Mindfulness
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REVIEW

Does Mindfulness Meditation Training Enhance Executive Control? A Systematic Review and Meta-Analysis of Randomized Controlled Trials in Adults

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Abstract

Objectives Over the last years, mindfulness meditation has been claimed to be effective in enhancing several cognitive domains, including executive control. However, these claims have been mostly based on findings pertaining to case-control and cross-sectional studies, which are by nature unable to reveal causal relationships. Aiming to address this issue, we set out to conduct the first quantitative assessment of the literature concerning mindfulness meditation as an enhancer for executive control considering only randomized controlled studies.

Methods We conducted a systematic review and meta-analysis covering experimental studies testing the effect of mindfulness meditation training on at least one executive control function (working memory, inhibitory control, or cognitive flexibility) in adult samples. Four databases were examined, resulting in the identification of 822 candidate references. After a systematic filtering process, a set of 16 studies was retained for evaluation, of which 13 could be included in a subsequent meta-analysis.

Results We found an average effect size of $g = 0.34$ [0.16, 0.51], indicating a small-to-medium effect of mindfulness meditation training in enhancing executive control. Effect sizes for individual functions were $g = 0.42$ [0.10, 0.74] for working memory, $g = 0.42$ [0.20, 0.63] for inhibitory control, and $g = 0.09$ [−0.13, 0.31] for cognitive flexibility. Funnel plot asymmetry analysis revealed no evidence of publication bias.

Conclusions Taken together, our findings provide preliminary and moderate yet positive evidence supporting the enhancing effects of mindfulness meditation on executive control. Shortcomings of included studies and considerations for future empirical and meta-analytical research are discussed.

Keywords Mindfulness · Meditation · Executive control · Executive functions · Working memory · Inhibitory control · Cognitive flexibility · Cognitive enhancement

The scientific interest in mindfulness meditation has grown exponentially over the last decades (Tang 2017; Van Dam et al. 2018). In recent years, studies have reported beneficial effects of mindfulness meditation on outcomes pertaining to a variety of domains, including mental and physical health (Grossman et al. 2004), brain and cognitive function (Tang et al. 2015), and interpersonal functioning (Mcgill et al. 2016). In parallel, several mindfulness-based programs are currently being integrated into a number of institutional settings including the healthcare system (Demarzo et al. 2015), the educational system (Sibinga et al. 2016), the workplace (Good et al. 2016), and the military (Johnson et al. 2014).

It is broadly acknowledged that there are two styles of mindfulness meditation practice: focused attention (FA) and open monitoring (OM) (Lutz et al. 2008; Malinowski 2013; Tang et al. 2015). In FA meditation, the practitioner sustains the attentional focus on a chosen object (most commonly one's breath) and returns it to this anchor each time the mind

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wanders. Accordingly, it is theorized that FA develops three attentional control processes, along with their underpinning neural networks: (a) the monitoring faculty that remains vigilant to mind-wandering while attention is sustained to the anchor (alerting network); (b) the ability to detect mind-wandering (salience network) and to disengage from it (executive network); and (c) the ability to redirect the focus to the anchor (orienting network) (Lutz et al. 2008; Malinowski 2013). Some proficiency in FA meditation is required to transition to OM practice, in which the aim is to remain solely in the monitoring state maintaining an open, nonreactive attention to all arising and passing mental events. OM would further develop the practitioner's meta-awareness of inner mental processes, including mind-wandering (Lutz et al. 2008).

Attentional processes can be enhanced by regular repetition of tasks that involve specific attention networks (Posner et al. 2015). On the other hand, it is well documented that mind-wandering can substantially compromise available attentional and executive control resources, especially when needed to be sustained over prolonged periods of time (Thomson et al. 2015). By systematically strengthening the aforementioned neurocognitive networks (via FA), as well as by increasing one's capacity to be aware of and disengage from mind-wandering (via FA and OM), mindfulness meditation has been proposed as a potential means for cognitive enhancement (e.g., Lindsay and Creswell 2017; Lutz et al. 2008; Malinowski 2013). Under this proposal, mindfulness meditation training would enhance (executive) attention processes both by increasing available resources and by allowing for more efficient use of them. Even though these explanations remain largely speculative, empirical evidence supporting the cognitive enhancing effect of mindfulness meditation has indeed started to emerge regarding various cognitive functions, which include—yet are not limited to—the executive control domain (Chiesa et al. 2011; Gallant 2016; Lao et al. 2016).

Executive control is a central piece of human cognitive architecture. Also referred to as executive functioning or cognitive control, executive control encompasses a family of top-down cognitive processes that scaffolds human goal-directed behavior and self-regulation. Research has shown that executive control is relevant for mental and physical health (Penadés et al. 2007; Will Crescioni et al. 2011), academic and professional success (Bailey 2007; Borella et al. 2010), or simply to enjoy a better quality of life (Brown and Landgraf 2010). To consider some examples, better executive control has been linked to healthier eating (Calvo et al. 2014), better math and reading competence in school (Checa et al. 2008), better marital satisfaction (Eakin et al. 2004), and more prosocial behavior (Broidy et al. 2003). In turn, dysfunction of this system, either due to aging, stroke, attention-deficit/hyperactivity disorder, or else, may hinder leading an independent life (Chan et al. 2008). As research shows, developing strategies and tools capable to strengthen executive control entails a highly relevant societal challenge to take on.

There have been various formulations of executive control, ranging from views considering it as a unitary multipurpose control system to fractionated models conceiving it as a collection of relatively independent executive functions (for a review, see Morton et al. 2011). Among fractionated perspectives, there is general agreement in differentiating three core executive functions: working memory, inhibitory control, and cognitive flexibility (Diamond 2013; Miyake et al. 2000).

The present study follows the conceptual framework and definitions proposed by Diamond (2013). According to Diamond (2013), working memory involves holding information for processing while simultaneously being able to manipulate it (e.g., maintaining task-relevant information and relating it to long-term memory content in order to solve a particular problem). Examples of tasks tapping into working memory are the Backward Digit Span (that requires to hold in memory a series of numbers while rehearsing them in inverse order) or the N-Back (where the subject is presented with a sequence of stimuli, having to indicate when the current stimulus matches the one shown n presentations earlier in the sequence).

Inhibitory control involves being able to control one's behavior, attention, thoughts, and/or emotions in order to override a strong internal predisposition or external lure in benefit of longer-term goals. The Stroop test (where the subject is required to respond to the color of the ink of words while inhibiting attending to its meaning in order to avoid the more automatic word naming response) and the Go/No-Go task (that requires the subject to repeatedly respond by pressing a button, but to inhibit that habitual response when certain rare stimuli are presented) are two popular examples of tasks tapping into different aspects of inhibitory control.

Cognitive flexibility is defined as the ability to change our mental set to efficiently adapt to the demands of the environment. It is typically measured by means of tasks such as the Trail Making Test (where the subject is required to continuously switch between responding to numbers and letters) or the Wisconsin Card Sorting Task (that requires the subject to flexibly switch response strategies based on experimenter's feedback on participants' performance). Other higher-order executive-related processes such as planning, reasoning, or problem solving would be built upon the three core executive functions (Diamond 2013).

To date, three systematic reviews have assessed the effect of mindfulness meditation over executive control in adult population (Chiesa et al. 2011; Gallant 2016; Lao et al. 2016). Chiesa et al. (2011) evaluated the effects of several mindfulness-related practices (including Zen meditation, Vipassana retreats, or Mindfulness-Based Stress Reduction programs, among others) on a wide range of cognitive functions. Regarding executive control, the authors concluded that mindfulness training may be effective in enhancing executive attention and response inhibition (aspects of inhibitory control), verbal fluency (an aspect of cognitive flexibility), and working memory at different stages of training. Lao et al. (2016) conducted a similar review but focused on standardized Mindfulness-Based Stress Reduction (MBSR) and Mindfulness-Based Cognitive Therapy (MBCT) programs, and found preliminary evidence for working memory and cognitive flexibility (but found no evidence for executive attention, while the evidence for response inhibition was mixed). Lastly, Gallant (2016) included mainly standardized mindfulness-based programs (such as MBSR or MBCT) but also other meditation practices (such as Vipassana or Shambala meditations) and narrowed the focus of the systematic review to just executive functioning. Gallant, in contrast, found inhibitory control to be the most consistently executive function improved by mindfulness meditation training, with more variable outcomes for working memory and cognitive flexibility.

Even though these findings may seem somewhat inconsistent, the discrepancies can arguably be attributed to two reasons. First, the reviews operationalized mindfulness meditation differently. Consequently, to an extent, each of them included studies evaluating different types of programs and meditations. This conceptual divergence likely affected their results and conclusions. Second, each review followed different taxonomies of cognition and executive control, thus seeming discrepancies may be partly just a terminological issue. To bring one example, Lao et al. (2016) concluded that "studies did not support […] executive function improvements. We found preliminary evidence for improvements in working memory and […] cognitive flexibility" (p. 109). These authors conceptualized working memory as a memory sub-function, while cognitive flexibility was not subsumed by any broader cognitive category. Such cognitive classification differs from the one followed by Gallant (2016), for whom executive functioning comprises inhibition (inhibitory control), updating (working memory), and shifting (cognitive flexibility). When taking into account these divergences, previous research appears to provide initial evidence for an enhancing effect of mindfulness meditation over executive control—even if such effect is still to be characterized in terms of both the particularities of the practices that bring it about and the specific cognitive sub-domains involved.

These preliminary findings of the positive effects of mindfulness meditation training in executive control outcomes (as well as in other cognitive and non-cognitive domains) are nonetheless paralleled by significant concerns that scholars from within and outside the field have raised about the methodological rigor behind much of extant evidence (e.g., Coronado-Montoya et al. 2016; Isbel and Summers 2017; Van Dam et al. 2018). One of the main limitations in the mindfulness literature refers to an overabundance of research methodologies that are unable to reveal causal relationships, such as the use of case-control and cross-sectional designs. High-standard methodologies such as randomized controlled trials (RCTs) have been rather scarce in the literature until recently (Creswell 2017). Accordingly, the above mentioned systematic reviews were largely based on non-experimental (i.e., non-RCT) studies. Moreover, none of them conducted a meta-analysis, likely because the inclusion of different study designs hindered the quantitative synthesis of the results. This circumstance underscores the need for a systematic and meta-analytic assessment of the literature circumscribed to only experimental studies, so as to validate—or otherwise update—our current understanding of the field.

On the basis of the above considerations, the aim of the present systematic review and meta-analysis was to evaluate and quantify the efficacy of mindfulness meditation (i.e., FA and OM practices) in enhancing executive control (i.e., working memory, inhibitory control, and cognitive flexibility) in adult population by—importantly—assessing RCTs exclusively. In addition, we also set out to assess whether findings in our review were likely to be overestimated by methodological biases in included studies (Higgins et al. 2011) and/or by publication bias (Sterne et al. 2011).

Methods

Search Procedure

The systematic review was conducted by following the PRISMA statement (Moher et al. 2009; see Supplementary Materials for a PRISMA checklist). We examined the databases Web of Science, PsycINFO, PubMed, and Cochrane Library in search of eligible studies, entering the following syntax: "(mindfulness OR "integrative body-mind training" OR meditat* OR MBSR OR MBCT OR IBMT OR MBRP OR MBRE OR "focused attention" OR "open monitoring" OR "body scan" OR zazen OR zen OR vipassana OR samatha OR "acceptance and commitment") AND ((executive OR cognition OR "cognitive function" OR prefrontal) OR (inhibition OR inhibitory OR "self-control" OR ("selective attention" OR "focused attention" OR cingulate)) OR ("working memory" OR updating OR monitoring) OR (flexibility OR shifting OR switching)))". The search was conducted on September 2017. It was limited to articles in English, Spanish, or French, published any time.

A set of 822 registers was obtained, from which we conducted a systematic filtering process (see Fig. 1). First, we removed 342 duplicates. Thereafter, inclusion criteria C1 to C5 (see "Selection Criteria") were applied while screening the title and abstract of the remaining papers. Papers not clearly violating at least one criterion were retained for full-text examination. On a later stage, the first author (LC) and the second author (VP) independently examined the remaining set of 57 papers while applying the inclusion criteria. When

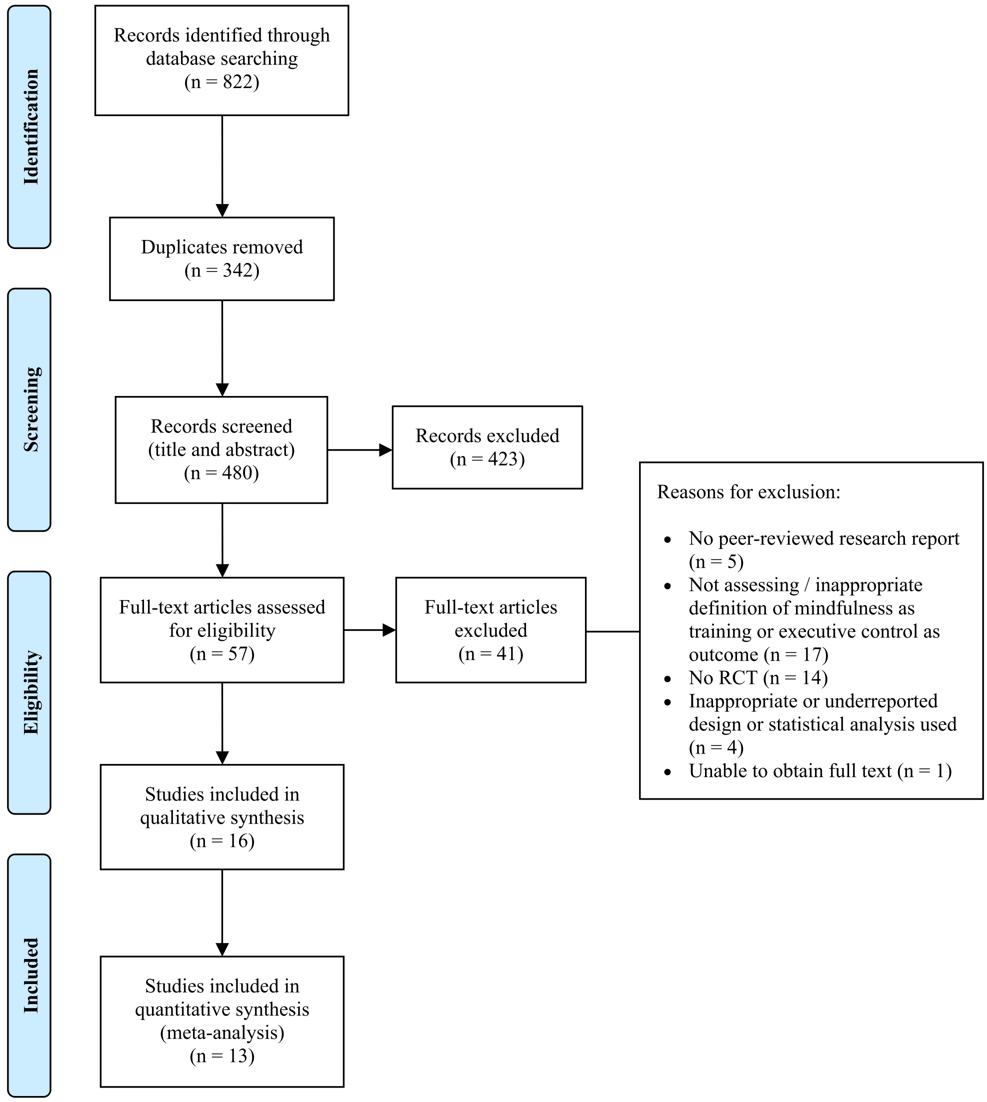

Fig. 1 PRISMA flowchart of study selection process

necessary, we contacted the authors of the studies for paper retrieval and/or further clarification of its content. Inter-rater disagreements regarding the inclusion of studies were solved through discussion. In case of persistent disagreement, the fourth author (JL) was brought into the discussion until consensus was achieved. A set of 16 studies was retained after full search procedure (Ainsworth et al. 2013; Allen et al. 2012; Greenberg et al. 2013; Josefsson et al. 2014; Mallya and Fiocco 2016; Mitchell et al. 2017; Moynihan et al. 2013; Mrazek et al. 2013; Prätzlich et al. 2016; Sahdra et al. 2011; Schoenberg et al. 2014; Tang et al. 2007; Tsai and Chou 2016; Valls-Serrano et al. 2016; Wetherell et al. 2017; Zeidan et al. 2010).

Selection Criteria

Studies needed to satisfy the five following criteria to be included in this review. (C1) The article is a peer-reviewed research report. Narrative and systematic reviews, doctoral dissertations, posters, registered study protocols, commentaries, books and book chapters, essays, and other theoretical accounts were therefore excluded. (C2) The study includes mindfulness mediation training as part of the intervention and assesses executive control as outcome according to the definitions provided in section below (see "Operational Definitions"). (C3) The study is a controlled trial with randomization of participants to experimental (receiving meditation training) and control group (not receiving meditation training). (C4) Study participants are adults (i.e., aged ≥ 18 years). (C5) Descriptions of experimental design, statistical analyses, and results of the study are complete and clearly described. Statistical analyses assess pre- to post-intervention differences in the experimental as compared with the control group (i.e., analysis of interaction between time of assessment [pre- and post-intervention] and group [experimental and control group] is addressed). Studies analyzing solely post-intervention differences were therefore excluded. In case of incompleteness or ambiguity, we contacted the first author of the study for further clarification. Studies for which clarification was not provided were excluded.

Operational Definitions

Mindfulness Meditation Training

We defined mindfulness meditation training as any training regime in which participants are taught one or both formal practices broadly recognized as mindfulness meditation (i.e., FA and/or OM meditation). Given that we aimed, to the extent possible, at evaluating the effects of mindfulness meditation free from ancillary factors (see Isbel and Summers 2017), at least one of the included mindfulness practices should be purely cognitive and, thus, not involve physical exercise or vocalization (interventions exclusively based on yoga or mantra repetition were therefore excluded). Lastly, the training regime should be sustained in time for more than one session (1-day brief laboratory inductions were therefore excluded).

Executive Control Assessment

Executive control assessments must include at least one neuropsychological test or computerized cognitive task involving reaction time or response accuracy measurement (studies using solely self- or other-report measures or only physiological, neurophysiological, or neuroimaging assessments were therefore excluded). Moreover, they must assess specifically either working memory or inhibitory control or cognitive flexibility as defined by Diamond (2013). General measures that conflate several sub-processes (e.g., the Symbol Digit Modalities Test, where participants presumably deploy working memory and set switching as well as other cognitive processes such as fine motor skills) were therefore excluded.

Data Extraction

We extracted the following information from each of the included studies: mean age of the sample being tested, sample size, duration of the intervention provided, total approximate dosage of the intervention (in minutes), population assessed (healthy or clinical), name of the intervention (when provided), control group used, and categorization of control group as active or passive (see Table 1). The first author extracted the data, and any queries were clarified with the second and fourth authors. Subsequently, we extracted from each study the data needed to calculate an effect size estimate (see "Data Analyses" for details). In three studies, data of interest were only depicted graphically (i.e., were not reported numerically). In those instances, we used the online software WebPlotDigitizer (Version 4.1; Rohatgi 2018) to extract the underlying numerical data from bar plots. WebPlotDigitizer has been shown as a valid and reliable tool (Drevon et al. 2017). When data were not available (either numerically or graphically), we contacted the corresponding authors of the study via e-mail for data retrieval. When no reply was obtained, we contacted all other authors. Unfortunately, for three of the included studies, either requested data were not available or a reply was not obtained from any of its authors. Therefore, only 13 of the 16 studies included in the systematic review could also be included in subsequent meta-analyses.

Risk of Bias Assessment

Methodological quality of included studies was assessed by means of the Cochrane Collaboration's tool for assessing risk of bias (Higgins et al. 2011). The tool evaluates six potential sources of bias: (a) selection bias (whether randomization was adequately performed and allocation of participants to experimental/control group adequately concealed); (b) performance bias (whether participants and personnel providing the intervention were blind to study hypothesis); (c) detection bias (whether outcome assessors where blinded to study hypothesis); (d) attrition bias (whether amount, nature, or handling of incomplete outcome data was adequately addressed); (e) reporting bias (whether selective outcome reporting was found); and (f) other bias (whether the study appears to be at risk of other biases not previously evaluated). The Cochrane Collaboration's tool diagnoses studies at *high* or *low risk of bias* for each of the aforementioned domains. Alternatively, if studies fail to provide enough information to assess their quality, they are evaluated as *unclear risk of bias*. We used RevMan (Version 5.3, The Cochrane Collaboration 2014) software to code information of each of the studies included and to generate graphical summaries of their individual and combined risk of bias (see Figs. 2 and 3).

Data Analyses

Effect Size and Variance

We conducted a meta-analysis in order to estimate the weighted averaged effect size found in our pool of included studies. To this end, Hedges' g was chosen as effect size estimate for each individual study. Hedges's g is a weighted version of Cohen's d that allows for unbiased estimation when sample sizes are small (Borenstein et al. 2009). Given that all included studies used a pretest-posttest control group experimental design, we followed the procedure for effect size estimation recommended by Morris (2008). Thus, Hedges' g is defined as follows:

Table 1 Characteristics of included studies

Study	Mean age	Sample size	Duration of intervention	Dosage (minutes)	Population	Intervention	Control group	Active control
Ainsworth et al. (2013)	20.3	73	3 × 1 h sessions (plus 10 min daily home practice during 8 days)	260	Healthy	Other (FA) Other (OM)	Relaxation	Yes
Allen et al. (2012)	26.5	38	6 weekly 2 h sessions (plus 20 min daily home practice)	1560	Healthy	Other	SRL	Yes
Greenberg et al. (2013)	26	65	7 weekly 2 h sessions and half a day retreat (plus 20 min daily home practice)	2180	Healthy	MBCT	Wait-list	No
Josefsson et al. (2014)	48.1	104	2 × 45 min weekly sessions, for 4 weeks	360	Healthy	Other	Relaxation Wait-list	Yes No
Mallya et al. (2016)	69.2	97	8 weekly 2.5 h sessions (plus 30 min daily home practice)	2880	Healthy	MBSR	R&R	Yes
Mitchell et al. (2017)	38.6	20	8 weekly 2.5 h sessions (plus 5 to 15 min daily home practice)	1760	ADHD	MAPs for ADHD	Wait-list	No
Moynihan et al. (2013)	73.4	208	8 weekly 2 h sessions and 7 h retreat	1380	Healthy	MBSR	Wait-list	No
Mrazek et al. (2013)	20.8	48	4 × 45 min sessions, for 2 weeks (plus 10 min daily home practice)	500	Healthy	Other	Nutrition program	Yes
Prätzlich et al. (2016)	28.6	59	3 × 20 min sessions	60	Healthy	Other (FA-HE) Other (FA-LE)	SM-HE SM-LE Silence	Yes Yes Yes
Sahdra et al. (2011)	48	59	7 h a day for 3 months	38,200	Healthy	Other	Wait-list	No
Schoenberg et al. (2014)	36.8	44	12 weekly 3 h sessions (plus 30 to 45 min daily home practice)	5310	ADHD	MBCT	Wait-list	No
Tang et al. (2007)	21.8	80	5 × 20 min sessions	100	Healthy	IBMT	Relaxation	Yes
Tsai et al. (2016)	20	40	12 weekly 50 min sessions	600	Healthy	Other	Wait-list	No
Valls-Serrano et al. (2016)	33.1	32	8 weekly 40 min sessions	320	Substance abuse	Other	TAU only	No
Wetherell et al. (2017)	71.9	96	8 weekly 90 min sessions	720	Depression/anxiety	MBSR	Health education	Yes
Zeidan et al. (2010)	22.5	49	4 × 20 min sessions	80	Healthy	Other	Book listening	Yes

Intervention: *FA* focused attention, *OM* open monitoring, *HE* high expectations, *LE* low expectations, *MBCT* mindfulness-based cognitive therapy, *MBSR* mindfulness-based stress reduction, *MAPs* mindful awareness practices, *IBMT* integrative body-mind training, *GMT* goal management training

Control group: *SRL* shared reading and listening, *R&R* reading and relaxation, *SM* sham meditation, *TAU* treatment as usual

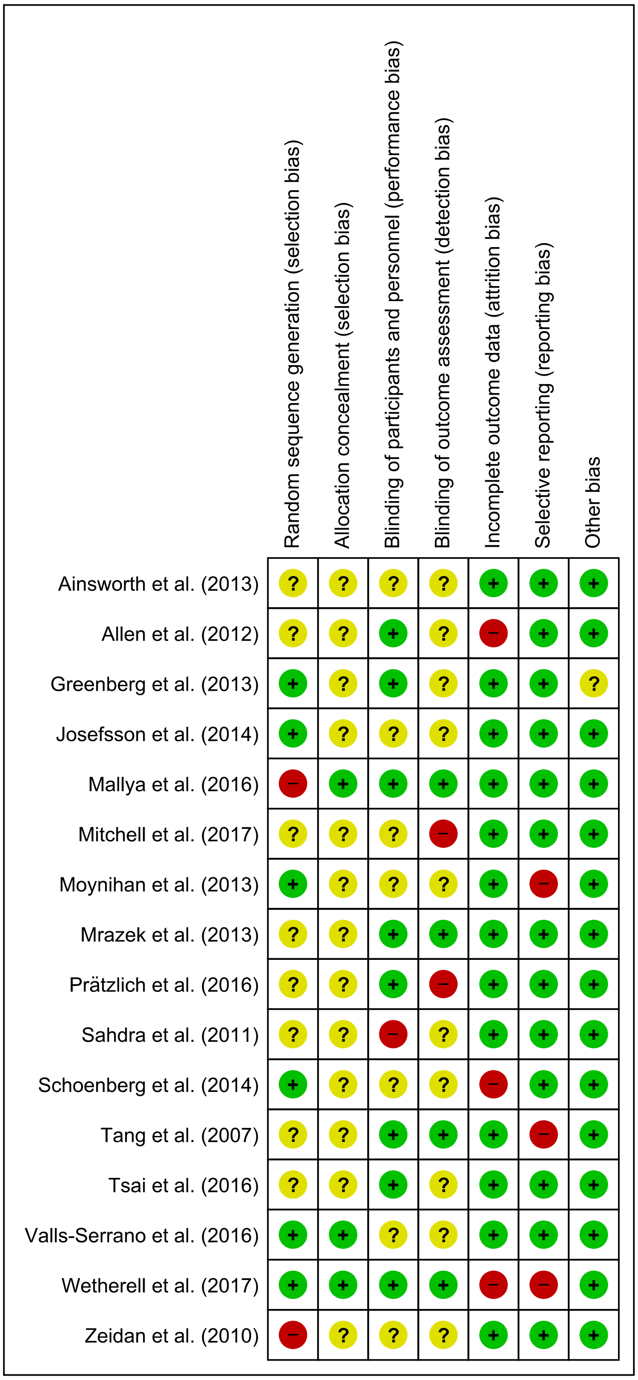

Fig. 2 Risk of bias summary

$$g = C_P\left[\frac{\left(M_{\text{post,T}} - M_{\text{pre,T}}\right) - \left(M_{\text{post,C}} - M_{\text{pre,C}}\right)}{SD_{\text{pre}}}\right]$$

where $M_{\text{post, T}}$, $M_{\text{pre, T}}$, $M_{\text{post, C}}$ and $M_{\text{pre, C}}$ are the post-intervention and pre-intervention mean scores for the treatment group and control group, respectively. In turn, SD_{pre} is the pooled standard deviation of the pre-intervention scores defined as follows:

$$SD_{\text{pre}} = \sqrt{\frac{(n_{\text{T}} - 1)\ SD^2_{\text{pre,T}} + (n_{\text{C}} - 1)\ SD^2_{\text{pre,C}}}{n_{\text{T}} + n_{\text{C}} - 2}}$$

where n_T, n_C, $SD_{pre,\ T}$ and $SD_{pre,\ C}$ are the number of participants and the standard deviations of the scores at pre-intervention for treatment and control group, respectively. Lastly, C_P is a correction for bias defined as follows:

$$C_P = 1 - \frac{3}{4\ (n_{\text{T}} + n_{\text{C}} - 2) - 1}$$

In turn, the variance of Hedges' g is defined as follows:

$$V_g = 2\ (C_P^2)\ (1-r)\ \left(\frac{n_{\text{T}} + n_{\text{C}}}{n_{\text{T}}\ n_{\text{C}}}\right)\ \left(\frac{n_{\text{T}} + n_{\text{C}} - 2}{n_{\text{T}} + n_{\text{C}} - 4}\right)\ \left(1 + \frac{g^2}{2\ (1-r)\ \left(\frac{n_{\text{T}} + n_{\text{C}}}{n_{\text{T}}\ n_{\text{C}}}\right)}\right) - g^2$$

where r is the correlation between pre-test and post-test scores. As this statistic was not reported in any of the studies under consideration, we conducted our analyses assuming $r = 0.5$ in all cases. However, to ascertain the robustness of the results under this assumption, we conducted sensitivity analyses also imputing $r = 0.25$ and $r = 0.75$ in the calculations. In both cases, we obtained virtually identical results than those for $r = 0.5$. For the sake of simplicity, we only report the results of the latter.

Aggregates

Some of the included studies contributed effect sizes for more than one outcome of interest. In those instances, we calculated aggregated effect sizes so that each study ultimately had one overall effect size to contribute to subsequent meta-analyses. The rationale for this approach is described in detail in Borenstein et al. (2009). In short, calculation of aggregates deals with the problematic practice of treating outcomes coming from the same study as if they were independent, therefore assigning more relative weight to these studies and improperly estimating the precision of its effect. The procedure followed to compute a single aggregated effect size, $\overline{g}$, from two individual outcomes is defined as follows:

$$\overline{g} = \frac{1}{2}(g_1 + g_2)$$

where g_1 and g_2 are the individual effect sizes. In turn, the variance of the aggregated effect size is defined as follows:

$$V_{\overline{g}} = \frac{1}{4}\ \left(V_{g_1} + V_{g_2} + 2r\sqrt{V_{g_1}}\ \sqrt{V_{g_2}}\right)$$

where V_{g_1} and V_{g_2} are the variances of g_1 and g_2, respectively, and r is the correlation between the two outcomes. In absence of the value of this correlation, it was set as 0.5 as proposed by Wampold et al. (1997). Some studies contributed three or four

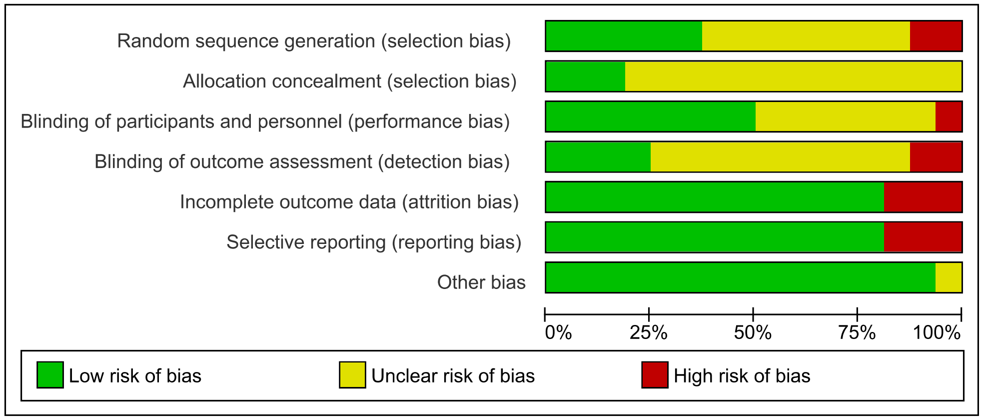

Fig. 3 Risk of bias graph

outcomes to our meta-analyses. Equations used to calculate aggregates in those instances are detailed in Borenstein et al. (2009).

To confirm the robustness of the results, we also conducted a multi-level meta-analysis including all the individual effect sizes, adding a random intercept at the study level to account for dependencies among effect sizes. The results of this analysis were virtually identical to those of the overall univariate meta-analysis with aggregate effect sizes (see "Meta-Analysis"). For the sake of simplicity, we only report the results of the latter.

Meta-Analysis

Four univariate meta-analyses were conducted: one to obtain the overall summary effect for all included studies and one per each individual executive function (i.e., working memory, inhibitory control, and cognitive flexibility). Random-effect models were fitted in all cases (Cumming 2013). Additionally, we conducted an Egger's regression test for funnel plot asymmetry to evaluate the potential presence of publication bias within the literature reviewed. Funnel plots depict effect estimates against their standard error. Given that precision in estimating an effect will increase as the sample size increases (and thus the standard error decreases), results from small studies will spread largely whereas those from large studies will collapse closer to the mean effect estimate. In the absence of bias, results should distribute symmetrically around the mean effect estimate. However, publication bias will usually induce asymmetry in the distribution of effect sizes, as small studies with negative results will be more likely to be missing. Egger's regression test statistically evaluates the degree of asymmetry of the distribution (Egger et al. 1997).

We used the metaphor package for R (Viechtbauer 2010) to conduct all meta-analytic procedures and to generate corresponding figures (forest plot and funnel plot, see Figs. 4 and 5). As proposed by Cohen (1992), we interpreted effect sizes of 0.2, 0.5, and 0.8 as small, medium, and large, respectively.

Results

Qualitative Results

The present systematic review included 16 studies sampling a total of 1112 participants. In most cases, participants were novices to the practice of mindfulness meditation, when not completely meditation-naive. In one study, though, a certain level of experience was required for participation (i.e., having completed prior to recruitment at least three 5- to 10-day meditation retreats; Sahdra et al. 2011), and four studies did not provide information regarding previous experience with meditation (Moynihan et al. 2013; Mrazek et al. 2013; Schoenberg et al. 2014; Valls-Serrano et al. 2016). The studies assessed participants from the entire adult life-span (mean ages ranging from 20.3 to 73.4 years) and primarily evaluated the effect of mindfulness meditation in healthy participants (only four studies addressed clinical populations). The use of active/passive

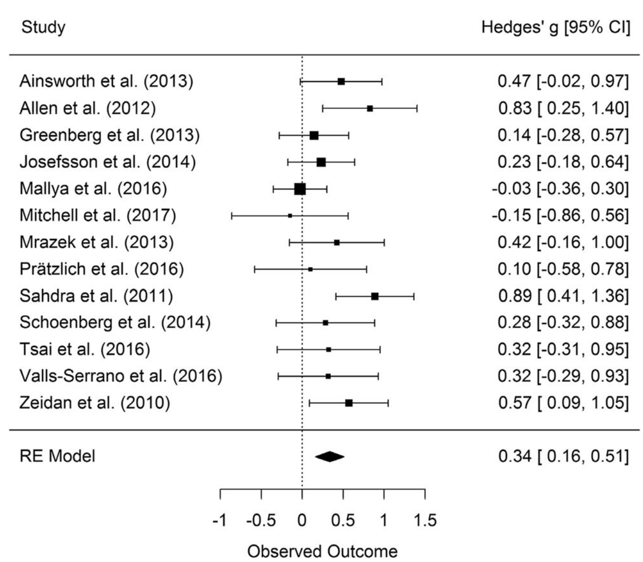

Fig. 4 Forest plot of studies included in overall meta-analysis

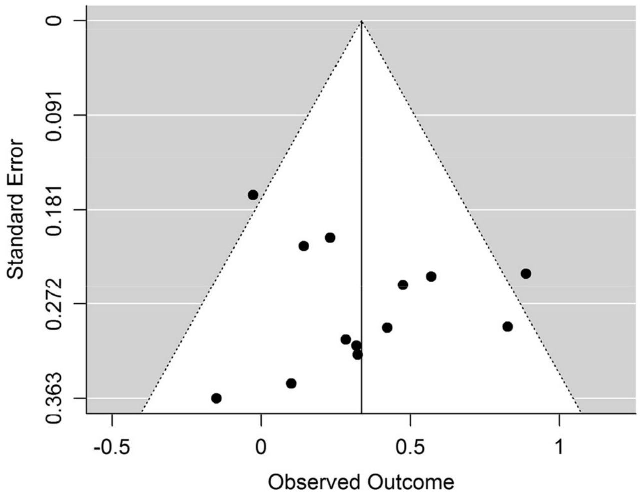

Fig. 5 Funnel plot of studies included in overall meta-analysis

control group was evenly distributed among studies (eight studies used the former, seven used the latter, and one used both). A summary of these and other main characteristics of included studies is provided in Table 1. In total, 32 outcomes were assessed throughout the studies. Of them, 15 reported a statistically significant effect favoring the mindfulness meditation training program over the control intervention. No significant effects were reported for the remaining 17 outcomes. A summary of the assessments used as well as the main findings across the studies can be found in Table 2.

The Cochrane Collaboration tool suggested that included studies are, overall, at low risk in regard to attrition and reporting bias. However, risk regarding selection, performance, and detection bias remains largely unknown, given that most studies failed to report sufficient information to evaluate them. Summaries of individual and combined risk of bias are provided in Figs. 2 and 3.

Quantitative Results

Meta-Analysis

As mentioned above, we were able to obtain enough information to estimate an effect size for 13 of the 16 studies, which were therefore included in subsequent meta-analyses. The overall weighted mean effect size reported in these studies was $g = 0.34$, 95% CI [0.16, 0.51], $z = 3.76$, $p < .001$, indicating a small-to-medium effect favoring mindfulness training over control interventions in enhancing executive control. A forest plot with individual effect sizes as well as the weighted mean is depicted in Fig. 4. The test for heterogeneity failed to reach statistical significance $Q_{(12)} = 17.18$, $p = .143$, $I^2 = 33.27\%$, 95% CI [0, 72.31]. A funnel plot representing individual effects against their standard error is depicted in Fig. 5. Egger's regression test for funnel plot asymmetry was far from statistical significance, $z = 0.40$, $p = .686$, indicating that the result of the meta-analysis is unlikely to be overestimated by publication bias.

Effect size estimates for each individual executive function were as follows: $g = 0.42$, 95% CI [0.10, 0.74], $z = 2.60$, $p = .009$ for working memory; $g = 0.42$, 95% CI [0.20, 0.63], $z = 3.83$, $p < .001$ for inhibitory control; and $g = 0.09$, 95% CI [−0.13, 0.31], $z = 0.80$, $p = .423$ for cognitive flexibility. These results indicate small-to-medium effect sizes for working memory and inhibitory control and no significant effect for cognitive flexibility. Respectively, the results for heterogeneity tests were $Q_{(3)} = 2.66$, $p = .446$, $I^2 = 0\%$, 95% CI [0, 94.70]; $Q_{(8)} = 9$, $p = .342$, $I^2 = 18.33\%$, 95% CI [0, 74.74]; and $Q_{(4)} = 4.28$, $p = .369$, $I^2 = 0\%$, 95% CI [0, 92.46].

Discussion

The aim of the present systematic review and meta-analysis was to assess the efficacy of mindfulness meditation as a cognitive enhancer for executive control. Our literature-search strategy allowed us to identify 16 randomized controlled studies conducted in adults, of which 13 could be included in a subsequent meta-analysis. Across these studies, the efficacy of mindfulness meditation (i.e., FA and OM meditation practices) in enhancing working memory, inhibitory control, and/or cognitive flexibility was assessed by means of neuropsychological tests and/or computerized cognitive tasks. Additionally, we assessed the methodological quality of the included studies and examined the possibility of publication bias in the literature reviewed.

Our findings indicate that mindfulness meditation exerts a small-to-medium effect in enhancing executive control ($g = 0.34$), with small-to-medium effect sizes for working memory ($g = 0.42$) and inhibitory control ($g = 0.42$) and no significant effect for cognitive flexibility ($g = 0.09$). Moreover, these effects seem to be consistent given the relatively small, non-significant heterogeneity found for each of them (especially in regard to each individual executive function). Furthermore, in light of the results of the funnel plot asymmetry analysis, the effects are not likely to be overestimated by publication biases. This pattern of findings, alongside the fact that they are obtained based on randomized controlled studies, suggests that mindfulness meditation training might indeed be effective in enhancing executive control.

The findings partially align with those from previous systematic reviews (Chiesa et al. 2011; Gallant 2016; Lao et al. 2016). Our results indicate that mindfulness meditation may be effective in enhancing working memory, as suggested by Chiesa et al. (2011) and Lao et al. (2016). Gallant (2016), in contrast, concluded that mindfulness meditation does not improve working memory in itself. In this author's view, working memory improvements would be driven by an indirect effect, namely, by reductions in mind-wandering. As

Table 2 Assessments used and main findings across included studies

Study	Assessment			Main findings
	WM	IC	CF	
Ainsworth et al. (2013)	–	ANT	–	Both experimental groups (FA and OM) improved executive attention after intervention as compared to relaxation group.
Allen et al. (2012)	–	EAT Stroop	–	Both groups improved stop accuracy after intervention in the EAT. Mindfulness group reduced Stroop conflict after intervention as compared to active control group.
Greenberg et al. (2013)	–	–	*Vertical boxes* and *Faces* paradigms	Improved backwards inhibition rendering improved switching performance after intervention in MBCT group relative to wait-list group.
Josefsson et al. (2014)	–	Stroop	–	No significant differences found in Stroop performance pre- to post-intervention when comparing the experimental group to neither to the active nor to the passive control group.
Mallya et al. (2016)	–	–	TMT COWAT	No significant differences were found pre- to post-intervention between MBI and control group in neither TMT nor COWAT.
Mitchell et al. (2017)	BDS	ANT CPT	TMT	No significant differences were found pre- to post-intervention between experimental and wait-list control group in any of the four tasks.
Moynihan et al. (2013)	–	–	TMT	Improvement in TMT B/A ratio in MBSR group relative to wait-list control group immediately after the intervention, but no differences found three nor 24 weeks after the intervention.
Mrazek et al. (2013)	OSPAN	–	–	Improvement in OSPAN after intervention in mindfulness group relative to reading group.
Prätzlich et al. (2016)	–	Stroop	RWT 5-Point test	Reduced Stroop interference after intervention in groups primed with positive expectations, both mindfulness meditation and sham meditation. Improvement in verbal fluency (RWT) only in mindfulness mediation high expectations group. No pre-to-post differences among groups were found for figural fluency (5-point test).
Sahdra et al. (2011)	–	RIT	–	Improvement in RIT score for experimental group, but not for wait-list group, both immediately after intervention and at 5-months follow-up.
Schoenberg et al. (2014)	–	CPT-X	–	Improvement after intervention in CPT-X in MBCT group but not in wait-list control group.
Tang et al. (2007)	–	ANT	–	Improved executive attention network efficiency after intervention in IBMT group as compared to relaxation control group.
Tsai et al. (2016)[a]	–	ANT	–	Improvement after intervention in executive attention in mindfulness group as compared to wait-list control group.
Valls-Serrano et al. (2016)	Letter-Number Sequence	Stroop	–	Intervention improved performance in Letter-Number Sequence in experimental relative to control group. No differences were found regarding Stroop test.
Wetherell et al. (2017)	–	Stroop	Verbal Fluency Test	No differential effect of intervention among groups group was found in any of the measures.
Zeidan et al. (2010)	BDS n-back	–	COWAT	Improvement after intervention in COWAT and n-back task scores in mindfulness group as compared to book listening group. No significant effect found for backward DS.

In accordance with the definitions in this review, extracted ANT scores refer only to the *executive attention network*

WM working memory, *IC* inhibitory control, *CF* cognitive flexibility

WM: *BDS* backward digit span, *OSPAN* operation span

IC: *ANT* Attention Networks Test, *EAT* error-awareness task, *CPT* Continuous Performance Test, *RIT* response inhibition task

CF: *TMT* Trail Making Test, *COWAT* Controlled Oral Word Association Test, *RWT* Regensburger Wortflüssigkeitstest

[a] Tsai et al. (2016) reported two studies out of which one is a RCT. Consequently, only results from that study are synthesized here

previously discussed, this is indeed a plausible mechanism underlying improvements in working memory following mindfulness meditation training. However, in our view, the "indirect" nature of the effect does not deny its existence. In fact, the three studies reviewed by Gallant reported working memory to be improved as a consequence of mindfulness meditation training (Jha et al. 2010; Mrazek et al. 2013; Zeidan et al. 2010).

A similar picture is found for inhibitory control, for which two previous reviews seem to align with ours (Chiesa et al. 2011; Gallant 2016) while one differs (Lao et al. 2016). The opposite pattern is seen regarding cognitive flexibility, with conclusions by Gallant (2016) more in line with ours, in contrast to those by Chiesa et al. (2011) and Lao et al. (2016). In these cases, differences in results seem to be based on conceptual discrepancies. As anticipated in the introduction, different conceptualizations of mindfulness meditation may lead to different search algorithms and study selection criteria and therefore to different sets of studies included. For instance, one researcher may define mindfulness training as comprised by only standardized mindfulness-based programs such as MBSR or MBCT, while another might also include Vipassana and other types of traditional meditation practice. The same applies to executive control. Following different cognitive taxonomies may render different sets of studies included. As an example, one researcher may consider executive attention to be part of inhibitory control, while another might consider it a separate attentional function. Ultimately, conceptual divergences—which are, to an extent, inherent to the study of psychological and cognitive constructs—may lead to different results and conclusions.

There is also a methodological reason potentially explaining differences between our results and those from previous reviews. In contrast to previous research, our study includes a meta-analysis. Findings obtained by meta-analyzing a set of studies can substantially differ from those rendered by simply "vote-counting" positive and null results in the same set of studies (Siddaway et al. 2019). For instance, a meta-analysis may find a significant positive effect when combining a set of non-significant findings coming from underpowered studies. One strength of our approach is to more accurately provide evidence in terms of the existence (or lack thereof) of an effect of mindfulness meditation in enhancing executive control, while additionally estimating the magnitude of such effect.

With all that being said, the results of our meta-analysis must be interpreted with caution due to at least two reasons. First, the risk of selection, performance, and detection bias in included studies is largely unknown. This is due to the fact that most studies failed to report sufficient information as for us to make informed judgments in this regard. In particular, details on how randomization and participants' allocation to groups were performed (selection bias) and regarding the blinding of participants, instructors, and outcome assessors (performance and detection biases) were largely underreported. Empirical research has shown that bias in randomized controlled trials is associated with overestimated intervention effects (Higgins and Green 2011). For instance, interventions not reporting to use double-blinding have been shown to be associated with overestimated intervention effects by 18%, on average, as compared with those reporting it (Pildal et al. 2007). This circumstance, added to the fact that a small proportion of items per study were at high risk of bias, calls for prudence when interpreting the size of the effects found in the meta-analysis. Moreover, it underscores the value of thorough reporting practices in future empirical research, especially when also considering that three of our 16 studies (i.e., more than 18%) could not be meta-analyzed due to the scarcity of reported statistical information.

The second reason for caution interpreting our findings relates to the small set of studies that we were able to include, which may not afford sufficient statistical power for our tests to detect existing heterogeneity, both overall and, especially, within each executive function. This limitation may also affect the test for funnel plot asymmetry and, therefore, the inferred unlikelihood of publication bias in the literature reviewed. The small number of included studies also prevented us from conducting moderator analyses to investigate whether effect sizes were related to any study-level independent variable. In fact, it is recommended to have no less than 30 studies to conduct such analysis, and, in some cases, even 60 studies would not be adequate to perform them (Lau 2006). Given the value of revealing distinctive patterns of effectiveness depending on variations in the interventions provided (e.g., duration) or in populations assessed (e.g., young versus older adults), future meta-analyses must consider this approach once more studies are available.

Importantly, readers must also be careful when interpreting the seeming different effect found for cognitive flexibility as compared with working memory and inhibitory control. Two aspects are worth discussing in this regard. First, even though these meta-analyses suggest that such differences may exist in the population, we cannot be certain that these divergences are not reflecting just sampling variation. This is especially true considering the limited number of studies contributing to the meta-analyses for working memory and cognitive flexibility (four and five, respectively). Although the estimate for cognitive flexibility did not reach statistical significance, the upper bound of its confidence interval falls at $g = 0.31$, indicating that the true population effect might actually be closer to the estimates for working memory and inhibitory control than it seems prima facie. Second, a closer look at the tests used to assess each executive function reveals that cognitive flexibility was measured by means of paper-based neuropsychological tests in all but one case. In contrast, working memory and inhibitory control were more consistently assessed by means of computerized cognitive tasks. As has been previously discussed (Mak et al. 2018), it is possible that paper-based neuropsychological tests and computerized cognitive tasks are not equally sensitive. Computerized tasks allow measuring reaction times down to the millisecond, likely being more sensitive than paper-based assessments, which are usually based on accuracy scores. If this is true, the smaller effect found for cognitive flexibility as compared with working memory and inhibitory control could be partially driven by an artifact. Once again, as the number of experimental studies on the topic grows, larger meta-analyses will be needed to evaluate the presence of this differential effect, ideally conducting moderator analysis to reveal potential confounds derived from the type of assessments used (computerized vs. paper-based tasks).

It is worth mentioning that only two of the studies under consideration in the systematic review reported being registered trials, none of which could be included in the meta-analysis. Several meta-research studies show that effect sizes tend to be substantially smaller in registered trials (Kaplan and Irvin 2015; Papageorgiou et al. 2018), possibly due to selective reporting and other biases in unregistered research that artificially inflate effect sizes (Kerr 1998; Simmons et al. 2011). Ideally, future RCTs conducted in this topic should adhere to preregistered protocols and analysis plans, to ensure that their results are free from these sources of bias.

Another aspect that may inform future directions in the field stems from the small number of clinical studies that we were able to include. Four studies conducted in clinical populations were included in the systematic review, of which only three could be meta-analyzed. As a consequence, we were not able to investigate the differential effects of mindfulness meditation in clinical as compared with healthy populations, let alone to compare different clinical populations with each other. This is unfortunate, especially given that executive control is compromised in a wide range of psychological and psychiatric disorders from attention-deficit/hyperactivity disorder or addiction to depression or schizophrenia (for reviews, see Diamond 2013; Royall et al. 2002). More experimental studies in this area are needed. In turn, ascertaining to what extent mindfulness meditation is effective in enhancing executive control in such populations as well as investigating whether or not and how much this improvement translates into symptom amelioration or remission entails a highly relevant research challenge that we encourage future meta-analytical studies to take on.

In summary, this systematic and meta-analytic review provides preliminary and moderate yet positive evidence supporting the enhancing effects of mindfulness meditation in executive control. We hope that the current meta-analysis will pave the way to future experimental studies further evaluating this subject. Importantly, these studies must consider upgrading current reporting standards regarding methods used and results obtained, so as to facilitate cumulative science. As in any other scientific field, only a cooperative endeavor will render the most valuable outcomes. In turn, as the field continues to grow, we hope that future meta-analytic research will be able to afford a more comprehensive account of the effectiveness of mindfulness meditation by revealing not only to what extent it enhances executive control but also under what specific circumstances and for which particular populations it does so.

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Author Contributions LC: conceptualized and conducted the systematic review, conducted the meta-analysis and wrote the manuscript. VP: participated as second reviewer of the literature. MAV: conducted the meta-analysis and provided critical review and feedback during the writing of the previous versions of this manuscript. JL: assisted conceptualizing and conducting the systematic review and provided critical review and feedback during the writing of previous versions of this manuscript.

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Compliance with Ethical Standards

Conflict of Interest The authors declare that they have no conflict of interest.

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