Reviewer’s report

Title: Effects of mental health self-efficacy on outcomes of a mobile phone and web intervention for mild-to-moderate depression, anxiety and stress: Secondary analysis of a randomised controlled trial

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Reviewer: Dror Ben-Zeev

Reviewer’s report:

This paper reports on two studies that lead to an examination of the role mental health self-efficacy plays in the impact a mobile phone and web based intervention has on depression, anxiety, and stress. The first sub-study focuses on construction and validation of a new measure developed by the authors for measurement of mental health self-efficacy. The second study examines the role the construct (as measured by the new instrument) played in the clinical outcomes of patients engaged in an RCT of the eHealth/mHealth intervention.

The paper is very well written and is clear in terms of objectives and findings. The statistical approach is thoughtful and thorough, and the interpretation of the findings seems grounded in the data. This manuscript would add to the literature both as report on the development of a new instrument, and in examination of mechanisms of action in a very promising technology-based intervention. This is a very strong submission and I have only a few points for the authors' consideration:

1) The rationale for developing their measure to assess mental health self-efficacy is not entirely clear; why would existing measures like the one they reference (Carpinello’s) or others like the Recovery Assessment Scale, which has a Domination by Symptoms factor (Corrigan et al., 2004) be unsuitable for people in the mild to moderate range of symptom severity? Do the authors suggest that people who have more severe symptoms constitute a qualitatively different group? This seems inconsistent with the view that symptoms and functioning are dimensional, i.e. everyone gets a rating on some level.

2) Did the authors verify that participants who were enrolled for the proof of concept study were not also enrolled later on for the RCT? If there is possible overlap between the groups, how would this impact the findings from study 2?

3) How specifically was consensus achieved in the item reduction phase for study 1? Agreement among the investigators? Were people with mental health conditions involved?

4) The authors assume that MHSE is malleable and a viable target for intervention. Correlating the new MHSE measure’s scores with the TIPI, a measure of personality traits (arguably scores would be more stable) seems conceptually counter intuitive.
5) The Analyses section (line 181) suggests the TIPI’s emotional stability subscale was selected a priori for evaluation—can you explain why?

Reference

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.