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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Vertical distribution of sub-micron aerosol chemical composition from North-Western Europe and the North-East Atlantic" *by* W. T. Morgan et al.

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Firstly, we would like to convey our gratitude to referees 1 and 2 for their helpful and encouraging comments on the manuscript. The following will detail our response to both referee's comments.

Response to referee 1:

We agree with the referee that the observations of organic and sulphate aerosol we observe in the westerly sector upwind of the UK may arise from marine sources but our data cannot be used to separate sources in this way due to the lack of sufficient

upwind UK profiles in the Atlantic cluster. Thus a full statistical analysis inline with the rest of the manuscript is not possible.

We will add a brief discussion to this effect in the revised manuscript.

Furthermore, we agree that it is important to make the relationship between the sizing methods clear, we will do so in the revised manuscript.

Response to referee 2:

Below we have responded to the main points of the review. The other points raised will be corrected and/or clarified in the revised manuscript where appropriate.

Main Points

- P9122-L3: the residence time between the tip of the inlet and the aerodynamic lens and well as the typical heating experienced by the aerosol sample (from ram heating and heat transfer from the cabin) should be given as these are very important parameters controlling the potential evaporation of species such as ammonium nitrate or organics after the heating and drying encountered in the inlet (e.g. An et al., J. Aerosol Sci., 2007; Huffman et al., ACPD 2009). These parameters may be altitude dependent and then typical values at e.g. 1, 5, and 10 km should be given. Also the issue of particle losses is not treated adequately. A similar calculation to that of Dunlea et al. (ACPD 2008, Fig 2d) could easily be carried out and documented, which is especially important since this instrument has flown in multiple missions.

Response: We agree that further information regarding sampling issues is required and the basis of the additional information is summarised here. A residence time of approximately 4 seconds is found. Ram heating of approximately 5K at the science speed of the aircraft with additional heating of 10-70K from cabin heating depending upon the operating altitude and thus the ambient temperature conditions. Regarding losses, Osborne et al. (2007) showed that submicron particle losses on the BAe-146

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are negligible. Furthermore, wing tip to wing tip comparisons between different AMS instruments using different inlets have shown less than 15% difference based on the work of Crosier (2008). Such information will be included in the revised manuscript.

- P9122-L10: The notation ug sm-3 is being increasingly used to refer to ug m-3 under STP conditions (see e.g. Peltier et al., ACP 2008). I highly recommend that the authors adopt this notation, and it makes this easily mistaken distinction clearer to the readers, especially those who mainly focus on the figures.

Response: This notation is already used for the figures and text but its usage will be more explicitly noted in the initial discussion of the mass metric used for the AMS.

- P9122-L18: the transmission efficiency of the aerodynamic lens may be totally degraded at 10 km when ambient pressure is an order of magnitude lower than at the surface. Was the lens pressure kept constant (or at least approx. constant) with a system along the lines of Bahreini et al. (2008 AST)? If not, the data at the higher altitudes may be questionable and the authors may wish to note this and indicate that they may be lower limits of the actual submicron concentrations.

Response: This will be highlighted in the revised text.

- P9124-L5: is there some relative weight applied to the deviations in x and y vs those in p? Otherwise the clustering could change drastically if the units of one of the variables are changed, e.g. if x is switched from km to m or P is changed from mbar to atm etc. What fraction of the weight (variance) between trajectories in the same cluster is due to differences in x-y vs p?

Response: The cluster analysis was also run when the pressure term was omitted, which yielded a similar result. Furthermore, Cape et al. (2000) reduced the pressure term by a factor of 10 in order to check the consistency and relative weight of the pressure term and found that like this study, the horizontal components dominated the cluster solutions. These points will be included in the revised manuscript.

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- P9127: negative concentrations are mentioned at a couple of instances here. It should be mentioned that these are to be expected due to when sampling concentrations close to the detection limit, due to low signal-to-noise, and that removing those concentrations would bias the averages (since when sampling with a filter the concentration averages to zero as a result of 50/50 positive and negative values). Otherwise this text may prove very confusing to some readers who are not used to this type of instrumentation.

Response: This will be clarified and noted in the methodology section.

- P9127: sulfate seems to have an increasing trend at the highest altitudes. Could this be a result of mixing of sulfate-enriched stratospheric air?

Response: Whilst we agree that there is an subtle increase in sulphate above 8000m, we believe it is most likely a consequence of long-range transport. Vertical mixing of sulphate-enriched stratospheric air to the troposphere could potentially contribute in this layer, although the coincident ozone profiles do not show an increasing trend with altitude in this region, which would be expected if troposphere-stratosphere exchange was prevalent. We will include a discussion of this area in the conclusion of the revised manuscript.

- P9127-L23-28: I don't follow the argument about counting statistics. Bahreini et al. (JGR 2003) and DeCarlo et al. (Anal. Chem. 2006) showed that there was a strong effect of particle size on these statistics for the Q-AMS, which does not appear to have been taken into account here. I suggest revising this to make it consistent with the previous literature.

Response: We will revise the manuscript to bring it into line with the previous arguments of Bahreini et al. (2003) and DeCarlo et al. (2006). Also, the Q-AMS used the Jump Mass Spectrum (JMS, Crosier et al., 2007) mode of operation which greatly improves the time resolution of the instrument. This was not previously mentioned in the methodology section but will now be explicitly discussed.

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