Reviewer's report

Title: Mindfulness-based cognitive therapy for multiple chemical sensitivity: a study protocol for a randomized clinical trial

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Reviewer: Jenny van Son

Reviewer's report:

First of all, I think the authors describe a protocol of an interesting study with an original study population. I think the protocol is suitable for publication in Trials. However, I do have several questions and comments on the article, see below. An important issue is that the discussion is missing.

Comments

Major Compulsory Revisions

Concerning the discussion:
1. Where is the discussion?

Minor Essential Revisions

Concerning the objectives and hypotheses:
2. Add ‘Illness perceptions’ to the objectives.
3. In the hypotheses, the authors state the following: “we expect a treatment effect will be mediated by the level of mindfulness...”. In addition, they are planning to examine several other possible mediating mechanisms. In which outcomes are you going to examine this?
4. The authors should add a third aim about the examination of different mediating variables.

Concerning the Inclusion criteria section:
5. What age-range do you apply?

Concerning the Exclusion criteria section:
6. What about meditation techniques that are comparable to mindfulness meditation (i.e., zen meditation)? What about experience with Vipassana meditation?

Concerning the Randomization section:
7. At what time point will the baseline assessment take place? Is that after or before the randomization?
8. In which order will the randomization take place? e.g., consecutive?
9. What type of randomisation will be used? Are there any restrictions, i.e.,
blocking?

10. Who will inform the participants about the allocation?

Concerning the Intervention section:
11. Can the authors give an example of the adjustments they have made to the original program of Segal et al.?

Concerning the Measurements section:
12. Which measurement time points do you apply?
13. I miss a reference after the statement of the authors that the PSS-10 has proven to be a valid and reliable measure of perceived stress.

Concerning the Statistical analysis section:
14. What will be the method for the analyses of the mediating mechanisms?

Concerning the Power calculation section:
15. In the power calculation, the authors should take into account patient attrition also.

Minor comments
16. Page 4 (background): the first time you use the word CNS, write it in full.
17. Page 5 (background): a comma instead of a dot after ‘example’ in the sentence “…a high degree of co-morbidity to MCS, for example,…”
18. Page 6: the aims and hypotheses should be put in a separate section, called ‘Aims’ and not in the methods section.
19. Page 9 (intervention): write RCT in full or put the abbreviation in brackets after the full written word the first time you use it.
20. Page 11 (measurements): “into Danish” instead of “in to Danish”
22. Page 15 (ethical considerations): two small mistakes, namely 1) “a” MBCT programme instead of “an”; 2) “Any adverse events reported by the participants will be registered and reported in future publications”.

Discretionary Revisions

Concerning the objectives and hypotheses:
23. The formulation of the concept “commonly experienced symptoms” in the ‘objectives’ is a bit vague. In addition, “the degree to which chemical tolerance affects participants’ lives” is vague because it is very broad. Perhaps you can state (in brackets) the domains or examples of the domains which the QEESI will assess.
24. The difference between ‘psychological distress’ and ‘perceived distress’ in the objectives is vague. Perhaps the authors can state the exact concepts they want to measure instead of the general term ‘psychological distress’ (like the
authors already did in their hypotheses).

Concerning the Design section:
25. In the section ‘Empirical design’, the authors state that the trial will be undertaken at suitable locations. Can the authors be clearer what they mean with suitable locations?

Concerning the Assessment section:
26. The authors state: “…we will use this information to verify whether symptoms reported in the SCAN can be attributed to an illness of known physical cause other than MCS.” Will this be done by a medical doctor? And what will be done with the information?

Concerning the No treatment section:
27. Will the participants in the no treatment group receive the programme at a later moment? If not, how are you going to keep these participants motivated?

Concerning the Measurements section:
28. The statement “The authors found the scale to have acceptable psychometric properties” is vague.

Concerning the Statistical analysis section:
29. The authors want to use the method of multiple imputation to address missing data. In our own trial we eventually decided to do this in a sensitivity analysis only. Because it can be questioned if you can reliable estimate the outcomes when for example only one assessment of a particular patient is available. In most cases you have demographic and clinical information about the patient, but there is still so much information that you miss. To date, there is no consensus about what to do. There are many other trials that use multiple imputations before performing their analyses. Nevertheless, if the authors have not done this already, I advice them to discuss this issue with a statistician.

Concerning the Power calculation section:
30. The authors state that they believe that a 25% reduction on the QEESI will represent a clinically meaningful reduction in the impact of MCS. Can you underpin this?

**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.