

PEER REVIEW HISTORY

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

ARTICLE DETAILS

TITLE (PROVISIONAL)	Attitudes of editors of core clinical journals about whether systematic reviews are original research: a mixed-methods study
AUTHORS	Krnic Martinic, Marina; Meerpohl, Joerg; von Elm, Erik; Herrle, Florian; Marusic, Ana; Puljak, Livia

VERSION 1 - REVIEW

REVIEWER	Perrine Janiaud Meta-Research Innovation Center at Stanford (METRICS), Stanford University, USA
REVIEW RETURNED	25-Feb-2019

GENERAL COMMENTS	<p>Thank you for letting me review this interesting article.</p> <p>The method is adequate and the new aspects added compared to the first study are pertinent and well-thought. I have one question though regarding the methods. It is mentioned that 24 editors accepted to be interviewed (page 7 line 51) but only 15 were interviewed. How did you select the 15 among the 24?</p> <p>Page 4 when you are describing the PubMed search, it is not clear at first that you limit the PubMed search to 2017.</p> <p>Regarding the results, I would recommend adding a table describing the Journals' characteristics.</p> <p>Also while reading the manuscript I was wondering if the samples were different between the two studies? Were they the same editors answering the survey?</p> <p>More of a comment as quite burdensome but it would be interesting to see the trend over the years in the number of SR in those journals.</p> <p>It is mentioned page 8 that 8 journals did not publish a single SR or MA based on your PubMed search and then on page 7, you mention that 6 editors answered they did not published SR in their journals. Are the 6 editors in the 8 journals for which you did not find SRs?</p> <p>I would also be interested in knowing the authors thoughts on how Editorial organizations should address the issue of what constitutes original research and what makes a SR original.</p>
-------------------------	---

REVIEWER	Joshua D Wallach Yale School of Public Health, New Haven, CT, USA In the past 36 months, I have received funding from the Meta-Research Innovation Center at Stanford and the Collaboration for Research Integrity and Transparency at Yale, both supported by the Laura and John Arnold Foundation.
REVIEW RETURNED	06-Mar-2019

GENERAL COMMENTS	<p>Thank you for the invitation to review this manuscript. In this interesting study, Martinic et al. report the results of a survey of editors of core clinical journals and determine whether editors consider systematic reviews to be original research. In addition to the email based survey, the authors conduct a follow-up qualitative interview study and assess a sample of systematic reviews published in the targeted journals. Martinic et al find that the majority of editors consider systematic reviews to be original research. The authors note that these results are similar to those obtained in a previous evaluation (conducted 2009 and published in 2012). Overall, the main finding from the email survey are straightforward and demonstrate that perceptions may not have changed over the last few years. However, there are a few gaps in this study that may need to be addressed.</p> <p>Over the past decade, there have been numerous publications related the importance and role of systematic reviews and meta-analyses. Among these studies was a 2012 survey conducted by Meerpool et al. In that clear and concise survey, the authors found that most editors of core clinical journals consider systematic reviews to be original research. For that study, the authors relied on 3 straightforward questions. One key item that the "Introduction" section was missing was a clear discussion outlining the rationale for repeating this evaluation. Although the authors state that they expect "that the proportion of editors who consider SR to be original research had increased since 2009 and the more of the surveyed journals now published SRs", the reason for these hypotheses are unclear. In 2016, Ioannidis wrote a piece called "The Mass Production of Redundant, Misleading, and Conflicted Systematic Reviews and Meta-analyses" (doi: 10.1111/1468-0009.12210). In this article, Ioannidis discusses how the publication of systematic reviews and meta-analyses has increased rapidly. This seems like a key statistic to mention in the current manuscript? Furthermore, Ioannidis discusses of the production of systematic reviews and meta-analyses "has reached epidemic proportions. Possibly, the large majority of produced systematic reviews and meta-analyses are unnecessary, misleading, and/or conflicted." These data could either support the hypothesis that the number of reviews has increased since 2009, or support a hypothesis that journal editors are aware of the mass production of systematic reviews and meta-analyses and may influence perceptions? It may be necessary for the authors to discuss these potential current trends and views related to systematic reviews? Currently, the authors include 11 references, but there are likely other articles on the promise and challenges of systematic reviews that could be related to journal editors' opinions.</p> <p>Another area that may need further justification is the follow-up qualitative study that was conducted via Skype. To the best of my knowledge, this was not a feature of the original survey article by</p>
-------------------------	--

	<p>Meerpool et al 2012? It is not quite clear why the authors expanded the survey to include a in depth survey. It is also unclear whether some of the open-ended questions are supposed to capture the individual views of the editors or the views of the journals (e.g., "do you think that systematic reviews are original studies?"). This question is fairly similar to one of the email survey questions: "Do you consider a systematic review manuscript an original research project?". Overall, additional clarity is necessary to justify why the open-ended questions were considered. Given the open-ended nature, it is difficult to determine trends and overall results. Furthermore, what specific knowledge is added to the literature by conducting the detailed survey? The authors conclude that "editors indicated that various elements are considered when deciding whether a SR is an original study." However, this conclusion seems relevant to any article that authors review.</p> <p>Reasons for "No" in Review Checklist above:</p> <p>2. Is the abstract accurate, balanced, and complete? The abstract is missing information related to the assessment of the published articles. Please see comment 1 below.</p> <p>3. Is the study design appropriate to answer the research question? Please see general comments above regarding the open-ended survey.</p> <p>4. Are the methods described sufficiently to allow the study to be repeated? Please see comments #2, 3, 5, and 6</p> <p>8. Are the references up-to-date and appropriate? Please see general comments above regarding the Introduction</p> <p>12. Are the study limitations discussed adequately? Please see comment #12 below.</p> <p>Specific comments:</p> <p>1. Page 2, Abstract: The Abstract does not appear to outline the methods the authors used to evaluate the sample of systematic reviews published in 2017. Furthermore, the results do not outline any of the findings. [This leads to a follow-up question: why was the analysis of systematic reviews published in targeted journals conducted?]</p> <p>2. Page 4, Line 12: The authors note that editors for 118 journals labelled as Core Clinical Journals by the National Library of Medicine in February 2018 were identified. Were these journals similar/the same as the 118 included in the 2009 survey? Could the authors provide a supplementary table with this information? Is it possible that the same editors were surveyed in 2009 and 2018?</p> <p>3. Page 4, Line 39:</p>
--	--

	<p>The authors searched for reviews using PubMed using the limits for SRs and meta-analyses. What were these limits ["systematic review" OR "meta-analyses"]? If the authors were selecting only one year, would the "hand search" method of each journal been a better way to identify relevant publications? The PubMed method relies on the accuracy of indexing on PubMed.</p> <p>4. Page 4, Line 58:</p> <p>Were any of the editors the same in both samples (i.e., correlated samples?)</p> <p>5. Page 5, Line 39:</p> <p>It is unclear exactly why the authors "stopped inviting further editors." Is n of 15 enough to establish thematic trends?</p> <p>6. Page 5, Line 30:</p> <p>Could the authors supply the Consolidated Criteria for Reporting Qualitative Studies guide?</p> <p>7. Page 7, Line 8:</p> <p>Could the authors provide the journal characteristics (impact factors etc.) for the responders vs. non-responders? It seems difficult to judge the extent of selection bias without additional information.</p> <p>8. Page 8, Line 26:</p> <p>What do the authors mean by first 30%? Are these the first 30% in order of publication?</p> <p>9. Page 8, Line 27:</p> <p>Were scoping reviews classified as systematic reviews in this study? Were the journal editors provided with a definition of systematic reviews?</p> <p>10. Page 9:</p> <p>It is somewhat difficult to determine the take-away message of the open-ended survey.</p> <p>Discussion:</p> <p>11. The discussion section appears to repeat a significant amount of the results. It may be necessary to talk more about the implications.</p> <p>12. Page 14, Line 19:</p> <p>Only one limitation is listed for the article. However, there are numerous limitations related to survey design and the definition and communication of terminology to survey participants (what types of articles fall under systematic review?). Furthermore, there are potential limitations related to the assessment of published articles</p>
--	---

VERSION 1 – AUTHOR RESPONSE

Reviewer(s)' Comments to Author:

Reviewer: 1

Reviewer Name: Perrine Janiaud

Thank you for letting me review this interesting article.

Author response: We are grateful to the Reviewer for kind words about our manuscript.

The method is adequate and the new aspects added compared to the first study are pertinent and well-thought. I have one question though regarding the methods. It is mentioned that 24 editors accepted to be interviewed (page 7 line 51) but only 15 were interviewed. How did you select the 15 among the 24?

Author response: The mentioned 24 editors answered that they would accept to be interviewed. As we reached the level of saturation of identified themes, we stopped further interviews. We clarified this now in the manuscript.

Page 4 when you are describing the PubMed search, it is not clear at first that you limit the PubMed search to 2017.

Author response: Thank you for the comment; we clarify now that the PubMed search was limited to 2017.

Regarding the results, I would recommend adding a table describing the Journals' characteristics.

Author response: All the Journals we analyzed belong to the NLM/PubMed Core Clinical Journals. Since the focus was not on the journals' characteristics, we did not extract any detailed data about those characteristics. We could add a descriptive table if the editors deem that this is necessary.

Also while reading the manuscript I was wondering if the samples were different between the two studies? Were they the same editors answering the survey?

Author response: In our study we reached out to the editors of journals labelled as Core Clinical Journals by the National Library of Medicine, USA in February 2018, while the first study included the journals labelled as Core Clinical Journals in April 2009. Despite the long time between two studies, it is possible that an editor-in-chief did not change during those 9 years. We did not analyze whether editors have changed, and we did not include questions about whether the editors participated in the first study. We indicated this now in the study limitations, with the following text: "Despite the nine years that passed between the two studies, it is possible that some editors-in-chief did not change over time and that they were included both in the first and in the second study. In our study, we did not include any questions to verify this."

More of a comment as quite burdensome but it would be interesting to see the trend over the years in the number of SR in those journals.

Author response: This would definitely be interesting, but we consider that this would be out of the scope of our study. In our study we focused on one recent year, to see whether journals publish SRs or not. We are not sure that such analysis would add considerable to our study message, and it would be a considerable additional burden.

It is mentioned page 8 that 8 journals did not publish a single SR or MA based on your PubMed search and then on page 7, you mention that 6 editors answered they did not published SR in their journals. Are the 6 editors in the 8 journals for which you did not find SRs?

Author response: Thank you for this question. Interestingly, there were only two editors that stated that they do not publish SR-s, and in whose journals our search did not find a SR or a MA published in in 2017. Four stated that they do not publish, but our search found SR-s or MA-s published in their journal in 2017. This is now clarified in the Results.

I would also be interested in knowing the authors thoughts on how Editorial organizations should address the issue of what constitutes original research and what makes a SR original.

Author response: We have now included in the Discussion the following text, based on this comment: "Considering that certain tangible benefits can be associated with definitions of research and definitions of a SR as original, it would be beneficial if relevant international organizations, such as associations of editors, would address the issue of what constitutes an original research, and what makes an SR original."

Reviewer: 2

Reviewer Name: Joshua D Wallach

Thank you for the invitation to review this manuscript. In this interesting study, Martinic et al. report the results of a survey of editors of core clinical journals and determine whether editors consider systematic reviews to be original research. In addition to the email based survey, the authors conduct a follow-up qualitative interview study and assess a sample of systematic reviews published in the targeted journals. Martinic et al find that the majority of editors consider systematic reviews to be original research. The authors note that these results are similar to those obtained in a previous evaluation (conducted 2009 and published in 2012). Overall, the main finding from the email survey are straightforward and demonstrate that perceptions may not have changed over the last few years. However, there are a few gaps in this study that may need to be addressed.

Author response: We are grateful for the feedback we received, and we did our best to respond to the reviewer's comments.

Over the past decade, there have been numerous publications related the importance and role of systematic reviews and meta-analyses. Among these studies was a 2012 survey conducted by Meerpool et al. In that clear and concise survey, the authors found that most editors of core clinical journals consider systematic reviews to be original research. For that study, the authors relied on 3 straightforward questions. One key item that the “Introduction” section was missing was a clear discussion outlining the rationale for repeating this evaluation. Although the authors state that they expect “that the proportion of editors who consider SR to be original research had increased since 2009 and the more of the surveyed journals now published SRs”, the reason for these hypotheses are unclear. In 2016, Ioannidis wrote a piece called “The Mass Production of Redundant, Misleading, and Conflicted Systematic Reviews and Meta-analyses” (doi: 10.1111/1468-0009.12210). In this article, Ioannidis discusses how the publication of systematic reviews and meta-analyses has increased rapidly. This seems like a key statistic to mention in the current manuscript? Furthermore, Ioannidis discusses of the production of systematic reviews and meta-analyses “has reached epidemic proportions. Possibly, the large majority of produced systematic reviews and meta-analyses are unnecessary, misleading, and/or conflicted.” These data could either support the hypothesis that the number of reviews has increased since 2009, or support a hypothesis that journal editors are aware of the mass production of systematic reviews and meta-analyses and may influence perceptions? It may be necessary for the authors to discuss these potential current trends and views related to systematic reviews? Currently, the authors include 11 references, but there are likely other articles on the promise and challenges of systematic reviews that could be related to journal editors’ opinions.

Author response: Thank you for the interesting comment. Indeed, we wanted to repeat this study because we considered that with the increased publication rates of SRs and increased awareness about SRs, there could be more editors who considered them original research. Ioannidis addressed the number of SRs-MAs being published as follows: “Currently, probably more systematic reviews of trials than new randomized trials are published annually” (Ioannidis JP. The Mass Production of Redundant, Misleading, and Conflicted Systematic Reviews and Meta-analyses. *Milbank Q.* 2016;94(3):485-514.). Our investigation was not aimed at answering the question whether SR-s and MA-s should be produced or should be published in such an extent, but should the ones editors decide to publish be regarded as original research. Ioannidis also states that “Ideally, meta-analyses should be primary research efforts where investigators collaborate preemptively in consortia with embedded replication across teams and joint analyses.” He also states that “Systematic reviews and meta-analyses are indispensable components in the chain of scientific information and key tools for evidence-based medicine” and emphasizes the role of SR-s and MA-s in current medical knowledge. Again, our question was not if SR-s or MA-s should be published in massive numbers, but should they be considered original work, and if yes, what makes them original.

We have added the following text in the Introduction, before aims: In the nine years since the study of Meerpohl et al. was conducted, the number of published SRs has increased, as well as their influence (Chalmers I and Fox DM. Increasing the Incidence and Influence of Systematic Reviews on Health Policy and Practice. *Am J Public Health.* 2016; 106(1): 11–13.), and therefore we hypothesized that now there will be more editors who considered them original research.

Another area that may need further justification is the follow-up qualitative study that was conducted via Skype. To the best of my knowledge, this was not a feature of the original survey article by Meerpool et al 2012? It is not quite clear why the authors expanded the survey to include a in depth survey. It is also unclear whether some of the open-ended questions are supposed to capture the individual views of the editors or the views of the journals (e.g., "do you think that systematic reviews are original studies?"). This question is fairly similar to one of the email survey questions: "Do you consider a systematic review manuscript an original research project?". Overall, additional clarity is necessary to justify why the open-ended questions were considered. Given the open-ended nature, it

is difficult to determine trends and overall results. Furthermore, what specific knowledge is added to the literature by conducting the detailed survey? The authors conclude that “editors indicated that various elements are considered when deciding whether a SR is an original study.” However, this conclusion seems relevant to any article that authors review.

Author response: The Reviewer is right that in the original study there was no follow-up qualitative study. The question that the Reviewer quoted simply served to re-iterate the editor’s opinion, given earlier in the online survey, and as an introduction to the additional questions.

In the first study, performed by Meerpohl et al. only the three-question survey was included, and in this study we repeated that part, but also added the qualitative study. With the qualitative study we wanted to gather in-depth data, to give the editors an opportunity to explain their attitudes, and to get more details about what they consider original or non-original regarding SRs. Since editors are busy persons, we were concerned that sending them a very detailed questionnaire would potentially jeopardize the response rate, and turn off many editors from responding to our initial survey. This is now clarified in the Discussion section.

2. Is the abstract accurate, balanced, and complete?

The abstract is missing information related to the assessment of the published articles. Please see comment 1 below.

Author response: The abstract is now revised to correct this issue.

3. Is the study design appropriate to answer the research question?

Please see general comments above regarding the open-ended survey.

Author response: We have responded above to the Reviewer’s comment about the study design. We did not want to repeat the earlier study, but also study new aspects with it and investigate the topic in more detail.

4. Are the methods described sufficiently to allow the study to be repeated?

Please see comments #2, 3, 5, and 6

Author response: We have now provided additional clarifications, and we hope that the manuscript is clearer now.

8. Are the references up-to-date and appropriate?

Please see general comments above regarding the Introduction

Author response: We have now updated the Introduction section.

12. Are the study limitations discussed adequately?

Please see comment #12 below.

Author response: We have now updated the limitations section.

Specific comments:

1. Page 2, Abstract:

The Abstract does not appear to outline the methods the authors used to evaluate the sample of systematic reviews published in 2017. Furthermore, the results do not outline any of the findings. [This leads to a follow-up question: why was the analysis of systematic reviews published in targeted journals conducted?]

Author response: The Abstract was revised and information added within the limits of the abstract word limit. If there are additional specific revisions to be made in the Abstract, we would be happy to do so. Regarding the comment about the targeted journals, the authors of the earlier study used the sample of Core Clinical Journals, which we also considered a pertinent sample to explore. As a limitation of our study, we indicated that the sample was limited to this group of journals.

2. Page 4, Line 12:

The authors note that editors for 118 journals labelled as Core Clinical Journals by the National Library of Medicine in February 2018 were identified. Were these journals similar/the same as the 118 included in the 2009 survey? Could the authors provide a supplementary table with this information? Is it possible that the same editors were surveyed in 2009 and 2018?

Author response: As suggested, we have now created a Supplementary file 1, showing which journals were included in both studies (2009 and 2018). The total number of journals that were considered Core Clinical Journals was the same in both years, but thirteen new journals were on the 2018 list, compared to the 2009 list, and 13 of the 2009 journals were dropped.

The first Reviewer also commented on the possibility that the same editors were surveyed in both years. See our response there. This is the response provided to the first Reviewer:

Despite the long time between two studies, it is possible that an editor-in-chief did not change during those 9 years. We did not analyze whether editors have changed, and we did not include questions about whether the editors participated in the first study. We indicated this now in the study limitations, with the following text: Despite the nine years that passed between the two studies, it is possible that some editors-in-chief did not change over time and that they were included both in the first and in the second study. In our study, we did not include any questions to verify this.

3. Page 4, Line 39:

The authors searched for reviews using PubMed using the limits for SRs and meta-analyses. What were these limits ["systematic review" OR "meta-analyses"]? If the authors were selecting only one year, would the "hand search" method of each journal been a better way to identify relevant publications? The PubMed method relies on the accuracy of indexing on PubMed.

Author response: The Reviewer is right that we could have used also manual searching. However, some of these journals publish a lot of titles annually, and therefore we used this approach that, indeed, depends on the accuracy of indexing on PubMed. However, to compensate for this potential

inaccuracy, we searched for SRs-MAs on the web sites of journals for which we could not find any SRs-MAs indexed on PubMed. We have now added the following text in the Limitations:

“In this study we used PubMed to search whether a journal published a SR or MA. However, this could have resulted in errors due to erroneous indexing on PubMed. For this reason, we hand-searched all journals for which we did not find any SRs or MAs indexed on PubMed.”

4. Page 4, Line 58:

Were any of the editors the same in both samples (i.e., correlated samples?)

Author response: As indicated above, we did not attempt to ask editors whether they were included in the first study. This is now highlighted in the Limitations, as described.

5. Page 5, Line 39:

It is unclear exactly why the authors “stopped inviting further editors.” Is n of 15 enough to establish thematic trends?

Author response: We stopped scheduling new editors after we reached saturation of themes. This is usual methodology in qualitative research.

6. Page 5, Line 30:

Could the authors supply the Consolidated Criteria for Reporting Qualitative Studies guide?

Author response: The editors have requested the SRQR checklist, and we have included it now as a supplementary file. We have referred to the COREQ checklist in the list of references: Tong A, Sainsbury P, Craig J: Consolidated criteria for reporting qualitative research (COREQ): a 32-item checklist for interviews and focus groups. *Int J Qual Health Care* 2007, 19(6):349-357.

7. Page 7, Line 8:

Could the authors provide the journal characteristics (impact factors etc.) for the responders vs. non-responders? It seems difficult to judge the extent of selection bias without additional information.

Author response: The aim of this study was not to analyze journals' characteristics. However, the Reviewer is right that there could have been selection bias when editors responded, and we have indicated this in our manuscript.

8. Page 8, Line 26:

What do the authors mean by first 30%? Are these the first 30% in order of publication?

Author response: Yes, the first 30% in order of publication. This was now clarified in the manuscript.

9. Page 8, Line 27:

Were scoping reviews classified as systematic reviews in this study? Were the journal editors provided with a definition of systematic reviews?

Author response: None of the editors raised the issue of scoping reviews. From our side, the editors were not provided with a definition of a systematic review, and, not a single editor asked us which definition of a SR we are using. We have now clarified this in the Discussion in a new paragraph.

10. Page 9:

It is somewhat difficult to determine the take-away message of the open-ended survey.

Author response: The open-ended survey gave richer information about editors' reasoning whether SRs are original studies or not.

Discussion:

11. The discussion section appears to repeat a significant amount of the results. It may be necessary to talk more about the implications.

Author response: We have revised the Discussion section based on the specific suggestions we received.

12. Page 14, Line 19:

Only one limitation is listed for the article. However, there are numerous limitations related to survey design and the definition and communication of terminology to survey participants (what types of articles fall under systematic review?). Furthermore, there are potential limitations related to the assessment of published articles

Author response: Based on the comments of both reviewers, we have now included a number of additional limitations in the manuscript.

VERSION 2 – REVIEW

REVIEWER	Joshua D Wallach Yale School of Public Health New Haven, CT USA I receive funding from the Laura and John Arnold Foundation
REVIEW RETURNED	31-May-2019

GENERAL COMMENTS	Thank you for the opportunity to provide a second review of this manuscript. I would also like to thank the authors for their detailed response. The manuscript is certainly improved, and I hope that these remaining comments will be helpful to the authors.
-------------------------	---

	<p>1. My apologies for not mentioning this last time, but the title is a bit confusing. The second part "...their value for publication" does not connect well with the first part of the sentence.</p> <p>2. Page 2, Line 35: Should this be "We conducted a survey and qualitative interview study of journal editors"?</p> <p>4. Page 2, Line 36: Does this imply that there were 118 participants?</p> <p>5. Page 2, Line 45: Will people know what "conditional acceptance" means at the abstract level, without further information? I think this term needs defining.</p> <p>Page 2, Line 42: It appears as if there were multiple editors for some of the journals. Is the 62% from the 118 journals? What is the response rate among editors?</p> <p>Page 2, Line 44: Should the Results section of the abstract provide actual proportions (i.e. what % considered SR to be original?)</p> <p>Page 2, Line 49: The opening sentence of the Conclusion section of the abstract is rather confusing (see comment about "conditional acceptance" above).</p> <p>Page 3, Line 120: The updated Strengths and Limitations section is good.</p> <p>Page 4, Line 139: The sentence about "influencers" is a bit confusing.</p> <p>Page 4, Line 156: When discussing the growth in SR/MA, it may be worth reference Ioannidis 2016 paper in Millbank on the mass production of systematic reviews over the last few decade. Considering the growth in redundant, biased, and lower quality reviews, this may impact editors opinions.</p> <p>Page 5, Line 201: Is a Chi-square test appropriate? Considering some of the same editors could have been present in both surveys, the two samples may not be completely independent?</p> <p>Page 6, Line 217: As the authors note in the first reviewer response letter, "We stopped scheduling new editors after we reached saturation of themes. This is usual methodology in qualitative research." Perhaps it would be helpful if the authors noted that this is usual methodology in the manuscript.</p> <p>Page 6, Line 239: Could the authors provide more information about the qualitative content analysis?</p> <p>The limitations section is now much stronger, and I particularly appreciate the discussion about the definition of a systematic review.</p>
--	---

VERSION 2 – AUTHOR RESPONSE

Reviewer(s)' Comments to Author:

Reviewer: 2

Reviewer Name: Joshua D Wallach

Institution and Country: Yale School of Public Health, New Haven, CT, USA

Please state any competing interests or state 'None declared': I receive funding from the Laura and John Arnold Foundation

Please leave your comments for the authors below

Thank you for the opportunity to provide a second review of this manuscript. I would also like to thank the authors for their detailed response. The manuscript is certainly improved, and I hope that these remaining comments will be helpful to the authors.

Author response: We are grateful to the Reviewer for kind words about our manuscript.

1. My apologies for not mentioning this last time, but the title is a bit confusing. The second part "...their value for publication" does not connect well with the first part of the sentence.

Author response: As suggested, we deleted "and their value for publication" from the title.

2. Page 2, Line 35: Should this be "We conducted a survey and qualitative interview study of journal editors"?

Author response: The text is now changed as the reviewer suggested.

4. Page 2, Line 36: Does this imply that there were 118 participants?

Author response: We tried to contact via e-mail all individuals that were indicated as editor-in-chief for 118 PubMed Core Clinical Journals, but some journals, 10 of them to be exact, listed more than one editor-in-chief. Of those 10 journals with more than one editor in chief, 8 journals had two editors-in-chief, one journal had three, and one journal four editors-in-chief. The text is now changed to clarify this.

5. Page 2, Line 45: Will people know what "conditional acceptance" means at the abstract level, without further information? I think this term needs defining.

Author response: We have now changed text so it is more understandable. The new version of the text is: "accepted SRs as original upon certain condition such as presence of meta-analysis"

Page 2, Line 42: It appears as if there were multiple editors for some of the journals. Is the 62% from the 118 journals? What is the response rate among editors?

Author response: The 62% is the percentage of journals that responded, but from only one journal we received two responses from two editors-in-chief, who were, interestingly, opposite in opinion. This is now indicated in the manuscript. If we count response rate based on the number of editors-in-chief who responded, 73 editors out of 131 responded, which would be 56% response rate per editor. This is now clarified in the manuscript.

Page 2, Line 44: Should the Results section of the abstract provide actual proportions (i.e. what % considered SR to be original?)

Author response: The percentages are now added in the Results section of the abstract as suggested.

Page 2, Line 49: The opening sentence of the Conclusion section of the abstract is rather confusing (see comment about "conditional acceptance" above).

Author response: We revised the opening sentence of the Conclusion section in Abstract.

Page 3, Line 120:

The updated Strengths and Limitations section is good.

Author response: We thank the reviewer for kind words about the manuscript.

Page 4, Line 139: The sentence about "influencers" is a bit confusing.

Author response: This sentence was revised into "This was also suggested in previous studies examining the status of systematic reviews in research community", to avoid confusing word "influencers".

Page 4, Line 156: When discussing the growth in SR/MA, it may be worth reference Ioannidis 2016 paper in Millbank on the mass production of systematic reviews over the last few decades. Considering the growth in redundant, biased, and lower quality reviews, this may impact editors' opinions.

Author response: We have now referred to the Ioannidis paper in Millbank in the Introduction section of the manuscript with the text: "On the other hand, it has been suggested that there is an overproduction of redundant, misleading, and conflicted SRs and MAs in addition to the publication growth."

Page 5, Line 201: Is a Chi-square test appropriate? Considering some of the same editors could have been present in both surveys, the two samples may not be completely independent?

Author response: As mentioned in Strength and Limitations section there is a possibility that some journal editors were surveyed both in the earlier study and in this follow-up study, but none of the

editors indicated that they participated in the earlier study. So, we have covered this as a potential limitation.

Page 6, Line 217: As the authors note in the first reviewer response letter, "We stopped scheduling new editors after we reached saturation of themes. This is usual methodology in qualitative research." Perhaps it would be helpful if the authors noted that this is usual methodology in the manuscript.

Author response: The suggested text is now noted in the manuscript in the Methods section.

Page 6, Line 239: Could the authors provide more information about the qualitative content analysis?

Author response: The qualitative content analysis we conducted is now described in more detail in the Methods section and a new reference is added.

The limitations section is now much stronger, and I particularly appreciate the discussion about the definition of a systematic review.

Author response: We thank the reviewer for encouraging words about our manuscript.

VERSION 3 - REVIEW

REVIEWER	Joshua D. Wallach Yale School of Public Health In the past 36 months, I have received research support through the Meta-Research Innovation Center at Stanford (METRICS) and the Collaboration for Research Integrity and Transparency (CRIT) at Yale, from the Laura and John Arnold Foundation.
REVIEW RETURNED	11-Jul-2019

GENERAL COMMENTS	Thank you for the opportunity to review this paper. The authors have done a nice job revising the manuscript, and I have no further comments. best, Josh
-------------------------	--