Author’s response to reviews

Title: The effectiveness of interventions targeting mental illness stigma in the workplace: A systematic review

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Author’s response to reviews: see over
Dear Dr Sean Lynch,

Enclosed is the revision to our manuscript titled “The effectiveness of interventions targeting mental illness stigma in the workplace: A systematic review” (4108992171628345).

My co-authors and I thank you and your reviewers for the insightful comments and suggestions in the review. We have paid close attention to the comments which, we believe, helped to strengthen the manuscript significantly. We believe all the reviewers’ comments were addressed and our replies are attached below.

Feel free to contact me if you have any questions or additional concerns. We thank you for your support of this paper and look forward to your response.

With best regards

Sabine Hanisch
Replies to Reviewers' Comments:

Reviewer: 1

R1.1 The paper would benefit from a fuller discussion of the concept of stigma and its dimensions, in order to appreciate the approach to data collection and measurement that follows. For example, it is stated that this review aims to provide a first systematic review on the effectiveness of workplace anti-stigma interventions by examining changes in: (1) knowledge of mental disorders and their treatment and recognition of signs/symptoms of mental ill health (2) attitudes towards people with mental health problems and (3) help-related behaviors. A clear explication of how these variables comprise the construct of stigma is needed, based on research and theory that has been developed around stigma to date. The authors cite research that relates to lack of knowledge and poor help-seeking behaviors, but a fuller conceptual discussion of stigma is warranted.

We thank the reviewer for making this point as it helped us to realize that we need to elaborate on the concept of stigma and especially its dimensions. We strongly agree that this will facilitate the understanding for the approach we adopted in this paper in terms of structure, data analysis and evaluation. To address this issue, we have rearranged the introduction and added small parts on each dimension (see lines 84-95 and 149-152).

R1.2 On a related note, the authors state that the anti-stigma intervention evaluated most commonly in the workplace was Mental Health First Aid (MHFA) training. MHFA is most often seen to be a mental health literacy program. An explanation of how programs were identified as anti-stigma programs would be useful.

We made a special remark as to how programs were identified as anti-stigma programs in the Methods section. Accordingly, any intervention that targeted at least one dimension of stigma (knowledge and/or attitude and/or behavior) was included even though it couldn’t be considered an anti-stigma program per se. Moreover, we emphasized this point again in the Results section with regards to the Mental Health First Aid training as requested (see line 178-180 and 292-295).

R1.3 The same holds true of outcomes; 11 studies targeted knowledge, including a) the identification of mental health problems and b) knowledge about effective treatments. A prior discussion of knowledge as a dimension of stigma would enable a fuller appreciation of knowledge of mental health problems and treatments is an indicator of stigma.

Please see response to R1.1. In the introduction, we have explained the link between knowledge of mental health problems and stigma as well as to help-seeking.

R1.4 The Discussion also states that the current review supports the notion that attitudinal change is neither required nor sufficient for a change in behavior. How was this conclusion reached?

Thank you for pointing out the necessity to refer to concrete findings of included studies in our review to explain how this conclusion was reached. While checking the achieved outcomes of individual studies again, we noticed a small mistake and could only find evidence for attitudinal change not being required, however not any for it to be not sufficient. Therefore, we have changed the passage accordingly (see line 420-424).
R1.5 Methods:
Was there a particular model followed for this systematic review?
It is not clear why the review excluded longitudinal studies, cohort studies, primary prevention studies, phase I and II study, ecologic studies, case reports, case series, cross-sectional studies, qualitative and economic evaluations, particularly because a meta-analysis was not conducted and a narrative synthesis approach was used.

This review followed several models:
- Conceptual framework of stigma as proposed by Thornicroft et al. (2007) (line 78-81)
- PRISMA guidelines in terms of reporting study selection (line 270)
- NICE guidelines (checklist) to check the quality of included studies(line 250-253)
- Cochrane review’s characteristics of included studies for a summary of extracted information (line 265-268)
- The approach adopted by Popay et al. (2006) for a narrative synthesis (please refer to R1.13, line 258-264)

Moreover, we have included an explanation of why those study designs were excluded (line 162-165). For a detailed description of eligibility criteria, we kindly ask the reader to refer to Additional File 1 in the Appendix.

R1.6 What is the rationale for choosing papers in English, German, Spanish or Portuguese?

We chose to include papers written in English, German, Spanish or Portuguese because those are the languages we are able to read in our working group (line 188-189)

Discretionary Revisions:

R1.7 With regard to attitudes, the authors report that one study examined specific attitudes related to perceived dangerousness, unpredictability and recovery of mentally ill individuals. All of these might be more accurately defined as beliefs, rather than attitudes. It would be interesting to hear more about specific attitudinal changes.

Thank you very much for raising this point. We agree that it would be very interesting to know more about changes in specific attitudes and therefore added a sentence to address this point (line 354-356). While we completely agree with the reviewer that all of these specific attitudes might be more accurately defined as beliefs, we decided to adhere to the authors’ definition in the original study. Nevertheless, we did make a remark in the introduction that public beliefs next to negative attitudes, thoughts and emotions are included in the prejudice dimension of stigma in this review (please refer to R1.1).

R1.8 Similarly, additional detail on behavior change would be useful. We are told that measures indicated that employees’ helping behavior improved, but what did this entail?

We rearranged and extended the paragraph on behavioral outcomes to make it more clear what behavioral change entailed specifically (see line 358/9 and 363-367).

R1.9 The Discussion addresses the spillover effect that was found but this is not reported in the results section, and should be.

We addressed this finding in a paragraph in the result section just above secondary outcomes (see line 368-371).
If MHFA showed similarly positive results in the general public (as described in the Discussion), why are targeted workplace approaches needed? The rationale for targeted approaches needs to be revisited in light of this discussion.

*We thank the reviewer for raising this point. We strongly agree that the need for targeted approaches such as workplace interventions as opposed to public programs needs to be emphasized. To address this point, we have included a paragraph in the discussion section (see line 400-411).*

**R1.11 Methods:**
Grey literature was searched and 3 unpublished studies were included. Were they checked for quality in the same way as the other peer-reviewed papers or was there additional quality assurance involved?

*Yes, they were checked in the same way as other included studies and we have now mentioned that in the text (line 250).*

*We agree that drug use is widely stigmatized in society and that stigma programs aiming to reduce drug use-related stigma are important. However, we did not end up including those programs from this review for the following reasons and emphasized those points in the text (see line 204):
(1) In this review we wanted to focus on interventions with a broader scope (e.g. focusing on general mental health problems). An extensive amount of literature evaluates interventions aiming to reduce substance abuse (e.g. smoking cessation etc.) and were very specific in nature. And maybe more importantly, (2) drug-use related anti-stigma interventions that are currently reported (e.g. Livingston, J. D., Milne, T., Fang, M. L., & Amari, E. (2012). The effectiveness of interventions for reducing stigma related to substance use disorders: a systematic review. *Addiction, 107*(1), 39-50.) seem to focus exclusively on health care providers or medical students - a target group which we chose to exclude from our review.
(3) Or the studies we found did not provide quantitative data.*

**R1.12** A flow chart illustrating the search strategy and selection process would be helpful. The review would be enhanced by a fuller description of the narrative synthesis approach used.

*We believe, a flow chart based on PRISMA guidelines which illustrates the search strategy (please see Additional File 1) and selection process (see Figure 1 in Additional File 2) is provided.*

*We appreciate the reviewer’s comment with regards to a fuller description of the narrative synthesis approach used and have addressed this issue accordingly under ‘data analyses’ in the Methods section (line 260-264).*

**Reviewer: 2**

*I would put the search strategy information in a Table. The narrative text disrupted the flow of the manuscript.*
We appreciate the comment of the reviewer. The full search strategy is provided in a Table in Additional File 1 and repeated in text but not in full detail. We believe, this facilitates the understanding of the approach adopted and would like to adhere to this standard.

**R2.2** The authors should go into more detail regarding the checklist that was used to assess Study Quality. Doing so would prepare the reader for the results involving the detection of different biases (detection, selection, attrition, information). The assessment of the different studies on these dimensions is a strength of the manuscript, so more detail on this assessment is needed.

We strongly agree that emphasizing how exactly the studies were assessed in terms of quality strengthens the findings of this review. Therefore, we have added a sentence on criteria of the checklist that was used for assessment (see line 251-253).

**R2.3** In the section on Sustainability of Change, the authors note that of the five studies that included a post-intervention follow-up, changes in knowledge, attitude, and behavior were, in part, sustained over time. I recommend the authors go into more detail regarding these five studies. Did all of them show sustained changes? Were the changes greater for certain outcomes than others? Given these five studies included the methodologically desirable attribute of following people over time, we need a detailed analysis of the studies to guide future research in the area.

Thank you for making this point. We addressed your questions in a couple of sentences under the section ‘Sustainability of Change’ (see line 380-384). Yes, all of those studies showed sustained changes. If an intervention succeeded in the first place to achieve a change in any outcome measure, this change was maintained to a great extent at follow-up. Therefore, it is difficult to answer your second question if there were greater changes for certain outcomes. Especially since not all studies targeted the same outcomes of stigma. As already mentioned, this review showed mixed results for attitudinal change, whereas positive improvements overall were found for knowledge and behavior (line 414).

**R2.4** Finally, I think Table 1 could be structured to better convey the effects of the interventions in the different studies. I know this is a narrative synthesis, but it would still be useful to have some index of the size of the effects in the 16 studies on the three primary outcomes. Right now only a crude indication of whether the effect was significant is provided. Some estimate of effect size would be desirable.

We agree that it would make the manuscript more interesting if some index on effect sizes on primary outcomes was presented. For the majority of studies, no effect sizes were available, however, we did include those we could find in a table to be found in supplementary material (please refer to Additional File 4). We refer to this in Methods under ‘data extraction’ (line 247) as well as in Results under ‘Effectiveness of anti-stigma interventions’ (line 324).

**R2.5** Some references are incomplete

Thank you for pointing this out. We have revised these errors throughout.

*Editorial request:

Copyediting:
As requested by the editor, this review was copy-edited.