Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC5
https://doi.org/10.5194/amt-2021-345-RC5, 2021

Comment on amt-2021-345

Anonymous Referee #4

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-RC5, 2021

Review of "Characterization of the MISG soot generator with an atmospheric simulation chamber"

General comments:

This paper discusses the physical, chemical, and optical properties of soot produced by burning propane and ethylene in a miniature inverted soot generator (MISG). Although some aspects of this work (such as flame shape vs. fuel and air flows) have been discussed in previous studies, there are some novel aspects to the paper: combining the MISG with an atmospheric simulation chamber and studying the optical properties of soot in depth. The methodology used in the paper is sound and valid but the paper itself is cluttered and poor in terms of readability. I suggest that the authors streamline the paper by omitting the discussion on flame shape with combustion conditions or moving it to the supplementary material, and instead focus on aspects that have not been covered in similar other studies. The whole manuscript should be edited for grammar and proper academic writing too. There are also some discrepancies between the results presented in this paper and previous papers that characterized the MISG soot, which need to be discussed in more details by the authors (see my comments below). Overall, the paper is not acceptable in its current form and needs major revisions before it can be published.

Specific comments:

Article title: Avoid using an acronym in the title without fully defining it first.

Abstract should be written as one paragraph.

Abstract: MAC stands for mass absorption cross-section, not mass absorption coefficient.

Section 1: Combine paragraphs 2 and 3.

Line 45: List the "several other purposes" in those references more specifically.

Section 1: The introduction is written poorly and needs to be improved in terms of readability and transition between paragraphs. It should also clearly state the objective(s) and novelty of the study near the end of the introduction.

Line 69: Change "air and fuel flow in an opposite way to the buoyancy force" to "… in opposite direction to the …"

Line 70: Change to "The resulting diffusion flame is more stable by reduced flickering of flame tip"

Line 77: lpm and mlpm should be defined (it is better to use L/min or mL/min as units of flow rate).

Line 79: This statement is not correct. Kazemimanesh et al. (2019) states that part of the air flow is used in combustion and the rest is used to dilute the exhaust products.

Lines 81-101: The definition of equivalence ratio is based on fuel-to-air ratio, thus the reader would not be confused if you define the fuel-to-air ratio (instead of AFR) first. Also, all equations should be numbered.

Line 93: The units used for AFR are not clear to me. AFR is a unitless parameter, so just get rid of any units.

Line 103: Many of the in-text citations in this article should be in format of Author (Date). Please consider this whenever suitable during revision. For example: Moore et al. (2014) demonstrated that fuel-lean flames produce soot particles …

Line 144: Consider changing to "at the fuel tube nozzle"

Line 125: I cannot find Section 2.1.2 in the paper.

Line 179: It is known that the multiple charge correction algorithm in the TSI AIM software breaks when the median mobility diameter is relatively large (>200 nm). Can the authors show the uncorrected and corrected size distributions for 2-3 cases in the supplementary material?

Line 218: Change peculiar to a better adjective.

Line 236: "To our knowledge, no 237 literature information is available for the ethylene in the flow range of Table 4." This statement is not true. Kazemimanesh et al. (2019) studied the MISG and its flame shape with ethylene and air flow rates (80-130 mL/min and 4.0-10.0 L/min, respectively) that partly cover Table 4.

Lines 241-251: The authors talk about various experiments that they did and the calculated repeatability (mistakenly noted as "reproducibility") in mode diameter and concentration. However, it is not clear what conditions were tested and the results are not shown in the paper or the supplementary material.

Page 10 – Fig. 4 and the discussion around it: The particle mode diameter reported for ethylene flames is constant at ~175 nm. This is inconsistent with previously reported values of ~240 nm and up to 270 nm (Kazemimanesh et al., 2019). The same reference also reported an initial sharp increase in particle size and concentration with increasing ethylene flow rate, which eventually levelled off to a relatively constant value. This is in contrast to the trend seen in this paper. These differences must be noted and discussed in the paper.

Fig. 3 and 4: The authors should consider adding error bars to the data points. In addition, it is not clear why a linear fit is shown for the data points when the paper does not offer any evidence or support for trend.

Fig. 5: I suggest that the authors show and discuss figure 5 before figures 3 and 4, as this will enhance the readability and flow of the paper. I was completely lost about the results shown in figures 3 and 4 when I first read the paper until I saw figure 5. Figures 3 and 4 are essentially the size distribution parameters extracted from figure 5 and shown with respect to equivalence ratio.

Lines 307-316: Can you show the number and volume distributions side by side in Fig. 6?

What is meant by “relative particle number concentration” in Fig. 6? [dN/dlog dp]/N_tot?

Section 3.2.3 (EC-OC analysis): The authors did not elaborate how they calculated TC (total carbon). OC can exist in gas-phase or as condensed semi-volatile particles and the authors need to distinguish between the two when calculating TC. The authors briefly mention the use of a second filter, which should help in determining the mass concentration of OC existing as semi-volatile particles.

Fig. 9 and 10: I do not quite understand why normalized EC concentrations are shown rather than the absolute values of EC concentration or the EC/TC ratio. The latter two parameters are more important for researchers when using a soot generator.

Line 384-385: Why is propane soot more light absorbing than ethylene soot at all three wavelengths?

Line 498: The formation of superaggregates is related to high particle concentration in the exhaust line. This means that by diluting the MISG exhaust, the formation of these large aggregates can be alleviated. Kazemimanesh et al. (2019) and Chakrabarty et al. (2012) suggest that these superaggregates are formed at the stagnation plane of the flame tip, which seems more plausible. The authors should note and discuss these differences in the paper (not in the conclusions section).