Title: Complementary and alternative medicines (CAMs) and adherence to mental health medications

Authors:

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Author's response to reviews: see over
Re: Response to reviewers comments from BMC Complementary and Alternative Medicines

Dear Reviewers,

Thank you very much for taking the time to review this paper. I am delighted by the fact that you appear to have enjoyed the paper overall, and agree that your recommendations will complete the paper. Below, I outline how I have addressed these recommendations. I hope this will be to your satisfaction, but if you feel anything needs further clarification, please feel free to let me know.

Reviewer 1: Jerome Sarris

1. “Please add in the abstract your a priori reason for thinking of the relationship between CAM and adherence to medication”

This has been done (Page 2 Par 1)

• “This was based on suggestions that within medical pluralism, CAMs may reduce adherence to conventional prescription medications for reasons such as their further complicating the medication regime or their being perceived as a substitute with less adverse side effects than conventional prescription medications”.

Reviewer 2: Ursula Werneke

1. “The categories of mental health problems seem quite broad and psychotic disorders are not included. Within each category, the range of severity is potentially large. For instance, an affective disorder may manifest itself as a mild depressive episode at one end of the spectrum and as a bipolar I disorder at the other. The authors take account of severity to a degree since they control for individuals with or without a manifest diagnosis of a mental health problem. Medication count may again constitute a marker for severity of illness or an expression of conventional medicines not working”.

13th November 2013
• With regard to the breadth of the categories of disorders, as is explained in an endnote associated with the methodology section, these are those followed by the World Mental Health team. This is explained in the article (endnote ii).

• You are correct in that psychotic disorders are not included and their distinctive nature does warrant attention. I have added a paragraph concerning this to the discussion (page 15/16). I hope this is to your satisfaction. I have also made it clearer throughout the paper that although we do not have information concerning diagnosis of psychosis, anti-psychotic medications were amongst the list of specified medications. I have also outlined my reasons for not including psychoses as a category of illness i.e. the issues surrounding diagnoses.

• You are correct in that within each category, the range of severity is potentially large and your other thoughts are also very interesting. I have added a paragraph to the discussion to outline your thoughts on this (page 15). I hope this is to your satisfaction. I hope you do not mind me sticking quite closely to your expressions, however I felt that they reflected the issue well and I did not want to stray from the issue.

• There was an additional question within the NCSR in which participants were asked to rate their physical health and their mental health, and this may have been interesting concerning the range of severity. However, the variable had extremely high incidences of missing data (>70%) and thus I felt they were not suitable for inclusion.

2. “In this model, BMI is included as a marker of physical health. But in the realm of mental health problem, an increased BMI could be associated with adherence to psychotropic medication. Many (but not all) are associated with weight gain”.

• Your comment re medication adherence and BMI is a valid point. I have amended the analysis such that BMI is no longer included in the model and a note to this effect has been added to the discussion (page 14/15). Instead the presence or absence of a chronic physical illness has been taken as a token marker of physical health within the regression analysis.
3. Other variables than included in the current model may affect adherence. Such might include social and educational status, ethnicity, family relations and severity and chronicity of mental health problems. Type of CAM may also play a role. Herbal remedies or supplements may impact on adherence to conventional medications in a different way than acupuncture or other non-pharmacological CAMs. Also the potential for interactions and adverse events would differ. That is why it would be helpful statistically to explore how much of the variance the model with the currently included variables actually explains (R²). From the odds ratios, it seems that age and medication count –albeit statistically relevant- may be of limited clinical significance.

- Again, I can only agree with your comment. I have included education in the model, as well as household income centred around the mean as this appears to be the closest thing available that may align with social status.
- With regard to ethnicity, this is a very interesting point however the way the ethnicity information is gathered within the NCSR gives dozens of small categories and I felt that to start to collapse these was beyond the scope of this paper. I hope you do not mind this and I have added a note to the discussion to reflect the need to consider this matter.
- Family relations is also a very interesting area however I do not see any of the information available within the NCSR as being satisfactory to answer this matter.
- I have already addressed your point re severity of illness within the earlier point, however, the matter re chronicity of illness is complicated also. For a lot of the illnesses addressed by the NCSR, the question re timing of first episode is asked. However, this is asked of the participant, therefore there may be confusion concerning first clinical episode versus first appearance of milder subclinical symptoms. Also, the question re timing of first episode is not asked concerning all illnesses, therefore there would be an issue where an individual meets the criteria for several mental health difficulties. I hope you find my handling of this matter satisfactory.
- You noted that with regard to CAM, it is actually possible that the type of CAM may play a role as the potential for interactions and adverse events would differ. I have taken this point on board and have separated out ingested forms of CAM such as herbal remedies or supplements from acupuncture or other non-pharmacological CAMs. Thank you for this point.
• I have listed the $R^2$ to outline how much variance the model explains. I have also added comments within the results and the discussion making explicit just how little variance is explained and that further research is necessary concerning factors underlying these relationships and their applied importance.

4. “The paper would benefit from a clearer structure and simplification. The statement that “Classification was unimpressive with a success rate of 62%” needs explanation”.

• I have clarified the statement re classification.

• I have also re-read the paper and clarified any apparent ambiguities. I hope this is to your satisfaction.
  o I feel that clarifying how the medication group were defined plays a large role in this. Also, given the desire to be succinct and clear, given how CAM use is categorised, I felt is no longer necessary to provide descriptive statistics for each form of CAM individually.
  o Please note that although I conducted the stratified and weighted analyses in MPlus, I chose to report the SPSS analyses. The reason for this is that the option for the chi square statistic to test the overall significance of the model, nor the option to assess the ability of the model to classify individuals are available in MPlus. I wanted to use these as I think they are interesting and show the applied meaning of the results. Nonetheless, it was important to consider the role of weights and stratification. The two sets of results were largely identical, except in one case where a variable narrowly missed statistical significance ($p=.07$) in the SPSS analysis but was significant when weights and stratification were considered in MPlus. I decided that I wanted to keep the positive features of the SPSS analysis noted above, and just point out this one minor anomaly.

I hope my addressing of these revisions has been to your satisfaction, but if you require anything further, let me know.

Best,

Edel