Reviewer's report

Title: Differences in the quality of interpersonal care in complementary and conventional medicine

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Reviewer: Aslak Steinsbekk

Reviewer's report:

This is a large and very interesting study. Although some years old it still warrants publication due to the lack of similar studies.

Major Compulsory Revisions

The author must respond to these before a decision on publication can be reached. For example, additional necessary experiments or controls, statistical mistakes, errors in interpretation.

I am a bit unsure why the authors have called this a cross sectional study and not a comparative observational study. Although not the same questions, the participants complete these both before and after, and patients visiting different types of practitioners are compared.

The manuscript is unconventionally set up with only one reference in the introduction and some of the references to what usually is described in the methods in the discussion. The authors probably have their reasons for doing this, but it was e.g. only when I read the first paragraph of the discussion I understood how the authors looked upon the questionnaire used. Therefore the authors need to either provide a very good reason for the set up or change it according to convention.

Most of the tables are very difficult to read/extract data from as they have no indication of whether there are significant differences between the groups. One way to do it is to do separate comparisons between CON vs noncertified CAM and CON vs certified CAM (one example can be found in: Steinsbekk A, Adams J, Sibbritt D, Jacobsen G, Johnsen R. The profiles of adults who consult alternative health practitioners and/or general practitioners. Scandinavian Journal of Primary Health Care. 2007 25:2 (86-92).

The follow up time of 3 weeks warrants much more consideration both in the analysis and in the discussion. It is very likely that patients with chronic conditions are less likely to have an improvement in three weeks and this obviously influences the likelihood of symptom resolution. Therefore This should be taken into 1) the discussion of the associations between the outcomes and 2) either included in the multivariable analysis or a post hoc analysis as they had predefined only to include age and gender (which I find a bit strange given the other variables collected).
I would like the authors' view of whether or not to include a table with a non respondent analysis. At the outset I think such a table gives very relevant information when there are data enough to make some sense of it and that seems to be the case in this study.

The aims in the abstract and the main text is not the same. In line with this, it is not clear to me if the conclusion is related to the research question.

There is a lack of references to studies comparing results experienced by patients of CAM and conventional practitioners. One study that comes to mind as many of the CAM physicians in this study uses homeopathy, is Witt C, Keil T, Selim D, Roll S, Vance W, Wegscheider K, Willich SN. Outcome and costs of homoeopathic and conventional treatment strategies: a comparative cohort study in patients with chronic disorders. Complement Ther Med. 2005 Jun;13(2):79-86. I can understand the reason why so much focus in the discussion is on making hypothesis about the findings. However I think more emphasis should be on the findings themselves and in relation to other studies.

The SF36 data is used somewhat contrary to what is stated in the methods ("to scale…") as it in the result section is reported as an outcome.

Minor Essential Revisions
The author can be trusted to make these. For example, missing labels on figures, the wrong use of a term, spelling mistakes.

Discretionary Revisions
These are recommendations for improvement which the author can choose to ignore. For example clarifications, data that would be useful but not essential.

Are there sub scales for EUROPEP?
It would be very interesting to see the results an analysis restricted to patients in the three groups that could be matched on e.g. age, gender, types of disease/length.

In an earlier publication the following numbers was given in the abstract: "A total of 6778 adult patients received the questionnaire and 3126 responded (46.1%)". Marian, F., Joost, K., Saini, K.D., Von Ammon, K., Thurneysen, A., Busato, A. Patient satisfaction and side effects in primary care: An observational study comparing homeopathy and conventional medicine. BMC Complementary and Alternative Medicine 2008 , 8, art. no. 52. What was the reason for the difference with this study?

Abstract/results. Why use "significantly" with only one result. Can make it look as if the other results are not significant.

Methods, last line p6. How long was the window for accepting returned questionnaires?
Subheading p9. Does not only relates to table 1, therefore take table 1 out of heading and place in text.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

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

**Declaration of competing interests:**
I declare that I have no competing interests