

Atmos. Meas. Tech. Discuss., referee comment RC3
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Comment on amt-2021-345

Anonymous Referee #3

Referee comment on "Characterization of soot produced by the mini inverted soot generator with an atmospheric simulation chamber" by Virginia Vernocchi et al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2021-345-RC3>, 2021

OVERVIEW

The present manuscript aims to characterize soot particles produced with a MISG. Overall the manuscript needs some rewriting and rethinking. Many sections appear to be a list of results which are scarcely interpreted, while many figures are barely described. Hence, some important results are shaded by many non-relevant information and figures. As final results, the conclusions and overall take-home message of the manuscript becomes extremely unclear. As mentioned in the second review, the authors show for the first time that MISG can produce large soot aggregates, which are not well characterized in other previous studies and cannot be generated, up to my knowledge, with the more traditional CASTburner. So, I suggest the authors to focus on this specific aspect of their research. Considering the presence of these large particles, PAX measurements must be corrected for truncation error. In its current form, the manuscript is not suitable for publication.

SPECIFIC COMMENTS

ABSTRACT: The abstract is very generic and does not provide any real information on the performances of the burner. I suggest rewriting of the abstract.

INTROUDUCTION: Introduction does not provide a context and does not present the motivation for this study. At the moment is a list of references without a clear story behind it. It does need some rewriting.

L27-34: the definition of "BC" is mostly based on its non-null imaginary part of the refractive index. However, Petzold 2013 made it clear that the term BC is a qualitative definition of BC rather than operational. From your text I have the impression that absorption photometers will directly provide BC concentration. I think that, however, a discussion on soot nomenclature is not needed so early.

L43-50: This part needs to be developed further since it will create the right context for your work.

L51-63: This part provides some sparse technical details of burner and a very generic description of smog chambers. It is not clear what the author wants to communicate here.

L74: reference

L77-80: what are the consequences of the absence of quenching or carrier gas?

L84: give number to equation. Recurrent

L85-91: this occupy more space than needed. Put it as normal text. Recurrent

155-156: revise indent.

L166: size distribution measurements

L181-182: what refractive index was used to derive diameter from OPS?

L196-197: Considering the extensive use of PAX measurements in the paper I am genuinely surprised that truncation errors are disregarded. I think it is important to show that truncation is not relevant in these conditions. As recently resumed by Modini et al. (2021) little is known on the dependency of scattering phase function on the particle

morphology and how this might impact truncation for highly absorbing aerosol particles. Scattering correction for absorbing aerosol is investigated for the nephelometer instrument by Bond et al., 2009. The argument of the authors is valid, but it should be contextualized if not verified.

L246-251: If I understand correctly this is simply the relative standard deviation. It is not clear, however, in what conditions these values were calculated.

L301-302: does it mean that all size distribution are measured 3 minutes after injection. The ageing time in the chamber should be always specified, since concentration and diameter of particles drastically change due to coagulation, especially at high concentrations.

L340-372: Describing both EC:TC and OC:EC is redundant. It is also hard to compare and understand the impact of large soot on the OC:EC fraction from the two figures. I would suggest to merge them or focus on the impact of large soot. To be honest, figure 9 could easily go in the supplementary. Is there any correlation between OC:EC and diameter mode, CO₂, NO ?

Section 3.2.4 -3.2.5: These sections could be merged. Especially considering the length of Section 3.2.4. Figure 11 is barely described or discussed in the text. Hence, it can be moved to supplementary or removed.

L412-417: This part is very hard to read and follow. The authors are requested to build a discussion on their result, rather than list numbers in series. This problem appears in almost every section of the paper. As a consequence, Figure 12-13-14 become hard to interpret too. I would suggest move the figures to supplementary, summarise the result in a table and construct a separate discussion for ethylene and propane.

Figure 15: since you do not correct for truncation error, this comparison between PAX and MWAA is highly questionable, especially for the experiments without cyclone.