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Interactive comment on "Aerosol source apportionment from 1 year measurements at the CESAR tower at Cabauw, NL" *by* P. Schlag et al.

Anonymous Referee #2

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This manuscript presents the results of a one-year study on aerosol composition at Cabauw, NL. The measured PM values exceeded the recommended WHO limits on 12 days. The data show that ammonium nitrate and SOA contribute most to the aerosol mass, thereby making a PM reduction difficult. The topic of paper is well suited for ACP, however, the way the data are presented is not always convincing and sometimes hard to follow. This mainly caused by the order of sections and paragraphs which can can be improved. After the authors have addressed my comments listed below, the paper may be acceptable for publication.

General comments

Order of sections: Please exchange 3.1. and 3.2 (data quality assurance should be before the results). You might even consider moving section 3.2 to chapter 2. Also

in the PMF results section, the applied contraints should be described first, and then the results should be discussed. See also my comments below. With respect to the discussion on organic nitrate, I am very sceptical that the data support the conclusions. The uncertanties of both instruments are too large for an estimation of organic nitrate from the difference between ACSM and MARGA.

Specific comments

Abstract: Please mention the height above ground of the ACSM. Until the end of section 2.2 the reader thinks that the ACSM was located on top of the tower.

Section 2.2: Instrument description (second paragraph of this section) should be placed before the statements on the agreement between different ACSMs and the ACTRIS comparisons.

Page 35124, line 5-8: Detection limits? It is not clear to me why they were not determined? As far as I understand, a blank filter measurement is done on a routine time schedule by an ACSM, and then the detection limit can be calculated as three times the average noise level.

Page 35124, line 12-15: Particle losses: I recommend using the PLC by von der Weiden et al (AMT, 2009) to estimate the losses in such long a sampling line (all vertical or horizontal?) www.atmos-meas-tech.net/2/479/2009/ http://www.mpch-mainz.mpg.de/∼drewnick/PLC/

Page 35124, lines 24 ff: MAAP: Please mention that a MAAP reports equivalent black carbon (eqBC), see Petzold et al., Atmos. Chem. Phys., 13, 8365-8379, doi:10.5194/acp-13-8365-2013, 2013

Page 35125, lines 23-29: For the 60 m inlet the sampling losses were calculated. So why not for the ACSM inlet?

Page 35127, line 15: 38Cl should read H37Cl, I assume? There is no 38Cl isotope

Page 35128, line 9-11: "Even when only considering the ACSM + MAAP PM1 concentration, where a campaign average of 9.5 μgm-3 was determined, the WHO PM2.5 limit was approximated."

I suggest rephrasing as: Even the PM1 concentration inferred from ACSM + MAAP data (campaign average 9.5 μg m-3) approached the WHO PM2.5 limit.

Page 35131 line 29 – page 35132, line 8: You calculate organic nitrate from the difference of ACSM and MARGA. I think that bears quite some uncertainty considering the large uncertainties of the instruments. At least you should provide an error estimation. What about looking at ion ratios, e.g., 30/46? This is known to differ between inorganic and organic nitrates? Can you plot 30/46 vs. the difference between MARGA-nitrate and ACSM-nitrate? Later on, on page 35133, lines 6-7, you state that the uncertainties are 30% for ACSM-nitrate and 10% for MARGA-nitrate. Thus, how can you infer 9% organic NO3?

Page 35132, line 10-12: What value was assumed for the density of organics?

Page 35133, line 12 ff: There seems to be a problem with sulfate here. The ACSM appears to underestimate SO4 by a factor of two compared to the HR-ToF-AMS. In the paper by Crenn et al (Atmos. Meas. Tech., 8, 5063–5087, 2015, www.atmos-meas-tech.net/8/5063/2015/ doi:10.5194/amt-8-5063-2015) it looked much better. How were the RIEs for the ACSM and for the HR-ToF-ASM determined? How often for the ACSM? Did all sulfate calibrations suffer from high background signals?

Section 3.3 Factor analysis:

You prefer the 4-piece separate analysis over the one-year analysis, but I don't see an objective reason for this. To my opinion this discrepancy reflects the uncertainty of the factor analysis. I appreciate that you openly show and discuss the differences, but the conclusion that the 1-year analysis "overestimates" something is not backed up by any data, or am I wrong? If you argue that BBOA is overestimated because of the high

degree of freedom (a = 0.3), why didn't you restrict it more (a = 0.1) as the HOA? You should state the reasons for choosing a certain a value at the beginning for the section. If the 1-year analysis would be run with a=0.1, would it be better? In general I would describe first the constraints, then the factors, then the alternative 1-year solution, and then conclude that latter cannot be correct (no m/z 60 in summer).

Page 35136 line 16: Typo: it's -> its

Page 35137 line 10: Explain humic-like substances (HULIS) on first occurence

Conclusions: This section is too short and too weak and should be expanded.

Figures:

Figure S2: Wind speed is hard to see. Please make extra graph.

Figure S11: Mention in figure caption that MARGA data are PM1.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 35117, 2015.