Author's response to reviews

Title: Prevalence and factors associated with the use of complementary and alternative medicine practitioners in 8 countries of the former Soviet Union

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Author's response to reviews: see over
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08/12/2012

Dear Editor,

I am writing to you in connection with the manuscript entitled ‘Prevalence and factors associated with the use of complementary and alternative medicine practitioners in 8 countries of the former Soviet Union’ by Andrew Stickley and coauthors which was submitted to the journal *BMC Complementary and Alternative Medicine* for consideration for publication.

Following its initial review, we were asked to revise the manuscript in light of the reviewers’ comments. We are writing to you now to resubmit the revised manuscript and our response to the reviewers’ comments which are detailed in this letter.

Thank you for considering this manuscript for consideration for publication.

Yours sincerely,

Andrew Stickley.
Reviewer's report

**Title:** Prevalence and factors associated with the use of complementary and alternative medicine practitioners in 8 countries of the former Soviet Union

**Version:** 2  **Date:** 15 October 2012

**Reviewer:** Samuel M Brown

**Reviewer's report:**

This is an interesting study that merits consideration. Unfortunately the survey instrument has substantial flaws that severely limit generalizability.

First, “folk medicine” varies in meaning from country to country. It is often pejorative, depending on the context.

**Response**

We agree that what constitutes CAM can vary between countries. That is why we employed a question which has been used before on CAM research in this region and which would be understood by all of the respondents who had to respond to it. We have now mentioned that the understanding of CAM can vary between countries in the limitations in the discussion.

Second “folk medicine” is only a very small part of CAM in the countries of the former Soviet Union.

**Response**

We have now mentioned this among the paper’s limitations in the Discussion.

Third, as the authors note, “mainstream” physicians will also commonly prescribe or employ CAM therapies, as would neighbors, family members, or other less formal healers.

**Response**

As the reviewer has noted we have already mentioned this among the possible limitations of this paper in the Discussion section of the paper.

Fourth, the authors don’t acknowledge the incredible diversity of non-Western therapies that range from biomedical-sounding (various low intensity lasers shone on skin and occasionally on the blood through an intravenous catheter) to clearly non-biomedical sounding (shamanism) throughout the former Soviet Union.
Response

We now cite the reference the reviewer lists below while mentioning about the wide diversity of CAM treatments that are available in this region and that vary between locations in the limitations section in the Discussion.

Bioanthropological field work would likely help the authors appreciate the diversity that lurks behind familiar terms and limits the generalizability of the findings from this survey instrument.

Given reports from other studies of the near-ubiquity of CAM in Russia, for instance, the reported rates have low face validity, unless they are restricted to a very limited subset of CAM therapies. As minimum possible prevalence values, or as assays of the prevalence of a very specific subset of CAM, or perhaps in the interests of licensing particular types of practitioners, this study may have some utility, but it should not be read as an accurate representation of the diversity and prevalence of non-Western, non-evidence-based treatments that are currently employed in the former Soviet Union.


Response

We thank the reviewer for his thoughtful comments. We are now careful to highlight the difficulties of generalizing our findings beyond the context of the immediate research question.

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Reviewer's report

Title: Prevalence and factors associated with the use of complementary and alternative medicine practitioners in 8 countries of the former Soviet Union

Version: 2 Date: 22 October 2012

Reviewer: Barbara Stussman

Reviewer's report:

Major Compulsory Revisions

1. Page 5/Study variables and statistical analysis: provide justification for
definition of CAM use (i.e. go to a person practicing non-traditional (folk) medicine). Was any pilot testing done on this phrasing to determine what modalities respondents included? Phrasing is very general and could include a wide variety of remedies.

**Response**

1. The use of CAM was one response option in a question asking respondents about how they treated 10 common symptoms where a range of possible treatment alternatives were provided. A very similar version of the CAM question has been used in previous public opinion survey research on the use of CAM in Russia. We agree with the reviewer that it would have been better if there had been a range of treatment alternatives provided (as there were in the earlier public opinion surveys), however, as the question was not exclusively about CAM use there was insufficient space to explore this issue in greater depth. This has now been elaborated in the text.

2. Page 5/Study variables and statistical analysis: Provide justification for selection of these 10 conditions for analysis.

**Response**

2. As mentioned above, these conditions were part of a larger question enquiring about the use of various treatment alternatives. The reason they were chosen was because they were deemed sufficiently serious to potentially require the seeking of treatment but at the same time, were not so severe as to limit the possibility of using any of the various treatment alternatives provided. This has now been elaborated in the text.

**Minor Essential Revisions**

1. Title: Reword title to clarify that the analyses are restricted to persons with at least one of 10 health conditions.

**Response**

1. We have decided not to change the title for several reasons. Firstly, as only 1.2% of the respondents had never had any of the 10 symptoms, few subjects were actually excluded from the analysis. Secondly, when the title is actually amended in this way so that it reads: ‘Prevalence and factors associated with the use of complementary and alternative medicine practitioners by persons with at least one of ten common health symptoms in 8 countries of the former Soviet Union’ we believe that it becomes too long, contains too many numbers and is slightly confusing. Finally, one of the other reviewers (3) described the title (and abstract) as ‘informative and appropriate’. We concur with this viewpoint. However, if the Reviewer does deem that this change is absolutely necessary we will of course comply.
2. Throughout the manuscript, the 10 health conditions are sometimes referred to as “disease conditions” and sometimes as “symptoms.” Since these are different, use the more accurate term consistently.

Response
2. We now use the term ‘symptoms’ consistently throughout the text.

3. Page 6/last sentence in first full paragraph: Provide the percent for those with missing values for age, sex, education, marital status, wealth, trust in doctors and distance to their health facility.

Response
3. We have now provided these numbers in the manuscript.

4. Page 10/Conclusions/2nd sentence: reword “distrust of doctors were associated with fewer symptoms” because as currently worded it sounds like distrust of doctors was associated with having a fewer number of health conditions, which is not the case.

Response
4. We have now reworded this sentence.

5. Figure 1/Title: Change title to clarify results are for “usually treating” and “for persons having at least one of 10 health conditions”

Response
5. We have now changed the title of Figure 1 in accordance with the Reviewer’s request.

Discretionary Revisions
1. Results/first sentence: Move first sentence to methods section.

Response
1. We have now moved the text to the methods section.
Reviewer's report

Title: Prevalence and factors associated with the use of complementary and alternative medicine practitioners in 8 countries of the former Soviet Union

Version: 2 Date: 31 October 2012

Reviewer: Brian P Hinote

Reviewer's report:

1. Is the question posed by the authors well defined?

Research question is well-defined.

2. Are the methods appropriate and well described?

Methods are generally appropriate, but I have minor issues with how some variables are operationalized and/or described in the manuscript (see comments below).

3. Are the data sound?

Data are indeed sound. I am quite familiar with these data, and the LLH set is a strong, valid, and reliable one.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?

Overall, yes. But I have some minor comments (see below) on how results are reported, and I encourage the authors to consider those in order to strengthen the paper.

5. Are the discussion and conclusions well balanced and adequately supported by the data?

Overall yes.

6. Are limitations of the work clearly stated?

Limitations are clearly stated, but the authors might consider mentioning that these data are now quite dated (>10 years old), but this weakness can also set up a need for more research and detailed data collection on CAM now.

Response
We have not mentioned about this as in the conclusion, we already state that there is an urgent need to collect more data on the use of CAM in these countries.

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?

Yes.

8. Do the title and abstract accurately convey what has been found?

Title and abstract are both informative and appropriate to findings.

9. Is the writing acceptable?

Overall the writing is acceptable. I have one comment below about the use of passive voice, but this is more of an author's preference. Thus it is the authors’ choice to use these constructions.

- Discretionary Revisions (which are recommendations for improvement but which the author can choose to ignore)

1. The authors make frequent use of passive voice constructions, which I think often weakens their writing style. This is not altogether uncommon in academic writing, but I encourage the authors to consider changing those constructions to active voice. But this is merely a stylistic suggestion, and the authors can ignore it if they wish to do so.

**Response**

*We thank the reviewer for this comment. As he notes, it is a stylistic matter and so we have decided not to change the way the text is written in this instance.*

- Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

1. In the Methods section, the structure of the CAM use question either: a) fails to meet the principle of inclusiveness with regard to variable operationalization, or b) is simply poorly described. Were there other possible responses to this item? If so, they should be included. Perhaps the paper just doesn't describe the variable very well, and if I am misunderstanding, then the text should be clarified. Or is the response just a yes/no option to the item on visiting a non-traditional healer. Regardless, this section should be edited to clarify.

**Response**

*For the sake of brevity we only described one part of a longer question about treatment options where the use of CAM was one such option. We have now given full details of the question and the response options that were available.*
2. I also have a minor issue with the characterizations of income as poor/middle/rich. Perhaps consider revising to low/middle/high distinctions, simply because “rich” might not always refer to altogether affluent persons in these regions. I realize that this is a word choice issue, but I simply suggest employing less value-laden terms to describe the categories (or perhaps just use the exact responses themselves, which other authors publishing from these data have indeed done).

*Response*

*We have followed the reviewer’s suggestion and changed the wealth category labels to ‘high’, ‘middle’ and ‘low’.*

3. In Table 2, the authors claim to present proportions (in the title), but percentages (as noted in a footnote) are actually presented. Please change “proportion” to “percentage” in the title to clarify and for consistency.

*Response*

*This has been changed as requested.*

4. The authors commonly refer to “significant” findings (i.e., statistical significance) but do not consider or mention the strength of these relationships (i.e., the sizes of odds ratios). Why? I recommend some discussion of what these odds actually mean (e.g., the odds of seeking non-traditional care are…….), as statistical and practical significance are separate issues, as are statistical significance and strength of relationship (as the authors are no doubt aware). This is a major advantage of logistic regression that I think is not utilized.

*Response*

*We have now amended the text accordingly.*

5. The authors mention (as a limitation) omission of “the old people in Ukraine” – I recommend editing to “older people/persons in Ukraine” to clarify language. Perhaps also include to what ages you are referring, as “old” in some regions/countries means something very different than the same term in other areas.

*Response*

*We have now amended the text accordingly.*

6. On page 8, the authors write that the “study reveals large differences in the use of CAM.” With few exceptions, are differences indeed that large (referring to Table 2)? I simply recommend including to which ranges of CAM use the authors are referring in parentheses immediately following this statement for clarification.
Response

This was referring to Figure 1. We have now amended the text to indicate the range of CAM use we were referring to.

- Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)

No major revisions; only minor revisions (see above) suggested before publication.

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Reviewer's report

Title: Prevalence and factors associated with the use of complementary and alternative medicine practitioners in 8 countries of the former Soviet Union

Version: 2 Date: 7 November 2012

Reviewer: Audhild Lohre

Reviewer's report:
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Review of manuscript:

I find this manuscript well written and interesting. The paper aims to add knowledge on visitors to CAM practitioners in 8 countries in former SU. The authors succeed by reporting the prevalence of visits to CAM practitioners, and further, by reporting on characteristics of the visitors.

Major Compulsory Revisions
None

Minor Essential Revisions
In my opinion, the title is not clearly written. I will advice the authors to re-write
the title.

Discretionary Revisions

There are some “commas” omitted throughout the manuscript.

1. The questions are well defined.
2. Methods are appropriate.
3. Data seems to be sound.
4. The manuscript adheres to relevant standards for reporting and data deposition.
5. Discussion and conclusions are well balanced and supported by the data.
6. Limitations are clearly stated.
7. Sources of data are acknowledged.
8. The abstract is fine, but I suggest a more precise and better formulated title.
9. The paper is well organized and well written.

Sincerely,
Audhild Løhre

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

Response

We have decided against changing the title as it conveys 3 pieces of information that we thought it important to convey to potential readers: 1) That we are presenting estimates of CAM practitioner use; 2) That we are also looking at what factors are associated with CAM practitioner use; 3) That this study relates to a number (but not all) of the former Soviet countries. In terms of what we want to convey we regard the title to be both precise and well formulated. However, if the Reviewer does deem that this change is absolutely necessary we will of course comply. In addition, we have now attempted to add the missing commas to the manuscript.