

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.

Anonymous Referee #2

Received and published: 23 June 2009

This paper analyzes aerosol chemical composition data from multiple vertical profiles around the UK over a period of 2 years. Data of this type are very scarce, and have generally only been obtained during short field campaigns. Thus the information in this paper is unique and very high interest to the atmospheric science community. In addition the cluster analysis technique is innovative and helps consolidate the high volume of data into some clear conclusions. I recommend publication of the paper in ACP after the following topics are addressed.

Main Points

- P9122-L3: the residence time between the tip of the inlet and the aerodynamic lens

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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.

- P9122-L10: The notation $\mu\text{g sm}^{-3}$ is being increasingly used to refer to $\mu\text{g m}^{-3}$ 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.

- 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 *AS&T*)? 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.

- 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 ?

- 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

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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.

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

- 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.

Other Points

- P9119-L21: another paper that recently demonstrated good flight results and extensive intercomparisons with an AMS is Decarlo et al. (ACP 2008).

- P9122-L22: suggest noting that the aerodynamic diameter calibration will vary with pressure.

- P9123-L2: 'error' should probably be 'deviation' here.

- P9124-L23: I think 'increase' should be 'decrease' here

- P9125-L4: I think there are many steps in the graph and those at 12 clusters are not particularly large. I would certainly not have chosen 12 when examining the graph based on the step criteria given.

- P9126-L20: here 'error' should be 'deviation' and 'MAE' should be 'MAD'. The word 'error' has a more specific meaning and should not be used to describe variability.

- P9127-L15: again 'error' should be 'deviation' or 'variability'

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- P9129-L13: the term “volume convolved” seems erroneous, I guess the authors mean that they have estimated the volume distributions from the measured number distributions? This is very different from a mathematical convolution operation.

- P9129-L15: although not the topic of this paper, could some information be given about whether the supermicron mode is composed of sea salt or dust? They must be some data over the UK that allows addressing this issue briefly with a couple of citations.

- P9130: Fig 8 would be much easier to interpret if the two distributions were plotted on a left and a right axis, scaled to match the first bin.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 9117, 2009.

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