

Interactive comment on “Aerosol chemical composition at Cabauw, the Netherlands as observed in two intensive periods in May 2008 and March 2009” by A. A. Mensah et al.

Anonymous Referee #1

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This manuscript deals with aerosol chemical composition measurements based on observations made at two intensive field campaigns. The paper is scientifically sound, but has serious problems in the following respects: 1) it is improperly structured, 2) it lacks clear scientific goals and conclusions, 3) it fails in putting the obtained results into a broad atmospheric context. I cannot recommend acceptance of this paper for publications before these problems, explained in more detail below, will be addressed.

Major comments:

Paper structure: First, it is difficult to see how the AMS CE (section 3.2.1) fits into the section "Aerosol particle composition" and why the instrument comparisons (section

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3.2.3) have been left until the end of the paper. In my opinion, all instrumental issues should be discussed under the same subsection and preferably before further analyses of the measurement data. Second, what is the point of discussing shortly on particle chemical composition in section 3.1 when there is a separate paragraph for this (section 3.2). Third, how is the last paragraph of section 3.2.1 connected with the CE?

Scientific goals and conclusions: The authors should state scientific goals for this paper in the introduction. Comparing instruments and presenting diurnal patterns does not fulfill this issue. What are the respective scientific conclusions?

Atmospheric context: The authors have done very little to compare their results those presented elsewhere in the scientific literature. This concerns specifically measured particle chemical size distribution and diurnal cycles. The authors should somehow be able to say whether their finding bring something new into our understanding on aerosol chemistry in Europe or whether these measurements just confirm what has been observed by others.

Minor/technical issues:

Section 2.3: The description of the SMPS is not sufficient. Has the used instrument participated in any inter-comparison measurements? How its performance has been tested and monitored? What are the expected uncertainties in measured size distributions and, most important for this paper, in particle mass concentrations derived from SMPS measurements?

Page 27675, lines 22–: It remains unclear where the authors have taken the CE curves (equations 1a and b)? Please explain explicitly. The assumed CE curve has a discontinuity (0.5 to 0.542) at the point when changing from 1a to 1b ($\text{MFNO}_3 = 0.3$). This is not physical.

Page 27676, lines 22–24: A statement like this cannot be made without giving a reference.

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Section 3.3: All the comparisons have been made against the AMS instrument. This should somehow be brought up in the subsection titles.

Section 3.3.1: The SMPS and AMS show very similar mass concentrations although the SMPS misses particles between 470 nm and 1 μm . Does this mean that there is negligible mass in this size range?

Section 3.3.2: This comparison has 3 problems: 1) the two instruments were at different heights, 2) different cut off sizes were used, 3) AMS has its own CE and so probably also MARGA. Based on this, I think it is too optimistic to say that the two instruments were in good agreement. In absolute concentration levels, the agreement is qualitatively good as best. The two instruments appear to reproduce the temporal variability of the chemical ionic compounds quite well, but even in this respect one can identify periods (fig 7) when the 2 instruments disagree substantially.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 27661, 2011.

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