Reviewer's report

Title: Poverty and health trajectories in early childhood: Exploring the influence of timing and duration of poverty on child health outcomes

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Reviewer: Amanda Sacker

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Poverty and health trajectories in early childhood: Exploring the influence of timing and duration of poverty on child health outcomes
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This manuscript deals with an important topic—the relationship between poverty and child health. I found the manuscript interesting and well-written. The complexity of the material and methods made it difficult to summarize the findings and present a clear “take-home” message for policy makers. I’m not sure it made a strong case for the eradication of child poverty even if we all support that goal.

Major compulsory revisions

1. The authors have carried out a multilevel model of the relationship between poverty and health in childhood but it is not a latent growth curve analysis as such, simply a repeated measures random effects model. The use of dummy variables provides estimates of changes in levels of the outcome at different measurement occasions but it does not model the growth rate. Because of this, the title of the article is somewhat misleading as it does not deal with poverty trajectories or health trajectories but with longitudinal patterns of health.

2. Since all the children in the QLSCD are not the same age when surveyed, the analyses really need to control for age at measurement occasion. At the moment any posited time effects are potentially confounded age-period-cohort effects. There is no information provided on whether parents were interviewed exactly 12 months apart or if the interview schedule was more variable. The authors will need to consider whether controlling for age at baseline is adequate or if time-varying age should be used. In fact, a growth curve model by age with dummy variables for survey wave as controls seems an appropriate way to model these data.

3. The cumulative health problems variable includes growth delays but this variable is not used as an independent outcome. Why not? There seems a certain inconsistency in approach. It is also relevant for the reader to know whether growth was adjusted for maternal height and whether paternal height was also available and could be similarly used before identifying growth delay. At the very least, the shortcomings of this measure should be acknowledged in the limitations section.
4. Did the authors check that the Poisson model was appropriate for these data? Was there any evidence that the data were characterized by over-dispersion or an excess of zeros? It would seem from table 1 that this might be the case.

5. I am not an economist so my understanding of instrumentation may be limited but I looked up the papers used to justify household income rank as a valid instrument. In both the cited articles, income was ranked relative to geographic location (village/city) whereas in the current manuscript it appears that the ranking was carried out relative to the appropriate LICO for the family. While ranking within residential location might be a valid instrument, the argument for ranking by LICO seems of a different kind. I think some discussion is needed to show how this ranking is only related to child health because of its relationship with reported income. My naive understanding is that reported income’s effect on child health depends on the sufficiency of that income given family characteristics such as household size and the cost of living where they live. In other words, LICO-standardized income seems to fail one of the basic criteria for a valid instrumental variable. I may have misunderstood how the instrumentation was carried out but even so it points to a need for greater clarity. I would also like to know the sample size for this analysis – how many children participated in all 4 survey rounds?

6. There is no information on the adequacy of the latent class analysis. How reliably were individuals assigned to a latent poverty class? Given that a 2-stage analysis was carried out (why not in a single model in Mplus?) then bias in the subsequent regression analysis is introduced by the assumption that the children are assigned to a latent poverty class without error. Are conclusions the same if a simpler method of characterizing cumulative poverty were adopted where assignment of children to the non-poor, transient poor and chronic poor categories was carried out based on a count of the number of observations with fitted annual income below the LICO?

7. The presentation of the results for perceived health is complicated by using 4 waves of data in tables 3 and 5 and 3 waves in table 4. It would be easier to compare both within and across tables if the intercepts were the same in all three tables and for all outcome measures. That is, at 17 months. It is also unclear from table 4 whether the coefficients for the survey round dummy variables are for models that include lagged poverty only or lagged plus current poverty.

8. I think the authors have not done true justice to their data. They could also usefully comment on the effect of current poverty status on child health after controlling for lagged poverty status. This would go some way towards addressing the question of social mobility raised in the introduction. Similarly, a measure of poverty at 5 months could be used in models to test for a sensitive period in very early life to complement the findings using a lagged poverty variable. There are some interesting findings in table 5 that weren’t discussed. For example, those children to go on to live in chronic poverty had significantly poorer health than the transient poor at baseline. I wonder if this finding relates to the definition of poverty: do the chronically poor have lower mean income at baseline than the transition poor group?
9. The health perceived less than very good data in table C2 in Appendix 3 and table 5 are not consistent with each other. The results for transient poverty in C2 are presented as the results for chronic poverty in table 5. I have not checked the results for the other outcomes. Please could the authors correct the tables? I was going to comment further on the findings in table 5 but have now refrained from doing so as I am unsure what interpretation to make.

Minor essential revisions
10. Page 3: “dynamics of the poverty and health” should be “dynamics of poverty and health”
12. Page 19. Some mention should be made that appendix 3 is an online supplement unlike the earlier appendices.
13. Tables 3-5. Event rate ratios are presented not event rates. Please correct the headings.

Discretionary revisions
14. Page 14. It is more usual to provide raw numbers and weighted percentages than weighted numbers and percentages.
15. Page 22. Research on inequalities in child health suggests that we would expect young children’s health to improve as they approach adolescence.
16. Page 23. If lagged poverty has an effect on health over and above current poverty, this doesn’t of necessity imply that poverty has a delayed effect – it might just be further evidence of a cumulative effect.
17. A figure summarizing the changes in health over time by cumulative health status might be helpful.

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

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