Atmos. Chem. Phys. Discuss., 13, C5540–C5542, 2013
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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Air quality resolution for health impacts assessment: influence of regional characteristics" *by* T. M. Thompson et al.

**Anonymous Referee #2**

Received and published: 6 August 2013

The paper addresses the issue of the spatial resolution of air quality models with respect to the modelled outcomes in terms of health impacts of air pollution (O3 and PM2.5). The issue is not a new one and has already been addressed at various scales and domains in literature before, however with a focus on the concentration of pollutants rather than on the impact of the pollution, which involves a convolution of concentration fields with population distribution.

The domain of the modelling study is the Eastern US. The methodology used is relatively straightforward: perform model runs for a base and a policy case for 3 different spatial resolutions, compare the results, and evaluate the differences against other uncertainties. Here the authors choose a base case the year 2005 and as policy case the year 2014 with projected emission reductions due to measures in the electricity



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production sector.

The paper is well written and well structured, and the methodology is sound without flaws, however as said above, the conclusions cannot be generalized to other regions or even other types of air quality measures.

General comments:

The title could be improved, e.g.:" Air quality model resolution for health impacts assessment: urban versus rural sites" because as far as I understand the regional characteristics considered here is just the contrast urban vs. rural. If not, this should be made more clear throughout the paper.

It would help the less-informed reader if already in the introduction the link is explained between the time scale (and associated spatial scale due to wind-driven transport) of physical-chemical processes and the model resolution needed to capture these processes. Indeed, O3 titration happens at a short time scale compared to the photochemical production of O3 and secondary aerosols, hence one could already anticipate which components in which areas will be sensitive to the model resolution.

The authors should stress in their conclusions that the findings are valid for the selected domain and pollution reduction measures (electricity production sector), but they may well be different in other regions and for different measures, e.g. in European cities, applying measures on the emissions from diesel engines: in such a case the resolution may be more important than stated here as mainly primary PM is involved.

I would suggest using the same population statistics for both years in order to eliminate the impact of changing population on the magnitude of the impacts. Indeed, the health impact benefit appears to be near zero in New York and Virginia, whereas the concentration change is comparable to that in the other locations. I presume this is due to a change in population between 2005 and 2014? Or is it just because the population inside the domain is so low?



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Specific comments:

P14147 L24: “meteorological inputs are consistent in both. . .cases. . .”: what is meant here? Is the same meteorology used for both runs?

P14148 L4: also here: what is meant by “Emission totals are consistent across all resolution”?

P14150 L10: suggest mentioning that the applied O3 crf is for short term effect.

P14152 L4: “changes in mortality. . .are insensitive to . . .regional characteristics” specify what type of characteristics (I presume urban/rural).

P14152 L4-15: Please explain better this statement.

P14152 L25: I may be mistaken, but I would believe that the larger time scale for secondary aerosol formation is more linked with the photochemical and in-cloud conversion from SO2 to SO4 than with the mixing of precursors from different sources (in casu the availability of NH3).

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 14141, 2013.