The Effect of Mindfulness-Based Programmes on Work Performance

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This thesis is submitted for the degree of Doctor of Philosophy

September 2023

Preface

This thesis is the result of my own work and includes nothing which is the outcome of work done in collaboration except as declared in the preface and specified in the text. It is not substantially the same as any work that has already been submitted before for any degree or other qualification except as declared in the preface and specified in the text. It does not exceed the prescribed word limit for the Clinical Medicine and Clinical Veterinary Medicine Degree Committee.

The research reported in Chapter 2, Chapter 3, and Chapter 4 has been undertaken collaboratively like most scientific enquiry today. To reflect the teamwork, I will, by default, use the first person plural (we, us, our, ourselves) to refer to the joint efforts throughout the thesis. However, where warranted by the circumstances, I will use first person singular (I, me, my, mine, myself).

Chapter 2 has been published in a pre-print repository as

> Vainre, M., Dalgleish, T., Bendriss-Otiko, T., Mariscotti, F., Martinez-Sosa, C., Sideri, A., Hitchcock, C., & Galante, J. (2022). Mindfulness training for work performance: A systematic review and meta-analysis of randomised controlled trials. PsyArXiv. https://doi.org/10.31234/osf.io/2vkru

Chapter 3 has been published as

Vainre, M., Galante, J., Watson, P., Dalgleish, T., & Hitchcock, C. (2022). Protocol for the Work Engagement and Well-being Study (SWELL): A randomised controlled feasibility trial evaluating the effects of mindfulness versus light physical exercise at work. BMJ Open, 12 (4), e050951. https://doi.org/10.1136/bmjopen-2021-050951

Chapter 4 has been submitted for publication and published in a pre-print repository as

Vainre, M., Dalgleish, T., Watson, P., Haag, C., Dercon, Q., Galante, J., & Hitchcock, C. (2023). The Work Engagement and Well-being Study (SWELL): A randomised controlled feasibility trial evaluating the effects of mindfulness versus light physical exercise at work. PsyArXiv. https://doi.org/10.31234/osf.io/wamkn

The thesis has been structured around these pre-printed and published studies with the pre-prints/publications forming the substantive content of each empirical chapter. These chapters are contextualised with a foreword and an afterword to the preprint/publication to place the preprint/publication within the overall themes of the thesis. The headings in each chapter vary dependent on the target journal's requirements. The references are located at the end of the thesis in the interest of reducing repetition and saving space. The tables and figures are numbered continuously throughout the thesis except in the appendices where they restart for each chapter.

Abstract

Mindfulness-based programmes (MBPs), suggested to improve work and academic performance, are increasingly used in occupational and educational settings. This thesis advances the field by synthesising the evidence, optimising the operationalisation of work performance, providing preliminary data on MBPs' effectiveness and mechanisms for work performance, and testing acceptability and feasibility for future trials.

First, I led a systematic review and meta-analysis which aimed to map how work performance has been operationalised in randomised controlled trials (RCTs) of MBPs and to assess the impact of MBPs on adults' academic and work performance. The pre-registered primary outcome was task performance, a key aspect of work performance, up to 4 weeks after the intervention (PROSPERO: CRD42020191756). Secondary outcomes were the remaining aspects: contextual performance, adaptive performance, and counterproductive work behaviours. Pairwise random-effects meta-analyses were used to calculate Hedges' *g*. A total of 47 RCTs with 5041 participants were included. Adaptive performance outcomes were collected most frequently. There was no support for MBPs significantly improving task performance (7 RCTs, 454 participants, Hedges' g = 0.52, 95% CI -0.03 to 1.07, p = 0.06) compared with passive control groups. However, MBPs statistically significantly improved adaptive performance and contextual performance. There were an insufficient number of RCTs to allow meta-analysis comparing MBP to active control interventions. All bar one RCT demonstrated high risk of bias. Confidence in the review results was 'low' to 'very low', according

to the Grading of Recommendations Assessment, Development and Evaluation (GRADE) criteria.

Second, I led a feasibility RCT which evaluated whether improved cognitive control and/or enhanced mental health may be mechanisms underpinning the effect of MBPs on work performance. Two hundred and forty-one employees were recruited from eight employers. The participants were randomised on 1:1 basis to the offer of a four-week, self-guided, digitally delivered intervention of either the Be Mindful MBP or a light physical exercise programme (control intervention). We assessed the acceptability of interventions and trial procedures, and estimated effect sizes to inform sample size calculations for a later-stage trial. The primary effectiveness outcome was self-reported work performance at post-intervention measured using the Work Role Functioning Questionnaire. Secondary outcomes included depression, anxiety, stress, and cognitive processes hypothesised to be targeted by mindfulness, including decentering and executive functions. All outcomes were assessed at pre-intervention, post-intervention, and 12-week follow-up. The trial protocol was pre-registered (NCT04631302) and published. We concluded that a full-scale trial would be feasible and acceptable, based on recruitment and retention rates. Yet, a full-scale trial may not be warranted: the MBP, compared to light physical exercise, offered negligible benefits for work performance at post-intervention (Cohen's d = 0.06) and 12 weeks later (Cohen's d = 0.02). For the potential mechanisms, we observed similarly small effect sizes for the differences between the MBP and the alternative intervention on mental health and cognitive control outcomes. Both interventions improved mental health outcomes compared to baseline.

In conclusion, while MBPs may have some potential in enhancing some aspects of work performance when compared to passive control groups, the results of this thesis suggest the evidence is of low quality. Furthermore, the effectiveness of MBPs compared to alternative interventions, like physical exercise, remains uncertain. We found no evidence to suggest cognitive control could be a mechanism underlying MBPs effects on work performance, when compared to light physical exercise. These findings underscore the importance carefully

operationalising work performance and conducting high-quality trials to establish the impact of MBPs on work performance in occupational settings.

Acknowledgements

It takes a village for someone to receive a doctorate degree. Throughout my academic journey, I have been supported to develop my thinking and research skills next to receiving much needed nourishment for my well-being, especially during the most challenging times.

First and foremost, I extend my deepest gratitude to my dedicated supervisors, Dr Caitlin Hitchcock, Dr Julieta Galante and Professor Tim Dalgleish. You have been instrumental in guiding me through this academic expedition. Your mentorship transcended academia: it has not only shaped my research but has also significantly contributed to my personal and professional growth. I am profoundly thankful for your patience, kindness and wisdom that sustained me during the lowest point of my doctoral studies as I recovered from an accident. Thank you also for paving the way to the highlights: notably visiting you and your new labs, Julieta and Caitlin, in Melbourne.

In addition to my supervisory team, the research presented in this thesis was made possible through the contributions of many colleagues, friends, and participants, as acknowledged in each chapter. I am genuinely humbled by your generosity and dedication. It is important to note that such collaborations do not occur spontaneously; they are facilitated by the organisational culture. Therefore, I want to emphasise my appreciation for the hospitable environment at the Medical Research Council's Cognition and Brain Sciences Unit (MRC CBU). The Dalgleish Lab fosters a friendly and inclusive atmosphere, providing fertile ground

for groundbreaking research. Help is readily available for problem-solving and learning research skills. Moreover, at the CBU, the support extends beyond labs: several people who donated their time and skills for the research projects in this thesis, were from other labs. I will dearly miss the coffee time, where I often sought solutions to research dilemmas and left with fresh ideas or at the very least, a sense of consoling commiseration.

In addition, I received an abundance support from outside the CBU. Märt, aitäh, et tuled minuga kasvõi maailma lõppu kui vaja! Ema, isa ja Mart, aitäh, et talusite järjekordset pikendust mu Eestist äraolekule! Praxikud, teiega koos töötamine aitas taastada mu eneseusku ajal, mil alles õppisin taas tudeng olema! Olen tänulik, et toetasite mu õpinguid kaitstes kiivalt mu töö-/õpiaega. My dear friends in the CBU, the Hitchcock lab and the Centre for Contemplative Studies at the University of Melbourne, from the dancing community and far beyond: your companionship has helped me to stay grounded and has made the whole doctoral journey infinitely more fulfilling. Finally, while the studies have brought me close to many friends and collaborators, it has also been marked by the loss of a dear friend, Kadi, whose resilience, determination, and compassion will continue to shape me.

Thank you all from the bottom of my heart.

My post-graduate studies at the University of Cambridge were partially funded by Kristjan Jaak degree studies abroad scholarship by Estonia's Education and Youth Board.

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Chapter 1

Introduction

Overview and structure of the thesis

The research presented within this thesis evaluates the impact of mindfulness-based programmes (MBPs) on work performance. The prospect of improving work performance is appealing to individuals and organisations alike. While the media and several academic authors have widely written about the benefits of mindfulness on work performance, there is little empirical evidence to support these claims. Further, it is unclear how MBPs may improve work performance, that is, the potential mechanisms through which MBPs may produce beneficial effects.

In **Chapter 1,** the present introduction, I will explain the reasons for focussing on work performance, provide a brief review of the main concepts used throughout the thesis – notably, mindfulness, MBPs, and work performance – and discuss the existing gaps in our knowledge, along with the challenges in bridging these gaps. The remaining chapters recount efforts to improve the evidence-base by addressing five key research questions.

Chapter 2 features a systematic review and meta-analysis of the effectiveness of MBPs on work performance. The meta-analysis sought to answer two questions:

Research Question 1: How is individual work performance operationalised in trials assessing the effects of MPBs?

Research Question 2: What is the effectiveness of MPBs for improving work performance based on the current literature?

Chapters 3 and 4 present a feasibility randomised controlled trial (RCT), specifically its protocol (Chapter 3) and methods and results (Chapter 4). The trial investigated two potential mechanisms underpinning MBPs' effects on work performance: improved mental health and increased cognitive control. In doing so, I sought to determine:

Research Question 3: Could improved cognitive control and/or enhanced mental health be potential mechanisms underlying the effect of MBPs on work performance?

Research Question 4: Is it acceptable and feasible to run an RCT to investigate the effect of MBPs on work performance?

Research Question 5: What is the effect size of MBPs on work performance when compared to an active control group?

Chapter 5 synthesises the preceding chapters, discussing the implications of the thesis results for research and practice.

Strategies to improve work performance.

Improving work or academic performance is an attractive goal for individuals and organisations alike. For individuals, such as employees and students, it may offer a sense of achievement, open career opportunities, or increase income. For organisations, such as employers, universities and schools, the performance of each individual contributes to the organisation's overall ability to meet organisational goals, to organisational growth, and ultimately, revenue. In turn, individual and organisational prosperity drives economic indicators across the industry and country.

Both individuals and organisations can take steps to enhance workplace and academic performance. At an individual-level, strategies to optimise cognitive performance are popular, with strong evidence that this can be achieved by improving general well-being: enhancing sleep quantity and quality, physical exercise and diet (Kent et al., 2015; Lieberman, 2003; Llewellyn et al., 2008). After all, according to the World Health Organization's definition of health[1], working well is an indicator of well-being. People may also turn to stimulants such as caffeine or nicotine (Lieberman, 2003); there is evidence of off-label and illegal drugs used among students and workers for the purpose of cognitive enhancement (Maier et al., 2018). In recent years, mindfulness has been suggested in the media as a means of improving work performance (e.g., Brownlee, 2020). This is supported by some peer-reviewed papers that argue that mindfulness improves work performance by enhancing the ability to maintain focus (e.g., Dane, 2011; Good et al., 2016). In the meantime, organisations seek to boost performance by setting targets to work towards (performance indicators), developing organisational, managerial and recruitment policies and establishing standard operational procedures. For example, they may seek to reduce bureaucracy, improve the ergonomics of working areas or train supervisors to be able to best support their staff and students.

Despite these efforts, individuals' and consequently organisational work performance can suffer when people feel unwell, not only physically, but also mentally. People who are mentally well are suggested to work better (García-Buades et al., 2020; Montano et al., 2017; Wright et al., 2007). Moreover, poor mental health is a leading cause of sickness absence[2]. Working and academic environments have a strong influence on the physical and mental health of employees and students. Therefore, organisations have a duty of care to ensure the environment they create for working and studying prevents ill health. This is

[1] Health is "a state of well-being that enables people to cope with the stresses of life, realise their abilities, learn well and work well, and contribute to their community" (World Health Organization, 1948).

[2] For an example, see reasons for sick leave in one of the world's largest employers, The National Health Service in England (NHS Sickness Absence Rates, 2023).

often regulated by law (e.g., Council Directive 89/391/EEC of 12 June 1989 on the Introduction of Measures to Encourage Improvements in the Safety and Health of Workers at Work, 1989; Health and Safety at Work Etc. Act, 1974) and related policies (e.g., The Swedish Work Environment Authority, 2015).

Greater awareness of mental health, its risks, and consequences (among them loss of productivity) motivates organisations to go further than the duty of care that laws mandate. To support these efforts, public institutions have been issuing supplementary guidance on best practices (e.g., National Institute for Health and Clinical Excellence, 2008, 2022; Stevenson & Farmer, 2017; The Swedish Work Environment Authority, 2015; What Works Centre for Wellbeing, 2020). The COVID-19 pandemic has accelerated the trend with organisations increasingly realising the need and benefit of supporting health of their employees and students (e.g., Leclerc et al., 2022).

In the hopes of gaining a competitive edge, organisations may go beyond addressing basic health needs to foster higher achievements. Employers and higher education institutions sometimes offer 'perks' that could range from health and wellness services (massages, access to therapy, private health insurance, gym memberships, and ping-pong tables, to name a few) and further occupational training opportunities to services to reduce the everyday burden of running errands (dry cleaning, hairdressing). These compensation packages are intended to support physical and mental well-being which, according to the happy-productive worker thesis, should enhance work performance (García-Buades et al., 2020). Additionally, the packages could increase the time people spend at work (e.g., Bock, 2015), and foster relationships within the organisation rather than in the community (e.g., Chen, 2022), with the end goal of improving work performance and organisational allegiance (Bock, 2015; Chen, 2022).

Yet, the 'perks' employers offer to support employees' well-being and performance often rely rest on a scant evidence-base, the strength of which is diminished by studies' participation bias and outcome selection (e.g., Janßen et al., 2012; Jones et al., 2019; Spence, 2015). The effects detected in occupational

health research paradigms may be more supportive of offering well-being programmes universally, that is, not only as a targeted prevention. It is unclear whether well-being interventions improve work performance in thriving populations.

Regardless, an increasing number of employers (e.g., Google, UK Parliament, Transport for London, US Marines) offer mindfulness as part of their universal employee well-being package (Data Bridge Market Research, 2020). Similarly, many universities and schools offer mindfulness courses for their students (Dawson et al., 2019; Dunning et al., 2022). The research on mindfulness programmes suffers from the same issues highlighted in the previous paragraph. Notably, outcomes for evaluations of mindfulness-based programmes (MBPs) in institutional contexts most frequently feature emotional well-being and stress (Bartlett et al., 2019; Dawson et al., 2019). This means little is known about whether mindfulness enhances work performance beyond improvements in psychological well-being. In addition, little information is available on the effectiveness of mindfulness training in comparison to other well-being programmes.

The difficulties of defining and measuring mindfulness

There is no scientific consensus on the definition of mindfulness (Bishop et al., 2004; Grossman & Van Dam, 2011; Van Dam et al., 2017). Mindfulness stems from Buddhism, where it is a component of practices said to lead to the cessation of suffering (Van Gordon et al., 2015). The Western scientific discourse has diverged from the original teachings of the Buddha (Van Gordon et al., 2015). In the scientific literature, the term *mindfulness* sometimes refers to a trait (dispositional mindfulness) or a state which is described through the concepts of attention, awareness, and acceptance. This differs from the millennia-old Buddhist meaning of mindfulness as a process (see Grossman & Van Dam, 2011, for a discussion). Other times, *mindfulness* refers to the cultivation of the aforementioned state via

formal meditation exercises, or an intervention that teaches the practice (Grossman & Van Dam, 2011). The latter use of the term has gained popularity since Jon Kabat-Zinn developed the Mindfulness-Based Stress Reduction programme to manage chronic pain in 1970s (Kabat-Zinn, 1990) and by doing so, Westernised and secularised mindfulness from Buddhism. He described mindfulness as: "the awareness that emerges through paying attention on purpose, in the present moment, and nonjudgmentally to the unfolding of experience moment by moment" (Kabat-Zinn, 2003, p. 145).

Because of the variation in its definition, mindfulness is difficult to operationalise and measure within the Western tradition of philosophy of science (Van Dam et al., 2017). As with many psychological phenomena, researchers rely largely on self-reported levels of mindfulness that are collected through questionnaires. Consequently, we do not exactly know what a given questionnaire measures when it is proposed to measure mindfulness, and we do not have an objective assessment of someone's level of mindfulness.

This poses a problem for mindfulness research. Without a clear operationalisation of mindfulness, it is difficult to make reliable claims about its effects. Consequently, the ground is fertile for a myriad of interpretations and sub-types of mindfulness, such as *collective mindfulness* (Sutcliffe et al., 2016; Weick et al., 1999) which has become to mean a variety of phenomena (see Sutcliffe and colleagues for an overview (2016)). Defining mindfulness is beyond the scope of this doctoral thesis. However, with the methods available to us now, we can observe outcomes, other than based on mindfulness questionnaires, of programmes that claim to teach how to practice mindfulness and propose the mechanisms that lead to those outcomes.

The lack of a universally accepted definition of mindfulness is reflected in the assortment of programmes that claim to teach mindfulness. The interventions may have a variety of perspectives on mindfulness, meditation practices and intervention logic (Crane, 2019; Crane et al., 2017; Crane & Hecht, 2018; Oman, 2023; Van Dam et al., 2017). The programmes may also differ in their content

(Crane, 2019; Crane & Hecht, 2018), duration (Van Dam et al., 2017) and teacher training (Crane & Hecht, 2018; Kenny et al., 2020). To increase attractiveness to clients and patients, programmes may claim to be mindfulness-based because mindfulness has become a buzzword (Oman, 2023; Van Dam et al., 2017).

To measure the effects of interventions that teach mindfulness skills more accurately, it is important to make a distinction between mindfulness-based programmes (MBPs) and programmes that use mindfulness as one component, among others. The definition of a programme relies on the programme's theory of change; it explains how the programme works, states its therapeutic components, and establishes the conditions under which the programme has its effects (Breuer et al., 2016; De Silva et al., 2014; Ringhofer & Kohlweg, 2019; Skivington et al., 2018). A defined theory of change helps to implement and evaluate programmes (Goldberg et al., 2017; Van Dam et al., 2017), indicating which outcomes are pertinent to measure, and distinguishing between programmes that may target the same outcomes but via a different mechanism (e.g., students' academic attainment could benefit from both mindfulness and/or time management skills). Without clear programme's theory of change, it is difficult to know what to conclude from evidence synthesis of that programme. For instance, when a meta-analysis defines a mindfulness intervention as "an intervention in which mindfulness meditation was the central component (as indicated by mindfulness either featuring in the title of the intervention or being given prominence in the abstract)" (Lomas et al., 2017, p. 493), it is unclear whether the effect can be attributed to trialists' willingness to bet on the buzzword to get their manuscript published or some joint trait within the programmes' content and/or their delivery.

Rebecca Crane and her colleagues (2017), an international collaboration of some of the most senior mindfulness teachers, formulated a framework for MBPs which serves as the most accepted current definition of an MBP. They proposed that MBPs are grounded in contemplative traditions as well as scientific disciplines such as psychology, medicine, and education. The suggested theory of change is that MBPs reduce distress through training meta-cognitive abilities and by cultivating a way of experiencing that is aligned with Jon Kabat-Zinn's definition of

mindfulness. This is achieved through sustained mindfulness meditation practice which in turn drives improvement of self-regulation capacities, and development of compassion, wisdom, and equanimity. The MBPs theory of change relies on our current understanding of the proposed mechanisms of mindfulness (for example, the role of cognitive control in mental health). Inevitably, as our understanding of the mechanisms improves, we might need to update the theory (De Silva et al., 2014), a notion that the authors acknowledge (Crane et al., 2017). Crane and colleagues' (2017) framework is often referred to as the 'warp and the weft model' of MBPs. In this thesis, the term "mindfulness-based programme" (MBP) is reserved for those programmes which align with the Crane and colleagues' (2017) definition.

The complexity of establishing work-related effects of MBPs

Compared to wait-list control groups, mindfulness-based programmes (MBPs) have been shown to reduce stress (Chiesa & Serretti, 2009; Dawson et al., 2019; Galante et al., 2021), anxiety, and depression (Galante et al., 2021) and improve overall well-being (Galante et al., 2021) in non-clinical populations. Workplace-based programmes teaching meditation have been shown to have similar effects (Bartlett et al., 2019) as have MBPs offered in universities (Dawson et al., 2019).

Several authors suggest that mindfulness also improves a wide range of work-related outcomes. For example, mindfulness is suggested to improve performance (Baltzell, 2016; Dane, 2011; Glomb et al., 2011a; Good et al., 2016; Hall, 2015; Reb et al., 2017, 2019; Zeller & Lamb, 2011), decision-making (Glomb et al., 2011a; Karelaia & Reb, 2014; Parsons et al., 2020; Schmitzer-Torbert, 2020), creativity and innovation (Chapman-Clarke, 2016; Cheung et al., 2020; Good et al., 2016; Henriksen et al., 2020; Kudesia, 2015; S. L. Shapiro et al., 2015), leadership (Boyatzis, 2015; Glomb et al., 2011a; Good et al., 2016; Reb et al., 2015, 2019), working relationships (Chapman-Clarke, 2016; Good et al., 2016), teamwork (Good

et al., 2016) and conflict management (Good et al., 2016) among other work-related outcomes. Yet, the evidence for these claims is very preliminary and fallible. Most of these claims rely on correlational studies investigating the association between dispositional mindfulness (a score on a self-reported questionnaire) with a construct of work performance (a score on a different self-reported questionnaire). Correlation, of course, does not equal causation. People may score high on the questionnaire without having practiced mindfulness or they may score low despite participating in an MBP. It is therefore unclear whether teaching mindfulness leads to improved work performance, as the impact of MBPs on work performance has not been sufficiently evaluated (Bartlett et al., 2019; Hyland, 2015; Jamieson & Tuckey, 2017; Rupprecht et al., 2018).

The National Institute for Health and Care Excellence (NICE) guidelines for well-being at work (2022) specifically note that individual-level programmes, including MBPs, should not be implemented at work solely to improve productivity[3]. Yet inevitably, the prospect of enhanced work performance will appear attractive to employers (Luyten et al., 2017), particularly if it is advocated as a quick fix for fundamental issues in work organisation and funding of public services (e.g., mindfulness as in Zeller & Lamb, 2011). Furthermore, the expectation of improving one's ability to work better through practicing mindfulness may be alluring to the individuals who are career- or achievement-oriented and are thus intrinsically driven to improve their performance. Given that MBPs are shown to benefit mental health (Galante et al., 2021) and are recommended as a workplace well-being programme (National Institute for Health and Clinical Excellence, 2022), it is worthwhile to investigate whether MBPs could also improve work performance.

[3] The NICE committee explains that as productivity could be enhanced by means that would lead to higher employee stress (for example, increasing workloads), the committee chose to favour benefits for employees over those for employers. They therefore caution against the use of individual-level programmes with productivity gains as the main aim (NICE, 2022, evidence review D).

Defining work performance

The term *work performance* is widely used yet inconsistently defined both in the economic (Pekuri et al., 2011; Tangen, 2005a) as well as the organisational psychology literatures (Koopmans et al., 2011). Campell and Wiernik (2015) argue that work performance is what people do to advance organisational goals. Work performance is thus a behavioural construct, rather than a trait, ability or an outcome (J. P. Campbell & Wiernik, 2015; Motowildo et al., 1997). Work performance is influenced by certain precursors, for example, cognitive ability and motivation (J. P. Campbell & Wiernik, 2015; Koopmans et al., 2011). Individual work performance in turn contributes to employee-level outcomes such as job satisfaction or again, motivation, as well as to indicators that reflect a joint effort by many employees (for example, company's profit).

The research described in this thesis has adopted Koopmans and her colleagues' definition of work performance (2011, 2014b), derived from a systematic literature review (Koopmans et al., 2011) and validated using a survey among academics, managers, human resource managers and occupational health experts (Koopmans et al., 2014b). Koopmans and her co-authors suggest that individual work performance has four dimensions.

The first dimension is task performance, or how well someone performs tasks that are central to their role (J. P. Campbell & Wiernik, 2015; Conway, 1996; Motowildo et al., 1997). These are activities that transform raw materials into the goods or services; for example, selling goods, teaching at a school, performing surgery, or making decisions about lending at a bank.

The second is contextual performance, or behaviours that support a functional working environment (J. P. Campbell & Wiernik, 2015; Conway, 1996; Motowildo et al., 1997). These behaviours do not directly contribute to an organisation's bottom line but create circumstances in which core tasks can be carried out. Contextual performance can be demonstrated through replenishing supplies, supervision of staff, or coordinating a team.

Third is adaptive performance, or the extent to which someone can adjust their actions in response to or in anticipation of changes in the environment (Charbonnier-Voirin & Roussel, 2012; Jundt et al., 2015; Park & Park, 2019; Pulakos et al., 2000). Here, it is important to distinguish skills (knowing how to handle stress), context (a stressful situation) and behaviour (self-regulation to inhibit an unhelpful response) (for discussion see J. P. Campbell & Wiernik, 2015). Pulakos and colleagues (2000) proposed eight dimensions of adaptive work performance:

1. handling emergencies or crisis situations (stepping up to act, quickly analysing options for actions),
2. handling work stress (not overreacting to unexpected news, demonstrating resilience in stressful circumstances),
3. solving problems creatively (generating new ideas in complex areas, developing innovative methods),
4. dealing with uncertain and unpredictable work situations (changing behaviour in response to unpredictable events, adjusting plans, goals, actions if needed),
5. learning work tasks, technologies, and procedures (keeping knowledge and skills current, taking action to improve work performance),
6. interpersonal adaptability (listening to and considering others, developing effective relationships with highly diverse personalities),
7. demonstrating cultural adaptability (adjusting behaviour to comply with or show respect to others, understanding the implications of one's actions),
8. demonstrating physically oriented adaptability (adjusting to challenging environmental states like heat, cold, dirtiness, adjusting physical fitness to perform job-related tasks).

There is some dispute about whether adaptive performance is a dimension on its own (Charbonnier-Voirin & Roussel, 2012; Jundt et al., 2015; Koopmans et al., 2013, 2014b; Park & Park, 2019; Pulakos et al., 2000) or whether elements of adaptive performance are captured within task performance and contextual performance (J. P. Campbell & Wiernik, 2015; Koopmans et al., 2013). Two studies have sought to collaboratively consolidate the field and found support for adaptive

performance as a separate dimension. Koopmans and her colleagues (2014b) surveyed attendees of one national and one international occupational health conference. The 695 respondents (including researchers, managers, human resources specialists and occupational health professionals) were asked to categorise 231 individual work performance indicators found in the 81 work performance questionnaires the authors had identified through systematic review (Koopmans et al., 2014b). Adaptive performance emerged as one dimension featuring resiliency, coming up with creative solutions, and keeping job knowledge and skills up-to-date as the most important indicators (Koopmans et al., 2014b). Similarly, Abbasi and his colleagues (2022) invited 74 researchers to complete a Delphi study to construct a framework for individual work performance, ranking indicators for each domain. The results identified similar indicators of adaptive work performance as Koopmans and colleagues (2014). These studies do not prove adaptive performance exists as a separate dimension. They do highlight what types of work behaviours are valued among experts in the field. Adaptive work performance is suggested to have become more important in modern working environments that are multinational, complex and unpredictable (Charbonnier-Voirin & Roussel, 2012) and could be particularly important in certain industries (e.g., healthcare Krijgsheld et al., 2022).

The final component is counterproductive work behaviour, or behaviour that harms the well-being of the organisation or its members and includes, among others, poor conduct, accidents, absenteeism (missing work for unplanned reasons) and presenteeism (attending work while unwell) (Spector et al., 2006).

Work performance is thus a multi-dimensional behavioural construct. Which construct explains most of the variation in the overall work performance can depend on job role as well as values and aims of the organisation. Yet, there is some evidence that task performance may seem most pertinent to experts. Koopmans and colleagues (Koopmans et al., 2014b) found that task performance was rated to have the most weight in describing individual work performance in a study involving researchers, managers, occupational health and human resource experts. However, the importance given to task performance did differ

significantly between managers and researchers, with the latter giving task performance more importance. Such divergence becomes critical when measuring work performance.

Measuring work performance

Since work performance is an aggregate of an individual's episodes of behaviour, it is important to capture performance in a way that encompasses the variety of behaviours within one job or degree (Koopmans et al., 2014b; Ramos-Villagrasa et al., 2019). To compare work performance across roles, organisations, and industries, the measurement tool needs to be generic enough. Although work performance is widely measured and its outcomes used for recruitment, promotion, appraisal as well as research purposes, the development of a generic individual work performance measure is in its infancy (Ramos-Villagrasa et al., 2019).

There are broadly three ways of collecting data on work performance: through performance ratings (by oneself, peer, supervisors or subordinates), using samples, simulations or proxies, or via technology-enhanced assessments (J. P. Campbell & Wiernik, 2015). Each of these approaches has their strengths and limitations, and choosing the right approach may also depend on the purpose of measuring work performance (e.g., recruitment, appraisal, or research).

Performance rating scales are widely used, yet they mostly do not measure the full range of individual work performance and are not suitable for generic use (Koopmans et al., 2013, 2014b). Additionally, performance ratings are subject to the usual psychometric concerns: construct validity, external validity, inter-rater reliability and rater bias (Arvey & Murphy, 1998; J. P. Campbell & Wiernik, 2015). These produce limitations in the ability (a) to verify that an instrument indeed measures what it is designed to measure (i.e., work performance) (Cronbach & Meehl, 1955); (b) to demonstrate that measurements in one context (e.g., a study) can predict measurements in other settings (the real world, or a different study)

(D. T. Campbell, 1957); (c) to exhibit that different raters concur in their ratings (Saal et al., 1980) and (d) to reduce the extent to which a rater is affected by pre-existing biases when making their judgement (Hoyt, 2000).

The other two collection methods – sample tasks or simulations and technology-enhanced assessments, both attempt to gauge someone's real work performance. Sample tasks and simulations occur in a limited setting, for example assessing how one assembles a product or analyses a dataset. Technology-enhanced assessments allow routine monitoring of tasks through performance indicators. Both approaches are limited to certain types of tasks and circumstances. These cannot capture the full range of work performance dimensions.

For research purposes, including in MBP research, self-reported instruments are often preferred over other methods. Self-reported questionnaires have the same limitations as any rating scales (see above). Additionally, self-rated job performance is generally more favourable than that of others (DeNisi & Murphy, 2017), yet it is unclear whether that reflects leniency in rating oneself or different perspectives on work performance (Murphy, 2008). At the same time, self-reported reduced ability to perform work-related tasks does not necessarily correlate with employer's metrics of productivity (Gardner et al., 2016). Despite these uncertainties, self-reported questionnaires offer several benefits, including: (a) providing an opportunity to compare results across contexts (often between organisations and industries); (b) easier administration while managing confidentiality issues and reducing the frequency of missing data (Schoorman & Mayer, 2008) and (c) potentially, accuracy: employees themselves have opportunities to observe their own behaviour more consistently, while others may overly rely on their general impression of the person they evaluate (i.e., the halo effect) (Koopmans et al., 2013). The randomised controlled trial reported in Chapters 3 and 4 therefore used self-reported work performance outcomes (see Chapter 3's foreword for a more detailed account of the choice).

MBPs' effects on work performance

The lack of clear definitions for mindfulness-based programmes (MBPs) and for workplace performance is reflected in the literature investigating the effects of MBPs. When I started my doctoral studies, there were two systematic reviews with meta-analysis of randomised controlled trials (RCTs) evaluating the effectiveness of teaching meditation at work (Bartlett et al., 2019; Vonderlin et al., 2020), one systematic review without meta-analysis (Lomas et al., 2017) and a systematic review and meta-analysis for MBPs for university students (Dawson et al., 2019). Since then, a realist review (without meta-analysis) of MBPs in the work context has been published (Micklitz et al., 2021). The two meta-analyses (Bartlett et al., 2019; Vonderlin et al., 2020) provided evidence of the high heterogeneity in work performance related outcomes in meditation-related research. The variety of outcomes used poses a limit on reviewers' ability to pool results. Indeed, Bartlett and her colleagues (2019) chose not to meta-analyse work performance outcomes for that reason. Meanwhile, Dawson and her colleagues (2019) found academic performance was so rarely rated, that a meta-analysis was impossible.

All five existing systematic reviews (Bartlett et al., 2019; Dawson et al., 2019; Lomas et al., 2017; Micklitz et al., 2021; Vonderlin et al., 2020) on meditation are limited in the implications they offer regarding the efficacy of MBPs for work performance. The realist review (Micklitz et al., 2021) was designed to explore how and why MBPs work and not whether MBPs work. Lomas and his colleagues (2017) included various designs, even those without a control group or reporting qualitative data, thus potentially inflating any effects and making the effects hard to quantify (they did not perform a meta-analysis). The remaining limitations stem from (a) their primary outcomes; (b) focus on occupational environment exclusively; and (c) definition of interventions to be included in the meta-analysis. I will visit each limitation in turn.

First, although three of the systematic reviews included the effects of mindfulness on work performance (Bartlett et al., 2019; Lomas et al., 2017; Vonderlin et al., 2020), their focus was on mental health outcomes. This shaped the exclusion

criteria. In other words, a study that measured work performance, but not mental health would have been excluded. In four studies (Bartlett et al., 2019; Dawson et al., 2019; Lomas et al., 2017; Vonderlin et al., 2020), the reviewers did extract work/academic performance outcomes where present. Yet only Vonderlin and colleagues (2020) pooled the results for a meta-analysis. The results therefore do not offer a comprehensive picture of known effects of MBPs on the outcome of interest in this thesis. Additionally, none of the three reviews (Bartlett et al., 2019; Lomas et al., 2017; Vonderlin et al., 2020) used a theory-driven definition of work performance, which is likely a by-product of work performance not forming a key focus of the reviews. For example, Vonderlin and colleagues (2020), extracted data on job satisfaction, which does not meet the standard definition of work performance (see Defining work performance, p. 19).

Second, the majority of the systematic reviews focussed on interventions delivered in the occupational environment (Bartlett et al., 2019; Lomas et al., 2017; Micklitz et al., 2021; Vonderlin et al., 2020). This excludes other contexts of potential interest. To begin with, interventions delivered in the community could also include a measure of work performance (admittedly a rare occasion). More importantly, occupational settings exclude higher education where performance is consistently measured. While the grading systems vary across countries and universities, the diversity is likely to be smaller than across different job roles (e.g., between strawberry pickers vs theoretical physicists). Students receive marks based on their ability to synthesise attained knowledge, write essays and reports. Such tasks, by design, are akin to those performed by many knowledge economy workers (journalists, researchers, analysts). The inclusion of academic performance as an outcome could thus improve the external validity of MBPs effects on work performance. Although academic performance is rarely measured in RCTs of MBPs (Dawson et al., 2019), pooling the results across other work performance measures could provide a way of reducing bias in outcome measurement. Grading is mostly done by an external rater, whereas work performance is predominantly measured through self-reports. In the context of RCTs, graders can be kept blind to the treatment allocation (i.e., whether the student completed an MBP) reducing the incidence of expectancy bias.

Finally, there was considerable heterogeneity in the reviews regarding what constitutes an MBP. While two followed the existing MBP definition used in this thesis (Dawson et al., 2019; Micklitz et al., 2021), the three remaining reviews included interventions that were "explicitly described as mindfulness programmes" (Bartlett et al., 2019 p. 111), were a "mindfulness-based program" (Vonderlin et al., 2020 p. 1581) and where "mindfulness meditation was the central component (as indicated by mindfulness either featuring in the title of the intervention or being given prominence in the abstract)" (Lomas et al., 2017, p. 493). The three reviews did not explicitly define what is an MBP. The names of the interventions included in the meta-analyses reflect a wide array of contemplative practices: zen meditation, *custom contemplative training*, yoga and mindfulness, transcendental meditation, mindful art processing, meditation, mantra meditation (Bartlett et al., 2019; Vonderlin et al., 2020). We should thus be careful in interpreting the results of these meta-analyses. Rather than MBPs' effects, the meta-analyses synthesised effectiveness data across a wider selection of contemplative interventions. Given there is little comparative research to understand the relative effectiveness of different contemplative practices, it is hard to estimate how well the reviews' results generalise to MBPs.

To guide future trials evaluating MBPs at work, Chapter 2 of this thesis will systematically map how work performance has been assessed in trials investigating the effectiveness of MBP, as defined by Crane et al (2017), on the different domains of work performance according to the Koopmans' framework (2011). This consolidates understanding of whether a standardised MBP programme has an effect on established operationalisations of work performance and helps to estimate which aspect of work performance may benefit. To my knowledge, this systematic review and meta-analysis presented, is the first attempt to seek systematic evidence of MBPs' effects on work performance and to meta-analyse the effects based on a theoretical model.

The mechanisms through which MBPs improve work performance

Another factor inhibiting the measurement and improvement of the effect of mindfulness-based programmes (MBPs) at work is the lack of understanding of potential mechanisms through which the benefits may come about (Alsubaie et al., 2017; Miksch et al., 2015; Pérez-Nebra et al., 2021; van der Velden et al., 2015). Understanding mechanisms of change (a) would help to design better, more targeted interventions, (b) would improve our attempts to assess MBPs' specific effects, by designing and selecting more stringent control interventions and (c) may promote a personalised medicine approach by informing understanding of what works for whom, and in which context (Nielsen et al., 2018).

The rare use of active control groups in evaluations of meditation programmes (Bartlett et al., 2019; Vonderlin et al., 2020) is holding back research and practice in many ways. First, while the use of passive control groups helps to control for some variables (e.g., passage of time, spontaneous recovery, regression to the mean), it does not allow assessment of intervention-specific effects beyond placebo and non-specific effects like attending group sessions (Mohr et al., 2009). It is thus difficult to know if MBPs are effective because of specific features (e.g., learning to be mindful) or non-specific aspects that could be achieved through other means (by taking a break for oneself) (Bishop, 2002). Understanding the "active ingredients", could help to make MBPs more effective by facilitating improved programme design which retains (or potentially enhances) the active ingredients but removes non-essential components. Moreover, active control groups help to establish comparative effectiveness of MBPs. If work performance could be improved more, or more economically, by another intervention, it would be reasonable for relevant commissioners to steer away from MBPs. Furthermore, without active control groups, it is also difficult to establish whether the MBPs offered do differentially activate the mechanisms described in the MBP programme theory (see *The difficulties of defining and measuring mindfulness*, p. 14). Finally, as will be explained further in Chapter 3, there have been so few active

control group trials evaluating the effects of MBPs, that we do not have an estimate of the likely treatment effect size which is needed to plan and fund larger, later-phase trials. The lack of later-phase trials limits our ability to make firm conclusions regarding the effectiveness of MBPs.

There are multiple proposed mechanisms through which MBPs may impact work performance. Three papers have sought to outline the mechanisms of action of mindfulness in general, without specifying the outcome. At the broadest level, Bishop and his colleagues (2004) suggested mindfulness meditation to has two pathways of action: self-regulation of attention and adoption of an orientation toward one's experiences that is curious, accepting, and open. This echoes Kabat-Zinn's (2003) description of mindfulness. Hölzel and her colleagues (2011) expanded this and proposed four mechanisms of change: attention regulation, emotion regulation, body awareness, and change in the perspective of the self. Then, Vago and Silbersweig (2012) proposed a refined model named S-ART for self-awareness, self-regulation, and self-transcendence. Finally, specifying the outcome and focussing on MBPs in particular, Crane and colleagues (2017), suggested that MBPs improve mental health through self-regulation capacities, development of compassion, wisdom, and equanimity. The four approaches are not conflicting but do conceptualise the pathways slightly differently. For example, whether it is important to distinguish between attention and emotion regulation, or whether it is specifically body awareness or more general self-awareness that is important. Some of the proposed mechanisms have been empirically supported in the context of Mindfulness-Based Cognitive Therapy (MBCT, an MBP). A systematic review found support for dispositional mindfulness, rumination, worry, compassion and meta-awareness as predicting or mediating MBCT outcomes (van der Velden et al., 2015). The authors additionally suggested that there is emerging evidence from a small number of randomised controlled trials (RCTs) that attention and emotional reactivity may be potential mechanisms of change in MBCTs.

These theoretical models can be applied to workplace performance. First, MBP-driven improvement in mental well-being (Galante et al., 2021) may subsequently

improve work performance since mental well-being is linked to better work performance (García-Buades et al., 2020; Montano et al., 2017). Mental health problems decrease employees' performance (Alonso et al., 2011; J. J. Collins et al., 2005; García-Buades et al., 2020; Montano et al., 2017; Rost et al., 2004; Stewart et al., 2003), particularly if these problems are not well managed (Rost et al., 2004). If this is the main pathway MBPs improve work performance, it would suggest that MBPs are likely to only have an effect when mental health is poor. Moreover, the effect of MBPs would not be unique: any intervention benefiting mental health could boost work performance.

A second potential pathway is via cognitive control (Chaskalson, 2011; Dane, 2011; Garland et al., 2017; Goilean et al., 2021; Good et al., 2016; S. L. Shapiro et al., 2015), that is, the ability to self-regulate attention at work to allow prioritisation of current goals (Ionescu, 2012; Schweizer et al., 2019). All four mechanistic models of mindfulness practice include self-regulation of attention (Bishop et al., 2004; Crane et al., 2017; Hölzel et al., 2011; Vago & Silbersweig, 2012), with supportive but preliminary empirical evidence from RCTs assessing MBCT (van der Velden et al., 2015). As it is difficult to measure self-regulation, researchers have relied on measuring the effects of MBPs on executive functions. Hofmann, Schmeichel, and Baddeley (2012) proposed that self-regulation may be improved through training executive functions: (a) shifting, that is, the ability to switch between multiple tasks or operations, (b) updating, that is, the ability to frequently refresh information in working memory to ensure a record of information that is currently relevant, and (c) inhibition, that is, the deliberate hindering dominant or automatic responses that are irrelevant to the task at hand (Miyake et al., 2000).

Yet, it is currently unclear whether MBPs improve executive functions and through them, cognitive control. There are five recent meta-analyses that have pooled the effects of meditation (not exclusively MBPs) on a range of executive functions, as described by Miyake and colleagues (2000) (Cásedas et al., 2020; Im et al., 2021; Millett et al., 2021; Yakobi et al., 2021; Zainal & Newman, 2023). The results (see Table 1) lend some support to meditation's beneficial effects on executive functions, which, through self-regulation, could lead to improved work

performance (Goilean et al., 2021; Good et al., 2016). However, the effect sizes vary and there is also evidence of negative effects whereby MBPs may have a detrimental effect on cognitive functions (see Table 1). The variation in effect sizes could be due to variations in what was considered a meditation intervention, to the categorisation of behavioural tasks for pooling their effects, as well as to the types of studies included. For example, Millett and colleagues (2021) included both non-randomised and randomised designs.

Table 1. Pooled effects of meditation practice on executive functions

Executive function	Meta-analysis	Hedges' *g*	95% CI
Shifting	Cásedas and colleagues (2020), indexed as cognitive flexibility	0.09	−0.13 to 0.31
	Millett and colleagues (2021)	−0.03	−0.27 to 0.21
	Zainal and Newman (2023)		
	Accuracy	0.2	0.06 to 0.34
	Latency	−0.03	−0.18 to 0.12
Working memory	Cásedas and colleagues (2020)	0.42	0.10 to 0.74
	Im and colleagues (2021)	0.16	0.15 to 0.47
	Millett and colleagues (2021)	0.22	0.01 to 0.44
	Yakobi and colleagues (2021)	0.15	−0.02 to 0.32
	Zainal and Newman (2023)		
	Accuracy	0.30	0.10 to 0.49
	Latency	0.11	−0.11 to 0.32
Inhibition	Cásedas and colleagues (2020)	0.42	0.20 to 0.63
	Millett and colleagues (2021)	0.22	0.06 to 0.39
	Zainal and Newman (2023)		
	Accuracy	0.19	0.08 to 0.31
	Latency	0.02	−0.70 to 0.73

The mixed evidence on meditation effects on executive functions could reflect the fact that the research has primarily focussed on the impact of meditation on executive functions applied to emotionally benign information. However, when utilising executive function to regulate daily activity (i.e., achieve cognitive control) (Friedman & Robbins, 2022), much of the mental content to be managed is of an emotional nature. We are constantly required to inhibit emotional thoughts (e.g., worrying about an argument with your spouse that morning) that are

irrelevant to the task at hand (e.g., completing statistical analysis). Emotional material is suggested to hinder cognitive control (Pessoa, 2009; Pessoa et al., 2012; Schweizer et al., 2019). In other words, emotional thoughts may be more distracting than neutral thoughts. During mindful meditation, the stimuli that participants practice inhibiting and moving away from is frequently of an emotional nature (e.g., thoughts or feelings of discomfort). Practicing mindfulness may therefore enhance cognitive control over positive and negative mental events more than neutral events (Crane et al., 2017; Hölzel et al., 2011). For example, moving attention away from negative content (such as worries about task performance) to refocus on the task at hand (Jamieson & Tuckey, 2017). Indeed, Frewen and colleagues (2008) found in a non-randomised study of undergraduates that higher levels of mindfulness correlated with lower levels of worry and improvements in the ability to let go of worrying. In an RCT (Mindfulness-Based Stress Reduction vs cognitive behavioural therapy), changes in cognitive control were found to predict decentering three months later (Garland et al., 2017). In sum, MBPs could have a differential effect over cognitive control of emotional information, yet there is little empirical evidence available on this topic.

To my knowledge, no study has investigated the effects of a MBP on cognitive control over emotionally valenced stimuli. However, there are some published studies that have investigated whether meditation (not MBP), when compared to a control group, improves cognitive control over negative information more than over neutral information (Ainsworth et al., 2013; Allen et al., 2012; Jha et al., 2017, 2020; Zanesco et al., 2019). There is also one on-going study (Matthews, 2019). The studies used an array of stimuli from the International Affective Picture System (IAPS) (Allen et al., 2012) and sample-specific potentially triggering images (combat vs civilian scenes from Afghanistan for military personnel) (Jha et al., 2017, 2020; Zanesco et al., 2019) to words (Ainsworth et al., 2013; Chambers et al., 2008). Just one of the studies reported statistically significant findings, favouring passive control group (Chambers et al., 2008). Unfortunately, they did not report the results of the statistical test nor the effect size. All remaining published studies reported statistically non-significant findings. Although none of the studies found support for the differential effect of meditation in cognitive control over

emotional content, they do not provide conclusive evidence. Together, the published studies used a variety of interventions from brief technology-assisted meditations (Ainsworth et al., 2013) to tailored “attention programmes” for military personnel (Jha et al., 2015; Zanesco et al., 2019). Whether these effects generalise to MBPs is unknown. Additionally, the sample sizes were small (40 to 200 participants), making effects in group x time x valence comparisons hard to detect.

The cognitive control pathway of MBPs on work performance is thus little researched, particularly when it comes to understanding whether MBPs help to gain cognitive control over negatively valenced mental content. Even if MBPs do have that effect, simply maintaining focus is not an indicator of work performance. It is a precursor: by maintaining focus, one may be able to complete writing a chapter for their thesis sooner but whether the cognitive control transforms into work performance depends on other choices one makes. For example, in their trial of a MBP, Galante and her colleagues (2018) found that the frequency of higher as well as lower grades was greater in the MBP group compared to the control group. The authors hypothesised that some students may have chosen to study less hard because they had recalibrated their priorities (Bóo et al., 2019). There is also evidence that employees may feel unable to apply their skills learnt in a MBPs at work (Micklitz et al., 2021). It is therefore important to investigate the effect of MBPs on work performance through cognitive control, to verify whether there is a translation effect.

To summarise, MBPs could use two pathways to impact work performance (see Figure 1). During my post-graduate degree course, I have sought to find evidence for the pathways and for the overall effect of MBPs on work performance. Specifically, I sought to determine:

Research Question 1: How is individual work performance operationalised in trials assessing the effects of MBPs?

Research Question 2: What is the effectiveness of MPBs for improving work performance based on the current literature?

Research Question 3: Could improved cognitive control and/or enhanced mental health be potential mechanisms underlying the effect of MBPs on work performance?

Research Question 4: Is it acceptable and feasible to run an RCT to investigate the effect of MBPs on work performance?

Research Question 5: What is the effect size of MBPs on work performance when compared to an active control group?

The following three chapters seek to answer these research questions. Namely, the next chapter (**Chapter 2**) will explore what is meant by work performance in MBP trials and what types of work performance are most likely to be improved by mindfulness-based programmes (Research Questions 1 and 2). Thereafter, **Chapters 3 and 4** will describe a feasibility randomised controlled trial (RCT) that was designed to establish whether an MBP improves cognitive control, particularly over emotional material and to explore whether improved cognitive control may mediate the effect of MBPs on work performance (see Figure 1) and whether it is feasible to run a fully powered trial to investigate this mechanism (Research Questions 3-5).

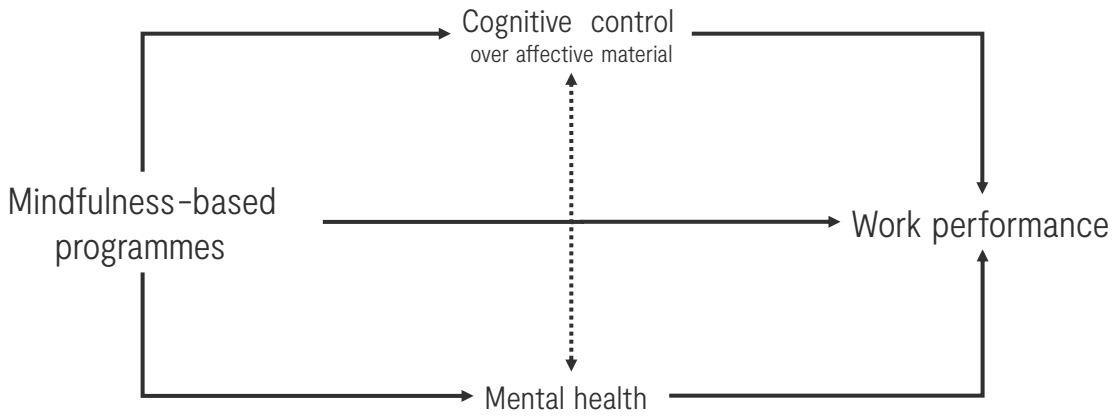

Figure 1. Model for potential mediators for the effects of mindfulness on performance at work

Notes. The dashed line indicates a link not investigated in this thesis.

Chapter 2

Mindfulness training for work performance: A systematic review and meta-analysis of randomised controlled trials

Foreword

As described in the Introduction (Chapter 1), work performance could be operationalised in various ways and there is no consensus as to which operationalisation should theoretically be improved by a mindfulness-based programme (MBP). To structure the existing knowledge and inform future trials, a comprehensive and systematic synthesis of existing knowledge on MBPs' effects on work performance was needed.

This chapter includes a pre-print of a manuscript that we have submitted for publication. The manuscript was deemed of interest, but the peer-reviewers suggested that the literature search should be updated ahead of publication, in light of a number of additional papers being published since we completed data extraction. We are currently working on updating the review. As this work is still in

progress, what follows is the review as is currently published as a pre-print: Vainre, M., Dalgleish, T., Bendriss-Otiko, T., Mariscotti, F., Martinez-Sosa, C., Sideri, A., Hitchcock, C., & Galante, J. (2022). Mindfulness training for work performance: A systematic review and meta-analysis of randomised controlled trials. PsyArXiv. https://doi.org/10.31234/osf.io/2vkru

Abstract

Mindfulness-based programmes (MBPs) are suggested to improve work performance despite scarcity of evidence. We synthesised randomised controlled trials (RCTs) assessing the impact of MBPs on adults' work performance. Notably, we provide the first attempt to systematise the measurement of work performance in mindfulness research, to guide more rigorous research in the future. The primary outcome was task performance. Secondary outcomes were contextual performance, adaptive performance, and counter-productive behaviour. In March 2021 we searched ASSIA, EMBASE, ERIC, MEDLINE, PsycINFO, Scopus, Web of Science, and the Cochrane Central Register of Controlled Trials (CENTRAL). Cochrane Risk of Bias tool 2 was used. We conducted pairwise random-effects meta-analyses to calculate Hedges' *g*s. A total of 47 studies (N = 5041) were included, all of them with high risk of bias. MBPs were not found to improve task performance (k = 7, n = 454, Hedges' *g* = 0.52, 95% CI -0.03 to 1.07, *p* = 0.059) up to 4 weeks post-intervention compared to passive control groups. However, MBPs improved adaptive performance and contextual performance. The number of studies was insufficient for comparisons with active control groups. Confidence in the review results, per Grading of Recommendations Assessment, Development and Evaluation (GRADE), is low to very low. Future trials of higher quality are needed to confirm effects on work performance.

Keywords: mindfulness, systematic review, meta-analysis, work performance

Introduction

Higher work performance awards a competitive edge to employees, students, and organisations alike. When improving performance, it is important to address the mental health needs of the affiliates (i.e., employees, students). As NICE Guidance suggests (National Institute for Health and Clinical Excellence, 2022), workplaces

and academic institutions incorporate mindfulness-based programmes (MBPs) in their well-being package (Barnes et al., 2017; Chen, 2022; Fleming, 2021; The Prince's Responsible Business Network, 2019). MBPs are complex interventions based on contemplative traditions, as well as evidence from the fields of medicine, psychology, and education (Crane et al., 2017). These programmes aim to improve compassion (Crane et al., 2017), attention, and self-regulation (Crane et al., 2017; Hölzel et al., 2011; Verdonk et al., 2020), through training the ability to maintain awareness of the present moment (Kabat-Zinn, 2003) and decentering from psychological stressors by applying a detached self-perspective (Bennett et al., 2021; Crane et al., 2017; Hölzel et al., 2011; Verdonk et al., 2020).

It has been argued that MBPs improve performance (Baltzell, 2016; Chapman-Clarke, 2016; Cheung et al., 2020; Dane, 2011; Good et al., 2016; Hyland, 2015; Reb et al., 2017), making MBPs more attractive to employers over other well-being interventions. There are two theoretical pathways for this effect. First, MBPs could benefit mental health and emotional well-being (Galante et al., 2021; Khoury et al., 2015; Spijkerman et al., 2016) leading to better work performance (Alonso et al., 2011; J. J. Collins et al., 2005; García-Buades et al., 2020; Montano et al., 2017; NHS Sickness Absence Rates, 2023; Rost et al., 2004; Stewart et al., 2003). Second, MBPs could improve cognitive control (Cásedas et al., 2020; Yakobi et al., 2021), that is, the capacity to attend to goal-relevant information and block out distractions, thus helping employees to prioritise task-relevant goals and maximise their performance while at work (Goilean et al., 2021; Ionescu, 2012; Schweizer et al., 2019). It is likely both mechanisms are at play, and interact with one another, but these relationships have not yet been well evaluated. To ensure that propagation of mindfulness in the workplace is empirically supported, we need to apply more rigorous research methods.

Two recent systematic reviews of RCTs (Bartlett et al., 2019; Vonderlin et al., 2020) offer a less enthusiastic summary of the impact of MBPs on work performance. However, methodological limitations to these prior reviews limit their ability to guide future, high-quality research, most notably, to further unpack what

outcomes mindfulness should and should not be expected to improve, and subsequently, in what contexts mindfulness should be delivered. First, both focused on interventions delivered within the occupational environment. This approach excludes higher education – a setting where work performance is consistently assessed. Similarly, work performance is also measured in interventions delivered in the community. Second, and most critically, the terms performance and productivity, as used in prior reviews, are diversely defined both in the economic (Pekuri et al., 2011; Tangen, 2005b) and organisational psychology literatures (J. P. Campbell & Wiernik, 2015; Koopmans et al., 2011; Mensah, 2015; Ramawickrama et al., 2017). Consequently, the two existing systematic reviews did not apply any theory-led inclusion criteria to determine which constructs qualify as indicators of work performance (e.g., productivity), and which constructs are productivity's antecedents and determinants (e.g., attention, job satisfaction, psychosocial job quality, motivation) (e.g., Blumberg & Pringle, 1982; Judge et al., 2001; Neal & Griffin, 1999). Here, we seek to overcome the limitations of the prior reviews, providing a more in-depth evaluation of the literature and a clear framework which can guide future research.

To have an overview of the various ways in which work and academic performance has been measured in MBP effectiveness research, we apply a theory-led framework of work performance developed by Koopmans and colleagues (2011). The result is a construct with four domains; 1) Task performance – the quantity and quality on tasks assigned to the individual; 2) Contextual performance – individual behaviour that supports the organisational environment (including its social and psychological aspects), such as proactivity, cooperating with others, and engagement in work; 3) Adaptive performance – the ability to adapt to changes in the organisation or in one's organisational role; 4) Counterproductive work behaviour – individual behaviours that are harmful to the organisation, such as absenteeism, presenteeism, misusing privileges, or disregard for safety. Having this framework allows us to identify which conceptualisations of work performance have not been captured: both reviews (Bartlett et al., 2019; Vonderlin et al., 2020) omitted indicators of adaptive work performance. Meanwhile, other variables investigated (such as work/life balance, burnout, and

job stress in Bartlett and colleagues (2019) and job satisfaction in Vonderlin and colleagues (2020)) may reflect employees' experiences related to work but not necessarily work performance in a theory-driven sense.

Expanding evaluation of the impact of MBPs to include all aspects of work performance is not only integral to advancing the scientific evidence base, but to ensuring best use of the resources organisations spend on training and well-being programmes. We therefore sought to provide a comprehensive systematic review and meta-analysis of the literature evaluating the effects of MBPs on all aspects work performance, pre-registering our methods, and using clearly defined and inclusive criteria to determine relevant outcomes. Rigorous evaluation of this literature is critical to ensuring that the millions of dollars invested in MBPs do indeed yield the intended benefits for the individual, and ensure economic and social value for investing organizations (Van Dam et al., 2017).

Our primary research question was: What is the effectiveness of mindfulness-based programmes (MBPs), compared with no intervention or comparator interventions for improving work performance? Our secondary aim was to explore the factors of MBP delivery and trial design that may influence the effects of MBPs. This could guide further research and real-world implementation of these programmes. In particular, we sought to evaluate a) What measures of individual performance are used in trials assessing the effects of MBPs? b) Do effect sizes differ between ratings made by external raters (e.g., metrics collected by the employer/higher education institution) and self-report ratings of individual performance? c) What is the effectiveness of shorter versus longer MBPs in work performance? d) How long does the effectiveness of MBPs in improving individual performance last? e) Does the effect of MBPs differ between delivery in organisational or community settings? f) What are the gaps in evidence and areas for further research?

Methods

This study is reported according to the Preferred Reporting Items for Systematic Reviews and Meta–Analyses (PRISMA) 2020 guidelines (M. J. Page et al., 2021).

Eligibility criteria

A study was eligible to be included if it:

- was published in a peer-reviewed journal,
- was published either in Catalan, English, Estonian, French, German, Italian, Portuguese, or Spanish,
- was a randomised controlled trial (RCT),
- evaluated a secular MBP (Crane et al., 2017) with a minimum duration of 4hrs, delivered synchronously or asynchronously, regardless of the medium (in-person, online, pre-recorded, such as an app or a book),
- included participants who were at least 17 years old, living in the community and who were not selected for having any particular health status (e.g., a health problem, health risk (substance abuse), or pregnancy),
- reported to have collected at least one outcome of interest (see below for details),
- compared an MBP with at least one control group that did not receive an eligible MBP as defined above (studies that only compared two eligible MBPs with no other arms were excluded).

We excluded residential programmes, for example, interventions retreat-based, as we deemed them to be unpractical to be used in organisational settings.

Information sources and search strategy

We searched the following databases for eligible records: ASSIA, EMBASE (via Ovid), ERIC (via EBSCOhost), MEDLINE (via Ovid), PsycINFO (via EBSCOhost), Scopus, Web of Science, and Cochrane Central Register of Controlled Trials (CENTRAL). The search was conducted on 30th March 2021, except for CENTRAL

(1st April 2021). We applied no restrictions to the results provided by the search terms (see Supplementary Material 1: PRISMA and Search terms) and searched for studies published since the inception of the database. Additionally, we searched for references to the two existing systematic reviews (Bartlett et al., 2019; Vonderlin et al., 2020). To minimise publication bias, we identified unpublished but completed trials preregistered in the World Health Organization (WHO) International Clinical Trials Registry Platform. We considered unpublished studies registered a minimum of 3 years before our search date as a potential indication of publication bias.

Selection and data extraction

Retrieved records were imported into EndNote and duplicates were removed. Records were then exported and fed into a machine learning programme to identify reports of randomised controlled trials (Marshall et al., 2018) using the sensitive setting recommended for systematic reviews. The output was uploaded to Rayyan (Ouzzani et al., 2016) where each title and abstract were assessed by two independent reviewers against inclusion criteria. The eligibility of full texts was evaluated when the titles and abstracts were rated relevant by at least one of the reviewers. Multiple reports of the same trial were combined. Data were extracted from the included full-text reports by two independent researchers using pre-piloted forms set up in Covidence (*Covidence Systematic Review Software*, n.d.) (see Supplementary Material 2: Data extraction forms). Disagreements at any stage were discussed and resolved within the review team.

Outcomes and comparisons

The primary outcome was task performance (e.g., work quality or quantity), as described in Koopmans and colleagues (2011). Secondary outcomes were measures that could be categorised into the three remaining domains of the

model, that is measures of contextual performance, adaptive performance, and counter-productive behaviours. The choice of the outcome to be extracted followed the hierarchy pre-specified in the protocol (Vainre et al., 2021). When choosing the outcomes, we based our decision on the intrinsic value, not the specific context in which it was collected. For example, writing creativity, a measure used in (Bellosta-Batalla et al., 2021), may have a specific value in certain professions (journalists, writers) while less pertinent in others (a student population). We ignored the context and extracted the outcome if it is deemed to contribute to work performance based on the Koopmans et al. model (2011). Outcomes deemed not to belong to any of the four outcome domains were excluded from the review. Outcome categorisation was done independently by the author who extracted data (MV). 100% of outcomes were second-rated by (MV). Where there were initial disagreements between the rater and (MV, kappa = .88), they were resolved by consulting with JG who was blind to the study outcomes.

Measures taken at post-programme were considered the primary end-point. Where time since the end of the MBP programme was reported, we considered data collected up to 4 weeks after post-programme as part of our primary time-point. Where authors described their outcome as collected post-programme, we considered it to belong to the primary end-point. Secondary time periods were a) outcomes collected between 5 to 24 weeks post-programme and b) outcomes collected at ≥25 weeks after the end of the programme. Where outcomes were measured more than once within these pre-specified time ranges, the longest follow up was used. Where reported, we also extracted the baseline outcome values for inclusion in analysis (as recommended by Clifton and Clifton (2019)). If there were multiple assessment time-points prior to randomisation, we used the time point closest to randomisation.

Control groups were categorised as follows to align with recent similar reviews (Dunning et al., 2018; Galante et al., 2020; Goyal et al., 2014); 1) Studies using either no contact or wait-list groups (i.e., passive controls); 2) Control interventions designed principally to take account of non-specific factors of the intervention

studied (i.e., placebo controls); 3) Control interventions with active ingredients specifically designed to drive change in one or more specified outcomes (i.e., active intervention controls).

Risk of bias and confidence in results

Two reviewers independently assessed the quality and risk of bias using the Risk of Bias tool (second version, RoB2) developed by the Cochrane Collaboration (Sterne et al., 2019). We made one deviation from the original RoB2: for the risk of the reported result, we rated the risk of bias as high when no information was available on any of the three items. The assumptions made during the rating are described in Supplementary Material 2. The decisions were recorded using pre-piloted forms set up in Covidence (*Covidence Systematic Review Software*, n.d.) (see Supplementary Material 2: Data extraction forms). Disagreements were resolved through discussion within the research team. Additionally, we collected information about allegiance and funding. Allegiance was considered a risk of bias when the authors of the paper designed the intervention or delivered it. Funding was considered a risk of bias where the organisation delivering the MBPs funded or conducted the study. The Grading of Recommendations Assessment, Development and Evaluation (GRADE) approach was used to assess overall quality of the synthesised evidence (Guyatt et al., 2008). We investigated publication bias with funnel plots.

Synthesis methods

We used the *meta* package (Balduzzi et al., 2019) in R v4.20 (R Core Team, 2022) run in RStudio v2021.09.0 to conduct pairwise random-effects meta-analyses within the four work performance domains and within the three comparator categories (passive, placebo, and active intervention controls). We set the α-level at 0.05 for primary as well as secondary outcomes. We treated the secondary

outcomes' meta-analyses as exploratory and thus did not correct the α-level for multiple comparisons. Where a multi-armed trial compared several eligible MBPs with a non-MBP control group, the MBPs intervention groups were combined (Higgins et al., 2022).

We used Hedges' *g* (also known as standardised mean difference, SMD) to index treatment effects as trials used different instruments to measure outcomes. Where baseline outcome values were reported, we calculated Hedges' *g* using the ANCOVA estimate (Mackenzie et al., 2006). The within-study baseline-endpoint correlations needed to calculate the ANCOVA estimate were derived from identified studies which reported change scores (Allexandre et al., 2016; Asuero et al., 2014; Nadler et al., 2020). Correlations for the task performance domain were calculated based on data available in Allexandre and colleagues (2016). The adaptive performance correlation was based on data published in (Asuero et al., 2014; Nadler et al., 2020). For the other 2 domains, we averaged correlations across the three studies for lack of a better alternative. For the 5-24 weeks post-intervention period, only one study published change scores (Allexandre et al., 2016). We used the correlation calculated based on their data in all four outcome domains.

Where baseline data were missing, we calculated the Hedges' *g* based on unadjusted final values. Ordinal and categorical data were transformed to Hedges' *g*s using approaches set out in the Cochrane Handbook (Higgins & Thomas, 2022). Subscales were combined by pooling their means and standard deviations. Where arm-specific sample sizes were missing, we divided the total sample size equally between the arms. The sample sizes for cluster-randomised controlled trials were adjusted for the meta-analysis (Higgins et al., 2022, p. 23) using an intraclass correlation (ICC) of 0.05 (Galante et al., 2020).

Estimation of heterogeneity was performed using the restricted maximum likelihood method. Confidence intervals for the overall mean were estimated with the modified Hartung, Knapp, Sidik and Jonkman method (Knapp & Hartung, 2003; Röver et al., 2015) using the *metagen* function in the meta package (Balduzzi et al.,

2019). The I^2 statistic and prediction intervals were also calculated with the *metagen* function.

We also conducted pre-specified analyses to investigate heterogeneity on the primary outcome. For continuous variables (duration of the intervention), we conducted meta-regression analyses. For categorical variables (delivery setting, reporter type) we used sub-group analyses. To investigate the effect of MBP duration on the outcome, we converted the duration into hours of guided content. The duration of self-help MBPs was calculated by multiplying the duration of guided meditation multiplied by that number of days a week the participants were asked to practice meditation and the number of weeks the intervention was intended to last. We excluded the duration of unguided mediation, as studies rarely quantified its duration. For face-to-face or other human-taught synchronously delivered programmes, we only included synchronously delivered sessions to estimate duration, that is, leaving out independent home practice.

Results

Study selection

The study selection is shown in Figure 2. In total, 47 trials were included in this review. We excluded one study (P. Shapiro et al., 2019) that meets the inclusion criteria, as the method stated that relevant measures were collected, however the results for that measure were not reported. We contacted four authors of studies where we were unable to retrieve reports to assess their eligibility based on the full text, and one author responded.

Study characteristics

Table 2 summarises the characteristics of the 47 studies included in the meta-analysis. Two were cluster-RCTs (Hwang et al., 2019; van Dijk et al., 2017). One (Glass et al., 2019) featured a cross-over design; thus, we extracted data up to the cross-over time point. The trials were carried out in 15 countries and published between 1998 and 2021. A total of 5041 (min = 18, max = 616, median = 75) participants took part in the trials. Most of them identified as female (71.5%). The mean age of the participants per study ranged from 18.0 to 63.6 years. Most studies recruited employees (k = 28) or students (k = 15) as participants. The remaining 4 studies were conducted in other settings (e.g., carers in a community setting). Passive control groups were used in 41 studies. The remaining six used either active non-specific control groups (n = 4) or active specific control groups (n = 2). One study was published in Spanish (Gómez-Odriozola et al., 2019), and the remaining were in English.

Most of the mindfulness-based programmes included in this review were delivered face-to-face (k = 43). The remaining four were delivered either completely or partially online or via an app. Two of these did not include human interaction (Nadler et al., 2020; Rich R.M. et al., 2021). The programmes' duration ranged from 4 to 16 hours (M = 7.77, SD = 1.88) and on average included 17.3 hrs (SD = 9.90) of guided meditation. Almost all studies (k = 45) encouraged participants to engage in meditation outside of guided sessions (this includes self-paced practice when delivered through an app or an online platform).

Performance was measured with 49 different measures in the four domains. Task performance was captured with 13 different measures. Contextual performance was measured with 16, adaptive performance with 14 and the remaining 6 indexed counterproductive work behaviour (see Table S 4).

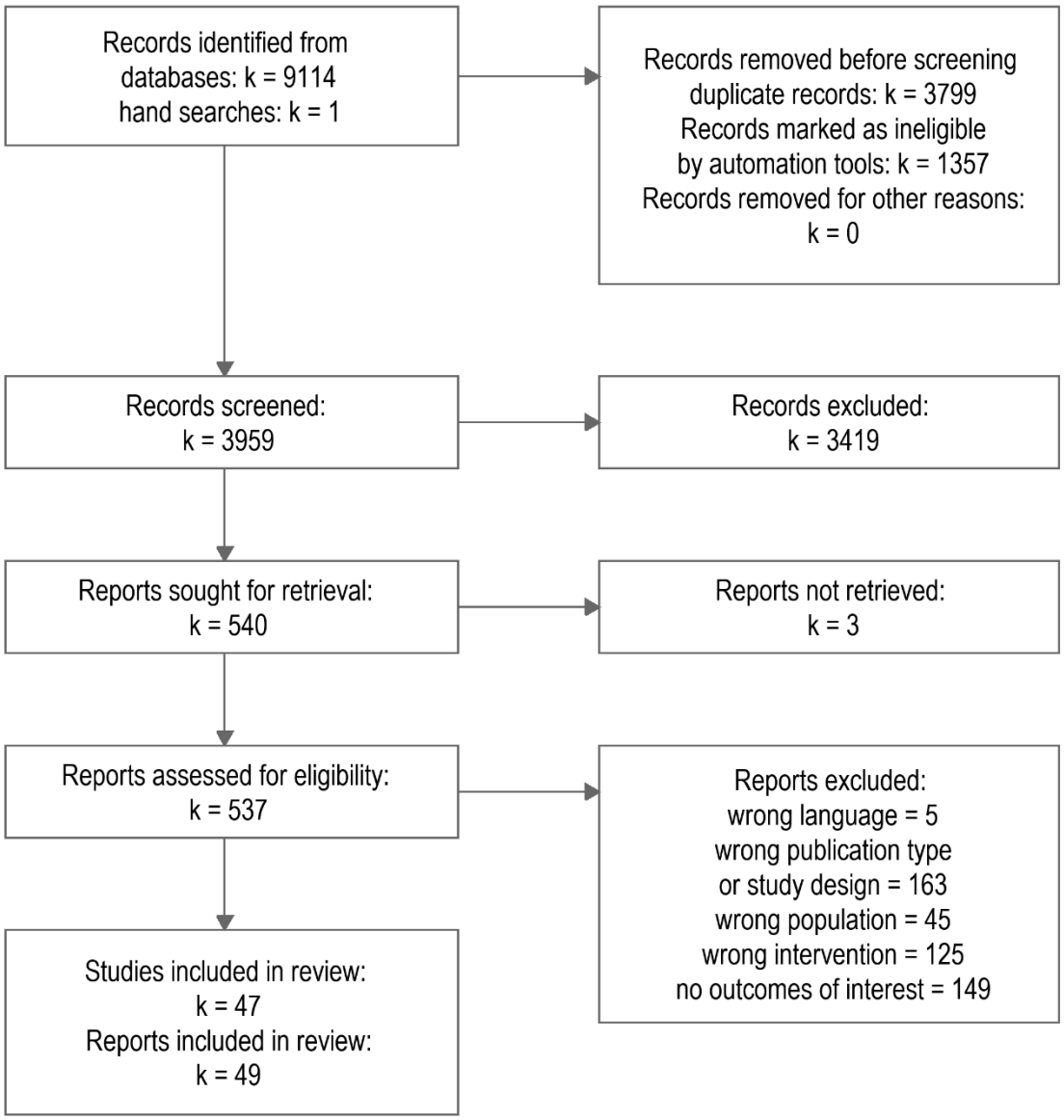

Figure 2. PRISMA 2020 Flowchart for study selection.

Table 2. Characteristics of the studies included

First author (year), country	Participants	n	Females %[a]	Age (years): M (SD)[a]	Control	Intervention (s)[b]	Duration weeks (hrs)[c]	Time periods[d]
Allexandre (2016), USA	Employees	161	83%	40 (12.6)	Waitlist control	a) Web-based stress management (WSM); b) WSM + group; c) WSM + clinical support[F2F,Online]	8 w (7.5 hrs)+	BL, <4 w, 5-24 w, >24 w
Asuero (2014), Spain	Employees	68	NR	48.1 (7.42)	Waitlist control	MBSR adaption[F2F]	8 w (28 hrs)+	BL, <4 w
Bartlett (2019), Australia	Employees	133	74%	NR	Self-help information resources	Mindfulness at Work Program[F2F]	5 w (7.5 hrs)+	BL, <4 w
Bellosta-Batalla (2021), Spain	Students	68	72%	23.6 (5.43)	Waitlist control	Mindfulness and Compassion[F2F]	8 w (16 hrs)+	BL, <4 w, 5-24 w
Benn (2012), USA	Employees	38	84%	45.6 (NR)	Waitlist control	SMART-in-Education[F2F]	5 w (36 hrs)+	BL, <4 w, 5-24 w
Braun (2020 a), Canada and USA	Employees	171	82%	45 (9.44)	Waitlist control	Mindfulness-Based Emotional Balance[F2F]	9 w (36 hrs)+	5-24 w, >24 w
Braun (2020 b), USA	Students	48	92%	25.96 (5.09)	Waitlist control	Mindfulness for Interdisciplinary Healthcare Professionals[F2F]	8 w (16 hrs)+	BL, <4 w
Brown (2016), USA	Other	38	84%	61.14 (10.41)	Alzheimer's Association-sponsored Social Support	MBSR adaption[F2F]	8 w (20 hrs)	BL, <4 w, 5-24 w
Can Gür (2020), Turkey	Students	123	70%	21.08 (2.18)	No intervention control	Mindfulness-based empathy training[F2F]	8 w (0 hrs)+	BL, <4 w
Chan (2021), Hong Kong	Students	50	60%	NR	Waitlist control	MBCT[F2F]	8 w (16 hrs)+	BL, 5-24 w
Christopher (2018), USA	Employees	61	11%	43.99 (6.07)	No intervention control	Mindfulness-Based Resilience Training[F2F]	8 w (22 hrs)+	BL, <4 w, 5-24 w
Daigle (2018), Canada	Employees	75	NR	46.21 (9.6)	Waitlist control	MBSR[F2F]	8 w (28 hrs)+	5-24 w
de Jong (2013), The Netherlands	Employees	60	43%	46.67 (8.11)	No intervention	MBSR[F2F]	8 w (20 hrs)+	BL, <4 w

Dvoráková (2017), USA	Students	109	66%	18.2 (0.4)	Waitlist control	Learning 2 breathe[F2F]	6 w (10.7 hrs)+	BL, <4 w
Erogul (2014), USA	Students	81	32%	23.44 (1.66)	Waitlist control	MBSR[F2F]	8 w (15 hrs)+	BL, <4 w, 5-24 w
Flook (2013), USA	Employees	18	89%	43.06 (9.87)	Waitlist control	MBSR adaption[F2F]	8 w (26 hrs)+	BL, <4 w
Galante (2018), UK	Students	616	63%	NR	Mental health support as usual	Mindfulness Skills for Students + mental health support as usual[F2F]	8 w (10.2 hrs)+	5-24 w
Glass (2019), USA	Students	57	77%	19.32 (1.25)	Waitlist control	Mindful Sport Performance Enhancement[F2F]	6 w (7.5 hrs)+	BL, <4 w, 5-24 w, >24 w
Gómez-Odriozola (2019), Spain	Students	114	81%	17.99 (0.69)	Waitlist control	Learning 2 breathe[F2F]	6 w (6 hrs)+	BL, <4 w
Hunsinger (2019), USA	Employees	61	10%	43.97 (6.03)	No intervention	MBSR adaption[F2F]	8 w (16 hrs)+	BL, <4 w, 5-24 w
Hwang (2019), Australia[C-RCT]	Employees	185	NR	43.08 (11.59)	Teaching-as-usual	Reconnected[F2F]	8 w (12 hrs)+	BL, <4 w, 5-24 w
Jennings (2013), USA	Employees	53	89%	36 (NR)	Waitlist control	CARE[F2F]	5 w (36 hrs)+	BL, <4 w
Jennings (2017), USA	Employees	224	93%	NR	Waitlist control	CARE[F2F]	16 w (30 hrs)+	BL, 5-24 w
Klatt (2015), USA	Employees	34	NR	NR	Waitlist control	Mindfulness in Motion[F2F]	8 w (8 hrs)+	BL, <4 w
Klatt (2017), Denmark	Employees	81	NR	42.91 (9.29)	Waitlist control	Mindfulness in Motion[F2F]	8 w (8 hrs)+	BL, <4 w, 5-24 w
Kor (2019), Hong Kong	Other	36	83%	57.1 (10.6)	An education programme on dementia care	MBCT adaption[F2F]	10 w (14 hrs)+	BL, <4 w, 5-24 w
Kor (2021), Hong Kong	Other	113	61%	61.7 (10.5)	Usual family care + a brief education session on dementia care	MBCT adaption[F2F]	10 w (14 hrs)+	BL, <4 w, 5-24 w
Lin (2019), China	Employees	110	76%	31.53 (6.92)	Waitlist control	Based on principles of MBSR and MBCT[F2F]	8 w (16 hrs)+	BL, <4 w, 5-24 w

Nadler (2020), USA	Employees	275	27%	NR	Waitlist control	MBSR adaption[Online]	8 w (1.6 hrs)+	
Orosa-Duarte (2021), Spain	Students	154	85%	23 (4.16)	Waitlist control	a) Mindfulness-Based Emotion Regulation "Going Home"; b) IMBP group[F2F,App]	8 w (11.7 hrs)+	BL, <4 w
Pang (2019), Switzerland	Employees	63	69%	44.2 (10)	Waitlist control	a) Mindfulness-Based Strengths Practice; b) MBSR[F2F]	8 w (16 hrs)+	BL, <4 w, 5-24 w
Perez-Blasco (2016), Spain	Other	45	67%	63.56 (4.1)	Waitlist control	Mix of MBCT and Mindful Self-Compassion[F2F]	10 w (20 hrs)+	BL, <4 w
Phang (2015), Malaysia	Students	75	76%	21.04 (1.13)	Waitlist control	Mindful-gym[F2F]	5 w (10 hrs)+	BL, <4 w, 5-24 w
Pipe (2009), USA	Employees	33	94%	49.79 (6.75)	Attention control	MBSR adaption[F2F]	4 w (10 hrs)+	BL, <4 w, >24 w
Rich (2021), UK	Employees	125	70%	NR	Waitlist control	Headspace app[App]	6.4 w (32.1 hrs)+	BL, <4 w
Roeser (2013), USA and Canada	Employees	113	88%	46.9 (9.2)	Waitlist control	Mindfulness Training Programme[F2F]	8 w (36 hrs)+	BL, 5-24 w, >24 w
Sampl (2017), Austria	Students	109	75%	22.28 (4.55)	Waitlist control	Mindfulness-based self-leadership training[F2F]	10 w (20 hrs)+	BL, <4 w
Schroeder (2018), USA	Employees	33	73%	42.76 (8.43)	Waitlist control	Mindful Medicine Curriculum[F2F]	4 w (15 hrs)	BL, <4 w, 5-24 w
Shapiro (1998), USA	Students	78	53%	NR	Waitlist control	Stress Reduction and Relaxation[F2F]	7 w (17.5 hrs)+	BL, <4 w
Shapiro (2011), USA	Students	32	81%	18.73 (1.29)	Waitlist control	a) MBSR; b) Easwaran Eight-Point Program[F2F]	8 w (0 hrs)+	BL, <4 w, 5-24 w, >24 w
Steinberg (2017), USA	Employees	32	NR	39.8 (NR)	Waitlist control	Mindfulness + light yoga practices with music[F2F]	8 w (8 hrs)+	BL, <4 w
Strauss (2021), UK	Employees	234	83%	43.95 (10.4)	Waitlist control	MBCT adaption[F2F]	8 w (16 hrs)+	BL, <4 w
Taylor (2016), Canada	Employees	59	90%	NR	Waitlist control	Mindfulness meditation[F2F]	9 w (36 hrs)+	BL, <4 w, 5-24 w

Valley (2017), USA	Employees	23	87%	NR	Waitlist control	MBSR[F2F]	8 w (27 hrs)+	BL, <4 w, >24 w
van Berkel (2014), van Dongen (2016), The Netherlands	Employees	257	NR	45.55 (9.49)	E-mail with a link to a webpage on health promotion	Vitality in Practice[F2F]	8 w (12 hrs)+	BL, 5-24 w, >24 w
van Dijk (2017), The Netherlands[C-RCT]	Students	167	78%	23.5 (1.85)	Clinical clerkships as usual	MBSR adaption[F2F]	8 w (16 hrs)+	<4 w, 5-24 w, >24 w
Verweij (2018), The Netherlands	Employees	148	88%	31.2 (4.6)	Waitlist control	MBSR[F2F]	8 w (26 hrs)+	BL, 5-24 w

Notes. [a]NR = not reported, [b]CARE = Cultivating Awareness and Resilience in Education, MBCT = Mindfulness-Based Cognitive Therapy, MBSR = Mindfulness-Based Stress Reduction, [c]Intervention duration: The symbol '+' denotes additional homework, [d]Timepoints: BL = Baseline, <4 wks = up to 4 weeks post-intervention, 5-24 wks = 5-24 weeks post-intervention, >24 wks = more than 24 weeks post-intervention, [C-RCT]Study design: Cluster-randomised trial, [F2F]Group-based, face-to-face, [Online]Individual, online, asynchronously, [App]Individual, app-based, asynchronously

Risk of bias in studies

All studies, except one (Galante et al., 2018), received a high risk of bias rating (Figure 3, details in Supplementary Material 3: Results Table S 2). Concerns related to risk of bias due to the randomisation process (87% studies) arose either because it was not clear whether allocation sequence was concealed or because the randomisation method excluded the possibility of concealment. In the effect of assignment to the intervention domain, elevated risk of bias ratings (98% studies) derived from two causes. First, the use of per-protocol rather than intention-to-treat analyses or lack of transparency around the approach used, or second, the nature of the intervention (a behavioural programme, rather than a pharmaceutical prescription) makes it difficult to conceal the treatment from the staff delivering it. Furthermore, the nature of the interventions prohibited blinding the participants. Elevated risk of bias ratings due to missing outcome data (85% studies) were influenced by high attrition rates coupled with lack of sensitivity analyses. The use of self-report outcome measures or the failure to mention whether observers were blinded were frequently the causes of high risk of bias ratings (89% studies) in the measurement of the outcome domain. Most of these

Figure 3. Summary of the risk of bias ratings

biases are likely to lead to inflation in effect sizes. Most studies (98%) did not have a pre-registered analysis plan available and thus received a high risk of bias rating in selection of the reported result.

We found some degree of allegiance to the MBP in 22 (47%) studies. That is, the authors had developed or delivered the intervention. The source of funding was a source of concern in 14 (30%) studies. In most cases, it was because the source of funding was not declared. In one study, the authors were affiliated with the organisation that developed the intervention (Allexandre et al., 2016). In total, 17 studies received a low rating for vested interest due to allegiance and funding.

Effects of MBPs on performance

Summary statistics for each study's outcome measures of interest are presented in Supplementary Material 3: Results, Table S 6-Table S 9) for each pre-specified time period.

Primary outcome: Task performance up to 4 weeks post-intervention

As none of the studies eligible for the main outcome meta-analysis reported active control groups, we were only able to meta-analyse studies with passive control groups. One study (Glass et al., 2019) did not report outcome measures for the control group and therefore had to be excluded from the analysis. We included 7 studies in the primary outcome analysis. Overall, MBPs did not significantly improve task performance, although the medium effect size did favour MBPs relative to passive control (Hedges' g = 0.52, 95% CI -0.03 to 1.07, p = 0.059, 95% PI -0.73 to 1.77, see Figure 4 and Table S 10).

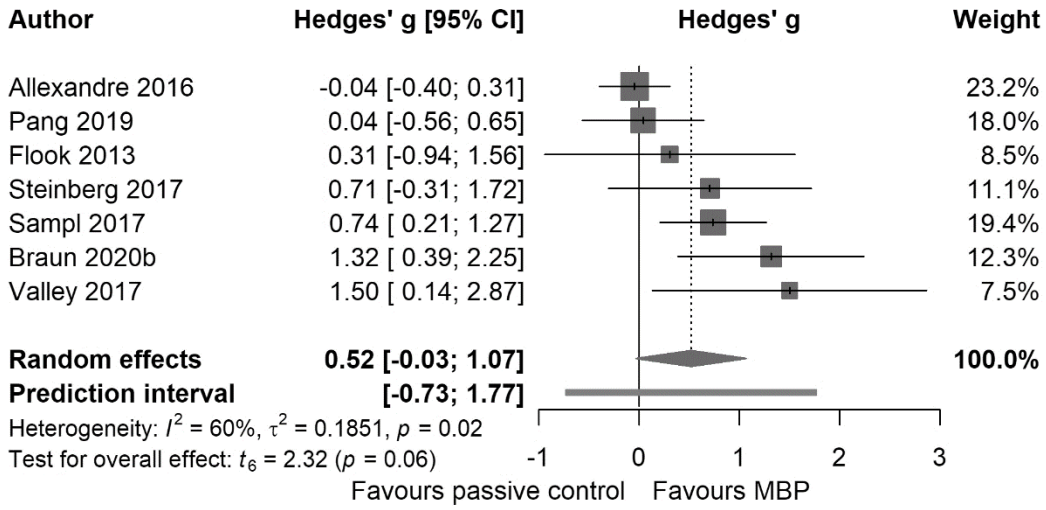

Figure 4. Task performance up to 4 weeks post-intervention compared to passive control groups

The heterogeneity (I^2) was 60% in the primary outcome analysis. A moderator analysis on the primary outcome (Cochran's Q-test χ^2 (1) = 3.88, p > 0.05) suggested that interventions offered to students (k = 2) had a larger effect size (Hedges' g = 0.9, 95% CI -2.4 to 4.2, I2 = 11.6%) compared to those offered to employees (k = 5) (Hedges' g = 0.23, 95% CI -0.39 to 0.84, I^2 = 35.51%). Hours of guided content (regression coefficient = 0.04, [95% CI -0.04 to 0.12], p = 0.23, see Table S 11) and report type (either self-reported or reported by others, including automatically collected) (Cochran's Q-test χ^2 (1) = 3.19, p = 0.07, Table S 13) did not significantly predict task performance. We were unable to test whether the delivery medium had any moderating effects as all but one study reported that MBPs were administered face-to-face. We also could not run the pre-registered sensitivity analysis where we planned to exclude studies with high risk of bias as all studies included in the primary outcome analysis were rated as having high risk of bias.

Secondary outcomes

Secondary outcomes were other time periods for the primary outcomes of interest and other ways of operationalising work performance at different time periods. Table 3 summarises the findings of the meta-analyses (forest plots in Supplementary Material 3: Results). Only studies using passive control groups were meta-analysed due to the low number of studies with active control groups (max = 2 per domain and time period).

Table 3. Impacts of MBPs on secondary outcomes, relative to passive control groups

Domain	Time	k	Hedges' g[a]	I^2	95% CI	Pred. int[b]
Task performance	5-24 w	6	0.05	0%	-0.15 to 0.26	-0.17 to 0.27
Contextual performance	<4w	11	0.33*	21%	0.09 to 0.57	-0.03 to 0.69
	5-24 w	6	0.28	28%	-0.06 to 0.62	-0.33 to 0.88
Adaptive performance	<4w	17	0.32***	0%	0.17 to 0.47	0.17 to 0.47
	5-24 w	8	0.4*	0%	0.1 to 0.69	-0.09 to 0.88
Counterproductive work behaviour	<4w	3	0.14	0%	-0.54 to 0.82	-1.86 to 2.14

[a]*p < 0.05, **p < 0.01, ***p < 0.001;[b]Prediction interval

Reporting biases and confidence in the evidence

We investigated selective under-reporting or non-reporting of results. We found no evidence for publication bias on the primary outcome (see Supplementary Material 3: Results for funnel plots (Figure S 1) and further detail), although this analysis only contained seven studies and thus should be interpreted with caution (M. Page et al., 2022). We also found no evidence of publication bias on secondary outcomes (see Figure S 2-Figure S 3).

Searching trial registries, we found 23 potentially eligible trials publicly registered over three years ago that had not made their results available (see Table S 3 for

details). We were not able to establish whether all of these 23 trials were eligible due to lack of information provided at pre-registration. Second, although this systematic review included 47 studies, we could not meta-analyse eight of these because the necessary data were not reported (Daigle et al., 2018; Glass et al., 2019; Klatt et al., 2015, 2017; P. Shapiro et al., 2019; Steinberg et al., 2017; Taylor et al., 2016; Valley & Stallones, 2017). See Table S 5 in the for details.

The overall certainty of evidence assessed with GRADE was very low for our primary outcome. For our secondary outcomes, the quality was very low except for three cases where it was low: (1) contextual performance up to 4 weeks post-intervention, (2) adaptive performance up to 4 weeks post-intervention and (3) adaptive performance at 5-24 weeks post-intervention. The consistent reasons for low confidence were high risk of bias, high non-reporting bias, imprecision, and inconsistency (see Table S 1).

Discussion

A rigorous evaluation of the effects of mindfulness-based programmes (MBPs) on real-world outcomes is essential to ensure responsible and effective implementation. This systematic review and meta-analysis synthesised evidence from randomised controlled trials (RCTs) which evaluated the impact of MBPs on a structured, evidence-based definition of work performance. We found no significant evidence that offering MBPs improves task performance (the quality and quantity of work) up to 4 weeks after the intervention ended. Compared to no intervention, we observed a moderate but non-significant effect favouring MBPs, with a high degree of between-study heterogeneity for this effect. The longer-term effect of MBPs on task performance at 4 weeks to 6 months was negligible. The subgroup analyses indicate some conditions under which MBPs may offer greater benefits to task performance. At post-intervention, the MBPs for the students led to larger effect sizes, compared to those offered for the employees. The other subgroup analyses yielded no statistically significant findings. Overall,

evidence to support the use of MBPs to improve task performance was limited. Concerningly, the overall quality of research in this area is low.

Effects on our secondary outcomes were varied. We found no evidence of an effect of MBPs, compared to passive control groups, on counterproductive work behaviour (e.g., absenteeism, presenteeism, discrimination) at any period investigated. Small, significant effect sizes in favour of MBPs were however observed on adaptive and contextual performance at post-programme, relative to passive control groups. This suggests that MBPs may yield some benefit for improving the effort that individuals put into creating a better professional or academic environment, along with how well they adapt to change. This supports previous research (Donald et al., 2019) in suggesting that MBPs may improve the effort that individuals put into creating a better professional or academic environment, along with how well they adapt to change. However, this effect may depend on organisational culture. Where conflicts of interest and issues with relative power are prevalent, MBPs may have a detrimental effect on prosocial behaviour (Columbus & Molho, 2022; Poulin et al., 2021).

We lack a good understanding of the conditions in which MBPs could work and whether there are context-specific effects on any of the work performance dimensions. Distinguishing these requires careful study design, particularly since some variables, like organisational culture, may be difficult to measure. Furthermore, more effort should be introduced into separating MBPs' specific effects on work performance from general mental health benefits they bring. If MBPs improve work performance work mainly (or perhaps only) through the mental health pathway, then it may be more practical to prefer a range of approaches to supporting well-being at work (National Institute for Health and Clinical Excellence, 2022) which would also boost work performance.

Further refinement and empirical investigation of MBPs is impaired by poor understanding of the mechanisms through which MBPs could lead to improved work performance. There are suggestions that self-regulatory mechanisms are at play (Glomb et al., 2011b) or that MBPs may lead to increase in motivation

(Hafenbrack et al., 2022) or engagement in the task at hand (Cheung et al., 2020). More comprehensive understanding of these mechanisms will guide the selection of appropriate workplace-based outcome measures. This review was not designed to explore these mechanisms, and further exploration of different work performance domains may shed some light as to skills and behaviours are likely to be affected by MBPs.

It is important to emphasise that the effects we found in the meta-analysis were relative to passive control groups (i.e., relative to no intervention), as there were not enough eligible studies to meta-analyse the effect of MBPs on work performance compared to nonspecific or specific action in any of the four domains. Four studies included in the systematic review (but not meta-analysis) used active control groups with content that is intended to improve work performance (K. W. Brown et al., 2016; Kor et al., 2019, 2021; Pipe et al., 2009). The effect sizes ranged from negligible (caring efficacy in nurses, Pipe et al., 2009) to small (resilience in caregivers, Kor et al., 2021). This suggests that there is currently little evidence that MBPs' effect on work performance outperforms other individual interventions commonly offered by organisations (e.g., time/stress management seminars, gym passes, free fruit) or organisational changes, such as improving workload and work relations.

The quality of the evidence is too low to suggest implications for policy and practice, except that caution needs to be exercised when implementing MBPs in higher-education and occupational environments if the main aim is to improve work performance (For meta-analysis of the significant effects on MBPs on mental health, see Galante and colleagues (2021)). In addition to risking high participant burden and organizational cost for a lack of effect, we need to be mindful of potentially negative consequences associated with MBPs (Cebolla et al., 2017; Hafenbrack et al., 2022).

Our meta-analysis has limitations. First, the small number of studies per outcome limits the conclusions that can be drawn from this study. Second, the quality of the studies was low, so we decided against including grey literature. We

acknowledge this decision might have heightened the effects of publication bias in the meta-analysis. Finally, in using a clear definition, we excluded some job-related outcomes (job satisfaction, work motivation, job-related stress and burn-out) that did not fit to the Koopmans et al (Koopmans et al., 2011) model of work performance but are related to work. This meta-analysis thus does not capture MBPs effects on all aspects of work- and study-related experiences.

Overall, this review emphasises the need for more rigorous research in this area. Further evaluation of controlled intervention effects is needed to determine any impact of MBPs relative to other lighter-touch and cheaper interventions commonly used by organisations. To advance the existing evidence base for the impact of MBPs on work performance, study designs and practices need to be advanced in two key areas. First, there is a strong need for improving open-science practices. We found 23 registered studies that had not published their results at least 3 years after initial trial registration. Some of them may have been dissertations or other study reports not published in peer-reviewed journals and were thus not indexed in the databases we searched. Where results were published, all studies, except one, were rated to have a high overall risk of bias. Some factors contributing to risk of bias are difficult to avoid, for example blinding participants or facilitators of behavioural programmes. However, upgrading randomisation and allocation concealment processes and pre-registration of outcomes and planned analyses are free, simple, and much-needed steps. Second, tightening the operationalisation of work performance in MBP effectiveness research will also benefit the field. In this meta-analysis, 49 different instruments were used to index work performance across four domains. To our knowledge, the current paper is the first attempt to systematise the way in which work performance is measured in MBP research. Researchers can use our findings to make better decisions on the outcome measures they select, to enhance construct as well as ecological validity of their outcomes. Similarly, further use of objective (e.g., blinded raters) rather than subjective measures of work performance is needed. Addressing these limitations in the research, and comparing the effects of MBPs against active control groups, are important next

steps to guiding the effective and responsible implementation of MBPs in professional and academic settings.

Other information

Registration and protocol

The data collected and the code to analyse this meta-analysis are available on OSF: https://osf.io/zr7yk/?view_only=3b18ac9d9711403e85260c68297485cb. This study was pre-registered at PROSPERO (Vainre et al., 2021).

Amendments to the protocol

Compared to the original protocol, we decided to exclude theses and outcomes measuring burn-out and work-related stress. These decisions were made during full-text screening due to limited capacity and in order to limit the scope of this review. For the primary outcome's sub-group analyses, we planned to analyse reporter type categorised either as self-reported, reported by someone else, or routinely collected. Due to the low number of eligible studies, we grouped non-self-reported outcomes together and thus compared self-report to non-self-report.

Funding

This study is supported by MRC Cognition and Brain Sciences Unit at the University of Cambridge. MV is supported by Kristjan Jaak degree studies abroad scholarship by Estonia's Education and Youth Board. JG is supported by the National Institute for Health and Care Research (NIHR) Applied Research

Collaboration East of England (NIHR ARC EoE) at Cambridge and Peterborough NHS Foundation Trust. The views expressed are those of the author and not necessarily those of the NIHR or the Department of Health and Social Care. CH by the Economic and Social Research Council (ES/R010781/1) and Australian Research Council (DE20010004) and TD by the UK Medical Research Council (Grant reference: SUAG/043 G101400) and Wellcome (Grant reference: 104908/Z/14/Z, 107496/Z/15/Z).

Competing interests

Authors declare no conflicts of interest.

Acknowledgements

Many thanks to Claire Friedrich for supporting review of articles published in German. We would also like to thank Kevin Symonds for doing detective work for finding papers that seemed inaccessible and the authors of published papers whose responses to our queries helped to improve the quality of this review.

Afterword

The systematic review and meta-analysis was designed to answer the following research questions:

Research Question 1: How is individual work performance operationalised in trials assessing the effects of mindfulness-based programmes (MBPs)?

Research Question 2: What is the effectiveness of MBPs for improving work performance based on the current literature?

As mentioned in the Foreword, this systematic review is currently being updated. At the time of the doctoral thesis submission, we have just started data extraction. As there are a considerable number of studies published between the first literature search and the second, I hope to be able to complete the updated analysis in late 2023 or early 2024 and update the manuscript thereafter.

As they stand now, the results offer partial answers to the research questions posed. In response to Research Question 1, the results of the review showcase the array of operationalisations of work performance, and subsequently, the large number of different measures used to index work performance. Such diversity limits the ability to draw generalised conclusions regarding the effects of MBPs on work performance. In other words, the results do not allow me to unequivocally answer Research Question 2. Overall, while some general domains of work performance may improve as a result of completing an MBP (notably contextual performance and adaptive performance), there is little evidence to suggest that MBPs have effects on specific constructs of work performance, such as decision-making, creativity or teamwork. Additionally, the quality of the evidence base is low due to high risk of bias, further limiting our confidence in these conclusions.

The results of the systematic review highlight that the standard conclusion of any scientific enquiry – 'more research is needed' – is apt here. More research, that is, running further trials to investigate various specific constructs of work performance that could be then pooled in a meta-analysis, is costly, especially if

we wanted to cover all possible aspects of work performance. With this in mind, my next step sought to isolate one aspect of individual work performance that is considered most pertinent among researchers as well as practitioners – task performance.

When determining the effects of a behavioural intervention, the processes which underpin treatment change should also be considered. This is an important step in determining how and why interventions work. Furthermore, a better understanding of the mechanisms through which MBPs have an effect on work performance may help to concentrate efforts on the general domains or specific constructs of work performance that are more likely to be effected. For example, if improved cognitive control is indeed an important mechanism of MBPs, as suggested (see Chapter 1), then it is less likely that MBPs will reduce counterproductive work behaviours such as theft, relative to task performance. In other words, knowing the mechanisms of action may help to identify which specific work performance constructs are more likely to be improved by MBPs.

The next two chapters will describe a randomised controlled trial to investigate the effect of MBP on work performance and the mechanisms underpinning that effect.

Chapter 3

Protocol for the Work Engagement and Well-being Study (SWELL): A randomised controlled feasibility trial evaluating the effects of mindfulness versus light physical exercise at work

Foreword

As described in the Introduction (Chapter 1), a key focus of the work in this thesis was to investigate whether, and via which specific mechanisms, a mindfulness-based programme (MBP) could improve work performance. Several procedural uncertainties suggested that a fully powered study was premature and that a feasibility trial was first needed (Bowen et al., 2009; Hallingberg et al., 2018; Skivington et al., 2018). The present chapter includes the protocol for this feasibility randomised controlled trial (RCT). In this foreword, I will summarise the procedural uncertainties surrounding this approach in more detail than was possible in this published protocol paper.

First, there was considerable uncertainty regarding the most appropriate measure of work performance. The existing literature measured an array of constructs from work engagement (i.e., contextual performance or behaviour that contributes to the organisation, as in van Berkel and colleagues (2014)) and absence from work (for example, taking sick leave, as in Bartlett and colleagues (2017)) to bespoke measures of task performance tailored to the specific job role under investigation — for example, call centre workers (Allexandre et al., 2016).

Broadly speaking, for the trial we had the choice between two approaches to deciding how to operationalise work performance. One was to wait for the meta-analysis (Chapter 2) to pool results to determine the domain and/or measure with the highest chances of detecting an effect. This was not feasible given the tight timeframe of completing an RCT during a post-graduate degree course. The other approach was to rely on what people think when they talk about work performance, i.e., an operationalisation based on *vox populi*. Koopmans and colleagues (Koopmans et al., 2014b) found that task performance was rated to have the most weight in describing individual work performance among a sample of researchers, managers, occupational health and human resources specialists (N > 600), a result replicated in a different sample (Abbasi et al., 2022), although the replication was published after we started data collection (Vainre et al., 2020). Based on the original results, we chose to focus on task performance, given the importance of task performance in operationalising "work performance" among specialists (Koopmans et al., 2014b) and as it seemed the most likely domain to be influenced by the mechanism of interest – cognitive control – which was suggested to lead to better focus on the task at hand (see Chapter 1).

My initial interest was to measure a real-life outcome to index task performance. For example, Allexandre and colleagues (2016) intended to measure the number of calls made and dollars collected by the participants — call centre workers. Such outcomes could potentially be valuable to the employer – this, I hoped, would increase the buy-in and help with recruitment to the trial. However, the same study (Allexandre et al., 2016) also highlighted potential issues with such a measure

as the main outcome: the employer in the trial changed the way the outcome was measured halfway through the RCT. The authors then had to use the company's 'global measure of work performance', a score of 1-5. While still a real-life metric, there is virtually no information about its generalisability even to other job roles within the same company. Indeed, tailoring the outcome to a specific job role poses a weighty problem for generalisability. An employer may be interested in offering an MBP to all its employees, not only to those with a specific job description (e.g., only nurses, not doctors; or only programmers, not managers or technicians).

The better strategy then seemed to be to select an outcome measure that is non-specific to a job role. This would allow recruitment of several employers and would mean future studies could also use the same measure. To retain the relevance to the employers in the trial, I offered to incorporate, as a secondary outcome, a performance outcome measure that the employer already used. Identifying such a measure proved impossible however, as none of the approached employers used such indicators. At best, they asked employees to give feedback on various initiatives.

While picking a more generic measure for task performance was a good choice for the reasons outline above, it also led to a potential problem of heterogeneity. In practical terms, it meant an increase in the sample size and careful choice of an outcome that would be appropriate across various occupational roles. One of the few real-life outcomes that fits that description is number of sick leave days taken. Yet organisational culture, not to mention legislative regulations, significantly influence whether people use sick leave, making pan-organisational and international comparisons problematic. Second, accessing the data through employers would have been complicated, and using self-reported sick days did not seem sufficiently reliable. Finally, and most importantly, my main interest was to measure whether MBPs improve work performance beyond improvements in

mental health. The plan was to recruit generally healthy people[4], and to compare MBP effects against a control intervention which was also expected to improve mental health, in order to determine whether MBPs could improve work performance beyond improvements in mental health. Sick leave would not have been a sensitive enough measure.

With my supervisory team we thus decided to use a standardised measure to index work performance. There is no generic measure for work performance or task performance (Koopmans et al., 2014b). I identified four potential self-reported work performance outcomes that, on face validity, could have captured constructs of interest:

1. The WHO Health Performance Questionnaire (HPQ) (Kessler et al., 2003, 2004) – measures presenteeism or showing up at work when feeling unfit to work;
2. The Work Limitations Questionnaire (WLQ) (Lerner et al., 2001) – designed to measure working limitation posed by chronic illnesses and impairments;
3. The Individual Work Performance Questionnaire (Koopmans et al., 2014a);
4. The Work Role Functioning Questionnaire (Abma et al., 2018).

The HPQ and WLQ were designed to measure work performance in people who are unwell. These scales thus have been found to suffer from ceiling effects, have poor criterion validity and showed poor correlations with employers' work performance measures (Gardner et al., 2016). Given the planned Work Engagement and Well-being study (SWELL) did not recruit people based on their health status, we decided against using these scales. The Individual Work Performance Questionnaire (Koopmans et al., 2014a) was designed to capture all aspects of work performance, not just task performance. Of the 18 items, only a few seemed to be influenced by the ability to focus on the task at hand (e.g., "I

[4] We acknowledged that the employees interested in participating may feel the need to receive some support regarding their well-being.

was able to separate main issues from side issues at work."). Most items seemed irrelevant for the pathway of interest (e.g., "I spoke with colleagues about the negative aspects of my work.", "I kept looking for new challenges in my job.", "I actively participated in work meetings.").

After some consideration, we settled on the Work Role Functioning Questionnaire (WRFQ). It was developed based on the WLQ and was intended to capture limitations of work performance due to health problems, but looking at its items, it seemed likely that it could detect changes in non-clinical populations (see Supplementary Material 2: Methods for the modified questionnaire). The WRFQ captures a wide range of work performance aspects making it universal enough to be used across different employers and employees. The items of the WRFQ also seemed to have some face validity in assessing constructs that are likely to be affected by a MBP, if MBPs improve the ability to focus. For example, the ability to "get going easily at the beginning of the workday" could be improved by cognitive control and may be improved also in non-clinical populations. Unlike the WLQ, the WRFQ was available for free. However, the WRFQ had not been used to measure the effects of an intervention (based on my personal communication with the corresponding author, Dr Abma), so we lacked information on its sensitivity to change.

The next dilemma that the feasibility trial was designed to solve was estimating the likely between-intervention effect size, to understand the need for, and to facilitate power calculations for a later-stage RCT. As described in the Introduction (Chapter 1) and as supported by the systematic review (Chapter 2), there is little information available on the probable effect sizes when using active control groups, and particularly when using control groups that are designed to rule out non-specific effects. The systematic review results later (again after the data collection for the RCT had started) highlighted this constraint as it identified just six studies with an active control group (Bartlett et al., 2017; K. W. Brown et al., 2016; Kor et al., 2019, 2021; Pipe et al., 2009; van Berkel et al., 2014). Three of them were aimed at carers for people with a health condition where the control group were offered information on the condition (K. W. Brown et al., 2016; Kor et al., 2019,

2021). The control conditions provided information about the health condition and so it is difficult to interpret the small effect sizes in the context of workplace interventions. Two of the six provided information on well-being resources as their control group intervention (Bartlett et al., 2017; van Berkel et al., 2014) which allowed the triallists to control for the effect of resource availability. For the purposes of SWELL study, it offered us little information for sample size calculation as we would have expected the between-group effect size to be smaller by an unknown amount given both arms were to receive a full behavioural programme. The third study (Pipe et al., 2009) did use a behavioural programme as their control group (a "structured educational series" on stress and leadership strategies) but they did not report their effect size. Furthermore, most trials that had been completed when we were planning our study, used face-to-face delivered programmes (Bartlett et al., 2019). The systematic review of MBPs effects on work performance (Chapter 2) identified just three trials testing online or app interventions (Allexandre et al., 2016; Nadler et al., 2020; Rich et al., 2021), and all of them compared the MBP with a passive control group. This could have heightened attrition in the control group compared with an alternative programme offered in an active control group study. Instead of relying past known empirical findings, we could have simply set a minimal effect size of interest (Anvari & Lakens, 2021; Lakens, 2022). Yet, the effect size would have been a guess at best since we lacked data on the sensitivity of WRFQ and of the meaning of an effect size considering the circumstances of the trial (Anvari et al., 2023).

The final procedural uncertainty I would like to describe in more detail is regarding the cognitive control measures. The mechanism mediating the effect of MBPs on WRFQ was hypothesised to be cognitive control over negatively-valenced mental material (see Chapter 1). Cognitive control is usually measured using behavioural cognitive tasks. These tasks are usually administered using a computer which allows precise control over the way stimuli are presented and recording of outcomes, usually participants' reaction times, responses, errors, and other

interactions they have with the tasks. Such tasks are used widely, so their protocols (called *paradigms*) are well-established.

Such behavioural cognitive tasks are mostly used to test hypotheses about how cognition works. For example, the role of affective information on working memory capacity (Schweizer et al., 2019) or on the chance of a certain memory error (boundary extension) (Patel et al., 2023). As described in the Introduction (Chapter 1), and revisited briefly in the next two chapters, the bulk of research investigating whether MBPs influence the performance on cognitive tasks uses emotionally neutral stimuli. MBPs may help to improve work performance in situations where people need to maintain goal-relevant information while ignoring distractive *negative* information (e.g., Schweizer et al., 2019). However, there is no information on whether an MBP could help people to move away from negative cognitive material to effectively perform the task at hand, and whether these effects, if present, would transfer to real life situations at work or while studying. We therefore decided to use two cognitive tasks, an affective stop-signal task (Lee, 2020; Verbruggen et al., 2019), as a measure for inhibition, and a learning task (Cools et al., 2002) to which I added the affective component (Vainre, 2021/2021a) to measure participants' ability to track dynamic changes in the environment.

To summarise, we faced many unknowns about whether the instruments measuring the outcome and what kind of effect size to expect. We concluded that we needed to run a feasibility trial to refine these procedural uncertainties and estimate whether a fully powered study was feasible and warranted.

The research questions to be addressed in the next two chapters are:

Research Question 3: Could improved cognitive control and/or enhanced mental health be potential mechanisms underlying the effect of MBPs on work performance?

Research Question 4: Is it acceptable and feasible to run an RCT to investigate the effect of MBPs on work performance?

Research Question 5: What is the effect size of MBPs on work performance when compared to an active control group?

The remainder of Chapter 3 features the published version of the protocol of the SWELL trial. It is published in BMJ Open as Vainre, M., Galante, J., Watson, P., Dalgleish, T., & Hitchcock, C. (2022). Protocol for the Work Engagement and Well-being Study (SWELL): A randomised controlled feasibility trial evaluating the effects of mindfulness versus light physical exercise at work. BMJ Open, 12 (4), e050951.

Abstract

Introduction

Mental ill health is a major cause of disability. Workplaces are attractive for preventative interventions since most adults work; meanwhile, employers are interested in improving employees' well-being and productivity. Mindfulness-based programmes are increasingly popular in occupational settings. However, there is inconsistent evidence whether mindfulness interventions improve work performance and how effective mindfulness-based programmes are, compared to other interventions, in preventing mental ill health.

Methods and analysis

In this online randomised controlled feasibility trial, an anticipated 240 employees will be randomised to either a 4-week light physical exercise course or a mindfulness course of the same duration (1:1 allocation). The primary outcome is work performance, measured using the Work Role Functioning Questionnaire. We aim to evaluate the acceptability, feasibility, and procedural uncertainties of a randomised controlled trial in a workplace, calculate an effect size estimate to inform power calculations for a larger trial, and explore whether improved executive function and/or enhanced mental health could be potential mechanisms underlying the effect of mindfulness on work performance. Outcomes will be collected at baseline, post-intervention and 12-week follow-up.

Ethics and dissemination

Approval has been obtained from Cambridge Psychology Research Ethics Committee (PRE.2020.072). Results will be published in peer-reviewed journals. A

lay summary will be disseminated to a wider audience including participating employers.

Registration details

Clinicaltrials.gov: NCT04631302

Strengths and limitations of this study

- A randomised trial to lay the foundations to investigate the mechanisms of mindfulness intervention underlying effects on work performance.
- The study employs a range of outcome measures, including self-reported measures and cognitive functioning tasks.
- This feasibility trial is not powered to detect significant effects, but rather to estimate effect size to inform design of a larger later-stage trial.
- Several feasibility outcomes will be collected to inform a later-stage trial.

Keywords: mindfulness, work, well-being, productivity, randomised controlled trial

Introduction

Background and rationale

Mental illness is a major cause of disability worldwide (James et al., 2018). Much of the adult population is employed and spends 28% of their waking hours doing paid work (OECD.Stat, n.d.; Office for National Statistics, 2014). The occupational environment is therefore an opportune location for preventative mental health interventions.

Poor mental health is responsible for 44% of work-related episodes of ill health (Health and Safety Executive, 2019) and according to conservative estimates, is thought to cost the United Kingdom's (UK) economy £45 billion annually (Deloitte UK, 2020) or 2% of UK's Gross Domestic Product. To reduce this burden, a growing number of employers provide programmes to improve well-being and work performance.

Mindfulness-based programmes (MBPs) are increasingly popular in occupational settings. Mindfulness is typically defined as "the awareness that emerges through paying attention on purpose, in the present moment, and nonjudgmentally to the unfolding of experience moment by moment" (Kabat-Zinn, 2003). Practicing such awareness is linked to reduction in symptoms of anxiety, depression, and stress in community populations (Galante et al., 2021; Khoury et al., 2015). There is also evidence that mindfulness training could improve overall well-being (Galante et al., 2021; Spijkerman et al., 2016), life satisfaction (S. L. Shapiro et al., 2005), and quality of life (Khoury et al., 2015).

Mindfulness practice may yield workplace benefits beyond emotional well-being. It has been proposed that mindfulness improves work performance (Dane, 2011) and reduces the negative effects of multitasking (S. L. Shapiro et al., 2015). Yet, there is little evidence to support these claims. A recent meta-analysis concluded that work performance was rarely assessed in trials investigating the outcomes of MBPs. When work performance was measured, wide-ranging operational definitions were used: e.g., engagement (Aikens et al., 2014; Klatt et al., 2015; van Berkel et al., 2014; van Dongen et al., 2016; Wilson, 2012), motivation (Wilson, 2012), absenteeism (Bartlett et al., 2017) and presenteeism (Bartlett et al., 2017; van Berkel et al., 2014; van Dongen et al., 2016), rate of errors (Verweij et al., 2018) and burnout (Luken & Sammons, 2016). Thus, estimating an overall effect is difficult (Bartlett et al., 2019; Jamieson & Tuckey, 2017). Methods for measuring performance in higher education have less variability, yet there is no clear indication that offering mindfulness training to university students improves their academic performance (Dawson et al., 2019).

The mechanisms underlying any effect of mindfulness on work performance are also yet to be determined while two mechanistic pathways stand out that could explain such an effect of MBPs. First, positive effects of MBPs on mental well-being are well-established (Galante et al., 2021; Khoury et al., 2015; Spijkerman et al., 2016), and mental well-being is linked to better work performance (García-Buades et al., 2020; Montano et al., 2017). Conversely, mental health problems decrease employees' performance (Alonso et al., 2011; J. J. Collins et al., 2005; Stewart et al., 2003), particularly if these problems are poorly managed (Rost et al., 2004). However, an indirect effect of MBPs on workplace performance via improved mental well-being has yet to be evaluated.

A second potential mechanism could be an improved cognitive control over mental activity, which allows one to prioritise current task-relevant goals (Ionescu, 2012; Schweizer et al., 2019). There are three potential facets of cognitive control that may be improved by MBPs: (a) shifting, that is, the ability to switch between multiple tasks; (b) updating, or the ability to frequently refresh information in working memory to ensure a currently relevant record of information; and (c) inhibition: deliberately hindering dominant or automatic responses that are irrelevant to the task at hand (Miyake et al., 2000). Improved cognitive control, in turn, may lead to better performance on workplace tasks (Chaskalson, 2011; Dane, 2011).

Mindfulness has been shown to have a small effect on cognitive control (Cásedas et al., 2020; Yakobi et al., 2021). A recent meta-analysis analysing outcomes of randomised controlled trials (RCTs) measuring the effects of cognitive control in MBPs for healthy participants found a small overall effect of Hedges' g = 0.2 (Yakobi et al., 2021). However, we know little about how these changes in cognitive control manifest in the workplace (Bartlett et al., 2019). While mindfulness may improve performance on tasks closely related to the practice, such as counting breaths (Levinson et al., 2014), it may not extend to other tasks, such as those completed at work (Simons et al., 2016).

Furthermore, to date, research has primarily focussed on the impact of mindfulness on cognitive control over emotionally neutral information. Yet, much of the everyday mental activity that we seek to regulate is emotionally positive or negative (Killingsworth & Gilbert, 2010; Kragel et al., 2016). In the two meta-analysis of MBPs' effects on cognitive control published to date (Cásedas et al., 2020; Yakobi et al., 2021), only one identified study used emotional stimuli within an cognitive control task. This study reported a null-effect of meditation on cognitive control measured via an attention network test when comparing negative and neutral conditions (Ainsworth et al., 2013). It is important to note that this study (Ainsworth et al., 2013) was likely underpowered.

At work, it is arguably beneficial to inhibit emotional thoughts (e.g., worrying about a recent argument with your spouse) that are irrelevant to the task at hand (e.g., writing a report). A reduced ability to inhibit internal emotional stimuli may interfere with our ability to maintain focus on workplace tasks. There is evidence that emotional stimuli inhibit cognitive control, when measured using the Stop-Signal Task (Herbert & Sütterlin, 2011; Kalanthroff et al., 2013; Verbruggen & De Houwer, 2007). As mindful meditation trains the ability to move away from thoughts and images of negative emotional valence, practicing mindfulness may enhance cognitive control over emotional mental events (Crane et al., 2017). It is therefore important to determine whether MBPs improve workplace performance via enhancement of cognitive control skills such as the ability to move away from negative stimuli(e.g., worries about task performance) or to decentre from negative mental content (Fresco et al., 2007; Safran & Segal, 1996) and refocus attention on the task at hand (Jamieson & Tuckey, 2017).

Understanding the mechanisms underlying effects of MBPs on work performance would (a) help to design more targeted interventions, (b) improve our attempts to assess MBPs, by designing and selecting more stringent outcome measures and control interventions and (c) inform an understanding of for whom MBPs may be most effective, and in which context (Nielsen et al., 2018).

Objectives

Current literature suggests that MBPs could improve work performance through increased mental well-being and/or cognitive control over emotional material. In order to test this, we need to control for one of the two pathways. Both the MBP and light exercise have been shown to reduce stress, depression and anxiety (Galante et al., 2016; Krusche et al., 2013; Querstret et al., 2018), however, only mindfulness is expected to improve cognitive control skills. We chose light exercises as a condition to help to distinguish between the different pathways through which work performance may improve.

A definitive randomised controlled trial is needed to evaluate these potential mechanisms. However, methodological uncertainties and questions of acceptability and feasibility need clarification to inform the design of such a trial (Bowen et al., 2009; Hallingberg et al., 2018; Medical Research Council, 2008). We aim to conduct a feasibility trial to clarify these uncertainties and complete a preliminary investigation of the relationships between mindfulness training, workplace performance and the proposed mechanisms of action.

This feasibility trial will:

1. Estimate the between-groups effect size for the effect of mindfulness, relative to a light exercise control condition, on our primary outcome of work performance, in order to inform power calculation for a larger trial;
2. Explore whether improved cognitive control and/or enhanced mental health could be potential mechanisms underlying the effect of mindfulness on work performance;
3. Assess the acceptability of the interventions and the study design by monitoring recruitment, retention, and adherence to the course;
4. Determine procedural feasibility of a later stage trial by evaluating the willingness of the participants to be randomised and other practical implications of running a randomised controlled trial at a workplace.

Methods

This protocol follows the guidelines for RCTs set by the SPIRIT 2013 statement (Chan et al., 2013) (SPIRIT checklist in Supplementary Material, p 247). The study's prospective registration number at clinicaltrials.gov is NCT04631302. Initial consent taking started in November 2020. Participants, irrespective of the time they consent, received access to baseline measures from the 23rd February 2021. Data collection will finish by the end of February 2022.

Study design

We will conduct a participant-level RCT. Employees will be randomly allocated in a 1:1 ratio to either of two parallel groups.

Eligibility criteria

Eligibility to participate in this study will be self-reported. The employers who have agreed to participate in the study are local councils or education providers or trade either in the publishing, electronics, or construction industry with employees in a variety of roles, mostly in desk-based occupations. Individuals can participate if they work for the employers taking part in this trial, are based in the UK, and are not currently on a long-term leave. We will recommend that a participant chooses not to join the study if they:

- are currently suffering from severe periods of anxiety, depression or hypomania/mania;
- are experiencing other severe mental illnesses;
- have had a recent bereavement or major loss;
- have already completed a mindfulness course or have meditated more than 10 hours in the past 10 years.

Intervention condition: *Be Mindful* MBP

Participants in the Mindfulness condition will complete the *Be Mindful* pre-recorded online course by Wellmind Media. It incorporates elements from Mindfulness-Based Stress Reduction (Kabat-Zinn, 2013) and Mindfulness-Based Cognitive Therapy (Segal et al., 2013). Course materials and instructional videos are accessed through a website (http://www.bemindfulonline.com).

The four-week course consists of 10 sessions led by two teachers, one female, one male. Participants are taught to use formal meditations (focusing attention on the practice of meditation) as well as informal mindfulness techniques, such as mindful walking and mindful eating. Daily homework includes a formal mediation practice with the assistance of video/audio recordings (up to 30 minutes), and one or two informal exercises per day (see Table 4 for an overview). Every week, participants receive e-mails motivating them to practice and informing them when the next module is available. As this is a feasibility examination for a pragmatic trial, no modifications to the procedures to maintain adherence to the intervention will be implemented.

Control condition: light physical exercise

The four-week control condition involves light exercises aimed at increasing mobility, reducing stiffness, improving blood circulation, and avoiding pain or repetitive strain injuries that may result from sedentary or repetitive tasks common in office environments. The pre-recorded exercises will include whole-body slightly aerobic exercises such as rotation of joints and stretching. The course was developed by JG, a public health doctor, together with an expert in body posture.

The control condition course is designed to match the intervention condition in overall time commitment, and the frequency of interaction with the participant

(see Table 4). It replicates the encouraged use of short breaks throughout the workday to focus on well-being, as in the intervention condition.

Table 4. Comparison of the intervention and control group. Table reused from Vainre et al (2022) under a CC-BY-4.0 license.

Condition	Intervention: Be Mindful	Control: Light physical exercise
Number of sessions in total	10	28
Online coursework frequency	Twice weekly	Daily
Typical session and its length	Self-paced. Includes videos (average of 3-4 minutes in total per session), self-reflection exercises and brief reading tasks.	Videos of 10-13 minutes.
Homework frequency	Daily	Daily
Typical assignment	A formal meditation (10-30 minutes) and shorter task such as journaling or noticing. The frequency of the latter varies from daily to once a week.	Using the exercises while taking brief breaks during the day.
Reminders to encourage practicing	4 times a week	4 times a week

Data collection

Data collection will take place at baseline (T0), after the courses finish (T1) and 12 weeks after completing the courses (T3) (see Figure 5). Additionally, a brief questionnaire will be sent to the participants each workday. Data collected at T1 will be considered as the primary end-point of interest.

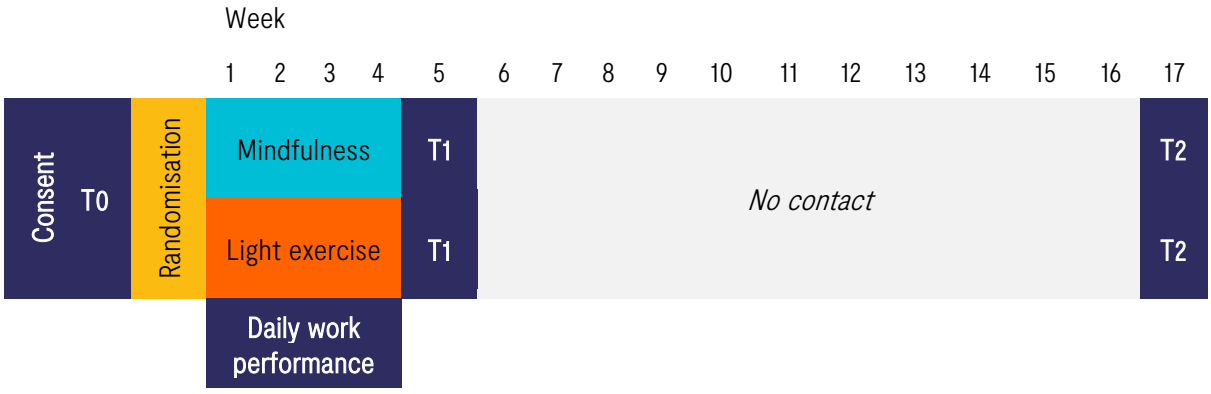

Figure 5. Study procedures and timeline.
Note. Items in white bold font denote data collection. Figure adapted from Vainre et al (2022) under a CC-BY-4.0 license.

Outcomes

Feasibility, acceptability, and procedural outcomes

To determine feasibility of a later-stage trial, we will examine descriptive statistics to:

1. Estimate between-condition effect sizes:
    a. for the primary outcome, to inform a power calculation for a later-stage trial;
    b. for the cognitive control outcomes, to determine suitability of these measures to index mechanisms of interest.
2. Determine feasibility of running a later-stage trial by monitoring recruitment (the percentage of employees who consent into the study), retention, including timing of measurements (by indexing percentage of participants completing each time point), and potential contamination issues, most notably, measuring the extent to which participants talked about their course with participants from the other arm;

3. Acceptability of interventions by indexing which course the participants would have preferred to be randomised to, their regularity in engaging in exercise and mindfulness, and intervention dose, notably the percentage of course materials attempted;
4. Procedural uncertainties, for example, by exploring the suitability of our primary measure in indexing our primary outcome. To this end, we have introduced several work-related outcomes (see Secondary Outcomes);
5. Potential covariates influencing key outcomes which may need to be considered in design of the later-stage trial, including:
    a. participant mental and physical health at baseline;
    b. importance of job to participants' identity at baseline.

Primary outcome: Work performance

Work performance will be measured by using the 25-item Work Role Functioning Questionnaire's (Amick et al., 2000) updated version (Abma et al., 2013), to capture perceived difficulties in meeting work demands. Items are rated on a 5-point scale ('difficult all the time' to 'difficult none of the time'), with higher scores indicating better functioning. A 6th option denotes 'does not apply to my job'. The questionnaire has not been validated in English. Validations completed in Dutch (Abma et al., 2013), Spanish (Ramada et al., 2014), and Norwegian (Johansen et al., 2018) have shown good internal consistency (Cronbach alphas 0.7-0.9) (Abma et al., 2013; Johansen et al., 2018; Ramada et al., 2014), and test-retest reliability (ICC=0.66, 95% CI: 0.54-0.76 for the total score) (Abma et al., 2013). The WRFQ features four subscales: work scheduling and output demands (α=0.92), physical demands (α=0.92), mental and social demands (α=0.91), and flexibility demands(α=0.96) (Abma et al., 2013). The WRFQ has been shown to possess decent convergent validity, correlating with similar measures including the Utrecht Work Engagement Scale (Schaufeli & Bakker, 2004) (r=0.304), Work Ability Index (Ilmarinen, 2007) (r=0.468). The primary endpoint in this trial will be the post-intervention measurement. Feasibility of using the 12-week follow-up as the main outcome end-point in the later-stage trial, will be assessed. We recognise that

effects at 12 weeks may not be sustained longer term. Retention at 12 weeks will help to plan the sample size for a larger trial which could also then measure outcomes longer term.

Secondary outcomes

Work-related outcomes

The participants are asked to report if they have health conditions that affect their ability to work, with options to pick one or several of the following: physical health problems, mental health problems, other health problems, no problems or prefer not to say. If a participant selects one of the first three options (i.e., they have had problems), they will be asked to briefly describe these problems.

Those who self-report experiencing mental or physical health problems in the item described above will be asked to fill in the Work and Social Adjustment Scale (Marks & Bird, 1986). The scale is widely used in the NHS psychology services in England and has good internal consistency, α=0.82 (Zahra et al., 2014).

To get a better understanding of daily fluctuations that may occur in work engagement, participants will be asked to complete a 5-item version of the Work Role Functioning Questionnaire (Abma et al., 2019) each workday afternoon. Items are rated the same as in the full Work Role Functioning Questionnaire.

Cognitive control mechanisms

Two online computerised cognitive tasks will be used, to index our potential executive function mechanisms of interest. Affective cognitive control will be assessed using the Emotional Stop-Signal Task (Verbruggen & Logan, 2008). At the beginning of each trial within the task, a negative or a neutral image appears, followed by a go-signal (left or right arrow). Participants need to respond with a

corresponding key-press. On a minority of trials (20%), the go-signal is followed by a stop-signal (upwards arrow) in which a go-response is required to be inhibited. Reaction times (in both, go- and stop-trials), response accuracy (failure or success in inhibiting response) and variability in reaction time throughout the task (a proxy for the ability to overcome errors) will be measured. The main outcome of interest is the response time in stop-trials.

Participant's ability to track dynamic changes in their environment and alter their response strategies will be measured using an affective modification of the probabilistic reversal learning task (Cools et al., 2002). The task will consist of 6 phases, 3 for the neutral and 3 for the negative condition. Each trial will begin with a negative or a neural image from the International Affective Picture System (IAPS) (Bradley & Lang, 2017). Next, pairs of stimuli (A-B or C-D) will be presented. Participants must select a stimulus with a key-press. In each pair, one of the stimuli is more likely to be rewarded (e.g., selecting A or C is reinforced on 80% of trials). Feedback is presented after each response. Through trial-and-error, participants learn which stimuli are more frequently rewarded. After a certain number of trials (a phase), the contingency of reinforcement switches. In Phase 2, the other stimulus in the pair is more frequently reinforced (e.g., instead of A, B is now reinforced on 80% of trials). In Phase 3, the reinforcement is switched again. Reaction times and response accuracy (i.e., selecting the reinforced stimulus) will be recorded. The main outcome of interest will be changes in learning performance indexed via the proportion of correct responses.

Other outcomes of interest

Well-being

Subjective mental well-being will be measured with the Short Warwick-Edinburgh Mental Well-being Scale(SWEMWBS), a 7-item questionnaire designed to capture a broad concept of well-being(Tennant et al., 2007). In SWEMWBS, items are scored on a scale of 1-5('none of the time'...'all of the time'), with higher scores

suggesting better mental well-being. The SWEMWBS internal consistency was α=0.84 in a study in the UK general population(n=27,169)(Ng Fat et al., 2017).

Stress

The Perceived Stress Scale (PSS) measures the extent to which the individual has perceived events as uncontrollable and overwhelming. The PSS consists of 10 items, answered on a scale of 0-4, higher scores indicate higher stress levels. The PSS possesses good internal consistency, α=0.84-0.86 (S. Cohen et al., 1983).

Depression

The Patient Health Questionnaire (PHQ-9) (Kroenke et al., 2001) is used to assess depression. It consists of 9 items answered using a scale from 0-3, and a further item asking about the level of difficulty associated with any checked off items. Total scores range from 0-27 with cut-off points for depression at 5, 10, 15 and 20 for mild, moderate, moderately severe and severe depression, respectively (Kroenke et al., 2001). A PHQ-9 score of at least 10 has been found to have 88% sensitivity and 88% specificity for major depression (Kroenke et al., 2001).

Anxiety

The General Anxiety Disorder 7-item Scale (GAD-7) (Spitzer et al., 2006) assesses anxiety and has good reliability and validity (Löwe et al., 2008). The items are answered using a scale from 0-3, yielding total scores between 0 and 21 with cut-offs at 5, 10 and 15 for mild, moderate and severe anxiety, respectively (Spitzer et al., 2006). The scale's internal consistency is α=.92. A total score of 10 has a 89% sensitivity and 82% specificity for generalised anxiety disorder (Spitzer et al., 2006).

Mindfulness-related outcomes

The following will be administered to ensure that the MBP does increase mindfulness more than the control condition.

Decentering

The Experiences Questionnaire (EQ) (Fresco et al., 2007) is an 11-item measure of decentering. The items were generated to represent the changes believed to occur due to mindfulness practice, including the extent to which one's self-identity depends on one's thoughts, nonreactivity to negative experiences, and self-compassion. Statements are rated on a 5-point scale ('never' to 'all the time'), with higher scores reflecting higher levels of decentering. The scale's internal consistency is α=.81–.84 (Fresco et al., 2007).

Mindfulness

Mindful Attention Awareness Scale (MAAS) (K. W. Brown & Ryan, 2003) is a self-report questionnaire consisting of 15 items designed to assess a core characteristic of mindfulness – a receptive state of mind in which attention simply observes what is taking place. Items are rated using a 6-point Likert scale ('almost always' to 'almost never'), with higher scores indicating more mindfulness. The internal consistency of MAAS is α=.87 (K. W. Brown & Ryan, 2003).

Sample size

One of the procedural uncertainties limiting the design of a fully-powered trial is the size of the effect on the main outcome in interest. As a traditional power calculation is unfeasible given the lack of previous data, we seek to determine the likely effect size in this study, to inform a later-phase trial.

We aim to recruit 240 participants. A fully online design may cause a high loss to follow-up; a systematic review of internet-based RCTs found the average attrition rate to be 47% at post-intervention (Mathieu et al., 2013). Based on this, we have selected a sample size which we anticipate will yield complete data for 128 participants at our primary end-point of post-intervention (64 per arm) and 68 participants at follow-up (34 per arm). In clinical research with lower attrition rates, feasibility trials tend to recruit 36 participants (Billingham et al., 2013). Considering

high risk of attrition and the considerable uncertainties regarding the feasibility of the trial, we estimate that our sample size is optimal to examine the feasibility of procedures and provide a reliable estimate of effect size.

Study procedures

Recruitment

Employers who have agreed to collaborate in the research project, have taken an active role in shaping the recruitment process to their organisational customs. The invitation, sent via web-based communication services used by the employer, will have a link to the participant information sheet and consent form.

Inducements for participation

There will be no inducements for completing either of the interventions. As a token of appreciation for completing the study assessments, participants will be given £10 at post-intervention and £15 at follow-up time points in the form of retail vouchers.

Randomisation procedure

After the participants have completed all baseline measurements, they will be randomised to either the mindfulness or the light physical exercise arm, stratified by employer. The randomisation process will be automated using REDCap, a platform for questionnaire data collection(Harris et al., 2009, 2019). The allocation tables were generated with *randomizeR* package (Uschner et al., 2018) in R using randomised permuted block randomisation with pre-specified seeds for reproducibility. The code is available at GitHub (Vainre, 2021/2021b).

Participants will not be blind to their allocation. The primary analysis will be completed by a statistician (PW) blind to intervention allocation.

Public involvement

The study's design has been formed by feedback from the employers participating in the study, including the perceived utility of the interventions, recruitment procedure and its timing, study materials, incentives for participation and outcomes. Changes to the initial design were proposed, some of which were implemented (e.g., offering vouchers rather than cash; channels and timing for recruitment).

Statistical methods

Central tendencies, dispersion, and data missingness will be reported for all time points. At baseline, descriptive statistics will be presented overall and by group allocation. At following timepoints, outcomes will be reported by group.

Any significance testing, though not the focus of this trial, will follow the intention-to-treat principle. A key limitation of feasibility trials such as this is that adequate power is not obtained to detect statistical significance. For the primary outcome, a linear multiple regression model will compare the WRFQ total score between trial arms at post-intervention, adjusted for baseline WRFQ and employer. Multiple imputation will be used to account for missing data. Further exploratory analysis will employ the same approach for other outcome measures at post-intervention and 12-week follow-up. Mediation analysis techniques will be employed to assess the suggested mechanistic pathways. Effect sizes obtained in these analyses will be used to inform a potential later-stage trial and are the focus of this trial, rather than statistical significance.

For the secondary outcomes, mixed model repeated measures analysis will be performed using the daily monitoring of work performance to study changes between arms during the intervention. The analysis will also compare different work performance measures. Again, the focus of this trial is on obtaining an estimate of likely effect sizes, rather than statistical significance.

Data monitoring and adverse events

An Independent Study Steering group has been established to monitor data and advise the conduct of the study to ensure participant safety and integrity of research. We have established the following safeguards (Baer et al., 2019; Van Dam & Galante, 2020):

1. Participants are made aware they may request a consultation with a clinical psychologist if they feel uncomfortable with the study or experience discomfort they associate with the interventions.
2. Where participants' responses to PHQ9 (depression) or GAD7 (anxiety) are above clinical cut-off scores (≥20 and ≥15, respectively), a warning is automatically sent to the researcher. For PHQ9, the alert is also triggered when the participant score is >0 on the self-harm item. The researcher (MV) will then consult the clinical psychologist who will contact the participant.
3. Participants wishing to leave the study will be encouraged to let the study team know why they have chosen to do so.

Any adverse events discovered through the mechanisms listed above will be discussed with the Independent Study Steering group who may decide whether they are attributable to the interventions (i.e., adverse effects) and any subsequent course of action.

Ethics and dissemination

The trial has received approval from the Cambridge Psychology Research Ethics Committee PRE.2020.072.

Consent

The consent form states eligibility criteria and the circumstances in which we recommend not to participate in the study. Participants are invited to join virtual information sessions or e-mail the study team should they have any questions.

Information about accessing mental health support services is made available to anyone visiting the participation information website and e-mailed to those who consent to the study. Only those who consent to participate will receive the link to baseline measurements.

Data management

Data will be collected and curated using the Research Electronic Data Capture (REDCap) (Harris et al., 2009, 2019), the Cohort Management System (CMS) and JATOS (Lange et al., 2015). Anonymised data will be shared for research purposes upon request, in line with open science principles. All personally identifiable data will be separated from study data and stored on separate encrypted servers.

Dissemination policy

Findings will be submitted to peer-review journals. Authorship in publications will be based on the International Committee of Medical Journal Editors' criteria. We will also send a lay summary of the results to the participating employers and participants.

Acknowledgements

The study team would like to thank Becky Gilbert, Camilla Nord, Marc Bennett, Rachel Knight, and Tierney Lee for their role in designing the cognitive tasks, Christina Haag for her consultations on planned analyses, and Markus Hausammann for substantially contributing to the design of the study logo.

Contributors

MV, CH, JG and TD developed the idea for the trial and applied for funding. MV, CH and JG drafted the protocol which was then revised through discussions with TD, the Emotion study group at MRC CBU and the participating employers. The analysis plan was devised by MV, CH, JG and PW. MV is the lead researcher. CH and JG are supervising the research. The trial is sponsored by University of Cambridge.

Funding statement

This study is supported by MRC Cognition and Brain Sciences Unit, MV is supported by Kristjan Jaak degree studies abroad scholarship by Estonia's Education and Youth Board, JG by the National Institute for Health Research (NIHR) Applied Research Collaboration East of England, CH by the Economic and Social Research Council and TD by the UK Medical Research Council (Grant reference: SUAG/043 G101400) and Wellcome (Grant reference: 104908/Z/14/Z, 107496/Z/15/Z). The views expressed are those of the authors and not necessarily those of the National Health Service, the NIHR or the Department of Health and Social Care.

Competing interests' statement

None declared.

Chapter 4

The Work Engagement and Well-being Study (SWELL): A randomised controlled feasibility trial evaluating the effects of mindfulness versus light physical exercise at work

Foreword

This chapter contains the report on the results of The Work Engagement and Well-being Study (SWELL) which has been submitted for peer-review and is available in a pre-print repository:

Vainre, M., Dalgleish, T., Watson, P., Haag, C., Dercon, Q., Galante, J., & Hitchcock, C. (2023). The Work Engagement and Well-being Study (SWELL): A randomised controlled feasibility trial evaluating the effects of mindfulness versus light physical exercise at work. PsyArXiv. https://doi.org/10.31234/osf.io/wamkn

Abstract

Background

Mindfulness-based programmes (MBPs) are increasingly offered in work settings, often in online and/or self-guided format. However, there is inconsistent evidence regarding whether MBPs improve work performance, and if so, whether MBPs outperform other alternative interventions.

Objective

This randomised controlled feasibility trial assessed procedural uncertainties including recruitment and retention and aimed to estimate the likely effect size of a self-guided online MBP on work performance, relative to an alternative intervention, to inform sample size calculation for a later-stage trial.

Methods

Two hundred and forty-one employees from 8 employers were randomised (1:1 allocation) to complete a four-week, self-guided, online delivered intervention of either the Be Mindful MBP or a light physical exercise programme (an active control intervention). The primary outcome was self-reported work performance at post-intervention measured using the Work Role Functioning Questionnaire. Secondary outcomes included depression, anxiety, stress, and cognitive processes hypothesised to be targeted by the MBP, including decentering and cognitive control. All outcomes were assessed at baseline, post-intervention, and 12-week follow-up. The trial protocol was pre-registered (NCT04631302) and published.

Findings

Eighty-seven percent of randomised participants started the course. The acceptability of both courses was high with typical retention rates for outcome measure collection (64% at post-intervention and 30% at follow-up). We found that the MBP, compared to the control, offered negligible benefits for work performance at post-intervention (d = 0.06) and 12-week follow-up (d = 0.02). We observed small effect sizes (ds = -0.09-0.04) for the difference between the MBP and the control for mental health and cognitive control secondary outcomes. Both interventions improved mental health outcomes from pre- to post-intervention (ds = -0.40-0.58, p < 0.001).

Discussion

Results provide little support for a later-phase trial comparing an MBP to a light exercise control. We summarise procedural challenges to inform future trials of online MBPs at work. Results suggest MBPs are unlikely to improve work performance relative to an active control. Although the MBP improved mental health outcomes, other active interventions such as light physical exercise may be just as efficacious.

Keywords: mindfulness, physical exercise, work performance, mental health

Key messages

Mindfulness-based programmes (MBPs) have been shown to improve mental health when compared to passive control groups, and there is some indication that they may also improve work performance. This trial is the first to compare a MBP to an active comparison intervention on their effects of work performance.

This early-phase trial determined the feasibility of a later-stage efficacy trial. We found that a MBP yielded no benefit compared to a light physical exercise programme, either for work performance or mental health outcomes. Public health recommendations on offering MBPs should consider comparative effectiveness of alternative approaches, along with users' preferences.

Background

Public health guidance in several countries encourages employers to support the physical activity (e.g., National Institute for Health and Clinical Excellence, 2009) and mental health (e.g., Directorate-General for Employment, 2016; National Institute for Health and Clinical Excellence, 2022) of staff. Employers, too, are increasingly seeking to support employees' health and well-being by incorporating mindfulness-based programmes (MBPs) into their well-being package (Barnes et al., 2017; Chen, 2022; Fleming, 2021; The Prince's Responsible Business Network, 2019), as recommended in National Institute for Health and Care Excellence (NICE) guidance (National Institute for Health and Clinical Excellence, 2022). MBPs aim to improve attention and self-regulation through training the ability to maintain awareness of the present moment (Kabat-Zinn, 2003). MBPs also cultivate compassion (Alsubaie et al., 2017; Crane et al., 2017) by fostering a detached self-perspective and thus training recipients to decentre from psychological stressors. There is existing evidence that MBPs have several mental health benefits, when compared to passive face-to-face control groups (usually waitlist control). This includes reduction in symptoms of anxiety, depression, and stress in community populations (Galante et al., 2021). There is also evidence that MBPs enhance well-being (Galante et al., 2021) and life satisfaction (S. L. Shapiro et al., 2005).

Additionally, in the workplace, mindfulness practice may have benefits beyond mental health; most critically, by improving work performance. Such anticipated additional effects would likely make MBPs more attractive to employers, relative to other well-being interventions or organisational changes (Zeller & Lamb, 2011).

This is in addition to the logistical benefits of offering MBPs: many MBPs are offered online, which suits the post-pandemic rise of remote and asynchronous working. Further, online interventions are scalable as they are low-cost, have no need for waiting lists, their access does not need calendar coordination, and the programme can be offered at a location comfortable for the employee. Thus, there are multiple aspects of MBPs which make them attractive for organisational wellbeing initiatives.

Yet, the empirical data on whether MBPs improve work performance remains equivocal. A recent systematic review suggests that work performance is rarely assessed in trials investigating the outcomes of MBPs at work (Bartlett et al., 2019). When work performance is assessed, a wide range of operationalisations are used, ranging from resilience (Erogul et al., 2014; Gómez-Odriozola et al., 2019) and work engagement (Klatt et al., 2015, 2017; Rich R.M. et al., 2021; Steinberg et al., 2017) to absenteeism/presenteeism (Bartlett et al., 2017; Roeser et al., 2013; Steinberg et al., 2017; Strauss et al., 2021; van Berkel et al., 2014; van Dongen et al., 2016). The current literature is therefore limited in the ability to conclude that completion of MBPs: a) improves an individual's perceived ability to complete their job; and b) delivers such effects with a greater magnitude than that achieved by other work-based interventions. Perceived ability to engage in work is a key influence on not only individual-level experience (e.g., feeling fulfilled and engaged in life, in line with the World Health Organisation definition of health (1948)) but also on the economy (e.g., through decisions not to engage in the work force due to low self-efficacy). Answering these questions will also enable informed decisions when purchasing MBPs at an organisational level.

MBPs could improve work performance through two pathways. First, MBPs have a demonstrated ability to reduce symptoms of poor mental health (e.g., of anxiety, depression and stress) which could subsequently enhance work performance. Better mental health is likely to impact several aspects of work performance (Koopmans et al., 2011), such as improving resilience and work engagement, and reducing absenteeism/presenteeism. A second potential pathway is via cognitive

control, that is, through the ability to self-regulate at work to allow prioritisation of current goals (Ionescu, 2012; Schweizer et al., 2019). According to recent meta-analyses (Cásedas et al., 2020; Im et al., 2021; Millett et al., 2021; Yakobi et al., 2021), mindfulness training, compared to passive control groups, could enhance cognitive control (Hedges' g = -0.03-0.42) but it has yet to be determined whether improved cognitive skills transfer to work performance. Furthermore, existing research has focussed on the impact of mindfulness on cognitive control over affectively benign information. Yet, much of the everyday mental activity that we seek to regulate while at work is emotionally positive or negative (Killingsworth & Gilbert, 2010; Kragel et al., 2016). Reduced ability to inhibit internal affective stimuli (e.g., remembering an argument with your spouse) may interfere with the ability to maintain focus on tasks at work (e.g., writing a paper). As mindful meditation is proposed to train the ability to move away from thoughts and images, in this study we sought to explore whether practicing mindfulness may particularly enhance cognitive control over affective mental events (Crane et al., 2017).

If cognitive control is a key pathway through which MBPs work, then the most likely domain of work performance (Koopmans et al., 2011) to improve is task performance, or the quantity and quality of work. This domain has been less frequently assessed, compared to other types of work performance (Vainre, Dalgleish, et al., 2022). It is difficult to assess task performance in a way that would allow for a comparison between different job roles and industries (Koopmans et al., 2014a). However, beliefs in one's ability to complete job-related tasks have been shown to predict improved work performance (Stajkovic & Luthans, 1998), particularly improved task performance (Abun, 2021), with some prior suggestions that this effect may occur via cognitive control (Themanson & Rosen, 2015).

In sum, it is currently unclear whether MBPs (particularly online, self-guided MBPs), can improve work performance, the mechanisms through which any such effect may occur, and whether their effects are superior to those of other workplace interventions. A better understanding of the effects of MBPs on work performance could lead to immediate applications in workplaces. Further investigation of potential mechanisms of action could also improve our attempts

to assess MBPs by designing and selecting more stringent outcome measures and control interventions, and guiding decisions regarding for whom MBPs may be most effective, and in which context (Nielsen et al., 2018).

A definitive randomised controlled trial is thus needed to evaluate the effect of an MBP on work performance and whether cognitive control is its mechanism. This is best done via a randomised controlled trial (RCT) with an active control group. However, little is known about the comparative effects of an MBP against a control condition that also improves mental health (Vainre, Dalgleish, et al., 2022). Given effect sizes are difficult to predict, we conducted a feasibility trial to clarify these uncertainties and completed a preliminary investigation of the relationships between a MBP, work performance and the proposed mechanisms of action.

Objective

This feasibility trial aimed to clarify methodological uncertainties and determine feasibility of a later-stage randomised controlled efficacy trial to evaluate the effects, and underpinning mechanisms of action, of online self-guided MBPs on work performance (Bowen et al., 2009; Hallingberg et al., 2018; Skivington et al., 2021). Participants were randomised to complete either an online, self-guided MBP, or a light physical exercise active control intervention designed to control for non-specific effects of being in a structured intervention requiring engagement with the body on wellbeing. Specifically, this feasibility trial:

1. Estimated the between-groups effect size for the effect of the MBP, relative to an active control on our primary outcome of work performance (at post-intervention), in order to inform power calculations for a later-phase trial;
2. Estimated the effect of cognitive control as mediator of the effect of MPB on work performance;
3. Assessed the acceptability of the interventions;

4. Sought to resolve design and procedural uncertainties in advance of a later-stage trial.

Methods

This trial adheres to CONSORT guidelines (Schulz et al., 2010). Please see Supplementary Material 1: CONSORT checklist, p 257. The trial received approval from the Cambridge Psychology Research Ethics Committee (PRE.2020.072) and was prospectively registered at clinicaltrials.gov (NCT04631302). Full methodological details can be found in the published protocol (Vainre, Galante, et al., 2022).

Study design and participants

Randomisation was conducted at the participant-level on a 1:1 ratio. We recruited participants by first approaching organisations with employees primarily engaged in desk-based occupations. The participants were able to start their course every Monday from 1st March to 28th June 2021 and from 4th to 25th October 2021. The participating employers distributed information about the study through their usual internal media channels (e-mails, MS Teams, Slack etc).

Inclusion criteria (all self-reported) were being a current employee of a participating employer, and based in the UK. Exclusion criteria (all self-reported and decided by the participant) were being currently on long-term leave, currently suffering from severe anxiety, depression, hypomania/mania or other severe mental illness, having experienced a recent bereavement or major loss, having already completed a mindfulness course or having meditated more than 10 hours in the past 10 years. All participants provided written informed consent. There were no incentives for completing the intervention. Participants received retail vouchers for completing the post-intervention (£10) and 12-week (£15)

assessments and were encouraged to complete the assessments regardless of how much (if any) of the intervention they had completed.

Sample size

As this was a feasibility trial, sample size was not guided by a formal power calculation to estimate effect size. We aimed to recruit 240 participants (Vainre, Galante, et al., 2022), anticipating this would yield 128 participants (64 per arm) at post-intervention and 68 (34 for per arm) at follow-up, given high attrition rate experienced by trials completed online (Mathieu et al., 2013). This sample size is standard for feasibility trials in the UK (Billingham et al., 2013) and provides enough data to evaluate procedural uncertainties and acceptability and to provide a range of effect size estimates on our primary outcome.

Intervention arm: *Be Mindful* mindfulness-based programme

Participants in the MBP arm completed the *Be Mindful* pre-recorded and fully automated online course by Wellmind Media (Querstret et al., 2018). Materials and instructional videos were accessed through a website (http://www.bemindfulonline.com).

The four-week course consists of 10 sessions, with two sessions completed per week. Sessions include various videos (between 28 s and 7 minutes of length) and text to teach formal meditations as well as informal mindfulness techniques, such as mindful walking and mindful eating. Participants are then asked to practice mindfulness meditation (the type of meditation varies week-to-week and is between 10 and 30 minutes long) and complete an informal exercise (e.g., eating a

meal mindfully) (see Querstret et al., 2017 and Supplementary Materials for an overview of the course).

Active control arm: Light physical exercise

The four-week light physical exercise programme aimed to enhance mobility, alleviate stiffness, stimulate blood flow, and prevent pain or repetitive strain injuries that may arise from tasks typical in office settings. It was not designed to improve strength or cardiovascular fitness. Participants followed pre-recorded videos that guided them to perform whole-body exercises such as joint rotation and stretching. The course was developed by JG, a public health doctor in collaboration with a body posture expert (Galante et al., 2016). This control arm matched the mindfulness arm in overall time commitment and frequency of interaction with the participant (Vainre, Galante, et al., 2022). It also encouraged use of short breaks throughout the workday to focus on well-being, replicating the mindfulness programme. A previous study has demonstrated the course to have active benefits for mental health (Galante et al., 2016), thus allowing us to control for non-specific and mental health intervention effects

Outcomes

We collected demographics and work-related details at baseline. Assessments were completed at baseline, post-intervention (primary end-point) and 12-weeks follow-up. Additionally, participants were invited to complete a brief questionnaire each day they worked. Participant-reported outcomes were collected online via RedCap (Harris et al., 2019) and jsPsych (de Leeuw, 2015) hosted on JATOS (Lange et al., 2015).

Feasibility and acceptability

The acceptability of the interventions was assessed by uptake at recruitment, retention, and monitoring adherence to the intervention protocol, indexed via tracking participants' logins to their respective intervention website. The design and procedural feasibility of a later stage trial was determined by monitoring recruitment of both organisations and participants and trial retention, evaluating the willingness of the participants to be randomised, intervention contamination (i.e., participants completing exercises that corresponded to the other arm of the trial or talking about the course with participants in the other arm), course preferences and outcome measures' completion rates.

Primary outcome: Work performance

Work performance was measured by using the 25-item Work Role Functioning Questionnaire (WRFQ) version 2 (Abma et al., 2013), to capture perceived difficulties in meeting work demands. As the WRFQ can be used applied to manual labour to desk-based jobs, it offers variety and comparability across industries and job roles. Items query the ability to focus on work and complete tasks in a timely manner. Items are rated on a 5-point scale ('difficult all of the time' to 'difficult none of the time'), with higher scores indicating better functioning. A 6th option denotes 'does not apply to my job' and was treated as missing when scoring. In the current study, Cronbach's $\alpha = 0.93$. Our primary endpoint was post-intervention.

Secondary outcomes

Work-related outcomes

Participants were asked to report whether their ability to work was impacted by physical health problems, mental health problems, other health problems, no

problems or prefer not to say. Those who reported any health problems were asked to fill in the Work and Social Adjustment Scale (Marks & Bird, 1986). To index daily fluctuations that may occur in work engagement, participants were invited to complete the 5-item version of the WRFQ (Abma et al., 2019) at 3pm each working day, to reflect on work performance that day. Items are scored as per the full WRFQ.

Cognitive control mechanisms

Two online computerised cognitive control tasks were written in JavaScript. Affective cognitive control was assessed using the Affective Stop-Signal Task (Lee, 2020; Verbruggen & Logan, 2008). At the beginning of each trial within the task, a negative or a neutral image appeared, followed by a go-signal (left or right arrow). Participants needed to respond with a corresponding key press. On a minority of trials (20%), the go-signal was followed by a stop-signal (upwards arrow) in which a go-response was required to be inhibited. When inhibition was successful, the stop-signal delay on the subsequent trial was increased by 20ms. Reaction times (in both, go- and stop-trials), response accuracy (failure or success in inhibiting response) and variability in reaction time throughout the task (a proxy for the ability to overcome errors) were measured. The main outcome of interest was the stop signal reaction time (ms), after excluding trials where the reaction time was improbably short (250 ms or less) or long (above 3000 ms) and where the Stop Signal Delay (SSD) was below 50 ms, as recommended by the task authors (Verbruggen et al., 2019).

Participant's ability to track dynamic changes in their environment and alter their response strategies was measured using an affective modification of the probabilistic reversal learning task (Cools et al., 2002; Vainre, 2021/2021a). The task consisted of 6 phases, 3 forming a neutral condition and 3 forming an emotionally negative condition. Each trial began with a negative or a neural image from the International Affective Picture System (IAPS) (Bradley & Lang, 2017). Next, pairs of stimuli were presented, and participants were asked to select one item in each pair to gain a reward. In each pair, one of the stimuli was more likely

to be rewarded (i.e., reinforced on 80% of trials). Feedback was presented after each response. Through trial-and-error, participants learnt which stimuli are more frequently rewarded. After a certain number of trials (a phase), the contingency of reinforcement switched. In Phase 2, the other stimulus in the pair was more frequently reinforced. In Phase 3, the reinforcement was switched again. Reaction times and response accuracy (i.e., selecting the rewarded member of the pair) were recorded. The main outcome of interest was change in learning performance indexed via the overall proportion of correct responses.

Other outcomes of interest

The Perceived Stress Scale (PSS) measured the extent to which the individual perceives events as uncontrollable and overwhelming (S. Cohen et al., 1983). The Patient Health Questionnaire (PHQ-9) (Kroenke et al., 2001) was used to assess depression symptoms. The General Anxiety Disorder 7-item Scale (GAD-7) (Spitzer et al., 2006) assessed anxiety symptoms. The Experiences Questionnaire (EQ) (Fresco et al., 2007) measured decentering. The Mindful Attention Awareness Scale (MAAS) (K. W. Brown & Ryan, 2003) assessed self-reported dispositional mindfulness. We also planned to use the Short Warwick-Edinburgh Mental Well-being Scale (SWEMWBS) (Tennant et al., 2007). However, due to a technical error we were unable to obtain the data.

Randomisation and masking

After completion of the baseline assessment, participants were randomly assigned to either the mindfulness or light physical exercise arm, stratified by employer. The randomisation process was automated in REDCap (Harris et al., 2009, 2019). The study manager (MV) clicked a button that randomised the participant using a pre-specified allocation table (created with randomizeR package (Uschner et al., 2018) in R with randomly selected block sizes (Efird, 2011). The allocation table could not

be edited once data collection had begun, and concealed the allocation process from the researchers. An automated e-mail informed the participant of their allocation and detailed how to access the relevant course. The randomisation code is available at GitHub (Vainre, 2021/2021b). Neither the participants nor the study manager were blind to intervention allocation, although the participants were not told which intervention is considered to be the control and study materials introduced both courses equivalently. The primary analysis was completed by a statistician (PW) blind to intervention allocation.

Statistical methods

Data were analysed in *R* (R Core Team, 2022) using RStudio (Posit team, 2022). As per our pre-registration, primary and secondary outcomes were analysed using the intention-to-treat principle. We ran multiple linear regression models with the *miceadds* package (Robitzsch & Grund, 2023), using separate models to compare post-intervention and follow-up scores between trial arms, adjusted for baseline and employer. The post-intervention questionnaire data analyses including our primary outcome (except MAAS and decentering) were completed by an independent statistician blinded to intervention allocation (PW). The remaining secondary outcomes at post-intervention and all follow-up analyses were completed by MV.

Missing data were multiply imputed using the *mice* package (Buuren & Groothuis-Oudshoorn, 2011). For questionnaire data, we used predictive mean matching models to impute the total score. Task data were imputed using random forest models as they provided a better fit. We included a large number of variables as predictors (L. M. Collins et al., 2001) during imputation; (a) full scores of the primary outcome, mechanism outcomes, mental health outcomes, process outcomes and work-related outcomes at all time-points; (b) the rating on the single-item outcomes of work-related outcomes at all time-points and preference of allocation; and (c) programme take-up. The variables for the two arms were imputed separately and then combined for data analysis (Enders & Gottschall,

2011). Imputation was performed for all randomised participants, including those who did not respond to any items at post-intervention or follow-up. We imputed 100 datasets.

Mediation analysis using the *mediation* package (Tingley et al., 2014) tested the hypothesis that mindfulness training, relative to the control intervention, modifies work performance via changes in cognitive control. The outcome was WRFQ total score at follow-up and the mediator was stop-signal reaction time in the negative condition at post-intervention. The predictor variable was the study arm. The statistical analysis plan had pre-specified that only participants who completed at least half of the sessions would be included. However, due to a data collection problem we were not able to verify the number of sessions attended. We therefore included all participants who did at least one session. Daily work performance was evaluated with mixed-effects models, with arm allocation and day as a fixed effects. Participant ID, nested within employer was set as the random effect.

Findings

Feasibility and acceptability

Recruitment feasibility

Eight employers and 241 employees participated in the trial. The median number of staff members per participating employer was 2130 (range: 180–7500, total of 20966 UK-based employees). The percentages of those staff members who agreed to take part and were randomised were modest (median: 0.91%, range: 0.27–2.85%). Compared to other industries, a larger proportion of local authority

employees joined the study (M = 2%; SD = 0.74 vs M = 0.58%; SD = 0.34). For sample characteristics at baseline, see Table 5.

Intervention acceptability

Eighty-seven percent of randomised participants started the course (87.7% in mindfulness, 86.55% in light physical control). The retention rates for outcome measure collection were 64% (60.66% in mindfulness, 68.07% in light physical exercise) at post-intervention and 30% (32.79% in mindfulness, 27.87% in light physical exercise) at follow-up). Six participants decided to abandon the programme (but agreed to provide outcome measures at regular time points). Of those six, three participants found the assigned programme unsuitable (1 in mindfulness, 2 in light exercise). No participant requested to withdraw from the study (for CONSORT diagram see Figure 6). Across both interventions, the median length of intervention engagement was 3 weeks out of four (IQR = 2). At post-intervention, participants in the light exercise programme showed a greater desire to have been assigned to mindfulness, while the participants in the mindfulness arm did not show a strong preference either way (W = 3523, p = 0.02). For further details, see Supplementary Material 3: Findings, Supplementary Table 2.

Contamination

At post-intervention, mindfulness participants reported to have talked about their course with light exercise participants slightly more frequently (M = 5.07; SD = 14.63 on a 0...100 scale) than the other way around (M = 2.81; SD = 5.85), the difference was not statistically significant (W = 2911, p = 0.93).

Participants in both intervention arms reported similar levels of weekly exercise at both post-intervention (p = 0.61) and at follow-up (p = 0.94) (for details, see Supplementary Material 3: Findings, Supplementary Table 2). At post-intervention, while participants in both arms had practiced mindfulness up to 3 hours a week,

those assigned to mindfulness were more likely to have done so: 54.91% (mindfulness) vs 16.8% (light exercise), (χ^2 = 12.8, p = 0.002).

Table 5. Baseline characteristics. Table reused from Vainre et al (2023) under a CC-BY-4.0 license.

Characteristic		Mindfulness (n = 122)	Light exercise (n = 119)
Gender	Female	105 (86.1%)	99 (83.2%)
Age M (SD)		44.22 (11.13)	45.04 (10.21)
Employer	Engineering	4 (3.3%)	4 (3.4%)
	Higher Education	1 (0.8%)	1 (0.8%)
	Local Authority 1	7 (5.7%)	6 (5%)
	Local Authority 2	69 (56.6%)	69 (58%)
	Local Authority 3	24 (19.7%)	23 (19.3%)
	Publishing	4 (3.3%)	2 (1.7%)
	Secondary Education	1 (0.8%)	1 (0.8%)
	Technology	12 (9.8%)	13 (10.9%)
Ethnicity	Asian	6 (4.9%)	9 (7.6%)
	Mixed or multiple	5 (4.1%)	2 (1.7%)
	Other	2 (1.6%)	3 (2.5%)
	Prefer not to answer	1 (0.8%)	-
	White	108 (88.5%)	105 (88.2%)
Education	Degree	87 (71.3%)	74 (62.2%)
Caring responsibilities	Yes	43 (35.2%)	46 (38.7%)
Condition that affects the ability to focus	Yes	9 (7.4%)	10 (8.4%)
Any experience with meditation	Yes	47 (38.52%)	53 (44.54%)

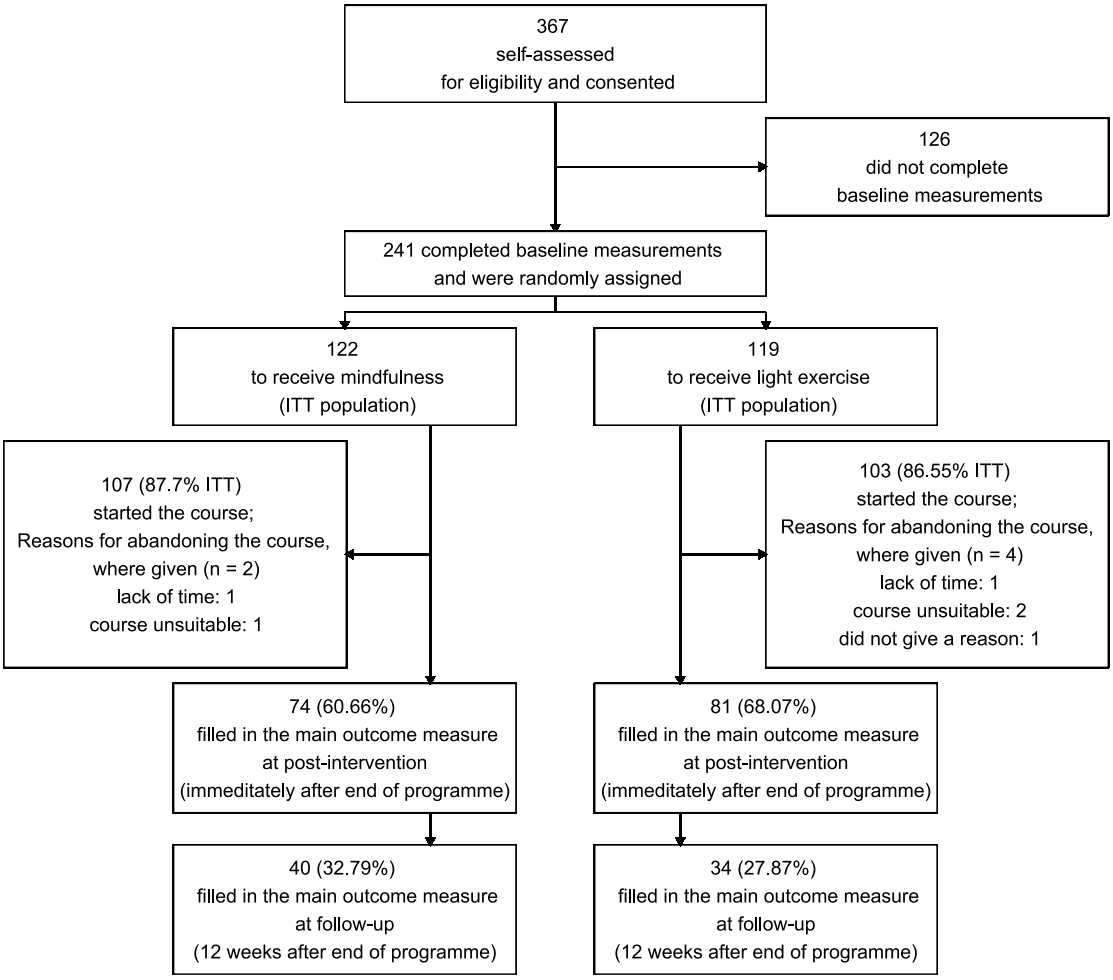

Figure 6. CONSORT diagram illustrating participant flow. Figure reused from Vainre et al (2023) under a CC-BY-4.0 license.

Primary outcome: work performance

The intention-to-treat analysis indicated that, adjusting for baseline and employer, there was a negligible effect size for the difference between the mindfulness and light exercise arms in work performance at our primary endpoint of post-intervention (*d* = 0.06, see Figure 7, Supplementary Material 3: Findings,

Supplementary Table 3). As expected for a feasibility trial, this difference was not statistically significant (t (237) = 0.49, p = 0.63).

Figure 7. Work role functioning questionnaire total score at baseline, post-intervention and 12-week follow-up compared across the two study arms. Figure reused from Vainre et al (2023) under a CC-BY-4.0 license.

Secondary outcomes

Further work-related outcomes

When examining pre-to-post-intervention change, trivial, non-significant effect sizes were observed for all participants, indicating minimal improvement in work performance, regardless of intervention allocation (d = 0.10, p = 0.28). Similar effect sizes were observed for change from pre-intervention to follow-up (d = 0.14, p = 0.12). The light exercise participants reported more overtime hours than those in the mindfulness programme (d = 0.22, p = 0.09) at post-intervention, along with more frequent health problems (d = 0.20, p = 0.11), with small effect sizes. At follow-up, the difference between mindfulness and light exercise on

WRFQ total score was trivial (*d* = 0.02, *p* = 0.91, see Figure 7, Supplementary Material 3: Findings, Supplementary Table 3).

We found daily monitoring a fraught approach to index work performance. On average, participants completed the daily questionnaire on fewer than half of the 28 days (mindfulness: M = 12.2, SD = 6.95, min = 1, max = 27; light exercise: M = 9.95, SD = 7.55, min = 1, max = 26). Some participants did not respond to any of the daily monitoring questionnaires (mindfulness n = 24 (19.67%), light exercise n = 16 (13.45%)). Across the 28 days, the average work functioning score improved across arms (the effect of day: beta = 0.20, SE = 0.05, t(25.96) = 4.03, *p* = 0.0004), with a negligible effect size for the between-arm difference (beta = 0.15, SE = 0.94, t(163.52) = 0.16, *p* = 0.873, Supplementary Material 3: Findings, Figure 4).

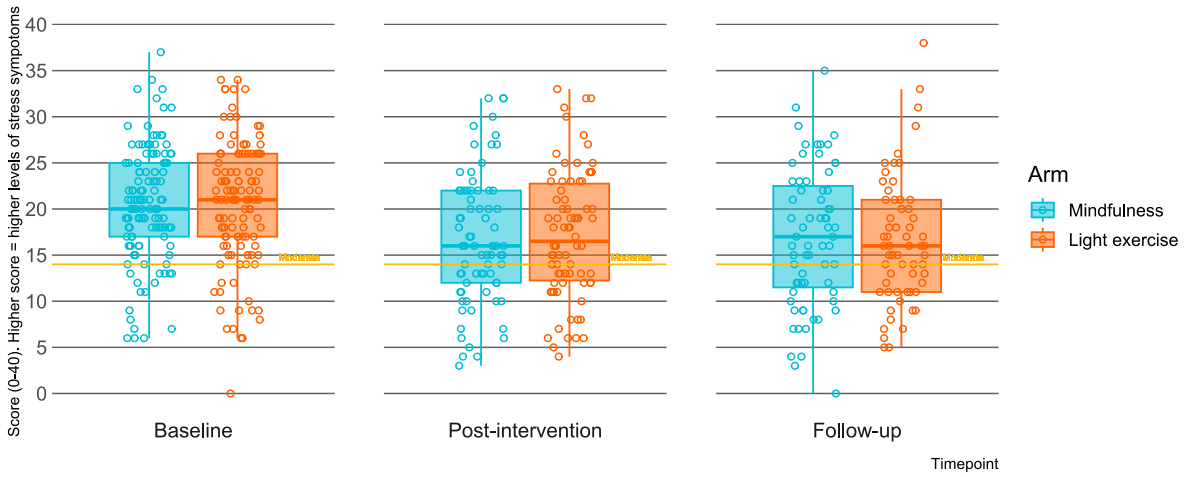

Figure 8. Perceived Stress Scale score at baseline, post-intervention and 12-week follow-up compared across the two study arms. The yellow lines indicate the cut-off score for moderate stress. Figure reused from Vainre et al (2023) under a CC-BY-4.0 license.

Mental health

Between-arm improvements at post-intervention and follow-up in stress, anxiety, depression, and mindful awareness were trivial (*d*s < 0.10) and not statistically significant, for all time points (Figure 8-Figure 10, Supplementary Material 3: Findings, Supplementary Table 3). Small effect sizes were seen in favour of the

mindfulness arm for decentering at post-intervention (d = 0.24, p = 0.07) and at follow-up (d = 0.22, p = 0.09) (Figure 10, Supplementary Material 3: Findings, Supplementary Table 3), although again, these were not significant as expected for a feasibility trial. The remaining effect sizes were smaller than 0.2 and are reported in the Supplementary Material 3: Findings, Supplementary Table 3. All participants demonstrated a significant improvement in mental health outcomes, regardless of intervention allocation: moderate, significant effect sizes were observed for baseline to post-intervention change and baseline to follow-up changes across all mental health outcomes (see Supplementary Material 3: Findings, Supplementary Table 4).

Cognitive control

The assumptions of normality and sphericity were not met for either cognitive task. We therefore analysed the data using linear mixed-effects models (Mair & Wilcox, 2018; Schielzeth et al., 2020). Descriptive statistics and figures are reported in Supplementary Material 3: Findings. For the Affective Stop-Signal task, when adjusting for baseline and allowing for random effects for each participant, we found a trivial effect size for the interaction between the intervention and affective condition at both post-intervention (beta = -0.82, SE = 2.04, t(139.05) = -0.4, p = 0.69, d = -0.05) and follow-up (beta = -1.16, SE = 2.17, t(129.01) = -0.53, p = 0.59, d = -0.01). Similarly, in the Affective Learning Task, when adjusting for baseline and allowing for random effects for each participant, we found trivial between-arm effect sizes for accuracy at both post-intervention (beta = 0.001, SE = 0.003, t(150.17) = 0.34, p = 0.74, d = 0.04) and at follow-up (beta = 0, SE = 0, t(172.91) = 0.09, p = 0.93, d = -0.07).

Mediation

We used the unimputed data set for mediation analyses. This comprised 43 participants with complete data. We were interested in whether Stop Signal

Reaction Time (SSRT) on negative valence trials at post-intervention mediated the effect of group allocation on the WRFQ total score at follow-up. A non-significant, indirect effect was observed (indirect effect: -0.71, p = 0.54). The direct effect (0.79, p = 0.84) and total effect (0.08, p = 0.98) were also non-significant. The statistically non-significant results are to be expected in a feasibility study, while the effect sizes can inform future power analyses in the future trials.

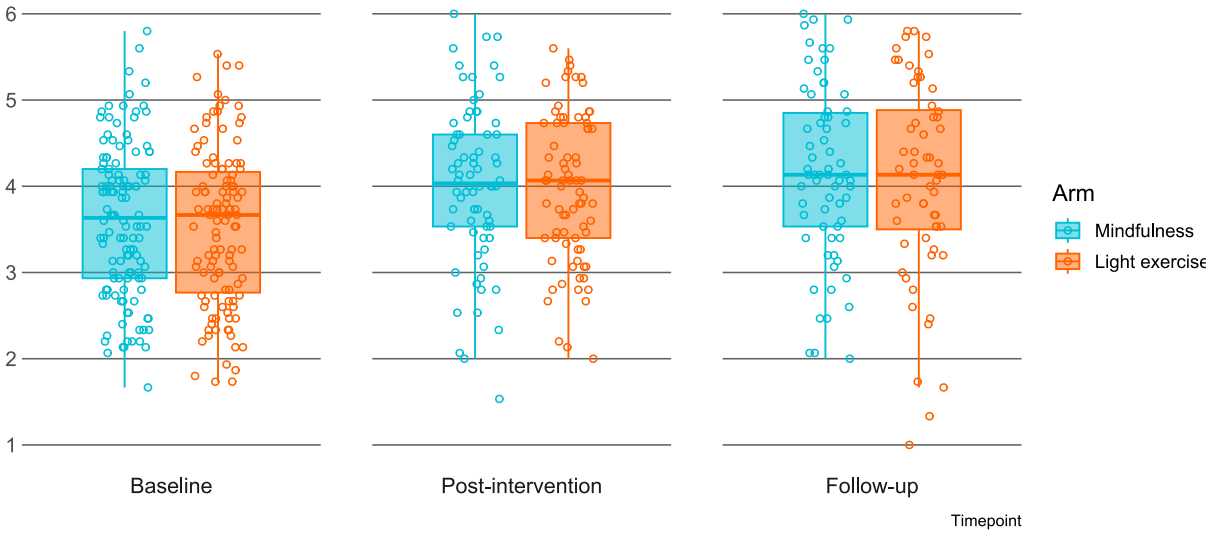

Figure 9. Mindful Attention Awareness Scale score at baseline, post-intervention and 12-week follow-up compared across the two study arms. Figure reused from Vainre et al (2023) under a CC-BY-4.0 license.

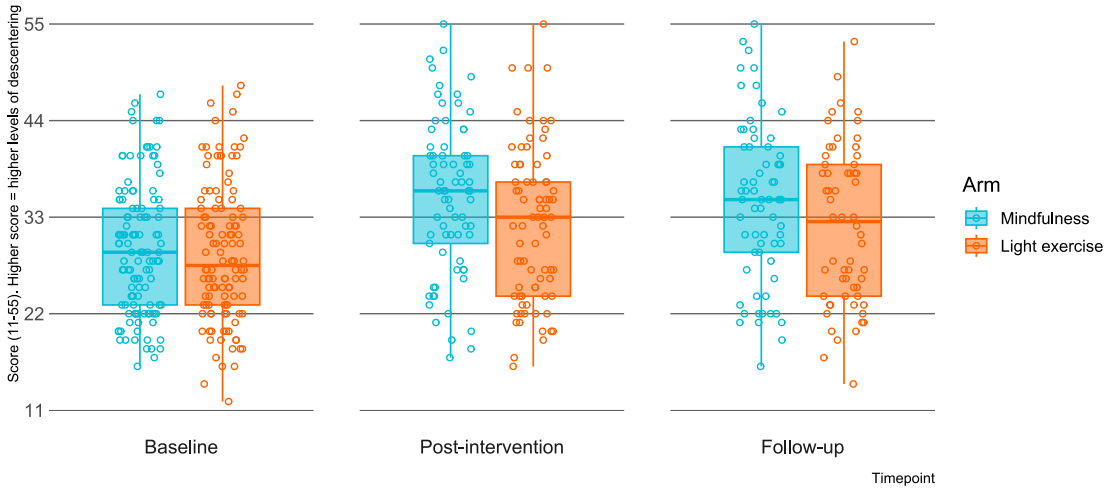

Figure 10. Decentering score at baseline, post-intervention and 12-week follow-up compared across the two study arms. Figure reused from Vainre et al (2023) under a CC-BY-4.0 license.

Discussion

This randomised controlled feasibility trial demonstrated the acceptability of using online MBP and a light physical exercise programmes in a workplace-based study: the attrition as well as outcome missingness rates were similar in both groups. The loss to follow-up was similar to other online trials (Mathieu et al., 2013). We found little evidence on cross-arm contamination in the rates of reported mindfulness practice and physical activity. The proportion of eligible employees that chose to partake the study was low. This reflects workplace well-being uptake in general (Spence, 2015).

While a full-scale trial is feasible, it is not warranted. The online, self-guided mindfulness-based programme (MBP), in comparison to an active control group (light physical exercise) delivered in a similar format, offered negligible additional benefits for work performance either immediately at post-intervention or 12 weeks later. Neither mindfulness nor light exercise improved self-rated work performance: we observed minimal effect sizes for pre-to-post-intervention

change across both arms. There was an improvement in day-to-day ratings of work performance across both arms but again no difference between arms.

This feasibility trial is one of the first to compare the effects of MBPs on work performance against an active control group (Vainre, Dalgleish, et al., 2022). Although diverse definitions of work performance have been used across trials, results align between the current trial and the three prior trials using active control groups. Two prior trials comparing an MBP against offering a list of self-help resources found trivial effect sizes for health-related absences (Bartlett et al., 2017) or work engagement (van Berkel et al., 2014). Similarly, Pipe and colleagues (Pipe et al., 2009) compared an MBP to a "structured educational series" on stress and leadership strategies and found no statistical differences between arms for caring efficacy in 33 nurses (effect size not reported). Our study therefore contributes to growing evidence that MBPs may offer minimal benefit for improving work performance when compared to an active control group.

Currently, there is no standard measure of work performance which allows for comparison between job roles and industries. While absenteeism and presenteeism could be used for that purpose, owing to their relatively low frequency in generally well populations, a considerable sample size is needed to detect between-arm differences. Use of WRFQ allowed us to recruit participants from various employers and without restricting recruitment to a particular role. While this novel approach would have made the results more applicable across industries and roles, there was some evidence of ceiling effects. Further work to identify appropriate outcome measures may be necessary prior to later-phase trials to evaluate work performance, to ensure that study results allow comparison across job roles and industries.

Both the mindfulness and light exercise interventions were anticipated to improve mental health and well-being, and our results demonstrated these benefits. Compared to baseline, there was an improvement in stress, anxiety, depression, decentering and mindfulness in both arms at post-intervention, with these effects sustained at 12 weeks. We found no evidence for superiority of the MBP over the

active control group on mental health outcomes, as found elsewhere (Galante et al., 2021). Indeed, the benefits of exercise for mental health are well established (Galante et al., 2021; Stubbs et al., 2018). The light physical exercise programme was effective in controlling for mental health effects of the MBP, which it was designed to do. Our results further suggest that as a low-intensity workplace-based intervention, exercise may yield similar benefits to mindfulness, when delivered in an online, self-guided manner.

Trivial between-arm effect sizes for cognitive control, regardless of affective valence, provide little support for a full-scale study to investigate the beneficial effect of MBPs via cognitive control when controlled for mental health benefits. Much of the prior evidence for a cognitive control pathway is based on trials with passive control groups (Galante et al., 2021), which may indicate that the previously reported effects are driven by non-specific factors. Recent meta-analyses found MBPs not to have cognitive control effects when compared to active control groups (Dunning et al., 2022; Galante et al., 2021). Interestingly, we did observe that decentering might improve more with MBPs than light physical exercise. Decentering has been previously posited as a core mechanism underpinning the effects of mindfulness (2011), and thus further exploration of decentering may be a more promising avenue for future research.

Clinical implications

Overall, our results do not support progression to a later-phase trial comparing an online MBP to a similarly delivered light exercise course. However, this feasibility trial does provide important insights for future trials of workplace-based interventions. First, sample characteristics should be considered. We found higher take-up in the local authorities, which seemingly have lower well-being budgets than the private sector and may represent an attractive setting for future trials of workplace-based wellbeing programmes. Although the majority of our participants

were female-self-identifying, the majority also reported no caring responsibilities. Future trials and delivery of workplace-based interventions may benefit from exploring how to facilitate engagement for male-identifying employees and carers.

In sum, this feasibility trial indicated that the two interventions used were both acceptable to participants and, based on contamination data, it is feasible to randomise colleagues into different study arms. Yet, online MBPs are unlikely to yield bigger effect sizes than an alternative well-being programme, and indeed, may provide little improvement in work performance at all. Therefore, we found little support for a future superiority trial comparing MBP and light physical exercise. There have been several studies to demonstrate that offering MBPs is better than passive controls (that is, doing nothing) (Dawson et al., 2019; Dunning et al., 2022; Galante et al., 2021; Vainre, Dalgleish, et al., 2022) and our own results do indicate that mental health is likely to be improved by MBPs. However, our findings should be considered when purchasing and/or making recommendations about delivery of workplace-based wellbeing programmes with the specific aim of improving work performance.

Acknowledgements

We would like to thank Camilla Nord, Becky Gilbert, Tierney Lee, Beth Longley, Harriet Armstrong, Norma Silva, Fabiana Mariscotti, Chase Antonacci, Sophie Fielmann, Monica Kullar, Junaid Bhatti, Barry White, Joe Fryer, Joanna Anderson, Anna Bevan, Markus Hausamman, Ben Michaud, Andi Kiissel, Liis Allmäe, Kristin Tammeoks, Dace Apsvalka, and Sebastian Kurten for help with various aspects of the study as well as the employers and participants of the study.

Competing interests

The authors declare no competing interests.

Funding

This study was supported by MRC Cognition and Brain Sciences Unit at the University of Cambridge. MV is supported by Kristjan Jaak degree studies abroad scholarship by Estonia's Education and Youth Board. JG was supported by the National Institute for Health and Care Research (NIHR) Applied Research Collaboration East of England (NIHR ARC EoE) at the Cambridgeshire and Peterborough NHS Foundation Trust. The views expressed are those of the author and not necessarily those of the NIHR or the Department of Health and Social Care. CH is supported by the Economic and Social Research Council (ES/R010781/1) and Australian Research Council (DE20010004) and TD by the UK Medical Research Council (Grant reference: SUAG/043 G101400) and Wellcome (Grant reference: 104908/Z/14/Z, 107496/Z/15/Z). The Wellmind Media offered the mindfulness course at a discount. They were not involved in any aspect of planning the study, data analysis or the decision to publish the results.

Afterword

The Work Engagement and Well-being Study (SWELL) was designed to address two research questions posed in this thesis:

Research Question 3: Could improved cognitive control and/or enhanced mental health be potential mechanisms underlying the effect of mindfulness-based programmes (MBPs) on work performance?

Research Question 4: Is it acceptable and feasible to run a randomised controlled trial (RCT) to investigate the effect of MBPs on work performance?

Research Question 5: What is the effect size of MBPs on work performance when compared to an active control group?

We concluded that, while it is feasible and acceptable (Research Question 4), it may not be worthwhile to run an RCT investigating the effect of MBPs on work performance (Research Question 5). In response to Research Question 3, we found that cognitive control is unlikely to be affected by an MBP. Furthermore, the effect size estimates suggested that mental health did improve as a result of the MBP, but that effect was not larger than produced by completion of a light physical exercise intervention.

Measuring work performance remains an impediment in studies, including MBP trials. Using existing measures, there is no evidence to suggest MBPs improve work performance more than an active control group when measured post-intervention. Yet, it is difficult to estimate whether this is a true lack of effect or reflects the issues with work performance operationalisation (see Chapter 1). We tried measuring work performance in two ways. We mainly relied on collecting data at baseline and at follow-up timepoints. Yet, people's memory might cloud everyday ebbs and flows, resulting in accuracies in post-intervention estimates. We therefore introduced daily monitoring of task performance to capture the finer nuance. While we did observe a statistically significant increase in ratings across the two arms, work performance, when comparing one arm to the other, improved

at a statistically indistinguishable rate. Moreover, the overall completion rate for the daily monitoring measure was low. The missingness may depend on true value – people may have felt like logging their performance when they felt particularly well about it. Furthermore, the very act of filling in the questionnaire may have helped the participants to think about their performance and thus keep on top of their work tasks. With this in mind, while it seems to be feasible to run a RCT to investigate the effect of MBPs on work performance, there is room for improvement in the way work performance is operationalised and measured. This will likely take an interdisciplinary effort to resolve.

The search for active ingredients in psychotherapeutic interventions is on-going and MBPs are no exception. The SWELL study suggests decentering may serve as a mechanism in MBPs: we observed the biggest between-arm effect sizes in decentering (post-intervention: d = 0.24; follow-up: d = 0.22), although they were not statistically significant, as anticipated for a feasibility trial. The suggestion of the importance of decentering as a therapeutic mechanism is not novel (Bennett et al., 2021) but it does highlight an avenue for improving the efficacy of workplace interventions. If, as previously suggested, decentering can be taught as a lower-intensity therapy (Bennett et al., 2022; Knight, 2023), its effectiveness when delivered in the workplace may be worthwhile to investigate. Furthermore, the skill has been theorised to be associated with adaptive performance (see Chapter 5). If teaching decentering does improve adaptive performance, it may prove to be a more cost-effective intervention for organisations, relative to MBPs.

Finally, given the light physical exercise programme matched the mental health benefits of MBPs, conducting a head-to-head comparison RCT could help to drive forward our understanding of the effectiveness of workplace interventions on mental health, as well as work performance. This would enhance the evidence-base in making procurement decisions for interventions to offer within organisations. It would also help us to detect the mechanisms of action for each of these programmes. In turn, this may indicate how we could refine interventions to enhance their effectiveness or, as I have suggested for decentering within

MBPs, isolate specific components to be taught independently, and potentially more cost-effectively. Additionally, such comparative effectiveness trials may help to establish a better understanding of the cost-effectiveness of programmes, given the various benefits the programmes offer. For example, if physical exercise is effective in improving mental health as well as physical health outcomes, while another programme only delivers mental health benefits, it may be worthwhile to consider providing exercise programmes as a default and offering other programmes where a standard physical exercise programme is not feasible (e.g., for someone in a wheelchair) or preferred (e.g., for an ultra-marathon runner).

Chapter 5

General discussion

The work reported in this thesis investigated the effects of mindfulness-based programmes (MBPs) on work performance. I sought to answer five research questions:

Research Question 1: How is individual work performance operationalised in trials assessing the effects of MBPs?

Research Question 2: What is the effectiveness of MPBs for improving work performance based on the current literature?

Research Question 3: Could improved cognitive control and/or enhanced mental health be potential mechanisms underlying the effect of MBPs on work performance?

Research Question 4: Is it feasible to run a randomised controlled trial (RCT) to investigate the effect of MBPs on work performance?

Research Question 5: What is the effect size of MBPs on work performance when compared to an active control group?

To address the questions, I conducted two studies, the findings of which I will subsequently summarise. Then, I will discuss their results in light of the existing research and practice. Thereafter, I will consider the limitations of these studies before concluding the thesis.

Summary of findings

Chapter 2

The systematic review and meta-analysis (Vainre, Dalgleish, et al., 2022) examined the effectiveness of mindfulness-based programmes (MBPs) on work performance by pooling data from randomised controlled trials (RCTs) according to a pre-registered plan (Vainre et al., 2021). It is the first known systematic review to analyse MBPs effects on work performance based on a defined theoretical framework of work performance. We found that there was high overall risk of bias within the reviewed literature, according to the Cochrane Risk of Bias tool (version 2). The use of active control groups was so rare that I could only pool results for passive control groups. The pooled results suggest that offering MBPs did not significantly improve workplace-based task performance up to four weeks after the intervention. The effect size, favouring MBPs compared to a passive control, was moderate but not statistically significant. The longer-term effect (4 weeks to 6 months) of MBPs on task performance was minimal and not statistically significant. Regarding the secondary outcomes, again compared with passive controls, we found no evidence for reducing counterproductive work behaviour. MBPs did show positive effects on adaptive (at least up to 6 months) and contextual performance (at least up to 3 months post-intervention), suggesting potential improvements in people's contributions to organisational culture and others' work, as well as people's ability to adapt to changes. As we did not adjust the alpha level for multiple testing in the secondary analyses, the statistically significant findings could also be due to chance (type I error). Overall, the results suggested that MBPs could have an effect on some aspects of work performance, but these effects depend on the operationalisation of work performance.

Chapters 3 and 4

The feasibility randomised controlled trial (RCT) — the Work Engagement and Well-being Study (SWELL) — summarised in Chapter 3 (Published Protocol (Vainre, Galante, et al., 2022)) and Chapter 4 (Trial and Results (Vainre et al., 2023)) is one of the few existing attempts to investigate potential mechanisms of a mindfulness-based programme (MBP) on work performance using an active control group. At the time, it was one of the first trials to test an online intervention at work, which has become highly relevant after increased use of online delivery in workplaces and universities due to the COVID-19 pandemic. We were interested in whether MBPs could improve work performance in healthy populations. We ran a feasibility trial as there were many procedural uncertainties regarding which outcome measures to use, their effect sizes and whether the recruitment and retention rates would be sustainable for a fully powered study. This meant that, by design, the SWELL Study was not powered to detect statistically significant effects, but rather, we aimed to recruit enough participants to be able to generate estimates of effect sizes.

We concluded that, although a full-scale trial may be feasible, it may not be warranted as the between-group estimate of effect size for work performance (measured using the Work Role Functioning Scale) was close to zero. Use of the light physical exercise intervention as a comparator was effective in controlling for mental health benefits: across all participants, there was a significant improvement in mental health outcomes, regardless of intervention allocation, at post-intervention (Cohen's *d*s between -0.4 and -0.58) and 12-week follow-up (*d*s between -0.25 and -0.4). Meanwhile, the between-group differences on mental health measures at post-intervention were below $d = 0.01$. There were negligible differences on the cognitive control measures, regardless of the affective valence of stimuli, from pre-to post intervention, as well as between-groups at each time point. These results lend no support to for cognitive control being a mechanism of action in MBPs. Decentering, however, was identified as a mechanism of interest for future studies.

Contributions to theory and research design

The work in this thesis advances methodology for investigating the effects of mindfulness-based programmes (MBPs) on work performance. Specifically, the findings could guide decisions on work performance operationalisations in future trials of MBPs, identify mechanisms of action worthy of investigation, and help calculate effect sizes for MBPs effects on work performance, including those with an active control group. This thesis thus fulfils an identified gap in research: despite calls to investigate the effect of MBPs effects on work-related outcomes (e.g., Bartlett et al., 2019; Jamieson & Tuckey, 2017), much of the focus of MBPs delivered in organisations, including universities and work-places, have largely been focussed on mental health outcomes (Bartlett et al., 2019; Dawson et al., 2019). Work performance outcomes are rarely collected (Bartlett et al., 2019; Dawson et al., 2019). This may reflect the researchers' interests as well the difficulty of operationalising work performance. Yet, proponents have suggested a myriad of putative effects ranging from task performance (Dane, 2011; Glomb et al., 2011a; Good et al., 2016; Reb et al., 2017, 2019; Zeller & Lamb, 2011) to creativity (Chapman-Clarke, 2016; Cheung et al., 2020; Good et al., 2016; Henriksen et al., 2020; Kudesia, 2015; S. L. Shapiro et al., 2015), and leadership (Boyatzis, 2015; Glomb et al., 2011a; Good et al., 2016; Reb et al., 2015, 2019). Still, empirical evidence to support these hypotheses has been scant and therefore, there is no consensus as to whether and what types of work and academic performance related behaviours are likely to improve with MBPs.

Implications for operationalisation of work performance in MBP research

The systematic review and meta-analysis reported in Chapter 2 proposes a framework that can guide the operationalisation of work performance in mindfulness-based programmes' (MBPs') research. The systematic review showed

that much of MBP research has focused on effects on contextual and adaptive performance. However, task performance is considered the most relevant domain of work performance (Koopmans et al., 2014b) and consequently it is the domain most often measured, for example in healthcare (Krijgsheld et al., 2022). When it comes to MBPs, the systematic review did not offer conclusive evidence for or against MBPs effects on task performance: the effect was positive, moderate in size based on Cohen's rule of thumb (J. Cohen, 1988, 1992) but not statistically significant. An update of the analysis (underway at the time of submission) may provide a more precise pooled estimate. Contextual performance and adaptive performance domains were the most investigated and demonstrated statistically significant yet modest effect sizes. These findings highlight the disparity in the discourse about workplace MBPs effects and the evidence base. As task performance is the archetype of work performance, many researchers have argued for MBPs benefits specifically on task performance (Baltzell, 2016; Dane, 2011; Glomb et al., 2011a; Good et al., 2016; Hall, 2015; Reb et al., 2017, 2019; Zeller & Lamb, 2011) citing improved cognitive control as a mechanism (Chaskalson, 2011; Dane, 2011; Garland et al., 2017; Goilean et al., 2021; Good et al., 2016; S. L. Shapiro et al., 2015). The systematic review therefore highlights a significant mismatch between the work performance aspects that are most investigated and likely to be effective, and the aspects that are viewed by advocates and the public as the most important, and as most likely to be improved by MBPs.

Future research can build on these findings by more carefully considering how to operationalise work performance. In addition to the likely effect sizes reported in the meta-analysis (Chapter 2, Vainre, Dalgleish, et al., 2022), triallists ought to consider other factors when operationalising work performance, such as the research question, the hypothesised pathway of action in the programme (MBP) and the type of work performance of interest (e.g., most valued in the workplace or in need of support). For example, when interested in MBPs effects on contextual performance, triallists may consider indexing work engagement with the Utrecht Work Engagement Scale (UWES, Schaufeli & Bakker, 2004), a common measure in MBP workplace trials (Chapter 2, Vainre, Dalgleish, et al., 2022). Before deciding upon this measurement, researchers should scrutinise the mechanism of

the MBP of interest that should affect UWES' total score given the scale consists of three subscales (vigour: "At my work, I feel bursting with energy", dedication: "I am proud on the work that I do" and absorption: "I feel happy when I am working intensely"). This exercise may help in deciding which sub-construct of contextual performance to index and which outcome measure to use. Additionally, practical considerations, like the type of work performance valued in the industry or in the roles investigated, should also figure in the operationalisation of work performance in MBP trials. For example, the ability to navigate complex decisions while considering others' needs (adaptive performance) may be more pertinent in client-facing roles and industries. Similarly, an employer may be undergoing a specific difficulty where organisational-level changes need a boost by an individual-level programme such as an MBP (e.g., if high levels of stress among employees have led to siloed or isolated working habits as an act of self-preservation). In addition to reducing stress, MBPs could have the added benefit of encouraging employees to contribute to the organisational environment (i.e., improving contextual work performance).

The exercise of carefully considering the operationalisation and its mechanism(s) of action is important in reducing inflation in claims regarding MBPs effects on work performance outcomes. This is a problem that the findings in Chapter 2 highlighted. The hype comes in three forms. First, there is poor operationalisation of what work performance constructs would improve as a result of an MBP. At best, we can currently suggest an effect on contextual or adaptive performance, i.e., domain-level effects compared to passive control. Therefore, previous claims that mindfulness improves specific sub-domain constructs like decision-making (Glomb et al., 2011a; Karelaia & Reb, 2014; Parsons et al., 2020; Schmitzer-Torbert, 2020), creativity and innovation (Chapman-Clarke, 2016; Cheung et al., 2020; Good et al., 2016; Henriksen et al., 2020; Kudesia, 2015; S. L. Shapiro et al., 2015), leadership (Boyatzis, 2015; Glomb et al., 2011a; Good et al., 2016; Reb et al., 2015, 2019), working relationships (Chapman-Clarke, 2016; Good et al., 2016), teamwork (Good et al., 2016) and conflict management (Good et al., 2016) are best seen as hypotheses. Yet, both the synthesis of existing evidence and original investigation in this thesis indicate that there is no good quality empirical evidence to support

these hypotheses. Such claims therefore rely on cherry-picked evidence, a practice that hinders the field if it leads triallists to track outcomes that are difficult to operationalise and measure.

Second, the pooled effect sizes across the work performance domains were small, so their significance is difficult to interpret. In other words, there is little evidence to estimate their practical meaningfulness (akin to clinical significance); that is, the effect size on these mostly self-reported measures, that produces a change in actual work behaviour (Anvari et al., 2023). The efforts to consolidate definitions of work performance is on-going and the next steps in validation of various work performance outcomes (e.g., Abma et al., 2018; Koopmans et al., 2014a, 2016; Ramos-Villagrasa et al., 2019) should also involve establishing benchmarks for meaningful change.

Third, the effect of MBP on a work performance construct may have differential benefits to the employer and the employee (if there is an effect) (see also Bóo et al., 2019). For example, a study found that trait mindfulness correlated positively with better decision-making, particularly, there was an increased chance that the individual decided to discontinue practices that no longer supported their goal (Schmitzer-Torbert, 2020). If employees become more likely to cease practices or end projects that are no longer beneficial, it may improve organisation's profits while making employees work more meaningful; yet if employees become more likely to resign their jobs, the overall improvement may not extend to employers. The benefit of the MBP may therefore not necessarily extend to employers and it could depend on organisational culture: are employees able to propose changes to their work practices and goals, or is their best option to seek these opportunities elsewhere?

To summarise, the evidence presented in the thesis suggest that careful consideration should be given to how work performance is measured, including in MBP trials. A more rigorous approach will improve the quality of future trials and the cumulative evidence base. Currently, researchers as well as practitioners

ought to remain cautious when claiming MBPs effects on work performance and be specific about what they mean by the term *work performance*.

Implications on potential mechanisms of MBPs' effects on work performance

While the work reported in Chapter 2 identified adaptive performance as a potential workplace outcome of interest for mindfulness-based programmes (MBPs), the Work Engagement and Well-being Study (SWELL, Chapters 3 and 4) lends it initial theoretical support. Namely, in Chapter 4, we identified decentering as a worthwhile mechanism of action thus supporting Crane and her colleagues' proposal on why MBPs work (2017). If decentering is confirmed as a mechanism of action in MBPs, it would be worthwhile to concentrate investigative efforts on the constructs of work performance that are more likely to be affected by meta-cognitive skills. Indeed, Jundt and colleagues (2015) have suggested that decentering skills may lead to adaptive work performance. If this is the main pathway of effect, it could mean that MBPs' benefits may manifest in limited contexts, i.e., among roles that rely heavily on understanding others' needs and making decisions in uncertainty, such as in sales, education, healthcare, and social care. These considerations are crucial when planning trials, as they may imply that MBPs' effects on work performance could be more likely to manifest in some contexts, and less so in others.

Additionally, it will be important to determine whether any MBP-driven improvements in decentering or adaptive performance are larger than other interventions that also teach decentering skills. As many interventions rely on it, decentering is not a skill specific to MBPs (Bennett et al., 2021). Moreover, decentering could be taught on its own (Bennett et al., 2022; Knight, 2023), although the feasibility of doing that in an organisational context has not been tested. If decentering is the main mechanism of MBPs effect on work performance, it may be more worthwhile to simply teach decentering. This is an

interesting avenue for future research that may also help to refine MBPs delivery, either by enhanced the decentering-focussed aspects of intervention, or by indicating in which workplaces MBPs are likely to be most beneficial.

The SWELL study provided no support to further investigate the mechanism of cognitive control in the context of MBPs compared to other programmes. This mechanism has been suggested to be important in previous theorisations of mindfulness meditation (Bishop et al., 2004; Hölzel et al., 2011; Vago & Silbersweig, 2012) with some emerging evidence from mostly lab-based studies and meta-analyses that mixed randomised designs with cohort studies and included meditation practices in general, not just MBPs (Cásedas et al., 2020; Im et al., 2021; Millett et al., 2021; Yakobi et al., 2021; Zainal & Newman, 2023). We hypothesised that by segregating affectively negative and neutral types of stimuli, we are able to better distinguish the cognitive control mechanism that a MBP activates. Yet, we saw no effects in cognitive control in neutral vs negative information when comparing the effects between MBP and light exercise. Admittedly, our results may have reflected a poor selection or implementation of the cognitive tasks we used. However, recent meta-analyses investigating MBPs effects similarly found no benefit of MBPs on cognitive control compared to active controls (Dunning et al., 2022; Galante et al., 2021). Considering our results together with these studies, it seems likely that the true effect on cognitive control outcomes is null. Anyone wishing to further investigate the pathway of cognitive control in MBPs at work as well as in other contexts should aim to minimise non-specific effects of MBPs to improve the current evidence-base regarding cognitive control specifically or executive functions more broadly.

Implications on choosing an active control group in MBPs' trials

The randomised controlled trial (RCT, Chapters 3 and 4) demonstrated that light physical exercise is an effective intervention to control for mental health effects

of mindfulness-based programmes (MBPs), at least when delivered online. Not only did the light physical exercise programme (LE) produce similar effect sizes to the MBP, but the study participants also engaged with both interventions similarly. LE thus fulfilled many criteria for an appropriate control group (Kinser & Robins, 2013): it was ethical as it improved mental health (as also evidenced in Galante et al., 2016) as well as feasible and attractive, based on the engagement information. LE also lends some external validity to the Work Engagement and Well-being Study (SWELL) (Rebok, 2015) as it is conceivably an intervention that could be chosen instead of a MBP in organisations. Yet, as the SWELL study was not a comparative effectiveness trial targeted at mental health outcomes, no conclusions can be drawn regarding which is the best choice of an intervention at workplace. Still, the effect sizes may inform future triallists and their funders wishing to undertake a head-to-head comparison between an MBP and light physical exercise.

Implications for RCTs of MBPs

I would like to highlight three implications for planning of future randomised controlled trials (RCTs) based on the findings reported in this thesis: (a) calculating sample size for trials of mindfulness-based programmes (MBPs), relative to an active intervention, (b) running online RCTs and (c) scientific rigour to support quality of evidence.

To begin with, the Work Engagement and Well-being Study (SWELL) (Chapters 3 and 4) provides essential information for sample size calculations in future studies focused on MBPs' mechanisms of actions. Unpicking the mechanisms of action involved in any work performance effects assumes the use of an active control group, and sample size calculations to account for the smallest expected effect size. Yet, when planning the SWELL study (Chapters 3 and 4), we found little information on reasonable effect sizes to expect (see Chapter 3 Foreword for details). Future triallists will hopefully benefit from our study using an active

control group in the context of MBPs for work performance if they wish to complete sample size calculations for their own study.

The SWELL study demonstrated the feasibility of running an online workplace-based RCT. At the time of writing the protocol, the SWELL study was one of the few RCTs to test online interventions in the workplace. Online programmes may seem attractive to employers and universities as they are cheaper and allow flexibility in attendance. The flexibility may also mean reduced engagement and thus effectiveness. The SWELL study, along with the emergence of online intervention trials brought on by the COVID-19 pandemic, will contribute to retention rates' predictions. Furthermore, the SWELL study is unique, to my knowledge, in that it used a realistic alternative control condition that could plausibly be implemented in the workplace.

Finally, yet perhaps most importantly, the systematic review (Chapter 2) highlighted the need for more rigorous research practices to reduce the risk of bias in trials of MBPs. We rated all included studies, except one, to have a high overall risk of bias. This result is not uncommon in MBP research (Galante et al., 2021). In behavioural interventions research, there are factors contributing to the risk of bias that are difficult to avoid, for example blinding participants or facilitators (in case of face-to-face delivery or live via a teleconference). Yet many practices are easy and low-cost to implement, including more rigorous randomisation and allocation concealment processes, pre-registration of outcomes and planned analyses, including a clear distinction between exploratory and confirmatory analyses. My thesis emphasises the critical need for high quality trials, to enable conclusions regarding what, whom, and how MBPs may benefit, when completed in the workplace, but also elsewhere.

Implications for practice

There was no suggestion that mindfulness-based programmes (MBPs) would be more effective in supporting mental health and work performance than light

physical exercise (see Chapter 4). This thesis further highlights that the evidence that MBPs may improve work performance is of low quality, the expected effects are likely to be small and may depend on the context.

When interested in optimal allocation of resources and having work performance as a goal in mind, this thesis concurs that employers' best investments may continue to be to reduce work-related risks factors for common mental health problems: poor job design, occupational uncertainty and lack of value and respect at work (Harvey et al., 2017). Indeed, National Institute for Health and Clinical Excellence (NICE) Guidance suggests that individual-level approaches (offering access to yoga or mindfulness) should be offered only in addition to organisation-wide approaches that cultivate safety and job quality (National Institute for Health and Clinical Excellence, 2022).

Before deciding to implement an individual-level health promotion programme, several considerations should be weighed. Organisations are free to choose which bonuses to offer to their employees and students and can make their decisions without public expectation to rely on empirical evidence. A decision to procure a particular well-being or work performance improvement programme may be due to its availability, financial cost, or perceived attractiveness, rather than effectiveness (personal communication with one of the employers in the Work Engagement and Well-being Study (SWELL)). There are a wide range of effective organisational practices to promote mental health and work performance in the workplace are available, including flexible working arrangements, supporting career opportunities, and mitigating factors that may erode physical and mental health in the working environment (National Institute for Health and Clinical Excellence, 2022). These practices are relatively equitable in that they can be offered to all staff. Meanwhile, individual-level programmes do not enjoy these benefits. To begin with, the offer of universal individual-level interventions may not be effective in improving well-being (Fleming, 2023; Jones et al., 2019) as people may not take part in them (Spence, 2015). Individual-level programmes require workers to invest their time, which may come at the expense of free time if work tasks are not reorganised to accommodate the participation in the

programme. Consequently, individual-level programmes may nurture social injustice (E. A. Brown, 2017; Chen, 2022); for example, they are more accessible for employees with less caring responsibilities. Additionally, employees' personal information may be compromised: in order to participate, employees could be asked to share sensitive health information with their employers, which could be misused or involuntarily leaked (E. A. Brown, 2017). Such risks should be considered and mitigated. The evidence in this thesis supports the initiation of informed conversations among the Human Resources specialists regarding which types of individual-level programmes seem attractive to employers and employees, as well as what are the likely effects of MBPs.

Limitations

The findings reported in this thesis need to be considered along with several limitations which were detailed in the corresponding chapters (Chapter 2, Chapter 4). This section expands upon three aspects: the operationalisation of mindfulness-based programmes (MBPs), work performance and the risk of bias.

MBPs remain a diverse group of interventions, even when limited to Crane and colleagues' definition of MBPs (2017). It has been argued that even Mindfulness-Based Stress Reduction, the quintessential MBP, may be teaching more than mindfulness (Rosch, 2015). The programme theory of MBPs therefore needs further scrutiny (see Oman, 2023 for discussion). In the meantime, within randomised controlled trials (RCTs), the descriptions of the programme used to teach mindfulness practices often leave room for interpretation as to their fit to the MBP definition. Given this subjectivity, it is difficult to estimate the extent to which the meta-analyses pooling effects of MBPs are comparable in their independent variable. The reproducibility of the application of the MBP-criterion in meta-analysis has to my knowledge not been tested. The effects reported in meta-analyses may therefore reflect the Crane-consistent school of thought in

the interpretation of the MBP definition and to a lesser extent, the effect of MBPs themselves.

Likewise, the measurement of individual work performance remains an important limitation in any study, including in those investigating MBPs effects. While work performance is widely measured, its definition and thus operationalisation remain disputed, as discussed in Chapter 1. Consequently, the lack of a psychometrically well-established measurement of individual work performance that would capture its many facets (Ramos-Villagrasa et al., 2019) poses a considerable caveat. The development of tools that can be used across industries and are sensitive to change is needed.

The operationalisation of work performance will have impacted results in several ways. The results of the meta-analysis (Chapter 2) do not account for all potential effects of MBPs on work- and study-related performance, given that we excluded outcomes that were job-related but did not fit to Koopmans' (2011) framework (e.g., job satisfaction, work motivation, job-related stress and burn-out). The framework helped us to distinguish between work performance and other work-related constructs that are antecedent or descendant of work performance (e.g., work motivation or satisfaction). Similarly, our operationalisation of work performance in the Work Engagement and Well-being Study (SWELL) may have affected our ability to detect effects. As discussed in Chapter 3, the focus in the trial was on task performance due to the mechanistic pathway under review. We used the Work Role Functioning Questionnaire (WRFQ) (Abma et al., 2018) which indexed people's perceived ability to complete work-related tasks. The WRFQ suffered from ceiling effects (Chapter 4). This may have impeded our ability to detect between-arm effects. A more generic measure for work performance (e.g., The Individual Work Performance Questionnaire (IWPQ) (Koopmans et al., 2014a)), may have been more sensitive in identifying the main effect and would have allowed exploration of the MBP effect across different domains of work performance. Yet, as outlined in the Foreword of Chapter 3, the IWPQ did not seem to be a viable choice for an outcome given the interest in cognitive control pathway. When exploring other mechanisms or indeed other research questions,

the IWPQ may be a candidate if the outcome of interest is individual work performance.

The risk of bias is a noteworthy limitation in both studies. In the systematic review (Chapter 2), we excluded non-peer reviewed publications (such as government reports and doctoral theses). This could have heightened the effects of publication bias in the meta-analysis. The practice of preregistration is not widespread in MBP research, so it is hard to know the extent of this bias. In the RCT, we were not able to blind the participants from knowing the type of intervention they were receiving. We did ask for their preferences of the interventions, but only at post-intervention, where the responses may have been influenced by their experiences of the intervention. The fact that all the outcomes were self-reported also contributes to risk of bias, although the risk here is lower than in waitlist controlled RCTs. Collecting others' reports is logistically challenging and thus rarely undertaken, except in studies involving vulnerable populations where parents or carers may be involved in the study. In work performance assessments, self-reported questionnaires are currently one of the more reliable generic methods for data collection (Ramos-Villagrasa et al., 2019). Overall, the results in this thesis should be interpreted in the context of construct conceptualisation and with the high risk of bias in mind. However, despite these factors, the work in this thesis offers a solid foundation for future studies of MBPs in workplace contexts and beyond.

Conclusions

This thesis explored the impact of mindfulness-based programmes (MBPs) on work performance. The systematic review and meta-analysis in this thesis demonstrated that while task performance is not significantly improved, there is low-quality evidence that MBPs could enhance other aspects work performance, specifically contextual performance (actions people take that contribute to the

way the organisation runs) and adaptive performance (adjusting to changes and showing resilience).

The randomised controlled feasibility trial (RCT) concluded that a full-scale trial to investigate cognitive control as a mechanism of action in MBPs on work performance is feasible. Yet, such a trial is not warranted based on our effect size estimates. The study found no evidence to suggest that the assessed MBP would be more effective in improving work performance than light physical exercise: we found close to zero between-group effect size estimates. However, we demonstrated the feasibility of recruiting participants into an online study where colleagues are randomised into different programmes, and also provided evidence that workplace-based light physical exercise is likely to be an effective intervention for mental health outcomes. The RCT suggested that cognitive control is not a mechanism that mediates the effects of MBPs on work performance relative to an active control, while decentering emerged as a potential mechanism of interest.

The research in this thesis highlights the importance and complexity of investigating work-related outcomes of MBPs. To further advance the field, we need more rigorous research practices to reduce bias in trials, and to refine definitions of work performance and MBPs. Applying the insights from the thesis will help to consolidate the efforts in investigating mindfulness-based programmes' effect on work performance, along with the mechanisms underlying any such effects.

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Appendices

Supplementary Materials

Supplementary material for Chapter 2. Mindfulness training for work performance: A systematic review and meta-analysis of randomised controlled trials

Supplementary Material 1: PRISMA and Search terms

The PRISMA Checklist

Section and Topic	Item #	Checklist item	Location where item is reported
TITLE			
Title	1	Identify the report as a systematic review.	Title page
ABSTRACT			
Abstract	2	See the PRISMA 2020 for Abstracts checklist.	p 3
INTRODUCTION			
Rationale	3	Describe the rationale for the review in the context of existing knowledge.	Introduction
Objectives	4	Provide an explicit statement of the objective(s) or question(s) the review addresses.	p 6
METHODS			
Eligibility criteria	5	Specify the inclusion and exclusion criteria for the review and how studies were grouped for the syntheses.	p 7
Information sources	6	Specify all databases, registers, websites, organisations, reference lists and other sources searched or consulted to identify studies. Specify the date when each source was last searched or consulted.	pp 7–8

Search strategy	7	Present the full search strategies for all databases, registers and websites, including any filters and limits used.	Supplementary materials pp 3–14
Selection process	8	Specify the methods used to decide whether a study met the inclusion criteria of the review, including how many reviewers screened each record and each report retrieved, whether they worked independently, and if applicable, details of automation tools used in the process.	p 8
Data collection process	9	Specify the methods used to collect data from reports, including how many reviewers collected data from each report, whether they worked independently, any processes for obtaining or confirming data from study investigators, and if applicable, details of automation tools used in the process.	p 8
Data items	10a	List and define all outcomes for which data were sought. Specify whether all results that were compatible with each outcome domain in each study were sought (e.g. for all measures, time points, analyses), and if not, the methods used to decide which results to collect.	p 8–10, Supplementary materials pp 15–16
	10b	List and define all other variables for which data were sought (e.g. participant and intervention characteristics, funding sources). Describe any assumptions made about any missing or unclear information.	p 8–10, Supplementary materials pp 15–16
Study risk of bias assessment	11	Specify the methods used to assess risk of bias in the included studies, including details of the tool(s) used, how many reviewers assessed each study and whether they worked independently, and if applicable, details of automation tools used in the process.	p 10; Supplementary materials pp 16–19
Effect measures	12	Specify for each outcome the effect measure(s) (e.g. risk ratio, mean difference) used in the synthesis or presentation of results.	pp 10–12
Synthesis methods	13a	Describe the processes used to decide which studies were eligible for each synthesis (e.g. tabulating the study intervention characteristics and comparing against the planned groups for each synthesis (item #5)).	pp 10–12
	13b	Describe any methods required to prepare the data for presentation or synthesis, such as handling of missing summary statistics, or data conversions.	pp 10–12
	13c	Describe any methods used to tabulate or visually display results of individual studies and syntheses.	pp 10–12
	13d	Describe any methods used to synthesize results and provide a rationale for the choice(s). If meta-analysis was performed, describe the model(s), method(s) to identify the presence and extent of statistical heterogeneity, and software package(s) used.	pp 10–12

	13e	Describe any methods used to explore possible causes of heterogeneity among study results (e.g. subgroup analysis, meta-regression).	pp 10–12
	13f	Describe any sensitivity analyses conducted to assess robustness of the synthesized results.	pp 10–12
Reporting bias assessment	14	Describe any methods used to assess risk of bias due to missing results in a synthesis (arising from reporting biases).	p 8, p 10
Certainty assessment	15	Describe any methods used to assess certainty (or confidence) in the body of evidence for an outcome.	p 10
RESULTS			
Study selection	16a	Describe the results of the search and selection process, from the number of records identified in the search to the number of studies included in the review, ideally using a flow diagram.	p 14
	16b	Cite studies that might appear to meet the inclusion criteria, but which were excluded, and explain why they were excluded.	p 14
Study characteristics	17	Cite each included study and present its characteristics.	p 15–18
Risk of bias in studies	18	Present assessments of risk of bias for each included study.	Supplementary materials pp 21–36
Results of individual studies	19	For all outcomes, present, for each study: (a) summary statistics for each group (where appropriate) and (b) an effect estimate and its precision (e.g. confidence/credible interval), ideally using structured tables or plots.	pp 21–22, Supplementary materials pp 48–58
Results of syntheses	20a	For each synthesis, briefly summarise the characteristics and risk of bias among contributing studies.	pp 22–23
	20b	Present results of all statistical syntheses conducted. If meta-analysis was done, present for each the summary estimate and its precision (e.g. confidence/credible interval) and measures of statistical heterogeneity. If comparing groups, describe the direction of the effect.	pp 21–22, Supplementary materials pp 52–58
	20c	Present results of all investigations of possible causes of heterogeneity among study results.	pp 21–22, Supplementary

			materials pp 52–55
	20d	Present results of all sensitivity analyses conducted to assess the robustness of the synthesized results.	NA
Reporting biases	21	Present assessments of risk of bias due to missing results (arising from reporting biases) for each synthesis assessed.	Supplementary materials p 37
Certainty of evidence	22	Present assessments of certainty (or confidence) in the body of evidence for each outcome assessed.	Supplementary materials p 20
DISCUSSION			
Discussion	23a	Provide a general interpretation of the results in the context of other evidence.	pp 23–24
	23b	Discuss any limitations of the evidence included in the review.	pp 24–26
	23c	Discuss any limitations of the review processes used.	p 24
	23d	Discuss implications of the results for practice, policy, and future research.	p 25
OTHER INFORMATION			
Registration and protocol	24a	Provide registration information for the review, including register name and registration number, or state that the review was not registered.	Title page, p 12
	24b	Indicate where the review protocol can be accessed, or state that a protocol was not prepared.	Title page, p 12
	24c	Describe and explain any amendments to information provided at registration or in the protocol.	p 12, also see protocol updates
Support	25	Describe sources of financial or non-financial support for the review, and the role of the funders or sponsors in the review.	Title page
Competing interests	26	Declare any competing interests of review authors.	Title page
Availability of data, code	27	Report which of the following are publicly available and where they can be found: template data collection forms; data extracted from included studies; data used for all analyses; analytic code; any other materials used in the review.	Title page, p 12

and other materials

Notes. From: Page MJ, McKenzie JE, Bossuyt PM, Boutron I, Hoffmann TC, Mulrow CD, et al. The PRISMA 2020 statement: an updated guideline for reporting systematic reviews. BMJ 2021;372:n71. doi: 10.1136/bmj.n71; For more information, visit: http://www.prisma-statement.org/

Search terms

ASSIA

Date of search	30/3/2021
Number of records found	798

Search strategy

employ* OR job* OR labo* OR occupation* OR personn* OR staff OR unemploy* OR work* OR MAINSUBJECT.EXACT("Academic staff") OR MAINSUBJECT.EXACT.EXPLODE("Academic work") OR MAINSUBJECT.EXACT.EXPLODE("Employability") OR MAINSUBJECT.EXACT.EXPLODE("Employee participation") OR MAINSUBJECT.EXACT.EXPLODE("Employees") OR MAINSUBJECT.EXACT.EXPLODE("Employers") OR MAINSUBJECT.EXACT.EXPLODE("Employment based education") OR MAINSUBJECT.EXACT.EXPLODE("Employment") OR MAINSUBJECT.EXACT.EXPLODE("Group work") OR MAINSUBJECT.EXACT.EXPLODE("Labor") OR MAINSUBJECT.EXACT.EXPLODE("Labor force") OR MAINSUBJECT.EXACT.EXPLODE("Personnel management") OR MAINSUBJECT.EXACT.EXPLODE("Psychiatric staff nurses") OR MAINSUBJECT.EXACT.EXPLODE("Teamwork") OR MAINSUBJECT.EXACT.EXPLODE("Security staff") OR MAINSUBJECT.EXACT.EXPLODE("Staff cafeteria") OR MAINSUBJECT.EXACT.EXPLODE("Staff development") OR MAINSUBJECT.EXACT.EXPLODE("Staff grade") OR MAINSUBJECT.EXACT.EXPLODE("Staff nurses") OR MAINSUBJECT.EXACT.EXPLODE("Staff") OR MAINSUBJECT.EXACT.EXPLODE("Staffing") OR MAINSUBJECT.EXACT.EXPLODE("Unemployed") OR MAINSUBJECT.EXACT.EXPLODE("Unemployment") OR MAINSUBJECT.EXACT.EXPLODE("Work") OR MAINSUBJECT.EXACT.EXPLODE("Workplaces") OR academi* OR college* OR educat* OR HEI OR student* OR university OR (higher NEAR/1 education) OR MAINSUBJECT.EXACT.EXPLODE("Colleges & universities") OR MAINSUBJECT.EXACT("Academic staff") OR MAINSUBJECT.EXACT.EXPLODE("Academic work") OR MAINSUBJECT.EXACT.EXPLODE("Adult education") OR MAINSUBJECT.EXACT.EXPLODE("Community colleges") OR MAINSUBJECT.EXACT.EXPLODE("Graduate studies") OR MAINSUBJECT.EXACT.EXPLODE("Education") OR MAINSUBJECT.EXACT.EXPLODE("Continuing education") OR MAINSUBJECT.EXACT.EXPLODE("Higher education") OR MAINSUBJECT.EXACT.EXPLODE("Postdoctoral education") OR MAINSUBJECT.EXACT.EXPLODE("Student health services") OR

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MAINSUBJECT.EXACT.EXPLODE("Clinical trials"))

Cochrane Central Register of Controlled Trials (CENTRAL)

Date of search	1st April 2021
Number of records found	98

Search strategy

(employ or job* or labo* or occupation* or personn* or staff or unemploy* or work*) or (academi* or college* or educat* or HEI or student* or university) in All Text AND ab(mindful* OR meditat* OR MBCT OR MBSR) or ti(mindful* OR meditat* OR MBCT OR MBSR) or MH "Mindfulness" or MH "Meditation" in All Text AND (absen* or achiev* or adher* or attainm* or attend* or burnout or conduct* or disengage* or distress* or effective* or effic* or engagem* or error* or function* or mistak* or motivate* or output* or perform* or present* or procastin* or product* or stress* or underperform*) in All Text

Embase

Date of search	30th March 2021
Number of records found	1641

Search strategy

((employ*.mp. or job*.mp. or labo*.mp. or occupation*.mp. or personn*.mp. or staff.mp. or unemploy*.mp. or work*.mp.) or (exp administrative personnel/ or exp clinical laboratory personnel/ or exp construction work/ or exp dental personnel/ or exp dental staff/ or exp field work/ or exp health care personnel management/ or exp health care personnel/ or exp hospital personnel management/ or exp hospital personnel/ or exp job accommodation/ or exp laboratory personnel/ or exp medical personnel/ or exp medical staff/ or exp mental health care personnel/ or exp military personnel/ or exp nursing home personnel/ or exp nursing staff/ or exp occupational health/ or exp operating room personnel/ or exp paramedical personnel/ or exp personnel management/ or exp personnel shortage/ or exp personnel/ or exp religious personnel/ or exp rescue personnel/ or exp rescue work/ or exp return to work/ or exp shift work/ or exp social work student/ or exp staff/ or staff nurse/ or exp staff training/ or exp telecommuting/ or exp unemployment/ or exp work resumption/ or exp work schedule/ or exp work/ or exp working time/ or exp workplace/) or (academi*.mp. or college*.mp. or educat*.mp. or HEI.mp. or student*.mp. or university.mp.) or (higher adj1 education).mp. or (exp adult education/ or allied health student/ or athletic training student/ or audiology student/ or baccalaureate nursing student/ or chiropractic student/ or exp clinical education/ or exp college student/ or exp college/ or exp community college/ or exp continuing education/ or dental hygiene student/ or dental student/ or dietetics student/ or disabled student/ or exp doctoral education/ or exp education program/ or education/ or foreign student/ or graduate nursing student/ or graduate student/ or health student/ or exp masters education/ or medical student/ or midwifery student/ or non-medical student/ or nontraditional student/ or nursing student/ or occupational therapy student/ or paramedical student/ or pharmacy student/ or PhD student/ or physical therapy student/ or physician assistant student/ or exp postdoctoral education/ or exp postgraduate education/ or postgraduate student/ or premedical student/ or

professional student relation/ or public health student/ or research student/ or respiratory therapy student/ or social work student/ or student assistance program/ or student athlete/ or exp student retention/ or student satisfaction/ or student/ or undergraduate student/ or exp university student/ or exp university/ or veterinary student/)) AND ((Mindful*.ti,ab. or Meditat*.ti,ab. or mbct.ti,ab. or mbsr.ti,ab.) or (exp focused attention meditation/ or exp meditation/ or exp mindfulness/ or exp mindfulness meditation/ or exp open monitoring meditation/ or exp transcendental meditation/)) AND ((absen*.mp or achiev*.mp or adher*.mp or attainm*.mp or attend*.mp or burnout.mp. or conduct*.mp or disengage*.mp. or distress*.mp. or effective*.mp or effic*.mp or engagem*.mp or error*.mp or function*.mp. or mistak*.mp or motivate*.mp. or output*.mp. or perform*.mp or present*.mp or procrast*.mp or product*.mp or stress*.mp or underperform*.mp) or (job adj2 strain).mp or (exp absenteeism/ or exp academic achievement/ or exp academic failure/ or exp academic success/ or exp achievement/ or exp athletic performance/ or exp behavioral stress/ or exp burnout/ or exp diagnostic error/ or exp distress syndrome/ or exp error/ or exp health personnel attitude/ or exp job experience/ or exp job performance/ or exp job satisfaction/ or exp job security/ or exp job stress/ or exp Maslach Burnout Inventory/ or exp Maslach Burnout Inventory-General Survey/ or exp Maslach Burnout Inventory-Human Services Survey/ or exp Maslach Burnout Inventory-Student Survey/ or exp medical error/ or exp medication error/ or exp mental capacity/ or exp motivation/ or exp performance/ or exp presenteeism/ or procrastination/ or exp productivity/ or exp professional burnout/ or exp "quality of working life"/ or stress/ or student burnout/ or exp task performance/ or exp work capacity/ or exp work engagement/ or exp work environment/ or exp work experience/ or exp work-life balance/ or exp workload/)) AND ((Clinical Trial/ or Randomized Controlled Trial/ or controlled clinical trial/ or multicenter study/ or Phase 3 clinical trial/ or Phase 4 clinical trial/ or exp RANDOMIZATION/ or Single Blind Procedure/ or Double Blind Procedure/ Or Crossover Procedure/ or PLACEBO/ or randomi?ed controlled trial$.tw. or rct.tw. or (random$ adj2 allocat$).tw. or single blind$.tw. or double blind$.tw or ((treble or triple) adj blind$).tw. or placebo$.tw. or Prospective Study) NOT (Case Study/ or case report.tw. or abstract report/ or letter/ or Conference proceeding.pt. Or Conference abstract.pt. or Editorial.pt. or Letter.pt. or Note.pt.))

ERIC

Date of search 30th March 2021
Number of records found 34

Search strategy

#	Search term
1	TX employ* or TX job* or TX labo* or TX occupation* or TX personn* or TX staff or TX unemploy* or TX work*
3	TX academi* or TX college* or TX educat* or TX HEI or TX student* or TX university
4	TX (higher N1 education)
5	DE "College Environment" OR DE "College Students" OR DE "College Freshmen" OR DE "College Seniors" OR DE "College Transfer Students" OR DE "First Generation College Students" OR DE "Graduate Students" OR DE "In State Students" OR DE "On Campus Students" OR DE "Out of State Students" OR DE "Preservice Teachers" OR DE "Two Year College Students" OR DE "Undergraduate Students" OR DE "Colleges" OR DE "Agricultural Colleges" OR DE "Black Colleges" OR DE "Business Schools" OR DE "Church Related

Colleges" OR DE "Cluster Colleges" OR DE "Commuter Colleges" OR DE "Dental Schools" OR DE "Developing Institutions" OR DE "Experimental Colleges" OR DE "Law Schools" OR DE "Library Schools" OR DE "Medical Schools" OR DE "Multicampus Colleges" OR DE "Noncampus Colleges" OR DE "Private Colleges" OR DE "Public Colleges" OR DE "Single Sex Colleges" OR DE "Small Colleges" OR DE "Two Year Colleges" OR DE "Universities" OR DE "Upper Division Colleges" OR DE "Education" OR DE "Academic Education" OR DE "Adult Education" OR DE "Aerospace Education" OR DE "Aesthetic Education" OR DE "African American Education" OR DE "After School Education" OR DE "Aging Education" OR DE "Agricultural Education" OR DE "Alcohol Education" OR DE "Allied Health Occupations Education" OR DE "American Indian Education" OR DE "Art Education" OR DE "Basic Business Education" OR DE "Bilingual Education" OR DE "Career Education" OR DE "Coeducation" OR DE "Community Education" OR DE "Comparative Education" OR DE "Compensatory Education" OR DE "Competency Based Education" OR DE "Compulsory Education" OR DE "Corporate Education" OR DE "Correctional Education" OR DE "Cultural Education" OR DE "Culturally Relevant Education" OR DE "Dance Education" OR DE "Distance Education" OR DE "Driver Education" OR DE "Drug Education" OR DE "Economics Education" OR DE "Energy Education" OR DE "Environmental Education" OR DE "Equal Education" OR DE "Extension Education" OR DE "Family Life Education" OR DE "General Education" OR DE "Global Education" OR DE "Health Education" OR DE "Humanistic Education" OR DE "Industrial Education" OR DE "Informal Education" OR DE "Inservice Education" OR DE "Intergroup Education" OR DE "Journalism Education" OR DE "Law Related Education" OR DE "Leisure Education" OR DE "Literacy Education" OR DE "Marine Education" OR DE "Mathematics Education" OR DE "Mexican American Education" OR DE "Migrant Education" OR DE "Music Education" OR DE "Noncategorical Education" OR DE "Nondiscriminatory Education" OR DE "Nonformal Education" OR DE "Nontraditional Education" OR DE "Open Education" OR DE "Outcome Based Education" OR DE "Outdoor Education" OR DE "Physical Education" OR DE "Place Based Education" OR DE "Police Education" OR DE "Popular Education" OR DE "Population Education" OR DE "Postsecondary Education" OR DE "Private Education" OR DE "Process Education" OR DE "Professional Education" OR DE "Progressive Education" OR DE "Public Affairs Education" OR DE "Public Education" OR DE "Religious Education" OR DE "Rural Education" OR DE "Safety Education" OR DE "Science Education" OR DE "Special Education" OR DE "STEM Education" OR DE "Study Abroad" OR DE "Supplementary Education" OR DE "Technology Education" OR DE "Tribally Controlled Education" OR DE "Urban Education" OR DE "Values Education" OR DE "Vocational Education" OR DE "Womens Education" OR DE "Education Courses"

6 1 or 2 or 3 or 4 or 5

7 AB Meditat* OR TI Meditat* OR KW Meditat*or AB Mindful* or KW Mindful* or TI Mindful* or AB mbct or KW mbct or TI mbct or AB mbsr or KW mbsr or TI mbsr

10 TX absen* or TX achiev* or TX adher* or TX attainm* or TX attend* or TX burnout or TX conduct* or TX disengage* or TX distress* or TX effective* or TX effic* or TX engagem* or TX error* or TX function* or TX mistak* or TX motivat* or TX output* or TX perform* or TX present* or TX procrastin* or TX product* or TX stress* or TX underperform*

11 TX (job N2 strain)

12 DE "Academic Ability" OR DE "Academic Failure" OR DE "Achievement" OR DE "Academic Achievement" OR DE "African American Achievement" OR DE "Graduation" OR DE "High Achievement" OR DE "Knowledge Level" OR DE "Low Achievement" OR DE "Overachievement" OR DE "Underachievement" OR DE "Attendance" OR DE "Average Daily

Attendance" OR DE "College Attendance" OR DE "Teacher Attendance" OR DE "Attention Control" OR DE "Burnout" OR DE "Teacher Burnout" OR DE "Behavior" OR DE "Competition" OR DE "Cooperation" OR DE "Group Behavior" OR DE "Health Behavior" OR DE "Leadership Styles" OR DE "Participation" OR DE "Performance" OR DE "Persistence" OR DE "Self Control" OR DE "Student Behavior" OR DE "Teacher Behavior" OR DE "College Attendance" OR DE "Educational Attainment" OR DE "Employee Absenteeism" OR DE "Employment Problems" OR DE "Efficiency" OR DE "Teacher Effectiveness" OR DE "Teacher Improvement" OR DE "Job Performance" OR DE "Job Satisfaction" OR DE "Motivation" OR DE "Achievement Need" OR DE "Learning Motivation" OR DE "Reading Motivation" OR DE "Self Motivation" OR DE "Student Motivation" OR DE "Teacher Motivation" OR DE "Performance" OR DE "Counselor Performance" OR DE "Failure" OR DE "Success" OR DE "Productivity" OR DE "Student Attrition" OR DE "Stress Management" OR DE "Mathematics Anxiety" OR DE "Test Anxiety" OR DE "Work Attitudes" OR DE "Job Satisfaction" OR DE "Work Environment" OR DE "Teaching Conditions"

13 10 or 11 or 12

14 TX allocat* random* OR (MH "Quantitative Studies") OR (MH "Placebos") OR TX placebo* OR TX random* allocat* OR (MH "Random Assignment") OR TX randomi* control* trial* OR TX ((singl* n1 blind*) or (singl* n1 mask*))
or TX ((doubl* n1 blind*) or (doubl* n1 mask*))
or TX ((tripl* n1 blind*) or (tripl* n1 mask*))
or TX ((trebl* n1 blind*) or (trebl* n1 mask*)) OR TX clinic* n1 trial* OR DE "Randomized Controlled Trials" OR PT Clinical trial OR (MH "Clinical Trials+") or DE "Randomized Clinical Trials" OR DE "Clinical Trials" OR DE "Randomized Controlled Trials"

15 6 and 7 and 9 and 13 and 14

ICTRP

Date of search	13/05/2022
Date restrictions	1/1/1900–30/03/2018
Number of records found	415 records for 414 studies

Search strategy

mindfulness NOT patient* NOT disturb* NOT disabilit* NOT disorder* NOT infection* NOT syndrome* NOT disease* NOT abus* NOT menopaus* NOT chronic NOT injur* NOT smok* NOT asthma NOT diabet* NOT cancer NOT stroke NOT pregn* NOT dement* NOT obese NOT weight NOT psychosis NOT PTSD NOT "Multiple Sclerosis" NOT insomn*

Medline and Pubmed

Date of search	30th March 2021
Number of records found	1303

Search strategy

Search terms

1 employ*.mp or unemploy*.mp or job*.mp. or labo*.mp. or occupation*.mp. or personn*.mp or staff.mp. or work*.mp or "Staff development".mp. OR telework*.mp

2 exp Employment/ or Employment, Supported/ or Unemployment/ OR exp Occupations/ or exp Occupational Groups/ or exp Health Occupations/ or exp Students, Health Occupations/ or exp Medical Staff, Hospital/ or exp Staff Development/ or exp Medical Staff/ or exp Nursing Staff, Hospital/ or exp Nursing Staff/ or exp Return to Work/ or exp Work/ or exp Teleworking/ OR exp Workplace/ OR Shift Work Schedule/
3 university.mp. or educat*.mp. or academi*.mp. or HEI.mp. or student*.mp. or college*.mp.
4 (higher adj1 education).mp.
5 Universities/ or exp Education, Medical, Continuing/ or exp Education, Medical/ or exp Education, Medical, Graduate/ or exp Education, Nursing/ or exp Academic Medical Centers/ or Student Health Services/
6 1 or 2 or 3 or 4 or 5

7 Mindful*.ti,ab. or Meditat*.ti,ab. or mbct.ti,ab. or mbsr.ti,ab.
8 Mindfulness/ or Meditation/
9 7 or 8

10 absen*.mp or achiev*.mp or adher*.mp or attainm*.mp or attend*.mp or burnout.mp. or conduct*.mp or disengage*.mp. or distress*.mp. or effective*.mp or effic*.mp or engagem*.mp or error*.mp or function*.mp. or mistak*.mp or motivate*.mp. or output*.mp. or perform*.mp or present*.mp or product*.mp or stress*.mp or underperform*.mp
11 (job adj2 strain).mp.
12 exp absenteeism/ or Academic Failure/ or exp Academic Performance/ or achievement/ or exp burnout, psychological/ or compassion fatigue/ or educational measurement/ or exp efficiency/ or exp Efficiency, Organizational/ or Guideline Adherence/ or job satisfaction/ or exp Medication Errors/ or exp Medical Errors/ or motivation/ or exp Occupational Health/ or exp Occupational Stress/ or presenteeism/ or Procrastination/ or exp professional competence/ or Psychological Distress/ or psychosocial functioning/ or psychology, military/ or exp Sick Leave/ or exp Stress, Psychological/ or student dropouts/ or exp "task performance and analysis"/ or time management/ or underachievement/ or exp Work/ or work engagement/ or work performance/ or Work Schedule Tolerance/
13 10 or 11 or 12

14 (Randomized Controlled Trials as Topic/ or randomized controlled trial/ or Random Allocation/ or Double Blind Method/ or Single Blind Method/ or clinical trial/ or clinical trial, phase i.pt or clinical trial, phase ii.pt or clinical trial, phase iii.pt or clinical trial, phase iv.pt or controlled clinical trial.pt or randomized controlled trial.pt or multicenter study.pt or clinical trial.pt or exp Clinical Trials as topic/) or ((clinical adj trial$).tw or ((singl$ or doubl$ or treb$ or tripl$) adj (blind$3 or mask$3)).tw or PLACEBOS/ or placebo$.tw or randomly allocated.tw or (allocated adj2 random$).tw) not (case report.tw or letter/ or historical article/)

15 6 and 9 and 13 and 14

PsycInfo

Date of search	30th March 2021
Number of records found	937

Search strategy

((TX employ* or TX job* or labo*.mp or TX occupation* or TX personn* or TX staff or TX unemploy* or TX work*) or (DE "Aerospace Personnel" OR DE "Agricultural Extension Workers" OR DE "Agricultural Workers" OR DE "Air Force Personnel" OR DE "Aircraft Pilots" OR DE "Allied Health Personnel" OR DE "Anthropologists" OR DE "Apprenticeship" OR DE "Architects" OR DE "Army Personnel" OR DE "Artists" OR DE "Astronauts" OR DE "Attendants (Institutions)" OR DE "Attorneys" OR DE "Blue Collar Workers" OR DE "Business and Industrial Personnel" OR DE "Child Care Workers" OR DE "Clerical Personnel" OR DE "Clinical Psychologists" OR DE "Clinicians" OR DE "Coast Guard Personnel" OR DE "College Teachers" OR DE "Commissioned Officers" OR DE "Corrections Officers" OR DE "Counselors" OR DE "Dentists" OR DE "Disabled Personnel" OR DE "Domestic Service Personnel" OR DE "Educational Personnel" OR DE "Emergency Personnel" OR DE "Engineers" OR DE "Enlisted Military Personnel" OR DE "Fire Fighters" OR DE "First Responders" OR DE "Foreign Workers" OR DE "Frontline Employees" OR DE "Government Personnel" OR DE "Health Personnel Attitudes" OR DE "Health Personnel" OR DE "Home Care Personnel" OR DE "Human Capital" OR DE "Human Resource Management" OR DE "Impaired Professionals" OR DE "Industrial Psychologists" OR DE "Information Specialists" OR DE "Job Resources" OR DE "Journalists" OR DE "Judges" OR DE "Labor Union Members" OR DE "Law Enforcement Personnel" OR DE "Lay Religious Personnel" OR DE "Legal Personnel" OR DE "Management" OR DE "Management Personnel" OR DE "Management Training" OR DE "Marine Personnel" OR DE "Mathematicians" OR DE "Medical Personnel" OR DE "Mental Health Personnel" OR DE "Middle Level Managers" OR DE "Migrant Workers" OR DE "Military Attrition" OR DE "Military Deployment" OR DE "Military Duty Status" OR DE "Military Enlistment" OR DE "Military Medical Personnel" OR DE "Military Personnel" OR DE "Military Psychologists" OR DE "Military Veterans" OR DE "National Guard Personnel" OR DE "Navy Personnel" OR DE "Noise Levels (Work Areas)" OR DE "Nonprofessional Personnel" OR DE "Nurses" OR DE "Occupational Health" OR DE "Occupational Therapists" OR DE "Optometrists" OR DE "Organizations" OR DE "Paramedics" OR DE "Paraprofessional Personnel" OR DE "Parole Officers" OR DE "Personnel" OR DE "Pharmacists" OR DE "Physical Therapists" OR DE "Physicians" OR DE "Physicists" OR DE "Police Personnel" OR DE "Prison Personnel" OR DE "Probation Officers" OR DE "Professional Personnel" OR DE "Professional Role" OR DE "Psychiatric Aides" OR DE "Psychiatric Hospital Staff" OR DE "Psychiatric Nurses" OR DE "Psychiatric Social Workers" OR DE "Psychiatrists" OR DE "Psychologists" OR DE "Psychotherapists" OR DE "Public Health Service Nurses" OR DE "Quality Control" OR DE "Religious Personnel" OR DE "Rescue Workers" OR DE "Sales Personnel" OR DE "School Administrators" OR DE "School Counselors" OR DE "School Nurses" OR DE "School Psychologists" OR DE "Scientists" OR DE "Secretarial Personnel" OR DE "Self-Managing Work Teams" OR DE "Service Personnel" OR DE "Skilled Industrial Workers" OR DE "Social Workers" OR DE "Sociologists" OR DE "Speech Therapists" OR DE "Teacher Aides" OR DE "Teacher Effectiveness" OR DE "Teachers" OR DE "Technical Personnel" OR DE "Technical Service Personnel" OR DE "Telecommuting" OR DE "Therapists" OR DE "Top Level Managers" OR DE "Unemployment" OR DE "Virtual Teams" OR DE "Volunteer Military Personnel" OR DE "White Collar Workers" OR DE "Work Teams" OR DE "Working Conditions" OR DE "Working Space" OR DE "Workplace Intervention") or (TX academi* or TX college* or TX educat* or TX HEI or TX student* or TX university.mp.) or TX (higher N1 education) or (DE "Academic Settings" OR DE "College Athletes" OR DE "College Environment" OR DE "College Graduates" OR DE “college students” OR DE "Community College Students" OR DE "Continuing Education" OR DE "Education Students" OR DE "Graduate Education" OR DE "Higher Education" OR DE "Junior College Students" OR DE "Nursing Students" OR DE "Postgraduate Training" OR DE "ROTC Students" OR DE "Schools" OR DE "Undergraduate

Education")) AND (AB Meditat* OR TI Meditat* OR KW Meditat*or AB Mindful* or KW Mindful* or TI Mindful* or AB mbct or KW mbct or TI mbct or AB mbsr or KW mbsr or TI mbsr or DE "Meditation" OR DE "Mindfulness" OR DE "Mindfulness-Based Interventions") AND ((TX absen* or TX achiev* or TX adher* or TX attainm* or TX attend* or TX burnout or TX conduct* or TX disengage* or TX distress* or TX effective* or TX effic* or TX engagem* or TX error* or TX function* or TX mistak* or TX motivat* or TX output* or TX perform* or TX present* or TX procrastinat* or TX product* or TX stress* or TX underperform*.mp) OR TX (job N2 strain) or (DE "Academic Achievement Motivation" OR DE "Academic Achievement" OR DE "Academic Aptitude" OR DE "Academic Environment" OR DE "Academic Failure" OR DE "Academic Overachievement" OR DE "Academic Stress" OR DE "Academic Underachievement" OR DE "Achievement Motivation" OR DE "Achievement" OR DE "Affiliation Motivation" OR DE "Aspirations" OR DE "Athletic Performance" OR DE "Career Change" OR DE "Career Development" OR DE "Chronic Stress" OR DE "Classroom Environment" OR DE "College Academic Achievement" OR DE "Compassion Fatigue" OR DE "Coping Behavior" OR DE "Coping Style" OR DE "Costs and Cost Analysis" OR DE "Course Evaluation" OR DE "Demoralization" OR DE "Distress" OR DE "Educational Attainment Level" OR DE "Educational Incentives" OR DE "Emotional Exhaustion" OR DE "Employee Absenteeism" OR DE "Employee Attitudes" OR DE "Employee Benefits" OR DE "Employee Characteristics" OR DE "Employee Efficiency" OR DE "Employee Engagement" OR DE "Employee Layoffs" OR DE "Employee Motivation" OR DE "Employee Productivity" OR DE "Employee Skills" OR DE "Employee Turnover" OR DE "Employee Well Being" OR DE "Endurance" OR DE "Environmental Stress" OR DE "Extrinsic Motivation" OR DE "Family Work Conflict" OR DE "Family Work Relationship" OR DE "Fear of Success" OR DE "Goals" OR DE "Group Performance" OR DE "Incentives" OR DE "Intrinsic Motivation" OR DE "Job Analysis" OR DE "Job Demands" OR DE "Job Enrichment" OR DE "Job Involvement" OR DE "Job Knowledge" OR DE "Job Performance" OR DE "Job Satisfaction" OR DE "Labor Management Relations" OR DE "Life Skills" OR DE "Morale" OR DE "Motivation Measures" OR DE "Motivation" OR DE "Motor Performance" OR DE "Occupational Adjustment" OR DE "Occupational Aspirations" OR DE "Occupational Attitudes" OR DE "Occupational Interests" OR DE "Occupational Safety" OR DE "Occupational Stress" OR DE "Occupational Success Prediction" OR DE "Occupational Success" OR DE "organizational behavior" OR DE "organizational climate" OR DE "Organizational Effectiveness" OR DE "Perceived Stress" OR DE "Performance" OR DE "Personnel Evaluation" OR DE "Personnel Placement" OR DE "Personnel Promotion" OR DE "Personnel Supply" OR DE "Personnel Termination" OR DE "Personnel Training" OR DE "Personnel" OR DE "Physical Endurance" OR DE "Physical Fitness" OR DE "Procrastination" OR DE "Productivity" OR DE "Professionalism" OR DE "Psychological Endurance" OR DE "Psychological Stress" OR DE "Readiness to Change" OR DE "School Environment" OR DE "Self-Efficacy" OR DE "Self-Expansion" OR DE "Social Functioning" OR DE "Social Motivation" OR DE "Social Stress" OR DE "Stress Management" OR DE "Stress Reactions" OR DE "Stress" OR DE "Student Attitudes" OR DE "Student Attrition" OR DE "Student Engagement" OR DE "Supervisor Employee Interaction" OR DE "Test Performance" OR DE "well being" OR DE "Work (Attitudes Toward)" OR DE "Work Related Illnesses" OR DE "Work Rest Cycles" OR DE "Work Scheduling" OR DE "Work Week Length" OR DE "Workaholism" OR DE "Workday Shifts" OR DE "Working Conditions" OR DE "Working Space" OR DE "Work-Life Balance")) AND (TX allocat* random* OR (MH "Quantitative Studies") OR (MH "Placebos") OR TX placebo* OR TX random* allocat* OR (MH "Random Assignment") OR TX randomi* control* trial* OR TX ((singl* n1 blind*) or (singl* n1 mask*)) or TX ((doubl* n1 blind*) or (doubl* n1 mask*)) or TX ((tripl* n1 blind*) or (tripl* n1 mask*)) or TX ((trebl* n1 blind*) or

(trebl* n1 mask*)) OR TX clinic* n1 trial* OR PT Clinical trial OR (MH "Clinical Trials+") or DE "Randomized Clinical Trials" OR DE "Clinical Trials" OR DE "Randomized Controlled Trials")

Scopus

Date of search 30th March 2021
Number of records found 2135

Search strategy
(TITLE-ABS-KEY (employ* or job* or labo* or occupation* or personn* or staff or unemploy* or work*) or (EXACTKEYWORD , "Workplace") or TITLE-ABS-KEY (academi* or college* or educat* or HEI or student* or university) or (EXACTKEYWORD , "Education") OR (EXACTKEYWORD , "University") OR (EXACTKEYWORD , "Universities") OR TITLE-ABS-KEY (higher W/1 education)) AND (TITLE-ABS-KEY (mindful* OR meditat* OR mbct OR mbsr) OR (exactkeyword AND , "Meditation") OR (exactkeyword AND , "Mindfulness Based Stress Reduction") OR (exactkeyword AND , "Mindfulness Meditation") OR (exactkeyword AND , "Mind-Body Therapies")) AND (ALL (absen* OR achiev* OR adher* OR attainm* OR attend* OR burnout OR conduct* OR disengage* OR distress* OR effective* OR effic* OR engagem* OR error* OR function* OR mistak* OR motivate* OR output* OR perform* OR present* OR procastin* OR product* OR stress* OR underperform*) OR ALL (job W/2 strain) OR (exactkeyword AND , "Stress") OR (exactkeyword AND , "Mental Stress") OR (exactkeyword AND , "Stress, Psychological") OR (exactkeyword AND , "Stress Management") OR (exactkeyword AND , "Burnout") OR (exactkeyword AND , "Burnout, Professional") OR (exactkeyword AND , "Job Stress")) AND (TITLE-ABS-KEY (randomise* OR randomize* OR rct OR "random allocation" OR "random assignment" OR randomly) OR (exactkeyword AND , "Randomized Controlled Trial (topic)") OR (exactkeyword AND , "Clinical Trial") OR (exactkeyword AND , "Intervention Study") OR (exactkeyword AND , "Single Blind Procedure") OR (exactkeyword AND , "Controlled Clinical Trial") OR (exactkeyword AND , "Clinical Effectiveness")) AND (EXCLUDE (EXACTKEYWORD , "Child"))

Web of Science

Date of search 30th March 2021
Number of records found 2168

Search strategy
(TS=(employ* or job* or labo* or occupation* or personn* or staff or unemploy* or work*) OR TS=(academi* or college* or educat* or HEI or student* or university) OR TS=(higher near/1 education)) AND TS=(mindful* OR meditat* OR MBCT OR MBSR) AND (ALL=(absen* or achiev* or adher* or attainm* or attend* or burnout or conduct* or disengage* or distress* or effective* or effic* or engagem* or error* or function* or mistak* or motivate* or output* or perform* or present* or procastin* or product* or stress* or underperform*) OR TS=(job NEAR/2 strain)) AND ALL=(randomise* OR randomize* OR RCT OR "random allocation" OR "random assignment" OR randomly)

Supplementary Material 2: Data extraction forms

Data extraction items

- Covidence #
- Study ID
- Title
- Reviewer Name
- Reviewer name
- Year of first publication
- Country in which the study conducted
- Notes
- Confirm eligibility for review
- Reason for exclusion
- Sample context
- Sample description
- Total number of arms
- Total number of mindfulness groups
- Study design
- Any comments on the method
- For each MBP arm
  - Name of intervention
  - Duration of intervention
  - Delivery medium of intervention
  - Delivery format of intervention
  - Any comments on intervention
- For each control arm
  - Name of control group
  - Type of control group
  - Content of control group
  - Duration of control
  - Delivery medium of control
  - Delivery format of control
  - Any comments on control
- Total number of participants
- Age
  - Total Mean
  - Total SD
  - Total Min
  - Total Max
- Sex
  - Females
  - Males
  - Others
- For each group

- Number of participants
- Age
  - Mean
  - SD
  - Min
  - Max
- Sex
  - Females
  - Males
  - Others

For each outcome of interest
- Dimension of outcome
- Construct measured
- Scale or instrument used
- Cronbach's alpha or other measure for quality of the measure
- Report type

Time period ranges reported
- Earlier than 4 weeks before intervention
- Up to 4 weeks before intervention
- Up to 4 weeks after intervention
- 5 – 24 weeks after intervention
- More than 24 weeks post-intervention

For each arm, outcome of interest and time period
- Sample size (n analysed)
- Mean
- SD
- Other info

Any other work-related instruments or scales used
Any comments notes about results
Conflicts of interest
References to other relevant studies
Correspondence with the author (s) required?
If correspondence needed, make a note as to why
Name of corresponding author
E-mail of corresponding author
Which of the following sources were *obtained* to help inform the risk-of-bias assessment?

Risk of bias evaluation

We made the following assumptions when responding to the signalling questions of RoB2:

1. **Risk of bias due to deviations from the intended interventions**: We assumed deviations arose because of the trial context when the study used a waitlist control group where the waitlist period was at least 6 months. We argued that the long delay may have compelled some control group participants to seek a MBP or other mental health interventions independently. Similarly, we assumed there to be a possibility of deviations where no intervention control was used – that is, the control group did not receive any intervention even after the end of the study.
2. **Risk of bias due to missing data**: For 3.3/3.4 we assumed missingness could depend on true value when the outcome measure was self-reported or reported by an observer where the participant needed to approve the observation. However, we responded "no information" for the likelihood of that dependence, unless triallists provided relevant evidence.
3. Risk of bias in measurement of the outcome:
    a. For 4.2, we responded "probably no" unless triallists explicitly described whether the measurements of outcomes differed between groups. This aspect is rarely described in the studies we included, and this rating allowed us to be open to all three levels of risk (low, some, high) following the suggested judgement algorithm.
    b. 4.4/4.5, we assumed assessment could have been influenced by knowledge of intervention when the outcome measure was self-reported. "No information" was the default for the likelihood of the influence.

For judgements of risk of bias, we followed the algorithm suggested by the tool, except for judgements of risk of bias of the reported result. The tool suggests that if all items receive a "no information" rating, the overall judgement should be "some concerns". Given the lack of pre-specified analyses plans can lead to a high risk for questionable research practices (John et al., 2012), we deemed

studies with no pre-registered information available on their outcomes and analyses plans as high risk of bias.

Evaluation items

Covidence #
Study ID
1.1 Was the allocation sequence random?
1.1 Was the allocation sequence random? supporting text
1.2 Was the allocation sequence concealed until participants were enrolled and assigned to interventions?
1.2 Was the allocation sequence concealed until participants were enrolled and assigned to interventions? supporting text
1.3 Did baseline differences between intervention groups suggest a problem with the randomization process?
1.3 Did baseline differences between intervention groups suggest a problem with the randomization process? supporting text
Risk-of-bias judgement risk of bias arising from the randomization process
Risk-of-bias judgement risk of bias arising from the randomization process supporting text
1.4 Optional: What is the predicted direction of bias due to selection of the reported result?
1.4 Optional: What is the predicted direction of bias due to selection of the reported result? supporting text
2.1a. Were participants aware of their assigned intervention during the trial?
2.1a. Were participants aware of their assigned intervention during the trial? supporting text
2.2a. Were carers and people delivering the interventions aware of participants' assigned intervention during the trial?
2.2a. Were carers and people delivering the interventions aware of participants' assigned intervention during the trial? supporting text
2.3a. If Y/PY/NI to 2.1 or 2.2: Were there deviations from the intended intervention that arose because of the trial context?
2.3a. If Y/PY/NI to 2.1 or 2.2: Were there deviations from the intended intervention that arose because of the trial context? supporting text
2.4a If Y/PY to 2.3: Were these deviations likely to have affected the outcome?
2.4a If Y/PY to 2.3: Were these deviations likely to have affected the outcome? supporting text
2.5a. If Y/PY/NI to 2.4: Were these deviations from intended intervention balanced between groups?
2.5a. If Y/PY/NI to 2.4: Were these deviations from intended intervention balanced between groups? supporting text
2.6a Was an appropriate analysis used to estimate the effect of assignment to intervention?
2.6a Was an appropriate analysis used to estimate the effect of assignment to intervention? supporting text
2.7a If N/PN/NI to 2.6: Was there potential for a substantial impact (on the result) of the failure to analyse participants in the group to which they were randomized?

2.7a If N/PN/NI to 2.6: Was there potential for a substantial impact (on the result) of the failure to analyse participants in the group to which they were randomized? supporting text
2.8a. Risk-of-bias judgement for risk of bias due to deviations from the intended interventions (effect of assignment to intervention)
2.8a. Risk-of-bias judgement for risk of bias due to deviations from the intended interventions (effect of assignment to intervention) supporting text
2.9.a Optional: What is the predicted direction of bias due to selection of the reported result?
2.9.a Optional: What is the predicted direction of bias due to selection of the reported result? supporting text
3.1 Were data for this outcome available for all, or nearly all, participants randomized?
3.1 Were data for this outcome available for all, or nearly all, participants randomized? supporting text
3.2 If N/PN/NI to 3.1: Is there evidence that the result was not biased by missing outcome data?
3.2 If N/PN/NI to 3.1: Is there evidence that the result was not biased by missing outcome data? supporting text
3.3 If N/PN to 3.2: Could missingness in the outcome depend on its true value?
3.3 If N/PN to 3.2: Could missingness in the outcome depend on its true value? supporting text
3.4 If Y/PY/NI to 3.3: Is it likely that missingness in the outcome depended on its true value?
3.4 If Y/PY/NI to 3.3: Is it likely that missingness in the outcome depended on its true value? supporting text
Risk-of-bias judgement for risk of bias due to missing outcome data
Risk-of-bias judgement for risk of bias due to missing outcome data supporting text
3.5. Optional: What is the predicted direction of bias due to selection of the reported result?
3.5. Optional: What is the predicted direction of bias due to selection of the reported result? supporting text
4.1 Was the method of measuring the outcome inappropriate?
4.1 Was the method of measuring the outcome inappropriate? supporting text
4.2 Could measurement or ascertainment of the outcome have differed between intervention groups?
4.2 Could measurement or ascertainment of the outcome have differed between intervention groups? supporting text
4.3 If N/PN/NI to 4.1 and 4.2: Were outcome assessors aware of the intervention received by study participants?
4.3 If N/PN/NI to 4.1 and 4.2: Were outcome assessors aware of the intervention received by study participants? supporting text
4.4 If Y/PY/NI to 4.3: Could assessment of the outcome have been influenced by knowledge of intervention received?
4.4 If Y/PY/NI to 4.3: Could assessment of the outcome have been influenced by knowledge of intervention received? supporting text
4.5 If Y/PY/NI to 4.4: Is it likely that assessment of the outcome was influenced by knowledge of intervention received?
4.5 If Y/PY/NI to 4.4: Is it likely that assessment of the outcome was influenced by knowledge of intervention received? supporting text
Risk-of-bias judgement for risk of bias in measurement of the outcome
Risk-of-bias judgement for risk of bias in measurement of the outcome supporting text
4.6. Optional: What is the predicted direction of bias due to selection of the reported result?

4.6. Optional: What is the predicted direction of bias due to selection of the reported result? supporting text
5.1 Were the data that produced this result analysed in accordance with a pre-specified analysis plan that was finalized before unblinded outcome data were available for analysis?
5.1 Were the data that produced this result analysed in accordance with a pre-specified analysis plan that was finalized before unblinded outcome data were available for analysis? supporting text
5.2 Is the numerical result being assessed likely to have been selected, on the basis of the results, from multiple eligible outcome measurements (e.g. scales, definitions, time points) within the outcome domain?
5.2 Is the numerical result being assessed likely to have been selected, on the basis of the results, from multiple eligible outcome measurements (e.g. scales, definitions, time points) within the outcome domain? supporting text
5.3 Is the numerical result being assessed likely to have been selected, on the basis of the results, from multiple eligible analyses of the data?
5.3 Is the numerical result being assessed likely to have been selected, on the basis of the results, from multiple eligible analyses of the data? supporting text
Risk-of-bias judgement for risk of bias in selection of the reported result
Risk-of-bias judgement for risk of bias in selection of the reported result supporting text
5.4 Optional: What is the predicted direction of bias due to selection of the reported result?
5.4 Optional: What is the predicted direction of bias due to selection of the reported result? supporting text
Overall risk-of-bias judgement
Overall risk-of-bias judgement supporting text
Risk of bias because of funding
Risk of bias because of funding supporting text
Risk of bias because of vested interest
Risk of bias because of vested interest supporting text
Other sources of bias
Other sources of bias supporting text

Supplementary Material 3: Results

Confidence in the evidence

Table S 1. GRADE Summary Findings: Offering a mindfulness-based programme compared to no action (passive control) for general public

Outcome (follow-up)	Number of participants (studies)	Ratings for quality of evidence	Certainty	Effect size
Task performance (Up to 4 weeks post-intervention)	454 (7)	Risk of bias: serious, Non-reporting bias: serious, Imprecision: serious, Inconsistency: serious, Indirectness: not serious, Other considerations: not serious	Very low	Hedge's g = 0.52, 95% CI: -0.03 to 1.07, 95% PI: -0.73 to 1.77
Task performance (5-24 weeks post-intervention)	1245 (6)	Risk of bias: serious, Non-reporting bias: serious, Imprecision: serious, Inconsistency: not serious, Indirectness: not serious, Other considerations: not serious	Very low	Hedge's g = 0.05, 95% CI: -0.15 to 0.26, 95% PI: -0.17 to 0.27

Outcome (follow-up)	Number of participants (studies)	Ratings for quality of evidence	Certainty	Effect size
Contextual performance (Up to 4 weeks post-intervention)	778 (11)	Risk of bias: serious, Non-reporting bias: serious, Imprecision: not serious, Inconsistency: not serious, Indirectness: not serious, Other considerations: not serious	Low	Hedge's g = 0.33, 95% CI: 0.09 to 0.57, 95% PI: −0.03 to 0.69
Contextual performance (5–24 weeks post-intervention)	704 (6)	Risk of bias: serious, Non-reporting bias: serious, Imprecision: serious, Inconsistency: not serious, Indirectness: not serious, Other considerations: not serious	Very low	Hedge's g = 0.28, 95% CI: −0.06 to 0.62, 95% PI: −0.33 to 0.88
Adaptive performance (Up to 4 weeks post-intervention)	1544 (17)	Risk of bias: serious, Non-reporting bias: serious, Imprecision: not serious, Inconsistency: not serious, Indirectness: not serious, Other considerations: not serious	Low	Hedge's g = 0.32, 95% CI: 0.17 to 0.47, 95% PI: 0.17 to 0.47
Adaptive performance (5–24 weeks post-intervention)	710 (8)	Risk of bias: serious, Non-reporting bias: serious, Imprecision: not serious, Inconsistency: not serious, Indirectness: not serious, Other considerations: not serious	Low	Hedge's g = 0.4, 95% CI: 0.1 to 0.69, 95% PI: −0.09 to 0.88

Outcome (follow-up)	Number of participants (studies)	Ratings for quality of evidence	Certainty	Effect size
Counterproductive work behaviour (Up to 4 weeks post-intervention)	327 (3)	Risk of bias: serious, Non-reporting bias: serious, Imprecision: serious, Inconsistency: not serious, Indirectness: not serious, Other considerations: not serious	Very low	Hedge's g = 0.14, 95% CI: -0.54 to 0.82, 95% PI: -1.86 to 2.14

Risk of bias rating for each study

Table S 2. Risk of bias ratings

Study	Randomisation process	Deviations from intervention	Data missingness	Outcome measurement	Result selection
Allexandre 2016	Some concerns	Some concerns	High	High	High
	No information on allocation concealment. Based on Table 1 and Table 2, the groups seem similar at baseline.	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context. For analysis: "In this primary intent-to-treat analysis, web-based program participants with and without group support showed..."	"...data are only available for participants who were debt collectors (N = 102, 63% of participants) and who were absent fewer than 20% of the workdays in a given month." Sensitivity analysis for missing data available. Missingness could depend on the true value as poorer health could lead to sick leave.	Not clear how score was calculated, however the raw data were collected independent of the trial.	No pre-specified analysis plan available.
Asuero 2014	High	High	High	High	High
	Not clear whether allocation was concealed. Also: "The number of participants in the intervention group (43) was larger than expected due to the high interest in the mindfulness educational program and the	No information about possible deviations due to the trial context nor on the principles upon which the analysis was conducted.	Data missingness not reported. Missingness could have been due attrition, which in turn could have been affected by engagement and perceived benefit.	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	No pre-specified analysis plan available.

	convenience of its schedule."				
Bartlett 2017	Low	Low	Low	High	High
	"A non-research team member handled the randomization to avoid investigator selection bias." Table 2 suggests no major imbalances	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context. Two participants assigned to the information control group as a reserve were transferred to the intervention group. The possibility of this happening seems to have been pre-planned. "To test robustness of findings, inverse pro-portion modelling was used to compensate for gaps in data, by proportionally weighting post-intervention scores of completers with similar pre-intervention characteristics to non-completers"	Overall, 66% of participants in the control condition analysed at post. 100% of participants in the intervention condition. Data missingness for outcome of interest is not explicitly reported. "A negligible difference in outcomes was observed for the complete cases versus OAG analysis."	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings." All participants were approached to be asked to fill in the questionnaires at the same time, regardless of which group they were assigned to."	No pre-specified analysis plan available.
Bellosta-Batalla 2021	Some concerns	High	High	Some concerns	High
	No information on randomisation procedure or concealment. No apparent between-group	Not possible to blind participants/intervention facilitators. No information about	Data missingness not reported. Missingness could have been due attrition, which in turn	It is not clear who scored the subscales. It is stated that "These subscales were	No pre-specified analysis plan available.

	differences in demographics or outcomes at baseline.	possible deviations due to the trial context. Analysis conducted per-protocol: "Students who practised meditation for fewer than 300 minutes during the MCBI were excluded because this was the time required for the meditation training session exercises". Approximately 12% (n = 5 out of 42) of the mindfulness and compassion group was eliminated due to low reported minutes spent meditating.	could have been affected by engagement and perceived benefit.	corrected by an external researcher who was previously trained in the PIC-A correction manual. She was not informed that there were different groups in the study, thus avoiding possible biases in the evaluation process." This could have been a mistranslation.	
Benn 2012	Some concerns	High	High	High	High
	Concealment unclear. "Following randomization, results showed that treatment and control participants did not significantly differ on any baseline measures" – not clear if powered enough. Tests run for differences, not equivalences.	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context. Not reported whether the analysis was ITT or per-protocol	"Of the study sample, 14% declined participation after randomization (see Table 4). One treatment participant dropped out of the study after the intervention training began." Participants who dropped out had statistically significantly higher scores for depression, stress, anxiety, negative affect and lower scores for	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	No pre-specified analysis plan available.

			mindfulness, positive affect and personal growth ($p < .05$).		
Braun 2020a	High	High	High	High	High
	No information on randomisation procedure or concealment. The groups were not of equal sizes. It is not known whether the randomisation was intended to be done at 1:1 ratio or not. Caregivers' relationship to the person they cared for also differed: in one group, 52.2% were spouses, in the other 26.7% were spouses. Also, in the MBSR group, nearly all (95.7 or 22 of 23 participants) were the main care providers. In the control condition, 66.7% described themselves as the main caregivers.	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context. "To permit investigation of the intent-to-treat (ITT) sample, all available participant data were included in the analyses"	Data missingness not reported. Missingness could have been due attrition, which in turn could have been affected by engagement and perceived benefit.	Self-reported measure used, thus blinding towards assignment was impossible. However, "After randomization and before interventions were begun, participants were asked to rate, on a 1 (not at all) to 10 (extremely) scale the extent to which they expected their assigned course to benefit them. MBSR and SS participants did not differ on expected benefit (MBSR M = 8.0, SD = 1.95; SS M = 8.6, SD = 1.40, $p = .32$). ". No info on power.	No pre-specified analysis plan available.
Braun 2020b	High	High	High	High	High
	Due to the randomisation procedure, unlikely that allocation was concealed. Baseline imbalances not evident	No information about possible deviations due to the trial context. "Preferential group allocation was offered /.../ if [participants']	Data missingness not reported. Missingness could have been due attrition, which in turn could have been	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the	No pre-specified analysis plan available. Productivity is not listed as an outcome measure in the trial registration.

		schedules did not allow them to participate in the group to which they were randomized". 13 completed post assessment in MBP; 22 analyses in full ITT – 19 completed post assessment (CONTROL); 26 analysed in full ITT	affected by engagement and perceived benefit.	effectiveness could have impacted the ratings.	
Brown 2016	High	Some concerns	High	Low	High
	No information on randomisation procedure or concealment. The groups were not of equal sizes. It is not known whether the randomisation was intended to be done at 1:1 ratio or not. Caregivers' relationship to the person they cared for also differed: in one group, 52.2% were spouses, in the other 26.7% were spouses. Also, in the MBSR group, nearly all (95.7 or 22 of 23 participants) were the main care providers. In the control confition, 66.7% described themselves as the main caregivers.	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context. "To permit investigation of the intent-to-treat (ITT) sample, all available participant data were included in the analyses"	Data missingness not reported. Missingness could have been due attrition, which in turn could have been affected by engagement and perceived benefit.	Self-reported measure used, thus blinding towards assignment was impossible. However, "After randomization and before interventions were begun, participants were asked to rate, on a 1 (not at all) to 10 (extremely) scale the extent to which they expected their assigned course to benefit them. MBSR and SS participants did not differ on expected benefit (MBSR M = 8.0, SD = 1.95; SS M = 8.6, SD = 1.40, p = .32). ". No info on power.	No pre-specified analysis plan available.

Can Gür 2020	Low	High	High	High	High
	"The allocation was concealed using opaque sealed envelopes." No information on baseline differences between groups – age, gender, ethnicity are all calculated for the whole group and not divided between control and intervention	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context. Control group seem to have received no intervention (including after trial finished) and may have sought an intervention elsewhere. Not reported whether the analysis was ITT or per-protocol, but based on Figure 1, seems to be ITT.	Data missingness not reported. Missingness could have been due attrition, which in turn could have been affected by engagement and perceived benefit.	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	No pre-specified analysis plan available.
Chan 2021	Some concerns	Some concerns	High	High	High
	No information on concealment. Some differences between groups at baseline, some statistically significant although lack of power and multiple testing makes it hard to judge real differences.	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context. ITT analysis using repeated measures ANOVA.	Data missingness not reported. Missingness could have been due attrition, which in turn could have been affected by engagement and perceived benefit.	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	No pre-specified analysis plan available.
Christopher 2018	Some concerns	Some concerns	High	High	High
	No information on concealment. Groups differed at baseline in the belief that MBP	Not possible to blind participants/intervention facilitators. No information about	"Conclusions with imputed data differed for four outcomes (see Table 3)". Authors ran	Self-reported measure used, thus blinding towards assignment was impossible. Pre-	No pre-specified analysis plan available.

	improves job stress, performance and resilience (favours control), MBP participants higher in self-compassion and resilience. Not enough info to suggest a problem in randomisation	possible deviations due to the trial context. "Intent-to-treat (ITT) analyses, without imputed missing data, assessed pre-training between-group differences for all outcomes, demographic variables, and expectancy data"	Little's missingness test and conclude data to miss at random. Power analysis not reported. Some participants dropped out because "they did not want to continue with MBRT".	conceived beliefs in the effectiveness could have impacted the ratings.	
Daigle 2018	Some concerns	Some concerns	High	High	High
	No info on how participants were randomised except that they were and that they were matched for scores of a measure on burnout. Not reported which measure for burnout was used. No apparent between-group imbalances.	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context. "ANCOVAs were performed using intention-to-treat analyses with the last observation carried forward method and pretest scores as covariates."	Nursing Errors Rating Scale data available for 28 out of 70 participants. "The Nursing Errors Rating Scale was sent by mail 3 months following MBSR to nurses in the second and third recruitment wave as part of this pilot study." Control group did not receive the measure.	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings. Measure only collected from intervention group. The rating scale used by the Nursing Errors Rating Scale is 0 – was never a problem to 5 – greatly improved. There seems to be no way in indicating worsening in nursing errors thus also creating an expecation of the outcome.	No pre-specified analysis plan available.
de Jong 2013	High	High	High	High	High
	No information on randomisation procedure	No information about possible deviations due	Data missingness not reported. Missingness	Attrition is high: 33% in the experimental group	No pre-specified analysis plan available.

	or concealment. No apparent between-group differences in demographics or outcomes at baseline.	to the trial context but the control group received no intervention and were given no intervention after the study which may have motivated them to seek mindfulness elsewhere. It seems that per protocol analysis is run since everyone who stopped the course also did not provide data post-intervention.	could have been due attrition, which in turn could have been affected by engagement and perceived benefit.	and 23% in the control group. Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings. Quitting the training was due to finding a new job and some gave up because of private matter	
Dvoráková 2017	High	High	High	High	High
	Allocation probably not concealed as participants were sent e-mails about their allocation. Also, from the flow chart (Figure 1) it appears participants were first randomised and then they were asked to fill in the pre-intervention questionnaires. The intervention group baseline scores for mental health problems are higher in every domain measured and they lower scores for scales that could	No information about possible deviations due to the trial context but waitlist group had to wait more than 6 months to receive intervention. "The analysis was conducted as an intent-to-treat, including all randomized participants."	Data missingness not reported. Missingness could have been due attrition, which in turn could have been affected by engagement and perceived benefit.	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	No pre-specified analysis plan available.

	correlate with wellbeing. They were also significantly more likely to attend therapy in the 6 months prior to data collection.				
Erogul 2014	High	High	High	High	High
	Participants were first randomised and then asked whether they want to participate in the study.	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context. Not reported whether the analysis was ITT or per-protocol	Data missingness not reported. Missingness could have been due attrition, which in turn could have been affected by engagement and perceived benefit.	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	No pre-specified analysis plan available.
Flook 2013	Some concerns	High	High	High	High
	No information on randomisation procedure or concealment. No apparent between-group differences in demographics or outcomes at baseline.	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context. Not reported whether the analysis was ITT or per-protocol	Data missingness not reported. As CLASS is not self-reported, missingness could come from abandoning the study (none reported) or the rater not completing CLASS. Participants could have withdrawn their consent to have an observer	Coders were blind to study hypothesis, no info on group allocation	No pre-specified analysis plan available.
Gómez-Odriozola 2019	Some concerns	High	High	High	High
	No information on randomisation procedure or concealment. No	Not possible to blind participants/intervention facilitators. No	Data missingness not reported. Likely that people who did not	Self-reported measure used, thus blinding towards assignment was	No pre-specified analysis plan available.

	apparent between-group differences in demographics or outcomes at baseline.	information about possible deviations due to the trial context. Based on the CONSORT diagram, probably per protocol analysis.	complete treatment were not invited to fill in measures.	impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	
Galante 2018	Low	Some concerns	Low	Low	Low
	"...the allocation process was concealed from the researchers." No apparent between-group imbalances	Not possible to blind participants/intervention facilitators. Some participants in the control group engaged in mindfulness elsewhere. Sensitivity analysis done.	"Examination results graded according to the British undergraduate degree classification system (examination ranking was unavailable);" Some participants were postgraduates (master's and PhD students) and had thus no examination results available. The missingness did not depend on the true value.	Examiners were unaware of which students participated in the study, including which group participants were allocated to.	No other relevant measure was planned to be collected, as per the trial protocol. Data analysed according to plan
Glass 2019	High	High	High	High	High
	Not clear whether allocation was concealed. Tested for differences between groups at baseline. No differences found, unclear whether analysis was well-powered.	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context. Only per-protocol data available.	Data missingness not reported. Data only available for per-protocol participants and only at 2 out of 4 time points collected	Not known whether trainer (reporter) knew about group allocation	No pre-specified analysis plan available. No ITT data reported, no control group data available
	Some concerns	High	High	High	High

Hunsinger 2019	No information on randomisation procedure or concealment. No apparent between-group differences in demographics or outcomes at baseline.	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context but no-intervention control may have led some participants to seek MBP elsewhere. Not reported whether the analysis was ITT or per-protocol	Data missingness not reported. Large attrition. Outcome measured with a behavioural task, not clear how the task was explained to the participants	Implicit bias is unlikely to be a good measure for racial bias (e.g., https://replicationindex.com/category/implicit-bias/).	No pre-specified analysis plan available.
Hwang 2019	High	High	High	High	High
	No information on randomisation procedure or concealment. School types considerably differed between intervention and control group.	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context but waitlist control was given access to the MBP more than 6 months after randomisation. Not reported whether the analysis was ITT or per-protocol	6% data missing, mainly at outcome level.	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	No pre-specified analysis plan available.
Jennings 2013	High	High	High	High	High
	No information on randomisation procedure or concealment. Not clear how many participants were	Not possible to blind participants/intervention facilitators. No information about possible deviations due	Data missingness not reported, not clear how many participants were assigned to groups	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the	No pre-specified analysis plan available.

	allocated to which groups.	to the trial context. Not reported whether the analysis was ITT or per-protocol		effectiveness could have impacted the ratings.	
Jennings 2017	Some concerns	High	High	Low	High
	No information on randomisation concealment. No apparent baseline between-group differences in demographics or outcomes.	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context. Not reported whether the analysis was ITT or per-protocol	Data missingness not reported. Teachers who were not happy to be observed and rated using the CLASS could have withdrawn their consent to be observed. Missingness is not reported so unclear how many if any participants did that.	Observations were conducted by 24 ethnically diverse certified coders who were blind to teacher intervention condition.	No pre-specified analysis plan available.
Klatt 2015	Some concerns	High	High	High	High
	No information on randomisation procedure, concealment or between-group differences at baseline.	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context. Not reported whether the analysis was ITT or per-protocol	Data missingness not reported. Missingness could have been due attrition, which in turn could have been affected by engagement and perceived benefit.	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	No pre-specified analysis plan available.
Klatt 2017	High	High	High	High	High
	No information on randomisation concealment. Group allocation is said to have been stratified based on sex, yet there are considerable differences	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context. Probably per-protocol	Data missingness not reported. Reasons to discontinue with study (n = 20) included work-related conflict, uninterested, injury.	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	No pre-specified analysis plan available. From Table 1 it seems data was also collected on presenteeism, absenteeism, capacity to work, and ability to

	in the proportion of males and females in the two groups (int: 22% male; control 40% male).	analysis, based on CONSORT diagram		Participants in the control group did not participate in the 9-week follow up.	perform daily life activities among other things. These data are only presented for baseline.
Kor 2019	Low	Some concerns	High	High	High
	"An independent research assistant randomized the subjects /.../ using the computer-generated random numbers /.../. The participants would be informed of their group allocation via a sealed opaque envelope, which was concealed to the researchers and the assessors". No information on baseline imbalances.	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context. ITT used.	Data missingness not reported. Missingness could have been due attrition, which in turn could have been affected by engagement and perceived benefit.	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	No pre-specified analysis plan available.
Kor 2020	Low	Some concerns	High	High	High
	Online sequence generation randomization tool was used. Participants "received notice of their group allocation in an opaque, sealed envelope /.../. The group allocation lists were concealed from the researchers, the staff of the older people centres,	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context. ITT used.	Data missingness not reported. Missingness could have been due attrition, which in turn could have been affected by engagement and perceived benefit.	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	Protocol pre-specifies MANOVA, paper reports on GEE instead.

	and the outcome assessors".				
Lin 2019	Some concerns	High	High	High	High
	No information on randomisation concealment. No apparent between-group differences in demographics or outcomes at baseline.	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context but long delay for waitlist control until they received intervention may have lead to participants seeking support elsewhere. Probably per-protocol analysis as 11 participants seem to have excluded due to low engagement in intervention.	Data missingness not reported but per protocol analysis excluded some participants. Missingness could have been due attrition, which in turn could have been affected by engagement and perceived benefit.	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	No pre-specified analysis plan available.
Nadler 2020	Some concerns	High	High	High	High
	No information on randomisation procedure or concealment. No apparent between-group differences in demographics or outcomes at baseline.	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context. Analysis was per-protocol	Data missingness not reported, attrition high post randomisation and post intervention. Missingness could have been due attrition, which in turn could have been affected by engagement and perceived benefit.	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	No pre-specified analysis plan available.
Orosa-Duarte 2021	High	High	High	High	High
	Website used to randomise rules out	22 of 29 participants in the IMBP group did not	Data missingness not reported but attrition is	Self-reported measure used, thus blinding	No pre-specified analysis plan available.

	concealment. No apparent between-group differences in demographics or outcomes at baseline.	receive the intervention. No information is provided for the App group nor the control group. Authors claim to have done ITT but also say they excluded participants who did not complete 8 week assessment.	high (no data available for 70 out of 154 participants). Missingness could have been due attrition, which in turn could have been affected by engagement and perceived benefit.	towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	
Pang 2019	High	High	Low	High	High
	Only 40% of the sample was randomised, the other received allocation based on participant availability. Concealment thus not possible. No apparent baseline between-group differences.	Not possible to blind participants/intervention facilitators. Waitlist was for 1 year and may have led to some participants seek support earlier. Although people did also pay their participation fee which reduced the likelihood of them seeking additional help. ITT performed	Data available for nearly all participants	Supervisors rated performance, it is unclear whether they were blind to allocation	No pre-specified analysis plan available.
Perez-Blasco 2016	High	High	High	High	High
	Randomisation procedure did not allow for concealment. No apparent between-group differences in demographics or outcomes at baseline.	"The participants did not know which group they belonged to until the second data collection was completed". Given it was a passive control group study, it does not seem to be possible to blind participants/intervention	Data missingness not reported. Missingness could have been due attrition, which in turn could have been affected by engagement and perceived benefit.	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	No pre-specified analysis plan available.

		facilitators. No information about possible deviations due to the trial context. Per-protocol analysis: "two participants did not have 90% attendance and were excluded from the analyses"			
Phang 2015	High	High	High	High	High
	Randomisation procedure does not allow for concealment. Also, intervention group has more favourable scores in all of the 4 outcomes collected at baseline. This could also be due to randomness but the probability of all of the 4 measures favouring the intervention group is low.	Not possible to blind participants/intervention facilitators. Waitlist participants had to wait 6 months to receive intervention and may have sought support independently. ITT used	Data missingness not reported. Missingness could have been due attrition, which in turn could have been affected by engagement and perceived benefit.	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	No pre-specified analysis plan available.
Pipe 2009	High	High	Low	High	High
	Participants met with the research team for a project overview prior to intervention, concealment thus unlikely. No baseline data available	Not possible to blind participants/intervention facilitators. When participants joined the study they were told there is a 1 year follow-up period. It is not clear when the decision was made to scrap it. The participants in the control condition could	Data available for nearly all participants	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	No pre-specified analysis plan available.

		have sought support elsewhere given the paper's authors report high stress levels. Probably per-protocol analysis done as the person who withdrew from the study is excluded (not clear if withdrew from the intervention or the study altogether).			
Rich 2021	Low	Some concerns	High	High	High
	Randomisation done in Qualtrics which does allow for concealment. Not explicitly stated whether allocation was concealed. No apparent between-group imbalances.	Not possible to blind participants but intervention was delivered through an app. The app, "Headspace" is available for anyone to access and control group participants may have accessed it despite their group allocation. ITT used.	Data missingness not reported, but attrition high. Those who dropped out (and thus were less likely to fill in T2), also had lower job engagement.	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	No pre-specified analysis plan available.
Roeser 2013	Some concerns	High	High	High	High
	No information on randomisation but also no apparent between-group imbalances.	Not possible to blind participants/intervention facilitators. Waitlist control had to wait 6 months to access intervention and may have sought support independently. Not	Data missingness not reported. But reported withdrawal reasons include not finding the intervention engaging/worthwhile and having a health crisis.	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	No pre-specified analysis plan available.

		reported whether the analysis was ITT or per-protocol	These could have influenced outcomes.		
Sampl 2017	High	High	High	Low	High
	No information on randomisation procedure or concealment. Unequal group allocation (51 vs 58). This could have happened randomly but unclear. Also mean age differs. Of the measures, control group fares slightly worse outcome measures except one (self-leadership). There are no statistically significant differences.	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context. Not reported whether the analysis was ITT or per-protocol	High missingness for grades, the main outcome. The outcome measures of interest are related to ability to perform in during the exam period. If participants felt they were not doing well, they may have had less motivation to complete measures.	Main outcome of interest was average grade - assessors were thus blind to allocation. Secondary outcomes were self-reported.	No pre-specified analysis plan available.
Schroeder 2018	Some concerns	High	Low	Low	High
	No information on randomisation procedure or concealment. No apparent between-group differences in demographics or outcomes at baseline.	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context. It's likely that per-protocol approach was used.	High missingness which was dealt with REML.	Outcomes not self-reported	No pre-specified analysis plan available.
Shapiro 1998	Some concerns	High	High	High	High
	No information on randomisation procedure or concealment. Between-group demographics and outcomes at baseline not	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context. Not	Data missingness not reported for outcome of interest. Missingness could have been due attrition, which in turn could have been	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the	No pre-specified analysis plan available.

	available. Significance testing for differences yielded a 0-result.	reported whether the analysis was ITT or per-protocol	affected by engagement and perceived benefit.	effectiveness could have impacted the ratings.	
Shapiro 2011	Some concerns	High	High	High	High
	No information on randomisation but also no apparent between-group imbalances.	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context. Not reported whether the analysis was ITT or per-protocol	Data missingness present for several variables. Missingness could have been due attrition, which in turn could have been affected by engagement and perceived benefit.	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	No pre-specified analysis plan available.
Steinberg 2017	Some concerns	Some concerns	High	High	High
	No information on randomisation concealment. No apparent between-group differences in demographics or outcomes at baseline.	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context. ITT used.	Data missingness not reported. Missingness could have been due attrition, which in turn could have been affected by engagement and perceived benefit.	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	No pre-specified analysis plan available. No group*time interaction analyses are presented, just time-interaction.
Strauss 2021	Some concerns	High	Some concerns	High	High
	Not explicitly stated but randomisation procedure used does allow for concealment. Lack of information on baseline imbalances.	Not possible to blind participants/intervention facilitators. There were more people in the intervention group who did not receive the allocated intervention (n = 21) compared to the control group (n = 4). ITT used.	Although missingness for the outcome of interest is not clearly reported, overall loss to follow-up was around 33% in the intervention group and 24% in the control group. "Missing-values analysis revealed that stress and wellbeing met criteria for MCAR [Little's MCAR2 (df = 71) = 58.34, p = .859].	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	The protocol specifies the use of mixed ANOVA. The main outcome paper uses regression. The protocol states that "Presenteeism is measured using items from the Institute for Medical Technology Assessment Productivity Cost Questionnaire". It does not specify which

					items and how many. Similarly it is stated that "Compassion is measured using the Compassion Scale" without specifying which scale.
Taylor 2016	High	High	High	High	High
	No information on randomisation procedure or concealment. The participants assigned to the intervention group were more stressed. Also, demographic information suggest the study had 59 participants, but 56 were randomised. There is no mention what happened to the three.	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context. Not reported whether the analysis was ITT or per-protocol	Follow-up data is not reported for anyone. Data missingness for the outcome of interest is not reported.	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	No pre-specified analysis plan available. Follow-up data not reported although collected.
Valley 2017	Some concerns	High	High	High	High
	No information on randomisation concealment. No apparent between-group differences in demographics or outcomes at baseline.	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context. Not reported whether the analysis was ITT or per-protocol	5 out of 12 participants in the control group dropped out. 6-months follow-up was collected only from those who completed the course (including wait-list controls who could do mindfulness after post-intervention)	Self-reported measure used, thus blinding towards assignment was impossible. Control group data bot collected d for follow-up	No pre-specified analysis plan available. Data were presented differently for different time-points.
	High	High	Low	High	High

van Berkel 2014, van Dongen 2016	Randomisation procedure probably did not allow for concealment. No apparent between-group differences in demographics or outcomes at baseline.	Not possible to blind participants/intervention facilitators. Waitlist participants needed to wait a year to access intervention and may have sought support independently. ITT used.	Data missingness is low	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	Analysis plan available but undated. Not all outcomes of interest are specified. In van Dongen 2016, the change in WAI is said to not be statistically significant. There could therefore be a file-drawer effect as the data is not reported in other reports.
vanDijk 2017	High	High	Low	High	High
	Group allocation not concealed. No apparent between-group differences in demographics or outcomes at baseline.	Not possible to blind participants/intervention facilitators. Control group participants had to wait for the intervention for 6 months and may have sought support independently. ITT used.	Data missingness not reported but sensitivity analysis run to account for their effect.	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	Pre-specified analysis plan not accessible.
Verweij 2018	Some concerns	Some concerns	High	High	High
	No information on randomisation concealment but also no apparent between-group imbalances.	Not possible to blind participants/intervention facilitators. No information about possible deviations due to the trial context.	Data missingness not reported. Missingness could have been due attrition, which in turn could have been affected by engagement and perceived benefit. Sensitivity analyses were performed but not for the outcomes of interest for the current review	Self-reported measure used, thus blinding towards assignment was impossible. Pre-conceived beliefs in the effectiveness could have impacted the ratings.	The outcome of interest scale is reported as subscales with no total score.

Table S 3. Potentially eligible studies pre-registered in ICTRP by March 30, 2018 but not published by March 30, 2021. Their eligibility could not always be assessed due to lack of clarity in pre-registrations.

#	Trial ID	Title
1	JPRN-UMIN000031435	Mindfulness for health professionals building resilience and compassion (MHALO program) - randomized control trial - MHALO program
2	CTRI/2018/02/011902	A Randomized Control Trial to assess the effect of counseling on Distress Tolerance, Interpersonal Relationship and Mindfulness among nursing students
3	JPRN-UMIN000029885	Mindfulness Training in Return-to-Work Program - Mindfulness Training in Return-to-Work Program
4	JPRN-UMIN000029791	The effect of mindfulness-based stress reduction program for workers
5	ACTRN12617001386325	Evaluating the effectiveness of app-based mindfulness training, with and without class-attendance, for reducing stress in a public service workforce.
6	NCT03148626	Can a Mindfulness Curriculum Prevent Burnout During Pediatric Internship? A Multi-center Cluster Randomized Controlled Trial.
7	ChiCTR-IOC-17011047	Effects of mindfulness intervention on Job Burnout of nurses in intensive care unit
8	IRCT201702054299N5	The effect of mindfulness intervention on job stress of nurses in intensive care units
9	IRCT2017022010063N6	Efficacy of mindfulness based emotional balance self-help program on psychopathology indicators, mindfulness and self-compassion in students
10	ACTRN12617000049370	Enhancing wellbeing of JMOs with mindfulness meditation pilot programme
11	ACTRN12616001252404	The influence of mindfulness training on anxiety and golf performance under pressure
12	NCT02897284	Evaluation of Mindfulness-based Self-care Programs for the Prevention of Burnout Among Primary Care Providers: Psychological, Inflammatory and Epigenetic Effects
13	NCT02867657	Enhancement of Presence, Compassion and Resilience Bringing the Practice of Mindfulness Into Nature - Preventing Mental Fatigue in Healthcare Professionals.
14	NCT02769403	Using a Daily Mindfulness Practice With Biofeedback to Improve Job Satisfaction and Performance in a Primary Care Outpatient Clinic
15	NCT02709551	Comparative Intervention Study of Stress Reduction in Corporate Health Management: Evaluation of the Effects of Heart Rate Variability (HRV) Biofeedback Training, Mindfulness Based Intervention (MBI) and Mindfulness Based HRV-biofeedback
16	NTR5001	Efficacy of a mindfulness app in promoting mindfulness, mental health, quality of life, and self-actualization.
17	ISRCTN88000243	A pilot feasibility trial of a brief mindfulness-based intervention in a mental health secondary care setting: a randomised controlled trial
18	ISRCTN62401721	Effects of mindfulness-based stress reduction (MBSR) on stress, depression, self-esteem and mindfulness in Thai nursing students: A randomised controlled trial

#	Trial ID	Title
19	ISRCTN03386834	Longitudinal evaluation of cost effectiveness and wellbeing related variables of mindfulness training in the workplace
20	ACTRN12610000833066	A randomised controlled trial of a fully automated online mindfulness program focussing on 18–25 year TAFE and Further Education Students
21	NCT01212497	Virtual Coach for Mindfulness Meditation Training
22	NCT01082497	Mindfulness Training and Developing the Ability of Empathy at an Inpatient Ward for Dual Diagnoses
23	NCT00214357	The Effects of Mindfulness Training on School Staff Emotions, Attention, and Stress

Reporting bias

The funnel plot for the main outcome: task performance measured up to 4 weeks post-intervention only includes 7 studies which is fewer than the 10 recommended by Cochrane. Thus, it should be interpreted with caution.

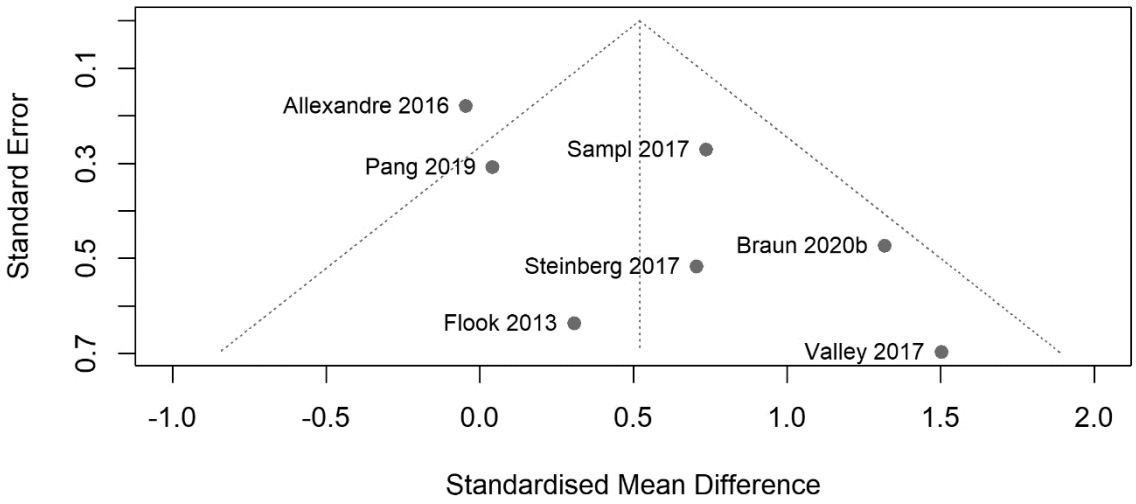


Figure S 1. Funnel plot for task performance measured up to 4 weeks post-intervention

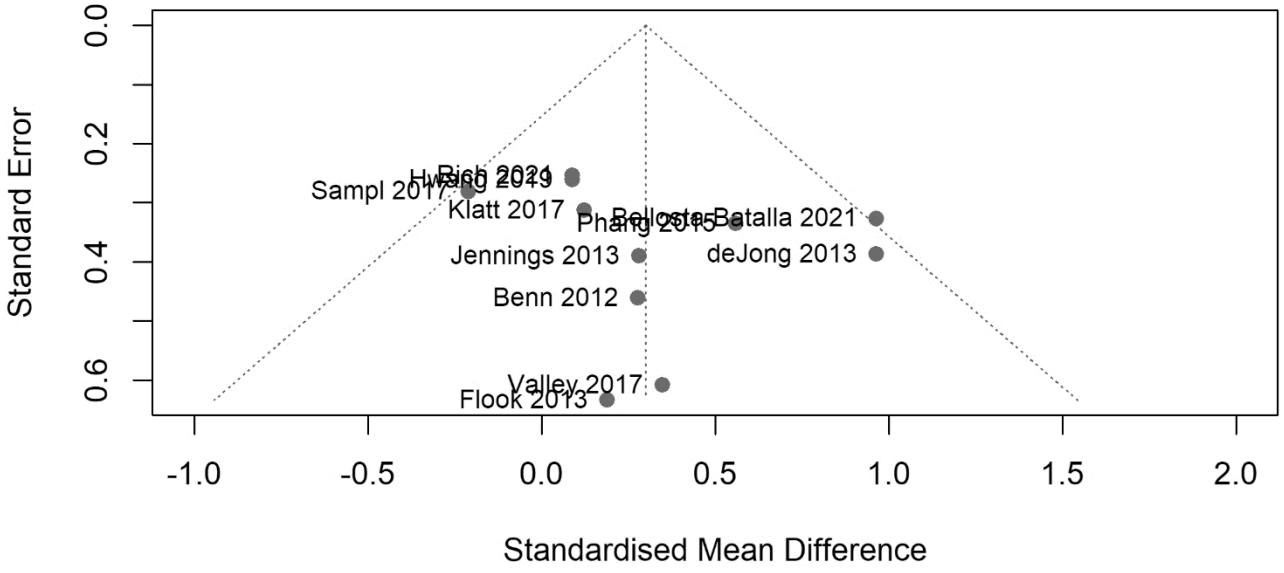


Figure S 2. Funnel plot for contextual performance measured up to 4 weeks post-intervention

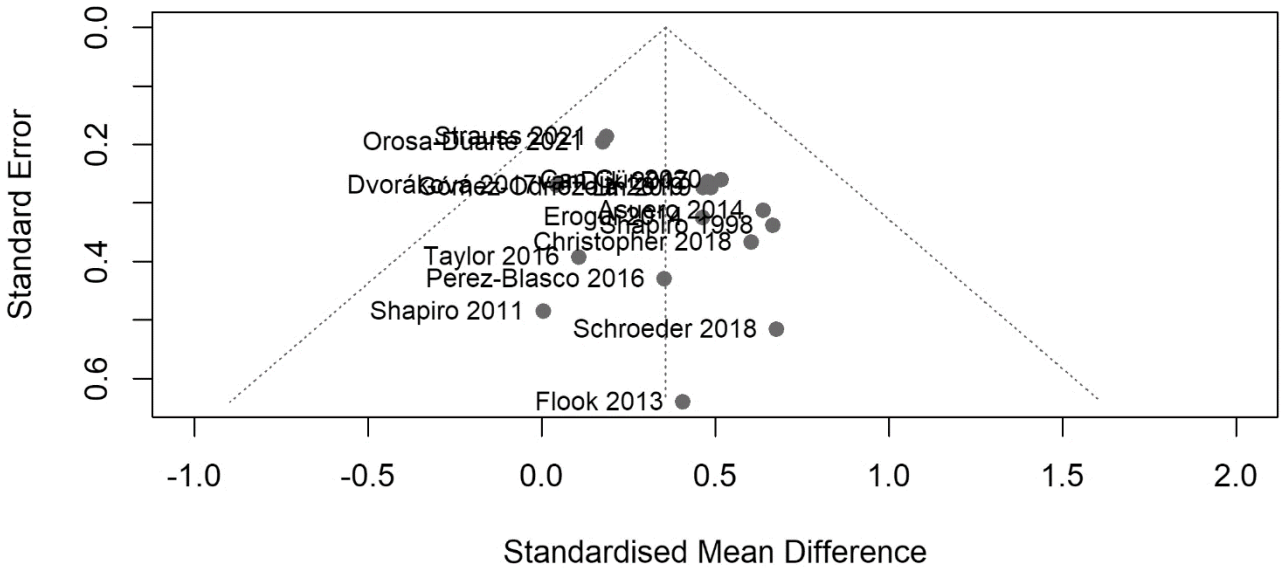


Figure S 3. Funnel plot for adaptive performance measured up to 4 weeks post-intervention

Outcome measures used

Table S 4. Outcome measures used to capture work performance

Domain Study	Construct	Scale	Authors	Direction for improvement
Adaptive performance				
Asuero 2014	Compassion towards others	Jefferson Scale of Physician Empathy	Hojat, M., Gonnella, J. S., Nasca, T. J., Mangione, S., Vergare, M., & Magee, M. (2002). Physician Empathy: Definition, Components, Measurement, and Relationship to Gender and Specialty. In American Journal of Psychiatry (Vol. 159, Issue 9, pp. 1563–1569). American Psychiatric Association Publishing. https://doi.org/10.1176/appi.ajp.159.9.1563	Increase
Can Gür 2020	Compassion towards others	Jefferson Empathy Scale	Yanık, A., & Saygılı, S. (2014). Validity and Reliability of the Turkish Version of Jefferson Scale of Empathy for Nursing Students. In Turkiye Klinikleri Journal of Medical Sciences (Vol. 34, Issue 1, pp. 111–119). Turkiye Klinikleri. https://doi.org/10.5336/medsci.2013-37793	Increase
Chan 2021	Compassion towards others	Interpersonal Reactivity Index: Empathic Concern	Siu, A. M. H., & Shek, D. T. L. (2005). Validation of the Interpersonal Reactivity Index in a Chinese Context. In Research on Social Work Practice (Vol. 15, Issue 2, pp. 118–126). SAGE Publications. https://doi.org/10.1177/1049731504270384	Increase
Christopher 2018	Resilience	Connor-Davidson Resilience Scale	Connor, K. M., & Davidson, J. R. T. (2003). Development of a new resilience scale: The Connor-Davidson Resilience Scale (CD-RISC). In Depression and Anxiety (Vol. 18, Issue 2, pp. 76–82). Wiley. https://doi.org/10.1002/da.10113	Increase
Dvoráková 2017	Compassion towards others	Compassion Scale	Pommier, E., Neff, K. D., & Tóth-Király, I. (2019). The Development and Validation of the Compassion Scale. In Assessment (Vol. 27, Issue 1, pp. 21–39). SAGE Publications. https://doi.org/10.1177/1073191119874108	Increase

Domain	Study	Construct	Scale	Authors	Direction for improvement
	Erogul 2014	Resilience	Resilience Scale	Wagnild, G. M., & Young, H. M. (1993). Development and psychometric evaluation of the Resilience Scale. Journal of nursing measurement, 1 (2), 165–178.	Increase
	Flook 2013	Understanding other groups or cultures	CLASS: Emotional support	La Paro, K. M., Pianta, R. C., & Stuhlman, M. (2004). The Classroom Assessment Scoring System: Findings from the Prekindergarten Year. In The Elementary School Journal (Vol. 104, Issue 5, pp. 409–426). University of Chicago Press. https://doi.org/10.1086/499760	Increase
	Gómez-Odriozola 2019	Resilience	Brief Resilience Scale	Smith, B. W., Dalen, J., Wiggins, K., Tooley, E., Christopher, P., & Bernard, J. (2008). The brief resilience scale: Assessing the ability to bounce back. In International Journal of Behavioral Medicine (Vol. 15, Issue 3, pp. 194–200). Springer Science and Business Media LLC. https://doi.org/10.1080/10705500802222972	Increase
	Jennings 2017	Understanding other groups or cultures	CLASS: Emotional support	La Paro, K. M., Pianta, R. C., & Stuhlman, M. (2004). The Classroom Assessment Scoring System: Findings from the Prekindergarten Year. In The Elementary School Journal (Vol. 104, Issue 5, pp. 409–426). University of Chicago Press. https://doi.org/10.1086/499760	Increase
	Klatt 2015	Resilience	Connor-Davidson Resilience Scale	Connor, K. M., & Davidson, J. R. T. (2003). Development of a new resilience scale: The Connor-Davidson Resilience Scale (CD-RISC). In Depression and Anxiety (Vol. 18, Issue 2, pp. 76–82). Wiley. https://doi.org/10.1002/da.10113	Increase
	Kor 2019	Resilience	Brief Resilience Scale	Smith, B. W., Dalen, J., Wiggins, K., Tooley, E., Christopher, P., & Bernard, J. (2008). The brief resilience scale: Assessing the ability to bounce back. In International Journal of Behavioral Medicine (Vol. 15, Issue 3, pp. 194–200). Springer Science and Business Media LLC. https://doi.org/10.1080/10705500802222972	Increase
	Kor 2020	Resilience	Brief Resilience Scale	Smith, B. W., Dalen, J., Wiggins, K., Tooley, E., Christopher, P., & Bernard, J. (2008). The brief resilience scale: Assessing the ability to bounce back. In International Journal of Behavioral Medicine (Vol. 15, Issue 3, pp. 194–200). Springer Science and Business Media LLC. https://doi.org/10.1080/10705500802222972	Increase

Domain	Study	Construct	Scale	Authors	Direction for improvement
	Lin 2019	Resilience	Connor-Davidson Resilience Scale	Connor, K. M., & Davidson, J. R. T. (2003). Development of a new resilience scale: The Connor-Davidson Resilience Scale (CD-RISC). In Depression and Anxiety (Vol. 18, Issue 2, pp. 76–82). Wiley. https://doi.org/10.1002/da.10113	Increase
	Nadler 2020	Resilience	Brief Resilience Scale	Smith, B. W., Dalen, J., Wiggins, K., Tooley, E., Christopher, P., & Bernard, J. (2008). The brief resilience scale: Assessing the ability to bounce back. In International Journal of Behavioral Medicine (Vol. 15, Issue 3, pp. 194–200). Springer Science and Business Media LLC. https://doi.org/10.1080/10705500802222972	Increase
	Orosa-Duarte 2021	Compassion towards others	Jefferson Scale of Physician Empathy	Hojat, M., Gonnella, J. S., Nasca, T. J., Mangione, S., Vergare, M., & Magee, M. (2002). Physician Empathy: Definition, Components, Measurement, and Relationship to Gender and Specialty. In American Journal of Psychiatry (Vol. 159, Issue 9, pp. 1563–1569). American Psychiatric Association Publishing. https://doi.org/10.1176/appi.ajp.159.9.1563	Increase
	Perez-Blasco 2016	Resilience	The Brief Resilient Coping Scale	Tomás, J. M., Meléndez, J. C., Sancho, P., & Mayordomo, T. (2012). Adaptation and Initial Validation of the BRCS in an Elderly Spanish Sample. In European Journal of Psychological Assessment (Vol. 28, Issue 4, pp. 283–289). Hogrefe Publishing Group. https://doi.org/10.1027/1015-5759/a000108	Increase
	Schroeder 2018	Compassion towards others	Santa Clara Brief Compassion Scale	Hwang, J. Y., Plante, T., & Lackey, K. (2008). The Development of the Santa Clara Brief Compassion Scale: An Abbreviation of Sprecher and Fehr's Compassionate Love Scale. In Pastoral Psychology (Vol. 56, Issue 4, pp. 421–428). Springer Science and Business Media LLC. https://doi.org/10.1007/s11089-008-0117-2	Increase
	Shapiro 1998	Compassion towards others	Empathy Construct Rating Scale	Adapted from Monica, E. L. L. (1981). Construct validity of an empathy instrument. In Research in Nursing & Health (Vol. 4, Issue 4, pp. 389–400). Wiley. https://doi.org/10.1002/nur.4770040406	Increase

Domain	Study	Construct	Scale	Authors	Direction for improvement
	Shapiro 2011	Compassion towards others	Interpersonal Reactivity Index	Davis, M. H. (1983). Measuring individual differences in empathy: Evidence for a multidimensional approach. In Journal of Personality and Social Psychology (Vol. 44, Issue 1, pp. 113–126). American Psychological Association (APA). https://doi.org/10.1037/0022-3514.44.1.113	Increase
	Strauss 2021	Compassion towards others	Sussex-Oxford Compassion Scale Other	Gu, J., Baer, R., Cavanagh, K., Kuyken, W., & Strauss, C. (2019). Development and Psychometric Properties of the Sussex-Oxford Compassion Scales (SOCS). In Assessment (Vol. 27, Issue 1, pp. 3–20). SAGE Publications. https://doi.org/10.1177/1073191119860911	Increase
	Taylor 2016	Compassion towards others	4-item Santa Clara Brief Compassion Scale	Adapted from Hwang, J. Y., Plante, T., & Lackey, K. (2008). The Development of the Santa Clara Brief Compassion Scale: An Abbreviation of Sprecher and Fehr's Compassionate Love Scale. In Pastoral Psychology (Vol. 56, Issue 4, pp. 421–428). Springer Science and Business Media LLC. https://doi.org/10.1007/s11089-008-0117-2	Increase
	van Dijk 2017	Compassion towards others	Jefferson Scale of Physician Empathy	Adapted from Hojat, M., Gonnella, J. S., Nasca, T. J., Mangione, S., Vergare, M., & Magee, M. (2002). Physician Empathy: Definition, Components, Measurement, and Relationship to Gender and Specialty. In American Journal of Psychiatry (Vol. 159, Issue 9, pp. 1563–1569). American Psychiatric Association Publishing. https://doi.org/10.1176/appi.ajp.159.9.1563	Increase
Contextual performance					
	Bellosta-Batalla 2021	Creativity	Creative Imagination Test for Adults: Fluency subscale	Artola, T., Ancillo, I., Barraca, J. & Mosteiro, P. (2010). PIC-A. Prueba de Imaginación Creativa para Adultos [Creative Imagination Test for Adults]. Madrid: TEA Edicione	Increase
	Benn 2012	Efficacy	Teaching self-efficacy	Adapted from Midgley, C., Maehr, M. L., Hruda, L. Z., Anderman, E., Anderman, L., Freeman, K. E., . . . Urdan, T. (2000). Manual for the Patterns of Adaptive Learning Scales (PALS). Ann Arbor, MI: University of Michigan.	Increase

Domain	Study	Construct	Scale	Authors	Direction for improvement
	Braun 2020a	Interpersonal relations	Tendency to forgive scale	Brown, R. P. (2003). Measuring Individual Differences in the Tendency to Forgive: Construct Validity and Links with Depression. In Personality and Social Psychology Bulletin (Vol. 29, Issue 6, pp. 759–771). SAGE Publications. https://doi.org/10.1177/0146167203029006008	Increase
	Brown 2016	Interpersonal relations	Mutuality Scale of the Family Care Inventory	Archbold, P. G., Stewart, B. J., Greenlick, M. R., & Harvath, T. (1990). Mutuality and preparedness as predictors of caregiver role strain. In Research in Nursing & Health (Vol. 13, Issue 6, pp. 375–384). Wiley. https://doi.org/10.1002/nur.4770130605	Increase
	De Jong 2013	Efficacy	Job Seeking Self-Efficacy Scale	Barlow, J., Wright, C., & Cullen, L. (2002). A job-seeking self-efficacy scale for people with physical disabilities: Preliminary development and psychometric testing. In British Journal of Guidance & Counselling (Vol. 30, Issue 1, pp. 37–53). Informa UK Limited. https://doi.org/10.1080/03069880220106500	Increase
	Flook 2013	Initiative and proactivity	CLASS: Instructional Support	La Paro, K. M., Pianta, R. C., & Stuhlman, M. (2004). The Classroom Assessment Scoring System: Findings from the Prekindergarten Year. In The Elementary School Journal (Vol. 104, Issue 5, pp. 409–426). University of Chicago Press. https://doi.org/10.1086/499760	Increase
	Hwang 2019	Efficacy	Teachers' Sense of Efficacy Questionnaire Short Form	Tschannen-Moran, M., & Woolfolk Hoy, A. (2001). Teacher efficacy: Capturing and elusive construct. Teaching and Teacher Education, 17, 783-805	Increase
	Jennings 2013	Efficacy	Teachers' Sense of Efficacy Questionnaire Long Form	Tschannen-Moran, M., & Woolfolk Hoy, A. (2001). Teacher efficacy: Capturing and elusive construct. Teaching and Teacher Education, 17, 783-805	Increase
	Jennings 2017	Initiative and proactivity	CLASS: Instructional Support	La Paro, K. M., Pianta, R. C., & Stuhlman, M. (2004). The Classroom Assessment Scoring System: Findings from the Prekindergarten Year. In The Elementary School Journal (Vol. 104, Issue 5, pp. 409–426). University of Chicago Press. https://doi.org/10.1086/499760	Increase

Domain Study	Construct	Scale	Authors	Direction for improvement
Klatt 2015	Engagement	Utrecht Work Engagement Scale-9	Schaufeli, W. B., & Bakker, A. B. (2004). Job demands, job resources, and their relationship with burnout and engagement: a multi-sample study. In Journal of Organizational Behavior (Vol. 25, Issue 3, pp. 293–315). Wiley. https://doi.org/10.1002/job.248	Increase
Klatt 2017	Engagement	Utrecht Work Engagement Scale-9	Schaufeli, W. B., & Bakker, A. B. (2004). Job demands, job resources, and their relationship with burnout and engagement: a multi-sample study. In Journal of Organizational Behavior (Vol. 25, Issue 3, pp. 293–315). Wiley. https://doi.org/10.1002/job.248	Increase
Phang 2015	Efficacy	General Self-Efficacy	Schwarzer, R., & Jerusalem, M. (1995). General Self-Efficacy Scale [Data set]. In PsycTESTS Dataset. American Psychological Association (APA). https://doi.org/10.1037/t00393-000	Increase
Pipe 2009	Efficacy	Caring Efficacy Scale	Adapted from Coates C. J. (1997). The Caring Efficacy Scale: nurses' self-reports of caring in practice settings. Advanced practice nursing quarterly, 3 (1), 53–59.	Increase
Rich 2021	Engagement	Job Engagement Scale	Rich, B. L., Lepine, J. A., & Crawford, E. R. (2010). Job Engagement: Antecedents and Effects on Job Performance. In Academy of Management Journal (Vol. 53, Issue 3, pp. 617–635). Academy of Management. https://doi.org/10.5465/amj.2010.51468988	Increase
Sampl 2017	Efficacy	Self-Efficacy Scale	Pintrich, P. R., & De Groot, E. V. (1990). Motivational and self-regulated learning components of classroom academic performance. In Journal of Educational Psychology (Vol. 82, Issue 1, pp. 33–40). American Psychological Association (APA). https://doi.org/10.1037/0022-0663.82.1.33	Decrease
Steinberg 2017	Engagement	Utrecht Work Engagement Scale-9	Schaufeli, W. B., & Bakker, A. B. (2004). Job demands, job resources, and their relationship with burnout and engagement: a multi-sample study. In Journal of Organizational Behavior (Vol. 25, Issue 3, pp. 293–315). Wiley. https://doi.org/10.1002/job.248	Increase

Domain	Study	Construct	Scale	Authors	Direction for improvement
	Valley 2017	Effort	Workplace safety performance: Safety participation	Adapted from Neal, A., Griffin, M. A., & Hart, P. M. (2000). The impact of organizational climate on safety climate and individual behavior. In Safety Science (Vol. 34, Issues 1–3, pp. 99–109). Elsevier BV. https://doi.org/10.1016/s0925-7535 (00)00008-4	Increase
Counterproductive work behaviour					
	Bartlett 2017	Absenteeism and presenteeism	Productivity loss in days	Bespoke scale	Decrease
	Can Gür 2020	Discrimination	Age Discrimination Attitude Scale	Yılmaz D. V., and Terzioglu, F. (2011). Development and psychometric evaluation of ageism attitude scale among the university students. Turk Geriatri Dergisi 14 (3), 259-268	Decrease
	Hunsinger 2019	Discrimination	Shooter Bias Task	Adapted from Correll, J., Park, B., Judd, C. M., & Wittenbrink, B. (2002). The police officer's dilemma: Using ethnicity to disambiguate potentially threatening individuals. In Journal of Personality and Social Psychology (Vol. 83, Issue 6, pp. 1314–1329). American Psychological Association (APA). https://doi.org/10.1037/0022-3514.83.6.1314	Increase
	Roeser 2013	Absenteeism	Teacher absences from work	Bespoke scale	Decrease
	Steinberg 2017	Absenteeism	Absenteeism	Bespoke scale	Decrease
	Strauss 2021	Presenteeism	Institute for Medical Technology Assessment Productivity Cost Questionnaire	Adapted from Bouwmans, C., Krol, M., Severens, H., Koopmanschap, M., Brouwer, W., & Roijen, L. H. (2015). The iMTA Productivity Cost Questionnaire. In Value in Health (Vol. 18, Issue 6, pp. 753–758). Elsevier BV. https://doi.org/10.1016/j.jval.2015.05.009	Decrease
	van Berkel 2014, van Dongen 2016	Absenteeism	Absenteeism	Bespoke scale	Decrease
Task performance					
	Allexandre 2016	Productivity	Bespoke score	Bespoke scale	Decrease

Domain	Study	Construct	Scale	Authors	Direction for improvement
	Braun 2020b	Productivity	Work Productivity and Activity Impairment Questionnaire plus Classroom Impairment Questions	Reilly, M. C., Zbrozek, A. S., & Dukes, E. M. (1993). The Validity and Reproducibility of a Work Productivity and Activity Impairment Instrument. In PharmacoEconomics (Vol. 4, Issue 5, pp. 353–365). Springer Science and Business Media LLC. https://doi.org/10.2165/00019053-199304050-00006	Decrease
	Daigle 2018	Accuracy	The Nursing Errors Rating Scale	Bespoke scale	Increase
	Flook 2013	Work quality	CLASS: Classroom organisation	La Paro, K. M., Pianta, R. C., & Stuhlman, M. (2004). The Classroom Assessment Scoring System: Findings from the Prekindergarten Year. In The Elementary School Journal (Vol. 104, Issue 5, pp. 409–426). University of Chicago Press. https://doi.org/10.1086/499760	Increase
	Galante 2018	Work quality	Examination results	Bespoke scale	Decrease
	Glass 2019	Skills or knowledge	Coach Rating Form	Bespoke scale	Increase
	Jennings 2017	Work quality	CLASS: Classroom organisation	La Paro, K. M., Pianta, R. C., & Stuhlman, M. (2004). The Classroom Assessment Scoring System: Findings from the Prekindergarten Year. In The Elementary School Journal (Vol. 104, Issue 5, pp. 409–426). University of Chicago Press. https://doi.org/10.1086/499760	Increase
	Pang 2019	Work quality	Task Performance Questionnaire	Adapted from Williams, L. J., & Anderson, S. E. (1991). Job Satisfaction and Organizational Commitment as Predictors of Organizational Citizenship and In-Role Behaviors. In Journal of Management (Vol. 17, Issue 3, pp. 601–617). SAGE Publications. https://doi.org/10.1177/014920639101700305	Increase
	Sampl 2017	Work quality	Grade point Average	Bespoke scale	Decrease

Domain	Study	Construct	Scale	Authors	Direction for improvement
	Schroeder 2018	Work quality	Consumer Assessment of Healthcare Providers and Systems–Clinician and Group Adult Doctor Communication Composite	Adapted from Dyer, N., Sorra, J. S., Smith, S. A., Cleary, P. D., & Hays, R. D. (2012). Psychometric Properties of the Consumer Assessment of Healthcare Providers and Systems (CAHPS®) Clinician and Group Adult Visit Survey. In Medical Care (Vol. 50, pp. S28–S34). Ovid Technologies (Wolters Kluwer Health). https://doi.org/10.1097/mlr.0b013e31826cbc0d	Increase
	Steinberg 2017	Work quality	Decreased ability to work	Bespoke scale	Decrease
	Valley 2017	Work quality	Workplace cognitive failure	Adapted from Wallace, J. Craig., & Chen, G. (2005). Development and validation of a work-specific measure of cognitive failure: Implications for occupational safety. In Journal of Occupational and Organizational Psychology (Vol. 78, Issue 4, pp. 615–632). Wiley. https://doi.org/10.1348/096317905x37442	Decrease
	van Berkel 2014, van Dongen 2016	Productivity	Work Ability Index	Adapted from Tuomi K, Ilmarinen J, Jahkola A, Katajarinne L, Tulkki A. (1998) Work Ability Index. Helsinki, Finland: Finish Institute of Occupational Health.	Increase
	Verweij 2018	Doing tasks incorrectly	Medical errors	Prins, J. T., van der Heijden, F. M. M. A., Hoekstra-Weebers, J. E. H. M., Bakker, A. B., van de Wiel, H. B. M., Jacobs, B., & Gazendam-Donofrio, S. M. (2009). Burnout, engagement and resident physicians' self-reported errors. In Psychology, Health & Medicine (Vol. 14, Issue 6, pp. 654–666). Informa UK Limited. https://doi.org/10.1080/13548500903311554	Decrease

Studies not meta-analysed due to lack of reports on outcomes

Table S 5. Studies not meta-analysed due to lack of reports on outcomes

Domain	Study ID	Time periods	Exclusion reason
Adaptive performance	Glass 2019	Up to 4 weeks post-intervention	Waitlist group data not reported
Adaptive performance	Klatt 2015	Up to 4 weeks post-intervention	Outcome data not reported
Adaptive performance	Shapiro 2019	All	Outcome data not reported
Adaptive performance	Taylor 2016	5–24 weeks post-intervention	Outcome data not reported
Contextual performance	Klatt 2017	5–24 weeks post-intervention	Waitlist group data not collected
Contextual performance	Steinberg 2017	Up to 4 weeks post-intervention	Outcome data not reported
Contextual performance	Valley 2017	More than 24 weeks post-intervention	Waitlist group data not collected
Task performance	Daigle 2018	5–24 weeks post-intervention	Waitlist group data not collected
Task performance	Glass 2019	Up to 4 weeks post-intervention	Waitlist group data not reported
Task performance	Glass 2019	5–24 weeks post-intervention	Waitlist group data not reported
Task performance	Glass 2019	More than 24 weeks post-intervention	Waitlist group data not reported
Task performance	Valley 2017	More than 24 weeks post-intervention	Waitlist group data not collected

Summary statistics for each study

Table S 6. Summary statistics of studies included: Task performance

	Baseline				Up to 4 weeks post-intervention				5–24 weeks post-intervention				More than 24 weeks post-intervention			
	MBP		Control		MBP		Control		MBP		Control		MBP		Control	
Study	n	M (SD)	n	M (SD)	n	M (SD)	n	M (SD)	n	M (SD)	n	M (SD)	n	M (SD)	n	M (SD)
Allexandre 2016	23	2.58 (0.47)	26	2.62 (0.64)	20	2.68 (0.44)	21	2.63 (0.49)	21	2.38 (0.63)	25	2.52 (0.65)		–		–
Braun 2020b		35.85 (33.26)		39.37 (29.7)		19.94 (28.48)		31.64 (33.21)		–		–		–		–
Daigle 2018		–		–		–		–		–		–		–		–
Flook 2013		5.19 (0.58)		5.35 (0.77)		5.5 (0.45)		5.27 (1.11)		–		–		–		–
Galante 2018		–		–		–		–	145	ordinal data	144	ordinal data		–		–
Glass 2019		7.23 (NA)		–		7.25 (NA)		–		–		–		–		–
Jennings 2017		4.86 (0.9)		4.97 (0.8)		–		–		5.13 (0.86)		5.01 (0.88)		–		–
Pang 2019		5.84 (0.73)	19	5.91 (0.71)		5.9 (0.76)	16	5.91 (0.54)		6.04 (0.64)	16	6.15 (0.44)		–		–
Sampl 2017		–		–	39	1.78 (0.53)	41	2.2 (0.77)		–		–		–		–
Schroeder 2018	16	9.12 (1.36)	17	9.01 (NA)		–		–	13	9.19 (1.44)	13	9.04 (1.23)		–		–
Steinberg 2017		2.07 (2.84)		2.75 (7.4)		1.29 (1.89)		3.79 (10.52)		–		–		–		–
Valley 2017		2.84 (NA)		2.77 (NA)		1.93 (NA)		2.49 (NA)		–		–		2.05 (NA)		–
Verweij 2018	80	2.4 (0.6)	68	2.3 (0.5)		–		–	71	2.3 (0.6)	67	2.3 (0.6)		–		–

Table S 7. Summary statistics of studies included: Contextual performance

	Baseline				Up to 4 weeks post-intervention				5–24 weeks post-intervention				More than 24 weeks post-intervention			
	MBP		Control		MBP		Control		MBP		Control		MBP		Control	
Study	n	M (SD)	n	M (SD)	n	M (SD)	n	M (SD)	n	M (SD)	n	M (SD)	n	M (SD)	n	M (SD)
Bellosta-Batalla 2021		50 (12.55)		49.73 (14.07)		64.84 (17.05)		54.54 (14.33)		64.68 (15.14)		55.38 (13.25)		–		–
Benn 2012	19	3.84 (0.55)	16	3.8 (0.52)	19	4.06 (0.55)	9	3.64 (0.43)	14	4.28 (0.56)	9	3.33 (0.9)		–		–
Braun 2020a	76	2.82 (0.77)	88	2.81 (0.79)		–		–	75	3.23 (0.7)	85	2.82 (0.87)	69	3.19 (0.73)	75	2.86 (0.79)
Brown 2016	23	41.52 (10.29)	15	41.07 (9.79)	19	40.53 (10.81)	15	42.53 (11.03)	19	40.42 (10.32)	15	41.36 (10.92)		–		–
deJong 2013		90.67 (13.07)		90.37 (13.33)		96.67 (9.08)		89.14 (17.01)		–		–		–		–
Flook 2013		3.49 (0.5)		3.98 (0.6)		3.69 (0.54)		3.84 (1)		–		–		–		–
Hwang 2019		6.84 (1.15)		7.22 (1.06)		7.03 (1.07)		7.35 (1.26)		6.77 (1.13)		7.23 (1.28)		–		–
Jennings 2013		6.69 (1.09)		6.92 (1.12)		7.13 (1.05)		6.7 (1.04)		–		–		–		–
Jennings 2017		2.75 (0.67)		2.77 (0.71)		–		–		2.49 (0.65)		2.51 (0.65)		–		–
Klatt 2017	27	4.55 (0.76)	30	4.59 (0.68)	26	4.71 (0.78)	30	4.57 (0.66)		–		–		–		–
Phang 2015		30.49 (4.27)		29.03 (4.28)		32.15 (3.77)		28.36 (4.47)		31.81 (4.3)		28.97 (4.38)		–		–
Pipe 2009		–		–		–		–		–		–		–		–
Rich 2021	62	69.66 (11.73)	63	71.71 (10.89)		70.16 (11.53)		71.37 (12.14)		–		–		–		–
Sampl 2017		4.21 (0.16)		4.36 (0.87)		4.66 (0.16)		4.16 (1.09)		–		–		–		–
Valley 2017		3.6 (NA)		3.67 (NA)		4.23 (NA)		4.1 (NA)		–		–		–		–
van Berkel 2014, van Dongen 2016	129	4.1 (0.8)	128	4 (0.9)		–		–	115	4 (0.9)	108	4 (0.9)	120	3.9 (0.9)	112	4 (0.9)

Table S 8. Summary statistics of studies included: Adaptive performance

	Baseline				Up to 4 weeks post-intervention				5–24 weeks post-intervention				More than 24 weeks post-intervention			
	MBP		Control		MBP		Control		MBP		Control		MBP		Control	
Study	n	M (SD)	n	M (SD)	n	M (SD)	n	M (SD)	n	M (SD)	n	M (SD)	n	M (SD)	n	M (SD)
Asuero 2014	43	119.7 (12.8)	25	120.8 (10.1)	43	123 (9.2)	25	119 (10.7)		–		–		–		–
Can Gür 2020		95.11 (13.94)		94.09 (15.54)		101.14 (15.19)		96.25 (16.38)		–		–		–		–
Chan 2021		18.5 (3.27)		16.22 (5.21)		–		–		17.14 (NA)		17.9 (NA)		–		–
Christopher 2018	31	81.48 (12.36)	30	76.1 (9.34)	24	83.66 (10.73)	26	77.07 (9.5)	24	83.2 (11.38)	25	77.48 (10.19)		–		–
Dvoráková 2017		3.8 (0.39)		3.78 (0.44)		3.8 (0.46)		3.76 (0.47)		–		–		–		–
Erogul 2014		78.1 (9.1)		76.3 (11)		80.5 (10)		77.1 (14.1)		82.4 (9.8)		77.3 (12.5)		–		–
Flook 2013		4.92 (0.57)		5.38 (0.49)		5.25 (0.76)		5.05 (0.7)		–		–		–		–
Gómez-Odriozola 2019		–		–		–		–		–		–		–		–
Jennings 2017		4.92 (0.8)		5 (0.7)		–		–		4.92 (0.76)		4.81 (0.74)		–		–
Klatt 2015		–		–		–		–		–		–		–		–
Kor 2019	18	18.72 (5.21)	18	17.78 (4.11)	18	18.94 (5.21)	18	19.06 (3.95)	18	19.66 (5.07)	18	17.69 (3.47)		–		–
Kor 2020	56	14.59 (4.32)	57	14.53 (4.46)	51	13.33 (5.45)	53	14.04 (5.46)	50	15.65 (2.81)	51	14.09 (4.99)		–		–
Lin 2019	44	54.43 (11.46)	46	55.17 (11.85)	44	57.98 (11.58)	46	55.11 (12.8)	44	59.7 (11.87)	46	53.85 (16.21)		–		–
Nadler 2020	37	3.29 (0.77)	65	3.63 (0.71)		3.78 (0.69)	65	3.41 (0.75)		–		–		–		–
Orosa-Duarte 2021		122.93 (NA)		123.57 (NA)		122.83 (NA)		122.43 (NA)		–		–		–		–

	Baseline				Up to 4 weeks post-intervention				5–24 weeks post-intervention				More than 24 weeks post-intervention			
	MBP		Control		MBP		Control		MBP		Control		MBP		Control	
Study	n	M (SD)	n	M (SD)	n	M (SD)	n	M (SD)	n	M (SD)	n	M (SD)	n	M (SD)	n	M (SD)
Perez-Blasco 2016	22	2.51 (0.5)	23	2.87 (0.51)	20	3.36 (0.51)	232.86 (0.5)			–		–		–		–
Sampl 2017		4.21 (0.16)		4.36 (0.87)		4.66 (0.16)		4.16 (1.09)		–		–		–		–
Schroeder 2018	16	26.31 (4.51)	17	27 (4.97)	15	27.66 (3.22)	14	26.07 (4.73)	13	27.84 (4.09)	13	25.07 (5.85)		–		–
Shapiro 1998		NA (27.6)		NA (22.7)		NA (24.4)		NA (21.6)		–		–		–		–
Shapiro 2011		2.78 (0.49)		2.75 (0.6)		2.82 (0.6)		2.8 (0.54)		2.82 (0.62)		2.79 (0.6)		2.85 (0.6)		2.78 (0.58)
Strauss 2021		83.78 (7.47)		84.22 (7.85)		83.97 (7.42)		83.24 (7.62)		–		–		–		–
Taylor 2016		3.77 (0.71)		3.65 (0.68)		3.89 (0.72)		3.73 (0.77)		–		–		–		–
vanDijk 2017		110.3 (10.3)		110.3 (9.3)		111.9 (9.7)		108.4 (10)		110.9 (11.5)		109.8 (8.6)		112 (11.6)		108.9 (11.2)

Table S 9. Summary statistics of studies included: Counterproductive work behaviour

Study	Baseline				Up to 4 weeks post-intervention				5–24 weeks post-intervention				More than 24 weeks post-intervention			
	MBP		Control		MBP		Control		MBP		Control		MBP		Control	
	n	M (SD)	n	M (SD)	n	M (SD)	n	M (SD)	n	M (SD)	n	M (SD)	n	M (SD)	n	M (SD)
Bartlett 2017	20	2.94 (5.18)	66	1.62 (3.52)	20	1.88 (2.8)	66	2.59 (3.69)		–		–		–		–
Hunsinger 2019	31	0.84 (0.09)	30	0.82 (0.05)	24	0.83 (0.07)	26	0.82 (0.11)	24	0.86 (0.07)	25	0.83 (0.07)		–		–
Roeser 2013		3.03 (3.67)		3.85 (4.67)		–		–		–		–		–		–
Steinberg 2017		1.06 (1.88)		2.56 (7.39)		0.78 (1.35)		0.69 (1.08)		–		–		–		–
Strauss 2021	77	5.31 (6.04)	91	4.53 (6.21)	77	4.36 (5.87)	91	4.46 (5.48)		–		–		–		–

Main outcome

Table S 10. Summary of the main outcome meta-analysis: task performance measured up to 4 weeks after intervention

```
## Review:   Mindfulness interventions for task performance
##
##           G      95%-CI %W (random)
## Allexandre 2016 -0.0438 [-0.3958; 0.3083]    23.2
## Braun 2020b   1.3193 [ 0.3905; 2.2481]    12.3
## Flook 2013    0.3085 [-0.9401; 1.5571]    8.5
## Pang 2019     0.0429 [-0.5621; 0.6478]    18.0
## Sampl 2017    0.7384 [ 0.2060; 1.2708]    19.4
## Steinberg 2017  0.7072 [-0.3065; 1.7209]    11.1
## Valley 2017   1.5038 [ 0.1366; 2.8711]    7.5
##
## Number of studies combined: k = 7
##
##             G      95%-CI  t p-value
## Random effects model 0.5210 [-0.0281; 1.0700] 2.32 0.0593
## Prediction interval     [-0.7263; 1.7682]
##
## Quantifying heterogeneity:
## tau^2 = 0.1851 [0.0039; 1.4998]; tau = 0.4302 [0.0628; 1.2247]
## I^2 = 60.4% [9.3%; 82.7%]; H = 1.59 [1.05; 2.41]
##
## Test of heterogeneity:
##   Q   d.f. p-value
## 15.17  6   0.0190
##
## Details on meta-analytical method:
## - Inverse variance method
## - Restricted maximum-likelihood estimator for tau^2
## - Q-profile method for confidence interval of tau^2 and tau
## - Hartung-Knapp adjustment for random effects model
```

Subgroup analyses

Length of intervention

Table S 11. Subgroup analysis by length of intervention: task performance measured up to 4 weeks after intervention

```
## Mixed-Effects Model (k = 7; tau^2 estimator: REML)
##
##  logLik deviance    AIC    BIC   AICc
## -4.2422  8.4843  14.4843  13.3126  38.4843
##
## tau^2 (estimated amount of residual heterogeneity):   0.1062 (SE = 0.1670)
## tau (square root of estimated tau^2 value):       0.3258
## I^2 (residual heterogeneity / unaccounted variability): 40.81%
## H^2 (unaccounted variability / sampling variability):  1.69
## R^2 (amount of heterogeneity accounted for):      42.63%
##
```

```
## Test for Residual Heterogeneity:
## QE (df = 5) = 8.5033, p-val = 0.1306
##
## Test of Moderators (coefficient 2):
## F (df1 = 1, df2 = 5) = 1.9045, p-val = 0.2261
##
## Model Results:
##
##                  estimate   se   tval df  pval
## intrcpt              -0.1546 0.5015 -0.3083  5 0.7703
## duration_hrs_taught_intervention  0.0419 0.0303  1.3800  5 0.2261
##                   ci.lb  ci.ub
## intrcpt              -1.4437 1.1345
## duration_hrs_taught_intervention -0.0361 0.1199
##
## ---
## Signif. codes: 0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
```

Setting

Table S 12. Subgroup analysis by setting: task performance measured up to 4 weeks after intervention

```
## Review:   Mindfulness interventions for task performance
##
##           G      95%-CI %W (random) Sample.context
## Allexandre 2016 -0.0438 [-0.3958; 0.3083]    23.2  Occupational
## Braun 2020b   1.3193 [ 0.3905; 2.2481]    12.3  Educational
## Flook 2013    0.3085 [-0.9401; 1.5571]    8.5  Occupational
## Pang 2019    0.0429 [-0.5621; 0.6478]    18.0  Occupational
## Sampl 2017    0.7384 [ 0.2060; 1.2708]    19.4  Educational
## Steinberg 2017  0.7072 [-0.3065; 1.7209]    11.1  Occupational
## Valley 2017   1.5038 [ 0.1366; 2.8711]    7.5  Occupational
##
## Number of studies combined: k = 7
##
##             G      95%-CI  t p-value
## Random effects model 0.5210 [-0.0281; 1.0700] 2.32 0.0593
## Prediction interval     [-0.7263; 1.7682]
##
## Quantifying heterogeneity:
## tau^2 = 0.1851 [0.0039; 1.4998]; tau = 0.4302 [0.0628; 1.2247]
## I^2 = 60.4% [9.3%; 82.7%]; H = 1.59 [1.05; 2.41]
##
## Test of heterogeneity:
##   Q d.f. p-value
## 15.17  6 0.0190
##
## Results for subgroups (random effects model):
##                 k  G      95%-CI tau^2  tau  Q
## Sample.context = Occupational  5 0.2261 [-0.3897; 0.8419] 0.0642 0.2535 6.20
## Sample.context = Educational  2 0.8991 [-2.4030; 4.2011] 0.0196 0.1399 1.13
##                 I^2
## Sample.context = Occupational 35.5%
## Sample.context = Educational 11.6%
##
## Test for subgroup differences (random effects model):
##           Q d.f. p-value
```

```
## Between groups  3.88  1 0.0489
##
## Details on meta-analytical method:
## - Inverse variance method
## - Restricted maximum-likelihood estimator for tau^2
## - Q-profile method for confidence interval of tau^2 and tau
## - Hartung-Knapp adjustment for random effects model
```

Reporter type

One study (Allexandre et al., 2016) did not clearly report who provided the task performance ratings. This study was excluded from the analysis.

Table S 13. Subgroup analysis by reporter type: task performance measured up to 4 weeks after intervention

```
## Review:   Mindfulness interventions for task performance. Subgroup analysi ...
##
##          G      95%-CI %W (random) report.type_recoded
## Braun 2020b  1.3193 [ 0.3905; 2.2481]    15.3    Self-reported
## Flook 2013   0.3085 [-0.9401; 1.5571]    9.9  Not self-reported
## Pang 2019   0.0429 [-0.5621; 0.6478]    24.9  Not self-reported
## Sampl 2017   0.7384 [ 0.2060; 1.2708]    27.8  Not self-reported
## Steinberg 2017 0.7072 [-0.3065; 1.7209]    13.5    Self-reported
## Valley 2017  1.5038 [ 0.1366; 2.8711]    8.6    Self-reported
##
## Number of studies combined: k = 6
##
##             G      95%-CI  t p-value
## Random effects model 0.6729 [ 0.0911; 1.2547] 2.97 0.0311
## Prediction interval     [-0.4440; 1.7898]
##
## Quantifying heterogeneity:
## tau^2 = 0.1106 [0.0000; 1.6269]; tau = 0.3326 [0.0000; 1.2755]
## I^2 = 35.4% [0.0%; 74.2%]; H = 1.24 [1.00; 1.97]
##
## Test of heterogeneity:
##   Q d.f. p-value
## 7.74  5 0.1713
##
## Results for subgroups (random effects model):
##                       k   g      95%-CI tau^2
## report.type_recoded = Self-reported    3 1.1330 [-0.2111; 2.4771]   0
## report.type_recoded = Not self-reported  3 0.3973 [-0.7576; 1.5523] 0.0850
##                       tau  Q  I^2
## report.type_recoded = Self-reported     0 1.12 0.0%
## report.type_recoded = Not self-reported 0.2916 2.90 31.0%
##
## Test for subgroup differences (random effects model):
##           Q d.f. p-value
## Between groups  3.19  1 0.0741
##
## Details on meta-analytical method:
## - Inverse variance method
## - Restricted maximum-likelihood estimator for tau^2
```

```
## - Q-profile method for confidence interval of tau^2 and tau
## - Hartung-Knapp adjustment for random effects model
```

Secondary outcome analyses

Task performance

No studies used active control groups. Although three studies reported to have collected outcomes after 6 months after the end of the intervention (Allexandre et al., 2016; Glass et al., 2019; Valley & Stallones, 2017) reported results. Meta-analysis was therefore not carried out for this time period. Valley and Stallones (2017) only collected data for participants who were either randomised to the intervention group or those allocated to the waitlist control group once they had completed the intervention. Glass and colleagues (2019) did not report outcomes of interest for those allocated to the control group.

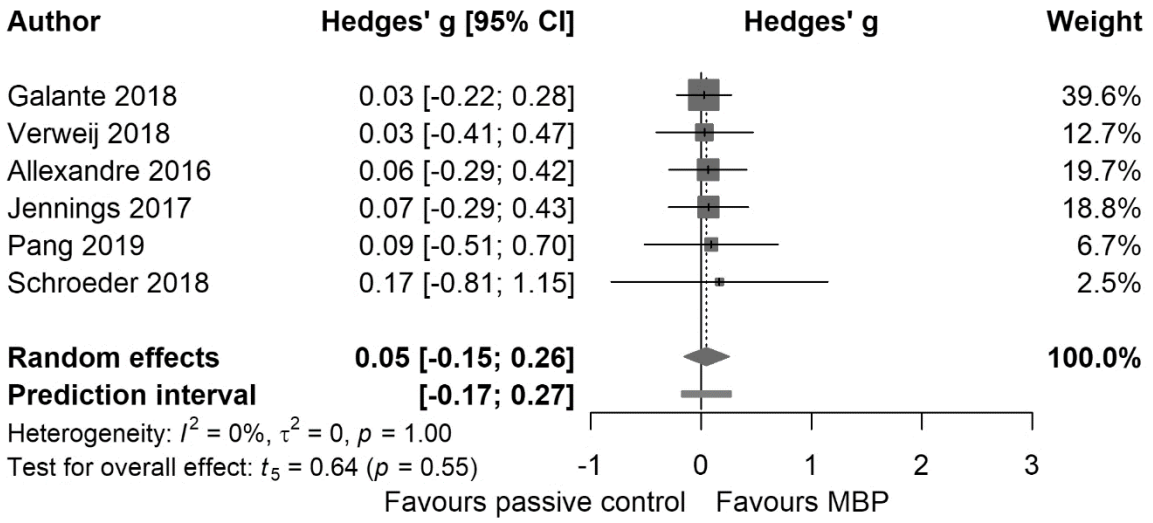


Figure S 4. Task performance 5-24 weeks post-intervention. Only passive control groups.

Contextual performance

2 studies or fewer reported using active control groups.

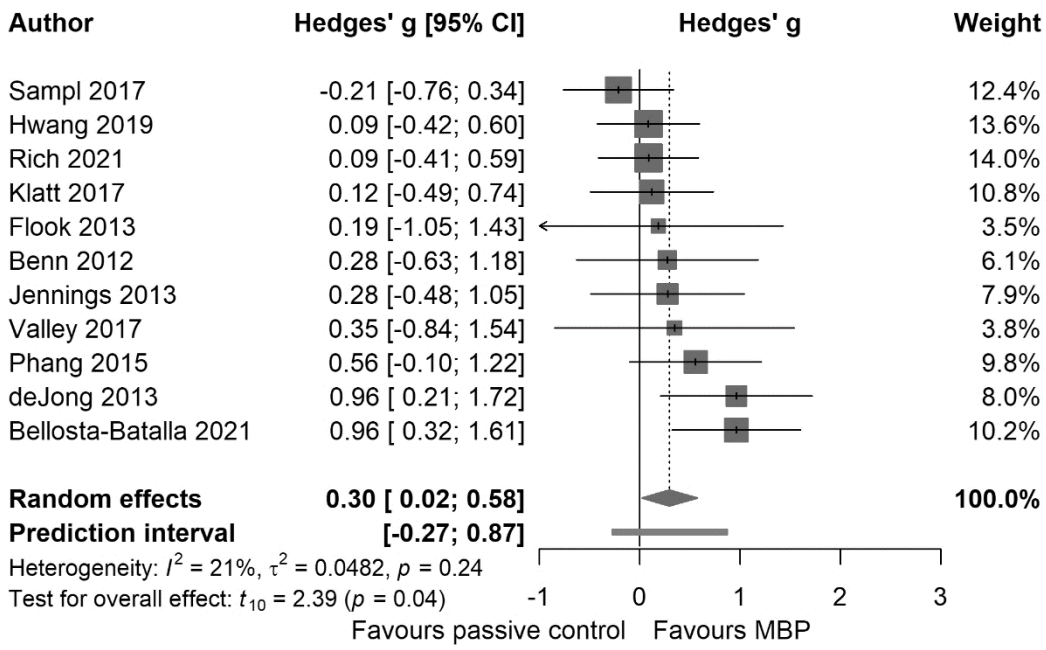


Figure S 5. Contextual performance up to 4 weeks post-intervention. Only passive control groups

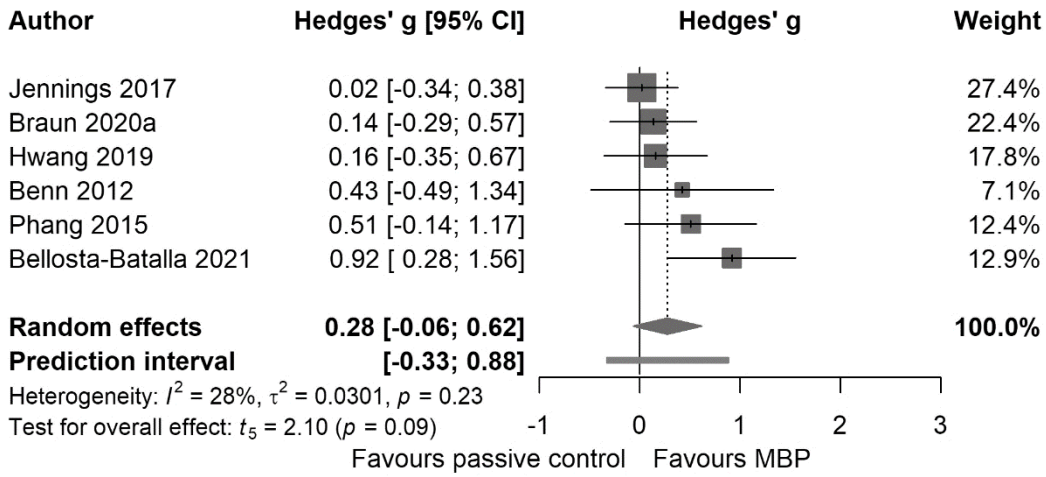


Figure S 6. Contextual performance 5-24 weeks post-intervention. Only passive control groups

Adaptive performance

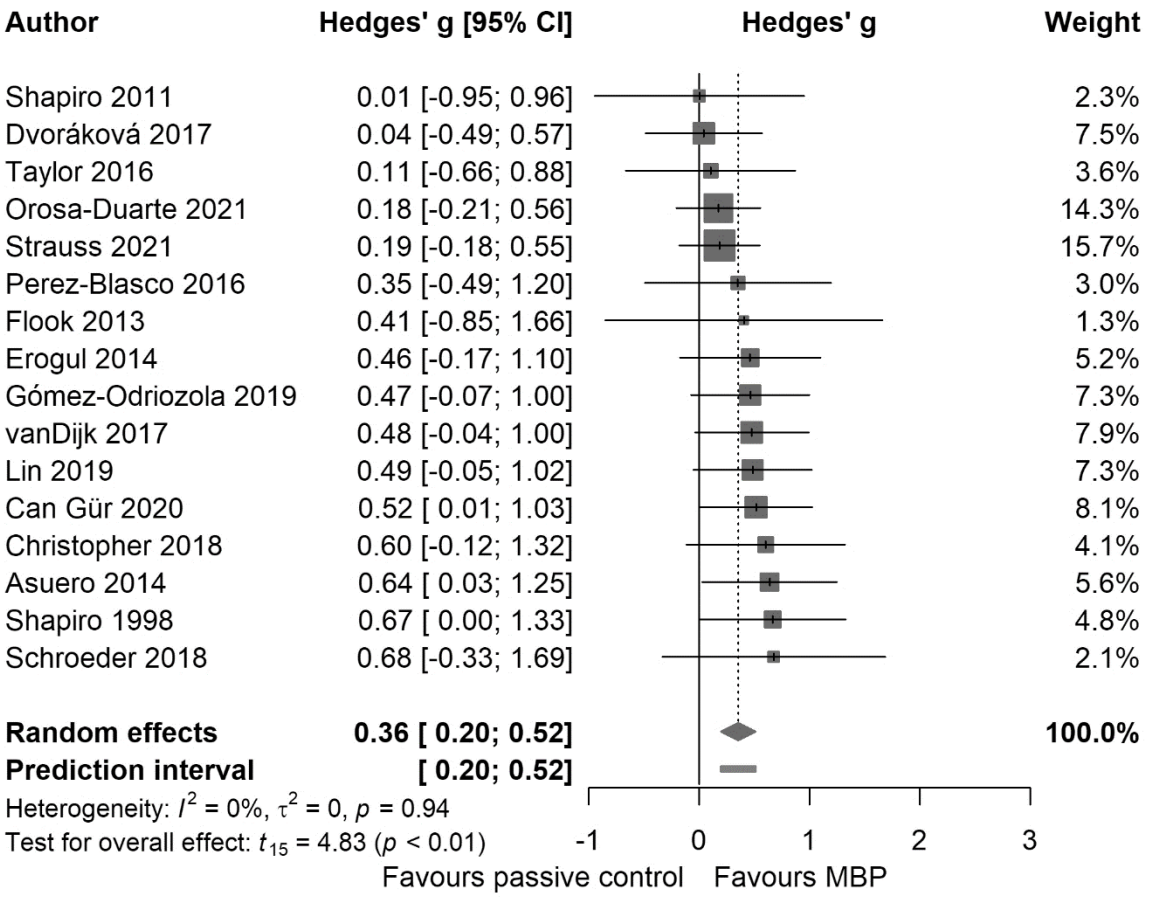


Figure S 7. Adaptive performance up to 4 weeks post-intervention. Only passive control groups

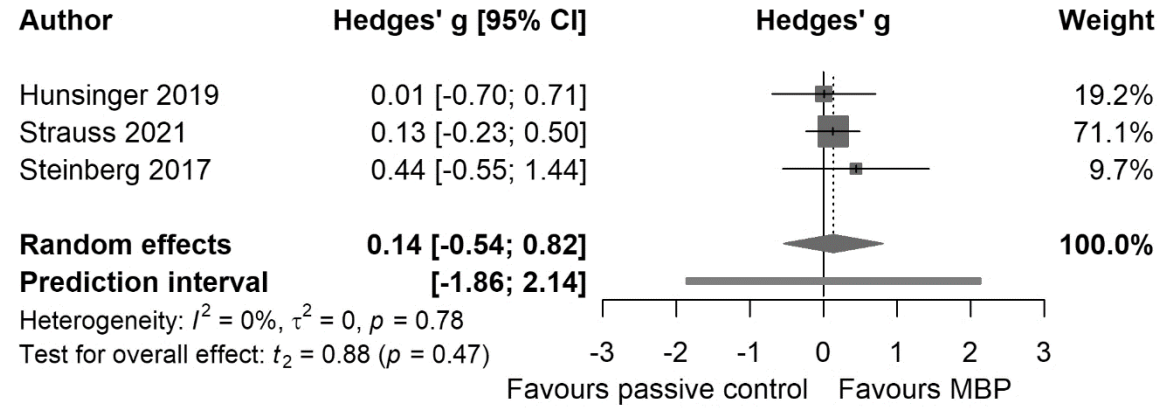


Figure S 8. Adaptive performance up to 5-24 weeks post-intervention. Only passive control groups

Counterproductive work behaviour

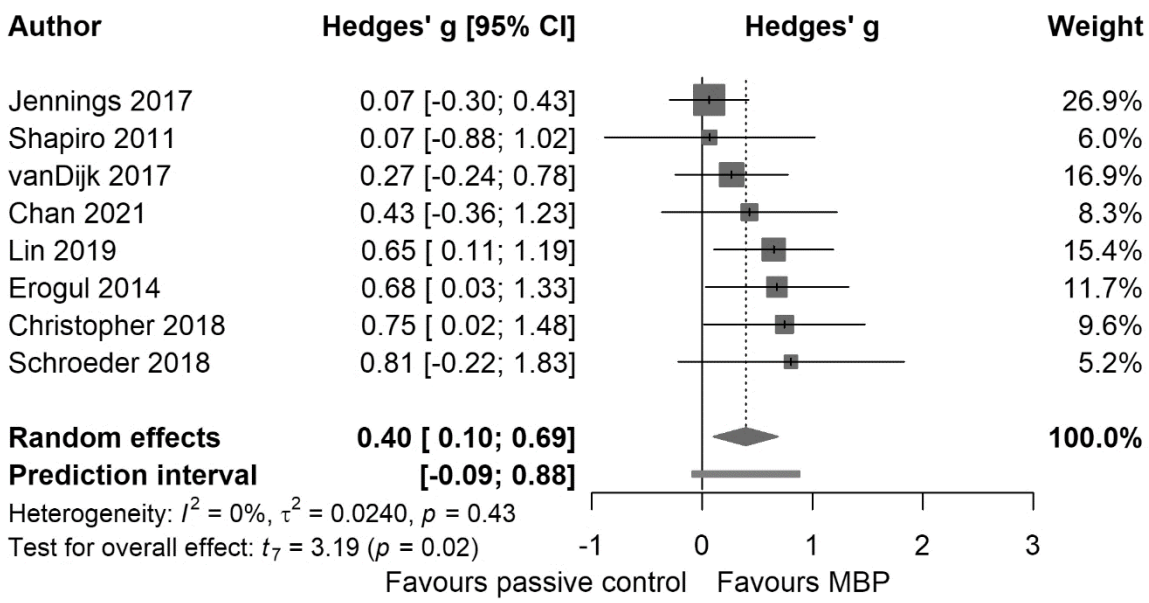


Figure S 9. Counterproductive work behaviour up to 4 weeks post-intervention. Only passive control groups

Supplementary Materials for Chapter 3. Protocol for the Work Engagement and Well-being Study (SWELL): A randomised controlled feasibility trial evaluating the effects of mindfulness versus light physical exercise at work

SPIRIT 2013 Checklist: Recommended items to address in a clinical trial protocol and related documents*

Section/item	ItemNo	Description	Where to find
Administrative information			
Title	1	Descriptive title identifying the study design, population, interventions, and, if applicable, trial acronym	Title page
Trial registration	2a	Trial identifier and registry name. If not yet registered, name of intended registry	Abstract
	2b	All items from the World Health Organization Trial Registration Data Set	Outlined in trial registration

Protocol version	3	Date and version identifier	Abstract
Funding	4	Sources and types of financial, material, and other support	Funding statement, p 15
Roles and responsibilities	5a	Names, affiliations, and roles of protocol contributors	Title page, Contributors, pp 15–16
	5b	Name and contact information for the trial sponsor	Contributors, trial registration
	5c	Role of study sponsor and funders, if any, in study design; collection, management, analysis, and interpretation of data; writing of the report; and the decision to submit the report for publication, including whether they will have ultimate authority over any of these activities	Contributors, p 15
	5d	Composition, roles, and responsibilities of the coordinating centre, steering committee, endpoint adjudication committee, data management team, and other individuals or groups overseeing the trial, if applicable (see Item 21a for data monitoring committee)	pp 13–14
Introduction			
Background and rationale	6a	Description of research question and justification for undertaking the trial, including summary of relevant studies (published and unpublished) examining benefits and harms for each intervention	pp 5–6
	6b	Explanation for choice of comparators	p 7
Objectives	7	Specific objectives or hypotheses	p 6

Trial design	8	Description of trial design including type of trial (eg, parallel group, crossover, factorial, single group), allocation ratio, and framework (eg, superiority, equivalence, noninferiority, exploratory)	p 6

Methods: Participants, interventions, and outcomes

Study setting	9	Description of study settings (eg, community clinic, academic hospital) and list of countries where data will be collected. Reference to where list of study sites can be obtained	p 6
Eligibility criteria	10	Inclusion and exclusion criteria for participants. If applicable, eligibility criteria for study centres and individuals who will perform the interventions (eg, surgeons, psychotherapists)	p 6
Interventions	11a	Interventions for each group with sufficient detail to allow replication, including how and when they will be administered	p 7
	11b	Criteria for discontinuing or modifying allocated interventions for a given trial participant (eg, drug dose change in response to harms, participant request, or improving/worsening disease)	p 7 and p 13
	11c	Strategies to improve adherence to intervention protocols, and any procedures for monitoring adherence (eg, drug tablet return, laboratory tests)	p 7
	11d	Relevant concomitant care and interventions that are permitted or prohibited during the trial	p 6
Outcomes	12	Primary, secondary, and other outcomes, including the specific measurement variable (eg, systolic blood pressure), analysis metric (eg, change from baseline, final value, time to event), method of aggregation (eg, median, proportion), and time point for each outcome. Explanation of the clinical relevance of chosen efficacy and harm outcomes is strongly recommended	pp 8–11
Participant timeline	13	Time schedule of enrolment, interventions (including any run-ins and washouts), assessments, and visits for participants. A schematic diagram is highly recommended (see Figure)	p 8

Sample size	14	Estimated number of participants needed to achieve study objectives and how it was determined, including clinical and statistical assumptions supporting any sample size calculations	Sample size, p 11
Recruitment	15	Strategies for achieving adequate participant enrolment to reach target sample size	Recruitment, p 12
Methods: Assignment of interventions (for controlled trials)			
Allocation:			
Sequence generation	16a	Method of generating the allocation sequence (eg, computer-generated random numbers), and list of any factors for stratification. To reduce predictability of a random sequence, details of any planned restriction (eg, blocking) should be provided in a separate document that is unavailable to those who enrol participants or assign interventions	Randomisation, p12 and GitHub
Allocation concealment mechanism	16b	Mechanism of implementing the allocation sequence (eg, central telephone; sequentially numbered, opaque, sealed envelopes), describing any steps to conceal the sequence until interventions are assigned	Randomisation, p12
Implementation	16c	Who will generate the allocation sequence, who will enrol participants, and who will assign participants to interventions	Randomisation, p12
Blinding (masking)	17a	Who will be blinded after assignment to interventions (eg, trial participants, care providers, outcome assessors, data analysts), and how	Randomisation, p12
	17b	If blinded, circumstances under which unblinding is permissible, and procedure for revealing a participant's allocated intervention during the trial	Randomisation, p12
Methods: Data collection, management, and analysis			

Data collection methods	18a	Plans for assessment and collection of outcome, baseline, and other trial data, including any related processes to promote data quality (eg, duplicate measurements, training of assessors) and a description of study instruments (eg, questionnaires, laboratory tests) along with their reliability and validity, if known. Reference to where data collection forms can be found, if not in the protocol	p 8, Data management, p 14
	18b	Plans to promote participant retention and complete follow-up, including list of any outcome data to be collected for participants who discontinue or deviate from intervention protocols	p 12
Data management	19	Plans for data entry, coding, security, and storage, including any related processes to promote data quality (eg, double data entry; range checks for data values). Reference to where details of data management procedures can be found, if not in the protocol	Data management, p 14
Statistical methods	20a	Statistical methods for analysing primary and secondary outcomes. Reference to where other details of the statistical analysis plan can be found, if not in the protocol	Statistical methods, p 13
	20b	Methods for any additional analyses (eg, subgroup and adjusted analyses)	Statistical methods, p 13
	20c	Definition of analysis population relating to protocol non-adherence (eg, as randomised analysis), and any statistical methods to handle missing data (eg, multiple imputation)	Statistical methods, p 13
Methods: Monitoring			
Data monitoring	21a	Composition of data monitoring committee (DMC); summary of its role and reporting structure; statement of whether it is independent from the sponsor and competing interests; and reference to where further details about its charter can be found, if not in the protocol. Alternatively, an explanation of why a DMC is not needed	p 13
	21b	Description of any interim analyses and stopping guidelines, including who will have access to these interim results and make the final decision to terminate the trial	p 13–14
Harms	22	Plans for collecting, assessing, reporting, and managing solicited and spontaneously reported adverse events and other unintended effects of trial interventions or trial conduct	p 13–14

Auditing	23	Frequency and procedures for auditing trial conduct, if any, and whether the process will be independent from investigators and the sponsor	p 13–14
Ethics and dissemination			
Research ethics approval	24	Plans for seeking research ethics committee/institutional review board (REC/IRB) approval	p 14
Protocol amendments	25	Plans for communicating important protocol modifications (eg, changes to eligibility criteria, outcomes, analyses) to relevant parties (eg, investigators, REC/IRBs, trial participants, trial registries, journals, regulators)	p 14
Consent or assent	26a	Who will obtain informed consent or assent from potential trial participants or authorised surrogates, and how (see Item 32)	p 14
	26b	Additional consent provisions for collection and use of participant data and biological specimens in ancillary studies, if applicable	NA
Confidentiality	27	How personal information about potential and enrolled participants will be collected, shared, and maintained in order to protect confidentiality before, during, and after the trial	p 14
Declaration of interests	28	Financial and other competing interests for principal investigators for the overall trial and each study site	p 15
Access to data	29	Statement of who will have access to the final trial dataset, and disclosure of contractual agreements that limit such access for investigators	p 14
Ancillary and post-trial care	30	Provisions, if any, for ancillary and post-trial care, and for compensation to those who suffer harm from trial participation	p 13–14

Dissemination policy	31a	Plans for investigators and sponsor to communicate trial results to participants, healthcare professionals, the public, and other relevant groups (eg, via publication, reporting in results databases, or other data sharing arrangements), including any publication restrictions	p 14
	31b	Authorship eligibility guidelines and any intended use of professional writers	p 14
	31c	Plans, if any, for granting public access to the full protocol, participant-level dataset, and statistical code	p 14
Appendices			
Informed consent materials	32	Model consent form and other related documentation given to participants and authorised surrogates	See supplementary file 2
Biological specimens	33	Plans for collection, laboratory evaluation, and storage of biological specimens for genetic or molecular analysis in the current trial and for future use in ancillary studies, if applicable	NA

*It is strongly recommended that this checklist be read in conjunction with the SPIRIT 2013 Explanation & Elaboration for important clarification on the items. Amendments to the protocol should be tracked and dated. The SPIRIT checklist is copyrighted by the SPIRIT Group under the Creative Commons "Attribution-NonCommercial-NoDerivs 3.0 Unported" license.

Consent form

Start of Block: Consent form

Welcome!

This study aims to find out whether mindfulness and light exercise help to improve employees' wellbeing and work engagement. This study has been reviewed by the Cambridge Psychology Research Ethics Committee [PRE.2020.XXX]

Please tick each line to tell us that you understand what participating in this study means.

- I confirm that I have read and understood the volunteer information on the previous page. Click the “previous” button to review this information.
- I understand I can ask questions by contacting the Principle Investigator, Maris Vainre, via e-mail <e-mail link> or phone.
- I understand that my participation is voluntary. I am free to withdraw at any time without giving a reason, without penalty or affecting my rights.
- I understand that the research data may be accessed by researchers working at or collaborating with the MRC Cognition and Brain Sciences Unit in similar ethically approved studies. At all times, my personal data will be kept confidential in accordance with data protection guidelines.
- I understand that on publication, research data may be shared with others through a protected database after removing information that might identify me
- I work at [employee name])
- I am able to contribute between 3 to 4 hours a week in the next 6 weeks
- I understand that I will be contacted again 3 months after the course finishes to follow-up on how things have been going for me

Display This Question:

If If Welcome! This study aims to find out whether mindfulness and light exercise help to improve emplo... q://QID2/SelectedChoicesCount Is Equal to 8

We recommend **not to take part** in this study if:

You are currently suffering from severe periods of anxiety, depression or

hypomania;

You are experiencing other severe mental illnesses;

You have had a recent bereavement or major loss;

You have already completed a mindfulness course or have meditated more than 10 hours in your life.

Do you agree to take part in the study?

- Yes , I have checked all the boxes myself and I agree to take part in the study (1)
- No (2)

Display This Question:

If Do you agree to take part in the study?= No

Thank you for your time!

You said you **do not agree** to take part in the study. If you don't want to join the study you don't have to do anything further, simply close this browser window.

Have a good day!

If this was a mis-click, just go back and have another go.

End of Block: Consent form

Start of Block: Contact details

Display This Block:

If Do you agree to take part in the study? = Yes

Thank you for agreeing to take part!

To get you started, we'd like to know a bit about you. We need this to understand a bit more about you, to keep in contact with you throughout the course and to keep track of your wellbeing and work engagement.

Let's get aquainted:

- How would you like us to call you? Give us your first name or nick name (1) _____
- How old are you (in years)? (2) _____
- What's your work e-mail address? We'll use it to verify that you indeed work at [employer] (3) _____
- What's the e-mail address you'd like us to use when contacting you about the trial? (4) _____

What is your gender identity?

- Female (1)
- Male (2)
- Other (please specify): (3) _____
- Prefer not to answer (4)

Do you have any conditions that may affect your ability to focus or exercise?

- Yes (1)
- No (2)
- Prefer not to answer (3)

Display This Question:

If Do you have any conditions that may affect your ability to focus or exercise? = Yes

You said you have a condition that may affect your ability to focus or exercise.

Please describe your condition so we understand it a bit more:

_____

_____

End of Block: Contact details

Supplementary Materials for Chapter 4. The Work Engagement and Well-being Study (SWELL): A randomised controlled feasibility trial evaluating the effects of mindfulness versus light physical exercise at work

Supplementary Material 1: CONSORT checklist

CONSORT 2010 checklist of information to include when reporting a randomised trial*

Section/Topic	Item No	Checklist item	Reported on page No
Title and abstract			
	1a	Identification as a randomised trial in the title	1
	1b	Structured summary of trial design, methods, results, and conclusions (for specific guidance see CONSORT for abstracts)	1-2
Introduction			
	2a	Scientific background and explanation of rationale	3-5

Background and objectives	2b	Specific objectives or hypotheses	6

Methods

Trial design	3a	Description of trial design (such as parallel, factorial) including allocation ratio	6
	3b	Important changes to methods after trial commencement (such as eligibility criteria), with reasons	NA
Participants	4a	Eligibility criteria for participants	6–7
	4b	Settings and locations where the data were collected	6
Interventions	5	The interventions for each group with sufficient details to allow replication, including how and when they were actually administered	7
Outcomes	6a	Completely defined pre-specified primary and secondary outcome measures, including how and when they were assessed	8–8
	6b	Any changes to trial outcomes after the trial commenced, with reasons	NA
Sample size	7a	How sample size was determined	7
	7b	When applicable, explanation of any interim analyses and stopping guidelines	NA

Randomisation:

Sequence generation	8a	Method used to generate the random allocation sequence	10
	8b	Type of randomisation; details of any restriction (such as blocking and block size)	10
Allocation concealment mechanism	9	Mechanism used to implement the random allocation sequence (such as sequentially numbered containers), describing any steps taken to conceal the sequence until interventions were assigned	10
Implementation	10	Who generated the random allocation sequence, who enrolled participants, and who assigned participants to interventions	10

Blinding	11a	If done, who was blinded after assignment to interventions (for example, participants, care providers, those assessing outcomes) and how	10
	11b	If relevant, description of the similarity of interventions	Supp Mat 2
Statistical methods	12a	Statistical methods used to compare groups for primary and secondary outcomes	10–11
	12b	Methods for additional analyses, such as subgroup analyses and adjusted analyses	10–11

Results

Participant flow (a diagram is strongly recommended)	13a	For each group, the numbers of participants who were randomly assigned, received intended treatment, and were analysed for the primary outcome	12, Supp Mat 3
	13b	For each group, losses and exclusions after randomisation, together with reasons	12
Recruitment	14a	Dates defining the periods of recruitment and follow-up	6, 8
	14b	Why the trial ended or was stopped	NA
Baseline data	15	A table showing baseline demographic and clinical characteristics for each group	11
Numbers analysed	16	For each group, number of participants (denominator) included in each analysis and whether the analysis was by original assigned groups	10–12, Supp Mat 3
Outcomes and estimation	17a	For each primary and secondary outcome, results for each group, and the estimated effect size and its precision (such as 95% confidence interval)	Supp Mat 3
	17b	For binary outcomes, presentation of both absolute and relative effect sizes is recommended	Supp Mat 3
Ancillary analyses	18	Results of any other analyses performed, including subgroup analyses and adjusted analyses, distinguishing pre-specified from exploratory	NA
Harms	19	All important harms or unintended effects in each group (for specific guidance see CONSORT for harms)	12

Discussion

Limitations	20	Trial limitations, addressing sources of potential bias, imprecision, and, if relevant, multiplicity of analyses	15
Generalisability	21	Generalisability (external validity, applicability) of the trial findings	14–16

Interpretation	22	Interpretation consistent with results, balancing benefits and harms, and considering other relevant evidence	14–16
Other information			
Registration	23	Registration number and name of trial registry	6
Protocol	24	Where the full trial protocol can be accessed, if available	6
Funding	25	Sources of funding and other support (such as supply of drugs), role of funders	17

Citation: Schulz KF, Altman DG, Moher D, for the CONSORT Group. CONSORT 2010 Statement: updated guidelines for reporting parallel group randomised trials. BMC Medicine. 2010;8:18.

*We strongly recommend reading this statement in conjunction with the CONSORT 2010 Explanation and Elaboration for important clarifications on all the items. If relevant, we also recommend reading CONSORT extensions for cluster randomised trials, non-inferiority and equivalence trials, non-pharmacological treatments, herbal interventions, and pragmatic trials. Additional extensions are forthcoming: for those and for up-to-date references relevant to this checklist, see www.consort-statement.org.

Supplementary Material 2: Methods

Outcomes

Primary outcome: Work performance

We made a number of modifications to the original questionnaire. First, we reframed the questionnaire instructions so as not to instruct people to think of their problems, by omitting the phrase “how much of the work time did your physical health or emotional problems make it difficult for you to do the following?”. The modified instructions thus read: “These questions ask you to rate the amount of time during the past four weeks that you had a difficulty handling certain parts of your job. It concerns the hours you worked in the past four weeks. Mark the "does not apply to my job" box only if the question describes something that is not part of your job.”. We hoped rephrasing would reduce the risk of ceiling effects. Second, we were concerned that there may be variances in the way people interpret the items and the rating scale. Particularly we found that the framing around difficulties slipped people’s mind, so they tended to express a sense of accomplishment and thus tick „most of the time“ when things went well. We thus changed the wording on the statements a) to reinforce the fact we are asking about difficulties, b) remind them about time frame and c) changed the wording from the third person (you) to the first (I). For example, the item „...feel a sense of accomplishment in your work“ became „In the past 4 weeks, I had difficulties feeling a sense of accomplishment in my work“. We also changed item 2 to reflect the fact many people were still working from home due to the COVID-19 pandemic: “In the past 4 weeks, I had difficulties starting on my job as soon as I arrived at work (or started workday if working from home)”.

Modified Work Role Functioning Questionnaire

These questions ask you to rate the amount of time during the past four weeks that you had a difficulty handling certain parts of your job. It concerns the hours you worked in the past four weeks. Mark the "does not apply to my job" box only if the question describes something that is not part of your job.

#		Never	Some-times	About half of the time	Most of the time	All of the time	Does not apply for my job	Prefer not to answer
1	In the past 4 weeks, I had difficulties getting going easily at the beginning of the workday	○	○	○	○	○	○	○
2	In the past 4 weeks, I had difficulties starting on my job as soon as I arrived at work (or started workday if working from home)	○	○	○	○	○	○	○
3	In the past 4 weeks, I had difficulties doing my work without stopping to take extra breaks or rests	○	○	○	○	○	○	○
4	In the past 4 weeks, I had difficulties sticking to a routine or schedule	○	○	○	○	○	○	○
5	In the past 4 weeks, I had difficulties working fast enough	○	○	○	○	○	○	○
6	In the past 4 weeks, I had difficulties finishing work on time	○	○	○	○	○	○	○
7	In the past 4 weeks, I had difficulties doing my work without making mistakes	○	○	○	○	○	○	○
8	In the past 4 weeks, I had difficulties satisfying the people who judge my work	○	○	○	○	○	○	○
9	In the past 4 weeks, I had difficulties feeling a sense of accomplishment in my work	○	○	○	○	○	○	○
10	In the past 4 weeks, I had difficulties feeling I have done what I am capable of doing	○	○	○	○	○	○	○
11	In the past 4 weeks, I had difficulties lifting, carrying, or moving objects at work weighing more than 10lbs/4.5 kgs	○	○	○	○	○	○	○

12	In the past 4 weeks, I had difficulties sitting, standing, or staying in one position for longer than 15 minutes while working	○	○	○	○	○	○	○
13	In the past 4 weeks, I had difficulties repeating the same motions over and over again while working	○	○	○	○	○	○	○
14	In the past 4 weeks, I had difficulties bending, twisting, or reaching while working	○	○	○	○	○	○	○
15	In the past 4 weeks, I had difficulties using hand-held tools or equipment (for example, a phone, pen, keyboard, computer mouse)	○	○	○	○	○	○	○
16	In the past 4 weeks, I had difficulties keeping my mind on my work	○	○	○	○	○	○	○
17	In the past 4 weeks, I had difficulties doing work carefully	○	○	○	○	○	○	○
18	In the past 4 weeks, I had difficulties concentrating on my work	○	○	○	○	○	○	○
19	In the past 4 weeks, I had difficulties working without losing my train of thought	○	○	○	○	○	○	○
20	In the past 4 weeks, I had difficulties easily reading or using my eyes when working	○	○	○	○	○	○	○
21	In the past 4 weeks, I had difficulties speaking with people in-person, in meetings or on the phone/videocall	○	○	○	○	○	○	○
22	In the past 4 weeks, I had difficulties controlling my temper around people when working	○	○	○	○	○	○	○
23	In the past 4 weeks, I had difficulties setting priorities in my work	○	○	○	○	○	○	○
24	In the past 4 weeks, I had difficulties handling changes in my work	○	○	○	○	○	○	○
25	In the past 4 weeks, I had difficulties processing incoming information, for example e-mails, in time	○	○	○	○	○	○	○
26	In the past 4 weeks, I had difficulties performing multiple tasks and the same time	○	○	○	○	○	○	○

27	In the past 4 weeks, I had difficulties being proactive, show initiative in my work	○	○	○	○	○	○	○

Selecting IAPS images

To select images to use in the affective Learning Task from the International affective picture system (IAPS (Bradley & Lang, 2017)), we first chose images that depicted humans as the main subject but excluded images that displayed nudity or were of sexual nature. We then used the affective ratings published as part of the manual (Lang et al., 2008) to calculate z-score for valence for each image. We divided the z-scores to deciles and assigned deciles 2 and 3 to the negative condition and deciles 5 and 6 to the neutral condition. We discarded images from the remaining deciles.

Intervention condition

The course draws from Mindfulness-Based Stress Reduction (Kabat-Zinn, 2013) and Mindfulness-Based Cognitive Therapy (Segal et al., 2013). The course consisted of 2 weekly sessions, first of it longer, the second a shorter top-up. Daily homework included a formal mediation practice with the assistance of video/audio recordings (up to 30 minutes), and one or two informal exercises per day (see). Twice a week, participants receive generic e-mails motivating them to practice and informing them when the next module is available.

Supplementary Table 1. Overview of the Be Mindful content. Table reused from Vainre et al (2023) under a CC-BY-4.0 license.

Week/session	Content	Homework
Getting started	Registration; introduction to course; completion of Stress, Anxiety, and Depression assessment	None
Week 1 – Stepping out of automatic pilot		
Session 1	Body scan; being mindful doing routine activities; mindful eating	Practice body scan
Session 2	Dealing with barriers	

Week/session	Content	Homework
Week 2 – Reconnecting with body and breath		
Session 1	Mindful breathing	Practice mindful breathing; keeping an Event Awareness Journal; practice moving mindfully
Session 2	Physical barometer	
Week 3 – Working with difficulties		
Session 1	Breathing space; sitting meditation	Practice breathing space and sitting meditation
Session 2	Thoughts are just thoughts	
Week 4 – Mindfulness in daily life		
Session 1	Preparing for stress; reflection on stress strategies	Practice activity awareness, breathing space, and action step; stress strategies
Session 2	Mindful walking	
Going forward		
Session 1	Additional resources; completion of Stress, Anxiety, and Depression assessment; completion certificate	None

Supplementary Material 3: Findings

Preferences and contamination

Supplementary Table 2. Preferences and contamination. Table reused from Vainre et al (2023) under a CC-BY-4.0 license.

Outcome		Timepoint	Item	Mindfulness		Light exercise	
				n	%/m (sd)	n	%/m (sd)
Engaged in other arm's practice		Post-intervention	Not at all	15	(12.3%)	34	(28.57%)
			A few times	33	(27.05%)	35	(29.41%)
			Often	27	(22.13%)	8	(6.72%)
			Missing	47	(38.52%)	42	(35.29%)
Experiences with meditation	Practiced mindfulness formally	Post-intervention	Not at all	7	(5.74%)	22	(18.49%)
			Less than half an hour a week	17	(13.93%)	11	(9.24%)
			Between 0.5 and 1 hour a week	23	(18.85%)	4	(3.36%)
			Between 1 and 3 hours a week	27	(22.13%)	5	(4.2%)
			More than 3 hours a week	1	(0.82%)	1	(0.84%)
			Missing	47	(38.52%)	76	(63.87%)
		Follow-up	Not at all	43	(35.25%)	38	(31.93%)
			Less than half an hour a week	10	(8.2%)	7	(5.88%)
			Between 0.5 and 1 hour a week	9	(7.38%)	10	(8.4%)
			Between 1 and 3 hours a week	4	(3.28%)	5	(4.2%)

Outcome		Timepoint	Item	Mindfulness		Light exercise	
				n	%/m (sd)	n	%/m (sd)
			More than 3 hours a week	1	(0.82%)	1	(0.84%)
			Missing	55	(45.08%)	58	(48.74%)
	Practiced mindfulness informally	Post-intervention	Never	2	(1.64%)	6	(5.04%)
			Rarely	3	(2.46%)	7	(5.88%)
			Sometimes	41	(33.61%)	19	(15.97%)
			Often	19	(15.57%)	9	(7.56%)
			Very often	10	(8.2%)	2	(1.68%)
			Missing	47	(38.52%)	76	(63.87%)
		Follow-up	Never	10	(8.2%)	17	(14.29%)
			Rarely	20	(16.39%)	8	(6.72%)
			Sometimes	18	(14.75%)	28	(23.53%)
			Often	13	(10.66%)	5	(4.2%)
			Very often	6	(4.92%)	3	(2.52%)
			Missing	55	(45.08%)	58	(48.74%)
Preference of intervention[a]		Post-intervention		75	1.24 (36.37)	77	−12.04 (28.32)
Talking about own training with colleagues in other arm[b]		Post-intervention		75	5.07 (14.63)	77	2.81 (5.85)
Weekly moderate intensity exercise (min)		Baseline		122	359.9 (310.21)	119	350.12 (298.25)
		Post-intervention		73	406.19 (288.98)	76	399.71 (322.19)
		Follow-up		61	380.85 (282.78)	61	379.77 (255.89)

Outcome	Timepoint	Item	Mindfulness		Light exercise	
			n	%/m (sd)	n	%/m (sd)

Notes. [a]Scale: -50 (stong preference towards MBP) to 50 (strong preference towards LE); [b]Scale: 0 (not even once) to 100 (almost daily);

Questionnaire-based outcomes

Supplementary Table 3. Observed questionnaire-based outcomes at all time points. Table reused from Vainre et al (2023) under a CC-BY-4.0 license.

Outcome[a]	Timepoint	Mindfulness		Light Exercise		Total			
		n	m (sd)	n	m (sd)	n	m (sd)	d	p
WRFQ	Baseline	66	77.4 (11.85)	53	77.32 (13.84)	119	77.37 (12.72)		
	Post-intervention	36	82.43 (11.6)	32	80.18 (13.56)	-	-	0.06	0.63
	Follow-up	33	82.49 (13.25)	29	78.26 (18.21)	-	-	0.02	0.91
PSS	Baseline	122	20.5 (6.18)	118	20.69 (6.74)	240	20.59 (6.45)		
	Post-intervention	74	16.76 (7.07)	78	17.22 (6.91)	-	-	0.04	0.73
	Follow-up	67	16.9 (7.53)	61	16.52 (6.88)	-	-	-0.01	0.92
GAD-7	Baseline	122	6.48 (4.3)	119	6.27 (4.78)	241	6.37 (4.53)		
	Post-intervention	75	5.23 (4.35)	78	5.05 (4.11)	-	-	-0.00	0.99
	Follow-up	66	5.23 (4.9)	61	4.84 (4.29)	-	-	-0.07	0.58
PHQ-9	Baseline	117	8.21 (4.6)	119	9.12 (5.06)	236	8.67 (4.85)		
	Post-intervention	75	6.37 (4.76)	78	6.58 (5.02)	-	-	0.03	0.82
	Follow-up	66	6.86 (5.85)	61	6.13 (4.72)	-	-	-0.09	0.47
WSAS	Baseline	119	1.76 (4.75)	117	1.95 (5.52)	236	1.85 (5.14)		
	Post-intervention	71	1.54 (4.9)	77	3.35 (7.41)	-	-	0.17	0.18
	Follow-up	65	3.68 (8.41)	59	2.20 (5.11)	-	-	0.00	0.99

Outcome[a]		Timepoint	Mindfulness		Light Exercise		Total			
			n	m (sd)	n	m (sd)	n	m (sd)	d	p
Decentering		Baseline	118	29.25 (7.41)	116	28.84 (7.84)	234	29.05 (7.61)		
		Post-intervention	73	35.33 (8.74)	77	32.04 (8.77)	–	–	0.24	0.07
		Follow-up	66	34.7 (9.18)	60	32.05 (9.05)	–	–	0.22	0.09
MAAS		Baseline	122	3.61 (0.88)	115	3.53 (0.91)	237	3.57 (0.9)		
		Post-intervention	72	4.03 (0.93)	77	3.98 (0.86)	–	–	0.02	0.85
		Follow-up	66	4.16 (1.01)	60	4.08 (1.15)	–	–	0.02	0.87
Job importance		Baseline	121	2.43 (0.75)	117	2.28 (0.81)	238	2.36 (0.78)		
		Post-intervention	74	2.3 (0.77)	77	2.42 (0.66)	–	–	−0.14	0.27
		Follow-up	67	2.24 (0.65)	61	2.31 (0.85)	–	–	−0.07	0.56
Overtime (hrs)		Baseline	122	3.82 (6.99)	119	3.29 (5.5)	241	3.56 (6.29)		
		Post-intervention	75	3.58 (5.96)	77	4.32 (5.44)	–	–	0.22	0.09
		Follow-up	66	3.18 (6.08)	61	2.98 (4.51)	–	–	−0.01	0.95
Experiencing health problems, n (%)	No problems	Baseline	98	(80.33%)	100	(84.03%)	198	(82.16%)		
	No problems	Post-intervention	61	(50%)	58	(48.74%)	–	–		
	No problems	Follow-up	49	(40.16%)	48	(40.34%)	–	–		
Experiencing health problems, n (%)	Some problems	Baseline	22	(18.03%)	17	(14.29%)	39	(16.18%)		
		Post-intervention	11	(9.02%)	19	(15.97%)	–	–	0.20[b]	0.11
		Follow-up	16	(13.11%)	12	(10.08%)	–	–	−0.11[b]	0.41
	Missing	Baseline	2	(1.64%)	2	(1.68%)	4	(1.66%)		
		Post-intervention	50	(40.98%)	42	(35.29%)	–	–		
		Follow-up	57	(46.72%)	59	(49.58%)	–	–		

Notes. [a]Abbreviations: WRFQ = work role functioning scale, PSS = perceived stress scale, GAD-7 = generalised anxiety scale, PHQ-9 = Patient Health Questionnaire-9, WSAS = work and social adjustment scale, MAAS = mindful attention awareness scale; [b]Compared to reporting no problems, adjusted for baseline;

Supplementary Table 4. Baseline to post-intervention and baseline to follow-up changes across both arms. Table reused from Vainre et al (2023) under a CC-BY-4.0 license.

Outcome[a]	Pre-to-post-intervention *d*, both arms	p	Pre-to-12-week-follow-up *d*, both arms	p
WRFQ	0.10	0.28	0.14	0.12
PSS	−0.58	<0.001	−0.40	<0.001
GAD-7	−0.40	<0.001	−0.26	<0.001
PHQ-9	−0.49	<0.001	−0.25	<0.001
WSAS	0.70	<0.05	0.67	<0.01
Decentering	0.60	<0.001	0.38	<0.001
MAAS	0.46	<0.001	0.37	<0.001

Notes. [a]Abbreviations: WRFQ = work role functioning scale, PSS = perceived stress scale, GAD-7 = generalised anxiety scale, PHQ-9 = Patient Health Questionnaire-9, WSAS = work and social adjustment scale, MAAS = mindful attention awareness scale;

Cognitive control

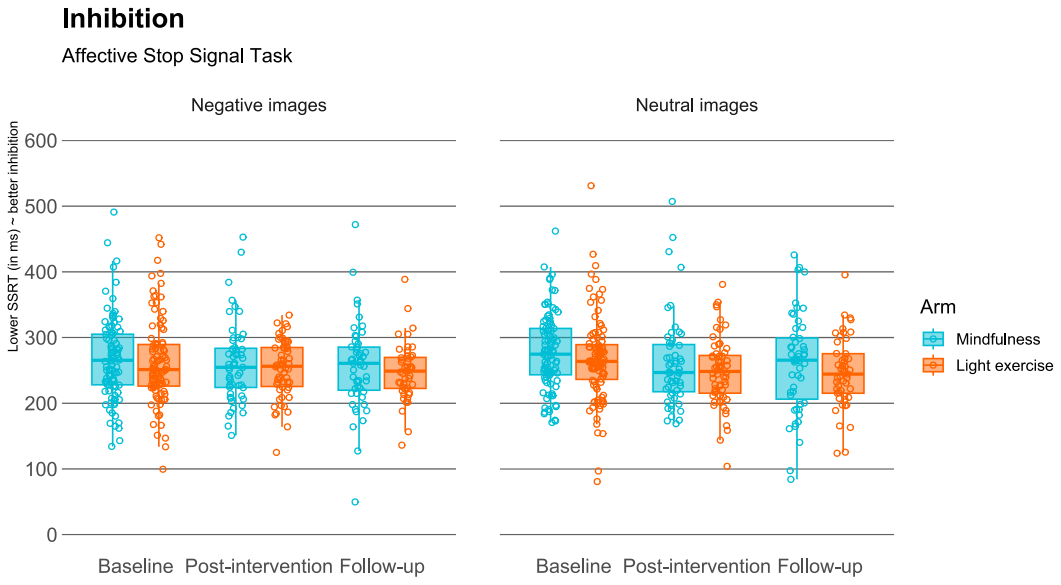
Supplementary Table 5. Observed cognitive control outcomes at all time points. Table reused from Vainre et al (2023) under a CC-BY-4.0 license.

Outcome	Valence		Timepoint	Mindfulness		Light exercise		Total	
				n	m (sd)	n	m (sd)	n	m (sd)
aSST	Negative	Missed go trials (%)	Baseline	101	20.96 (19.37)	101	18.59 (16.61)	202	19.77 (18.04)
			Post-intervention	58	19.05 (20.58)	62	18.04 (19.27)	–	–
			Follow-up	53	16.84 (17.93)	48	15.88 (20.42)	–	–
		Accuracy in go trials (%)	Baseline	101	83.73 (4.54)	101	84.67 (3.56)	202	84.2 (4.1)
			Post-intervention	58	83.33 (4.02)	62	83.97 (4)	–	–
			Follow-up	53	82.41 (11.9)	48	84.4 (5.09)	–	–
		Reaction time in go trials (ms)	Baseline	101	729.17 (98.02)	101	735.63 (96.7)	202	732.4 (97.17)
			Post-intervention	58	738.53 (105.11)	62	736.83 (99.42)	–	–
			Follow-up	53	739.76 (104.34)	48	735.12 (102.39)	–	–
		Probability of responding in stop-signal trials (%)	Baseline	101	58.52 (9.28)	101	59.22 (8.21)	202	58.87 (8.75)
			Post-intervention	58	57.01 (8.51)	62	57.15 (8.18)	–	–
			Follow-up	53	57.09 (10.12)	48	57.71 (7.81)	–	–
		Stop Signal Delay (ms)	Baseline	101	479.8 (134.92)	101	483.98 (116.15)	202	481.89 (125.59)
			Post-intervention	58	497.7 (139.8)	62	505.44 (122.47)	–	–
			Follow-up	53	492.01 (132.69)	48	499.08 (122.89)	–	–
		Reaction time in failed stop-trials (ms)	Baseline	101	232.62 (75.13)	101	213.21 (63.67)	202	222.91 (70.14)

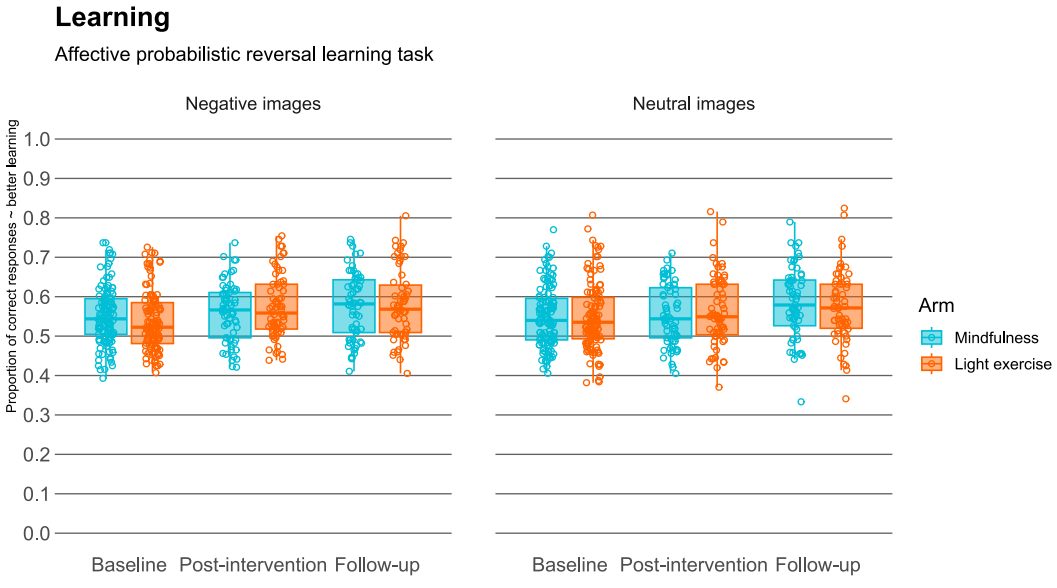
Outcome	Valence		Timepoint	Mindfulness		Light exercise		Total	
				n	m (sd)	n	m (sd)	n	m (sd)
			Post-intervention	58	219.85 (69.61)	62	214.14 (62.7)	–	–
			Follow-up	53	214.08 (50.4)	48	197.74 (59.38)	–	–
		Stop Signal Reaction Time (ms)	Baseline	101	267.59 (63.02)	101	262.73 (65.08)	202	265.16 (63.94)
			Post-intervention	58	260.4 (60.48)	62	252.17 (43.49)	–	–
			Follow-up	53	255.64 (66.5)	48	248.23 (43.81)	–	–
	Neutral	Missed go trials (%)	Baseline	102	20.1 (18.42)	105	19.68 (17.67)	207	19.89 (18)
			Post-intervention	55	17.47 (20.18)	62	18.46 (20.35)	–	–
			Follow-up	54	18.69 (18.48)	50	18.71 (21.92)	–	–
		Accuracy in go trials (%)	Baseline	102	84.05 (3.86)	105	84.32 (3.56)	207	84.19 (3.7)
			Post-intervention	55	84.38 (4.35)	62	84.32 (5.18)	–	–
			Follow-up	54	82.14 (12.5)	50	84.17 (4.79)	–	–
		Reaction time in go trials (ms)	Baseline	102	736.53 (91.39)	105	743.56 (96.51)	207	740.1 (93.86)
			Post-intervention	55	734.28 (100.78)	62	738.55 (97.41)	–	–
			Follow-up	54	738.83 (104.04)	50	744.35 (105.71)	–	–
		Probability of responding in stop-signal trials (%)	Baseline	102	58.22 (8.63)	105	58.6 (7.23)	207	58.42 (7.93)
			Post-intervention	55	59.64 (7.72)	62	59.09 (7.17)	–	–
			Follow-up	54	58.13 (8.2)	50	59 (7.54)	–	–
		Stop Signal Delay (ms)	Baseline	102	478.66 (129.44)	105	492.73 (113.96)	207	485.8 (121.74)
			Post-intervention	55	486.89 (135.23)	62	501.75 (120.89)	–	–

Outcome	Valence		Timepoint	Mindfulness		Light exercise		Total	
				n	m (sd)	n	m (sd)	n	m (sd)
			Follow-up	54	491.86 (130.17)	50	512.17 (130.79)	–	–
		Reaction time in failed stop-trials (ms)	Baseline	102	221.08 (69.16)	105	216.97 (65.92)	207	218.99 (67.4)
			Post-intervention	55	216.42 (74)	62	212.7 (58.89)	–	–
			Follow-up	54	215.01 (58.72)	50	207.21 (68.04)	–	–
		Stop Signal Reaction Time (ms)	Baseline	102	277.96 (57.28)	105	267.41 (64.79)	207	272.61 (61.28)
			Post-intervention	55	261.2 (68.8)	62	248.14 (52.16)	–	–
			Follow-up	54	260.32 (75.46)	50	246.51 (53.14)	–	–
aLT	Negative	Reaction Time (ms)	Baseline	118	697.51 (161.77)	115	691.23 (154.96)	233	694.41 (158.13)
			Post-intervention	65	687.58 (177.18)	68	641.92 (153.52)	–	–
			Follow-up	61	650.8 (179.57)	58	633.07 (145.98)	–	–
		Accuracy (%)	Baseline	118	55.13 (7.39)	115	53.81 (7.52)	233	54.48 (7.47)
			Post-intervention	65	56.23 (7.39)	68	57.38 (7.96)	–	–
			Follow-up	61	57.69 (8.44)	58	57.98 (9.03)	–	–
	Neutral	Reaction Time (ms)	Baseline	118	694.47 (159.65)	115	680.54 (151.25)	233	687.6 (155.38)
			Post-intervention	65	679.28 (172.09)	68	649.72 (144.21)	–	–
			Follow-up	61	655.5 (178.72)	58	631.5 (147.1)	–	–
		Accuracy (%)	Baseline	118	54.86 (7.72)	115	54.84 (8.6)	233	54.85 (8.15)
			Post-intervention	65	55.5 (7.63)	68	56.33 (8.9)	–	–
			Follow-up	61	58.45 (8.98)	58	57.57 (9.27)	–	–

Notes. [a]aSST = affective stop-signal task, aLT = affective learning task

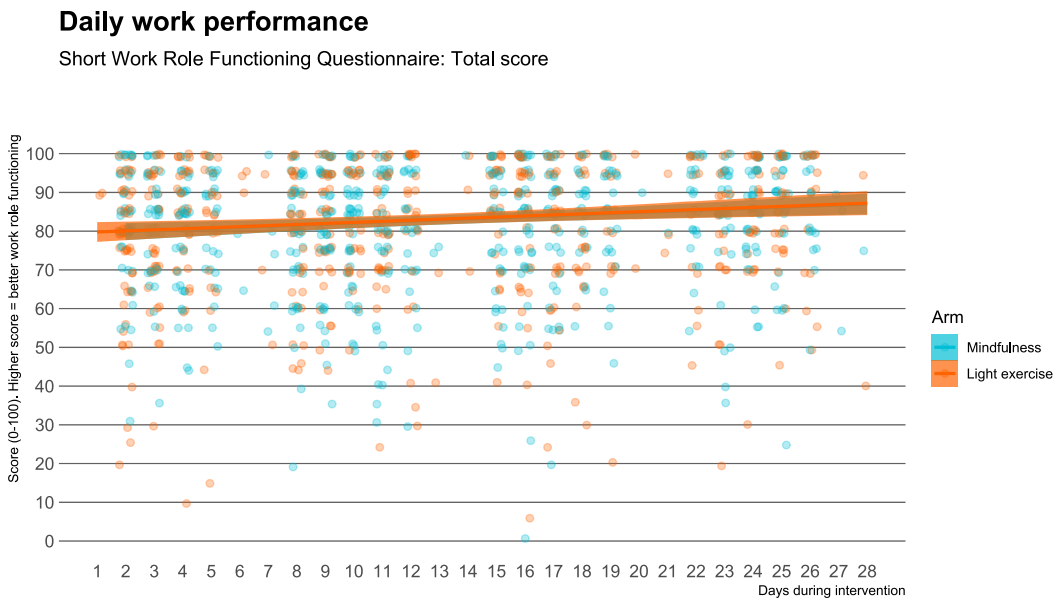


Supplementary Figure 1. Affective Stop Signal Task results at baseline, post-intervention, and follow-up. Figure reused from Vainre et al (2023) under a CC-BY-4.0 license.



Supplementary Figure 2. Affective Probabilistic Learning Task results at baseline, post-intervention, and follow-up. Figure reused from Vainre et al (2023) under a CC-BY-4.0 license.

Daily monitoring



Supplementary Figure 3. Daily monitoring of work role functioning. Figure reused from Vainre et al (2023) under a CC-BY-4.0 license.