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Effects of short mindful breathing meditations on executive functioning in two randomized controlled double-blinded experiments

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ABSTRACT

While current models of mindfulness propose benefits to the executive functions of inhibition, updating and shifting through mindful breathing meditations, empirical findings on the effects of short mindful breathing meditations are inconclusive regarding their specificity and dose-response relations. Therefore, we compared short mindful breathing meditations (Experiment 1, 45 min over three sessions; Experiment 2, 80 min over four sessions) with relaxation trainings (progressive muscle relaxation; active control) and listening to podcasts (passive control) in two randomized controlled double-blinded trials. Reaction time tasks were used to assess the executive functions of updating (N-Back), inhibition (CPT-II), and shifting (Number-Letter Task). Results of both experiments suggest no mindfulness-specific improvements in executive functions. We conclude that effects following the first stages of mindfulness training may not be specific to the practice or too transient to be reliably measured in pre-post intervention designs. Implications for research in the field are discussed.

1. Introduction

In contemporary scientific approaches, mindfulness is conceptualized as the ability to focus one's attention on being aware of the present moment, while maintaining a non-judgmental attitude towards current experiences, including any emotions, thoughts, and feelings that may be present (Kabat-Zinn, 1994). It has been proposed that mindfulness as a mental state comprises two facets, namely 1) the intentional regulation of attentional resources to achieve awareness of the experience of the present moment and 2) the quality of paying attention, or mindful attitude, which can be characterized as maintaining a curious, accepting and non-judgmental attitude towards thoughts, feelings or emotions that may arise in the current moment (Bishop et al., 2004; Isbel & Summers, 2017; Lau et al., 2006). The evocation of a mindful state can be trained and achieved by meditation, with breathing meditation being a central practice in most mindfulness trainings. During this practice, the practitioner is instructed to focus their attention on the in- and outflow of breath, for example by focusing on the sensation of air streaming in and out of the nostrils or the sensation of the chest rising and falling during breathing. If the mind is distracted by emerging thoughts, emotions or bodily sensations, the practitioner is to direct their attention back to the breath, while maintaining a non-judgmental attitude towards any distractions and gently letting them go. This practice is an example of a focused attention meditation in the classification by Lutz et al. (2008).

It has been proposed that this intentional orientation, monitoring and maintenance of attention during meditation practice trains cognitive control abilities, with attentional and executive functions considered to be core mechanisms (Bishop et al., 2004; Hölzel et al., 2011; Jankowski & Holas, 2014; Malinowski, 2013; Shapiro et al., 2006; Tang et al., 2007). For example, Bishop et al. (2004) posit two cognitive aspects of mindfulness meditation (alongside the non-cognitive aspect of the mindful attitude): 1) the monitoring of directed attention to the object in focus (e.g., the breath during a breathing meditation) and 2) executive control, enabling the inhibition of irrelevant thoughts, feelings, and sensations, as well as shifting attention back to the object in focus when distractions arise. Similarly, Jankowski and Holas (2014) postulate that the evocation and maintenance of a mindful state stem from monitoring current experiences (e.g., thoughts, perceptions, imaginations), which in turn enables disengagement from distractions through cognitive control mechanisms such as sustained attention, inhibition and task switching. Thus, both models consider the executive functions as defined by Miyake et al. (2000; i.e., updating, inhibition, shifting) as central mechanisms involved in eliciting and maintaining a mindful state and propose beneficial effects on these functions given repeated practice. Updating refers to the updating and monitoring of information in working memory, inhibition to the ability to inhibit

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dominant or prepotent responses or task-irrelevant mental content and shifting to the ability to flexibly switch between tasks or mental sets.

However, it has yet to be determined whether focused attention meditation leads to domain-general improvements in executive control, as models propose, or rather to domain-specific improvements. In accordance with Bishop et al. (2004) and Jankowski and Holas (2014), Gallant (2016) argued that the withdrawal of attentional resources away from distractions and the subsequent re-focusing of attention back to the task at hand specifically involve and train cognitive inhibition and shifting, respectively, and do not directly involve or improve updating abilities. However, focused attention meditation may have an indirect effect on working memory capacity by reducing stress or heightening relaxation and thereby enhancing attentional focus to the current task set (see also, Eysenck et al., 2007; Jha et al. 2010 & 2019). In their systematic review, Gallant found evidence of improved inhibitory control due to mindful breathing meditation, while results for shifting and updating were inconclusive. Further reviews have similarly concluded that mindful breathing meditation has beneficial effects on inhibitory ability, but not on updating and shifting (Leyland et al., 2019; Lodha & Gupta, 2022; see Chiesa et al., 2011, and Gill et al., 2020, for different classifications of the cognitive effects on mindful breathing meditation).

While models thus propose domain-general improvements in executive functioning, reviews have mainly argued in favor of inhibition-specific benefits. Such inhibition-specific improvements may be in line with the finding that cognitive inhibition largely overlaps with general attentional control processes captured by the construct of common executive functioning, while updating and shifting are considered to contribute separate executive functionality (Miyake & Friedman, 2012). Thus, cognitive inhibition may be considered a core aspect of the common executive control ability to deliberately maintain task goals and effectively manipulate lower-level processing. In line with this, Isbel and Summers (2017) consider inhibition to be the foundation for the development of the sustained attention required for the present-moment experience during mindfulness meditation. While the ability to deliberately inhibit distractors that interfere with the current task is a core aspect of Isbel & Summers' capacity model of mindfulness, they also propose that the repeated engagement of all three executive functions leads to corresponding beneficial effects.

In order to reconcile model predictions and empirical findings, it seems essential to take level of expertise and training durations into account. For novices, practicing mindful breathing meditation is effortful and accompanied by frequent mind wandering; thus, monitoring and inhibiting distractions will be a central aspect in initial practice. Therefore, beneficial effects on inhibitory as well as updating abilities may be expected in initial training phases (e.g., Lutz et al., 2015), and accordingly, Lodha and Gupta (2022) note that studies utilizing shorter training periods also provide evidence for improvements in updating. In contrast, long-term practitioners may succeed in maintaining a mindful state with greater ease and without continuously monitoring their mental content (Lodha & Gupta, 2022; Lutz et al., 2015). Relevant to this question of dose-response, a growing body of research has focused on postulated effects on executive functioning after brief mindful breathing meditation trainings. Studies investigating the underlying cognitive mechanisms have either employed a single training session or several short meditation sessions over a brief period, usually over the course of a week.

Regarding cognitive inhibition, Mrazek et al. (2012; second experiment) reported improved inhibitory control following an 8-minute breathing meditation compared to a relaxation control condition (relax without falling asleep) and a reading control condition. Following 10 min of mindful breathing, Norris et al. (2018) found improvements in inhibition compared to a passive control condition in which participants listened to a recording of a magazine article. Likewise, Wenk-Sormaz (2005; Study 1) reported improved cognitive inhibition following a 20-minute breathing meditation in comparison to a cognitive task and a rest condition. However, further studies utilizing comparable meditation durations have reported no beneficial effects: Larson et al. (2013) compared a 15-minute breathing meditation to a passive control condition (listening to educational material about mindfulness) and found no improvement in cognitive inhibition, and Polak (2009) reported no effects of 2 × 15 min breathing meditation compared to a relaxation condition and a control condition in which participants were asked to make a mental list of what they did the previous day. Vieth and von Stockhausen (2022) assessed the effects of a 2 × 20-minute breathing meditation compared to a relaxation induction and a passive control condition (podcast listening), finding that both mindful breathing meditation and relaxation led to improved inhibitory control. Thus, findings on cognitive inhibition after brief breathing meditations are inconclusive.

For shifting, Jankowski and Holas (2020) found no significant improvements following a 10-minute breathing meditation compared to an auditory worry induction and a free mind-wandering condition. Similarly, Johnson et al. (2015) reported no differences in shifting between participants who listened to a 25-minute breathing meditation, a sham meditation (lacking instructions to guide one's attention to the breath and including longer periods of silence) or an audiobook listening control condition. Vieth and von Stockhausen (2022) found improved shifting after breathing meditation (2 × 20 min) compared to relaxation, but both trainings were outperformed by the passive control condition (podcast listening). Considering the lack of studies finding improvements in shifting, it has been argued that the tasks utilized to measure shifting abilities are more related to visual and motor shifting (e.g., tasks in which participants are required to respond to stimuli according to certain rule sets and need to shift motor responses in accordance with visually presented cues), while mindful breathing meditation trains mental set shifting by refocusing attentional resources away from internal distractions and back to the task at hand (Heeren et al., 2009; Lodha & Gupta, 2022). As a further complication, research has shown that common executive ability and shifting can be considered somewhat opposing constructs, in that the former describes the stability in maintaining a current task set, while the latter can be defined as the ability to flexibly transition to a new task (Miyake & Friedman, 2012). Therefore, it may be argued that increased stability in task-oriented attentional focus (i.e., increased cognitive inhibition or common executive functioning) following mindful breathing meditation hinders flexibly shifting between new task-set representations. In support of this are findings by Wolff and Beste (2020), who found shifting abilities to be impaired following 15 min of mindful breathing. However, the majority of findings discussed above do not provide evidence for such impairment compared to passive or active control conditions.

Johnson et al. (2015) assessed updating/working memory after 25 min of breathing meditation and reported no effects compared to both control conditions. Vieth and von Stockhausen (2022) found that both mindful breathing meditation and relaxation were followed by improved updating compared to the passive control condition. Likewise, Zeidan et al. (2010) reported improvements in updating after a week-long breathing meditation training (4 × 20 min) compared to a passive control condition listening to an audiobook. Taken together, these results are in line with Lodha and Gupta's (2022) notion of improvements in updating following short trainings, but the effects may require more than a single practice session.

In summary, the studies discussed so far utilizing brief mindful breathing meditation trainings have provided at least partial evidence for domain-specific improvements in the executive functions of cognitive inhibition and updating/working memory. Yet the findings are inconsistent, and what is more, the integration of the described results is impeded by methodological differences between the studies. One challenge in comparing results stems from variations between active control conditions. While active controls are necessary to identify specific effects of mindfulness training, those discussed above control for very different aspects of training or contrasting mental states (e.g., mind wandering in Jankowski & Holas, 2020; sham meditation in Johnson et al., 2015; relaxation in Vieth & von Stockhausen, 2022). Furthermore, some studies include only passive control conditions (e.g., Zeidan et al., 2010). However, to interpret effects as mindfulness-specific and going beyond effects of repeated testing, both an active and a passive control condition are required (Davidson, 2010; Vago et al., 2019).

Methodological differences and inconsistencies also render it impossible to estimate dose-response relations. For example, using pre-post designs with active and passive control conditions, Polak (2009) and Vieth and von Stockhausen (2022) found no mindfulness-specific effects on inhibitory control after total training durations of 30 and 40 min, while Mrazek et al. (2012) report improvements in inhibitory control following 8 min of mindful breathing compared to a relaxation condition and a passive control condition in a post-test only design.

In summary, while models (Bishop et al., 2004; Isbel & Summers, 2017; Jankowski & Holas, 2014) as well as reviews (Gallant, 2016; Jha et al., 2019; Lodha & Gupta, 2022) propose beneficial effects of mindful breathing meditation on some or all of the executive functions of updating, cognitive inhibition and shifting (Miyake et al., 2000; Miyake & Friedman, 2012), it is still not understood whether the proposed benefits on executive functioning are domain-specific or domain-general, and what the underlying dose-response relation of such effects would be. The described results of studies utilizing brief mindful breathing meditation trainings to investigate beneficial effects in initial training phases are inconclusive, but provide partial evidence for improvements in cognitive inhibition and updating. However, the high heterogeneity in experimental designs (e.g., lack of active control conditions, variety of active control conditions, or the lack of pre-post designs) hinders the integration and generalizability of findings.

Obviously, these methodological problems cannot be solved with one or two studies. However, the present study aims to address some of these issues across two experiments in the following way: First, we follow up on the discussion of underlying mechanisms of brief trainings. Given that maintaining attentional focus is difficult and effortful for meditation novices (Lutz et al., 2015), it has been argued that effects of brief mindful breathing meditation trainings in beginners may be unspecific to the practice and may in fact be achieved through relaxation (Fell et al., 2010; Vieth & von Stockhausen, 2022). It may be that for novices, mindful breathing meditation does not specifically improve executive functioning, but rather evokes relaxation, which reduces dysfunctional tension, freeing up resources for improved cognitive control (Eysenck et al., 2007). Contrasting the effects of mindful breathing meditation with relaxation techniques in designs utilizing brief trainings may thus provide insights into which mechanisms are at play at the first stages of meditational training. Therefore, we controlled for relaxation effects (active control) as well as for effects of repeated testing (podcast listening; passive control), while keeping the mode of delivery constant across conditions (i.e., all conditions were delivered via audio recordings). Secondly, we addressed the question of dose-response relation by varying the duration of practice time while keeping the total experimental time frame constant (over the course of 5 days). To this end, we increased practice time from 45 min (Experiment 1) to 80 min (Experiment 2), in order to provide insights into when mindfulness-specific processes may start to unfold. Third, we utilized the same reaction time measurements as Vieth and von Stockhausen (2022) to assess executive functions as defined by Miyake et al. (2000) to increase comparability across experiments and studies. Furthermore, we assessed state mindfulness and relaxation with the Smith Relaxation States Inventory 3s (SRSI3s; Smith, 2019) as well as positive and negative affect with the Positive and Negative Affect Schedule (PANAS; Watson et al., 1988) as manipulation checks and controlled for differences in trait mindfulness between groups with the Five-Facet Mindfulness Questionnaire (FFMQ; Baer et al., 2006), as trait mindfulness has been shown to have a moderating role on the effectiveness of mindfulness training (e.g., Shapiro et al., 2011). As training and recording took place during the COVID-19 pandemic, we also assessed experienced pandemic impact and burden by self-report to control for possible effects on intervention effectivity (Di Corrado et al., 2020; Kontoangelos et al., 2020).

In the present study, we report on two randomized controlled double-blinded trials in which we compared the effects of a brief mindful breathing meditation training with a total of 45 min (Experiment 1, three sessions of a breathing meditation) and a brief mindfulness training with a total of 80 min of practice (Experiment 2, four sessions of a breathing meditation) with the effects of a relaxation training as an active control condition and podcast listening as a passive control condition on the executive functions of updating, inhibition, and shifting (Miyake et al., 2000). Relaxation was induced by Progressive Muscle Relaxation (PMR), which was selected as a standardized and evidence-based method for inducting a relaxed state (Manzoni et al., 2008; McCallie et al., 2006; Toussaint et al., 2021). All aspects of the experimental design except for training duration and number of training sessions were kept constant between the experiments to allow for a direct investigation of the dose-response relationship of mindful breathing meditation. Furthermore, the experimental designs closely followed Vieth and von Stockhausen (2022), who found specific effects of a short mindfulness induction (compared to relaxation) only for shifting but not for inhibition, updating/working memory or attentional networks and argued that mindfulness-specific effects might need more practice time to unfold. To test this prediction, we implemented a similar total duration of practice time in Experiment 1 and extended the training duration and number of training sessions in Experiment 2. The reaction time tasks utilized were selected based on their validity to separately assess the executive functions of updating, cognitive inhibition and shifting (Miyake et al., 2000; Suchy, 2009). Both inductions and testing were delivered online.

If the effects of brief trainings in mindful breathing meditation on executive functions are domain-general and are different from those of relaxation, Experiment 1 should find improved shifting (as found by Vieth & von Stockhausen, 2022), improved inhibition (in accordance with Mrazek et al., 2012; Wenk-Sormaz, 2005), and improved updating (in line with Zeidan et al., 2010) in comparison to relaxation training (and compared to the passive control condition). Furthermore, if effects of short mindful breathing meditation trainings are stable and unfold with practice time, Experiment 2 should at least reproduce the findings of Experiment 1 or go beyond them.

2. General method

2.1. Participants

A total of 96 participants took part in Experiment 1 (74 female, 1 non-binary, $M_{age} = 23.5$, $SD_{age} = 5.43$), and a total of 69 participants in Experiment 2 (57 female, 1 non-binary, $M_{age} = 22.0$, $SD_{age} = 5.62$). Participants were recruited through social media and university internal message boards for research participation. Participation was restricted to individuals aged 18 or older who reported not having engaged in meditation or other mindfulness practices on a regular basis during the last three months.[1] If a person met the respective criteria, they were contacted by e-mail to arrange dates for online participation. At the date the participant had chosen for their first measurement point, they received an e-mail invitation with further information about participating in the online research (technical requirements and appropriate environment for participation) and a participation link. The link directed them to a page with written information about the used

[1] As experiments were conducted one year apart from each other, it was possible for participants to take part in both studies. Based on participation codes and demographics, we identified 11 participants who took part in both experiments. Statistical analyses for all dependent measures excluding repeated participants revealed no differences in the direction or size of effects as well as no change in significance of model terms or likelihood ratio tests.

methodology, data protection and research ethics. After giving informed consent to participation and data storage, participants could begin with the first measurement. Invitations to the practice sessions and post-measurement were sent by e-mail as well. After completing the study, participants received course credit for compensation. Data collection, storage and anonymization met the current standards of the General Data Protection Regulation of the European Union (GDPR 2016/679). The experiments were approved by the ethical commission of the Department of Psychology at the local university.

2.2. Assessment of executive functions

All tasks were programmed with OpenSesame (Mathôt et al., 2012; version 3.3.11) and presented on a server using a JATOS interface (Lange et al., 2015; version 03.06.2001). Access to reaction time tasks was restricted to desktop computers and participants were instructed not to use mobile devices or devices with a touch screen and to wear headphones to reduce background noise. Responses were recorded via participants' keyboards.

2.2.1. Inhibition: Continuous Performance Test-II

The Continuous Performance Test-II (CPT-II; Conners, 2000; Conners et al., 2003) measures executive and impulse control in responses to a rarely occurring non-target and allows for the investigation of set maintenance/cognitive inhibition (Suchy, 2009). During the CPT-II, participants were presented with a continuous stream of single capital letters and were to indicate by key press every time a letter appeared on the screen, with the exception of the letter “X” (90 % target trials; 10 % non-target trials). Each letter was presented for 250 ms and participants were able to make a response until the letter disappeared. Because the CPT-II requires responses to the frequent event of a target and not to the rare event of a non-target, it is well suited for collecting and investigating response times. Additionally, interstimulus intervals varied (ISI; 1000 ms, 1500 ms or 2000 ms), preventing habituation to a certain rate of presentation (i.e., adaptation of response speed in correspondence with ISI duration; Ballard, 2001).

Participants were instructed to place their right index finger on the space bar, to indicate targets by key press and to respond as quickly as possible without making mistakes. A session consisted of 360 trials (18 experimental blocks; 20 trials per block). After three blocks participants could take a short break if needed and were instructed to then continue with the experiment by key press. Between trials, participants were instructed to fixate on a fixation cross. Total task duration was 14 min on average.

2.2.2. Updating: N-Back Task

The N-Back task (Kirchner, 1958) is used to assess updating abilities and working memory capacity (Chatham et al., 2011; Suchy, 2009). During the task, participants are presented with a stream of single letters and are to indicate if the letter currently presented matches the letter shown n steps before. Letters were presented for 300 ms, the ISI was kept constant to 2500 ms, during which participants were still able to provide a response. To complete this task, participants need to keep information about previously presented letters in memory and make a comparison to the currently presented letter. As the task runs consecutively, participants need to constantly update the information held in memory. For example, during a 2-back block ($n = 2$), participants would indicate a target trial correctly if the following stream of letters occurred: H – G – H, but not if the presented letters were: H – G – X. The factor n was varied between blocks from 1- to 3-back trials to increase or decrease task difficulty. Blocks were presented with equal frequencies and in randomized order.

Participants were instructed to place their right index finger on the ‘L’ key and their left index finger on the ‘A’ key and indicate n-back and non-n-back trials accordingly. During the task, participants were instructed to wear headphones for auditory feedback during the practice blocks and for noise reduction during the rest of the task. Participants were instructed to respond as quickly as possible without making mistakes. A session consisted of two practice and eight experimental blocks (48 trials each). After each block, participants could take a short break if needed and were instructed to then continue with the experiment by key press. Total task duration was 18 min on average.

2.2.3. Shifting: Number-Letter Task

The Number-Letter Task assesses set shifting/task switching (Rogers & Monsell, 1995; Suchy, 2009). During the task, participants are presented with pairs of numbers and letters (for example A2, G5, K9) and are to execute one of two tasks depending on the stimulus' location. If the number-letter combination appears in the upper half of the screen (50 % of all trials), participants should indicate whether the presented number is odd or even. If the number-letter combination appears in the lower half of the screen, participants should indicate whether the letter is a consonant or a vowel. Stimuli were presented for 150 ms and the ISI was kept constant to 4000 ms, during which participants were still able to respond. Stimulus location was randomized, with participants either responding to repeated trials in the same location or switching tasks when the stimulus location changed. Comparing reaction time and response accuracy between tasks with and without task switching allows for investigating the ability to flexibly shift attention between tasks.

Participants were instructed to place their right index finger on the ‘L’ key and their left index finger on the ‘A’ key and to indicate either vowels or consonants or even or odd numbers, depending on the trial. Participants were instructed to respond as quickly as possible without making mistakes. A session consisted of three short practice blocks of eight trials each, during which participants received visual feedback in the form of either a green fixation cross indicating a correct response or a red fixation cross indicating a false response, followed by a single experimental block of 160 trials. Participants were instructed to fixate on a fixation cross in the center of the screen between trials. Total task duration was about 17 min.

2.3. Questionnaires

As a manipulation check, changes in state relaxation and mindfulness were assessed with the German version of the Smith Relaxation States Inventory 3s (Smith, 2019 & 2020; German version: Vieth et al., 2020). The SRSI3s is a multi-dimensional inventory which assesses states of relaxation-, meditation-, and mindfulness-related experiences on four levels: basic relaxation (e.g., “*I feel rested and refreshed*”), quiet focus and awakening (e.g., “*My mind is quiet and still*”), transformation/transcendence (e.g., “*Things seem timeless, boundless, or infinite*”), and positive transcendent emotion (e.g., “*I feel thankful*”). Furthermore, the inventory includes scales for physical, emotional, and cognitive stress, which will not be utilized in the current paper. Participants are asked to indicate on a 6-point Likert-type scale to what degree the presented statements apply to their current feelings. The SRSI3s consists of 38 items in total.

To control for possible influences of mood (Van Steenbergen et al., 2010) and dispositional mindfulness on executive control, the Positive and Negative Affect Schedule (Watson et al., 1988; German version: Breyer & Bluemke, 2016) and the Five-Facet Mindfulness Questionnaire (Baer et al., 2006; German Version: Michalak et al., 2016) were used. The PANAS measures positive and negative emotions on two sub-scales and consists of 20 items in total, with ten items each assessing positive and negative affective states. Participants are asked to indicate on a 5-point Likert-type scale how well a certain affective state (e.g., “*scared*”) applies to them currently. The FFMQ is a multi-facet questionnaire and assesses trait mindfulness on five dimensions: observing, describing, acting with awareness, nonjudging of inner experience and nonreactivity to inner experience. Participants were asked to indicate on a 5-point Likert-type scale how often the presented experiences (e.g., “*I am good at finding words to describe my feelings.*”) applied to them generally. The FFMQ consists of 39 items in total.

Sociodemographic items concerned participants' age, gender, marital status, education, employment, and number of people in the household. Additionally, experienced impact due to the COVID-19 pandemic (work/personal) and experienced burden due to the COVID-19 pandemic (work/personal) were assessed to control for their influences on intervention effectiveness. Lastly, prior experience with mindfulness, related meditation practices and relaxation training were assessed.[2]

2.4. Sound and fidelity checks for online inductions and testing

2.4.1. Sound check

To ensure that participants could hear the audio inductions (breathing meditation, PMR) and podcasts correctly and to give them an opportunity to adjust the audio volume prior to inductions, a sound check was conducted with participants before audio deliverance. During the sound check, participants listened to a short audio-snippet of an animal noise (the sound of a cat, frog, horse, or dog) and were to select the correct animal out of 10 options. The sound was chosen randomly and could be replayed as often as participants chose. If the sound check was completed correctly, participants were directed to a page with written instructions regarding sitting posture, usage of headphones, and appropriate environment (participants were asked to find a separate room, if possible, where the induction and testing could be carried out in silence and to eliminate possible disturbances prior to starting the induction). After participants confirmed that they had carefully read the instructions and adhered to them, they were redirected to a page where they could start the audio training (breathing meditation or PMR) or start the podcast by pressing a play button. If the sound check was not answered correctly, participants had two more trials to complete the task of identifying the correct animal. If participants failed to identify the animal correctly three times, they were notified that participation would only be possible if they were able to play and listen to audio files on their computer without complications. They were instructed to contact the experimenter if any technical issues had occurred during the sound check and that they would be able to continue their participation once the issues were resolved.

2.4.2. Fidelity of online testing and deliverance

Participants' compliance was ensured by collecting data on whether audio files were played to completion and by informing participants that full compensation depended on completing all audio instructions as well as tasks and questionnaires. Participants who did not complete all audio files received partial compensation but were excluded from the final sample. Furthermore, participants were asked whether they were able to follow the audio instructions and to name any disturbances that had occurred (noise, interruption by another person, issues with internet connection or other). To ensure that participants completed the RT measures correctly, they were given written information on what to expect next throughout the experiment. In addition to detailed instructions for each RT task, this also included reminders between RT tasks regarding sitting posture, height of the computer screen, creating a distraction-free environment and using headphones for auditory feedback or noise reduction. Participants were also reminded to follow the task-specific instructions regarding taking breaks and received notification that the RT tasks would open in a separate browser window. After completion, each RT measurement was followed by a manipulation check in which the participant had to answer a multiple-choice question about which task they had just executed (4 options were given for each task; e.g. for the CPT-II, the correct answer would be "*During the task I had to react to every letter presented on the screen, except for when the letter 'X' occurred.*" and an example of an incorrect option is: "*During the task I had to react to a stimulus every time an audio signal was played.*"). Also, participants were asked to indicate whether distractions had occurred during the RT tasks (noise, interruption by another person, issues with internet connection or other). After each training as well as after finishing the assessment of executive functions (before the first training and after the last one), participants were asked to rate the overall disturbance intensity on a 4-point Likert-type scale from weak to strong.

2.5. Research design

The research design was a 3 (mindful breathing meditation, PMR or podcast listening) x 2 (measurement point) experimental design. Audio trainings were delivered three times during Experiment 1 (after pre-measurement, between pre- and post-measurement and before post-measurement) and four times during Experiment 2 (after pre-measurement, two times between pre- and post-measurement, and before post-measurement). The mindful breathing meditation was the experimental condition, PMR was the active control group and listening to podcasts served as a passive control condition. Participants were not informed about the existence of different experimental conditions but were all given the same information about taking part in a concentration training. Assignment of experimental group was randomized and executed automatically within the online experimental environment. Therefore, both experiments reported qualify as randomized controlled double-blinded research designs.

2.5.1. Experimental conditions

In Experiment 1, mindful breathing meditation and relaxation instructions as well as podcast listening were delivered three times over the course of five days and in increasing length (10, 15 and 20 min). In Experiment 2, the trainings were delivered four times over the course of five days, with an equal length of 20 min, resulting in a longer total training duration while keeping the delivery time frame constant. Delivery took place via pre-recorded audio files. To vary the length of trainings, a 20-minute recording was shortened to obtain 10- and 15-minute versions. This was done to keep the trainings as standardized as possible (i.e., no added variations in speaker voice or speaking rate).

2.5.1.1. Mindful breathing meditation. The mindfulness breathing meditation instructions followed the framework described by Kabat-Zinn (1990) and focused on being present in and aware of the present moment, observing the flow of breath without interfering with it, observing and letting go of occurring thoughts and emotions, and overall acceptance of the present moment (source material for both mindful breathing meditation and PMR can be found in Appendix A). To achieve a conceptually clear differentiation between the mindful breathing meditation and relaxation conditions, meditation instructions were clear of any phrases implying or directly instructing relaxation. The meditation instructions had been previously recorded by speakers (both male and female) whom the authors had trained to deliver mindful breathing meditation inductions from a written script with a calm voice and a total length of approximately 20 min. Instructions for focusing attention on the breath included sentences such as: "*Now focus your attention on your breath. Breathe in and breathe out and be aware of this process of*

[2] Further measures which were assessed but are not reported in this paper were the state anxiety scale of the State-Trait-Anxiety Inventory (STAI; Spielberger et al., 1970; German Version: Grimm, 2009) and the HEXACO scales for emotionality, openness to experience and agreeableness from the short version of the HEXACO Personality Inventory (HEXACO-60; Ashton & Lee, 2007; German Version: Moshagen et al., 2014). Furthermore, both experiments assessed implicit bias (as the final task after assessment of executive functions): during Experiment 1, implicit bias was measured with the Shooter Task (Correll et al., 2002), which assesses the effect of ethnicity on shooting decisions, and during Experiment 2, the Avoidance Task (Essien et al., 2017) was utilized, in which the effect of ethnicity on avoidance behavior in a social situation is measured.

breathing.". Instructions for observing one's current experience with a non-judgmental and accepting mindset included sentences emphasizing the observational nature of the practice (e.g., "*You don't need to change anything or intervene. You are an observer of the processes that come naturally, from one moment to the next.*"). Participants were instructed to acceptingly observe distractions and gently redirect their attention through sentences instructing the participant that distractions are part of the practice and to gently let them go (e.g., "*Thoughts of all kinds may come to your mind. This is totally fine. Once you've taken note of this, simply direct your attention back to your breathing.*").

2.5.1.2. Progressive muscle relaxation. During PMR (Jacobson, 1938), the participant is instructed to contract a muscle group (e.g., the upper thighs) for 5 to 10 s while inhaling, and to let go of the contraction while exhaling. The instructions begin with contraction and release focused on the lower extremities and gradually progress upwards through the body. Between muscle groups, participants are asked to relax for 10 to 20 s and to focus on the changes in physiological experience when releasing the tension. The audio files for PMR with a female and male speaker were provided by two experienced PMR trainers who gave the authors permission to use their work (Bödeker, 2013; Kristenich, 2017). The recorded instructions generally followed the original framework by Jacobson (1938) but were edited in such a way that they did not include any mindfulness-related phrasings (e.g., acceptance of the present moment). Instructions directed participants to a certain muscle group and instructed them to create and then observe tension and relaxation, for example: "*Now clench your left hand into a fist. Hold the tension for five seconds, paying attention to the feeling of tension, while the rest of the body is relaxed.*"

2.5.1.3. Listening to podcasts. Four different podcasts were used which discussed historical sites and exceptional landscapes. They had been pretested for not evoking strong positive or negative emotional reactions (assessed via subjective levels of arousal and valence) and for eliciting an average level of interest and engagement. In Experiment 1, podcast length was adjusted to the length of the breathing meditation and PMR instructions. In both experiments, podcasts were presented in random order and no participant listened to the same podcast twice.

2.6. Procedure

Both experiments consisted of two experimental sessions (i.e., two measurement points, including practice sessions), with either one (Experiment 1) or two (Experiment 2) audio-guided practice sessions (of meditation or relaxation or listening to a podcast) in between. After clicking on the URL provided by e-mail, participants were greeted in writing and provided with the written informed consent form. After participants read the form, were informed how they could reach the authors in case of questions and gave their consent, the first session started with a questionnaire regarding sociodemographic variables followed by questionnaires and assessments of executive functions in the following order: SRSI3s, CPT-II, STAI, PANAS, N-Back Task, FFMQ-D, Number-Letter Task, HEXACO-60 subscales, task to assess implicit bias (not reported here). Manipulation checks for the RT measures and the assessment of participation compliance and fidelity were placed as described above. The sequence of questionnaires and tests was the same across all measurement points and for all participants. Participants could take self-paced breaks between tasks. Following pre-measurement, participants proceeded to the sound check, and after completing it received the first audio induction (breathing meditation, PMR or podcast listening). Afterwards, participants were thanked and notified that they would be invited to the second session via e-mail. The following session (Experiment 1) or the following two sessions (Experiment 2) only consisted of instructed practice (mindful breathing or PMR) or of podcast listening. The final session started with meditation or PMR practice or podcast listening. Afterwards, participants again completed all tasks and questionnaires. After completing all parts of the study, participants were debriefed about the purpose of the study, thanked for their participation, and received course credit.

3. Statistical analyzes

Data pre-processing included trimming RTs below 100 ms and above 1500 ms for the CPT-II and N-Back tasks. Due to a greater response time window of up to 4000 ms in the Number-Letter Task, RT cut-off values below 100 ms and above 2000 ms were chosen. Unless stated otherwise, RT data included accurate and inaccurate trials, so that accuracy could be modeled as a fixed main effect. Participants were excluded if >40 % of trials were missing. Following data pre-processing, single-trial RT data were analyzed with generalized linear mixed models (GLMMs) by maximum likelihood estimation. To account for the typically positively skewed distribution of empirical reaction times (Balota & Yap, 2011; Lo & Andrews, 2015) and based on the model fit of the current data, the inverse Gaussian distribution (Johnson & Kotz, 1970; Tweedie, 1957) was selected. To test our hypothesis of mindfulness-specific effects on executive functioning following short training periods, likelihood ratio tests were used to compare models with an interaction term between measurement point and condition against a restricted model with main effects only. If the model with the interaction term explained substantially more variance in the data and the interaction reached significance, planned comparisons between conditions were conducted by calculating and testing estimated marginal mean (EMM) contrasts. The Tukey method for *p*-value adjustment was selected.

For the CPT-II (inhibition) and the N-Back task (updating), responses were analyzed based on discriminability ($d' = z[\text{Hits}] - z[\text{False Alarms}]$) in order to assess participants' ability to discriminate between target and non-target stimuli by accounting for hits and false alarms. Discriminability was analyzed with linear regressions. For the Number-Letter Task (shifting), response frequencies (accuracy) were analyzed with logistic regressions, which allow for modeling a binomial distribution. Modelling of discriminability and accuracy (i.e., fixed effects, contrast coding schemes, model testing) was analog to the analysis of reaction times.

Linear regressions were also used to analyze participants' experienced burden due to the COVID-19 pandemic, changes in state relaxation and mindfulness (SRSI3s) as a manipulation check, as well as to assess possible differences in trait mindfulness (FFMQ) at pre-measurement and changes in positive and negative affect (PANAS) as covariates in the RT models. To assess pre-post changes between conditions in state relaxation and mindfulness as well as affect, comparison of linear regression models with and without an interaction between measurement point and condition was done with likelihood ratio tests. For the assessment of differences in trait mindfulness at pre-measurement, a linear regression was performed containing the factor condition. Contrast coding schemes for measurement point and condition for the described models were performed as described above.

Data were modeled in R (version 4.0.2). Generalized linear mixed modeling was done with the lme4 package (Bates et al., 2015; version 1.1-23), *p*-values were obtained with the lmerTest package (Kuznetsova et al., 2017; version 3.1-2). Measures of signal detection (i.e., discriminability) were obtained with the psycho package (Makowski, 2018, version 0.5.0). EMMs were calculated with the emmeans package (Lenth, 2020; version 1.4.8.), figures of the results were obtained using the ggplot2 package (Wickham, 2016; 3.3.6).

4. Results

The reported model results will focus on effects of interest, which encompass task-specific effects (e.g., variation of ISI in the CPT-II) as well as interactions central to the hypothesis of the current paper (i.e., interaction terms comprising the factors measurement point and condition and their respective lower-order terms). The condition factor was coded with a custom contrast scheme with podcast listening as the reference category, comparing mindful breathing meditation and PMR to podcast listening. Therefore, simple effects for mindful breathing meditation and PMR as well as interactions including the factor condition always compare either mindful breathing meditation or PMR to podcast listening, while the intercept corresponds to the grand mean. Measurement point, accuracy, as well as task-specific effects of interest (e.g., ISI during the CPT-II) were contrast coded, meaning that each level of the respective categorial variable was compared to a reference category. The reference category for measurement point was pre-measurement, the reference category for accuracy was inaccurate trials. Furthermore, models included random intercepts for participants and random slopes for measurement point as recommended by Barr et al. (2013). Further references for test-specific variables are defined in the respective model descriptions below. Full models for all described analyses can be found in the supplementary material Appendix B (Experiment 1: Table B.1-B.6; Experiment 2: Table B.7–B.12). Likelihood ratio tests for model comparisons including fit indices for GLMMs (Akaike information criterion, AIC; and Bayesian information criterion, BIC) and regressions are reported in Table 1. Planned comparisons (EMM_{t1} – EMM_{t0}) are reported to follow up on significant interaction terms, allowing for a comparison between all three conditions.

4.1. Experiment 1

4.1.1. Fidelity of online testing and deliverance

Overall disturbance intensity was rated as weak on average for all measurement points and practice sessions. In terms of possible distractions during RT measurements, 77.78 % of participants at pre-measurement and 80.81 % at post-measurement reported no distractions whatsoever (see Table 2). Most disturbances were either noise-related or interruptions by another person, while internet connectivity or other issues were less prominent. Other disturbances reported were mostly technical issues or technical distractions (e.g., a software pop-up notification appearing on the screen).

During the audio listening periods, 75.76 % of participants at the first audio listening period, 76.77 % at the second, and 80.81 % at the third audio listening period reported no distractions. Most disturbances fell into the category of noise, while interruptions by another person, internet connectivity issues and other issues were less prominent. Other disturbances during audio listening were mostly related to intrapersonal states or sensations as well as pets interrupting the practice.[3]

4.1.2. Questionnaires

Descriptive statistics for the SRSI3s, PANAS, and FFMQ can be found in Appendix C. Model comparisons for the SRSI3s, PANAS, FFMQ, and experienced burden of the COVID-19 pandemic can be found in Appendix D, Table D.1 – Table D.12. For the SRSI3s subscale basic relaxation, model comparisons showed a better fit for a model including an interaction between measurement point and condition, $F(2, 186) = 8.14$, $p < 0.001$, compared to a main effects-only model, and the interaction between measurement point and mindfulness was significant, $\beta = 0.37$, $SE = 0.09$, $p < 0.001$. Further analysis of differences between groups in changes in basic relaxation from pre- to post-measurement by planned comparisons showed that both mindful breathing meditation and PMR exhibited greater increases for basic relaxation compared to podcast listening (see Table 3). Furthermore, an interaction model provided a better fit for the SRSI3s scale quiet focus and awakening, $F(2,186) = 3.16$, $p = 0.045$, for which again the interaction between measurement point and mindfulness was significant, $\beta = 0.24$, $SE = 0.10$, $p = 0.013$. As can be seen in Table 3, planned comparisons showed that both mindful breathing meditation and PMR led to greater increases in quiet focus and awakening compared to podcast listening. In model comparisons for the SRSI3s subscales transformation/transcendence and positive transcendent emotion the interaction model did not fit significantly better than a model without an interaction term, $p \geq 0.540$. Thus, no differences in changes from pre- to post-measurement between conditions were found for transformation/transcendence and positive transcendent emotion. In conclusion, mindfulness breathing meditation as well as PMR led to greater increases in feelings of detachment and physical relaxation (basic relaxation) as well as in experiences of focus on the present moment and acceptance (quiet focus) compared to podcast listening, suggesting that both inductions were successful but also similar in inducing states of relaxation and focus.

Model comparisons for the PANAS scores did not indicate a better fit for models including an interaction between measurement point and condition, $p \geq 0.586$. Linear regressions for the FFMQ showed no differences in trait mindfulness between groups at pre-measurement, all $p \geq 0.316$. Thus, analyses of RT and accuracy did not include covariates controlling for trait mindfulness or affective state. Participants rated their experienced burden and impact due to the COVID-19 pandemic as moderate (work) and a lot (personal) on average, and this did not differ between experimental conditions.

4.1.3. Cognitive inhibition

Cognitive inhibition was assessed with the CPT-II. One participant from the podcast listening condition provided only data at pre-measurement and was therefore removed. As incorrect responses (i.e., responses to non-targets) were rare at pre- and post-measurement ($\leq$ 3.08 % of all trials), only correct response times were included in the analysis (total data loss after data trimming and removal of incorrect responses 3.89 %). The interaction model for both reaction time and discriminability included a fixed effect for ISI (with 1000 ms as the reference category), a two-way interaction between measurement point and condition as well as a random intercept for participants and a random slope for measurement point.

4.1.3.1. Reaction time. The likelihood ratio test showed that the interaction model did not fit significantly better than a model without an interaction term (see Table 1). According to this finding, neither mindful breathing meditation nor PMR affected inhibition RT performance from pre- to post-measurement beyond the effects of repeated testing, and the main effects-only model will be reported.

For the main effects-only model, the analysis showed a main effect for the ISI of 1500 ms, $\beta = 6.87$, $SE = 1.01$, $p < 0.001$, and the ISI of 2000 ms, $\beta = 14.87$, $SE = 1.02$, $p < 0.001$, with larger RT for longer ISIs in comparison to the ISI of 1000 ms, replicating effects of adaptation to task demands (i.e., increase in RT corresponding with ISI duration). Further main effects were present for measurement point, $\beta = -10.01$, $SE = 2.75$, $p < 0.001$, with RT decreasing from pre- to post-measurement, and for mindful breathing meditation, $\beta = 5.22$, $SE = 2.35$, $p = 0.026$, indicating overall greater RT for mindful breathing meditation compared to podcast listening.

4.1.3.2. Discriminability. The likelihood ratio test showed that the interaction model did not fit significantly better than a model without an interaction term. The results thus indicate that a short mindful breathing meditation or relaxation training had no effect on inhibition accuracy beyond repeated testing. The results of the main effects-only model showed that all main effects were non-significant, $p \geq 0.075$.

4.1.4. Updating

Updating was assessed with the N-Back task. One participant (podcast condition) for which only data at pre-measurement was available was removed. Total data loss after data trimming: 6.70 %. The

[3] In both Experiments 1 and 2, statistical analyses for all dependent measures including a fixed effect indicating the presence or absence of disturbances for each participant revealed no differences in the direction or size of effects and no changes in the significance of model terms.

Table 1
Model comparisons for the assessment of executive functioning.

Experiment 1									
Generalized linear models for reaction time							LRT against final		
Task	Model	AIC	BIC	logLik	Deviance	*df*	X^2 *df*	X^2	*p*
CPT-II	Two-way Interaction	800,604	800,713	−400,290	800,580	12	–	–	–
	Main Effects	800,603	800,694	−400,291	800,583	10	2	2.43	0.296
N-Back	Two-way Interaction	870,343	870,470	−435,158	870,315	14	–	–	–
	Main Effects	870,339	870,448	−435,158	870,315	12	2	0.05	0.976
Number-Letter	Three-way Interaction	399,564	399,704	−199,765	399,530	17	–	–	–
	Two-way Interaction	399,630	399,729	−199,803	399,606	12	5	76.36	**< 0.001**
	Main Effects	399,624	399,707	−199,802	399,604	10	7	74.50	**< 0.001**
Regression models for response behavior							LRT against final		
Task	Model	Res. *df*		*RSS*		*df*	*SS*		*p*
CPT-II	Two-way Interaction	513		311.5		–	–		–
	Main Effects	515		314.57		2	3.07		0.080
N-Back	Two-way Interaction	1131		399.63		–	–		–
	Main Effects	1133		399.72		2	0.09		0.878
		Res. *df*		Res. Deviance		*df*	Deviance		*p*
Number-Letter	Three-way Interaction	30,516		42,302		–	–		–
	Two-way Interaction	30,521		42,312		5	9.34		0.096
	Main Effects	30,523		42,318		7	15.27		**0.033**

Experiment 2									
Generalized linear models for reaction time							LRT against final		
Task	Model	AIC	BIC	logLik	Deviance	*df*	X^2 *df*	X^2	*p*
CPT-II	Two-way Interaction	581,997	582,102	−290,986	581,973	12	–	–	–
	Main Effects	581,994	582,082	−290,987	581,974	10	2	1.29	0.525
N-Back	Two-way Interaction	594,718	594,840	−297,345	594,690	14	–	–	–
	Main Effects	594,715	594,819	−297,345	594,691	12	2	0.52	0.772
Number-Letter	Three-way Interaction	260,849	260,983	−130,408	260,815	17	–	–	–
	Two-way Interaction	260,885	260,979	−130,430	260,861	12	5	45.28	**< 0.001**
	Main Effects	260,881	260,959	−130,430	260,861	10	7	45.50	**< 0.001**
Regression models for response behavior							LRT against final		
Task	Model	Res. *df*		*RSS*		*df*	*SS*		*p*
CPT-II	Two-way Interaction	354		220.01		–	–		–
	Main Effects	356		220.86		2	0.85		0.505
N-Back	Two-way Interaction	783		320.76		–	–		–
	Main Effects	785		322.88		2	2.12		0.075
		Res. *df*		Res. Deviance		*df*	Deviance		*p*
Number-Letter	Three-way Interaction	20,022		27,749		–	–		–
	Two-way Interaction	20,027		27,769		5	19.75		**0.001**
	Main Effects	20,029		27,770		7	20.58		**0.004**

Note. LRT = Likelihood ratio tests. Comparisons of the final models against restricted models, with significant tests indicating a better fit for the final model. Corrected the use of bold in the respective tables, only letters used as statistical symbols should have been bold.

Table 2
Experiment 1: participants' reports of distractions during task execution and audio-listening.

Disturbance Intensity (1 = *weak* to 4 = *strong*)	M_{t0}	SD_{t0}	M_{t1}	SD_{t1}	M_{t2}	SD_{t2}
	1.27	0.57	1.20	0.61	1.25	0.61

	Pre-Measurement		Second Audio Listening		Post-Measurement	
Distraction Categories	Audio Listening$_{t0}$	Task Execution$_{t0}$	Audio Listening	(No Task Execution)	Audio Listening$_{t1}$	Task Execution$_{t1}$
No Distractions	75.76 %	77.78 %	76.77 %	–	80.81 %	75.76 %
Noise	12.12 %	9.09 %	10.10 %	–	8.08 %	10.10 %
Interruption by a Person	7.07 %	7.07 %	6.06 %	–	4.04 %	10.10 %
Internetconnection	4.04 %	3.03 %	5.05 %	–	6.06 %	5.05 %
Other	4.04 %	7.07 %	4.04 %	–	1.01 %	1.01 %

Note. t0 = Pre-measurement; t1 = Post-measurement. Items for possible distractions were not mutually exclusive, total percentages may exceed 100 %.

interaction model included a fixed effect for accuracy, n-back level (with 1-back as the reference category) and target type (with non-target trials as the reference category), a two-way interaction between measurement point and condition, a random intercept for participants and a random slope for measurement point.

4.1.4.1. *Reaction time.* A likelihood ratio test showed that the interaction model did not fit significantly better than a model without the

Table 3
Experiment 1: planned comparisons of the SRSI3s from pre- to post-measurement.

	Podcast – mindful breathing		Podcast - PMR		Mindful breathing - PMR	
	T1 - T0: Estimate (*SE*)	*p*	T1 - T0: Estimate (*SE*)	*p*	T1 - T0: Estimate (*SE*)	*p*
Basic relaxation	−1.02 (0.32)	**0.005**	−1.23 (0.32)	**<0.001**	−0.21 (0.31)	0.778
Quiet focus and awakening	−0.77 (0.33)	**0.056**	−0.68 (0.33)	0.100	0.09 (0.32)	0.959

Note. T0 = pretest; T1 = posttest. *P* value adjustment: tukey method for comparing a family of 3 estimates.

interaction term. Thus, results do not suggest a benefit of mindful breathing meditation or relaxation for updating latencies beyond repeated testing, and the main effects-only model will be reported.

The analysis showed a main effect for accuracy, $\beta = -42.83$, $SE = 1.89$, $p < 0.001$, with smaller RTs for correct than incorrect trials, and for target type, $\beta = -34.93$, $SE = 1.93$, $p < 0.001$, indicating shorter RTs for target compared to non-target trials. Furthermore, main effects were present for the n-back level 2-back, $\beta = 118.23$, $SE = 1.90$, $p < 0.001$, and 3-back, $\beta = 123.51$, $SE = 1.95$, $p < 0.001$, with RT larger for 2- and 3-back compared to 1-back trials, replicating findings of increasing task difficulty with greater n. A main effect was also present for measurement point, $\beta = -96.61$, $SE = 3.45$, $p < 0.001$, with RT decreasing from pre- to post-measurement, and for PMR, $\beta = -9.58$, $SE = 3.40$, $p = 0.005$, with RTs smaller in the relaxation group compared to podcast listening.

4.1.4.2. Discriminability. Results of the likelihood ratio test showed that the interaction model did not fit better than the main effects-only model. A main effect was found for 3-back trials, $\beta = 0.14$, $SE = 0.04$, $p = 0.001$, indicating higher levels of discriminability for 3-back in comparison to 1-back trials, and for target type, $\beta = -2.43$, $SE = 0.04$, $p < 0.001$, indicating lower discriminability for target compared to non-target trials.

Thus, discriminability was not improved by the brief trainings compared to the passive control condition. Interestingly, higher discriminability was found for 3-back trials. As the RT analysis suggested longer RT for 3-back trials, participants may have achieved higher discriminability by trading speed for accuracy in trials with higher task demands.

4.1.5. Shifting

Shifting was assessed with the Number-Letter Task. No participants needed to be removed. Total data loss after data trimming: 2.29 %. The interaction model included a fixed effect for accuracy, a three-way interaction between measurement point, condition, and switch factor (with non-switch trials as the reference category), a random intercept for participant and a random slope for measurement point.

4.1.5.1. Reaction time. A likelihood ratio test compared the interaction model to a set of restricted models (a model including a two-way interaction excluding the switch factor and a main effects-only model). The three-way interaction model fit significantly better than the model with a two-way interaction between measurement point and condition and the model without an interaction term. Thus, results are reported for the model including the interaction between measurement point, condition, and switch factor.

A main effect was present for accuracy, $\beta = -10.32$, $SE = 3.16$, $p = 0.001$, indicating shorter RTs for correct trials. Simple effects were present for measurement point, $\beta = -116.68$, $SE = 8.48$, $p < 0.001$, indicating a decrease in RT from pre- to post-measurement in non-switch trials, and for the switch factor, $\beta = 359.09$, $SE = 5.07$, $p < 0.001$, with RT slower in switch compared to non-switch trials at pre-measurement, replicating the effects of switch costs on response latencies. Additionally, a simple effect for condition showed shorter RT for the mindful breathing meditation, $\beta = -39.24$, $SE = 7.29$, $p < 0.001$, and longer RT for PMR, $\beta = 18.68$, $SE = 8.82$, $p = 0.034$, in non-switch trials in comparison to podcast listening at pre-measurement. There was a two-way interaction between measurement point and mindful breathing meditation, $\beta = -15.02$, $SE = 6.10$, $p = 0.014$, and for PMR, $\beta = -70.60$, $SE = 5.90$, $p < 0.001$, indicating that both trainings led to a greater speed up in non-switch trials than podcast listening. The model also showed a three-way interaction between measurement point, switch factor and mindful breathing meditation, $\beta = 68.20$, $SE = 5.63$, $p < 0.001$, indicating that breathing meditation was followed by a greater difference in RT between switch and non-switch trials compared to podcast listening and similarly for PMR, $\beta = 116.26$, $SE = 6.72$, $p < 0.001$, again indicating that compared to podcast listening, PMR was followed by a greater difference in RT between switch and non-switch trials.

Fig. 1 displays estimated marginal means contrasts for switch costs (EMM_{switch}- $EMM_{non\text{-}switch}$) resulting from the three-way interaction between measurement point, condition, and switch factor. Planned

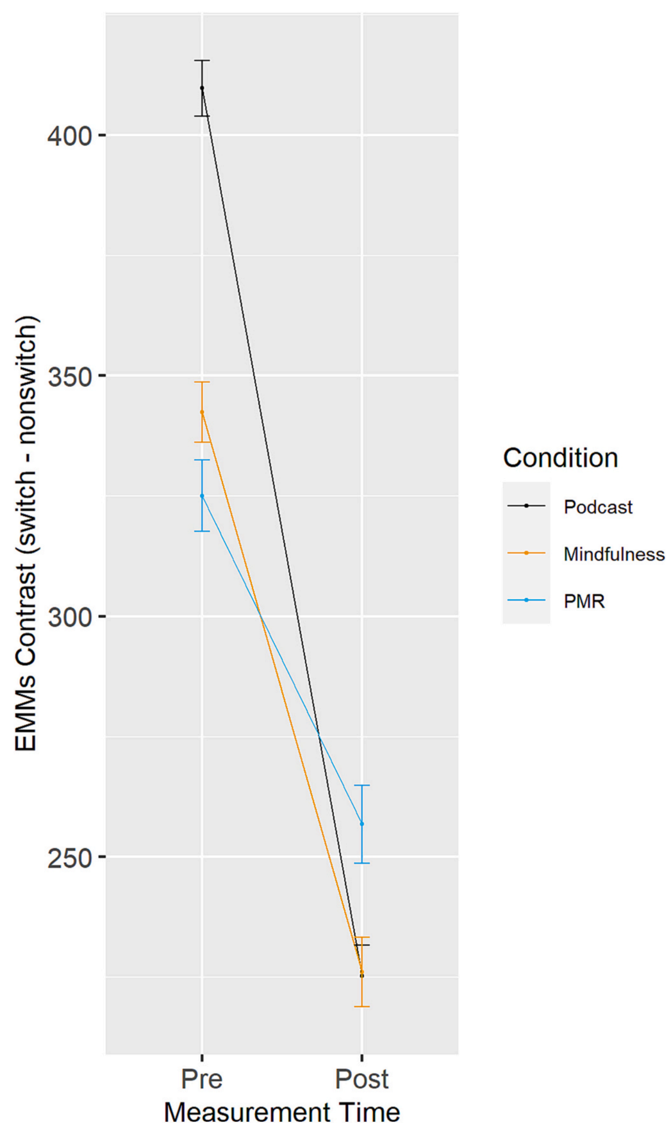

Fig. 1. Experiment 1: switch cost in RT during the Number-Letter Task.

comparisons between conditions were calculated for pre-post differences in switch costs (see Table 4). Results show that podcast listening led to a larger decrease in switch costs from pre- to post-measurement than both mindful breathing meditation and PMR, whereas mindful breathing meditation resulted in a larger decrease in switch costs than PMR. Thus, the results indicate no effects of brief trainings on shifting abilities beyond repeated testing, but an advantage for mindful breathing meditation compared to relaxation.

4.1.5.2. Accuracy. Likelihood ratio tests showed that the three-way interaction model did not fit better than a model including only an interaction between measurement point and condition, but better than a main effects-only model. Subsequently, comparing the two-way interaction model to the main effects-only model revealed a better fit for the latter, χ^2 (2) = 5.93, $p = 0.051$. Thus, there is no indication that response accuracy in shifting benefits from a brief training in mindful breathing meditation or relaxation compared to repeated testing. The main effects-only model showed no significant main effects, all $p \geq 0.224$.

4.2. Experiment 2

The models for all executive functions as well as questionnaires were identical to those described in Experiment 1. Full models can be found in Appendix B (Table B.7–Table B.12).

4.2.1. Fidelity of online testing and delivery

Overall disturbance intensity was rated as weak on average for all measurement points and all audio listening periods. Regarding possible distractions during RT measurements, 78.08 % of participants at pre-measurement and 72.60 % at post-measurement reported no distractions whatsoever (see Table 5). Most disturbances were noise related. Disturbances by another person, issues with the internet connection or other issues were less prominent. Other disturbances reported were mostly related to participants indicating fatigue.

Regarding the audio listening periods, 78.08 % of participants at the first audio listening period, 75.34 % at the second, 79.45 % at the third and 75.34 % of participants at the fourth audio listening reported no distractions. Most disturbances either fell into the category of distractions by another person or noise; issues with the internet connection or other distractions were less prominent. Other disturbances were mostly related to intrapersonal states or sensations as well as pets interrupting the practice (see Footnote 3 for control of possible effects of disturbances).

4.2.2. Questionnaires

Results for the analyses of the SRSI3s, FFMQ, PANAS, and participants' experienced burden during the COVID-19 pandemic can be found in Appendix D, Table D.13 – Table D.24. Model comparisons revealed no better fit for models containing an interaction between measurement point and condition for any of the SRSI3s subscales, all $p \geq 0.467$, and the PANAS scales, all $p \geq 0.820$. Linear regressions of the FFMQ scales showed no differences in trait mindfulness between groups at pre-measurement, all $p \geq 0.059$. Thus, results indicate no improvements in relaxation or state mindfulness following the breathing meditation or relaxation training compared to a podcast listening, and no covariates for positive and negative affect or trait mindfulness were added into the models of RT and accuracy. Participants rated their experienced burden and impact due to the COVID-19 pandemic as moderate (work) and between moderate and a lot (personal) on average, and this did not differ between experimental conditions.

Table 4
Experiment 1: planned comparisons of switch cost (RT) reduction from pre- to post-measurement during the Number-Letter Task.

	Podcast – mindful breathing		Podcast - PMR		Mindful breathing - PMR	
	T1 - T0: Estimate (*SE*)	*p*	T1 - T0: Estimate (*SE*)	*p*	T1 - T0: Estimate (*SE*)	*p*
Switch Cost (RT)	−68.2 (5.63)	**<0.001**	−116.3 (6.72)	**<0.001**	−48.1 (8.48)	**<0.001**

Note. T0 = pretest; T1 = posttest. P value adjustment: tukey method for comparing a family of 3 estimates.

4.2.3. Cognitive inhibition

As in Experiment 1, cognitive inhibition was assessed with the CPT-II. As incorrect responses (i.e., responses to non-targets) were rare at pre- and post-measurement (≤ 3.47 % of all trials), only correct response times were included in the analysis (total data loss after data trimming and removal of incorrect responses: 5.11 %).

4.2.3.1. Reaction time. A likelihood ratio test of the interaction model compared to a restricted main effects-only model showed no significantly better fit (see Table 1). Thus, as in Experiment 1, neither mindful breathing meditation nor PMR improved inhibition RT performance from pre- to post-measurement beyond the effects of repeated testing.

The main effects-only model showed a main effect for the ISI of 1500 ms, $\beta = 11.06$, $SE = 1.34$, $p < 0.001$, and the ISI of 2000 ms, $\beta = 21.32$, $SE = 1.27$, $p < 0.001$, with larger RT for both longer ISIs in comparison to the ISI of 1000 ms, again replicating the effects regarding RT adaptation in correspondence with ISI duration. There was also a main effect for measurement point, $\beta = -8.60$, $SE = 4.08$, $p = 0.035$, indicating a decrease in RT from pre- to post-measurement. Furthermore, main effects were found for mindful breathing meditation, $\beta = -26.73$, $SE = 6.15$, $p < 0.001$, and PMR, $\beta = -33.30$, $SE = 3.70$, $p < 0.001$, indicating that both trainings were associated with lower RT compared to podcast listening.

4.2.3.2. Discriminability. Results of the likelihood ratio test showed that the interaction model did not provide a better fit. The main effects-only model revealed no significant effects, all $p \geq 0.101$. Thus, inhibition accuracy was not improved by any training.

4.2.4. Updating

Updating was measured with the N-Back task. Two participants (one from mindful breathing meditation and one from podcast listening) for which only data at pre-measurement was available were removed. One participant for which only 10.27 % of trials remained after initial data cleaning was also removed from the analysis. Total data loss after data trimming: 7.40 %.

4.2.4.1. Reaction time. The interaction model did not fit significantly better than a model without an interaction term. Thus, neither mindful breathing meditation nor PMR led to beneficial decreases in updating latencies compared to a passive control condition. Accordingly, the main effects-only model will be reported.

The analysis showed a main effect for accuracy, $\beta = -51.81$, $SE = 3.11$, $p < 0.001$, with smaller RTs for correct than incorrect trials, and for target type, $\beta = -34.41$, $SE = 2.62$, $p < 0.001$, indicating shorter RTs for target compared to non-target trials. Furthermore, main effects were present for the n-back level 2-back, $\beta = 103.32$, $SE = 2.29$, $p < 0.001$, and 3-back, $\beta = 118.39$, $SE = 2.37$, $p < 0.001$, with RT larger for 2- and 3-back in comparison to 1-back trials, again replicating the effects of increasing task difficulty with greater *n*. Main effects were also present for measurement point, $\beta = -103.31$, $SE = 3.42$, $p < 0.001$, with RT decreasing from pre- to post-measurement, as well as for mindful breathing meditation, $\beta = -68.86$, $SE = 4.97$, $p < 0.001$, and for PMR, $\beta = -59.17$, $SE = 4.88$, $p < 0.001$, with shorter RTs overall in both training groups compared to podcast listening.

Table 5
Experiment 2: participants' reports of distractions during task execution and audio-listening.

Disturbance Intensity (1 = *weak* to 4 = *strong*)	M_{t0}	SD_{t0}	M_{t1}	SD_{t1}	M_{t2}	SD_{t2}	M_{t3}	SD_{t3}
	1.25	0.49	1.19	0.49	1.22	0.58	1.23	0.51

	1st Audio Listening & Pre-Measurement		2nd Audio Listening		3rd Audio Listening		4th Audio Listening & Post-Measurement	
Distraction Categories	Audio $Listening_{t0}$	Task $Execution_{t0}$	Audio Listening	(No Task Execution)	Audio Listening	(No Task Execution)	Audio $Listening_{t1}$	Task $Execution_{t1}$
No Distractions	78.08 %	78.08 %	75.34 %	–	79.45 %	–	75.34 %	72.60 %
Noise	8.22 %	12.33 %	8.22 %	–	8.22 %	–	10.96 %	13.70 %
Interruption by a Person	12.33 %	8.22 %	10.96 %	–	13.70 %	–	8.22 %	8.22 %
Internetconnection	4.11 %	0.00 %	1.37 %	–	1.37 %	–	0.00 %	0.00 %
Other	2.74 %	1.37 %	4.11 %	–	1.37 %	–	5.48 %	6.85 %

Note. t0 = Pre-measurement; t1 = Post-measurement. Items for possible distractions were not mutually exclusive, total percentages may exceed 100 %.

4.2.4.2. Discriminability. The model including the interaction term did not fit better than a restricted main effects-only model. Thus, again as in Experiment 1, neither training showed a beneficial effect on updating accuracy compared to a passive control condition. The main effects-only model showed a main effect for target type, $\beta = -2.44$, $SE = 0.05$, $p < 0.001$, indicating lower discriminability for target compared to non-target trials. All other main effects $p \geq 0.070$.

4.2.5. *Shifting*

Shifting was again assessed using the Number-Letter Task. Five participants (one from the mindful breathing meditation condition, two from PMR and two from podcast listening) for which only data at pre-measurement were available needed to be removed. One participant (PMR condition) with an average RT of 42 ms was removed from the analysis. Total data loss after data trimming: 7.25 %.

4.2.5.1. Reaction time. The model including the three-way interaction between measurement point, condition and switch factor fit significantly better than a model with a two-way interaction between measurement point and condition and a main effects-only model. Simple effects were present for measurement point, $\beta = -92.44$, $SE = 6.87$, $p < 0.001$, indicating a decrease in RT from pre- to post-measurement for non-switch trials, and for switch factor, $\beta = 327.39$, $SE = 4.69$, $p < 0.001$, with RT being slower for switch compared to non-switch trials at pre-measurement, replicating the effects of switch costs on response latencies that were also found in Experiment 1. A two-way interaction was present for measurement point and mindful breathing meditation, $\beta = -20.77$, $SE = 9.12$, $p = 0.023$, indicating a greater decrease in non-switch compared to switch trials for mindful breathing meditation compared to podcast listening. The two-way interaction between measurement point and PMR was nonsignificant, $p = 0.062$. A three-way interaction was found for measurement point, PMR and switch, $\beta = 32.07$, $SE = 8.31$, $p < 0.001$, indicating a greater difference in RT between switch and non-switch trials following PMR compared to podcast listening.

Fig. 2 displays estimated marginal means contrasts for switch costs (EMM_{switch}- $EMM_{non\text{-}switch}$) resulting from the three-way interaction between measurement point, condition, and switch factor. Planned comparisons between conditions were calculated for pre-post differences in switch costs (see Table 6). The results show that PMR reduced switch costs to a lesser extent compared to mindful breathing meditation and podcast listening, while there was no difference in the decrease in switch costs between breathing meditation and podcast listening. Thus,

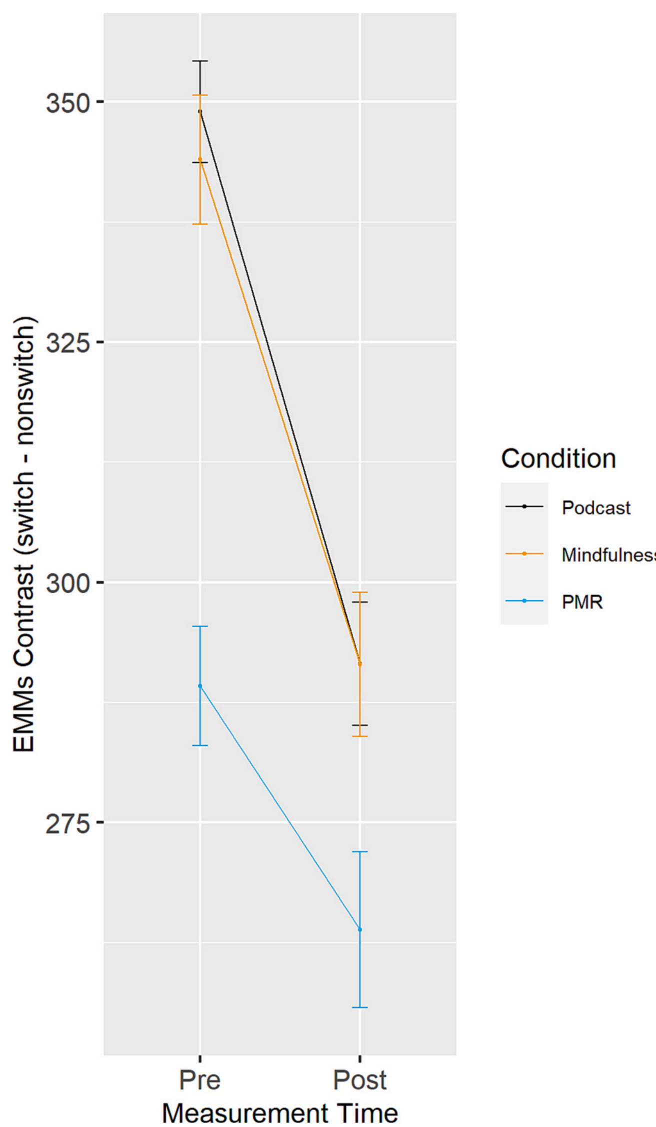

Fig. 2. Experiment 2: switch cost in RT during the Number-Letter Task.

Table 6
Experiment 2: planned comparisons of switch Cost (RT) reduction from pre- to post-measurement during the Number-Letter Task.

	Podcast – Mindful Breathing		Podcast - PMR		Mindful Breathing - PMR	
	T1 - T0: Estimate (*SE*)	*p*	T1 - T0: Estimate (*SE*)	*p*	T1 - T0: Estimate (*SE*)	*p*
Switch Cost (RT)	−4.91 (6.78)	0.749	−32.07 (8.31)	**<0.001**	−27.16 (10.79)	**0.032**

Note. T0 = pretest; T1 = posttest. P value adjustment: tukey method for comparing a family of 3 estimates.

while the results again indicate no effects of trainings on shifting abilities beyond repeated testing, a brief mindful breathing meditation training was again associated with an advantage in terms of switch cost reduction compared to a brief relaxation training.

4.2.5.2. Accuracy. The three-way interaction model fit better than a model including a two-way interaction and a main effects-only model. The three-way interaction model showed a simple effect for mindful breathing meditation, $\beta = -0.17$, $SE = 0.07$, $p = 0.015$, indicating lower accuracy for breathing meditation compared to podcast listening in non-switch trials at pre-measurement. All two-way interactions including the measurement point factor were non-significant, $p \geq 0.078$, as were all three-way interactions, $p \geq 0.149$. Thus, response accuracy on a shifting task was not affected by either training.

5. General discussion and conclusion

Contemporary scientific models of mindfulness emphasize the role of the executive functions of updating, cognitive inhibition and shifting during mindfulness practice and postulate that mindful breathing meditation trains these cognitive control capacities (e.g., Bishop et al., 2004; Isbel & Summers, 2017; Jankowski & Holas, 2014). However, it remains unclear whether mindful breathing meditation training leads to domain-general or domain-specific effects in executive functioning (e.g., Gallant, 2016) and how much practice is required (e.g., Lodha & Gupta, 2022), as respective studies have yielded mixed results. As outlined above, methodological differences between studies in the field, such as a lack of active control conditions and high heterogeneity in active controls used, make it difficult to integrate the current state of research. As a result, questions regarding mechanisms and the dose-response relation remain open. We aimed to address these issues in two ways. First, we contrasted the effects of mindful breathing meditation with a relaxation technique to address the proposition that in early phases of mindful breathing meditation training, the practice does not produce specific states or effects (Fell et al., 2010; Vieth & von Stockhausen, 2022), but may induce relaxation, thereby freeing cognitive resources for improved attentional and executive functioning (Eysenck et al., 2007). Second, we increased the training length from Experiment 1 to Experiment 2 while keeping other aspects of the research design constant and thereby addressed the dose-response question. We conducted two randomized controlled double-blinded trials in a pre-post design with an active (relaxation training, PMR) and passive control condition (podcast listening). We will discuss the results for identical tasks and associated cognitive functions from Experiment 1 and 2 jointly. Implications of the results for future research will be discussed below.

The findings from both experiments showed that neither mindful breathing meditation nor PMR resulted in beneficial effects on response inhibition. This is in line with Polak (2009), who found no effects compared to a relaxation induction and a cognitive control condition, and Larson et al. (2013), who found no improvement compared to a passive control condition. However, our findings are in contrast with studies that did find improvement in cognitive inhibition following mindful breathing meditations compared to active (relaxation) and/or passive control conditions (Mrazek et al., 2012; Norris et al., 2018; Vieth & von Stockhausen, 2022; Wenk-Sormaz, 2005). Previous studies that similarly utilized evidence-based relaxation trainings as active control conditions have either reported no improvements (Polak, 2009) or similar improvements for mindful breathing and relaxation (Vieth & von Stockhausen, 2022), while studies that utilized relaxation or rest conditions without detailed instructions (Mrazek et al., 2012; Wenk-Sormaz, 2005) report significant effects on cognitive inhibition following brief mindful breathing meditation trainings compared to relaxation. Our results together with findings from other studies do not seem to support the proposition of specific effects of mindful breathing meditation on inhibition in early phases of training (Isbel & Summers, 2017; Lodha & Gupta, 2022).

Moreover, we found no specific benefits of mindful breathing meditation for updating abilities. Previous studies showed no effects compared to a sham meditation and a passive control condition (Johnson et al., 2015), improvement stemming from both mindful breathing meditation and relaxation compared to a passive control condition (Vieth & von Stockhausen, 2022) or improvement in only one of two updating tasks compared to a passive control condition (Zeidan et al., 2010). Studies on updating abilities in this research context are scarce and reported effects, as well as study designs, are heterogenous. The proposition that updating abilities are more likely to be trained in novices (Lodha & Gupta, 2022; Lutz et al., 2015), who require constant monitoring of mental content to adhere to task goals, cannot be confirmed based on the presented evidence. Furthermore, while Vieth and von Stockhausen (2022) found similarly improved updating following mindful breathing and relaxation, such improvements could not be replicated in the present experiments, even though the same experimental design was employed, and practice duration and frequency were increased.

Considering shifting abilities, the changes in shifting latencies and accuracy we found after mindful breathing meditation and relaxation were either less beneficial compared to repeated testing or did not go beyond the effects of repeated testing. However, switch costs decreased more strongly following mindful breathing meditation than relaxation in both experiments. These results (i.e., no benefit beyond repeated testing, but greater improvement for mindful breathing meditation compared to relaxation) are similar to those reported by Vieth and von Stockhausen (2022). Moreover, the finding of a lesser improvement in shifting after mindful breathing meditation compared to podcast listening in Experiment 1 is in line with Wolff and Beste (2020), who found impaired shifting after 15 min of mindful breathing meditation. However, the results of Experiment 2 of the present study, as well as those by Vieth and von Stockhausen (2022) and Jankowski and Holas (2020), do not suggest differences in shifting abilities following breathing meditations and passive control conditions.

To summarize, in two randomized controlled double-blinded trials no specific benefit of mindful breathing practice was found for any of the assessed executive functions of inhibition, updating and shifting. We also did not find specific benefits of relaxation practice, which was induced via an evidence-based technique and involved as thorough instructions as the mindful breathing practice. In the following, we discuss possible reasons for our findings.

States of mindfulness and relaxation were assessed as a manipulation check with the SRSI3s. In Experiment 1, participants in the mindful breathing as well as the relaxation condition reported increased scores in focus and relaxation from pre- to post-measurement, indicating that the practice had effects on the subjective experience of these states. However, in Experiment 2, which prolonged participants' practice duration and frequency, SRSI3s scores did not indicate changes in states of mindfulness and relaxation. Confounding factors that may have influenced the interventions' effectiveness should be considered. As a majority of the participants were university students, academic stress could have affected training effectiveness. However, both experiments were carried out during the same period of the academic year, just one year apart from each other. Therefore, experienced academic stress was likely comparable in both experiments. Testing and training were carried out during the COVID-19 pandemic, and restrictions on social as well as academic/professional aspects of participants' lives could have affected the effectiveness of the intervention. We therefore assessed experienced pandemic burden and found comparable levels in both samples. Thus, experienced burden could not account for the differences between Experiment 1 and 2.

We administered all trainings and tests online, and it might be argued that the online practice and testing conditions were too noisy to render systematic effects. Regarding the online measurement of reaction time tasks, research has shown that while technical (e.g., keyboards, CPU performance, operational systems and browsers utilized) as well environmental variations (e.g., background noise, viewing angle and distance, presence of distractions) can decrease precision, online measurement reliability is high given an adequate sample size and number of trials (e.g., Anwyl-Irvine et al., 2021; Brand & Bradley, 2012; Chetverikov & Upravitelev, 2016; Pronk et al., 2020). To comprehensively control for environmental variations, participants received detailed instructions regarding their sitting position, usage of headphones for noise cancellation and were asked to perform testing and training in a disturbance-free environment. To increase compliance with these instructions, participants had to manually check a box that they were adhering to each instruction before testing and training. Furthermore, as described above, we implemented several steps to secure fidelity by providing participants with thorough instructions about the necessary equipment and context for participation, sound checks for the audio listening, and verification of the fidelity of the online reaction time measurements and adherence to the audio instructions. We also assessed the frequency and quality of disturbances during practice and testing. A large majority of participants reported no disturbances at all, and the disturbances reported were rated to be of low impact for study participation. Complementary statistical analyses that included disturbance variables did not suggest any systematic influence on the results. It therefore seems unlikely that our testing scheme was responsible for the lack of effects of mindful breathing meditation or relaxation practice on executive functions. In addition, analyses of the CPT-II, N-Back task and Number-Letter Task reliably replicated known effects of task characteristics (e.g., RT adaptation to prolonged ISIs during the CPT-II, increased RT with higher n-levels during the N-Back task, RT switch costs for switch compared to non-switch trials during the Number-Letter Task) as well as improved performance in general from pre- to post-measurement. This also supports the claim that the tasks and testing were sufficiently sensitive to measure the respective executive function in an online environment, as has been previously shown for online assessments of attentional and executive control (Semmelmann & Weigelt, 2017).

Even though the online assessments appear to have been valid, the trainings of mindful breathing and relaxation might have been compromised through online delivery. Effect sizes of online mindfulness training on various psychological constructs have been shown to be lower compared to face-to-face training (Mrazek et al., 2019; Spijkerman, Pots, & Bohlmeijer, 2016), with adherence to training protocol being discussed as an important moderating factor for the effectivity of online delivery (e.g., Fish et al., 2016). As mentioned before, we carefully controlled for adherence to training exercises both in the sense that the tasks were carried out to completion as well as asking participants whether they had adhered to the training instructions. A further important difference between in-person and online mindfulness practice that is discussed in the literature is the lack of trainer support in online interventions. However, this point refers more to longer-term interventions, and experimental designs for brief trainings rarely include trainer support, irrespective of lab-based or online delivery. To our knowledge, there have been no studies to date assessing the effects of a short-term online mindful breathing meditation training on executive functions in a pure online experimental design. Studies utilizing app-based home training with longer training periods and lab-based measurement of executive functioning have found mixed results. While Noone and Hogan (2018) reported no improvement on working memory following 6 weeks of app-based meditation training compared to a sham meditation, Bennike et al. (2017) found reduced mind wandering (which may be interpreted as a marker of improved executive functioning, Mittner et al., 2014) following 30 days of app-based meditation training compared to a cognitive training. In comparison to the present study, both studies employed substantially longer training durations and utilized a mindfulness meditation course on Headspace app, which encompasses various exercises besides mindful breathing meditation as well as psycho-educational aspects about mindfulness.

In summary, considering factors that may have compromised the validity of our study, we would argue that previous research on online testing, our own precautions to ensure fidelity and measurement accuracy, the fact that we replicated basic task-specific effects in both experiments and finally statistical analyses controlling for possible confounds support the interpretation of our data as valid. Engaging in a mindfulness breathing meditation three (Experiment 1) or four times (Experiment 2) across 5 days did not result in mindfulness-specific improvements in inhibition, updating or shifting. The question of domain-general versus domain-specific effects of mindful breathing meditation on executive functions after brief trainings remains open. Also, the proposition that in the first stages of training, relaxation may yield similar effects as mindful breathing meditation cannot be rejected based on the present data. Previous studies suggest some parallels between early-stage mindful breathing and relaxation practice (Fell et al., 2010; Vieth & von Stockhausen, 2022), and the manipulation check for Experiment 1 showed similar increases in focus and relaxation in both training groups. It therefore seems worthwhile to pursue this question further and specify when the effects of both approaches, which have very different goals and use quite different means, begin to diversify. Based on our results, findings from other studies, and reviews on the effects of brief mindfulness trainings, we would conclude that if brief trainings in mindful breathing meditation have effects on executive functions, they may be too transient to surface reliably across (traditional response time) tasks, laboratories, and variations in instruction length. A major question is therefore that of dose-response relations and underlying mechanisms.

The present findings may indicate that the benefits of mindful breathing meditation on executive functions require longer periods of practice to unfold, and/or that the effects of short training periods are rather transient. In our experiments, we tried to address this issue of dose-response by increasing practice time and frequency across experiments. However, the results suggest that our time window was still too short to assess longer lasting, or more stable, benefits of the practice with traditional cognitive tasks. Thus, the effects of short mindful breathing meditation trainings may not simply cumulate, and may therefore require assessment methods better suited to capturing transient state benefits. Future studies may include, for example, momentary assessments throughout participants' practice, thereby monitoring changes, even transient ones, more closely while they occur. For example, in an experience sampling study over 21 days of mindfulness-based home practice, Levi et al. (2021) reported no cumulative effects of the practice after completion of the 21-day training, but that the daily dose of meditation predicted state mindfulness, decentering, and emotional valence on a day-to-day basis. Designs utilizing experience sampling could also be beneficial for investigating the cognitive benefits of short mindful breathing meditation trainings. Assessing the transition from transient to more stable effects, which are reported after longer-term trainings and in respective reviews (e.g., Fell et al., 2010), would furthermore require longitudinal designs with measurement points between pre- and post-measurement.

6. Limitations and future research

There was a strong over-representation of female participants in both samples (77 % in Experiment 1 and 88 % in Experiment 2), which may have affected the generalizability of the results. For example, a recent study by Wang et al. (2021) has shown that a mindfulness training of 4 × 20 min was more effective in reducing the attentional blink effect for female participants compared to a video game training, from which male participants profited to a larger degree. Therefore, future studies may control for gender influences by collecting a more balanced sample.

Furthermore, our participant inclusion criteria regarding prior meditational experience excluded participants who had engaged in mindfulness related practice within the last three months. While we would argue that continued practice is necessary to preserve any effects of mindfulness training (e.g., Fell et al., 2010; Malinowski, 2013), and complementary statistical analyses did not show any effects of previous practice, future studies may consider longer periods of non-practice or the inclusion of naive participants only to control for a possible reactivation of prior mindfulness training effects.

As several authors have pointed out before (e.g., Davidson & Kaszniak, 2015; Vago et al., 2019), future research in the field would benefit from a more standardized approach regarding experimental design and methodology to allow for a better comparability of results. We would suggest that such an approach include a baseline measurement to account for possible baseline differences within the respective sample, which need to be controlled for in order to establish the true effects of (brief) meditation trainings on executive functions (e.g., Leyland et al., 2019). Furthermore, the inclusion of passive control groups and random assignment to experimental conditions is recommended to control for effects of repeated testing (i.e., pretest sensitization effects, see for example Willson & Putnam, 1982; Kim & Willson, 2010). Regarding active control conditions, we chose to control for induced relaxation, as it is thought of as a by-product of mindful breathing meditation training, especially in novices (Fell et al., 2010). Another promising active control condition is sham meditation, which accounts for expectancy effects that may affect results either positively or negatively (e.g., Prätzlich et al., 2016). For both active and passive control conditions, detailed instructions are necessary to control for variations between conditions within an experiment and for ease of comparison between studies. Additionally, assessing state mindfulness and relaxation as a manipulation check seems advisable to show that trainings had the intended effects and to assess possible differences or similarities between mindful breathing meditation and active control conditions.

Naturally, different authors have utilized different measures and operationalizations of cognitive functions, posing a further challenge to comparing results across studies. Also, they refer to different theoretical frameworks and classificational systems (e.g., Diamond, 2013; Miyake et al., 2000; Nigg, 2017; Petersen & Posner, 2012; Posner & Petersen, 1990). Many authors in the field of meditation research have argued that a unifying theoretical approach is vital for advancing our understanding of the mechanism through which meditation works (e.g., Sedlmeier, 2022; Vago et al., 2019). This long-term endeavor could be supported if research findings were easier to integrate and interpret, as discussed before. An obvious step in that direction could be multiple assessments of cognitive functions within one study, which would strengthen a more conceptual (rather than task dependent) level of interpretation of findings.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data will be made available on request.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.actpsy.2023.104006.

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