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

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Author's response to reviews:

Dave Ozonoff
Editor, Environmental Health
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Dear Dr. Ozonoff.

Thank you for your e-mail of January 23, 2008, regarding our manuscript:

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

After revising our manuscript in early December, we now received the good news from you that if we could address two issues raised by one of the reviewers, an acceptance of our article would be possible.

We have addressed both concerns of the reviewer in the new version of the manuscript. We hope that the reviewer is satisfied with our clarifications and no longer see the two issues as concerns. The two issues were:

1. Dr. Jalaludin wrote: I still have a major concern regarding what is actually being measured by the question asking about air pollution problems [¿Are you (in your local environment) troubled by air pollution from traffic¿]. Is the question asking about how air pollution from traffic is affecting the respondent¿s health OR is the question asking about how the respondent rates the level of traffic related air pollution in their locality. What was the original intent of this question? My impression is that the question about APP was intended to ask about the effects on the respondent¿s health. This is crucial and central to the whole manuscript. I do not have a solution to this question. If the question is indeed asking about the effects on health, then I do not believe we can use the responses as indicating self-reported levels of ambient air pollution unless there are some validation data.
Our response: We do not believe the question is asking about the effects on health. That was certainly not the intention. In the questionnaire, this question is found under the heading: ¿Local environment/network and housing¿, following the section ¿Schoolings/Studies and Work¿. Questions directly related to health are found other places in the questionnaire. Being troubled by air pollution from traffic in the local environment was one of several items that were asked, e.g. being troubled by:

- Damp, draughts, cold in your house/apartment
- Other forms of poor indoor climate
- Noise from neighbours
- Poor drinking water
- Air pollution due to wood or oil heating, factory etc.

The intention of the question, like the others, was in our opinion not to ask about potential health effects from local features, but to ask the study participants whether they felt troubled by the presence of these features, i.e. to what extent they considered the amount/frequency/presence of such features to cause daily annoyance, irritation, disturbance, dissatisfaction, etc. From a personal point of view, I could say (which is a true real-life example) that I consider the traffic frequency in my neighbourhood very high, and that this is reflected in several ways, such as dirty windows, dust and particles entering the living-room when the windows are open, and occasionally a visual impression of ¿grey, polluted air¿. But I do not feel that this affects my health. Still, I wish the traffic frequency was lower. We believe the participants have responded to the question with the amount of traffic that they consider their local neighbourhood to have in mind.

In our revised version of the manuscript, we have added new text describing this on page 6.

2. Dr. Jalaludin wrote: The authors also conduct OLS regression analyses with modelled NO2 as the outcome (or dependent) variable. Modelled NO2 levels, as stated in the manuscript, depend on meteorology, emissions, topography and background air pollution levels. Modelled NO2 levels do not depend, I would have thought, on socio-demographic factors, APP, presence of chronic disease, and the other factors listed in Tables 2 and 3. Therefore, I do not believe it is possible to model NO2 levels in this way.

Our response: We partly agree with the reviewer, but hope our rationale for this model set-up may convince the reviewer that it is still possible to model NO2 levels in the way we did.

The purpose was to explore whether health was associated differently with APP and NO2. It proved to be. In the logistic regression analysis (where APP was the outcome) we found that all health variables (except diabetes) were significantly associated with APP. Theoretically, if APP is a good indicator of air pollution levels, then this result might indicate an association between air pollution and
health (which could be interpreted as causal air pollution effects on health). By specifying NO2 as the outcome, we were able to test the correctness of this, i.e. if APP was a good indicator of NO2 then health would be rather similarly associated with NO2 (as with APP).

In the OLS regression analysis we found that none of the health variables were associated with NO2. This is very important, because it demonstrates that APP and NO2 are related to health in very different ways, making APP an unreliable indicator of air pollution.

The reviewer is of course right, when writing that modelled NO2 levels do not depend on socio-demographic factors, APP, presence of chronic disease and other factors. However, since it is reasonable to assume that people are selected into different parts of the city due to prices in the housing market, it is not unreasonable to assume that people in the lower socioeconomic classes, or people living in socioeconomically deprived areas, will experience higher levels of NO2.

This is recently discussed in an article by three of this article’s authors: Næss Ø, Piro FN, Nafstad P, Smith GD and Leyland AH: Air Pollution, Social Deprivation, and Mortality. A Multilevel Cohort Study, Epidemiology 2007;18: 688-694. It may be problematic to estimate contextual neighbourhood effects that are independent from the people residing in them. And even for apparently exogenous factors such as aspects of the physical environment, in this case air pollution, these may be endogenous to the local communities as people have ultimately selected or been selected to live in these places. (page 693).

We agree that the model-set up is unusual. But we do not consider the direction of the estimates as a key matter. They do not attempt to indicate causal effects, they are only measuring associations. Whether or not the health variables or NO2 is on the right side of the equation is not important. For purposes of simplicity we chose to report one model with all six health variables included as independent variables, rather than one model for each health variable (then treated as outcomes and NO2 as an independent variable). By doing so, we were also able to see to what extent other variables (age, socio-demographic variables etc.) differed in their associations with APP and NO2.

We have added a brief discussion on this topic on page 10.

To further illustrate our point, but without modelling NO2 as an outcome, we performed new logistic regression analyses where the seven health variables were treated as outcomes. In these analyses (that are not shown in tables, but briefly referred to at the bottom of page 13, and the beginning of page 14) we found that:

- NO2 measured as a continuous variable was not associated with any health outcomes (while APP was, as previously found).
- NO2 measured as a categorical variable (i.e. quintiles, which we have used in the article) was not significantly associated with any health outcomes, except the
highest and second highest levels of NO2 that were significantly associated with fibromyalgia, but with the opposite direction as APP was. APP was associated with higher probability for reporting fibromyalgia (OR=1.47 (1.21 – 1.79), while NO2 was associated with lower probabilities for reporting fibromyalgia:

* NO2 fourth quintile OR=0.73 (0.55 – 0.97) p=.034
* NO2 fifth quintile (highest) OR=0.73 (0.55 – 0.97) p=0.33

This last result adds additional support for our argument that APP and NO2 are very differently related to health, but since it only applies to two NO2-quintiles in relation to one health outcome, we did not report this in our revised version of the manuscript.

If the reviewer is still uncomfortable with the modelling of NO2 as an outcome in the analyses, it is not a problem for us to remove the results from the OLS regression in tables 2 and 3 (and to have the results in table 3 replaced by estimates based on analyses where NO2 is an independent variable, and health variables are treated as outcomes). But we hope the reviewer agrees that the associations between health variables and NO2 should still be reported in the text, because they provide essential information regarding the meaning of the relationship between health and APP.

In addition to this, we have made some minor grammatical changes in the text. All changes are marked in red text.

We hope we have adequately responded to the reviewer’s concerns, and that the manuscript is now considered publishable in Environmental Health. We believe our revised manuscript conforms to the journal style.

On behalf of all five authors,

Kind regards,

Fredrik Niclas Piro