

## PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (<http://bmjopen.bmj.com/site/about/resources/checklist.pdf>) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Gender disparities in high-quality dermatology research - a descriptive bibliometric study on scientific authorships
<b>AUTHORS</b>	Bendels, Michael; Dietz, Michelle; Brüggmann, Dörthe; Oremek, Gerhard; Schöffel, Norman; Groneberg, David

### VERSION 1 – REVIEW

<b>REVIEWER</b>	Juan M Pericas Health Inequalities Research Group, Johns Hopkins University- Pompeu Fabra University Public Policy Center Health Systems Research Group, ISGlobal Hospital Clínic de Barcelona Barcelona, Spain
<b>REVIEW RETURNED</b>	16-Nov-2017

<b>GENERAL COMMENTS</b>	<p style="text-align: center;"><b>bmjopen-2017-020089</b></p> <p>Using Gendermetrics, the authors provide an analysis of the females' contributions to publications in Q1 Dermatology journals from 2008 to 2017. Results are presented separately for first, co- and last authorships. According to authors, findings suggest that females are slightly underrepresented at prestigious authorships compared to men but relatively well-represented as compared to other academic disciplines.</p> <p>The manuscript is well-written and adds relevant information to a still emerging field of studies. However, I would like to make some remarks:</p> <p><b><u>Major comments</u></b></p> <p>1. Given that due to various reasons (basically exclusion of journals and countries because of use of initials instead of full names) some countries are even more underrepresented than in the mainstream</p>
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literature concerning scientific production, I am not sure that this study shall attempt to provide a global picture on female authorship in dermatology but instead that of High-income Western countries.

2. Moreover, authors removed from the analysis those countries in which the production was lower (<750 authorships) to increase the statistical power. I am afraid that this might remarkably bias the results. Authors found interesting differences between HIC Western countries (e.g. Denmark, Finland, the Netherlands vs. Spain, Italy, Japan, Austria) and superficially discuss some of the potential causes. Nonetheless, the mechanisms and historical and cultural fingerprint of machismo and female discrimination in other countries cannot be analyzed with the same lens. The exclusion of China from the analysis, for instance, might have an enormous impact on the final results and conclusions. So, unless authors are willing to review all articles to find out if a determinate author is a male or a female, I will focus on

3. If I am not wrong, authors have used as a reference the Q1 in Dermatology by May, 2017, and looked retrospectively at the identified journals (once those with initials restrictions were ruled out). Thus, this might mean that authors have considered that Q1 remained the same from 2008 to 2017. Please, clarify. If that is the case, authors should find a way to chose journals using an indicator of top-impact that is uniformly distributed along the study period.

4. Related to the previous comment, and mainly focused on how results are depicted in Table 2, I believe that a Q1, in any discipline, an even more if it has been changed due to restriction criteria, does not reflect a homogenous platform in terms of prestige, visibility and therefore, a potential impact of gender in authorship. Consequently, I'd suggest authors to relate every journal to the median impact factor (or other indicator of the journal importance in the academic field) of each journal along the period and analyse how this influences the prestige index and FAP/FAOR.

**Minor comments**

5. Introduction, page 5, lines 7-12. Even if you decide to focus only in HIC Western countries (see comments 1 and 2), do not provide context info only from the U.S.

6. Introduction, page 5, line 23. References are incorrectly cited (5-9)

7. Introduction, page 6, lines 10-17. Paraphrasing Murphy (ref. 15), authors cite the following: "In original medical articles, the assignment of authorship follows, by convention, the rule that "the first author indicates the person whose work underlies the paper as a whole", whereas the last authorship "indicates a person whose work or role made the study possible without necessarily doing the actual work". However, in the current study the two last authors share equal contribution while from the contributorship statement in page 22, one might deduce that actually most work has been performed by the first author. Could you please expand further on that?

8. Methods, pages 8 and 9. Supplementary figures are not cited in numerical order. Fix it, please.

9. Discussion. Include a limitations section addressing all the methodological and conceptual issues of the article. Besides the points mentioned above, please give some support to or change the following statement made in page 21: "it is plausible to prognosticate a considerable increase of women in academic leadership positions in the next decade. This trend will likely be intense due to the high annual increase of female authorships (1.74%) with the highest rates for the last author position (2.97%), and the global trend of more and more female physicians entering the field of medicine". At least you need to address two points: Is it really "global"? Is quantitative change (more women entering medicine and more articles published) synonymous of power turnover? My suggestion is to be careful with this kind of statements. There is still a long way to go and the history is full of examples of power being held by minorities.

	<p>Thanks.</p> <p>10. References. Please, review the references. I am afraid that a number of journals' names (abbreviations) are incorrectly written.</p>
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<b>REVIEWER</b>	Jennifer Plank-Bazinet National Institutes of Health, United States of America
<b>REVIEW RETURNED</b>	20-Nov-2017

<b>GENERAL COMMENTS</b>	<p>Major concerns:</p> <ol style="list-style-type: none"> <li>1. One conflating factor in this analysis is the number of women in each career stage in the Academic Dermatology pipeline. For example, if there are a great percentage of men in the professional stages, and a greater number of women in residency, the results provided in the paper are exactly what would be expected. Therefore, a more comprehensive analysis would include normalizing the percentage of authorships to the percentage of women at each career stage. The analysis as it was conducted can not distinguish between 2 possibilities: representation of women in key authorship positions OR representation of women at key career stages.</li> <li>2. One of the conclusions in the abstract is "Female scholars are well-represented in the field of high-quality dermatological research compared to other medical disciplines." This conclusion is not supported by the publication.</li> <li>3. The introduction says "we here focused on the following question: When gender disparities are present in academic rank, how is gender balanced in the academic community that undertakes and publishes high-quality clinical, translational, and basic research in dermatology worldwide?" This statements is problematic for a few reasons. First, a normalization by representation of women by rank would be needed to address this question. Second, there are only a subset of countries included in the analysis due to logistical concerns. Finally, many of the journals included in the analysis would not capture basic research.</li> </ol> <p>Minor concerns:</p> <ol style="list-style-type: none"> <li>1. Sex and gender are used interchangeably throughout the text. This should be corrected as they are distinct terms.</li> <li>2. Page 12- the use of the percentages of women at each authorship point are confusing. Recommend removing. Intuitively, 50.2% does not seem like relatively more first-authorships.</li> <li>3. Page 13, Line 41- the statement "no single journal currently exists" is misleading as the authors mean to imply "no journal in our analysis".</li> </ol>
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<b>REVIEWER</b>	Pavel Ovseiko University of Oxford, United Kingdom
<b>REVIEW RETURNED</b>	25-Nov-2017

<b>GENERAL COMMENTS</b>	<p>This article makes an important contribution to addressing gender imbalance in science (see <a href="http://www.nature.com/news/gender-imbalance-in-science-journals-is-still-pervasive-1.21348">http://www.nature.com/news/gender-imbalance-in-science-journals-is-still-pervasive-1.21348</a>) by investigating gender disparities in high-quality dermatology research. The article is designed as a rigorous bibliometric study using an innovative Gendermetrics.NET platform. To the best of my knowledge, the study uses adequate statistical methods and the</p>
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results are reported robustly. I recommend that the article is accepted for publication as it is.

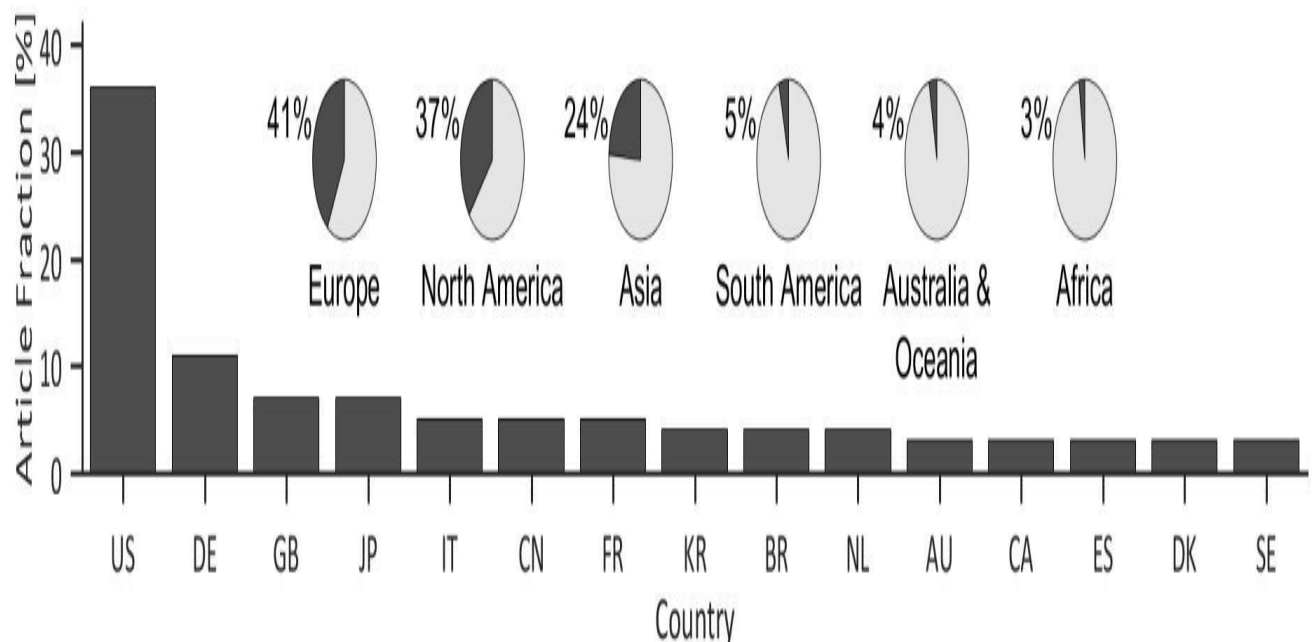
### VERSION 1 – AUTHOR RESPONSE

#### Reviewer 1:

1. Given that due to various reasons (basically exclusion of journals and countries because of use of initials instead of full names) some countries are even more underrepresented than in the mainstream literature concerning scientific production, I am not sure that this study shall attempt to provide a global picture on female authorship in dermatology but instead that of High-income Western countries.

#### Reply:

We agree with the referee that the word global can be misleading. However, the high-quality dermatological research is (at least quantitatively) almost exclusively dominated by the Western countries, especially the U.S. (more than every third article had by at least one author from an U.S. American institution). We refer here to supplement figure 3E showing the 15 top most productive countries and their fraction of articles. In this respect, the study reflects the actual global situation of women in high-impact dermatological research. We think that the global analysis is important as it makes a statement about the scientific community as a whole and enables thereby comparisons between different subject areas and disciplines.



2. Moreover, authors removed from the analysis those countries in which the production was lowered (<750 authorships) to increase the statistical power. I am afraid that this might remarkably bias the results. Authors found interesting differences between HIC Western countries (e.g. Denmark, Finland, the Netherlands vs. Spain, Italy, Japan, Austria) and superficially discuss some of the potential causes. Nonetheless, the mechanisms and historical and cultural fingerprint of machismo and female discrimination in other countries cannot be analyzed with the same lens. The exclusion of China from the analysis, for instance, might have an enormous impact on the final results and conclusions. So,

unless authors are willing to review all articles to find out if a determinate author is a male or a female, I will focus on.

**Reply:**

Methodically, it is obvious that we had to define a quantitative threshold criterion for the inclusion of a country, particularly with regard to the confidence intervals of the FAOR-classification (if N is too low, all statistical differences between the two genders vanish).

Importantly, our pure descriptive methodology does not allow for analysis of "the mechanisms and historical and cultural fingerprint of machismo and female discrimination". Rather, our objective was to provide an objective, quantitative and neutral evaluation of the situation, particularly in the light of the different and individual life paths and experiences of scientists.

The contribution of Chinese institutions to the global research output is relatively low as only 5% of all authorships were held by authors from Chinese institutions.

We here analyzed a total of more than 100,000 authorships. Methodically, the gender determination cannot be performed manually. Indeed, a manual approach remains very difficult even for small datasets as a) in some cases information about the authors' gender is not or no longer available (e.g. in form of a portrait on a web page) and b) the interindividual variability between researchers weakens the reproducibility of the findings.

-> To address this important limitation, we added a paragraph 'methodical limitations' in the discussion section (compare to point 9).

3. If I am not wrong, authors have used as a reference the Q1 in Dermatology by May, 2017, and looked retrospectively at the identified journals (once those with initials restrictions were ruled out). Thus, this might mean that authors have considered that Q1 remained the same from 2008 to 2017. Please, clarify. If that is the case, authors should find a way to choose journals using an indicator of top-impact that is uniformly distributed along the study period.

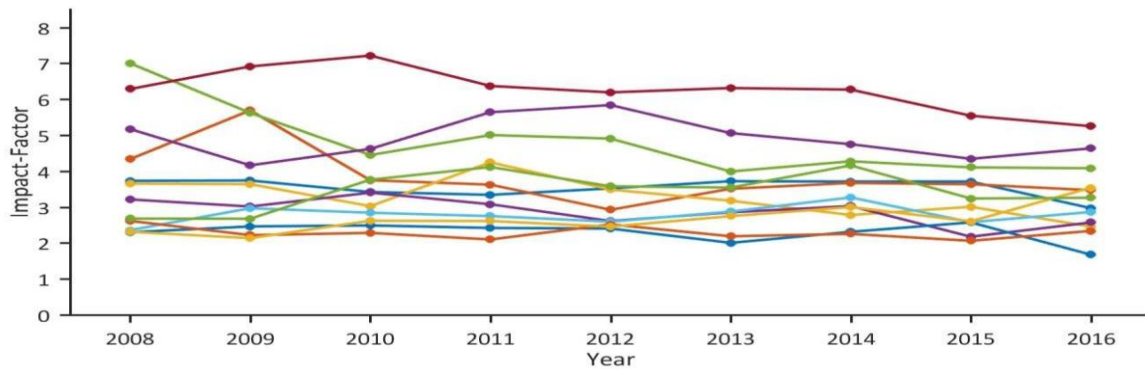
We clarified this issue:

Research articles from high impact dermatology journals listed in the Scimago Journal & Country Rank database (<http://www.scimagojr.com/journalrank.php?category=2708>) were acquired on May 15, 2017 from the Web of Science Core Collection (Thomson Reuters). The journals constitute the subset of dermatological Q1 journals in 2016 representing the top 25% of the corresponding impact factor distribution.

4. Related to the previous comment, and mainly focused on how results are depicted in Table 2, I believe that a Q1, in any discipline, and even more if it has been changed due to restriction criteria, does not reflect a homogenous platform in terms of prestige, visibility and therefore, a potential impact of gender in authorship. Consequently, I'd suggest authors to relate every journal to the median impact

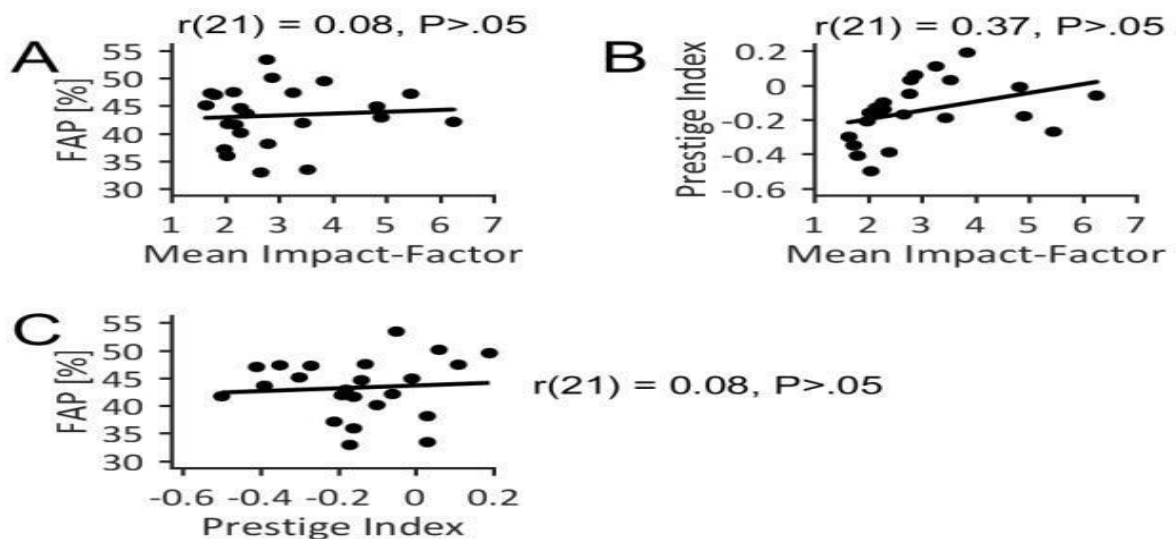
factor (or other indicator of the journal importance in the academic field) of each journal along the period and analyse how this influences the prestige index and FAP/FAOR.

We agree that there is some variability in the Q1-composition throughout the period 2008 to 2017, as documented by the figure below showing the Impact-Factor of journals by year (impact factors were extracted from the SCIJournal-Webpage for each journal, e.g. <http://www.scijournal.org/impact-factor-of-WOUND-REPAIR-REGEN.shtml>, only journals with yearly updated impact factors where shown).



Consequently, we changed the indicator of journal quality from Scientific Journal Rank (SJR) to Mean Impact Factor calculated over the years 2008 - 2016/2017.

Notwithstanding, the results of the correlation analysis show no significant changes, as we reveal no significant correlation between the three parameters FAP, Prestige Index and Mean Impact Factor:



Minor comments

5. Introduction, page 5, lines 7-12. Even if you decide to focus only in HIC Western countries (see comments 1 and 2), do not provide context info only from the U.S.

We added the results from recent studies about female editors-in-chief of dermatology journals by Gollins et al. (2017) and about sex and leadership in academic dermatology by Shi et al (2017).

6. Introduction, page 5, line 23. References are incorrectly cited (5-9)

Corrected.

7. Introduction, page 6, lines 10-17. Paraphrasing Murphy (ref. 15), authors cite the following: "In original medical articles, the assignment of authorship follows, by convention, the rule that "the first author indicates the person whose work underlies the paper as a whole", whereas the last authorship "indicates a person whose work or role made the study possible without necessarily doing the actual work". However, in the current study the two last authors share equal contribution while from the contributorship statement in page 22, one might deduce that actually most work has been performed by the first author. Could you please expand further on that?

The technical part of the submission process was done by our MD-student Michelle C. Dietz as 'an exercise'. She added independently a contributor ship statement after request by the editorial office (see E-Mail below) and forgot to mention Norman Schöffel as a designer of the study. She was not aware of the importance of this statement. However, as the corresponding author and doctoral supervisor I assume full responsibility for this error.

19-Oct-2017

Dear Miss Dietz,

Thank you for submitting your manuscript entitled "Gender disparities in high-quality dermatology research – a study on scientific authorships" (manuscript ID bmjopen-2017-020089) to BMJ Open. (...)

**- Please embed your CONTRIBUTOR SHIP STATEMENT in your main document file as shown in scholar one.**

8. Methods, pages 8 and 9. Supplementary figures are not cited in numerical order. Fix it, please.

Corrected.

9. Discussion. Include a limitations section addressing all the methodological and conceptual issues of the article. Besides the points mentioned above, please give some support to or change the following statement made in page 21: "it is plausible to prognosticate a considerable increase of women in academic leadership positions in the next decade. This trend will likely be intense due to the high annual increase of female authorships (1.74%) with the highest rates for the last author position (2.97%), and the global trend of more and more female physicians entering the field of medicine". At least you need to address two points: Is it really "global"? Is quantitative change (more women entering medicine and more articles published) synonymous of power turnover? My suggestion is to be careful with this kind of statements. There is still a long way to go and the history is full of examples of power being held by minorities. Thanks.

- We added a methodical limitations section.





							in	rates
						multi-author articles	highest impact journals	
<b>Q1 Dermatology</b>	43.0%	1.41	1.07	0.60	-0.11	Stable	Stable	minor
<b>Epilepsy<sup>17</sup></b>	39.6%	1.25	1.17	0.57	-0.22	Decline	-	major
<b>Schizophrenia<sup>15</sup></b>	37.6%	1.30	1.20	0.57	-0.22	Sharp Decline	-	major
<b>Lung Cancer<sup>24</sup></b>	31.3%	1.22	1.19	0.59	-0.22	Sharp Decline	-	minor
<b>Nature Index Journals<sup>36</sup></b>	29.8%	1.19	1.35	0.47	-0.42	Sharp Decline	Decline	major

**Table 3: Synopsis of different subject areas.** In high-quality dermatological research, the integration of female scholars is advanced as compared to other (medical) disciplines. However, in all subject areas that have been examined until now, a considerable career dichotomy is still present, with many female researchers at the beginning of their career and few women in academic leadership positions. Please note that the Nature Index offers a database for the specific analysis of high impact scientific efforts from the journal categories of multidisciplinary, life science, earth & environmental, and physics<sup>46</sup> (physics was excluded from analysis).

3. The introduction says "we here focused on the following question: When gender disparities are present in academic rank, how is gender balanced in the academic community that undertakes and publishes high-quality clinical, translational, and basic research in dermatology worldwide?" This statements is problematic for a few reasons. First, a normalization by representation of women by rank would be needed to address this question. Second, there are only a subset of countries included in the analysis due to logistical concerns. Finally, many of the journals included in the analysis would not capture basic research.

We removed this imprecise statement.

Minor concerns:

1. Sex and gender are used interchangeably throughout the text. This should be corrected as they are distinct terms.

The name (birthname or assumed) reflects a person's social identity and thus is related primarily to their gender.

□ In the introduction, we used the term "sex" since the cited article uses the term.

Sadeghpour M, Bernstein I, Ko C, Jacobe H. Role of **sex** in academic dermatology: results from a national survey. Archives of dermatology. 2012;148(7):809-814.

However, we substitute the term "sex" with "gender".

2. Page 12- the use of the percentages of women at each authorship point are confusing. Recommend removing. Intuitively, 50.2% does not seem like relatively more first-authorships.

□ Please note that the relation refers to the total proportion of female authorships (43.0%).

The analysis reveals an underrepresentation of female authorships with a FAP of 43.0% (Fig. 1A, bottom), relatively more first-authorships (50.2%), an almost equal proportion of female co-authorships (43.7%) and a substantially less fraction of last-authorships (33.1%).

We believe that the percentages of female first, co and last authorships are a core result of the study.

3. Page 13, Line 41- the statement "no single journal currently exists" is misleading as the authors mean to imply "no journal in our analysis".

Corrected!

**Reviewer 3:**

This article makes an important contribution to addressing gender imbalance in science (see [http://www.nature.com/news/gender-imbalance-in-science-journals-is-still-pervasive-](http://www.nature.com/news/gender-imbalance-in-science-journals-is-still-pervasive-1.21348)

1.21348) by investigating gender disparities in high-quality dermatology research. The article is designed as a rigorous bibliometric study using an innovative Gendermetrics.NET platform. To the best of my knowledge, the study uses adequate statistical methods and the results are reported robustly. I recommend that the article is accepted for publication as it is.

We appreciate all of your insightful comments. Thank you for taking the time and energy to help us improve the paper.

**VERSION 2 – REVIEW**

<b>REVIEWER</b>	Jennifer Plank-Bazinet National Institutes of Health, United States
<b>REVIEW RETURNED</b>	12-Feb-2018

<b>GENERAL COMMENTS</b>	I do not have the expertise to conduct a statistical analysis of the publication. However, it appears as though reviewer 3 has conducted such an analysis, and I am satisfied with that result.
<b>REVIEWER</b>	Juan M Pericàs Health Inequalities Research Group, JHU-UPF Public Policy Center ISGlobal, Hospital Clínic de Barcelona Barcelona, Spain
<b>REVIEW RETURNED</b>	14-Feb-2018
<b>GENERAL COMMENTS</b>	The authors have accurately addressed all main remarks raised in the first review.

### VERSION 2 – AUTHOR RESPONSE

Response to Editor

Editor:

Please include the study design and setting in the title. This is the preferred format of the journal.

Reply:

We changed the title to 'Gender disparities in high-quality dermatology research - a descriptive bibliometric study on scientific authorships'

Please discuss the limitations of the study in the discussion section.

Reply:

We modified the subsection 'Limitations of the study' in the discussion.



