Reviewer's report

Title: More insight into the metabolism of new biomedical information: a systematic review

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Reviewer: Antje Timmer

Level of interest: A paper whose findings are important to those with closely related research interests

Advice on publication: Unable to decide on acceptance or rejection until the authors have responded to the compulsory revisions

Comments on:
More insight into the metabolism of new biomedical information: a systematic review

General comments, compulsory revisions
This is thoroughly performed study, reviewing data on the publication process from abstract submission to full publication, with specific reference to the various stages of this process. I have no major problems with the methodology used or the main objectives, results and discussion of this study.
However, the report is difficult to read, and seems overly complicated. I suggest the authors go through the report, preferably with the MOOSE checklist (available from the CONSORT-statement website) at hand and simplify wherever possible. Please use common terminology and adjust this report to standard reports of systematic reviews.
Overall, the amount of new data and insights seems limited. The authors should be more concise about why they thought another systematic review was necessary, what their findings were, whether there are discrepancies in the literature and what they feel was the reason for any heterogeneity. These potential basic questions a systematic review could answer get a bit lost in this at times somewhat muddled report.
The following specific comments are headed discretionary, however, there are many, and most should be answered to make this an acceptable report.

Specific comments, discretionary revisions
Title:
1. The title is not informative, at least not to readers (like me) not familiar with the paper of 1961 you refer to. It would be more helpful to let the readers know what the study is exactly about.
Background:
2. Metabolism (meaning change, in my understanding as well as according to my dictionary) and flow imply a natural evolution of the process which I do not see in view of what we know about the importance of the authors in the publication process. Why not call it different stages and fates, as others have done. If the authors feel they absolutely need to stick to metabolism, they should explain what they mean.
3. Abstracts were used as the primary source of information". I know what the authors mean, but the
sentence is misleading. Please explain what kind of studies your are interested in: those using abstracts as a basis for follow up.

4. Rationale: I do not agree that little is known about the proportion of submitted abstracts accepted for presentation, or the determinants of acceptance. Why would you do a systematic review if there are no data available for inclusion? Please clarify why you chose to do a systematic review, especially in view of the recently updated meta-analysis on publication rates by R. Scherer. What makes your study different from hers?

Methods:
5. Please be more specific about the inclusion criteria - populations studied, research questions examined, study designs.
6. I wonder whether it would have been appropriate to set a time period limit. A 1957 meeting is not likely to be followed up as completely as a more recent one.
7. How did you separate between non human biology and basic science research meetings?
8. Why did you chose accepted abstracts only as the nominator of publication rates as opposed to submitted abstracts?
9. we assumed that publication occurred at the end of the reported follow-up interval" - what do you mean?
10. the four meeting characteristics" - this seems a very limited spectrum of potential determinants.
11. dichotomous data on abstract characteristics..." please specify
12. fixed effects model" - which?

Results
13. Please provide a detailed table of characteristics of the included studies
14. What countries were other than US”?
15. Table 1 is not very informative - what about including a flow diagram instead
16. It is a pity that some of the better reports on this topic could not be included because they combined data from several meetings. Perhaps you could appreciate these omitted reports in the discussion.
17. What makes you think analyses over- or underestimated results. In comparison to what? How can longer follow up periods lead to an overestimation of publication rates? Don't you think, a certain degree of underascertainment is likely to occur in any study of publication rates?
18. Are abstract acceptance rates associated with publication rates? There should be an inverse relationship - which would make it the more important to provide data on published/submitted rather than or at least in addition to published/accepted rates (see below)
19. I miss an appreciation of the validity or representativeness of the included studies.

Discussion
20. It seems somewhat odd that you originally chose to use accepted abstracts as the nominator, and then construct a published/submitted rate based on accepted/submitted and published/accepted rates - obviously considering this one of the major statements. Why not do an analysis using all submitted abstracts as the nominator. I appreciate that in most reports this information is not available. However, this would be the direct way to get this information! It could be complemented with or contrasted to the indirectly derived published/submitted rate.
21. I am not sure about the finding regarding basic science studies. Is your information on basic science studies representative, or may there have a problem as biology meetings were excluded? There may have been a disbalance in favor of clinically focused meetings, resulting in only more important basic science reports submitted.
22. I am not convinced about your findings on year of submission. At some meetings year is an important confounder due to logistic reasons only - the number of accepted abstracts depends directly on the space available for poster presentations. Financing may also be an important determinant of the number of abstracts accepted. As with the effect of meeting location (US vs. non US) these findings are very hard to interpret and possibly not very important, unless more is known about the specific background.
23. What were these other countries (see above)? It is difficult to make anything of this finding if we
don't whether we talk about e.g. Japan, or Western-Europe etc. Did you do a more detailed analysis exploring differences between various country meetings? What about international meetings vs. meetings primarily restricted to national associations?

24. I consider R. Scherer's meta-analysis very closely related to your study. Please be more specific in discussing your results in the light of hers.

Figures

25. Figure 1 and especially 2 confuse more than they help. I suggest simplify figure 1 and skip figure 2. Also, figure 3 is not helpful.

26. Figure 4: I doubt log rank tests are meaningful if curves cross (year). The authors should check with their statistician.

**Competing interests:**

None declared.