Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC2
https://doi.org/10.5194/acp-2021-568-RC2, 2021

Comment on acp-2021-568

Anonymous Referee #2

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-RC2, 2021

General Comments:

Garcia Rivera and coauthors present particulate air quality model results for the Pittsburgh region using state-of-the-art emissions and relatively robust modeling approaches. The analysis provides a useful modeler-oriented walkthrough of the potential source- and speciation-level changes the scientific and regulatory communities can expect when moving from 36 km resolution (not used anymore for most regulatory applications) to 1 km resolution. The paper is well- constructed and presented, but I have concerns about some aspects of the analysis.

As discussed below, I also urge the authors to discuss more deeply the Environmental Justice (EJ) aspects of this work. There is some mention that the high-resolution data could be vital for EJ applications, but this point is not explored, and the main conclusion that 1 km resolution may not be important for effective urban epidemiological analysis is certainly a stronger statement that leaves a more lasting impression on the reader. I think the data provided here make a strong case for the high-resolution domain to pick up signals for EJ communities, in this case in heavily industrialized areas outside urban cores that may have been overlooked by existing ambient air quality standard enforcement. The framing of this study could be expanded modestly in this direction and provide greater impact on the near-future regulatory landscape.

Major Concerns:

1. Figure 9 provides a helpful framing of the source-oriented contributions to total $PM_{2.5}$. I am concerned about its interpretation though considering Tables 1 and 2. For example, power generation in Table 1 is nearly double the $PM_{2.5}$ emissions of biomass burning. But in Fig. 9, it is about one third of the biomass burning contribution for February. Is this an issue of biomass burning SOA contributing heavily to $PM_{2.5}$ in winter, or has the massive

'other' PM category for power generation potentially been thrown away? In July, Table 2 suggests the power generation contribution to $PM_{2.5}$ should be more like 60%, not 9.5%. This issue is critical also for understanding and discussing Fig. 10. Please confirm that power generation is not seriously underrepresented here.

2. I don't believe the city northwest of Pittsburgh is Butler – it's Beaver. Note that the county directly northwest of Allegheny is Beaver County. Please update this throughout the text and figures. It looks like the particle emissions are mostly sulfate in Fig. 4. If these emissions are due to one source, it would be interesting to identify it. Other well-known sources like the Clariton Cokeworks receive public attention in Allegheny county for their proximity upwind of downtown and yet this Beaver County source (or sources) appears to be crucial for understanding and managing air quality there (160,000+ people).

3. The authors have overlooked a dramatic result from this high-resolution exercise – according to the model, the residents of Beaver, PA are exposed to similar or even higher $PM_{2.5}$ concentrations as if they lived directly in downtown Pittsburgh. I'm sure this would come as a surprise to most of them (especially since it's not EC and thus less routinely visible) and is not captured well by the standard 36 km or 12 km models. Thus reduced-complexity tools like EASIUR, which I believe is resolved at 36 km, likely miss it and managers have potentially underestimated it as well. And to the extent that $PM_{2.5}$ from power generation (see major point 1) may be underestimated by the model, the results may be even more concerning than shown here. The authors provide a couple of figures and basic discussion of the results in Beaver, but I think this dataset provides a real opportunity to frame the analysis from the perspective of EJ, since a relatively small number of people are impacted by a few important point sources that could be regulated with more ease than distributed sources like residential wood or volatile chemical products, for example. In recent years, Shell has opened a massive ethane cracker facility located exactly within the sulfate emission hot-spot, so it would be interesting to discuss the potential impact of that new source, if the authors can find basic annual emission data for that facility (and its support operations) to put it in context with the results presented here.

4. Figures 12-14 illustrate an interesting issue for exposure assessments. We typically think of population-weighting as being the best way to translate air quality model fields to exposure estimation. But in the context of EJ, the real impacts on communities in the upper tail of exposure are obfuscated by a population-weighted approach. I would love to see the authors use the data in this manuscript to clearly amplify this interesting point.

5. It is unclear to me how the fact that population-weighted average concentration is similar among the different resolution cases necessarily means that higher resolution data are not useful for epidemiological work. Is this statement based on the assumption that health data would be matched at the county-level? If census-tract data were used, would the authors' conclusion be different?

Minor Suggestions/ Typos:

- Line 31: Consider adding 'primary' before organic aerosol.
- Line 43: 'reduced lung development and function in children, reduced function in people with lung diseases such as asthma, and…'
- Lines 102-104: For fine and coarse PM, or just for fine?
- Line 104: partitioning
- Line 105: What version of ISORROPIA?
- Line 106: Can you describe the volatility distributions used to describe the POA from the various sources? And what assumptions are being used for IVOC emissions across sources? A lot of information has been written about this over the years, so no need for long discussion, but perhaps a table summarizing the parameter choices (in the SI?) would be helpful. This is particularly important for comparing sources of OA emissions in Tables 1 and 2 and then interpreting Fig. 9.
- Line 109: second aSOA should be bSOA
- Section 2: Can you add some details about the aqueous-phase chemistry package and version?
- Lines 115-120: How much spin-up?
- Line 120: How long does it take the model to run each domain?
- Line 122: Fig. S1 does not have this information.
- Lines 126-127: Interpolating the met fields down is risky and might break the density continuity equation in a big way. Are you concerned about the impact this might have in a place like Southwestern PA with relatively extreme elevation variability, especially in populated areas (i.e. valleys)? An easy way to address this would be to provide some basic evaluation results (e.g. O3, SO2, NO2, total PM, etc.), although the I understand you want to save the bulk of that discussion for a follow-on paper.
- Tables 1 and 2: What is the "other" material coming from power generation? Presumably there are lots of metals in here, but what else? Are these dry particles? Is this mass included in an 'Other' category in PMCAMx or is it neglected?
- Can you also explain in the main text whether these emissions are reflective of filterable or condensable PM for the power generation sector?
- Lines 133-141: Were coarse-mode PM emissions considered?
- Lines 142-153: Were the cooking and traffic emissions calculated specifically for 2017, projected, or estimated for another similar year and pasted in?
- Figs 3, 4, 6, and 7: It took me a while to figure out that these maps have a variable lower color limit. I think that gives the false impression that there are no emissions in areas of the domain for certain species or sources. For example, Fig. 5 shows power generation is elevated in Beaver county, but these emissions are not pictured in Figs. 3 or 4. They should show up in the sulfate map, no? I recommend adjusting the speciation maps to using log color scales and reducing the lower limit to 0. Or you could perhaps choose some clever discontinuous ticks for the axis but that would be tough considering the purpose of showing continuous changes due to increased resolution moving right to left in the panels.
- Section 4.1: All components from figures are discussed except sulfate. It would be interesting to discuss, especially in the context of the other inorganics and spatial refinement of aerosol pH predictions.
- Lines 255-257: Probably worth mentioning that the Bruce Mansfield power plant was shut down in 2019.
- 8: Probably want to move the 'Biomass Burning' label completely outside of the map.
- Lines 381-388: Fig. 11 is referenced but those results are not discussed.
- Lines 413-416: This statement is fundamentally neutral, but the authors may want to

rephrase considering they have tied this work to the goals of Environmental Justice. Some could interpret this statement to suggest that the population density being low indicates the problem is not meaningful. I recommend tying this result directly to Environmental Justice (see major point 3). The same is true for the summer period, where the large values in the 1x1 km case (> 11 ug m-3) are not even mentioned in the discussion.