Reviewer's report

Title: Patients understanding of depression associated with chronic physical illness: a qualitative study

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Reviewer: Peter Bower

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• Major Compulsory Revisions

1. This is a useful and timely review exploring understanding of depression associated with chronic disease, in a sample of patients with chronic disease, with and without coexisting depression.

2. I am not a qualitative researcher, but the methods (and their description) seemed generally rigorous and the sample size conventional for a study of this type. I had no major concerns about the way the study was conducted.

3. I was surprised that the study did not reference the study by Coventry et al (BMC Family Practice, 12(1),10) which deals with similar issues. A comparison may be useful

4. The overall list of themes is clear and relevant, although some of the insights offered are fairly well known in the general ‘attitudes to depression’ literature (i.e. the focus placed on external determinants as cause of depression, medication viewed as treating ‘symptoms not causes’). Although they need to be reiterated as they reflect the data set, I feel some could be reported very briefly without major discussion (where they link to known findings), so as to allow greater detail to be provided in relation to some of the more innovative themes that will not be so well known to this audience, and where I feel the paper has the potential to make a more important impact.

5. For example, the issues of resilience and responsibility are very interesting, but were only briefly discussed. Given the basis of some of this work in the common sense model and the link to psychological attributes such as personality, this might benefit from more detail. Do the authors feel that these themes represent common sense models only (and thus need to be understood to better communicate and engage with patients) or do they have some basis in the wider psychological literature as core psychological attributes?

6. What are the implications of the issue of self-identity? The related issue of narrative was brought up, and seemed very relevant in terms of current directions in service delivery and therapeutic communication. This may be especially true in the context of IAPT and minimal therapies, where the focus is away from the ‘story’ and towards understanding of depression and the development of skills for coping. Many of the authors’ conclusions resonate with an understanding of the
nature of general practice that links to the work of Balint and a different perspective on the role of the GP. I have a lot of sympathy for that work, and accept that responding to patients ‘ideas, concerns and expectations’ is potentially critical for engagement and patient satisfaction, it might be noted that it is not entirely clear whether such a focus necessarily maximises clinical effectiveness of treatments, if time and resource is limited, compared to models which focus on activation, cognitive restructuring, and motivation (for example). Clearly there is a middle ground but the two models are not necessarily highly compatible.

7. There was an interesting finding about psychotherapy, and the differing attitudes of those who had experience compared to those who did not. This might be considered in more detail. The general literature tends to suggest very high levels of interest in psychotherapy, but my own limited experience of this issue is in agreement with these findings. It may be that psychotherapy is seen as positive based on inaccurate expectations as to the content, or because it is implicitly compared with medication.

8. The authors mention in the discussion about variation in the strength of the link between depression and chronic disease, but do not analyse this in depth or consider factors related to the strength of the link. What are the implications of this finding?

9. I am not a clinician and cannot really judge the clinical utility of table 2, but it looked to me like a useful addition to the paper. However, I would encourage the author to perhaps replicate this in terms of the more general issues of current service delivery for depression as well, as some of those implications were a little underdeveloped., and I think their core themes relate very strongly to some of the current debates about the nature of mental health treatment in the context of NICE, IAPT and collaborative care models.

10. The revision should give a little more thought to the impact of the choice of diabetes and CHD, both as diseases, and diseases that were incentivised. They consider the issue of the gender of participants, but might consider how their choices might impact on the themes that resulted from the study, and how the results may or may not differ in the context of other, high prevalence conditions associated with depression, such as COPD or arthritis.

11. They might usefully clarify what they mean by depression as an ‘illness type disorder’ or a ‘discrete disorder’. I know what they are getting at here, but it is not a simple issue. I think their current presentation conflates the issue of the biological nature of the cause of depression, the potential benefits from physical treatments (irrespective of the primary cause), the use of the label ‘depression’, and the suggestion that depression can usefully be distinguished from ‘stress’ or other life issues in terms of it being medically distinct. There is literature on GP attitudes to some of these issues, as I am not sure whether the majority would necessarily agree about biological cause, even if attitudes to the relevance of physical treatments are more positive.
• Minor Essential Revisions

1. It might be useful to give details of the exact deprivation indices of the practices involved, for comparative purposes.

2. I realise that this is a qualitative study and that issues of ‘representativeness’ cannot be applied uncritically, but some details of response rates to the systematic and opportunistic recruitment methods might still be useful for context.

3. I did not understand the statement that ‘participants were not recruited for their depression status and so the analysis did not concentrate on differing beliefs in those with a history of depression and those without’. Surely, having patients with and without current depression would facilitate such an analysis. Maybe I am misunderstanding what is meant here?

4. What is the actual number of patients with previous depression, as different numbers seem to be given (16 in the abstract, 17 in the text)? This needs to be clarified.

• Discretionary Revisions

None

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

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