Author's response to reviews

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

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Author's response to reviews: see over
Dear Dr Alam

Thank you for your further consideration of our manuscript and provision of further helpful reviews. We have tried to address each point made by the reviewers below. Please find attached a revised manuscript with changes tracked.

Reviewer: Jane Barlow

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.

We have deleted the sentence as suggested.

Page 4, para 3.

2. Please add the references for the papers that do not report subscales (in the Results - Risk of Bias section)

There were two such studies – we have cited them as requested.

Page 11, para 2

...and also indicate how many and which studies did not include families who were self-referred (Discussion)

We have provided this information as requested.

Page 15 para 3

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.

We agree with this point to some extent. The reason that we think our review is important to a general audience is because very substantial resources are consumed by population (“public health”) interventions, the case for investment being in large part founded upon the studies in question. As we described, it was not possible to incorporate these papers into the quantitative synthesis but they did meet the criteria for our qualitative review.

That said, we agree that text with this level of detail does not belong in the Discussion section so we have moved much of it, unchanged, into the Results (“risk of bias within studies”) section.

Pages 11 (para 3)-12
Page 13 para 3
Page 18 para 2

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?

We agree that the effect size of 0.46 is not bad, but this only applies to the six studies where paternally-reported ECBI or CBCL results were available and which were included in the meta-analysis. One further paternal ECBI study reported results as “not significant” but did not give figures. More importantly, Table 4 lists a much larger number of studies (the great majority) that reported non-significant outcomes based on teacher reports, blinded observational measures, and paternal reports using measures not incorporated in our meta-analysis. We rather doubt Prof Barlow’s contention that further studies might produce significant results in a meta-analysis: Malti’s recent very large study produced negative teacher and parental report results. We have amended the text to clarify these points.

Page 18 para 3
5. Limitations - 'the narrow scope of the literature search' ...do you mean high specificity or focused search possibly...'narrow' makes it sound limited.

We are grateful for this observation: we meant highly specific and have altered the text accordingly.

Page 18 para 4

6. 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.

We accept this point (though still maintain that there is insufficient evidence of benefit) and have deleted this comment.

Page 19 paras 2&3

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?

We think that Malti’s research may well be the only study published in English (apart from Gallart & Matthey’s small study) which is truly independent - see Prof Gardner’s comment below. We have discussed this and have now cited Eisner’s independent German-language monograph which produced negative results as well as his important review which provides evidence that independent research in the psychosocial arena often fails to replicate positive findings from developer-led research.

Page 19 para 4

7. 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.

We agree and have added some text supporting the desirability of clinical trial registration etc. The following paragraph (from the earlier draft) expands on this theme and we think the line of argument is now clearer.

Page 20 para 1

8. 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.

This is a helpful comment. We think that the changes made in light of comments 6 and 7 have enabled us to comply with this recommendation.

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.

We agree and have inserted text to this effect.

Page 20 Para 2

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.

We believe that we have now addressed these issues adequately (see points 1, 3 and 6 above)

Other minor changes: 1. Abstract: Brackground - 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.
This is a fair point, though it should be noted that the reviewers have produced much of the rigorous work in this field. We have amended the text.

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.

We agree and have made a small change to the text.

Reviewer: Frances Gardner

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?

This is of course a good point and we think we have addressed it adequately. See our response to ‘minor change 1’ above. Our “one liner rationale” would indeed be that previous reviews have not been conducted to a standard which would allow dispassionate assessment of the benefit of Triple P, but we don’t think it is necessary to make that statement in the abstract.

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.

We thank Prof Gardner for this suggestion and some of the suggested changes have been made in response to point 4 made by Prof Barlow. We have made other changes to the Results and Discussion section in line with these suggestions.
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.

We thank Prof Gardner for this comment. We have investigated this matter further and have established that Prof Hahlweg and Dr Heinrichs are affiliated to the Triple P organisation: specifically, Hahlweg has been publically credited with the translation of Triple P into German and with bringing the programme to Germany. Hahlweg and Heinrichs are also named as the research contacts for the European Triple P Dissemination Network. On a general level it has been difficult to establish the financial structure of the Triple P organisation outside Australia, so it has not been possible to confirm the existence/nature of any payments to Triple P licence-holders.

We have therefore found it necessary to reduce to one the number of papers which appear to be independent of the Triple P organisation.

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.

We are grateful for this suggestion and have amended the text accordingly.

We have checked that the revised manuscript conforms to the journal style. The numbering of the references in figure 2 has been updated and we have created a high resolution version of figure 4.

We hope that you now find the paper suitable for publication in BMC Medicine, and look forward to hearing from you in the near future.

With best wishes,

Philip Wilson, on behalf of the authors.