Title: 'Walk This Way': Results from a pilot randomised controlled trial of a health coaching intervention to reduce sedentary behaviour and increase physical activity in people with serious mental illness

Version: 0 Date: 23 Sep 2018

Reviewer: Magnus Lindwall

Reviewer's report:

The topic of the paper is timely and highly relevant, dealing with the effect of a health coaching intervention on physical activity for individuals with severe mental illness. However, a number of problematic issues and uncertainties exist which makes the potential impact of this paper very unclear at the present stage.

Some major limitations are:

To start, I'm wondering about the scientific impact of the paper and its results. What is the contribution to the field of this report of a pilot study? What are the lessons learned? In general, the paper brings about very little new relevant information. The authors claim that the primary aim of the study is to establish the feasibility and acceptability of the intervention by evaluating if participants could be recruited, randomized to and complete the intervention. As a primary aim of the paper, this seems not so relevant. Examining if one is able to recruit participants and randomize them to an intervention study is not of any large scientific interest and does not contribute to the literature. The answer to this question should be self-evident, of course the authors will be able to recruit and randomize participants. A question that could be of interest, in a different paper, is to what extent the authors manage to recruit enough participants, given some standards. Overall, the primary purpose of the study does not seem relevant enough to highlight in a paper such like this, the effect of the intervention is of much higher interest. I can understand why the authors would like to describe not only the effects of the intervention, but also the feasibility of it, but the rationale for highlighting this issue is simply not provided. If the authors still like to focus mainly on the intervention study set-up, its feasibility, I suggest they focus only on this issue and rebuild the paper accordingly, building a strong case for the need of this information in the introduction and only highlighting results linked to this in the Results part.

The main lesson learned from the results seem to be that it is very hard to recruit enough participants to intervention studies such as this one, and to keep them in the study. This information is not new to the community, and therefore the results does not provide any new information to the field.

The study seem unpowered, in particular to examine effects of the intervention. If one wants to examine if the intervention could reduce SB and increase PA (as in the second aim), aiming for
20 persons in each group seem not very realistic. The argument that the number of 40 was based on recruiting an adequate sample to assess feasibility of the study with the resources available is very unclear. What resources what available?

A key aspect of any intervention study is the nature of the intervention itself and not least the hypothesized mechanisms therein. Very little information is provided about this in the paper. For example, what is the theoretical rationale that the health coaching program would help the participants change behavior? What are the hypothesized theoretical mechanisms of this program? If the authors would have addressed the aspect of mediation effects, the paper would be of much higher interest. In light of this, the lack of information on the health coaching program is problematic. What is the scientific backbone of this program? What theoretical foundations does it stand on? Has it been tested before? Given the reference, I wonder about the evidence-base of this program. I assume that the authors have chosen a program with sound evidence-base, given its importance in the intervention and the study, but this can not be evaluated given the present information given in the paper. I suggest the authors develop the rationale for the health coaching program and the mechanisms of the intervention and make this a bigger part of their paper. This will also open up for examining not only main effects of the intervention, but also mediating effects and mechanisms of the intervention, which would be of large interest to the field, answering questions such as why, through which mechanisms, does the intervention work (or not work). In light of what we know in the field, such information will be of much more interest and relevance compared to what is now highlighted in the paper as the primary aim.

More minor limitations:

The tables are not in line with normal standards in scientific journals.

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

No

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.

Unable to assess

**Are the conclusions drawn adequately supported by the data shown?**
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