Reviewer’s report

Title: The associations between weight-related anthropometrics during childhood and lung function in late childhood: a retrospective cohort study

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Reviewer: Joshua Lawson

Reviewer’s report:

Thank you for the opportunity to review this paper. It was a well written paper presenting research around the association between anthropometric measurements in childhood and lung function in late childhood. The study had several strengths including objectively measured outcomes and exposures as well as a longitudinal design. Below are some questions and concerns I have that may help the paper.

1. While the measures being used were generally measured longitudinally, there should be some additional mention in the abstract and early on that this was not a conventional cohort study. It appears that this was from a nested case-control part of a cohort study, which had some matching involved. I think it is important to make this clear up front as it could have some implications on the analysis, interpretation, and appraisal.

2. With regard to the nested case control methods, I am a little confused and the description of the study design may need to be improved. It is my understanding that a nested case control study is where there is a cohort study being completed then cases and controls, based on an outcome condition, are selected then the exposure information for those participants are investigated. It looks like in the current study, there is a population cohort then children are selected based on an exposure (maternal preeclampsia) then followed forward to look at the outcomes. I looked into the article referenced as the source of the methods description (Ogland et al) and this referenced a previous study (Vatten et al), which then referenced a previous study (Odegard et al). Some additional information about the study population can be gained from each article but the description of the selection and design is a little confusing and should be better described.

3. Another concern I have is the breaking of the matching in this analysis. Participants were selected based on a matched design, which then biases the sample. The matching should be accounted for in the analysis otherwise there is the potential for biased associations. The authors should try and convince the reader that this the associations should remain unbiased and that the analysis was appropriate. The authors mention that preeclampsia and maternal age were adjusted for then also mention the issue in the methods but I still believe that some further discussion and justification is warranted.

4. It appears that subscapular skinfold was only measured at the second follow-up, which is the same time the spirometry was performed so this becomes a cross-sectional analysis. This should be discussed briefly, possibly in the limitations.
5. Some of the other anthropometric measures were taken in the first follow-up which would be either 1 or 2 years earlier for boys and girls, respectively. Was this enough time to see an effect on lung function? Given that there was some interaction by sex and that girls had these measures a year earlier than boys, could that longer duration between measures have accounted for the associations in girls but not boys in some way?

6. Related to the difference in time period of data collection between boys and girls, understandably, this was done to try and account for puberty. However, this is a very crude method. Was any other data collected related to puberty? This should be discussed as a limitation especially since there are differences in lung development through childhood and adolescence between boys and girls and there are difference in the results between boys and girls.

7. From what I can tell, 463 out of 1025 children took part resulting in approximately a 45% participation rate. Is this true? In the results, the authors mention a comparison between those who took part in both follow-ups vs only one follow-up but how many completed the first follow-up? In the limitations, the authors mention that the population is not representative of the general population given the selection procedures but they should also address the low participation rate from the original cohort and potential implications on the results. The number included in the first follow-up should also be described.

8. In paragraph 6 of the results where the stratified analyses were described, was formal interaction assessment completed as mentioned in paragraph 4 of the results? P-values should be added for these assessments.

9. Asthma history is highlighted in Table 2 and at the end of paragraph 2 of the results but seems a little out of place given the lack of focus on this variable until this point. I agree that it is important but if it is to be used, there should be some more justification. Was there any assessment of interaction by asthma status completed? This seems like a plausible investigation given the existing literature on anthropometric measures and asthma outcome and the possible mechanisms between anthropometry, asthma, lung function, and the gender switch in asthma prevalence around puberty, especially since there are differences in results between boys and girls.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.
Yes

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