

Interactive comment on “Increased Primary and Secondary H₂SO₄ Showing the Opposing Roles in SOA Formation from Ethyl Methacrylate Ozonolysis” by Peng Zhang et al.

Anonymous Referee #2

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Zhang et al. investigated the formation of secondary organic aerosols produced from the ozonolysis of ethyl methacrylate under different experimental conditions (RH and seed aerosols). SOA were characterized using mass spectrometry and particle formation monitor using an SMPS system. Overall, the paper is very hard to follow and the discussion/interpretation weakly constrained. As a result, I recommend that the authors restructure the manuscript and provide deeper/quantitative analyses.

Method parts: The authors should discuss the effect of CO, how much did CO decrease the OH chemistry? Overall you should be more quantitative when presenting the experiments and results.

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The authors mentioned that they used a nano-DMA, which is not possible as a nano-DMA can go up to 150 nm only.

Please provide the number of particles as well and the evolution of the mean size. In addition, you need to provide the surface area of the particles for all the experiments. Size diameter is very different between the experiments which can play a critical role in the difference observed in the study.

I would suggest also using a numbering system to discuss the experiments, it would help the reader.

All experiments need to be presented in the corresponding section. The authors discuss additional experiments within the result section within providing any information.

Results:

Line 176: Why did you look at the mass of sulfate and not organics to show the SOA formation? Sulfate cannot be considered as a secondary particle → please provide the mass of organics. Please estimate the acidity for AS, AAS, and nucleated H₂SO₄ particles

Line 182: The authors should discuss aerosol surface areas before reaching such a conclusion. Provide the surface areas for the different experiments performed in this work?

Line 184: which level/concentration?

Lines 191-192: The wording is inaccurate, SOA formation is not suppressed by reduced.

Lines 199-204: I found the impact of RH more dramatic than the impact of the aerosol acidity (i.e., reduced by a factor of 2). The authors should discuss more this aspect. I also find the conclusion of the authors poorly constrained. More details on surface area and acidity must be discussed. Indeed condensation of H₂SO₄ will result in acidic

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seed particles.

Lines 227-230: Are the authors suggesting that EM is oxidized by O₃ in the condensed phase and sCI react with SO₂ to form H₂SO₄ in the condensed phase? This sounds quite speculating with the data shown in this paper. In addition, if acidity aerosol promotes condensation of oxidized species, the results should show a greater formation of SOA in the presence of acidic particles, which is not the case. Later the authors mentioned that this "speculation" (line 232) is supported by some experiments. However, we have very little information on these additional experiments. For example, how experiments performed using an ATR can be used to simulate chemical reactions potentially occurring at the interface of an aerosol? The results briefly discussed in the SI are not convincing.

Lines 240-243: This is confusing, what do the authors mean?

Lines 244-248: That's not consistent with previous works and previous studies must be discussed here. It is actually a bit concerning if the authors observed a different tendency.

Lines 262-264: It is not clear why the authors decided to run PMF. By looking at the results I found the discussion too qualitative and at some places even contradictory. I strongly suggest that the authors provide a simpler look at the AMS data first and then provide a more deep and consistent analysis.

Finally deriving a chemical mechanism from the dataset (i.e., identifying chemical products using an AMS) presented in this manuscript appears quite speculative. Indeed, without additional evidence (e.g., additional MS data) confirming the presence of the proposed molecules in the gas and/or particle phase it is not constrained enough.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-929>, 2020.