Reviewer's report

Title: Child neglect in one-child family from Suzhou City of Mainland China

Version: 1 Date: 18 June 2013

Reviewer: Michael P Dunne

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This paper addresses an important topic in the global epidemiology of child maltreatment. However, there are many parts of the manuscript that need to be improved.

The major compulsory revisions include attention to comments numbered 2,3,5 and 7 below.

1. Is the question posed by the authors well defined?
   No. The purpose just seems to be to estimate prevalence and examine a number of family and child factors that correlate with neglect. There are no specific questions or hypotheses that derive from prior research or theory in this field.

2. Are the methods appropriate and well described?
   The questionnaire is well described initially and the sampling method for reaching children is clear enough. However, there needs to be more detail about precisely how and where the parents completed the questionnaires. The response rate, even for China, is remarkably high. How could this have been achieved if parents took the questionnaires home? There must have been assiduous follow-up by the schools or researchers to ensure questionnaires were returned.

   One important matter must be clarified regarding the measures. See the 2nd page of the ‘measures’ section. The source(s) of the information on children and their parents is unclear. What information comes from the parents’ report and what information comes from other sources? Some of those variables are highly complex and difficult to measure. How was “impaired brain function” determined? How exactly was child/mother recent usage of hospitals measured? It is not sufficient to refer obliquely to “clinical records developed by local bureau of social security” (see page 8). The authors must provide specific information about how those data were gathered and how they could be linked to the questionnaires.

3. Are the data sound?
   There are three significant limitations. First, the basic trends in the data are not visible to the reader. There is some text in the first paragraph of the Results section, but that information should be tabulated and more detail added to illustrate exactly how the scores were distributed by item or neglect subtype.
   Second, the authors do not clarify precisely how their main dependent variables (5 types of neglect) were classified statistically. This vital information appears to be missing. Was a parent classified as neglectful if they endorsed just one item within a domain? A third, and very difficult analytical issue in this paper is that the
authors simply compare percentages across the five domains, as if those percentages reflect the relative importance of the domain. However, this is not appropriate because there were different numbers of items in each domain (item numbers ranged from 2 to 16!). Endorsement of items in domains that are so different in size creates the obvious problem of under-estimation in the very small domain due to under-sampling of the attributes of that type of neglect. Therefore, simple comparisons are likely to misrepresent the relative severity.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?

I cannot comment on this question about data deposition in this journal.

5. Are the discussion and conclusions well balanced and adequately supported by the data?

No. The discussion does not delve any more deeply than re-stating what was reported in the data analysis. Too much attention is given to simple comparisons of the percentages in each domain. There is limited success in integrating the findings from this study with previous literature, especially Chinese literature. It over-states the influence of this paper to say (as they did several times) that this is the “first time” the issue of neglect has been looked at in one child families. The authors have not adequately considered prior research published in China from 2005-2012 (see below).

6. Are limitations of the work clearly stated?

No. There are several important factors that should be addressed. One particular issue is whether parents’ self-report of their own neglectful behavior is likely to be affected by social desirability response bias.

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?

This is very weak in the manuscript. The authors suggest in the introduction (para 2) that “there is a scarcity of data on child neglect in China. That is not true. Indeed, the authors cite several times later in the paper two previous studies (Pan et al 2012; Lui et al, 2012) but they did not discuss what the authors have found. Also, the authors have not been comprehensive in searching Chinese language literature. They missed papers such as:


Pan, Yang Ren et al ((2005) Study on the current situation and influential factors of child neglect.. Chinese J of Epidemiology, 26(4), 258-264

The authors should consider relevant findings from these and similar papers in both the introduction and discussion.

8. Do the title and abstract accurately convey what has been found?
Yes.

9. Is the writing acceptable?
In general it is acceptable, although the authors should seek editorial assistance form a native English speaker because at times the grammar is cumbersome.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Not suitable for publication unless extensively edited

**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'