Author's response to reviews

Title: Internal Health Locus of Control in users of Complementary and Alternative Medicine: a cross-sectional survey

Authors:

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Version: 2
Date: 14 July 2014

Author's response to reviews: see over
Dear Editor,

Please find the revised version of our manuscript

**Internal health locus of control in users of complementary and alternative medicine: a cross-sectional survey.**

We are grateful for the reviewers' comments which helped to improve the manuscript a lot. We clarified our statistical approach, and added a more detailed description of our sample. We also included a paragraph regarding the question of ethical approval as you recommended. You find our answers to the reviewers' comments below.

Content-related changes are highlighted in grey. The manuscript was also copyedited to improve the style of written English. We did not highlight changes that were made during this copyediting process in order to maintain good readability. However, if you would like to have a version of the manuscript with all changes highlighted, we can provide you with one.

We hope that the paper will now be suitable for *BMC Complementary & Alternative Medicine* and are looking forward to your decision.

Thank you for your consideration.

Sincerely,

Lena Schützler, Psychologist
Responses to the reviewers’ comments

Reviewer 1:

Title: Internal Health Locus of Control in users of Complementary and Alternative Medicine: a cross-sectional survey

Version: 1 Date: 27 May 2014

Reviewer: Masa Sasagawa

Reviewer’s report:

- Major Compulsory Revisions

1. The abstract is the most read part of an article. This abstract requires major revisions for three improvements: a) the authors are confused about analyses between cross-sectional and causative; therefore the conclusion, “It remains unclear whether differences in IHLOC are reason for or consequence of CAM use” is senseless from the title of a cross sectional survey, b) the lack of acknowledgement of the geopolitical climate of CAM use in Germany relative to other countries with cultural or complex multi-payer healthcare systems, and c) the statistical analysis was to compare means, which is neither correlational analysis nor analysis of association. In the background section (from the abstract), “We evaluated such associations using different indicators for CAM use” is incorrect. The correct description of the analysis is to test a hypothesis which distinguishes the three patterns of CAM use by the level of IHIOC. Statistical associations have to be analyzed by chi-square or correlation. By comparing the means, the authors made the assumption that populations of a different appraisal level of CAM and CAM use were distinguishable by IHIOC as the normally distributed indicator. In the result section (from the abstract), “1054 undergraduate students” hardly represents a general population sample. This information is repeated in the body of the text, Results section paragraph 1. The mean age 32.74 ± 9.32 seems older for the undergraduate sample of typical universities. The reviewer wonders if there are further explanations necessary because the undergraduate samples are usually between 18 – 30 years old. In the conclusion (of the abstract), the two sentences are not appropriate and should be moved to the discussion section. Authors should state the statistical numbers in the result section, and state the interpretation of the statistical results in the conclusion section. The discussion topics will be given secondary importance in the abstract. However, the limitations of a study (geopolitical CAM acceptance and “undergraduate sample”) will be an important disclaimer in the abstract.

Response: Thank you for your valuable comments. We made the following changes following your suggestions: (1) All references to the evaluation of associations were removed, instead we described that we compared means. However, Reviewer 2 suggested a multivariate model including several predictors at once to evaluate possible confounding effects, therefore a multivariate regression was added to the work. (2) We changed the sentence “It remains unclear whether differences in IHLOC are reason for or consequence of CAM use”, however, we still state that because of the cross-sectional design it is not possible to determine the nature of influence (if there is any at all and not an influence from another underlying construct as is discussed in the discussion section, p. 15/16). We find it important to emphasize this limitation of the cross-sectional design even though it should be common knowledge for most of the readers. (3) You are certainly right in that this sample is not a normal student sample. Since the participants study at the only distance-teaching university in Germany, they vary considerably in age, background, previous knowledge, lifestyle, and so on. Most of the students have work...
experience, work besides their course of studies, and have family. We added a
description of the peculiarities of this student sample to the discussion section. (4) We
changed the structure of the abstract following you suggestions. We also added a
sentence about the limiting factors of the student sample and the geopolitical climate in
Germany.

2. Statistical analysis is a weakness of this article. Table 1 (cross tab table)
should match rows and columns. If data are missing, an additional column should be used so the
rows will add up correctly as a cross tabulation table. Chronic condition (357) plus no chronic
condition (651) equals 1008, not 1020. The same problem is found in the Table 3; the rows do not
add up correctly. In Figure 1, no legend is found about the difference between “Others 1”, “Other
2”, and “Others 3”. Can all “others” be converged into one? The ANOVA does not indicate what
post-hoc analysis was used to determine the statistical significance. Authors did not indicate
whether or not the assumption of the normal distribution was met in the second paragraph of
Results. In the first sentence of the discussion section, “We aimed to evaluate association of
IHLOC and CAM…” is incorrect. The aim was to test a hypothesis to distinguish two or more
populations from the mean scores of IHLOC.

Response: Again, thank you for your comments which helped improve the paper to a great
extent. We changed the following as you suggested: (1) The reason that in Tables 1 and 3
the columns do not add up exactly is missing information on if participants suffered from a
chronic condition or not. We added a footnote to the tables explaining this. (2) In Figure 1
the “others” referred to treatments that were not on the list, but could be specified by the
participants. We specified the procedure in the text (p.6). We did not find it suitable to merge
them into one since then it would not be clear why the appraisal score has a maximum of
11. (3) Instead of post-hoc analyses we did pre-defined orthogonal contrast to evaluate how
groups differed in IHLOC. (4) We did check for the meeting of the normality assumption.
There were slight deviations, however, after discussion with our statistician we decided to
run t-tests and ANOVAs since they are quite robust when violations of normality are not big.
In addition to that, this makes the study comparable to other studies focusing on IHLOC
since in most cases they use comparisons of means between different groups. We added a
description of our proceeding to the results section. (3) We changed the first sentence of
the discussion section deleting all references to associations.

- Minor Essential Revisions

1. As stated in the major revision requirement, although the sample size was large, it was a
homogeneous undergraduate sample, which weakened the generalizability. This limitation
needs to be expressed in the discussion section.

Response: We added a respective paragraph to the discussion section (p. 15).

2. The mean age of the sample needs to be explained. If necessary, an additional
demographic table is recommended to indicate age, school grade, gender, employment
status, insurance statutes, income level, etc.

Response: We added a description of the sample and the reason for the high mean age
to the discussion section (see our response to you first comment).

- Discretionary Revisions

1. CAM-use measurement is not only the methodological issue but also due to geopolitical climate
around CAM within conventional medicine. It is helpful for readers in the other countries to
understand the article which describes the result of CAM use in Germany.
Response: We shortly described the peculiarities of CAM use in Germany in the background section and also described in the discussion section that the results might not easily be generalized to other countries where CAM might have a different status.

2. The Cohen’s effect size discussion in the second paragraph of the Results requires satisfying the assumption of normal distributions and post-hoc power analyses. The results of these analyses were not discussed.

Response: We added a paragraph on meeting the normality assumption as described above (response to your comment #2). We reported the effect sizes according to Cohen in order to help readers interpret our results. Of course this involved doing post-hoc power analyzes, however, we did not report the power since that was not our focus. The effect sizes mostly were small to moderate as were the numerical differences on the IHLOC scale.

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

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 or conflict of interest to this article or authors.

Reviewer 2

Title: Internal Health Locus of Control in users of Complementary and Alternative Medicine: a cross-sectional survey

Version: 1 Date: 13 June 2014

Reviewer: Felicity Bishop

Reviewer's report:

This manuscript reports a large cross-sectional survey of students, which assesses the relationships between internal health locus of control and a number of indicators of CAM use. The rationale for the study is generally clear, in that previous studies have produced mixed results regarding associations between CAM use and internal health locus of control and this study revisits this question using better measures of the key variables and a large sample size. The data appear to be robust and the results can thus contribute to the evidence-base regarding CAM use and internal health locus of control.

1. Is the question posed by the authors well defined? YES
2. Are the methods appropriate and well described? YES
3. Are the data sound? YES
4. Does the manuscript adhere to the relevant standards for reporting and data deposition? YES
5. Are the discussion and conclusions well balanced and adequately supported by the data? YES
6. Are limitations of the work clearly stated? YES
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? YES
9. Is the writing acceptable? YES – but would benefit from proof-reading.

Response: The manuscript was copyedited, so the writing should be improved now (changes made during the copyediting process were not highlighted to maintain readability of the manuscript).

Discretionary Revisions (which are recommendations for improvement but which the author can choose to ignore)
1. The final sentence in the abstract could be clearer.
   Response: We changed the final sentence in the process of changing the whole abstract as Reviewer 1 suggested.
2. The two sentences at the start of results paragraph 2 (Participants without chronic condition had...) could be moved to methods section.
   Response: We chose to leave those sentences in the results section since we think the description of the participants fits well into the results section.
3. Known co-variates of CAM use may be confounding the results, as the authors acknowledge in the discussion; could you not control for some of these, e.g. gender, in the analyses?
   Response: see response to comment 4 below.
4. In the discussion (p8 paragraph 1), the authors tentatively compare the strength of relationship between IHLOC and 1, chronic illness vs 2, CAM use. I would be interested to see a fuller analysis of this dataset using multivariate statistics which could explore such questions.
   Response: We added a multivariate linear regression to predict IHLOC using CAM appraisal, medication and consultation profile, chronic condition, sex and age as predictors. CAM appraisal was the strongest predictor, however, all variables did not explain more than 9% of variance in IHLOC.

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. Ensure descriptions of all the instruments are complete, i.e. include number of items on each instrument and response scales/options.
   Response: We added more detailed descriptions of the German version of the IHLOC scale as well as additional information – if necessary – on the other instruments.
2. Report the extent and type of missing data (e.g. missing at random?) and rationale for not imputing it.
   Response: Since in the German manual of the IHLOC scale there is no description of how to deal with missing values we decided to not compute the IHLOC score for participants with missing items. This resulted in missing IHLOC values for 3.2% of the sample. This number being small we decided to not impute missing values.
3. The BEE should be written out in full on page 9.
   Response: We changed “BEE” to “body-efficacy expectation”.

Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)
1. For the CAM appraisal variable: It would be more usual to do a median split to create 2 equal groups instead of a mean split. Please do a median split or justify using mean split. Please also report the mean (and SD) scores on CAM appraisal for the high/low groups and comment on the meaning of these scores, e.g. are the high appraisal group scoring towards the maximum score on this scale?

Response: A median split was not feasible in this sample because 159 participants had an appraisal score of 6 which is the median itself. Splitting the sample at its mean also meant splitting the appraisal score (which had a range of 0-11) exactly in two equal parts. We added a more detailed description of this on page 9/10 of the manuscript. We also provided the mean and SD of the high/low appraisal group as you suggested.

2. Add the direction of differences to the text reporting the results of the ANOVAs (results paragraphs 3-4).

Response: We added the direction of differences of the compared groups in the results paragraph as you suggested. This truly makes the report of the results much clearer.

4. Please justify in the manuscript why a student sample is appropriate for this study.

Response: As described in our other publication using this sample, this is not a normal student sample. Since they study at the only distance-teaching university in Germany, they vary considerably in age, background, previous knowledge, lifestyle, and so on. Most of the students have work experience, work besides their course of studies, and have family. We added a description of the peculiarities of this student sample to the discussion section.

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

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