current study). Better yet, we strongly encourage future research to make full use of our actual data. As we elaborate further below, our results and shared analysis code are readily amenable to Bayesian updating procedures (Ly et al., 2019; Verhagen & Wagenmakers, 2014; Wagenmakers et al., 2018), through which exact values of all model parameters can be specified and tested against newly accumulated data, and subject to Bayesian sequential stopping rules until a desired amount of evidence is obtained (Schönbrodt et al., 2017; Schönbrodt & Wagenmakers, 2018).

It also must be cautioned that our null findings do not necessarily imply that FA exerts no influence over, or is fundamentally unrelated to, executive attention, error monitoring, or cognitive functioning more broadly. One of the most salient limitations of the study pertains to the fact that our novice participants did not actually complete any extended form of mindfulness training. Although the absence of prospective training was useful in isolating acute state and trait effects, it remains unclear whether and how extended FA or OM training would differentially impact flanker task performance. Given the moderating role of trait mindfulness, as well as the selective relationship between trait mindfulness and error monitoring ERPs reported here, there are compelling reasons to posit that the nature and strength of influence of mindfulness states on cognitive functioning may at least be partially dependent on the amount of previous mindfulness training.

Lastly, although it may be intriguing to consider how the influence of longer-term mindfulness training may impact the effects of the state inductions, it must be explicitly noted that the pattern of findings reported here are circumscribed to novice (i.e., mindfulness-naïve) participants. Thus, any extrapolation beyond the limits of this important boundary condition should be made with caution. Relatedly, despite our best efforts to ensure proper manipulation and maintenance of the target states, it remains fundamentally unclear the extent to which novice participants were able to successfully perform and adhere to the guided inductions. Toward this end, future studies may benefit from incorporating subjective and/or objective queries (e.g., task or user-initiated probes throughout induction and task performance) to directly assess state engagement quality. With all that said, research examining the interplay between state mindfulness and mindfulness training remains rare and is in fact a specific line of investigation that our converging operations framework was designed to stimulate.

Indeed, one highly promising future direction that our group has already begun to pursue involves systematically expanding the current scope of investigation to include mindfulness training. As outlined in our previous work, this would essentially involve embedding the current state induction protocol into the pre- and post-training assessment periods of a prospective mindfulness training design. This approach offers a number of analytic advantages (see Lin, Tang, et al., 2022 for more details), including the crucial ability to conduct incremental replication. In other words, because the state induction protocol is standardized across studies, we can fully capitalize on Bayesian updating procedures to not only conduct a replication of the current study (by entering the parameter estimates obtained as informed priors in the analyses of the pre-training data collected during the longitudinal study) but also systematically extend the work by deriving precise estimates for the degree to which mindfulness training modulates the state induction effects by analyzing post-training changes to the posterior distribution of parameter estimates. Completing this study would provide an invaluable opportunity to incrementally evaluate both the validity and generalizability of our findings in addition to further testing the utility of the converging operations framework more broadly.

As mentioned earlier, the current study was circumscribed to measures collected from the flanker task to provide a first "proof-of-concept" empirical demonstration of our state induction protocol. In the near future, we plan to build off this foundation to show how FA and OM influence other cognitive tasks to establish convergent and divergent validity. For example, cognitive control tasks, including the Stroop, AX-CPT, and cued task-switching have been adapted to isolate dimensions of proactive and reactive control (Braver et al., 2021; Tang et al., 2022). We and others have suggested that OM may be particularly aligned to enhance reactive control (which may be more strongly utilized in the current flanker version), whereas FA may be particularly aligned to enhance proactive control (Samuel & Costanzo, 2020; Tops et al., 2014). Therefore, using other cognitive tasks, such as the AX-CPT that are well-adapted for the purpose of differentiating modes of cognitive control, would allow for a more precise test of this hypothesis. Likewise, other categories of tasks can be utilized to assess psychological constructs that are distinct from attention and cognitive control (e.g., affective picture viewing task to probe emotion reactivity).

Finally, we urge the use of other analytic tools to further probe the boundary conditions that undergird the effects reported here. For example, the application of drift diffusion modeling (DDM) could provide further knowledge of how FA and particularly OM might influence the process of evidence accumulation and decision making parameters behind flanker response selection (see van Vugt et al., 2019 for an extended discussion about the use of DDM approaches in mindfulness research). Additionally, trial-by-trial ERP approaches could provide further clarification on the functional relationship between OM related P3 modulation and behavioral performance. Last but certainly not least, we strongly encourage other researchers to consider adopting our converging operations framework, including the state induction protocol described here. Together, we look forward to advancing a more systemized science of mindfulness, where incremental replication and extension are prioritized through the use of a unified yet flexible methodological framework.

Open practices statement The study was preregistered (https://osf.io/2zk3s), and all data, analytic code, and model output are publicly available on the Open Science Framework (OSF; https://osf.io/uv9yn/)

Data Availability All data are shared on the Open Science Framework (OSF; https://osf.io/uv9yn/).

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