Reviewer's report

Title: Implementing an Outreaching, Preference-led Stepped Care Intervention Programme to Reduce Late Life Depressive Symptoms: Results of a Mixed-methods Study

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Reviewer: Peter Bower

Reviewer's report:

• Major Compulsory Revisions
  None

• Minor Essential Revisions

This is a detailed, well written and interesting description of a mixed methods analysis of the implementation of a new model of care for depression

It deals with a number of core issues in relation to care in this clinical area (stepped care, the importance of choice and so on) and is likely to be of interest to those conducting related research

The mixed methods work well and add significantly to the impact of the paper.

1. The introduction talks about preference led and stepped care, but the preference model (to me) seems to be a version of stepped care (choice within steps) rather than the more usual comparison of stepped and stratified care. This might be noted in the introduction. It is interesting that the current 'stepped versus stratified' debate misses one of the key points raised by the current paper – that certain populations of depressed patients may not perceive a high need for help.

2. The use of the stepped wedge design is interesting, although may benefit from a more detailed introduction as they are not that conventional in this area. I know readers can refer to another paper, but it would be helpful if they didn’t have to. A description in a separate box might be best to provide detail without ruining the narrative flow.

3. From the reading of the description of the implementation of the programme, the reader might forget that the study showed a positive effect! It might be useful if the overall effect could be provided in a more standardised form (as an effect size) so as to encourage comparison with other depression interventions. The low rates of take up ARE noteworthy and a genuine issue for implementation science researchers, but many depression interventions show small effects. Is Lust for Life any different in terms of the overall effects, even in the context of poor take up? Is it necessarily an issue if the overall effect is reasonable? The data on uptake could also be compared with some recent studies in other
contexts

4. The authors clearly undertake a major data collection exercise and the description of it is very long and could be cut down – Figure 2 is very much more useful in this regard. The text was hard to read through and could detract from the much more interesting core issues.

5. The limitations of the use of proxies to collect data on non respondents might be highlighted in the discussion.

6. The themes from the qualitative interviews are relevant and interesting and I had no major concerns with the analyses. I felt the section on ‘illness perceptions’ (page 15) may have conflated beliefs about the existence of a ‘mental health problem’ with beliefs about the cause of the problems, which might be distinguished.

7. The general lack of belief in the need for care is an important finding. I felt the authors could explore in more detail whether this reflects low expectations about the effectiveness of interventions, or and a belief that they are not deserving ‘candidates’ for care, or something different. These different explanations might have very different policy implications.

8. The authors make some statements about the effectiveness of the mental health and home care nurses. Are those comparisons potentially confounded – presumably allocation was not randomised? I think the interpretation is probably correct, but it would be useful to be clear about other potential causes of those differences.

9. Similarly, there needs to be some consideration of threats to the validity of the comparisons of the different interventions (page 23), including non randomised allocation and power. I felt it would be useful to more clearly distinguish findings which benefit from the protection of randomisation, and those that did not.

10. In a similar vein, the strong statement that ‘offering one single preference led intervention is effective by itself and might therefore be just as effective as providing a stepped care intervention programme’ might need more justification. Do they have the data from this study to make a strong statement of that? I wondered if that needed to be presented with more caution.

11. Does the statement on page 28 that ‘many older patients in the target group’ were missed have a strong empirical backing, or is it based on staff perceptions? If the latter, that might be made explicit.

12. Our Dutch colleagues always write beautifully in English, but there were terms (‘quitted’) which need editing. I would prefer the term ‘Proactive case finding’ to Outreach but that is a personal preference.

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

None
Level of interest: An article of importance in its field

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