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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.

**T. M. Thompson et al.**

tammyt@mit.edu

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Response to Reviewers

We would like to thank both reviewers for the particularly helpful and intelligent suggestions (copied below). We have addressed all questions, comments and suggestions below (responses directed to reviewers follow the reviewers comments, and all text copied from or added to the paper is in quotes).

Reviewer #1 General Comments: It is not clear from the methodology described in the manuscript that the authors generated 4 km emissions and meteorology similarly to what was done for the 36 and 12 km domains. The authors are fairly clear in that 4 km meteorology was not developed, but interpolated from the 12 km domain using CAMx.

If the authors used a similar process to interpolate the gridded emissions from 12 to 4 km with CAMx then it is not surprising that no differences in health impacts are seen between 12 and 4 km because they have not improved the spatial representation of large near-surface emissions such as mobile and area sources with respect to the 4 km spatial representation of population. This is a critical issue with this manuscript.

The authors do not provide any operational or diagnostic model performance comparing the different grid resolution predictions for these areas. The authors incorrectly reference an EPA technical support document for a description of all the model inputs and for model performance evaluation. An inspection of the EPA document reveals that no 4 km inputs were developed as part of the modeling for the CSAPR rule. Since the focus of this paper is showing the impact of grid resolution on health impacts the authors need to provide much more detail about how the 4 km inputs were developed and provide an evaluation of those domains. Also, in order to make conclusions about how 4 km health impacts differ from impacts estimated using coarser grid resolution, the authors need to develop emissions at 4 km rather than interpolating the 12 km emissions to 4 km. Without 4 km spatial surrogates underlying the emissions the authors are effectively using the same emissions at 4 and 12 km and it is not at all surprising they see no difference in estimated health impacts.

The authors appropriately chose to include the health effects using a variety of epidemiological functions in the results. However, similar to needing appropriate data to support a 4 km photochemical modeling assessment, the authors should discuss what aspects of the BenMAP health benefits model need more detailed information for urban-specific 4 km applications compared to national scale assessments. Is population the only BenMAP input that changes at 4 km? Are the health impact functions appropriate at 4 km and for all urban areas?

There are not very many Figures or Tables supporting this assessment. Since variable grid resolution is the focus of this paper some spatial plots showing ozone and PM2.5 (and primary and secondary since it is discussed in detail) at different grid resolutions



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would be useful.

Response:

While not utilized for the U.S. EPA's regulatory CSAPR modeling project (and therefore not mentioned in the referenced technical document), 4 km spatial surrogates were developed by the U.S. EPA for the 2005 basecase modeling episode that was used to conduct this research. The U.S. EPA transferred those surrogate files to the authors via secure ftp site on April 4, 2012. The 4 km spatial surrogates for the Eastern U.S. were used to develop 4 km area source emissions inventories with improved spatial resolution over the 12 km domain area source emissions. We strongly agree with the reviewer that this is an important detail that was not made clear enough in the document so we have added the following text to the last paragraph of section 2.1 in order to make this point clear: "The 4 km domain was not included in the specific CSAPR modeling, but 4 km spatial surrogate files were created by the U.S. EPA for the 2005 base case modeling episode using the same procedures used to create the 36 and 12 km spatial surrogates. We obtained those 4 km surrogate files from the U.S. EPA in order to spatially allocate the low-level area source emissions to the 4 km grid with spatial detail that is improved over the 12 km domain. Emissions totals are the same across all resolutions and the spatial distribution, while showing increasing detail as resolution improves, is also the same (i.e. the emissions totals in the 4 km grid boxes contained within each 12 km grid box sum to equal the emissions totals of that 12 km grid box, and similarly from 12 km to 36 km grids)."

Population resolution is already indicated in Section 2.2, but we have edited/added the following sentences, also in Section 2.2, to clarify the spatial resolution of the health incidence data: "We use a 2015 baseline mortality rate that is based on 2004-2006 individual-level mortality data (i.e., from records of individual deaths), as reported to the US CDC (CDC, 2006), incorporated within BenMAP as county-level mortality rates and projected using national-level census mortality rate projections (Abt, 2010). These county-level mortality rates available within BenMAP were spatially allocated to the air



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quality grids at 4km, 12km and 36km resolution, respectively."

We also added to Section 2.2: "Throughout this study, we used the same population data (projected for the year 2014), health incidence data (projected for the year 2015), and health impact functions in BenMAP. Native county-level population and incidence data in BenMAP were simply re-gridded within that program to our resolution of interest. From the discussion of local, fine-scale health impact assessment found in Hubbell, Fann, and Levy (2009), we chose to maintain consistent methodologies across our regions of interest so that our results could be compared across settings."

BenMAP's mortality rates rely on individual-level mortality data (i.e. reports of individual deaths), and should thus be fairly reliable, especially in dense urban areas. As Hubbell, Fann and Levy (in Air Qual Atmos Health (2009) 2:99–110) describe, fine-scale incidence reporting in lightly populated areas can trigger data suppression rules, limiting the reliability of finer-scale incidence rates. Additionally, locations may differ in their exposure, susceptibility, and demographics, which might bias the application of non-locally derived health impact functions. Here, we again rely on Hubbell, Fann and Levy, who describe the general lack of locally-derived concentration response functions of sufficient statistical power, and the increasing robustness of multi-city studies.

We agree that more visuals will help the reader understand our study and the conclusions that we draw from our work. Therefore we have added maps of three cities (Atlanta, Rural New York and New York City) to the Supplemental Information that show the difference in concentrations of ozone and PM2.5 at each of the three resolutions. Additionally, we added the following text to the main document in Sections 3.1 (ozone) and 3.2 (PM2.5) to direct the reader to those additional maps: "Maps showing the difference between 2014 and 2005 daily maximum 8 h ozone concentrations averaged for the ozone season over Atlanta, New York City and Rural New York are presented in the top row of Supplemental Information Figures S-1,2&3 respectively." And "Maps showing the difference between 2014 and 2005 annual average PM2.5 concentrations over Atlanta, New York City and Rural New York are presented in the bottom row of



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Supplemental Information Figures S-1,2&3 respectively."

Specific Comments from Reviewer #1:

Page 14143 lines 15-20. I am not convinced anything was presented that addresses the influence of varying meteorological patterns on optimal grid resolution. I don't see any subsequent discussion detailing different weather patterns and how different health impacts were across various mesoscale and micro scale meteorological patterns.

Response: We agree with this comment and we have altered the following text throughout the paper to change our claims to reflect that we did not conduct a study that addressed the influence of specific meteorological conditions on resolution, instead we looked at specific regional characteristics as identified in Figure 1: In the last paragraph of the introduction: "Using an air quality policy episode for the entire eastern US, we conduct nested simulations of 36, 12 and 4 km in nine regions of the US, evaluating the influence of urban versus rural land use, emissions mix, current pollution levels and differing meteorological patterns on (1) the ability of coarse scale modeling to simulate changes in population-weighted concentrations of ozone and PM similarly to finer scale modeling, and (2) the errors contributed by model resolution changes relative to benefits evaluations." was changed to read: "Using an air quality policy episode for the entire eastern US, we conduct nested simulations of 36, 12 and 4 km in nine regions of the US, evaluating the influence of urban versus rural land use, current attainment status (with respect to U.S. National standards) and coastal versus inland location on (1) the ability of coarse scale modeling to simulate changes in population-weighted concentrations of ozone and PM similarly to finer scale modeling, and (2) the errors contributed by model resolution changes relative to benefits evaluations."

The following sentence in Methods Section 2.1 was changed from: "These sub-domains are selected to represent a variety of meteorological conditions, population and industrial density, local emissions, and existing pollution concentration levels." To read "These sub-domains are selected to represent a variety of regional character-



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istics including population and industrial density, proximity to the coast, and existing attainment status."

Reviewer Comment:

Page 14146 Methods section. Much more detail on the development of the 4 km emissions and meteorology is needed. Also, more detail is needed regarding the CAMx application. For instance, are the 4 km domains run with 1 or 2-way grid nesting from the 12 km domain? It would be useful to know what feedback choice was made for the readers interpreting the results. The version of CAMx used for this study needs to be included in the manuscript.

Response: As mentioned in the general comments section, we strongly agree that more information regarding the development of the 4 km area source emissions inventory is needed. We have edited/added the following text in the last paragraph of section 2.1 to illuminate this subject: "The 4 km domain was not included in the specific CSAPR modeling, but 4 km spatial surrogate files were created by the U.S. EPA for the 2005 base case modeling episode using the same procedures used to create the 36 and 12 km spatial surrogates. We obtained those 4 km surrogate files from the U.S. EPA in order to spatially allocate the low-level area source emissions to the 4 km grid with spatial detail that is improved over the 12 km domain. Emissions totals are the same across all resolutions and the spatial distribution, while showing increasing detail as resolution improves, is also the same (i.e. the emissions totals in the 4 km grid boxes contained within each 12 km grid box sum to equal the emissions totals of that 12 km grid box, and similarly from 12 km to 36 km grids)."

We also added the following text to paragraph two of section 2.1 to introduce the two-way nesting that was used to model the 4 km domain regions: "The 36 km and 12 km model runs were conducted individually, using for our analysis only the model output from the grid cells falling within the nine selected regions. The 4 km results were obtained by running the nine regions as nine individual nested 4 km grids within the 12



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km domain using two way nesting."

Finally, we have altered the following sentence at the beginning of section 2.1 to include the version of the CAMx model. "We use CAMx version 5.3 (www.camx.com), a US EPA-approved regional air quality model (US EPA, 2007)."

Reviewer Comment:

Page 14152 Discussion section. The authors speculate about how primary and secondary PM2.5 have different and sometimes possibly compensating impacts on total PM2.5 in an urban area. It would be much more effective if the authors did a controlled experiment where primary PM2.5 and separately secondary PM2.5 precursors are systematically adjusted to quantify the impacts of both on human health benefits estimates.

Response: In order to address this question we evaluated the human health impacts of primary and secondary PM2.5 individually by following the same methods used in the paper to evaluate total PM2.5. We have removed the previous hypothesis regarding primary versus secondary PM2.5 from both the abstract and the main paper and in the discussion section we have added the following text: "In order to investigate the relative impacts of primary and secondary PM2.5 on total PM2.5 we evaluated the human health impacts of primary and secondary PM2.5 individually following the same methods outlined in this paper. We found that secondary PM2.5 dominates the total health impacts. We also found that the magnitude of human health benefits of primary PM2.5 increase as resolution increases, but the magnitude of human health benefits associated with secondary PM2.5 shows no clear pattern with resolution."

Reviewer Comment:

Page 14154 lines 14-17. This is a very useful and important conclusion that 36 km grid resolution overestimates health benefits. Since global models such as GEOS-CHEM and MOZART are commonly applied with grid resolution much greater than 36 km



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this suggests that these models should not be coupled with health effects models like BenMAP to make conclusions about health impacts, or at a minimum those projects should recognize that their impacts are overstated. The authors should point out the implications for global modeling in the conclusions as well.

Response: We agree that this is a very important result of our paper as we often see coarse scale modeling results used to evaluate human health impacts. We have added the following text to the second paragraph of the discussion in order to make this point more clear: "These ozone results are important to keep in mind when researchers present the human health benefits related to changes in ozone concentrations that are evaluated using coarse scale or global scale modeling." We also changed the following sentence at the beginning of the conclusion section to speak to this important result as well: "The coarse scale resolution (36 km) showed the largest decrease in pollution exposure from the base case to the control scenario case, indicating the potential for coarse scale modeling results to over-estimate the benefits due to reductions in ozone precursor emissions."

Reviewer Comment:

Figure 3. It is very interesting to see that there is more variability in health impact estimates due to the epidemiological function used compared to grid resolution. The authors should include that as an important conclusion for this study.

Response: Once again, we agree and found this to be an interesting and important finding of our study. We have added the following text to our abstract: "Additionally, we found that the health impacts calculated using several individual concentration response functions varied by a larger amount than the impacts calculated using results modeled at different resolutions." And the following text at the end of our discussion section, both to communicate this finding to our readers: "These results also show that the choice of concentration response function can have a larger impact on the health benefits estimated than the choice of resolution."



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Reviewer #2 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?

Response:

While we do feel our title is appropriate, we agree that we need to be more clear about the specific regional characteristics that we evaluate in this paper with regards to model resolution. As such, we have made the following changes to text throughout the paper



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to address this issue: In the last paragraph of the introduction: "Using an air quality policy episode for the entire eastern US, we conduct nested simulations of 36, 12 and 4 km in nine regions of the US, evaluating the influence of urban versus rural land use, emissions mix, current pollution levels and differing meteorological patterns on (1) the ability of coarse scale modeling to simulate changes in population-weighted concentrations of ozone and PM similarly to finer scale modeling, and (2) the errors contributed by model resolution changes relative to benefits evaluations." was changed to read: "Using an air quality policy episode for the entire eastern US, we conduct nested simulations of 36, 12 and 4 km in nine regions of the US, evaluating the influence of urban versus rural land use, current attainment status (with respect to U.S. National standards) and coastal versus inland location on (1) the ability of coarse scale modeling to simulate changes in population-weighted concentrations of ozone and PM similarly to finer scale modeling, and (2) the errors contributed by model resolution changes relative to benefits evaluations."

The following sentence in Methods Section 2.1 was changed from: "These sub-domains are selected to represent a variety of meteorological conditions, population and industrial density, local emissions, and existing pollution concentration levels." To read "These sub-domains are selected to represent a variety of regional characteristics including population and industrial density, proximity to the coast, and existing attainment status."

Additionally, we would like to call the reviewer's attention to Figure 1 which outlines the specific characteristics associated with each location.

We have added the following sentences to the methods section 2.1 to describe the link between model time step and spatial scale: "CAMx output files are reported at an hourly time step, however each process within CAMx is calculated at a time step that is internally determined by the CAMx model based on the spatial resolution (grid cell size) and on the process that is being calculated. As resolution increases, the internal model time step will decrease."



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While the federal regulation that is applied and modeled using the 2014 modeling run (CSAPR) does not explicitly contain on-road sector rules, there are significant on-road reductions between 2005 and 2014. Therefore we argue that the application of this study is suitable for regulations beyond just those of the electricity sector. This is why we used the 2005 case as our base versus using the 2014 base without the regulation applied. In order to support this claim, we have added the following text to the first paragraph of section 2.1 where we explain why we are using the 2005 emissions inventory as our base case versus the 2014 emissions inventory without CSAPR policy applied: "The choice of policy case also encompasses a range of policy options covering emissions sources with different characteristics. While the CSAPR targeted the electricity sector, EPA projects that additional policies and improvement in mpg reduce total emissions of NOx, SO2 and VOCs from the on-road mobile sector by 45%, 85% and 45% respectively between 2005 and 2014."

Nonetheless, we appreciate your comment that the wide global scope of potential populations, emissions, land use, and pollution mixes may warrant caution in a broad application of our conclusions. To address this, we have added the following text to the Discussion Section 4, "This result is expected to be robust to the variety of policies and pollution levels studied here, but may not apply in all global or future contexts."

Finally, to address the issue of the population: the small value that is calculated for the health impacts in the rural areas of Virginia and New York are in fact due to the very small populations in those areas (small relative to the other areas that represent larger cities). The way BenMAP calculates the population weighted change in concentration of a species is to first calculate the change in concentration of that species within each grid cell, and then multiply that change by the population within that same grid cell. The values are then summed across all grid cells within the region of interest and divided by the total population of that region. BenMAP uses only a single population year to investigate a given change in concentration. In this case, because we investigated the proposed changes in 2014, we used population data for 2014. Thus, as you sug-



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gest, we do in fact use the same population data for both years. We have changed the following sentence in Section 2.2 to make this point more clear. “For each of the 9 locations and 3 model resolutions, the inputs to BenMAP included: model grid cell domain definitions; projected 2014 US population data; and pollutant concentrations for each day of the 2005 base case and the 2014 control case. These inputs are combined within BenMAP to estimate the change in average population weighted pollutant concentrations between the base case and the control case. (Concentration changes presented herein are averaged for May through September in the case of ozone, and annually in the case of PM2.5.)”

Specific Comments from Reviewer #2:

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

Response: Yes, the same meteorological input files were used for both runs. We changed the following line beginning at line 24 of page 14147 (Section 2.1) to make this more clear: “Meteorological inputs are consistent in both the 2005 base case and the 2014 policy case and were developed using the fifth generation Penn State/NCAR mesoscale model MM5 (Grell et al., 1994) for every day of 2005; for the 4 km domain, meteorological data is interpolated by CAMx from 12 km.” was changed to read: “Meteorological input files are the same for both the 2005 base case and the 2014 policy case (representing 2005 meteorological conditions) and were developed using the fifth generation Penn State/NCAR mesoscale model MM5 (Grell et al., 1994) for every day of 2005; for the 4 km domain, meteorological data is interpolated by CAMx from 12 km.”

Reviewer Comment:

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



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Response: The following line 4 of page 14148 (the last paragraph of Section 2.1): "Emissions totals are consistent across all resolutions." Was changed to read: "Emissions totals are the same across all resolutions and the spatial distribution, while showing increasing detail as resolution improves, is also the same (the emissions totals in the 4 km grid boxes contained within each 12 km grid box sum to equal the emissions totals of that 12 km grid box, and similarly from 12 km to 36 km grids)."

Reviewer Comment:

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

Response: We have edited and added the following sentences to specify that the O3 crf used in section 3.1 estimates mortality from acute exposure, and the PM2.5 crf used in section 3.2 estimates long-term effects of PM2.5: "Figure 2.a shows the calculated decrease in mortalities due to changes in ozone between the 2005 base case and the 2014 control case (2005-2014), based on modeled population-weighted concentration data within each area, from the three different modeling resolutions applied to the mortality results from acute exposure estimated with Bell et al. (2004)." and "The function developed by Laden et al. (2006) and used here estimates long term effects from PM2.5".

Reviewer Comment:

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

Response: We direct the reviewer to Figure 1 in the main paper which highlights the specific characteristics associated with each region. As mentioned in the response to general comments above, we have added the following text to also more clearly explain that these are the characteristics we are evaluating. In the last paragraph of the introduction: "Using an air quality policy episode for the entire eastern US, we conduct nested simulations of 36, 12 and 4 km in nine regions of the US, evaluating



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the influence of urban versus rural land use, emissions mix, current pollution levels and differing meteorological patterns on (1) the ability of coarse scale modeling to simulate changes in population-weighted concentrations of ozone and PM similarly to finer scale modeling, and (2) the errors contributed by model resolution changes relative to benefits evaluations." was changed to read: "Using an air quality policy episode for the entire eastern US, we conduct nested simulations of 36, 12 and 4 km in nine regions of the US, evaluating the influence of urban versus rural land use, current attainment status (with respect to U.S. National standards) and coastal versus inland location on (1) the ability of coarse scale modeling to simulate changes in population-weighted concentrations of ozone and PM similarly to finer scale modeling, and (2) the errors contributed by model resolution changes relative to benefits evaluations." The following sentence in Methods Section 2.1 was changed from: "These sub-domains are selected to represent a variety of meteorological conditions, population and industrial density, local emissions, and existing pollution concentration levels." To read "These sub-domains are selected to represent a variety of regional characteristics including population and industrial density, proximity to the coast, and existing attainment status."

Reviewer Comment:

P14152 L4-15: Please explain better this statement.

Response: In order to more clearly communicate this point, the following text from the discussion section, page 14152, lines 5-15: "The results shown in Fig. 2a suggest that 36 km resolution modeling has the potential to over-estimate ozone benefits in populated urban areas. For all nine regions evaluated, human health impacts due to changes in ozone calculated using 36km resolution modeling were larger than impacts calculated using finer scale modeling. On average, in urban areas, the human health response calculated at 36km resolution was 200% larger than the response calculated at 12 km resolution, while the difference was only 8% greater in rural areas. Even excluding Houston and New York City and the extreme differences between resolution results in those two regions, the remaining urban areas showed response to emissions



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changes that was 50% larger in 36km resolution. In contrast, the impact of resolution did not seem as important when considering an area's current ozone attainment status or proximity to the coast." Was changed to: "The results shown in Fig. 2.a suggest that 36 km resolution modeling has the potential to over-estimate ozone benefits in populated urban areas. Human health benefits were larger for ozone calculated at the 36 km resolution than at the 12 km or 4 km resolution for all nine regions evaluated. Most of the difference between resolutions in these regions occurs in urban areas. In urban areas, the human health response calculated at 36 km resolution is, on average, 200% larger than the response calculated at 12 km resolution, compared to 8% in rural areas. Houston and New York City have extreme differences between resolution results. Even excluding those two regions, the remaining urban areas showed a 50% greater ozone benefit at the 36 km resolution compared to 12 km. In contrast, other regional characteristics considered did not seem as sensitive to resolution. Specifically, the impact of resolution did not seem as important when considering an area's current ozone attainment status or proximity to the coast."

Reviewer Comment:

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).

Response: We removed this text from the paper.

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