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.

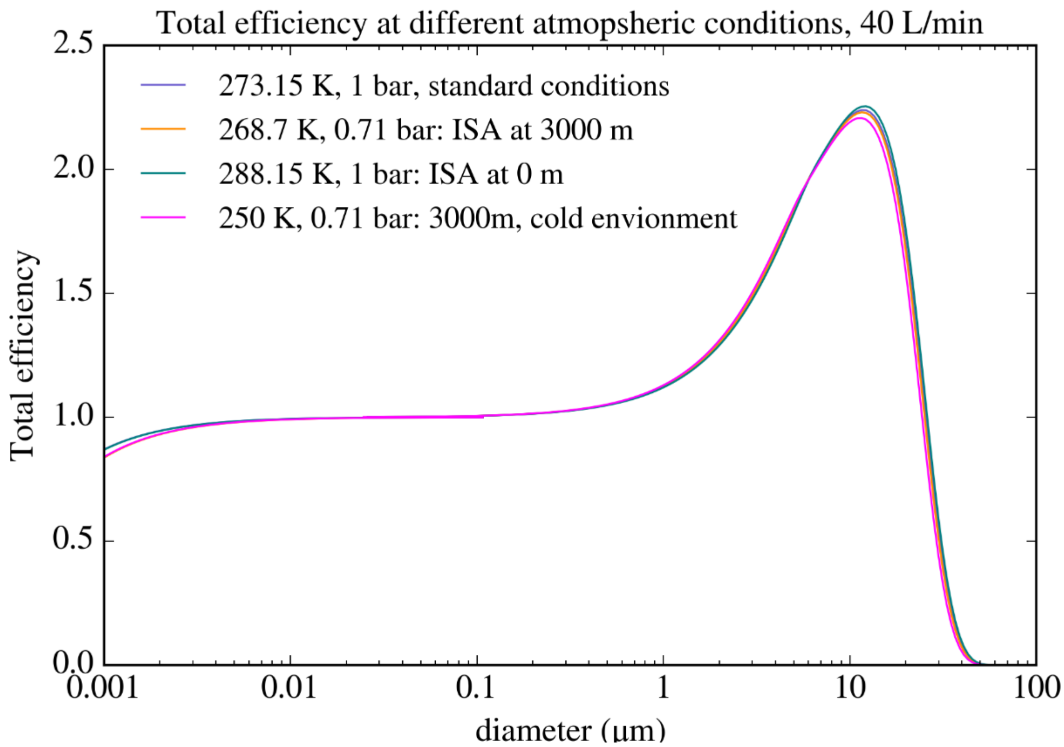
The authors compare an experimental approach for inlet transmission characterization with a theory-based one and come to the conclusion of a general approximate agreement. They propose a range of operational conditions based on their results.

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.

Thank you very much for this comments. They are very useful and will definitely improve the manuscript. We address the specific comments below.

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.
Line Added in Sect. 2.1: "The air flow through the filter (filter flow) is measured by a mass flow meter, which measures the sampled air mass and reports it in equivalent litres at standard conditions (273.15 k, 1013.529 hPa)".
Line Added in Sect. 2.2: (volumetric L at standard conditions: 273.15 k, 1013.529 hPa),
Line Added in Sect. 2.2: (all the flow rates of our calculations are given in L min-1 at standard conditions: 273.15 k, 1013.529 hPa).

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 modelling. 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.
C2
A temperature and pressure dependence test was performed. In the figure below, one can see the total efficiency for the 40L/min case (including the diffusion) for standard conditions, International standard atmosphere conditions at 0 and 3000m (the range in which the filter inlet system works. The differences are negligible. A similar negligible dependence was calculated for another inlet on board of the FAAM aircraft in Trembath (2012).



Line added at the third paragraph of Appendix A: "The effect of changes in pressure and temperature (and therefore air density and dynamic viscosity) that normally occur in the filter inlet system sampling range (0 to 3000 m) are negligible in all the used equations".

Further thermodynamic analysis in order to estimate the heating losses are impossible to carry out because of the lack of temperature and pressure measurement instruments through the inlet, which are not possible to have because of certification issues. This is a frequent problem of aircraft research. This is now mentioned in section:

Added in fourth paragraph of Sect. 5: "Also, volatilization of certain type of aerosol particles (which are more abundant in the submicron fraction (Seinfeld and Pandis, 2006)) can happen during heating (in this case produced by deceleration of the flow in the inlet) or sampling…"

The authors are aware that CFD (if carried out properly) would be a good way to characterise the inlet system, but decided not to include any CFD because it is far beyond the scope of the PhD project of the main author and it is very unlikely that it would change the conclusions of the paper. In addition, we think that the use of the appropriate empirical equations in combination with the comparison with underwing optical probes is an effective means of determining the best method of using the inlet and what biases can be expected.

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 on board 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.
We interpreted that this comments refers to the sections 7 and 8 (SEM compositional categories, and an example of an application).

Sensitivity tests have now been included in the main paper (Appendix C).
Sect. 7 (The Section regarding to the compositional categories) has been moved to the Appendix B, in order to make the paper flow better.

Sect. 8 (now Sect 6), which includes some examples of what the technique can do has been kept just as an example without further discussion since this is a techniques paper. Further publications including all the SEM data collected by the authors and its discussions are already in preparation, and they will refer to this publication, rather than describing the technique in multiples SI sections of these future publications. The SEM technique has been used by other parallel projects in ground collected samples, which will also refer to this work.
Add in Sect. 8: "The purpose of this section is purely to give examples of the capabilities of the technique, further analysis is planned for subsequent papers"

On the comment about the SEM technique being used in the past. Yes, it has been used in the past, but our approach draws on elements of a number of previous studies and the classification scheme is novel. It therefore needs to be described somewhere. Hence, we think this techniques paper is the perfect place to include this.

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

40 Missing "."
Corrected

55 "... has been limited." was not carried out?
Some efforts have been made, we reviewed these previously in Price et al. (2018), but they are very limited. We are more specific in section 8 where we refer to the relevant papers. We have added the relevant references to this statement in the introduction as well.

57-67: These lines are more a summary than an introduction. As it is partly redundant to section 9, I suggest removing it.
Yes, this paragraph is the summary of what will come in the paper. We usually structure our papers in this way since it helps focus in on the specific objectives of the paper.

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.
The word curved has been added to the text, as done in the given references.

97: The numbers indicate a high precision, which is usually not achieved by mass flow meters (1 – 3 % uncertainty). How water vapour was treated, which influences the reading?
The uncertainty of the MFM has been added (See caption of Fig. 4). Since the error is 1 % of the full scale (and this one is 400 L/min), the errors are above 1%.

Water vapour was neglected, since its effect is negligible. The difference in the heat capacity of dry air and saturated air at 20 ºC is about 2%. The difference in the molar mass of dry air and saturated air at 25 ºC is about 1.2%.
Add in Sect. 2.2: "The presence of water vapour hasn't been corrected since its effect is negligible."

98: It probably reports the gas mass, not volume.
It measures the gas mass but reports the equivalent volume at standard conditions.

101: Rietschle?
Added in Sect. 2.1: Elmo Rietschle (Gardner Denver Inc.)

110: Particles are not necessarily lost (to the wall), but can be diluted (i.e. not entering the inlet).
It is not clear how aerosol would be diluted in this inlet.

112-113: and depending on pressure and temperature...?
Added to second paragraph of Sect. 2.2: "The sampling efficiency of any inlet depends on the flow rates, and the flow regime (laminar vs turbulent), the pressure and the 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.

We have included and excluded the same mechanisms as described in von der Weiden et al. (2009) (the reference has been added to this line in the text).
Add in third paragraph of Sect. 2.2: (a discussion on the choice of equations, how they have been applied and the excluded mechanisms can be found in Appendix A)
Added in last paragraph of Appendix A: “Other losses: Some mechanisms (thermophoresis, diffusiophoresis, interception, coagulation and re-entrainment of deposited particles) have not been considered, since they are second order mechanisms under our conditions when compared with the calculated mechanisms (Brockmann, 2011; von der Weiden et al., 2009) and for one of them (electrostatic deposition) it is not possible to quantify them. Electrostatic deposition is normally avoided by using grounded conductive materials so no electrical field exists within the tubing (Brockmann, 2011). Since the FAAM BAe-146 research aircraft is not grounded during the flight, we cannot state this mechanism is irrelevant. However, the experimental agreement between the SEM and optical probes suggest that this is a minor loss mechnanism.”

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.
Most of the explanations have been removed.

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?
The fact that it is not possible at all to have temperature and pressure measurements through the inlet system (for certification reasons) limits our understanding of the bypass system. We can only state qualitatively that, as you mention, they bypass will decrease heating trough less deceleration (and maybe removing some heat from the system).

213: It is somewhat surprising that the filter flow appears to be unregulated. Maybe, a regulation system should be included in future as well.
Yes, we are recommending this as a part of a mid-life upgrade of the FAAM aircraft.

217: “microscope”
Fixed

244: “highly unlikely” instead of “not likely”?
Added
262: “regarded” instead of “shown”?
Added

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.
Although we are aware that the probes might have some sampling biases, as stated in Rosenberg et al. (2012), we still decided to use them as a refernece, as in previous works (Chou et al., 2008; Young et al., 2016; Ryder et al., 2018; Price et al., 2018).

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

284: Is it possible to quantify the undercounting of backscatter versus secondary electron? That might be valuable information for people dealing with similar questions.
We thought about it, however, this would be extremely dependent on the aerosol sample so we decided to state it in a qualitative way.

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.
The only thing we could do is taking some carbon coated images of some areas and some Ir coated images of different areas of the same filter (we cannot take Carbon coated images of an area, recoat it with Ir and go to the same area to take more images). The comparison would also be dependent upon the specific settings of the

instrument, hence we feel that a qualitative statement that we found Ir to be a better coating material is warranted, but a more detailed comparison is not.

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.
Done

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.
Added in third paragraph of Sect. 4: "This equivalent circular diameter hasn't been corrected or transformed into an optical or other equivalent diameter"
Added to fourth paragraph of Sect. 5: "Disagreement in the measurements can be also produced by the fact that the techniques are measuring different diameters; (optical and geometric)"

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?
Added to fourth paragraph of Sect. 4: "In Fig. 7 one can see the radial distribution of aerosol particles on top a filter collected using the inlet system. In spite of some fluctuations (which are up to a factor 3 and appear to be random), one can see that the particles are homogenously distributed all over the central ~30mm of the filter. The areas were chosen by the user from all over the surface of the selected fraction of the filter".

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

322-323: Where these charging problems observed despite the relative thick Ir coating?
Added to fifth paragraph of Sect. 4: "This reduces the likelihood of image defocusing over the SEM automated run".

We observed a frequent image focusing problem during long overnight runs, when the filters we were scanning had large numbers (above 20) of particles per image. We performed some tests and long exposure to the electron beam seemed to be the only reason of this defocusing effect. After adding the 12-15 particle limitation, this defocusing as a consequence of beam exposure effect was mostly eliminated.

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?

We would rather keep this discussion qualitative for several reasons, not least that we are contrasting optical sizes with geometric sizes and also that the flow rates in the theoretical calculations were not identical to those in these specific experiments. However, the qualitative conclusion that the bypass being open reduces the isokinetic enhancement is valid and this should be written more clearly. We have amended the line to read:

(Added to second paragraph of Sect. 5) "The results of these comparisons are in qualitative agreement with the theoretical calculations in Sect. 2.2, i.e. that the sub-isokinetic enhancement is reduced with the bypass open."

In addition we have stated why we do not make a quantitative comparison or use the theory to 'correct' 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 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."

In addition, on reviewing section 5 in light of the referee's comments we decided to restructure it. We now present the information in a more logical manner, which reflects the order of the figures. Please refer to the revised section 5 for the changes.

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.

Yes, this description was poor. We have replaced this text with the following (Appendix B3):
"Aerosol particles in this category contained a substantial amount of S. This S might be in the form of inorganic or organic sulphate compounds. Some sulphate compounds, such as sulphuric acid, are relatively volatile and will be lost in the SEM chamber."

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).
Added to Sect 7.5

512: How about fractionated crystallization of a sea-water droplet on the filter, leading to separate NaCl, MgCl, CaCO3 or CaSO4 particles?
Added to Appendix B6: "Some Ca rich particles could originate from the crystallization of sea water, loosely attached to NaCl. The latter component would dominate over the rest of the elements of the conglomerate and they would appear as Na rich particles, unless they shatter in the air (Parungo et al., 1986){Andreae, 1986 #503}(Hoornaert et al., 1996)"

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.
Added to appendix A: "This approach doesn't account for potential additional inertial losses that could occur as a consequence of the enlargement of the flow in the conical section."

However, the angle of enlargement is small (5.7^o). It was designed to be below 7^o in order to avoid flow separation (Andreae et al., 1988). In addition, the bending towards the wall that the particles could experience as a consequence of this 5.7^o expansion is smaller than the bending towards the wall that the particles already have before entering the nozzle because of the sub-isokinetic expansion of the flow, which has already been quantified.

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.
In Brockmann (2011) (page 94) they suggest to use this approach for flow constrictions such as a tee.

Add line: "This assumption might underestimate the losses since some large aerosol particles will become accumulated in the bypass".

683: For diffusion a constant diameter bend can probably be well-approximated by a straight tube.
That is what we did. This is stated in Appendix A (9th paragraph).

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.
Added in Appendix A (9th paragraph): "However, the fact that some aerosol particles with diameters below the pore size could be deposited in the filter pores and therefore not be detected by the SEM technique could contribute to the undercounting".

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).

The criteria of the classification appears again in Belyaev and Levin (1974), where they state that inlets which had certain ratios in between the diameters of the inlet edge, thickness and angles could be considered thin-walled or thick-walled. According to them, the problem with the thick-walled nozzles is that the air streamlines are distorted when they approach the inlet edge. Belyaev and Levin (1974) state that the ratio in between the external and the internal diameter of the inlet edge must be below 1.1, but we cannot really define this parameter because of the curved profile of the edge. An alternative criteria is that the ratio in between the thickness of the inlet edge and the diameter of the edge is below 0.05. Again, it is not possible to define the thickness of the edge. The numerical criteria thin/thick walled seems to be designed for truncated conical sections, not for curved edges like our case.

However, the inlet we are considered has been designed to "avoid distortion of the pressure field at the nozzle tip and the resulting problems associated with flow separation and turbulence" (Andreae et al., 1988), and it has been described as thin-walled in the literature (Talbot et al., 1990; Andreae et al., 2000; Formenti et al., 2003), because this design that avoids flow separation and turbulence places it closer to the "thin-walled" category than the "thick-walled" category. As a consequence, we decided to apply the thin-walled equations to it.

The fact that the experimental data shows the same trends in the inlet behaviour than predicted using the thin wall assumption helps to strengthen this assumption.

A short explanation of this has been added to the text (Fourth paragraph of Appendix A).

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?
It is true that for the polycarbonate case, the Cape Verde sampling was consistently about 10 L min-1 above the Icelandic sampling. However, we don't believe there is enough Icelandic samples to say there is an inverse trend for the Teflon case.

References

Andreae, M.O., Berresheim, H., Andreae, T.W., Kritz, M.A., Bates, T.S. and Merrill, J.T. 1988. Vertical-Distribution of Dimethylsulfide, Sulfur-Dioxide, Aerosol Ions, and Radon over the Northeast Pacific-Ocean. *Journal of Atmospheric Chemistry.* **6**(1-2), pp.149-173.

Andreae, M.O., Elbert, W., Gabriel, R., Johnson, D.W., Osborne, S. and Wood, R. 2000. Soluble ion chemistry of the atmospheric aerosol and SO2 concentrations over the eastern North Atlantic during ACE-2. *Tellus B.* **52**(4), pp.1066-1087.

Belyaev, S.P. and Levin, L.M. 1974. Techniques for collection of representative aerosol samples. *Journal of Aerosol Science.* **5**(4), pp.325-338.

Brockmann, J.E. 2011. Aerosol Transport in Sampling Lines and Inlets. *Aerosol Measurement.* John Wiley & Sons, Inc., pp.68-105.

Chou, C., Formenti, P., Maille, M., Ausset, P., Helas, G., Harrison, M. and Osborne, S. 2008. Size distribution, shape, and composition of mineral dust aerosols collected during the African Monsoon Multidisciplinary Analysis Special Observation Period 0: Dust and Biomass-Burning Experiment field campaign in Niger, January 2006. *Journal of Geophysical Research Atmospheres.* **113**(D17), pp.1-17.

Formenti, P., Elbert, W., Maenhaut, W., Haywood, J. and Andreae, M.O. 2003. Chemical composition of mineral dust aerosol during the Saharan Dust Experiment (SHADE) airborne campaign in the Cape Verde region, September 2000. *Journal of Geophysical Research.* **108**, p8576.

Hoornaert, S., Van Malderen, H. and Van Grieken, R. 1996. Gypsum and Other Calcium-Rich Aerosol Particles above the North Sea. *Environmental Science & Technology.* **30**(5), pp.1515-1520.

Parungo, F.P., Nagamoto, C.T. and Harris, J.M. 1986. Temporal and spatial variations of marine aerosols over the Atlantic Ocean. *Atmospheric Research.* **20**(1), pp.23-37.

Price, H.C., Baustian, K.J., McQuaid, J.B., Blyth, A., Bower, K.N., Choularton, T., Cotton, R.J., Cui, Z., Field, P.R., Gallagher, M., Hawker, R., Merrington, A., Miltenberger, A., Neely Iii, R.R., Parker, S.T., Rosenberg, P.D., Taylor, J.W., Trembath, J., Vergara-Temprado, J., Whale, T.F., Wilson, T.W., Young, G. and Murray, B.J. 2018. Atmospheric Ice-Nucleating Particles in the Dusty Tropical Atlantic. *Journal of Geophysical Research: Atmospheres.* **123**(4), pp.2175-2193.

Rosenberg, P.D., Dean, A.R., Williams, P.I., Dorsey, J.R., Minikin, A., Pickering, M.A. and Petzold, A. 2012. 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. *Atmospheric Measurement Techniques.* **5**(5), pp.1147-1163.

Ryder, C.L., Marenco, F., Brooke, J.K., Estelles, V., Cotton, R., Formenti, P., McQuaid, J.B., Price, H.C., Liu, D.T., Ausset, P., Rosenberg, P.D., Taylor, J.W., Choularton, T., Bower, K., Coe, H., Gallagher, M., Crosier, J., Lloyd, G., Highwood, E.J. and Murray, B.J. 2018. Coarse-mode mineral dust size distributions, composition and optical properties from AER-D aircraft measurements over the tropical eastern Atlantic. *Atmospheric Chemistry and Physics.* **18**(23), pp.17225-17257.

Seinfeld, J.H. and Pandis, S.N. 2006. *Atmospheric Chemistry and Physics: From Air Pollution to Climate Change.* Wiley.

Talbot, R.W., Andreae, M.O., Berresheim, H., Artaxo, P., Garstang, M., Harriss, R.C., Beecher, K.M. and Li, S.M. 1990. Aerosol Chemistry during the Wet Season in Central Amazonia - the Influence of Long-Range Transport. *Journal of Geophysical Research Atmospheres.* **95**(D10), pp.16955-16969.

Trembath, J. 2012. *Airborne CCN Measurements, Doctor of Philosophy*. thesis, University of Manchester.

von der Weiden, S.L., Drewnick, F. and Borrmann, S. 2009. Particle Loss Calculator – a new software tool for the assessment of the performance of aerosol inlet systems. *Atmospheric Measurement Techniques.* **2**(2), pp.479-494.

Young, G., Jones, H.M., Darbyshire, E., Baustian, K.J., McQuaid, J.B., Bower, K.N., Connolly, P.J., Gallagher, M.W. and Choularton, T.W. 2016. Size-segregated compositional analysis of aerosol particles collected in the European Arctic during the ACCACIA campaign. *Atmospheric Chemistry and Physics.* **16**(6), pp.4063-4079.