Author's response to reviews

Title:A Gloomy Picture: Disappointing Effectiveness of Programs Aiming at Preventing Child Maltreatment. A Meta-Analysis of Randomized Controlled Trials

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Dr. Celine Zapanta

BioMed Central

Dear Dr. Zapanta:

We would like to thank you and the reviewers for the constructive comments and suggestions regarding our paper “A Gloomy Picture: Disappointing Effectiveness of Programs Aiming at Preventing Child Maltreatment. A Meta-Analysis of Randomized Controlled Trials”. We revised the previous draft of our paper accordingly, and responded to each of the editor’s and reviewers’ comments in the letter below.

We hope our revision adequately addresses the reviewers’ comments and we look forward to the results of the ongoing editorial process.

Yours sincerely,

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Editorial Request:

PRISMA guidelines: In accordance with BioMed Central editorial policies (http://www.biomedcentral.com/about/editorialpolicies#StandardsofReporting), could you please ensure your manuscript reporting adheres to PRISMA guidelines (http://www.prisma-statement.org/) for reporting systematic reviews. This is so your methodology can be fully evaluated and utilised. Can you please include a completed PRISMA checklist as an additional file when submitting your revised manuscript. We would also ask that you include a completed copy of the PRISMA flowchart for your study as a figure in your manuscript.

R: We have reviewed the PRISMA guidelines, attached the PRISMA checklist as an additional file and changed our previous flow chart using the PRISMA flowchart template (see Figure 1).

Reviewer 1:

1. See report attached. I am not reporting any revisions.
2. Readability is fine, and the topic is highly relevant for researchers and practitioners, judges, and social workers, or any setting delivering parenting intervention programs.

Attached report:

1. The manuscript is an important topic with relevant findings for the court and researchers of parenting programs and child maltreatment.
2. The findings are not really “new news” as it is well known that parenting programs do not address neglect and other forms of maltreatment sufficiently, and there is always difficulty finding a good enough match to send parents to effective programs.
3. Having concerns about the effectiveness as well as identifying those program and parent characteristics that are promising is still encouraging.
4. RCTs are difficult to get funding for and the replication of the parenting programs is not always possible in various settings. I do agree with the conclusions and future directions discussed in the paper but many states and settings will not participate. Dependency courts would welcome having greater knowledge of effective parenting programs at all intervention levels.
5. On the surface the data looks relevant and the authors have used sound analysis- I did not check the statistics.
6. There is excellent description of the search for the studies, and the figures are helpful.
7. The title – is okay, the first part “A Gloomy Picture” seems too strong from my perspective and it may not work in the title to pick up the manuscript in a search- in other words, it may be missed.
8. The authors do a great job discussing the findings, the limitations and the challenges in implementing RCTs in child maltreatment field.
9. Writing is good. There is a balance in the manuscript of strengths and limitations and concerns of child maltreatment and parenting program research.

Reviewer 2:
This largely well-written paper explores whether outcome evaluation studies of parent training and parent support programmes show evidence of effectiveness in preventing child maltreatment from happening in the first place, and reducing its re-occurrence in families where it is already occurring.

The following concerns should be addressed before the paper can be published.

1. The definitions of prevention and reduction (at the end of paragraph one in the “intervention programs” section) should be made earlier, ideally in the abstract.

R: We have extended the description of prevention and reduction in the abstract of the revised manuscript (p. 2, line 28-30):

In the present meta-analysis we synthesized findings from 27 independent samples from randomized controlled trials (RCTs) on the effectiveness of 20 different intervention programs aimed at (i) preventing the occurrence of child maltreatment in the general population or with at-risk but non-maltreating families, or (ii) reducing the incidence of child maltreatment in maltreating families.

2. The distinction between “training” and “support” requires clarification.

R: This distinction is now explained in the Introduction, in the paragraph Intervention characteristics (p. 6, line 139-157):

An important characteristic of the intervention is the focus of the program. In some programs, parents receive various sorts of support (e.g., social, emotional, material) in order to build on strengths and improve overall family functioning, without actual parenting skills training. For example, in Healthy Families America parents receive support to reduce social isolation, access resources such as food, housing, employment, and health care, and improve their knowledge about child development [27]. Other programs do provide actual training for parents to improve their parenting skills, such as SOS! Help for parents [14], in which parents are instructed about (the role of) parenting skills and common mistakes in parenting, or Parent Child Interaction Therapy [20], in which parents receive (among other things) live parent-child coaching sessions to improve parent-child interaction skills. Finally, some intervention programs combine parent training and support. For example, in the Project Support intervention [28], mothers are taught skills for child behavioral management by instruction, practice, and feedback, and they are provided with instrumental and emotional support, such as training in how to evaluate a child care provider.

3. The third paragraph of the “methods” section notes that studies were excluded if they used clustered randomization. The earlier “design characteristics” section should state that cluster randomized trials were excluded, and explain why. Ideally, the paper would have reviewed both randomized control trials and cluster randomized trials and compared their findings. Cluster randomized trials are increasingly used to evaluate the outcomes of programmes to prevent violence (especially in low- and middle-income countries), and it would therefore be of value if this paper could have examined how such a design might affect the findings. At the very least, the paper should indicate how many studies were excluded because they were cluster randomized. It is also noted in the “literature search” section that, although it was a cluster randomized trial (with counties as the unit of randomization), the Prinz et al study was excluded not because of that, but because it was “impossible to calculate an effect size”. This suggests inconsistency in how cluster randomized trials were treated.
R: We have noted the exclusion of cluster randomized trials in the Introduction section, on page 6, line 127-129: Clustered randomized trials were excluded, because participants are not fully randomly assigned and therefore participants (or their contexts) in one cluster may not be comparable to participants in other clusters.

In the complete set of 374 full text papers that were assessed for eligibility, three studies were excluded because they used cluster randomized designs. Because of the small number of cluster randomized trials, comparing the findings of full randomization and clustered randomization in a moderator analyses would not have been possible. The number of cluster randomized trials has been added to the Method section (p.10, line 239-243): Another reason for exclusion was the use of non-fully random assignment to intervention and control conditions, such as including new participants to the intervention or control condition after randomization was completed. In addition, three studies evaluating the SafeCare [40] or SEEK program [41, 42] were excluded because clustered randomization was used.

Finally, most excluded full text papers were excluded based on more than one exclusion criterion. In Figure 1 however, excluded papers are categorized in only one of the exclusion criteria, to keep the figure readable. In the “Literature search” section, the Prinz et al study [36] is mentioned as an example of a study in which the effect sizes cannot be calculated due to unknown sample size. To be more consistent in why this trial was excluded, we added the following sentence (p. 9, line 219-220): Another reason for excluding this Triple P trial was the use of clustered randomization.

4. The reporting of the results in the abstract and other relevant sections is somewhat confusing, since while the abstract states that significant effects on maltreatment were not found, the section on “moderator analyses” states that “programs with a focus on parenting training, either with … or without support … were significantly more effective than programs that solely provide support”.

R: Indeed, we did not find an overall significant effect size on child maltreatment, when publication bias was taken into account. Moderator analyses are performed to examine if the effect size for a specific subset of studies is significantly larger compared to that for another subset. In the revised manuscript, we added the word “however” to the results section of the Abstract (p. 2, line 30-36), to clarify that although the overall effect size was not significant, we did find significant differences in effect sizes for specific subgroups in the moderator analyses: A significant combined effect on maltreatment (d = 0.13; N = 4883) disappeared after the trim-and-fill approach that takes into account publication bias against smaller studies without significant outcomes. However, moderator analyses showed that larger effect sizes were found for more recent studies, studies with smaller samples, programs that provide parent training instead of only support, programs that target maltreating instead of at-risk families, and programs with a moderate length (6-12 months) or a moderate number of sessions (16-30).

Also, in the Results section (p. 14, line 352-353), we have added a sentence about the difference between the combined effect sizes and the moderator analyses: Although no significant combined effect was found, moderator analyses indicated significant differences in effects among subsets of studies.

5. The finding that programmes to reduce maltreatment show more promise than programmes to prevent maltreatment contradicts what most other meta-analyses and reviews conclude, which is that the evidence for prevention is stronger than the evidence for treatment. For instance, a 2013 systematic review by the United States
Agency for Healthcare Research and Quality of interventions to reduce (psychological) trauma and further abuse in children following maltreatment concluded that: “Given the nascent state of the field, it is too early to make strong recommendations based on the available comparative effectiveness research” (see http://www.ncbi.nlm.nih.gov/books/NBK137808/). These differing conclusions should be discussed.

R: We thank the reviewer for pointing out this systematic review. However, in this review, only effectiveness studies for programs to reduce maltreatment are included. In the current meta-analysis, for the first time, the comparison between programs to reduce maltreatment and programs to prevent maltreatment has been made. We commented on this issue in the Discussion on page 17 (line 427-432): Secondly, we found significant intervention effects in maltreating samples, but not in at-risk samples, indicating that programs are only effective in reducing (but not preventing) child maltreatment. Although previous reviews have suggested limited effectiveness of programs in reducing child maltreatment (e.g., [58, 59]), the current study is, to our knowledge, the first to meta-analytically compare programs to prevent and reduce child maltreatment.

6. The “future directions” section makes some useful suggestions. However, in light of there still being almost no outcome evaluation studies of parenting programmes from low- and middle-income countries (with the exception of several cluster randomized trials), this section would be stronger if recast to address the specific challenges facing such studies in low- and middle-income countries.

R: Besides the need for more RCTs in general, we now added the urge for RCTs in low- and middle-income countries in the Discussion (p. 20; line 506-515): This meta-analysis implies several possibilities for improvement of program effectiveness studies. First, we clearly need more RCTs that examine the effect of intervention programs on the prevention or reduction of child maltreatment, also outside the USA and in low- and middle-income countries. Nearly half of all full text papers that were screened for eligibility had to be excluded because no RCT was done. Indeed, it has been argued that it is too difficult for practical and ethical reasons to conduct RCTs with maltreating or at-risk families [60], and this may be even more so for low- and middle-income countries. However, the fact that we did find six RCTs in maltreating families and 20 RCTs in at-risk families, including one study in Iran, indicates that it is possible to carry out rigorous effectiveness studies in various populations. Only RCTs can strengthen the evidence base for maltreatment interventions needed so badly in practice.

7. The data analysis section refers to an outlying sample size as having been “winsorized”. This jargon should be replaced by a plain English explanation of what was done.

R: We have now added an explanation of winsorizing on page 12 (line 294-295): One outlying sample size [46] was winsorized, by replacing it with a marginally lower score, while remaining the largest sample size in the set of studies.