Reviewer's report

Title: Do Nonphysical Punishments Also Increase Antisocial Behavior? A Replication of the Strongest Causal Evidence Against Customary Spanking

Version: 3 Date: 1 October 2009

Reviewer: Deborah M Capaldi

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This revised study is much improved from the original version, and the tone regarding physical discipline now is much more balanced. There are some remaining issues, detailed below.

The use of the term “replication” is misleading. As the study is a reanalysis and extension of the Straus study, and not a replication (i.e. a study of a different sample or different wave of the same sample etc.) it should not be described and discussed as a replication. The title needs revision, such as “Maternal discipline and increases in antisocial behavior: A reanalysis and extension of Straus et al.”

It is easy for the reader to fall into interpreting the 4 discipline tactics as independent alternative approaches – and for example, on the first page of the Introduction the authors state that “the present study is one of the first to…..compare the child outcomes of alternative disciplinary tactics with those of customary spanking”, and in the Discussion section refer to “between tactic differences” – which rather implies the different tactics are independent. However, mothers may have endorsed any or all of the tactics as occurring in the past week. Although the majority of the sample of 785 endorsed “0” times in the past week for 3 of the tactics (and it was about 50% for the 4th), the total of 1+ time answers across all 4 tactics is a little over 1200, indicating that a substantial number of parents at least endorsed multiple tactics. This needs to be made more clear in the manuscript, and the number of mothers endorsing 2 or more tactics, and spanking plus another tactic would be helpful in interpreting the data. Ideally, the examination of the prediction effects of each discipline tactic should be in a regression analysis that controls for the other discipline approaches, or controls at least for spanking, so that the effect of a non-violent discipline in the absence of violent discipline would be clear.

The description of participants and sample sizes (p. 11-12) is much clearer, but could the authors clarify whether dropping poor Whites from the sample meant only dropping the oversample of poor Whites, or the entire subgroup of poor Whites, which could affect representativeness of the findings, especially given that lower SES parents are more likely to use physical discipline.

The fact that only maternal report was used is appropriately discussed as a limitation. However, that is not quite the same as the fact that only maternal discipline tactics were measured, rather than total parental discipline (e.g.,
mother’s could have been asked about father’s discipline in the past week in addition to their own), which should be mentioned as a limitation.

On p. 15, the direction of the association of SES and emotional support with subsequent antisocial behavior should be made clear.

On p. 20 half way down it seems the sentence beginning “When initial differences…” should say “none of the corrective actions” rather than “no corrective action” which appears to mean cases of no discipline.

P. 21 “this type of study” at the end of the middle paragraph might be more clearly stated as “the current and similar studies”.

At the bottom of p. 20 the last phrase should read “which is the only..:

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

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

**Declaration of competing interests:**

I declare that I have no competing interests.