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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

T. M. Thompson and N. E. Selin

tammyt@mit.edu

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We would like to thank the reviewer for the helpful comments. We agree that the results from this paper alone cannot be justifiably applied to other pollutants, areas or meteorology and we have edited the language in the paper to make sure this is clear throughout. We also agree with the reviewer that this is an important topic and that additional research on the impact of resolution on ozone and other pollutants and other geographical areas is necessary, and consider this paper an important first step to address this larger question. Our particular contribution through this paper is to illustrate a method for examining model resolution and how it relates to the uncertainty of human health impacts and therefore evaluate what resolution is necessary in order to provide meaningful results.

We have also added wording that should make clear that our selections for mortality response functions are based on the latest studies with published responses to daily maximum 8-hour ozone concentrations (versus 8-hour responses adjusted from other metrics as we wished to avoid the associated uncertainty) as described in our response related to page 14533 below and in section 2.3 in the paper. Based on reviewer feedback, we have deleted results from morbidity health endpoints in the final version. Several of the issues this reviewer mentions, including lack of city-specific baseline incidence rates, and lack of direct impact assessment with 8-hour ozone among others, created challenges in applying this data, and the results for morbidity did not add meaningfully to results. We have added a mention that these endpoints were evaluated, and we discuss our findings and limitations in a general sense, without including hard numbers in the results.

Finally, we now acknowledge and briefly discuss the disconnect between the spatial resolution of response functions, and the model resolution. Our methods mirror those of U.S. regulatory procedures and many of the procedures used in peer-reviewed human health impact studies.

We have copied and responded to specific comments below: Abstract

–How well has the analysis characterized the temporal variability in ozone concentrations by modeling for 60 days rather than the full ozone season?

In this particular case, specifically the non-Attainment area of Houston/Galveston/Brazoria(HGB), this time period was chosen because it was deemed by the Environmental Department of Texas (TCEQ) as representative of conditions leading to high ozone. This episode was used for the HGB Attainment demonstration for the 1997 ozone standard. We have edited the abstract and the methods section 2.1 respectively as shown below to include this information.

“Using a regional photochemical model (CAMx), we ran a modeling episode with meteorological inputs simulating conditions as they occurred during August through Septem-

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ber 2006 (a period representative of conditions leading to high ozone), and two emissions inventories (a 2006 base case and a 2018 proposed control scenario, both for Houston, Texas) at 36, 12, 4 and 2 km resolution."

"We use a well-documented air quality episode developed in part during the Texas Air Quality Study II (TexAQSII) and selected as representative of high ozone episodes in this region (TCEQ, 2006; TCEQ, 2010a)."

–In what year were the impacts modeled?

The meteorological conditions and basecase emissions inventory represent conditions as they occurred in 2006. The control case represents growth plus proposed emissions controls in 2018. We include this information in the methods section 2.1(page 14530 line 25 through page 14531 line 4).

–Need to be clear that the sensitivity of the estimated impacts are constrained by the model specification, and that this inherently limits your ability to apply these results to other contexts.

We have reworded the final two sentences of the abstract (provided below) in order to make this more clear. We have indicated that these results are applicable to this particular study only and that further research would be required to extend these results to other regions and pollutants.

"Study results show that ozone modeling at a resolution finer than 12 km is unlikely to reduce uncertainty in benefits analysis for this specific region. We suggest that 12 km resolution may be appropriate for uncertainty analyses of health impacts due to ozone control scenarios, in areas with similar chemistry, meteorology and population density, but that resolution requirements should be assessed on a case-by-case basis and revised as confidence intervals for concentration-response functions are updated."

–I appreciate the authors sense to only include the parts of the coarser domains that cover the area of the finest domain in this analysis. That makes comparison between

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grid resolution much easier to interpret.

We agree that it only makes sense to include the area that each modeling domain has in common.

Introduction –14527/11: Can you provide a citation to support this claim?

We have altered the sentence and added a citation as follows:

"However, the ability to model ozone production is sensitive to model resolution, and it has been suggested that fine-scale modeling (at resolutions up to 2 km by 2 km) is often necessary to reproduce ozone chemistry if results are to be used to policy decisions (US EPA, 2007)."

–14528/17: the characterization of impacts with and without the Clean Air Act was the central policy question they were trying to answer, rather than an uncertainty analysis.

We have reworded the sentence to make this point more clear.

"The US EPA (2011a) conducted an uncertainty analysis as part of an evaluation of the US Clean Air Act (CAA). The goal of the study was to estimate the human health impacts of ozone and particulate matter concentrations in 2020 under the environmental regulation mandated by the CAA, versus likely concentrations of those two pollutants if the CAA were not implemented."

–14529: It's not immediately obvious how the discussion on this page regarding the downscaling of climate modeling relates to cross-scale comparisons. What does this tell us about the sensitivity of model predictions of concentration and health impacts to the use of alternate grid resolutions?

We have added the following sentence to our introduction to draw a link to our study and common use of global scale modeling and downscaling.

"Because of the increasing use of human health impact analyses from both global scale modeling and downscaling, it is important to evaluate how model resolution impacts the

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uncertainty associated with human health impacts of air pollution and to move towards identifying a resolution target for human health impact analyses."

Methods –14531: Which version of CAMx did you use?

We used CAMx version 4.5.3 for this study and we have added the version number to the paper. (page 14530 line 22 in Methods section 2.1)

–14531: Additional contextual information regarding the emissions inventories would be helpful. In particular, additional information about: (1) the sources affected; (2) whether link-level emissions were modeled; (3) the spatial distribution of emissions. These are each factors that would tend to affect the influence of spatial resolution on model predictions, and would also help the reader determine how generalizable this episode might be.

1. We have added a more detailed description of the emissions inventories by source in a Supplementary section. We agree that it is important to specify how the controls will impact both low level and elevated emissions totals and so by adding a sentence outlining the total reductions we have made more clear in the paper the total reductions of both low level emissions, and elevated emissions. "The change in total emissions (low level plus elevated) is as follows for NOx, CO, and VOCs respectively: -35%, -23%, +10%." 2. Link-level mobile source emissions were included in the HGB area, however, low level emissions, including mobile sources, are immediately and homogeneously distributed to the grid cell they fall within for modeling, therefore, this level of detail is not necessarily important. The supplemental information however now includes this detail. 3. The spatial distribution of emissions was consistent between modeling resolutions.

–14531-2: Missing here is a discussion of the role of the meteorological model in specifying fine-scale input data. How readily available are these data, and are there any special challenges or uncertainties associated with generating these estimates?

We used here the met data developed by the Texas Commission on Environmental

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Quality (TCEQ) for the State Implementation Plan for the Houston non-attainment area. Detailed analysis of the meteorological data was previously conducted by the TCEQ; we have edited the sentence in section 2.1 as quoted below. Further analysis of meteorological data specifically is beyond the scope of our study, though we acknowledge that this is a potential uncertainty.

"A detailed description of the episode is provided by the TCEQ and includes a performance evaluation of the meteorological data (2010a). Meteorological inputs provide an additional source of uncertainty that is beyond the scope of this study (Vautard et al., 2012)."

–14531/16: The author incorrectly states that US EPA has published criteria for ozone performance in the 2007 modeling guidance document as being +- 15% for mean normalized bias and mean normalized error. There are not criteria published in the modeling guidance document and in fact it states quite clearly that there is not specific bright-line performance criteria. This incorrect statement is made in multiple sections of this paper.

We agree that the language in the paper incorrectly gave this impression, and have altered the wording in sections 2.1 and 3.1 to more clearly represent the EPA guidance.

–14532: How did you project the population size and distribution?

The following sentence has been edited in section 2.3 in order to answer this question.

"Population distribution is from US Census data, provided with census block spatial detail, and is projected by GeoLytics (GeoLytics, 2010) and mapped to our modeling domain grid cells using Geographical Information System (GIS) software."

–14532: It's not clear why population-weighted air quality changes were calculated only at cells containing monitors. This approach would bias-low your estimates of health impacts.

The following sentence (found in section 2.3) was changed to reflect that population-

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weighted air quality changes were calculated in grid cells containing monitors in order to be able to compare measured data (from monitors) to modeled data. For the remainder of the study, all grid cells within the 2 km domain were included.

"For population-weighted analysis of monitor data, only those grid cells with monitors located in them are used in the calculation, otherwise, all grid cells within the HGB area are included in the calculation."

–14533: There's no discussion here of what rationale the authors used to select either these endpoints or studies.

The studies were selected based on the availability of published factors for response to changes in daily maximum 8-hour averaged ozone concentrations consistent with the most recent studies used in U.S.EPA's Environmental Benefits Mapping and Analysis Program (BenMAP) and the European Union's ExternE studies. We have added the following sentence in section 2.3 to make this clear.

"The response functions selected for this study are those with published responses for daily maximum 8-hour averaged ozone concentrations consistent with the latest EPA's Environmental Benefits Mapping and Analysis Program (BenMAP) (Abt, 2010) and the European ExternE study (Bickel and Friedrich, 2005)."

* What was the source of the baseline morbidity rates? These vary spatially and could have an important influence on your results across scales. * When applying effect coefficients from Bell et al. (2004) and Zanobetti and Schwartz (2008), which estimates did you use? The national estimate, or the city-specific estimate?

Unlike the response functions for mortality, the baseline morbidity rate is built in to the response function. For some cities and/or counties, there may exist region-specific morbidity functions that include region-specific baseline morbidity rates, but we are not able to include that data in this study. We agree that the issue of the disconnect in the spatial resolution between morbidity baseline data and modeled ozone concentrations

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distracts from our purpose and our method without adding to our findings. As a result, we have removed those specific results from the paper choosing instead to discuss them briefly.

We do use mortality functions obtained using nationally averaged data, however, our results are based on 2006 Houston-specific mortality baseline. The national average provided a conservative (lower) median, and less uncertainty than city-specific estimates due to the larger sample size included. We feel this is a better choice and we have added a paragraph to section 2.3 that introduces and briefly discusses this uncertainty.

–14536/26: It's not clear how the effect coefficient used to quantify impacts would be sensitive to the model resolution. The underlying population projections and baseline incidence rates might be–but not the effect coefficient used in the health impact function.

We have attempted to make this more clear by editing the following sentence found in section 4:

"The spread of the confidence interval will determine how accurate the air quality data needs to be. As confidence in human health functions improves, there will be less overlap between results calculated from each resolution and therefore differences between resolutions may become significant."

–14537/5: The negative tail is likely due to weak statistical power in the study. This is one of several reasons we have chosen to remove these results from the paper.

–14537: I am not clear why this is an "error analysis". It seems that the process analysis is used to understand which processes contributed most to changes in ozone between the baseline simulation and the "control" scenario. More information about the process analysis setup is needed. How many grid cells were included in the process analysis box and how many vertical layers?

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We have renamed this section “Process Analysis” and we added the following sentence to clarify the PA setup:

"For each resolution, the area included in the Process Analysis was the HGB area in the horizontal, and up to the mixing height (determined for each hour of the day by the pyPA program) in the vertical."

Conclusions –It is important to note that if 36 km results tend to overestimate health impacts then simulations greater than 36 km could suffer from a similar tendency. This is critical to note as many researchers take output from global models such as GEOS-CHEM and estimate health benefits using much coarser grid resolution than even 36 km.

We have added the following sentence at the end of our conclusion to drive home this very important point.

“This result is important given the increasing use of global scale models in research related to human health as many global scale models are run at resolutions coarser than even 36 km.”

Technical corrections –The cost/benefit requirements you refer to are stipulated by Executive Order 12866 and not the Clean Air Act

We thank the reviewer for pointing this out, and have edited the following sentence found in the abstract:

“Given the cost/benefit analysis requirements motivated by Executive Order 12866 as it applies to the Clean Air Act, . . .”

–The use of the term "improve uncertainty" in the abstract is a little awkward.

That term has been changed to:“reduce uncertainty”

–14527/7: suggest rephrasing "Many elements of ozone concentration and impacts are uncertain: : :" to "Predicting ozone concentrations and health impacts is subject to

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a number of sources of uncertainty, including: : :"

Good suggestion and that change has been made

–14538/24: This sentence is unclear. We have reworded the sentence in our conclusions to help make our message more clear:

"Additionally, we evaluated the model predicted values of population-weighted calculations of 8-h maximum ozone using the same statistics we used to evaluate the daily 8-h maximum. We found that the model was better able to reproduce these derived values than the 8-h maximum concentrations (the latter being the focus of the regulatory process)."

–14539/8: This sentence is unclear. We have reworded the sentence in our conclusions to help make our message more clear:

"The mean value for change in mortality, calculated using coarse resolution model results, fell within the health impact response range of uncertainty as calculated by the 2 km resolution for all three mortality response functions evaluated."

–14539/17: This sentence is unclear. The confidence intervals around the mean estimates reflect the standard error reported in the epidemiological study–and are entirely unrelated to model resolution.

We have reworded the sentence in our conclusions to help make our message more clear:

"However, because the median values of all health impacts evaluated that were calculated using coarse modeling do fall within the health impact uncertainty range of fine resolution results, there does exist the possibility for uncertainty analyses (for example: Monte Carlo analysis) on 36 km resolution air quality modeling results, which are on average 300 times more computationally efficient than running the same episode and same domain with 2 km resolution."

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