Reviewer's report

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

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Reviewer: Cathy Mihalopoulos

Reviewer's report:

This paper describes the cost-effectiveness of alternative models of practice-nurse involvement in the management of depression. The study uses a technique called risk-adjusted cost-effectiveness (RAC-E) analysis. The main results of the study showed that the low-level model cost less and resulted in more DFD than the high intensity model however this result was highly uncertain.

Overall, the paper is well written and clear in intent and purpose. A strength of the study is the rigorous approach to the statistical analyses to try and ensure as far as possible that potential confounders within a non-experimental design are controlled for as far as is practical. However, there are some important issues which need to be addressed by the authors.

Major Revisions

Firstly, there are major assumptions made within the classification process and I must say I am unconvinced of how predictive this classification system really is. GP notes are highly variable in terms of details recorded and there are many factors which are considered when starting or stopping medications. So while this study is an interesting case study I definitely do not think the methods used to classify outcome should be considered sufficient to make policy decisions without rigorous validity tests. Such tests should include a separate study which assesses people periodically using standardised outcomes measures for depression including symptomatic measures, functioning measures and quality of life measures and compare these to the categorisation procedures developed by the authors. It is very difficult for me to accept the categorisation method used in this study as a sufficient proxy for outcome without such validity testing.

Secondly I found myself somewhat confused in how the classification and transition process in the outcomes really worked. The paper discusses “transitions” which are reminiscent of Markov models in economic modelling studies however, it appears that a Markov model structure was not employed in the current study. The authors need to clarify this and ensure that terms are not confusing for readers.

Thirdly, the authors need to ensure that the elements of a good economic evaluation are included explicitly within the study. For example, the authors should refer to the ten quality criteria of economic evaluation developed by Drummond et al (2005). The paper should ensure that each of the criteria are assessed. For example, the paper currently does not contain a stated
perspective or any information on important costs not included (e.g. productivity impacts or community welfare services not included in Medicare). The reference year for costs is not stated and discounting is not mentioned even though the average time in the study is greater than one year.

Discretionary Revisions

To help show the large amount of uncertainty in the study results I suggest that the authors graphically plot this in a cost-effectiveness plane. I would think that the results will span all four quadrants therefore suggesting that there is no firm conclusions to be made. In my experience the presentation of results in this way is certainly understandable by different stakeholders including economists, policy-makers and clinicians.

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

**Quality of written English:** Acceptable

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

'I declare that I have no competing interests'