**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Investigating the influences of SO$_2$ and NH$_3$ levels on isoprene-derived secondary organic aerosol formation using conditional sampling approaches" *by* Y.-H. Lin et al.

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Received and published: 11 March 2013

In a literature group meeting in our research group, we read this manuscript, and comments are based on that group discussion.

We certainly enjoyed the publication and appreciated that it provides strong evidence through the molecular speciation and quantification of SOA oxidations that a large portion of the ambient organic carbon can be related to the photo-oxidation of isoprene under low NOx conditions. We really enjoyed this story.

We had two comments that we think the authors could consider for greater clarification of the manuscript.

1. The first comment relates perhaps more to our own group's work and less to the authors, but nevertheless is quite germane to use of the AIM and Figure 9 of the manuscript. The authors appear to have run the AIM model with the assumption of no interaction between the organic material and the inorganic material. Our findings presented in Smith etl., ACP, 2012, 12, 9613-9628 show that isoprene-derived secondary organic material mixes miscibly with aqueous ammonium sulfate. In this case, the efflorescence and deliquescence points of the mixed particles are shifted. The bottom line result is that Figure 9 of the authors' manuscript might not be accurate with respect to the cases of "no LWC".

2. The second comment, unlike the first, relates strongly to the authors' thesis, both in the title and in particular in the strong sentence in the abstract, which reads: "IEPOX-derived SOA tracers were enhanced under high-SO2 sampling scenarios..." We as readers were not able to locate a compelling data set or arugment presented in the manuscript in this regard, so the authors might want to consider some clarification or clearer statements.

2a For instance, the highest SumIEPOX/OM between Tables 2 and 3 is 19.1% and occurs for the case of high NH3. Our understanding would be that this data set on its face would then be entirely opposite to the statement in the manuscript.

2b. In Table 2, we wonder why "low SO2" and "high SO2" have yields of 11.9% and 13.3%, i.e., again not a strong statement of an influence of acid and perhaps just a correlation with total available surface area.

2c. We would wonder, in relation to the authors' thesis of the importance of acidity, of why there are yields >0% for either "low SO2" (Table 2) or "high NH3" (Table 3).

2d. The organic carbon data of Table 1 appears to us, within uncertainty, to be inde-

pendent of the 4 conditional sampling strategies.

In regard to these comments, likely we as readers have a misunderstanding and so in this regard some clarification from the authors would be valuable for the readers (at least for us).

One possibility occurring to us is that acidity is important but that the history of the particles in the atmosphere is complicated (e.g., perhaps starting as acidic and then becoming more neutralized with time) and, if this is the case, the fact remains that the data that could be collected (i.e., representing an observation after integration) does not appear (at least to us) to provide positive evidential support to the statement in the abstract.

We enjoyed the paper in the literature discussion, and we hope that our feedback as readers can be useful to the authors.

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