Author's response to reviews

Title: Understanding psychological distress among mothers in rural Nepal: a qualitative grounded theory exploration

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Author's response to reviews: see over
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Dear Sir, Madam

We would like to resubmit an original research article to BMC Psychiatry.

We thank the reviewers for their comments. Our response to each set of comments/corrections is detailed below. Please let me know if there is anything else you require.

Yours faithfully

Kelly Clarke

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**Editorial comments**

It is noted that you have recruitment participants from a branch of a cRCT. Please can you include the trial registration number of this study within your manuscript.

Done (line 94)

Please also remove the section in your methods titled "Roles in data collection and analysis", as this information is present in your Authors Contributions section."

We have removed this section, although we have retained three sentences regarding the background and experience of individuals involved in data collection (lines 188-191), as recommended by qualitative research appraisal tools [1].
Reviewer 1

This study covers a topic of great interest: the cultural variables in depressive disorders and in particular of postpartum depression (see Kirmayer 2001). The study presents interesting results. However, the mode of expression of discomfort during the postpartum through reference to somatic symptoms have been explored in different cultures such as Europe (Jalenques et al. 2009), South America (Rojas et al. 2006) and the Indian sub-continent (Savarimuthu et al. 2006). This last work has shown, for example, than viewed from a biomedical framework, fails to acknowledge the role of context in the production of emotional distress in the postpartum period. Therefore, the current state of research, the results of the study have a value more specifically local, and should be published in a journal more specifically oriented to transcultural psychiatry or to social psychiatry or a regional psychiatric journal rather than an international journal of general psychiatry.

We are grateful to the reviewer for their comments. Certainly our findings build on those of previous studies in terms of the importance of context in understanding experiences of maternal distress, and highlighting potential shortcomings of a biomedical framework (discussed extensively on lines 511-571). We also relate our findings concerning somatisation of distress to earlier studies (lines 488-496).

However, we think that our analysis also makes a novel and important contribution to the literature for several reasons. First, tension is commonly used as an idiom of distress across South Asia [2-5]. Our rigorously conducted qualitative analysis is one of the most in depth explorations of tension to date. Second, there is a disproportionately large burden of perinatal common mental disorders in South Asia and other low-income settings [6]. Epidemiological studies are of limited use in terms of understanding context and determinants of mental ill health, and qualitative investigations are needed to understand the social roots of these disorders. Well-conducted qualitative studies rarely find their way into leading, open access psychiatric journals, which impedes deeper understandings of the social determinants of mental ill health. Third, mental health research in Nepal is urgently needed: a recent study found that suicide is the leading cause of death in women of reproductive age [7]. PRIME, an international research consortium, is currently working in
Nepal to adapt packages of mental health care, including maternal mental health care [8]. Our study on maternal psychological distress is therefore timely, informative and of significant international interest, and for this reason we hope that it will be of interest to readers of BMC Psychiatry.

Reviewer 2

I would have liked to see more linkage to demographics in data presentation; how did for example, religion play into the construct of tension, strategies for coping etc.

There were no apparent differences in the experience of tension between Hindu and Muslim mothers. Muslim communities in Nepal are generally poorer, less educated and more marginalised than Hindu communities, and Muslim women may have more restricted mobility, potentially reducing access to social support [9]. This combination of factors is likely to put Muslim women at increased risk of tension, exacerbate their symptoms, and affect how they express distress. However, there is inter-mixing of Muslim and Hindu communities in Dhanusha, to the extent that Hindu communities have adopted traditionally Muslim practices including purdah, which requires women to conceal their bodies and faces in the presence of marital relatives and men, and to avoid being seen in public by remaining in the home [10]. Furthermore, experiences of poverty, domestic abuse, absent husbands and health problems were common to both Muslim and Hindu mothers in our study, suggesting similar contexts and predisposing factors for tension.

We have added this discussion to the revised manuscript (lines 498-509).

The GHQ-12; while this instrument has been widely used in screening for psychiatric morbidity worldwide, there has been some recent concern whether the instrument is in fact a reliable screener. E.g., Hankins, BMC PH 2008, 8:355.

We have added a sentence to the Strengths and imitations section to mention potential limitations associated with use of the GHQ-12, citing the reference above (lines 581-583). However, selecting interview participants using a higher threshold score (≥5) enabled us to sample mothers with significant levels of psychological distress.
Please elaborate/cite rationale for the cut-off \( \geq 5 \). Please cite a validation study for GHQ-12 for Nepalese women.

We sampled from a total of 1272 mothers who had participated in the Dhanusha cRCT and completed the GHQ-12 in the previous two months. We selected participants with a GHQ-12 score \( \geq 5 \) as some studies have found that this threshold discriminates well for maternal psychological distress [11-13]. Furthermore, enough mothers scored above this threshold to enable us to purposively sample participants based on severity of distress and demographic factors. This threshold score is higher than that identified in the Nepali validation study \( \geq 2 \), [14] in order to ensure the sampling of women with significant distress. We have explained this rationale in the Methods section on lines 155-162.

The description of the original cRCT study is confusing and it would be helpful to have some more elaboration beyond the one citation. How many women were in original study; how many scored \( \geq 8 \) in total (as all of those were recruited to this study)? How many declined participation for this piggybacked study? Also it is a bit unclear who exactly was then recruited for interview and who for focus group from the parent study, how many were mothers? How many group facilitators, how many from the community involved in the study and how many from parent-study-unrelated communities? I suggest to make a flow chart for recruitment and indicate sample size for each recruitment arm.

We agree with the reviewer that providing further information about the sampling strategy would be helpful for the reader, although a description of the design of the Dhanusha cRCT is beyond the remit of this paper and described at length elsewhere [15].

Regarding the FGDs, we have added a sentence to clarify that we purposively sampled six women’s groups from a total of 270 groups in six Village Development Committees (VDCs) (lines 127-128 and 134-135). We present characteristics of purposively sampled VDCs in an additional table (table 1). The number of group facilitators included in the FGDs, as well as the number of those from intervention and control areas is presented in table 2.

On lines 156-158 we have stated that we sampled interview participants from a total of 1272 mothers who had participated in the Dhanusha cRCT and completed the GHQ-12 in the previous two months. As explained above, we selected interview participants with a GHQ-12
score ≥5 as some studies have found that this threshold discriminates well for maternal psychological distress [11-13]. Furthermore, enough mothers scored above this threshold to enable us to purposively sample based on severity of distress and demographic factors. We also mention that, from a total of 1272 mothers, 116 had a GHQ-12 score ≥5 (line 162). We have added a sentence to state the number of mothers who were unavailable for interview (lines 167-169).

However, we do not feel a flow chart for recruitment by trial arm would be appropriate. The final trial results have not been published yet and sample sizes are therefore still preliminary. Participants were selected on the basis of specific characteristics rather than systematically excluded, as implied by a flow chart. In this qualitative study, we were not interested in evaluating the impact of the women’s groups on distress. Presenting sample sizes for each recruitment arm therefore does not seem relevant to this study.

Overall a wide range of respondents, with diverse educational/religious, socioeconomic spread, was sampled- a definite strength of this study; I suggest a more stringent description of recruitment and sample characteristic to make easier for reader to get the full richness of this group.

As described above, we have provided a richer description of the sampling strategy. We have also added a few sentences to describe interview and FGD participant characteristics (lines 237-246).

It is helpful to put the participants’ demographics in context to the region, and I appreciated the overall geographic descriptors and the tables (1+2) depicting the participant’s education, age range and religious affiliation. I wonder whether the authors could make more use of the demographics, literacy, religious affiliations etc of their participants for a more elaborate/richer interpretation of their narrative data? (e.g., common or differential themes across Hindu and Muslim mothers?)

As mentioned above, we have added a discussion on the role of religion in the experience of tension to the Discussion section (lines 498-509). In the Results section we outline differences in use of terms for psychological distress between more educated Pahadis and less educated Madheshis (lines 295-301). Although we sampled a range of castes, we did not
identify differential themes across these groups. Theoretical sampling on the basis of caste may have enabled more in depth comparisons, however this was not feasible due to limited opportunities for data collection.

Regarding data collection/analysis: Here a statement to clarify would be helpful as to the how the data collection for this study intersects with data collection of the parent study (the cRCT).

We have clarified that participants were identified during the Dhanusha cRCT (lines 93-94), and that interview participants comprised a subsample of trial participants (lines 156-158). We have also explained that FGD participants were members and facilitators of women’s groups implemented during the Dhanusha cRCT (lines 124-132). Facilitators were female community health volunteers or local women elected by group members [15], and although groups targeted women of reproductive age, group members also included older and unmarried women. Only those mothers who had recently delivered were eligible to participate in the Dhanusha cRCT.

It seems that the data analysis following grounded theory was an after-thought to the original qualitative data collection, and this should be openly stated. If this is not the case, it needs to be clarified as it seems confusing.

In the text we have clarified that we selected a grounded theory approach from the outset of the study, however we were unable to carry out theoretical sampling due to time and financial constraints (lines 118-121).

The paragraph on roles in data collection and analysis is confusing/irritating and seems more appropriate in acknowledgement section. Make sure that all authors are accounted for, I felt AC was left out.

We have removed the section on “Roles in data collection and analysis” as described above. AC was involved in the design and management of the Dhanusha cRCT from which we sampled participants and obtained data, as well as commenting on and approving the final version of the paper (lines 605-613).
There is no discussion on IRB approval from local committee on the presented study (just on parent cRCT)- was there a second consenting? Again, this may be just confusing as these studies may be all part of one.

The Dhanusha cRCT was undertaken under a Memorandum of Understanding with the Government of Nepal Ministry of Health. It had ethical clearance from the Nepal Health Research Council and the Ethics Committee of the Institute of Child Health and Great Ormond Street Hospital for Children, UK. We obtained further ethical approval for the qualitative study from UCL Research Ethics Committee (2656/001). We have clarified this on lines 228-234.

Also, were any men in this narrative study?

Unfortunately, apart from the Dhami, there were no men in this study. We have mentioned this in the limitations section (lines 579-581). It would certainly be interesting to explore male perspectives on maternal psychological distress in a future study.

Were participants reimbursed for interviews or FGs?

Participants were not reimbursed for interviews or focus groups. We have stated this on line 191.

References


