

Interactive comment on “Enhancement of Secondary Organic Aerosol Formation and its Oxidation State by SO₂ during Photooxidation of 2-Methoxyphenol” by Changgeng Liu et al.

Anonymous Referee #1

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This is an interesting manuscript describing experiments on SOA formation from guaiacol oxidation by OH radicals in various chamber conditions (seed concentrations, NO_x levels, and SO₂ levels). While the experimental results are certainly worth publishing, the interpretation of those results is extremely speculative and, while plausible, for the most part unsubstantiated. This discussion needs to be significantly restructured.

My first issue is with the discussion around seeds. First and foremost, seeds provide surface area for condensation. It is essential that the authors consider the microphysics of condensation. Specifically, the authors can integrate the smps data to determine the "Fuchs corrected surface area" and relate that to the condensation sink of vapors of

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some chosen molecular weight (conventionally, 98 g/mole is common because that is H₂SO₄, but a number more like 300 g/mole may be more representative of condensible organic vapors). It is essential to consider the condensation sink at the onset of precursor oxidation as well as the average condensation sink over the course of an experiment. The most straightforward consequence of having no seeds at all (so-called "nucleation" experiments) is an "induction" period first described by Kroll et al (EST 2005). Simply put, ELVOC and LVOC products driving nucleation and growth are lost to the chamber walls with the vapor loss timescale (which MUST also be reported here, for some species with a known diffusion constant - it scales with sqrt(D)), and so the overall SOA mass yields are reduced because of this wall loss. This induction period is clearly evident in Figure 2. Because of this, the "null hypothesis" for the seed effects is that the condensation sink of the seeds differs for the different seed types, and that the condensation sink additionally grows when SO₂ is also oxidized, causing 20 ug/m³ or so of added sulfate condensation as shown in Fig S3. Until there is a coherent discussion of the condensation sink for these various experiments (and, ideally, the ratio of the condensation sink to the vapor wall-loss timescale), it is difficult to assess all of the other interpretation.

My second issue is with the mechanism by which SO₂ might lead to an increased OSc. It is possible that the SO₂ simply results in a higher condensation sink, as discussed above, but it is suggested that the SO₂ oxidation catalyzes SOA formation (and specifically highly oxidized SOA formation). It seems implausible that SO₂ would act as an oxidant, and so it (or H₂SO₄) would need to enhance condensation of vapors that were already highly oxidized. This is NOT completely implausible (i.e. condensation of glyoxal via an acid catalyzed reactive uptake process) but we would need to see actual direct evidence for such a pathway.

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