Reviewer's report

Title: Practice nurse involvement in primary care depression management: an observational cost-effectiveness analysis

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Reviewer: David Ekers

Reviewer's report:

Thank you for asking me to review this interesting report of retrospective audit of primary care nurse intervention for depressed patients. It is a well written paper that will be of interest to those working in the field of primary care depression management. Methods are well described and appear appropriate. Data are reported in detail with clarity. Limitations of the approach are well described by the authors and reference is made to similar reports they have published linked to this research.

I found the report well-constructed and easy to follow and I read it with interest. I do have a number of points I feel it is important for the authors to consider outlined below, divided into categories as defined by BMC family practice.

Minor Discretionary revisions

Minor essential revisions

Background second paragraph (and in discussion) reference is made to the True Blue study as current. This research is now available with 2 publications of relevance to this report. This should be updated.

Ensure fig 1 is correctly placed and labelled it had moved in my copy and hence was somewhat confusing.

Major Compulsory revisions

The paper would benefit from a clear well defined question to close the background section.

Nurse characteristics in results section. Training sessions for depression. A difference is noted in results between low and high level nurses? It is unclear how long a training session is or what training is. Some clarity would help readers understand what this means in practice (it may mean a difference of half an hour over 2 years)

Linked to the above point in the discussion (paragraph 7) the authors suggest nurses without specialist training have limited effect. This is also repeated in paragraph 11 (starting (since 2007), It states that using nurses is not efficient in use of depression an argument followed on in the final paragraph of the discussion. Whilst I feel the paper is well written the authors seem to make very strong statements that go beyond what can be assumed from the findings taking into account the method and the limitations (which are well presented). I feel,
based upon the evidence supporting collaborative care delivered by nurses in numerous trials and meta-analysis, to state nurses involvement in depression care is not efficient is presumptuous. In this study (retrospective and observational) we do not know what training was or how well nurses performed. The findings are used to make the case to focus on specialist MH nurses in primary care for research and practice development, where it might be (as RCTs indicate) specifically trained PC nurses can deliver effective care following brief training.

Linked to this the first paragraph of the conclusion states it is feasible to classify depression based upon routinely collected data. Again we do not know this and the approach requires further validation using diagnostic interview, results must be discussed with this in mind. The study shows it is feasible to collect the data that may be able to classify depression state but before any conclusions can be reliably drawn this process requires validation.

In summary I felt the paper was well written and informative, but the authors make too bold conclusions taking into account the limitations of methods and other research on nurses interventions in this area.

**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