

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	What can we learn from top-cited articles in inflammatory bowel disease? A bibliometric analysis and assessment of the level of evidence
AUTHORS	Azer, Samy; Azer, Sarah

VERSION 1 – REVIEW

REVIEWER	Yi Bu Indiana University, U.S.A.
REVIEW RETURNED	22-Jan-2018

GENERAL COMMENTS	<p>My research topic mainly lies in bibliometrics and scientometrics, so my suggestions focus more on this perspective.</p> <p>This paper learns top-cited papers and provides a thorough bibliometric analysis in the field of inflammatory bowel disease. I would like to reconsider it for publishing before a round of major revision. Here I provide some of my suggestions and hope the authors can consider:</p> <ol style="list-style-type: none">1. P. 6: The authors have listed several reasons for using top-cited articles in the field of IBD. However, when elaborating these reasons, they should at least list some references. Also, these reasons need more interpretations. For instance, why is this correct? "These articles reflect the work of key authors and research teams from particular institutes and countries that have made significant contribution to inflammatory bowel disease over the last 50-60 years".2. Why did you use SCI-Expanded instead of just SCI? Please detail this in the manuscript.3. One potential reason that there is almost no correlation between the number of citations and the papers' age might attribute to the difference between citation behaviors over time. For instance, in 1950 and 1960s, the authors cited fewer papers in their papers than now, partly because of the peer-review rule established in 1960s and 1970s, say Nature starts to peer review all of its newly-submitted papers in 1967, if I am not wrong (Sinatra, R., Deville, P., Szell, M., Wang, D., & Barabási, A. L. (2015). A century of physics. Nature Physics, 11(10), 791-796.).4. The ways to identify the gender of an author in this paper are good and accurate. But are there any authors' names that cannot be identified?5. What if a research changes his/her institution? Have you considered that in your data set? If so, how did you consider that?6. How to define the "same" institution? Say one author lists his institution as "Center for XX, School of YY, ZZ University", another lists as "School of YY, ZZ University". Do you still think they are in
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	<p>the same institution? Also, what if an author co-affiliates in two or more institutions?</p> <p>7. I would suggest the authors to visualize their citation network or other types of networks. You might want to use Gephi (https://gephi.org/) or other tools as a software to do that, and try to run Modularity algorithm (Newman, M. E. (2006). Modularity and community structure in networks. Proceedings of the national academy of sciences, 103(23), 8577-8582.). After that, you will be able to know the scientific community (structure) of the IBD field.</p>
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REVIEWER	Arfon G M T Powell Division of Cancer and Genetics, Cardiff University
REVIEW RETURNED	02-Apr-2018

GENERAL COMMENTS	<p>Thank you for the opportunity to review this manuscript. I have made a few comments below which the authors may find useful.</p> <p>Abstract</p> <ol style="list-style-type: none"> 1. Why did the authors aim to identify the top cited articles in IBD. What material effect will this have on the medical literature? 2. The methods section should include some details on the search terms. It would allow a reader to see the relevance of the results on glancing through the abstract. May encourage people to read the article. 3. The abstract would benefit from including informative data in the results section. Particularly related to the correlations and the number of articles by country. 4. One could argue that the agreement between evaluators doesn't necessarily need to be included in the results section of the abstract but should be in the main manuscript. This allows more space for informative data in the results section. 5. Grammatical errors in the conclusion section needs revised. 6. Quality of evidence is difficult to measure. A cohort study in prognostics is the best level of evidence yet in interventions it is not. Therefore, quality descriptors exist for different types of studies and the authors should consider this when making their final conclusions. <p>Introduction</p> <ol style="list-style-type: none"> 1. The introduction feels to long winded and needs revised. 2. The aims and hypotheses are not clear with several secondary outcome measures. The paper would be benefit from a focussed hypothesis aims. 3. The authors discuss the weaknesses of citations, however, this manuscript is based on citations and its derivatives. I don't think this adds any value to the introduction and may be best placed in the discussion. 4. The introduction would benefit from a major revision, streamlining the argument for the aims/hypotheses as mentioned above. There needs to be a relation between bibliometric analysis and the challenges of dealing with IBD. <p>Methodology</p>
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	<p>1. I don't think that the following sentence is necessary "the author who is a professor of medical education, consultant gastroenterologist, and a fellow of the American College of Gastroenterology together with a research assistant who has medical background". This does not explain how the investigation was conducted. The front page states the authors job titles/experience and therefore this additional information isn't needed.</p> <p>2. The search terms are exhaustive but it doesn't look as though a boolean search was conducted. Web of Science will normally give you a boolean style search which should be included in the methodology. A combined term1 Or term 2 or term 3 etc will give a final set of result. This methodology is different to others published including my own and a reference of more information is needed. I imagine that this will yield the same results but this should be compared with a top 50 search using the other methodology to ensure it is translatable.</p> <p>3. The level of evidence will differ depending on the type of study. Prognosis, diagnostic, treatment or epidemiology. The authors should ensure that an appropriate evidence level is to the correct study. A cohort study for an intervention would be low level, yet in prognosis it could be level 1 evidence.</p> <p>Results</p> <p>1. The abstract states the USA and UK were leading the number of publications in the top 50, yet there is not information in the results section. Maybe I am missing it but I can't seem to see it.</p> <p>2.The results generally need to be revised and streamlined.</p> <p>Discussion</p> <p>1. The discussion needs revised entirely. As it stands, it simply offers a commentary of the results with no really critique. Several similar studies have looked at citations, time and evidence. The authors should compare and contrast their study with other published studies to reveal what is new.</p> <p>2. The conclusion section needs to be shorter and streamlined. Again this is a commentary of the study rather than 2-3 sentences of a conclusion.</p> <p>General</p> <p>A similar study has already been published in ulcerative colitis (Connelly TM, Devane L, Kelly JC, Wrafter P, Messaris E. The 100 classic papers in ulcerative colitis: a bibliometric analysis. Expert Rev Gastroenterol Hepatol. 2016 Aug 22:1-9.website: https://www.tandfonline.com/doi/abs/10.1080/17474124.2016.1216786) and therefore the authors need to state what makes their paper novel. This will improve the number of citations it will likely accrue and its relevance. The study lacks a conclusion which might be related to the lack of a priori hypothesis.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer name: Yi Bu

Indiana University, USA

We would like to thank the reviewer for his useful and helpful comments

1. P6. References have been added.
2. The Web of Science has been used in the search.
3. The suggestion made by the reviewer has been taken and a sentence has been added in the discussion about the changes in the citation behaviors over time and a reference has been added.
4. We found five authors' names that were difficult to identify the gender because the first name was not stated but an abbreviation of the first letter in the first name. We emailed the corresponding authors of two articles, but unfortunately, we did not receive a response from one of them. This information has been added under the methods.
5. We believe this question is a follow up on question number 4. We search Google database using the author's full name and IBD, we also search for other papers written by the author that may give a clue. In some cases, we searched ResearchGate, LinkedIn, and Google Scholar database as well. As stated under item 4, we contacted the corresponding author as a final way to get information. This information has been added to the methods.
6. We mean by an institute, the university where they belong to. So in regard to your example, it will be ZZ university. If the authors belong to two universities, this will be considered two different institutes.
7. We thank the author for the suggestion, however, we felt after doing some work that this will shift the manuscript from its focus and we need to analyze the figure, which is not part of our objectives.

Reviewer:2

Reviewer name: Arfon GMT Powell

Division of cancer and genetics, Cardiff University

Abstract

1. The aim to identify the top cited articles in IBD has been added and the effect on the medical literature has been stated.
2. The search terms have been added.
3. The informative data have been added to the results section including correlation and number of publications for each country.
4. The sentence on the agreement between evaluators has been omitted from the abstract.
5. Grammatical errors have been corrected.
6. The quality of evidence has been reviewed for all articles. The final conclusion has been reviewed and amended.

Introduction

1. The introduction has been reviewed and shortened.
2. The aims and hypotheses have been clearly defined. A focused hypothesis has been stated.
3. We agree with the reviewer, and the discussion part about the weakness of citations has been moved to the discussion. The derivatives related to citations have been moved to the discussion.
4. A major revision has been conducted to the introduction as suggested.

Methodology

1. The sentence has been omitted as suggested.
2. The method has been amended and references have been added.
3. The level of evidence of all articles has been reviewed.

Results

1. A sentence summarizing the leading countries and number of publications produced by each has been added.
2. The results section has been reviewed and improved.

Discussion

1. The discussion has been reviewed and a critique has been added to compare and contrast with other related publications/studies in the literature regarding citations, time, and evidence.
2. The conclusion section has been shortened and streamlined.

General

We thank the reviewer for bringing our attention to this article, we have compared and contrasted our findings against the authors' reported findings. The reference has been added.

We trust that we have addressed the points raised by the reviewers' and editors. We look forward to hearing from you.

VERSION 2 – REVIEW

REVIEWER	Yi Bu Indiana University, U.S.A.
REVIEW RETURNED	02-May-2018
GENERAL COMMENTS	This paper has been significantly improved. I believe it is ready for publishing. I really appreciate the authors' revision.
REVIEWER	Arfon Powell Division of Cancer and Genetics, Cardiff University, South Wales, United Kingdom
REVIEW RETURNED	13-May-2018
GENERAL COMMENTS	Thank you for the opportunity to review this manuscript. The authors have made a considerable effort to address the suggestions made by both reviewers. My only suggestion is some of the tables could be included supplementary files such as table 5.

VERSION 2 – AUTHOR RESPONSE

Reviewer: 1

Reviewer name: Yi Bu

Indiana University, USA

We would like to thank the reviewer for his positive comments. No changes were requested.

Reviewer: 2

Reviewer name: Arfon GMT Powell

Division of cancer and genetics, Cardiff University

We would like to thank the reviewer for his positive comments.

We believe the suggestion to move table 5 as a supplementary file is not needed as this table is important to the article.