Reviewer’s report

Title: Implicit Bias in Healthcare Professionals: A Systematic Review

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Reviewer: Stephen Smith

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Review of Implicit Bias in Healthcare Professionals: A Systematic Review

In general, I think the article is of sufficient quality for inclusion after some corrections have been made. In my view, I think these corrections are probably best classified as minor although if it is decided that there requisite changes would take longer than that specified by 'minor corrections' they should be classified as major.

The more substantial corrections deal with the introduction and the discussion section. I felt that both were a bit light. For example, in the introduction, more explanation is probably needed on page 3 at line 53. It is not clear to me who constitute 'people' in that context - is it everyone in all places? Limited to a particular geographic location, etc. This is worth clarifying because one of the benefits of the review is that it includes non-US studies. However, the reason given as to why this should be preferred is because of the understandable focus in the US on race. If, however, the US is not different from other countries on that basis, then there is little benefit on that score to looking more broadly. The idea of 'corrosive disadvantage' in page 4 (line 32-34) also requires further explanation. I believe I probably agree with the authors on its importance but they need to do a better job showing it.

The discussion section later on also requires further exploration. Indeed, at the moment, its place in the article is a little unclear. It doesn't quite replicate the results section but it also doesn't add a lot of new information or insight either. I would have thought the best use of the discussion section would be to synthesize themes across the studies. It does a bit of this but other important ideas (e.g. the intersectionality one on page 12, lines 46-53) are insufficiently explored. The authors ought to better explain what arises from the various studies which indicate that would be a rich vein for research exploration. Again, I would agree that it is most likely true, but detailed evidence presented by the authors would confirm that. The same is true of the first paragraph on the 'of the field' limitations section (page 8, lines 24-32). It's not clear to me, for example, why we would want implicit bias to have an 'established methodology' or why it is important that groups necessarily have communicated with each other.

I would also say that the structure of the methods section didn't work very well from my perspective. There is no real explanation for why the 2003-2013 period was chosen. (There is an
ambiguous statement about there being an increase in the number of articles on implicit bias in 2003 but it was not clear whether this was meant to justify using that as a cut-off date or, indeed, how the authors knew that.). The study selection section needs some structural revision as well. It's not entirely clear how the authors used the inclusion criteria - it's referenced at the initial title cull but probably formed the method used to explore the abstracts. (So, we have the statement that 'the ineligible abstracts were discarded' but not why or under what real method). It's also unclear whether they both looked at the titles or whether they each took a selection and went from there. I'm not entirely clear I understand why self-report questionnaires weren't used. The authors seem to suggest that it could only catch explicit bias but I would have thought cleverly worded questions could also catch implicit bias. I think all of the information is in there, but it just needs to be better explained as to what they were doing. (This links with the tick box question about methods being appropriate and well-described above. The methods are appropriate but I think the description needs some work. That's why that answer is a no.)

As a final major concern, I am unclear as to why the differences between US and non-US studies was not explored further. It's mentioned in the introduction as a justification for the article but then the article never returns to explore what, if any, differences there are between these geographical groups. That probably should have been done.

There are a range of more minor concerns. They include:

1. IAT and SES need to be referenced the first time they are used not on subsequent occasions. Both acronyms are explained later on but that needs to happen the first time they are used.

2. Especially on page 8, the authors should use a range for the studies instead of referencing each one individually. So, at line 44, after clinicians, it should probably just be 21-37 in superscript instead of writing each individual number. The same is true in the next line.

3. It's a bit unclear what it meant by bias towards 'brain injured patients perceived to have contributed to their injury' in connection with implicit bias. Considering the other categories are things like race, gender and socio-economic status, it is not clear what that is to mean.

4. The first sentence of the second paragraph on page 9 (lines 16-19) needs to be clarified. I am not sure positive and negative are on the same matrix and the sentence becomes confusing.

5. When including several sources, please put them in chronological order. So, line 10 on page 10 should look like line 43 on page 11 (19, 20, 40 not 19, 40, 20.)

6. On page 11, line 20, it should probably be '42 articles' not '42 studies' seeing as how the point is that there are several articles written on the back of one study.
7. There needs to be a citation on page 12, line 35 (after recorded probably)

8. The word powerful on page 13, line 42 needs an explanation

9. I do not understand how Table 1 is organized. The studies appear to show up at random rather than in chronological or alphabetical order.

10. Tables 2 and 3 would be significantly more helpful if the authors used names rather than the citation number of the article.

11. At two points on page 6, the authors state 'All disagreements were resolved by consensus,' As there were only two of them, consensus is probably the wrong word. My guess is the basic idea is that they talked it through between them and came to a conclusion - that's just reaching an agreement, not a consensus. Consensus probably only applies if there is a few of them.

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

No

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.

Yes

**Are the conclusions drawn adequately supported by the data shown?**
If not, please explain in your comments to the authors.

Yes

**Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?**
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

Not relevant to this manuscript

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