Reviewer’s report

Title: A comparison between health research output and burden of disease in Arab countries: evidence from Palestine

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Reviewer: Abla M. Sibai

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The authors searched Scopus and PubMed for health-related literature in Palestine for the time period (2000-2015) and compared research output with disease burden in the country using death rates and DALYs. This type of reviews is increasingly becoming of interest to researchers, notably in resource-scarce setting for assessing research focus and research quality, and for better direction of research funds.

This is an interesting study and I was intrigued to read the manuscript; yet my excitement was somehow dampened when going into the manuscript itself, which appeared to have been written in haste. My concerns are presented below under general comments and more specific ones.

General comments:

The abstract and introduction are well written, focused and clear.

Methods:

The methods are not explicit enough and need more elaboration. The eligibility criteria are a bit blurred: for example, did the authors exclude a paper that included Palestinian participants but where none of the authors were affiliated with a Palestinian institution? Also, it is not clear whether papers on health systems research were included or not? Most of the reasons posted for exclusion in the full-text reading could have been identified while screening titles and abstracts (e.g. affiliation, qualitative research, basic science research); and this makes the reader wonder, what were the main reasons identified for excluding the 1650 records based on title and abstract screening? Also, it is not clear how did the authors handle the 'description of disease or condition evaluated according to groups of global burden of disease' when the study tackled more than one disease (e.g. in cross-sectional or cohort studies) and how was the 'sample size' coded when it was a review-non META analyses- paper? Or were the latter excluded as well?

Table 2 (last 2 columns) and Figures 2 and 3 appear to be addressing the same research question and telling us the same message: there is a mismatch between disease burden and research
output. Using various analytical modalities, the question was addressed in table 2 on a multiplicative scale (measured as deaths and DALYs per study), in fig 2 as correlation analyses (measured as rho), and in fig 3 addressed as differences between actual and expected research measured on additive scale. It appears to me that these various modalities have resulted in conflicting results for some conditions (e.g. Maternal, Neonatal and congenital were judged to be most understudied in table 2 while these are presented with the group of Research surplus in Fig 3; also Injuries were slightly more likely to be understudied than cardiovascular diseases (77.1 vs 61.9) in Table 2 but appeared on the null line in Fig 3). The authors may wish to examine and comment on these discrepancies in their discussion.

Discussion

The discussion of the paper is not necessarily supported by the analyses and was largely dissociated from the results. For example, while the findings point to a relative lack of studies on NCDs such as cancer and cardiovascular disease, the discussion focused solely on the literature that addressed infectious diseases with no reference to NCDs. Also, while the current submission was not about RCTs specifically, a big portion of the discussion (p. 7-9) was devoted to compare the study findings with others in the literature focusing on those that examined gaps in health research using RCTs (refs 9, 15 and 16). Clearly, some conditions are more appealing to be examined using RCTs than others.

While the authors alluded to one limitation issue (failure to capture unpublished reports and the grey literature), the paper lacked a clear and focused statement on other limitations. It should be sufficiently discussed that findings from this study and other similar reviews are very much dependent on the search strategy and the inclusion/exclusion criteria used. For example, the exclusion in this paper of lab-based studies - which are generally the main focus of cancer-related research in the Arab region- is likely to result in an undercount of cancer-related studies. Also, the authors point to certain strengths for their study which in my view are only the standard practice in such a research and not unique to this study (e.g . the independent and in duplicate screening, or the use of the GDB internationally developed measures to assess burden).

An ample space was devoted in the discussion on the implications of the findings (p. 9-10), yet the suggested recommendations do not necessarily emanate from the results and/or were not related to the focus of the paper. The observed discordance between the focus of the research output and disease burden cannot be explained by "limited research infrastructure and resources or because of lack of communication between policy makers and researchers" as the authors note in their discussion (p. 8, lines 32-33). This paper was not about the quality of the research output in Palestine to conclude that it is important to 'Enhance the capacity of national researchers to conduct prioritized research' (p. 9), and additionally there was no reference in the paragraph here about how enhancing capacity can lead to the conduct of prioritized research. Also, there were several messages written under the recommendation 'Foster dialogue between researchers, policy makers, funders and end users/patients' (p. 9) that do not necessarily fit either with the subtitle or with the overall theme of the paper. The implications of the findings need to be revised in a succinct and more focused approach that is in harmony with the main research questions addressed in the manuscript.
Finally, the paper needs major edits and revision. The language is often ambiguous with several grammatical and typo errors. For example, one can easily spot 4 such instances in p 3, (Lines 2-3: The statement "which consists of West Bank, including East Jerusalem, and Gaza Strip" appeared to refer to the MENA countries and not to Palestine; Lines 13-15, statement is incomplete; Lines 23-24: "Burden of disease is a measure 'that' represents"; Line 31, 'weak' instead of 'week'; and line 33: "however, there are -instead of is- no previous studies" .. etc)

The reference list is not aligned with the text. I may have missed these, but it appears that the two studies on sub-Saharan Africa referred to in the discussion on p. 8 are lacking in the ref list.

Specific comments

Title:

I am not convinced that the paper is about 'relation' (as is stated in the title). It is more a 'comparison' between output and disease burden. And again on P.4, line 4, the paper is not about 'association' in as much about comparison/contrast or differences/fit between research output and disease burden.

Page 4:

Line 26: Did the two reviewers abstract the needed data also in duplicates?

The authors note that they abstracted data on impact factor (IF) and number of citations. The limitations of the IF are well noted in the literature. Also, the fact that recent articles are not given thorough opportunity to be cited as the older ones and because the quality of research output is not the focus of this submission, I would suggest that these two parameters be removed from the results. Findings in the case of IF and number of citations may raise more questions than answers.

A comment as to the rationale behind considering the first author rather than the corresponding author affiliation in assessing collaborations may be warranted.

Page 5:

Line 3: "international/or NGO". It is not clear why the investigators tagged NGO to the international source of funding, it can also apply to local source of funding. Suggest deleting the NGO.

Lines 6-7: Why did the investigators choose two sources for assessing the disease burden in Palestine (refs 12 and 14); and what was their decision if there were some discrepancies between the published estimate in the Journal (Lancet) and the one online (Visualizationdata) source?
Line 22: The description related to the difference between published articles for each disease/condition with the expected/proportionate number of published is blurred and needs elaboration. It is not clear how were the figures on research deficit and research surplus in Figure 3 estimated?

Page 6:

Lines 15-16: Suggest revising to "increased over time, almost doubling every 5 years (starting with the %s associated with the earlier years and then the latter ones)

Line 25: Suggest moving the sentence "Table 1 provides information on the general characteristics of the included articles" to the beginning of the paragraph (line 10).

Many sentences in the section entitled 'Characteristics of included studies' start with a number (eg. 152 on line 11; 279 in line 15; 368 in line 20.. etc). Avoid starting sentences with a number.

Page 7:

Lines 5-6: : Suggest moving the sentence "The distribution of Palestinian health research output and disease burden (DALYs and deaths) across the diseases/conditions is presented in Table 2" to P. 6, line 28.

Line 8: the authors picked up cancer, cardiovascular, and maternal, neonatal and congenital conditions as the most understudied topics (based on the DALYs per 100,000 population per study and on deaths per 100,000 population per study). Table 2 shows that 'Musculoskeletal and Neurological' conditions and 'Injuries' were also understudied when considering DALYs but less so when considering death rates. Whilst high case fatality rates for some conditions may inflate death rates (e.g. cardiovascular diseases and cancer), disabling conditions with no cure and long duration (e.g. Musculoskeletal and Neurological) inflate DALYs. Such a kind of distinction may be warranted in the discussion.

Page 8:

Lines 22-23: As mentioned earlier, discrepancies may have been additionally arisen because of differences in inclusion/exclusion criteria, notably the lab-based studies that characterize cancer-related output in the Arab region

Line 26: skew 'towards or away' from infectious diseases?

Line 28: suggest to revise to "with the burden of disease, and found that funding was more strongly associated with DALYs (r=0.62, p <0.001) than the number of deaths (r=0.40, p=0.03)"
Line 4-9, from "important to note… controlled trials": this sentence fits better in a section on limitations.

Line 28: add here in addition 'university research boards'

Line 33: McGregor et al study on priority-setting initiatives was about low and middle Income countries; and hence the statement (McGregor et al found that the majority of the identified priority-setting initiatives took place at the global level with a developing countries focus) is not reflective of the findings of McGregor et al paper.

Table 1:
Consider revising the categories for the publication year as (2000-2004; 2005-2010; 2011-2015)

Line 43: define non-RCTs, and how is this category different from the other observational studies listed?

Table 2:
Comment: need to elaborate on the calculations of the % of total DALY and death as well as DALY and deaths per study in the methods. And what do these measures signify, i.e. what does a higher value or a lower value mean?

Consider adding the source of the data on DALYs and death rate as a footnote.

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Please indicate how interesting you found the manuscript:

An article whose findings are important to those with closely related research interests

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Please indicate the quality of language in the manuscript:

Not suitable for publication unless extensively edited

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