Author’s response to reviews

Title: Living alone and mortality among older people in Västerbotten County in Sweden: a survey and register-based longitudinal study

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Living alone and mortality among older people in Västerbotten County in Sweden: a survey and register-based longitudinal study
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Response to the editor’s and reviewers’ comments

Editor Comments:

Comment: The introduction has to be improved. The authors mention inconsistency in the literature in the first sentence of the abstract. However, this is not further explained in the Introduction.

Response: We thank the editor for this comment. We have updated the Abstract and rewritten the Introduction to make its descriptions more coherent.

Comment: It is not clear what this study adds to the literature. What is essential here? The case of Sweden or a general mechanism regarding social isolation vs. mortality. Please improve the argument. Why is this study needed?

Response: In the revised Introduction, we have presented the added value of our research to the literature. As Sweden has the highest proportion of households being a single household in Europe, the Swedish case can inform other European countries which are going to experience the same trend. Moreover, using the rich longitudinal register data in Sweden, we can control for many potential confounders which are not available in other studies.
Reviewer reports:

Jussi Tanskanen (Reviewer 1)

Comment: The manuscript "Living alone and mortality among older people in Västerbotten County in Sweden: a survey and register-based longitudinal study" is mostly well written paper focusing a current and important topic. The study is based on a big and good sample, which is one of the many strengths of the paper. There are however some concerns relating to the study and the manuscript needs to be developed.

Response: We thank Dr Tanskanen for the appreciation of our study, as well as the constructive comments for us to improve our paper. We have tried our best to address the specific concerns in the revised manuscript.

Comment: The introduction needs to be strengthened regarding the health effects of living alone. Now the paper discusses only about the health problems related to loneliness and social isolation and only kind of implicitly hints that living alone is connected to loneliness and social isolation. This link needs to be grounded better theoretically and empirically.

Response: In accordance with the reviewer’s and the editor’s comment about the introduction, we have strengthened the Introduction section and present it in more coherently. We have now explicitly described the linkage between living alone, loneliness, and social isolation on poor health outcomes and survival. We have briefly described the non-health impacts of living alone.

Comment: The procedure of obtaining the data needs to be clarified. What measures were obtained from VIP data and what from the register data? If the measures of living alone and confounders were obtained from register data as you state ("we extracted their socioeconomic, living arrangement, and death data from the Linnaeus database") would it be possible to utilize follow-up data on these instead of only baseline information. If only baseline measurement for living alone was utilized, then the limitation of the study design should be discussed as it is possible that living arrangement can change in the long follow-up time.

Comment: There are some issues in the measurement section that need rewriting. First, this definition is hard to understand: "(ii) married (or cohabitant with common children) living without children at home".

Response: We have now rewritten the working definition of living alone variable. The revised text reads as the following, which we hope is clearer.

“We categorised the ‘living alone’ variable into four categories of (i) married or cohabited with children living at home, (ii) married or cohabited without children living at home, (iii) single parent with children living at home, and (iv) single parent without children living at home. We defined the participants as ‘living alone’ if they belonged to the last category.”
Comment: In addition, the age of the participants is unclear. First, the paper mentions that participants were aged 50 or 60 when entering to the study, but later the paper defines "age group (40-49, 50-59,…, 80+ in the first study and 40, 50, 60 years in the second study). Also, the second study is mentioned here for the first time. The number of the studies needs to be addressed in the introduction.

Response: We thank the reviewer for spotting our mistake in defining the age groups of the participants. We confirm that we only report one study in this paper. We have now revised the text.

“Socio-demographic variables included were: sex (men and women); age group (40, 50, 60 years); and education level (basic, middle, and high education).”

Comment: Acronyms need to be defined in the text: "Individuals who responded positively to at least two of the CAGE questions were categorised as having a risk of alcohol dependency [19]."

Response: The CAGE is an acronym of four questions in the CAGE questionnaire. We have tried to describe what CAGE questionnaire measures, as a way to define the acronym.

“CAGE is the acronym of the four questions in the CAGE questionnaire, which measures if the respondents: had felt the need to Cut down on drinking, had been Annoyed by people who criticised the drinking, had ever felt Guilt about drinking, and had felt the need to drink first thing in the morning as Eyeopener.”

Comment: Why was factor analysis conducted with chronic disease risk factors? Paper needs to clarify what excess value stems from the factor analysis comparing to modeling risk factors separately and how the limits of low (the lowest 60%) and high (top 40%) load were decided. Have you considered the possibility that some of the chronic disease risk factors are on the causal pathway between living alone and mortality?

Response: Even though it is quite a common practice to model risk factors as separate variables in regression analysis, it can lead to multicollinearity as the risk factors (for example physical inactivity, obesity and hypertension) are related to each other. We, therefore, argue for the importance to reduce the dimension of the risk factor variables before including them in the regression analysis. We have added that the limits of 60% lowest vs 40% highest were chosen arbitrarily, which was reasonable to differentiate the population with low- vs high-level of risk factor loads.

We have added the following sentence in the beginning of the Statistical Analysis section to motivate the choice of conducting factor analysis.

“As many of the chronic disease risk factors are related to each other, the inclusion of the factors as separate variables in a multivariable regression analysis will cause the problem of multicollinearity. We, therefore, performed a multivariate analysis to reduce the dimension of the risk factor data.”
We fully agree with Dr Tanskanen that some of the risk factors could mediate the causal association between living alone and mortality. Unfortunately, the linked data we have access to does not allow us to ascertain the temporal association between living alone and risk factors as the information was linked on the same year. We have now acknowledged this as a limitation in our study.

“This study did not focus on exploring the pathway between living alone and mortality, for example, if individuals who live alone are more prone to adopt certain lifestyle behaviours, or might have more medical conditions, poorer physical and cognitive functions, which in turn increase their mortality risk. Understanding of the mechanisms of living alone and mortality will allow tailoring intervention strategies to prevent the negative impacts of living alone among the older population.”

Comment: It would be good to have some kind of information about how to study population represent the whole population. The percentages of those who are in partnership with children at home (40% men, and 30% women) at the age of 50 or 60 seems a bit high.

Response: As Sweden is one of the countries with the highest singulated mean age at marriage, it is not uncommon for individuals aged 50 (mainly) or 60 to have children living at home. In 2016, the mean age at marriage in Sweden was as high as 33 years old for men and 31 years old for women as shown in the Figure below.

We understand the concern of Dr Tanskanen on the representativeness of our study population. In our primary analysis, we had included the VIP participants (aged 50 and 60) and followed them until death or censored in 2015. We have now conducted a sensitivity analysis by analysing all the population aged 50+ in Västerbotten County (as shown in Appendix 3), and we observed quite similar results when compared to the primary VIP analysis. Therefore, we are quite sure that our results are robust and could be generalised to the county population.


Tjalling Jan Holwerda (Reviewer 2):

Comment: A very interesting study, with a large number of participants. Thank you for giving me the opportunity to read this study. There are some points which need attention

Response: We thank Dr Jan Holwerda for the appreciation on our study, as well as the constructive comments for us to improve our paper.

Comment: Page 3: The findings about Sweden need replication, it has been written to strong

Response: In accordance to the reviewer’s and the editor’s comment about the need to improve the Introduction, we have strengthened the Introduction section and present it in a more coherent
manner. Some of the findings from the previous studies in Sweden, which are not directly related to this paper, have now been removed.

Comment: Page 7: The table is unclear, I would suggest a table with Living alone HR, Correction for Age HR, Employment status HR, Education HR, Cardiovascular risk HR

Response: As per Dr Holwerda’s suggestion, we have moved the presentation of living arrangement variable to the first part of the table.

Comment: Page 10: The authors were not able to adjust for cognitive functioning and other medical diseases such as cancer and depression which account also for a large mortality in the western world. Also network size and physical functioning were not adjusted for. These points need to be addressed in the discussion

Response: We acknowledge the point raised by Dr Holwerda as one of the limitations in our study. We have added a few sentences in the Discussion section as the following.

“This study did not focus on exploring the pathway between living alone and mortality, for example, if individuals who live alone are more prone to adopt certain lifestyle behaviours, or might have more medical conditions, poorer physical and cognitive functions, which in turn increase their mortality risk. Understanding of the mechanisms of living alone and mortality will allow tailoring intervention strategies to prevent the negative impacts of living alone among the older population.”