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Atmospheric Chemistry and Physics Discussions
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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 #3

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This paper reports for the first time a full year of continuous observations of the NR-PM1 chemical composition performed with Q-ACSM at the CESAR tower (Cabauw), a rural site in the Netherlands. Efforts are given to provide quality controlled data using co-located on-line instrumentation. Emphasis is given in the interpretation on PM levels and exceedances. Source apportionment is performed on this dataset (season-wise) and factors discussed in the perspective of temporal variabilities (diurnal/seasonal) and their contribution to OA in PM1.

General comments:

This paper is well written. Figures are clear and provide important information. The supporting information is also well presented. The source apportionment study is very well conducted in terms of methodology and scientific interpretation.

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Out of this, the scientific interest for this paper remains (too) limited with no clear added-value compare to previous AMS studies performed at Cabauw. Providing a 1-year continuous observation of NR-PM1 with Q-ACSM (with source apportionment) is not any more self-sufficient for a scientific paper. Scientific interpretation is often set here at the minimum with poor perspectives; lack of comparison neither with previous studies related to Cabauw nor Q-ACSM measurements and source apportionment studies. The scientific motivation is not convincing enough. Air Quality issues related to PM are important in densely populated regions (with high exposure to particulate pollution), much less in a rural area like Cabauw. In this AQ context, it would have been more meaningful to use this background (Cabauw) PM dataset to interpret the contribution of regional pollution to PM in Dutch cities. You missed the point that this dataset remains an important contribution to a larger one obtained at the European scale within the EU-FP7-ACTRIS project (ACTRIS Q-ACSM network).

Specific comments:

+ Hyphen is often missing (gas-to-particle, long-term, time-resolved, water-soluble, etc)

+ Page 35119, line 10: Why are you focusing only on WHO and not on EU-regulated PM2.5? Because PM2.5 at Cabauw may exceed WHO AQ guidelines? (and not EU-regulated PM2.5)

+ Page 35120, line 16: it should be NR-PM1 (and not ambient aerosol).

+ Page 35120, line 25: Please state that it is a Q-ACSM (Q=Quadripole).

+ Page 35122, line 20: Remove Fröhlich et al. (2015) which is focused on Source apportionment intercomparison only.

+ Page 35123, line 23: Did you install a dryer at the inlet? If so, please specify. Otherwise, you should address the impact of sampling aerosols at ambient RH.

+ Page 35124, line 15: 3LPM is supposed to be the nominal flow in the Q-ACSM for isokinetic sampling. Why did you use 1LPM. Impact?

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+ Page 35124, 17: “The” before “following”

+ Page 35125, line 1-2: MAAP provides absorption data and uncalibrated BC. You did not mention here how you address this issue.

+ Page 35125, line 23: The number here (33% of particle loss) is very high and not address correctly in the paper. It should be a function of size. Brownian diffusion (and associated losses) may be an important issue for number concentration in the nanometer range; not sure it is an important issue for PM calculation using SMPS. More information should be provided here.

+ Page 35126, line 23: “Source apportionment . . . was performed” (and not “were performed”).

+ Page 35127, line 23: BBOA is not 100% POA. But if you want to go in this direction, you may specify here “pBBOA” (primary BBOA).

+ Page 35129, line 2: You want to address here risk assessment (WHO air quality guideline); but for a rural site having few inhabitants. This is not very relevant.

+ Page 35129, line 7: You state here that MARGA PM2.5 is calculated as the sum of chemical compounds in the aerosol phase (NO3, NH4, SO4, Cl, Na, K, Mg, Ca) AND in the gas phase (SO2, HCl, HNO3, HNO2). Are you sure? (if so MARGA PM2.5 data is not correct).

+ Page 35129 : You have 30-min time resolution data and you interpret seasonal variability (averaging data over typically 3 months). Why don’t you go for monthly mean variability? You may better see some trends here with 12 points instead of 4.

+ Page 35130, line 10: You state here that you may observe some inversions a low altitudes (typically below 60m). How often? Then you would assume that concentrations below/above the inversion layer are disconnected. If so, how are you dealing with SMPS and BC data which are sampled at 60m height?

+ Page 35130, line 25: You are located at a rural site and you report diurnal BC variations with maxima at rush hours (evening/morning). What does it mean? You are locally influenced by traffic? Are you sure?

+ Page 35131: the discussion on organic nitrates is not convincing at all. You show in SI that you have strong differences between SO4 (and NO3) between ACSM and MARGA. For me, it clearly shows that IENO3, RIESO4 and RIENH4 are not properly determined. As a result, I am not surprised to see that NH4 (measured vs predicted) is not matching well. And for that reason, I am not convinced that the difference is due to organic nitrates.

+ Page 35133, line 25: You completely skip sea salt chloride (measured by MARGA and not by ACSM). Please revise your conclusions here which are not correct.

+ Page 35134, line 3: "PM2.5 values were up to 33% higher than PM1". This is a very interesting statement that points out the lack of efficiency of Q-ACSM to properly characterize PM2.5 pollution events. This should be highlighted at least when addressing these pollution events. How far is the Q-ACSM from MARGA PM2.5 during these episodes? Consequences on the conclusions of the paper?

+ Page 35136; line 18: I am a little bit loss. You are using BC and CO as external tracers to constrain both HOA and BBOA. Based on HOA and BBOA concentration levels and using literature data, you may be able to give a range of BC originating from both sources. I am pretty sure you will find that BC is coming mainly from traffic and thus cannot be used to constrain BBOA as done in the paper.

+ Page 35137, line 19: It should be R^2=0.39 for NO3 (not 0.47).

+ Page 35138, line 15: BC is not POA!

+ Page 35140, line 6: Why do you want to speak about hygroscopicity ? It has never been addressed before in the paper and has nothing to do with AQ-regulated PM.

+ Page 35140, line 23: I think P. Croteau is from Aerodyne Research Inc., not from

PSI.

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

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