

Interactive comment on “Competition between water uptake and ice nucleation by glassy organic aerosol particles” by T. Berkemeier et al.

Anonymous Referee #3

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First of all, I am very sorry for the delay with sending these comments.

This paper presents model calculations and conceptual analysis of the competing effects of particulate phase diffusion vs. ice nucleation, commenting on the ways that organic aerosols can interact with clouds. The paper makes useful points and is probably a good starting point for further studies, although a lot needs to be done still to resolve the different processes through which organic aerosols interact with clouds. I think this paper can be considered for publication in ACP once the following points, along with the points raised by the other reviewers, have been addressed by the authors.

Major/general comments:

C6442

1. If I understand correctly, you assume constant hygroscopicity parameter for your mixtures. As you probably know, however, the hygroscopicity parameter depends on the RH/supersaturation in case the organic material is not completely dissolved. Now the authors limit the discussion in the main paper to kinetic transport vs. ice nucleation, while the organic solubility is not discussed at all. I think the authors should add a discussion on how the solubility of organic compounds and its dependence on external conditions (water content, temperature) would affect the results / play into the conceptual scheme.

2. Besides the solubility, another thermodynamic parameter that is likely to be strongly influenced by the water content as well as temperature is the volatility of the organic material. I think this too deserves some discussion on the paper. This paper seems to focus on the importance of the phase state on the kinetics of water in the organic matrix, but I think the importance of the phase state for the energetics/thermodynamics of the organic system (manifested in solubility and equilibrium vapour pressures) deserve some discussion as well.

3. I would have appreciated a discussion on the potential limitations of the model and what kind of experiments the authors would need to constrain it better.

Minor/specific comments:

4. Abstract, p. 16452, line 20: I find the concluding statement of the abstract a bit too general and vague. Please be a bit more specific here. What kind of formalisms are needed? What would be the first, most critical, improvements in the atmospheric models that one should start with?

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 16451, 2014.

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