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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Viscous organic aerosol particles in the upper troposphere: diffusivity-controlled water uptake and ice nucleation?" *by* D. M. Lienhard et al.

Anonymous Referee #1

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Review of "viscous organic aerosol particles in upper troposphere: diffusivity-controlled water uptake and ice nucleation?" by Lienhard et al.

This manuscript investigates diffusion rates of water in one type of SOA (SOA generated by the photooxidation of alpha-pinene) and several proxies for SOA. In addition, implications of these results for the atmosphere are carefully considered. The measurements of diffusion coefficients of water in SOA are an important contribution and the modelling studies exploring the implications of the results are excellent. I highly recommend this manuscript for publication in ACP after the authors have addressed the following comments:

Comments:

1) In the abstract the authors should indicate the type of secondary organic aerosol (i.e. photooxidation of alpha-pinene) they investigated.

2) Abstract, lines 20-25; Page 24485, lines 3-6; page 24486, lines 3-5: The authors state that "condensed-phase water diffusivity is unlikely to have significant consequences on the direct climatic effects of SOA particles under tropospheric conditions." I assume here the authors are referring to the "direct effect of aerosols on climate" whereby the aerosols can affect climate by scattering and absorbing radiation. The authors may want to define the "direct climatic effect" somewhere in the manuscript for clarity.

3) Methods. It would be useful to add a few additional details on the conditions used when generating SOA in the PAM. For example what was the mass loading and collection time.

4) The SOA results shown here are made with one type of SOA (photooxidation of alpha-pinene) generated with one oxidation level (O:C = 0.53). The authors then use results from this SOA and other proxies of SOA to make general conclusions on the atmospheric importance of condensed-phase water diffusivity. How confident are the authors that these results will extrapolate well to other types of SOA? Somewhere in the manuscript the authors should add the caveat that the results and implications were based on one type of SOA, and further studies are needed to understand the importance of water diffusion in other SOA.

5) Figure 5. In the simulations, how much does the homogeneous freezing temperature change as the updraft velocities are changed? It would be useful to state this information somewhere in the manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 24473, 2015.