PEER REVIEW HISTORY

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ARTICLE DETAILS

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VERSION 1 – REVIEW

<table>
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<th>Micek , Mark A. University of Wisconsin-Madison School of Medicine and Public Health</th>
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GENERAL COMMENTS

This article is a thorough review of the literature related to a certain scope of published interventions aimed at reducing burnout among healthcare professionals. As the authors point out, the lack of standardization for both the interventional and measurement components of published studies precluded their ability to perform a meta-analysis, and therefore they instead synthesized and categorized the available studies to give the reader the ability to make general conclusions about the strength of evidence for various interventional strategies.  

The authors structure the domains of workplace interventions into those focused on the individual vs organization. This has some relationship- although not fully- to a common set of domains affecting well-being developed by at Stanford University, which includes Personal Resilience (which is similar to the authors’ individual category), Efficiency of Practice (which is similar to the authors’ organizational category), and Culture of Wellness (which has elements of both individual and organizational categories). I wonder why the authors choose their structure of domains, also as it might blur some of the distinctions that are present in the Stanford model, for example that some of the strategies targeted at individuals could frequently be adopted and implemented by organizations.

Another concern deals with the fact (as admitted by the authors) that the great majority of studies included in their review are individual, and specifically focused on mindfulness and meditation, with only 3 organizational- and only one of which was focused on workflow changes (Gregory et al). I wonder if the authors’ literature review included studies looking at the effects of interventions aimed at improving Electronic Health Record (EHR) efficiency and reducing EHR time with interventions such as training, intensive optimization sessions, and scribes, as each of these have been studied in pre/post fashions using various outcome indicators (i.e.
burnout, job satisfaction, EHR time). I would be interested to know if these types of studies were considered for inclusion, or if they were captured but excluded for other reasons.

Other specific comments:
• Page 3, line 34: should probably start with: “This review analyzed various workplace…”
• Page 22, Table 4: should this perhaps be included in the Results section rather than referenced initially in the Discussion, with perhaps an overall assessment of the results relative to the quality of the studies themselves? You mention your concerns about the quality of the studies later in the last paragraph of Discussion (page 23 lines 34-37), but perhaps it should be called out more in an interpretation of Table 4 (ie I count only 4 studies that have all 3: Large Sample Size + Control Group + Follow up).
• Page 22, Table 4, column “Large Sample Size”, perhaps qualify this with an asterisk for your cutoff, or rename to “Large Sample Size > X”
• Page 22, Table 4, column “Follow up” – what does that mean?
• Page 23, line 3, should probably change “deny” to “refute”. I’m also curious how you think the literature can suggest that organizationally focused interventions are more effective (page 22 line 60, and page 23 line 12), yet you only found 3 studies that evaluate this. Is this because this belief is based on theory only, or because studies showing this effect are not published, or perhaps methodologically inferior? See my comments above on the lack of inclusion of other studies focused on organizational—or Efficiency of Practice—interventions.
• Page 23, line 3: capitalize “However”, and change “there was” to “there were”.

REVIEWER
Erschens, Rebecca
University Medical Hospital Tuebingen, Internal Medicine,
Department of Psychosomatic Medicine and Psychotherapy

REVIEW RETURNED
28-Jan-2023

GENERAL COMMENTS
Thank you for the opportunity to review the manuscript on ‘Workplace interventions to improve wellbeing and reduce burnout for nurses, physicians, and allied healthcare professionals: A Systematic Review’

This review explores the impact of workplace interventions to support well-being and to reduce burnout among health care professionals.

The title is chosen precisely and appropriately, so that the reader directly gains an impression of the content. This is a well and comprehensibly written paper with high social relevance as the pressure on the health system continues to grow. Revisions should be made to certain parts, which are explained in more detail below. In addition, the authors are especially advised to check again the tables and figures according to the APA conformity, as there are indications that these are not fulfilled.

Title and abstract

The abstract clearly states the intention of the review, and the methodological approach is concisely summarised. In the result section, you should also indicate how many hits the search has achieved in total, so that the reader gets a direct overview of the
available research findings. In addition, it should be explained what is meant by secondary level interventions, as this will probably not be familiar to every reader. Why a meta-analysis could not be carried out is clearly described. Additionally, in the conclusion section, what is meant by “..do need workplace wellbeing reform”? Please specify. When discussing the lack of a control group, it should be explained what form of control group would be seen as appropriate.

Introduction

In general, the introduction is well-written and comprehensible. However, some minor remarks should be made at the point. In line58, the term mental ill-health is used which is uncommon. The authors should revise this and use mental illness / mental disease instead.

For a more precise introduction to the topic, it could be mentioned to what extent the various interventions are directed at the occupational groups studied in the review, i.e. nurses, doctors, etc., as it might be interesting to see whether different stressors and consequent interventions result for the different occupational groups. In addition, it should also be stated which reviews and overviews already exist on the topic.

Methodology

In the methodology section, it should be noted that the full search protocol, including search terms, can be found in the appendix.”. In addition, it should be described here in more detail what the self-developed search strategy looked like, e.g. was the full-text screening carried out by only one or two people? Was a senior researcher also involved in case of uncertainty? This will partly be described in more detail later, but should be noted regarding the outcome measures it gets not quite clear to the reader why the search was only started in 2015, please further explain as this might indicate that there is a review published in 2015 already. Can this statement be supported with literature if necessary?

The literature research part should be more specific about what the three-step approach looked like.

Furthermore, the figures should be labelled in accordance with the APA (using headings and significant subtitles).

In Table 1 in particular, it should be described in more detail how the evaluation is composed and what the abbreviations and the respective evaluations mean and what range is used for the respective scales. This would make it easier for the reader to see the quality of the included studies. The APA conformity of the table should also be checked here, as there are indications that this has not been complied with.

The summary of the characteristics of the studies is clear and gives the reader a good summary of the included studies at first glance. However, when describing the outcome measures, it should be added which dependent variables were focused on, i.e. which forms of mental health / wellbeing outcomes were potentially of interest to the authors.
A similar comment should be made for table 2. The table seems logical, relevant aspects of the interventions are reported. However, the areas explained in this table should be described in more detail below the graph. In addition, the content of the "focus of intervention" section should be shortened. The headings of tables 2 and 3 should be chosen in such a way that it is immediately clear to the reader what content they contain and for what purpose (e.g. the general overview and the detailed breakdown of outcomes), currently it is not obvious at first glance. The overview in table 4 is well chosen and gives a good overview of the content and quality of the study. In due course, it should be described why exactly these outcome categories were chosen.

Conclusion

In the conclusions, the remarks made earlier in the comments on the abstract should be taken into account.

I wish you all the best with your revision.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Dr. Mark A. Micek, University of Wisconsin-Madison School of Medicine and Public Health

Comments to the Author:

This article is a thorough review of the literature related to a certain scope of published interventions aimed at reducing burnout among healthcare professionals. As the authors point out, the lack of standardization for both the interventional and measurement components of published studies precluded their ability to perform a meta-analysis, and therefore they instead synthesized and categorized the available studies to give the reader the ability to make general conclusions about the strength of evidence for various interventional strategies.

Thank you for your time, consideration, and thoughtful feedback.

The authors structure the domains of workplace interventions into those focused on the individual vs organization. This has some relationship—although not fully—to a common set of domains affecting well-being developed by at Stanford University, which includes Personal Resilience (which is similar to the authors’ individual category), Efficiency of Practice (which is similar to the authors’ organizational category), and Culture of Wellness (which has elements of both individual and organizational categories). I wonder why the authors choose their structure of domains, also as it might blur some of the distinctions that are present in the Stanford model, for example that some of the strategies targeted at individuals could frequently be adopted and implemented by organizations.

Thank you for highlighting additional models for workplace wellbeing. This review accepted articles published between 2015-2022 and no study referenced the abovementioned wellbeing domains as set by Stanford University. The most universally applied classification of workplace interventions observed throughout the development of this review, were organisationally focused versus individually focused workplace interventions. The authors therefore opted to remain as consistent with the studies synthesised in this review, as possible by adopting this same structure.

We agree that, individually focused wellbeing interventions designed to manage stress in the individual employee may be implemented by an organisation. For example, an organisation may provide weekly yoga sessions for a group of healthcare workers (providing a culture of wellness), however this would still not be categorised under the organisationally focused workplace interventional umbrella, as this type of wellbeing intervention does not affect any kind of change in the source of workplace stress i.e., workloads, daily tasks, degree of role autonomy etc.

Due to the significant financial and/or productivity consequences organisations must overcome if they wish to implement meaningful workplace strategies, for example: reducing employee workloads, employing additional staff, or adapting daily tasks, there is less current literature investigating the
success of organisational interventions. There is also the additional difficulty of conducting randomised controlled trials (and therefore producing robust data) within an organisation. However, despite the abundance of published individually focused interventional studies, the data reflects more promising results from proactive, organisational change.

Another concern deals with the fact (as admitted by the authors) that the great majority of studies included in their review are individual, and specifically focused on mindfulness and meditation, with only 3 organizational- and only one of which was focused on workflow changes (Gregory et al). I wonder if the authors’ literature review included studies looking at the effects of interventions aimed at improving Electronic Health Record (EHR) efficiency and reducing EHR time with interventions such as training, intensive optimization sessions, and scribes, as each of these have been studied in pre/post fashions using various outcome indicators (ie burnout, job satisfaction, EHR time). I would be interested to know if these types of studies were considered for inclusion, or if they were captured but excluded for other reasons.

The inequitable ratio of organisational versus individually focused interventions reflects the literature consensus that organisational change is far more difficult and costly to implement and is therefore less researched. The effect of the optimisation of the electronic health record on employee burnout and job satisfaction was not specifically excluded from this review. We did, however, have targeted search criterium and no article relating specifically to the EHR was eligible. Studies that implemented workplace interventions whereby improving employee wellbeing or reducing burnout was not the primary focus of the intervention, were excluded. Studies that did not utilise a valid wellbeing/burnout inventory were also excluded. The inclusion criterium has now been amended to reflect these statements.

Studies whereby improving employee wellbeing or reducing burnout was not the primary focus of the intervention, were excluded.

All study designs were accepted if objective and quantifiable pre- and post-intervention outcome measures such as observer ratings were reported, utilising a valid and reliable wellbeing or burnout survey inventory.

This point pertaining to the EHR or effects of technology on workplace wellbeing is a great example of an organisational change to promote wellbeing in healthcare workers. As such, the authors intend to discuss the effects of technology in the next stage of this research project, which involves focus group interviews.

Other specific comments:

- Page 3, line 34: should probably start with: “This review analyzed various workplace...”
  This has been amended.

This review analysed various workplace wellbeing strategies, both individually and organisationally focused for physicians, nurses, and allied healthcare workers.

- Page 22, Table 4: should this perhaps be included in the Results section rather than referenced initially in the Discussion, with perhaps an overall assessment of the results relative to the quality of the studies themselves? You mention your concerns about the quality of the studies later in the last paragraph of Discussion (page 23 lines 34-37), but perhaps it should be called out more in an interpretation of Table 4 (ie I count only 4 studies that have all 3- Large Sample Size + Control Group + Follow up).
  This is a valid suggestion, and the manuscript has been amended accordingly.

It is difficult to compare results to comment on overall impact, therefore this study has created Table 4 to highlight four positive attributes of the studies reviewed, partially informed by the MERSQI guidelines for quality studies. As demonstrated, only four4,12,26,43 studies reported statistically significant positive outcomes whilst implementing a large sample size, control group and post intervention follow up.

- Page 22, Table 4, column “Large Sample Size”, perhaps qualify this with an asterisk for your cutoff, or rename to “Large Sample Size > X”
  This has been amended.

Sample Size >30
• Page 22, Table 4, column “Follow up” – what does that mean?
Not all studies distributed additional follow up surveys beyond the conclusion of the intervention timeframe. This column is for the studies that conducted additional 1-month, 3-month or 6-month post intervention follow ups. The column has now been re-named.
Post Intervention Follow up
• Page 23, line 3, should probably change “deny” to “refute”. I’m also curious how you think the literature can suggest that organizationally focused interventions are more effective (page 22 line 60, and page 23 line 12), yet you only found 3 studies that evaluate this. Is this because this belief is based on theory only, or because studies showing this effect are not published, or perhaps methodologically inferior? See my comments above on the lack of inclusion of other studies focused on organizational—or Efficiency of Practice—interventions.
This phrasing has been amended.
The literature suggests that organisationally focused wellbeing interventions may be more effective in promoting and maintaining worker wellbeing however, with only three studies falling under the organisational umbrella, this review cannot confirm or refute this notion. The belief that organisationally focused interventions are more effective is not solely based in theory. Many non-interventional papers (see below references) as well as the interventional studies included in this review, reiterated this notion. For example:
“Based on the findings of landmark studies, it is clear that working conditions for nurses (including nurse-patient ratio, shift hours, and role strain/ overload) must be improved to effectively prevent burnout.” (Alexander et al. 2015)
“However, these supportive physician directed approaches may be insufficient because they address individual solutions. Burnout more often stems from organizational or system-level factors, and interventions to prevent burnout may be more effective when they focus on changing the system rather than individual physicians.” (DeChant et al. 2019).
The top stressors reported for healthcare workers in 2015 were “increasing workloads, changes to meet the requirements of external bodies, insufficient time to do the job justice, paperwork and increasing patient demand” (Murray et al. 2016). Theoretical frameworks such as the Job-Demands Resources Model (JD-R Model) further recognises the importance of effecting organisational change through adaptation of employee demands (job crafting, reducing workloads) and increasing employee resources (autonomy, control, feedback). Crafting of employee demands and/or resources can improve motivation and reduce job strain. That is not to say that individually focused interventions are not at all effective, but that the overwhelming literature consensus shows organisational interventions are more effective in the long term.
Examples of papers that report the effectiveness of organisational interventions:
• Page 23, line 34: capitalize “However”, and change “there was” to “there were”.
This has been amended.
However, there were limited validation factors to suggest these improvements were the true result of the intervention itself (i.e., comparison against an appropriate control group and follow up conducted beyond the intervention period).
Reviewer: 2
Miss Rebecca Erschens, University Medical Hospital Tuebingen, Internal Medicine
Comments to the Author:
Review on “Workplace interventions to improve wellbeing and reduce burnout for nurses, physicians, and allied healthcare professionals: A Systematic Review”

Thank you for the opportunity to review the manuscript on “Workplace interventions to improve wellbeing and reduce burnout for nurses, physicians, and allied healthcare professionals: A Systematic Review. This review explores the impact of workplace interventions to support well-being and to reduce burnout among health care professionals.

The title is chosen precisely and appropriately, so that the reader directly gains an impression of the content.

This is a well and comprehensibly written paper with high social relevance as the pressure on the health system continues to grow.

Revisions should be made to certain parts, which are explained in more detail below. In addition, the authors are especially advised to check again the tables and figures according to the APA conformity, as there are indications that these are not fulfilled.

Thank you for your time, consideration, and thoughtful feedback.

Title and abstract

The abstract clearly states the intention of the review, and the methodological approach is concisely summarised. In the result section, you should also indicate how many hits the search has achieved in total, so that the reader gets a direct overview of the available research findings.

This information has been added to the results section of the abstract.

Results A total of 1596 articles were screened for eligibility, of which, 33 met the inclusion criterium for this review. The vast majority (30) of these studies utilised individually focused interventions and three were organisational.

In addition, it should be explained what is meant by secondary level interventions, as this will probably not be familiar to every reader.

This has been removed from the abstract to comply with word limits and instead replaced with:

Most of these studies (30) utilised individually focused interventions, only three were organisational. Mindfulness-based education accounted for 20 of these studies, with the remainder utilising meditation, yoga, acupuncture, gratitude journaling, choir singing, professional coaching, and massage. The three organisational studies involved workload reduction, job crafting and peer support network interventions.

Why a meta-analysis could not be carried out is clearly described.

Thank you.

Additionally, in the conclusion section, what is meant by “..do need workplace wellbeing reform”? Please specify.

The conclusion has been rephrased for clarity.

The results of this review suggest that healthcare workers benefited from workplace wellbeing interventions, with a wide array of positive outcomes (improvements in wellbeing, work engagement, quality of life and resilience, as well as reductions in burnout, perceived stress, anxiety, and depressive symptoms) reported. However, many of these reported positive outcomes, were somewhat diluted by the limitations of the various study designs i.e., no control group, utilisation of a waitlist style control group, and/or lack of post intervention follow up surveys.

When discussing the lack of a control group, it should be explained what form of control group would be seen as appropriate.

This has been rephrased for clarity – please see the above text.

Introduction

In general, the introduction is well-written and comprehensible. However, some minor remarks should be made at the point. In line58, the term mental ill-health is used which is uncommon. The authors should revise this an use mental illness / mental disease instead.

The two instances of this have been rephrased to “poor mental health”.

The Australian Institute of Health and Safety estimated in 2019, that worker’s absenteeism due to poor mental health, cost between $13 and $17 billion per year.13 These figures were exponentially compounded throughout the recent Covid-19 pandemic.9,10,14 For example, in a study conducted in
England during the first wave of the Covid-19, sickness absences due to poor mental health in National Health Service (NHS) staff, increased from 519,807 days in March-April of 2019 to 899,730 days 12 months later. For a even more precise introduction to the topic, it could be mentioned to what extent the various interventions are directed at the occupational groups studied in the review, i.e. nurses, doctors, etc., as it might be interesting to see whether different stressors and consequent interventions result for the different occupational groups.

A very interesting point regarding specific interventions directed at the various healthcare occupations. Many of the interventional studies published to date contain populations of mixed healthcare professionals. Some of which specify job roles included and some of which do not. For example, in this review, four out of five studies that specified allied healthcare workers were included, also included nurses and physicians. Therefore, it may not be possible to draw conclusions regarding what types of interventions tend to be selected for different healthcare occupations. Additionally, given the well-documented challenges associated with implementation of an organisational workplace intervention (see comments to reviewer 1) and its consequent underrepresentation throughout the literature, the authors feel that any statements regarding which interventions are more commonly implemented for different occupational groups would not be accurate.

In addition, it should also be stated which reviews and overviews already exist on the topic. A paragraph has been added discussing which reviews already exist on the topic.

To date, a multitude of systematic reviews have investigated the effects of mindfulness-based education or yoga interventions for healthcare professionals in a wide array of contexts. For example, Lomas et al.22 conducted a meta-analysis investigating the impact of mindfulness-based interventions on healthcare workers, Cocchiara et al.29 investigated the use of yoga to manage stress and burnout in healthcare workers and Klein et al.30 investigated the benefits of mindfulness-based interventions on burnout among health professionals. Other systematic reviews have focused on specific populations, for example DeChant et al.31 investigated the effect of organisation-directed workplace interventions on physician burnout and Murray et al.32 investigated interventions to improve the psychological wellbeing of general practitioners. To the authors knowledge, no systematic review has been conducted to provide an overview of all types of wellbeing interventions for allied healthcare professionals including physicians and nurses. As such the search criterium for this review was restricted to 2015 to enable all types of wellbeing interventions for these professional groups to be included in this review.

Methodology
In the methodology section, it should be noted that the full search protocol, including search terms, can be found in the appendix."

This statement has been added.

The full search protocol, including the search terms, can be found in the appendix.

In addition, it should be described here in more detail what the self-developed search strategy looked like, e.g. was the full-text screening carried out by only one or two people? Was a senior researcher also involved in case of uncertainty? This will partly be described in more detail later, but should be noted

This information has been added.

Title and abstract screening as well as full-text screening was carried out by two members of the research team (CC,JC). Where there were disagreements or uncertainty, a third, senior member of the research team was available for resolution if reconciliation sessions were unsuccessful.

Regarding the outcome measures it gets not quite clear to the reader why the search was only started in 2015, please further explain as this might indicate that there is a review published in 2015 already.

Can this statement be supported with literature if necessary?

An additional statement has been added to further explain this at the end of the introduction (see above text) as well as further reiterated in this section. To the authors knowledge, no similar systematic review exists. The statement regarding changes in workplace wellbeing over recent years can be supported with literature. However, this statement has been replaced with the same statement
mentioned at the conclusion of the introduction as this may provide more clarity regarding the year restriction for the reader.

Studies published prior to 2015 were also excluded to ensure all wellbeing intervention types, as well as study populations involving nurses, physicians and allied healthcare professionals were included. The literature research part should be more specific about what the three-step approach looked like. This has been added.

Five healthcare/medicine databases (CINAHL, Embase, Emcare, Medline, PsycInfo) were subsequently searched on 02.05.2022 and again on the 05.10.2022 using a three-step (PIO: Population, Intervention, Outcome) systematic approach (see inclusion criteria for details). Furthermore, the figures should be labelled in accordance with the APA (using headings and significant subtitles).

The headings have been amended.

In Table 1 in particular, it should be described in more detail how the evaluation is composed and what the abbreviations and the respective evaluations mean and what range is used for the respective scales. This would make it easier for the reader to see the quality of the included studies. The APA conformity of the table should also be checked here, as there are indications that this has not been complied with.

The table has been amended for easier interpretation by the reader. Scoring of the individual categories was performed using the MERSQI quality assessment guidelines. See “Cook D, Reed D. Appraising the quality of medical education research methods: The medical education research quality instrument and the Newcastle-ottawa scale-education. Academic Medicine. 2015;90(8):1067-1076.”

Note. Study Design refers to the number and randomisation of participant groups. Sampling Institutions refers to the number institutions participants originate from. Response Rate refers to attrition levels. Date type refers to whether the data collected was objective (observer ratings are considered objective). Validity refers to the type of evidence reported taking into consideration: content evidence (using theory, frameworks, guidelines, experts, or existing instruments), internal structure (reliability of data, consistency, test, and re-test) and relationships to other variables (expert or novice comparisons and concurrent or predictive correlation with other variables). Sophist. Refers to sophistication of data analysis. Approp. Refers to appropriateness of data analysis with 1 point given for appropriate analysis conducted for the study type. Outcome refers to whether the intervention outcomes had actual effects on real patients, programs, or society.

The summary of the characteristics of the studies is clear and gives the reader a good summary of the included studies at first glance. However, when describing the outcome measures, it should be added which dependent variables were focused on, i.e. which forms of mental health / wellbeing outcomes were potentially of interest to the authors. A statement has been added addressing this.

All outcome measures relating to wellbeing or burnout were evaluated for this review, however burnout, perceived stress, depression, and anxiety as well as resilience, quality of life, and work engagement levels were of particular interest to the authors.

A similar comment should be made for table 2. the table seems logical, relevant aspects of the interventions are reported. However, the areas explained in this table should be described in more detail below the graph. In addition, the content of the "focus of intervention" section should be shortened. The headings of tables 2 and 3 should be chosen in such a way that it is immediately clear to the reader what content they contain and for what purpose (e.g. the general overview and the detailed breakdown of outcomes), currently it is not obvious at first glance.

Thank you for pointing this out. The “focus of intervention” column has been shortened, and the headings of these tables have been rephrased for clarity. A more thorough description of the columns has been added below the table as well where necessary.

Note. Level of wellbeing intervention refers to the level at which the intervention was targeted i.e. primary, secondary, or tertiary. Target of behavioural change refers to the level at which interventions target behavioural changes i.e. individual or organisation. CD (Compact Disc), FU (Follow up in
addition to post intervention surveys), MBSR (Mindfulness-Based Stress Reduction), MMC (Mindful Medicine Curriculum), n (number of participants), NADA (National Acupuncture Detoxification Association), SMART (a) awareness of neural predispositions to stress, b) attention training to improve the depth and intentionality of attention and c) learning 5 core principles to enhance emotional resiliency (gratitude, compassion, acceptance, meaning and forgiveness).

The overview in table 4 is well chosen and gives a good overview of the content and quality of the study. In due course, it should be described why exactly these outcome categories were chosen.

A sentence has been added to explain that these attributes were partially informed by the quality assessment tool (MERSQI).

It is difficult to compare results to comment on overall impact, therefore this study has created Table 4 to highlight four positive attributes of the studies reviewed, partially informed by the MERSQI guidelines for quality studies. As demonstrated, only four 4, 12, 26, 43 studies reported statistically significant positive outcomes whilst implementing a large sample size, control group and post intervention follow up.

Conclusion
In the conclusions, the remarks made earlier in the comments on the abstract should be taken into account.

This has been amended to be consistent with the abstract.

The results of this review suggest that healthcare workers benefited from workplace wellbeing interventions, with a wide array of positive outcomes (improvements in wellbeing, work engagement, quality of life and mindfulness as well as reductions in burnout, perceived stress, anxiety, and depressive symptoms) reported. Relaxation techniques targeted at the secondary interventional level (designed to manage stress in the individual worker) was the predominant wellbeing strategy of choice. The literature suggests this is due to the feasibility of implementing such a study. Reported positive outcomes included: improvement in emotional exhaustion, resilience, mindfulness, wellbeing, quality of life, and work engagement and/or reduction in burnout, perceived stress, anxiety, and depressive symptoms. However, many of these reported positive outcomes, were somewhat diluted by the limitations of the various study designs i.e. no control group, utilisation of a waitlist style control group, and/or lack of post intervention follow up surveys. As such, more research utilising robust interventional study designs, such as randomised controlled trials, is warranted.

I wish you all the best with your revision
Thank you again for your time, consideration, and very thoughtful feedback.

Reviewer: 1
Competing interests of Reviewer: None

Reviewer: 2
Competing interests of Reviewer: I have no competing interests

VERSION 2 – REVIEW

| REVIEWER | Micek, Mark A.  
| University of Wisconsin-Madison School of Medicine and Public Health |
| REVIEW RETURNED | 02-Mar-2023 |

GENERAL COMMENTS
I appreciate the authors’ thoughtful response to the reviewer comments.

I also appreciate the additional detail about the criteria used to select studies for inclusion, which perhaps explains why many organizationally-focused studies that attempted to improve workload were not included in the review as they focused on other
facets of physician satisfaction (ie with the EHR, for example). I still note that the authors, in their response as well as in their manuscript, cite several papers (most notably DeChant et al) which reference organizationally-focused studies that seemingly would fit the criteria for this study, and I’m not clear why they were not captured in this review and it may be worthwhile to state why. As an example, the following 2 articles used interventions to improve clinician wellness and included validated measures of burnout; while there are other limitations (low n, lack of control groups), this was not dissimilar to other studies included in the manuscript.

- Pozdnyakova A, et al, Impact of Medical Scribes on Physician and Patient Satisfaction in Primary Care. JGIM 2018; 33(7):1109-15. This study included only 6 physicians but used a validated single-item burnout measure (adapted from the Mini-Z).
- Sieja A et al, Optimization Sprints: Improving Clinician Satisfaction and Teamwork by Rapidly Reducing Electronic Health Record Burden. Mayo Clin Proc. 2019;94(5):793-802. This study included 220 clinicians and used questions from the “emotional exhaustion domain” of the MBI.

Incidentally, our recent publication evaluating the effect of remote scribes in primary care- which had a control group and used several validated measures of burnout- might have qualified for the review but was published outside of the time window (Micek MA et al, Healthcare, Dec 2022).

One small suggestion for wording: page 10 line 40: “Because it is difficult to compare results to determine the overall impact of these studies, Supplementary Table 4 was created to highlight…”

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**REVIEWER**  
Erschens, Rebecca  
University Medical Hospital Tuebingen, Internal Medicine, Department of Psychosomatic Medicine and Psychotherapy

**REVIEW RETURNED**  
07-Mar-2023

**GENERAL COMMENTS**  
Thank you for the opportunity to re-review the manuscript ‘Workplace interventions to improve wellbeing and reduce burnout for nurses, physicians, and allied healthcare professionals: A Systematic Review’. The aim of this review is to present the current research since 2015 on workplace interventions to prevent burnout among health workers and improve their well-being. The last amendments noted have been implemented by the authors. We recommend that the authors revise the manuscript in accordance with the guidelines set out below, especially the discussion should be revised using appropriate literature.

**Title and abstract**

The abstract has been changed according to the editor’s and the reviewer’s comments. Also, the description “secondary interventions” has been changed and the whole abstract was revised.

The title of the paper is now concise and gives the reader a direct idea of the content of the review. Also, the abstract is coherent in content and contains the most important information for the review.
The authors could preface it with a sentence on the relevance or theoretical background and outline briefly why this topic is relevant to this target population. In addition, the outcomes recorded should be described in brief.

Introduction
The last comments on the introduction were fully implemented, rewordings were made. The other comments regarding the specificity of the interventions for the individual groups were sufficiently and logically addressed. Also, the explanation of previous reviews has been appropriately revised only, it still is not sufficiently clear why exactly the search criterion was set to 2015, this should be explained again in more detail by the authors.

Methodology
Also, the comments in the methods section regarding search protocol and search strategy were fully implemented. Also, headings have been added if necessary. The revised method section is now comprehensive and well written in terms of content and form. However, some changes/adjustments should still be made by the authors: The hand search using google scholar described later should also be described in the methods section.

Results
The result section has also been fully revised in accordance with the latest comments. The tables have been changed significantly according to the last comment and according to the APA-guidelines, as columns were shortened and notes were added. Also, the assignment of outcomes was made by the authors and the annotation regarding MERSQI was added. In overall terms, the report on outcomes is now more logical, and the division into individual and organisation-based interventions makes sense and gives the reader a comprehensive picture of the studies found. However, the first part on the general results should be introduced briefly, the authors could also integrate subheadings (country of publication, etc) for better clarity. On page 9, line 56 (promoting a positive outlook), the authors should briefly explain why, according to their own judgement, exactly these interventions have been summarised.

Discussion
Also, the comments on the control group (isb on the use of a control group) were also fully implemented. The discussion now succinctly summarises the results reported in detail above and contrasts them with the existing literature. However, at the beginning of the discussion section, reference should be made once again to the overview of the MERSQI rating in the appendix. From line 24 onwards, attention should again be paid to the need to substantiate the statements with appropriate literature (e.g. after line 25 on page 12). It would also be interesting to discuss the sustainability of the interventions at this point, i.e. which interventions/forms of delivery were measured at follow-up time points and which types of interventions show long-term effects. This could also provide the basis for further discussion points and for the meaningful implementation of interventions.
Conclusions and limitations

The comments on the conclusion previously made by the reviewer have been fully implemented and adapted to the revised abstract. Yet, the authors should further address how, despite the limited significance of the results, institutions can implement the interventions mentioned and what steps are needed, e.g. also on the part of employers, to enable this on a broad scale. In the limitations, the authors should once again point out the pronounced heterogeneity at the different levels, which complicates not only theoretical but also practical implementations.

I wish the authors much success with the minor changes that still could be implemented.

VERSIO 2 – AUTHOR RESPONSE

Reviewer: 1

Dr. Mark A. Micek, University of Wisconsin-Madison School of Medicine and Public Health

Comments to the Author:

I appreciate the authors’ thoughtful response to the reviewer comments.

Thank you for your time and consideration.

I also appreciate the additional detail about the criteria used to select studies for inclusion, which perhaps explains why many organizationally-focused studies that attempted to improve workload were not included in the review as they focused on other facets of physician satisfaction (ie with the EHR, for example). I still note that the authors, in their response as well as in their manuscript, cite several papers (most notably DeChan et al) which reference organizationally-focused studies that seemingly would fit the criteria for this study, and I’m not clear why they were not captured in this review and it may be worthwhile to state why. As an example, the following 2 articles used interventions to improve clinician wellness and included validated measures of burnout; while there are other limitations (low n, lack of control groups), this was not dissimilar to other studies included in the manuscript.

Thank you for pointing out that the inclusion criteria for this review requires further clarification. Papers that have investigated the use of medical scribes or improving the use of the EHR, were most commonly excluded because they did not investigate burnout or wellbeing of participants as the primary study objective. For example, Pozdynakova et al. implemented a 21-item pre- and 44-item post-pilot physician survey, as well as a 27-item patient satisfaction survey, of which only one question was relating to physician burnout.

The papers included in this review have all implemented studies where the primary objective of the intervention was to improve burnout and wellbeing of participants and as such issued multiple, validated complete burnout, and wellbeing survey inventories.

Sieja et al. also investigated clinicians’ satisfaction with the intervention, satisfaction with the EHR before and after the sprint intervention, as well as asking participants to assess their own personal EHR clinical processes before and after the intervention and lastly asked participants to complete the Emotional Exhaustion domain of the Maslach Burnout Inventory. This specific paper therefore cannot report burnout levels of participants but only changes in emotional exhaustion levels before and after
the intervention. Other papers in the DeChant et al. systematic review were excluded for a variety of reasons, such as: study outcomes were not reported, immediate post intervention surveys were not distributed, published outside of the timeframe for this review, or scored too low on the MERSQI quality assessment.

To avoid any confusion however, as no paper involving medical scribes or the EHR met the inclusion criterion for this review, the authors will amend the exclusion criteria to state that these papers were also excluded.

Studies that implemented interventions involving the use of medical scribes or electronic health record (EHR) were also excluded from this review.

• Pozdnyakova A, et al, Impact of Medical Scribes on Physician and Patient Satisfaction in Primary Care. JGIM 2018; 33(7):1109-15. This study included only 6 physicians but used a validated single-item burnout measure (adapted from the Mini-Z).

• Sieja A et al, Optimization Sprints: Improving Clinician Satisfaction and Teamwork by Rapidly Reducing Electronic Health Record Burden. Mayo Clin Proc. 2019;94(5):793-802. This study included 220 clinicians and used questions from the “emotional exhaustion domain” of the MBI.

Incidentally, our recent publication evaluating the effect of remote scribes in primary care- which had a control group and used several validated measures of burnout- might have qualified for the review but was published outside of the time window (Micek MA et al, Healthcare, Dec 2022).

Thank you for sharing, it was a very interesting read!

One small suggestion for wording: page 10 line 40: “Because it is difficult to compare results to determine the overall impact of these studies, Supplementary Table 4 was created to highlight…”

Thank you. This phrasing has now been amended.

As it is difficult to compare results to determine the overall impact of these studies, Table 4 was created to highlight four positive attributes of the studies reviewed, partially informed by the MERSQI guidelines for quality studies.

Reviewer: 2

Miss Rebecca Erschens, University Medical Hospital Tuebingen, Internal Medicine

Comments to the Author:

Thank you for the opportunity to re-review the manuscript ‘Workplace interventions to improve wellbeing and reduce burnout for nurses, physicians, and allied healthcare professionals: A Systematic Review’.

Thank you for your time and consideration.

The aim of this review is to present the current research since 2015 on workplace interventions to prevent burnout among health workers and improve their well-being. The last amendments noted have been implemented by the authors. We recommend that the authors revise the manuscript in accordance with the guidelines set out below, especially the discussion should be revised using appropriate literature.

Title and abstract
The abstract has been changed according to the editor's and the reviewer's comments. Also, the description "secondary interventions" has been changed and the whole abstract was revised.

The title of the paper is now concise and gives the reader a direct idea of the content of the review. Also, the abstract is coherent in content and contains the most important information for the review. The authors could preface it with a sentence on the relevance or theoretical background and outline briefly why this topic is relevant to this target population. In addition, the outcomes recorded should be described in brief.

Thank you. A statement outlining the relevance of this topic has been added. The abstract has also been completely re-worked in order to add the requested information and stay within the 300-word limit.

The sentence on recorded outcomes has also been re-phrased for further clarity.

There is a growing need for interventions to improve wellbeing in healthcare workers, particularly since the onset of COVID-19.

Objectives: To synthesise evidence since 2015 on the impact of interventions designed to address wellbeing and burnout in physicians, nurses, and allied healthcare professionals.

Design: Systematic literature review.

Data sources: Medline, Embase, Emcare, CINAHL, PsycInfo and Google Scholar were searched in May-October 2022.

Eligibility criteria: Studies that primarily investigated burnout and/or wellbeing and reported quantifiable pre- and post- intervention outcomes using validated wellbeing measures were included. Studies involving medical scribes or the electronic health record were excluded.

Data extraction and synthesis: Full-text articles in English were independently screened and quality assessed by two researchers using the Medical Education Research Study Quality Instrument (MERSQI). Results were synthesised and presented in both quantitative and narrative formats. Meta-analysis was not possible due to variations in study designs and outcomes.

Results: A total of 1,663 articles were screened for eligibility, with 33 meeting inclusion criteria. Thirty studies utilised individually focused interventions, whilst three were organisationally focused. Thirty-one studies utilised secondary level interventions (managed stress in individuals) and two were primary level (eliminated stress causes). Mindfulness-based practices were adopted in 20studies; the remainder utilised meditation, yoga, and acupuncture. Other interventions promoted a positive mindset (gratitude journaling, choirs, coaching) whilst organisational interventions centred on workload reduction, job crafting and peer networks. Effective outcomes were reported in 29 studies, with significant improvements in wellbeing, work engagement, quality of life and resilience, and reductions in burnout, perceived stress, anxiety, and depression.

Conclusion: The review found that interventions benefitted healthcare workers by increasing wellbeing, engagement and resilience, and reducing burnout. It is noted that the outcomes of numerous studies were impacted by design limitations i.e., no control/waitlist control group, and/or no post intervention follow-up. Suggestions are made for future research.

Introduction

The last comments on the introduction were fully implemented, rewordings were made. the other comments regarding the specificity of the interventions for the individual groups were sufficiently and logically addressed. Also, the explanation of previous reviews has been appropriately revised only, it
still is not sufficiently clear why exactly the search criterion was set to 2015, this should be explained again in more detail by the authors.

Thank you. A further statement explaining why the search criteria was set to 2015 has been added. The authors wished to review the latest research pertaining to healthcare worker wellbeing, however did not want to heavily bias the review to studies published since Covid-19 and therefore opted to include 5 years prior to Covid-19.

The authors also acknowledge the effects that Covid-19 has had on healthcare practices as well as healthcare worker mental wellbeing. Therefore, in order to include all above-stated professional groups and types of interventions in this review, as well as sampling the most recent research (including five years prior to the onset of Covid-19), the search criterion for this review was restricted to 2015.

Methodology

Also, the comments in the methods section regarding search protocol and search strategy were fully implemented. Also, headings have been added if necessary. The revised method section is now comprehensive and well written in terms of content and form. However, some changes/adjustments should still be made by the authors: The hand search using google scholar described later should also be described in the methods section.

Thank you. This statement appears in the literature review sub-heading for the methodology.

A secondary search of Google Scholar was also performed with results limited to the first five pages of articles (n=50).

Results

The result section has also been fully revised in accordance with the latest comments. The tables have been changed again significantly according to the last comment and according to the APA-guidelines, as columns were shortened and notes were added. Also, the assignment of outcomes was made by the authors and the annotation regarding MERSQI was added.

In overall terms, the report on outcomes is now more logical, and the division into individual and organisation-based interventions makes sense and gives the reader a comprehensive picture of the studies found. However, the first part on the general results should be introduced briefly, the authors could also integrate sub-headings (country of publication, etc) for better clarity.

Thank you. A sentence incorporating the sub-headings of Table 2 has now been added.

Details pertaining to the wellbeing interventions analysed in this review, including sample locations, wellbeing intervention levels, behavioural change targets, attrition rates and focus of the interventions are summarised in Table 2.

On page 9, line 56 (promoting a positive outlook), the authors should briefly explain why, according to their own judgement, exactly these interventions have been summarised.

Thank you. The following has been added to explain this particular summary and the phrasing has been amended to “promoting positive mindsets”.

Gratitude journaling interventions are designed to shift thoughts away from the negativity of stress and towards a more positive primary appraisal of a situation. Similarly, choral singing research has shown that participants who regularly engage, may experience cognitive benefits through the addition of social routine and meaningful activity to everyday life. The professional coaching intervention
was also specifically implemented for participants to design and achieve goals, thereby strengthening personal resilience and mindset positivity47.

Discussion

Also, the comments on the control group (isb on the use of a control group) were also fully implemented. The discussion now succinctly summarises the results reported in detail above and contrasts them with the existing literature. However, at the beginning of the discussion section, reference should be made once again to the overview of the MERSQI rating in the appendix.

Thank you. Reference to the MERSQI ratings has now been added to the discussion.

Overall, the studies included in this review scored moderate-highly on the MERSQI rating system yet were markedly heterogeneous pertaining to intervention types, study designs, outcome measures and sample sizes implemented.

From line 24 onwards, attention should again be paid to the need to substantiate the statements with appropriate literature (e.g. after line 25 on page 12).

Thank you. Reference to the appropriate literature has been added to this section to substantiate the statements made.

Many of the studies in this review that implemented MBP interventions reported reductions in participant levels of burnout, perceived stress, and anxiety as well as increased resiliency. However, there were limited validation factors implemented to suggest these improvements were the true result of the intervention itself (i.e., comparison against an appropriate control group and follow up conducted beyond the intervention period). Multiple systematic reviews conducted by Lomas et al.22,23 achieved similar results with overall study findings reporting that mindfulness was generally associated with positive outcomes in relation to most measures, however the quality of assessed studies was inconsistent, especially noting the lack of RCT study designs that were eligible for inclusion. Kriakous et al.24 also found that mindfulness based stress reduction techniques were effective in reducing healthcare professionals’ experiences of anxiety, depression and stress but were less effective at reducing burnout or improving resiliency.

It would also be interesting to discuss the sustainability of the interventions at this point, i.e. which interventions/forms of delivery were measured at follow-up time points and which types of interventions show long-term effects. This could also provide the basis for further discussion points and for the meaningful implementation of interventions.

Thank you. This is a great suggestion and has been added to the discussion accordingly.

Mindfulness based education interventions can be implemented quite sustainably as they are generally performed in the workplace (during scheduled breaks) or through self-directed practices.22-24 Interventions that do not greatly disrupt daily productivity are more easily implemented for longer time periods and make longer term follow-up assessments more achievable.30-32 Despite this, most studies (n=20) in this review did not collect post-intervention follow-up data and only one57 study conducted a long-term follow up (12-months post the gratitude intervention), in which reported improvements were sustained. It is therefore difficult to suggest which interventions show long-term benefits and therefore may be more effective. With the acknowledgment that healthcare workers face especially difficult psychosocial hazards in the workplace, this review suggests that healthcare employees are likely to benefit from a stronger focus on wellbeing promotion in the workplace. However, with such a large volume of studies attempting to relieve symptoms of burnout by encouraging new health behaviours in employees themselves, rather than affecting change at an institutional level, further interventional research is demanded. Particularly in the form of RCTs with adequate long-term follow up data collection.
Conclusions and limitations

The comments on the conclusion previously made by the reviewer have been fully implemented and adapted to the revised abstract. Yet, the authors should further address how, despite the limited significance of the results, institutions can implement the interventions mentioned and what steps are needed, e.g. also on the part of employers, to enable this on a broad scale.

Thank you. A paragraph discussing these points has been added to the conclusion.

Despite this, healthcare institutions can implement interventions pertaining to mindfulness-based education interventions, promoting positive mindsets or organisational changes that are shown to illicit positive results in the wellbeing of their staff. Employers need to address the bigger picture for healthcare worker wellbeing. A short-term inconvenience, can cultivate an environment conducive to improved wellbeing and reduced burnout, thereby fostering long-term productivity and retention of staff.

In the limitations, the authors should once again point out the pronounced heterogeneity at the different levels, which complicates not only theoretical but also practical implementations.

Thank you. This statement has been added to the limitations.

Theoretical and practical implementations of the results are therefore complicated by the marked heterogeneity in intervention types, intervention timeframes, participant groups and conduction of post-intervention follow ups.

I wish the authors much success with the minor changes that still could be implemented.

Many thanks again.

Reviewer: 1

Competing interests of Reviewer: None

Reviewer: 2

Competing interests of Reviewer: i have no competing interest.

VERSION 3 – REVIEW

<table>
<thead>
<tr>
<th>REVIEWER</th>
<th>Micek, Mark A.</th>
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<tr>
<td>University of Wisconsin-Madison School of Medicine and Public Health</td>
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<tr>
<td>REVIEW RETURNED</td>
<td>18-May-2023</td>
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<tr>
<td>GENERAL COMMENTS</td>
<td>Thank you again for your thoughtful response to the additional reviewers’ comments. I believe your response adequately addressed why some of the studies that used organizational strategies, including scribes, were excluded—ie because you interpreted that they did not have burnout as a primary objective or measure it in a standardized or comprehensive way. You have</td>
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also made changes to the verbiage around these additional sentences to make it clearer that you only included studies that primarily investigated burnout and/or wellbeing with specific objective measures (ie Page 4, lines 12-13 and Page 6, lines 48-49). In that light, the additional sentences stating you excluded studies involving scribes or the electronic health record now read (to me) as though you simply excluded them because of their content, which I don’t believe was the case. To rectify this, you could either delete those 2 sentences in the abstract (ie Page 3, line 15) and methods (Page 5, line 50), and instead mention this as a limitation. For example, you could add a sentence to your limitations section Page 12 line 31 after you mention that the review had a lack of organizational studies that met inclusion criteria, ie something like “For example, several studies that implemented interventions involving the use of medical scribes or the electronic health record (EHR) were also excluded from this review as they did not focus on wellbeing as a primary outcome or did not use validated measures of wellbeing.”

VERSION 3 – AUTHOR RESPONSE

Reviewer: 1
Dr. Mark A. Micek, University of Wisconsin-Madison School of Medicine and Public Health
Comments to the Author:
Thank you again for your thoughtful response to the additional reviewers’ comments. I believe your response adequately addressed why some of the studies that used organizational strategies, including scribes, were excluded—ie because you interpreted that they did not have burnout as a primary objective or measure it in a standardized or comprehensive way. You have also made changes to the verbiage around these additional sentences to make it clearer that you only included studies that primarily investigated burnout and/or wellbeing with specific objective measures (ie Page 4, lines 12-13 and Page 6, lines 48-49). In that light, the additional sentences stating you excluded studies involving scribes or the electronic health record now read (to me) as though you simply excluded them because of their content, which I don’t believe was the case. Thank you, the authors do agree with this thought as yes, the original intent was not to exclude these studies from the outset.
To rectify this, you could either delete those 2 sentences in the abstract (ie Page 3, line 15) and methods (Page 5, line 50), and instead mention this as a limitation. For example, you could add a sentence to your limitations section Page 12 line 31 after you mention that the review had a lack of organizational studies that met inclusion criteria, ie something like “For example, several studies that implemented interventions involving the use of medical scribes or the electronic health record (EHR) were also excluded from this review as they did not focus on wellbeing as a primary outcome or did not use validated measures of wellbeing.”

Thank you very much. The authors agree that removing those statements from the abstract and the methods section and instead adding them to the limitations section as a discussion point, does align more authentically to the process of how articles were selected and excluded. The limitations section has now been corrected to reflect this:
This review was also limited by the lack of organisational studies that met the inclusion criteria. For example, several studies that implemented interventions involving the use of medical scribes or the electronic health record (EHR) were excluded from this review as they did not focus on wellbeing as a primary outcome or did not use validated measures of wellbeing.

Thank you very much, the authors are exceptionally pleased with how the paper has been shaped following the invaluable input from the editors and reviewers.
Reviewer: 1
Competing interests of Reviewer: None.