

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Comparison of the contributions of female and male authors to medical research in 2000 and 2015: a cross-sectional study
AUTHORS	Gayet-Ageron, Angele; Poncet, Antoine; Perneger, Thomas

VERSION 1 – REVIEW

REVIEWER	Elizabeth Whipple Indiana University School of Medicine, USA
REVIEW RETURNED	29-Jun-2018

GENERAL COMMENTS	<p>While this seems like an interesting topic, my main concern is the inclusion of only 2 years' worth of data for the analysis. The authors cite several other papers to say they are using the same methodology, both those other papers looked across several journals to pull their data. I would like other examples of this methodology being validated, or for the authors to include at least another year (maybe halfway in between--2008?) to feel more confident in their conclusions and results. I am concerned that two years' worth is not sufficient to show trends over time, while I do know that a decade every 4 years can show trends and has been validated:</p> <p>Bibliometric Analysis of Female Authorship Trends and Collaboration Dynamics Over JBMR's 30-Year History. Winger AE, Fischer JP, Likine EF, Gudeman AS, Brinker AR, Ryu J, Maupin KA, Lunsford S, Whipple EC, Loder RT, Kacena MA. J Bone Miner Res. 2017 Dec;32(12):2405-2414. doi: 10.1002/jbmr.3232. Epub 2017 Sep 6. PMID: 28777473</p> <p>I recognize this would be an insignificant undertaking, yet I think it would be necessary to truly show trends over time as the authors are hoping to do.</p> <p>I also have an issue with Europe being referred to as a country, since it is a continent made up of many countries.</p>
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REVIEWER	Joachim Hasebrook zeb.business school Steinbeis University Berlin, Germany
REVIEW RETURNED	01-Aug-2018

GENERAL COMMENTS	<p>The paper addresses an important and highly underrepresented issue: While a "female shift" or "feminization" in medicine is widely discussed, there is only little empirical evidence about the role of women in academic medicine and medical research.</p> <p>The paper is very well written, all methods and results are clearly presented. I would like to point out one minor revision and two</p>
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	<p>additional aspects which might - or might not - be regarded while revising the manuscript:</p> <ul style="list-style-type: none"> - Minor revision: The inference statistics applied is appropriate. As correlation indices are (partly implicitly) compared to each other, the results should be shown as standardized indices (that is, Fisher z values). As correlation indices are directly depending from the variance within the data sample, comparing different groups or sample asks for data standardization before any comparison. - Additional aspects: The explanatory model presented by the authors for the different contributions of men and women is related to (1) content / subject matter and (2) age / experience. It would be interesting to examine whether (1) the topic (e.g. keywords about the actual subject matter of the article) has an influence on the gender gaps. (2) As the age contribution of male and female physicians as well documented for many countries (OECD databases), it might be useful to check for the nationality of the authors and whether they come from a background with many young female physicians / researchers (e.g. OECD calculates the proportion of female physicians under the age of 35 years; even the change of the proportion over the last 15 years might bring some valuable insights). <p>These two hints should be considered as optional additions, not mandatory revisions.</p>
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VERSION 1 – AUTHOR RESPONSE

*** Reviewer 1

Answer 1:

We agree that an analysis of multiple journals and multiple years would provide a more solid database; more data is always better, and we intended initially to include a sample of journals. However we found out that journals do not record author contributions in a consistent way, and this forced us to include one journal only. We selected the Annals of Internal Medicine because this journal introduced a structured contribution format in 2000 and has kept it unchanged throughout the years (see also page 6 at the end of introduction). Adding intervening years (2001-2014) would indeed describe precisely a time trend, but this was not our main objective. A priori we did not know if the gender-division of research tasks changed over time or not, and by selecting extreme years, 2000 and 2015, we maximised the chances of observing a contrast (still assuming that most societal changes are gradual). Our main result is that the gender division of research activities has NOT changed all that much; there are more women authors but they do not have the same roles as males, and the gender inequality has remained unchanged between 2000 and 2015. Therefore we believe that a year by year analysis would add little value.

We have added a reference to the paper by Wininger et al. (reference n° 16) and we mentioned in the discussion that the percentage of female first authors was the highest in Europe compared to other regions (page 13).

Answer 2:

Thank you for your remark about Europe being referred to as a country. We have modified the wording in the current version of the manuscript by using “country/continent” in place of “country” (pages 8, 9, 26 in Table 1).

*** Reviewer 2

Answer 1:

We report odds ratios, not correlations, since the variables (i.e., presence or absence of a contribution) are binary. In this case the Fisher z transformation is not applicable. If we misunderstand your comment please clarify.

Answer 2 about additional aspects:

Since we analyzed author contributions for the same set of articles, the male and female coauthors were "matched" on subject matter. If there was a simple way of categorizing subject matter we might perform a stratified analysis, but this entails a risk of type 1 errors. We have no a priori hypothesis that gender roles should change in one area of research and not in another. We would prefer to present overall results only, as planned.

However, we have added some description on the subject matter in this new version of the paper and compared their distribution between the two years (methods on page 9; results on pages 10-11).

We did not have data on the nationality of the authors. Moreover, as married women could use either their birth name or their married names, it could introduce some misclassification bias.