I would like to thank the authors for their responses to the referee comments and think the manuscript has improved from the last iteration. However, the overall impression that this manuscript is poorly presented in text and figure, remains. Arguments are often difficult to follow and overly long sentences distort the reading process. Figures are difficult to read and often not well labelled. Also the newly added paragraphs are often not well-crafted and contain orthographical or grammatical errors.

However, presentation aside, I would like to raise a major and a minor point on the science that I would like to see addressed before I can recommend this article for publication in ACP.

Major Comment - Line 177f

“Aerosols in a highly viscous or a semisolid state are assumed to be phase separated in a core-shell morphology as a model simplification (refer to Section 2.3). This simplification is based on recent observations showing higher than anticipated rebound fractions in phase-separated OA that has been attributed to organic aerosol constituents with viscosities > 102 $Pa.\ s$ (Reid et al., 2018).”

I still do not understand this logic. Just because phase-separated particles have been seen to rebound, does not mean that every particle whose organic phase by itself would be semi-solid, undergoes phase-separation. The authors also state this in their rebuttal:

“Furthermore, there is little information on the criteria necessary for a particle that is specifically in a semi-solid or glassy state to undergo phase separation. (…) The threshold of viscosity > 100 Pa.s, only indicates the transition in phase state of the ‘organic coating’ from liquid (< 100 Pa.s) to semisolid or solid (> 100 Pa.s) and should not be confused with dictating phase separation itself.”

I also cannot find information about the rebound fractions of phase-separated OA in Reid *et al*. (2018).

The authors further state:

“Therefore the 100 Pa.s cut off was chosen as an extreme upper bound scenario for particle morphology to better understand the effects of a core-shell morphology on the heterogeneous chemistry of IEPOX.”

Where does the better understanding come from if the authors over-declare particles as phase-separated (68.7 % vs 42.7 % in updated simulation PhaseSep2)? Does this not simply inflate the modelled effect of phase separation? Fig. S6 paints a much more modest picture of the effects of phase separation compared to Fig. 8. It is difficult to compare these two figures though, since the color axis is very different. In my opinion, this publication should focus on scenario PhaseSep2 as the more realistic scenario. Why is PhaseSep2 not used as default scenario?

Minor Comment - Lines 560-565

“Wider ηorg ranges in the higher RH bins can be explained by increased diffusivity with higher aerosol liquid water in SOA causing faster mixing times leading to quick changes in composition. Furthermore, these contain the most atmospherically relevant RH ranges for the model simulation period. Therefore, a wide range of particle compositions is expected which also contributes to the wide range of predicted ηorg values.”

This is a poorly crafted argument. (i) Why are “quick changes in composition”, affecting ηorg? Why is there a time domain of importance? (ii) a larger sample size should not affect quartile position.