

## PEER REVIEW HISTORY

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### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Non-response in a nationwide follow-up postal survey in Finland: A register-based mortality analysis of respondents and non-respondents of the Health and Social Support (HeSSup) Study
<b>AUTHORS</b>	Sakari Suominen, Karoliina Koskenvuo, Lauri Sillanmaki, Jussi Vahtera, Mika Kivimaki, Kari Mattila, Pekka Virtanen, Markku Sumanen, Paivi Rautava and Markku Koskenvuo

### VERSION 1 - REVIEW

<b>REVIEWER</b>	Jørn Olsen, Professor, Department of Public Health, University of Aarhus, Denmark.  No conflicts of interest.
<b>REVIEW RETURNED</b>	09/12/2011

<b>THE STUDY</b>	They may be right that the topic is not well published, although rather well studied. There are two key publications missing from "Epidemiology". The paper by Nohr et al and a paper by Green et al from this year
<b>RESULTS &amp; CONCLUSIONS</b>	Some of the prominent conclusions are not justified. Abstract - conclusion - this study does not address whether postal survey can be considered valid or not. The difference in mortality was not small - perhaps of the same magnitude as for smoking? It would be nice to get results on mortality among the late responders.

<b>REVIEWER</b>	Professor Fjalar Finnäs Åbo Akademi University Vasa Finland
<b>REVIEW RETURNED</b>	12/12/2011

<b>RESULTS &amp; CONCLUSIONS</b>	The paper deals with an important question namely potential bias in (postal) surveys due to non-response. The study is a retrospective seven year follow-up of 64797 persons in high quality Finnish population registers. The 1174 deaths were classified in two categories, external causes and diseases, according to cause of death. Thus the authors have an extensive data set and they calculate Cox proportional hazards regressions, and the results are clearly presented in three tables (2-4). However, in my opinion the presentation and discussion of the findings are not very elegant, focusing too much on statistical significance. In my opinion the estimates show a very clear pattern with higher death risks among non-respondents than among respondents. Whether the estimates are statistically significant or not is mainly a matter of number of deaths and observations. As a result the authors also comment on
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	<p>the results for Total age and both sexes combined, because in this case the parameters are statistically significant thanks to the great number of observations. I think that the results for both sexes combined are of very little interest since in practice men and women are mostly studied separately.</p> <p>My conclusion is that these empirical results are important because they show that there are indeed differences in mortality rates between respondents and non-respondents. The results are indisputable, and since there is no additional information to cling to anyone may interpret them in their own way. Whether the differences should be considered big or small is very much a matter of taste, and we can only speculate about the consequences for other covariates when analysing survey data. Evidently the authors want to interpret the results in a very positive way to justify future postal surveys. They write, "the associations between the variables studied are not necessarily biased". This is certainly true and I do not find the results or this conclusion alarming, but I would be inclined to warn for the evident deficiencies as well.</p> <p>Due to these comments I marked Minor Revision in the Recommendation field, but I can accept a publication as well. I do not think that there are errors or ambiguities in the text, but to some extent it can be improved.</p>
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### VERSION 1 – AUTHOR RESPONSE

Reviewer: Jørn Olsen, Professor, Department of Public Health, University of Aarhus, Denmark.

No conflicts of interest.

We thank you for your constructive criticism.

They may be right that the topic is not well published, although rather well studied. There are two key publications missing from "Epidemiology". The paper by Nohr et al and a paper by Green et al from this year

We found the paper from Greene et al 2011 in the journal Epidemiology but we are sorry to have to inform that we failed to find the paper Nohr et al from that particular year. We came across a paper by Nohr et al from 2006 from Epidemiology where the conclusion was that female participants of the Danish National Birth Cohort (DNBC) were non-biased in terms of 1) in vitro fertilization and preterm birth, 2) smoking during pregnancy and birth of a small-for-gestational-age infant, and 3) prepregnancy body mass index and antepartum stillbirth in comparison with nonparticipants. However, we also came across a study by Jacobsen et al 2010 from European Journal of Epidemiology where Ellen Aagaard Nohr is included as co-author. This latter study shows that groups with low socioeconomic resources in terms of education, occupation, income and civil status are underrepresented in the DNBC compared to the background population. This observation on bias has already been substantiated with e.g. references Jooste et al 1990 and our previous non-response analysis Korkeila et al 2001. In the previously mentioned study by Greene et al 2011 the authors conclude that bias from loss to follow-up in a lifecourse cohort study may be quite modest for medical factors whereas for behavioral factors it may be large. In particular, maternal smoking appeared strongly related to loss and outcome and hence we decided to replace the previous reference number 15 Mattila et al 2007 with this reference since the higher rate of mortality among non-respondents is already substantiated with reference number 14 Ferrie et al 2009 and hence the previous number 15 is not necessarily needed anymore. Moreover we have cited the study Greene et al 2011 once more

on page 8 where the influence of health selection on the estimates of association between the variables studied is commented.

Some of the prominent conclusions are not justified. Abstract - conclusion - this study does not address whether postal survey can be considered valid or not.

We agree with the referee and the conclusion in the Abstract as well as in the ms have been revised accordingly by removing all statements relating to the general validity of postal surveys.

The difference in mortality was not small - perhaps of the same magnitude as for smoking?

This is in the last end a question of how to interpret the numerical results. However, with such a large data set statistically significant differences are rather easily achieved. We would like to point out that in spite of the great number of observations many cells in the Tables still fail to show significant differences although this applies mostly to the younger age groups. We have decided to replace the expression moderate with the more neutral formulation 1.5 - 2 fold.

It would be nice to get results on mortality among the late responders.

We carried out a separate but in other respect identical mortality analysis for early and late respondents who were defined as in the previous non-response analysis (Korkeila et al 2001) of this data set, i.e. early respondents reacted before the reminder was sent out which took place approximately 10 weeks after the initiation of the survey. In this way early respondents comprised 18,737 individuals and the late respondents 6,553 individuals. The late respondents showed, as expected, as high or higher mortality rates throughout all the causes of death examined compared to the results from the whole data set. However, since the total number of observations in this group was considerably lower than in the whole data set a number of significancies of the detected differences were lost. Consistently the early respondents showed in average somewhat lower mortality rates as compared to the total data set but principally the results in both of these groups were in accordance with the results of the whole data set. Naturally, we are willing to provide more details in forms of Tables if so is wished.

Reviewer: Professor Fjalar Finnäs  
Åbo Akademi University  
Vasa  
Finland

We thank you for your constructive criticism.

The paper deals with an important question namely potential bias in (postal) surveys due to non-response. The study is a retrospective seven year follow-up of 64797 persons in high quality Finnish population registers. The 1174 deaths were classified in two categories, external causes and diseases, according to cause of death. Thus the authors have an extensive data set and they calculate Cox proportional hazards regressions, and the results are clearly presented in three tables (2-4). However, in my opinion the presentation and discussion of the findings are not very elegant, focusing too much on statistical significance. In my opinion the estimates show a very clear pattern with higher death risks among non-respondents than among respondents. Whether the estimates are statistically significant or not is mainly a matter of number of deaths and observations. As a result the authors also comment on the results for Total age and both sexes combined, because in this case the parameters are statistically significant thanks to the great number of observations. I think that the results for both sexes combined are of very little interest since in practice men and women are mostly studied separately.

We understand the point raised by the referee but have still chosen to include the results for both sexes combined in the ms, since if these data were omitted a potential reader might still wish to see them. However, in accordance with the referee's comment we have added a statement of consistency of the results to the Key message, to the beginning of the Discussion and the Conclusions.

My conclusion is that these empirical results are important because they show that there are indeed differences in mortality rates between respondents and non-respondents. The results are indisputable, and since there is no additional information to cling to anyone may interpret them in their own way. Whether the differences should be considered big or small is very much a matter of taste, and we can only speculate about the consequences for other covariates when analysing survey data. Evidently the authors want to interpret the results in a very positive way to justify future postal surveys. They write, "the associations between the variables studied are not necessarily biased". This is certainly true and I do not find the results or this conclusion alarming, but I would be inclined to warn for the evident deficiencies as well.

In accordance with the comments given by the reviewer and the second reviewer we have removed the expressions related to the magnitude of the findings and have replaced them with the more neutral expression of 1.5 - 2 fold higher mortality among non-respondents. We have also removed all conclusions related to the validity of the survey method, since they can't directly be drawn from this particular study.

Due to these comments I marked Minor Revision in the Recommendation field, but I can accept a publication as well. I do not think that there are errors or ambiguities in the text, but to some extent it can be improved.

#### VERSION 2 – REVIEW

<b>REVIEWER</b>	Professor Jørn Olsen Aarhus University Denmark  I have no competing interests.
<b>REVIEW RETURNED</b>	16/01/2012

<b>GENERAL COMMENTS</b>	The literature is still incomplete.  If the authors want to stress differences in non-responding related to gender, they should address this in their statistical analyses. My guess is that the gender differences do not reach statistical significance.  The conclusion in the abstract does not follow the results they describe.  The authors should take absolute differences into consideration.
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#### VERSION 2 – AUTHOR RESPONSE

Reviewer: Professor Jørn Olsen  
Aarhus University  
Denmark

We thank you for additional comments.

The literature is still incomplete.

The referee is somewhat unspecific here. As we in our previous response stated, we were able to find the reference Greene et. al. which was published in 'Epidemiology' in 2011 but are still unable to locate the reference Nohr et al which should have come out in the same journal that same year. We are aware of the fact that our list of references is not complete, i.e. in a research article it is impossible to include each and every relevant study in the review of literature and hence the final reference list always represents a kind of compromise. However, we extended our search of literature and came across a short report, Batty and Gale from 2009 which was published in Journal of Epidemiology and Community Health and dealt with the resurvey participation activity and its association with cardiovascular (CV) mortality. The point in this article was that although the resurvey non-participants showed a higher CV mortality the association between a number of risk factors and the outcome measure was unbiased in relation to resurvey participation. Thus, we find this article, although very informative, not optimal for being cited in our present study.

We also came across another interesting study by Harald et al from the same journal and the same year as above which concluded that non-response in a health survey is associated with higher mortality as compared to responders. However, the main focus of this article is according to our view that the mortality gradient between low and high socioeconomic groups was not clearly related to response status.

We are aware of several studies conducted in the eighties dealing with participation status and mortality as e.g. Rosengren et al and Walker and Cook, both from the year 1987 but due to the format of a research article we decided to include mainly more recent studies. However, we are happy to include any relevant study that according to the referee is considered necessary to be cited as e.g. Harald et al 2009 and/or Nohr et al 2006 but a more detailed comment would here be of great help.

If the authors want to stress differences in non-responding related to gender, they should address this in their statistical analyses. My guess is that the gender differences do not reach statistical significance.

We are unsure here about whether the referee's point is raised by the Introduction where we cite studies indicating that women tend to respond to surveys more actively than men, e.g. Jacobsen et al 1988 and Rönmark et al 1999? These studies were mainly included in order to grasp the total picture but this was not the central point we were aiming at in our own empirical analyses, i.e. the focus was not intended to be on gender differences in mortality according to response status but more on the influence of non-response as a whole. During the previous round of review the second referee indicated that only gender-specific results could be given but we found that it still might be useful to keep the results for both genders combined.

Or does the referee in his comment now refer to the gender specific mortality in itself regardless of response status? However, we have made additional statistical analyses that should cover both of these questions and the results can be condensed as follows:

For mortality of external causes a gender and response status interaction term was insignificant in the statistical analyses for all age groups separately and combined. However, in separate analyses without the interaction term mentioned above, the mortality for men was significantly higher ( $p < 0.001$ )

in all age groups separately and combined.

For mortality of diseases a gender and response status interaction term was highly significant ( $p=0.0003$ ) for all age groups combined and for the oldest age group (initially 50-54 years,  $p=0.0016$ ). In the following analyses without the interaction term men showed an almost significantly ( $p=0.0544$ ) higher mortality in the age group 20-24 years and a highly significant ( $p<0.001$ ) surplus mortality in the age groups 40-44 and 50-54 years as compared to women as well as when all age groups were combined. The interpretation of the results related to the interaction term analyses indicated that non-response had a significantly higher mortality increasing effect on non responding women when all age groups were combined as compared to men.

According to the analyses for total mortality a gender and response status interaction term was significant ( $p=0.0056$ ) when all age groups were combined and in separate analyses for the age group of 50-54 years at baseline ( $p=0.0092$ ). In the following analyses without the interaction term men showed a very significantly ( $p<0.001$ ) higher mortality in all age groups combined and when analyzed separately. As above, the interpretation of the results related to the interaction term analyses indicated that non-response had a significantly higher mortality increasing effect on non responding women when all age groups were combined as compared to men.

Based on these results, we find it justified also to present gender-specific results. Naturally, we are prepared to deliver more detailed results of these additional analyses as Tables.

The conclusion in the abstract does not follow the results they describe.

We agree with the referee and have removed this statement from the Abstract and the Keypoint.

The authors should take absolute differences into consideration.

Here we are not quite sure about what the referee means. The hazard ratio of approximately 2 for greater mortality among non-respondents as compared to respondents means already in this population sample circa three hundred extra deaths during the follow-up of seven years. We have added a sentence about this in the Discussion.

We hope these additional clarifications and edits are regarded as sufficient or further specifications are provided.

On behalf of the research group

Yours sincerely,

Sakari Suominen