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.

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 Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?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

- 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.

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).

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.

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.

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.

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.

- 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.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

No competing interests to declare.