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Limited Effectiveness of Consumer Grade Neurofeedback with Mindfulness Meditation: A Meta-Analysis

Isaac Treves, Zia Bajwa, Keara D Greene, Paul A Bloom, Nayoung Kim, Emma Wool, Simon B Goldberg, Susan Whitfield-Gabrieli, Randy P Auerbach

Submitted to: Journal of Medical Internet Research
on: October 30, 2024

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[1]Massachusetts Institute of Technology Cambridge US
[2]Department of Psychology Berkeley Berkeley US
[3]Center for Precision Psychiatry Massachusetts General Hospital Boston US
[4]Department of Psychiatry, Columbia University New York City US
[5]Department of Psychiatry Columbia University New York City US
[6]Department of Counseling Psychology UW-Madison Madison US

Corresponding Author:
Isaac Treves PhD
Massachusetts Institute of Technology
Building 46-4037
550 Memorial Dr
Cambridge
US

Abstract

Background: There is burgeoning interest in the application of neuroscientific technology to facilitate meditation and lead to beneficial psychological outcomes. One popular approach is using consumer-grade neurofeedback devices to deliver feedback on brain targets during meditation (mindfulness-based neurofeedback; mbNF).

Objective: To systematically review and meta-analyze the impacts of consumer-grade mbNF.

Methods: Sixteen randomized controlled training trials, as well as 5 randomized within-subject designs were included (21 total), which examined effects on psychological distress, cognitive function, physiological health, mindfulness, and brain measures. Study risk of bias, reporting bias, and publication bias was assessed.

Results: Samples were typically small, and the majority of studies employed mindfulness apps as controls. There was a modest effect for decreases in psychological distress compared to controls (g = -0.16). However, there was no evidence for improvements in cognition, mindfulness, physiological health compared to controls. Mechanistic modulation of brain targets was not found. Gender (male/female), age, clinical status, study quality, sample size, and neurofeedback duration did not moderate effects. There was some evidence for reporting bias, but no evidence of publication bias. Adverse effects were not assessed in 19/21 studies.

Conclusions: Assertions that consumer-grade devices can allow participants to modulate their brains and deepen their meditations are not currently supported. It is possible that neurofeedback effects may rely on neurosuggestion (placebo effects of neurotechnology). Future research should examine more extensive calibration and individualization of devices, larger sample sizes, and gold-standard sham-controlled RCTs. Clinical Trial: https://osf.io/8fz73

(JMIR Preprints 30/10/2024:68204)
DOI: https://doi.org/10.2196/preprints.68204

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[1]McGovern Institute for Brain Research, Massachusetts Institute of Technology, Cambridge, MA
[2]Department of Brain and Cognitive Sciences, Massachusetts Institute of Technology, Cambridge, MA
[3]Department of Psychology, University of California, Berkeley, Berkeley, CA
[4]Department of Psychiatry, Columbia University, New York, NY, USA
[5]Division of Child and Adolescent Psychiatry, New York State Psychiatric Institute, New York, NY, USA
[6]Center for Healthy Minds, University of Wisconsin–Madison, Madison, WI
[7]Department of Counseling Psychology, University of Wisconsin–Madison, Madison, WI
[8]Department of Psychology, Northeastern University, Boston, MA
[9]Center for Precision Psychiatry, Department of Psychiatry, Massachusetts General Hospital, Boston, MA, USA.

Author Note

IN Treves https://orcid.org/0000-0001-7802-6145
Correspondence should be addressed to Isaac Treves, 43 Vassar Street 46-4077, Cambridge, MA 02139, 1-608-770-6805, treves@mit.edu

Abstract

Background: There is burgeoning interest in the application of neuroscientific technology to facilitate meditation and lead to beneficial psychological outcomes. One popular approach is using consumer-grade neurofeedback devices to deliver feedback on brain targets during meditation (mindfulness-based neurofeedback; mbNF).
Objective: To systematically review and meta-analyze the impacts of consumer-grade mbNF.
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Results: Samples were typically small, and the majority of studies employed mindfulness apps as controls. There was a modest effect for decreases in psychological distress compared to controls (g = -0.16). However, there was no evidence for improvements in cognition, mindfulness, physiological health compared to controls. Mechanistic modulation of brain targets was not found. Gender (male/female), age, clinical status, study quality, sample size, and neurofeedback duration did not moderate effects. There was some evidence for reporting bias, but no evidence of publication bias. Adverse effects were not assessed in 19/21 studies.
Conclusions: Assertions that consumer-grade devices can allow participants to modulate their brains and deepen their meditations are not currently supported. It is possible that neurofeedback effects may rely on neurosuggestion (placebo effects of neurotechnology). Future research should examine more extensive calibration and individualization of devices, larger sample sizes, and gold-standard sham-controlled RCTs.

Keywords: neurofeedback; mindfulness; device; neurotechnology; meditation; stress; cognition; Muse

Introduction

Mindfulness meditation involves cultivating non-judgmental attention to experiences in the present moment [1]. Mindfulness-based interventions (MBI) are an increasingly popular means of promoting well-being, and there is systematic evidence of benefits for children, adolescents, adults, and the elderly [2–4]. They are employed in non-clinical and clinical populations [5] and for mental health disorders like anxiety, MBIs have been documented to be equally effective as pharmacological treatment [6]. Traditional MBIs involve in-person group training led by experienced teachers; however, they are not easily scaled, teacher training is largely non-standardized, and some participants may be resistant to group settings [7]. An alternative is technology-supported mindfulness, an umbrella term encompassing mobile applications [8], virtual reality/augmented reality (VR/AR) [9], video-games [10], biofeedback [11] and neurofeedback [12,13]. Here we systematically review and meta-analyze mindfulness-based neurofeedback (mbNF) of consumer-grade neurofeedback devices to understand its effectiveness.

One of the most commonly employed MBIs is mindfulness-based stress reduction, or MBSR [14]. MBSR consists of 8 weeks of group classes and meditation training, with a full-day retreat in the sixth week. Meditations include practices like breath awareness, which involves orienting attention to one’s breath and practicing returning to the breath every time one’s attention wanders away, and body scans, which involve moving the spotlight of attention from body part to body part with a curious and non-judgmental attitude towards the sensations one encounters. The MBSR program recommends 45 minutes of practice per day, although true adherence is often substantially less [15,16].

Overall, the goal of MBSR is to teach participants to become more aware of their experiences in the present moment, and to cultivate a non-judgmental, accepting attitude towards those experiences.

Mindfulness interventions like MBSR have been shown to decrease anxiety, stress, and negative affect [17–22]. Interventions may also lead to increased sense of life's meaning and purpose [23]. Positive cognitive outcomes have also been observed. Along with sustained attention (Mak et al., 2018; Semple, 2010; c.f., MacCoon et al., 2014), MBIs may benefit working memory and other executive functions [27–30], Thus, MBIs have been associated with both positive cognitive outcomes and decreases in psychological distress. One reason is that repeated meditation practice may facilitate mindful dispositions outside of practice; indeed, this 'trait' mindfulness often increases in MBIs [31].

MBSR and other teacher-led, in-person mindfulness interventions are beneficial but relatively hard to scale. This has led to an proliferation of technology supported mindfulness, principally, app-based mindfulness programs. Briefly, app-based mindfulness programs, which involve progressing through a series of recorded meditations, can show many of the same benefits as traditional mindfulness [32,33], but with smaller effect sizes [34,35]. One reason may be lack of adherence [36,37]. Another reason may be the lack of teachers, who provide social support and help participants learn practices through expert feedback [38]. In the absence of trained teachers, other methods of providing feedback to mindfulness practitioners may be beneficial.

Neurofeedback may be one method of supporting or enhancing mindfulness learning [12,39,40]. Neurofeedback consists of measuring brain signals during a task and relaying those signals (targets) to the participant. The participant may learn with repeated practice to modulate the target, with beneficial outcomes. The primary proposed mechanism of neurofeedback is that it involves facilitating the learning of strategies or skills [41,42]. In the context of meditation, a practitioner may want to learn how to attend mindfully to the breath and regulate mind-wandering or distractions. Neurofeedback may be relayed through auditory or visual stimuli. As the participant becomes calmer and more focused (assuming this may be detected by brain signals), the stimuli relayed to them are systematically altered, and with practice the participants can learn to alter the stimuli. In summary, neurofeedback has been proposed to act as a 'technological mirror' reflecting the intricacies of the mind back to the practitioner [43]. We call this application of neurofeedback 'mindfulness-based neurofeedback' (mbNF).

Neurofeedback may be conducted in the lab, with techniques like EEG and fMRI, or in the real world with consumer-grade devices. Lab-based neurofeedback generally has been found to aid in learning attentional skills, emotion regulation skills, and pain management skills [44–50]. Yet, neurofeedback research has been criticized for inadequate control conditions and other methodological shortcomings [51,52]. In a recent systematic review, we examined the state of the evidence in laboratory mbNF [13]. While the studies were too heterogeneous to conduct a formal meta-analysis, we identified that mbNF shows promise for improving hard-to-change clinical symptoms as well as increasing state mindfulness. It is possible that these effects are driven by participants learning to harness the default-mode network (fMRI) and theta bands (EEG). However, we also identified that many studies lacked gold-standard control conditions, and reporting standards were not met.

Consumer-grade neurofeedback (which relies on small sets of dry electrodes, Textbox 1, Figure 1) is much cheaper and easier to implement at scale than laboratory mbNF [53]. However, the evidence base for consumer-grade neurofeedback is scarce [54], and the enthusiasm for these devices may outpace the evidence. For example, the Muse headband, which markets itself as 'your personal meditation coach', offers to elevate mental performance, improve sleep, improve focus, and more (choosemuse.com). Muse reports over 500,000 users. Motivated by the enthusiasm-evidence gap and by the potential of new frontiers in technology-supported mindfulness, the aim was to systematically review and meta-analyze consumer-grade neurofeedback applications for mindfulness.

For the present study, two types of randomized studies were included. We leveraged randomized controlled trials (RCTs) with between-subject controls as well as within-subject controls (e.g., participants perform mbNF and meditation only, with the order randomized). The following open questions using quantitative synthesis were addressed: (i) Is there evidence that participants are learning to regulate brain signals using neurofeedback, and reporting higher mindfulness? and (ii) are there benefits of neurofeedback compared to control conditions? Additionally, we examined methodological limitations and possible moderators (e.g., clinical conditions). To our knowledge, this is the first systematic review and meta-analysis of consumer-grade neurofeedback for mindfulness.

Textbox 1. Description of consumer-grade neurofeedback devices.

Among the consumer-grade devices commonly used for mindfulness-based neurofeedback is Muse, a portable dry EEG system. Dry EEG systems like Muse detect brain activity through sensors that do not require conductive gels, making them convenient for real-world use. Muse features sensors on the forehead (AF7, AF8) and behind the ears (TP9, TP10) to detect electrical signals generated by brain activity. These signals are amplified, filtered, and transmitted via Bluetooth to a connected device, where they are analyzed in real-time or stored for later use. Participants engage in a one-minute calibration word-association task before mbNF, which serves as a baseline for algorithms that compute brain states. Although algorithms are private due to proprietary concerns, it is likely that MUSE uses a +alpha/-theta training model, rewarding alpha waves, and inhibiting theta waves, among other EEG frequencies like delta and beta, while an artifact correction algorithm is used to remove muscle and ocular activity [55–58]. Signals are classified into “Active,” “Neutral” and “Calm” states by the Muse app and relayed to the participant using auditory feedback during the meditation session. Other systems include Emotiv, with 14 electrodes, Lowdown Focus, which has 5 electrodes and resembles eyewear, and Omni, with two electrodes.

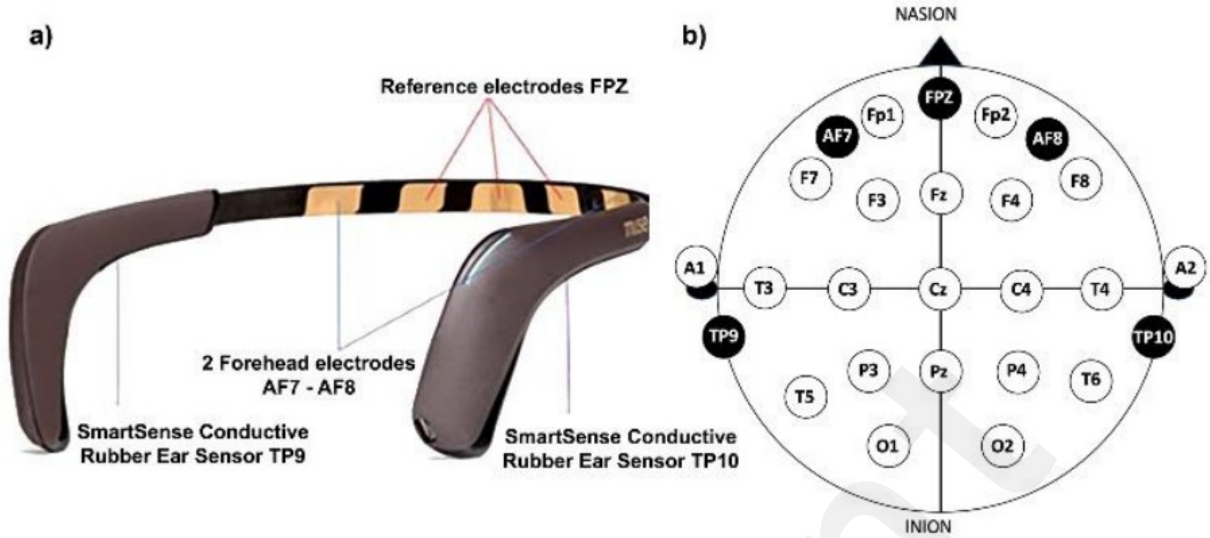

Figure 1. Consumer-grade device, Muse. a) Muse 2 headband sensors overview. b) Top-down view of the EEG electrode positions on the subject's head. Adapted from [59].

Methods

Objectives

The objective of this meta-analysis was to assess whether to mindfulness meditation concurrent with consumer-grade neurofeedback (mbNF) had beneficial effects compared to control conditions. Mindfulness meditation is known to have benefits for psychological distress, cognitive functioning, and physiological health [3,60–62], and we assessed whether mbNF had significantly more positive outcomes in these domains. We also assessed effects of mindfulness on process variables like state/trait mindfulness, as well as whether participants can modulate brain signals through mbNF.

Protocol and registration

We preregistered this meta-analysis before examination of the data, on OSF [63]. All deviations can be found in Supplement.

Eligibility criteria

We included two main categories of randomized studies. First, we included between-person randomized controlled trials with either passive or active control conditions. Second, we included within-person randomized single-session designs with a non-neurofeedback control condition, which we call mindfulness 'inductions'. Inductions were included to assess the state effects of neurofeedback.

Studies were included if they used consumer-grade neurofeedback devices (e.g., Muse, Emotiv, Lowdown focus). Lab-based neurofeedback studies were reported in a previous manuscript [13]. Studies must have delivered acceptable mindfulness meditation training consisting of focused attention meditation or open monitoring attention (as both practices involve purposeful redirection of attention to the present moment) [64,65]. Papers, dissertations, chapters, and preprints were included.

Outcomes of Interest

We extracted a comprehensive set of outcomes from the methods section of the included papers, and addressed any missing statistics using reporting bias sensitivity analyses (see **Analysis**). We collected psychological distress outcomes, including anxiety, depression, fatigue, as well as positive affect, where decreases reflect decreased distress e.g. decreased anxiety or increased positive affect. We collected cognitive outcomes – exclusively behavioral tasks like vigilance and complex reaction time where improvements reflect better cognitive functioning e.g. increased accuracy or reduced reaction times. We also collected physiological health outcomes including heart-rate variability, where increases reflected better health after confirming that this matched hypotheses of the studies. Process variables also were extracted. Specifically, state and trait mindfulness were collected, consisting of established questionnaires like the Five-Facet Mindfulness Questionnaire [66], as well as any study-specific questionnaires that assessed mindfulness as present-moment attention or acceptance. Finally, brain target measures as reported by the consumer-grade devices to assess whether participants were able to successfully modulate brain targets were extracted.

Systematic Search

A search of PubMed, Web of Science, PsycInfo, and Scopus, was completed on November 11, 2023. Databases were identified based on previous mindfulness systematic reviews and meta-analyses [67,68]. Search terms were “(mindfulness OR meditation) AND (neurofeedback OR neural feedback OR neuro feedback)”. We additionally searched reference sections of included papers.

Study Selection

All studies were first screened for duplicate publications. Next, all abstracts were screened, including studies based on two main criteria: an empirical study (examples of excluded articles were review papers and protocol papers) and content relevance (based on above stated eligibility criteria). Then remaining studies were screened by reviewing the methods section and full paper to further evaluate the presence of inclusion criteria. Determination of inclusion was established in cases of disagreement by consulting with the first author.

Data collection process

A coding manual was developed by the first author to guide the extraction of study descriptive and effect size data. Extraction of these data were conducted by the first author and confirmed independently by ZB, KDG, and NK. Coding disagreements were discussed by the team.

Data items

We extracted the following descriptive variables: duration of neurofeedback in minutes/training weeks, subjects in mbNF and control conditions (after dropout), control condition type, mbNF device used, delivery modality, type of meditation, age, percentage female (using reported gender), whether or not it was a clinical population, and outcome types. We reported these variables separately for within-subject and between-subject designs (**Tables 1, 2**). In the case of multiple intervention groups or control groups, we combined the groups (and pooled their means and standard deviations). A minority of studies reported overall N but did not report final group numbers, in which case we imputed equivalent group sizes, and coded it as additional risk of bias.

For between-subject designs, we primarily calculated Becker’s del for effect sizes, which is the Cohen’s *d* for the mbNF group minus the Cohen’s *d* for the control group [69]. In cases of incomplete reporting, we converted t-tests or partial eta-squared to Cohen's *d*. All effect sizes were converted using Hedges' *g* correction. Variances of effect sizes were calculated using standard methods [69,70]. For within-subject designs, we conducted simple mean differences between the mbNF and control condition, converting using Hedge's *g* correction.

Risk of bias

Bias assessment was conducted on the RCTs included in the review, following PRISMA guidelines [71]. RCTs were rated using the ROB-2 [72]. Two authors (IT and either ZB, KDG, or NY) independently rated the risk across several domains, including 1) randomization, 2) blinding, 3) objective measurement of outcomes, 4) attrition, and 5) reporting bias. This estimated risk in each domain was then compared between raters and studies were classified as having low, some concerns, or high risk of bias. Methods for quantifying study quality are detailed in **Supplement.**

Analysis

Synthesis

Meta-analysis was conducted using the *metafor, MAd,* and *dmetar* packages [73–75]. All measures that met the inclusion criteria were included (i.e., psychological distress, cognition, mindfulness, physiological health, or brain target). When studies reported multiple effects (e.g., multiple objective measures of cognition), these were first aggregated within each study using the *MAd* package. Aggregating within study ensured that studies with multiple measures (or multiple measures from the same task) did not carry undue weight in the omnibus effect size estimates. For each study, an overall effect size in Hedges' *g* units along with a 95% confidence interval (CI) was computed.

Omnibus estimates were calculated for psychological distress, cognitive functioning, physiological health, state/trait mindfulness, and brain targets; this was done separately for within-subject and between-subject designs. Omnibus estimates were only conducted if there were at least four studies reporting an effect [76]. Additionally, if the effect did not have a clear hypothesized relationship to the construct, it was not included (e.g. is higher or lower resting heart rate beneficial?). Heterogeneity was reported in terms of I^2 and *tau.* Analyses used random effect models with study effect sizes weighted by the inverse of their variance, in *metafor*, using restricted maximum likelihood. We removed any individual studies that showed confidence intervals that did not overlap with the omnibus confidence interval [74]; this only minorly affected results for psychological distress (see **Results**). We additionally tested the following moderators: percentage female, age, clinical population (0 or 1), total sample size, neurofeedback duration (for RCTs, days; for within-subject, minutes), and ROB quality of study (**Supplement**). We did not conduct moderations by control type, neurofeedback device type or preregistration status, as there were not sufficient studies in each class.

Reporting bias

During effect size extracting, we observed possible reporting bias. Many studies reported 'non-significant' effects for a measure, and then did not report stats ('class A'). In addition, other studies reported measures in their methods and then did not report stats nor significance ('class B'). We conducted sensitivity analyses to determine if the omnibus effects were sensitive to these classes of missing reporting. Moderate correction consisted of imputing zeros for all effects in class A, and strict correction consisted of imputing zeros for all effects in class A and B. We reported analyses using moderate correction, and noted if the corrections resulted in any differences.

Publication bias

The fields of psychology and neuroscience are affected by publication bias (the likelihood that positive results have a higher probability of getting published [77] and so-called 'data contingent' analyses [78]). For example, one study estimated that psychology's published findings contain greater than 90% significant results [79]. Such a high percentage of positive findings is statistically highly unlikely especially given widespread low power. It is possible that publication bias affects the field of consumer-grade neurofeedback. We conducted two different approaches - trim-and-fill, which corrects for publication bias in small samples, and three-parameter selection models which explicitly model the proportion of studies below a *p*-threshold. We considered applying p-curve approaches, but they require at least three significant findings which was not the case for multiple models.

Results

Selected studies

A PRISMA flow diagram is shown in **Figure 2.**

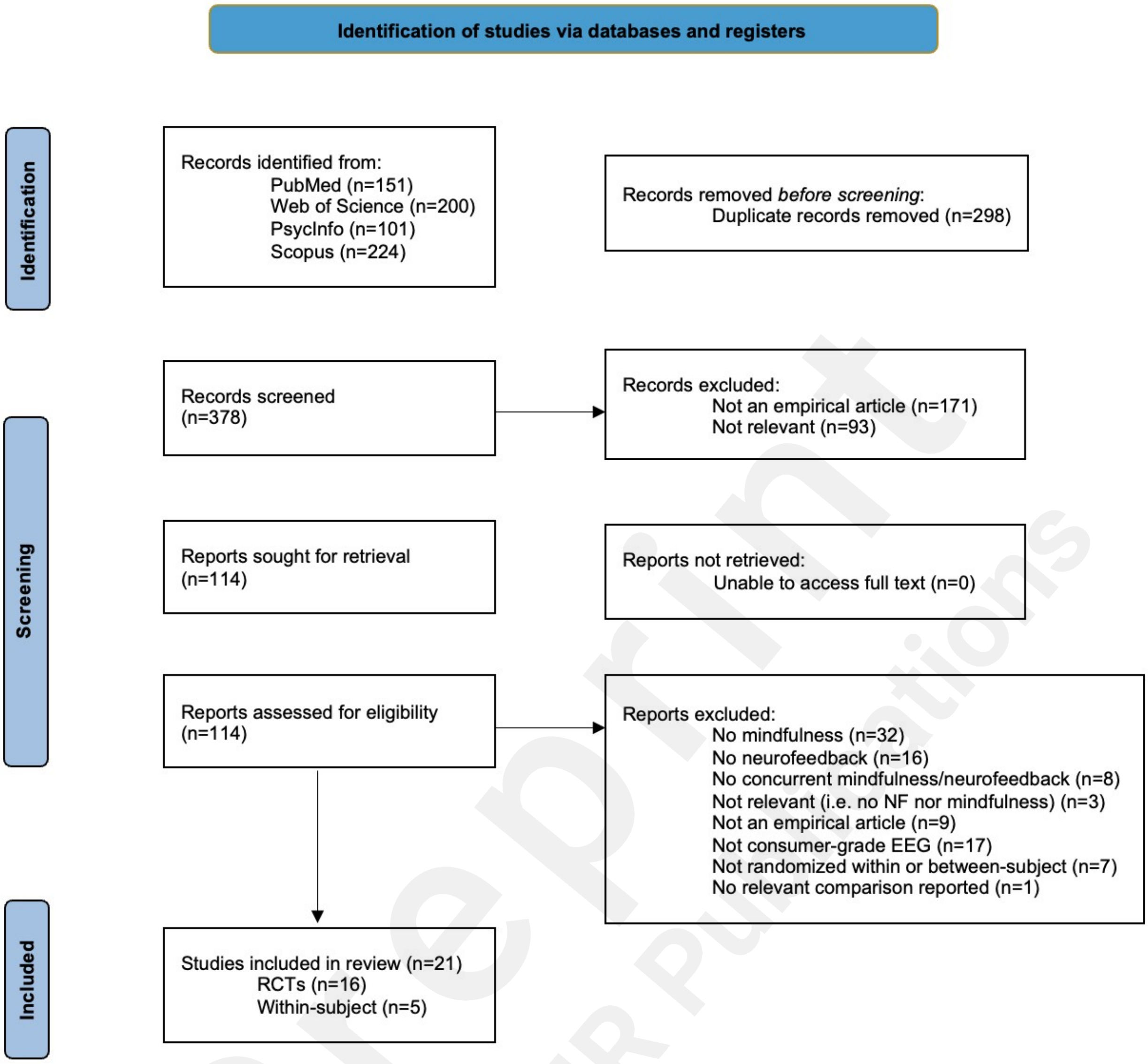
Identification of studies via databases and registers
Identification
Records identified from:
PubMed (n=151)
Web of Science (n=200)
PsycInfo (n=101)
Scopus (n=224)
Records removed before screening:
Duplicate records removed (n=298)
Screening
Records screened
(n=378)
Records excluded:
Not an empirical article (n=171)
Not relevant (n=93)
Reports sought for retrieval
(n=114)
Reports not retrieved:
Unable to access full text (n=0)
Reports assessed for eligibility
(n=114)
Reports excluded:
No mindfulness (n=32)
No neurofeedback (n=16)
No concurrent mindfulness/neurofeedback (n=8)
Not relevant (i.e. no NF nor mindfulness) (n=3)
Not an empirical article (n=9)
Not consumer-grade EEG (n=17)
Not randomized within or between-subject (n=7)
No relevant comparison reported (n=1)
Included
Studies included in review (n=21)
RCTs (n=16)
Within-subject (n=5)

Figure 2: PRISMA flow diagram. Depicts number of identified and evaluated articles for concurrent mindfulness and consumer-grade neurofeedback procedures [71].

Study characteristics

Sixteen RCT studies were identified, encompassing 763 unique participants, as well as five within-subject randomized studies, encompassing 157 participants. Average sample size was 47.68 participants and 31.2 participants, respectively. The average age was 25.69 years (*SD* = 10.01 yrs) and 26.12 years (*SD* = 9.99 yrs), respectively. Three RCTs involved children [58] or adolescents [79,80]. The average proportion female was 66.19% and 47.83%, respectively. Sociodemographic variables were not consistently reported. Four RCTs involved clinical samples (e.g. obsessive compulsive disorder [80]), and two inductions involved individuals with intellectual disability [81,82]. Most studies used the Muse neurofeedback system (11/16 interventions and 4/5 inductions). The predominant meditation type was focused attention on the breath. Control conditions for studies were primarily active, consisting of mindfulness apps for RCTs (11/16), and short mindfulness meditations for within-subject designs. Passive controls (three studies) consisted of waitlist or treatment-as-usual. Of the sixteen RCTs, seven were conducted by the same research group in Italy (e.g. [83,84]). Two of the within-subject randomized studies had extremely small sample sizes (*n* < 10). Four studies were preregistered. Full descriptions may be found in **Table 1 & 2.**

Table 1. Randomized controlled trials.

Study Identifier	Subject Info	Subject Num	Condition	Meditation	Outcome Types
(Acabchuk2021)[85]	20.52 yrs[l], 73.1% F[a], NT[b]	25	Muse	FA[e], 4 weeks	Brain Target, Mindfulness, Psychological Distress
		27	Active (MAU app[g])		
(Balconi2017)[86]	NR[j], NR, NT	20	Lowdown Focus	FA, 4 weeks	Cognitive, Mindfulness, Psychological Distress
		20	Active (MAU app)		
(Balconi2019a)[83]	24.2 yrs, 76 % F, NT	25	Lowdown Focus	FA, 3 weeks	Cognitive, Mindfulness, Physiology
		25	Active (MAU app)		
(Balconi2019b)[87]	NR, NR, NT	18	Lowdown Focus	FA, 4 weeks	Physiology, Psychological Distress
		17	Active (MAU app)		
(Balconi2019c)[88]	23.12 yrs, 69.09% F, NT	28	Muse or LF[i]	FA, 4 weeks	Physiology, Psychological Distress
		27	Active (MAU app)		
(Balconi2019d)[89]	23.58 yrs , NR, NT	19	Lowdown Focus	FA, 4 weeks	Cognitive, Mindfulness, Physiology, Psychological Distress
		19	Active (MAU app)		
(Bhayee2016)[90]	32.65 yrs,	13	Muse	FA, 6	Cognitive,

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	46.16 % F, NT	13	Active (math education)	weeks	Mindfulness, Physiology, Psychological Distress
(Crivelli2019a)[84]	22.94 yrs, NR, NT	17	Muse	FA, 2 weeks	Cognitive, Mindfulness, Psychological Distress
		18	Active (MAU app)		
(Crivelli2019b)[91]	23.47 yrs, NR, NT	18	Muse	FA, 4 weeks	Cognitive
		18	Active (MAU app)		
(Hawley2021)[80]	26 yrs, NR, OCD[c]	25	Muse	unclear, 8 weeks	Mindfulness, Psychological Distress
		24	Passive		
(Min2023)[92]	38.64yrs, 90.22% F, NT[k]	30	Omni	FA/Body scans, 4 weeks	Brain Target, Mindfulness, Physiology, Psychological Distress
		63	Active (MAU app/ Self-care)		
(Polich2020)[57]	45.4yrs, 85%F, TBI[d]	10	Muse	FA, 6 weeks	Brain Target, Cognitive, Mindfulness, Psychological Distress
		10	Active (MAU app)		
(Schuurmans2019)[93]	14.46 yrs, 40% F, PTSD	8	Muse/MindWave	OM[f], 6 weeks	Physiology, Psychological Distress
		3	Active (breathing game)		
(Schuurmans2021)[94]	15.25 yrs, 40.3% F, PTSD	37	Muse	unclear, 6 weeks	Physiology
		40	Passive (TAU[h])		
(Tarrant2022)[95]	41.75 yrs, 91%F, NT	50	BrainLink/VR	Body scan, single-session	Psychological Distress
		50	Active (MAU app)		
(Vekety2022)[58]	9.92yrs, 51% F, NT	15	Muse	FA/Body scans, 4 weeks	Cognitive, Brain Target
		15	Passive		

[a] F: female
[b] NT: neurotypical
[c] OCD: obsessive-compulsive disorder
[d] TBI: traumatic brain injury
[e] FA: focused attention
[f] OM: open-monitoring
[g] MAU app: mindfulness-as-usual with app or audio tracks played at home (self-administered)
[h] TAU: treatment-as-usual
[i] LF: lowdown focus
[j] NR: not reported
[k] Subjects had elevated levels of anxiety
[l] yrs: years

Table 2. Within-subject inductions.

Study Identifier	Subject Info	Subject Num	Control Condition	Meditation	Outcome Types
(Hunkin2021)[96]	22.66 yrs[h], 58.82% F[a], NT[b]	35	FA[d]	Muse, FA,10 min	Brain Target, Mindfulness
(McMahon2020)[81]	20.8 yrs, 20% F, ID[c]	5	FA	Muse, FA, 50 min*	Brain Target, Mindfulness, Psychological Distress
(McMahon2021)[82]	20 yrs, 50% F, ID	4	FA	Muse, FA, 25 min[f]	Brain Target, Mindfulness, Psychological Distress
(SasChopra2015)[97]	41 yrs, 62.5% F, NT	16	unclear	Emotiv binaural / monaural[g], unclear, 10 min	Brain Target, Mindfulness
(Svetlov2019)[98]	NR[e],NR, NT	96	FA	Muse, FA, 7 min	Physiology, Brain Target

[a] F: female
[b] NT: neurotypical
[c] ID: intellectual disability
[d] FA: focused attention
[e] NR: not reported
[f] Multiple runs
[g] Binaural/monaural feedback were aggregated
[h] yrs: years

Risk of bias (RCTs)

The greatest concern in the RCTs was blinding (**Figure 3** and **Figure S1)**. One study had a biofeedback control condition [93], and no studies had sham neurofeedback. There were also concerns with inadequate reporting of randomization and attrition. We address reporting bias in sensitivity analyses.

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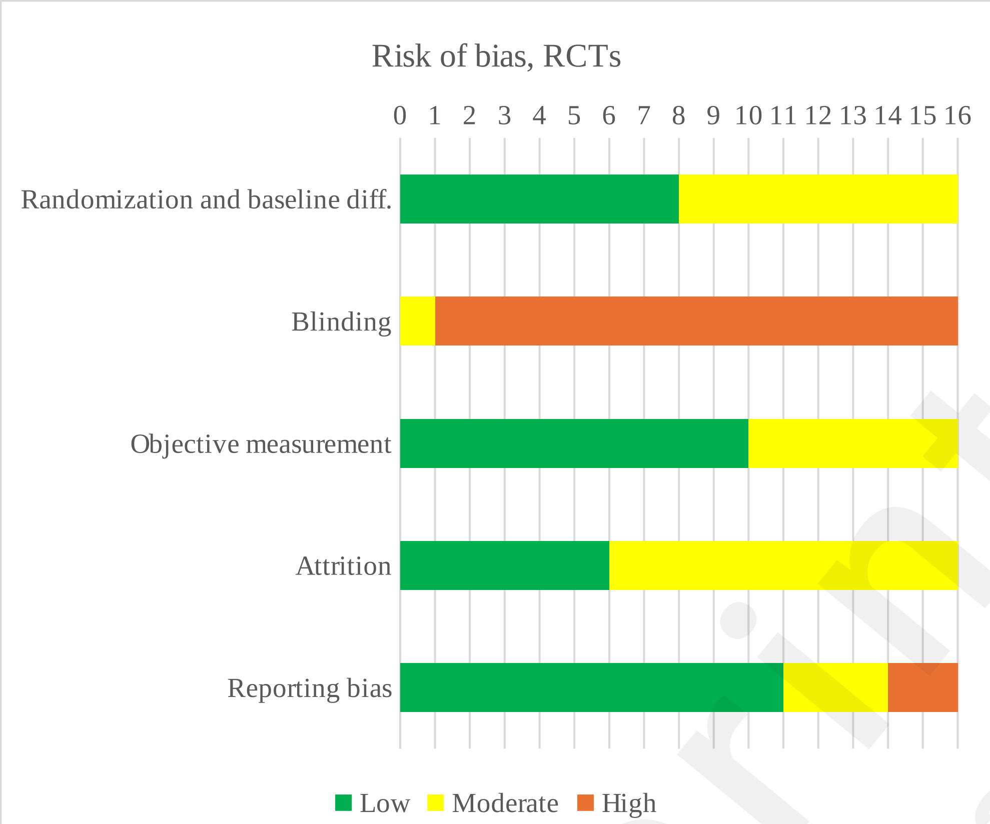

Figure 3: Risk of bias for randomized controlled trials.

Synthesis of RCTs

Effects for psychological distress were significant across a range of reporting bias corrections (*g*: -0.29 to -0.16) (**Table 3 & 4, S2; Figure 4**). Heterogeneity was low after removing one outlier (I^2 < 25%). Effects for cognitive function were sensitive to reporting bias, without correction the omnibus effect was significant (*g* = 0.31), but when correcting the effects were not significant (moderate *g* = 0.20, strict *g* = 0.07) (**Table 3 & 4, S2).** Heterogeneity was high for cognitive outcomes (50% > I^2 > 25%). Effects for physiological health, and mindfulness, were not significant, and heterogeneity for physiology was high (I^2 >75%). **Figure 5** presents the omnibus effect sizes for comparison. Results were similar when examining only studies with active controls (**Table S3)**. There were not sufficient studies to detect whether training increased the brain target scores. However, we conducted an exploratory analysis with the three RCTs that reported brain target scores, and found no significant effect (*g* = -0.01, 95% CI [-0.39, 0.36], I^2 =23,18, *tau* = 0.16). We conducted an exploratory analysis on single-arm effects (no controls), which showed increased brain target scores, see **Supplement Text**.

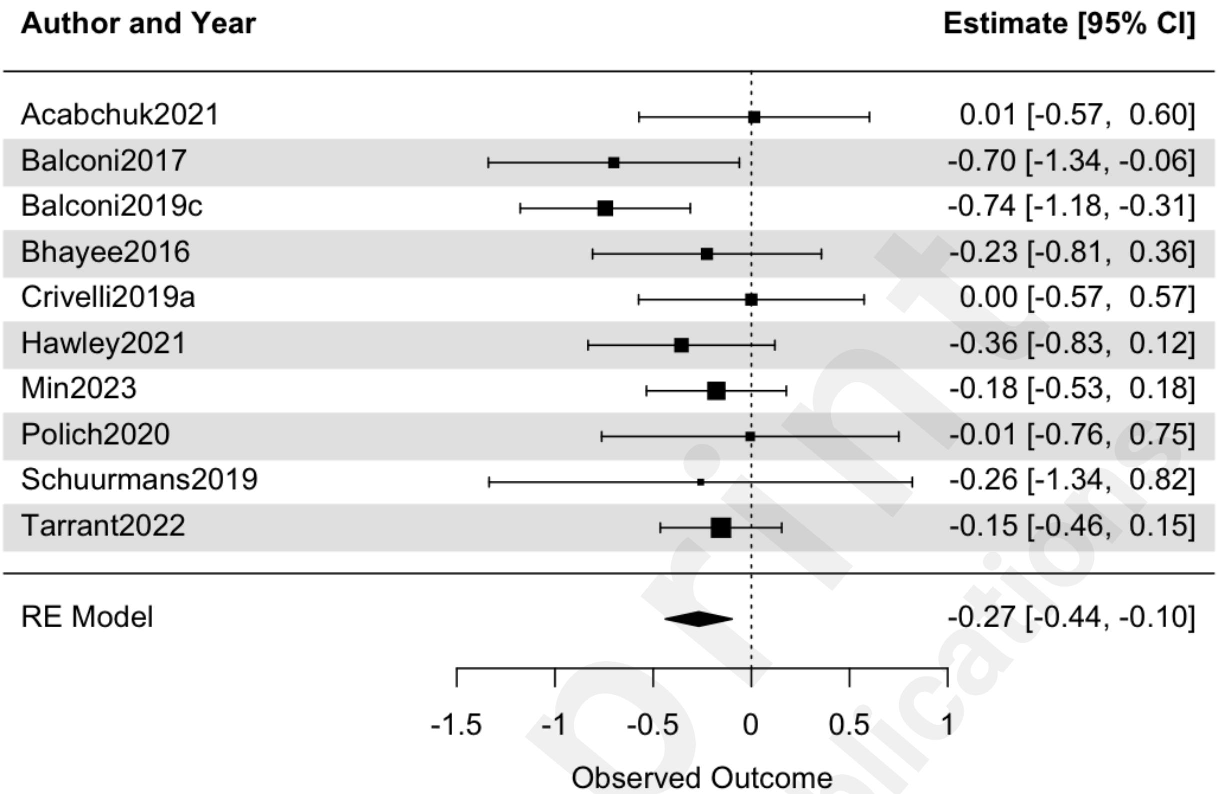

Figure 4: Effects of mbNF on psychological distress, randomized controlled trials. Moderate reporting bias correction was conducted (non-significant effects imputed), and one outlier removed.

Table 3. Omnibus effect sizes for RCTs, without adjustment.

Domain	N[a]	K[b] (Outliers)	ES[c] (g) [95% CI]	I^2 (tau)[d]
Psychological Distress	466	9 (1)	-0.29 [-0.47, -0.11]	16.19 (0.11)
Cognitive	230	7	0.31 [0.01, 0.61]	42.32 (0.26)
Physiological Health	113	4	0.25 [-0.44, 0.94]	88.9 (0.66)
Process Variable				
Mindfulness	310	6	0.08 [-0.15, 0.31]	0 (0)
Exploratory				
Brain Target	165	3	-0.01 [-0.39, 0.36]	23.18 (0.16)

[a] N: total number of participants
[b] K: number of studies after outlier removal (outliers in parentheses)
[c] ES: effect size, in Hedge's *g*
[d] I^2 (tau): heterogeneity measures.

Table 4. Omnibus effect sizes for RCTs, with moderate adjustment for reporting bias.

Domain	N_adj[a]	K_adj[b] (Outliers)	ES_adj[c] (g) [95% CI]	I^2 (tau)[d]
Psychological Distress	501	10 (1)	-0.27 [-0.44, -0.10]	13.21 (0.10)
Cognitive	230	7	0.20 [-0.03, 0.43]	19.31 (0.14)
Physiological Health	321	6	0.13 [-0.31, 0.56]	77.6 (0.46)
Process Variable				
Mindfulness	345	7	0.02 [-0.17, 0.21]	0 (0)
Exploratory				
Brain Target	NA[e]	NA	NA	NA

[a] N: total number of participants
[b] K: number of studies after outlier removal (outliers in parentheses)
[c] ES: effect size, in Hedge's *g*
[d] I^2 (tau): heterogeneity measures.
[e] NAs are for non-existent reporting bias.

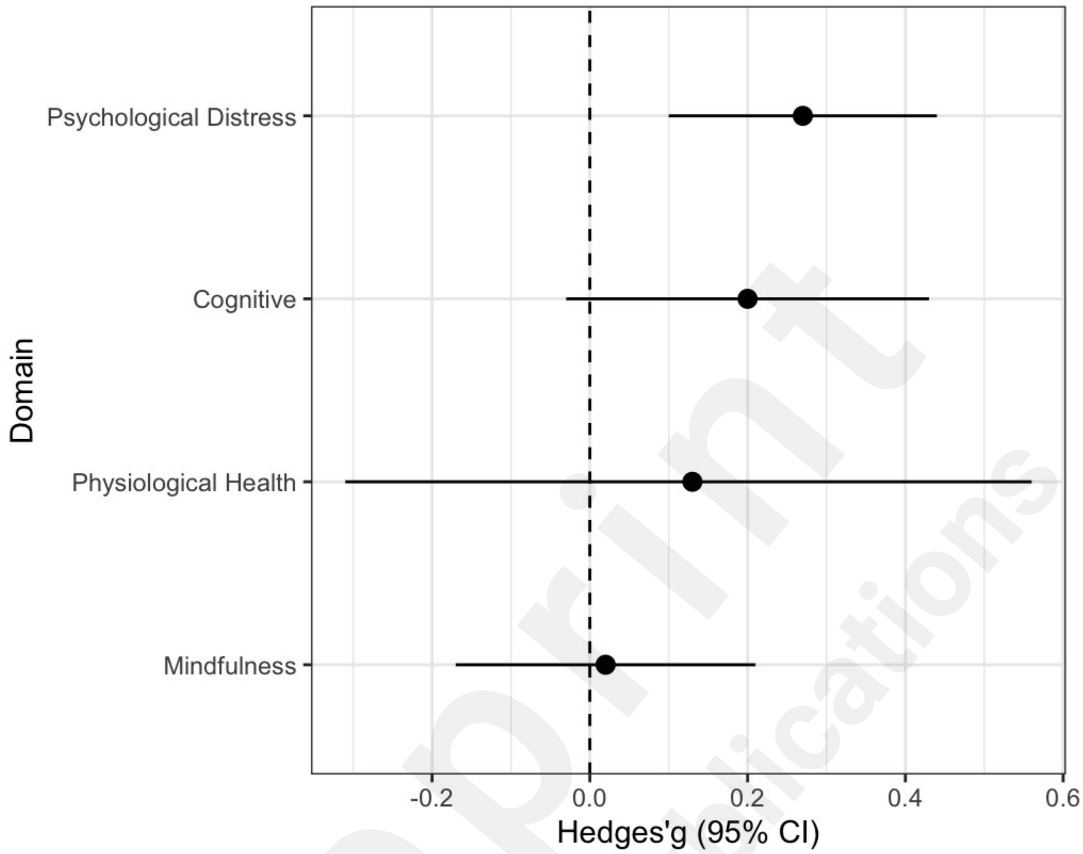

Figure 5. RCT Omnibus effect sizes with moderate reporting bias correction.

Synthesis of within-subject designs

We had sufficient studies to analyze whether mindfulness was higher during neurofeedback than during control conditions (k = 4, g = 0.14) and whether the brain target was modulated more during neurofeedback than during control (k = 5, g = 0.12) (**Table 4, Figure 6**). Neither effect was significant. There was no indication of reporting bias.

Table 5. Omnibus effect sizes for inductions.

Domain	N[a]	K[b] (Outliers)	ES[c] (g) [95% CI]	I^2 (tau)[d]
Brain Target	157	5	0.12 [-0.10, 0.34]	25.36 (0.13)
Mindfulness	60	4	0.14 [-0.07, 0.36]	0 (0)

[a] N: total number of participants
[b] K: number of studies after outlier removal (outliers in parentheses)
[c] ES: effect size, in Hedge's *g*
[d] I^2 (tau): heterogeneity measures.

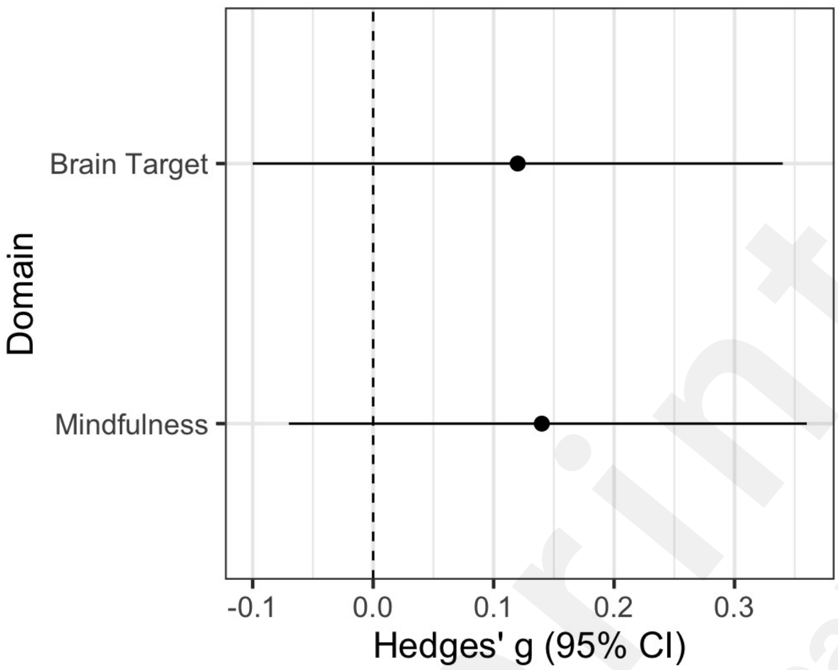

Figure 6. Omnibus effect sizes for inductions.

Moderation

We largely did not find significant moderation effects for clinical population, age, percentage female, quality, or neurofeedback duration (**Tables S4 & S5**). Physiological effects correlated negatively with overall sample size (B = -0.02, p = .02) such that stronger effects were observed in smaller samples.

Publication bias

For RCT studies, there was no indication of publication bias either through trim-and-fill or through selection models (**Table S3**). Selection model regressions minimally changed omnibus estimates, except for physiological health, in which the effect size reversed from g = 0.13 to g = -0.13, but neither was significant.

For within-subject studies, trim-and-fill detected low sample size studies with high effect sizes (**Table S3**), and accordingly filled in studies on the left of the distribution, resulting in decreased *gs* that remained non-significant (Brain Target g = 0.01, Mindfulness g = 0.06). This assessment should be qualified as one of the studies was a thesis, and not a publication. Selection models failed due to lack of significant p-values.

Adverse Effects

Two of 21 studies monitored for adverse events, and neither reported any adverse effects.

Discussion

There is increasing interest in technology-supported mindfulness, for promoting scalability, facilitating beginner practice, and increasing motivation and adherence. Indications from the mobile phone app domain suggest that these innovations can be effective, although perhaps not as effective as in-person teaching [33,35]. Here we examined consumer-grade neurofeedback for supporting mindfulness practice (mindfulness-based neurofeedback; mbNF). Consumer-grade neurofeedback devices provide metrics of brain function to the user, putatively allowing them to optimize their brain and psychological states. We meta-analytically examined the effectiveness of mbNF in improving mindfulness, psychological and cognitive functioning, and modulating brain targets across 16 RCT training studies and 5 within-subject randomized studies. There is some evidence that mbNF may reduce psychological distress compared to control conditions, although the effect is small. Improvements in cognitive function were not robust to reporting bias correction. There was no evidence for improvements in trait or state mindfulness (a primary measure of the effectiveness of a mindfulness intervention), as well as physiological health. Likewise, there is not conclusive evidence that participants can learn to modulate brain targets (e.g. Muse 'Calm' scores).

Psychological distress was measured by questionnaires measuring mood, anxiety, post-traumatic stress disorder symptoms, etc. When examining only the reported effects from studies, mbNF training had a small-to-moderate effect on psychological distress (g = -0.29). However, many studies did not report all their measures. With reporting bias correction, the effect shrank (g = -0.16). There is little theoretical work on why neurofeedback may benefit mindfulness practice, but mechanisms in the neurofeedback literature generally fall into two categories. One, given the correct brain target, neurofeedback may actually lead to facilitated learning and self-regulation [41,42]. For example, in the context of lab-based neurofeedback, it is suggested that learning to regulate the default-mode network may lead to enduring changes in self-awareness, alleviating deleterious cycles of self-criticism [99]. Outside of the laboratory, it is less clear what exactly consumer-grade devices target in terms of brain mechanisms, and thus why they should alter psychological states [54]. We did not find evidence that consumer-grade neurofeedback improves mindfulness nor lead to changes in brain targets more than control conditions. This lack of mechanistic evidence makes it difficult to conclude that mbNF is alleviating psychological distress through brain modulation.

A second, more plausible mechanism is *neurosuggestion* [100]. Western societies place a strong emphasis on biological determinants of the mind, and there is widespread trust and enthusiasm for technology and neuroscience. Meditating with consumer-grade neurofeedback may lead to enhanced motivation, feelings of self-efficacy, and expectations of benefits [101,102]. It is possible that this led to larger decreases in psychological distress compared to control conditions (which were typically potentially less motivating app trainings). The best way to rule out these explanations is through sham-controlled neurofeedback, where participants in the control condition also wear consumer-grade devices but receive 'sham' neurofeedback [103]. Even in lab-based studies, this evidentiary standard is rare (an exception is a sham-controlled RCT that found mbNF participants increased theta oscillations)[13,104]. None of the included consumer-grade studies in the present review used sham controls. This is a fundamental step to establishing the efficacy of mbNF.

It is important to note that the field of mbNF is nascent, less than ten years old. Our current meta-analysis did not find any evidence of inferiority compared to mindfulness-only conditions. In addition, mbNF was conducted with various patient groups with fidelity (although adverse effects were largely not assessed). In this sense, our review supports the use of mbNF as another tool in the toolkit of technologically-supported mindfulness. However, mechanistic and efficacy assertions should be heavily tempered, and more research is necessary. It should be tested whether mbNF is comparable to first-generation mindfulness interventions like MBSR[14].

Limitations

Effects may have been obscured because of limited power. In general, sample sizes were small (N < 50). In addition, adherence to the training was inconsistently reported. Per-protocol or dosage analyses could show that some participants experience greater benefits. Relatedly, none of the studies reviewed here extended longer than eight weeks. An open-label study of Muse over 90 days found positive outcomes, good adherence and 71% of participants wanted to continue using Muse after the period was over [105]. Long-term effects of neurofeedback are unclear. Finally, some of our methods, while standard for meta-analysis, may have obscured effects. To correct for reporting bias, we first contacted authors, and then imputed zeros for mean effects in case of no response. There may have been non-significant but trending effects for these measures. We did not find evidence of publication bias, which is present in other biofeedback fields[106].

Future directions

Limited efficacy of mbNF may arise from technological limitations. First, consumer-grade neurofeedback relies on dry electrodes on the scalp, which limits signal quality [107]. Signal may be contaminated by muscle movement in many cases, and limited by narrow temporal windows of measurement [54]. Second, even given high fidelity measures of underlying brain signals (e.g. alpha or beta power), these signals are not straightforward to map to psychological processes like attention [108,109]. Third, there may be serious variability between individuals in these mappings. For example, mind-wandering episodes are predictable within-individuals using whole-brain fMRI measures but the measures show extensive spatial variability between individuals [110]. To address these concerns, one approach is to conduct more lab-based studies on mechanisms, including multimodal fusion studies where different imaging modalities are combined to assess common signatures of psychological processes. Another approach may be more extensive data collection and algorithm development by device companies. In the face of great individual variability, it may be useful to more extensively calibrate devices (‘personalize’) to users before meditation sessions. Currently, users do one-minute free association tasks before meditation, and one assumes that brain signals during calibration are baselines for the meditation session (e.g., reduce alpha compared to baseline [55]). There is no participant input in the calibration. If participants could indicate when they feel more calm or more focused, the devices could learn from their responses. In summary, more neuroscientific research on devices may lead to more effective mbNF, and this does not necessarily entail expensive fMRI studies.

A broader challenge is that mbNF assumes that some degree of monitoring during meditation may facilitate mindfulness. Feedback may actually be distracting in some cases [13]. Breath meditation typically emphasizes paying fine attention to the details of the breath and the sensations in various body parts. When attention wanders, one may lose track of the motion of the breath. Or one’s attention may become less precise and the details of the breath become harder to track. Meditation involves learning to notice these changes and develop insight into your mind. If the practitioner is instead paying attention to an auditory feedback signal, they may lose this awareness, and more importantly, lose out on the learning process of watching awareness fluctuate. This is speculative, and more research on qualitative experiences of neurofeedback (especially with advanced practitioners) is necessary. It may be the case that open monitoring or 'mental noting'[99] which allows for attention to shift between the stimulus, sensations, thoughts, etc. is more amenable (we were unable to examine moderation by meditation practice type in the current review). Further, 'intermittent' neurofeedback may facilitate awareness and insight without distractions [111].

Ultimately neurofeedback, biofeedback, and neuromodulation as technological supports for meditation may be constrained by the limits of non-invasive measurement, and the difficulty of linking such limited measures to mental states in a generalizable way. Of course, other forms of technology-supported mindfulness may be more powerful. For example, VR and videogames provide immersion, enhance mindful states and may reach people who are not interested in formal practice [9,10,112,113]. However, we should not overlook the significant role that teachers and therapists play in the success of standard mindfulness interventions [38,114–117], and more broadly, the key role of the therapeutic alliance on mental health outcomes even within technology-based treatments [118–121].

Conclusions

The present meta-analysis of mindfulness-based neurofeedback finds evidence for modest decreases in psychological distress compared to controls. However, there is little conclusive evidence for mechanistic engagement, nor improvements in cognition, mindfulness, nor physiological health. Future research is necessary to improve efficacy and rule out neurosuggestion confounds.

Data and Code Availability: Full code and formulas can be found in provided code (https://osf.io/vc9tr/?view_only=d1cfde7233464ed6bc09c1ca6c8ae892)

CRediT Statement: INT contributed conceptualization, methodology, investigation, writing-original draft, writing- review & editing, visualization and project administration. ZB contributed investigation, writing- review & editing. KDG contributed investigation, writing- review & editing. NK contributed investigation, visualization. EW contributed investigation. SBG contributed methodology, investigation, writing- review & editing. SWG contributed supervision, funding acquisition. RPA contributed supervision, review & editing, funding acquisition.

Acknowledgements: We thank Beom Jun Min, Artem Svetlov, and Hugh Hunkin for contributing summary statistics for meta-analysis.

Funding: SWG and RPA were partially supported through funding from NIMH (R61 MH132072). The content is solely the responsibility of the authors and does not necessarily represent the official views of the NIH.

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Declaration of Competing Interests: No competing interests are present.

Ethics Statement: This article does not contain original research.

Disclosures: Dr. Auerbach is an unpaid scientific advisor for Ksana Health, and he is a paid scientific advisor for Get Sonar, Inc.

Abbreviations:
CI: confidence interval
EEG: electroencephalography
fMRI: functional magnetic resonance imaging
MBI: mindfulness-based interventions
mbNF: mindfulness-based neurofeedback
MBSR: mindfulness-based stress reduction
PRISMA: Preferred Reporting for Systematic reviews and Meta-Analyses
RCT: randomized controlled trial
SD: standard deviation
VR: virtual reality

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Supplementary Files

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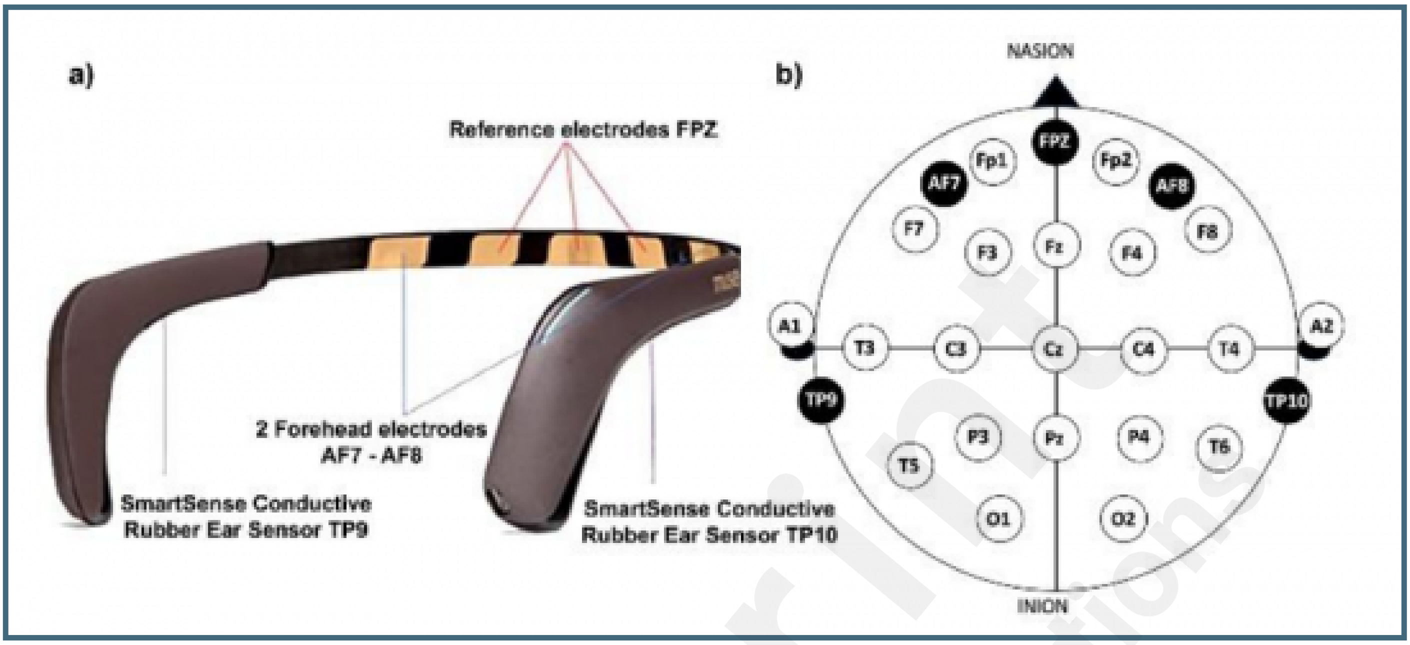
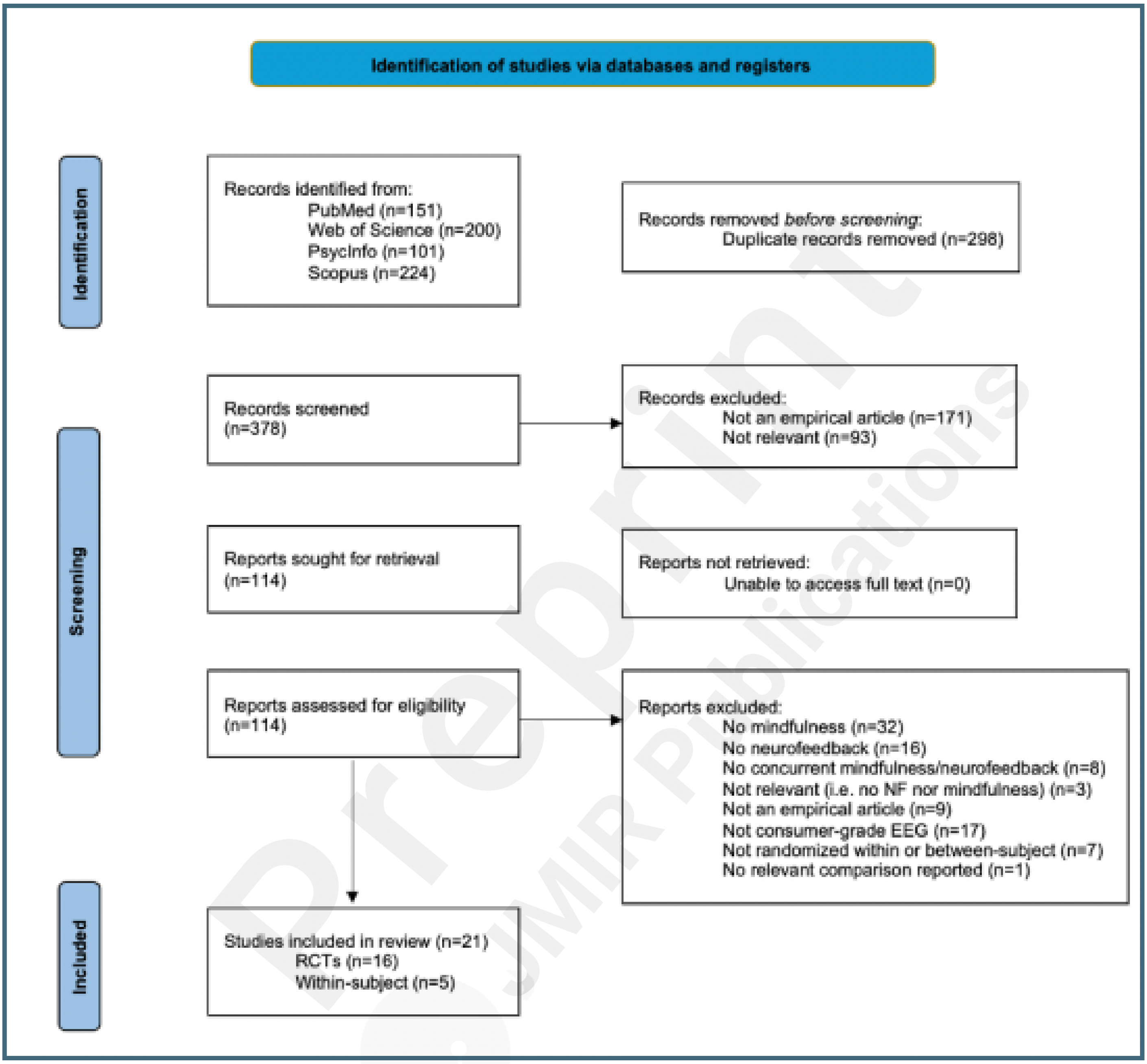
Figures

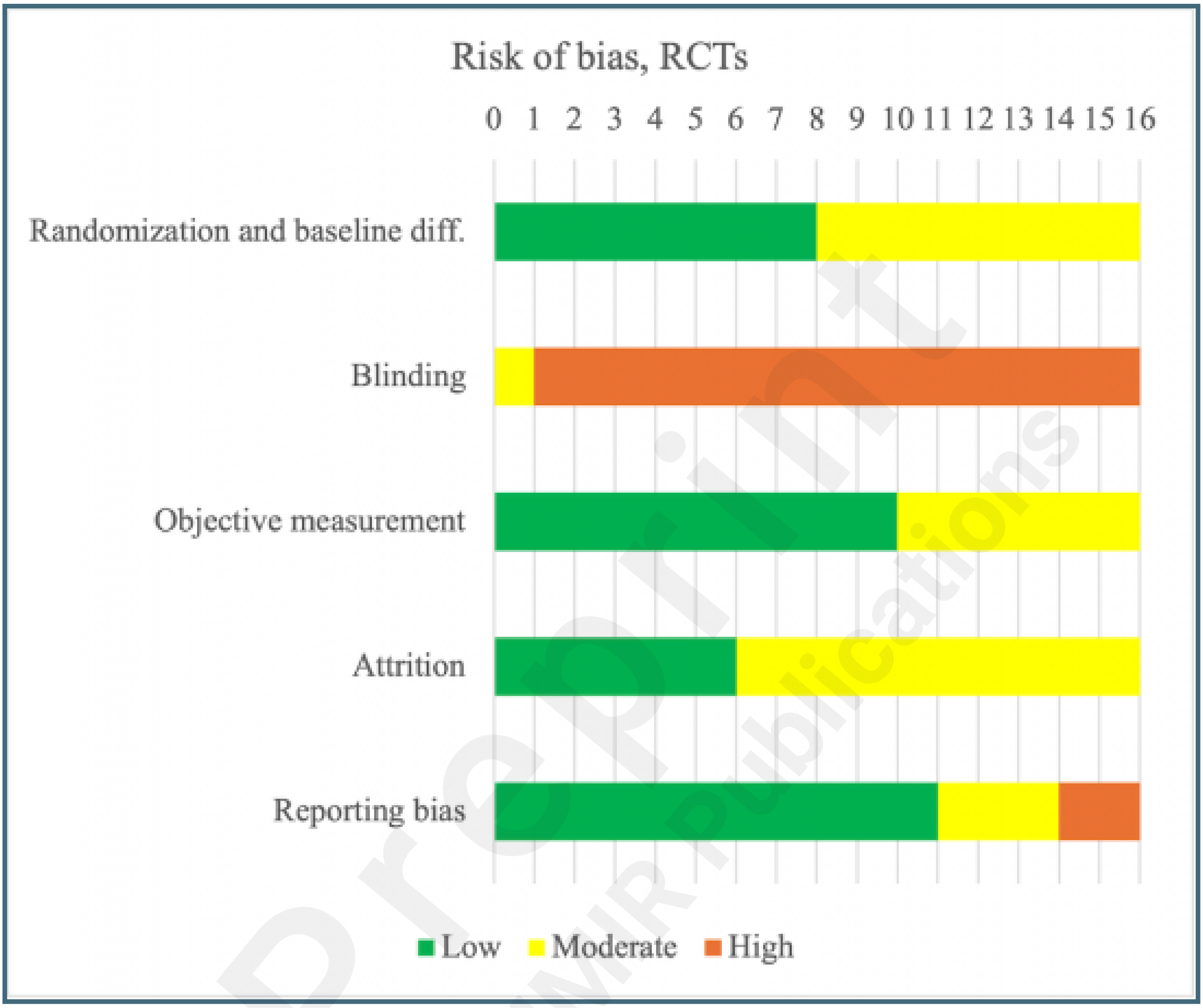
Consumer-grade device, Muse. a) Muse 2 headband sensors overview. b) Top-down view of the EEG electrode positions on the subject's head. Adapted from Mansi et al., 2021.



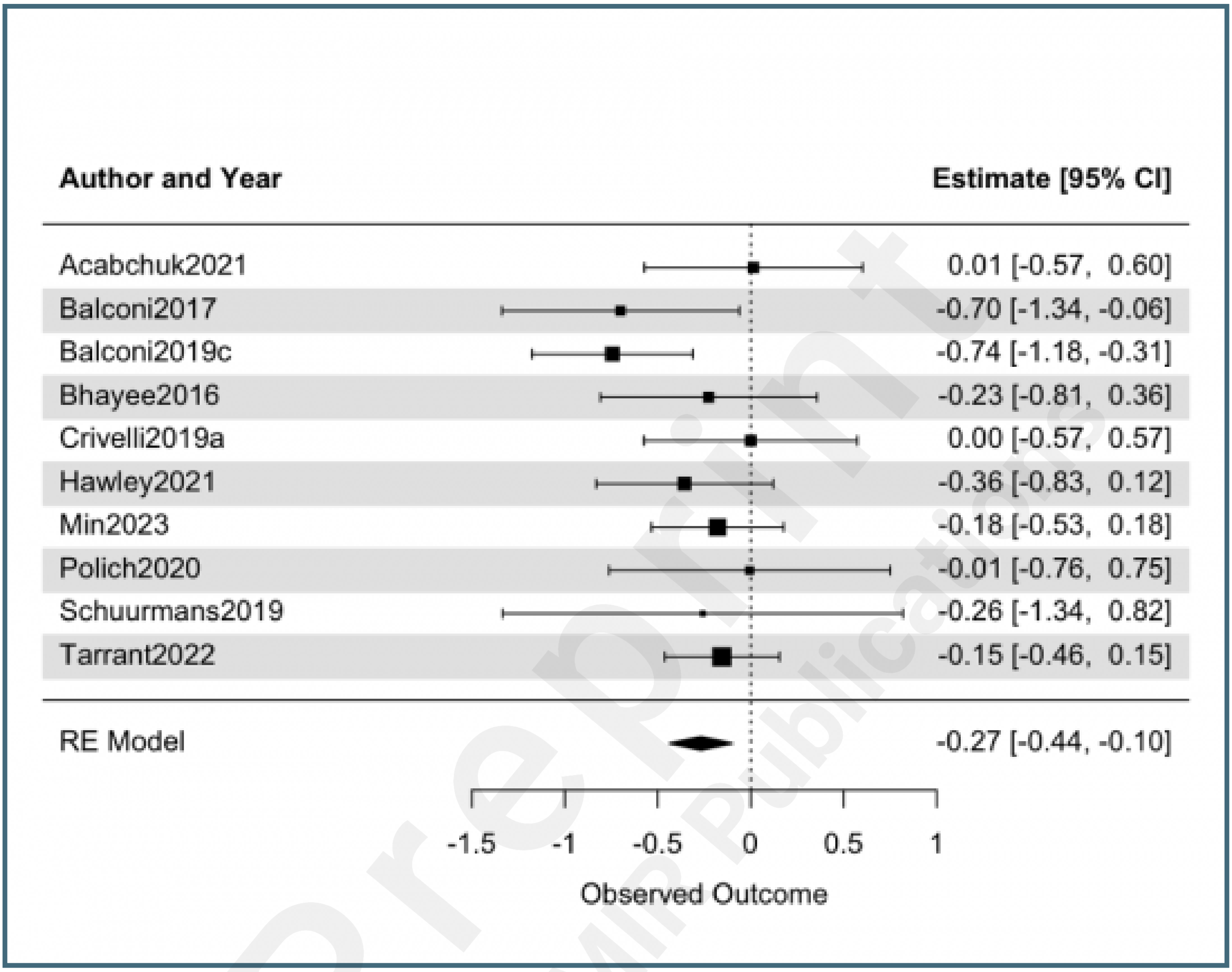
PRISMA flow diagram. Depicts number of identified and evaluated articles for concurrent mindfulness and consumer-grade neurofeedback procedures (Page et al., 2021).



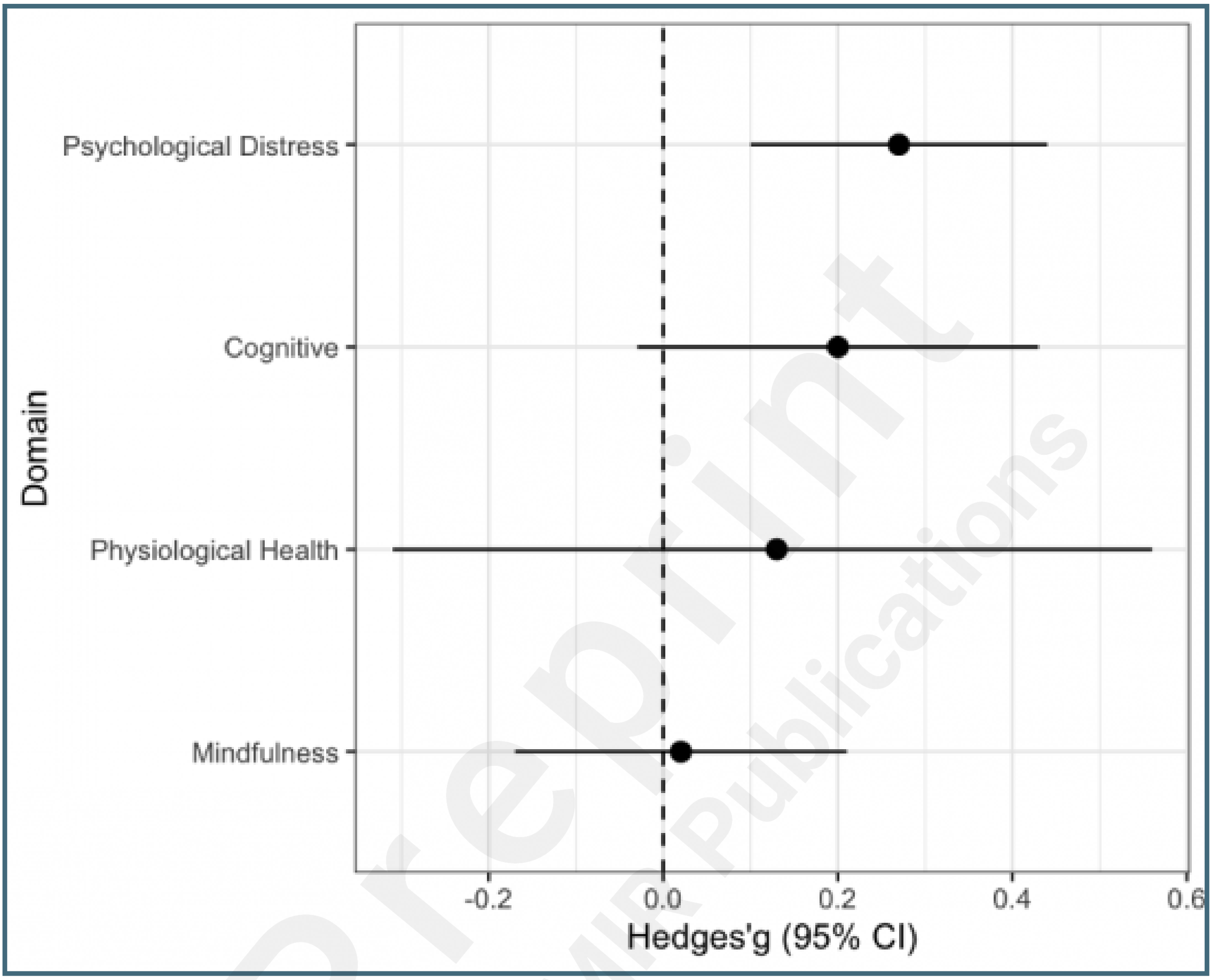
Risk of bias for randomized controlled trials.



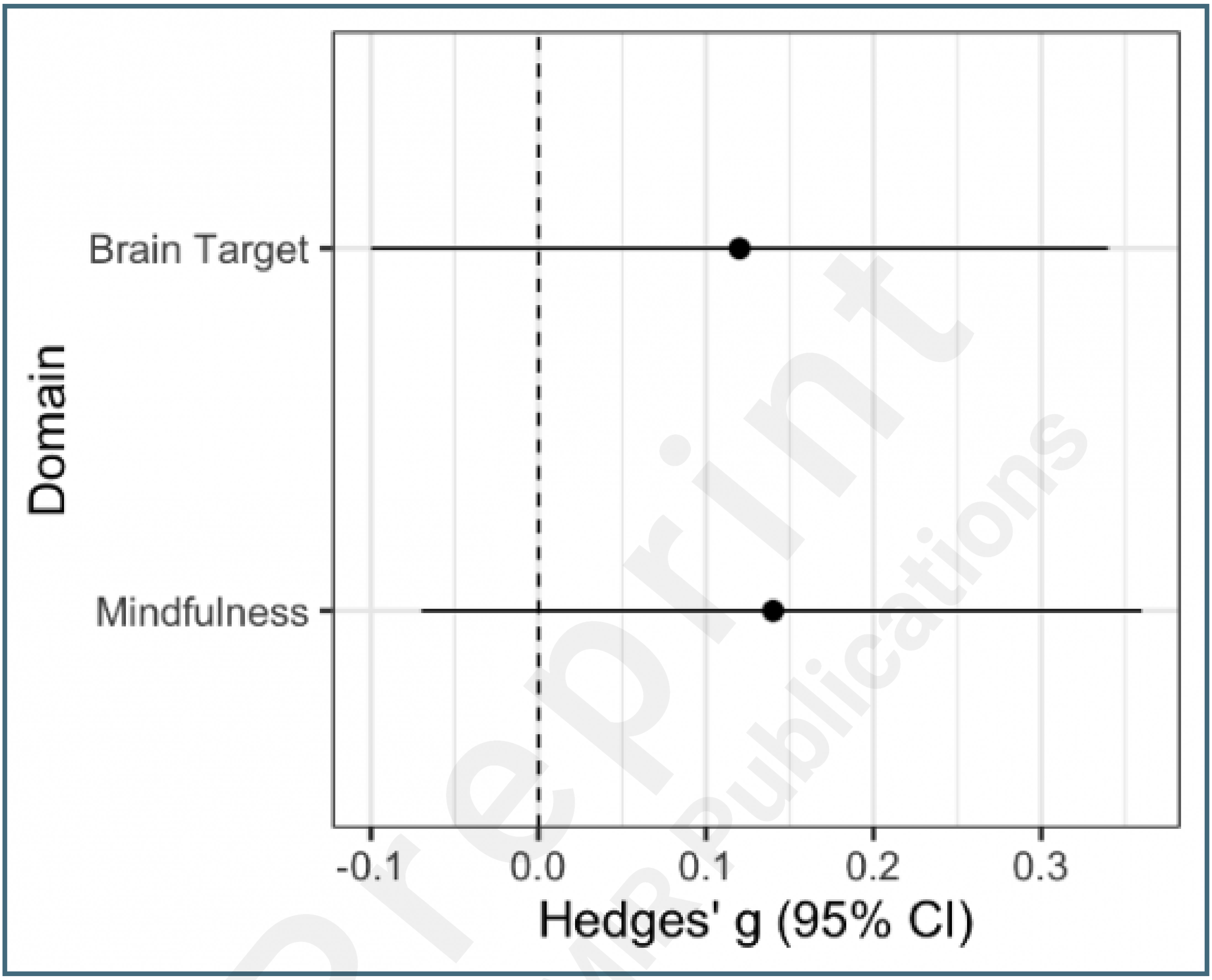
Effects of mbNF on psychological distress, randomized controlled trials. Moderate reporting bias correction was conducted (non-significant effects imputed), and one outlier removed.



RCT Omnibus effect sizes with moderate reporting bias correction.



Omnibus effect sizes for inductions.



Multimedia Appendixes

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