

Interactive comment on “The effect of hydrophobic glassy organic material on the cloud condensation nuclei activity of internally mixed particles with different particle morphologies” by Ankit Tandon et al.

Anonymous Referee #2

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In the study at hand, Tandon and co-workers investigate the role of hydrophobic organic material on the cloud condensation ability of ammonium sulfate (AS) particles by coating them with polyethylene (PE). In this careful study with innovative design, no effect of the coatings was observed, rendering hydrophobic organic coatings as unimportant under the investigated conditions. The paper is well-written, the topic is of high relevance to the Atmospheric Chemistry and Physics community and fits well within the scope of Atmospheric Chemistry and Physics.

Regarding the title, I have two issues with the use of “internally mixed” here.

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1. This paper really rather looking at the effect of organic coatings on inorganic particles (admittedly, by creating an internally mixed particle) instead of the effect of organic coating on internally mixed particles.
2. While it is a set phrase in the aerosol community, it might paint the picture of hydrophobic substances being “mixed in” with inorganic substances.

There are not big issues, but the title threw me off slightly in the beginning and I wonder if this addition is needed.

The biggest critique for the relevance of this work must be, as the author's state themselves in their discussion, the relevance of PE coatings for atmospheric particles. PE is indeed very different to the most common organic material in the atmosphere, Secondary Organic Material (SOM). The authors argue that SOM is rather irrelevant for this study since it is hydrophilic and hence would activate regardless under supersaturated conditions, but the authors have to do a better job communicating this early on in the article and providing proof. Since only a few corner cases can be thought of where a hydrophobic, viscous coating similar to PE might be generated in the atmosphere, it seems correct to at least describe the expected behavior of the vast majority of organic material that is present in the atmosphere. Here, it would be worthwhile to briefly summarize the effects of these glassy, but hydrophilic, coatings to water uptake under subsaturated conditions, a research topic that has received much attention in the past (Mikhailov et al., 2009, Zobrist et al., 2011, Berkemeier et al. 2014, Pajunoja et al., 2015, to name a few studies that looked at water uptake [kinetics]). The authors discuss this issue very openly in Sect. 4, however, this discussion is absent from the introduction. The abstract reads especially strong: “it is concluded that mass transfer limitations by glassy organic shells are unlikely to affect cloud droplet activation near laboratory temperatures”. As long as the authors do not show experiments with atmospherically more relevant material, this conclusion cannot stand by itself (also, p.9, l.17 needs a reference).

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The authors describe their experimental and theoretical approach in detail. However, could the authors, in simple words, recite from their 2016 paper (Rothfuss and Petters, 2016) how they infer the slope of the viscosity vs temperature curve from a coalescence experiment, which to me seems to be more like a point measurement of viscosity?

The authors make a reasonable case that the PE/AS dimer particles should exhibit core-shell morphology (e.g. change in mobility diameter), but also point out that they have no exact proof for that. Since these doubts are communicated clearly, I can concur with their approach.

The authors give an explanation why the activated fraction for the core-shell particles never reaches unity (Figs. 4 and 5), a curious observation since without the thermal conditioning step, the activated fraction seems to go a little higher (at least at 0.62 % water supersaturation, Fig. 4). While their explanation seems logical overall, can they exclude the possibility that some of these 10-20 % of particles are PE/AS particles and don't activate because they are the only ones having a "true" core-shell morphology (not cracked, not partially-engulfed)? In general, the fitted line shown here is somewhat misleading since it doesn't account for the fact that activation does not reach unity.

Hence, despite few, small issues, I find this work very publishable and an interesting data point for the relevance of organic coatings for cloud activation. I can recommend it for publication in ACP after minor revision.

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