Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
https://doi.org/10.5194/acp-2020-1323-RC1, 2021

Comment on acp-2020-1323

Anonymous Referee #1

Referee comment on "The driving factors of new particle formation and growth in the polluted boundary layer" by Mao Xiao et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-1323-RC1, 2021

Review of "The Driving Factors of New Particle Formation and Growth in the Polluted Boundary Layer" by M. Xiao et al.

This is a well-written and extremely interesting manuscript describing a comprehensive set of experiments on the chemical mechanisms of new particle formation (NPF) in an environment representative of a polluted urban atmosphere. The study provides an explanation for the occurrence of NPF when there is very strong competition for precursor vapors by high concentrations of pre-existing aerosol particles. The successful nucleation and growth of particles in these environments is important, because it distributes aerosol mass to smaller sizes, changing the interaction of these particles with sunlight, the numbers of cloud-nucleating particles, and the fate of the aerosol mass in the human respiratory system. These topics have broad interest, and the manuscript is definitely suitable for publication in ACP.

The investigators demonstrate that sulfuric acid + dimethylamine and other bases like ammonia dominate NPF in polluted atmospheres, even in the presence of high concentrations of oxidation products of aromatics, which can have quite low volatility. The growth of the newly formed particles to larger sizes involves many other compounds, including organics. There is a very strong temperature dependence to the NPF.

This work employs cutting-edge research instruments and the CERN CLOUD chamber to make these measurements. The results are well described and the interpretation is clear. The methods are also well described, and earlier work is appropriately referenced and discussed. The manuscript is very well written. I believe it is suitable for publication with only minor technical changes.

Comments:

1) Throughout the PDF manuscript, following a capital "A" there is a space and the remainder of the word is shifted to the right. This must be some sort of PDF conversion problem, but the authors and copy editors should carefully proof the online and print versions of the final manuscript to see if this issue still exists.

2) It's a bit odd in ACP to have the Methods following the Results. This is more common in journals such as Nature and Science, where a wide audience is presumed and the methods are relegated to an appendix-like attachment, with a smaller font. I don't have a problem with this structure for ACP, but wish to call attention to it in the event there are editorial norms that would suggest moving the Methods toward the front.

3) Line 134: define "GR"

4) Line 138: For clarity, perhaps say "cluster self-coagulation dominates the growth." It took me a moment to understand what the authors were meaning.

5) Line 144: "The multicomponent system used in the simulations. . . ." What simulations? Stolzenburg et al. (2020)?

6) Line 168: The chamber simulations do reproduce observed urban GRs; however, it is probably worth noting here that the more complex chemistry of the ambient urban atmosphere may lead to other species contributing to particle growth than are investigated at the CLOUD chamber. One should not presume that the relatively simple mixtures explored here can replicate the full complexity of atmospheric processes.

7) Line 214: In the previous paragraph you state that the experiments were at the CLOUD chamber; no need to repeat that here.

8) Line 240: DMA is already defined.

9) Line 244: No mention here of how OH was determined; this appears later in the manuscript (Eq. 5*)* but would logically be verbally described here.

10) Line 265: Replace "DMA-train" with "differential mobility analyzer train". Too many "DMA"s.

11) Lines 266-7: Define PSM, SMPS, and CPC.

12) Eq. 1: Perhaps have a subscript "dp" for the J and N variables, since you calculate these values for different size particles (e.g., 1.7 nm, 3 nm).

13) Line 305: Provide model number and company name for the H3O+ CIMS and state the method (e.g., cavity ringdown spectrometry), model number, and company name for the NH3 analyser.

14) Line 311: Is the PTR3 a custom-built instrument? If so, say this and if not, give model number and company name.

15) Line 314: Define STOF and give model number and company name for the "PTRS", which should be "PTRMS".

16) Line 359: Please reference the form of the Cunningham slip correction equation you are using; there are several and they do have some differences.

17) References: Please review the reference formatting and ensure it follows Copernicus guidelines. EndNote-style reference managers always make mistakes; for example, for Breitenlechner et al. the title of the paper is capitalized, and in Dunne et al. the page range is not completed. If you correct these errors now it will save the copy editors the effort of finding all them and asking you to fix them later.

18) Fig. 1. I used a pen to label all the lines and symbols because this is such a busy graph. I suggest you go ahead and do that to make it much easier to interpret.

19) Fig. 1 caption. The penultimate sentence should say that the nucleation rate of H2SO4+DMA at the kinetic limit is shown by the cyan curve.

20) Fig. 2 caption: In the last sentence there needs to be a space in bicycloalkylradicals.

21) Fig. 4 caption: What do you mean by, "At larger particle sizes, the contribution of organics will increase further."? What's the basis for this statement.

22) Fig. 4b: It's hard to distinguish the green and blue curves (biogenic with and without NO) from each other. Can you use a different line type for each?

23) Fig. 5: In the caption please list the name and country of each location indicated in the condensation sink box in the graph, as you did for Fig. 7.

24) Fig. 6. What information is conveyed by the size of the symbols?