

Atmos. Chem. Phys. Discuss., referee comment RC1
<https://doi.org/10.5194/acp-2021-568-RC1>, 2021
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Comment on acp-2021-568

Anonymous Referee #1

Referee 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-RC1>, 2021

Source-Resolved Variability of Fine Particulate Matter and Human Exposure in an Urban Area
Rivera et al.

In this paper the authors apply PMCAMx to model PM concentrations in the Pittsburgh area under different horizontal grid spacings, ranging from 36x36 km to 1x1 km. The maximum and minimum concentrations and variability (standard deviation across grid cells) of monthly mean concentrations are compared for one winter and one summer month in 2017. Zero-out sensitivity simulations are conducted to assess the influence of specific emissions sources within the focal area. Finally, the population-weighted exposure to PM_{2.5} is assessed. The authors show that directly emitted PM components (elemental carbon and organic aerosol during winter) have sharp local gradients at 1x1 km, while components such as nitrate that are formed in the atmosphere are more smoothly varying. The authors find that the population weighted PM_{2.5} concentration changes little across the different grid spacings, suggesting that extremely high resolution may not be necessary for effective exposure assessments.

Determining what horizontal resolution is necessary and/or sufficient is an important question for air quality modeling and exposure assessments. Though the experimental design is rather simple, I am not aware of a study like this having been done previously. However, there are some aspects which need to be clarified before I can recommend publication.

Major comments

- 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 onroad 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.

- 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.

Minor comments by line number

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.

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.

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

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.

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

149-153: Per Table 1, I calculate that on-road emissions are about 2.4% of total emissions. (Total emissions = $7918.5 \text{ kg d}^{-1} \text{ km}^{-2}$ 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 onroad emissions are concentrated in downtown Pittsburgh, but the onroad 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 onroad fraction, but still it seems counterintuitive. What sector is even more disproportionately located downtown (and thus a better tracer for primary PM) than onroad emissions?

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.

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.

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?

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

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?

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.

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

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

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"?