Reviewer's report

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

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Reviewer: Richard Nahin

Reviewer's report:

This is an interesting paper that takes advantage of a unique national experiment in Switzerland in which patients are given the choice of seeing physicians cross-trained in one of several complementary and alternative medicine (CAM) systems of care. The prospective observational data presented by the authors are fairly unique and suggest some interesting hypotheses to be investigated further. This reviewer has a number of comments/questions concerning the manuscript as noted below.

Major Compulsory Revisions

GENERAL ISSUES

While the introduction mentions that five CAM therapies were added to the Swiss basic health insurance coverage, only four of these are included in the present analyses as noted in the Methods section: homeopathy, anthroposophical medicine, neural therapy, traditional Chinese medicine. Why was Western herbal medicine not included? Without including these therapies, how can the authors make any general statements about CAM in Switzerland? After all, the groups of patients/physicians using these Western herbal medicines may be very different from the rest.

METHODS

Inadequate information was provided as to how patient sampling occurred. The authors sated that consecutive patients were sampled on “four given days.” Who chose these days? Were they the same days for all physicians? Were these days randomly chosen? If not, how might non-randomized sampling effect the authors' conclusions?

Why was “three weeks” chosen as the date questionnaires were mailed to patients? This seems like too short a time to account for the normal waxing and waning of musculoskeletal pain or mental health disorders.

How were covariables of age and gender chosen for multivariate models? What information was used to make this choice? Why was education not chosen? As shown by the authors in their own Table 1 and by countless other papers in the literature, education is a consistent predictor of CAM use. Unless the authors had a compelling reason to exclude education, they will need to rerun their regression models to account for this variable.
Not adjustments were made for multiple statistical comparisons. Given the number that were made several are likely do to chance alone. This should also be mentioned as a limitation in the Discussion section.

RESULTS
The authors often make statements that one group is different from another without providing the statistics to validate the statement: e.g., “Certified CAM physicians treated considerably more patient with musculoskeletal mental and behavior problems . . .” What is the p-value and what statistical test was used – certainly not regression methods, which is all that is described in the “Data Analysis” section of the Methods.

Data on logistical regression analyses are missing. It is not satisfactory to simply say that “age and gender adjusted odds of completed symptom resolution were significantly lower . . . “ or that “resolution of symptoms was significantly associated with age.” At a minimum, the authors have to state the Odds Ratio's and 95% CI in the text, but even better would be a table with all the Odds Ratio’s and 95% CI calculated for this manuscript. It would also be useful to discuss whether the covariates made any difference in the model fit – e.g., the authors should show both unadjusted and adjusted OR and well as adjusted R-squared and a likelihood ratio test (or equivalent) comparing the two models.

Table 1 and 2: Where any of these differences statistically significant? If so, what statistical test was used?

Table 6: While the authors present percentages in this table, they used logistic regression to determine levels of significance. If the authors are comparing simple proportions as presented in the table, then they should have used another statistic such as the chi-square test. If they want to present statistics on adjusted data they should show the output from the regression model they used – e.g., Odds Ratio for logistical regression.

Not rationale was made for some of their comparisons – e.g., why did the authors choose “complete symptom resolution” as an endpoint instead of “any improvement” (complete resolution, plus much better plus better)? Clinically, any improvement in symptoms for musculoskeletal and mental health conditions is an accomplishment.

Minor Essential Revisions

GENERAL ISSUES
The authors refer to “perceived efficacy” in the Discussion section. Yet from most of the diagnoses for which CAM was used by the Swiss population, the only outcome measures are patient-reported (e.g., perceptions of pain, anxiety, depression). I would suggest that the authors refer to these outcomes as patient-reported instead of perceived. A subtle difference, but patient-reported outcomes are a more correct term in the literature.
Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:

I declare that I have no competing interests.