Reviewer's report

**Title:** Family Income Trajectory during Childhood is Associated with Adiposity in Adolescence: A Latent Class Growth Analysis

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**Reviewer:** Per E Gustafsson

**Reviewer's report:**

**GENERAL**

This is a very interesting manuscript about income trajectories across childhood and adiposity in mid-adolescence, based on data from a prospective cohort of children. The research question is interesting and it makes a clear contribution to the literature. The methodology is generally good, e.g. income data from as much as 14 time points during childhood and appropriate and innovative analytical approaches, although the relatively large and systematic drop-out might constrain the inferences somewhat (although this is rather a common feature of such extended cohorts). The results are also interesting, and the language is great throughout the ms, and the reasoning generally lucid and fluid. So, overall, I am very positive to this manuscript, and think it will make an important contribution to the literature.

There are however a number of major as well as a few minor points that need to be addressed, although I consider all points to be remediable should the authors be prepared to make some major revisions. The major general points (specified below) are:

- Much more details are needed about the design, measures, analysis and results in the methods and results sections. This relative lack of information makes it difficult to really evaluate some key points of the manuscript.
- I am not sure I agree with the authors' interpretation of the results. Although we agree on the inferences from the results, the conceptual interpretation needs to be revised, or explained more clearly.

**INTRO**

The introduction clearly outlines relevant literature and makes a case for the present study. Very good, and I enjoyed reading it.

A very minor point:

1) I would suggest switching ref 11 (Pollit et al, 2005) to Senese (Senese, L. C., Almeida, N. D., Fath, A. K., Smith, B. T., & Loucks, E. B. (2009). Associations between childhood socioeconomic position and adulthood obesity. Epidemiologic Reviews, 31, 21-51). I think that the latter is more specific for the question at hand. The authors might also find the little bit dated but comprehensive review by

interesting.

Discretionary change:

2) The timing, accumulation and change models correspond to the critical/sensitive period, cumulative risk and social mobility models used within life course epidemiology, see e.g. Politt 2005, Hallqvist 2004. I do not know if you feel that this means straying too far from your target due to the different time span considered, but I think it would be a nice touch if this conceptual correspondence was mentioned. It also ties well into your notion that the change model is the one least examined, as this is also the case within LCE (see e.g Politt 2005).

METHODS

In general, I think that the methods section would be improved by providing more information about the study design and measures.

Major:

3) I would appreciate more information about the SECCYD to understand the data pertinent for the present paper. For example, the age of participants, attrition, briefly about the selection procedures at each phase, as well as the nature of the data collected (e.g. questionnaires?). Moreover, it is a bit unclear whether the 2010/11 study analyses (=data collection phase?) was included in the present paper. Although I understand that this information is available elsewhere, it's practical to be able to get a overview of the design in without having to look up other sources, and there is no max word count constraining this paper.

Minor:

4) Why was BMI percentile and not raw BMI used?

Major:

5) It is not clear to me how family income was measured – self-reported or register? I understand that it was measured at 13 time points, but I do not understand how these time points relate to the four phases (=data collection waves?) of the study.

Major:

6) How many was excluded due to missing data? What was the analytical n? This should also be stated in the tables.

Minor:

7) Why use dichotomous rather than continuous income-to-needs as the basis for the LGCA? Even though the cut-off for the dichotomization is the poverty level, which has some practical and conceptual meaning and therefore could be
a suitable dichotomization, the double poverty level is used as the cut-off. I understand that using the original poverty level would yield too low frequencies, but this also has the consequence that the cut-off becomes somewhat arbitrar. I therefore wonder why one would use a cut-off at all; are there conceptual reasons, or perhaps technical? Although it is perhaps not reasonable to expect a dose-response association between income and various outcomes, I think it is safe to say that there is a gradient well above the poverty threshold. I would be glad if the authors could elaborate on this issue.

Minor:

8) Although I am not averse against choosing the 5- rather than the 6-class model, I do not really understand the argument that the unstable # adequate income trajectory did not seem to be a useful or clearly delineated category. Except for the unstable# low class, it seems to be the only class that involves a worsening income trajectory over time. To me, this would appear to be a interesting and unique trajectory to consider in the analyses. However, parsimony – and maintaining large cell sizes, if power would become a problem - are of course desired, and the 5-class solution also differed more clearly from the 1-class solution (as gauged from the p values). So, even though the 5-class solution might still be the soundest choice, it is a bit difficulat to evaluate as I do not completely understand the specific rationales appealing to usefulness and delineation.

RESULTS

As for the methods section, I think that the Results would be much better with more information presented about the analyses.

Major:

9) Much more information is needed for the main ANCOVA analyses. First, what were the specific models tested – which effects were included? Interactions are mentioned but I do not know if these were included in the main models or in complementary analyses; i.e. are the main analyses only main effects or full factorial, or were only specific interactions tested, and were they performed as separate analyses? As it was not part of the aim, I was a bit surprised about the interactions, and I think that a main effects model would be more directly associated with the aim, i.e. not going the full factorial approach if it is not really addressing the aim. What, if any, correction for multiple comparisons were used in the post hoc tests (e.g. LSD/none, Bonferroni, Tukey), and were all or only certain pairwise comparisons made?

Major:

10) Second, I would prefer to have the main results reported in ANOVA tables, not only in the text. It think it is much easier to get an overview from tables than text, and this would also be a good place to reiterate all covariates included and report the test statistics and estimates from the ANCOVAs, e.g. F, df, model explained variance and exact p values, main effect partial eta squared (or similar effect size estimate) and exact p values, and summary of post hoc comparisons
(e.g., exact p values for the significant results).

I think such information about the analyses and the results of the analyses really is needed for main results to allow for assessment of the details beyond rounded p values for the main effect and descriptives.

**Minor:**

11) I am also a bit hesitant about the inclusion of income/needs-ratio at age 1mo and 15yrs (concurrent) in the model. Although I understand the rationale for including these factors, I wonder if you run the risk of overadjustment. As seen in Table 1, they are very strongly related to trajectory, which is not surprising, and one would expect that those who have been stable low/high throughout their childhood would also tend to be poor/non-poor at age 15yrs, which could lead to unstable estimates if included in the same model. I would therefore suggest that the ANCOVAs are presented in separate steps, starting with one-way ANOVA and thereafter adding covariates/the other factors, perhaps in two steps with the income variables in a last step, and keep an eye on any instability in the model occurring due to too closely related factors.

**Discretionary:**

13) A couple of minor points in Table 1. BMI was higher than expected; mean = 65th percentile. Are the p values from one-way ANOVAs? (I assume they are, but it would be better if this would be clearly stated in the table)

**DISCUSSION**

I have some thoughts about the interpretations of the analyses. The conclusions that the income trajectory matters for adiposity, and that it is trajectories corresponding to class 1 and 2 that differ from 3-5 I totally agree with. However, I think I view the results a bit different than the authors when it comes to the interpretation of these conclusions - let me elaborate:

**Major:**

14) The main conclusion drawn is that the results support a change model – that downward mobility is the risky trajectory and upward mobility is a favorable trajectory. I agree that the results are consistent with that interpretation, specifically based on the results for the unstable# low trajectory.

However, the fact that this trajectory was similar in outcome to the stable low trajectory seems to disprove the interpretation that downward mobility is a very important feature, as there is no downward mobility in the stable low category. Class 1 and 2 also have in common 1) high accumulated time in poverty, and 2) recent poverty, and any of these dimensions could preliminarily explain the result that these two trajectories differ from the rest. The observation that the low# adequate trajectory displayed, which also seem to have a high accumulation of poverty (see below though), more similarity with the unstable# adequate and the stable adequate trajectories than with the stable low and unstable# low trajectories, seem to tentatively support that it is not the accumulation that matters.
What is left, after consideration of potentially confounding factors, seems to be the interpretation that it is first and foremost recent (not concurrent, as this is controlled for in the analyses) poverty that matters for adiposity; this is the single defining feature that alone is consistent with the following set of observations: 1) class 1 differs from 3-5, 2) class 2 differs from 3-5, 3) 1 and 2 do not differ, and 4) 3-5 do not differ. Of course, one should not disregard the plausible possibility that several dimensions of poverty over time may impact on health, but recent poverty being the most important dimensions seems to be the most parsimonious interpretation of the results.

I would suggest that an accumulation model also is specifically tested. The reason I suggest this is that the cumulative exposure is difficult to gauge from Figure 1, due to the varying time-intervals between measure points, and based on the above reasoning analyzing accumulation. This could be accomplished by, in principle, multiplying the number of time points with income/needs-ratio. The time points need however to be weighted by the actual time between time points, similar to an Area Under the Curve calculation. Alternatively, the accumulated time in relative poverty (with a dichotomous poverty variable similar to what was used in the LCGA), which perhaps is a more easily interpreted operationalization (i.e. time in poverty, rather than the product of time and severity of poverty).

Although I understand that this strays from intended focus of the paper, I think that examining the accumulation, would important additional information that would strengthen the inferences and yield clearer results from the trajectories. Right now, whether the accumulation hypothesis is consistent with the data or not is kind of a loose end in the paper, with the only information given to assess its validity is Figure 1, which as pointed out is difficult to assess.

As such, I think that the authors need to re-evaluate their interpretation of the results, including assessing the support for the change hypothesis in light of the (possibly more fitting and parsimonious) alternative hypotheses of recent and/or accumulated poverty, and where possible, address any weaknesses of the reasoning by data analysis. Alternatively, they need to make a stronger case for their interpretation.

Minor:
15) Additionally, I would suggest that the x axis of Figure 1 should be adjusted so it corresponds to the actual time lapsed between the measure points. Please also label the x axis consistently with age rather than mixed age and grade.

The rest of the discussion is very good, with clear reasoning and relevant discussion.

**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