Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-832-RC3, 2018

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Sources of Airborne Ultrafine Particle Number and Mass Concentrations in California" *by* Xin Yu et al.

Anonymous Referee #1

Received and published: 30 December 2018

Yu et al., use the UCD/CIT model to simulate ultrafine particulate matter in California, focusing on the Los Angeles and San Francisco areas. To do so, they have developed an inventory of relevant emissions and added a nucleation model to the code. They find acceptable model performance. A particular finding is that non-residential gas combustion is a dominant contributor.

The paper has some interesting aspects to it, particularly the assignment of sources to their impacts on particle number. This may also be its weakness as there is little means to assess the validity of some of the resulting conclusions that might be drawn and the results are striking and don't really line between the model simulations and the observations. Further, they don't bring in recent findings.

Their main result is that non-residential natural gas (NRNG) combustion is the major

contributor to particle number often contributing over half. Looking at Fig. 10, NRNG contributes about 60-70% of the total at almost all the cities (slightly more at Rubidoux, somewhat less at Livermore). This is remarkably consistent given what has been found about the contribution of mobile sources and aircraft emissions to UFPs in other studies (e.g., U Wash, USC studies). They don't include aircraft in these plots: this is a huge shortcoming, and on this alone, the manuscript requires much more work before being considered for publication. A major weakness here is also that the emissions from NRNG, vs. residential NG, is from a recently published manuscript. However, in my reading of that manuscript, they do not include the conditions referred to in this manuscript (a dilution factor of 25), and they seemed to focus on biogas. Maybe the use of the word "same" is of issue here as well. It should be noted that the observations also do not support that the main source is NRNG (and their model results suggest this as well), as particle number increases at night in December, starting about rush hour and going until about 8 pm. This very much looks like mobile source emissions, but certainly not an industrially-related source that would likely decrease after 17:00. During the summer, there appears to be more of a mid-day, photochemically-generated peak. Overall, the observations tend to suggest something very different than the model.

They make the statement that "traffic sources contributed to PNC but did not dominate over regions more than 300 m away from freeways." This is a rather strange statement given that their model resolution is 4 km. They have no way of supporting this statement. Their making this statement is worrisome.

They also state in the Abstract that the performance meets the threshold normally required for regulatory modeling. I am not aware that such a threshold has been set. I don't believe the Boylan and Russel paper is accepted by any agency. Further, they need to be much more informative as to how they actually calculated the performance statistics given that the number concentrations are available at a finer time resolution than the species concentrations often used in performance determinations. Maybe they should also look at the AQMEII studies. The current table of performance (Table

1) is insufficient.

Their reference to Shet et al., referring to Taylor's hypothesis, is not relevant here. Taylor was looking at turbulence correlations, and the relationship between temporal fluctuations and spatial fluctuations. Here, one has to assume that emissions and chemistry play a huge part, particularly since the observations are averaged over scales much larger than the Taylor scale. It was not even apparent why they cited the paper.

Looking at Fig. 12, there are a number of locations where there appears to be a mismatch between 23:00 and 0:00.

Boundary conditions can be very important in regions close to the coast. A diagram of the modeling domain should be provided along with the boundary conditions. A test of the impact of boundary conditions on the results should be provided.

Their modeling domain height is only 5 km. This is lower than most any other model used, from what I recall. Citing some of their studies without really doing a comparison as to the impact of having a higher domain is not sufficient.

What is meant by "Model source code and model input files are available to collaborators via direct email… It should be made available to anyone looking to check their results. A more general statement of availability should be provided. All files and data needed to recreate the results should be available.

Fig. 3. Two issues here. First, the caption suggests that both CMB and UCD results are shown. Are CMB results labeled as "Obs.". This would be a wrong interpretation. Further, how are the uncertainties determined? Second, they should also show secondary fractions.

Figs. 5-6, a correlation plat would be useful. The obs seem to be rather less variable. Fig. 10-11. These results bring up a question: Were the same size distribution on the emissions used everywhere on a source-by-source basis.

Line 514: It should be "under".

Summary: At present, there a serious issues with the paper, including not including aircraft impacts, that the result that NRNG is the dominant contributor does not appear to explain the observations, some statements that are off-base (scale of impact of freeways, performance metric for regulatory acceptance, Taylor's hypothesis) and the need to better describe how performance was evaluated. A major rewrite, alone, may not be able to address all of the concerns.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-832, 2018.