Author's response to reviews

Title: Tracking and prevalence of cardiovascular disease risk factors across socio-economic classes: A longitudinal substudy of The European Youth Heart Study

Authors:

Peter Lund PLK Kristensen (plkristensen@health.sdu.dk)
Niels NW Wedderkopp (NWedderkopp@health.sdu.dk)
Niels Christian NCM Moller (ncmoller@health.sdu.dk)
Lars Bo LBA Andersen (lars.bo.andersen@nih.no)
Charlotte Norkjaer CNB Bai (CBai@health.sdu.dk)
Karsten KF Froberg (kroberg@health.sdu.dk)

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Author's response to reviews: see over
We appreciate the comments provided by the reviewers in order to improve our manuscript. The responses to each reviewer's concerns are outlined below.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

**Reviewer:** Hanno Ulmer

**Review:**
The classification regarding socio-economic status was based on the job of the mother. How were the children classified in case their mother has been working as a housewife?

**Response**
Mothers working as housewives were classified as housekeepers in *The International standard classification of occupation* scheme. (i.e. classified as belonging to the blue-collar group)

**Review:**
To account for dropouts, a weighting approach was used. JWR Twisk used General Estimating Equation technique to overcome this problem. Did the author try this approach as well? If yes, were the results comparable? Please justify the selected estimation method used for the regression analyses and the procedure to handle dropouts.

Before the article was written and any analyses were carried out, we discussed the General Estimating Equation technique with JWR Twisk by email. As he informed us, when measurements are obtained at only two points in time it is not necessary to use GEE - the exact same results will be produced by traditional regression techniques with robust variance estimates. Regarding the procedure to handle dropouts, the technique is briefly described in terms comprehensible for non-statisticians in the article *Methods for Handling Dropouts in Longitudinal Clinical Trials* [1]. In this article references are also provided to more thoroughgoing statistical articles on the subject. The reference has been added to the statistical paragraph of our paper.


**Review:**
A weakness of the paper are the unclear definitions of low PF and overweight. Please specify these definitions in the paper. Are there really no accepted cut-offs for PF so that it is necessary to use the first quartile?

**Response**
Overweight was defined according to objective BMI cut-points, proposed by the International Obesity Task Force (IOTF). The definition is given in the section *Body fatness*. In contrast there are, regrettably, no commonly accepted objective cut points of low physical fitness when studying children. Thus, low physical fitness was defined according to the lowest sex and age group specific quartile of physical fitness, as defined in the paragraph *Physical fitness*
Reviewer: Richie Poulton

Review:
I did not find the rationale for using only the mothers occupation to define SES particularly strong, and in fact believe the better variable is that which combines information from both mother and father. Thus I recommend this be done for the main analyses. Any differences that appear when using either mother OR fathers occupation by themselves (conducted as secondary analyses) can still be discussed when trying to reconcile/explain previously inconsistent findings.

Response
It is absolutely correct, as pointed out by the reviewer, that the rationale for using only the mothers occupation to define SES was not particularly strong, as it was presented in the text. However, in our opinion, this is not a reflection of that a strong rationale does not exist. Therefore we have elaborated on the arguments for the classification of socio-economic status in the article in the paragraph Socio-economic status. Two recent European studies on large cohorts of children (i.e. 1420 and 3436 respectively) have shown that the risk of being overweight in childhood/early adolescence is related to the educational level of the mother rather than the father. Furthermore, in the discussion paragraph we have described the results in more details as they turn out, if socio-economic status was defined by a combination of information from both the mother and the father. This way the reader can get an insight into the results derived from analyses based on each of the two definitions of SES.

Review:
As it stands, the Discussion lacks focus, and is a bit lengthy. The substantive findings should be discussed in more detail (whatever they are following re-analysis as above), and the method issues (be they about tracking or about previous inconsistent findings) should be presented to 1. elucidate and 2. contextualise the current findings. The 'take-home' public health message for interventionists and policy makers can also be stated more succinctly.

Response
In order to make the discussion more focused, we have removed sections, which in our opinion could be avoided. We have changed the order of some of the sections and added new text to clarify certain points in the discussion.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

Reviewer: Hanno Ulmer

Review:
regarding participants, 384 children returned to EYHS-II this is in fact 50% of children invited, it would be more honest to give this percentage (50%) rather than 65.2%

Response
We agree and have made the changes

Review:
. and , are used as decimal points, I think . should be used throughout the paper
Response
Changes have been made accordingly

Review:
Table 1 is difficult to read, definitions for at-risk prevalence and means of PF and BMI should be given in the footnotes. In contrast p-values should be included into the table (for instance by using an additional row) rather than in the footnotes.

Response
We have added definitions for at-risk prevalence in the footnotes and also stated which units PF and BMI are measured in. The detailed information on P-values in the footnotes has been removed, and in order to indicate significant differences between groups of SES, symbols have been applied.
We have kept the mean values of PF and BMI in the table, since it would require a lot of text in the footnotes to explain which gender and SES groups each of the mean values represents.

Review:
Stability coefficients should also be labelled as 'tracking coefficients' since this term is more widely used.

Response
In table 2 where stability coefficients are given, we have now also labelled the stability coefficients as tracking coefficients. However in the main text of the article we have only used the expression stability in agreement with the definition of tracking given in the statistics paragraph.

Review:
Please give definitions for low PF and overweight also in footnotes of table 4 and 5. I think it would help to give absolut frequencies in table 5 as well for instance such as 10/100 10%. This would give necessary information about sub-group samples sizes.

Response
Definitions of low PF and overweight have been included in tables 4 and 5, and the number of subjects on which the percentages are based in table 5 are now visible in the table.

Reviewer: Richie Poulton

Review:
Be consistent in the use of SES group descriptors. That is, Blue-collar and White-collar are used frequently, but Low and High are also used. The latter terms are more informative, and should probably be used thoughout.

Response
We agree and have used Low and High as SES group descriptors throughout the article.