Reviewer's report

Title: How evidence-based is an 'evidence-based parenting programme'? A PRISMA systematic review and meta-analysis of Triple P.

Version: 2 Date: 1 August 2012

Reviewer: Jane Barlow

Reviewer's report:

The authors have made considerable improvements to this report, and I apologise to them for not spotting their reference in the introduction to the existing reviews. There are just a few further changes that really are still needed, and which I very much hope they will be willing to make:

Important changes:

1. Most of the changes in relation to the use of the term public health and population-based parenting programmes have now been addressed. However, the final paragraph of the introduction still suggests that this review is addressing Triple P in terms of its effectiveness as a population-based intervention and on a targeted basis, but there is no subgroup analysis undertaken to do this, so this sentence should also be removed;

2. Please add the references for the papers that do not report subscales (in the Results - Risk of Bias section; and also indicate how many and which studies did not include families who were self-referred (Discussion);

3. I feel that the section in the discussion on Whole Population approaches makes this section far too long, and is not appropriate given that this paper is really not about population approaches. The issues raised are extremely interesting, and deserve discussion, but not here.

4. Summary of Evidence - the wording here needs toning down because you do not indicate that the other observers are fathers (who we know rate children's behaviour differently) and there were only 6 studies measuring paternal outcomes compared with 23 measuring maternal outcomes, and an effect size of 0.46 is really not a bad improvement...and with more numbers might have been significant?

4. Limitations - 'the narrow scope of the literature search'...do you mean high specificity or focused search possibly...'narrow' makes it sound limited.

5. Conclusion - I feel that this first paragraph needs toning down. This results of this review do not permit you to comment on the effectiveness of Triple P as a population approach, or its cost-effectiveness. You could of course contrast the findings of the independent research (ref 39) with the other non-independent research more strongly, given independence was analysed...but is that the only independent study, and if not, what do other independent studies report?

6. I'm not sure it is helpful to suggest the application of the same criteria for
purchasing as pharmaceutical or medical devices unless you are going to say what these are.

7. I think that the conclusion should be more focused on the specific deficits that have been highlighted and also be more constructive in your working because Triple P are not the only programme that are guilty of not registering trials and having the designer involved in them without declaring a conflict of interest, and we need to be encouraging everyone to address these issues. The failure to report subscales is of course very serious and the extent and nature of that could be discussed more fully here alongside other methodological issues that were identified.

I get the feeling that you want to warn commissioners and others about the problems inherent in the blanket commissioning of programmes such as Triple P (with which I completely agree) but you are currently going beyond your findings in the discussion section, and if you want to discuss costs and population based programmes you need to have results that address these.

Other minor changes:

1. Abstract: Background - I don't think it is accurate to say that interventions to promote parents have rarely been the subject of rigorous appraisal but maybe point to the benefits of rigorous and indeed, ongoing, appraisal of the sort undertaken here.

2. Introduction: Rationale 3rd para ‘There remain...’. I think this should also be rewritten slightly because it doesn't provide a good link with what is proposed.

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

None