

Interactive comment on “Ultrafine Particulate Matter Source Contributions across the Continental United States” by Melissa A. Venecek et al.

Anonymous Referee #2

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This manuscript focuses on the simulation of ultrafine particle mass during summer pollution episodes across the United States. Given the recent interest in these smaller particles due to their impact on the health of exposed populations, I find the topic to be relevant to ACP. The paper is generally well written (see comments below), making it straightforward to follow. Tables and Figures are appropriate, as are the citations, abstract, and conclusions. The methodology used is proven and scientifically sound. Based on these, assuming the issues below are addressed, I recommend publication.

1. Page 2, lines 22-23. I would suggest removing “As expected.” This minimizes the work, as if it were expected, why bother?

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2. In general, the paper could deal with some cleaning up of language, punctuation, etc. Examples Page 2, lines 31-32, use ... for commercial use Page 3, line 57, should this be low birth weight Page 3, line 64, word national is not necessary, as monitors in the continental US are specified Page 4, lines 80 and 100 (and elsewhere) US or U.S.? On page 22, line 410, United States is written out. In the SI, page 14, line 118, states is not capitalized. Page 4, line 85, add 'to' between exposure and ultrafine Page 7, line 156, missing a closing parenthesis Page 9, line 194 (and elsewhere including Fig 1 and Fig S2 caption), vs. not vs Page 17, lines 337-340 – Chang et al. (2004) measured. . . Add (2007) to Lane et al. In the SI, Figure S3 should appear after Table S2, as it is cited after Table S2 in the main text. SI, page 14, line 111, Figures compare, not compares

3. In the abstract (Page 2, lines 35-37) and on page 20, lines 364-366), 'higher' and 'lower' ratios should be quantified. Is there a cutoff to determine higher versus lower based on the scatter plot shown in Figure 7?

4. Page 3, line 60. I assume this should be surface area to volume ratio, not just surface area?

5. Page 5, Table 1. Please provide more information about why these 39 cities were selected. Was it the availability of observations? Was it the number of O3 days above 70 ppb? As an example, why Charlotte and not Raleigh, NC? Or why Tulsa and not Oklahoma City, OK? Why were Pittsburgh and Chicago not included?

6. Page 7, line 141. Please justify why nucleation is not considered. This is in line with a later comment about fraction of PM_{0.1} that is secondary versus primary.

7. Section 2.3 and page 8, line 188. Please provide more information about monitors used and the comparison to model output. It says 'measurements averaged' – does this mean multiple monitors were used? Or was a single monitor compared to the model output for the grid cell in which it resides? For cities with multiple monitors with a grid cell/domain, if multiple are used, it would be appropriate to include that information

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(perhaps in the SI).

8. Page 8, line 190. Even though it appears that secondary material is not a huge contribution to PM_{0.1}, it would be appropriate to say ‘also emit ultrafine particles and their precursors’

9. Page 9. In reviewing Table S2, it appears that only one city does not meet the MFE for O₃? If that is the case, it should be more specific on lines 199-200. It would be appropriate to provide the average O₃ model performance statistics at this point. Then at the bottom of the page, the authors could discuss PM model performance statistics (and again, specify that only one city does not meet the MFE for PM model performance). Right now, it is slightly confusing to discuss O₃, then PM, then both in terms of the averages.

Page 10, line 231. While I recognize that the submitted Yu et al. manuscript describes the ‘good agreement’ for PM_{0.1} modeling assessment in California, I think it could be summarized more quantitatively here in only one or two sentences.

Page 11, line 262. I recognize that the focus of this work is summer. However, would it be appropriate to highlight that the biomass contribution might be different in winter when wood burning for home heating could be a prevalent source of PM_{0.1} in colder regions?

Page 13, Figure 2. How does the model convert from OM to OC? Does the two product model used (Carlton et al.) predict OM or OC? I thought it was OM, but if I am mistaken please correct me. If a conversion is done to estimate OC based on the simulated OM, it would be appropriate to include this in the caption to Figure 2.

Page 14, Figure 3; Page 15, Figure 4. Would it be possible to somehow show on these figures the relative contribution of primary PM_{0.1} versus secondary PM_{0.1}? This would truly drive home (and quantify) the relative contributions of direct emission versus in situ formation (I realize it is predominantly primary, but doing this would show it).

Page 18, line 343. This paragraph does not seem necessary to me, as it focuses on previous work that simulated PNC, which as the authors point out in the nucleation discussion (see comment above), is not equivalent to PM_{0.1} (the focus of this work).

Page 22, Figure 8. A suggestion for improved readability: break up this figure into four panels by geographic region of the nation (since the focus is determining how cities in the same region compare – as discussed as ‘regional clusters’ on page 21).

SI, Page 13, line 95. The MFE given in the caption (0.67) is for O₃? The MFE for PM given in the text is 0.75. Please specify both in the caption here. Also, note that the bold lines reflect cities that do not meet one of those criteria.

SI, Tables S3 and S4. These do not appear to be called out anywhere (if they were, and I missed it, I apologize). I assume this is the data that were used to create the vectors for the dot products? If so, that discussion is an appropriate place for them to be called out.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-833>, 2018.

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