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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The efficiency of secondary organic aerosol particles to act as ice nucleating particles at mixed-phase cloud conditions" *by* Wiebke Frey et al.

Anonymous Referee #1

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This manuscript examines biogenic and anthropogenic SOA surrogate particles for their ice nucleating ability. The SOA particles were photochemically generated in the Manchester aerosol chamber (MAS) and then transferred to the Manchester Aerosol and Ice Cloud Chamber (MICC) where ice nucleation was probed between -20 C to -28.6 C at water saturation mimicking mixed-phase cloud formation conditions. Reference ice nucleation experiments employing ammonium sulfate and kaolinite particles were conducted. Under probed conditions only kaolinite particles initiated ice nucleation.

The authors present a study of increasing interest, i.e. if and how organic, in particu-

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lar, secondary organic aerosol (SOA) particles form ice in the atmosphere. This is an important topic and I am in support that new experimental results should be published. However, I find that this manuscript lacks discussion of recent literature on ice nucleation and diffusion of SOA particles to set the new results in the right context. SOA ice nucleation or diffusion has been studied by several groups in recent years (among others, Wang et al., 2012, Ignatius et al. 2016, Mohler et al., 2008, Charnawskas et al., 2017, Price et al., 2015, Wagner et al., 2017, Lienhard et al., 2015, Kanji et al., 2017, Ladino et al., 2014, recent review by Knopf et al., 2018). These papers should be present in introduction and may be further discussed in other sections of the manuscript.

Furthermore, the SOA generation procedures may vary among this and other studies. This should be mentioned/discussed in places.

I find the supplemental material should be better implemented within the main text. As is, there are some notes to it, but the supplement has a lot of important information. I feel the bounce experiments would be better situated in the main text, also to be more visible, but I leave this to the authors.

The figures in text and supplement reporting ice nucleation experiments should also include the supersaturation of ice, Sice. This is crucial information missing.

I recommend that the abstract states explicitly the particle systems investigated for ice nucleation

p. 2, l. 10-15: other studies mentioned above in general comment should be mentioned.

p. 3, l. 24-25: The 33 m transfer line. You show data later but please elaborate on particle losses due to diffusion, gravitational settling etc. What is the flow speed and pressure in this transfer line? Since this is a new experiment, it would be beneficial to know these parameters.

p. 5, l. 4-8: Here, I would give the bounce experiments more exposure. “Bouncy” is not really a physical parameter, is it possible to use phase state definitions, such as semi-solid, solid etc.?

p. 5, l. 19: Here, the reader learns the first time the NOx is involved in SOA formation. This can be different from above mentioned studies. What does this mean for SOA composition, viscosity etc.? This could be important but is not discussed.

p. 5, l. 26 – p. 6, l. 8: Some details are not entirely clear to me: The SOA from MAC flows into evacuated MICC. Then MICC is filled with gas. Do you expect losing SOA species due to evaporation (low pressure) and due to dilution? The VOCs then diffuse to the cold walls of MICC? Also going from a warm (MAC) to a cold environment (MICC), does this not affect RH fields, thus affecting organic phase state?

p. 6, l. 24-25: The air from MAC was humid and entered MICC. Are the particle RH trajectories known for the transfer? Does this impact phase state? See, e.g. discussion in Ignatius et al. (2016) and Knopf et al. (2018). The humid air condenses onto cold MICC walls?

p. 6, l. 30-31: Here and Fig. 4 case: Are activated droplet sizes what would be expected from Kohler theory and diffusional growth?

p. 7, l. 27-35: For this discussion it is crucial to know also the temperature and Sice values during measurement of the activated fraction. At this point the discussion is confusing and one wonders about these results. Maybe at fast pumping speed, i.e. at high Sice, the activated fraction of HD is not as sensitive compared to lower pumping speed and thus lower Sice?

p. 8, l. 16-21: Is it possible to make this speculative discussion a bit more quantitative?

p. 8, l. 29-31: I find this too simplified and feel it needs more discussion. Please look at studies mentioned above in general comment.

p. 9, l. 1-5: I find this needs more discussion. Please look at studies mentioned

above in general comment. SOA particles are produced in different ways, different temperatures are probed etc.

p. 9, l. 12-13: Different heptadecane properties due to different VOC/NOx ratio applied?

p. 9, l. 14-15: Not necessarily higher supersaturation are needed but longer times. The need of larger supersaturation may be “apparent”, i.e. disequilibrium between gas and condensed phase.

p. 9, l. 15-16: Indeed, different temperature in chambers or sampling lines can affect particle properties. See e.g. discussion in Knopf et al. (2018) and Ignatius et al. (2016).

p. 9, l. 28-30: Above mentioned literature may enhance this discussion.

p. 10, l. 5-6: Please refer to, e.g., Wagner et al. (2014, 2012) articles.

p. 10, l. 16-18: Heptadecane is more viscous and therefore the activated fraction is lower?

Figures: 3-5 and similar ones in supplement: specific for figures in main text: particle images are not described in caption. Panel indicators are missing. As stated above, please include Sice. It is confusing to have a legend in third panel that includes definitions for other panels. Please split legend to corresponding panels.

Figure 5: What do you mean by “eating up”? A Wegener-Bergeron-Findeisen process? Please change expression.

Table 2: You mean MAC and not “aerosol chamber”? “Amount injected”: unit? Please elaborate. Have aqueous solutions been injected? What is the mass? The mole fraction or other information is needed. Units missing for gas species. The mass difference, last column, between pinene and TMB is correct?

Table 3: How can mass in MICC be larger than in MAC (mass_DMPS vs.

mass_SPMS)?

Supplement: p. 1: change “cooking” to “processing” or other terminology.

Figure S4: Please include pinene SOA bounce fraction.

p. 4: Change “ingredients” to “species” or “compounds”, etc.

What is the difference between S17 and S19 and S18 and S20 experiments? Maybe additional text is necessary?

Technical corrections:

I suggest throughout manuscript and supplement to change the expression “cloud evacuation” to “cloud activation experiment” or something along those lines.

p. 7, l. 25: Change language. Avoid the term “sister run”.

p. 8, l. 26: Maybe use “employed” or ”applied” instead of “used”.

p. 9, l. 8: Avoid “kicks in”. Change language.

p. 9, l. 30: Exchange “than” with “as”.

p. 9, l. 32: Instead of “sucking” use” pumping” or “evacuating”.

p. 10, l. 6-7: It feels there is an error in this sentence.

p. 10, l. 30: Exchange “no” for “not”.

p. 11, l. 22: Exchange “bump” to “maximum” or similar.

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