Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., author comment AC1
https://doi.org/10.5194/acp-2021-568-AC1, 2021

Responses to the comments of Referee 1

Pablo Garcia Rivera et al.

Author comment on "Source-resolved variability of fine particulate matter and human exposure in an urban area" by Pablo Garcia Rivera et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-568-AC1, 2021

Major comments

(1) *The authors note that one often limiting factor in higher-resolution modeling is the spatial allocation of the emissions data. Here they use standard approaches for most emissions sectors but use alternative methods for on-road vehicles and commercial cooking establishments. However, no evaluation of these alternative methods is provided. If the authors are reserving a full evaluation against observations for another manuscript, they could still perform a comparison of results using their new methods vs those obtained using standard approaches, or at least compare the emissions obtained using the standard and new methods.*

We have followed the suggestion of the reviewer and added a comparison of the emission fields using the alternative approaches together with the corresponding discussion. The changes are especially important for the cooking organic aerosol emissions. A detailed evaluation of the model predictions against observations is the topic of a subsequent paper.

(2) *Somewhere, either in the methods or conclusions, the authors should acknowledge that their approach of interpolating meteorological data for the 4 km and 1 km domains from the 12 km WRF simulation (rather than conducting separate higher-resolution meteorological simulations) may affect their results and limit the benefit of higher-resolution PMCAMx simulations. The same may be said for the emissions, if they are using surrogates from the 12 km domain.*

We agree that the interpolation of the meteorological fields from the 12 km WRF simulation is a potential limitation. This information has been added to the conclusions as an area of future improvement. The emission fields were not based on interpolation. Data sources from which the 12 km EPA surrogates were built were used to build new surrogates at 4 km and 1 km resolutions. This information about the actual resolution of the emissions at the higher resolutions has been added to the revised paper.

(3) *93-112: Many of the papers that are cited in the manuscript are missing from the list of References, particularly in the model description section.*

The missing references have been added to the reference list.

(4) *122-132: How many vertical layers were used in the WRF modeling? Were simulation results used to provide chemical lateral boundary conditions for the nested domains? I would assume that is the case, but the authors should say so.*

28 layers were used in the WRF modeling. 14 of them corresponded to the vertical layers in the CTM to avoid interpolation errors. The results from the parent simulations were indeed used as chemical lateral boundary conditions for the nested grids. This information has been clarified in the manuscript.

(5) *Tables 1 and 2: Does biomass burning include wildfires, or only residential wood combustion?*

The biomass burning emissions shown in these two tables only include residential wood combustion emissions. There were no significant wildfires in this smaller domain during the simulation periods. This has been clarified in the revised manuscript.

(6) *142-143: The previous paragraph said custom surrogates were developed for commercial cooking for "the higher resolution grids", but this sentence suggests that the normalized restaurant count approach was used only for the 1 x 1 km grid. Please clarify.*

The actual restaurant locations were used for both the 1x1 km and 4x4 km grids. This information has been added to the paper.

(7) *148: The emissions units kg d^{-1} km^{-2} specified for Tables 1 and 2 make sense. However, I am unable to interpret what is meant by a unit of kg g^{-1} km^{-2}. Is this a typo?*

This typo has been corrected in the manuscript.

(8) *149-153: Per Table 1, I calculate that on-road emissions are about 2.4% of total emissions. (Total emissions = 7918.5 kg d^{-1} km^{-2} for winter; 188/7918.5 = 2.4%). (Incidentally, the caption to Figure 2 should specify whether the plots are for February or July.) If emissions were uniformly distributed in space, then Fig. 2b would show 2.4% everywhere. The authors state that on-road emissions are concentrated in downtown Pittsburgh, but the on-road fraction of total emissions in Fig. 2b has a maximum value of about 1.2%. I suppose this could be because other emissions sources are even more concentrated downtown, bringing down the on-road fraction, but still it seems counterintuitive. What sector is even more disproportionately located downtown (and thus a better tracer for primary PM) than on-road emissions?*

The percentage illustrated in Figure 2 refers only to the total emissions in the modeling domain by this specific emissions sector and not to the total emissions in this grid cell. In other words, the max of 1.2% is the percentage of total on-road emissions in the domain, not the percentage of total emissions from all sectors in that individual computational cell. We have improved the label of the color bar and also the figure caption to avoid this

misunderstanding. The name of the corresponding month is also included in the figure caption.

(9) *Figure 1: The maps are almost impossible to read, particularly Fig. 1B. The county(?) lines are much too faint. Perhaps it would help if the grid lines for the 1 x 1 km boxes were omitted.*

The maps have been improved, removing grid lines.

(10) *230-234: I was quite confused trying to reconcile the figures with the text, and eventually pulled up maps from Google and Wikipedia. Butler County is to the north of Pittsburgh; the town of Butler is due north and slightly to the east of downtown, and actually outside the modeling domain. I believe that with the exception of line 230, every place in the text that says Butler (and there are MANY such places) should actually be Beaver, which indeed is northwest of downtown Pittsburgh. Making better maps would help.*

We thank the reviewer for correcting this rather embarrassing geographical mistake for authors living in western Pennsylvania. We have replaced "Butler" with "Beaver" county in the manuscript.

(11) *245-250, 345-347: The large contribution to $PM_{2.5}$ from commercial cooking downtown (16%) is remarkable and strains credulity. At minimum, further analysis is warranted comparing these emissions to the inventory. If commercial cooking emissions are really that large, then what about residential cooking? Is this accounted for in "other sources"? Under-represented in the NEI?*

Cooking OA is predicted to be 16% of the total $PM_{2.5}$ in this restaurant-dense downtown area. This rather surprising result is consistent with the measurements of Ye et al. (2018) using an AMS inside a mobile laboratory moving around Pittsburgh. These authors concluded that in the downtown Pittsburgh area, cooking OA contributes up to 60% of the non-refractory PM_1 mass Additionally, mobile AMS results from Gu et al. (2018) showed that cooking OA contributes 5-20% of PM_1 mass over a lot of areas of Pittsburgh. Even if the PMCAMx average predictions cannot be compared directly with these results, they are quite consistent with these measurements regarding the local importance of cooking OA. Similar measurements in Pittsburgh showed that the cooking OA concentrations were clearly elevated in the vicinity of restaurants in contrast to the residential areas (Robinson et al., 2018). A brief discussion of this issue together with the corresponding references has been added to the revised paper.

(12) *351-360: Why is the Mitchell plant plume not visible in the winter? Did the plant operate?*

The Mitchell plant plume is visible in the lower left corner of the Power Generation map in Figure 5. Its plume is not as clearly defined during the winter as during the summer in the maps, because they show the ground level $PM_{2.5}$ concentrations. The emissions stack of this plant is very tall (almost 400 m) and adding the plume rise the effective emission altitude is even higher. As a result, a significant fraction of the emissions from this source

is trapped above the shallow mixing layer especially during the nighttime during this wintertime period and does not reach the ground until it has been diluted. A short discussion of this has been added to the main text and a figure has been added to the supplementary material that shows the average $PM_{2.5}$ concentration from power generation in the upper air layers.

(13) *361-365 and Figure 8: There appears to be a concentrated plume at the central portion of the western boundary of the modeling domain. Is this a wildfire?*

This high concentration area is indeed to the transport of $PM_{2.5}$ from outside the inner domain. It is actually due to power plants and other industrial sources in the Ohio River valley and not to a wildfire. This point has been added to the revised paper.

(14) *392: What is the resolution of the population data? Is it available at 1 km resolution? Given that one of the principal conclusions of this paper concerns population-weighted PM concentrations, more discussion of the population data is warranted.*

Population data is at the census block group level which is smaller than our grid cell size. This has been clarified in the manuscript.

(15) *430: This should refer to Figure 13 (or perhaps the figures should be reordered).*

This typo has been corrected in the manuscript.

(16) *Figure 8: Fix the "Biomass Burning" caption so that it is one line.*

This has been fixed in the manuscript.

(17) *Figure 9: This caption refers to the "Allegheny County simulation domain", which is not mentioned elsewhere in the text. Should this just say "downtown Pittsburgh"?*

This refers to the entire inner domain. References like this have been adjusted in the manuscript for consistency.