Author's response to reviews

Title: Use of complementary and alternative medicine by those with a chronic disease and the general population - results of a national population based survey

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Author's response to reviews: see over
Dear Ms Pafitis,

Thank you very much for reviewing our manuscript. The editorial comments we have received from your journal have been very helpful. We have revised our manuscript to address the concerns raised by the reviewers (attached) using track changes. I will also briefly address below where in the manuscript the changes can be found.

**Editorial Comment**

Document, within your manuscript, whether the data used for your study is openly available. If not, please document the name of the ethics committee which approved your study.

The data used for this study is not publicly available. We added a statement on page 5 (last paragraph of the methods section) indicating that this study was approved by the Conjoint Health Research Ethics Board at the University of Calgary.

**Reviewer 1 – Bob Phillips**

**Major Compulsory Revisions**

I would suggest strongly that homeopathy and herbalism are separated clearly. Homeopathy is the prescription of vehicle/placebo with no pharmacologically active ingredients. Herbalism is the administration of potentially pharmacologically active plant-derived products, with the significant risk of interaction with allopathic medicines.

Thank you for clarifying this for us. We have removed references to homeopathy in both the introduction (page 3) and discussion (page 10).

**Acknowledging the multiple related testing which has been undertaken (28 different comparisons) may be risky even with large samples, and spurious 'significance' arise from the data by chance alone.**

This is a recognized limitation to our study, and a comment has been added to our discussion (page 10) addressing this.

**Exactly which factors have been tested for, which covariates selected, which method of logistic (linear?) regression by which statistical package needs description in the methods section. I suspect that there may be different effects of covariates across different chronic disease groups, and that**
averaging them fails to recognise this. Doing this will also make it clear that some factors - like age - could not have been assessed using anything but a simple categorical approach.

Thank you for pointing this out, we have expanded our methods section (page 5) to include this information.

**Minor Essential Revisions**

**Tense.** The report frequently switches between present and past. (For example Results>> Predictors of CAM use>> Last sentence: "women were more likely" .. "are not currently married" )

Consistency please, and for my mind, the past tense (as they all reported some time ago) would be best.

Thank you for mentioning this. This has been changed throughout the manuscript.

**Methods >> paragraph 3 (Data on the use..).** The list is poorly punctuated. The use of commas may define more clearly the categories of CAM permitted.

This change has been made on page 5 (methods) of the manuscript.

**Results >> Prevalence >> comparisons.** The data here are presented as percentages, but it would be more useful to have them presented as OR (where the comparison can actually be made directly, rather than by reference to the tabulated estimates of general population use and coarse comparisons). As these are unadjusted estimates, I’d also not spend too long on discussing them as other factors may play into them.

This change has been made on pages 6 and 7 in the results section.

**Results >> Prevalence >> paragraph 2, last sentence.** Starts with "While those ..". Whole sentence may be better restructured.

This sentence has been re-worded (page 7).

**Discretionary Revisions**

**Discussion.** Suggest a much greater exploration of why there may be variation between diagnoses. The life threatening disease (diabetes) uses little CAM, where the morbid but non-lethal (migraine) uses lots of CAM. Are there other data to suggest use of CAM for psychosocial/pain problems more commonly than organ dysfunction, such as renal impairment and blindness?

Thank you for pointing this out, a paragraph has been added to the discussion to address this (page 8).

**Discussion.** Suggest a brief exploration of the (to me surprising) locality difference in diabetes. Does this reflect a data blip, or is it well known and age-distribution related?

Please see the new paragraph that has been added to page 8 of the discussion.
Discussion. I would have liked to see a further discussion of the inverted-U-shaped relationship between age and CAM use.

A sentence has been added to page 7 in the discussion to address this.

Conclusion. There's a suggestion that emphasis needs to be placed on how to integrate CAM services. Why? Just because people do something, does it need to be integrated? (For example - provision for smokers in in-patient facilities?) Should this CAM integration be publicly funded or on private insurance only?

Our conclusion (page 10) has been re-worked to focus more on the results of this paper and less on broader health care system issues.

Reviewer 2 - Michael Goldstein
First... the lit review: It is very brief and basic. One problem is that it is unclear if the pieces cited are meant to refer to Canada or to the broader scene in the US and other developed nations as well. The statement that "little attention has been paid to how CAM use differs by type of disease" (or words to that effect) is flat out wrong. There are literally hundreds of articles addressing this point, beginning with Eisenberg’s classic pieces decades ago.

While the introduction was primarily intended to focus on the Canadian literature, we have re-executed our literature search. We have now included (on pages 3-4 of the introduction) one of Eisenberg’s classic piece “Eisenberg DM, Kessler RC, Foster C, Norlock FE, Calkins DR, Delbanco TL: Unconventional medicine in the United States. Prevalence, costs, and patterns of use. N Engl J Med 1993, 328(4):246-252 along with several other studies.

We were not able to locate other population-based studies that examined differences in CAM use among individuals with different types of chronic diseases and adjusted for predisposing characteristics such as age, sex, income and education. There were lots of studies looking at CAM use for various chronic conditions, but these were not comparative studies, but rather a study looking at a single chronic condition. If you are aware of other key studies we may have missed, we would be more than happy to review them and make reference to them in this paper.

It is also not really clear why the 4 specific conditions used here were selected. The stated reason (that they exist over the lifecourse) could apply to hundreds of other conditions as well. It would have been much better to select conditions that might be related to the types of practitioners the study asked about. For example, if you are examining the use of chiropractors, then ask about musculoskeletal conditions, etc. In addition a high proportion of CAM use is in the form of self care which is left out of this study. That should be mentioned somewhere.

We have expanded this section on the bottom of page 4 in the methods section. Unfortunately the data set that we were using for this study does not include information on self care, so we were not able to incorporate that into this paper.
The discussion deals with all sorts of things that are not really in the data. For example, the refusal of many CAM users to reveal their use to their MDs. But the major problem is one the authors themselves see: There is no link in the data between the health problems they are looking at and the use of providers. So what if I have migraines. Perhaps I saw a chiropractor for low back pain, etc.

Unfortunately this is a limitation of the dataset that we are using for this study. And we are unable to address this any further than mentioning it in the limitations section of the discussion (page 10).

**Reviewer 3 - Marek Brabec**

Here is a minor suggestion, motivated mainly by my own curiosity. On Page 17, there appears a statement “individuals who are not currently married or in a common law relationship are more likely to have used CAM services than those with a live-in partner”. That is certainly interesting and motivates various possible explanations. Given the fact that women tend to use the CAM more frequently (as reported earlier in the paper), one might want to explore the partnership status influence more thoroughly. Is it a woman who drives the use of CAM for men in a partnership? Or, is the increase in CAM use between single and in-partnership individuals similar between men group and women group? Nice thing is that this type of questions can be easily answered with the data the authors have already.

We too were intrigued by this; however, exploring it further hasn’t turned out to be a simple as we had hoped. We ran into sparse data issues and the Research Data Centre (which houses all of the Statistics Canada surveys) has very stringent sample size requirements for data release. Additionally, the dataset that we used for this paper does not contain any information on motivations for seeking out care. Our group is interested in exploring this further, but think that we may need to further explore some additional datasets to see if we can go beyond stating that differences exist and trying to figure out some of the underlying causes.

Thank you again for taking the time to review our paper. We hope that you find the revisions to be satisfactory. We look forward to hearing from you shortly.

Sincerely,

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