PEER REVIEW HISTORY

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ARTICLE DETAILS

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<th>TITLE (PROVISIONAL)</th>
<th>Conflicts of interest and spin in reviews of psychological therapies: A systematic review</th>
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<td>AUTHORS</td>
<td>Lieb, Klaus; Osten Sacken, Jan; Stoffers-Winterling, Jutta; Reiss, Neele; Barth, Juergen</td>
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VERSION 1 - REVIEW

<table>
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<th>REVIEWER</th>
<th>Professor Stefan Priebe</th>
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<td>Barts and the London School of Medicine and Dentistry, Queen Mary University of London</td>
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GENERAL COMMENTS

First of all, I would like to congratulate the authors on emphasising this issue, which is overdue to be discussed in the scientific community. As a regular reviewer of studies on psychological therapies (and researcher in the field), I find the increasing bias and spin in research on all psycho-social treatments obvious, although rarely acknowledged.

The paper reports the findings of a review of reviews and makes its points very well. I am not sure whether the authors should – in this paper – go beyond their current report and mention that the potential conflict of interest in psychology therapy research is rather complex, and arguably much more complicated than in pharmacological research.

In pharmacological research, the financial interest of the drug company and the subsequent financial benefit to the academic author are relatively straightforward. Yet, individual academics are rarely as dependent on specific findings as in psychological therapy research: if a drug is found not to work, most academics can go on and study a different drug and/or link with a different company. In psychological research, this is hardly possible, and personal careers depend much more on the results of the research. For example, if an academic has spent much of his/her career on developing and promoting CBT, they could not just switch and be experts in psychoanalysis – or vice versa – if the own therapy was found to be ineffective. Thus, there is a strong interest of individual academics – and of whole research communities – to have positive findings about the own treatment. This goes beyond mere allegiance, belief and vanity. It also reflects a rather complex financial dependency in psychology therapy research (and possibly other research areas): there may be no direct link to personal payments, but to academic evaluations (the hope to be cited more often alone drives bias), future research funding and career prospects. This affects not just authors of reviews, but of course also authors of primary research and peer reviewers for scientific journals.
The authors arrive at the conclusion that more transparency and management are required. I guess nobody would argue with that, but the concern remains that it will be very challenging to tackle such bias successfully.

I find the paper itself well written and the presentation of the findings clear. I do not have any quibbles with the methodology, particularly as the review intends to make a point of principle and not to provide a precise quantification of the problem.

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**REVIEWER**

Adam Dunn
Centre for Health Informatics, Macquarie University

**REVIEW RETURNED**

26-Dec-2015

**GENERAL COMMENTS**

I was confused by the abstract because the definitions of "bias" and "researcher allegiance" were not immediately clear and I had to read the rest of the manuscript to understand exactly what these were measuring. In this case, "bias" refers to spin, where the results of the systematic review were different from the conclusions; "researcher allegiance" means that the authors have concluded favourably about the interventions they have developed/studied in the past. Can these definitions be concisely explained in the abstract somehow?

The authors identified spin in 28% of the systematic reviews and researcher allegiance in a bit less than half of the systematic reviews.

I am not sure it makes sense to suggest that self-citation/inclusion and the closely related researcher allegiance "explain" the presence of bias in the systematic reviews given that the results were not clearly significant at any reasonable level, and especially given that the authors performed quite a barrage of statistical tests, reporting only the strongest in the abstract.

How did the authors deal with systematic reviews in which authors were responsible for studying/developing different types of interventions? Were there no reviews that included authors with expertise in pharmacological treatments and other authors with expertise in psychological interventions?

How many of the conflict of interest statements (or lack of) were inconsistent with the policy of the journals regarding non-financial and personal conflicts of interest specifically? It would be nice to know if the journals were "at fault" or if the reviewers had failed to disclose relevant conflicts of interest consistent with the journals' policies.

It is much easier to detect "spin" when the investigators are not blinded to the names of the reviewers and their allegiances/affiliations. Please make sure to note specifically whether you were blinded to the authors, the name of the journal, etc. when looking for spin in the conclusions.

This study is now quite old (nearly 2 years since the search was undertaken), but I understand that this is a labour-intensive piece of work.
Why does the ICMJE need to be written in all capitals and described as prestigious?

I don't see any problem with the speculation that researchers working on psychological interventions are doing a worse job of disclosing their conflicts of interest because the focus on financial conflicts of interest in definitions and policies fails to capture the potential risk of bias associated with an allegiance. This could be strengthened.

The strategy of excluding studies to mitigate bias to be a very strange suggestion. That would undoubtedly break the central premise of a systematic review (i.e. to include all relevant studies), and means that the researchers with the highest levels of expertise would be excluded from reviewing the work they know best. Much more useful strategies for mitigating biases include (a) the detection and removal of spin at the editorial stage; (b) having independent reviewers interpret the findings of meta-analyses; (c) rejecting systematic reviews that demonstrate selective citation biases. There are many other options, most of which involve improving the release and access to data to ensure that systematic reviews can be more easily replicated.

**VERSION 1 – AUTHOR RESPONSE**

Reviewer: 1 (Professor Stefan Priebe, London)

1) “First of all, I would like to congratulate the authors on emphasising this issue, which is overdue to be discussed in the scientific community. As a regular reviewer of studies on psychological therapies (and researcher in the field), I find the increasing bias and spin in research on all psycho-social treatments obvious, although rarely acknowledged.

“The paper reports the findings of a review of reviews and makes its points very well. I am not sure whether the authors should – in this paper – go beyond their current report and mention that the potential conflict of interest in psychology therapy research is rather complex, and arguably much more complicated than in pharmacological research. In pharmacological research, the financial interest of the drug company and the subsequent financial benefit to the academic author are relatively straightforward. Yet, individual academics are rarely as dependent on specific findings as in psychological therapy research: if a drug is found not to work, most academics can go on and study a different drug and/or link with a different company. In psychological research, this is hardly possible, and personal careers depend much more on the results of the research. For example, if an academic has spent much of his/her career on developing and promoting CBT, they could not just switch and be experts in psychoanalysis – or vice versa – if the own therapy was found to be ineffective. Thus, there is a strong interest of individual academics – and of whole research communities – to have positive findings about the own treatment. This goes beyond mere allegiance, belief and vanity. It also reflects a rather complex financial dependency in psychology therapy research (and possibly other research areas): there may be no direct link to personal payments, but to academic evaluations (the hope to be cited more often alone drives bias), future research funding and career prospects. This affects not just authors of reviews, but of course also authors of primary research and peer reviewers for scientific journals.”

Response: We followed the suggestion of the reviewer and added a section to the discussion where we discussed that conflict of interest in psychology therapy research is rather complex, and arguably much more complicated than in pharmacological research (revision with marked changes p. 11/12).
2) “The authors arrive at the conclusion that more transparency and management are required. I guess nobody would argue with that, but the concern remains that it will be very challenging to tackle such bias successfully.”

Response: See also proposals of reviewer 2. We mentioned that management of COI in psychotherapy research is very challenging and added some more ideas how to better manage COI in psychotherapy research (revision with marked changes p. 14).

3) "I find the paper itself well written and the presentation of the findings clear. I do not have any quibbles with the methodology, particularly as the review intends to make a point of principle and not to provide a precise quantification of the problem.

Response: No response necessary

Reviewer: 2 (Adam Dunn, Australia)

1) “Thank you for asking me to review this manuscript. The authors have presented a cohort study of a relatively neglected area of research - the effects of non-financial conflicts of interest on the presentation of evidence in systematic reviews. The methods are relatively clear and I believe that the results are important. I have some concerns about the presentation of results and I have recommended some clarifications below (in chronological order).”

Response: No response necessary

2) “I was confused by the abstract because the definitions of "bias" and "researcher allegiance" were not immediately clear and I had to read the rest of the manuscript to understand exactly what these were measuring. In this case, "bias" refers to spin, where the results of the systematic review were different from the conclusions; "researcher allegiance" means that the authors have concluded favourably about the interventions they have developed/studied in the past. Can these definitions be concisely explained in the abstract somehow? The authors identified spin in 28% of the systematic reviews and researcher allegiance in a bit less than half of the systematic reviews.”

Response: Throughout the manuscript, we made more clear that “bias” referred to spin and also defined spin not only in the methods section (revision with marked changes p. 6/7), but also in the abstract (revision with marked changes p. 2). The same was done for definition of “researcher allegiance”, for which a short definition is also given in the abstract (revision with marked changes p. 2).

3) “I am not sure it makes sense to suggest that self-citation/inclusion and the closely related researcher allegiance “explain” the presence of bias in the systematic reviews given that the results were not clearly significant at any reasonable level, and especially given that the authors performed quite a barrage of statistical tests, reporting only the strongest in the abstract.”

Response: We agree with the reviewer that the term “explain” is too strong given the fact that the results were not significant. We therefore more carefully expressed the possible relation between self-citations and researcher allegiance throughout the manuscript and used “association” instead of “explanation” to make it more clear that causal relations are not seen.

4) “How did the authors deal with systematic reviews in which authors were responsible for studying/developing different types of interventions? Were there no reviews that included authors with expertise in pharmacological treatments and other authors with expertise in psychological
Response: As outlined in the Methods section, we assessed researcher allegiance only in primary studies included in the reviews. All those 86 included primary studies addressed psychological therapies (this is now added in the results section; revision with marked changes p. 8). We therefore were not able to compare allegiance to psychological and pharmacological therapies. Second, an allegiance to pharmacotherapy was never declared as a COI. Third, we were not able to investigate who of the review authors was responsible for which kind of therapy or whether review authors have more COI than the declared ones. Such analyses would have a high risk of being inaccurate and incomplete (see discussion in revision with marked changes p. 14).

5) “How many of the conflict of interest statements (or lack of) were inconsistent with the policy of the journals regarding non-financial and personal conflicts of interest specifically? It would be nice to know if the journals were "at fault" or if the reviewers had failed to disclose relevant conflicts of interest consistent with the journals’ policies.”

Response: We found only one inconsistency: In one review the authors disclosed financial COIs although the journal did not request it. In all other COI statements, the authors declared only requested COIs (now mentioned in revision with marked changes p. 8). We further analysed the relation of journal requirements for declaration of non-financial COI, author declarations and COI found by us (i.e. inclusion of own primary studies and researcher allegiance). The results are now given in detail in the results section on p. 9). Supplement table 5 now also shows, which journals specifically asked for declaration of included own studies or researcher allegiance. These results underline the importance that journals should place more emphasis on the clear definition of non-financial COI and request their disclosure. This is now being emphasized at the end of the results section (revision with marked changes p. 14).

6) “It is much easier to detect "spin" when the investigators are not blinded to the names of the reviewers and their allegiances/affiliations. Please make sure to note specifically whether you were blinded to the authors, the name of the journal, etc. when looking for spin in the conclusions.”

Response: We now mentioned in the methods section that all authors were blind to the author names of the review as well as the Journal having published the review when assessing spin in review conclusions (revision with marked changes p. 7).

7) “This study is now quite old (nearly 2 years since the search was undertaken), but I understand that this is a labour-intensive piece of work.”

Response: We agree, but it was indeed a labour-intensive piece of work.

8) “Why does the ICMJE need to be written in all capitals and described as prestigious?”

Response: We deleted “prestigious” and did not write ICMJE in all capitals (revision with marked changes p. 12).

9) “I don't see any problem with the speculation that researchers working on psychological interventions are doing a worse job of disclosing their conflicts of interest because the focus on financial conflicts of interest in definitions and policies fails to capture the potential risk of bias associated with an allegiance. This could be strengthened.”

Response: We agree with the reviewer and strengthened this point in the conclusion section of the
10) “The strategy of excluding studies to mitigate bias to be a very strange suggestion. That would undoubtedly break the central premise of a systematic review (i.e. to include all relevant studies), and means that the researchers with the highest levels of expertise would be excluded from reviewing the work they know best. Much more useful strategies for mitigating biases include (a) the detection and removal of spin at the editorial stage; (b) having independent reviewers interpret the findings of meta-analyses; (c) rejecting systematic reviews that demonstrate selective citation biases. There are many other options, most of which involve improving the release and access to data to ensure that systematic reviews can be more easily replicated.”

Response: We agree with the reviewer and deleted our suggestion for management to mitigate bias. Instead, we included the proposals of the reviewer as potential strategies at the end of the discussion section (revision with marked changes p. 14).

### VERSION 2 – REVIEW

<table>
<thead>
<tr>
<th>REVIEWER</th>
<th>Stefan Priebe</th>
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<tr>
<td>Queen Mary, University of London, United Kingdom</td>
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| REVIEW RETURNED | 09-Feb-2016 |

| GENERAL COMMENTS | I think the revision has improved the paper. I have only one concern. I understand that the authors changed the term ‘bias’ to ‘spin’ in response to the comment of the other reviewer. One might argue about whether the influence of interests on the findings of reviews reflects more bias or spin. Yet, spin has a different connotation than bias, and I am worried that the categorical use of the term spin may unnecessarily provoke some readers, who would accept that results of reviews may be biased, but not that there is a deliberate spin behind it. I suggest inserting a brief explanation that no claims are made about the nature of the influence which might be mere bias or more intentional spin. I also appreciate that the authors took up the recommendation to insert a statement about the difference between allegiances to medication and to psychological treatments. Yet, they refer principally to practising therapists and doctors, whilst I feel the issue is even more relevant for researchers (who are the subject of this paper), as their whole careers may depend on the effectiveness of a given psychological treatment, but rarely on the effectiveness of one drug. |

### VERSION 2 – AUTHOR RESPONSE

I responded to the 2 minor comments of the reviewer in the second revision as follows:

Please leave your comments for the authors below I think the revision has improved the paper. I have only one concern. I understand that the authors changed the term ‘bias’ to ‘spin’ in response to the comment of the other reviewer. One might argue about whether the influence of interests on the findings of reviews reflects more bias or spin. Yet, spin has a different connotation than bias, and I am worried that the categorical use of the term spin may unnecessarily provoke some readers, who would accept that results of reviews may be biased, but not that there is a deliberate spin behind it. I suggest inserting a brief explanation that no claims are made about the nature of the influence which
might be mere bias or more intentional spin.

- I inserted a respective brief explanation in the section "strengths and limitations of the study" (3rd bullet Point)

I also appreciate that the authors took up the recommendation to insert a statement about the difference between allegiances to medication and to psychological treatments. Yet, they refer principally to practising therapists and doctors, whilst I feel the issue is even more relevant for researchers (who are the subject of this paper), as their whole careers may depend on the effectiveness of a given psychological treatment, but rarely on the effectiveness of one drug.

- On p. 12 of the manuscript, I focussed the respective argument on researchers instead of therapists since the manuscript has its focus on researchers.
Conflicts of interest and spin in reviews of psychological therapies: a systematic review

Klaus Lieb, Jan von der Osten-Sacken, Jutta Stoffers-Winterling, Neele Reiss and Jürgen Barth

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