Author's response to reviews

Title: A comparison of self reported air pollution problems and GIS-modeled levels of air pollution in people with and without chronic diseases

Authors:

Fredrik Niclas Piro (fnp@econ.no)
Christian Madsen (christian.madsen@fhi.no)
Oyvind Naess (oyvind.nass@medisin.uio.no)
Per Nafstad (per.nafstad@medisin.uio.no)
Bjorgulf Claussen (bjorgulf.claussen@medisin.uio.no)

Version: 2 Date: 3 December 2007

Author's response to reviews:

The Editor

Environmental Health

Oslo 03.12.2007

Dear Editor,

Please find enclosed our revised version of the article:

A comparison of self reported air pollution problems and GIS-modelled levels of air pollution in people with and without chronic diseases

It was sent to Environmental Health on 16.07.2007, and we received the comments from three reviewers on 12.09.2007. We were asked to resubmit our revised manuscript within 11.12.2007. We are grateful for the opportunity to resubmit our manuscript, and for the comments we received from the reviewers. We hope we have been able to respond to the comments in a satisfactory manner and that our article is now in agreement with the quality standards of Environmental Health, and therefore ready for publication.

All changes in the manuscript are marked in red text.

Below we will respond the each comment from the three reviewers.

Reviewer 1: Gerard H. Hoek

1. My major problem with the current analysis is that the positive associations between health and APP can be interpreted as overreporting but also as a causal effect. More needs to be done to make the case for overreporting. In particular,
the current analyses do not make full use of the fact that data are available on objective and subjective exposures. Table 1 suggests that there is very little relationship between health and objective exposures, which would support the authors’ argument. In addition, table 1 suggests that the difference in NO2 related to APP does not seem to differ for those who report asthma and those who do not, which would argue against overreporting.

Our response: We agree that our conclusions in the first version of the manuscript did not sufficiently investigate over-reporting compared to causal effects. We think this has become much clearer in this version. We have chosen two strategies for the argumentation that our results reflect over-reporting rather than causal effects. Firstly, on page 14 there are two new sections discussing this. Our main point is that while any association between asthma or COPD and APP may be interpreted as causal effects on health, this is very unlikely to be the case in the non-respiratory diseases that are included in our analyses. Secondly, in line with the reviewer’s point that we have not fully made use of data available at both the objective and the subjective level, we have followed what we thought was a very good idea given to us by reviewer number 3 (see comment 21). We performed a new analysis where NO2 values was the outcome, and found that no health variables were associated with NO2. In our opinion, this more or less eliminates the possibility that our associations between health and APP are due to causal air pollution effects. This is explained in the analyses section on page 9, on page 12 (last sentence in mid-paragraph), on page 12 (first sentence, bottom paragraph), on page 13 (last sentence, first paragraph), on page 2 (last sentence in the results section), on page 3 (throughout the conclusion in the abstract) and elsewhere in the text where we argue that APP is much dependent on levels of air pollution, but also many other factors (see e.g. first sentence in the abstract, bottom paragraph on page 4, on page 13 (the first section of the discussion). See also our conclusion on page 18-19.

We have included a new column in table 2 on page 25 (and table 3 on page 26) where the result from an OLS regression analysis with NO2 as the outcome is included.

2. Age classes are unclear labelled

Our response: We have tried to make this clearer in the text and in the tables. In the methods section (page 5) the following sentence is now included: “40 888 persons in five age cohorts were invited (i.e. all inhabitants in Oslo that were either: 30, 40, 45, 60 or 75 years old). This is also clearer explained in the methods section in the abstract, in the bottom-paragraph on page 7, and in tables 1 and 2.

3. Statistical analysis section unclear: suggests that health was modelled as an outcome.

Our response: We removed all unfortunate wordings that indicate that health
variables are outcomes in the analyses. In tables 2 and 3 we have specified which variable is the outcome (APP or NO2). It is also made clear on page 9 (new section under analyses), and in the last sentence on page 12.

4. Tables are large. I am not convinced that all the different scenarios in tables 2 and 3 are necessary.

Our response: We agree that some of this information was not fully justified to be reported in tables. We have, however, decided to keep the scenario from our former table 2 where some of the variables were excluded (pollution from factories, dwelling variables, depression, etc). Our justification for this is explained in the top section on page 10, results reported in the last sentence on page 12 and forwards on page 13, and discussed in the last sentence on page 15 and first section on page 16.

In the former version of our manuscript, table 3 had five columns (or scenarios). Three of them (former c, d and e) have been removed.

5. Correlation between APP and NO2 is not informative as APP is a 0/1 variable.

Our response: We have removed the sentence describing this correlation.

6. The difference between APP and NO2 could be further discussed. APP is probably a more local scale variable than NO2 so one would not expect a very good agreement.

Our response: We have allowed ourselves to use some of the reviewer’s sentences above on page 15 (bottom paragraph). Our main response to the reviewer’s comment, however, is that APP and NO2 are much dependent on one another. But APP is a constructed phenomenon of several factors, of which NO2 is only one. We have argued that disease may be one such factor, and what we thought was interesting, cf. our comment 1 above, is that many variables in our analyses were associated with APP and not NO2 (or vice versa) and that none of our health measures were associated with NO2, while they were associated with APP.

We have reformulated our objective of this article to make this point clearer. For example in our abstract: Objective: To explore various contributors to people’s reporting of self reported air pollution problems in area of living, including GIS-modelled air pollution, and to investigate whether those with respiratory or other chronic diseases tend to over-report air pollution problems, compared to healthy people.

It is also mentioned on page 13 (Discussion, first section), last paragraph on page 15 and first paragraph on page 16, and throughout our discussion on pp. 16-18 (contributors to the construct of self reported air pollution problems).
Reviewer 2: Bin Jalaludin

7. Measurement of air pollution problems. The question that the authors have used asks whether the participant has been troubled by air pollution from traffic. I am not sure whether this question is asking about the participant’s perception of traffic related air pollution levels or about the effect of traffic related air pollution on the participant’s health or both. It is very important that this is made clear because it is the central outcome measure. This issue needs to be clarified in the manuscript.

Our response: We have discussed this on several occasions in our revised manuscript. In particular on page 6 (in the APP-section), and we also feel that our discussion in the first section on page 14 is related to this. The bottom line is that we accept that the APP question is ambiguous, but we feel it is a sufficient measure in order to compare people’s perception of the local levels of air pollution with the objectively measured levels.

8. Measurement of pollution from factories. In the question that was asked, participants were asked to report whether they were troubled by pollution from factories/firewood/oil furnace, etc. In the manuscript, the authors usually only report about pollution from factories. How can the authors be certain that a response to this question was related to pollution from factories rather than from firewood or oil furnace? Also, a description should be given of what is meant by the terms firewood and oil furnaces. Again it is unclear what is meant by troubled, see comment above.

Our response: We apologise for having used an incorrect term. We translated the Norwegian questionnaire (to our best knowledge) into English and came up with the term troubled by factories/firewood/oil furnace, etc. The Norwegian Institute of Public Health has an official English version of the questionnaire that was used. In this English version the question is formulated as: Are you (in your local environment) troubled by air pollution due to wood or oil heating, factory etc. which is now used throughout the article. Like the reviewer says, this question has some of the same problems as the APP question. This is discussed on page 8 in the mid-section, in which the discussion is theoretically the same as in our comment 8 above.

We have also made two other changes based on the English version of the questionnaire. The term `bad indoor climate` has been changed to `poor indoor climate`. And the term `dwelling moisture, draught or cold` has been changed into `dwelling damp, draughts or cold` (throughout the text and in all tables).

9. I do not think that the authors have reported data on their third aim, that is, to what extent self reported and modelled air pollution are associated.

Our response: We have moderated this third aim. APP and modelled air pollution
are clearly dependent on each other, i.e. the higher the levels of air pollution, the higher the reporting of APP. This was found in our analyses, and is of course of little surprise. But they are measured on different scales, and correlation coefficients etc. is not of much use in comparing them. Our solution, which is one of the aims of this article, is to investigate what other factors than NO2 that contribute to people’s reporting of APP. In our analyses we identified several such factors, and our main interest was to find out whether disease(s) was one of them.

In the abstract, and on page 4 (bottom paragraph) we have rephrased the aim of this article to better fit what we have actually investigated, which is to explore which factors over and above GIS-modelled air pollution levels that contribute to people’s reporting of self reported air pollution problems in area of living.

10. Would it have been better to use a validated tool to measure depression? How certain are the authors that their single question on depression is adequate?

Our response: We agree with the reviewer, but we had no validated (or multiple question) measure of depression at our disposal. This is obviously a weakness and it is discussed on page 10 (first paragraph). The reason for still using this variable is justified on page 16 (top-paragraph).

11. The question on bad indoor climate is unvalidated and I am not sure exactly what it may be measuring.

Our response: Our comment is similar to comment 10 above. We would also like to refer to page 15 (mid-section) where the importance of dwelling conditions is described. We thought it was important to include some measures of indoor climate/conditions as well as NO2 which is largely an outdoor factor.

12. In the analyses, logistic regression is also conducted after removing the pollution from factories, depression and housing conditions one by one (e.g. columns c, d and e in table 3) what is the rationale for this?

Our response: We refer to comment 4 above.

13. Can the authors offer an explanation for why there was no association between air pollution problems and diabetes whereas they found an association with the other chronic diseases?

Our response: We can only speculate, but one recently published article provides some explanation. This is described on page 17 at the end of the first section (reference 34).

14. In view of the response rates, there may also be issues around potential for selection bias and this should be discussed.
Our response: There are two types of selection bias relevant to our study. The first concerns the missing respondents to the Oslo Health Study. The second concerns the respondents in our study with missing values that were excluded in the analyses.

Regarding the first, we have referred to an article discussing self-selection in the Oslo Health Study (page 5, mid-section, pointing to reference nr. 7). Regarding the second type of selection, we have discussed this on page 10 (first section under Results).

15. The question around air pollution problems relates to traffic related air pollution. The modelled air pollution proxy is NO2. There should be some discussion around why NO2 was chosen as the proxy (it is probably because it is considered that NO2 is a good marker for traffic related air pollution).

Our response: In our opinion, NO2 is widely used without explicit justification of why it is better than other modelled air pollution indicators. It appears to be common knowledge that NO2 is a good indicator in traffic related air pollution.

However, with two new references on page 6 (in the section: modelled exposure assessment) we have briefly described the adequacy of NO2. We may also mention that in a recent study of air pollution in Oslo - NO2 and PM2.5 were found to be highly correlated (Næss Ø, Piro FN, Nafstad P, Smith GD and Leyland AH: Air pollution, Social Deprivation, and Mortality. A Multilevel Cohort Study. Epidemiology 18(6): 686-694).

16. The age groups should be specified, for example, 30-39 years, etc. It is not clear to me what the five age groups are.

Our response: See our comment 2.

17. What was the response to the supplementary questionnaire?

Our response: The response was 84% (see page 7, mid-section).

18. The authors should report on the test they used to determine correlation coefficients.

Our response: The correlation coefficient is no longer reported cf. reviewer 1’s fifth comment, and our response.

19. In tables 2 and 3, the initial and full models should be described in a footnote to the tables. Also state what SRH is in the footnote. In tables 1 and 2, it may be also useful to indicate which quintiles are ‘high’ and which ones are ‘low’.

Our response: We have followed the reviewer’s instructions, but are a bit
uncertain about what the reviewer meant by writing “In tables 2 and 3, the initial and full models should be described in a footnote to the tables.” However, in table 1 we think it is now clear what is meant by the initial and full model (see the heading), and in table 2 through footnotes 2-3.

20. Please briefly describe fibromyalgia so the readers may not be familiar with this term.

Our response: We have briefly described fibromyalgia on page 4 (last sentence, and the beginning of page 5).

Reviewer 3: Veronica Vieira

21. In order to fully explore the association between self reported air pollution and GIS-modeled air pollution, the authors should repeat the statistical analysis using NO2 values as the outcome to observe the relationship between the variables and true air pollution.

Our response: Thank you, we believed this was a good idea, and we have followed your advice. This is described in detail in our comment 1 to reviewer 1.

22. More discussion of the correlation between APP and modeled NO2 is necessary.

Our response: This correlation coefficient has been removed, cf. comments 5 & 18. It is, as reviewer 1 points out, not informative as APP is a 0/1 variable.

23. The authors should provide a stronger argument for their claim of “over reporting air pollution problems” given that there is “strong association between self reported and GIS-modeled air pollution.”

Our response: Yes, we agree. This was also pointed out by reviewer 1, so please see our comment 1.

24. The word “modeled” is repeated misspelled.

Our response: We have changed this. The term “modeled” is now used. But we believe that the word “modelled” is correct since the article was written in British English. We leave the final choice of how to spell this word up to the editor.

25. Table 1 needs a clearer description for the two columns of ORs.

Our response: We assume that the reviewer is referring to table 2 (since odds ratios do not appear in table 1). We have specified in the heading that this is a logistic regression analysis, which variable is the dependent variable, and specified which column is the initial and which is the full model. This is done in table 3 as well.
26. Last sentence of paragraph 2 in Background is confusing and should be rephrased.

Our response: We have edited this sentence (which was very long). Basically, the first part of the sentence has been removed, and some new words now feature in the sentence. We now hope our message has become clearer (see page 2, last sentence of mid-paragraph).

27. The use of the word `susceptible` to report disease, etc. is awkward and should be replaced with another word, for example `likely`.

Our response: This word is no longer used in the article. We have changed it into more appropriate words or phrases.

28. The second-to-last sentence before the Study limitations section (these associations were independent) is confusing and should be rephrased.

Our response: This is now rephrased, please see bottom of page 13 and at the beginning of page 14.

After making all the changes required by the three reviewers, the manuscript has changed considerably, and on several places in the text we felt it was necessary to make some grammatical changes or adding/removing some words. All these changes that do not necessarily relate directly to any of the reviewers' comments are marked in red.

On behalf of all five authors,

Kind regards,

Fredrik Niclas Piro