Author's response to reviews

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

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Version: 2 Date: 10 July 2014

Author's response to reviews: see over
Dear Editor,

We have addressed the points raised by the reviewers and have made major revisions to the introduction and throughout the document to accommodate the issues raised where appropriate.

Reviewer 1

Major essential revisions:

- 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 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.

We have adapted the introduction to clarify the relationship between groups in the theory underpinning the interventions and added debate around specialist services vs the need for and difficulty of addressing diverse populations via routine services in interventions to conclusion of the discussion section

- 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.

This was addressed in the community engagement component - we have clarified this and added discussion of the to the Background section and referenced papers where this is discussed in detail.

- 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.

We have highlighted the scope of the programme grant and wider literature in the background paragraph 3 and final paragraph.

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, group work 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.

We have highlighted that the variability in these interventions correspond to common underpinning theories reported elsewhere and referenced in the paper.

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.

We have highlighted the community and primary care engagement interventions carried out in the programme and some of the wider work done in this area, more could be added but we’ve tried to focus specifically on the psychosocial intervention as the paper and references are already quite long.

I think the limitations could be stated more clearly.

We have added clarification of the limitations.

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.

We have added to discussion and highlighted reference to further discussion of this issue.
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.

We have clarified that we are referring to the acceptability of the intervention – not the acceptability of the recruitment strategy that was addressed in the community engagement and primary care components reported elsewhere. We have added discussion of the recruitment issues faced in the Somali intervention site.

(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)

- 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.

We have clarified the 4 sites in which the intervention was delivered and the impact on classification of patients or reporting.

- 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(2).

We have added these references to inform the discussion section.

- 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)

This appears to have been an issue in uploading the content to the author center, we will resolve this on resubmission.
(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).

We agree that the conceptual differences are important – however we also believe that lay appropriation of professional terms is inevitable in this type of work and that the onus is on academic audiences to maintain their more refined conceptions whilst accommodating to lay usages (lay usage of stigma is another common example). We have used italics when introducing the term to try to indicate this distinction since readers reportedly find using ‘wellbeing’ in inverted commas, or italics throughout a paper to be irritating. The rationale for this is discussed in methods paragraph 3.

Responses to Reviewer 2

- The author specifically identifies "black" and minority ethnic communities, separating this group out from the other ethnic minorities in the sample but provides no summary statement or information about findings specific to this group. A more appropriate term may be “ethnic minorities” unless the author plans to speak specifically about Blacks and the other minority group.

- This section is where the first mention of who the “Black” people are in the sample. The “Black” in the BME group are actually Somali service users in the UK. If the sample includes both South Asian and Somali participants it is important to understand the importance of using a more appropriate term to capture the ethnicity and culture of the Somali group like “ethnic minorities”. Somali are Africans which allows them to fall under the “ethnic minorities” umbrella. What then is the purpose of separating the Somali from the South Asians when they are both ethnic minorities in the UK.

- Given that only 1 Somali participant is represented in this paper the term "BME" seems less appropriate and the term "Black" should be removed because not only is it an incorrect term to associate with the Somali culture but the results cannot generalize to the Somali minority group. As such, the author should use the term ethnic minority or specifically identify and report “South Asian” as the population of focus throughout the manuscript.

- Furthermore, the author has to be careful to provide a better description of the sample considering the term “black” suggests a culture rooted in American Culture that is used as a racial categorization but may not capture the ethnic and cultural differences of Somali participants living in the UK.
More importantly, South Asian minorities and older people seem to make up the majority of the sample. The author does highlight the specific needs of South Asian families and older persons and this should be reflected throughout the paper.

We have removed the term BME and Black and Minority Ethnic from the paper, replacing it with 'ethnic minority'. We accept that the broad term is problematic given the preponderance of South Asians in the sample, but we cannot ignore the fact that the sample is mixed. We have highlighted in the text that, although we use the broad 'ethnic minority' term, we are referring to a largely South Asian sample.

Group Intervention:

- The author offered participants “a group session on intergenerational conflicts, such conflicts were mainly focused on living with in-laws”. Where the literature that suggests this is a culturally appropriate adaptation and not just a factor in treatment that should be addressed with any group (older group, Europeans/Whites, non-minorities)?

We have added appropriate indicative references to the literature on the role of ‘izzat’ family honour in South Asian mental health issues – and an accompanying reference to arts for elders.