Author’s response to reviews

Title: Education-based health inequalities in 18,000 Norwegian couples: The Nord-Trondelag Health Study

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Author's response to reviews: see over
Dear editor of BMC Public Health

Resubmission of our manuscript:
*Educational health inequalities in 18,000 Norwegian couples*
*The Nord-Trøndelag Health Study*

We thank you for your, and the three referees’, incisive and thoughtful comments to our paper, and for the opportunity to resubmit it with appropriate changes. We appreciate the positive acknowledgement of it as “an interesting paper” and based on an “excellent data base”. We interpreted two of the referees to be very positive, while the third found the couple perspective of the paper to be of limited interest. The last reviewer did not, however, indicate any errors in the analyses.

While it is hard to say at this point to what extent the couple perspective will prove useful, we believe that our data and analyses contributes considerably to that discussion.

We have discussed all questions and remarks from the referees, and our specific responses and changes to all referee suggestions are presented point-by-point below in this cover letter. As a response to one of the reviewers’ very justified request, and for clarity of the readers, we kindly ask the permission to include an extra figure, attached as Figure 2.

We hope the revised version of the paper is now suitable for publication in your journal. All authors have taken part in the revision and have read and approved the revised version.

Yours sincerely,
Sara Marie Nilsen,
Research fellow
1 Reviewer's report
Anton Carl Jonas Lager

1. A first motive for studying couples is, according to the authors, that this context has received "scarce attention". Yet, according to the next sentence; “the health impact of partner’s socioeconomic position has been thoroughly studied”. This seems a bit contradictory.

IN RESPONSE
We agree with the reviewer and have rephrased the introduction, highlighting our motives for studying couples

2. Secondly, the authors state, couple characteristics are likely to be important “above and beyond” individual characteristics. How exactly? It is easy to see how “neighbourhoods, wards, districts”, or even households, can be important over and beyond individuals (e.g. through local governments, policies, physical environments etc). But what type of factors act on this level for couples? What separates your partner’s education from other “individual” factors that you are exposed to?

IN RESPONSE
We have rephrased the introduction substantially to take account of these comments (please, see page 4, paragraph 2 – the original paragraph has now been deleted). Please, also see the next comment.

3. Thirdly, studies on couples are “scarce” according to the authors. But in the next sentence, “the health impact of partner’s socioeconomic position has been thoroughly studied”. This seems a bit contradictory.

IN RESPONSE
We agree with the reviewer and have changed the introduction accordingly.

4. A fourth point with this paper is to properly account statistically for the correlation of health outcomes within couples. But whether this is a big problem or not is not possible to assess from the study. Showing the results for “Model 1” with adjustment for partners’ education would be illuminating.

IN RESPONSE
The variance attributable to the couple level was 16% for subjective health, 19% for anxiety, 25% for depression and 42% for education – reflecting a considerable outcome and exposure (education) clustering within couples. Our aim was not to particularly compare our results from a model based on partner adjustment as such. Adjustment for partner’s education would give the individual’s own education gradient holding partner’s education constant – an approach that has provided interesting results in many studies. Our aim was to investigate health differences in couples with different education – an approach that rarely has been investigated in this area of research. Furthermore, as education is highly clustered in couples, we also wanted to assess the educational gradient between couples. To do this we have undertaken an analysis which is commonly used to model clustered data (including clustered data of dyads) [1-4]. Nevertheless, we believe that showing the results using
partner’s education as a covariate also would be illuminating and have provided those in table 2a and 2b and in the manuscript.

5. In the methods section, it is stated that the authors used “cubic splines to prevent any restrictions dependent on the particular choice of categorization of education”. This is very confusing. No models with education as a categorical variable are presented if I understand it correctly – and the models that are presented seem to treat education as a continuous variable (with estimates corresponding to 1 extra year of education)?

IN RESPONSE
In the revised version we have rephrased this section (please, see page 8, paragraph 1). Our point was that if we had made a choice of categorization of education first, this would be a restriction on form of the association.

6. In the results, one year of own education is compared to “one year increase in the couple average education”. But isn't this a very unfair comparison as it compares 1 year of education to 2 years: i.e. 1 year increase of the average means either 1 year of own education and 1 year of partner's education, or that the partner has studied for 2 extra years?

IN RESPONSE
The way we have modeled the data provides us with within and between estimates that are directly comparable. If the between coefficient was similar to the within coefficient, this would imply that the between effect was in line with what would have been expected given the composition of education in the couples. However, we believe that the reviewer is addressing an important point and have tried to clarify this in a figure illustrating health differences in three couples with different level of education (Figure 2). We expect this additional figure to improve the clarity of the paper.

7. Finally, in the discussion it is stated that; “the individual educational gradient may be overestimated in traditional models” (assuming partner's education is not a consequence of your own…). But you could easily turn that point around: if individual education has an effect also for people in our close surroundings – we have previously underestimated the effect of education!

IN RESPONSE
We agree with the reviewer on this point. It is possible that our use of the term ‘individual education’ may be misleading. As the reviewer correctly notes, our results could be interpreted as a strengthening of the educational gradient in health. This point has been discussed, for example, on page 14, paragraph 1.
2 Reviewer’s report
Niels Schenk

1. The authors are unclear on what the estimates of within and between-couple parameters actually entail. In the abstract they summarize the result of their within-couple estimate as “one year increase in education as compared to one’s partner”. On page 8 they write “one year increase in education was …” without the reference to an individual’s partner.

IN RESPONSE
In the revised version of the manuscript we have rephrased this accordingly.

2. As far as I can see (the formulas on page 6 contain unreadable symbols), the authors estimate how a one year difference in education from the overall couple mean is associated with the dependent variables used, while holding the overall mean constant. This is slightly different from an interpretation that refers to the difference in education with the partner.

IN RESPONSE
In the revised manuscript we have clarified the interpretation of within vs. between coefficients. The “unreadable” symbols are means, and are now corrected.

3. A related problem with interpretation of the within and between effects is that they are used in comparison to traditional estimates of individual educational gradients. However, when comparing the within-couple estimate with the traditional estimate, the authors should realize that these are two different things. The within-couple estimate refers to differences in education from the couples’ mean, the traditional estimate does not. The authors do not merely properly account for educational resemblance in couples (as stated on page 11), they estimate something qualitatively different. Stating that “the individual educational gradient may be overestimated in traditional models” on page 10 may be problematic for this reason. Of course it may be true that this gradient is overestimated but in my view the authors are not sufficiently clear on the difference between the quantities they estimate and the estimate they compare it to.

IN RESPONSE
We agree with the reviewer on this point and have rephrased the manuscript accordingly. It is possible that our use of the term ‘individual education’ may be misleading. Our aim was to investigate health differences in couples with different education – an approach that rarely have been used in this area of research. Furthermore, as education is highly clustered in couples, we also wanted to assess the educational gradient between couples.

4. The formulas presented on page 6 do not include a description of the random effects estimated. Perhaps this is not problematic but in my view the authors should at least be clear what is exactly estimated (and what is left out of the results section).

IN RESPONSE
The formulas were presented as the expected outcome, but we agree with the reviewer that it could be informative to present the error part of the model also (please, see page 9, paragraph 3).

5. On page 6 the authors state that education was used as a continuous measure. Is this a recoded version of the trichotomy presented in table 1, or an actual continuous measure of the number of years respondents went to school?

IN RESPONSE
This is a continuous measure of number of years respondents went to school, education in years. This is clarified in the revised manuscript (please, see page 7, paragraph 3).

6. On page 8 the authors present figure 1 but do not (or hardly) discuss it. In my view it would be more interesting to see figures of the models estimated in table 2. The authors now seem to estimate and plot individual educational gradients that they themselves criticize.

IN RESPONSE
Given the comments from this and the other reviewers, we believe that is a good idea to illustrate the within and between couple educational gradient with a figure (attached as Figure 2).

7. On page 11 the authors state that “women were more affected by the co-partners”. This is of course very interesting but not something the models directly estimate. The authors estimate how an individual's difference from a couple mean is associated with the dependent variable. Of course the couple's mean includes the partner's score but it is not the partner's score itself that is used to estimate the discussed coefficient.

IN RESPONSE
In additional analysis, we tested for statistical interaction between sex and education. We have in the revised manuscript included these estimates for anxiety as we did not have any indications of interaction effects on the other outcomes (see page 12, paragraph 2).

8. Related to the comment above: the authors state on page 9 that there was an interaction between sex and education when predicting anxiety scores. I would like to see how much difference there was between men and women, instead of only know that there was a difference.

IN RESPONSE
In the revised version we have provided this information.

9. The text contains many typos and constructions of sentences that seem odd if not incorrect.

IN RESPONSE
The revised manuscript has now been edited by a professional English language editing bureau.
3 Reviewer’s report
Anton Kunst

1. The introduction of this paper does not clearly specify, in substantive terms, what it contributes to the existing literature on health inequalities. In Background, the only argument given for this new paper is that the tendency of educational resemblance in couples “has generally not been properly accounted for” (page 3, lines 8-9 from below). This technical argument however comes without further explanation or references.

IN RESPONSE
We have rephrased the introduction to our paper substantially to take account of these comments. As mentioned in the introduction (page 4, paragraph 1) and in the discussion section of the paper (page 13, paragraph 3), there has been a discussion on how to measure socioeconomic position, as the social health gradient is usually less pronounced in women, at least for mortality. Furthermore, it has been argued that partner’s socioeconomic position is important, particularly for women. However, the health impact of the partner’s socioeconomic position has not been resolved, and measures of socio-economic position and health inequalities must still be refined.

Our aim was not to particularly compare our results from a model based on partner adjustment as such. Adjustment for partner’s education would give the individual’s own education gradient holding partner’s education constant – an approach that has provided interesting results in many studies. Our aim was to investigate health differences in couples with different education – an approach that rarely has been investigated in this area of research. Furthermore, as education is highly clustered in couples, we also wanted to assess the educational gradient between couples. To do this we have undertaken an analysis which is commonly used to model clustered data (including clustered data of dyads) [1-4].

2. In methods, the authors give a more statistical argument for their new approach (page 6, lines 7-8 from below) but this argument is unclear and refers only a review study on twins, not on health inequalities (ref34).

IN RESPONSE
The reference to the analysis of twin studies was chosen because couples are dyads like twins. As Amanda Sacker argues on page 251 in the book Family Matters [4], referring to the above mentioned review of twin studies; “…Carlin et al present different parameterizations of a general regression model that allows for different covariate effects between and within twin pairs. … These models can also be applied to other family relationships besides twins.” In the revised version we have provided more references for our methodological approach.

3. In Discussion, the only argument for this paper that is given in retrospect can be found on page 11 (lines 3 and 4) but again the reasoning is quite technical and the only reference is to the same paper on twins (ref 34).

IN RESPONSE
We have rephrased the introduction and the discussion in order to take account of these comments.

4. The frequent reference to “educational resemblance” is in itself an insufficient justification, as we all know - and take into account - that the SES of subjects is strongly associated to the SES of their partners.
IN RESPONSE
We agree that this is not a surprising result, but our point is that this may have important implications for the analytical strategy.

5. The application of the multi-level analysis may be a promising approach if the authors were able to identify a true couple-level effect as separate from direct effect of individual-level SES (of subject or of partner). It seems that the authors pretend to have identified couple-level effects (see e.g. the Conclusions at the end of the paper). However, to identify such a couple-level effect, the regression models would need to control for SES of the subject and for the SES of the partner. This is not done (and probably cannot be done).

IN RESPONSE
We agree with the reviewer that the terminology ‘individual education’ may be misleading in this context (see reviewer 2). However, we do not agree that a separation of within and between effects in couples can not be done. We have undertaken a well known analytical strategy for the analysis of clustered data. The use of within dyad models is commonly used in matched case control studies, twin studies, sibling studies and studies of “other family relationships” [1-4]. However, as we expected an influence of the couple’s education as well, we also modeled the between couple effect of education in a multilevel framework.

6. As a result, the existence of a couple-level effect is not shown, and all observed associations may be just due to the direct effects of individual-level SES (of subject and of the partner, respectively). And such effects have already been shown in previous studies. The key findings of the paper are phrased in terms of educational differences “both between and within couples”. Even the words ‘within’ and ‘between’ may be simple to understand, and therefore be attractive, it is unclear what these terms actually mean in substantive terms. They seem to suggest a couple-level effect, similar to area-level effects or nation-level effects. But, as argued above, this type of effects could not be demonstrated.

IN RESPONSE
In the revised manuscript we have clarified the terminology in order to meet these comments. We agree that several studies have provided results of the impact of partner’s education. However, it is important to notice that, in a multilevel frame, these two strategies will give the same expected outcome value given the covariates. It is the interpretation that differs (see reviewer 1). We think it is an interesting result that despite differences in education between couples, their perceived health within couples differs very little. We believe that our analytical strategy can illuminate educational gradients in health showing the combined couple effect of education as well as the differences within couples. However, this does not imply that it is incorrect to analyze data otherwise.

