PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (**http://bmjopen.bmj.com/site/about/resources/checklist.pdf**) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Mindfulness-Based Programs to reduce stress and enhance well-being at work: a realist review
AUTHORS	Micklitz, Katrin; Wong, Geoffrey; Howick, Jeremy

VERSION 1 – REVIEW

REVIEWER	Renée Scheepers Erasmus University Rotterdam, the Netherlands
REVIEW RETURNED	27-Nov-2020

GENERAL COMMENTS	This is a very interesting and valuable realist synthesis on mechanisms of mindfulness interventions in workplace-based settings. It is clearly written and was rigorously conducted. I do have some questions. In the method section, the authors refer to a list of theories on mechanisms of mindfulness. When reading the supplementary file about this, this reads as a summary, however the full breadth of all theories on mindfulness mechanisms is not covered. Could these theories also be reported in a table, with a short explanation of each theory? The search strategy includes the term 'work' in the search string. However, I think there are many articles that studied mindfulness in specific work settings (e.g medicine) that did not use the word 'work' yet 'medicine', 'medical practice', 'physicians' etc but are still relevant. Why did the authors not choose to add relevant occupations to the search string? Furthermore, the authors mention that a random 10% of title/abstracts were checked by a second author. What was the kappa? How were inconsistencies translated into the further selection process by the first author? In step 4 of the methods, the authors state that they excluded articles with concerning credibility based on 'current accepted standards'. What are these standards? Which criteria were used? Was there a protocol or checklist for this? In step 6, the authors mention that they selected the COR and psychological safety theory to explain patterns in the data. Why specifically these theories? There are numerous theories that could have been used and the selection substantially affects the results. Should these theories be somehow embedded in the aim or research question of the review, as this was the dominating perspective on clarifying the mechanisms?

REVIEWER	Ebrahim Norouzi
REVIEW RETURNED	04-Dec-2020

GENERAL COMMENTS	The manuscript entitled "Mindfulness-Based Programs to reduce stress and enhance well-being at work: a realist review" presents a topic that may interest readers of the "BMJ Open". I really like this

	study. Although I consider an interesting topic, I would like to make some minor comments about the present manuscript below:
	In the Background, the authors need to be more explicit about the real justifications of the review. Also, why is essential a study this topic? Why workplace and not others such as sports, military, and art context?

REVIEWER	Jason Wingert UNC Asheville, USA
REVIEW RETURNED	22-Dec-2020

GENERAL COMMENTS	This paper is a realist review of mindfulness-based practices (MBP) in the workplace. It provides the reader with a comprehensive analysis of the benefits, mechanisms, and realities, as well as circumventing challenges and obstacles, of implementing a MBP in the workplace. The paper is exceptionally well-written and researched. Overall, the paper will be useful for employers or MBP teachers for designing effective MBPs. Specific comments: Page 7, line 22 (Text box 1): 'in stress' should be 'of stress' Page 19, line 8 (Text box 4): 'dividuals' should be 'individuals' Page 19, line 28 (Text box 4): 'setting perceived' should be 'setting is perceived' I personally do not think the participant quotes add substance to the paper, only unnecessary length

VERSION 1 – AUTHOR RESPONSE (See the next page)

Reviewer 1	
1. In the method section, the authors refer to a list of theories on mechanisms of mindfulness. When reading the supplementary file about this, this reads as a summary, however the full breadth of all theories on mindfulness mechanisms is not covered. Could these theories also be reported in a table, with a short explanation of each theory?	Thank you for pointing this out. We have changed the wording in our methods section from "overview of the most common theories of mindfulness" to "summary of the most common theories of mindfulness". As suggested, we have added in supplementary file 1 a table with brief explanations of each theory/mechanism. A more comprehensive discussion of all potential mindfulness mechanisms would be beyond the scope of this review. We have provided references though that we hope will help direct readers who are interested in exploring these theories in more depth. We have added in the supplementary file the following sentences that will hopefully make clearer that our review focusses on mindfulness theories related to mental health and well-being in MBPs (rather than on mindfulness theories in general): "Different theories have looked at different, partially overlapping mindfulness mechanisms. A full discussion of all potential mindfulness mechanisms would be beyond the scope of this review. The following mechanisms have been theorized to be involved in enhancing mental health and well-being in MBPs: ..."
2. The search strategy includes the term 'work' in the search string. However, I think there are many articles that studied mindfulness in specific work settings (e.g., medicine) that did not use the word 'work' yet 'medicine', 'medical practice', 'physicians' etc but are still relevant. Why did the authors not choose to add relevant occupations to the search string?	Our search was designed so that we would capture all workplace MBPs regardless of whether the participants were healthcare professionals or not. We developed and piloted our search strategies in conjunction with a health research librarian. We used a review of workplace MBPs by Lomas et al. [1] as a starting point for the design of our searches. (A later systematic review and meta-analysis by Lomas et al. on the impact of MBPs on the wellbeing of healthcare professionals [2] is based on the same general search strategy.) We found the searches were returning documents that contained the data we needed and hence we judged it to be fit for purpose. One strength of a realist approach is that it allows us to extrapolate our findings to healthcare workers as well, because we have data to suggest that the same mechanisms are in operation in different settings depending on certain conditions (e.g., psychological safety). Based on the available data we have no reason to believe that our theories might be different for healthcare professionals. However, there will most likely be additional explanations that we were not able to identify with the given data. We have addressed this limitation at the end of our Strengths and Limitations section (page 18): "Our realist review has been able to shed light on this issue through its CMOCs, but we do not claim to have developed an exhaustive and definitive explanation of all outcomes from MBPs."
3. Furthermore, the authors mention that a random 10% of title/abstracts were checked by a second author. What was the kappa? How were inconsistencies translated into the	We followed standard realist review methodology [3] which does not involve doing a kappa calculation. In brief, this is due to the nature of the included studies and the need to read the full papers before deciding to include them [4]. We agree, however, that it would be a good idea for methodological work on realist review methodology to be done in general regarding obtaining agreement between reviewers at the screening stages and its implications on the plausibility and coherence of any explanatory theories developed. We will explore the possibility of

3

	further selection process by the first author?	taking this point forward in our future work.
		All disagreements between the authors were resolved via discussion. To clarify remaining questions, the main author used a framework by Crane et al. [5] that defines the essential characteristics of the family of MBPs and adaptations for different populations. We refer to that framework in our manuscript ('Search for Evidence', page 6): "Our decisions about which program types to include were guided by Crane *et al.*'s framework of MBPs.[5]"
4.	In step 4 of the methods, the authors state that they excluded articles with concerning credibility based on 'current accepted standards'. What are these standards? Which criteria were used? Was there a protocol or checklist for this?	We agree that this paragraph has been misleading, and we have made the following changes on page 6 of our manuscript: "Assessment of rigour was not performed on the basis of pre-defined quality standards or with regards to the entire study but instead was made only for specific sections of relevant data contained within included studies [6]. In one case [7], for instance, the strong relationship between researcher (who was also coach in the MBP) and participants may have increased the risk of social desirability bias in that study for some outcomes. However, the study contained rich data on the experience of (self-) acceptance which we judged to be less prone to the influence of social desirability bias and so could be used to understand the link between trust/safety (context), feeling accepted (mechanism) and outcomes like (self-)compassion. Consistent with realist methodology [4], rigour was further judged at the level of explanatory power of the realist program theory developed in this review."
5.	In step 6, the authors mention that they selected the COR and psychological safety theory to explain patterns in the data. Why specifically these theories? There are numerous theories that could have been used and the selection substantially affects the results. Should these theories be somehow embedded in the aim or research question of the review, as this was the dominating perspective on clarifying the mechanisms?	We did not embed COR and psychological safety theory in the research aim or research question as we did not know at the outset which substantive theory would best explain what we have found. Theory development in realist research iteratively moves back and forth between data and theory.[8] Our choice was guided by criteria of consilience (whether it accounted for more of the data than other theories), simplicity (whether it contained as few exceptions as possible), and analogy (whether it fit with what is already known/substantive theory).[4] In other words, COR and theory of psychological safety provided the best analogy and helped us deepen our understanding of our CMOCs/ and our program theory. We explained the selection of our substantive theory in supplementary file 3 (under the subheading "Retroduction and integration of substantive theory" on page 3). To illustrate how our choice of substantive theory was guided by criteria of analogy, we have added the following (supplementary file 3, page 3): "COR Theory had been mentioned in one of our included studies as an explanation for the reported beneficial outcomes.[9] Other authors had previously cited the COR model with regards to mindfulness.[10, 11] In all these examples, mindfulness was seen as a resource to help individuals buffer against organizational stress and enhance job engagement and job satisfaction. More recently, a study by Hülsheger *et al.*[12] on state mindfulness in working populations showed how previous day recovery experiences benefitted mindfulness and

		subsequent recovery experience (gain spiral), whereas workload hampered the experience of mindfulness as well as subsequent recovery experience (loss spiral). Hülsheger *et al.* explained the relationship between mindfulness and recovery experience by the availability of energetic resources that are necessary to bring awareness to present moment experience. They proposed to look at additional work-related factors that might impede mindfulness, including situational constraints, role conflict, or customer-related stressors."
Reviewer 2		
6.	In the Background, the authors need to be more explicit about the real justifications of the review.	Thank you for the comment. We have strengthened our explanation of the need for this review by adding the following to manuscript: "Evidence suggests that these programmes can be effective [13], especially if they are used as preventative strategies and address sub-threshold conditions [14]. Among these preventative workplace health interventions, mindfulness- based programs (MBPs) …" The main issue however remains application/implementation. In other words, while there exists ample evidence that workplace MBPs can work, that evidence is heterogeneous and suggests that workplace MBPs work in certain contexts, under certain conditions, for certain people, in certain respects. In order to be able to implement effective and sustainable programs we need a better understanding of when these programs work and when they do not work. This is something we have pointed out in our manuscript (page 4): "However promising, these findings leave us with unresolved issues that limit our ability to apply the evidence. For one, MBPs are multifaceted, generally consisting of numerous potential active ingredients (e.g. experiential practices, psychoeducation, social support)[15] and they vary considerably with regards to their duration, and mode of delivery.[16] At present the current evidence does not indicate what exactly makes these programs successful. In addition, high attrition rates[17, 18] together with evidence of publication bias[2, 16, 17, 19, 20] and great heterogeneity in outcomes between studies[19, 21-23] indicate that workplace MBPs might work less well than is believed or, more likely, only in certain settings, for certain individuals, and under certain circumstances. Given these limitations of existing evidence on workplace MBPs, it seems rational that in order to be able to develop and implement effective and sustainable programs that can be applied across groups and settings, including in health care, we should not only look at their net effects but investigate *how* and *why* they work (or do not work)." In order to stress the implementation issue, we have added the following sentence: "Such an investigation will generate the knowledge needed to understand better what needs to be done to implement workplace MBPs."

7.	Also, why is essential a study this topic?	See point 9
8.	Why workplace and not others such as sports, military, and art context?	Research done in the fields of sports, arts, and military was not considered, as contexts in these settings were judged to differ considerably from general workplace settings. For instance, persons who are on duty in the military work full time (i.e., they do not go home in the evening). In addition, MBPs for veterans are often special programs, designed to address posttraumatic stress disorder which is not the usual purpose of programs used for civilian employees. In our review we have focussed on MBPs as preventive strategies, addressed at sub-threshold levels. We excluded athletes and artists because they often perform as individuals rather than in teams, hierarchies, and organisational structures and culture. We do agree though that for certain military personnel, athletes or artists there exist overlaps with other professions. Our theories would be transferable to these cases as we have argued in response to #5 (reviewer 1). We have noted this in our discussion section: "The explanations from the refined program theory are based on understanding the behaviour of widely occurring mechanisms under different contexts and on data from documents that include a broad range of professional groups. This provides a warrant for transferability of the findings and is one of the strengths of this realist review."
Reviewer 3		
9.	Page 7, line 22 (Text box 1): 'in stress' should be 'of stress'	We have corrected that.
10.	Page 19, line 8 (Text box 4): 'dividuals' should be 'individuals'	We have corrected that.
11.	Page 19, line 28 (Text box 4): 'setting perceived' should be 'setting is perceived'	We have corrected that.
12.	I personally do not think the participant quotes add substance to the paper, only unnecessary length	This is a reasonable comment. We have provided these verbatim sections of text to illustrate to the reader the types of data we have used in our analyses. This is commonly done in reports of realist review. [24, 25] Maybe there is editorial guidance on this matter?

1.   Lomas T, Medina JC, Ivtzan I, Rupprecht S, Hart R, Eiroa-Orosa FJ. The impact of mindfulness on well-being and performance in the workplace: an inclusive systematic review of the empirical literature. *European Journal of Work and Organizational Psychology*. 2017;26(4):492-513.

2.   Lomas T, Medina JC, Ivtzan I, Rupprecht S, Eiroa-Orosa FJ. A systematic review and meta-analysis of the impact of mindfulness-based interventions on the well-being of healthcare professionals. *Mindfulness*. 2019;10:1193-216.

3.   Wong G, Greenhalgh T, Westhorp G, Buckingham J, Pawson R. RAMESES publication standards: realist syntheses. *BMC Medicine*. 2013;11(21).

4.   Wong G. Data gathering for realist reviews: looking for needles in haystacks. . In: Emmel N., Greenhalgh J., Manzano A., Monaghan M., Dalkin S., editors. Doing Realist Research. London: Sage; 2018. p. 131-45.

5.   Crane RS, Brewer J, Feldman C, Kabat-Zinn J, Santorelli S, Williams JM, et al. What defines mindfulness-based programs? The warp and the weft. *Psychol Med*. 2017;47(6):990-9.

6.   Wong G, Westhorp G, Pawson R, Greenhalgh T. Realist Synthesis RAMESES Training Materials. The Rameses ProjectsI. 2013. https://www.ramesesproject.org/media/Realist_reviews_training_materials.pdf. Accessed 06 May 2019.

7.   Linger RA. A Qualitative Study of a Mindfulness-Based Coaching Intervention for Perception Shifts and Emotional Regulation around Workplace Stressors and Quality of Worklife. PhD thesis. Saybrook University, Pasadena, USA; 2016.

8.   Emmel N. Sampling and choosing cases in qualitative research: A realist approach. . London: SAGE Publications Ltd.; 2013.

9.   Hugh-Jones S, Rose S, Koutsopoulou GZ, Simms-Ellis R. How is stress reduced by a workplace mindfulness intervention? A qualitative study conceptualising experiences of change. *Mindfulness*. 2018;9:474-87.

10.   Eatough EM. How does employee mindfulness reduce psychological distress? *Industrial and Organizational Psychology: Perspectives on Science and Practice*. 2015;8(4):643-7.

11.   Zivnuska S, Kacmar K, Ferguson M, Carlson DS. Mindfulness at work: resource accumulation, well-being, and attitudes. *The Career Development International*. 2016;21(2):106-24.

12.   Hülsheger UR, Walkowiak A, Thommes MS. How can mindfulness be promoted? Workload and recovery experiences as antecedents of daily fluctuations in mindfulness. *J Occup Organ Psychol*. 2018;91(2):261-84.

13.   Bagnall A, Jones R, Akter H, Woodall JR. Interventions to prevent burnout in high risk individuals: evidence review.: Public Health England; 2016.

14.   Leka S, Jain A. Mental health in the workplace in Europe. European Comission; 2019. Contract No.: 08.03.

15.   Good DJ, Lyddy CJ, Glomb TM, Bono JE, Brown KW, Duffy MK, et al. Contemplating mindfulness at work. *Journal of Management*. 2016;42(1):114-42.

16.   Bartlett L, Martin A, Neil AL, Memish K, Otahal P, Kilpatrick M, et al. A systematic review and meta-analysis of workplace mindfulness training randomized controlled trials. *J Occup Health Psychol*. 2019;24(1):108-26.

17.   Burton A, Burgess C, Dean S, Koutsopoulou GZ, Hugh-Jones S. How effective are mindfulness-based interventions for reducing stress among healthcare professionals? A systematic review and meta-analysis. *Stress and Health: Journal of the International Society for the Investigation of Stress*. 2017;33:3-13.

18.   Suleiman-Martos N, Gomez-Urquiza JL, Aguayo-Estremera R, Canadas-De La Fuente GA, De La Fuente-Solana EI, Albendin-Garcia L. The effect of mindfulness training on burnout syndrome in nursing: a systematic review and meta-analysis. *J Adv Nurs*. 2020;76:1124-40.

19.   Vonderlin R, Biermann M, Bohus M, Lyssenko L. Mindfulness-based programs in the workplace: a meta-analysis of randomized controlled trials. *Mindfulness*. 2020;11:1579-98.

20.   Slemp GR, Jach HK, Chia A, Loton DJ, Kern ML. Contemplative interventions and employee distress: a meta-analysis. *Stress and Health*. 2019;35:227–55.

21.   Lomas T, Medina JC, Ivtzan I, Rupprecht S, Eiroa-Orosa FJ. Mindfulness-based interventions in the workplace: an inclusive systematic review and meta-analysis of their impact upon wellbeing. *The Journal of Positive Psychology*. 2019;14(5):625-40.

22.   Klingbeil DA, Renshaw TL. Mindfulness-based interventions for teachers: a meta-analysis of the emerging evidence base. *Sch Psychol Q*. 2018;33(4):501-11.

23.   Ruiz-Fernandez MD, Ortiz-Amo R, Ortega-Galan AM, Ibanez-Masero O, Rodriguez-Salvador MDM, Ramos-Pichardo JD. Mindfulness therapies on health professionals. *Int J Ment Health Nurs*. 2020;29:127-40.

24.   Weetman K, Wong G, Scott E, MacKenzie E, Schnurr S, Dale J. Improving best practice for patients receiving hospital discharge letters: a realist review. *BMJ Open*. 2019;9(6):e027588.

25.   Jagosh J, Bush PL, Salsberg J, Macaulay AC, Greenhalgh T, Wong G, et al. A realist evaluation of community-based participatory research: partnership synergy, trust building and related ripple effects. *BMC Public Health.* 2015;15(1).

VERSION 2 – REVIEW

REVIEWER	Renée Scheepers
	Erasmus School of Health Policy and Managament, the Netherlands
REVIEW RETURNED	17-Feb-2021

GENERAL COMMENTS	Thank you for your explanations of the changes. All is clear, I have no further comments.

Correction: *Mindfulness-based programmes to reduce stress and enhance well-being at work: a realist review*

Micklitz K, Wong G, Howick J. Mindfulness-based programmes to reduce stress and enhance well-being at work: a realist review. *BMJ Open* 2021;11:e043525. doi: 10.1136/bmjopen-2020-043525

This article was previously published with an error. The author Geoffrey Wong should have been listed as Geoff Wong.

Check for updates