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:** 21 August 2012

**Reviewer:** Frances Gardner

**Reviewer's report:**

The authors have generally been very responsive to the reviewers' comments, including adding useful new meta-regression results which address the issue of applicability of the intervention to children with different levels of clinical need. The main point made by both reviewers, namely that there was inadequate definition and use of concepts such as 'public health approach', 'population level effect' etc., has been dealt with by removing these issues as a main focus of the review. Although this is somewhat disappointing, as it was one of the features lending originality to this review, it also makes the paper much clearer. Having re-read it carefully, I think that there is still substantial originality and usefulness for the field in conducting a rigorous review of the Triple P interventions according to accepted guidelines in the field (PRISMA- and thus also at a standard similar to that expected for Cochrane reviews). The discussion of (mostly excluded) public health level interventions is also very useful.

Major Compulsory Revisions. I would like the following addressed, as compulsory but very straightforward revisions to the paper.

1) The abstract states: "Background: Interventions to promote positive parenting are often reported to offer good outcomes for children but they can consume substantial resources and have rarely been the subject of rigorous appraisal".

The authors need to think carefully how to rephrase this last clause: ".... have rarely been the subject of rigorous appraisal". I guess this made sense in the previous version, when the abstract referred to ‘public health approaches’ (etc). As phrased now, it is quite untrue. There are probably more randomised trials of parenting interventions than any other psychosocial intervention; many trials, of many of these interventions, have stood up well in the face of rigorous Cochrane reviewing, NICE reviews etc. So what is the one-liner rationale for this review? Is it that reviews of Triple P trials have not yet been conducted to that standard?

2. Data from independent observers: The authors need to make some further, more nuanced comment in the discussion concerning the use of maternal report as the primary outcome in parenting trials, and the possible solutions. As the authors make clear, there are considerable limitations in relying on maternal report data alone. Moreover, paternal report data is often difficult to assess, as there tends to be substantial missing data, that is not missing at random. Hence, one of the well-accepted solutions in the field has been to use independent direct
observations of parent and child behaviour (eg Hutchings et al., 2007; Gardner et al., 2006). I note that in table 4, seven of the trials included data from independent observers, although most of these found no change. It would be helpful if the authors made brief comment on the observational findings in their results (were any of them encouraging?) and discussion/recommendations, and then more carefully relate this finding to the wider literature, pointing out that other rigorous systematic reviews of parenting interventions (eg the cited Furlong 2012 review), attest to the importance of including data from independent observers in trials and in reviews, in order to reduce risk of bias and provide more convincing data on the effects of parenting interventions. This is an important point for the wider field- and the findings of this review can usefully draw our attention to it.

3. Financial conflicts of interest in trials need checking. On page 11 it says: “All eligible papers were co-authored by a Triple-P affiliated author, apart from three [13,23,24], but in one of these [24] a Triple-P affiliated researcher is acknowledged as a contributor”. However, I believe #13 and 24 are authored by Triple P licence holders (Hahlweg K, & Heinrichs N) who receive payment for Triple P sales in Germany - where Triple P is organized as a private for-profit shareholder company. This should checked and corrected on page 11 & 16; it underlines the authors important points about the need for greater transparency about conflicts.

The para goes on to say “Conflict of interest statements were found in two papers: one [13], where no conflict was reported and another [25] where royalty payments to authors were mentioned” - thus raising the question of whether the conflict of interest statement in #13 was accurate.

4. Recent Cochrane review of parenting interventions: It is good to see this incorporated into the conclusions. However, it should be described more accurately. Rather than: “A recent Cochrane review of parent training interventions for established conduct problems [40] provides robust evidence of the effectiveness of such targeted programmes.”, it should be made clear that the review includes trials of kids at high risk (eg preschoolers showing early signs of conduct problems; Hutchings et al) as well as those with established or diagnosed conduct disorders. As this is very relevant to the argument being made, this should be corrected.

Quality of written English: Acceptable

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

Declaration of competing interests:

I have received funding but not fees for a keynote speech at a Triple P conference, from the conference organisers. I am involved in two small pilot trials
of Triple P interventions.