Reviewer's report

Title: Evaluation and treatment of low and anxious mood in Chinese speaking international students studying in Scotland: Study protocol of a pilot randomised controlled trial.

Version: 2
Date: 17 December 2014

Reviewer: David D Ebert

Reviewer's report:

This manuscript is a study protocol for a feasibility study that aims to investigate take-up, drop-out, and completion rates of an online-interventions for Chinese speaking university students in Scotland.

The authors certainly addresses a timely and very important topic. There is clearly a need for low-threshold evidence-based interventions for the treatment of depression and anxiety in migrant populations. And it certainly makes sense to conduct a pilot study in order to test the feasibility of recruitment etc.

I also do have some further remarks the authors may want to consider to further increase the quality of the manuscript:

. The need for this study on the basis what is already known should be elaborated in the introduction

. The authors might also want to consider to include prevalence rates of anxiety and depression in students in general and in foreign students in particular

. Gellatly (2007)= could be replaced by something more up to date (e.g. Richardson 2012 Clin Psych Rev; Baumeister, Internet Interventions 2014)

with depression: randomized controlled trial. Journal of Medical Internet Research, 15(10), e227. doi:10.2196/jmir.2853,

. The authors state that the intervention has been evaluated extensively, but do not review the results, I think this would be helpful to get an impression about the evidence base of the intervention.

. Has the intervention in English been evaluated as internet-version, if so what were the results?

. The rational the authors give in the introduction for why they do not want to assess a diagnosis of participants (i.e. non-clinical sample, symptom and not diagnose inclusion criteria) is not convincing. One can still assess a diagnose in order to get an impression what sample one has included. Thus I would suggest that the authors remove this explanation. If the authors leave it in, I would suggest to remove it from the introduction to the method section.

. Reference for the PHQ-9 version is missing in the inclusion criteria section.

. The authors indicate that they use measurements both in English and Chinese, but do not give references to validated Chinese versions.

. The measurement section could benefit from more information about the reliability of the scales e.g. alphas.

. Inclusion criteria: the authors state that the trial is aimed at mild-to moderate depression/anxious individuals, but do not indicate that they plan to use an upper cut-off for exclusion.

. In general the authors make quite a few statements in the manuscript without referring to adequate literature to support their statements. E.g. Discussion: "we now that depression is significant in this group" and that "access to help in the appropriate language is limited"

. The authors refer for statements that cCBT is effective only to their own manuscripts instead of available meta-analysis. Moreover the manuscripts they refer to are all under preparation and not published. I would suggest to refer to published meta-analysis.

. Is the trial registered? if yes, please include the information about it. If not, I would suggest to register it, otherwise the authors might have problems to publish the results (as most, also open access journals nowadays require a priori trial registration.

. The information about the ethical approval could be removed from the discussion section to the method section.

. The authors state that it is unlikely that participants will experience worsening of symptoms. On what basis do the authors come to this conclusion? There are so far almost no studies on deterioration rates in internet-based treatments for depression/anxiety.

. Having said this, the authors might want to consider also to assess potential negative effects of the treatment, as it has been recently suggested for internet-intervention trials (e.g. Rozental, A., Andersson, G., Boettcher, J., Ebert, D. D., Cuijpers, P., Knaevelsrud, C., . Carlbring, P. (2014). Consensus statement
on defining and measuring negative effects of Internet interventions. Internet Interventions, 1(1), 12-19. doi:10.1016/j.invent.2014.02.001

. The authors might want to consider to elaborate in the discussion how this study add significant knowledge to the literature.

. Most of the information about ethical consideration could be removed from the discussion section into the method section.

editorial

. I think it would be very interesting to have more information about the specific intervention used in the study described in the manuscript. The study protocol is great possibility to provide detailed information, that usually do not fit into the study results paper.

1. Will the study design adequately test the hypothesis? YES
2. Are sufficient details provided to allow replication of the work or comparison with related analyses: if not, what is missing? YES
3. Is the planned statistical analysis appropriate? yes
4. Do the figures appear to be genuine, i.e. without evidence of manipulation? YES
5. Is the writing acceptable? medium