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,

We are very grateful for the extremely high quality reviews you have provided and for your request for resubmission of an amended manuscript. We believe that the changes we have made in light of the reviews have substantially improved the paper. Our responses to the reviewers’ comments are detailed below.

Reviewer: Jane Barlow

1. The paper moves between the use of the term 'public health' in the title, and 'population-based' interventions in the main body of the paper, without distinguishing or defining either. My real concern about this, is that the review proceeds to include all evaluations of Triple P parenting programs, which I find strange because level 4 and 5 of the Triple P programme do not in my opinion constitute a population-based approach on their own. In the discussion, the authors then refer to 'whole population interventions' which again immediately moves to a discuss of the public health benefit of such programmes without definition... My point here is that this is a review of the effectiveness of the Triple P programme not a review of whole population parenting programmes because it only includes studies that have evaluated component parts of the Triple P public health programme.

This is a very fair point, and we admit that we found it difficult to find the right balance in our first draft. We have now changed the title of the paper to reflect the fact that we have attempted to review the programme as a whole and have tried throughout to clarify that the focus is on the whole programme. Nevertheless we have provided a detailed critique of the whole-population studies because it is these which have been used most frequently in arguments supporting the most resource-intensive approaches to the provision of parenting programmes.

2. The background section is very brief and on the whole a rather poor overview of the issue of population-based approaches or for that matter parenting programmes. There have been at least 3 very good systematic reviews/meta-analyses of the Triple P parenting programme none of which are referenced here. I would also really like to know in what way the current review differs from the earlier ones. Why didn't the authors simply update one of the earlier reviews?

We did try to keep the Introduction as brief as possible - and that inevitably led to some simplification of the issues - but we found one aspect of this comment a little difficult to understand. We did refer to the four systematic reviews/meta-analyses in the first paragraph of the Introduction to our previously submitted draft and, we think, managed to describe some of the gaps in those analyses we have attempted to fill. Nevertheless we have added some text to explain how our review differs from previous ones and why we have undertaken the type of review that we have.

In relation to the more general issue of the extent to which we discuss existing literature on population-based parenting programmes in the Introduction, we think that our amendments in light of question 1 above make this unnecessary.

4. The eligibility inclusion are not to mind adequate. If the authors are interested in population-based approaches, where are the inclusion criteria that define what a population-based approach should include? The application of levels 4 or 5 of Triple P are not a population-based approach. Surely such an approach would involve the use of a population-level programme (e.g. Prinz study) or levels 4/5 of Triple P but with a population sample. Why are the authors only including Triple P?

See our response to comment 1. We have added some text at the end of the Rationale section to explain why we focussed on Triple P.

5. Some of the referencing is rather bizarre. For example, in the discussion the Barlow review of parenting programmes for children under 2 is used as a reference to a sentence that reads 'All the studies involved only children aged over two years'?
We have carefully checked the referencing, and apologise for the error pointed out by the reviewer (and two other errors) which have now been corrected.

**Reviewer:** Frances Gardner

1. The review could cover rather better the issue of universal vs. Targeted prevention. These issues could be addressed in a revised paper; this would add considerably to the value, clarity and originality of the review. The Triple P programme is disseminated as a population-wide system of parenting support, aimed at all parents, and operating both at a ‘universal’ level, and as an impressive ‘stepped’ series of more intensive interventions targeted at populations with increasing levels of need, including clinical populations. My impression from the literature, and from previous reviews of related interventions (eg Furlong et al’s 2012 Cochrane review), is that the evidence is strong for targeted parenting programmes, aimed at those showing clinical or subclinical levels of behaviour problems. However, it is much less convincing for ‘universal’ samples, for which several trials of Triple P and other interventions have shown no effect (eg Malti et al, 2011 Swiss trial). Thus I think it would be important for the review make better use of its extensive data to draw conclusions about the effectiveness of the programme for ‘universal’ vs. targeted samples. This is a really critical question to answer for informing policy, and will have a profound effect on the cost of dissemination. The authors would need to categorise the programmes more fully by level of need of children, then comparing effects in samples where all/ or the majority are above the clinical cut off, vs. those that are not. At the moment, this information is made explicit in table 1 only for some of the trials, and there is no subgroup analysis based on this factor. I know that for many of these trials, doing this is not easy, as there is often unclear reporting about inclusion criteria, however, one approach might be to inspect mean baseline scores on child behaviour problems. I note that this is also the first objective of the paper, to ‘identify characteristics of the populations’ in the trials; without this information, this objective is not met.

This is a very good point. We have now categorised the studies by likely level of need (clinical/non-clinical), inserting an extra column into Table 1 based on questionnaire clinical thresholds reported by participants’ families at baseline. We considered a subgroup analysis based on this categorisation but so many studies involved a mixture of children with scores on either side of the clinical cut-off that we thought an analysis using baseline score as a continuous measure was more valid. We therefore opted to use the studies included in our original meta-analysis to meta-regress effect sizes on baseline behaviour scores. We have interpreted these findings in the Discussion. There are some other published data (eg de Graaf’s Triple-P meta-analysis) addressing this issue and we have indicated this in the Discussion section.

We have cited the recent (and important) Malti et al. trial in the Discussion as suggested.

The authors spend some time critically discussing the evidence for whole-population interventions, however, it may be that much of what is delivered in these populations is in fact programmes for targeted use (eg a high percent of Triple P level 4, aimed at high need children, appears to have been delivered in the South Carolina population trial); so although this is a valuable discussion of an important question, it does not address the somewhat different question of universal-level delivery of Triple P. In order to live up to the title of the paper, the authors should ensure that these concepts are used as clearly and precisely as possible throughout.

Again a very good point, but please see our response to comment 1 from Prof Barlow above. We have checked the text throughout and amended it where necessary to increase clarity.

2. The discussion is generally of a high standard, however, I think could be a little more balanced- after all, many might argue that an average effect size on child outcomes of .6
is very good, and that since reporting standards were poor, perhaps for some studies the risk of bias could be described as 'unknown' rather than 'high', especially as the majority were published at a time when standards were not so high. The authors should include some further discussion about whether alternative interpretations of the data could be made.

We have carefully reviewed the tone of the discussion and have softened some of our conclusions, as well as providing an alternative explanation for the limited effects reported by fathers and independent observers. We have adopted the suggestion about making a statement concerning the level of risk of bias and potential problems in its assessment.

3. Eligibility criteria: studies had to have a 'comparison group’, but is that all? Were there no further inclusion criteria based on methodological quality – did the authors intend to include studies that were not only non randomised, but which had comparison groups that were unmatched, or selected to be different? This should be clarified.

We did decide to use very broad inclusion criteria for the data reported in our qualitative synthesis. We only included RCTs in the meta-analysis and we have clarified these issues in the text.

4. What definition of reporting bias was used? Was it only based on comparing abstract and results? This could be made a bit clearer, and any limitations of the approach drawn attention to. Later on, in the risk of bias section, the authors make many useful points about ‘selective reporting’, which seems to be the same as what Cochrane calls ‘outcome reporting bias’. Perhaps they could clarify the usage of these important terms, and make sure they are consistent with PRISMA/ Cochrane handbook terminology.

We have attempted to make the text consistent with Cochrane Handbook terminology, and have clarified our approach to the examination of selective outcome reporting in article abstracts compared with results sections.

5. There are many very important recommendations for the field, in particular the need for better adherence to trial reporting guidelines. However, the final para of the conclusions should be revised to include a strong plea to journal editors and reviewers, and not just to authors. There are many psychology journals (particularly but not always, lower impact ones), which, despite publishing many trials, have not signed up to/ or do not adhere to CONSORT guidelines. It could be emphasised that authors who choose journals that do not adhere to these guidelines, and editors who choose not to adopt them, are doing the field, as well as their own work, a disservice.

We agree wholeheartedly and have inserted text as suggested to indicate our agreement!

6. The abstract conclusions should reflect a bit more accurately the content of the review. They refer only to the credibility of the results with respect to conflicts of interest. The results find high risk of bias / or poor reporting in many spheres, and this should be stated more fully in the abstract conclusions, rather than focusing on the single issue of conflicts of interest.

We thank the reviewer for this suggestion and have amended the abstract accordingly.

7. in the section on whole-population interventions, the authors should make it clear which trials under discussion are included in the review, and which (if any) are not.

Each of the whole population interventions described in this section were included in the qualitative synthesis but none were incorporated into the meta-analysis. We have clarified this issue in the text.

8. in the 1st para of the conclusions, when the authors suggest that targeted programmes may work better, this should be backed up by recent high quality evidence, including Furlong et al 2012 Cochrane review on group based parenting programmes.
We have provided text and citations to this effect, as well as indicating the support our meta-regression provides for this contention.

**Editorial comments:** Abstract- Following on from our previous discussions, as you are aware the abstract needs to be no more than 350 words. You previously expressed your concerns regarding this as you (quite rightly) have adhered to the PRISMA guidelines. It is possible to include all the necessary information, but some sections may need to be removed, and the relevant information can be placed in the relevant abstract sections (broken down into Background, Methods Results, Conclusions).

Thanks for your advice. The abstract has been shortened to 347 words.

Please also note that in the process of rechecking and updating the data for Table 3, we identified and corrected two errors. The more important of these increased the intervention group sample size of the four-group Sanders et al 2000 study from 123 to 184 (we had erroneously added up the n for three groups only). As a result we re-ran the meta-analysis without any substantial change to the summary findings. The result of the Egger test for small study effects did, however, change a little: the estimated bias coefficient dropped from 2.31 to 1.96 (and p value correspondingly rose from 0.036 to 0.067). This does not, in our view, change the interpretation of our findings to any significant extent, but we have changed the wording of the sections on publication bias slightly.

The ordering of the references is sequential, but we were not sure whether the numbers would need to change when tables are embedded within the text if the manuscript. Similarly, figure 2 is now annotated with the citation numbers used in the text. If you choose to accept the paper, we would be happy to liaise with your subeditor to do any reordering that might be necessary.

Finally, we would like to acknowledge the reviewers for their invaluable comments on the manuscript and we hope that is permissible.

We look forward to hearing from you.

Kind regards,

Philip Wilson, on behalf of the authors.