

## PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form ([see an example](#)) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below. Some articles will have been accepted based in part or entirely on reviews undertaken for other BMJ Group journals. These will be reproduced where possible.

### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Fate of manuscripts rejected by a non-English general medical journal
<b>AUTHORS</b>	Siri Vinther

### VERSION 1 - REVIEW

<b>REVIEWER</b>	<p><b>Ana Marusic</b> Zagreb University School of Medicine, Anatomy</p> <p>I am the editor of an ICMJE journal member, of which the Journal of the Danish Medical Association is also a member. I therefore know one of the authors of the journal. We have never collaborated on a research project but have co-authored ICMJE statements.</p>
<b>REVIEW RETURNED</b>	02-May-2011

<b>THE STUDY</b>	This study does not involve patients, but data sources, so the answers to questions on that issue have been answered yes to indicate that they are actually not applicable.
<b>REPORTING &amp; ETHICS</b>	The authors submitted a STROBE checklist
<b>GENERAL COMMENTS</b>	<p>The topic of the article is important and relevant for the majority of scholarly journals – usually small journal, and often in non-English language. While it is well known what happens with submissions to prestigious journals, little is known what happens to the manuscripts rejected by non-English language journals. The data in the submitted manuscript present a relevant insight into this issue. My major concerns are the following:</p> <ol style="list-style-type: none"> <li>1. it is not clear why the search for publications was performed in PubMed and EMBASE and the citations in Web of Science and Google Scholar. According to the producers of EMBASE, this database includes PubMed items, plus other sources. On the other hand, SCOPUS, a citation database from the same publisher as EMBASE (Elsevier) has a 100% overlap with EMBASE and provides citation counts for indexed material (<a href="http://igitur-archive.library.uu.nl/DARLIN/2006-1220-200432/Scopus%20doorgelicht%20&amp;%20vergeleken%20-%20translated.pdf">http://igitur-archive.library.uu.nl/DARLIN/2006-1220-200432/Scopus%20doorgelicht%20&amp;%20vergeleken%20-%20translated.pdf</a>). It is not clear why the authors did not perform the obvious – search SCOPUS in addition to PubMed. This is a standard procedure in bibliometric research – searching Scopus, Web of Science and Google Scholar (Kulkarni AV, Aziz B, Shams I, Busse JW. Comparisons of citations in Web of Science, Scopus, and Google Scholar for articles published in general medical journals. JAMA 2009;302:1092-6.). Such a search would make the results comparable to other studies.</li> <li>2. The authors state that they also collected the information on the reasons for rejection in their journal, but they do not present that data in the Results section, although it may be relevant. There is also no detailed presentation of the data for the types of manuscripts</li> </ol>

	<p>submitted – it would be clearer if these data were presented in a table.</p> <p>3. The authors claim in the Discussion section that original research articles constituted 11% of rejected manuscripts, and then claim that 89% of the rejected original research articles were not published elsewhere. This is confusing, as the reader presumes that 89% of the rejected manuscripts were not original research. It should be clearly stated what was the percentage of original research articles that found other publication outlets after rejection.</p> <p>4. The main reason for non-publication of submitted articles may have been their lack of originality, ie, lack of original research contribution, and focus on local professional issues. This could be more clearly elaborated in the Discussion section, after summarizing the results and addressing the limitations of the study. The limitations of the study are not clearly addressed in the submitted manuscript.</p> <p>5. I cannot quite agree with the suggestion that locally relevant original research papers should be published only in English. Such papers have not only global but local relevance, and bilingual publishing, as practiced by some journals and publishers, may be a better solution (<a href="http://www.metmeetings.org/metm05/Kerans_fri_15-30.htm">http://www.metmeetings.org/metm05/Kerans_fri_15-30.htm</a>).</p> <p>Specific comments.</p> <p>Abstract, line 35 – medians should be followed by some indicator of data variability, at least range or interquartile range.</p> <p>Results, line 25 – the information on the rating of journals should be in the Methods section.</p> <p>Results, line 42-54 – as the data on the original research articles are important, they could be presented in a table for clearer view.</p> <p>Discussion, line 48-58 – the discussion on the importance of the journal with respect to journals with an impact factor is really not necessary or relevant for the topic – it draws attention from the main message of the paper.</p> <p>Discussion, line 40 – it would be nice to have a source for the statement that Danish is spoken by 0.08% of the world population.</p> <p>Table 2 – it is not clear why the year is stated with some of the journals' impact factors. This should be explained in the legend.</p>
--	---

<b>REVIEWER</b>	<b>Liz Wager</b> Publications Consultant, Sideview, UK
	I have no competing interests
<b>REVIEW RETURNED</b>	05-May-2011

<b>THE STUDY</b>	Several answers = not applicable
<b>GENERAL COMMENTS</b>	<p>General comments</p> <p>This study examined the fate of manuscripts rejected by a Danish language journal. It found that only 10% of them were subsequently published in indexed journals (PubMed and Embase) although the authors acknowledge that this does not account for those published in non-indexed journals. This figure is much lower than previous studies which were carried out at English language general journals such as BMJ and Annals of Internal Medicine. The findings are hard to interpret because the Danish journal has a very high (c90%) acceptance rate. I feel this is likely to explain the difference in findings from the previous studies which were done in high impact</p>

	<p>journals.</p> <p>The study methodology seems sound and the authors address the major limitations of the study.</p> <p>The manuscript would benefit from editing by a native English speaker</p> <p>Specific comments</p> <p>Abstract / key message instead of saying 'differing notably' I suggest it would be more helpful to indicate the direction of this difference (in case readers are not familiar with the earlier studies) i.e. notably (or considerably / markedly) lower than previous studies</p> <p>Methods (or perhaps Introduction) it would be useful to mention the acceptance/rejection rate of UfL (although I realise this is available in Table 1, but an average or range would be a helpful indicator for readers not familiar with UfL). The low rejection rate may also explain the discrepancy between the authors' findings and those of previous studies which were done at journals with much higher rejection rates.</p> <p>p 5, line 52: how was access to this data obtained (presumably it is not freely available)? Was this study done with cooperation with the journal? It is unusual for journals to release information on the identity of peer reviewers (unless they use open review) – the authors should comment on this.</p> <p>p7, line 52 – I did not understand this sentence it seems to disagree with the sentence immediately before it (ie that original research constituted 43% of the manuscripts)</p> <p>Discussion It would be interesting, for comparison, to know the average number of citations to papers published in UfL (or at least to a sample)</p> <p>The authors comment that original research was more likely to be published than review articles. In the case of non-systematic review articles, these may be considered secondary publications and there is therefore less ethical imperative to publish them. However, there is an ethical responsibility to publish original clinical research. It might even be argued that it was a good thing that the non-systematic reviews were never published if they were found to be biased / incomplete.</p> <p>The very low rejection rate of UfL makes these findings hard to interpret – the authors should discuss this in more detail.</p> <p>Minor corrections</p> <p>p5, line 8 should be 'the Journal ...' p5, line 21 should be 'relatively small' (not little) readership p5, line 33 should be 'specialty' (x2) not speciality (also on p 7 &amp; 8)</p>
--	--

Reviewer: Ana Marusic

1. It is not clear why the search for publications was performed in PubMed and EMBASE and the citations in Web of Science and Google Scholar. According to the producers of EMBASE, this database includes PubMed items, plus other sources. On the other hand, SCOPUS, a citation database from the same publisher as EMBASE (Elsevier) has a 100% overlap with EMBASE and provides citation counts for indexed material (<http://igitur-archive.library.uu.nl/DARLIN/2006-1220-200432/Scopus%20doorgelicht%20&%20vergeleken%20-%20translated.pdf>). It is not clear why the authors did not perform the obvious – search SCOPUS in addition to PubMed. This is a standard procedure in bibliometric research – searching Scopus, Web of Science and Google Scholar (Kulkarni AV, Aziz B, Shams I, Busse JW. Comparisons of citations in Web of Science, Scopus, and Google Scholar for articles published in general medical journals. JAMA 2009;302:1092-6.).

- We have searched the SCOPUS-database for citation counts and inserted the results (p. 5, 7).

2. The authors state that they also collected the information on the reasons for rejection in their journal, but they do not present that data in the Results section, although it may be relevant. There is also no detailed presentation of the data for the types of manuscripts submitted – it would be clearer if these data were presented in a table.

- Information about reasons for rejection is inserted in the Results section (p. 7)

- We have created figure 1a, 1b and table 3 to provide an overview.

3. The authors claim in the Discussion section that original research articles constituted 11% of rejected manuscripts, and then claim that 89% of the rejected original research articles were not published elsewhere. This is confusing, as the reader presumes that 89% of the rejected manuscripts were not original research. It should be clearly stated what was the percentage of original research articles that found other publication outlets after rejection.

- The section has been rephrased

- We have created figure 1a, 1b and table 3 to provide an overview.

- Please note that the creation of table 3 allows for a distinction between publication rates for manuscripts in general and for original research, respectively.

4. The main reason for non-publication of submitted articles may have been their lack of originality, ie, lack of original research contribution, and focus on local professional issues. This could be more clearly elaborated in the Discussion section, after summarizing the results and addressing the limitations of the study.

The limitations of the study are not clearly addressed in the submitted manuscript.

- This section has been elaborated and rephrased (p. 7-8)

- The limitations of the study have been addressed more clearly.

5. I cannot quite agree with the suggestion that locally relevant original research papers should be published only in English. Such papers have not only global but local relevance, and bilingual publishing, as practiced by some journals and publishers, may be a better solution ([http://www.metmeetings.org/metm05/Kerans\\_fri\\_15-30.htm](http://www.metmeetings.org/metm05/Kerans_fri_15-30.htm))

- We have mentioned this possibility of bilingual publication in the conclusion.

Specific comments.

Abstract, line 35 – medians should be followed by some indicator of data variability, at least range or interquartile range.

- Interquartile ranges have been added.

Results, line 25 – the information on the rating of journals should be in the Methods section.  
•This sentence is has copied to the relevant section.

Results, line 42-54 – as the data on the original research articles are important, they could be presented in a table for clearer view.  
•We have created figure 1a, 1b and table 3 to provide an overview.

Discussion, line 48-58 – the discussion on the importance of the journal with respect to journals with an impact factor is really not necessary or relevant for the topic – it draws attention from the main message of the paper.  
•This paragraph has been deleted.

Discussion, line 40 – it would be nice to have a source for the statement that Danish is spoken by 0.08% of the world population.  
•Relevant references have been added

Table 2 – it is not clear why the year is stated with some of the journals' impact factors. This should be explained in the legend.  
•The explanation in the legend is improved.

Reviewer: Liz Wager

Abstract / key message

instead of saying 'differing notably' I suggest it would be more helpful to indicate the direction of this difference (in case readers are not familiar with the earlier studies) i.e. notably (or considerably / markedly) lower than previous studies  
•This sentence has been rephrased in the abstract and article summary.

Methods (or perhaps Introduction)

it would be useful to mention the acceptance/rejection rate of UfL (although I realise this is available in Table 1, but an average or range would be a helpful indicator for readers not familiar with UfL). The low rejection rate may also explain the discrepancy between the authors' findings and those of previous studies which were done at journals with much higher rejection rates.  
•The acceptance rate of UfL has been added to the Results section (p. 6); we have commented on this acceptance rate in the Discussion section (p. 8).

p 5, line 52: how was access to this data obtained (presumably it is not freely available)? Was this study done with cooperation with the journal? It is unusual for journals to release information on the identity of peer reviewers (unless they use open review) – the authors should comment on this.  
•This has been clarified (p. 4).

p7, line 52 – I did not understand this sentence it seems to disagree with the sentence immediately before it (ie that original research constituted 43% of the manuscripts)  
•We have created figure 1a, 1b and table 3 to provide an overview.

Discussion

It would be interesting, for comparison, to know the average number of citations to papers published in UfL (or at least to a sample)  
•As UfL is not indexed in all databases, this otherwise interesting comparison is not possible.

The authors comment that original research was more likely to be published than review articles. In

the case of non-systematic review articles, these may be considered secondary publications and there is therefore less ethical imperative to publish them. However, there is an ethical responsibility to publish original clinical research. It might even be argued that it was a good thing that the non-systematic reviews were never published if they were found to be biased / incomplete.

- Relevant additions/comments have been made on this in the Discussion section

The very low rejection rate of UfL makes these findings hard to interpret – the authors should discuss this in more detail.

- The acceptance rate of UfL has been added to the Results section (p. 6); we have commented on this acceptance rate in the Discussion section (p. 8).

Minor corrections

p5, line 8 should be 'the Journal ...'

- corrected

p5, line 21 should be 'relatively small' (not little) readership

- corrected

p5, line 33 should be 'specialty' (x2) not speciality (also on p 7 & 8)

- corrected

#### VERSION 2 - REVIEW

<b>REVIEWER</b>	<b>Ana Marusic</b>
<b>REVIEW RETURNED</b>	19-May-2011

<b>THE STUDY</b>	This is not a study on patients, and CONSORT guideline is not relevant.
<b>GENERAL COMMENTS</b>	The revised version of the manuscript is significantly improved and the comments and concerns have all been addressed. There are just some minor spelling errors (such as "was considerable lower" instead of "was considerably lower" on page 2, line 46. It would be good to go over the manuscript and check style and spelling.

<b>REVIEWER</b>	<b>Elizabeth Wager</b>
<b>REVIEW RETURNED</b>	20-May-2011

<b>THE STUDY</b>	<p>The study question could be more clearly stated in the Intro. At the moment, the authors start talking about why the study is important before they define the study (ie to assess the fate of MS rejected by a Danish language journal)</p> <p>The MS would still benefit from a language edit -- it is understandable but there are a few grammatical errors</p> <p>The 1st sentence in the Methods confused me -- I suggest it should read 'investigators' because 'authors' can be confused with the authors of the original papers</p> <p>Some questions on the score sheet not applicable to this study</p>
------------------	--

<b>RESULTS &amp; CONCLUSIONS</b>	<p>Previous studies should be described in more detail rather than just saying that the findings were markedly different.</p> <p>In the Discussion, the actual number of MS (ie 21) should be mentioned to remind readers this was a very small sample so 10% is presumably just 2 papers.</p>
----------------------------------	--

## VERSION 2 – AUTHOR RESPONSE

Reviewer: Ana Marusic

The revised version of the manuscript is significantly improved and the comments and concerns have all been addressed. There are just some minor spelling errors (such as "was considerable lower" instead of "was considerably lower" on page 2, line 46.

- Corrected

It would be good to go over the manuscript and check style and spelling.

- The authors have gone over the manuscript again, checking spelling and phrasing. Hopefully, it is done satisfactorily.

Reviewer: Elizabeth Wager

The study question could be more clearly stated in the Intro. At the moment, the authors start talking about why the study is important before they define the study (ie to assess the fate of MS rejected by a Danish language journal)

- Rephrased / rearranged

The MS would still benefit from a language edit -- it is understandable but there are a few grammatical errors

- The authors have gone over the manuscript again, checking spelling and phrasing. Hopefully, it is done satisfactorily.

The 1st sentence in the Methods confused me -- I suggest it should read 'investigators' because 'authors' can be confused with the authors of the original papers

- Rephrased

Previous studies should be described in more detail rather than just saying that the findings were markedly different.

- Rephrased / elaborated

In the Discussion, the actual number of MS (ie 21) should be mentioned to remind readers this was a very small sample so 10% is presumably just 2 papers.

- Rephrased