Atmospheric Measurement Techniques Discussions

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Addendum to RC2 review

Anonymous Referee #2

Referee comment on "Characterization of the MISG soot generator with an atmospheric simulation chamber" by Virginia Vernocchi et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-345-RC4, 2021

Addendum to review of 10.5194/amt-2021-345, Characterization of the MISG soot generator with an atmospheric simulation chamber, by Vernocchi et al.

My original review recommended publication of the manuscript by Vernocchi et al. primarily for its new data on supermicron aggregates in terms of optical particle size and optical properties. This recommendation stands.

This addendum adds to my earlier review to clarify three minor points regarding the manuscript by Kazemimanesh et al. (K2018), which is the only previous manuscript to study superaggregates from MISG ethylene flames.

The points are as follows:

1) My original review stated that K2018 reported TEM size distibutions up to 2 um. This is true, but K2018 also reported aerodynamic size distibutions. (Moallemi et al. 2018 reported only TEM.) The physical interpretation of aerodynamic and optical size distributions should be discussed in detail (see e.g. https://dx.doi.org/10.1080/027868290903907). What is the optical equivalent diameter of 2 um aerodynamic diameter soot aggregates in the supermicron regime, considering morphology? Calculating the answer to this question is difficult, but measuring it is simple: the authors can compare OPS size distributions with/without the cyclone. (This comment extends one of my original minor comments.)

Since our atmospheric chamber is currently engaged full time in non-postponable experiments, we will perform the request experiments as soon as possible, in agreement with the editor. We'll try our best to characterize these super-aggregates, using the instruments we have in our lab and within the scope of the present work.

2) K2018 discussed superaggregate formation in a stagnation plane, citing literature by Chakrabarty et al. different to the citation I gave earlier. The stagnation plane hypothesis is inconsistent with the present manuscript's hypothesis that coagulation occurred in the sampling lines. The stagnation plane hypothesis may also better explain the difference in EC:TC of the superaggregates. Regardless, I still recommend that the authors test different sampling line lengths directly since that test is simple. (This comment extends my original 2nd major comment.)

We have planned these experiments in the next weeks. We'll try our best considering our comments in the original review of the Referee 2.

3) K2018 also showed that superaggregate formation depends on fuel flow rate, with negligible superaggregates observed at the lowest flow rate (which also produced a lower number concentration). So did the authors observe 'larger' superaggregates because they used a higher fuel flow rate, or because they used an optical particle sizer instead of an aerodynamic one? (This comment extends my original 1st and third comments.)

We thank the Referee for the thorough speculation on the super-aggregates origin. We don’t have the answer to this question, but we can try a simple experiment: we will use lower flow rates to see how their dimension change with them.