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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Particle partitioning potential of organic compounds is highest in the Eastern US and driven by anthropogenic water" *by* A. G. Carlton and B. J. Turpin

Anonymous Referee #1

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In general, this is a well written, appropriately cited manuscript that briefly and succinctly presents a modeling analysis that indicates that aqueous phase partitioning drives SOA formation in the eastern US and that the formation of this aerosol aqueous phase is controlled largely by anthropogenic emissions of SO2 and the subsequent formation of sulfate aerosol. Its subject is of great interest to the atmospheric chemistry community, and the subject matter is very appropriate for ACP. I recommend its publication but feel that the manuscript can be improved if the following issues are addressed. Scientific significance: good; scientific quality: good (both these could be excellent if issues below are addressed); presentation quality: excellent.

Major comments

It would be appropriate to note or summarize those modeling efforts that have focused on aqueous partitioning and to indicate their findings (i.e., the Pun et al. model, any modeling efforts that simply used Henry's law). The Pun model allows for the type of aqueous partitioning process described by the authors, but it may underestimate the aqueous process by not considering enough of the gas-phase oxidation products (what the authors call WSOM).

In general, it should be better emphasized that the water-soluble organic gases that can form aqueous SOA and the semi-volatile organic gases that partition into OM are not necessarily two different sets of compounds. Although the WSOM species are too volatile to partition into the OM according to their vapor pressures, there are likely reactive pathways that lead to these species being present in the OM. Correspondingly, it is highly probable that some of the species included in the semi-volatile class are at least partially water-soluble. This type of analysis would be best served by a model that allows species to partition into whichever phase is preferred or to split their mass. I recognize that this is a large model development effort that is likely beyond the 'proof of concept' idea of this manuscript. Still, I believe it should be better emphasized as a limitation in the manuscript.

Page 12751. I understand why the aqueous SOA forming potential is set to unity. However, would it be possible to use a range of values based on their Henry's law constant? Or based on some other parameter that is available in the model (including those mentioned by the authors and the free acidity). With regard to the free acidity, I understand that quantity is linked fairly strongly to particle water and such, but it was not until I read the supplement that I truly made this connection and saw how it fit into the authors' arguments. There is adequate room in the manuscript for the supplement to be incorporated into the body of the text, obviating the need for the supplement altogether.

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Following on the previous comment, why not just do a first-order simulation in which individual WSOM material is allowed to partition to the particle water via its Henry's law constant? The WSOM would then distribute itself based on changes in said water with meteorology, changes in aerosol predicted by ISORROPIA etc. One could then look at the change in OA compared to a base case. This would then truly address the issue without necessarily having to rely on showing the absolute concentrations of all of the species and making a qualitative comparison (which are good frameworks and introduction for the full model run). I recognize that there would be uncertainty in this type of model calculation (i.e., the phi values discussed above are not unity), but I believe this would be an appropriate addition to paper if possible.

Minor comments Line 9, page 12744 needs a semicolon

Line 24, page 12744 Yet should start a clause, not a sentence

Line 3, page 12745 add 'as a result' at the end of the sentence

Line 16, page 12746 although likely minor, I assume that the aqueous phase also becomes a positive feedback on the formation of aqueous SOA from anthropogenic precursors (since they might not have been emitted from the same source as the SO2). This could be included.

Line 5, page 12748, is it Aiken or Aitken?

Line 8, page 12748, the nomenclature for the inorganic ions should be explained

Line 25, page 12753 And should start a clause, not a sentence

In Table 1, it appears that acetic acid and peroxy acetic acid are mixed up, values should be corrected (i.e., acetic acid does not have a MW that is greater than that of peroxy acetic acid)

For clarification, in Figure 2, it specifically says that all species in Table 1 are included. It does not say this in Figure 1. Are we comparing apples to oranges then? Should

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primary VOCs be included?

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