Reviewer's report

Title: Triple P - Positive Parenting Program for parents of preterm born preschoolers: A randomized, clinical trial

Version: 1 Date: 25 January 2013

Reviewer: Frances Gardner

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This trial tests the effects of a brief, primary care version of the Triple P parenting programme for reducing behavioural and emotional problems in a sample of preschoolers born preterm. The families were chosen by screening for behavioural and emotional problems from a larger group (n = c 500) of parents of preterm children, to yield some 100 children who screened in, of which 67 consented to the trial. Suggested revisions, although not minor, should be very feasible to complete.

- Major Compulsory Revisions

1. This is a clear and well-written report on what appears to be a well-conducted trial. However, it should include a CONSORT checklist to show that it has conformed with best reporting standards.

2. It should be made much clearer in the title, abstract and conclusions that this is a very brief intervention, and that the findings do not necessarily generalise to longer versions of Triple P, or to other parenting programs.

3. The rationale for using parenting interventions with this group is made clear, but why a brief one? In the discussion, the authors state that most of the other 5 studies of this brief Triple P primary care either found it didn't work, or were designed so it was hard to draw conclusions. So what is the rationale for using it with this group? This should be made clear in the introduction - for example, was it warranted because of weak methods in the other trials, or because they were conducted by the developer, and independent replication may be worthwhile? Moreover, if the results of prior trials were not robust with this brief program, then why power the trial using a one-sided significance level? Here I'd repeat the wise words of Iain Chalmers, that all trials should be justified with reference to the lesson from a (new or recent existing) systematic review.

4. Furthermore, if this intervention is of unclear effectiveness with non-preterm born children, then there seems little to be gained by speculating about why the intervention was ineffective for preterm born children per se. (top of p15, and then twice on p 16-17).

5. In my opinion it goes MUCH BEYOND THE DATA to state that the experience of being in the trial, for this preterm group, “may have beneficial effects in its own right for parents in both the Triple P intervention and the wait-list control group”.
This is stated in different ways 3 times and should be removed. It is surely illogical to make such a statement on the basis of (modest) improvements in both groups, in the absence of a third comparison group that had no such attention. After all, if we could make such inferences without a comparison group, then we would not need to bother with randomised trials! I appreciate that it is not very practical to make such a comparison. Luckily we don’t need to, as there are 25 year of trials and systematic reviews showing superiority of many parenting interventions over the mere filling in of questionnaires and talking to someone.

6. Clinical implications: arguably it is implausible, given what we know about parenting and the development of children’s behaviour problems, both in the general population and in those born preterm, and given the programme theories underlying these interventions, that they would be effective for kids identified as having behavioural problems, but not for those who are so identified, but who are also preterm. After all, the children included in trials of parenting interventions for conduct problems are a very heterogeneous group anyway, many of whom will have neonatal and other biological and social risk factors. Although I do not know of any studies that directly test the question of whether neonatal risk is an effect modifier, analyses of other subgroups that may include a higher percentage of children with neonatal risk factors (eg boys, high ADHD scores; very low-income families; Jones et al 2008; Gardner et al, 2009; 2010), do not suggest any differential (diminished) effects. Most good parenting programmes are flexible and at least somewhat tailored with respect to parent and child needs (Gardner et al, 2009; 2010), and so for many reasons we can be optimistic that evidence based parenting interventions will work with preterm born children. The paper should draw on this wider literature on risk factors, effect modifiers and applicability of parenting programmes, in order to make a brief but more nuanced discussion of the implications of the trial for this moderately high risk population. (In terms of conduct problems, the long term outcomes may not be so bad for these very premature & very low birthweight kids, Gardner et al 2004; Hack et al 2002).

7. Given point #3, is the trial underpowered?

References:


Psychology, 77, 543-553. IF 4.2
http://www.pitt.edu/ppcl/PUBLICATIONS.html#EarlyStepsMultisite


**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.