Atmos. Chem. Phys. Discuss., 15, C13293–C13295, 2016
www.atmos-chem-phys-discuss.net/15/C13293/2016/

Atmospheric Chemistry and Physics Discussions

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

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Received and published: 23 March 2016

The final referee was unable to finish her/his review, but sent the following rough comments to me via email.

This paper gives the results of long-term ACSM analysis at the Cabauw site in the Netherlands. This is becoming an increasingly common measurement in Europe and beyond and builds on previous work done at this site, in particular during the EUCAARI campaign. The results are not particularly surprising and do not in themselves contribute significantly to scientific understanding. However, these are the first measurements of this nature to be presented from this site and the technical issues surrounding

data processing and quality assurance are very comprehensively presented. These should be of importance for future measurements of this type at this and other sites, so I would consider this paper publishable in ACP on this merit, subject to the following comments.

Specific comments:

P35123, L27: Contrary to what is implied, the Middlebrook parameterisation does take account of the nitrate mass fraction. More justification should be given in the main article on why it was not suitable here because it would be preferable to use the SMPS data for an independent validation, rather than informing the CE.

P34124, L25: Was the factory default inversion and calibration of the MAAP used? If so, this should be specified.

P35125, L3: The model numbers of the SPMS and CPC should be given

P35125, L23: More detail should be given regarding how the losses down the inlet pipe were calculated, given the magnitude of the correction. In particular, if diffusional losses were significant, whether this correction should be size-dependent should be commented on.

P35126, L13: Polyethylene is not a conductive polymer, so electrostatic losses of particles should be expected. Has this inlet line been characterised for this?

P35126, L15: The method of size selection (e.g. impaction, cyclone) should be specified.

P35133, L3: I don't see how the sulphate comparison can be regarded as "high quantitative agreement" given that it is of the order of 50% out. Given that historically, comparisons regarding sulphate generally tend to be quite favourable, this is quite surprising. It is also a little worrying that the ACSM measures more than both the AMS and the MARGA. The authors should investigate this further.

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