

Atmos. Chem. Phys. Discuss., referee comment RC2
<https://doi.org/10.5194/acp-2020-1323-RC2>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.



Comment on acp-2020-1323

Anonymous Referee #2

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-RC2>, 2021

Comments on "The driving factors of new particle formation and growth in the polluted boundary layer" by Xiao et al.

Xiao et al. make an important contribution on nailing down the different role of precursors and environment factors (mainly temperature) on new particle formation and growth in contrast/diverse conditions in the atmosphere. In their controlled chamber experiments, large variability in NPF rate has been observed, which covers the observed variability in the real atmosphere. The results and conclusion will be very helpful on understanding and interpreting the ambient observations of NPF rates and growth, especially in polluted urban environment. The manuscript is well written, and plots are nicely crafted.

I recommend its publication and have following comments for improvements.

- As the main goal of this study is to explain the large variability of the NPF rate in the different environment, it would be nice if the related real atmospheric environment that can be clearly defined, for example, Beijing clean, Madrid, Nanjin, Nanjin Beijing transition, Beijing haze, SE US, SW US, Tartu, Xian, Zurich and so on. I see that the authors have somehow made such attempt by marking them in some plots (e.g., Figure 2A and Figure 5 etc.), but a table of concentrations ranges of the varied precursors, as well as the ambient temperature when the observations were made would be very helpful. I also suggest strengthening a bit the discussion on comparison between chamber conditions and real environmental conditions.

- "These conflicting observations are subject of intense scientific debate of late (Brean et al., 2020; Cai et al., 2020; Guo et al., 2020) and highlight..." It would be nice if the authors could elaborate a bit here what are the "scientific debate" here, and I believe it will help the readers to have a better overview and understanding of the motivation of this study.

- I understand that putting the Method section at the end is probably to obey the formatting requirement of letter, the authors may want to refer to sub-sections, descriptions and equations of the Method section in the main text, so the readers could cross check. For example, how are the "Organic fraction of GR" in Figure 4B derived?

- In Figure 3, organic seems having stronger effects when DMA is absent. Could the authors comment on it? For clarity, I suggest explaining that "Base" means both NH₃ and DMA are present in the figure caption. There is an additional "Figure 3." in the figure caption.

- In Figure 5, survival probability in Beijing haze condition seems extremely low. Here, CS₆ is used, and I am wondering how pollute are the Nanjin Beijing transition and Beijing haze condition? E.g., PM₁ or PM_{2.5} concentrations? A related question is that "polluted boundary layer" is with what PM₁ or PM_{2.5} levels approximately?

- "We compare chamber simulations and atmospheric observations in Fig. 7 using sub-3 nm GR as a proxy for total condensable vapour concentrations..." It seems that total condensable vapor concentration in BJ is not high as marked in Figure 7. One would expect that high as the pollution level there is quite high. Could the authors comment on it?