Reviewer's report

Title: Parental Depression and Child Conduct Problems: evaluation of parental service use and associated costs after attending the Incredible Years Basic Parenting Programme

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Reviewer: Cathy Mihalopoulos

Reviewer's report:

The paper by Charles et al report the results of a study investigating depression levels and costs in parents of children who were at risk of developing conduct problems. The parents underwent a parenting intervention - the Incredible Years intervention. The current study is a secondary analysis of the primary randomised trial which evaluated the outcomes of the children. The current study looked at the changes of depression levels in intervention parents (compared to controls only up to 6 months and longer term up to 18 months). The resource use and costs of parents were also evaluated and stratified by initial levels of depression (based on a clinical cut-off score on the BDI-II). The paper is well written and clear in intent and is a useful addition to the literature regarding the value of parenting interventions for children and families with conduct disorder.

Major Revisions:

My most important comment relates to the conclusion of the study that the parenting intervention results in improved levels of depression for parents. While there were significant improvements over time in levels of depression in parents who underwent the intervention (though the levels increased again at 18 month follow-up), significant differences were not observed between intervention and control group parents at 6 month follow-up (which is the only time point where comparisons between interventions and controls were possible). This is clearly stated in the 4th paragraph in section 1 of the results). Depression levels in the control group also reduced between baseline and 6 month follow-up though this change was not significant. However it must also be remembered that there were fewer parents in the control group, therefore potentially resulting in wider confidence intervals and therefore less chance of observing a significant difference.

Of course the study was not powered to detect such differences anyway so any conclusions need to be appropriately tempered and interpreted within this context.

I suggest the authors modify their conclusions regarding the impact of the parenting intervention on levels of depression given that differences between the control group and the intervention group were not observed at the 6 month follow-up.

Minor Revisions:
The authors need to further explain why at baseline no statistically significant differences were observed for the control group between people who scored above and below the clinical cut-off, while statistically significant differences were observed for the intervention group at baseline between parents who scored above and below the clinical cut-off. Again this may be a question of power or perhaps the groups were different at baseline.

While the authors state that the intervention costs were not included in the cost estimates I think they should be included (or at least reported) because the costs of the actual intervention need to be factored into the total resource implications.

The authors need to further discuss the important design limitations inherent within this study. The very small sample size of the control group parents once delineation by BDI-II cut-off was made particularly needs to be discussed. For example there are only 9 control group parents who meet the above cut-off criteria at 6 month follow-up. Mention of the limitations of such post hoc sub-group analyses should be made in the discussion.

**Level of interest:** An article of importance in its field

**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'