PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Non-response in a cross-sectional study of respiratory health in Norway
AUTHORS	Abrahamsen, Regine; Svendsen, Martin; Henneberger, Paul; Gundersen, Gølin; Torén, Kjell; Kongerud, Johny; Fell, Anne-Kristin

VERSION 1 - REVIEW

REVIEWER	Linsay Gray MRC/CSO Social and Public Health Sciences Unit at the University of Glasgow, UK
REVIEW RETURNED	23-Sep-2015

GENERAL COMMENTS	This paper seeks to identify possible causes and effects of non-response in an Norwegian population-based respiratory health study. In general, the work has been suitably analysed and is reasonably presented. I have one major comment and various minor comments: Major comment 1. Both the identification of and adjustment for non-participation rely on a “re-contact sample”. With just 260 individuals constituting a 37% response in the re-contact sample, the validity of using those as a standard reference is uncertain since the assumption that the results from those participating in the recontact sample are representative of all those who did not participate in the original study is implausible. More emphasis should be placed on this considerable limitation. Minor comments Abstract 1. The abstract currently lacks detail on statistical methods – please address. 2. Attempt has been made to assess the respective significance of current smoking and exposure to vapour, gas, dust or fumes at work as risk factors for respiratory symptoms separately for responders and non-responders. Given the vastly different sample sizes between responder and non-responders, the power to detect associations is imbalanced. Can the test of homogeneity be mentioned here? 3. Clarify that the “study” is the original study. (Conclusion section) 4. Here and in the introduction, “survey effort” is an ambiguous term – please address. (Conclusion section) Strengths and limitations 5. The original study has been designed to yield a random sample. However, it is misleading to refer to the sample as random given the non-response – please address.

	Introduction 6. “Trends” is ambiguous here – please address. (Sentence 2) 7. The literature on assessment of non-response could be given broader coverage. 8. The text on non-responders/early/late responders is mixed up – please resolve. (Paragraph 2) Methods 9. “Registration of ” in relation to physician-diagnosed asthma etc. is unclear – please address. (pg 4) 10. Figure 1 should be reconfigured to allow the text to be enlarged. 11. The timing of the lottery conduct in relation to the stages of the study conduct is unclear – please address. (pg 5) 12. More detail and clarity on the statistical analyses section is merited. 13. Explicitly describe the process for weighting the estimates here in addition to the information provided on pg 12. Results 14. The term “baseline study” is misleading and the phrasing of response in relation to non-responders is confusing – it would be helpful to make a distinction systematically throughout the paper between the initial survey and the non-responder survey for clarity. 15. Table 1: For completeness and clarity, you could add percentage figures to the last column. 16. There is a mismatch in the percentage figure for current smoking among non-responders in the commentary and table 2. 17. There is an apparent contradiction in the statements made in relation to occupation classification by responder status - please clarify. (pg 11) 18. Table 3: It is confusing to use the labels LR1 and LR2 to mean “all responders after the first reminder” and “all responders after the second reminder” – it would be clearer to label them “ER + LR1” and “ER + LR1 + LR2”, respectively. 19. The term “Calculated” would be better replaced with “Weighted”. 20. Table 3: There is a mistake in the commentary - occupational exposure to VGDF is not associated with any of the respiratory symptoms among the non-responders – please resolve. 21. The commentary on pages 12 to 13 should read “With this correction, the calculated odds ratios for occupational exposure to VGDF as a risk factor for respiratory symptoms were HIGHER compared to the odds ratios in the baseline study (LR2) (Table 3). On the contrary, the calculated odds ratios of current smoking on productive- and chronic cough were LOWER compared to the odds ratios in the baseline study except for wheezing last 12 months.” – please resolve. Discussion 22. Given the location of the study in Norway, it could be possible to track and follow up both respondents and non-respondents via register-based record-linkage. This may be a more effective means of assessing and addressing non-participation and should be discussed. General 23. Typo – “retuning” should be “returning” (pg 4)

REVIEWER	Punam Pahwa University of Saskatchewan, Canada
REVIEW RETURNED	30-Sep-2015

GENERAL COMMENTS	It's an important topic to investigate. There are some concerns/changes to be addressed before publication: Table 1: It's not clear what statistical test was used to compare mean age in Table 1 and it is also not clear what groups were compared for mean age. I think, authors can include additional tables to justify the following text in the manuscript: Page 10. Section on Causes for non-response: There is no Table for the results discussed under this section. Page 11. Section on Early responders versus Late and non-responders: There is no table for the results explained under this section Page 12. Section on Multivariable relationships: There is no table for the results explained under this section. I have several comments related to Table 3: Table 3. What is the reference category when authors are presenting odds ratios and 95% CI for current smoking. How it is possible to adjust for smoking habits (please see the heading of Table 3) when you are computing odds ratios for current smoking? Table 3. What's the reference category when you are presenting odds ratios and 95% CI for VGDF? Interpretation of odds ratio for Table 3 need more explanation. In Table 3, Column 'Homogeneity between LR2/NR' : Why homogeneity of odds ratios was tested only for LR2 and NR and not among all fours groups (ER, LR1, LR2 and NR). Table 3: It's not clear how the last column 'Calculated* ' was computed. Weights used to compute this needs more explanation. Is there any reference where the underlying concept of calculation and use of weights has been explained? If yes, please provide that reference. Page 12 Computation of weights need more explanation as explained below: - Computation of weight for all those who provided data :

	total number of people who provided data (=16359) was the numerator and total population (=50000) was the denominator. How would you interpret this calculated weight? - Computation of weight for non-responders: total number of non-responders (=33901) was the numerator and non-responders (=260) who provided data was the denominator. Compared to the above weight calculations (for responders) this weight calculation makes more sense and can be interpreted that each of the 260 non-responders represents other 130 (weight 130.39) non- responders. Figure 1: In the flow chart - Highlight the boxes of Responders 194 subjects and Responders 66 subjects that will explain how you obtained number 260 non-responders who provided data. Minor: spelling error Page 4, section on 'Design of the baseline study' , first line – change 'envelope for retuning ..' to 'envelope for returning'

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: Linsay Gray
Institution and Country: MRC/CSO Social and Public Health Sciences Unit at the University of Glasgow, UK

Please leave your comments for the authors below

This paper seeks to identify possible causes and effects of non-response in an Norwegian population-based respiratory health study. In general, the work has been suitably analysed and is reasonably presented. I have one major comment and various minor comments:

Major comment
1. Both the identification of and adjustment for non-participation rely on a "re-contact sample". With just 260 individuals constituting a 37% response in the re-contact sample, the validity of using those as a standard reference is uncertain since the assumption that the results from those participating in the recontact sample are representative of all those who did not participate in the original study is implausible. More emphasis should be placed on this considerable limitation.

Response: We agree that this is an important limitation in the study. Therefore, we have added a paragraph in the discussion section where we address this considerable limitation (page 17-18). Additionally, we have added information regarding restrictions from the Regional Committees for

Medical and Health Research Ethics in Norway on how non-responders may be contacted. More information on statistical analyses has also been added to address this limitation.

Minor comments

Abstract
1. The abstract currently lacks detail on statistical methods – please address.

Response: We have added a section on outcome measures with some more details on weighting and test of homogeneity in the revised manuscript.

2. Attempt has been made to assess the respective significance of current smoking and exposure to vapour, gas, dust or fumes at work as risk factors for respiratory symptoms separately for responders and non-responders. Given the vastly different sample sizes between responder and non-responders, the power to detect associations is imbalanced. Can the test of homogeneity be mentioned here?

Response: Thank you for this suggestion. We have added a sentence in the abstract describing that a test of homogeneity among responders and among responders versus non-responders were done.

3. Clarify that the “study” is the original study. (Conclusion section)

Response: We have changed the term “study” to the Telemark study to make a clear distinction between the initial study and the non-responder study. Hopefully this distinction between the studies continued throughout the paper will make it easier to interpret the results.

4. Here and in the introduction, “survey effort” is an ambiguous term – please address. (Conclusion section)

Response: Thank you for pointing this out. We have changed the term “survey effort” with reminding letters.

Strengths and limitations
5. The original study has been designed to yield a random sample. However, it is misleading to refer to the sample as random given the non-response – please address.

Response: We have removed the term “random” from the sentence and describe the Telemark study as a large sample of the general population of Telemark instead.

Introduction
6. “Trends” is ambiguous here – please address. (Sentence 2)

Response: We have made some changes in the introduction and removed the term “trend” to clarify.

7. The literature on assessment of non-response could be given broader coverage.

Response: The introduction is changed in order to add new references to give broader coverage of the literature on assessment of non-response. Therefore, the entire introduction is now marked as altered although some parts are the same as in the previous manuscript.

8. The text on non-responders/early/late responders is mixed up – please resolve. (Paragraph 2)

Response: The mix-up of non-responders/early/late responders is corrected in the introduction.

Methods

9. “Registration of ” in relation to physician-diagnosed asthma etc. is unclear – please address. (pg 4)

Response: We have changed this sentence to make this clearer.

10. Figure 1 should be reconfigured to allow the text to be enlarged.

Response: Figure 1 has been enlarged. During the revision, a mistake in the numbers for Figure 1 was found. This has been corrected.

11. The timing of the lottery conduct in relation to the stages of the study conduct is unclear – please address. (pg 5)

Response: This relevant information has now been added to the paper as follows; “To increase the response rate, a lottery with a financial incentive was conducted after the inclusion period stopped”.

12. More detail and clarity on the statistical analyses section is merited.

Response: We have now included more details regarding the statistical analyses section in the article.

13. Explicitly describe the process for weighting the estimates here in addition to the information provided on pg 12.

Response: We have described the process for weighting the estimates in more detail under the statistical analyses and also added a reference for this method.

Results

14. The term “baseline study” is misleading and the phrasing of response in relation to non-responders is confusing – it would be helpful to make a distinction systematically throughout the paper between the initial survey and the non-responder survey for clarity.

Response: We have changed the term “baseline study” with the Telemark study throughout the paper. Hopefully, this will make the distinction between the initial survey and the non-responders survey clearer.

15. Table 1: For completeness and clarity, you could add percentage figures to the last column.

Response: The percentage figures have been added to table 1.

16. There is a mismatch in the percentage figure for current smoking among non-responders in the commentary and table 2.

Response: Thank you for pointing this out. We have now corrected this mistake.

17. There is an apparent contradiction in the statements made in relation to occupation classification by responder status - please clarify. (pg 11)

Response: We have corrected this in the paper.

18. Table 3: It is confusing to use the labels LR1 and LR2 to mean “all responders after the first reminder” and “all responders after the second reminder” – it would be clearer to label them “ER + LR1” and “ER + LR1 + LR2”, respectively.

Response: Thank you for this suggestion. We have changed the lables in the table as suggested.

19. The term “Calculated” would be better replaced with “Weighted”.

Response: We have changed the term calculated with weighted in Table 3 as suggested.

20. Table 3: There is a mistake in the commentary - occupational exposure to VGDF is not associated with any of the respiratory symptoms among the non-responders – please resolve.

Response: We have now changed this and written that only current smoking was identified as a risk factor for productive- and chronic cough among the non-responders.

21. The commentary on pages 12 to 13 should read “With this correction, the calculated odds ratios for occupational exposure to VGDF as a risk factor for respiratory symptoms were HIGHER compared to the odds ratios in the baseline study (LR2) (Table 3). On the contrary, the calculated odds ratios of current smoking on productive- and chronic cough were LOWER compared to the odds ratios in the baseline study except for wheezing last 12 months.” – please resolve.

Response: Thank you for pointing out this error, we have resolved as suggested.

Discussion

22. Given the location of the study in Norway, it could be possible to track and follow up both respondents and non-respondents via register-based record-linkage. This may be a more effective means of assessing and addressing non-participation and should be discussed.

Response: We agree that this is an important issue to address in the study. Therefore, we have added a paragraph in the discussion section where we discuss the possibility of using register-based records that are available in Norway (page 18).

General
23. Typo – “retuning” should be “returning” (pg 4)

Response: The typographic error has been corrected.

Reviewer: 2
Reviewer Name: Punam Pahwa
Institution and Country: University of Saskatchewan, Canada

Please leave your comments for the authors below Please see the attached file for comments.

It’s an important topic to investigate. There are some concerns/changes to be addressed before publication:

1. Table 1: It’s not clear what statistical test was used to compare mean age in Table 1 and it is also not clear what groups were compared for mean age.

Response: Mean age was tested by Mann-Whitney and this has been clarified in the statistical analysis.

I think, authors can include additional tables to justify the following text in the manuscript:

2. Page 10. Section on Causes for non-response: There is no Table for the results discussed under this section.

Response: We have added a table for the causes for not responding to the questionnaire.

3. Page 11. Section on Early responders versus Late and non-responders: There is no table for the results explained under this section

Response: We have added a table for the results on early responders versus late- and non-responders.

4. Page 12. Section on Multivariable relationships: There is no table for the results explained under this section.

Response: We have added a table on multivariable relationship.

I have several comments related to Table 3:

5. Table 3. What is the reference category when authors are presenting odds ratios and 95% CI for current smoking.

Response: The reference category for current smokers are never-smokers. We have added a

footnote in Table 3 to make this clearer.

6. How it is possible to adjust for smoking habits (please see the heading of Table 3) when you are computing odds ratios for current smoking?

Response: Thank you for pointing this error. We have changed the footnote in Table 3 regarding current smoking as risk factors for respiratory symptoms. We have only adjusted for age, sex and area of domicile.

7. Table 3. What's the reference category when you are presenting odds ratios and 95% CI for VGDF?

Response: The reference category for those occupational exposed to VGDF is compared to those who are not exposed to VGDF at work. We have added a footnote in Table 3 to make this clearer.

8. Interpretation of odds ratio for Table 3 need more explanation.

Response: We have added footnotes to Table 3 to make the interpretation of the results more apparent.

9. In Table 3, Column 'Homogeneity between LR2/NR' : Why homogeneity of odds ratios was tested only for LR2 and NR and not among all fours groups (ER, LR1, LR2 and NR).

Response: Thank you for this suggestion. We have now tested homogeneity of odds ratios for early responders, responders after the first reminder and responders after the second reminder. In addition, these corrections revealed that the initial tests for homogeneity between responders and non-responders were conducted with the weights included. We have now recalculated without weights and corrected those results.

10. Table 3: It's not clear how the last column 'Calculated*' was computed. Weights used to compute this needs more explanation. Is there any reference where the underlying concept of calculation and use of weights has been explained? If yes, please provide that reference.

Response: We have described the process for weighting the estimates in more detail under the statistical analyses and added a reference for this method.

11. Page 12 Computation of weights need more explanation as explained below:

- Computation of weight for all those who provided data : total number of people who provided data (=16359) was the numerator and total population (=50000) was the denominator. How would you interpret this calculated weight?

- Computation of weight for non-responders: total number of non-responders (=33901) was the numerator and non-responders (=260) who provided data was the denominator. Compared to the above weight calculations (for responders) this weight calculation makes more sense and can be interpreted that each of the 260 non-responders represents other 130 (weight 130.39) non-

responders.

Response: We have described the process for weighting the estimates in more detail under the statistical analyses and added a reference for this method and hope this now adds the necessary information regarding the method.

12. Figure 1: In the flow chart - Highlight the boxes of Responders 194 subjects and Responders 66 subjects that will explain how you obtained number 260 non-responders who provided data.

Response: Thank you for this suggestion; the boxes of responders have been highlighted and the figure has been enlarged. In addition, a mistake in the numbers for the figure was found during the revision. These numbers have been corrected.

13. Minor: spelling error
Page 4, section on 'Design of the baseline study', first line – change 'envelope for retuning..' to 'envelope for returning'

Response: The typographic error has been corrected

VERSION 2 – REVIEW

REVIEWER	Linsay Gray MRC/CSO Social and Public Health Sciences Unit at the University of Glasgow, UK
REVIEW RETURNED	18-Nov-2015

GENERAL COMMENTS	The reworking has addressed almost all the points made in the first review, with the two outstanding minor ones outlined below (numbers refer to those in the original review for ease of reference). Abstract 2. Some detail on the test of homogeneity has been provided in the Strengths and limitations section but has not been mentioned explicitly in the abstract – please resolve. Methods 10. The text in Figure 1 remains small – please resolve. 12. Further detail and clarity on the statistical analyses section is merited – for instance, state explicitly what the tests of statistical significance are comparing. Results 14. The term "baseline" (pg 10, paragraph 3) remains confusing. 17. There remains an apparent contradiction in the statements made in relation to occupation classification by responder status: 'There were no significant differences in occupational groups based on responder status (p-value 0.30).' vs 'Non-responders worked more often in "manual" or "blue-collar" occupations (ISCO 6-9) when compared to responders (30 % vs 24 %). Among the non-responders, craft and related trade workers (ISCO 6) was the most common "manual" occupation (17 %). Responders were more likely to work as technicians and associated professionals compared to

	non-responders (30 % vs 25 %).' - please clarify. (pg 11) Discussion 22. The thinking behind the suggestion of register-based record-linkage is that it offers a means to identify hospital admissions and/or death from respiratory conditions, comparing in respondents vs non-respondents. Please consider and discuss. General 23. Please review the manuscript for remaining typos (e.g. correction of the spelling of eligible)

VERSION 2 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: Linsay Gray
Institution and Country: MRC/CSO Social and Public Health Sciences Unit at the University of Glasgow, UK.

Please leave your comments for the authors below

The reworking has addressed almost all the points made in the first review, with the two outstanding minor ones outlined below (numbers refer to those in the original review for ease of reference).

Abstract
2. Some detail on the test of homogeneity has been provided in the Strengths and limitations section but has not been mentioned explicitly in the abstract – please resolve.

Response: In the abstract, we have added that the Breslow-Day test of homogeneity was conducted and that it detected heterogeneity between productive cough and occupational VGDF exposure among responders.

Methods
10. The text in Figure 1 remains small – please resolve.

Response: The text in Figure 1 has been enlarged and changed to better the quality of the figure.

12. Further detail and clarity on the statistical analyses section is merited – for instance, state explicitly what the tests of statistical significance are comparing.

Response: The statistical analysis section is updated with information regarding what tests have been used alongside specifying that a p-value <0.05 was considered significant throughout the paper.

Results
14. The term "baseline" (pg 10, paragraph 3) remains confusing.

Response: Thank you for pointing this out. We have deleted the term baseline on page 10, paragraph 3 and in the ethics section on page 6. We have also changed the term "baseline" questionnaire to "initial" questionnaire under the section "Design of the Telemark study" and "Design of the non-responder study".

17. There remains an apparent contradiction in the statements made in relation to occupation classification by responder status: 'There were no significant differences in occupational groups based on responder status (p-value 0.30).' vs 'Non-responders worked more often in "manual" or "blue-collar" occupations (ISCO 6-9) when compared to responders (30 % vs 24 %). Among the non-responders, craft and related trade workers (ISCO 6) was the most common "manual" occupation (17 %). Responders were more likely to work as technicians and associated professionals compared to non-responders (30 % vs 25 %).' - please clarify. (pg 11)

Response: Thank you for pointing this out. This contradiction has now been corrected. We have changed the sentence and clarified that "a weak trend was observed with non-responders reporting slightly more often "manual" or "blue collar" occupations when compared to responders", but that this difference in occupational groups was not statistical significant. This was also clarified in the discussion section as it could have been misinterpreted as it was stated in the last version.

Discussion

22. The thinking behind the suggestion of register-based record-linkage is that it offers a means to identify hospital admissions and/or death from respiratory conditions, comparing in respondents vs non-respondents. Please consider and discuss.

Response: We agree that using register-based record linkage to identify possible differences in hospital admissions and/or deaths from respiratory conditions between responders and non-responders could have been an interesting addition to the article. However, we do not have permission to use registers on hospital admissions and deaths in this study from the Regional Committees for Medical and Health Research Ethics and it was not included in the patient consent form that the participants signed in the Telemark study.

General

23. Please review the manuscript for remaining typos (e.g. correction of the spelling of eligible)

Response: The typographic error of eligible has been corrected two places in the manuscript and the remaining of the manuscript was reviewed for other typos .

VERSION 3 - REVIEW

REVIEWER	Linsay Gray MRC/CSO Social and Public Health Sciences Unit University of Glasgow
REVIEW RETURNED	01-Dec-2015

GENERAL COMMENTS	The further reworking has addressed the substantive points made in the previous reviews, although I remain unable to critique Figure 1 due to the low quality image. This should be resolved prior to publication.