Reviewer's report

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

Version: 1 Date: 30 September 2011

Reviewer: Arash Rashidian

Reviewer's report:

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.

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).

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-colinearity in the regression analysis are required. I suggest either address the following points, or drop the regression analyses.

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.

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.

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?

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.

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)

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

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

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.

Declaration of competing interests:

I declare that I have no competing interests.