While the author responses to the reviewer comments were largely dismissive, the resultant changes have improved the manuscript. From the perspective that it is part of a reviewer's job to provide comments that will help improve a paper and increase its impact, it is suggested that the authors carefully consider the following comments, as well as those made previously, when finalizing the manuscript.

Partitioning/Solubility:
In regard to phase partitioning and solubility, the added paragraph clarifying that WIOM does not necessarily imply that organic compounds in the WIOM phase have low water solubility, should help clarify the authors' conceptualization of the phase space. In that paragraph the author's state: "Even relatively water-soluble compounds can have a higher preference for solvation in an organic phase than in a mostly aqueous phase." This is in fact shown in Fig. 6A for oxidation products of alpha-pinene, when the absorbing phase is the ester dimer or salting out is considered. Zuend and Seinfeld (2012) noted the same salting-out behavior: "Thus, while the SOA from α-pinene would classify as water-soluble organic carbon (WSOC) (e.g. Sullivan and Weber, 2006), it does not mean that WSOC compounds partition preferentially to what is commonly called the "aqueous phase"; rather, the concentration of dissolved inorganic ions in the aqueous electrolyte-rich phase will be a key influence and most often prevent WSOC from partitioning substantially to the aqueous phase." In Fig. 4 of Zuend and Seinfeld it can be seen that at water activity <0.86, all organic compounds considered are predicted to partition preferentially to the organic phase (or "aqueous organic" phase).

In section 3.2, on lines 368-369 the authors categorize compounds as water-soluble (WSOC, preference for aqueous phase). On lines 375-376, the authors report that less than 1% of a WSOC partitions into the WIOM even at high organic loadings. I find this use of terminology to be inconsistent with previous uses of the term WSOC (e.g., as referenced in Zuend and Seinfeld) and our (the authors and my own) shared understanding of solubility and phase partitioning, namely: 1) "Even relatively water-soluble compounds can have a higher preference for solvation in an organic phase than in a mostly aqueous phase", and 2) "For the phase distribution of an organic chemical in a phase-separated aerosol, it is not this water solubility that is relevant (there is no pure substance present in the aerosol), but the compound's relative solubility in the mostly aqueous and in the mostly organic phase." (from the author comments). The main point here, is that this use of terminology may lead to confusion and misinterpretation of the results, and may hinder placing the work in the context of other partitioning studies of atmospherically relevant compounds.

Activity Coefficients:
On page 492, the authors conclude that the SPARC predictions are suspect because they imply activity coefficients of the compounds in different solvents can vary by four orders of magnitude, "which is not very plausible". Variations of that magnitude, for mixtures of organic compounds, have been reported previously. See for example: Barley et al., Atmos. Chem. Phys., 11, 13145–13159, 2011.

If the properties of the solvents and partitioning compounds were summarized in a table, the latter by parent compound, it would facilitate assessment of whether such a variation in activity coefficients was indeed plausible and comparison with the results of Barley et al. (and other relevant studies). While the necessary information for the partitioning compounds is presented graphically, it is not easy to digest since the parent compound identities and various physical properties are all in separate figures.

Notation in Figures:
It is suggested the $K_{WIOM/G}$ partitioning coefficients for the individual solvents be represented using consistent notation in the figures (e.g., Fig 6a,b). One suggestion is to use: $K_{WIOM/G}$ (B), $K_{WIOM/G}$ (ester dimer), $K_{WIOM/G}$ (C813OOH).