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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Carbonaceous aerosol source apportionment using the aethalometer model – Evaluation by radiocarbon and levoglucosan analysis at a rural background site in southern Sweden" *by* Johan Martinsson et al.

Anonymous Referee #2

Received and published: 13 February 2017

General comments

The authors present an evaluation of the aethalometer model for carbonaceous particle source apportionment using radiocarbon and levoglucosan measurements, and quantify wood burning (WB) and fossil fuel combustion (FF) aerosol for a year-long dataset from a rural station in southern Sweden. The model is modified to allow for apportioned non-light absorbing biogenic aerosol to vary in time, improving the aethalometer-based source apportionment compared to radiocarbon and levoglucosan data. This is an interesting and solid study. The manuscript is very well written (with a few grammatical

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Discussion paper

errors here and there, I suggest having it checked by a native English speaker), and the analysis is sound. I therefore recommend publication in ACP after the following comments have been addressed:

Specific comments

P. 4, l. 13: How efficient are the active carbon denuders? Please add information on that and the expected background. Is there any information on the evaporation of semi-volatiles from the particles after disturbance of the gas-particle equilibrium due to the denuders?

P. 6, l. 1 – 4: Why were the AAE values not calculated based on the actual data, or at least calculated and compared to literature data? Assuming an AAEFF of 1, and plotting/fitting babs vs wavelength (either averaged, or time-dependent, more appropriate here) can be used to derive AAEWB.

P. 6, l. 11-12: If site specific sigma_abs were calculated based on linear regression of babs against EC, doesn't that imply an overestimation of sigma_abs, as light-absorbing OC is not included in EC? Please clarify.

P. 6, l.13: CM could also be SOA from WB and FF; should be mentioned here.

P. 12, l. 2-3: This statement should be supported by references and more explanations.

P. 12, l. 29: Is the year-long time series correlated, or the diel evolution? I am assuming you are talking about the correlation of the time series. Apart from the non-optimal apportionment, a reason could also be, similar to the correlation of NOx and CMwb, a similar trend, but a different cause. CMFF is higher in winter than in summer, and so is NOx – potentially traffic emissions become more important in winter as well, or meteorological conditions favor the build-up of pollution episodes?

P. 13, l. 15: CMbio was assumed to be 0 for calculations of C1 and C2. Could this be a reason for the overestimation of CMFF?

Technical comments

P. 7, l. 14: typo in chloride (also in subsequent use of trimethylsilyl chloride) P. 7, l.16: typo, should be dichloromethane

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