Author's response to reviews

Title: Life style related to blood pressure and body weight in adolescence: Cross sectional data from the Young-HUNT study, Norway.

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Author's response to reviews: see over
Dear Dr. Zauner,

Thank you for the positive evaluation by your Editorial Board, giving us the possibility to re-submit this paper, and for the comments by the reviewers.

We also appreciate the reviewers’ comments, which we think have improved the paper. None the less the main results are unchanged.

In the following you will find a point by point response to the comments, and how we have amended the paper.

**Reviewer no 1 (Pitsavos)**

1. “…the authors should have measured central obesity and not only BMI.

   In addition to BMI we also measured waist circumference, which may be a proxy for central obesity. We have in the revised paper included the results of these measurements in a new table (Table 3); however the overall results were the same as those we found for BMI.

2. “…..the nutritional questionnaire is rather poor and they should have used a more detailed FFQ with weekly consumption of all food variables, including alcohol, protein and type of protein intake (fish, red meat, poultry,...) and try to create a nutritional score in order to evaluate the nutritional habits as healthy or unhealthy.

   Both referees correctly comment that these questions have not been validated, and we have added a comment on this in the revised version of the paper. However, very similar questions have been applied in similar studies (large, cross-sectional, populations based studies) (two recent example: Delva et al. J Adolesc Health 2006 and Am J Prevent Med 2007), whereas FFQs and dietary food records mostly have been used in smaller studies. Moreover, in this study where nearly 10 000 adolescents were invited to participate, and where dietary habits
were only one among a number of other life style domains, we would most likely have had a much higher dropout if we had included a detailed FFQ, or even asked for daily dietary records. None the less, it is striking that our main results are consistent with many studies that have used FFQ or dietary records (see for example Utter et al. J Am Diet Assoc. 2007). Finally, since the intention of our study was to differentiate between dietary habits considered healthy versus unhealthy habits, we propose that the applied questions were appropriate.

3. **The authors should have checked also the abuse of drugs**

The only (illegal) drug asked for in the Young-HUNT study was marijuana (cannabis), but this question was only asked to those in high school, 3487 adolescents. The proportion of ever-users was low; approximately 9% of the high school population, and when we adjusted for this in the multivariable analyses, the estimates were unchanged. We have added a sentence on marijuana (cannabis) – experience in the methods and results section.

4. **Physical activity status should be measured using a more detailed questionnaire.**

The same arguments as for the dietary questions (point 2) may apply here. However, in contrast to the questions on dietary habits, these questions have been validated, and have been used in a number of previous publications. We have emphasised this in the revised paper in the methods section.

5. **The authors should inform about the statistical power of their sample size for the presented results.**

It is questionable whether it is appropriate to perform post-hoc power calculations, and as we read it, the majority of the literature concludes that calculating statistical power after the study has been completed is inappropriate. However, the power of the study is best reflected in the presented (95%) confidence intervals for the effect measures.

6. **The authors should investigate further explanations why smokers have lower systolic blood pressure.**

We agree that this observation warrant further discussion. However, several epidemiological studies have reported similar results, but apart from a hypothesis that the low blood pressure precedes onset of smoking (Charlton and While, 1995), the mechanisms remain unclear. In fact, when we followed the reviewer’s advice and analysed pack years (see point 7 b), we found that this finding was restricted to adolescents with short duration of smoking exposure. Our discussion of this finding has been expanded in the revised manuscript, and may also be seen in relation to one of the comments under point 7.

7. **The should investigate pulse pressure and from other studies in adolescence the prognostic factor of blood pressure in that age and also the impact of packet years of smoking.**

We appreciate the comment, and have explored the possibility of using pulse pressure as an outcome; however, the main results are essentially unchanged whereas there are minor findings in one or the other direction. We believe that this information may partly add to the length of the paper, and more importantly, it may be even more difficult for the reader to understand and to interpret the meaning of these results. We have therefore chosen not to include pulse pressure in the revised paper.
We also appreciate the comment on package years. However, only 318 (4%) adolescents indicated that they had tobacco exposure corresponding to a package year score of one or more. Thus, in this young population tobacco exposure was short and 50% of smokers were occasional smokers. Interestingly, the 318 adolescents with one or more pack year of smoking exposure had mean BP values that did not differ from mean BP values among non-smokers. This finding could therefore probably be seen in connection with point 6 above, suggesting that the lower blood pressure among smokers may probably be a transitory finding, due to either lower blood pressure before they start smoking (as proposed above), or a transitory effect. We have, in the revised paper, described pack years and this finding in the methods, results and discussion section.

Reviewer no 2 (Meininger)

1. "Blood pressure values were adjusted for age, arm circumference, height, BMI and smoking. Controlling for arm circumference and BMI essentially controlled for adiposity and thus may have attenuated the associations of dietary factors and physical activity with systolic or diastolic blood pressure".

   We agree with the referee since and we have therefore in the revised paper, only adjusted for height and age; however, the results were essentially the same as in the first version of the article. In addition, in the revised paper we have done the multivariable analyses both with and without BMI as a covariate. Interestingly, we found little evidence that the associations between dietary factors and physical activity were attenuated when we included BMI. Instead, we found that BMI had a significant additional effect on the explained variance of mean blood pressure (in addition to height and age), and we consider these findings to be in line with the early origin of adult disease hypothesis, suggesting that both blood pressure and overweight at this age, is more dependent on genetic factors and early adverse environmental factors (in utero or early infancy). None the less, low levels of physical activity were significantly associated with higher diastolic blood pressure both in boys and in girls.

   In the revised paper, in the tables 3 and 4 we now present blood pressure values, adjusted (only) for height and age, whereas we describe the results of the multivariable analyses in the results section, and comment these findings in the discussion.

2. "It would be useful to know the extent of overlap among the lifestyle variables and whether they explain unique or overlapping variance in the dependent variables”.

   As already commented under point 1 we have explored this in multivariable analyses. For most of the associations the lifestyle variables had independent effects, however, their contribution to the explained variance in blood pressure was marginal. For overweight we found some evidence that there might have been some overlap between lifestyle variables. These results are included in the revised paper; in the results section (p 12; para 2 and 3) and commented in the discussion (p 15; para 2).

3. "From an epidemiologic perspective, the strength a cross-sectional survey is the ability to assess the prevalence and clustering of risk factors. The strength is not realized in the selection of findings presented in the manuscript. No data are
We appreciate the comment, and have in the revised paper included information on clustering, and briefly commented these findings in the last paragraph of the discussion.

4. “....there is no information presented on blood pressure differences among the normal weight, overweight and obese respondents”.

In the revised paper we have now included a new table (Table 2) showing blood pressure for the strata of normal, overweight and obesity, and the table nicely shows the association with increasing blood pressure.

We have also changed the sequence of the tables, in such a way that they follow the text more naturally. The new list of tables is:

Table 1: Characteristics of the population
Table 2: Weight status and systolic and diastolic blood pressure.
Table 3: Association between life style factors and systolic and diastolic blood pressure in girls.
Table 4: Association between life style factors and systolic and diastolic blood pressure in boys.
Table 5: Odds ratio for overweight and obesity related to life style.
Table 6: Waist circumference related to life style.

We hope that you will find that we have amended the paper according to the reviewers’ comments, and that you will find the revised paper acceptable for publication.

On behalf of the authors,
Yours, sincerely

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