Author's response to reviews

Title: The level of non-citation of articles within a journal as a measure of quality

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Version: 2 Date: 23 Apr 2004

PDF covering letter
We are grateful for the reviewers’ comments.

We have addressed the points each reviewer has made, and indicated where changes to the manuscript have resulted, below. For clarity we have also highlighted the areas of changed text.

Response to Reviewer 1

General

Paragraph 1.

1. We have been transparent regarding the limitations of any form of citation analysis as a measure of quality of journals – hence our statement regarding the validity of non-citation for comparing journal fields. We believe non-citation is more logical than impact factor, as we are defining a high quality journal as one which most articles are cited and then actually measuring this. What, in contrast, does the impact factor really represent? We have demonstrated across 30,000 articles in over 200 journals that citation of articles within journals is not parametric. For impact factors to represent the number of citations an average article collects, this distribution must be parametric. We therefore believe that this article adds considerably to the discussion about how journal quality is currently defined.

2. We have referenced that Eugene Garfield acknowledges that impact factors are a poor measure of article quality. We are not aware that Garfield has stated that impact factors are a poor measure of journal quality – most of his work states the contrary

3. The reviewer states that it is “well known” that reviews receive more citations. However we could find very little evidence that this was the case. This is why we have included this within our analysis.

Paragraph 2

4. The issues of availability of journals and the relationship between impact factor/citation are of great interest – particularly with the advent of open access publishing. In our experience the presence of journals within an institution is more complex than the reviewer suggests and often reflects the needs of the institution rather than the actual costs of subscription. What is the reviewer’s evidence that low impact journals cost more to subscribe to than high impact journals (e.g Nature)?

Paragraph 3

5. Obtaining the same data as ISI use to calculate impact factor (i.e citations made in a particular year to articles published in the previous 2 years) is not practicable, given the way data is made available. As such we cannot calculate what the median impact factor would be. However we wholeheartedly agree that ISI should publish this number and not the mean.
6. We speculate about using just original articles to define the quality of a journal as a point for debate because of the differences we found between review articles and original articles. We have changed the text accordingly.

Paragraph 4

7. We have addressed the issue of temporal bias with an extra paragraph in the discussion.

Response to Reviewer 2

We agree with the reviewer that numeric measures of quality will always have limitations, but it is imperative that the scientific community are aware of these.

Response to Reviewer 3

General

Paragraph 1

1. We agree that any use of citation analysis to measure quality is to some extent imperfect due to the many reasons cited by the reviewer and others\textsuperscript{2}. We hope that our article is transparent in communicating these points to the reader.

2. Whilst we enjoy the analogy about the half empty beer glass, ranking by non-citation is more than just a change in perspective. Furthermore, if non-citations are the “glass half empty”, then the proportion of content cited, not impact factor, is the glass half full. As we emphasise, non-citation provides a statistically correct and logical definition of journal quality (i.e. giving an indication of the proportion of articles which have been useful to other authors). Due to the non-parametric distribution of citations to articles in the literature, it is clear that it is incorrect to interpret the impact factor of a journal as representative of the number of citations an average article in that journal will collect. Impact factor is therefore just a number with little tangible meaning.

Paragraph 2

3. We have addressed the issue of the use of only 2001 articles in the revised methods section and the discussion.

4. We agree the relationship between impact factor, self citation and non-citation would be interesting to examine in the future. Prior to embarking on further studies we await the response to this preliminary study of non-citation.

Major Compulsory Revisions

1. Background
   We have deleted the first paragraph and altered the wording in the last paragraph to reflect the reviewer’s comments.

2. Methods
In line with the reviewer’s request we have added an outline of our experimental design.
To further aid transparency within the report we have clarified to the reader how the data we have used to produce non-citation differs from citation data used to produce a journal’s impact factor.

Minor Essential Revisions

1. We entirely agree that science should be evidence based. However this part of the discussion is to examine whether it is necessary to rank journals. As such the sentence of “Indeed why is it necessary….” was intended to be contentious. We have reluctantly removed it.
2. We acknowledge that important clinical papers do get numerous citations and have altered the text accordingly. However there is no evidence that articles which change clinical practice get more citations than those that do not – this is an area we are currently investigating.
3. We have clarified this point.
4. The uploading of files has made the Figure 3a and 3b become Fig 4 and Fig 5. We have altered the way that these graphs are dealt with accordingly.
5. We have altered the scales as requested.

Reference List