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,

We have revised our manuscript in line your reviewers’ suggestions and are happy to present a new version. Again, we are grateful for valuable comments. A response to each of their comments is presented below.

Reviewer 1

1. What “has not been resolved” in relation to “the health impact of the partner’s socioeconomic position”? And in what way does this study move things forward in this field? According to previous literature, are we not expecting the partner’s socioeconomic position to be correlated with the first person’s health, and isn’t this exactly what also this study suggests? (Note also that “health impact” seems to suggest causality – something that cannot be approached in the data).

Response: In the revised manuscript we have rephrased the second paragraph in the background chapter in order to make our point clearer (see also reviewer 3, point 3).

2. Although the “social context”-motive for studying partners has been toned down in this version, there is still something hinted here that I don’t think is called for. The concept of social context” to me suggests that we must assume a more complicated picture (in one way or another) than the one that can be captured by simply studying the exposures that each individual is exposed to. In this study, one of the exposures is the partner’s education. As long as this exposure does not mean different things in different couples, I think the use of “social context” confuses more than it clarifies.

Response: In the revised version of the second paragraph in the background we have tried to keep in line with the aim of the study as the reviewer suggests.

3. It is stated that “As shown in Figure 1, we used cubic splines to assess the associations between education and the health outcomes” but as I understand it; you ONLY use cubic splines for this figure, which is only there to give a description of the data, and not in the main analyses, right? Then this information could with benefit be moved, e.g. to the figure legend, or at least moved from the very beginning of the “Statistics” section.

Response: We agree with the reviewer and have moved this paragraph to the end of the statistics section.

4. The problem with comparing the effect of education within and between couples that I pointed to last time has not been resolved. You state in the abstract that “A one-year increase in education relative to that of one’s partner was associated with an improvement of 0.6 scale points (95% confidence interval 0.5 to 0.8) in the subjective health score (within-couple coefficient). A one-year increase in a couple’s average education was associated with an improvement of 1.7 scale points (95% confidence interval 1.6 to 1.8) in the subjective health score (between-couple coefficient)” and consequently that “Education-based inequalities in health were more pronounced
between than within couples”. But this is still an unfair comparison. What you seem to have shown is that 1 year of education for 1 person explains less variation in health than for example 1 year of education for 2 persons, or 2 years of education for 1 person (that is, in both cases, an average increase of 1 year in the couple’s average education). Isn’t this stating the obvious?

Response: We are not certain that we fully understand the reviewer’s argument here. Averaging over groups will not change the unit of measurement which in this case is education in years. If there was no between effect of education, a couple with a mean education of 10 years compared to a couple with 11 years would have the same expected health score difference as spouses with 10 and 11 years of education (other factors being held constant). Hence, the within and between estimates are directly comparable. Equal between and within coefficients would imply that the between effect was in line with what would have been expected given the composition of education in the couples (the lines in figure 2 would be equal). For all the health outcome measures, the between coefficient is more than twice the size of the within coefficient (about 3 to 7 times the size dependent on the different outcome measures. Hence, this allows for the conclusion that there is a larger between than within association, and for the mental health measures the within estimate is close to 0 – indicating that the socio-economic gradient is only notable between couples.

Furthermore, looking at the results from model 1, table 2a gave an education estimate of -1.3 scale points, model 2 -1.2 when adjusting for partner’s education. Model 3 comparing couples with different education gave an estimate of -0.6 – half of the estimate in model 2. From our point of view, this suggests that health related educational differences are less pronounced within couples than any of the other measures in our models. Nevertheless, the reviewer is raising an important question regarding the interpretation of the results and in the revised manuscript we have tried to clarify and rephrase in order to address the reviewer’s concerns (see conclusion part of the abstract and the first and second paragraph in the discussion section).

5. Further, and probably more importantly, if you control for “couple average education” – it does not seem surprising that individual education becomes relatively unimportant (both to own education in model 1-2 and to “between couple” education in model 3) – given that most of the individual’s education in model 3 is subsumed by the couple’s average.

Response: In situations where the exposure is clustered within a multi-level framework, cluster mean adjustment is regarded an adequate analytical strategy [1, 2]. Again, as we stated in the previous point: equal between and within coefficients would imply that the between effect was in line with what would have been expected given the composition of education in the couples (the lines in figure 2 would follow the same line). That is, we do not agree with the reviewer that the individual’s education in model 3 is subsumed by the couple’s average. To further address the reviewer’s concerns we have performed a fixed effect regression model (xtreg with fixed effects in STATA), using the original educational scale (adjusting for all variability between couples – leaving only within variability). In this model, a one-year increase in education relative to that of one’s partner was associated with an improvement of 0.7 scale points (95% confidence interval 0.5 to 0.8) in the subjective health score. That is, almost identical to the estimate in Table 2a (without roundings -0.63 vs -0.66).
6. Finally, I wonder if there is a specific reason for the interest in “within couple inequalities”: are you expecting some kind of relative, rather than absolute, “effects” due to e.g. social comparison between the spouses, and if so, could you expand on this? If not, is it necessary to refer to “inequalities”?

Response: We agree with the reviewer that the phrase “within couple inequalities” might be misleading. In the revised version we have rephrased using “inequalities” when we are referring to socio-economic differences in general and “within couple differences” when we refer to situation between spouses.

Reviewer 2

1. Although the authors have improved the interpretation of their results, there still seem to be some unresolved issues. The authors now consistently use "relative to one’s partner" when discussing the within-couple coefficients. However, this coefficient denotes differences relative to the couples’ mean, not to one’s partner. This is different because as far as I can see a one-point difference from the couple mean entails a two-point difference to the partner (this is also what another reviewer is pointing at, see his point six in the first reviewers report). I do agree with the authors that the within and between-couple coefficients are comparable to each other, they are just not directly comparable to traditional measures of educational inequalities.

Response: We think that this is now covered in the response to reviewer 1 (see response point 4 and 5, reviewer 1). Regarding the scale question, a look at the results from figure 2 for the first couple could be clarifying. They have 9 and 13 years education respectively with a mean of 11. That is, they have four years difference in education. If we subtract the partners’ education from the couples mean, we will have -2 and +2, which still is 4 years difference. Hence, one unit change is one year of extra education.

2. Figure 2: There seems to be a mistake in the figure where anxiety scores are the dependent variable. I’m assuming the authors used 19 years of education to estimate the predicted anxiety score, the figure is plotted as if they used 20. I find it strange that the authors do not describe in the text what values the independent variables were set to when calculating these predicted scores. How old are these virtual couples and how much education did they have?

Response: In the revised manuscript we have corrected the anxiety figure accordingly. We follow the reviewer’s argument, and in the revised manuscript we have provided the information that the prediction was for the age group between 45-54 years. Sex was set to 0.5 as each couple consists of man and woman (see first paragraph in the statistics section). In the results section we have provided the information regarding length of education in each of the three couples (see the end of the fourth paragraph in the results section).

3. Moreover, the authors state in their reviewer’s report that they expect this figure to improve the clarity of the paper. This might be true (I definitely think so), but shouldn’t they discuss it at more length if the figure is able to summarize the author’s results more clearly than the number in the tables?
Response: Our aim with the figure was to visualize the results from model 3 in Table 2a and b. Hence, we believe that more thorough discussion of figure 2 could confuse the reader, as it is a visualizing of the multivariable analyses and not results presentation on its own. Furthermore, reviewer 3 suggested that we compressed the paper, rather than expanded it.

4. There is a subscript missing in the formula describing how the ICC is calculated.

Response: This is corrected in the revised manuscript.

5. The manuscript still contains many typos and incorrect sentence constructions.

Response: We have contacted the professional English language editing bureau again and have got a second language review.

Reviewer 3

1. The key finding of the paper is that the relationship of health with the between-couple educational variable is stronger than with the within-couple educational variable. Following the key assumption specified in the sentence on page 8-9, the authors interpret this larger effect as evidence that there is some kind of aggregate couple-level effect, 'suggesting that partners form their socioeconomic position together'. But can this result indeed be interpreted this way? I have two concerns. - This result could have other explanations. My intuition tells me that, if a subjects' educational level is not directed included into the regression model, but instead used to derive two other variables (a couple-level variable that comes close to the subjects' education, and a unusual residual within-couple difference variable) that the former variable would be much more able to capture the effects of the subject's own education. Thus, it seems hardly a surprise to note that this couple-level variable is more predictive.

Response: We believe that this question is related to the concerns raised by reviewer 1 (point 4 and 5) and 2 (point 1).

2. The % variance explained in models with the couple-level education variables (as measured with the ICC) is NOT larger than the variance explained with individual-level education variables (of the subject and its partner). We would however expect a LARGER percentage explained in the model with the couple-level variable, if these variables were to capture couple-level effects that are unmeasured at the individual level. But they do not. I think that the authors should at least be able to respond to these comments, as they concern the essence of their paper.

Response: We agree with the reviewer and have included this argument in the discussion section (see discussion section, the end of paragraph 4).

3. The paper could be written more concisely. The introduction and discussion parts contain many parts that are not essential to the paper. These sections could be reduced by one third.

Response: In the revised version we have tried to write the paper more concisely. The second paragraph of the introduction is compressed, and we have rephrased large part of the discussion section to make it more concise.
4. Related to this, a stronger focus is needed on the key implication of the paper, that the evidence suggests that couples form their socioeconomic position together. The authors should aim to discuss how educational sources could be shared. The "shared resources" theory seems useful, but is mentioned only once without reference.

Response: In the revised version of the manuscript we have provided a reference as the reviewer requests (see third paragraph in the introduction). Also, in the discussion section we have discussed this topic further (see the second paragraph in the discussion section).

5. Despite the authors claim that the paper is reviewed by native speaker, the paper contains some small editorial errors (to start with: two times 'in' at the very beginning of the Abstract).

Response: We have contacted the professional English language editing bureau and have got a second language review. We have also tried to edit the manuscript for errors and hopefully, the reviewers should not be bothered with these in the present version of the manuscript.