Reviewer's report

**Title:** Employment and Financial Burden of Families with Preschool Children Diagnosed with Autism Spectrum Disorders in Urban China

**Version:** 2  **Date:** 25 August 2014

**Reviewer:** Roderick A Rose

**Reviewer's report:**


The results of this study are profound and the article stands to contribute to our understanding of the problems that families in an international setting have in raising children with ASD. Nevertheless, there are issues that must be resolved before I can recommend this paper for publication. All of these issues are straightforward.

**Lit review & questions**

1. **Major:** In the literature review, the authors should discuss (1) what prior studies (including not in China) suggest about the greater employment and financial burdens of families with children having ASD; (2) what is known about all of the variables involved in the analysis, including the outcomes, independent variables and covariates.

2. **Major:** A research question or hypothesis is not clearly stated. A research question or set of questions would help to focus the reader on why the analysis was carried out.

3. **Major:** I am unclear why the authors refer to the study as case-control (Background, 3rd paragraph). There is no further description that alludes to case-control in the paper. It is clear that the sample is not random, but “case-control” is a specific type of design for conducting non-randomized (quasi-)experiments. Can the authors elaborate?

4. **Major:** The authors should say somewhere prior to the methods that this is a descriptive study.

**Methods**

5. **Discretionary:** I question the soundness of excluding children with co-morbid factors such as congenital heart disease, particularly if these co-morbid factors tend to co-occur with ASD. I understand the authors’ reluctance to include these children as these factors might inflate the effects of having ASD, but I would at least subsequently re-examine the data without excluding these families (a sensitivity analysis).

6. **Discretionary:** Related, this co-morbidity might be another factor worth studying. 7. Related to my previous point, if these co-morbidities are more likely
to occur in ASD populations, then they should not be corrected for when comparing ASD, OD and TD populations (if this is intended as a descriptive study).

8. Minor: Because the study is not a randomized design the “control” groups should be referred to as “comparison” groups.

9. Major: Regarding the effect of extreme values of income on the analysis (Measures, second paragraph), why not just trim outliers? Converting an interval measure of income to an ordinal represents a significant loss of information.

10. Major: Regarding the second paragraph of “Statistical Analysis”, I simply do not follow the purpose of all of these modifications. Partly, I cannot understand what the authors are doing. But partly, I question the motivation for it. Why go through all of this trouble to match cases when the study is descriptive?

11. Major: If the income measure is converted to ordinal, why are the authors using a linear regression (results, paragraph 6)? It should be a multichotomous logistic regression. The results description also does not match the methods description (Statistical analysis paragraph 4), nor does it match the title on the table. However, I suggest restoring income to its original form and then using methods that address the non-normal distribution of income.

12. Minor: What is the purpose of the stepwise regression (described in results, paragraph 6)? What does it add to the study?

Data

13. Major: It is not clear why the comparison groups were not recruited from the same provinces as the ASD group of children.

14. Major: I question the value of using a hypothetical income measure (Measures, second paragraph): “How much was the expected total income that all family members would have earned in the past year if your child did not have the disease?”. This is a kind of counterfactual. Why not just use the comparison groups as a counterfactual as we normally do? It is possible I have misunderstood something, so if I have, please clarify.

15. Major: In Results, 4th paragraph, I question the exclusion of ASD persons from two provinces because of higher income. This adjustment, like the adjustment I described in my previous comment, are not well-motivated, particularly for a descriptive study.

16. Major: A better description of the covariates is needed, i.e., it is not clear how history of pregnancy or housing conditions are measured. (Discretionary): It would help if these variables, and their association with having ASD, were included in the literature review.

Reporting?

The results are profound, showing that families with children with ASD have much greater income hardships in particular. Although I have recommended many changes above, many of these findings are so strong that they are likely to hold even after the changes are made.
Discussion balanced and supported?

17. The findings are more or less as expected, despite the problems with the design and methods. The discussion points follow from the results.

18. Minor: Discussion paragraph 1: Change “loss of income” in the last sentence to “lower income”.

19. Minor: Discussion paragraph 2: regarding the basis for comparing with west countries, it should be made clearer in the first statement that this is based on comparing the results of this study with other studies, as no such comparison is actually conducted in the study.

20. Discretionary: Discussion paragraph 3: regarding the negative correlation between paternal education and employment burden, can this not be tested by including income as a predictor in the employment burden model?

21. Major: For limitations, I would add a caution against interpreting the findings as causal and indicating explicitly that the study is a descriptive study; I would also make it clearer and more explicit that the sample is not representative.

22. Errors: there are numerous typographical and grammatical errors throughout the submission. Given the likelihood that English is a second language in this context, I recommend that the authors have the paper reviewed for grammar and spelling prior to being re-submitted.

Level of interest: An article of importance in its field

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:

No competing interests.