Author’s response to reviews

Title: Is any job better than no job at all? Studying the relations between employment types, unemployment and subjective health in Belgium.

Authors:

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Author’s response to reviews:

Major Compulsory Revisions

1. Original comment:

Row 177: It is written "the second main objective". The first objective is not obvious from the paragraph. I strongly recommend to write two-three sentences in an own paragraph where the objectives are clearly written. If not, it must at least be obvious what the first objective is.

Author's response:

We followed the suggestion of the reviewer. The original statement was removed from the manuscript and replaced by a short paragraph focusing only on the objectives of the study (lines 161-165).

Comment: The second part of the aim is ok but can be more straightforward. No need to revise.

Response: We have tried to describe the second part of the aim in a more straightforward way (lines 163-165).

2. Original comment:

There is no information on how self-perceived general health is measured.

Author's response:

The missing information was added to the manuscript (lines 289-293).
Comment: With current writing it is not possible to determine what group "fair" belongs to.

Response: The manuscript was adapted to make it more clear that “fair” belongs to the “bad general health” group (lines 293-296).

3. Original comment:

The "Center for Epidemiological Studies Depression Scale" is used for self-rated mental health. Seven of twenty items are used in the manuscript but the rationale behind this reduction is not explained. Such explanation should be added to the manuscript.

Author's response:

The reduction from twenty to seven CESDS-items occurred because only these 7 items were included in the GGS. We have tried to make this clearer by adapting the wording in the manuscript (lines 296-298).

Comment: Ok. However could the sentence be improved by being clear about that these were used by us because they were the only available in the GGS. But no need to revise the text.

Response: The wording was adapted to make clear that the 7 CESDS-items used were the only CESDS-items available in the GGS (lines 299-301).

4. NEW REVISION REQUIRED: The counts and not only the percentage is needed for the variables in the article, e.g. exact number of individuals with low social support. This is required for the readability of the article but also for having the exact numbers.

Response: This information was added in table 1 of the manuscript, which is a new table.

5. Original comment:

Row 404-409: As in previous comment, you need to rely on your analysis. Now you are arguing about how additional variables are affecting the estimates and not about which estimate is most reliable. You must not be able to judge which one of your estimates is the most reliable one but you cannot assume that both are somewhat correct, which current phrasing are. The true estimates of the effect from different labour market positions is not affected by which variables
are in the model. It is simply a parameter value that our statistical model to the best of its ability tries to solve. How small the bias is depend on the set-up of variables in the model and the lowest bias is of course for the best set-up of variables which we cannot confirm as the true parameter remains unknown. The different model set-ups need to be validated as far as possible and there should be a good explanation. If the estimate is strongly affected by collinear variables then you need to make sure that collinear variables are not part of the analysis. That some estimates are so highly affected by the addition of household variables needs to be better taken care of in the analyses and the discussion of them. An alternative way of dealing with this is to present stratified estimates and to check whether the estimates are the same for all outcomes for the controlling variable. If not it is a sign of the household variables explaining that the effect on health from e.g. unemployment are different on individual level and it might for such reason be needed to present things differently. However, the possibility to present results on group level are often difficult to do in a trustworthy way due to small sample sizes and might not work for your study for such reason.

Author's response:

We have adapted this part of the manuscript and shifted the focus from the effect on the estimates towards the validation of our model and the effect on the relationship under study, namely that between labour market position and health (lines 392-396). Presenting stratified estimates is unfortunately not possible due to the limited size of our sample.

Comment: Ok. However, I do not agree that the sample size is too limited for stratified estimates as if I correctly interpreted the numbers in the article there are over 4377 individuals participating and no variable have less than 10% of individuals in the outcome, i.e. at least a sample of 400 for all strata and for most potential groups from strata at least over 800 individuals. Stratified estimates would be valuable even if they are not presented to verify that the added variable actually are a confounder and not an effect modifier. From stratified estimates it might be possible to get a better understanding of what is happening between model 1 and 2. I still don't think that it is necessary to present stratified estimates in the manuscript and they are also likely to be unstable for many groups.

Response: To meet the suggestion of the reviewer, we repeated the analyses using the household composition variable to stratify. The following six tables (added as supplementary documents) present the relationship between labour market position and general/mental health for three groups of respondents: those with a working partner (tables 1.1 and 2.1), those with a non-working partner (tables 1.2 and 2.2) and those without a partner (tables 1.3 and 2.3). In general, odds ratios appear to be smaller for the respondents with a non-working partner and without a partner than for the group of respondents with a working partner. Moreover, confidence intervals are quite large and almost none of the estimates for the former two groups are statistically
significant. This is mainly due to the small sizes of subgroups following the stratification according to household composition. The group of respondents reporting poor (general or mental) health is not very large and the same can be said for some labour market positions and for the household composition categories ‘respondents with a non-working partner’ and ‘respondents without a partner’. On top of this, the different labour market positions and household composition categories are clearly linked to the health outcomes. Less advantageous labour market positions and household situations are associated with poorer general/mental health. Therefore, it is difficult to have enough respondents per labour market position and household situation in the poor general/mental health category. For instance, stratified crosstables show that there are only 3 self-employed respondents with a non-working partner who report poor mental health. For these reasons, we have decided to leave the stratified analyses out of the manuscript.

Minor Essential Revisions

6. Original comment:

There is no information about the number of invited in the GGS (only that 7163 participated). This information should be added.

Author's response:

The information about the number of individuals that were invited to participate in the Belgian GGS was added to the manuscript (lines 176-179).

Comment: The information is now in the manuscript but the sentence needs to be improved as it not complete and also starts with a number.

Response: The sentence was adapted (lines 176-179).

7. Original comment:

Material deprivation and perceived financial situation are likely to measure very similar things. Please explain how they could both be used in your analysis. It feels obvious that there is a collinearity problem between variables.

Author's response:

Table 3 shows the bivariate correlations between the five social situation indicators.
The highest correlation is indeed seen between the material deprivation variable and the financial situation variable. Because of this, we have checked what happens to the model if we leave out either material deprivation or financial situation. The results from these analyses are presented in tables 4 (general health) and 5 (mental health). From these results, it is clear that including material deprivation in a model which already contains financial situation is not very useful. The models containing only the financial situation are almost identical to the model containing both financial situation and material deprivation. Therefore, we have decided to leave the material deprivation variable out of the analyses presented in the manuscript.

Table 3. Bivariate Pearson Correlations between the five social situation indicators

Table 4. Results for three different models relating labour market positions to poor selfperceived general health

Table 5. Results for three different models relating labour market positions to poor selfrated mental health

Comment: The tables are missing in the response but I trust that they are showing what the authors are explaining. The information about testing material deprivation is highly relevant for the manuscript and it should be added when mentioning the selection of variables.

Response: The information about material deprivation and the reason for leaving it out of the final analyses was added to the manuscript (lines 277-281).

8. Original comment:

The social support cut-off value surprises me. From the description, the scale seems to be from 0-60 (6 items with scale 0-10). Is it really as few as 10% that scores 5 or lower or have I misunderstood the calculation of scores for social support? Even if this is correct it needs to be specified what a high score corresponds to. It is confusing that the first question (availability of someone to talk with) is indicating a positive situation while the second question (emptiness) is indicating a negative situation. Please clarify how the score for this measurement is calculated and also check if the cut-off value is correct.

Author's response:

The procedure for the construction of the social support indicator is clarified here and also in the corresponding part of the manuscript (lines 280-286). First of all, three of the six original items were recoded because they indicate a positive situation instead of a negative situation such as the remaining three items. Then, the six items were summed and the resulting scale ranging from 0 until 6 was transformed in a scale ranging from 0 until 10. We agree with the reviewer that the
original cut-off - isolating 10% of the sample - is rather strict. Therefore, a new cut-off value of 7 (out of 10) was applied to dichotomise the social support scale. The adapted cut-off value means that 20.2% of the sample is now classified as reporting 'low social support'.

Comment: In the text it is unclear how the score is calculated. It is only said that it ranges between 0 and 10. If this score is calculated in the same way as the original article then it is sufficient to mention this. It would also be valuable if it is clarified for the user why current cut-off is chosen.

Response: The text was adapted to meet the comments of the reviewer (lines 285-289).

Discretionary Revisions

9. NEW REVISION: The manuscript would be much improved if a demographic table was added. The extra work for trying to find this important information through reading the text would be good to avoid for the reader. As I have added it as a discretionary revision should this only be considered as an advice and not as something required to do.

Response: A new table (table 1) was added to the manuscript. This table contains information about the variables included in the analyses.