Reviewer's report

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

Version: 3 Date: 21 November 2013

Reviewer: Ursula Werneke

Reviewer's report:

Major compulsory revisions

1. My comments on the first version: 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.

My comments on the revision:

I suggest that the method is expanded to account for how disorder and categories were chosen rather than adding this in a short endnote.

Severity of illness and non-inclusion of psychotic illnesses: The author addressed both points, but they should be moved to the paragraphs on limitations.

2. My comments on the first version: 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.

My comments on the revision:

P12: Sentence beginning with: “Based on the Wald statistics, results partially rejected hypothesis one as neither chronic physical health conditions (absence versus presence) nor clinical mental health diagnosis (presence versus absence) nor CAMs (use of herbal remedies / supplements, use of nonpharmacological CAMs, use of both herbal remedies / supplements and non-pharmacological CAMs) significantly (Table 1)” Change to “reduced adherence significantly”.

If I have then understood the sentence correctly, the question would arise why physical health problems do not affect adherence to conventional medications. With severe acute physical health problems, I would expect the likelihood of adherence go up. With chronic problems, adherence may possibly go down. Again, not only the nature but also the degree of a respective physical health
problem most likely matters.

3. My comments on the first version: Other variables than included in the current model may affect adherence. Such might include ethnicity, social and educational status, 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.

My comments on the revision:
I understand from the author’s comments that variables like ethnicity cannot be accounted for. I should like to point out in response to the author’s letter of amendment that it is not a matter whether I mind personally, but whether the choice of variables compromises the scientific validity of the model. This point becomes clearer when looking at the R2, which gives the percentage of variance explained by this model. The model only explains 4% of the variance. Thus, there are likely other variables, which could yield a better explanation, but are not captured by the NCSR dataset. The low R2 unfortunately raises questions about the relevance of the current model and the included variables. Unless other variables from the NCSR dataset can be included in this model, which can increase R2, the scientific relevance of the model remains extremely limited.

Minor essential revisions

4. My comments on the first version: The paper would benefit from a clearer structure and simplification. The statement that “Classification was unimpressive with a success rate of 62%” needs explanation.

My comments on the revision:
The classification sentence is still difficult to grasp, the question arises what an “impressive” rate would be. In my view, it may be advisable to report neutrally in the result section and then move on to comments.

Level of interest: An article of limited interest

Quality of written English: Needs some language corrections before being published

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

Declaration of competing interests:
I have no competing interests