

## ***Interactive comment on “Airborne measurements of the spatial distribution of aerosol chemical composition across Europe and evolution of the organic fraction” by W. T. Morgan et al.***

**Anonymous Referee #2**

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Review of “Airborne measurements of the spatial distribution of aerosol chemical composition across Europe and evolution of the organic fraction” by W. T. Morgan et al.

This is an important paper on aerosol chemical composition across Europe as measured by state of the art instrumentation and analysis onboard research aircraft.

The paper has a double focus: a) It addresses the technicalities of how to derive some conclusions as to the relationship between emission sources and the most important chemical fractions of the aerosols. b) It also tries to communicate the main results - the spatial variability of the sub micron aerosol composition in the boundary layer and the free troposphere over Europe in selected weather situations.

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For the broad readership part b) is most important. Here the graphical material is quite important (Figs 1-4, 6b, (7) and in particular 8 and 9). In particular Figs 2-4 are somewhat frustrating as it is hard to get an overall picture of the total mass and composition of the aerosol at a given location. It is also difficult to get an impression of the height dependence of mass and composition, or its temporal variability. The box-whiskers' plots provide some insight, but the 3d (or 4d when time is included) picture disappears.

Part a) of the paper is dealt with mainly in ch 4, which is very hard to follow and which follows and relies on the terminology and methods of Ulbrich et al., 2009. I would think very few readers are capable of appreciating the content of chapter 4.

The part of the paper that deals with b) above, in a number of places statements are made on how gases and particles evolve chemically with time, and what kind of sources that contribute to the different fractions of the aerosol. The role of biogenic emissions of aerosols or precursors is not discussed much or at all. This is a bit puzzling. Also, in view of the qualitative nature of the discussion, the solidity of statements and conclusions can be questioned. One example (among many): (p 27237-27238) “Once formed, ammonium nitrate exists in a chemical equilibrium with its chemically unreactive gas phase precursors, whereas OOA undergoes complex and continual processing involving repartitioning and oxidation.” NH<sub>3</sub> and HNO<sub>3</sub> can hardly be said to be unreactive, and their concentrations are influenced by dilution and removal (and formation) processes which readjusts the equilibrium continuously. At the same time OOA obviously also is modified through chemical transformation processes, growth and fractionation. The picture can only be understood through model calculations where the most important processes are included.

The abstract contains a last sentence “Such anthropogenically perturbed air masses can significantly perturb regional climate far downwind of major source regions.” This is not addressed in the paper, while being stated in the abstract the reader may think the claim is substantiated in the paper. I suggest to remove this sentence.

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The paper is long and difficult to read. I think the authors should reassess ch 4 and shorten it considerably.

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 27215, 2009.

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