This paper presents a characterization of the filter inlet system of the research aircraft BAe146. It includes calculated inlet sampling and transmission efficiency, a description of the analysis of the filter samples by scanning electron microscopy (SEM), and a comparison of the size distributions obtained by SEM with underwing aerosol and cloud probes.

Unfortunately, the manuscript suffers from being vague at important points. Especially for a technical journal, a comparison between calculations and measurements needs to discussed in more detail. Also, expressions like "in agreement" are used frequently where a precise numbers (with error limits) would have been necessary. Thus, I cannot recommend publication in the current stage and suggest some major revision before publication.

Major points:
As said above, the manuscript lacks precise numbers. Many statements are vague, like "in agreement" or "minor fraction" etc. This is not sufficient for a technical journal.

We have amended Sect. 4 and in Sect. 5 in order to remove subjective statements where possible and replace them with more quantitative statements.

Furthermore, the SEM part is a description of the classification, but no further validation is done. Additional aircraft-based gas (e.g. CO) and particle measurements (mass spectrometers?) may help to characterize the air mass origin and the particle properties and thereby validate the composition.

We gave this some thought when planning these experiments, but concluded that it was not possible to find an established technique that we could quantitatively validate our SEM technique against. Validation of the size resolved composition would require significantly more detailed particle by particle information than could be inferred from tracers like CO or even from the available aerosol mass spectrometers. The AMS for example only provides information on the non-refractory components of fine mode aerosol.

In order to address the comment we have included a new figure (Fig 11) where we show an additional six size resolved compositions. This is accompanied by a new discussion in Sect. 6. Sect. 6 now focuses on examples and the paragraph on mass spec techniques has been removed. Fig. 7 includes data for SE England and Alaska (three samples for each). The extremely good agreement between the Figure 11c and 10a, which were samples from the same flight, helps to demonstrate the reproducibility. We also, show that the composition of the aerosol in the two locations is different in ways which we would expect, which shows that we are sensitive to different aerosol types and the composition varies in a consistent way.

Of course, the ideal situation would be to have a standard instrument to compare against, but given this standard instrument does not exist, we suggest that the best way forward would be to take part in a suitable inter-comparison at some point in the future.

The comparison of SEM size distribution with the PMS probes is not very conclusive, because only qualitative statements ("in good agreement") are made.
The discussion of the SEM-Optical probes (Sect. 5) comparisons has been restructured and improved.

Furthermore, the size distributions of the PCASP (Fig. 5) seem to have a problem at 300 nm and above 2 _m. The PCASP shows decreasing number concentrations above 2 _m while the CDP starts at 5 _m with much higher number concentrations. Does the PCASP underestimate particle number above 2 _m? If so, would it be better to omit these points and use a lognormal fit to the reliable CDP and PCASP data to obtain realistic fine and coarse mode distributions? To what extend can such size distributions validate the inlet efficiency if the uncertainties are so high?

Unfortunately there were some errors in Fig 5, which have now been corrected. Nevertheless, we sometime see an apparent discrepancy between the PCASP and CDP at above 2 um (the discrepancy at

300 nm is resolved using the correction detailed in Rosenberg et al. (2012) which is already discussed in the text (we accidently plotted the uncorrected data). In addition, horizontal error bars have been added to all the PCASP-CDP data.

In the reference paper for the PCAS_CDP calibration, one can read: "Some bumps seen in the PCASP distribution have been accentuated by the calibration and refractive index correction presented here. It could be the case that these are real modes or there is the potential that this is an artefact caused by imperfect knowledge of the particle scattering properties" (Rosenberg et al., 2012). Hence, it is very difficult to address if the feature at 2 um is physical or just an artefact. However, in other data (Fig 8b, Fig 9a, b and c), these bumps cannot be seen as easily and in all the cases as in Fig 5 (which is the same data as Fig 8a), so they are likely to be physical.

Added to end of Sect. 5: "Some of the PCASP size distributions contain some bumps (above 2 μm), but it is not possible to address if they are physical or just an artefact produced by the calibration (Rosenberg et al., 2012)."

We strongly disagree that it is a good idea to show fits for the comparisons instead of the data with errors. The fitting can have some subjective parameters (number of modes and restrictions on the fit) and not showing the actual data would potentially omit a lot of information. As a consequence we decided to keep only the data without any fitting on it and understand the uncertainties and potential artefacts of the system. In addition, Rosenberg et al. (2012) does not recommend showing a fit instead of the data as a way to deal with the bumps.

Figure 8-10: Have the SEM data been corrected for the calculated inlet transmission and aspiration efficiency? I could not find a statement on this in the text. If not, then an overestimation of about a factor 3 - 4 around 10 _m should be observed (from Fig 3b). Is that the case? By bare eye, the factor seems to be larger than three, but there is no discussion in the text, except for a "good agreement" statement.

We do not correct the data for the inlet efficiency. This is now clearly stated in the text (fifth paragraph of Sect 5):

"Given the uncertainties on both techniques and the fact that they measure different diameters (optical diameter in the case of the PCASP-CDP and geometric equivalent circular diameter in the case of the SEM), this comparisons cannot be used to quantify the biases in the system, but can be used to make a qualitative comparison. For similar reasons, the SEM data hasn't been corrected using the theoretical efficiency"

The referee refers to a factor of 3-4 enhancement. Based on the calculations we recommend that sampling is performed with total flow rates greater than 50 L min^{-1} with the bypass open, which result in enhancement smaller than about a factor of 2.

Regarding the 'good agreement' comment, we have made an effort to be more quantitative throughout the manuscript, particularly in discussion of the size distributions. We have reorganised section 5, also in light of the other referee's comments.

Minor

I was a bit confused by the mixture of sampling efficiency study and chemical composition study. I see that both needs to be done, but I needed some time to realize that the manuscript focuses on these two topics. Maybe a change of the title would help the reader.

Both aspects of the study are mentioned in the title, so it is not clear how we would change it to make it clearer. We reinforce this in the abstract and also in the (revised) final paragraph of the introduction.

Specific comments

Line 353: "This happens more frequently for smaller particles, but it can also happen with some larger particles..." What is "smaller" and "larger" here? Please be more precise and give a size range.

Done.

Added to 6th paragraph of Sect 4.

Line 366-368: "The number of particles is very low, typically about the order of magnitude of one particle per 100 by 100 _m square, which is well below the typical particle loading on a filter exposed to the atmosphere" Please give numbers for typical particle loading. "Well below" is not quantitative.

Added to end of Sect. 4: "The number of particles is typically about the order of magnitude of one particle per 100 by 100 µm square, which is more than an order of magnitude below all the samples in this study (apart from the sample shown in Fig 9c, where it is only about a factor 2)"

Line 373: "...from the analysis of atmospheric aerosol (it was only ever a very minor component)." Please specify "very minor".

Sentence was deleted for simplicity. The only purpose of the explanation was stating that that element is not very necessary for most of the aerosol studies.

Line 374: "By doing this, we make sure that we excluded more than half of the artefacts of the analysis" I don't understand. Before that, you said that >90% contained Cr, so you would remove >90 of the artefact, isn't it?

Now it has been better explained:

Added to end of Sect. 4: "In Fig. S2 one can see that about half of particles found in both blank filters and the handling blank belong to the metallic rich category. However, further examination of the composition of these metal rich particles revealed that almost all of them were Cr rich particles (about 97 % in the case of the blank filters and about 96% in the case of the handling blank). As a consequence, we excluded all the Cr rich particles from the analysis of atmospheric aerosol. By doing this, we make sure that we exclude about half of the artefacts of the analysis"

Section 7. Did you observe any signs of meteoric material (see Murphy et al., 2014)? Particles dominated by Fe, Mg, Si and S ?

Although we did observe particles dominated by these elements, we cannot conclude that they are meteoric material since most of them were taken in the troposphere (most of them in the first kilometre), rather than the stratosphere where meteoric material has been observed. Analysis of meteoric material with the SEM seems more complicated since it only provides the weight percentages of the elements in the aerosol particles without any information about the isotope or the mass to charge ratio of what it is in the sample, but we will consider this while analysing the composition data which will be included in future papers.

Line 501: "sodium chlorine" -> sodium chloride

Done

Fig 4, caption: "FAAM core datasets" have not been explained before:

Added to first paragraph of Sect. 3: "All the PCASP-CDP data shown here has been extracted from the FAAM cloud datasets corresponding to each specific flight via the Centre for Environmental Data Analysis"

Added to caption of Fig. 4: "The altitude data was extracted from the FAAM core datasets C019, C022, C024, C025, C058, C059, C060, C061, C062, C063, C085, C086, C087, C088, C089, C090 and C091 (via the Centre for Environmental Data Analysis)"

Fig 5 + lines 257-264: As already written above, the size distributions of the PCASP (Fig. 5) seem to have a problem at 300 nm and above 2 _m. The PCASP shows decreasing number concentrations above 2 _m while the CDP starts at 5 _m with much higher number concentrations. Does the PCASP underestimate particle number above 2 _m? If so, would it be better to omit these points and use a

lognormal fit to the reliable CDP and PCASP data to obtain realistic fine and coarse mode distributions? What happens at 10 _m with the CDP?

We addressed the first points above.
In most cases, the CDP counting decreases around 10 um, but this is likely to be an actual measurement and not an artefact since particles above those sizes are relatively rare in the atmosphere.

Figs 8 and 9: I suggest combining Figs 8 and 9 into one figure with 4 graphs.
Done

Fig 8, 9, 10 and line 415: "The results of these comparisons are in agreement with the theoretical calculations in Sect. 2.2." Did you correct the SEM size distribution with the calculated sampling efficiency? Can you divide SEM dN / PMS dN and derive an "experimental" sampling efficiency and compare that to the calculated curves in Sect. 2.2? One of the above should be done, otherwise your statement "are in agreement" is too weak.

We regard the efficiency calculations as qualitative, i.e. they provide a qualitative indication of losses and how to best use the inlet while minimising sampling biases. We therefore cannot use them to 'correct' the data, doing so would likely introduce a unquantifiable error to the data.

Added to end of Sect. 5: "Given the uncertainties on both techniques and the fact that they measure different diameters (optical diameter in the case of the PCASP-CDP and geometric equivalent circular diameter in the case of the SEM), this comparisons cannot be used to exactly quantify the biases on the system but understand its presence. For similar reasons, the SEM data hasn't been corrected using the theoretical efficiency"

References

Rosenberg, P. D., Dean, A. R., Williams, P. I., Dorsey, J. R., Minikin, A., Pickering, M. A., and Petzold, A.: Particle sizing calibration with refractive index correction for light scattering optical particle counters and impacts upon PCASP and CDP data collected during the Fennec campaign, Atmos. Meas. Tech., 5, 1147-1163, 2012.