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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Diffusivity measurements of volatile organics in levitated viscous aerosol particles" *by* Sandra Bastelberger et al.

Anonymous Referee #2

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The authors present an important addition to the literature on diffusivity in potentially viscous aerosol states. The focus on organic compounds takes the research in a direction that is essential to quantify any potential effects. I recommend publication after some minor general comments are addressed.

Is there a potential for a 'non zero' limiting partial pressure of the evaporating organic to bias any inferred diffusion coefficient in these experiments? I appreciate this might be very difficult to quantify, but please add to any appropriate references. One might expect this would only really be a problem for low viscosity states.

I am a little confused by the comment in point '2' on page 9 that you assume the condensed phase diffusion of PEG to not rely on its own concentration? Given the

mixture composition will dictate the changing viscosity, dosnt this restrict the use of your fit in other studies? Or, is it that you are targeting the use of your fit in other mixtures where you only need to track water?

How do you account for changing solubility as the droplet composition changes in additional studies? Given you account for non-ideality as noted in point 5, does this treatment persist between each condensed shell? It seems in section 4.1 that you optimize the activity coefficient, but how much error is inherent from other experimental sensitivities in this procedure? In other words, would it be possible to show the impact on inferred diffusion if you used a purely theoretical calculation of activity coefficients? It might be in more complex systems, unless you could use a robust multidimensional optimization strategy, this would be required anyway. I appreciate you discuss some of these issues in section 4.2 but the paper would benefit from a 'pure' sensitivity simulation. Figure 5 is very useful by the way.

Could you please add more specific details on how you optimize your activity coefficient values? Is this done by a specific algorithm, set tolerance or by eye?

On this note in section 4.2 the authors comment on the challenge in propagating errors in this model. With the availability of some algorithms to do this, it would be great to add a note on where others might be able to obtain the code to perform such calculations or how best to collaborate on this.

I find section 5 an excellent addition to this paper and studies restricted to studying a small subset of atmospherically relevant organics. It begs the question how we might move towards more general quantification. Perhaps a useful addition is to ensure we start to study affects in ensemble populations through chamber and box-model studies. The impact on size distribution should also be studied.

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