Reviewer’s report

Title: Contribution of smoking and air pollution exposure in urban areas to social differences in respiratory health

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Reviewer: michael jerrett

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This paper was a difficult read, probably partly as a result of the second language issue, but also due to some conceptual clarity problems. Although the authors will have to strive to improve the presentation, the results are potentially interesting and for Germany fairly novel. The lung function-PM association is possibly the most important finding and the authors need to emphasize this more. I am therefore recommending a resubmission with major revisions.

Review
This paper attempts to determine what portion of the association between respiratory disease and SES is attributable to either smoking or air pollution. This study builds on a cohort of about 1250 women aged 54-55 in Ruhr Germany. Although the question is worthy of investigation, the paper as currently written lacks key pieces of information which make it difficult to judge the adequacy of the methods. My comments are intended to clarify these ambiguities and offer suggestions for a more defensible analysis.

Specific comments are below:

Introduction
Change “most” to “many” when referring to SES in air pollution studies.
“The present analysis was performed in order to determine the contribution of smoking and air pollution as competing factors for the association between SES and the development of respiratory symptoms.”

Need to clarify this key statement for “explaining” the association.
Otherwise no problems found.

Methods
It’s not clear how many waves or cross sections are taken during the study
period or whether this is a “cohort” study or a repeated cross section. The cohort design would be much stronger. Seems however it’s cross sections. Exposure conversion of TSP problematic because the 0.71 ratio is higher than in other places and may change over space in relation to different source profiles. Can the authors justify the adequacy of this ratio?

Not clear how many monitors were used in total and in particular how many TSP measurements were used. Again more clarification and detail needed. Possibly a table summarizing all the data support would be useful here.

The coverage of monitors is probably inadequate to capture the spatial variations in NO2. This usually requires a very dense network. Seems that this may have contributed to the homogenous exposures and the authors need to acknowledge this limitation.

No control for neighborhood clustering; could this underestimate standard errors.

Results
The results are difficult to follow because there are so many outcomes/modifiers considered. Could the researchers focus on only a few of the more convincing results to make the discussion more tractable?

Stepwise regression may produce poorly specified models; was this conducted with an automated program or manually by the analysts. This needs clarification and if it was automated a more deliberate manual selection process should be used.

Discussion
The authors really need to reiterate the key points of their study here and discuss them more directly. There is not much difference between the air pollution exposures between groups. It’s difficult then to claim an environmental equity explanation when so little exposure variation exists. A more fundamental problem here it the temporal and spatial clustering of these attributes in neighborhoods. It does not appear as though these clustering issues have been accounted for in the analysis and as a result the standard errors may be underestimated. Some sensitivity analyses or justification for why a multilevel model is not needed is required to defend their results from this criticism.
Another note is whether the past coal exposures may have contributed disproportionately to the respiratory disease observed – seems the environmental injustice may have been worse in the past? What implications does this have for the analysis?

Finally the authors need to recognize that the small inequalities that do exist in exposure are likely the result of longer term socioeconomic processes whereby property values become depressed in areas of higher pollution and poorer people live in areas of low rent. Vicki Been’s classic study reveals this and might be discussed here. Therefore, to say that pollution explains part of the SES effect is misleading because the pollution distribution may in fact be a result of the longer SES processes. The study design does not address the longitudinal issue and therefore must be very modest about its claims.