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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Influence of air quality model resolution on uncertainty associated with health impacts" *by* T. M. Thompson and N. E. Selin

Anonymous Referee #1

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The paper shows the results of chemical simulations with the CAMx model for Houston Texas at four different resolutions: from 36 km to 2 km. Each resolution is evaluated against measured ozone. The main point of the paper, however, is to examine the health impacts at various resolutions.

For the most part this paper presents the results clearly and unambiguously. It is difficult to see how to extend the results from this one case study in Houston to other locations, so the results are perhaps somewhat limited. It seems, nevertheless, that this type of study is important. Commendably, the authors do not really attempt to generalize from this one study with a few minor exceptions.

Subject to the minor comments below I would recommend publication.

Minor comments. p14528/l25: I don't understand this sentence as written.

p14529/l11: I understand what the authors are trying to say here but the sentence is rather technically written and detailed. The meaning of "full meteorological files" is not clear. Perhaps a more conceptual explanation would work better?

p14529/l16: I assume the study by Bell and others mentioned below this line used downscaling. Please clarify and relate to the discussion on downscaling.

p14531/l9: What is the resolution of the meteorological inputs? It is not clear what the authors mean by "consistent in both scenarios". Consistent with what? Is the same meteorology used for both future and present scenarios. Please clarify.

p14539/l19: "there does exist the possibility for uncertainty analyses". Could the authors state more explicitly what type of uncertainty analyses they have in mind?

In the conclusions the authors claim: "we conclude that population weighted ozone concentrations obtained using regional photochemical models at 36 km resolution are likely to over- estimate the benefits associated with human health impacts relative to values obtained using fine (12 km or finer) resolution modeling". This seems like a strong claim from one study. Is there additional evidence to back this up? If not they should modify restrict this claim to the Houston area.

In the abstract the authors suggest that this study may be appropriate for analysis with similar chemistry. I would add similar meteorology and population density also.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 14525, 2012.