Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2019-196-RC1, 2019

Atmospheric Measurement Techniques Discussions
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Interactive comment on "Characterisation of the filter inlet system on the BAE-146 research aircraft and its use for size resolved aerosol composition measurements" *by* Alberto Sanchez-Marroquin et al.

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Received and published: 12 June 2019

The manuscript of Sanchez-Marroquin et al. deals with the characterization of an aircraft inlet frequently used in the British BAE-146 research aircraft. Despite of the importance of inlet characterization, it happens frequently that aerosol inlets are built and used, but remain uncharacterized. Therefore, these type of studies is valuable to rate the results of aerosol research done with the according systems, in particular with respect to their (size) representativity.

The authors compare an experimental approach for inlet transmission characterization with a theory-based one and come to the conclusion of a general approximate agreement. They propose a range of operational conditions based on their results.

The paper is mostly well-written; the methods are explained and applied. Some unclear sections remain (detailed below). References are adequate. However, some effort should be placed into the thermodynamic considerations, and the SEM part should be structured partly into a second publication. Also, some intentions for future work are given scattered through the paper, which should be either moved to the motivation section or omitted.

General remarks

The paper goes into details about aerosol flows, but the properties and values reported in the text should be treated with more precision. E.g., flow rates are reported in L per minute, but it is unclear, whether this means volumetric L at the outside conditions, volumetric L at the inlet conditions, mass equivalent L at standard conditions.

636-637: “All calculation were done under standard conditions” – Why? Most aerosol/carrier gas interactions depend on the air viscosity and free mean path, some on Reynolds number and therefore density. As result, most efficiency functions at the end have temperature and density in them. It doesn’t seem to be a wise choice to neglect these dependencies, in particular not for aircraft measurements with their strong variation. Also, the thermodynamic conditions change considerably from the outside conditions through in inlet and tubing to the filter.

Estimates were done using ‘classical’ aerosol aspiration / transmission formulas, which don’t appear to be relevant in all cases (see detailed comments). A major question in this context is why the authors decided not to use computational fluid dynamics modeling. While these techniques are work-intensive and in turbulent situations also not necessarily precise, in particular for the inlet, diffusor and bend / inertial separator section, they might have been more useful.

The section 8 and 9 appear as a misfit in the context of this technical paper. I suggest removing it here and extending it into a standalone paper or letter in another journal. A proof of capability of SEM and measurements with the filter system onboard the aircraft is not really required, as this has been done during decades (Johnson et al., 1991; Formenti et al., 2003; Chou et al., 2008; Johnson et al., 2012). In addition, the plots are shown, but remain un-discussed, and no context (e.g., meteorology, trajectories, campaign aims) is given. If anything of the SEM compositional results should be included, I suggest including the sensitivity tests for the classification scheme as function of the detection limit (currently in the supplement), as from this you can derive recommendations with respect to element quantification settings.

Detailed remarks / corrections

Abstract 28-30: While this is surely true, it's not part of the paper.

40 Missing "."

55 "... has been limited." was not carried out?

57-67: These lines are more a summary than an introduction. As it is partly redundant to section 9, I suggest removing it.

88: It doesn't get clear from the picture: does the 0.7 cm inlet have the inner edges rounded? From the references literature I would think that it is.

97: The numbers indicate a high precision, which is usually not achieved by mass flow meters (1 – 3 % uncertainty). How water vapor was treated, which influences the reading?

98: It probably reports the gas mass, not volume.

101: Rietschle?

110: Particles are not necessarily lost (to the wall), but can be diluted (i.e. not entering the inlet).

112-113: and depending on pressure and temperature...?

122: It seems that to rate the importance of a mechanism, its effect needs to be compared to all others. Was this done, or were the only most probably important mechanisms selected? Please explain the reasoning.

126-162: Too redundant with the appendix. Suggestion: either refer to the appendix and remove all short explanations here, or include the full discussion currently in the appendix. For a technical journal, also the latter would be appropriate.

181-182: How does the bypass change the temperature in the inlet (probably mitigate heating by less deceleration)? Is the effect strong enough to impact on volatile particles?

213: It is somewhat surprising that the filter flow appears to be unregulated. Maybe, a regulation system should be included in future as well.

217: “microscope”

244: “highly unlikely” instead of “not likely”?

262: “regarded” instead of “shown”?

263: Regarding the “reference”: just recently, there was a publication showing size distribution distortion for the ‘free-stream’ instruments, too, (Spanu et al., 2019), which might be worth checking.

281: Kandler et al. used mostly backscatter electron, except for small particles on TEM grids. Check also the other references please.

284: Is it possible to quantify the undercounting of backscatter versus secondary electron? That might be valuable information for people dealing with similar questions.

283-298: Can you include an image showing the benefit of the Ir coating and the potential size increase? Again that appears to be valuable information.

301-304: it appears to be more meaningful to specify the pixel size in nm (scanning grid size), instead of the magnifications, which are screen-related.

310: Was the ECD converted into aerodynamic or optical equivalent diameter or just used "as it is"? Please discuss, as this might introduce certain biases.

316: "evenly": In Fig. 7, a min/max variation of a factor of three is visible, interestingly without a size bias. Was it the same in all radial directions, or is that random fluctuations?

320: Please 'link' "ECD" to "equivalent circular diameter".

322-323: Where these charging problems observed despite the relative thick Ir coating?

414-415: I suggest treating this more precisely. At 10 μm, there is a disagreement of about a factor of 10 or slightly more, and the theory predicts between 2 and 5. Considering the uncertainties, it's probably fair to call this agreement. At 2 μm, there is the same factor $>$ 10 difference, but the theory says 1. Here, 'agreement' becomes stretched. However, the optical particle counter curves have persistent minima (3 μm, 10 μm) and maxima (2 μm, 5 μm), where the SEM curve is smooth. Are these minima/maxima realistic or potentially an artifact of a failing Mie inversion?

479: "sulphate aerosol particles, which are solid or liquid sulphuric acid particles" If it is sulphate (probable), then it is a salt as reaction product from an acid with something else. Solid sulphuric acid on a filter is improbable. Please correct the phrasing. Also, particles in this category could be organo-sulphates.

501: "chloride". Potassium-rich Cl- (and/or S-)containing particles are known from biomass burning (Li et al., 2003; Lieke et al., 2011), and other Cl-rich from (waste) incineration (Willison et al., 1989; Graedel and Keene, 1995).

512: How about fractionated crystallization of a sea-water droplet on the filter, leading to separate NaCl, MgCl, CaCO3 or CaSO4 particles?

648-650: This approach appears to be questionable, as turbulence for an increasing diameter tube probably has an additional generation mechanism (inertia), compared to turbulence in a constant diameter tube (mostly by shear). Please comment.

654-669: The bend approximation assumes a smooth tube, too. If it was used for the droplet separator, the conditions are not met. Also, if the flow is decelerated during the bend, large particles might become accumulated on the outer side, which is not accounted for by the simple approximation. Please discuss.

683: For diffusion a constant diameter bend can probably be well-approximated by a straight tube.

691-703: While it is correct that the particles are retained by the filter, not necessarily all particle sizes can be analyzed by microscopy techniques (representatively), as the smaller particles might be deposited inside the pores, too.

707: The referred equations apply to sharp-edged nozzles, while in the setup blunt and probably rounded ones are used (according to the aircraft engine inlet description). In particular the inlet rounding is done to mitigate misalignment effects (Hermann et al., 2001).

924: Caption “Polycarbonate”. As many effects discussion above might be closer related to the volumetric flow rate than to the mass flow rate, it should be shown in addition. The Iceland/Cape Verde ratio is inverted for the two filter types or two inlet types. How can this be explained?

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Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2019-196, 2019.