Author’s response to reviews

Title: Success in publication by postgraduate students in psychiatry in Brazil: an empirical evaluation of the relative influence of English proficiency and advisor expertise

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Author’s response to reviews: see over
To Prof. Fernando Marques  
Academic Editor, BioMed Central Editorial

RE: MS: 1480494731107014  
Title: Success in publication by postgraduate students in psychiatry in Brazil: an empirical evaluation of the relative influence of English proficiency and advisor expertise

Dear Professor Fernando Marques,

Many thanks for your letter dated on April 9th 2014, referring to the above manuscript and for granting us the opportunity to re-submit the paper after making the revisions that were requested.  
Please find below our answers to each of the concerns/suggestions raised by the reviewers.  
We sincerely hope that you will judge that we have addressed the reviewers’ comments and concerns satisfactorily. We thank you very much for your attention, and look forward to hearing from you in due course.

Yours sincerely,

Alexandre Cunha  
Geraldo F. Busatto  
(On behalf of all authors)
Reviewer: David I. I Hanauer

Major Compulsory Revisions:

1. Appropriate Literature Review: The fields of applied linguistics and to a certain extent literacy education have spent about twenty years considering the question of how research articles are published in English as a Second Language. Researchers such as myself, have researched and looked carefully at the processes of publication. The issues are complex and range from educational background, linguistic knowledge, literacy ability, psychological processes, emotive responses, cultural interactions, social networks, scientific training, editorial bias, global developments in usage of English and economic resources. The current paper does not address this wide range of contexts and issues that influence the process of publication. I think this paper would greatly benefit from a far deeper understanding of the processes that are being explored. This body of literature could inform a wide range of aspects of this paper. For an overview of this literature you might look at a book I published last year entitled: Scientific Writing in a Second Language. This might function as an easy entry point into the literature I think is crucial for the aims of your study.

>>We thank the referee for his thoughtful comments. We agree with the main points that he makes: that the fields of applied linguistics and literacy education have produced important research findings concerning the topic of our study, including literature produced by the reviewer himself; and that the issues involved in the process of publishing in English as a second language are complex, influenced by the several different variables cited by the reviewer. In order to take account of those points, we have included the following sentences in the Introduction section:

- “The issues involved in the publication of research articles in English as a second language have been extensively investigated for more than two decades (Hanauer and Englander, 2013) (see page 3, paragraph 2)."
- “…qualitative investigations in the fields of applied linguistics and literacy education have highlighted the influence of other variables in the process of successfully publishing in English as a second language, including educational and cultural backgrounds, types of academic and social networks, persistence and other psychological processes, scientific training, editorial bias, global developments in the usage of English, geographical location and socio-economic resources (Swales, 1990, Cargill & O'Connor, 2006; Cameron et al, 2009; Hanauer and Englander, 2013)” (see page 3, paragraph 2).

2. Study Description: Neither the study nor the results are adequately described. Your study utilized a survey tool which has several different parts and rating scales. To adequately understand the study and perhaps offer some support in ways of analyzing this data, it is important that the full survey and any
validation data concerning this survey (such as outcomes of an exploratory factor analysis) be presented. At the very least showing the actual scales would have been very beneficial. The same is true of the results section. I would have liked to have had access to all the descriptive data (even if this is in an appendix) and all outcomes of any inferential statistics conducted. By moving from the descriptive to the inferential and by presenting the full data set in table form, it would be far easier to understand what the results mean.

>> We concede that the description of both the questionnaire and the results should be improved. In accordance to such needs, the following changes have been made:

(a) We included an appendix (Appendix 1) with the full questionnaire translated into English.
(b) We included a table (Table 1) providing descriptive statistics regarding data for English proficiency, number of articles, impact factor for journals and H-index of supervisors.
(c) We included a table (Table 2) providing descriptive statistics regarding the provision of language editing assistance to students.
(d) We included an appendix (Appendix 2) providing the full list of ISI-indexed journals where articles were published, the number of authors in each article, and the language used by these journals.
(e) We included two tables (Table 3 and Table 4) providing all outcomes of the multiple regression analyses.
(f) The text for the revised Results section provides additional information regarding to the varied sources of editing assistance provided to students, apart from professional translators.

3. Interpretation: When reading this study without having access to full data, I was unsure what the outcomes actually mean. In some cases, the lack of significant relationships (particularly if they are predicted in the existing literature) is important. As it stands, I do not see where the contribution in this paper lies. It is possible that there is a contribution here and I encourage the authors to read the existing literature and allow others full access to their results so as to facilitate a discussion that might lead to better understanding of what this data actually means.

>> In the Discussion section, we introduced a new paragraph in order to take account of the observations of the reviewer (see page 13, paragraph 3). We state that the interpretation of our statistically significant relationships has to be made with caution given the complexity of the variables involved in the process of publishing in English. For instance, our study design did not allow the distinction between language proficiency and other intellectual and psychological skills of the scientists; it is conceivable that young researchers who are more proficient than their peers in English have also a better previous
scientific training or are also better in other intellectual domains that are instrumental to becoming a successful researcher, such as creativity or logical thinking. We also state that the advantage gained by the students who were more proficient in English could be related to broader linguistic issues, such as better comprehension and expression of verbal material in any language, which would certainly increase the chances of a publishing a scientific manuscript. We also mention, in this paragraph, acculturation processes that may influence on English language proficiency, as well as other relevant variables that could not be evaluated in our investigation, such as social networks, emotional issues and overall economic resources. We finish this paragraph by stating that future quantitative studies in settings as ours should attempt to control for the influence of those additional variables, as well as saying that replication of our findings is warranted in other emerging countries using samples of graduate students from different cultural, social and economic environments.
Reviewer: Carrie Cameron

Minor Essential Revisions:

1. A very few non-idiomatic usages in English (e.g., “invest on” rather than “invest in”). Please have native speaker double-check.

>> As requested, we asked a native speaker to proof-read the entire revised manuscript. We have replaced “invest in” by “invest on”, as requested, and several other minor mistakes have also been reviewed and corrected.

Major Compulsory Revisions:

2. Not a revision recommendation per se, but the authors can benefit from examining the substantial body of specialist literature on scientific writing and communication of L2 scientists working in English. After seeing some of this literature the authors may want to modify their introduction and discussion.

>> We deeply thank the reviewer for such a very interesting list of articles, several of which directly related to the topic of our paper. The inclusion of such references helps us to make the reading of our text clearer, more understandable and better contextualized. We have made substantial changes to the paper include information from several of the articles, as follows:

A. Swales JM. English as Tyrannosaurus rex. World Englishes. 1997;16(3):373-382 Cited in the Introduction section (see page 3, paragraph 1) and in the Discussion section (see page 14, paragraph 1).

B. Swales J. Genre Analysis: English in Academic and Research Settings. Cambridge: Cambridge University Press; 1990. Cited in the Introduction section (see page 3, paragraph 2) and in the Discussion section (see page 13, paragraph 2).


D. Cameron C, Chang S, Pagel W. Scientific English: A program for addressing linguistic barriers of international research trainees in the United States. Journal of Cancer Education. 2011;26(1):72-78. Cited in the Introduction section (see page 3, paragraph 2) and in the Discussion section (see page 14, paragraph 1).

E. Cameron C, Deming SP, Notzon B, Cantor SB, Broglio KR, Pagel W. Scientific writing training for academic physicians of diverse language backgrounds. Acad Med. 2009 Apr;84(4):505-510. Cited in the Introduction section (see page 3, paragraph 2) and in the Discussion section (see page 15, paragraph 1).

Reviewer: Christopher Baethge

Major points:

A) With this approach it is impossible to differentiate between language proficiency and other intellectual skills of the scientists. It is conceivable that young researchers who are better than their peers in English are better in other intellectual domains too that are instrumental to becoming a successful researcher (for example, creativity, logical thinking, focus, or whatever it takes).

>> We concede that our study design does not allow one to differentiate between language proficiency and other intellectual skills of the scientists, and we agree that young researchers who are better than their peers in English may also be better in other intellectual domains that are instrumental to becoming a successful researcher. This limitation is now clearly acknowledged in the revised Discussion section (see page 13, paragraph 2).

B) In general, current psychiatric research papers are authored by more than one writer, and even by more authors than the junior and the senior researcher (which are the researches considered in this paper). Therefore, publication success or failure may be the consequence of the skills of more scientists than the researchers under study. This factor – probably sometimes large, sometimes small – is not accounted for in this project.

>> This is also a very valid point. We have included such considerations in the revised Discussion section (see page 12, paragraph 1).

C) The regression models employed in this study came to different results: While the model using number of publications as dependent variable yielded no statistically significant result, the one with impact factor did. Still, it is imaginable (and should be shown by the authors) that both dependent variables do correlate to a considerable degree. This makes diverging results hard to interpret. At the very least the authors should mention this in the abstract. They should also in methods whether the multiple regression analyses were specified before the study started or whether they were conceived (and in what order) after the results had been watched by the authors. This is important because it gives the reader an idea of the reliability of the results (higher if analysis plan was pre-specified and if the significant multiple regression was the primary outcome).

>> We thank the reviewer for such thoughtful comments. Indeed, the variables “number of publications” and “impact factor of journals” are significantly related to each other in our study. As requested, this information is now included both in the Abstract (see page 2, line 14) and in the revised Results section (see
The strength of such correlation is moderate, and in our opinion, the diverging results of the two multiple regression analyses may be explained by the fact that such variables do measure different things. Also, we would like to clarify that the two multiple regression analyses were specified before the study started, given our view that those two outcome measures of publications are both relevant and different from each other. This is now stated in the revised Methods section (see page 7, paragraph 3).

Minor suggestions:

1. Add page numbers. For the purposes of this review I have added page numbers beginning with the page that starts with „Introduction” (page 1) and ending with the page that includes the acknowledgements (page 10).

   > As requested, we have included page numbering throughout the manuscript.

2. p. 1, first sentence: What is the definition of high in “high-impact scientific journals“?

   > In the first paragraph of the Introduction section, we have now provided two types of information:
   (a) We state that the impact of a scientific journal can be quantitatively measured by indices such as the average number of citations given to recently published articles.
   (b) We explain that postgraduate services in Brazil are nowadays classified and funded by the federal government based on a ranking that gives the greatest weight to the number of publications by students and their supervisors in peer-reviewed periodicals, as well as the impact factor of such periodicals as measured by the Institute for Scientific Information (ISI) (CAPES, 2011).
   We believe that such paragraph now informs, in an objective way, what is meant by “high impact” in the context of our investigation.

3. p. 1, first paragraph: Is reference 2 also considered to be the reference for the second to last sentence in this paragraph? If so, this should be indicated. Also, I do not think that a paper reporting on publications published between 1998 and 2002 qualifies as recent as the authors put it („in recent years“).

   > After completely reformulating the Introduction, this paragraph is now the fifth of this section of the manuscript. Regarding the first point raised by the reviewer, the sentence has been reformulated in order to clarify that the same reference applies to both statements. Regarding the second point, we do agree that the cited study reporting on publications published between 1998 and 2002 should not be referred to as a recent
one. On the other hand, this is the latest publication addressing this specific topic in Brazil. Therefore, we have decided to keep the citation to that study in our paper, but we withdrew the word “recent” in order to comply with the reviewer’s request.

4. p. 2, third paragraph („Participants and Procedures“), first sentence: typo in year: can’t be „May 2001“.

>> We apologize for the error. We have replaced “2001” by 2011”.

5. p. 3, first paragraph, description of the requirements for obtaining a doctorate at the Sao Paulo postgraduate program in psychiatry: The authors state that research students have to publish in an ISI indexed journal. As far as I understand, however, this doesn’t necessarily mean publishing in an English language journal: Many non-English journals have an impact factor. For example, the journal Revista de Psiquiatria Clinica, of the University of Sao Paulo, publishes research articles in Portuguese. This and nine other examples of psychiatric journals distributing original material in languages other than English – along with a citation analysis of articles so-published – have been described in more detail recently (BMC Psychiatry 2013, 13: 102). In this connection, it may be instructive to the reader if the authors presented a list of the journals the research students published in. In fact, why not present the complete list articles under study in a list of references to be published as supplementary material.

>> The referee is right when he reminds that the local rule to publish in an ISI-indexed journal does not necessarily mean publishing in an English language journal, since some non-English journals have an impact factor, such as the Revista de Psiquiatria Clinica, of the University of Sao Paulo. Following his suggestion, we have included a table in the paper showing the complete list of the articles published by each subject (see Appendix 2).

6. p. 3, second paragraph, first sentence: Is it possible to make the questionnaire available to the reader?

>> We thank the referee for this suggestion. As requested, we have included an appendix (Appendix 1) with the questionnaire translated into English.

7. p. 3, third paragraph, first sentence: If the authors received test scores of the English test in anonymized form how did they, in their analysis, assign the test scores to research students?
>> This is an important observation. We have modified the 2\textsuperscript{nd} paragraph on page 6, in order to make clear that each subject was assigned a number, and this number was used as a variable in both the spreadsheet for the questionnaire answers and the spreadsheet for English test scores, filled by the admissions office of the FMUSP Graduate Program in Psychiatry. The final datafile used in the analyses, merging those two spreadsheets, kept the subject numbers but omitted their names, in order to preserve their anonymity.

8. p. 4, last paragraph, third sentence: Did the authors use the sum of impact factors?

>> We would like to clarify that we have used the mean impact factor of the journals where articles were published.

9. p. 4, last sentence: As for the multiple linear regression: How about colinearity of independent variables „English test score at enrollment“ and „self-reported level of overall English proficiency“? The authors report a substantial and statistically significant correlation of the two variables. It is conceivable that eliminating one of the factors would strengthen their regression analysis.

>> The referee is right when he points out that there is some degree of colinearity between the independent variables related to test scores at enrollment and self-reported level of English proficiency. However, as pointed out by Prof. Vasconcelos (see below), such correlations had only modest to moderate strength. This is now stated in the revised Results section (see page 8, paragraph 2). Besides, the two variables are measured in a substantially different way from each other, despite their similar aim to estimate English proficiency. Due to those reasons, we honestly believe that it is reasonable to maintain the design of the multiple regression analyses as they are. We hope that these arguments are going to be judged as satisfactory to the reviewer.

10. p. 5-6, results: Present numbers: Give the reader details of both regression analyses in tables, including R-square of both models and the effect sizes of all independent variables that turned out to be significant (to say nothing about the fact that even bivariate correlations would be interesting). Without R-square the reader cannot estimate the effect size of the results, and hence their importance.

>> We agree that it is important to give the reader details of the regression analyses in the tables of the paper. This is now included in Table 3 and Table 4, including R-squares of the models, as requested.
11. P. 8, second paragraph: The authors speculate on an association between scientific ability of supervisors and English proficiency of their students. However, such a hypothesis can be tested using data from this study. Why not correlate?

>>Following the excellent suggestion of the reviewer, we have calculated the correlation between the variables H-index of supervisors and English test scores of students. The r value obtained was -0.025 (p=0.876). Based on this finding, we now state in the revised Discussion section that this possibility is not supported by our data, since we found no statistically significant correlation between the H-index of supervisors and English test scores of students.
Reviewer: Sonia MR Vasconcelos

Reviewer’s Comments:
The manuscript addresses an important topic in research productivity and assessment. The data shed some light on factors influencing publication in international journals by non-native English speakers. The work builds upon previous studies in the literature and poses well-defined questions. However, I found some inconsistencies that should be explained by the authors. The contribution is part of a dissertation entitled "Relação entre conhecimento em língua inglesa e sucesso das publicações de pós-graduandos de psiquiatria e neurociências em periódicos internacionais de impacto", by Alexandre Cunha, the first author of the manuscript. I had access to the document, as it is publicly available, during my review. One of the inconsistencies between the dissertation and the manuscript is the number of participants in the study. Please, consider my comments in the pdf with highlights attached. Only after the authors address these inconsistencies, I will be able to make my recommendations to the editors.

>> We confirm that the present article is part of a Master’s dissertation by Cunha (2013). We believe that the inconsistencies detected by the reviewer are all due to the different number of individuals: n=41 in the article submitted to BMC Psychiatry, and n=43 in the dissertation. In the sample described in Cunha’s dissertation (2013), there were two additional individuals who performed the Test of English as a Foreign Language (TOEFL), while all other individuals (n=41) underwent the English language test specifically designed for the post-graduate program in Psychiatry. When we initially submitted the present manuscript to BMC Psychiatry, we felt that we should not add those two individuals, since they had undergone a different testing process to measure their knowledge of English.

However, after the careful considerations by Prof. Vasconcelos, we now believe that the sample reported in the article should be entirely consistent with the one of the dissertation by Cunha (2013). Based on that decision, the following changes have been made to the revised manuscript:

(a) At the beginning of the revised Methods section, we state that 43 out of 46 students agreed to participate in our survey.

(b) In the third paragraph of the Methods section, we now state that two individuals had undergone a different English testing procedure (TOEFL). In the same paragraph, we explain that their TOEFL scores were converted to a 1-100 range by cross-multiplication in order to make the ratings for the two types of English knowledge tests comparable.

(c) Throughout the Results section, we changed the statistical results in order to reflect the inclusion of those two individuals. The numbers are now entirely consistent across the manuscript and the dissertation.
Finally, we would like to mention that the results of the study are highly similar whether we use the sample with 43 or 41 individuals, both in regard to their direction and level of significance.

In regard to her comments in the pdf file directed to us, please see below our answers point-by-point:

1. Throughout the manuscript, we now use “graduate” rather than graduation / postgraduation.

2. As stated in our reply to Dr. Baethge above, we have corrected the year to 2011.

3. The term “indexed for” has been replaced by “indexed by”, as requested.

4. We agree that the addition of more data shown and discussed in the dissertation by Cunha (2013) could improve the manuscript. We hope that our responses to the items 5, 6, 8, 9 and 10 below will demonstrate that we took such general recommendation fully into account.

5. We agree that it is important to show the impact factors of the journals and h-indexes of the advisors. We have now included a table (Table 1) providing descriptive statistics regarding data for English proficiency, number of articles, impact factor for journals and H-index of supervisors. In addition, we have included an appendix (Appendix 2) providing the full list of ISI-indexed journals where articles were published, the number of authors in each article, and the language used in these articles.

6. The referee is right when she points out that translation was only part of the assistance sought by students, which included also revision and editing. In order to reflect that, we have made several changes to the revised manuscript, as follows:
   (A) Throughout the paper, we now use the term “translating and language editing assistance” rather than simply “translation services”.
   (B) We have included a table (Table 2) showing descriptive statistics regarding the degree of provision of language editing assistance to students.
   (C) In the revised Results section (see page 9, paragraph 3), we state that across the entire sample, the assistance to students in editing/translating articles was provided by different sources, including: professional translators (n=9); the official supervisor (n=12); other Brazilian co-authors of the article (n=7); international scientific collaborators (n=3); the Brazilian supervisor followed by a professional translator (n=3); the supervisor plus other Brazilian co-
author followed by a professional translator (n= 4) and a Brazilian proof reader proficient in English (n=2).

7. We agree with the referee that the strength of the correlations between the variables related to test scores at enrollment and self-reported level of English proficiency is modest to moderate. This is now stated in the revised Results section (see page 8, paragraph 2).

8. As requested, we now state in the revised Results section (see page 9, paragraph 4) that the variable “degree of third-party assistance in editing/translating articles” did not correlate with the average English test score at enrollment (F = 0,289, p = 0,833), nor with the self-reported level of English proficiency, in reading (F = 1,081, p =0,369), or writing (F = 0,297, p = 0,827).

9. We agree that more information should be presented to indicate what is being considered a high-impact journal in the present study. Please refer to our response to Dr. Baethge, who raised exactly the same issue (minor point number 2).

10. The referee pointed out that the Discussion section of the dissertation by Cunha (2013) highlighted other factors that may help to explain the relationship between English issues and research productivity (measured by publications) of the group. We believe that she is referring here to the several sources of support (beyond translation) that students received when finalizing their manuscripts in English (even those students who were most proficient in English). As stated in our reply number 6(C) above, the revised Results section now indicates that across the entire sample, the assistance to students in editing/translating articles was provided by different sources (see page 9, paragraph 3). Also, in the revised Discussion section, we now suggest that the network of people that interacts with students when they come to a knowledgeable research group, including both local and international collaborators, may substantial propel such young researchers to achieve success in their publications (see page 12, paragraph 2).

11. We agree that assuming that the quality of publications can be explained by the type of journals where students published their papers may be questionable. In the revised Discussion section, we acknowledge the limitation of using the impact factor of scientific journals as outcome measure, since this index is essentially aimed to provide a measure of quality of journals rather than to evaluate the productivity of individual scientists (Alberts , 2013 - Science editorial).
We entirely agree that we should be careful about the conclusions of the paper. Therefore, we have made substantial changes in the last paragraph of the paper, toning down the claim the graduate services would be better served by providing translation services rather than investing in increasing students’ proficiency in English. Instead, we now say that there have been suggestions that public universities in emerging countries should provide translation services to facilitate global dissemination of local research findings. We go on to say that our finding that the vast majority of our subjects required language editing services might suggest that graduate programs in emerging countries should invest on providing financial support for the translation of articles authored by its students (even if those students are known to be relatively proficient in English). However, we also cite recent studies which suggest that other alternatives may be effective to improve scientists’ skills for publishing in English as a second language. We conclude the paper stating that future studies should address the comparative short-term and medium-term efficacy of such varied strategies in graduate services in emerging countries. We hope that the referee will consider the concluding paragraph of the paper more balanced and consistent with our actual data.