Reviewer's report

**Title:** Development and Evaluation of culturally sensitive psychosocial interventions for under-served people in Primary Care

**Version:** 1  **Date:** 22 May 2014

**Reviewer:** June Brown

Reviewer's report:

(1) Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)

Discussion

a) I think there needs to be more discussion about the reasons about the poor take-up of the study. I was personally a little puzzled by the authors’ decision to combine an intervention for two difficult to engage groups - older people and BME groups (p6). I imagine that different considerations will be important for these 2 groups. I think there needs to be discussion about the issue about whether the decision to combine an intervention for elders and BME groups was the right thing to have done.

It is stated that this was to find a ‘common model for developing interventions appropriate (for) under-represented groups’. I can accept the logic of this, given the issues - common problems of chronicity, social isolation, stigma and lack of identification with routine biomedical understandings of mental health – as well as issues of differential mental health models, perceived candidacy for treatment, recursivity in cultural expectations of treatment. This poses a major challenge about how the new intervention can circumvent all these problems. It is a hugely ambitious goal to do all this. I was not clear from the paper how all this was being attempted. This probably needs to be made clearer, specifying both the successful and less successful aspects.

I think there needs to be more discussion about the publicity. There is little information given about the publicity that was used for this study. This is particularly important if self-referrals are to be encouraged. I imagine that it would be difficult to publicise interventions for both groups, without this being rather bland. And I think that the publicity is the key process by which one can begin to portray a different ‘mental health model’ that would be acceptable to the difficult to engage groups. I also wondered if publicity was targeted, e.g. sent to care homes and hospitals where older people may reside. The information needs to be given.

b) The issue about the intervention needs some discussion. It seems to be a one-off session with a wellbeing practitioner, followed by a choice of 3 options: individual work, group work and signposting. However, groupwork content may vary according to the group – BME (intergenerational conflicts) or elders
(Creative activities). I think it is quite a difficult task to evaluate this type of intervention, when there is likely to be so much variability. I was not clear if staff with different professional training had different roles. I suggest there is a statement about the origins of this idea but that this intervention may be difficult to evaluate. Another issue that arises is the balance of decisions about interventions being influenced by professional views about evidence-based treatments and how far by the views of user groups.

c) Finally, I am not sure that the authors can necessarily conclude that this is the preliminary stage to a larger trial. It may be that some new interventions first need to be developed to engage these groups, particularly the BME groups. I would therefore recommend some toning down of the conclusions and further discussion about the issues that have arisen from this study.

I think the limitations could be stated more clearly.

a) These results suggest that more elders were referred/referred themselves (n=84) compared to BME groups (n=39), over the recruitment period of 16 months. I wonder if this study suggests that it may be easier to recruit elders using current recruitment methods, but that recruiting BME groups (of different ethnic backgrounds) may need a slightly different approach. This needs to be discussed more fully as this could have implications for further interventions and future trials.

b) Aims of study: given the major recruitment problems, it is difficult to state that the trial showed that the intervention was acceptable. While the intervention was acceptable to those who came, it cannot be said of those who did not come. We lack information about the feasibility of delivering the intervention in routine primary care, if the volume of referrals increases. The clinical results may appear promising, but cost-effectiveness calculations are very much affected by the number of people treated as well as the cost of resources used.

(2) Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

a) The study was conducted in 2 different sites. It is not clear what would happen if one person in one site wanted a service (e.g. elders group) provided in the other site. There is also little information about the 2 sites – apart from there being 4 deprived localities - and whether there were differences in people recruited in the two sites. I was not sure how older BME people were categorized. This information needs to be included.

b) The authors clearly acknowledge work upon which they are building, both published and unpublished. The authors rightly say access is a major issue and that this is a policy priority in the UK. This needs to be referenced. I also suggest the authors include some other references relevant to the development of the intervention: one is White et al (2008) who developed the STEPS approach for communities through proactive community work, Grant et al (2012) who report the higher attrition among deprived communities and Horrell et al (2014) who found a non-stigmatising self-referral system did recruit depressed BME groups
c) There are some mistakes with the labelling of figures and tables. There seems to have been a mistake with the additional files. Table 2 looks like Fig. 2 (intervention) and Table 5 looks like Fig 5 (elders’ results). And there are no labels for the Figures. (3)

(3) Discretionary Revisions (which are recommendations for improvement but which the author can choose to ignore)

I have a concern about the word ‘wellbeing intervention’. I know that the word ‘wellbeing’ was suggested by groups who were consulted. However, there is some evidence that suggests that wellbeing and distress may be conceptually different (Keyes 2005).

Good
a) The aims of the study are clearly stated,
b) The methods are generally well described.
c) The consultation work that was done in the development of the programme is commendable.
d) the data is sound

In summary, I think this is a very useful study, that demonstrates the difficulty in recruiting ‘difficult to engage’ groups. I am however not convinced that the publicity and intervention used are really strong enough to change the existing patterns of non help-seeking by these groups. However, I think the development work has been extremely worthwhile with the systematic reviews and wide consultation.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:

No, I declare I have no competing interests