

Interactive
Comment

Interactive comment on “Airborne observations of aerosol microphysical properties and particle ageing processes in the troposphere above Europe” by T. Hamburger et al.

Anonymous Referee #1

Received and published: 19 September 2012

General comments

This manuscript provides a statistical summary of European aerosol microphysical observations taken from two aircraft over a period of one month. The data are segregated into two time periods, one of anticyclonic circulation and stagnation, and a second of active frontal passages. The data are geographically separated into four regions and into vertical bins. They are interpreted with the assistance of a trajectory and emissions model.

The paper is reasonably well written and is easy to understand. The topic is of interest to ACP readers, and the dataset described is unique. The manuscript consists primar-

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ily of a recitation of statistical results, with relatively little interpretation and certainly no surprising conclusions. That said, there is some value to the summary that is presented here, as it can provide a 1-month dataset against which models that incorporate detailed aerosol microphysics and chemistry can be compared. Thus I recommend it be published in ACP following revision as described below.

Specific comments

- 1) The manuscript indicates that "nucleation events" could be observed (e.g., p.20400 line 2). You are actually observed the end product of new particle formation, not the nucleation of stable molecular clusters.
- 2) p. 20401, lines 1-5. This discussion of sampling issues should be moved to Section 2.2, where the measurement details are discussed.
- 3) p. 20401, line 12, define \overline{RH} as the arithmetic mean RH.
- 4) p. 20401, line 20. This section discusses decoupled, stable layers of enhanced aerosol concentrations above the boundary layer. However, the only discussion of how the BL is identified has been with respect to the ECMWF gridded data. How was the BL height determined from the aircraft data? Or did you use the ECMWF BL height determined whether samples were in the FT or BL? If so, these decoupled layers might actually be within the BL.
- 5) p. 20401, lines 25-27. It is not clear if the lognormal parameters were fitted to each individual size distribution and then averaged, or whether the size distributions were averaged and then the lognormal parameters fitted. These can produce drastically different results (particularly for standard deviation), with the former method being more correct.
- 6) p. 20402, lines 19-21. I don't fully understand this sentence. Are you suggesting a growth of particle sizes between time periods (a) and (b) in the free troposphere? Surely transport would result in advection of a completely different air mass between

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these 2, two-week periods—not a temporal evolution of aerosol characteristics.

7) p. 20403, lines 9-19. It is very hard to discern much from the grey-shaded points shown on Figure 7. Would you please bin the data into the regions shown on Fig. 4 and then produce a box plot showing the changes in age class as a function of longitude? This would more clearly show the spatial variation in age class, as well as the variability.

8) What is a "classified sequence", (e.g., line 17 and elsewhere). Is this one of the horizontal stacked legs that comprised the vertical profiles? Please explain your nomenclature.

9) Table 4 (discussed on p. 20404) is hard to follow—there is such a range of literature values that it's difficult to see what is consistent between the current observations and the literature. Is there a clear way to show this graphically (e.g., a plot showing each parameter and the range of measured and literature values)?

10) p. 20405 lines 10-11. Surely airmass exchange during frontal passages also explains much of the changes between periods (a) and (b). You seem to focus on vertical transport and removal processes here.

11) p. 20406 lines 12-16. Are aircraft particles non-volatile? I thought chemi-ion-assisted nucleation and condensational growth resulted in many volatile particles (e.g., Schroeder, Kaercher, Schumann).

12) p. 20407, lines 22-26. I don't know what you mean by the "static nature" of sulfate. Time scales for gas-phase SO₂ oxidation are probably of order of a few days at this latitude and season, so you could expect to see substantial secondary sulfate formation over the aging time scale studied here.

13) p. 20408, lines 11-16. I had a hard time understanding the point of these sentences until re-reading them. You are saying that synoptic-scale descending motion limited convection so that there was little exchange between the free troposphere and the polluted boundary layer. Please clarify.

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14) In the abstract, introduction, and conclusion, you state that understanding spatial variability in aerosol microphysical properties is "essential" to improve understanding of aerosol climatic effects. However, you don't really say why this is the case—and this presumption is the motivation for the entire manuscript. Are there studies you can cite that look at the effect of regional-scale aerosol variability of this magnitude on radiative forcing? If so, please discuss and cite.

15) Please add the dates of the project to the Abstract.

Technical corrections

1) Please define the "Benelux" (Belgium, Netherlands, Luxembourg) States. This is not commonly used outside of the EU.

2) p. 20389, line 25. The instrument is a condensation particle size analyser. However, Stein et al. is a conference abstract and is not a citable, peer-reviewed paper. Please remove.

3) p. 20389, line 19 and elsewhere (e.g., p.20394, lines 21-27, etc.). Please do not capitalize "South of England" (and, elsewhere, South Germany, etc.). Unless these are formal geographical place-names the geographic word should not be capitalized.

4) p. 20390, line 4, change "low volatile" to "low-volatility"

5) p. 20392 line 7, change "weather" to "whether"

6) p. 20395 lines 16, etc. Do not capitalize "mean", "median", or "quartile".

7) p. 20397, line 3 and elsewhere. "Data" is a plural noun. "the data were. . ." is correct.

8). p. 20397, lines 13-16. This information belongs in the figure caption, not here.

9) p. 20398, lines 16-17. How did the vertical mixing occur? Deep convection? Frontal uplift?

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- 10) p. 20399, line 11. Change to "Only a few events. . . ."
- 11) p. 20400, line 6. Reference Fig. 5 here.
- 12) p. 20404, line 2. Change to "deviations for all OTHER age classes vary from"
- 13) p. 20404, line 15. Should this be $\sim 40\%$?
- 14) p. 20404, line 21. Change "conform with" to "are consistent with". "Conform" means "is constrained to agree" in this context.
- 15) p. 20409, line 16. Change "effects" to "interactions".
- 16) Figure 1. I don't understand the vertical plot. Is this showing the median altitude and the 1st and 3rd quartiles of the altitude range?
- 17) Figure 3. Change "floating columns" to "bars".
- 18) Table 3. Change "sinlge" to "single"
- 19) Fig. 8. The axis labels are inconsistent. Fig. 8c is labeled "Rv(vol/pm2.5)" while the others are labeled with the ratios (no "R").
- 20) Fig. A2. Could you show this as a scatterplot instead? It would give a better sense of bias and variability.
- 21) I have not checked the references for errors. The one citation I did look at (Stein et al.) had an error—the page numbers are S381–S382, and the non-peer-reviewed abstract should not be cited. Please check all references thoroughly.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 20383, 2012.

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