Author's response to reviews

Title: Use of Health Systems and Policy Research Evidence in the Health Policymaking in Eastern Mediterranean Countries: Views and Practices of Researchers

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Version: 2 Date: 2 November 2011

Author's response to reviews: see over
Dear Editors-in-chief,

Thank you for reviewing our manuscript entitled “Use of Health Systems and Policy Research Evidence in the Health Policymaking in Eastern Mediterranean Countries: Views and Practices of Researchers”. We have modified the manuscript (all changes are indicated in red font) taking into consideration the comments by the reviewer and provide you below with a detailed response and description of the changes made.

Editors' comments:

Comment 1
I agree with the reviewer's concerns. My strong recommendation is to consider the review of the companion paper, and decide whether to attempt to combine the findings into a single, concise, carefully crafted report of the study. Issues of over-generalization should be carefully addressed, and care should be taken to ensure that the report reflects the limitations experienced in conducting the study.

Response 1
Thank you for your suggestion. Upon discussion with the rest of the authors and after considering several factors (detailed herewith), we decided to submit the researchers paper as a standalone study. Given the nature of the data, the scales used and vastly different survey tools, it would be difficult to combine both surveys in one manuscript without diluting the findings from one of the two components and increasing the word count of the paper. While
this is a valid suggestion, combining the findings from the two components (besides being methodologically challenging) warrants careful consideration to ensure that none of the specific findings from either of the two components end up not being reported or sufficiently addressed.

**Referee 1**

**Comment 1**

This is an important topic. There is not much evidence from the region on the issue and I support the publication of the manuscript.

I am pleased to see the authors have used existing opportunities to elaborate on health systems and policy researchers’ views in the EMR region.

**Response 1**

We thank the reviewer for his comment.

**Comment 2**

The paper has some limitations, which I have addressed below.

**Major compulsory revisions**

1. The study cannot be considered representative of the region. The number of the researchers that have been surveyed (responded to the questionnaire) from different countries do not correlate with the scientific output and the number of publications from the countries (e.g. Iran and Saudi Arabia with only 3 respondents and Pakistan with no respondents). This may to some extent have occurred due to the non-response (although it is not clear), which is inevitable, but is likely to have risen due to the limitation (non-existence) of a sampling frame. This is a major limitation of the study and has to be highlighted in the discussion section. The discussion of the paper at the moment is presented in a way that seems to claim representativeness of the respondents for the researchers in the region (e.g. see the first line of the discussion: "Our study shows that researchers’ engagement in a variety of KTE activities ... was undertaken by less than half of researchers in the EMR". This is a grand conclusion required representative sampling not conducted in this study).

**Response 2**

We would like to thank the reviewer for his comment. The discussion section has been slightly modified, particularly the section he mentioned, to indicate that the findings refer to the sampled researchers. We would also like to point out that issues around sample size were already mentioned in the limitations section of the previous version of the manuscript.
Comment 3

2. Regression analyses are confusing. The outcomes are not well defined. The authors developed the 'dependant variables (transferring research results or undertaking KT activities) by summation of the scores of items in these two scales'. No estimate on the scalability of the 'dependant variables' has been provided in the manuscript. Also as the 'independent' variables are likely to have semantic overlaps, tests for potential multi-collinearity in the regression analysis are required. I suggest either address the following points, or drop the regression analyses.

Response 3

We reviewed the regression analysis to account for issues on scalability of the dependant variables and multicollinearity of independent variables. As discussed in the revised version of the manuscript, we used factor analysis to provide more information on development of dependant variables. Three dependant variables were constructed using exploratory factor analysis; two related to the transfer of research results and the third related to undertaking KT activities (henceforth referred to as production and dissemination of evidence). The questions relating to transfer of research to potential users (8 items) loaded on three factors; the first related to transfer of results to policymakers, directors of NGOs and donor agencies, the second related to transfer of results to policymakers, directors of NGOs and donor agencies, the second related to provider organizations (those associated with syndicates, orders, health care institutions and service providers). Eigen values and % variance explained were acceptable and as such, two scores were computed relating to these two factors by summation of the score of items. This resulted in two dependant variables, the first related to policymakers (those associated with government, NGOs and Donor agencies) and the second related to provider organizations (those associated with syndicates, orders, health care institutions and service providers).

The third factor related to production and dissemination of evidence from the samples’ own research (6 items). Exploratory factor analysis showed loading on one factor and acceptable eigen values and % variance explained. Similarly to the first two factors, a score was created for this factor by summation of the scores of items.

The independent variables included in this model related to undertaking knowledge transfer and exchange activities related to contact and exchange with health policymakers and stakeholders in addition to the investments/resources available for the production and transfer and exchange of evidence from HPSR. Multicollinearity between independent variables was
tested for prior to data analysis using the Pearson correlation coefficient statistic and no multicollinearity was detected.

Kindly take note that the results section was also revised in light of the above.

**Comment 4**

Minor essential revisions

3. The analysis of the 'qualitative section' has not strictly followed qualitative analysis methods. I suggest the authors select one of these alternatives: EITHER keep the reporting of the results as it is (i.e. emphasis on what proportion of the respondents has mentioned which theme), and change the way the methods has been described as something like this: "for the analysis of the responses to the open-ended questions we followed a simple thematic analysis approach ..." (avoid using the word 'qualitative'). OR change the way the findings are reported, similar to what is expected from analysis of a qualitative text. Given the structure of the paper, I personally suggest the first option as it only requires some wording changes in the methods section.

**Response 4**

As the reviewer suggested, we added a clarification to the Methods section regarding the analysis of the open- ended questions. The following changes were made on page 10 line 21:

Open- ended questions were analyzed using the simple thematic analysis approach. Responses were broken into similar concepts and ideas (open coding). Axial coding followed which involved organizing concepts into themes [21]. The data was then analyzed by recurring themes and emerging patterns.

**Comment 5**

4. The authors refer to translating the tool to Arabic and then back translating it. But not all the researchers in the region (and those based in other countries) speak Arabic or use Arabic as their professional language. It is probably the case that some respondents received the English version and some received the Arabic version of the questionnaire. This needs to be clarified in the paper.

**Response 5**

All respondents were given the option to complete the survey in either English or Arabic. Initially, respondents were approached by an email, in both English and Arabic, with the link to the online survey. They were informed within the body of the email that they could complete the survey in either English or Arabic. The link allowed respondents to choose their
language preference using a drop-down list. This clarification was added to the Methods section page 10 line 2.

Comment 6
5. Further explanation on how the questionnaire was developed will be helpful. As three different sources from three different countries were used for developing the questionnaire, it is important that further details are provided on exactly how the authors developed the questionnaire. E.g. did the authors use one questionnaire as the basis, and the others to complement it? Did they add any new items to the questionnaires?

Response 6
The main themes and most of the questions were adapted from the questionnaire by Lavis et al., 2010. Additional questions were also adapted from the questionnaires by Campbell et al., 2009 and Tehran University of Medical Sciences. The wording of questions was retained whenever possible; however, some questions were customized to fit the context of the region. Furthermore, we added the open-ended questions as new items to the questionnaire. An explanation on the development of the questionnaire was added to the Methods section page 7 line 4.

Comment 7
6. In Table 1, for one researcher the country of origin has been stated as 'African horn'. Please state the name of the country instead of the region.

Response 7
Kindly take note that origins of researchers were self reported. To avoid confusion, this was removed (to reflect a missing value) from Table 1 since it does not refer to a specific country.

Comment 8
7. Add details on gender distribution of the respondents in Table 1, if available. (The Table 1 in the manuscript pdf file I have received seems to be incomplete)

Response 8
Thank you for your comment. We added gender as one of the variables in Table 1. We are not sure about the problem the reviewer faced with viewing Table 1 in the previous submission and hope that its format will be clearer in this submission.

Comment 9
8. No confidence interval has been provided for the 'percentages' reported in the paper.
Confidence intervals for the estimates should be provided.

**Response 9**
Confidence Intervals (95%) were provided for scale-based questions (Tables 2 and 4) in the revised version of the manuscript.

**Comment 10**
Discretionary revisions

9. There are more relevant papers from the region that the authors may want to consider for enhancing the discussion section of the study

**Response 10**
Based on the reviewer’s suggestion, we included additional publications from the region to further enrich the discussion. References to additional studies from the region were added to the Introduction and discussion sections. These additional references are:


The additional references were also added to the References section (references 18 to 21).

**Comment 11**
Level of interest: An article of importance in its field
Quality of written English: Acceptable
Statistical review: Yes, and I have assessed the statistics in my report.

**Response 11**
No response is required.

Thank you for your kind revision of our manuscript. We thank you for your interest in this paper and look forward to the outcome of the second round of peer review.

Sincerely,

Fadi

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