Author responses to 2nd reviews of amt-2021-320:

“Aircraft-engine particulate matter emissions from conventional and sustainable aviation fuel combustion: comparison of measurement techniques for mass, number, and size”

by J. C. Corbin et al.

We are grateful to the Reviewers for their additional efforts in identifying further opportunities for improvements to our manuscript. We respond point-by-point in the following.

1. Reviewer 3

Reviewer report on the manuscript “Aircraft-engine particulate matter emissions from conventional and sustainable aviation fuel combustion: comparison of measurement techniques for mass, number, and size” by Corbin et al.

General: this work compares the response of various PM mass and number instruments when sampling in the near field of a V2527-A5 and a CFM56- 2C1 aircraft engine. The manuscript is well written, and the data processing is of good quality; however, the content is of somewhat limited scientific relevance. The comparison between the real time and filter based mass measurements is of high relevance but is unfortunately hampered see below.

Major:

The drastically reduced flows in the filter based clap and psap are a major concern and make the shown comparison not very useful. While the authors point out the potentially added noise, there are also other problems associated with this e.g. particle losses within the instruments, more undefined spot size, potential dependence on commonly/normally occurring leaking in these type of instruments. The authors discuss this adequately by citing other literature, but it is very important to clearly mention that the instruments were not operated according to their specifications.

Presumably the reviewer did not notice this in their first round of comments, so we added a statement to highlight this in the abstract:

The TAP and PSAP were operated at 5% and 10% of their nominal flow rates, respectively, to extend the life of their filters.

and conclusions:

as noted, the TAP and PSAP were operated at 5% and 10% of their nominal flow rates, respectively

and caption of Figure 10:

Note that the TAP and PSAP were operated at 5% and 10% of their nominal flow rates, respectively, for all measurements in this study.

And a footnote in Table 3:

[a]PSAP operated at 10% of its nominal flow rate. [b]TAP operated at 5% of its nominal flow rate.

We did not extend the discussion which the reviewer described as "adequately citing other literature".

Minor:

Ln 28/29 for an easier read I would add "absolute" in front of magnitude of emissions

Done

Ln 44: albedo is albedo in plural (CAPS PMSSAs) therefore I would remove the s but would add "instruments" for an easier read

Done

Ln 47 to 49. The use of integrative seems to be a little misleading in general (different things are integrated in the two techniques). The main difference is for the filter measurements is that they are not performed in situ

Internally, we discussed this at length during preparation of the manuscript. We had originally considered the terms "in situ" vs. "filter-based", but this was modified because "in situ" is sometimes used to mean "analyzed on site" rather than "analyzed in the aerosol phase" (e.g. photometers versus TOA or solvent extraction and analysis). Ultimately we found that no one expression is perfect and we made sure to define our terms at first use.

Ln 122/123 As in the PMP, the catalytic stripper is not the key requirement an evaporation tube could also be used as stripper. The key is the additional dilution step after the stripper to prevent re nucleation

Changed to "volatile particle remover".

Ln 128 There is also a more recent publication by Durdina Empa?

Cited

Ln 376: I do not think one can call them "low cost"… and "portable" as mentioned in the response to reviewers. They are long term monitoring devices with great sensitivity (10 -100x better than a LII) but with a limited temporal resolution. Theoretically, they should actually work really well for low SSA/ strongly absorbing aerosol as measured here if operated correctly, but only with low filter loadings…

On line 376 (tracked changes version) we wrote low-cost and low-maintenance, rather than portable. Low-maintenance fits the reviewer's comment that they are long-term monitoring devices. Low-cost is an objective fact relative to other instruments in this study! Also, the instruments are indeed more lightweight than many others, which is an advantage in aircraft-based measurements.

We agree with the reviewer's theoretical expectations, which is why we included Figure 10 and the associated discussion.

Ln 512: please clarify wording what has the turbine/ fan speed to do with thrust? Isn't the low-pressure turbine linked to the fan?

We changed to "Nominal low-pressure jet-engine primary fan speeds" to minimize confusion.

Ln 771 to 783: Valuable discussion see major comment above – please clearly mention that the instruments were not operated according to their specification

Done – see above.

Ln 802 to 806: Since the instruments were not operated according to their specs, this correction is definitely not valid and should not be done – please delete this paragraph.

We kept the paragraph because it is cited in companion papers where the same instruments were used at the same flow rate, but we added a clear statement about the flow rate modification. We also added footnotes to Table 3 stating this flow rate modification.

Ln 824 might be typical for SAC combustors but not for other engine technologies

The cited study is a recent review that included double annular combustor technologies. We believe the statement is justified by the present literature.

Ln 862 more crystalline structure (e.g. A. Liati ES&T or Papers from Vander Wal group),

Cited both groups, thank you for pointing out the opportunity.

Figure 8. Could this high variability in the integrated SMPS data also be explained by the fast scanning (45s) which pushes the limits of Scanning DMAs transfer functions theory etc…

Considering that we measured a single engine with a single sampling system, we believe that the likelihood of variability due to issues with the SMPS inversion is minimal and does not justify a mention.

2. Reviewer 2

I'd like to thank the authors for their thorough explanations and the modifications they've made during this first round of reviewing. I'm generally satisfied with the answers and modifications to my original concerns. However, I still have a couple of concerns as listed below.

SMPS based EIm: I disagree with your rebuttal about the "negligible" impact of particle effective density on EIm derived from SMPS size measurement (L410 & L747).

You quote Durdina et al. 2014 paper stating an uncertainty of 20%, but this uncertainty is for ksl_{mass} calculation in which the engine exit VSD is divided by the instrument-location VSD, and therefore in which the particle effective density has a much smaller impact than deriving total mass from a size measurement. Furthermore, Durdina et al. 2014 paper is only reporting data for one engine for which the measured particle effective density was near 1 g/cm3, when other aircraft engines have displayed particle effective densities ranging from 0.2 to 1.9 g/cm3 (Saffaripour et al. 2019 https://doi.org/10.1016/j.jaerosci.2019.105467). For example, if the particle effective density for the engine you investigated was to be ~0.5 g/cm3, then the SMPS-derived EI mass would be over- reported by a factor 2 (i.e., 100% uncertainty) with a particle effective density assumption of 1 g/cm3. My concern is that the SMPS was significantly under-reporting number (50% less than APC), but your "high" particle effective density assumption made the SMPS based EIm appear to agree better. I suggest re-phrasing to highlight that there are significant uncertainties associated with the use of an assumed particle effective density when deriving EI mass from a particle size distribution on an unknown engine which may not have a particle effective density ~1 g/cm3 at any given powers.

The reviewer's arguments are well-made and thorough. To incorporate these points, we modified the text as follows:

The CS-SMPS data were systematically higher than the geometric mean, potentially due to an overcorrection of the penetration of large particles to the SMPS or due to uncertainty in the effective density that must be assumed when converting SMPS data to EI_m. As noted in Section 3.3.3, we assumed an effective density of 1000 kg m^{-3} based on the work of Durdina et al. (2014). Considerable uncertainty could be introduced due to this assumption, as the effective density of the nvPM particles (Momenimovahed and Olfert, 2015) may vary with the monomer diameter (Abegglen et al., 2014; Durdina et al., 2014) and/or shape of soot aggregates.

EI number loss correction factor: In your summary data spreadsheet, the "number line loss correction factor" can be seen to fluctuate between 1.33 to 2.66. Given the sampling system length between the probe and the APC/DMS (container 2), I would expect much larger loss correction factors (e.g., ranging between 2 and 10). What does this "number line loss correction factor" correspond to? It should also be different for the different number analysers.

The reviewer correctly noticed that we only described mass-based correction factors in the text. We now modified the last paragraph of Section 3.4.2 to:

All reported data were corrected using penetration functions. Size-resolved data (SMPS PSDs) were corrected using the size-resolved penetration functions shown in Error! Reference source not found.. Size-integrated data were corrected using either number-based (for the APC) or mass-based (for all other instruments). The number-based line loss corrections were calculated as the ratio of the corrected to uncorrected PSDs. The mass-based corrections were calculated using the corresponding ratio of PVDs. Correction factors for each test point are given in the Data Availability section.

Two members of our team independently verified these calculations and the reviewer's expectation of larger correction factors was not met. The values between 1.33 and 2.66 correspond to ordinate values of 75% and 38% in Figure 4. Comparing the abscissa of Figure 4 with the GMDs in Figure 6, these values are found to be consistent.

Thank you for catching this oversight.

While working on the above correction, we also noticed a minor omission in Section 3.4.2, and fixed it by adding the sentence:

Function 2 was adapted slightly for each instrument in the NARS due to the relatively small additional losses in the sampling lines of each instrument.

Can you please clarify why the loss correction you've applied does not bring better closure for EI number?

Of course, applying this loss correction did improve closure compared to the uncorrected data. But errors potentially remained. We believe this is because Penetration Function 2 was not included in the line loss correction measurement, because the NARS had its own long sampling line (line 435, 542, Figure 1).

Can you please also clarify if APC number was corrected for VPR loss and CPC cut point.

Consistent with NARS results reported elsewhere, the APC was not corrected for VPR losses. The APC was not corrected for CPC cut-point because the measured PSDs suggesting the particle counts below 10 nm was negligible. The same is true for the SMPSs.

We note that further corrections for losses in the APC VPR would only make the discrepancy between APC and SMPSs even larger.

We modified the text to:

APC (AVL Inc., which contains a TSI Model 3790E CPC and a volatile particle remover), ...